

# *People, Land, and Politics*

DEMOGRAPHIC DEVELOPMENTS  
AND THE TRANSFORMATION  
OF ROMAN ITALY, 300 BC-AD 14

*Edited by*

L. DE LIGT & S.J. NORTHWOOD

## People, Land, and Politics

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Demographic Developments and the  
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## INTRODUCTION: NEW APPROACHES TO THE DEMOGRAPHIC, AGRARIAN, AND POLITICAL HISTORY OF THE MIDDLE AND LATE REPUBLIC

Until recently most historians of Italy during the last two centuries BC accepted a causal connection between imperial conquest, a vast enrichment of the Roman elite, a rapid increase in the number of urban and rural slaves, the gradual proletarianization of an ever-growing proportion of the Italian peasantry, and the political destabilization of the Republic after 133 BC. It was also thought that these developments were made possible or at least accelerated by the devastations of the Hannibalic War, which allowed the Roman elite to set up large slave-staffed estates on the vastly increased *ager publicus* of the post-Hannibalic period. In all this the heavy recruitment required for the wars in the East and in Spain was seen as a factor which contributed to the immiseration of the country-dwelling population. The land reforms initiated by Tiberius Gracchus were seen as a logical response to these developments and, more specifically, as an attempt to stem the numerical decline of the free peasantry from which the armies of the Republic were traditionally recruited.<sup>1</sup>

In recent years the validity of many assumptions underlying this reconstruction has been questioned. An important development which stimulated ancient historians and archaeologists to rethink the history of post-Hannibalic Italy was the emergence of survey archaeology. From the early 1970s onwards it was claimed that the fieldwalking campaigns carried out in South Etruria had revealed the presence of numerous farm sites of the second century BC, a finding which seemed to be at odds with the traditional view that this period witnessed the uprooting of the free peasantry and a decline of the free rural population.<sup>2</sup>

When fieldwalking campaigns were carried out in other parts of Italy, it also seemed to emerge that there were hardly any large-scale villas for the production of wine and olive oil before the early decades of the first century BC. This has contributed to the recent emphasis on good transportation locations as a vital prerequisite for intensive

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<sup>1</sup> E.g. Hopkins (1978); Cornell (1996).

<sup>2</sup> Frederiksen (1970–1).

farming on slave-run estates.<sup>3</sup> It has also been calculated that even in the Augustan period no more than a few hundred thousand slaves were needed to produce all the wine and all the olive oil consumed by the urban population of Italy.<sup>4</sup>

In recent years some ancient historians have also begun to dispute the theory that the heavy recruitment in the last two centuries BC and the sending out of large armies to theatres of war far away from Italy disrupted the traditional peasant economy and led many peasant families to give up their farms. To begin with, it seems likely that many rural families were of the extended type. This must have made it easier to cope with the temporary absence or even with the death of one or two male family members. There is also some comparative evidence to suggest that on farms without adult men women could become the temporary *de facto* heads of rural families and maintain production at adequate levels. Finally and perhaps most importantly, recent research emphasizes that the rural economy of republican Italy is likely to have experienced a high level of structural underemployment. Viewed in this light, military service may actually have had beneficial economic effects by removing surplus labour from the countryside and providing peasant families with employment opportunities outside agriculture that were potentially very remunerative.<sup>5</sup>

Yet another important challenge to the orthodox interpretation of mid- and late-republican history has appeared in the form of Elio Lo Cascio's 'high count' model of demographic developments between 225 and 28 BC. The cornerstone of this model is the assumption that the Augustan census figures are to be interpreted as referring only to adult male citizens.<sup>6</sup> If we adopt this reading, we must conclude that Italy had some 15 million inhabitants (including slaves) in the early years of the Principate. The logical corollary of this theory is that the post-Hannibalic period must have witnessed a very rapid expansion of the free country-dwelling population. It would then follow that the Gracchi were faced with the onset of a Malthusian crisis of overpopulation rather than with a gradual decline of the free Italian peasantry.<sup>7</sup>

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<sup>3</sup> De Neeve (1984); Morley (1996).

<sup>4</sup> De Ligt (2004); Scheidel (2005).

<sup>5</sup> Rosenstein (2004).

<sup>6</sup> Lo Cascio (1994); Lo Cascio and Malanima (2005).

<sup>7</sup> Lo Cascio (2004).

Although these new perspectives have been set out and developed in many articles, no new synthesis of mid- and late-republican history has been attempted. One reason for this must be that the task of assessing the strengths and weaknesses of these new approaches is complicated by the large number of questions that must be looked at afresh. Precisely for this reason, the Department of Ancient History of Leiden University decided to organize a three-day conference which would bring together a wide range of specialists on various aspects of mid- and late-republican history in an attempt to clarify the most important issues at stake and explore some of these issues in the light of the new theories referred to above. Because the Gracchan land reforms have been a pivotal element in all previous reconstructions, it was decided to devote a separate session to this topic. Up to a point the theme of the conference as a whole can be described as ‘the Gracchi in context’. However, it must be emphasized that instead of focusing narrowly on the decades preceding and following the passing of the *lex Sempronia agraria* of 133 BC, the conference aimed to place the events of the 130s and 120s BC in a variety of contexts spanning several centuries. This explains why the temporal limits of the topics covered in this volume range from the passing of the *lex Licinia de modo agrorum* of 367 BC all the way to the Augustan period. The editors of this volume feel that the overall aim of the conference not only justifies this wide scope but in fact makes it absolutely necessary.

Since almost all new interpretations of the economic, social, political, and military history of the Middle and Late Republic which have been attempted in recent years are connected with theories concerning Italy’s demographic make-up, the volume begins with four papers on population dynamics and the development of the urban network. In a long survey of the recent literature on Italy’s population history Walter Scheidel explores some of the strengths and weaknesses of the ‘low count’ and ‘high count’ models. He argues that many features, such as urbanization rates and military participation rates, do not favour either the low count or the high count. At the same time slave imports, the high costs of military recruitment, and elevated living standards (as reflected in average body length) would seem to support the low count. This leaves the much higher population of Italy during the High Middle Ages as the main challenge to the low count.

Scheidel’s argument that the high standard of living which seems to have characterized early-imperial Italy cannot be squared with a scenario of population pressure is challenged by Geoffrey Kron, who

argues that high standards of living in the towns of Roman Italy created a strong market for meat, which made it possible for Roman farmers to set up mixed farms based on convertible husbandry and the cultivation of grapes, olives, and industrial crops alongside cereals. Since the presence of numerous animals provided Roman farmers with large amounts of manure, they were able to obtain excellent yields on their modestly-sized holdings. On this view high living standards, rather than being at odds with the existence of a large population, would actually be one of its preconditions. This optimistic view of the Italian peasant economy leads the author to cast doubt on the reality of the agrarian crisis supposedly lying behind the Gracchan land reforms. In his view the archaeological evidence demonstrates the survival of many small and medium-sized farms in the Italian countryside.

Where Kron focuses on the countryside, Neville Morley discusses urbanization. As he points out, the tendency of ancient historians to define towns on the basis of legal criteria has had the unfortunate consequence of making most attempts to compare urbanization rates in Roman and late-medieval Italy completely meaningless. More generally, Morley argues that instead of trying to identify towns in what was essentially a continuum of larger and smaller settlements, we should concentrate on the causes and implications of processes of concentration, crystallization, integration, and differentiation in mid- and late-republican Italy. He argues that the concentration of population in urban centres may help us to explain some of the upheavals of the late Republic, because towns seem to have developed faster than the political institutions and economic structures needed to sustain them. He also emphasizes that the political integration of Italy, in which towns played an important part, created the networks and structures which the Gracchi exploited in order to challenge the traditional elite.

De Ligt also looks at Italy's urban system but from a demographic point of view. Focusing on Cisalpine Gaul, he argues that the archaeological evidence presently available makes it impossible to arrive at a reliable population estimate for this region using inductive methods. Nonetheless he finds it significant that the towns of Roman Cisalpina, which he defines as all settlements covering 20 or more hectares, were much smaller than those of the late-medieval and early-modern period. Assigning between 120 and 150 inhabitants to each urban hectare, he argues that the high count can be maintained only by assuming that only 4–5% of the North-Italian population lived in 'towns'. In fact, even the low count implies a northern urbanization rate not much higher

than 12%. De Ligt interprets this as an indication that the low count is more likely to be correct.

A second group of papers looks at the numerical data which have been preserved in the literary tradition and in the epigraphic record. In a wide-ranging contribution Saskia Hin argues that the republican census figures are to be interpreted as referring only to adult male citizens *sui iuris*. She also disputes the traditional view that the census figure for 234 BC and the manpower figure given by Polybius for ‘the Romans and Campanians’ are *in pari materia*. Arguing that the Polybian manpower figures refer to *iuniores* only, she demonstrates that a subpopulation of 273,000 *iuniores* implies a citizen population only one eighth higher than that implied by a subpopulation of 270,713 adult male citizens *sui iuris*. More importantly, she goes on to defend the hypothesis that the Augustan census figures can be explained by assuming that these were the first figures to comprise all citizens *sui iuris*, including wards and widows and perhaps even married women *sui iuris*. This reinterpretation results in an Italian population of between 7.5 and 10 million, significantly higher than the low count but far below the high count.

While Hin accepts the census figure for 234 BC as representing more or less accurately the number of adult male citizens *sui iuris*, Elio Lo Cascio questions the usefulness of the republican census figures, especially those for the second century BC, as a basis for any demographic reconstruction. As he points out, the size of the citizen body was determined not only by purely demographic factors but also by many other variables, such as the number of citizen-soldiers serving abroad, the registration of immigrants from Latin communities, and the bestowal of full citizenship on communities of *ciues sine suffragio*. While most low counters would accept the relevance of these distorting factors, Lo Cascio also argues that the existence of a centralized census procedure and the likelihood that most proletarians remained unregistered resulted in a very low overall registration rate. He also disputes the view that the threshold for membership of the fifth class was lowered in 141/0 or 130/129 BC and that this led to more (former) proletarian citizens being registered by the censors. Taken together, his arguments imply that the republican census figures can be used only to estimate the minimum number of adult male citizens at any given time.

As far as the registration rate is concerned Lo Cascio’s findings clash directly with those of Simon Northwood, who offers a detailed reconstruction of the relationship between census taking and the imposition

of *tributum*. His main points are that the property valuations declared at the census were genuine market values, and that only citizens in the five Servian classes are likely to have paid *tributum*. In arguing in favour of this dual thesis, he draws attention to the fact that declarations had to be made in public. In his view this must mean that only the truly landless were able to disappear, so that the proportion of the census population which remained unregistered would have been much lower than envisaged by Lo Cascio.

A common theme which emerges from the articles by Hin, Lo Cascio, and Northwood is that our views on the efficiency of census taking are determined by our answers to a number of technical questions which have often been ignored in recent publications but are of vital importance for those aspiring to make a realistic assessment of the strengths and weaknesses of the various demographic models which have been proposed.

A third theme covered in this volume is the dialectic relationship between demographic history and the interpretation of archaeological field surveys. In the first article of this volume Walter Scheidel expresses skepticism regarding the use of archaeological data in assessing the strengths and weaknesses of competing demographic models. However, his criticisms are directed mainly against those who have followed a bottom-up approach in reconstructing the population histories of various Italian landscapes. As the five archaeological papers in this volume demonstrate, this is certainly not the end of the story. In the first of these papers Rob Witcher discusses the many pitfalls encountered by those who have tried either to refute or to uphold the notion of a demographic downturn in the Italian countryside in the second century BC on the basis of the survey data from South Etruria. In doing so, he focuses on the results of the recent Tiber Valley Project, which have confirmed Liverani's finding that most of the blackslip pottery recovered from 'small' sites in this area belongs to the third rather than to the second century BC.<sup>8</sup> It is tempting to infer from this that, at least in this part of Central Italy, there was a 'crisis of the second century BC' after all. However, as Witcher points out, the scarcity of Late Republican 1 material and the sampling techniques which were used in South Etruria make it impossible to state emphatically that there was a reduction in the number of small sites during this period.

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<sup>8</sup> Liverani (1984).

Witcher also makes the important observation that instead of trying to derive population figures from site numbers, archaeologists should concentrate on the various assumptions concerning the archaeological evidence implied by competing demographic models. As he points out, the low count implies relatively high recovery rates which can only be explained if rural populations were well integrated into urban and regional economies through the consumption of finewares. By contrast, the low recovery rates implied by the high count would point to impoverished subsistence peasants with minimal economic contact with urban markets. There can be no doubt that this is a promising avenue for further research.

The problem of recovery rates is also central to two other contributions to this volume. In the first of these Dominic Rathbone offers a fundamental critique of the tendency of archaeologists to make a simple distinction between large and small sites. As he points out, there is a good deal of literary evidence for peasants living in huts (*tuguria*) built with perishable materials. He also offers a detailed discussion of the handful of small and medium-sized republican farmhouses which has been excavated. Even the small sample presently available is enough to reveal that there was a broad spectrum in size of farmsteads. It also appears that most of the very few small farms which have been thoroughly investigated were relatively solid structures of the Gracchan and triumviral periods. Since small farms of this type are unlikely to have been the norm in earlier periods, and also because small farms appear to have had limited access to finewares, we must conclude that the vast majority of the *tuguria* described in the written sources have escaped detection.

In a closely related paper Jeremia Pelgrom focuses on another possible explanation for the extremely low recovery rates implied by the survey data collected in the territories of Cosa and many other Latin colonies. In his view the small size of the urban centres of these territories rules out the possibility that the majority of the colonists lived within the town walls, especially because very few traces of dwellings of the fourth and third centuries BC have been discovered in these towns. Why then have the Latin colonists of the Middle Republic escaped detection? Pelgrom argues that part of the explanation must be that the early colonists preferred to live in hamlets and villages. As he points out, quite a few such nucleated settlements have been discovered in recent survey campaigns. Many others must have been located on hill tops offering natural protection and are therefore likely to have escaped

detection by archaeologists looking for concentrations of sherds in fertile fields. Like Witcher's and Rathbone's methodological observations, this is another valuable pointer for further research.

In a general discussion of the demographic picture emerging from the extensive archaeological campaigns carried out in Apulia, Douwe Yntema argues that a combination of high-intensity urban surveys, high-intensity rural surveys, and excavation of selected key sites makes it possible to arrive at a rough estimate of the population of the Messapian districts of Apulia and indeed of Apulia as a whole. If we assume that intensive surveys have discovered *c.* 50% of all farmsteads in the investigated areas and if we add the very large number of Messapians who must have lived in towns, we obtain an estimate of between 126,000 and 163,200 Messapians. As Yntema points out, this crude figure is compatible with the manpower figures given by Polybius if these are interpreted as referring to adult males aged between 18 and 45, and if it assumed that Messapians made up between 40% and 45% of the 56,000 able-bodied men that the 'Iapygians and Messapians' were theoretically able to put in the field in 225 BC. An interesting feature of the demographic reconstruction underlying these calculations is that it assigns between 80% and 90% of the Messapian population to towns. As Yntema realizes, this must imply that the urban agglomerations of Messapia were essentially agro-towns a large proportion of whose population was engaged in agriculture. This picture is intriguingly similar to the demographic reconstructions which John Bintliff and Mogens Hansen have advanced for classical Greece,<sup>9</sup> and very different from the settlement pattern postulated by De Ligt for late-republican Cisalpina.

The last archaeological contribution in this volume, by Maurizio Gualtieri, offers a panoramic view of developments in Lucania between the third and first centuries BC. In the Mingardo/Bussento region the most striking change is the desertion of the *oppidum* of Roccagloriosa. However, the survival of numerous farms and hamlets suggests that the fate of this Lucanian settlement resulted from a drastic reconfiguration of the region's administrative and economic structures, which is likely to have been associated with the foundation of Buxentum, rather than from a general demographic collapse. Although the material from some other parts of Lucania is of uneven quality, most of it points in the same

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<sup>9</sup> Bintliff (2004, 211); Hansen (2006).

general direction. In the case of Volcei signs of continuing prosperity have been detected both in the town and in the countryside. There are also indications that the farms of the Gracchan settlers who received allotments in the Vallo di Diano continued to flourish well into the early Principate. Finally, villas and villages appear to have coexisted in the area of Oppido Lucano. All in all, these scattered data reveal the old picture of Lucania becoming dominated by a slave-based plantation economy to be highly inaccurate.

The most important lesson to be learned from these five archaeological papers is perhaps that approximately fifty years after the appearance of Italian survey archaeology there is still plenty of room for new approaches. One reason for this is the enormous advance in methodological sophistication that has been achieved since the late 1950s. Another is an acute awareness that the written sources may be a poor guide to the complex realities in the Italian countryside, and also the realization that population trends and the evolution of settlement patterns are likely to have followed very different trajectories in different parts of post-Hannibalic Italy.

Two further important topics for those interested in the demographic composition of Italy are migration and developments affecting the demographic balance between citizens and allies. As Paul Erdkamp points out, comparative data suggest that people in premodern societies were far more mobile than previously thought. In the case of republican Italy this would mean that mobility is unlikely to have been confined to state-sponsored migration to colonies and voluntary migration to Rome. Focusing more specifically on migration to the capital, Erdkamp argues that Rome is likely to have attracted many seasonal migrants in search of temporary jobs, and also that the sex ratio in the city must have been heavily skewed in favour of males because there were few employment opportunities for freeborn women. Partly for this reason he upholds the traditional view that the fertility rate in Rome was far too low to offset mortality, implying that large-scale migration was needed to sustain the capital's population.

Addressing another aspect of migration, William Broadhead points out that the system used to recruit allied manpower and certain features of the political system in Rome were based on the unrealistic assumption that people would continue to live where they had been born. This mentality helps to explain why Rome preferred to send back large numbers of Latin immigrants rather than reduce the military burden imposed on the Latin communities concerned. Broadhead also suggests

that the Gracchan land reforms can be seen as an attempt to redistribute citizens over the *ager Romanus* and the tribes, and also that the decision to make the allotments of the Gracchan settlers inalienable by sale may have been prompted by the wish to keep these settlers in place.

Manpower concerns of a different kind are at the centre of Henrik Mouritsen's contribution. As he observes, the numerical balance between Romans and allies is likely to have been a matter of concern for the political elite in Rome. In theory the number of citizens eligible for service in the legions could be increased by lowering the threshold for membership of the fifth class or by assigning land to proletarian citizens. There was, however, a third possibility which has not received sufficient attention: the number of citizen-soldiers could also be increased by bestowing the (full) Roman citizenship on the *ciues sine suffragio* and on the Latins, most of whom were descendants of Roman citizens. In Mouritsen's view this is the key to Flaccus' proposal to grant the citizenship to certain categories of allied communities. If one of Flaccus' aims was to bring back the Latins into the citizen body, this would have had the most welcome effect of altering the internal demographic and military balance in Rome's favour.

As noted above, all reconstructions of Italy's demographic and agrarian history during the last two centuries BC have to take account of the literary tradition concerning the Gracchan land reforms. One illustration of this is Lo Cascio's theory that the *lex Sempronia agraria* of 133 BC was issued with the aim of resolving a social crisis caused by fast population growth (cf. above). In other words, the formulation of new quantitative models in the field of Roman demography is bound up inextricably with attempts to read the literary sources in a new light and also with the need to reassess the reliability of the literary tradition concerning the role of state-owned land in the republican economy. In this volume these topics are dealt with in three closely related articles, each of which looks at different aspects of the historiographical tradition (both ancient and modern) concerning the Gracchan land reforms and their background.

In the first of these articles Daniel Gargola undertakes an in-depth analysis of Appian's account of the background to the promulgation of the *lex Sempronia agraria*. His main contention is that although almost every single element in this account can be paralleled elsewhere, Appian has assembled the basic facts into a unified story which is clearly designed to present the unprecedented land reforms of 133 BC as being in keeping with traditional Roman policies reaching back as far

as the fourth century BC. In order to achieve this goal, Appian (or his source?) appears to have projected into the past not only the agrarian conditions of the second century BC (including the existence of large slave-staffed estates) but also the goals pursued by Tiberius Gracchus and the new legal categories created by his agrarian law. Gargola concludes that because of these distortions Appian is a very problematic guide to Roman practices governing public lands or to social and economic conditions in any period of republican history, including the second century BC. At most Appian reveals how some people perceived or presented conditions in the countryside.

In another paper John Rich focuses on the scope of the *lex Licinia* of 367 BC, which is reported by Livy and other sources to have declared it illegal for Roman citizens to hold more than 500 *iugera* of land. In almost all of the countless publications on the *lex Licinia* which have appeared since the early 1860s this ban is presented as affecting only holdings of *ager publicus*. In the first part of his article Rich shows that this interpretation was not shared by some distinguished scholars of the early-modern period, including Machiavelli and Montesquieu, who supposed that the law of 367 BC referred to all landholding. Rich also demonstrates that the alternative view that the Licinian law dealt exclusively with *ager publicus* goes back all the way to the writings of Carlo Sigonio but did not become dominant until the publication of Niebuhr's *Römische Geschichte*. Even then the theory that the *lex de modo agrorum* of 367 BC affected all types of land continued to be defended, for instance by Huschke, until Niebuhr's interpretation was endorsed by Mommsen. In the second half of his contribution Rich goes on to argue that Machiavelli's and Huschke's interpretation is to be preferred not only to Niebuhr's reading but also to the theory that the *lex Licinia* applied only to land held in private ownership. An interesting implication of his thesis is that Tiberius Gracchus made no attempt to revive the Licinian law in its original form, opting instead to apply the old and no doubt obsolete maximum only to holdings consisting of certain types of state-owned land.

The section on *ager publicus* ends with a paper by Saskia Roselaar, who sets out to reassess the reliability of the literary tradition from a different angle. Tracing the history of various types of state-owned land between the fourth and second centuries BC, she argues that in most parts of Central Italy the so-called *ager occupatorius* which looms so large in Appian had almost disappeared by the beginning of the second century BC. Even if we allow for the development of slave-staffed

villas on *ager quaestorius* and *ager in trientabulis*, it seems to follow that most farms of this type must have been set up on private land. In her view it was the growth of such privately owned estates, combined with a slow but steady increase in the number of rurally based citizens, which lay behind the social problems encountered by the Gracchi. If this reconstruction is accepted, Appian's quasi-exclusive focus on *ager occupatorius* is deeply misleading. Roselaar's explanation for this distortion is that the surviving literary tradition has been heavily influenced by the speeches of the Gracchi, who are likely to have focused on *ager occupatorius* because this was the only type of land which could be redistributed and also because this enabled them to cite the *lex Licinia* of 367 BC as a precedent. This analysis is fully compatible with the findings of Gargola and Rich, and also establishes an interesting link between the history of the republican *ager publicus* and the ongoing debate concerning Italy's demographic composition during the last two centuries BC.

The volume ends with two papers which explore the demographic dimensions of the disintegration of the Republic from the final decades of the second century BC onwards. Nathan Rosenstein approaches this topic by considering the applicability of Jack Goldstone's theory that demographic growth and its economic, social, and political ramifications were a key factor behind the English and French revolutions of 1642 and 1789. Focusing on various aspects of this model, Rosenstein argues that the absence of large-scale epidemics and a reduction of military commitments caused the number of young adult men to bulge in the crucial period between 133 BC and 91 BC. At the same time an increase in the number of young aristocrats is likely to have intensified intra-elite competition. A third factor was a growing perception of corruption among members of the senatorial order. In Rosenstein's view this last development was particularly harmful in a premodern society which possessed no bureaucracy to speak of. Without minimizing the many differences which existed between late-republican Italy and pre-revolutionary England and France, he argues that these developments go a long way to explaining the breakdown of the Republic.

In the final contribution Michael Crawford calls attention to the presence of a very large number of Roman citizens outside Italy as another factor which helps to explain the political and military developments of the first century BC. As he points out, the years between 88 BC and 49 BC witnessed the emergence of what he calls 'alternative empires' in Spain, Gaul, and parts of the East, which were outside the

control of the senate for prolonged periods. The dynasts governing these peripheral empires had their own armies and *consilia*, and also conducted independent negotiations with foreign kings. According to Crawford these alternative states could function only because a vast number of Romans and Italians had settled in the provinces from the middle of the second century BC onwards. He also points to evidence for regional dynasts bestowing the Roman citizenship on large numbers of indigenous inhabitants. It will be clear that this thesis has interesting implications for the debate between low counters and high counters. As Crawford observes, the high-count interpretation of the Augustan census figures can be reconciled with a low-count reconstruction for Italy if we assign *c.* 7 million citizens to the provinces. Even though this spectacular suggestion is essentially a thought experiment, there can be no doubt that it will stimulate both low counters and high counters to rethink not only the scale of emigration from Italy but also the quantitative importance of the creation of new citizens by powerful Roman patrons in the provinces.

Taken together, the twenty essays in this volume provide a wealth of new perspectives not only on the demographic, social, and legal background to the Gracchan land reforms but also on the demographic, economic, social, and political history of the Middle and Late Republic generally. The editors hope that the many new ideas which are presented by the contributors will be picked up soon by other specialists working on pre-imperial Italy and will stimulate them to join the lively debate concerning the multi-faceted impact of demographic developments on Roman Italy during the last two centuries BC.

Luuk de Ligt  
Simon Northwood

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I

DEMOGRAPHY



## ROMAN POPULATION SIZE: THE LOGIC OF THE DEBATE

Walter Scheidel

### I. *Roman population size: why it matters*

Our ignorance of the size of ancient populations is one of the biggest obstacles to our understanding of Roman history. After generations of prolific scholarship, we still do not know how many people inhabited Roman Italy and the Mediterranean at any given time. When I say we do not know, I do not simply mean that we lack numbers that are both precise and known to be accurate: that would surely be an unreasonably high standard to apply to any premodern society. What I mean is that even the appropriate order of magnitude remains a matter of intense dispute. This uncertainty profoundly affects modern reconstructions of Roman history in two ways. First of all, our estimates of the overall Italian population are to a large extent a direct function of our views on the size of the Roman citizenry, and inevitably shape any broader guesses concerning the demography of the Roman empire as a whole. These guesses determine in turn how we assess Roman conditions in relation to other, later periods of Mediterranean population history. Secondly, and moreover, this is by no means an antiquarian issue, a case of wanting to know for the sake of filling in blanks in our knowledge. Absolute and relative population numbers matter greatly for the simple reason that they are critically related to key variables of development, such as economic performance: a 'large' population (by premodern standards) might imply a 'strong' economy (by the same standards), or, alternatively, might suggest relatively low living standards. Since it is impossible for us to measure Roman GDP directly from actual evidence, and difficult, though perhaps not entirely impossible, to ascertain living standards, a better understanding of population size is essential for our appreciation of Roman economic performance and human development. This would help us to account for the limits of Roman growth and the ultimate failure of the Roman world. This information is also required in order to relate the Roman experience to larger historical patterns, and to choose between an essentially linear view of historical

development, characterized by gradual long-term growth in economic output and population density, and a more cyclical model in which early peaks might match or even exceed later phases of expansion (most notably, the Roman period vis-à-vis the High Middle Ages or even the early modern age). Only comparisons of this kind would enable us to gauge the relative significance of specific contextual conditions, such as the aggregate benefits of reduced transaction and information costs engendered by pan-Mediterranean political unification and centuries of ecumenical peace and stability.

## II. *Purpose and method*

For all these reasons, a better understanding of Roman population size is a vital concern for ancient and indeed all of premodern history well beyond the ambit of the recent Leiden project with its focus on Italy during the last two and a half centuries BC.<sup>1</sup> At the same time, the Leiden initiative calls for a broader vision of Roman demography which would allow us to contextualize more specific findings and claims. In order to bring us closer to this goal—and to show how far we still have to go to reach anything like a consensus—I provide a critical assessment of the current state of the debate that does not seek to advance a particular interpretation but instead aims to identify the strengths, weaknesses, and logical corollaries of competing reconstructions. This approach is meant to serve several purposes. In keeping with the dominant conventions of scholarly discourse, existing contributions usually strive to make a case for a particular version of Roman population history, and in so doing tend to give disproportionate weight to data or readings that favor their own argument and weaken others, making it hard for non-specialist observers to gauge the relative merits of conflicting claims. Moreover, the debate has all too often focused on individual source references or narrow technical points without giving full consideration to the various logical implications of a particular position. All specific arguments about Roman population need to be evaluated within a more general historical context. Ideally, this exercise ought to be performed by a disinterested party with no stake in ongoing

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<sup>1</sup> 'Peasants, citizens and soldiers: the effects of demographic growth in Roman Republican Italy (202–88 BC)', University of Leiden, 2004–2009. Despite its title, the project covers the period from 225 BCE to 14 CE.

debates who is nonetheless intimately familiar with their details. I am not sure if such a person exists, and there is no denying that I am on record as having taken sides, and even that I continue to find certain readings more plausible than others. Against this background, my presentation is bound to be slanted one way or another; then again, much the same would probably be true of potential alternative accounts. The best I can do is to make explicit some problems and implications that do not always receive proper attention, even if this makes it harder to answer key questions. If this survey can help my colleagues make up their own minds, it will have served its purpose.

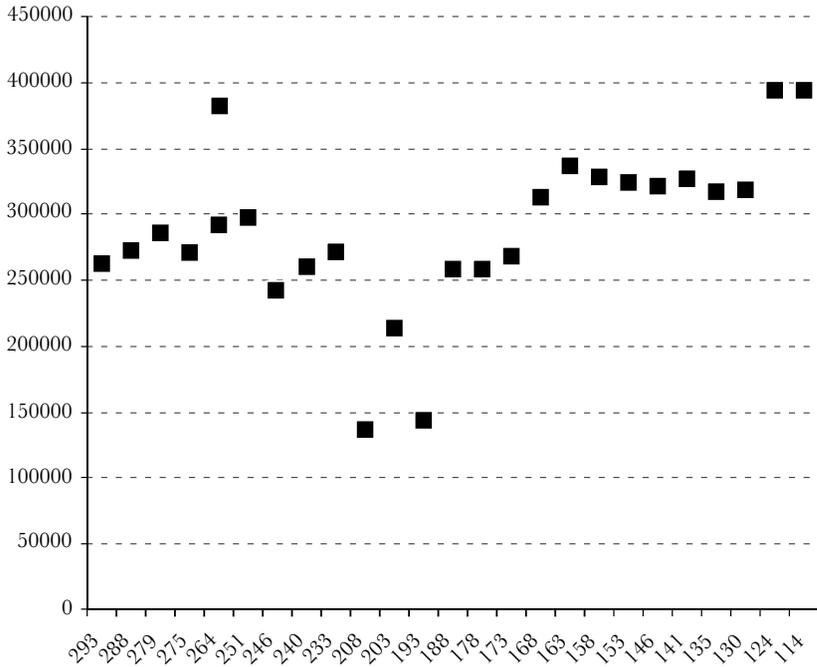
A few words about organization. After setting out the main object of the debate, I weigh the merits of competing claims by focusing on a number of features associated with Roman population size: urbanization, military service, labor markets, internal conflict, living standards, settlement patterns, and ecological conditions. My survey concludes with a look at comparative population data from antiquity and later periods. I choose this approach in the hope of clarifying the terms of the debate by establishing the potential of specific variables to contribute to our understanding of the size of the Roman population: while commonly examined bodies of data can be shown to be of little or no relevance to this issue, consideration of other, previously neglected aspects needs to be elevated to a more prominent position.

### III. *Roman population counts*

Modern controversy about Roman population size stems from the fact that surviving tallies, if taken at face value (i.e. if thought to apply to the same reference group), are impossible to reconcile with one another. The basic problems have been set out at great length many times before and need not be recounted here in detail.<sup>2</sup> To summarize very briefly, Roman sources dating from the first century BCE to the fourth century CE, but presumably drawing on earlier records, report citizen head counts for twenty-five different occasions from the beginning of the third century BCE to the end of the second century BCE. Unamended, these totals range from 137,000 to 395,000 registered individuals. The distribution of the data suggests a measure of corruption in the manuscript tradition

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<sup>2</sup> See esp. Brunt (1971/1987, 15–120); Lo Cascio (1994a).



Source: Brunt (1971/1987, 13).

Fig. 1. Reported census tallies, 294/3 to 115/4 BCE.

(Fig. 1), which speaks against retention of the two lowest and one of the highest of these figures. Alternatively, one might prefer to regard sudden—and demographically impossible—fluctuations as a function of recording practices which were contingent on the execution of each particular census. Both explanations have intrinsic merit: while Latin numerals were highly susceptible to corruption by scribal copying, early Chinese census tallies, with their sudden wild swings,<sup>3</sup> show that the results of such counts could at times be dramatically influenced by the circumstances of the recording process.

We are left with the general impression that, discounting rare outliers,<sup>4</sup> these totals fluctuate within a band from 214,000 (using the figure for 204/3 BCE, which is the lowest that is not completely incompatible with surrounding figures) and 395,000 (125/4 and 115/4 BCE),

<sup>3</sup> Cf. Bielenstein (1987).

<sup>4</sup> 137,108 (for 209/8 BCE) and 143,704 (for 194/3 BCE).

and that most of them (if we disregard for a moment the highest and lowest of the demographically possible tallies)<sup>5</sup> fall in a much narrower bracket from 242,000 to 337,000. The mean for the demographically possible tallies (using one total each for 23 events)<sup>6</sup> is 297,000, and the median is 292,000.

This method establishes a rough order of magnitude for the third and second centuries BCE, with a ‘trend tally’ of close to 300,000 that could move up or down due to military attrition and/or intermittent variation in registration quality or coverage. (I ought to stress that this is a ‘trend tally’ for the *census population*, and not necessarily for the citizen population that actually existed at those dates: it is essential to keep this distinction in mind.) Reported numbers soared in the following century, to 463,000 in 86/5 BCE; 900,000 or 910,000 in 70/69 BCE; and 4,063,000 in 28 BCE. Later tallies conform to the last of these counts, creating a gently rising plateau of 4,233,000 in 8 BCE; 4,937,000 in 14 CE; and 5,984,072 in 47 CE. In view of the enfranchisement of the Italian allies after 89 BCE and of Gallia Transpadana in 49 BCE, we would expect a strong increase in the number of citizens in this period. However, the recorded increase between 70/69 BCE and 28 BCE is so dramatic that it cannot be explained in this way alone:<sup>7</sup> either registration prior to 28 BCE had been massively deficient, thereby creating an inflated impression of the growth in citizen numbers between the mid-80s BCE and early 20s BCE, or the mode of registration had changed from 28 BCE onward and census tallies had come to include a larger share of the citizen population than before.

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<sup>5</sup> 214,000 (for 204/3 BCE) at the low end, and 382,233 (alternative tally for 265/4 BCE), 394,736 (for 125/4 BCE), and 394,336 (for 115/4 BCE) at the high end.

<sup>6</sup> I define a demographically possible tally as one that can be reconciled with the tallies preceding and following it. The two records cited in n. 4 cannot be reconciled with much higher counts in the same periods. The 382,233 reported for 265/4 BCE can be defended only by considering all immediately preceding or following counts to be marred by massive under-reporting, and is not deemed demographically possible here.

<sup>7</sup> Even the increase from 115/4 to 28 BCE is hard to credit if we take the tallies at face value: even if the reported high tally for 115/4 BCE were correct and if Italian allies and Transpadanians had outnumbered Roman citizens by a factor of four, the number of citizens would have had to double through natural growth, manumission of slaves, and enfranchisement of provincials in order to raise the tally from 400,000 in 115/4 to 4 million in 28 BCE. And even if this were to be accepted, it would imply that all counts prior to 125 BCE were massively deficient. In other words, there is no way of accepting all of these tallies at face value.

IV. *Competing interpretations*

As is well known, both interpretations have been forcefully advanced by modern scholars. Karl Julius Beloch and Peter Brunt are the most prominent exponents of the view that whereas the republican census results refer to all male citizens aged 17 and over, Augustus modified these reports to include women and children as well, thereby creating much larger totals for the official record.<sup>8</sup> No such switch in reporting practices is explicitly attested in our sources.<sup>9</sup> For a variety of reasons that have been set out elsewhere, most notably in Brunt's massive account, this reading requires us to accept a whole series of assumptions: that the allied population outnumbered the Roman citizenry by less than two to one in the early first century BCE; that Transpadane Gaul was sparsely settled and did not account for more than a quarter of the free population of Italy in the same period; that natural population growth between 70/69 and 28 BCE was at best very limited, or even nil or slightly negative; and that republican census counts were at least as accurate as the later Augustan tallies, or even more so.<sup>10</sup> All these auxiliary assumptions are logically necessary in order to sustain the Beloch-Brunt reading of the census data. None of them, however, can be independently verified or falsified with the help of ancient evidence: their acceptance or rejection is contingent on probabilistic claims.

As I have argued on a previous occasion, allowing for a certain amount of under-registration, this reading is consistent with an Italian

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<sup>8</sup> Beloch (1886, 370–8); Brunt (1971/1987, 113–20).

<sup>9</sup> It seems to me rather fruitless to argue about the intrinsic plausibility of such a change. Scholars have pitted arguments emphasizing Augustus' conservatism (which speaks against any changes: e.g. Lo Cascio (1994a, 31 and n. 52); Kron (2005, 456–7)) against others that highlight the long abeyance of the census (especially since we do not strictly speaking know how the Augustan census results were publicized prior to 14 CE: Scheidel (2004, 5)), parallels with provincial censuses (which might have provided a model for the suggested adjustment), references in Augustan and post-Augustan texts that may—but need not—be read as implying that readers were familiar with the practice of including women and children in census counts (Beloch 1886, 342 and 376); Brunt (1971/1987, 113 n. 2), and a variety of other reasons why there is no need to exaggerate the supposed novelty of such a measure (De Ligt 2007, 178–81). The heart of the matter is that none of these claims is ultimately testable: they are a matter of taste. It is true that the most economical default position would favor continuity over an undocumented switch. We also need to bear in mind, however, that ancient historiographical coverage of the Augustan period is relatively poor, and arguments from silence are bound to be correspondingly weak.

<sup>10</sup> Brunt (1971/1987, 97 [allies], 117 with 198–203 [Transpadana], 121–30 [growth], 116 [census accuracy]).

population of maybe 3.9–4.2 million citizens in 28 BCE and 4.4–4.8 million citizens in 14 CE, or a grand total for Italy of somewhere around 5.5–6.5 million (including 1–1.5 million slaves and some free aliens).<sup>11</sup> Subsequent developments are a matter of conjecture: if the Augustan rate of increase in the number of citizen residents of Italy implied by this estimate had continued until the time of the census of 47 CE and the number of slaves and aliens had remained stable, the Italian population might have numbered between 6 and 7 million in the mid first century CE. Even with a lower and continually slowing post-Augustan growth rate, the Italian population could well have peaked at 7 or even 8 million by the late first or early second centuries CE.<sup>12</sup>

Back in 1886 Beloch linked his estimate of some 6 million people in Augustan Italy to one of 54 million in the Roman empire as a whole.<sup>13</sup> With some modifications, this (highly conjectural) reconstruction was most recently accepted by Bruce Frier, who posited populations of 7 million for Italy and 45.5 million for the empire in 14 CE, and of 8.6 million and 61.4 million respectively in 164 CE.<sup>14</sup> The last of these guesses falls far short of Beloch's subsequent preference for an imperial population of up to 100 million in the second century CE.<sup>15</sup> Frier's version of the Beloch-Brunt model accords Italy a significantly higher population density than any of the provinces other than Egypt, Syria-Palestine, and Cyrenaica, which foreshadows higher Italian population densities (in a European context) in the medieval and early modern periods.<sup>16</sup>

The main alternative to the interpretation that suggests a Roman Italian population in the order of 6 to 8 million in the early monarchical period—which I have dubbed the 'low count'—is represented by

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<sup>11</sup> Scheidel (2004, 9 [citizens]); (2005a, 64–71 [slaves]). My main adjustment of Brunt's estimate of 7.5 million people in Italy in 14 CE concerns the number of slaves, which must have been much smaller than assumed by him (his 3 million is a pure guess: Brunt 1971/1987, 124–5) or Beloch (1886, 416: two million).

<sup>12</sup> Beloch (1886, 507) posited a gross total of 6 million in 14 CE and 7 million in 47 CE (*ibid.* 437; and see also Beloch 1903) but allowed for substantial growth all over the empire later on: Beloch (1899, 619–20). Brunt (1971/1987) does not cover the post-Augustan period.

<sup>13</sup> Beloch (1886, 507).

<sup>14</sup> Frier (2000, 812 table 5; 814 table 6) largely based (with some adjustments) on McEvedy and Jones (1978) for 14 CE and the schematic assumption of a mean annual growth rate of 0.15% from 14 CE to 164 CE.

<sup>15</sup> Beloch (1899, 620).

<sup>16</sup> Cf. Lo Cascio and Malanima (2005, 210).

the readings of Tenney Frank, Elio Lo Cascio, and Geoffrey Kron that consider the monarchical census tallies to refer to adult male citizens.<sup>17</sup> This approach requires us to adjust the reported census figures by a multiplier in order to arrive at the overall size of the citizenry: model life tables suggest that men aged 17 and over would have comprised roughly one-third of a high-mortality population, which implies that citizens were at least three times as numerous as indicated by the census counts:<sup>18</sup> in other words, a ‘high count’.

Back in 1924 Frank assigned to Italy 3,500,000 of the 4,063,000 adult males that he thought had been recorded in the census of 28 BCE, extrapolated from this number a total ‘free’ (in this context, citizen) population of 10 million, and speculatively added 4 million slaves.<sup>19</sup> He did not offer any conjectures for later censuses. However, if we take a cue from Frank’s statement in 1940 that, with respect to the distribution of Roman citizens at the time of all three Augustan censuses, “at least 80–90% lived in Italy”,<sup>20</sup> this assumption logically entails the presence of between 4 and 4.5 million adult males in Italy in 14 CE, for a grand total of somewhere between 15.5 and 17 million Italians including slaves at that time.

In 1994 Lo Cascio raised the possibility of an Italian gross population of 14–16 million under Augustus but argued more specifically for the presence of 13.5 million citizens in 28 BCE and 16.4 million in 14 CE—“ipotizzando trascurabile la percentuale degl’*incensi*”—of whom 12,250,000 lived in Italy in 28 BCE and 14,470,000 did so in 14 CE.<sup>21</sup> In this article Lo Cascio gives no estimate for conditions in Italy in 47 CE but thinks that by then the total number of citizens had reached 20 million: while he maintains that this increase must have been fed by

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<sup>17</sup> Frank (1924 and 1940, 1); Lo Cascio in multiple works, most notably (1994a), (1994b), (1996), (1999a), (2001), and again in Lo Cascio and Malanima (2005); Kron (2005b).

<sup>18</sup> Coale and Demeny (1983, 57 and 108): at  $e_0 \sim 25$ , 30–31.5% of a given population consist of men aged 17+. The ‘South Europe high mortality life table’ of Woods (2007, 379) implies a marginally smaller proportion at this level of  $e_0$ . By tweaking the putative sex ratio (which is completely unknown) it is possible to arrive at somewhat different multipliers, e.g. Lo Cascio (1994a, 38).

<sup>19</sup> Frank (1924, 340–1).

<sup>20</sup> Frank (1940, 1).

<sup>21</sup> Lo Cascio (1994b, 93 and 116). Lo Cascio (1994a) critiques the low count *in extenso* but does not proffer alternative figures.

extensions of citizen status and manumission, there is no indication of how much of this growth might have occurred in Italy itself.<sup>22</sup>

In 1996 Lo Cascio proposed between 20 and 21.5 million citizens in 47 CE, a range that is somewhat more generous than that suggested two years earlier. Once again, the question of the share of the Italian citizenry is not addressed.<sup>23</sup> The numerical implications of ongoing growth continue to be avoided in a subsequent treatment from 1999, which repeats the estimates of 12,250,000 and 14,470,000 citizens in 28 BCE and 14 CE respectively, although on this occasion Lo Cascio notes explicitly his contention that “the Italian population went on to increase during the first two centuries of the Empire”.<sup>24</sup>

By 2005, however, instead of exploring the logical implications of this assumption, Lo Cascio had opted to lower his previous estimates and abandon the idea of post-Augustan population growth in Italy.<sup>25</sup> We are now given an estimate of between 15 and 16.4 million citizens in 14 CE (a range that is up to 8.5% lower than the 1994 estimate of 16.4 million), of whom between 13.5 and 14.5 million are thought to have resided in Italy proper (compared to 14.47 million in the 1994 estimate, or up to 6.7% fewer than before). The inclusion of slaves raises the grand total to 15–16 million. As in previous discussions, resident aliens and *incensi* remain unaccounted for.<sup>26</sup> Post-Augustan developments are relegated to a single footnote that dismisses the census tally of 5,984,072 for 47 CE as “less reliable” (presumably relative to the Augustan figures, although no reason is given for this qualification) and avers that it “probably reflects not so much the possible natural increase of the citizen population over 33 years, as the grant of the Roman citizenship to provincial individuals and communities and a high rate of manumission of slaves”.<sup>27</sup> Lo Cascio does not comment on the relative weight of these factors: whereas “not so much” would seem to assign the bulk—though not all—of this increase to extra-Italian sources, the

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<sup>22</sup> Lo Cascio (1994b, 116).

<sup>23</sup> Lo Cascio (1996, 292–3).

<sup>24</sup> Lo Cascio (1999a, 164 [population estimates for 28 BCE and 14 CE], 170 [continuing growth after Augustus, with reference to Lo Cascio (1994b), where no figures are given for this process]).

<sup>25</sup> Lo Cascio and Malanima (2005). Here and in the following I ascribe to Lo Cascio the statements in this article that deal with details of Roman history. Kron (2005b), published in the same year, defends the high count and attacks the low count at great length but does not offer any actual population estimates for Roman Italy.

<sup>26</sup> Lo Cascio and Malanima (2005, 203).

<sup>27</sup> *Ibid.* 229 n. 24.

emphasis on slave manumissions implies a significant association with Italy, which housed the largest concentration of slaves in this period. Even so, manumission might best be envisioned as a zero-sum game that reduced the number of slaves to the same extent as it increased the citizen population, leaving the overall size of the Italian population unaffected.

This leaves open the question of how much of the attested growth in the citizen population following the census of 14 CE occurred in Italy, where (in Lo Cascio's own view) 88–90% of all citizens may have resided at that time. In the 2005 version of his argument, and contrary to his earlier view, Lo Cascio assumes zero net demographic growth in Italy between 14 CE and the 'Antonine Plague' of the 160s CE.<sup>28</sup> This scenario requires us to believe that whereas the number of citizens in Italy increased by some 20% during the 41 years from 28 BCE to 14 CE, absolutely no further growth occurred during the following century and a half. This implies that by sheer coincidence the third Augustan census managed to capture the maximum size of the Italian population in antiquity. It likewise requires us to believe that although Italy accounted for up to nine-tenths of the citizenry in 14 CE, all subsequent growth—both through reproduction and through status change—was exclusively confined to the one-tenth of the citizen body that was domiciled in the provinces. In addition, it requires us to believe that the number of citizen residents of Italy who eluded census registration was so small as to be negligible in the context of these calculations, and that the free non-citizen population of Italy was also a *quantité négligeable*.

Any one of these assumptions would seem unlikely *a priori*, and the notion that all of them applied simultaneously ought to strain the credulity of even the most sympathetic observer. For example, if we were to accept the presence of 14 million registered citizens in Italy in 14 CE, even a low undercount of a mere 7% would add another million residents. Alternatively, an undercount of 5% would leave room for 300,000 free aliens in a free total of 15 million. Slaves numbered at least 1 million in that period, although a range from 1 to 1.5 million

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<sup>28</sup> Ibid. 208 list 15–16 million in 1 CE and 100 CE and 12 million in 200 CE. In this context 1 CE is a stand-in for 14 CE, whereas the reduction to 12 million by 200 CE indicates that the total for 160 CE is likewise thought to be 15–16 million, given the notion of a 20–30% fall during the epidemic (ibid. 204).

might be the most plausible estimate.<sup>29</sup> It is hard therefore to envision a minimum of fewer than 16 million Italians in 14 CE. The difference between the censuses of 14 CE and 47 CE amounts to 1,047,000, that is roughly one million adult men or three million citizens overall. If we were to speculate, if only for the sake of argument, that during this interval the non-Italian citizen population (generously put at 2 million, although Lo Cascio's latest estimate implies no more than 1.7 million) increased five times as fast as that of Italy proper, this would yield a net gain of 1.75 million citizens in Italy (an increase of 12.5%) and 1.25 million in the provinces (an increase of 62.5%). This alone would be enough to raise the Italian total closer to 18 million. If we furthermore assumed that due to a massive deceleration in the growth rate the Italian citizenry grew as much in absolute terms between 47 CE and 164 CE as it did between 14 CE and 47 CE, and therefore much more slowly in relative terms, we would arrive at a final total of 19 to 20 million.

Even if we assumed that from 14 CE to 47 CE non-Italian population growth proceeded *ten times* as fast as in Italy itself, we would still need to allow for an additional 1.2 million citizens in Italy by 47 CE (at a growth rate of 9%), compared with 1.8 million in the provinces (at a rate of 90%). In fact, unless we are prepared to believe that in the period of Augustus a much larger share of all citizens resided outside Italy than is commonly surmised, there is no realistic scenario that would produce a final Italian population below 18 million. In the context of the high count model, a final tally of closer to 20 million would seem to be the most likely outcome.

Lo Cascio has offered no support for his most recent view that the Italian population did not grow after 14 CE. He even concedes that “the three Augustan censuses indicate a rising trend” but nevertheless speculates that “the level attained by the Italian population at the beginning of the first century of our era is probably the peak of a long growth”.<sup>30</sup> While this is not strictly speaking impossible, there is nothing particularly probable about it: why would a “rising trend” have ended overnight just because the first emperor had died? In fact, it was only at that time that Italian manpower contributions to the military were beginning to decline in earnest, alleviating constraints on

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<sup>29</sup> Scheidel (2005a, 66–71).

<sup>30</sup> Lo Cascio and Malanima (2005, 204).

the reproductive capacity of the Italian population.<sup>31</sup> Moreover, possible indications of mounting population pressure primarily date from the time after Augustus, as Lo Cascio himself has argued in previous work.<sup>32</sup> The massive increase by 3 million within the following 33 years is arguably the most powerful indirect evidence suggestive of further demographic growth in Italy itself. All the same, there is no compelling reason to extrapolate early monarchical growth trends all the way into the second century CE: field survey data, for what they are worth, point to more widespread decline in settlement intensity in that century.<sup>33</sup> Even so, this would still leave us with a conservative estimate of 17 to 18 million Italians, even allowing for stagnation or decline from the late first or early second centuries CE onwards.

In general, the high count logically implies significant net natural growth in late-republican Italy; very substantial under-registration of citizens in the same period but vastly improved coverage later on (to the extent that under-registration ceased to be a significant problem at all); and a much more populous Transpadana than envisioned by proponents of the low count.<sup>34</sup> The only other interpretation that would enable us to sustain the notion of continuity in census reporting practices demands a dramatic expansion of the citizenry outside Italy via mass enfranchisement during the civil war era:<sup>35</sup> this scenario, to the best of my knowledge never properly developed in contemporary scholarship, would translate to a less crowded Italy but also change our perception of conditions in the provinces in ways that are not verifiable from the record (although not necessarily completely impossible).

Unlike proponents of the low count, advocates of the high count have yet to present an estimate for the imperial population as a whole. Lo Cascio has argued for a relatively large population in Roman Egypt but has not dealt with other regions,<sup>36</sup> while Frank does not appear to have addressed this issue at all. Most recently, Kron defended the claim that Pompeius had conquered 12,183,000 people, on the grounds that

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<sup>31</sup> See below, section 6.

<sup>32</sup> Lo Cascio (2004a), (2004b). This fact is elided in Lo Cascio and Malanima (2005, 204).

<sup>33</sup> See below, section 10.

<sup>34</sup> Morley (2001, 53 [growth]); Kron (2005b, 444–53 [under-registration] and 461–82 [Transpadana]).

<sup>35</sup> I owe this suggestion to Michael Crawford (personal communication, September 28, 2006, and June 29, 2007).

<sup>36</sup> Lo Cascio (1999c).

this figure resembled Ottoman census tallies of 12,045,791 for Asia Minor, Armenia, and Syria down to Sinai in the 1870s.<sup>37</sup> However, even if we were to accept that Roman Egypt had reached a population of 8 million or more,<sup>38</sup> and that the Roman Levant had attained nineteenth-century levels of population density,<sup>39</sup> this would not tell us very much about Roman Italy. I will return to this problem in sections 12 and 13.

It is fair to say that the low count has long dominated modern scholarship and in recent publications is still widely considered superior to the alternative represented by the high count.<sup>40</sup> However, this observation does little in and of itself to validate the former in intellectual terms: historical research cannot be reduced to a popularity contest, and the number of trees that continue to die in order to sustain ‘scholarly’ publication on the Roman regal or earliest republican periods bears witness to the sad fact that scholarly acceptance can sometimes be a rather poor measure of intellectual respectability. What is more, scholarly debate has come to focus very tightly on the perceived dichotomy between a switch from the reporting of the number of adult male citizens to that of all citizens on the one hand, and continuity in reporting adult men on the other. It may be time to remind ourselves that this stark choice constrains our options to such a degree that it may distort the terms of the debate.

As Brunt’s own discussion of earlier scholarship shows, the view that republican census tallies are meant to report the total number of all adult male citizens was not always as uncontroversial as it is now.<sup>41</sup> Saskia Hin argues that this position suffers from considerable logical

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<sup>37</sup> Kron (2005b, 485–6 [Plin. *NH* 7.97–98]). This contention does not challenge the low count because it is consistent with the (low count) estimate for the same region in Frier (2000, 812): 12.5 million in 14 CE. For the population of Roman Syria cf. now Kennedy (2006) in favor of the low count. See below, n. 151.

<sup>38</sup> Lo Cascio (1999c). For a much more detailed argument in favor of a lower range from 5 to 7 million see Scheidel (2001a, 184–250).

<sup>39</sup> This is widely accepted: see above, n. 37, and below, n. 151.

<sup>40</sup> The most recent examples include Patterson (2006, 33) and Witcher (2006b, 121 and n. 190), both acknowledging abiding uncertainties and recent debates but leaning towards the low count. For other instances see, e.g. Suder (1997, 120–1); Frier (2000, 811–16); De Ligt (2004) and (2007). Morley (1996, 46–50) defended the low count against higher alternatives but in Morley (2001) explored the potential of the latter without, however, committing himself to them. Kron (2005b, 442–3) conveniently gathers references to earlier secondary scholarship.

<sup>41</sup> Brunt (1971/1987, 15–25).

inconsistencies.<sup>42</sup> In brief, a list of all adult men would be of no immediate military purpose (as only the younger cohorts would be called up to serve, and some men at any age would be unfit for service), and of no obvious fiscal use either (as it would exclude property owners who were not adult men); at best it could have served as a roster of the electorate (most of whom never voted at all). If the census was meant to collect valuations of citizen property for the purpose of status ranking and tax assessment, it ought to have covered all Romans who were *sui iuris*, that is, all fatherless or emancipated men and widows. Rare allusions to the exclusion of orphans and widows from republican census tallies, which would not otherwise be readily explicable, are consistent with this view.<sup>43</sup> Republican as well as monarchical citizen censuses always sought to count everyone: it is the scope of the publicized results that is controversial. Therefore, if we were to reckon with a shift from reporting property-owning men in the republican censuses to one of including minors and women *sui iuris* from Augustus onward, we would need to apply a multiplier of up to 2.5 for the reported census figures, for totals of up to 10 million Roman citizens in 28 BCE and no more than 15 million in 47 CE: in other words, an intermediate scenario between the somewhat modest Italian population implied by the low count and the very large one produced by the high count.<sup>44</sup> The promise of such compromise positions will have to be explored by others since my focus on the existing debate is meant to highlight the logical properties of the two main rival models. I will, however, briefly return to the possibility of alternative readings at the end of section 12.

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<sup>42</sup> Hin in this volume. I am grateful to Saskia Hin for sharing her work with me.

<sup>43</sup> Liv. 3.3.9 and *Per.* 3; *Per.* 59. In forthcoming work, I argue that Livy wrote books 3 and 59 right after the results of the censuses of 28 BC and 8 BC had been publicized. If the Augustan tallies had indeed included widows and orphans, these two events would have provided Livy with a motive for specifying the somewhat different character of the republican census. This seems to me the most economical explanation of the fact that Livy mentioned these details only twice (out of 26 known occasions when he reported republican census results) and that he did so at the beginning and near the middle of his work.

<sup>44</sup> As Hin (in this volume) points out, a variety of factors would have lowered this multiplier and hence the total size of the citizen population. For 28 BCE 8 to 10 million citizens overall might translate to a similar number of residents of Italy (if the number of overseas citizens roughly equaled that of slaves and aliens in Italy) and an eventual peak of closer to 10 than to 15 million, in line with high medieval and early modern population numbers (see below, sections 12–13).

### V. *Urbanization*

In the most general terms, given the large size of the city of Rome and the presence of more than 400 urban communities in Roman Italy in the first century CE, the low count would seem to imply a higher urbanization rate than the high count.<sup>45</sup> Neville Morley's reconstruction, for example, suggests that 25% of Italy's population resided in cities (excluding Rome itself), or almost 40% if the capital is included.<sup>46</sup> Lo Cascio has repeatedly maintained that the low count translates to a level of urbanization that is implausibly high for a premodern society and that the strongly urban character of Roman Italy therefore speaks in favor of larger population overall.<sup>47</sup>

It is undoubtedly true that the implied urbanization rates are very high by premodern standards and therefore represent a challenge to the low count. Nevertheless, Lo Cascio's line of reasoning suffers from several problems. First of all, it is not strictly speaking true that similarly extreme levels of urban primacy are unknown. In 28 BCE a metropolitan population of some 800,000 to 1 million in an Italy of around 5.5 million would have accounted for 15–18% of the regional total, and for 38–43% of the number of regional urban residents.<sup>48</sup> By comparison, in the late seventeenth century London is thought to have comprised 70% of the residents of all English cities with a population of 5,000 and over, and to have housed 9.5% of the total population of England at the time.<sup>49</sup> Thus, controlling for urban communities below the 5,000 threshold, London was as dominant in England as Rome would have been in a low count version of Italy. That Rome would have accounted for an even larger share of the regional population than London can be

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<sup>45</sup> For the size of the city of ancient Rome in different periods see most recently in great detail Lo Cascio (1997). Cf. Witcher (2005) for the importance of the *suburbium*.

<sup>46</sup> Morley (1996, 182) for 1 million in Rome and 1,325,000 in other cities. Hopkins (1978, 68–9) does not strictly speaking propose an urban percentage but argues for a non-agricultural population amounting to 32% of the total or approximately (this is not quite clear from the text) 18% excluding Rome. For the relationship between the urban and the non-agricultural population see below.

<sup>47</sup> E.g. Lo Cascio (1994a, 39), (1999a, 164–5), and forthcoming.

<sup>48</sup> For the size of Rome see above, n. 45. For the population of Italy see above, section 4. For the urban population see above, n. 46.

<sup>49</sup> Wrigley (1987, 162). The next largest city, Norwich, was only one-twenty-fourth as populous as London (*ibid.* 160). In the early nineteenth century Cairo had approximately fifteen times as many inhabitants as the next-most populous cities of Egypt: Baer (1969, 134) with Panzac (1987, 28).

explained as a function of its position as a pan-Mediterranean capital bloated by coerced transfers of food and other resources.

Secondly, and for this very reason, it makes little sense to relate the size of the imperial city of Rome to the size of the population of Roman Italy and pronounce on the plausibility of particular ratios. Unlike today, or even in the third century BCE, Rome was not the capital of Italy in the modern sense of the term but served as the political and tribute-taking center of a much more extensive empire. In economic terms, much of the coastal regions of the Mediterranean formed Rome's hinterland or catchment area that would provision it with food and various other supplies. Italy, for all of its economic orientation towards the capital, was only one element in this network of tributary transfers and market exchange. Indeed, one could argue that areas such as Sicily or Sardinia or parts of North Africa had a stronger claim to being part of Rome's hinterland than the more isolated and less integrated Po Valley. In this context, attempts to relate conditions in the capital to the demography of Roman Italia (essentially mainland Italy within its modern borders) are of no obvious relevance to estimates of overall population or to our understanding of urban hierarchies.<sup>50</sup> As an exceedingly rough guess (and applying the parameters of the low count), at the time of Augustus the coastal areas of Italy, Gaul, Iberia, North Africa, and Egypt, together with the western Mediterranean islands, may have been inhabited by at least 10 million people, which means that the actual catchment area of the imperial capital would have been at least twice as large as an Italian population of (say) 5 million outside Rome itself.<sup>51</sup> In other words, it does not logically follow from Rome's impressive size that the remainder of Italy *ought* to have been inhabited by a population much larger than 5 million.<sup>52</sup> The expanding extra-Italian catchment area of the cities of Roman Italy also obviates the

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<sup>50</sup> *Contra* Kron (2005b, 487), rank-order models for individual countries are therefore of little value for our understanding of Rome's relative standing in Italy (as opposed to the Mediterranean as a whole). If Rome served as the center of much of the western Mediterranean, the huge gap between its size and that of the next-largest cities is easy to explain. (cf. also below, n. 59). Different conditions prevailed in the eastern Mediterranean, with a polycentric system consisting of several former imperial centers (Alexandria, Antioch, Pergamum) and a number of secondary centers.

<sup>51</sup> Crudely conjectured from the numbers in Frier (2000, 812 table 5).

<sup>52</sup> It is striking that while Lo Cascio and Malanima (2005, 222–3) emphasize the importance of Italy's integration into larger Mediterranean structures in boosting population size, Lo Cascio does not seek to explain Roman 'Italian' urbanization levels in the same terms.

need for the assumption that urban growth had to be accompanied by correspondingly substantial rates of overall population growth in Italy proper.<sup>53</sup> Moreover, comparative evidence shows that, under the right circumstances, urban and rural growth rates could diverge widely for a considerable time.<sup>54</sup> For all these reasons, urban growth in Roman Italy may be a poor indicator of net population growth in that region.

Thirdly, we do not know for sure how large most Italian towns really were. The fact that so many of them were concentrated in central Italy indicates that those communities at least were relatively small. The average territory of a town in Roman Italy was 580 km<sup>2</sup>. In 1300 mainland Italy may have boasted 71 cities with a population of 10,000 or over, which translates to an average catchment area of 4,225 km<sup>2</sup>, more than seven times the Roman mean. If we include medieval cities of 5,000+, we arrive at a total of 161 settlements with an average catchment area of 1,860 km<sup>2</sup>, or three times the Roman mean.<sup>55</sup> Therefore, unless we are prepared to believe that the Roman population of Italy was several times as large as that of the High Middle Ages, which is impossible (see below, section 12), most Roman towns must have been fairly small, with populations in the low rather than high four digits. This must have been even more true of the urban settlements of *regio I* (Latium and Campania), with an average territory of 180 km<sup>2</sup> and a mean inter-town distance of a mere 11 km.<sup>56</sup> That Italian cities were modestly sized is also brought out by a simple comparison with Roman Egypt, where the average size of urban territories was roughly the same as in Italy (around 500–600 km<sup>2</sup>) but most of that land was under cultivation and much more productive, which means that the average population of these cities was dramatically larger than in Italy regardless of which count we employ.<sup>57</sup> At the same time, we know of Egyptian villages with thousands of residents, that is, of a size similar to that of small Italian towns (see below).<sup>58</sup> As is well known, in the

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<sup>53</sup> Pace Lo Cascio (1994a, 29 n. 36).

<sup>54</sup> Between 1600 and 1750 the urban population of England increased by 260%, compared to 20% rural population growth: Wrigley (1987, 162).

<sup>55</sup> 1300 data from Malanima (1998, 110–16).

<sup>56</sup> Duncan-Jones (1982, 339), after Nissen; Bekker-Nielsen (1989, 21–2).

<sup>57</sup> For urbanism in Roman Egypt see Tacoma (2006, 21–68).

<sup>58</sup> Rathbone (1990, 124–37).

Roman empire urban status was above all a legal issue and not directly correlated to demographic features.<sup>59</sup>

Morley's low count model assigns approximately 1.3 million residents to Italian cities outside Rome, for a notional mean of about 3,000. As I noted on a previous occasion, the only inscription that allows us to infer the probable size of the *plebs urbana* of a city in Roman Italy (*CIL* XI 2650 from Saturnia in Etruria) points to between 1,000 and 2,000 free urban residents.<sup>60</sup> It is therefore misleading to insinuate that euergetic texts imply a substantially larger urban population than predicted by the low count.<sup>61</sup> Literary references are similarly unhelpful.<sup>62</sup>

The aggregate urban population of 2,231,000 estimated for the 161 cities of 5,000+ residents in mainland Italy *c.* 1300 is of the same order as the aggregate urban population of Roman Italy (including Rome) in Morley's reconstruction. If we exclude at least part of the population of the imperial capital in order to control for its extraordinary capacity to draw on resources from outside the peninsula, the adjusted Roman tally (say, 1.8 million) falls short of the medieval figure. Moreover, if

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<sup>59</sup> As it happens, early modern Egypt (*c.* 1820) provides an interesting real-life example of a country with a population of 5–6 million (Scheidel 2001a, 212) that was endowed with a single large capital city (Cairo, probably in excess of 250,000), 6 cities of between 10,000 and 20,000 residents, a few markets towns below that range, and a large number of sizeable villages: Baer (1969, 134) with Panzac (1987, 28). In Morley's speculative model of a similarly-sized Italy we encounter one very large metropolis, 5 cities in the 25–40,000 range, 25 cities in the 5–25,000 range, and 400 smaller towns: Morley (1996, 182). The small towns of Roman Italy are functionally equivalent to the larger villages of (Roman as well as early modern) Egypt. Thus, even if we were determined to treat Roman Italy in isolation (which is unwarranted: see above, n. 50), the Egyptian case shows that there is nothing inherently unlikely about this kind of urban system.

<sup>60</sup> See Duncan-Jones (1982, 272). Other texts that record communal cash handouts fail clearly to identify the provenance of the beneficiaries or the per capita amounts: Duncan-Jones (1982, 262–77) with Scheidel (2004, 15 and nn. 83–4).

<sup>61</sup> *Contra* Lo Cascio (1999a, 165), (2001, 122).

<sup>62</sup> The references given by Kron (2005b, 488) fail to support larger urban totals: they are annalistic kill or capture tallies that may well be inflated or include rural residents. Kron shows no appreciation of ancient rounding practices, as for example when he (488–9) refers to the claim that Arpi could raise 4,000 foot and 400 horse (decupled multiples of four being among the most common symbolic figures in the Roman literary tradition) or that Tarentum was able to raise 34,000 troops, i.e. 30,000 (infantry) plus 4,000 (horse) according to Strabo 6.3.3 (30,000 being another extremely popular symbolic figure in the canon), a number that is conveniently reproduced for Capua in Livy 23.5, quite tellingly once again as a potential tally and not even as a fact. These figures do not show anything at all beyond the ancient penchant for certain numerical symbols: for detailed demonstrations of Roman number stylization see Duncan-Jones (1982, 238–56); Scheidel (1996b); Duncan-Jones (1997).

we include towns of fewer than 5,000 inhabitants in the guesstimate for 1300, the gap between the medieval and Roman totals grows even further. For example, if we conjecture the existence of another 161 smaller medieval towns with an average population of 3,000, the medieval tally rises to 2.7 million, 50% higher than the adjusted Roman tally of 1.8 million. If we were to assume, if only for the sake of argument, that urbanization rates in Roman and medieval Italy had been the same, the total population of Roman Italy would have been some 30% lower than in 1300, in line with the assumptions of the low count.<sup>63</sup> Conversely, for the population of Roman Italy to match that in 1300, Roman urbanization rates would have had to be correspondingly lower than in the High Middle Ages. Hence, the high count logically implies an urbanization rate far below medieval levels, unless of course Roman towns are thought to have been much more populous than allowed by the low count.<sup>64</sup> For example, in a high count Italy of anywhere from 16 to 20 million (see above, section 4), an urbanization rate of 25–28% would put some 4–5.6 million people into towns, for an average of 9,300–13,000 residents per settlement, or 7,000–10,700 excluding Rome. This is very broadly consistent with putative medieval means of 13,900 for cities of 5,000+ and of 8,400 if we add another 161 smaller towns of 3,000 each. So, if we took medieval urbanization rates to be the standard for Roman Italy as well, in order to fit the high count the average Roman town (excluding Rome) would have had to be between 2.3 and 3.6 times as big as for the low count.

It remains to be seen if it will ever be possible to determine with confidence whether the average Roman town counted 3,000 or three times as many residents. However, and this brings me to my fourth and arguably most important point, there is no need to presuppose anything like a normative urbanization rate. Because of this, the size of Roman towns ultimately does not matter a great deal for estimates of overall population size. Even if it could somehow be established that the aggregate urban population in Roman Italy far exceeded Morley's estimate, this finding would not automatically translate to a much larger

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<sup>63</sup> In 1300, for a total population of 9.5–11 million (see below, section 12) and with 2.7 million of them located in towns, the urbanization rate would have been 25–28%. At that rate, an adjusted urban population of 1.8 million in Roman Italy would translate to a total population of 6.8–7.7 million (i.e. 6.3–7.2 million plus the 0.5 million in Rome who were supported from external sources). This total is in line with the final population maximum implied by the low count (see above, section 4).

<sup>64</sup> Thus Lo Cascio (1999a, 165); Kron (2005b, 488–9).

Italian population. Instead, we must allow for the possibility that direct analogies between urbanization levels in antiquity and in later periods may simply be irrelevant because they seek to equate conditions in two very different environments: Greek and Roman societies, with their *poleis* and *civitates* that fused cities with their respective hinterlands, and post-Roman Europe, with its much more pronounced boundaries between city and countryside. As Mogens Hansen has recently argued in considerable detail, in classical Greece the tight integration of city and countryside appears to have raised average ‘urbanization’ levels to historically very high levels of approximately 50%.<sup>65</sup> It goes without saying that ‘urban’ is a questionable description for the centers of many of these *poleis*. It simply means that a large proportion of the citizens of a given *polis* resided in the chief nucleated settlement of their community. In that environment, preference for urban residence was primarily a political and cultural phenomenon that was not straightforwardly associated with the size of the non-agricultural sector: the majority of these urban residents must have engaged in farming.

This case highlights the limitations of Paul Bairoch’s estimate that in premodern populations the proportion of all non-farmers tended to exceed the proportion of all urban residents by several percentage points.<sup>66</sup> Hansen’s work leaves little doubt that this principle cannot be applied to the *polis*. Roman communities in Italy, where cities and their hinterland were controlled by the same city-based elites, may well have had more in common with the Greek *poleis* than with later urban *communi* that had to overcome their separation from a countryside controlled by rural lords.<sup>67</sup>

In his most recent contribution Lo Cascio invites us to choose between two scenarios for urbanization rates in Roman Italy: one in which the proportion of urban residents approximates to the non-agricultural share of the total population, and one in which the majority of all Italians were concentrated in ‘agro-towns’ similar to those that dominated nineteenth-century Sicily.<sup>68</sup> No justification is provided for this stark dichotomy, and it remains unclear why our choice must be

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<sup>65</sup> Hansen (2006, 24, and 73–4 [for the necessity to adopt an ‘urban’ threshold of far below 5,000 residents]). For the fusion of city and hinterland see Hansen (2004).

<sup>66</sup> Bairoch (1989, 266). Lo Cascio has repeatedly sought to apply this principle to the ancient world: see Lo Cascio (1994a, 39), (1994b, 110 n. 56), (1999a, 164).

<sup>67</sup> Cf., e.g. Epstein (2000) on the development of medieval Italian city-states.

<sup>68</sup> Lo Cascio (forthcoming). Cf. already Lo Cascio (1994a, 29 n. 36). Malanima (2005, 98–9) notes that if agro-towns are considered ‘urban’, Sicily boasted an ‘urban-

confined to either one or the other of these ideal types. The ancient Greek case as reconstructed by Hansen falls right in between these two extremes: it suggests that it was perfectly possible for the proportion of the non-rural population greatly to exceed the proportion of the non-agricultural population without representing the majority of the overall population. In actual fact, there is simply no way of telling what proportion of the population of Roman Italy outside the capital lived in nucleated settlements that legally enjoyed urban status: 10%, 20%, 30%? None of these possibilities would have caused the countryside to be deserted.<sup>69</sup>

In a sense, the 'low' and 'high' counts are logically associated with two different models of urbanization. In the low count a relatively small total population might have been the result of a preference for urban residence (driven by the spread of tenancy or the urban focus of euergetism) that curtailed intensification in the exploitation of agrarian assets (which would have benefited from rural residence) and thereby limited population growth. Conversely, a high count might have arisen from comparatively lower nucleation rates that helped boost agricultural output and thus population size.

It is not legitimate for Roman historians to impose supposedly normative urbanization ratios imported from the more recent past, or to assume without further argument that the extent of nucleation was directly determined by economic development and agricultural productivity.<sup>70</sup> By themselves, Roman urbanization rates, even if they could somehow be empirically determined, would not necessarily tell us much about overall population numbers. I conclude that arguments from or about urbanization rates cannot make a meaningful contribution to the question of Roman population size.

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ization' rate of two-thirds at the beginning of the early nineteenth century. See also Malanima (1998, 102–3).

<sup>69</sup> Nothing in Garnsey (1998, 107–31) speaks against this possibility. I note in passing that field surveys tend to imply high nucleation rates, which might be taken to support the notion that Roman towns bore at least some resemblance to much later 'agro-towns': e.g. Fentress (forthcoming). However, I am inclined to suspect that these findings owe much to the inability of field surveys to account for small sites: see below, section 10.

<sup>70</sup> Lo Cascio (forthcoming) develops an elaborate equation that links urbanization and economic development but conflates urban and non-agricultural population. Once we allow for the possibility of a significant agrarian complement to urban populations, the whole schema becomes meaningless.

VI. *Military service*

During the last three centuries BCE the Roman state repeatedly mobilized large citizen armies. Modern observers must take account of how their estimates of Roman citizen numbers relate to reported military strength. In a paper published in 2001 Lo Cascio demonstrated that the low count implies very high military mobilization rates that seem implausible by historical standards.<sup>71</sup> Two separate issues are at stake, mobilization in emergencies, and baseline levels of military commitments.

It is the former that require the most careful attention.<sup>72</sup> Lo Cascio calculates that during the crisis of the Second Punic War, Rome drafted 9.5% of the entire citizenry in 215 BCE, 11.8% in 214 BCE, and 12.6% in 212 BCE.<sup>73</sup> These rates exceed those for the following years.<sup>74</sup> However, these percentages are vitiated by Lo Cascio's method of multiplying the number of adult male citizens by 3.33 in order to arrive at the size of the overall population (and thus to calculate mobilization rates). This approach neglects heavy selective attrition in the adult male element of the population: if we refrain from extrapolating war casualties to women and minors, the resultant population is somewhat larger and recruitment somewhat less intense: at 607,000 women and children and 190,000 adult males from late 215 BCE the implied mobilization rates are 7.5% in late 215 BCE, 9.4% in 214 BCE, and 11.9% in 212 BCE.

Similarly high rates reappear in 83–81 BCE, at 8.3%, and again in 43 BCE, at anywhere from 5.9% to 8.6%.<sup>75</sup> Lo Cascio calculated all these ratios based on Brunt's account of Roman manpower for the purpose of exposing the weaknesses of this particular reconstruction. The fact that Lo Cascio disallows Brunt's assumption of some undercount in the censuses does not make a great difference since this would not lower implied mobilization rates by more than around 1%.

<sup>71</sup> Lo Cascio (2001, 122–37).

<sup>72</sup> Long-term rates are lower, around 4–5% of the citizenry, or approximately one-fifth of all *iuiores*, a level of mobilization that could easily have been maintained by drafting only unmarried men under 30; cf. Rosenstein (2002).

<sup>73</sup> Lo Cascio (2001, 136). His figure for 212 BCE omits naval personnel, which appears to be a mistake.

<sup>74</sup> Brunt (1971/1987, 418 table X).

<sup>75</sup> Lo Cascio (2001, 136).

Lo Cascio references ratios of men under arms in relation to the total population in several European countries in the seventeenth through nineteenth centuries which consistently indicate much lower mobilization rates.<sup>76</sup> These comparanda, together with the observation that Brunt's version of the low count requires us to accept that in a few years in the 210s BCE the Roman state temporarily managed to mobilize between 50% and 75% of all *iuniores* (male citizens aged 17 to 45) for the war effort, are meant to discredit the assumptions of the low count.

There is no question that the recorded peak levels in particular represent a formidable challenge to any formulation of the low count. At the same time, three problems must be noted. First of all, it is imperative to compare like with like. Comparisons with early modern Europe miss the point because of fundamental differences in mobilization practices. The Roman republic operated a militia system that drew on all able-bodied men. The closest and most obvious parallel is provided by Greek militias which achieved very high temporary mobilization rates.<sup>77</sup> What is striking about republican Rome is not that it matched Greek rates in its capacity as a city-state but rather that it managed to maintain large-scale mobilization as it drew other Italian polities into its state and alliance system. Rome differed from (much) later European states by preferentially taxing military labor instead of material resources. For this reason alone, military mobilization rates in those two periods were bound to differ greatly.

Secondly, the mobilization rates implied in Brunt's account, whilst undoubtedly extreme, are not entirely without parallels in the historical record. They are short-term figures, confined to periods of crisis from a single year to maybe four consecutive years. From 1861 to 1865 some 11% of the free population of the Confederate States served in the military, equivalent to the maximum mobilization rate at the peak of the Second Punic War.<sup>78</sup> In 1760 and again in 1813, 6–7% of the Prussian population served in the army, as did 7.7% in Sweden in 1709, comparable to Roman rates during the Social and Civil Wars.<sup>79</sup> It merits attention that during the Hannibalic War most Roman troops were deployed within Italy, and the same is true *a fortiori* of the Social

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<sup>76</sup> Lo Cascio (2001, 137).

<sup>77</sup> See esp. Morris (2005b) for calculations.

<sup>78</sup> McPherson (1988, 306 n. 41) with Haines (1998).

<sup>79</sup> Clark (2006, 366).

War. Very large forces operated overseas only in the second triumviral period, when the recruitment of non-Italians may have played a more important role.

And thirdly, even the high count does not greatly detract from the extraordinary character of the Roman war effort in times of emergency. Lo Cascio elides this issue by stating that while the low count implies that “the burden of military service was tremendously heavy for long periods”, “Frank’s solution” (i.e. the high count) requires us to accept that “the burden of military service was again high, but comparable to that experienced in other pre-industrial states”.<sup>80</sup> This may overstate the capacity of the high count to address this problem. As outlined above, according to the low count, the highest mobilization rates occurred in the 210s BCE, on rare occasions approaching or even exceeding 10% of the total citizen population. I agree that this would have been a “tremendously heavy burden” that indeed beggars belief. However, Lo Cascio’s claim that in 225 BCE the Roman citizenry comprised 514,000 adult males, instead of 325,000 as posited by Brunt, has only a limited impact on the scale of subsequent mobilization rates.<sup>81</sup> By 215 BCE the number of adult male citizens must have dropped to somewhere around 400,000 (or perhaps even 350,000, if we take Polybius’s casualty figures seriously): for 212 BCE this yields a mobilization rate of 24% (or 27%) of all adult men, compared with 50% in Brunt’s scenario. Reckoning with a total population of 1.45–1.5 million in 212 BCE,<sup>82</sup> the overall rate is 6.3–6.5% for the high count, compared with 11.9% for the low count. Thus the high count brings the Roman experience in line with reported maxima for Sweden and Prussia, whereas the low count suggests conditions comparable to those in the Confederacy. It seems rather pointless to argue over whether the Roman Republic resembled Prussia more closely than it resembled the Old South, although it is striking that Rome and (on a much bigger scale) the Confederate States (unlike Prussia and Sweden) had access to slaves (who are excluded from the present population tallies, causing us to overstate overall mobilization rates) and their labor (which helped offset the absence of male workers). Just as in classical Athens

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<sup>80</sup> Lo Cascio (2001, 112–13).

<sup>81</sup> Lo Cascio (1999a, 169).

<sup>82</sup> 1,106,000 women and minors (the same as for 225 BCE, extrapolated from the presence of 514,000 adult men in that year, although civilian losses must also have occurred) plus anywhere from 344,000 to 404,000 adult men.

or in the Confederacy, chattel slavery must have increased the military mobilization *potential* of the Roman state.<sup>83</sup>

We are left with the basic fact that regardless of which count we prefer, Roman mobilization rates were very high by more recent historical standards. Republican Rome was not an early modern European state struggling to squeeze soldiers and resources from a previously demilitarized population but a confederation of city-states and other polities with strong traditions of seasonal mobilization and which enjoyed the added bonus of slave labor. The question is not whether in terms of military organization Rome was more like Prussia or more like the Confederacy, it is whether Rome was more like Greek *poleis* or more like later western states, and it would seem to me that the answer to this question is perfectly clear. I conclude that just as in the case of urbanization rates, military mobilization rates are of no particular relevance to our understanding of Roman population size: they are largely neutral with respect to our reading of the census figures.

By contrast, recruitment practices during the Principate deserve more attention than they have received. Judging from the evidence furnished by the epitaphs of Roman soldiers and veterans, Italy's contribution to the military declined steeply during the first 150 years of the monarchy: the proportion of Italians among all legionaries whose provenance is known dropped from 62% in the period 30 BCE–41 CE to 37% in the years 41–68 CE; to 22% in the years 69–117 CE; and to 2% from 117 CE to the end of the third century CE.<sup>84</sup> This trend would seem hard to reconcile with the notion of a very densely populated heartland whose population continued to grow at least in the early stages of this period. In the face of considerable population pressure, as envisioned by Lo Cascio, why did more residents of Italy not swap crowded cities or shrinking plots of farmland for a relatively well-remunerated life of service in the legions, at a time when landowners do not appear to have had the ability to constrain their movement? Proponents of the high count have yet to address this logical inconsistency.<sup>85</sup>

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<sup>83</sup> Military service of slaves was an additional benefit: cf. most recently Hunt (2006); Rosenstein (forthcoming). Although the scale of Roman chattel slavery increased during the last two centuries BCE, there is no good reason to suppose that it had been insignificant at the time of the Second Punic War.

<sup>84</sup> Forni (1953, 159–212) supplemented by Forni (1974, 366–80). Cf. Scheidel (1996a, 95–6 n. 18).

<sup>85</sup> I return to this issue below in section 8.

VII. *Labor markets*

The significance of labor relations as an indicator of demographic conditions has received no attention at all. It is commonly accepted that the number of slaves in Italy grew very substantially during the last few centuries of the republican period. No reliable numbers are available: my own conjecture of an increase from perhaps 200,000 slaves in the late third century BCE to somewhere around 1.2 million 200 years later is meant to indicate merely a certain order of magnitude.<sup>86</sup> An expansion of forced labor on this scale poses a problem for the notion of strong free population growth during the same period. In the most general terms, slave imports imply demand for labor. Romans' willingness to purchase slaves in large numbers logically reflects relatively high real incomes, that is, a shortage of laborers relative to demand. Various factors mediate the availability of non-slave workers. Next to their absolute numbers (i.e. population size), the character of the labor market is a critical variable: labor markets may be 'thinned out' by the mobility of workers that makes employment and supply arrangements less predictable and less stable and raises turnover costs (i.e. the expenses in time and money associated with the replacement of workers and suppliers).<sup>87</sup> 'Thin' labor markets are created by competing demands on workers, for instance—and most likely in the Roman case—by commitments to the military sector.<sup>88</sup> Whatever the underlying causes of relative labor scarcity, from an economic perspective there is simply no way Romans would have paid cash in order to acquire several million slaves unless demand for labor was considerable for an extended period of time.<sup>89</sup> these transactions occurred so consistently and on such a large scale that they cannot be explained with reference to cultural preferences for forced labor that might somehow have superseded fundamental

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<sup>86</sup> Scheidel (2005a, 76). My figures supersede Brunt's guess of a corresponding rise from 500,000 to 3 million (Brunt 1971/1987, 67 and 124), which suffers from the fact that there is no obvious way in which the economy of Roman Italy could have accommodated 3 million slaves at any time: see the critique in Scheidel (2005a, 64–71).

<sup>87</sup> See Hanes (1996) for the concept.

<sup>88</sup> For the role of civic commitments see Scheidel (2008). In the Americas, the land/labor ratio was the crucial variable; in the ancient world, it was the commitments of the free citizenry.

<sup>89</sup> As I have tried to show in Scheidel (2005a, 75–8), a net increase by 1 million slaves over 200 years required the importation of some 3–4 million slaves overall. Scholars have long reckoned with both a larger slave population and higher sex ratios, a combination of features that would necessitate even larger imports.

economic (dis)incentives.<sup>90</sup> In other words, if Romans imported millions of slaves, demand for labor, and hence average real incomes, must have been relatively high.<sup>91</sup> This scenario is fundamentally at odds with the notion of population pressure (i.e. a surplus of labor relative to assets and demand), which is logically associated with depressed real incomes and ‘thick’ labor markets (where the labor supply is stable and/or abundant relative to demand).

The low count can be more readily reconciled with a shift to forced labor: in a society where slave ownership was legally and socially condoned, a combination of demographic attrition due to war, urbanization, and (later on) emigration with growing capital inflows and improved access to enslaveable individuals was likely to precipitate an expansion of the unfree workforce.<sup>92</sup> For a free population that experienced strong net growth, however, massive investment in slaves is more difficult to explain. The high count compels us to assume that intensive—i.e. per capita—economic growth in republican Italy was so strong that even as millions of free citizens were added to the population of a region already densely settled by historical standards, millions of additional unfree laborers were required to satisfy overall demand for labor. Whilst not strictly speaking impossible, this model implies some kind of miracle economy that would put most other known premodern economies to shame (see below, section 13).

The same logic applies to military labor: it is striking that payments to the Roman armies of the civil war period were extremely high.<sup>93</sup> This well-documented fact reinforces the impression of high real incomes and strong demand for labor: the presence of a relatively immiserated citizenry (as the result of an unfavorable ratio of labor supply to labor demand caused by population pressure) ought to have reduced

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<sup>90</sup> Roman Italian slave prices in this period are empirically almost unknown, but even if slaves were cheap, their acquisition must nevertheless have required significant capital outlays; and skilled slaves at the very least are known to have commanded high prices even in this period: see Scheidel (2005b).

<sup>91</sup> For high real incomes see Scheidel (2007b).

<sup>92</sup> Sixteenth-century Portugal is a good example of this process, where demographic loss (through massive overseas migration) and capital inflows led to urbanization, high real wages, and an expansion of slave labor: see Scheidel (2007b), with references. In the absence of slavery, the same mixture of preconditions favored the inflow of free foreign labor, as for example in the early modern Netherlands (*ibid.*, based on de Vries and van der Woude (1997)).

<sup>93</sup> Aggregate payouts reached at least 1 billion denars in the period from 69 to 29 BCE: see Scheidel (2007b).

the outlays required to raise large military forces. It merits attention that *both* phenomena—slave imports and costly large-scale recruitment alike—indicate strong demand for labor and hence either strong economic growth in the face of demographic stagnation (as envisioned by the low count) or even stronger economic growth coinciding with a much larger base population and net natural population growth (for the high count). I conclude that while these observations are necessarily inconclusive with regard to absolute population size, they favor the low count over rival higher estimates.

### VIII. *Political stability*

By contrast, references to conflict over Italian land in the late-republican period and more generally to political conflict provide circumstantial evidence in favor of population pressure that may be more readily compatible with the high count. As Morley pointed out in 2001, “the bitterness of the late Republican agrarian disputes” is easier to understand within the context of a very densely populated peninsula where access to land would have become an increasingly contested means of well-being or even survival.<sup>94</sup> In fact, it is possible to expand this observation in various ways. For instance, the Roman-Latin colonization boom in the late fourth and early third centuries BCE as well as ambitious settlement projects in the wake of the demographically wasteful Second Punic War are indicative of some measure of ongoing population pressure.<sup>95</sup> Moreover, formal historical models link population growth to political instability. Elaborating on earlier work by Jack Goldstone, Peter Turchin and his associates are in the process of devising a comprehensive reinterpretation of much of world history that hinges on the notion of predictable relationships between demographic developments and state formation.<sup>96</sup> However, while Turchin and Sergey Nefedov seek to link the collapse of the Roman republican system to demographic growth, they do so on the basis of the low count developed by Beloch and Brunt.<sup>97</sup> Luuk de Ligt likewise argues that even the relatively moderate demographic growth associated with the

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<sup>94</sup> Morley (2001, 59–61).

<sup>95</sup> Scheidel (2004, 10–12) for quantification.

<sup>96</sup> Goldstone (1991); Turchin (2003, 118–69); Turchin (2005); Turchin and Korotayev (2006); Turchin and Nefedov (forthcoming).

<sup>97</sup> Turchin and Nefedov (forthcoming).

low count was sufficient to fuel conflict over land and trigger political unrest from the late second century BCE onward.<sup>98</sup>

Political instability cannot be meaningfully related to a particular level of population density: there is no way of telling if an average of (say) 100 persons per square kilometer was required to set off unrest in Roman Italy, or if a mean of half as many would have been sufficient to produce the same outcome. For this reason alone, extrapolations from observed crisis to inferred population numbers are inherently untestable. All we can say is that, in light of comparative evidence, social conflict may become easier to account for as our estimates of population density rise.

At the same time, the notion of crisis precipitated by population pressure and attendant land hunger is hard to reconcile with the logic of labor relations as set out in the preceding section: it is hard to see how strong demand for labor can coincide with violent struggle over farmland. Strong segmentation of the labor market may go some way to explaining this combination: if demand was centered on scarce skilled labor, wage competition between slaves and free farmers would have been weak. However, this scenario would nevertheless fail to account for the use of slaves in farming and menial labor, or for increasingly costly military recruitment. At present, I see no way of resolving this paradox.

To complicate matters further, my earlier suggestion that colonization might reflect population pressure makes it more difficult to explain developments after the end of the civil wars: if rising population densities had impelled migration in the republican period, the presence of an even larger population in the Augustan period (and probably even more so in the following generations) would sit uneasily with the cessation of colonization programs after 14 BCE and the concurrent lack of social unrest. And although lifetime military service overseas would have continued to serve as the functional equivalent of overseas settlement, its significance appears to have diminished at the same time as population numbers either peaked or at any rate remained very high (see above, section 4). These correlations cast doubt on the superficially plausible notion that the instability of the late-republican period was causally linked to population pressure. Morley himself implicitly acknowledges this problem by stressing that “it is a measure of the

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<sup>98</sup> De Ligt (2004).

achievement of Augustus and successors in unifying and pacifying Italy that this crisis of overpopulation did not apparently lead to any major unrest; Italy under the Principate, it has been said, ‘has no history’.<sup>99</sup> If high population densities had been a serious problem earlier, why did even higher ones not have any unfavorable consequences later on? As it is, the evidence can be read both ways, in support of either the ‘low’ or the ‘high’ counts. It is therefore inconclusive with regard to the question of absolute population numbers.

### IX. *Living standards*

Recent and ongoing research on proxy indicators of Roman living standards tends to emphasize beneficial developments in the late-republican and early monarchical periods. For example, Wim Jongman’s new meta-survey of meat consumption shows a surge in the incidence of animal bones at numerous sites in both Italy and the provinces.<sup>100</sup> Osteological data for body height, a marker of physiological well-being, point in the same direction. Kron has demonstrated that average male body height in the Roman period (from 500 BCE to 500 CE) reached mid-twentieth-century means.<sup>101</sup> In a more sophisticated analysis, Jongman and Gerda Klein Goldewijk aim for a more refined chronological resolution (grouping data by half-century): while we still await the final results, preliminary findings support the impression of improvements during at least parts of this period.<sup>102</sup> These trends match late-republican and early-imperial peaks in the number of Mediterranean shipwrecks, in mining output, and in the coin supply, all of which are broadly indicative of economic development.<sup>103</sup> In this context, evidence of physiological well-being in sub-elite groups assumes pivotal importance: whilst the benefits from an expansion in long-distance trade or monetization might in theory have been largely confined to the better-off, upward trends in body height or nutritional status would suggest more widespread improvements.

<sup>99</sup> Morley (2001, 59), quoting Fergus Millar.

<sup>100</sup> Jongman (2007, 613–14), based on King (1999) and MacKinnon (2004).

<sup>101</sup> Kron (2005a), and in this volume.

<sup>102</sup> Jongman and Klein Goldewijk (in progress). But cf. Koepke and Baten (2005a) for a conflicting reading of the data.

<sup>103</sup> De Callatay (2005) attempts a brief synopsis.

In the most general terms, evidence of improved living standards in the sub-elite population of Roman Italy would be logically incompatible with the notion of overpopulation or population pressure, defined as a ratio of labor to resources that made it more difficult to maintain normative living standards, let alone generate per capita consumption growth. As we will see in section 12 below, the high count posits a population of Italy that was very large by premodern standards. This would make it seem *a priori* unlikely that this population experienced significant improvements in physiological well-being. Lo Cascio, in his defense of the high count, has indeed presented ancient evidence that may be read as suggestive of population pressure in the early Principate, and has noted resultant vulnerability to later epidemic events.<sup>104</sup> Lo Cascio and Malanima also argue that the relatively very large population of Roman Italy was only made possible by an unusually favorable concatenation of different factors ranging from climatic change and improved labor arrangements to technology and institutions that “displaced outward the production possibility curve”, that is, temporarily raised output beyond otherwise sustainable levels.<sup>105</sup> All this underlines the ecologically precarious position of an ancient Italian population that had grown to the levels implied by the high count. However, solid archaeological support for elevated levels of physiological well-being in this period would be inconsistent with these predictions. This poses a particular challenge to the position of Kron, who, unlike Lo Cascio, argues *both* for a very large population *and* high living standards in Roman Italy.<sup>106</sup> This logically requires exceptionally strong economic performance, capable of sustaining not only a population density of peninsular Italy that was not attained until some time in the nineteenth century (see below, section 12) but also levels of well-being that may not have been reached until the twentieth century, most notably in terms of body height. Kron has yet to provide a coherent presentation of this historically implausible model. Judging from his published work, this reconstruction will be justified with reference to advanced Roman techniques in farming and husbandry that supposedly enabled the Romans to square the circle and contain Malthusian pressures.<sup>107</sup>

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<sup>104</sup> Lo Cascio (2004a), (2004b).

<sup>105</sup> Lo Cascio and Malanima (2005, 214–24).

<sup>106</sup> Kron (2005a and 2005b).

<sup>107</sup> For this see Kron (2000), (2002), (2004a), (2004b), (2005c), and work in progress. See also in this volume.

This optimistic approach faces two problems. One is that the fact that the Romans had developed certain productivity-enhancing techniques such as crop rotation does not tell us how widely they were employed. More importantly, Kron's scenario is one of extreme Roman exceptionalism: comparative evidence shows ever more clearly that in later periods of European history population growth invariably depressed average real wages, and the situation in China appears to have been similar.<sup>108</sup> Economic-demographic 'efflorescences' did occasionally occur but were invariably terminated by Malthusian constraints.<sup>109</sup> Some measure of support for concurrent demographic expansion and intensive economic growth that continued for a number of centuries may be found in Ian Morris's recent work on standards of living in ancient Greece.<sup>110</sup> However, these findings rely entirely on proxy data and do not draw on direct evidence for mean real incomes, and are therefore of a different character than better supported and more pessimistic observations regarding the more recent past. None of this means that any model of a—by historical standards—very large and prosperous population of Roman Italy is necessarily incorrect; yet it is certainly implausible and therefore requires solid evidentiary support to merit serious consideration. The less likely a reading is, the better the supporting data have to be. In a situation such as this, the exact opposite is the case: the data for the Roman period are generally poor, contested, and ambiguous, whereas the comparative evidence in support of long-term Malthusian constraints is fairly consistent and of better quality.

In essence we have a choice between a densely populated Italy (and, by extension, empire) that enjoyed a relatively high degree of well-being, a situation that would have been truly exceptional and inconsistent with broader historical patterns; an equally large but increasingly immiserated population, an option that seems less far-fetched but would clash with evidence of elevated living standards; and lower population densities that coincided with a measure of generalized prosperity, which is likewise plausible and more readily compatible with existing indicators of well-being. At present the available information about living standards is still insufficient to provide a reliable guide in identifying the most likely scenario: too much remains unclear about the representative

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<sup>108</sup> E.g. Allen (2001); Hoffman *et al.* (2005).

<sup>109</sup> Goldstone (2002).

<sup>110</sup> Morris (2004), (2005a).

character and the precise chronology of the material evidence. What I have meant to show instead is that our reconstructions of Roman population are logically tied to our interpretation of diverse sets of data and tightly enmeshed with our understanding of the nature of the Roman economy: population estimates do not exist in a vacuum but are interdependent with our assessment of other major elements of Roman history.

### X. *Field surveys*

Until and unless archaeologists are able to address the grave concerns raised by Robin Osborne in his recent critique of demography and survey, field survey data will be of little value to the debate at hand, especially given that survey results cannot shed much light on population numbers per se.<sup>111</sup> I include this category of data merely because ongoing work continues to feature claims about absolute population size: the most pertinent example is Rob Witcher's survey of Rome's *suburbium*.<sup>112</sup> Based on the findings of a number of field surveys conducted close to the capital, Witcher conjectures "an average rural density of two farms and one villa per km<sup>2</sup>" within a 50-km radius from the city of Rome. This suggests a total of 10,830 farms and 5,415 villas inhabited by between 135,375 and 433,200 persons (a range determined by a series of assumptions about the size of the average farm or villa), to which we need to add another 2,500–10,000 persons in villages and 55,400–201,000 in urban or other nucleated centers (depending on another series of assumptions about the average population of different types of settlements). The implied total population ranges from 193,275 to 644,200, or 35.7 to 119 people/km<sup>2</sup> of cultivable land. Witcher opts for an estimate of 326,000. For a second, outer ring covering the area located between 50 and 100 km from Rome, other field surveys indicate an average of 1.5 farms and 0.2 villas/km<sup>2</sup>, and thus—applying the same method as before—a population of 95,000–294,000 in the countryside (with a preferred estimate of 154,000), or 384,000 including cities and villages. This implies mean population densities of 60/km<sup>2</sup> in

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<sup>111</sup> Osborne (2004). For earlier discussions of this topic cf. Sbonias (1999a, b); Osborne (2001). Cf. also Witcher (2006a) for a general discussion of field survey.

<sup>112</sup> Witcher (2005). See also Fentress (forthcoming) on Cosa and Jerba.

the inner zone and 42/km<sup>2</sup> in the outer zone, and urbanization rates of one-third in the former and one-half in the latter.<sup>113</sup>

As Witcher observes, his preferred ‘informed estimate’ yields a population density for cultivable land near Rome that fits the overall Italian mean implied by the low count but greatly falls short of the average densities required by the high count.<sup>114</sup> This notion is correct but entails two key assumptions that are by no means obvious. For one, this reading compels us to accept that the ‘informed estimate’ is preferable to high end estimates. If we consistently adopt the latter, it is the high count that provides the best match. Moreover, it presupposes very high recovery rates of farm sites through field survey. The more rural sites there are that remain undiscovered, the further upwards we need to adjust population density even if we accept the validity of the site size estimates of the ‘informed estimate’. Taken together, these two problems undermine much of the demographic analysis of the survey data.

The two zones together comprise 14,466 km<sup>2</sup> of cultivable land, or somewhere around 19,520 km<sup>2</sup> of any kind of land overall.<sup>115</sup> Witcher’s preferred assumptions about site size produce a total population of 710,000, or 49 persons/km<sup>2</sup> of cultivable land and 36 persons/km<sup>2</sup> of all land. By contrast, the most generous assumptions about site size yield an aggregate population of 1,398,000, or 97 persons/km<sup>2</sup> of cultivable land and 72 persons/km<sup>2</sup> of all land. The notion of a non-metropolitan Italian population of 5 to 6 million and Beloch’s guess of 100,000 km<sup>2</sup> of cultivable land in mainland Roman Italy (i.e. 40% of its surface area) translate to a mean of 50–60 persons/km<sup>2</sup> of cultivable land and 20–24 persons/km<sup>2</sup> of all land.<sup>116</sup> The former is perfectly consistent with Witcher’s mean of some 49 persons/km<sup>2</sup> on cultivable land near Rome, especially if we adopt his own guess that 50% of Italy’s surface may have been under cultivation (for a low

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<sup>113</sup> Witcher (2005, 126–130). Witcher’s own total of 356,000 for the inner zone is the result of a computational error: in table 2 on p. 128 the maximum population estimate for Ostia (60,000) rather than the ‘informed estimate’ for Ostia (30,000) is included in the ‘informed estimate’ of the total.

<sup>114</sup> Witcher (2005, 130), reckoning with 50% cultivable land in Italy, 6 million people outside Rome in the low count (for a mean of 48 persons/km<sup>2</sup> of cultivable land, between his figures of 60 and 42 for the inner and outer *suburbium* respectively), and with 14 million Italians in the high count, for an overall mean of 112 persons/km<sup>2</sup> of cultivable land.

<sup>115</sup> Witcher (2005, 127 and 129).

<sup>116</sup> Population: above, section 4. Italy: Jongman (1988, 67); cf. already Beloch (1886, 439–40), quoting Nissen.

count mean of 40–48 persons/km<sup>2</sup> of cultivable land). These figures also make sense if we conjecture that the region around Rome was 1.5 times as likely to contain cultivable land as was Italy overall.

By contrast, the high count logically implies a much larger non-metropolitan population: at a minimum of 15 million and a more generous 60% of Italy under cultivation (to bias the estimate in favor of the high count), the required density is 100 persons/km<sup>2</sup> of cultivable land and 60 persons/km<sup>2</sup> of all land; at 17 million the corresponding rates are 113 and 68 persons/km<sup>2</sup> respectively; at a notional maximum of 19 million they reach 127 and 76 persons/km<sup>2</sup> respectively. Thus the high count predicts some 100–120 persons/km<sup>2</sup> of cultivable land, or between two and two-and-a-half times Witcher's preferred number for the *suburbium*. At the same time, this requirement is more or less met by the highest estimate allowed by Witcher's scheme: 97 persons/km<sup>2</sup> of cultivable land. Naturally, proponents of the high count would be tempted to support high site estimates on *a priori* grounds (in favor of larger farms, villages, towns, etc.) simply because they are consistent with their view of conditions at the time. Unfortunately, since these variables cannot be fixed empirically, we are left with a circular approach: the results depend on the starting assumptions that different observers find most congenial. For this reason, the survey data do not permit independent testing of competing hypotheses about population size, because their demographic interpretation is inevitably conditioned by unfalsifiable starting assumptions.

Additional uncertainties arise from the questionable accuracy of field survey in detecting small sites, i.e. 'farms'. Even if we were to adopt, for the sake of argument, the middling starting assumptions of Witcher's 'informed estimates', it might nevertheless be possible to raise overall population levels to a degree that would render them more readily compatible with the high than with the low count. The difference between the 710,000 residents conjectured by Witcher and the 1,446,000–1,776,000 persons predicted by the high count (at 100–120 persons/km<sup>2</sup> of cultivable land) amounts to between 736,000 and 1,066,000 individuals. If we—no doubt over-schematically—accounted for this entire shortfall as the result of farms that had been missed by the surveys, we would need to posit the presence of an additional 92,000 to 133,250 farms (at 8 persons per farm) to supplement the 24,400 farms inferred by Witcher. If some 80–85% of all farms that once existed had remained invisible, actual population density could have matched the projections of the high count.

Archaeologists will object that this notion is unfair or extreme. However, any combination of the two possible sources of bias that I have identified—site size estimates and recovery rates for small sites—would also permit us to conjecture much higher population densities. In fact, for Witcher’s reconstruction to lend support to the low count, we would have to accept not only that his ‘informed estimates’ about site size are consistently of the right order of magnitude, but also, and far less plausibly, that the surveys his calculations are based on achieved near-perfect recovery rates even for small farmsteads. In view of the well-documented relationship between survey intensity and recovery rates as well as other problems,<sup>117</sup> this would seem a fairly heroic assumption to make.

From that perspective, the most reasonable reading of the field survey data might actually suggest a somewhat higher population density than that predicted by the conventional low count.<sup>118</sup> For example, if the surveys had missed merely one-half of all farm-sized sites, we would need to add another 24,400 farms with 195,000 residents, raising overall population density to 63 persons/km<sup>2</sup> of cultivable land. If they had missed two-thirds, this density rises to 77 persons/km<sup>2</sup> of cultivable land. Even with Witcher’s conservative site size estimates, this would push overall population tallies into an intermediate zone between the low and high counts.

All in all therefore the evidence of field surveys from the *suburbium* indicates that while the low count may require unrealistically optimistic assumptions about the level of survey resolution, the high count compels us either to adopt the highest estimates of site size or reckon with a very low recovery rate for small sites, or a mixture of both. This reinforces my contention that, given the right starting assumptions, the survey data can be made to fit dramatically divergent models of Roman demography.

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<sup>117</sup> For the former see e.g. Terrenato (2004, 39–40, figs. 4.1 and 4.3). For other problems see Patterson (2006, 14–16).

<sup>118</sup> Although Witcher (2005, 126 n. 43) acknowledges that “most surveys recover only a small percentage of rural sites due to erosion and stochastic processes affecting visibility” and that his conjectures “are more likely to lead to under- rather than over-estimation of population”, he nevertheless fails to apply these powerful caveats to his own demographic inferences and more importantly does not even attempt to assess the potential impact of these problems on his ability to choose between the ‘low’ and ‘high’ counts. As I hope to have shown here, once these problems are properly taken into account rather than buried in a footnote, the demographic promise of this evidence all but vanishes. For a partial retreat see now Witcher’s chapter in this volume.

Extrapolations from the (essentially obscure) population density in the *suburbium* to that of the whole of Italy are fraught with additional problems. Mattingly and Witcher have shown that even if we try to control for differences in survey intensity, settlement density in early imperial Italy tends to be positively correlated with the presence of urban settlements and proximity to Rome (two features that are themselves positively correlated).<sup>119</sup> This might suggest potentially significant variation in actual population density even on cultivable land, in the sense that land may have been less densely settled farther away from the capital than nearby. If that impression could be substantiated, it would become more difficult to reconcile the evidence from the *suburbium* with the high count.

The problem of site visibility also undermines attempts to derive urbanization rates from survey data. If a significant proportion of small sites routinely eludes detection, estimates such as Witcher's nucleation rates of 30% for the inner zone of the *suburbium* and of 50% for the outer ring cannot carry much weight:<sup>120</sup> although they might seem consistent with the (low count) notion of strong nucleation discussed in section 5, they may just as well be more apparent than real.

For the purposes of historical demography, the principal value of survey data may lie in their capacity to illuminate relative changes in settlement density over time. While it is true that such changes may reflect variation in nucleation patterns rather than absolute population numbers, positive correlations between site density and population density have been observed: for example, a comparison of field survey data and Ottoman population registers for Boeotia from the fifteenth through nineteenth centuries indicates parallel trends across three broadly defined periods.<sup>121</sup> Unfortunately, given the lack of local population records, it is impossible to replicate these results for Roman Italy.

It is becoming increasingly clear that no consistent trends in the amount of scatter and site density can be attributed to Roman Italy as a whole.<sup>122</sup> This means that even if survey data could be linked to demographic developments, the picture would nevertheless remain

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<sup>119</sup> Mattingly and Witcher (2004, esp. 181–3 and 183 fig. 13.6). For the correlation of urbanism and distance to Rome see Duncan-Jones (1982, 339).

<sup>120</sup> Witcher (2005, 129 [corrected as per n. 112] and 130 n. 67). The same is true (e.g.) of the 42% urbanization rate for the territory of Jerba in Fentress (forthcoming).

<sup>121</sup> Sbonias (1999b, 223–5, esp. 225 figs. 16.6–7).

<sup>122</sup> See now esp. Patterson (2006, 6–7 and 72–88).

ambiguous. Recent studies have emphasized regional diversity and the multidirectionality of change. For instance, in a study of more than thirty surveys of Roman Etruria, Witcher distinguishes between three distinct zones—the *suburbium*, the coast, and the interior.<sup>123</sup> The evidence indicates growth close to the capital, stagnation or decline in the interior, and mixed signals on the coast.<sup>124</sup> In as much as it is permissible to assign demographic significance to these shifts, they may well reflect population movement towards Rome and its *suburbium* rather than natural growth or decline.<sup>125</sup> To be sure, migration would make it even more difficult to relate changes in survey patterns to changes in overall population.

However, population movement is perhaps less likely to account for changes in more peripheral regions. This raises the question of how to interpret decline in scatter density in areas such as Samnium or southern Italy in the early monarchical period.<sup>126</sup> In the most comprehensive analysis of field surveys from peninsular Italy, John Patterson has shown that in roughly two-thirds of all cases decline either occurred already from the first century CE onward or commenced in the following century. Most other areas exhibit continuity during this period, whereas examples of second-century CE growth are relatively rare.<sup>127</sup> To the extent that these findings reflect demographic developments, they may seem inconsistent with the notion of ongoing population growth beyond the Augustan period (see above, section 4). This would speak in favor of the view that the Italian population peaked in the first rather than the second century CE.

### XI. *Carrying capacity*

Given the right mix of favorable guesses about cultivated area, grain yields, fallowing, and crop rotation, Roman Italy would probably have

<sup>123</sup> Witcher (2006b, esp. 91).

<sup>124</sup> Ibid. 101 fig. 4. However, when Witcher (90–91) speaks of “direct indicators of general (i.e. agricultural) prosperity”, this betrays a common misconception: in a premodern economy more people or more farming do not normally mean more per capita prosperity, which is the only kind that really matters (cf., e.g. Frier (2001) and above, section 9).

<sup>125</sup> Ibid. 121–2. Cf. also Witcher (2005) for the economy of the *suburbium*. For migration in general see Scheidel (2004) and Patterson (2006: 33–48).

<sup>126</sup> Noted by Witcher (2006b, 121), with reference to Witcher (1999).

<sup>127</sup> Patterson (2006, 72–88).

been capable of producing enough food to support a population of the size predicted by the high count.<sup>128</sup> Conversely, under less optimistic starting assumptions, the low count will seem a more plausible option. There is no obvious way to extrapolate from agrarian potential (which is itself in part determined by population numbers) to demographic conditions. I would merely like to note in passing that for any given system of production, carrying capacity was sensitive to climatic conditions. Broadly speaking, the period of the late Republic and the early Principate coincided with a climate optimum of relatively high temperatures that would have been conducive to farming and elevated population densities.<sup>129</sup> It is, however, important not to overstate the extent of this trend;<sup>130</sup> and in any case, it is not at all clear how one would establish a direct connection between this observation and any particular estimate of absolute population size.

## XII. *Comparative demographic evidence*

Given the inconclusive nature of much of the ancient source material and modern arguments, comparative evidence assumes especial importance in this debate. On previous occasions I have referred to both ancient and more recent comparanda in order to demonstrate some of the weaknesses of the high count.<sup>131</sup> As described in section 4, that scenario, if taken to its logical extremes, implies the existence of a very large population in peninsular Italy, similar to conditions at different stages of the nineteenth century: with anywhere from 17 to 20 million people residing in Italy, and perhaps three-fifths of them located in the peninsula, we have to reckon with 10 to 12 million peninsular residents, which equals the corresponding totals for the 1840s or even

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<sup>128</sup> For various maximum estimates and their implications see, e.g. Scheidel (2001b, 54 n. 216); Morley (2001, 56); Scheidel (2004, 7). Cf. also Lo Cascio (1999b).

<sup>129</sup> E.g. Greene (1986, 81–5); Schmidt and Gruhle (2003); Fagan (2004, 189–212); Koepke and Baten (2005b, 152 fig. 3); Malanima and Lo Cascio (2005, 218–19); Salaries (2007, 19–20), with further references. For the connection between climate and population growth see in general Galloway (1986).

<sup>130</sup> See now esp. the reconstructions in Jones and Mann (2004). Cf. also Heide (1997).

<sup>131</sup> It should be noted that this method does not *eo ipso* validate the low count, despite the fact that I have sought to employ it for this purpose, if only for want of more appealing alternatives.

the 1880s.<sup>132</sup> I sketched out some of the logical implications of this reconstruction back in 2004.<sup>133</sup> To reiterate my main points in all brevity, if we retained the provincial population figures suggested by defenders of the low count for the provinces, Italy would have been much more densely populated than other Mediterranean regions with a similar ecology (with one-quarter of the imperial population concentrated in one-sixteenth of its territory, and an Italian population equalling that of Gaul, Iberia, and half of the Maghreb combined, which it never did in later periods). Conversely, if we assume that other parts of the empire were correspondingly more densely settled as well, the Roman empire would have been inhabited by up to 160 million people, a tally that was not attained in this region as a whole until the mid-nineteenth century; that would indeed push implied population levels in some of the eastern provinces into early to mid-twentieth-century territory, and would make it hard to explain why the empire was unable to tax its way out of later barbarian invasions. If we were to believe that only the western provinces were as densely populated as Italy, up to four-fifths of the total imperial population must have been concentrated in the ‘Latin’ half, which raises the question why that part collapsed earlier than the far less populous eastern half of the empire. There is currently no exposition of the high count that has even begun to engage with the crucial issue of how its claims regarding the imperial heartland affect our understanding of the empire as a whole and of its position relative to other historical periods.<sup>134</sup> This neglect is all the more unfortunate as these implications are among the most serious challenges to the high count, if not indeed—as I am inclined to believe—its single most serious handicap.

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<sup>132</sup> Population in 1850: 10.6 million (Del Panta *et al.* 1996, 277); in 1881: 12.2 million (Bellettini 1987, 176).

<sup>133</sup> Scheidel (2004, 6–8).

<sup>134</sup> As far as I can see, Frank (1933 and 1940) had no interest in these wider implications of his population estimate for Roman Italy. Lo Cascio usually focuses on Italy alone and has defended a moderately high count only for Egypt (Lo Cascio 1999c), which is, however, far below the adjustment necessitated by an empire-wide high count: see Scheidel (2004, 8 and n. 47). Kron (2005b) maintains the same focus on Italy to the near-exclusion of the outside world: he merely points out that Roman Egyptian population levels were not reached again until the nineteenth century, which is uncontroversial (2005b, 484–5); cf. already Frier (2000, 814); Scheidel (2001a, 242–8), and that Pompey’s boast (Plin. *NH* 7.97–98) that he conquered 12,183,000 people in the East matches the Ottoman population of the Asian provinces of the Roman empire for the 1870s (2005b, 485–6), which is likewise not implausible and is consistent with the low count: see already above, n. 37, and below, n. 151.

At the same time, however, comparative evidence also poses what I would consider the most serious challenge to the low count as proposed by Beloch and developed by Brunt. This challenge arises from modern estimates of the size of the population of Italy in the High Middle Ages, on the eve of the Black Death. Kron's recent assertion that the low count suggests that the population of Roman Italy "was barely half as great" as the same region's medieval population provides a convenient starting point.<sup>135</sup> Put this way, his claim is clearly excessive: Kron appears to juxtapose a Roman population of "less than six million, as implied by the hypothesis of Beloch and Brunt" with a medieval peak of unspecified magnitude but in excess of Beloch's estimate of 11,647,000 for 1660.<sup>136</sup> This comparison misrepresents the logic of the low count. Yet even if we allow for a Roman Italian population maximum of closer to 7 or 8 million (as explained in section 4) and a medieval maximum in the order of 10 to 11 million (as explained below), the fact remains that without Brunt's indefensible *deus ex machina* of additional millions of slaves, the low count falls short of the likely medieval peak by a considerable margin.

This conclusion is hard to resist even though the size of the Italian population around 1300 is in fact empirically unknown (and forever unknowable). Modern estimates are merely crude extrapolations from aggregate (estimated) urban population numbers. Thus the latest estimate of 12.5 million Italians in 1300 (which includes Sicily and Sardinia) is derived from three assumptions: that urban residents numbered between 2.5 and 3 million; that they represented some 20–25% of the total population, despite the fact that this would have been "eccezionale per l'Europa del tempo"; and that the middle value of the resultant range of 10 to 15 million (12.5 million), "ci pare verosimile".<sup>137</sup> In other words, the final total is the result of questionable estimates or

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<sup>135</sup> Kron (2005b, 495).

<sup>136</sup> Kron (2005b, 486). Kron does not cite modern population estimates for the Middle Ages. His claim misrepresents the position of Brunt, who reckons with 7.5 million people in Italy in 14 CE. It is true that Brunt's figure of 3 million slaves at least needs to be halved, reducing the overall tally to 6 million, in keeping with Beloch's estimate (see above, n. 11). However, in the characteristic fashion of the 'high counters', Kron does not seem ready to account for further Italian population growth after Augustus (see above, section 4). Lo Cascio and Malanima (2005, 205 and 207) cite "old" or "traditional" estimates of a Roman peak of 7 million and a medieval peak of 11 million, which would make the latter only less than one-and-a-half times as large as the former.

<sup>137</sup> Pinto (1996, 42).

outright guesses regarding urban populations and a sweeping *a priori* assumption about the average urbanization rate.<sup>138</sup> Needless to say, the latter is strictly speaking irrecoverable. Moreover, the existing data do not permit us to assign accurate population figures to the approximately 200 cities of 5,000 or more inhabitants which are thought to have existed in Italy around 1300. On closer inspection, the numbers in Malanima's comprehensive tabulation of medieval city sizes turn out to be highly schematic: 55 of 199 cities in his database, or 28% of the total, are assigned 5,000 inhabitants each; 99, or fully one-half of all tallies, end in multiples of 5 (i.e. are crudely rounded); cities of 5,000 (55) are as numerous as those of 6,000 (26), 7,000 (21) and 8,000 (9) taken together.<sup>139</sup> Moreover, in some cases recent guesstimates far exceed earlier ones.<sup>140</sup>

For all these reasons, the medieval 'evidence' may seem to furnish an exceedingly flimsy basis for serious comparisons. Despite these unpromising circumstances, however, the limits of the plausible are not unduly flexible. The guesstimate of 12.5 million for 1300 matches the population total assumed for the late sixteenth century, when the recovery from the Black Death had been completed, and is close even to the tally of 13.6 million for 1700.<sup>141</sup> Earlier conjectures reckoned with 10 or 11 million around 1300/1340, comparable perhaps to the most recent estimate for the early sixteenth century.<sup>142</sup> Increasingly well-known demographic developments in the early modern period constrain our assumptions about the medieval period: for that reason

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<sup>138</sup> It merits attention that Malanima's studies treat this population estimate as an independent variable, using it to 'calculate' urbanization rates. In so doing, he merely reverses the process that created this total in the first place. An additional problem arises from the fact that Malanima (1998, 118) arrives at only 2,571,000 urban residents, which—using Pinto's multiplier—would translate to a total population of 10.3–12.9 million, or a mean of 11.6 million, although Malanima himself expresses a preference for Pinto's tally of 12.5 million (*ibid.* 97).

<sup>139</sup> Malanima (1998, 110–118). This should not be taken as a criticism of Malanima's valiant efforts but is simply meant to demonstrate the inevitable limits of this kind of exercise.

<sup>140</sup> Venice has grown from Beloch's 30,000 (1961, 341) to 110,000 in Malanima (1998, 111) and Ginatempo and Sandri (1990, 100); Modena from Beloch's 5,357 (1961, 341) to 19,000 in Malanima (1998, 112) and 20,000 in Ginatempo and Sandri (1990, 88); Milan from 62,500 in Beloch (1961, 342) to 150,000 in Malanima (1998, 111) and 150–200,000 in Ginatempo and Sandri (1990, 100).

<sup>141</sup> Del Panta *et al.* (1996, 275).

<sup>142</sup> References in Malanima (1998, 124 n. 14): 11 million in 1300; and see also McEvedy and Jones (1978, 107): 10 million in 1300; cf. Del Panta *et al.* (1996, 275) for the early sixteenth century.

alone, none of these figures is likely to be very wide of the mark. However, since there is no way of telling whether the population of Italy around 1300 most closely resembled that of the early sixteenth century, the late sixteenth century, or the late seventeenth century, we cannot meaningfully distinguish between conjectures in a range from perhaps 10 to 13 million for the medieval demographic peak: in statistical terms, they are all equally likely.<sup>143</sup> When excluding the islands, the range for mainland Italy has to be reduced by close to one million,<sup>144</sup> to a somewhat lower range of between 9 and 12 million.

*Faute de mieux*, this conjectural framework is what we have to work with. However, unless we were to posit some kind of population explosion in early modern Italy that would enable us to project a far lower medieval population maximum, it is hard to conceive of a substantially different scenario for the High Middle Ages. As far as the peninsula is concerned, the proposed medieval tally can readily be reconciled with the predictions of the low count as developed in section 4. Hence, if we accept the notion that 1,244,000 million people lived in cities of 5,000+ inhabitants in the peninsula in 1300,<sup>145</sup> and that they accounted for 20 to 25% of the overall population,<sup>146</sup> the total population in 1300 would have amounted to between 4.1 and 5 million. This broadly matches low count estimates for the Roman monarchical period: of around 6 million people in mainland Italy in 14 CE and maybe 7 or even 8 million later on, some 4.5–5.5 million might have inhabited the peninsula proper. If we exclude half of the Roman metropolitan population in order to control for the exceptional size (and external supply) of the imperial capital, we obtain an adjusted peak estimate of between 4 and 5 million for the Roman-era peninsula, in keeping with the ‘best’ conjecture for 1300.

The picture for northern Italy is very different. For 1300 this region has been assigned an urban population of 987,000, thought to represent a mere 15% of the overall total,<sup>147</sup> and hence indicative of a regional tally of some 6–7 million. The discrepancy between this number and

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<sup>143</sup> As explained above in n. 137, Malanima’s method, if properly employed to his own specifications, suggests a total of 11.6 million for 1300, which lies in the middle of my range.

<sup>144</sup> Del Panta *et al.* (1996, 277) assign 900,000 inhabitants to Sicily and Sardinia in 1300.

<sup>145</sup> Talled up from Malanima (1998, 112–16).

<sup>146</sup> As assumed by Pinto (1996, 43).

<sup>147</sup> Malanima (1998, 110–12); Pinto (1996, 43).

a putative Roman population of 1.5–2.5 million that can be accommodated by the low count is undeniably quite dramatic.

We may conclude that the latest estimates for Italy in 1300, for what they are worth, are not incompatible with a low count population estimate for peninsular Italy under the early Principate. By contrast, proponents of this scenario must inevitably assume massively different levels of demographic development in northern Italy in the two periods: if that region was not four times as populous in 1300 as it had been under Augustus, and maybe two-and-a-half times as populous as it may have become by the second century CE, the low count model cannot be sustained.

Kron is surely right to put particular emphasis on the demography of northern Italy and its relevance for competing scenarios.<sup>148</sup> As he points out, Brunt's reconstruction implies that the (free) population of northern Italy in the mid-first century BCE was equivalent to around 20% of that of the peninsular population, whereas comparative evidence from the early modern period consistently suggests a respective value of closer to 75%.<sup>149</sup> While this problem might to some extent be mitigated by positing substantial post-Roman growth in the north, even under the most favorable assumptions Roman population levels in that region were unlikely to have reached even half of those in 1300.<sup>150</sup> Thus, while Kron's assertion that the Roman population implied by the low count was "barely half as great" as the medieval population<sup>151</sup> does not in fact apply to the peninsula, it correctly describes the situation in northern Italy, which thereby becomes a pivotal element in the debate. In view of this, it is all the more important that De Ligt's contribution in this volume makes a strong case that Roman northern Italy may indeed have been as sparsely populated as predicted by the

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<sup>148</sup> Kron (2005b, 461–82). I concur with his assessment that "the most important factor undermining the plausibility of his [viz., Brunt's] hypothesis (and one which has received surprisingly little emphasis given its importance) is the dramatic effect upon the population of Italy resulting from the extension of the citizenship to the province of Gallia Cisalpina" (461).

<sup>149</sup> Kron (2005b, 462).

<sup>150</sup> Since a certain population size in peninsular Italy is required to match high medieval population estimates even if we control for the unusual size of Rome, this leaves only a limited population of perhaps (as a very crude guess) 1.5–2.5m for the north, equivalent to 33–45% of the population of the peninsula, or about half of the corresponding proportion from 1550 to 1800.

<sup>151</sup> Kron (2005b, 495).

low count: all future discussion of this issue will have to engage with his premises and findings.

Broader historical context also matters. As noted at the beginning of this section, the high count invites us to accept that population densities in the Roman peninsula approached those of the mid or late nineteenth century. Depending on one's perspective, this need not be entirely impossible: after all, current consensus has it that the southern and eastern provinces of the empire, from the Maghreb and the southern Balkans to Asia Minor, the Levant, and Egypt, were as richly populated in the Roman period as they came to be at different times in the nineteenth century.<sup>152</sup> This raises the question whether a similar analogy might legitimately be envisioned for peninsular Italy as well. If we are prepared to accept that Roman population levels in the western European provinces (Iberia, Gaul, Britain) may have resembled very roughly those of the High Middle Ages, and that those for Greece and the African and Asian provinces tended to match those of the nineteenth century, we are forced to make difficult choices for an Italy that is rather inconveniently positioned right in between these two principal zones of post-ancient development. Did this region follow a 'western' or 'southern-eastern' trajectory? While the high count puts peninsular Italy firmly in the 'southern-eastern' camp, estimates for Roman northern Italy fall far short of nineteenth-century levels. The low count, by contrast, puts peninsular Italy squarely in the 'western' camp, whereas northern Italy is left far behind. Taken as a whole, the Italy of the low count clearly fails to reach likely medieval maxima. This should be a source for serious concern, given Italy's prominent position in the Roman world, and greatly diminishes the appeal of the low count.

From a comparative demographic perspective, some kind of intermediate scenario might be worth considering. For instance, Hin's model, as introduced in section 4, suggests a peninsular population perhaps even somewhat ahead of the medieval maximum, and more importantly helps to align northern Italy more closely with later totals.<sup>153</sup> It would yield the double benefit of making Italy as a whole more compatible with the 'western' scenario of post-ancient development, and lowering

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<sup>152</sup> Frier (2000, 814). See also Scheidel (2001a, 242–8) for Egypt, and more generally McEvedy and Jones (1978, 139, 143, 151, 227). Cf. also Hansen (2006, 87–91) for Greece.

<sup>153</sup> See above, n. 42, and in this volume.

it from the precarious heights of the high count that is—rather inadequately—given visual expression in Lo Cascio and Malanima’s graph of demographic change across 2,000 years of Italian history and pushes Roman population estimates *far* beyond medieval levels.<sup>154</sup> A stronger presence of Roman citizens overseas would produce a similar outcome. These ‘convenient’ consequences, needless to say, in no way establish that such alternative readings are correct, and/or that they are defensible on other grounds; they do, however, lend some urgency to calls for a critical reconsideration of the intellectual validity of the rigid dichotomy of low versus high counts that has come to dominate—and perhaps stifle—the debate.

### XIII. *Where do we go from here?*

This survey has failed to produce a conclusive answer to the question of the size of the population of Roman Italy. The census data are open to too many conflicting readings to offer any simple solutions. A number of features do not strongly favor either ‘high’ or ‘low’ estimates of overall population size: by my reckoning these include urbanization rates, military mobilization rates in the republican period, data generated by field surveys, and potential carrying capacity. Some facts speak

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<sup>154</sup> Lo Cascio and Malanima (2005, 208 fig. 2 and tab. 2) juxtapose a Roman peak of 15–16 million in the first century CE and a medieval peak of 12.5 million in 1300; this would make the Roman population exceed the medieval one by some 20–30%. However, this tabulation compares apples and oranges: the medieval tally of 12.5 million for Italy *includes* Sicily and Sardinia (Pinto (1996, 43) and most clearly in the appendix of Del Punta *et al.* (1996, 277)), whereas the Roman tally of 15–16 million does not. Thus the Roman tally of 15–16 million would exceed a medieval mainland peak of around 11.6 million (i.e. 12.5 million minus 0.9 million for the islands) by 30–40%. To be sure, following Malanima’s own method, the medieval peak for the mainland would have to be put closer to 10.8 million (see above, notes 137 and 142: 11.6 million minus 0.8+ million for the islands), for a Roman excess margin of 40–50%. And if we accept a final Roman tally of anywhere from 17 to 19 million (see above, section 4), the Roman peak exceeds the medieval maximum by at least 60% and perhaps by as much as 75%. Even the lower end of this range would push total Roman population into early-nineteenth-century territory, making it very different from the medieval maximum. Lo Cascio and Malanima also note that “if the Italian population was able to attain more than 18 million inhabitants in 1800, this depended primarily on the spread of maize” (221). Once again this tally includes the islands, which suggests that the population size of the high count might have been difficult to reach without New World crops. (Malanima (2005, 127–8) stresses the importance of maize for eighteenth-century Italian population growth, especially in the north.) For a corrective to their flawed graphs see below, figs. 2–3.

against population pressure and, although they may not directly support any specific scenario, are more readily consistent with the low count: slave imports, costly recruitment in the late Republic, falling military participation rates in the early monarchy, and, conceivably, elevated living standards all belong in this category. Other factors are simply puzzling in their logical inconsistency, most notably the incidence of internal violence in the late Republic and its successful termination under the monarchy. If we accept that Roman Italy as a whole was unlikely to be less densely populated than the same region in the High Middle Ages, the low count becomes very difficult to sustain. At the same time, comparisons with modern Italy represent a serious—though not insuperable—challenge to the demographic requirements of the high count. These problems are illustrated in Figures 2 and 3.<sup>155</sup>

In view of all this, some kind of intermediate scenario might be the most superficially appealing solution, which would either require the presence of several million citizens outside Italy as early as the late first century BCE or a different interpretation of Roman census reporting practices. What we need above all is an open mind: when I said that the low count was “the worst solution, perhaps—except for all the others”,<sup>156</sup> I should perhaps have been more willing to contemplate the potential of compromise models. However, nothing in the present review alters my view that the high count remains the least persuasive option currently on offer.

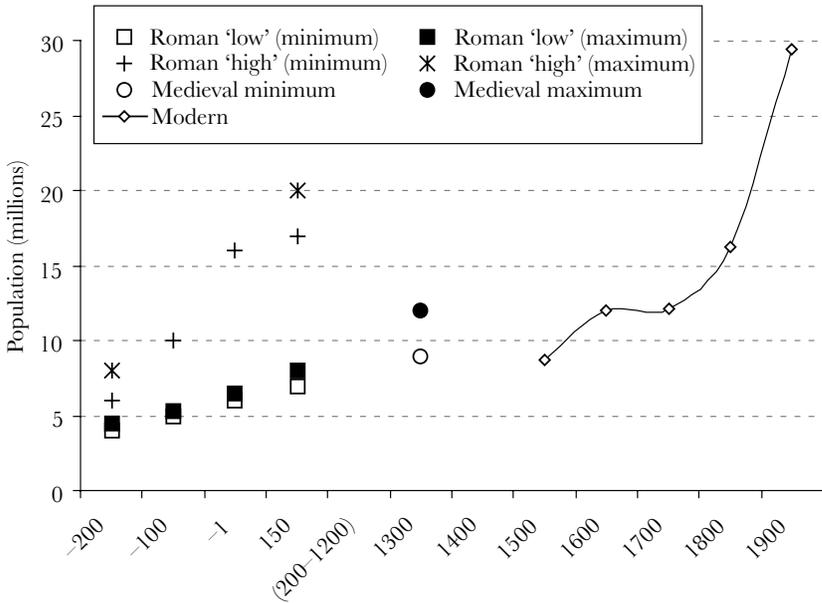
It is true that a ‘core-wide’ empire, a unique phenomenon in Mediterranean history, may have created unique conditions for economic and thus demographic expansion.<sup>157</sup> A giant peace dividend in the form of reduced protection costs, transaction costs, and information costs could very well have supported unusually high population densities. The key problem, however, is that we cannot simply presuppose what we need to document: the notion that imperial unification yielded unique demographic

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<sup>155</sup> The Roman-period estimates in figs. 2 and 3 are based on my discussion of the ‘low’ and ‘high’ counts in section 4, and on Lo Cascio and Malanima (2005, 208 tab. 2) for the high count estimates for 200 and 100 BCE. For the estimates for 1300 in fig. 2 see section 12. The estimates for 1300 in fig. 3 are derived from section 12 (lower estimate of 4.1 million) and from Del Panta *et al.* (1996, 277): 6.8 million, which is too high if we adopt Malanima’s method: see above, section 12. The estimates for the modern period in both figures are taken from Del Panta *et al.* (1996, 277).

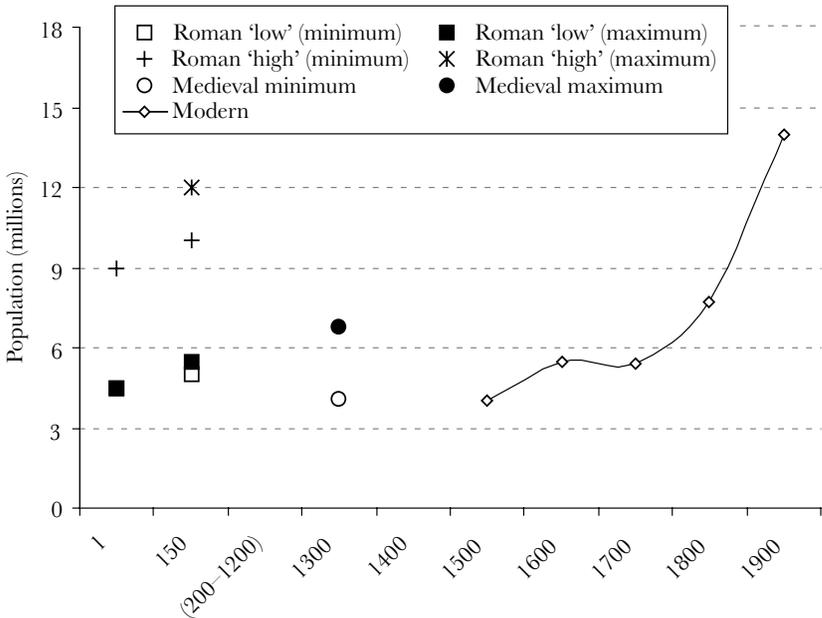
<sup>156</sup> Scheidel (2004, 9).

<sup>157</sup> Alternatively, specific configurations of institutional features may have limited this effect more narrowly to Greek and Roman citizen communities, as suggested by Kron in forthcoming work.



Source: Sections 4 and 12, and n. 154.

Fig. 2. Population estimates for mainland Italy, 200 BCE to 1900 CE.



Source: Sections 4 and 12, and n. 154.

Fig. 3. Population estimates for peninsular Italy, 1 to 1900 CE.

benefits is plausible enough as a working hypothesis but would have to be tested against actual data rather than accepted on *a priori* grounds. As things stand, we can only hypothesize, but not verify.

Uncertainty about Roman population size matters more in certain areas than in others. For example, Roman military mobilization rates were very high regardless of which demographic scenario is correct. Likewise, my own model of shifts in the scale of mobility in Roman Italy is largely insensitive to overall population density.<sup>158</sup> On the other hand, our understanding of the driving forces behind conflict in the late Republic is significantly influenced by demographic estimates. But where population matters most of all is in the sphere of economic history. This may be news to many ancient historians: in the wake of Rostovtzeff's and Finley's influential works, demographic conditions have long been thoroughly marginalized in our accounts of the Roman economy, and this situation is only beginning to change.<sup>159</sup> Roman historians would be able to make a very substantial contribution to our understanding of economic growth if they were able to demonstrate that conditions in the Roman empire supported considerable intensive economic growth and population growth at the same time, perhaps along the lines of developments in Song China.<sup>160</sup> It would be equally exciting if they could show that universal empire did not in fact create a trajectory that differed from that of the High Middle Ages or the early modern period, when population growth ate into income growth.<sup>161</sup> These would be findings that would turn Roman economic and demographic history into an object of great interest to other historians and economists. However, such findings cannot be obtained as long as we are unable to establish absolute population size. Our apparent inability to do so is particularly vexing because the stakes are so high: unbeknownst to most proponents of the two principal rival scenarios, preoccupied as they are with the finer points of Roman history, the logical corollaries of their models are of profound significance for our understanding of premodern history in general.

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<sup>158</sup> Scheidel (2004, 21). I note in passing that the average per capita emigration rate for adult males implied by my low count model of human mobility in late-republican Italy matches that for sixteenth-century Portugal: Scheidel (2007b, nn. 79–80); and the mean Dutch emigration rate from 1600 to 1800 resembles my Roman Italian emigration rate in the first century CE (*ibid.*).

<sup>159</sup> See now Scheidel (2007a).

<sup>160</sup> Elvin (1973, 113–99); Maddison (2001, 42).

<sup>161</sup> See above, n. 107.

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THE MUCH MALIGNED PEASANT.  
COMPARATIVE PERSPECTIVES ON THE PRODUCTIVITY  
OF THE SMALL FARMER IN CLASSICAL ANTIQUITY

J. Geoffrey Kron

*The ruins of the ancient civilizations of the Greeks and Romans which we see still standing, bear witness that the wealth of nations can be brought almost to the highest state without the science having been practiced which teaches how to hasten its development.* Sismondi (1820, 33)

I. Introduction

My primary concern in this paper is to defend the productivity of Rome's small farmers, who played a critical role in making the Italian countryside, as Varro so memorably claimed,<sup>1</sup> the most intensively cultivated in the ancient world, and to question the widespread assumption that these Roman owner-occupiers were necessarily unable to compete with large 'capitalist' farms.<sup>2</sup> I intend to address this narrow question in the broader context of the overall productivity of the Roman agrarian economy. In so doing, I will also address, albeit rather tangentially, Walter Scheidel's charge<sup>3</sup> that in simultaneously defending the plausibility

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<sup>1</sup> Var. *R.* 1.2.3–8. See also *Plb.* 1.15.1; *Lucr.* 5.1367–78 and the additional sources cited in Martin (1971, 261–9).

<sup>2</sup> See especially Brunt (1972), Toynbee (1965, 2.296–310), and, more cautiously, De Neeve (1984b). In speaking of productivity, I am concerned with productivity per arable hectare rather than labour productivity. The latter is notoriously difficult to measure for owner-occupier farm operations, and high levels of labour productivity can be achieved through exploitative systems of extensive farming with minimal labour inputs which are execrably poor in terms of the effect on the land and the amount of food produced.

<sup>3</sup> Scheidel in this volume. For reasons of space, I do not intend to reopen the broader question of the population of Italy or to address Scheidel's criticisms here. Although my principal focus will be to dispel common misconceptions about the productivity of peasant agriculture, I do hope that my essay will also serve to explain why I am unconvinced by Scheidel's core objection, his skepticism that a population comparable to that of nineteenth century Italy could be supported by the Roman agricultural regime. For an explanation of why I believe that the high count is not only possible, as I contend here, but also demanded by the sources and general demographic considerations, one will have to turn to Kron (2005b).

of a Roman citizen population of over 4 million adult males, as attested by the Augustan census,<sup>4</sup> and the equally clear anthropometric evidence of a high level of Roman nutrition,<sup>5</sup> I am positing a Roman ‘miracle economy’.

The Roman achievement of feeding an Italian population comparable to that of the early nineteenth century to a significantly higher standard is remarkable, but it was far from a miracle. Since, as I have noted before,<sup>6</sup> the census of 1861 enumerated more than 26 million Italians, in what was still, as far as food production is concerned, a pre-industrial society,<sup>7</sup> neither the Augustan, nor even the Claudian, census figures are inconsistent with securely attested levels of Italian agricultural productivity.<sup>8</sup> Certainly there is little reason to believe that nineteenth-century Italy was cultivated to the limit of its productive potential. For example, southern Italy and Sicily, a model of intensive mixed farming and a source of many superb large cattle under the Greeks and Romans,<sup>9</sup> had been largely abandoned to rough grazing and the most cavalier and destructive extensive farming in the nineteenth century.<sup>10</sup> Moreover, the Romans were able to supplement their

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<sup>4</sup> See Kron (2005b), defending the thesis proposed by Lo Cascio (1994).

<sup>5</sup> Kron (2005a).

<sup>6</sup> Kron (2005b, 482–84).

<sup>7</sup> See Warringer (1939, 7–10) for the limited value of mechanization or other post-industrial innovations for the highly productive intensive farming practiced by twentieth-century western European peasants. For the limited use of tractors, steam threshers, artificial fertilizers, and other industrial farming techniques before the end of the nineteenth century and often through much of the twentieth see e.g. Desplanques (1969, 336–8; 341); INEA (1964, 41); Sereni (1968, 208).

<sup>8</sup> If we assume with Brunt that 28% of the citizen population were adult males, the 5.94 million enumerated in the Claudian census of AD 48 (Tac. *Ann.* 11.25) would represent just over 21 million citizens. Unlike the figure for the Augustan census in the *Res Gestae*, however, which was preserved on stone, we are dependent on a very tenuous manuscript tradition and cannot rule out corruption of the figure. More likely, grants of citizenship under Claudius, whose policy, as reflected in the admission of Gauls to the senate, as well as the healthy growth of colonies and settlements of veterans on lands outside of Italy under both Augustus and Claudius, will account for the rapid increase of the citizen population (most of which will have occurred outside of Italy).

<sup>9</sup> For a statement of the case, focused on the question of animal husbandry, see Kron (2004a). For the evidence for intensive mixed farming, oleiculture, and viticulture in southern Italy in the ancient period see e.g. Frederiksen (1970–1); Jones (1980); Tchernia (1986, 334–7); Volpe (1990, 62–5; 71–5; 77–81; 251–71); Manarcorda (1993); Lo Cascio & Storchi Marino (2001); Buonacuore (2002, 1.62–3); Accardo (2002, 41–9); Carter (2006).

<sup>10</sup> The bibliography is limitless, but see e.g. Cassa per il Mezzogiorno (1953); Carlyle (1962, 95–100); Villani (1968); Franklin (1969, 123–74); De Felice (1971); King (1971); Prampolini (1981); Snowden (1986); Cuboni (1992); Rogari (2002). It is worth noting that the viticulture, oleiculture, and fruit trees which provide southern Italy with most

agricultural production with massive imports of wine, wheat, olive oil, and fish from Egypt, Spain, Gaul, and North Africa.<sup>11</sup>

Italian agronomists of the nineteenth century, like their predecessors throughout Europe, recognized that the Roman *scriptores rei rusticae* had an expert knowledge of a wide range of highly intensive and productive agricultural practices,<sup>12</sup> many of which, as we shall see, were still only fitfully applied in most of Italy during the nineteenth century. Recent research has begun to document the Roman achievement, and I will briefly summarize some of this evidence below. One must also recognize, however, that low effectual demand for agricultural produce, rather than low productivity or ignorance of more intensive methods, is one of the principal constraints on agricultural production. The greater prosperity and social equality which Roman peasants and urban consumers enjoyed compared to their nineteenth-century Italian counterparts, as reflected in their superior state of nutrition, is the best explanation for the high productivity of Roman agriculture. Good nutrition not only made Roman peasants healthier, stronger, and more productive, but the rich diet of the rest of the population made it economically viable for farmers to keep livestock, and to employ more productive intensive methods. As Esther Boserup showed in a classic study,<sup>13</sup> peasant farmers can significantly increase agricultural productivity, simply through more careful and labour-intensive application of traditional methods.<sup>14</sup> Yet, as we shall see, the Romans incorporated most of the critical technical advances of seventeenth-century Dutch and nineteenth-century English farming into their already intensive traditional peasant agriculture.

## II. *Roman Agronomy and Agricultural Productivity*

The agrarian economy of Roman Italy enjoyed a remarkable confluence of advantages. Italy is blessed with fertile soil, ample rainfall, a warm Mediterranean climate, and a long growing season. As have Italian

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of its present-day agricultural revenue were only introduced on any scale in the 1870s and 1880s. See e.g. Snowden (1986, 35–40).

<sup>11</sup> See e.g. Rickman (1980); Tchernia (1986); Mattingly (1988); Remesal Rodriguez (1998); Remesal Rodriguez (1999); Sahrhage (2002: 76–9); Brun (2003); Hojte (2005).

<sup>12</sup> Fussell (1972); Ambrosoli (1997); Marcone (1997, 206–17).

<sup>13</sup> Boserup (1965).

<sup>14</sup> See Ambrosoli (1997, 293–5). The English farmer Robert Loder raised his yields significantly, even without new crops or additional manure, simply by applying Roman labour-intensive methods of weeding and ploughing.

*contadini* since the Renaissance, the Romans exploited these natural advantages by using an intensive system of intercultivating cereals and legumes with such high value tree crops as wine, olive oil, fruit, and nuts,<sup>15</sup> combined with market gardening. But the Romans combined the traditional strengths of modern Mediterranean agriculture with the sort of intensive mixed farming and advanced animal husbandry practiced in England and the Low Countries, but largely neglected in Italy, in the late medieval and modern era.<sup>16</sup>

The Roman agronomists, Varro and Columella, as well as Virgil in his *Georgics*, show a clear understanding of the principles of convertible husbandry, also known as alternate husbandry or ley farming,<sup>17</sup> one of the most effective methods of simultaneously increasing pasture quality, arable yields, and stocking rates. A key innovation in the agricultural revolution,<sup>18</sup> and the dominant method of intensive mixed farming today,<sup>19</sup> ley farming, along with the suppression of the fallow through the introduction of new forage crops,<sup>20</sup> allowed significant improvements in the size and fecundity of livestock, and established England and the Low Countries as leaders in both livestock production and arable yields.<sup>21</sup> Like the best Dutch or English animals, Roman cattle

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<sup>15</sup> See further Kron (2005c). See also Desplanques (1969, 345–62); Pazzagli (1973, 257–60).

<sup>16</sup> See e.g. Ghisleni (1961); Desplanques (1969, 415); Pazzagli (1973, 267–321); Prampolini (1981); de Felice (1971); Cuboni (1992); Kron (2004a, 120–2 and table 1). Even today competition with more fully established livestock producers and the mechanization of arable farming has suppressed animal husbandry through much of Italy and made it reliant on imports of meat for its increasing consumption. See Barsanti (2002, 122–4). Only the Po valley, which began to specialize in milk and meat production in the nineteenth century, has developed large scale intensive animal husbandry. Many classical agricultural historians wrongly assume that environmental constraints are responsible, but there is no sound basis for this view.

<sup>17</sup> Kron (2000).

<sup>18</sup> Slicher Van Bath (1963, 249–54); Kerridge (1967); Abel (1980, 106–9); Mingay (1989, 47–51; 293–6); Beckett (1990, 11–19); Ambrosoli (1997, 362–8). Although Kerridge has produced convincing evidence for widespread application of convertible husbandry in England when demand for livestock boomed in the sixteenth and seventeenth centuries, these more intensive methods seem to have been scaled back significantly when demand slackened. In the Midlands ley farming contracted in favour of permanent grass in the eighteenth century, for example. See Broad (1980). Writing in the late 1780s, Adam Dickson describes ley farming as though it were a relatively recent discovery: “But it is now found, that arable land, being some years in grass, when turned up, is in a much better condition for carrying crops of corn, than when it was laid off.” Dickson (1788, 1.98).

<sup>19</sup> See Stapleton and Davies (1948).

<sup>20</sup> See MacKinnon (2004, 130–1) for recent references.

<sup>21</sup> Slicher van Bath (1963, 239–54); De Vries (1974, 119–74).

were over 20 cm taller at the withers and as much as twice as heavy as Iron Age or medieval cattle.<sup>22</sup> Such high standards of nutrition demand excellent fodder management and imply intensive mixed farming on many Roman farms.

The ubiquity of large Roman livestock in Italy and their spread throughout the provinces of Pannonia,<sup>23</sup> Gaul,<sup>24</sup> and even Germany (where the improved stock would become almost universal in Roman-occupied regions by the mid-Empire)<sup>25</sup> argues for the rapid dissemination of the best techniques. The contrast with the tardy spread of the best breeds and methods in nineteenth-century Western Europe, particularly Italy, is striking. Even at the end of the nineteenth century, Englishmen could still deride other European cows as “poor beasts of burden” and boast that “the greater weight and superiority of our stock is acknowledged by the special advocates of other countries”.<sup>26</sup> In fact, French or German cattle would not consistently match the size of the best Dutch, English (or Greco-Roman) cattle until the mid-to-late nineteenth century.<sup>27</sup> Well into the twentieth century many Italian cattle, even in relatively prosperous regions such as Tuscany,<sup>28</sup> weighed in at around 200 kg, half as much as many Roman cattle.

Roman standards for other domestic animals were equally high. The Romans raised pigs in a more humane and sustainable version of modern factory farming, with sows farrowing twice a year and producing large litters of piglets. Roman fine-wooled sheep were also significantly larger than medieval animals and comparable in size and wool quality to the modern Merino. Finally, the housing provided to Roman domestic animals was well-designed, and Roman veterinary

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<sup>22</sup> See Kron (2002); Kron (2008, 180).

<sup>23</sup> Bökönyi (1984) and (1988).

<sup>24</sup> Lepetz (1996).

<sup>25</sup> Peters (1998).

<sup>26</sup> Craigie (1887, 127).

<sup>27</sup> Moricau (1999: 47); Kautsky (1899: 38); Kron (2002, 63). Roman cattle, which generally reached withers heights of 135 cm, would therefore weigh approximately 400kg; Kron (2002). In 1806, German cattle weighed on average 204 kg, reaching Greco-Roman or modern Dutch or English live weights only towards the end of the nineteenth century. French oxen and steers averaged only 225 kg in 1862, rising to 262 kg, still smaller than Roman cattle, by 1892. See Kautsky (1899, 38). Even in England at the turn of the twentieth century the average weight for all English cattle was still only 300 kg; see Collins (2000, 310 table 3.3).

<sup>28</sup> Pazzagli (1973: 305–7).

manuals give sound instructions for performing most of the surgical procedures current in the late nineteenth century.<sup>29</sup>

The Romans were able to increase manuring rates markedly through convertible husbandry and the substitution of fodder crops for fallow, thus allowing the integration of more livestock into arable farming. The Roman agronomists recommended applying more than twice as much manure as was customary in modern Italian farming, rates rarely applied outside of the leading nineteenth-century mixed farming regimes, England, Belgium, and the Netherlands.<sup>30</sup> All sources of compost or manure available on the farm were to be exploited and carefully managed to achieve optimum results, as Adam Dickson noted, contrasting it with the wasteful approach taken by most late eighteenth-century English farmers.<sup>31</sup> Modern southern Italian peasants took even less care in the management of their exceedingly scanty manure resources.<sup>32</sup> Roman estimates of eight-, ten-, or fifteen-fold yields for wheat, while higher than those typically achieved through most of medieval and modern Italy,<sup>33</sup> are credible with manuring rates this high<sup>34</sup> and match the yields of the most advanced English and northern European nineteenth-century mixed farms.<sup>35</sup> The best Roman wine yields matched or exceeded

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<sup>29</sup> See Kron (2008, 183–5). Roman chickens were raised and fattened in large numbers in highly productive battery farms and were as large as many modern breeds, generally weighing from 1.5 to 2 kg compared to the 1 kg or so common with earlier breeds.

<sup>30</sup> Plin. *Nat.* 17.50; Col. 2.15.1; Palladius 10.3.2. See Spurr (1986, 128–131); Kron (2002, 55 n. 9); Kron (2005c, 293–5); and compare Slicher van Bath (1963, 256–60); De Vries (1974, 149–53).

<sup>31</sup> For the high quality of Roman manure management see Dickson (1788, 1.253; 271; 289–90; 299–301) and Kron (2005c, 293–5).

<sup>32</sup> See Carlyle (1962, 105): “In 1876 Sonnino commented on the devastating failure of the peasants to use manure. There were hardly any stables and the cattle shared the owner’s dwelling, so heaps of manure were piled up outside the towns. These were actually allowed to go to waste and were sometimes even burnt in summer. Sonnino added that it was a blessing the ploughs were so primitive they only scratched the top crust of the soil. In 1958 piles of manure could still be seen outside the hill villages, waiting to be carted away and sold in Palermo for the market gardens, while the local peasants bought and used artificial manures, if they used any at all.”

<sup>33</sup> See Spurr (1986, 82–8). Not surprisingly considering the extensive methods used, relatively poor yields persisted in southern Italy into the modern period. See Craigie (1887, 128); Carlyle (1962, 95–100).

<sup>34</sup> For extensive experimental trials demonstrating the value of manuring in raising cereal yields, significantly superior to artificial nitrogen fertilizers in their effects, see e.g. Hall (1917) and Clark (1992, 72–5). The fertilizing effects of manuring can persist for more than 75–100 years in the soil, unlike expensive artificial fertilizers, which tend to run off or leach quickly out of the soil and into the water table.

<sup>35</sup> See Slicher Van Bath (1963, 280–82); De Vries (1974, 151–2); Collins (2000, 309–10); Turner et al. (2001, 146–7).

those achieved by twentieth-century French vineyards, as Tchernia notes.<sup>36</sup> Duncan-Jones has cast doubt on the plausibility of such high yields,<sup>37</sup> but given the Roman achievement in animal husbandry and forage management, there is no compelling reason the Romans could not have matched modern wine yields with their highly skilled labour-intensive approach.

Roman forage and pasture management techniques were exceptionally sophisticated. The botanists and agronomists of medieval and modern Europe struggled for almost three centuries to match the Romans' expert exploitation of lucerne or alfalfa, as Mauro Ambrosoli has documented at length in a classic work.<sup>38</sup> Over the course of the Middle Ages the cultivation of alfalfa had entirely disappeared in Italy<sup>39</sup> and northern Europe. Only the survival of ancient techniques in Provence and Arab al-Andalus facilitated its revival, both as a source of seed and of the first-hand practical knowledge required to understand the ancient botanists and agronomists.<sup>40</sup> The Romans exploited most of the best forage crops currently used by modern farmers to improve their meadows and pastures. These include a number of valuable Mediterranean drought-resistant legumes first introduced in the twentieth century by Australian farmers. Notable Roman forage crops include subterranean clover, now widely acknowledged by Australian and Californian livestock farmers as one of the best forage legumes for seeding in grasslands, and shrub trefoil, closely related to alfalfa and matching it in nutritional value, but ideally suited to the pasturing of sheep and goats on thin calcareous soils in semi-arid conditions.<sup>41</sup>

Roman mixed farms were therefore able to match the high grain yields, good fodder production, and large healthy livestock of nineteenth-century England. Yet in other respects the Romans went well beyond the English in the intensification of their agricultural regime. In 1890, despite the incentives of a large urban population and a persistent depression in grain prices, the English, having stripped most of their peasantry of any independent role in agriculture, still devoted only

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<sup>36</sup> See Tchernia (1986, 359–60).

<sup>37</sup> See Duncan-Jones (1974, 39–46); De Neeve (1984a, 163 n. 215). Extremely high yields could be achieved more often, but they are restricted by French law in the interests of maintaining quality—and presumably controlling supply.

<sup>38</sup> See Ambrosoli (1997).

<sup>39</sup> See Ambrosoli (1997, 104–7).

<sup>40</sup> See Ambrosoli (1997, 176–80).

<sup>41</sup> See Kron (2004b).

2% of their arable land to fruit, vegetables, eggs, poultry, or industrial crops, most of which were imported from the peasant farms of Continental Europe.<sup>42</sup> Like the Dutch, however, the Romans carried out much of their mixed farming on small peasant farms and integrated it with heavy production of valuable labour-intensive crops, in the Romans' case wine, olive oil, industrial crops, and a huge range of fruits, nuts, and vegetables. In the words of Adam Dickson: "In Britain, we have not so many different kinds of crops as the Romans had. We have different types of corn, pulse, grass, and roots; we have likewise some others, but they are so trifling, that they do not deserve to be mentioned as the produce of the country. The Romans had a greater variety of these different kinds."<sup>43</sup> Roman wine and olive oil production, as has been well documented, was on a massive scale.<sup>44</sup> The agronomists and modern archaeobotanical studies show that the Romans cultivated a remarkable diversity of produce, including most of the principal fruits and vegetables consumed in Europe prior to the discovery of the Americas, many known in dozens of different varieties.<sup>45</sup>

Recent studies have demonstrated that at the turn of the twentieth century the value of the agricultural produce of England, per arable hectare, was only about 70% as great as that of Italy.<sup>46</sup> This was the case, notwithstanding the fact that Italian grain yields were still gener-

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<sup>42</sup> See Collins (2000, 206). Even English cheese producers were ill-equipped to compete, as 58 million pounds per year was imported from Canada, despite the fact that the wages paid there were at least 20% higher. See also Sismondi (1820, 188): "No more orchards, no more fruit trees brighten the countryside; it is not the climate that keeps them out—it is equal to that part of France, and better than that of Germany; but the diligent care of fruit trees is beyond the attention of a farmer of five hundred acres; similarly he does not make an effort to raise poultry—boats loaded with eggs come from Normandy to supply English markets. He has great herds of cows, and his milk sheds are managed with an elegance and cleanliness that makes us envious, but he sells no butter, cream, or milk products. Finally, he scorns even more the art of gardening, such that one finds vegetables in abundance only in the vicinity of large cities, or in the kitchen gardens of lords. The rich farmer concerns himself only with the wheat and cattle markets; all the small aspects of agriculture which bring little money, but much happiness to the poor households of the Continent appear to him as beneath his dignity."

<sup>43</sup> Dickson (1788, 1.175). See Collins (2000, 206).

<sup>44</sup> Tchernia (1986); Mattingly (1988); Brun (2003).

<sup>45</sup> See e.g. Col. 5.7.3–6; 5.10.11; 15; 17–20; Plin. *Nat.* 15.55; 69–70; André (1961); Carandini (1988); Carandini (1990); Henderson (2004); Jacomet et al. (2002); Bakels and Jacomet (2003); Thurmond (2006, 173–4).

<sup>46</sup> O'Brien & Toniolo (1991).

ally low (since very few livestock were raised or sold for meat, thereby depriving the arable of manure), and that new forage crops, artificial leys, and convertible husbandry were rarely exploited outside of the cattle and milk producing centre of the Po valley.<sup>47</sup> One can well imagine the potential agricultural productivity of Roman Italy, combining much of the best of nineteenth-century Dutch and Italian farming systems with strong effectual demand from a large and prosperous urban population and the full integration of the Mezzogiorno into a system of mixed farming.

### III. *Nutrition, meat consumption, demand, and agricultural productivity*

Even the most skillful farmer cannot exploit his land to the full without some assurance that he will have a market for his surplus produce. One must often seek the key to agricultural productivity outside the farm, in the standard of living of the broader society. A population wealthy enough to eat meat provides a critical stimulus to the intensification of an agrarian economy.<sup>48</sup> Farmers generally cannot afford to invest in livestock solely in order to provide manure to maximize the productivity of their arable land. They also need, as Lavergne explained,<sup>49</sup> a market for meat (or wool) strong enough to permit them profitably to carry out more intensive mixed farming: “Why does the English farmer, for example, give a preference for the production of meat? It is not only because the animals maintain by means of their manure the fertility of the land, but also because meat is an article very much in demand and

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<sup>47</sup> See e.g. Ghisleni (1961, 16–20; 127–43); Desplanques (1969, 422; 426); Pazzagli (1973, 45; 53–8); Prampolini (1981, 42; 48); Rinaldi (1995, 114–5; 244–5); Ambrosoli (1997, 123–62). After a brief revival during the Middle Ages—see e.g. Cortonesi (1988, 287)—convertible husbandry and artificial pastures do not become common until the mid twentieth century in many regions. See e.g. Desplanques (1969, 422). The low productivity of contemporary Italian grazing land given by Tibiletti (1949, 11) is arguably not a fair comparison to the Roman stocking rates he seeks to discredit, as the latter may presuppose mixed farming rather than simple grazing; but they are suggestive nonetheless.

<sup>48</sup> See further Kron (2004a).

<sup>49</sup> De Lavergne (1855, 162). The Romans recognized this very clearly. See e.g. Col. 8.1.2. As Slicher van Bath (1963, 282) puts it: “From the sixteenth century onwards the literature shows a dispute between those who advocated keeping a good stock of cattle on the farms, and their opponents, who recommended a minimal stock or even none at all. The former pointed out that cattle were necessary for the production of manure; the latter adduced the unfavourable prices and small demand for livestock and its products in the market.”

which sells with the greatest facility throughout England. If our French producers could all at once furnish as much meat, the price would fall below the expense of production because the demand is not great enough. Our population is not rich enough to pay for meat.” Lavergne’s analysis is surely correct.<sup>50</sup> Because of England’s precocious role as a source of wool and cheap woolen cloth,<sup>51</sup> and the massive growth of its capital, London, as a burgeoning market for meat,<sup>52</sup> England supported enough livestock to allow exceptionally high manuring rates.<sup>53</sup> England’s livestock farming permitted her to achieve yields of wheat which were consistently higher than on most of the Continent, with the notable exception of the Low Countries.<sup>54</sup>

The relatively high level of nutrition and biological standard of living of the Roman population played a critical role in facilitating the high productivity of Roman farming. The evidence of good Roman nutrition and health also casts considerable doubt on the widespread view that Roman society was necessarily characterized,<sup>55</sup> supposedly like all pre-industrial societies, by the same profound social inequality, malnutrition, poverty, overcrowding, and consequent ill health which were so well documented for nineteenth-century England.<sup>56</sup> Instead, it suggests that even relatively poor Romans received an adequate caloric intake, faced only moderate health stresses from poor sanitation, overcrowding and disease, and enjoyed a healthy level of meat and/or fish consumption. The mean height of Roman males of approximately 168.1 cm, while decidedly lower than the 172 cm reached in the more democratic and egalitarian society of Hellenistic Greece, is nonetheless comparable to that of many Western European nations in the mid twentieth century

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<sup>50</sup> See e.g. Abel (1978, 254–9) for confirmation of the importance of this phenomenon.

<sup>51</sup> Although possessing fewer cattle per square mile than Holland, Germany, Belgium, and several other European countries, English sheep vastly outnumbered those on all the other Continental nations: Craigie (1887, 127).

<sup>52</sup> See Sombart (1913, 139); Caird (1852, 483–4); Mingay (1989, 191–4).

<sup>53</sup> Collins (2000, 537–9). Beckett (1990, 30) explains the dynamic in the eighteenth century: “English farmers compensated for falling grain prices by switching their attention to animal products, but this brought the rather perverse result of increasing grain output. The introduction of fodder crops enabled farmers to keep more animals. More animals meant more manure, and more manure meant more fertile soil.”

<sup>54</sup> Craigie (1887, 128); Collins (2000, 309–10).

<sup>55</sup> See e.g. Yavetz (1958); Brunt (1966); Toynbee (1965); Brunt (1971); Garnsey (1991); Garnsey (1999).

<sup>56</sup> Booth (1902–3); Rowntree (1910); Gilboy (1934); Fussell (1949); Burnett (1979). This extremely low standard of living characterized much of Continental Europe as well. See Abel (1978, 242–60).

and is decidedly superior to that of the populations of the European *ancien régime*.<sup>57</sup> In 1851 the mean height for Italian males as revealed by conscription statistics was only 162.6 cm, and for southern Italians the situation was surely a great deal worse, as the minimum height requirement for recruits to Napoleon's army in the South was a mere 149.8 cm. The Roman standard of nutrition would not be achieved in the Netherlands until 1921, nor in Italy until 1956. Yet, American heights demonstrate that high levels of nutrition can be achieved in a fairly egalitarian pre-industrial society.<sup>58</sup> The Americans reached the Hellenistic Greek standard as early as 1715, a level the Dutch would not achieve until 1950, the Italians not until 1977.

Reduced protein consumption is a principal reason, along with simple under-nutrition, for reduced heights and health.<sup>59</sup> Since protein comes primarily from relatively expensive foods such as meat and fish,<sup>60</sup> it is one of the first elements of the diet to be sacrificed. As we can see from table one, nineteenth- and early-twentieth-century Italians could afford to eat very little meat or fish. Farm labourers, who represented the vast majority of the population in southern Italy,<sup>61</sup> lived on pasta, bread, and beans, and could expect to eat meat only for Christmas and Easter, generally pork sausage,<sup>62</sup> and the diet of *contadini* throughout nineteenth-century Italy was extremely poor.<sup>63</sup>

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<sup>57</sup> Kron (2005b).

<sup>58</sup> See further Kron (2005b, 75–6). We can attribute greater North American heights in the more inegalitarian eighteenth and nineteenth centuries in large part to the continued existence of large tracts of unworked arable land in the Midwest and West, of course. They are, therefore, arguably not exactly commensurate with European heights.

<sup>59</sup> Floud et al. (1990, 259–61).

<sup>60</sup> Although legumes—the poor man's meat—can be and often are substituted. See Spurr (1986, 111).

<sup>61</sup> Snowden (1986, 21) notes that at Cerignola in 1901 a full 7,947 of the total male agricultural population of 9,746 were wage labourers.

<sup>62</sup> Snowden (1986, 30); Ciuffoletti & Nanni (2002, 475). Snowden (1986, 44) quotes the testimony of a typical labourer from Cerignola in Apulia: "Oh, what a life, what an ugly life we led... in those days... husband, wife and all the children slept together. And they shared their beds with lice, fleas, and bed bugs. You could barely live—three, four families all squeezed together. When you got home you ate an anchovy. We bought those, and put them on our bread. Then we sat by the door and ate, and that was what we had. What did we know of soup in those days? We didn't know about anything. Four, five children, and the mother and the father all ate from the same plate, and sat on the floor because there weren't any chairs."

<sup>63</sup> Ciuffoletti & Nanni (2002, 473–5). A large proportion of the population suffered from pellagra, the result of the substitution of maize for wheat in the diet, for example.

Table 1. Annual per capita consumption of meat and fish in Italy (kg)<sup>64</sup>

Date	Beef (kg)	Pork (kg)	All Meat (kg)	Fish (kg)
1861–70	4.9	4.6	14.8	2.9
1901–10	6.0	4.7	15.5	3.9
1955	9.2	5.0	20.5	7.4
1985	25.1	23.7	77.0	12.8

Even among the English, who took great pride in their herds of superb large livestock, the poverty of the English labourer reduced overall consumption to a relatively low level of just under 38 kg per year<sup>65</sup>—little changed from Gregory King’s seventeenth-century estimate of 33 kg per year,<sup>66</sup> and scarcely greater than French consumption in 1892 of 33 kg per year.<sup>67</sup> The wealthy consumed a disproportionate amount of this meat, of course. A study of meat consumption in Bradford-on-Avon shows that two thirds was consumed by the richest 29% of the population.<sup>68</sup> Social reformers complained that the typical English rural labourer’s family could rarely expect to eat more than a pound of bacon a week,<sup>69</sup> and many families, even in full employment, could barely afford enough bread to stave off malnutrition.<sup>70</sup> Other pre-industrial populations could and did eat more meat, however. Medieval Germans ate approximately 100 kg per capita, more than twice as much as the English, as much as was consumed by nineteenth-century or contemporary Americans.<sup>71</sup> By the eighteenth century, however, German consumption had declined to a mere 14 kg per year, part of a pan-European decline

<sup>64</sup> Source: ISTAT (1985, table 105). ‘All meat’ includes both beef and pork.

<sup>65</sup> Craigie (1887, 127), eager to maximize English meat consumption, calculates a level of 83 lbs or 37.7 kg per capita. All these per capita averages are, of course, somewhat deceptive as they do not take into account the limited access to meat among the poor or the rural population, and the lavish consumption among the elite. See Burnett (1979).

<sup>66</sup> Livi Bacci (1987, 127).

<sup>67</sup> Kautsky (1899, 36).

<sup>68</sup> See Abel (1978, 250). Even the average level of consumption was hardly lavish, barely 400 g per person per week.

<sup>69</sup> Davies (1795).

<sup>70</sup> See e.g. Eden (1797); Hasbach (1908, 138–45); Rowntree (1910); Riches (1934); Fussell (1949); Burnett (1979); Mingay (1989, 953–71).

<sup>71</sup> For references see Kron (2004a, 123). Contemporary meat consumption is 64.6 kg per capita in Europe and 107.9 kg in the USA.

in living standards in the post-medieval period.<sup>72</sup> Simple per capita averages are deceptive, however. Levels of consumption can vary radically with social class, as we have seen,<sup>73</sup> and urban populations tend to eat much more meat, as Kautsky observed, noting that consumers in Paris ate 79.3 kg of meat per person in the late nineteenth century, residents of other French towns 58.9 kg and peasants only 21.9 kg per year.<sup>74</sup> The high level of urbanization in Roman Italy therefore provided an additional stimulus to meat consumption.

The higher real wages and superior diets of Roman legionaries, labourers, and even slaves were immediately apparent to well-educated eighteenth- and nineteenth-century writers,<sup>75</sup> who used such comparisons to dramatize the plight of their own working classes. The nineteenth-century Italian writer Cagnazzi points out that base rations offered by Cato to his slaves were significantly superior to the diet of most southern Italian labourers in the 1840s.<sup>76</sup> Of course, Cato was notorious for his selfish and parsimonious attitude to his slaves<sup>77</sup> (although we ought to recall the range of elaborate recipes set down by Cato for the *uilica* to prepare, which will presumably have made the slaves' diet a bit richer and less monotonous).<sup>78</sup> Roman legionaries in Egypt were issued 3 lbs of bread, 2 lbs of meat, 2 pints of wine, and 1/8 of a pint of olive oil each day.<sup>79</sup> Compare these rations with the

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<sup>72</sup> See Abel (1978, 253 fig. 60) for a dramatic illustration of the downward trend commencing in the mid-seventeenth century.

<sup>73</sup> Peasant farmers who do raise livestock will often see them as hard won capital or a source of income rather than as an article of consumption. Nonetheless, there are some intriguing hints that even poor Roman peasants were accustomed to eat meat, as when the anonymous author of the *Moretum* congratulates his poor peasant for frugally refusing to buy butcher's meat with the income from his vegetable garden.

<sup>74</sup> Kautsky (1899, 36).

<sup>75</sup> Dickson (1788, 1.106–35) offers an especially detailed and persuasive discussion. See also Jongman (2007).

<sup>76</sup> Romani (1968, 471–4). Compare the nutritional analysis of Cato's prescribed diet in Carandini (1983). For the shockingly low real wages, poor nutrition, and wretched housing of southern Italian labourers see also Snowden (1986, 28–30; 42–3; 59). See also Prampolini (1981, 276–91).

<sup>77</sup> See Astin (1978, 240 n. 1; 261 ff).

<sup>78</sup> Cato *Agr.* 74–82, 84–7.

<sup>79</sup> Davies (1971, 122). According to my rough calculation, this ration represents 6,076 calories.

pay of Apulian labourers in 1906 expected to live on 850 grams of bread a day and a small jug of olive oil each month.<sup>80</sup>

Meat played a much more significant role in the Roman diet. The most decisive evidence comes from anthropometry and the heights achieved by ordinary Roman citizens, but a great deal of literary and archaeological evidence corroborates and fills out this picture. The impressively large market for meat at Rome, and throughout Italy, is clear from the many literary sources which attest to large-scale imports from Sicily, Spain, Gaul, and Sardinia to supplement the already thriving livestock farming of Italy itself.<sup>81</sup> As early as the third century BC the demand for meat in Italy was such that livestock farming outperformed even wine and olive oil as the most lucrative branch of farming, as Cato never tired of emphasizing.<sup>82</sup> While wheat was still an important staple crop, prices were stable and low,<sup>83</sup> and the relative indifference of the Roman agronomists to cereal farming and their strong preference for mixed farming and livestock production<sup>84</sup> further illustrates the relative

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<sup>80</sup> Snowden (1986, 25). By the brutally exploitative standards of the Italian Mezzogiorno, 330 grams of bread a day represented a minimum standard for subsistence in 1907. See Snowden (1986, 28–9).

<sup>81</sup> See Yeo (1948, 284–7) for an excellent summary of the evidence. However, he entirely ignores the possibility that the Romans raised many livestock on mixed farms rather than on poorly managed ranches. See Kron (2004a). As Polybius 2.15 emphasized, Cisalpine Gaul was already exporting massive quantities of pork throughout Italy in the third century BC.

<sup>82</sup> See Col. *pr.* 6; Cic. *Off.* 2.25; Plin. *Nat.* 18.5. Cato's emphasis on livestock is clear from the prominent place given to stabling livestock, and to fodder and meadows (Cato *Agr.* 4.1; 8.1; 9.1; 27.1; 30; 53–4) as well as the way his division of manure privileges fodder—one half for forage crops, a quarter for trenching olives and trees, with the last quarter for meadows (Cato *Agr.* 29). For the great scale of Roman livestock farming and broad public recognition of its profitability and importance see, e.g. Cic. *Att.* 4.19.1; *Quinct.* 3.12; *Clu.* 161 f.; *Planc.* 8–9; *3 Ver.* 50.119; *Var. R.* 2.2.1; 2.10.11; *Mart.* 4.37; Plin. *Nat.* 33.135; Tac. *Ann.* 4.27; 12.65.1).

<sup>83</sup> The policy of the Greek and Roman state had always been to keep the prices of staple foods cheap and stable. In this they anticipated the approach of the Dutch, as lauded by Sir James Steuart: “keeping food cheap, and still more the preserving it at all times at an equal standard, is the fountain of the wealth of Holland”. Ormrod (1985, 90).

<sup>84</sup> See Yeo (1946, 226–30) for a detailed discussion. This phenomenon is hardly likely to be attributable to the costs of transportation within Italy, however, as Yeo claims, as is made entirely clear by the history of Apulian farming since the Renaissance, for example, or the history of the English and Polish grain trades. The response to the depression in English grain prices in the late nineteenth century, demanding a transition to the sort of high farming advocated by Caird (1852), offers a more attractive parallel.

importance which more expensive foods such as meat, vegetables, wine, and olive oil had come to assume in the Roman diet.

In marked contrast to the English or Italian working classes of the *ancien régime*, even relatively poor Romans could afford to eat meat and fish, even fowl, shellfish, and game. A more careful examination of the literary sources has cast doubt on the long-standing assumption that the Roman diet included very little meat.<sup>85</sup> More importantly, however, archaeological studies and documents from military camps reveal that meat, fish, and shellfish were eaten by common legionaries, quarry workers, and large segments of the working population of predominantly working class towns such as Ostia and Portus, and of recently romanized towns like Augusta Raurica.<sup>86</sup> Moreover, nitrogen isotope studies show little variation according to social class in the level of protein in the diets of Roman populations.<sup>87</sup> While meat consumption was not restricted to the wealthy, social distinctions are discernable in the types of meat eaten. Pork, fish, fowl, and game were clearly more expensive and prestigious, while the poor ate cheaper meats (beef and mutton).<sup>88</sup> The market for ordinary domestic animals was sufficiently mature that wealthy and ambitious Roman farmers, generally of senatorial or equestrian status, branched out into *pastio ullaica*,<sup>89</sup> the farming of new and more exotic game meats (boar, deer, hare, elk, even gazelle) and wild fowl (ducks, crane, pigeon, partridge, plover, thrush, quail, peacock, swan, bustard, grouse, ostrich, flamingo, and many more).<sup>90</sup> The Romans farmed fish, shellfish, game, and fowl with a skill that would not be matched until the late twentieth century.<sup>91</sup> While this branch of Roman intensive mixed farming had truly taken off in the first century BC, Cato had already begun advising farmers to fatten hens, geese, and squabs for sale.<sup>92</sup>

Yet even these exotic and expensive foods were not restricted to the wealthy. Oysters and other popular shellfish were cultured and

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<sup>85</sup> See e.g. André (1961); Corbier (1989); Meggitt (1994).

<sup>86</sup> See Davies (1971); Schibler & Furger (1988); Van der Veen (1998); Prowse et al. (2004); Richards et al. (1998).

<sup>87</sup> See Prowse et al. (2004); Richards et al. (1998).

<sup>88</sup> See Schibler & Furger (1988).

<sup>89</sup> See Rinkewitz (1984); Bortuzzo (1990); Kron (2008, 185–204).

<sup>90</sup> See Kron (2008, tables 3 & 4).

<sup>91</sup> See Rinkewitz (1984); Kron (2008, 187–8; 206–13).

<sup>92</sup> Cato *Agr.* 89–90.

transported fresh hundreds of miles from the sea,<sup>93</sup> and not simply to villas or large cities, but to small rural villages.<sup>94</sup> Game and fish farming, of a sort, which would not be revived until the late twentieth century, brought many of these prestigious meats within the means of many ordinary citizens. For example, red and roe deer sold for the same price as pork, and the Romans ate game meat in quantities rarely seen outside of contemporary France and Italy,<sup>95</sup> in legionary camps, and in relatively humble settlements.<sup>96</sup>

Higher Roman living standards not only stimulated agricultural productivity through greater consumption of meat, they also helped to drive up demand for wine and olive oil. As André Tchernia points out, late-republican Rome saw the development of a trade in cheap wines for mass consumption on a scale which would not be seen again until the end of the nineteenth century.<sup>97</sup> Roman olive oil production for a mass market was equally impressive, as is attested by archaeological evidence from Italy, Spain, and North Africa.<sup>98</sup> Hesnard has calculated a conservative estimate of per capita olive oil consumption at Rome (not all of which would have been for alimentary use) at 13–20 litres or 11–17 kg.<sup>99</sup> Modern olive oil consumption, which reached 8.8 kg per person in the 1870s, declined to barely 6 kg, and occasionally less, from the 1880s through World War II. By the 1960s consumption had reached nearly 10 kg, and contemporary consumption in Italy is around 12 kg.<sup>100</sup> Roman olive oil production is arguably more reminiscent of contemporary Italy (where a full 7% of the arable land is covered with olive plantations and over 35% of all farm enterprises produce at least some olives or olive oil),<sup>101</sup> rather than the much more circumscribed role prevalent before the end of the nineteenth century.<sup>102</sup>

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<sup>93</sup> Thüry (1990). For the methods of cultivating and preserving fresh oysters see Kron (2008, 212–3).

<sup>94</sup> Brien-Poitevin (1996).

<sup>95</sup> Kron (2008, 187–8).

<sup>96</sup> Lepetz (1996, 225–26, 228); Peters (1998: 241, 246–48); MacKinnon (2004, 228–9); Kron (2008, 188).

<sup>97</sup> Tchernia (1986, 58–60; 172–9).

<sup>98</sup> See Brun (2003) for references.

<sup>99</sup> See Brun (2003).

<sup>100</sup> See ISTAT (1985) table 105 and FAO (2005) 14.

<sup>101</sup> Ciuffoletti & Nanni (2002, 477).

<sup>102</sup> Although 80% of Italian olive oil is now grown in southern Italy: FAO (2005) 14; these huge plantations of olive trees, along with many of southern Italy's vineyards

IV. *The continued vitality of the Roman peasantry*

Although I cannot discuss my reasoning in detail here, I share the skepticism of a large number of scholars<sup>103</sup> towards the Toynbee-Brunt theory of the domination of Italian farming by *latifundia* and the deracination of the peasantry.<sup>104</sup> I am still convinced, for the reasons I have already stated,<sup>105</sup> that the extremely low population of Italy on which they base their hypothesis is internally inconsistent and cannot be reconciled with the subsequent demographic history of Italy. Moreover, a large and constantly growing corpus of archaeological studies shows no evidence for the supposed disappearance of small or medium sized farms from the countryside of Roman Italy.<sup>106</sup> Nor do our sources, notwithstanding their clichéd and hyperbolic rhetoric about estates the size of nations,<sup>107</sup> ever explicitly claim that peasants had ceased to work, as they presumably always had, most of the agricultural land in Italy.<sup>108</sup>

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and fruit trees, date only to the end of the nineteenth century. See e.g. Snowden (1986, 35–40) and Prampolini (1981, 119–25). In the Roman period, as we have noted, *oliveta*, like vineyards, were ubiquitous in southern Italy.

<sup>103</sup> Most notably Kuziscin (1957); White (1967); Frederiksen (1970–1); Evans (1980); Rich (1983); Rosenstein (2004). See Rosenstein (2004, 3–25) for a brief orientation, with references, to recent scholarship. Also note the arguments of Roselaar, this volume.

<sup>104</sup> Toynbee (1965, 2.87–105, 155–89); Brunt (1971, 345–75). It is fair to note that Brunt is rather more circumspect in his portrait, conceding, for example, that “it is absurd to pretend that no use, or little, was made of tenants and free labourers on the property of great landlords, or that the yeomanry of Italy had been virtually eliminated by the time of the Gracchi”—Brunt (1971, 353). See also Brunt (1971, 344). On the other hand, he makes this admission only for Etruria, in response to the results of the Tiber valley survey, and most of his use of the evidence is extremely strained.

<sup>105</sup> Kron (2005a).

<sup>106</sup> See e.g. Day (1932); Frank (*ESAR* 5.168–75); Kuziscin (1957); Frederiksen (1970–1); Evans (1980); Evans (1981); Jones (1980); the references listed by Keppie (1983, 125 n. 120); De Neeve (1984a, 159–73); Volpe (1990, 101–207); Lewit (1991); Bradford (1993); Accardo (2000); Lo Cascio & Storch Marino (2001); Buonopane (2003, 117–20).

<sup>107</sup> See Col.1.3.12; Plin. *Nat.* 18.7.35–7; Sen. *Ep.* 90.39. For the rather hackneyed and rhetorical nature of some of these traditional moralizing statements see Martin (1971, 10–12).

<sup>108</sup> For the continued role of small farmers, better attested in our sources when they are leasing land from wealthy writers such as Pliny than when they are cultivating their own land, see e.g. Frank (*ESAR* 5.168–75); Sherwin-White (1966) 254–9, 518–22; Garnsey (1980); De Neeve (1984a); Scheidel (1994); Capogrossi Colognesi (1996); Kehoe (1997, 139 ff.). Like de Ligt (2007), I do not endorse De Neeve’s claim that tenancy was not a significant phenomenon in the early republic, however.

What I wish to concentrate on here is the implausibility of the presumption—stated most clearly by Toynbee, but widely shared—that peasants or small owner-occupiers are unable to compete with large ‘capitalist’ farms.<sup>109</sup> Certainly, our sources never attribute the hardships of small farmers to economic inefficiency on their part, but rather to the use of intimidation or force by wealthy landowners,<sup>110</sup> yet most historians of the ancient world simply take it for granted. I do agree with Toynbee on one important point. He is surely right that Italian agriculture was booming in the second century BC, with a flood of money from Rome’s conquests, accelerating urban growth, and the spread of Hellenistic standards of personal comfort leading to strong demand for agricultural produce, including meat, wine, and olive oil. But Roman peasants would have benefited a great deal more from this increased demand than wealthy farmers, since any increase in the market for livestock would erode the advantage normally enjoyed by wealthy farmers in a depressed agricultural economy, by permitting peasants to keep more animals.

More importantly, Toynbee’s claim that the Roman ‘establishment’ took advantage of this boom to convert ‘inefficient’ mixed farms into highly lucrative ranches for large scale transhumant pastoralism is surprising. It betrays a striking misconception about the relative productivity of these two modes of farming, one which our Roman sources certainly did not share. Extensive ranching cannot match mixed farming in overall agricultural production per arable hectare, even for the production of livestock, and is most lucrative in regions where environmental constraints, severe poverty, depopulation, or feeble demand for meat rule out more intensive methods and demand that one minimize the cost of production.<sup>111</sup> There is certainly no doubt among the Roman authorities that intensive mixed farming was superior, morally or socially,

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<sup>109</sup> Toynbee (1965, 2.155–61, 286–312). See also, with rather more circumspection, De Neeve (1984b).

<sup>110</sup> See e.g. Sal. *Jug.* 41.8; Sen. *Ep.* 90.39; Juv. 14.140–155; App. *BC* 1.7. Nor are the victims in this *topos* always described as poor. The passage in Juvenal refers to an estate coveted because it is larger and better cultivated.

<sup>111</sup> See Kron (2004a) for a brief discussion. The literature one could cite is vast, but see Cassa per il Mezzogiorno (1953) or, more briefly, King (1971) for the economic as well as social benefits of replacing extensive wheat farming and rough grazing with mixed farming on small family farms. See Franklin (1969, 129–35) for the high value of intensively cultivated land in the South.

but also in terms of productivity, to rough grazing or the farming of *latifundia*,<sup>112</sup> a term which was applied, very stringently and almost always pejoratively, to refer to any farm so large as to require extensive methods.<sup>113</sup> The Romans certainly exploited *saltus*, mountain pastures and wastes less suited for arable agriculture, using extensive ranching and short and long transhumance,<sup>114</sup> as the Samnite, Bruttian, and Daunian tribes presumably had for centuries, and did so very well.<sup>115</sup> There is no evidence, however, that they were so irrational as to convert mixed farms into rough grazing worth a fraction of their value. On the contrary, the disconnect between the agronomists' bitter denunciations of properties the size of nations and the relatively small size of most Roman farms, as revealed by the epigraphical, literary, and archeological evidence,<sup>116</sup> shows that they were determined to take the opposite approach. Roman farmers were endlessly optimistic that even North African semi-desert pastures and Bruttian uplands could and should be rehabilitated and brought into cultivation, and not without reason, as Aemilius Scaurus' massive drainage project in the Po valley<sup>117</sup> or the vast expansion of cultivation in North Africa<sup>118</sup> show.

It is precisely because of this Roman obsession for fine farming, lavishing a great deal of capital, labour, and care on every *iugerum*,<sup>119</sup> that large slave-staffed plantations are unlikely to have supplanted to any significant extent smaller farms run by tenant farmers and owner-occupiers, as Brunt and Toynbee would have it. Even for a man as wealthy as Pliny, staffing a huge (by Roman standards) estate of perhaps 1,000 *iugera*<sup>120</sup> entirely with slaves would have been prohibitively

<sup>112</sup> Col. 1.3.8–16; Plin. *Nat.* 18.7; 18.20; Verg. *G.* 2.412–3; Palladius 1.6.8.

<sup>113</sup> See the classic account in White (1967); Martin (1995); and the references in Compatangelo (1995, 51 n. 21). Roman authors frequently classify as *latifundia* farms of less than 1,000 *iugera*, often far less.

<sup>114</sup> See Pasquinucci (1979); Pasquinucci (2002).

<sup>115</sup> See Kron (2004b).

<sup>116</sup> See esp. Mommsen (1884); Day (1932); Frank (*ESAR* 5.168–75); Kuziscin (1957); Frederiksen (1970–1); De Neeve (1984a, 108–9; 163; 167–9).

<sup>117</sup> See Strabo 5.1.11 and Dall'Aglio (1995).

<sup>118</sup> See Mattingly (1996).

<sup>119</sup> A phenomenon illustrated most starkly by the remarkable success of the gram-marian Remmius Palaemon's venture in viticulture and the disastrous attempt at high farming of Tarius Rufus. See Plin. *Nat.* 14.49; 18.7.37.

<sup>120</sup> See Sherwin-White (1966, 257) noting Col. 3.3.8 and Plin. *Nat.* 14.48–52 with their estimates of HS 1,000 for unplanted land and HS 2500–3000 for vineyard.

expensive, as Sherwin-White properly points out,<sup>121</sup> and this does not include the cost of livestock, buildings, and moveable equipment. The villa at Settefinestre illustrates just how much capital and labour wealthy Roman farmers devoted to what were,<sup>122</sup> by the standards of the nineteenth-century English or southern Italian landowner, very modest farms—in terms of area, but not in terms of their value and productivity. Wealthy landowners farming even a few hundred *iugera* to this high standard using slave labour would quickly exceed even the equestrian or senatorial census. Even if the wealthy had in fact engrossed much of the arable land of Italy—and we have very little evidence that this was in fact the case, as we shall see—they could hardly have farmed the land properly except through tenant farmers drawn from the peasantry. Not only because of the prohibitive cost it would have entailed, but because it is highly unlikely that the Romans will have imported enough slaves to replace a very large proportion of Roman Italy's millions of peasants.<sup>123</sup>

It is helpful to put Toynbee and Brunt's vision of Roman landownership into a broader historical context. Land tenure systems in which most of the arable is held by a tiny percentage of the population are familiar from the history of most European nations, so much so that many historians easily assume the same of Roman Italy. But the vast estates of the English aristocracy and gentry or the *latifondi* of the modern Mezzogiorno were not built up gradually by surreptitious expansion. They were the legacy of a feudal land tenure system in

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Sherwin-White chooses HS 2,000 as a reasonable estimate of the average value of Pliny's estate.

<sup>121</sup> See Sherwin-White (1966) 257. The same argument is made by Martin (1971, 352) without, however, drawing the most logical conclusion, that very large estates exploited entirely by slave labour were exceedingly rare.

<sup>122</sup> See Carandini (1985).

<sup>123</sup> Toynbee (1965, 2.171–3) lays out the large numbers enslaved in the Greek East during the campaigns of the Romans. While these campaigns will have significantly increased the supply of slaves in the Mediterranean, the numbers flowing into Italy are unlikely to have changed the nature of the Roman rural workforce as radically as Brunt or Toynbee suggest. Chattel slavery had been a part of the Roman economy long before the Hannibalic War, and many slaves, most of them probably better suited for agricultural work in Italy, were taken during the conquest of Italy. Of those enslaved in the East, even those enslaved by the Romans are by no means certain or even likely to have been transported wholesale to Italy; many will presumably have been sold to local inhabitants or sold by slave traders throughout the Mediterranean. Most importantly, though, on any reading the numbers, while large, will have been a small proportion of the population of Greece and an even smaller proportion of the surely larger Italian population. See further Rosenstein (2004, 9–12).

which the nobility had seized the vast majority of the arable land and compelled the subject population to work the land as serfs.<sup>124</sup> Rome's conquest of the Mediterranean will have had a significant effect on the prosperity of the Italian peninsula, but it can hardly have armed the Roman officer class with enough capital to sweep away a densely settled, long established, and productive peasantry millions strong.

A comparison of Roman and nineteenth-century English landownership will reveal a vigorous and prosperous class of Roman smallholders and a radically different distribution of land. As we can see from Table 2, Bateman's survey of landholding in England demonstrated a staggering concentration of landed wealth in the hands of a tiny elite of fewer than 15,000. The richest 1% of landowners, those who owned more than 300 acres, held just over 70% of England's arable land.<sup>125</sup>

Table 2. Ownership of landed property in the United Kingdom<sup>126</sup>

<b>Landowners</b>	<b>Total acreage held</b>	<b>% of land held</b>	<b>Acreage (avg)</b>
400 Peers	5,728,979	17.4%	14,300
1,288 Greater Gentry	8,497,699	25.8%	6,600
2,539 Gentry (1,000 to 3,000 acres)	4,319,271	13.1%	1,700
9,585 large farmers (300 to 1,000 acres)	4,782,627	14.5%	500
241,461 farmers (1 to 300 acres)	8,076,078	24.5%	33
14,459 public bodies	1,443,548	4.4%	78
703,289 cottagers & labourers	151,148	0.5%	0.2

<sup>124</sup> See Warringer (1939, 7–17) for an admirably brief and clear sketch tracing the origins of the land tenure regimes of modern Western Europe and the origin of its highly productive peasant proprietorship. For further discussion see Blum (1978); Gibson & Blinkhorn (1991). The legacy of feudalism still marks rural society and agriculture in much of the Mezzogiorno, as Arlacchi (1983, 152–62) argues, pointing out how seven or eight great landowners continue to dominate agriculture in a region of 200,000 people, occupying almost 20% of rural Calabria. See also Blok (1966).

<sup>125</sup> Bateman (1883). The survey had been commissioned in the hope of discrediting claims that the number of English landowners had declined significantly, but instead showed much more concentration than had been imagined, with three-quarters of the arable land of England owned by only 7,000 people. See Beckett (1994, 90–1). This 300 acre threshold falls just below the 500 *iugera* ceiling imposed upon Roman landownership by the Licinian-Sextian Laws of 367 BC (Col. 1.3.12; Var. *R.* 1.2.9; Plin. *Nat.* 18.17). I am impressed by the arguments of Rich (in this volume) that we need not doubt our sources, which put this law in the fourth century BC and argue that it applied to all landownership, not simply ownership of *ager publicus*.

<sup>126</sup> Source: Beckett (1986, 50 table 2.1); based on the results of Bateman (1883).

English landowners could farm these massive estates only through tenant farmers, and they preferred to turn exclusively to the wealthiest members of the yeomanry. Caird suggested that in the grain growing district most of the acreage was farmed in units of 430 acres, and even in the mixed farming regions of the Midlands tenant farms generally consisted of 220 acres.<sup>127</sup> Larger farms of 1,000 acres or more were relatively common.<sup>128</sup> English landlords strongly preferred to let land to their tenants in large parcels. As a result, the census of 1851 revealed barely 250,000 active farmers (whether tenant farmers or owner-occupiers) in the whole of England and Wales, employing 976,000 proletarian labourers.<sup>129</sup> The number of Roman smallholders was surely many times greater. Augustus and his fellow triumvirs alone created by one estimate over 300,000 small farms<sup>130</sup> in just one of a long series of patronage and agrarian reform measures, and these individuals, drawn from the poorest strata of the Roman peasantry, will have represented a very small fraction of their number. The Roman republic frequently granted landless or impoverished peasants and demobilized soldiers farms on public land or in colonies,<sup>131</sup> often of significant size and well suited for intensive cultivation as a family farm with the labour of a handful of slaves.<sup>132</sup> Even Roman conservatives had always

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<sup>127</sup> Mingay (1989, 608–9). For a more detailed analysis see Collins (2000, 1836–76). Craigie (1887, 94) gives a breakdown of holdings farmed, rather than land owned, which shows that only just over 31% of the acreage was farmed in units of over 300 acres, but 72% in units of over 100 acres. See also Collins (2000, 1844 Table 37.2a). There were, of course, smaller farms, primarily on marginal lands or regions where determined and often (as at Otmoor) violent resistance had prevented the engrossment of all small farms. More than half of English farms were of less than 20 acres—Craigie (1887, 91)—but these represented only around 6% of the arable land.

<sup>128</sup> Frederick Law Olmstead (1859) comments with some disgust on the vast estates common in southern England: “The farms are all very large, often including a thousand acres of tillage land, and two, three, or four thousand of down. A farm of less than a thousand acres is spoken of as small, and it often appears that one farmer, renting all the land in the vicinity, gives employment to all the people of a village. Whether it is owing to this (to me) repugnant state of things, or not, it is certainly just what I expected to find in connection with it, that laborer’s wages are lower probably than anywhere else in England—seven, and sometimes six, shillings (\$1.68 and \$1.44) being all that a man usually receives for a week’s labour.”

<sup>129</sup> See Mingay (1989, 295). The Census of 1851 also returned 111,604 relatives of these farmers aiding in the cultivation of the fields.

<sup>130</sup> See Frank (*ESAR* 1, 322).

<sup>131</sup> Amply documented by e.g. Mommsen (1883); Kornemann (1901); Frank (*ESAR* 1.40–1; 59–61; 110–124; 218–21; 315–22); Salmon (1969, 110–1; 158–64); Keppie (1983, 122–7).

<sup>132</sup> For the size of colonists’ plots see e.g. Frank (*ESAR*, 1.122–4); Keppie (1983, 91–6); De Neeve (1984a, 109 n. 214); Moatti (1993, 24–5). For the successful farming

recognized the critical importance of land reform and a commitment to the smallholder as an essential prop of successful Roman imperialism. They publicly expressed their support for agrarian legislation in principle, even when they bargained to limit its impact upon their interests in private. The recognition of the economic as well as social advantages of peasant cultivation persisted even after the fall of the Republic stripped the peasantry of any potential electoral influence, as the encouragement of peasant proprietorship on imperial estates in North Africa illustrates.<sup>133</sup>

The attitude of the English landed interest was diametrically opposed, and the effect is apparent in the feeble state of the peasantry and the destitution of the English rural labourers, who constituted the vast majority of the rural population.<sup>134</sup> For example, privileged English tenant farmers opposed allowing their landless labourers to *rent* even tiny plots of an eighth of an acre, suitable for little more than keeping a garden or raising a few chickens, pigs, or other livestock.<sup>135</sup> Their attitude is summed up neatly in a report of Poor Law Commissioners in 1834: “We can do little or nothing to prevent pauperism; the farmers will have it: they prefer that the labourers should be slaves; they object to their having gardens, saying ‘The more they work for themselves, the less they work for us.’”<sup>136</sup> These tenant farmers recognized that even such tiny plots, when combined with use of common or rented pasture,<sup>137</sup>

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of their plots by veteran colonists in the late Republic see Keppie (1983, 122–7). For epigraphical evidence for the possession of a few agricultural slaves by Roman colonists see Keppie (1983, 124 n. 117).

<sup>133</sup> See Kehoe (1997). It is worthwhile to contrast the favourable terms granted to Roman peasants with the exploitative nature of the 25 year emphyteutic leases granted to southern Italian peasants. See Snowden (1986, 35–40).

<sup>134</sup> See Mingay (1989, 953).

<sup>135</sup> See Hasbach (1908, 75–6; 96–102; 209–16); Hammond & Hammond (1920, 60–3; 130–7; 208–9; 298–300). An Elizabethan law requiring that every cottage be provided with a minimum of four acres of land was repealed in 1775 and the farmers successfully resisted any attempt to reintroduce the regulation: Hammond & Hammond (1920, 130).

<sup>136</sup> See Hammond & Hammond (1920, 135–7). For similar attitudes see Hasbach (1908, 13–4). Access to a small plot of land could be of great economic benefit even to poor labourers. Most tenant farmers refused even to allow their labourers to keep pigs. See Hasbach (1908, 206). Contrast Var. *R.* 1.17.7, advising farmers to reward slaves by allowing them to pasture livestock of their own on the farm.

<sup>137</sup> Roman peasants continued to enjoy free grazing of up to ten large animals, and presumably 100 smaller animals (although this passage has been lost) on the common pastures in the agrarian law of 111 BC. See the commentary of Lintott (1992) on lines 14–15. In marked contrast to England, common pastures, a very valuable resource for small peasant proprietors, were preserved and respected even in this law, which

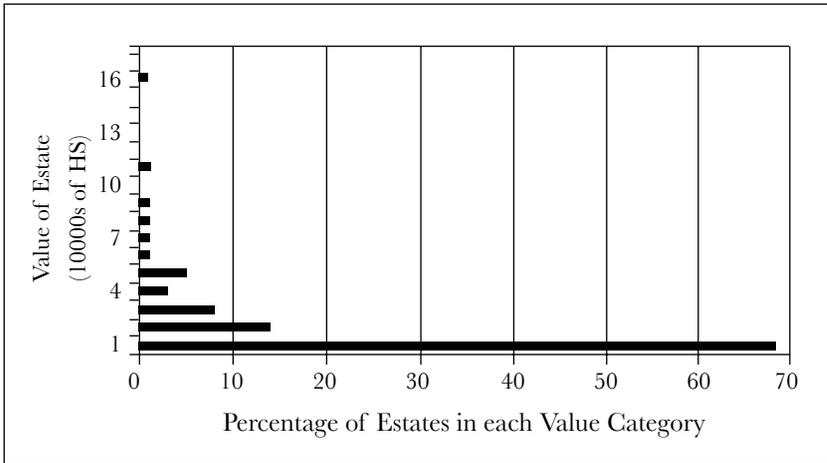


Fig. 1. Percentage of Estates of Various Values at Ligures Baebiani & Veleia.

could have a dramatic and unwelcome effect upon the independence and standard of living of a labourer.<sup>138</sup>

Our evidence for Roman landownership is far less secure, but still leaves little doubt that it was much more egalitarian than nineteenth-century English or Italian landownership. The surveys of landed property mortgaged to finance the Trajanic *alimenta* program at Veleia and at Beneventum, among the Ligures Baebiani,<sup>139</sup> show clearly that extremely large farms were exceedingly rare, and that most wealthy landowners owned several scattered holdings of relatively modest size.<sup>140</sup> (See fig. 1, above).

The *alimenta* inscriptions seem to have excluded small peasant farms from the estates providing payments, given that there are in fact over 200 individuals named as neighbours of the 53 property owners recorded in

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is normally considered a measure taken by the reactionaries in order to discourage further agrarian legislation.

<sup>138</sup> Hasbach (1908, 97) quotes Nathaniel Kent's claim that he knew cottagers personally who "were possessed of two or three milch cows, forty or fifty sheep, two or three pigs, and fifty to a hundred head of poultry, including chickens, geese, and turkeys. For all of which they only had to pay the rent of their house and vegetable garden, together with that of a bit of meadow from one to three acres in extent."

<sup>139</sup> See further e.g. Mommsen (1884); De Neeve (1984a, 167–9); Criniti (1991); Cason (1997); Di Cocco & Viaggi (2003).

<sup>140</sup> This is not surprising, of course. Day (1932) estimated that the vast majority of villas excavated near Pompeii and Herculaneum were less than 100 *iugera*, with only a few in the 200 to 300 *iugera* range. Sextus Roscius' HS 6,000,000 in landed property was divided among thirteen separate farms (Cic. *S. Rosc.* 21).

the inscription.<sup>141</sup> More intriguing, perhaps, the total value mortgaged is only HS 13,500,000. If we discount the *salvus* from our consideration and presume an average value of HS 2,000, as Frank suggests,<sup>142</sup> this would represent less than 17 km<sup>2</sup>, a relatively small proportion of the likely territory of Veleia.<sup>143</sup> It seems tolerably clear therefore that the farms listed represent the holdings of the wealthier landowners, and that most of the smaller peasants have been excluded. This sample will therefore tend to exaggerate the average size of Roman farms as well as the level of inequality in Roman landownership. Moreover, there is some evidence of a possible consolidation of estates since the late Republic and early Principate.<sup>144</sup> Nevertheless, almost 70% of these larger farms are valued at less than HS 100,000 (presumably around 12 ha or so),<sup>145</sup> and these smaller farms represent over 30% of the total value of the land donated. The wealthiest 1% of these landowners owned less than 8% of the total value of the land mortgaged: compare this to the 70% of the total acreage in 1870s England owned by the richest 1%.

Figure 2, below, gives the Lorenz curve, graphically comparing the relative inequality in the distribution of land ownership for England in the 1870s and at Veleia and Beneventum. A completely egalitarian distribution of wealth would run along the diagonal: the greater the deviation of the curve from the diagonal, the greater the level of inequality.<sup>146</sup> The contrast could not be more dramatic, and it probably understates the reality. We can be reasonably confident that a large and vigorous class of small owner-occupiers, not just tenant farmers, had survived into Trajan's reign, and that at no time was there ever a concentration of landed wealth in Roman Italy comparable to that in nineteenth-century England, or nineteenth-century Italy, for that matter.

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<sup>141</sup> See e.g. De Neeve (1984a, 171).

<sup>142</sup> See Frank (*ESAR*: 2, 173).

<sup>143</sup> See Di Cocco & Viaggi (2003) for a reconstruction of the likely locations of the *pagi* and estates attested in the inscription, and the extent of the region from which the wealthier landowners were drawn.

<sup>144</sup> As pointed out e.g. by Mommsen (1884). One ought not exaggerate the extent of this consolidation, however. See Tchernia (1986, 260–99); Lewit (1991); Marzano (2005).

<sup>145</sup> Although, as De Neeve (1984a, 172) rightly insists, the valuation of the lands, particularly *salvus*, will often have had an uncertain relationship with the area being farmed.

<sup>146</sup> See Aitchison & Brown (1966, 101–2; 107–20) for an explanation of the calculation and use of the Lorenz curve.

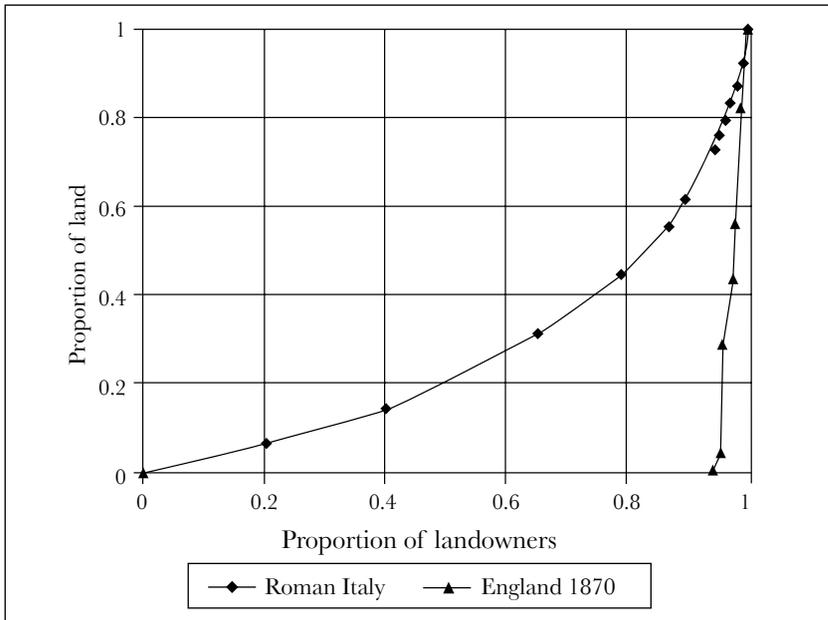


Fig. 2. Land distribution in England and Roman Italy.

Italian landownership was also profoundly inegalitarian (the Borghese possessed over 22,000 ha, more than all but the greatest English peers),<sup>147</sup> although most central and northern Italian landowners continued to rely upon the *contadini* to cultivate their lands in small peasant plots.<sup>148</sup> In Lazio and the Mezzogiorno, however, vast tracts were denuded of inhabitants, the land divided into huge farms or *latifondi* of up to 4,000 ha,<sup>149</sup> generally owned and managed by absentee landlords ignorant of farming,<sup>150</sup> poorly stocked, and abysmally farmed.<sup>151</sup> The contrast

<sup>147</sup> See Desplanques (1969, 119 n. 2). This estate alone represented over 10% of the 205,368 ha of arable land of the Roman Campagna.

<sup>148</sup> Estates of over 200 ha, which were restricted to only around 1% of landowners, constituted 44% of the total arable acreage in Umbria in 1946, 46% of the acreage in Tuscany, and 50% in Lazio. Even in regions with smaller estates, such as Emilia, estates of over 200 ha covered 19.7% of the arable. See Desplanques (1969, 118).

<sup>149</sup> See e.g. Blok (1966); Sereni (1968, 173); Prampolini (1981, 179–84) and Arlacchi (1983, 124–40).

<sup>150</sup> Prampolini (1981, 168–74).

<sup>151</sup> See e.g. Franchetti & Sonnino (1925); King (1971, 19–24); Prampolini (1981, 93–111); Cuboni (1992).

with Roman Italy did not fail to strike many. As Mill noted,<sup>152</sup> and Sismondi explains:

The Roman territories, so prodigiously fertile, where five acres fed a family and provided one soldier, where vines, olives, and figs intermingled in the fields, and allowed three or four crops per year... this land has seen single houses gradually disappear, villages, the whole population, enclosures, vineyards, olives, and all those crops which demanded continuous attention, work, and above all human care. Then vast fields followed... The native population in the Roman Campagna would be useless to the farmers, and it has completely disappeared... Nepi and Ronciglione may quickly see their inhabitants, who were alienated from the soil by which they ought to live, disappear, and one can calculate in advance the expected day when the plow will go over the land where their houses stand, as it has passed already over the ruins of San Lorenzo, Vico, Bracciano, and Rome herself.<sup>153</sup>

#### VI. *The Productivity of Peasant Farming*

Many Classical historians, but by no means all (Pleket, Kolendo, Carandini, Capogrossi Colognesi, Spurr, Marcone, even White are notable exceptions),<sup>154</sup> have long portrayed the Roman peasant as an unproductive subsistence farmer, often relying on theoretical models of Russian and Eastern European peasant farming,<sup>155</sup> which fit very uneasily with Roman social conditions.<sup>156</sup> Medieval and early-modern agrarian history reveals, however, abundant evidence of peasant

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<sup>152</sup> See Mill (1906, 324).

<sup>153</sup> Sismondi (1820, 185–6). The remarkable agricultural wealth of Roman Campagna, particularly the *suburbium* of the city, is admitted grudgingly by Brunt (1971, 345–50) and is amply documented by Strabo 5.3; Kolendo (1993); Morley (1996); Dalby (2000, 30–42). For the very heavy settlement of Latium and South Etruria see e.g. Potter (1979); Quilici Gigli (1993); Witcher (2005); Witcher (2006).

<sup>154</sup> Pleket (1990); Pleket (1993); Kolendo (1980); Kolendo (1993); Carandini (1985); Spurr (1986); Marcone (1997); Forni & Marcone (2002); White (1970).

<sup>155</sup> Most notably Kula (1976); Chayanov (1966). The often ill-informed condescension of some social scientists towards peasant agriculture is striking. Rogers (1969) lists the characteristics of the peasant as: “mutual distrust in interpersonal relationships; lack of innovativeness; fatalism; low aspirational levels; lack of deferred gratification; limited time perspective; familism; dependency upon government authority; localiteness; and lack of empathy”.

<sup>156</sup> Eastern European peasants laboured under very heavy taxes and *corvées*, feudal control, serfdom, low levels of urbanization and consequently of demand, and few livestock. Many of these disadvantages have persisted throughout the twentieth century. See Warringer (1939); Melton (1998). The transition to mechanization actually exacerbated the uncompetitiveness of Eastern European agriculture by depriving the arable of critical manure. See Warringer (1939, 177).

farmers, free owner-occupiers producing for urban markets,<sup>157</sup> who were remarkably productive, as Pleket recognized in a magisterial comparative analysis. Pleket's insights can be pushed further, however. He did not fully appreciate the remarkable productivity and sophistication of Greco-Roman agronomy. Nor did he ask whether a more democratic and egalitarian Greco-Roman society could have opened up much greater levels of demand, and hence production, than was possible in Europe before the collapse of the *ancien régime* in the late nineteenth and early twentieth centuries.<sup>158</sup>

Although economic historians have always lauded the large-scale English 'capitalist' tenant farmer, the peasants of Holland, Flanders, and Brabant were the true innovators in the agricultural revolution.<sup>159</sup> Working smallholdings of 16–18 hectares, generally stocked with small herds ranging from 15 to 24 cattle, they bred the first large modern livestock (at least since Greco-Roman antiquity), and produced cheese, meat, and vegetables for the densely populated Dutch towns and for export.<sup>160</sup>

The standard of living which these modest peasants achieved was remarkable and should warn us not to underestimate the wealth which many Roman *coloni* could have attained. By early 1700 most Dutch rural households enjoyed a large array of consumer goods. In one region 55% owned books, over 70% owned clocks, 94% had mirrors,

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<sup>157</sup> Warringer (1939, 157) explains the highly productive model of Western European peasant farming, one which, I shall argue, is also well attested for Roman Italy: "the type of animal husbandry in connexion with arable farming which is prevalent in Western Europe and Great Britain needs a lucky combination of economic factors, technical methods, and market conditions. First, market conditions favour pork and veal, which a peasant farm can well produce. Second, technical conditions in Western and Central Europe favour farming in family units, owing to the regular labour requirements of meat and milk production, and the possibility of investing extra crop production in additional livestock."

<sup>158</sup> While acknowledging the conservative and relatively inegalitarian nature of Roman politics and society, as judged by Classical or Hellenistic Greek standards, the real electoral influence of the Roman plebs was still radically greater than that of the poor in most European states before the late nineteenth century. I therefore prefer to credit the analysis of Millar, Lintott, Hölkeskamp, and Yakobson, endorsing Polybius' assessment of Roman politics, against the old Münzer orthodoxy. For a useful sketch of the debate with recent bibliography see Yakobson (2006).

<sup>159</sup> The early English pioneers in new techniques, most notably Sir Richard Weston, did not hide the critical role of peasants from the Low Countries in all the most important innovations. See Weston & Hartlib (1650); Ambrosoli (1997, 305–20).

<sup>160</sup> Slicher van Bath (1960); De Vries (1974); Van der Wee (1978). These methods were also used on considerably smaller farms, of course. See Vanhaute (2007, 123–25) for references.

and 63% some silverware.<sup>161</sup> This prosperous Dutch peasantry also enjoyed a very high level of education and literacy,<sup>162</sup> as well as great skill and knowledge of agriculture.

Yet, despite the resounding success of peasant agriculture in the Low Countries, eighteenth- and nineteenth-century economists and agricultural writers produced a flood of propaganda decrying the ignorance, lack of innovation, traditionalism, and inefficiency of the peasantry.<sup>163</sup> Arguing that only tenant farmers with considerable capital were prepared to utilize the most advanced agricultural techniques, this concerted campaign did a great deal to salve the consciences of the English landed classes as they completed the dispossession of what remained of the English peasantry.<sup>164</sup> Anglo-Irish, Scottish, Prussian, and southern Italian landowners also fastened eagerly on this literature as a justification for brutally exploiting and ultimately driving off the land millions of nineteenth- and early-twentieth-century peasants and rural labourers.<sup>165</sup>

Jean-Charles Léonard de Sismondi (1773–1842), one of the most prescient of nineteenth-century critics of Classical Economics, challenged the propaganda in favour of English agrarian capitalism with incisive analysis and a thorough knowledge of the agricultural regimes of Switzerland, Tuscany, and England. He ably argued that small owner-occupiers consistently achieved very high levels of productivity:

The high state of culture to be found in the finest parts of Italy, above all of Tuscany, where the lands are generally managed in this way; the accumulation of an immense capital upon the soil; the invention of

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<sup>161</sup> De Vries (1974, 214–23).

<sup>162</sup> De Vries (1974, 211–13).

<sup>163</sup> See Hasbach (1908, 147–70) for a summary and his bibliography for full references to the debate. Some of this literature, particularly Arthur Young's jaundiced accounts of French and Italian peasant farming, had a patriotic or chauvinistic tinge, but all was well suited to appeal to the prejudices and self-interest of large-scale landowners and tenant farmers. See esp. Hammond & Hammond (1920) and Hasbach (1908, 69–102) for an incisive exposure of these biases.

<sup>164</sup> For the course of the process Hasbach (1908) provides a classic account. See also Beckett (1994) for more recent scholarship.

<sup>165</sup> The Anglo-Irish landowning class and the British government callously exploited the Irish potato famine as an opportunity to institute a system of large estates in Ireland on the English model. Over the course of the crisis a million Irish peasants died, at least another million were forced to emigrate, and millions more were stripped of their small cottier tenancies. See Gray (1999, 8–12, 76–8, 331–3). Nearly 4.2 million people, primarily peasants from the Mezzogiorno, fled Italy between 1861 and 1911. See Sereni (1968, 351–6). For the situation in Prussia see Slicher van Bath (1963, 323–4).

many judicious rotations, and industrious processes, which an intelligent, observing spirit alone could have deduced from the operations of nature; the collection of a numerous population, upon a space very limited and naturally barren, shows plainly enough that this mode of cultivation is as profitable to the land itself as to the peasant, and that, if it imparts most happiness to the lower class who live by the labor of their hands, it also draws from the ground the most abundant produce, and scatters it with the most profusion among men.<sup>166</sup>

Given the wretched poverty of the English and southern Italian rural labourer,<sup>167</sup> it was not difficult for Sismondi to demonstrate the relative prosperity of the rural population in a peasant society. He also showed how much more labour- and even capital-intensive peasant farming can be,<sup>168</sup> and pointed out the immense wealth of skill, innovation, and knowledge derived from the direct experience of farming by a large number of cultivators. The reduction of the peasantry to the status of landless labourers<sup>169</sup> destroyed this rich human capital and store of knowledge<sup>170</sup> through much of England. In southern Italy the rural labour force, in the words of an unemployment commissioner at Bari: “merely carries out the physical tasks that he is ordered. He is a manual labourer in the literal sense of the word and he has no understanding of agriculture”.<sup>171</sup>

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<sup>166</sup> Sismondi (1820, 161). See also Sismondi (1820, 143–7; 180–6); Mill (1906, 324–6).

<sup>167</sup> Conditions were significantly different for the *contadini* of central Italy, most of whom were sharecroppers using the *mezzadria* system rather than owner-occupiers, but fully engaged nevertheless in managing the farming enterprise. See e.g. Desplanques (1969, 184–203).

<sup>168</sup> Offer (1996, 84) cites the testimony of Albert Pell and Clare Read, MPs, before a Royal Commission on the English agricultural depression of the late nineteenth century, who explained the industry of owner-occupiers: “Few English farmers have any idea of the hard and constant work which falls to the lot of even well-to-do farmers in America. Save in the harvest, certainly no agricultural labourer in England expends anything like the same time and strength in his day’s work... He adds to all the mental cares of ownership the physical stress of manual labour of the severest description.”

<sup>169</sup> Frederick Law Olmstead remarks of the rural labourers he met in his travels in England: “...I did not see in Ireland, or in Germany, or in France, nor did I ever see among our Negroes or Indians or among the Chinese or Malays, men whose tastes were such mere instincts, or whose purpose of life and whose mode of life was so low, so like that of domestic animals altogether, as these farm labourers.” See Olmstead (1859, 238–9).

<sup>170</sup> See Thirsk (1957); Whittle (2000).

<sup>171</sup> See Snowden (1986, 21–4) for the dehumanizing working conditions and the limited knowledge and experience of farming of most labourers. For the problems faced by agrarian reformers in the 1950s in attempting to teach the rudiments of peasant farming to southern Italian farm labourers see Carlyle (1962, 86–7).

John Stuart Mill broke with the orthodox consensus in favour of large-scale capitalist farming and strongly defended Sismondi. Mill recognized the disastrous wastefulness of giving the English rural population no share in the wealth of the countryside, no incentive to work harder, and no opportunity to innovate. He devoted two full chapters of his *Principles of Political Economy* to laying out a compelling case,<sup>172</sup> helping to reverse a long-standing prejudice of most upper-class Englishmen. Further studies in a wide range of societies have corroborated his conclusions, showing that small farms are consistently more heavily stocked with cattle,<sup>173</sup> more labour-intensive, and more productive than large farms.<sup>174</sup> Even in the profoundly hostile social and political environment of eighteenth- and nineteenth-century England, peasant farming proved its extremely high productivity in those marginal areas, such as the Fenland of Lincolnshire in Northern England, where it survived, as Joan Thirsk has shown.<sup>175</sup>

Mill helped inspire many others to question England's long tradition of vast landed estates,<sup>176</sup> but it was a prolonged crisis in English farming and the inexorable competition of England's long-despised competitors on the Continent that eventually settled the argument decisively in favour of the peasant. As the feudal land tenure regimes of the Continent were broken up and the land distributed to a free peasantry,<sup>177</sup> and as the rest of Europe began to catch up with England's precocious urban growth and meat consumption, English agriculture was driven into a prolonged depression.<sup>178</sup> English tenant farmers, saddled with inflated rents imposed on them in the tripartite land-owner-tenant-labourer system, found themselves unable to compete

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<sup>172</sup> Mill (1906, 321–75).

<sup>173</sup> Franklin (1969, 39 table 2.11). Mingay (1989, 183 table 2B.3). Craigie (1887, 104) yields the following figures for 1887, if one converts cattle, pigs, and sheep to standard livestock units (LSU): farms from 1 to 5 acres: 259 LSU per 100 acres; from 20 to 50 acres: 229 LSU; from 100 to 300 acres: 187 LSU; for farms over 1,000 acres: 163 LSU per 100 acres. I have excluded horses from the calculation, but they only increase the effect.

<sup>174</sup> See e.g. Levy (1911); Warringer (1939); INEA (1964, 124–5); Villani (1968, 137–8); Franklin (1969, 17–20; 51–71); Arlacchi (1983, 140 table 30); Carter (1984); Beckett (1990, 48–53); Mingay (1989, 179–89); Allen (1992); Whittle (2000).

<sup>175</sup> Thirsk (1957).

<sup>176</sup> Offer (1996, 83–4).

<sup>177</sup> For an analysis of the nature and limitations of the process see Gibson & Blinkhorn (1991). For the broader historical background see Blum (1978).

<sup>178</sup> Mingay (1989, 590–615); Perren (1995); Collins (2000, 138–57).

with European, Australian, or North American owner-occupiers.<sup>179</sup> As Avner Offer puts it: "...history was soon to vindicate Mill and his acolytes. Owner-occupiers from across the channel, the North Sea, and the Atlantic, captured large parts of the British food market from the British tenant farmer. Wheat, butter, eggs, bacon, apples, even hay: all were imported in large quantities. Owner-occupiers overseas, working with their own hands, had a capacity for adaptation, learning and sacrifice which British tenant farmers on the whole did not."<sup>180</sup> When English farming finally recovered from the crisis, many decades later, much of England's arable land had been abandoned or converted into pasture, and owner-occupiers gradually came to play an increasingly important part in the revival of English agriculture.<sup>181</sup>

### VII. *Further thoughts on Roman peasant farming*

While equally productive and efficient, the Roman *colonus* enjoyed a measure of social prestige, political influence, and personal dignity rarely enjoyed by the peasants of the *ancien régime*, even in the Netherlands or central and northern Italy.<sup>182</sup> Idealized in Roman poetry and constantly lauded in Roman stump speeches and literature,<sup>183</sup> the Roman peasant farmer had won political influence and patronage<sup>184</sup> through his crucial military role,<sup>185</sup> his tenacious resistance in the Struggle of the Orders, and his industrious farming. Roman peasants were relatively free from

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<sup>179</sup> For the financial difficulties faced by the parties in English 'capitalist' agriculture see Mingay (1989, 609–16). The huge social cachet associated with landownership drove land prices up to 30 years purchase, leaving only about a 3.5% gross or 2.5% net return on land, even at high rack rents: Mingay (1989, 552). For the uncompetitiveness and high costs imposed by this system see also Sismondi (1820, 132; 197–208).

<sup>180</sup> Offer (1996, 83).

<sup>181</sup> For the increase in small farms during the agricultural depression see Collins (2000, 1836–7).

<sup>182</sup> See e.g. Blum (1978, 29–49) and, for Italy, Epstein (1998). For the antipathy or paternalism that characterized the attitudes of urban landlords to peasant sharecroppers in the Middle Ages and particularly in eighteenth- and nineteenth-century Italy see e.g. Desplanques (1969, 189–90); Laurent (1984). For the brutal treatment of southern Italian labourers see Snowden (1986, 17–34; 41–64).

<sup>183</sup> The *topos* is ubiquitous, with the assimilation even of early-republican senators to the peasant lifestyle, but see e.g. the sources in Heitland (1921, 135–41); Col. *pr.* 12–18; Var. *R.* 2.*pr.*3; Verg. *G.* 2.401; 2.458–540; Juv. 16.161–72; Plin. *Nat.* 18.8; Cato *Agr. pr.* 2. See also e.g. Tib. 1.1; 1.7; 2.1; 2.3; Cic. *Sen.* 51; *S. Rosc.* 47–52; Cic. *Cato* 15–7.

<sup>184</sup> This is reflected most clearly perhaps in the electoral favouritism shown to the rural tribes. See Millar (1998, 35–7).

<sup>185</sup> Cato *Agr. pr.* 4; Vegetius 1.3.2; cf. Livy 42.34. See also e.g. Brunt (1962, 72–8).

the punishing taxes, feudal dues, and *corvées* faced by peasants in the *ancien régime*,<sup>186</sup> as well as the extremely heavy indirect taxes which English and Italian rural labourers paid on their food and shelter.<sup>187</sup> Furthermore, as we have seen, Roman peasants were far more likely to be landowners like the Dutch, rather than tenants, sharecroppers, or labourers, as in England or Italy.<sup>188</sup> Military service, despite its hardships, would have provided a valuable bulwark to the Roman peasant economy, offering a source of steady income for underemployed farmers as well as working capital for farming operations from the distribution of booty.<sup>189</sup>

The Roman equestrian and senatorial elite, whatever their true feelings, consistently professed their admiration for the *industria*, *parsimonia*, and *frugalitas* of the peasant,<sup>190</sup> and acquiesced in a long list of censorial regulations designed to convince the people that they shared their values.<sup>191</sup> Contrast this respectful attitude with the contempt of the English tenant farmer or landowner for the rural labourer, well

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<sup>186</sup> See e.g. Blum (1978, 50–94). For the persistence of many of these burdens on the peasant economy see also Gibson & Blinkhorn (1991). Boyd (1952) documents the heavy burden of tithes on the Italian agrarian economy, particularly on the peasant.

<sup>187</sup> Thompson (1963, 356) notes that in the mid-nineteenth century indirect taxes paid by the working class could amount to as much as half their annual income. For the shifting of the tax burden onto the rural labourers in southern Italy see Snowden (1986, 50–6). For indirect taxes in Italy generally see Craigie (1887, 125).

<sup>188</sup> Although central and northern Italy possessed a vigorous peasant class cultivating the land in relatively small plots using the system of share-cropping, called *mezzadria*, land ownership throughout Italy was very highly concentrated in the hands of the Church, nobility, and bourgeoisie. See e.g. Desplanques (1969, 119–34; 147–8); Herlihy & Klapisch-Zuber (1989, 115–7). In the South, peasant farming was overshadowed by *latifondi* worked by labourers, with the exception of coastal areas furnished with plantations at the end of the nineteenth century. See e.g. Prampolini (1985, 179–84); Snowden (1986, 17–40).

<sup>189</sup> See Rosenstein (2004) for a critically important analysis of the impact of military campaigns on the Roman farming economy. For a list of sources for the booty taken in the period between 200 and 157 BC see Frank (*ESAR* 1.127–38). In the more regular distributions described by Livy the ratio between legionaries and cavalry is only one to three, although staff officers will have received much more. MacMullen (1974, 94–5) following Frank (*ESAR* 1.324–5) emphasizes the less representative figures from Pompey's triumph. Nevertheless, the soldiers received 71 million *denarii* compared to 25 million for Pompey and his staff officers (a sum which may include some of the expenses of the triumph).

<sup>190</sup> As in Cato's claim that he had spent his entire youth in *parsimonia atque in duritia atque in industria*. See Astin (1978, 3). The notion of individuals of relatively high social status turning their hand on occasion to farm work is far from inconceivable even in a much later age. See *Juv.* 3.223–9 and *Mart.* 14.49.

<sup>191</sup> See Baltrusch (1989).

attested in a rich vocabulary of abuse, including such choice epithets as hodge, chaw-bacon, swain, hick, gaffer, bumpkin, boor, clodhopper, churl, clown, bog-trotter, hayseed, yokel, hillbilly, hind, and village idiot.<sup>192</sup> Ramsay MacMullen's lexicon of Roman snobbery looks rather tepid by comparison.<sup>193</sup>

Although frequently portrayed as a villain in the alleged demise of the Roman peasant,<sup>194</sup> Cato in his *de Agricultura* preserves for us one of the best accounts of some of the traditional methods of the Roman small farmer,<sup>195</sup> and more importantly of the labour-intensive philosophy of farming,<sup>196</sup> which is the ultimate legacy of the Roman peasant. Cato's decision during his censorship of 184/3 BC to impose the censorial *nota* upon any member of the senate who had failed properly to purge and weed his fields, aptly highlighted by Fraccaro and Kolendo, perfectly encapsulates his determination to maintain this venerable Roman tradition.<sup>197</sup> His manual gives an account of his own farming practice, and therefore describes the farming of two relatively large farms by his

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<sup>192</sup> Snell (1992, 162).

<sup>193</sup> See MacMullen (1974, 138–41).

<sup>194</sup> The connection is too commonplace to document in full, but notice the particularly colourful denunciation in Toynbee (1965, 2.296–310) reminiscent of Cobbett in full flight, but with far less cause. For a more insightful and sensible discussion see Astin (1978, 240–76). Interestingly, Astin notes the complete obliviousness of Cato to any crisis in Roman peasant agriculture, without canvassing the possibility that perhaps modern accounts of this crisis are somewhat overblown.

<sup>195</sup> Cato has received far less attention than Columella as an agronomist, but see Gummerus (1906) and Brehaut (1933) for analysis of the soundness of his advice. As Frayn (1979, 52–6) shows, the later agronomists also include advice for, and preserve information derived from, the practice of poor peasants and wealthier owner-occupiers. Cato's work is more representative of traditional Roman peasant farming, as he seems consciously to eschew references to recent developments due to Hellenistic and Carthaginian agronomy. For example, although Cato focuses a good deal on forage, he makes no reference to several fodder crops which would be of critical importance in later agronomists, most notably alfalfa and shrub trefoil. There are also no clear references to the terminology of convertible husbandry. His references to peasant lore and superstitions are also much more frequent than in the other Roman agronomists. All the agronomists show a healthy respect for the experience and knowledge of the peasant farmer. See e.g. Col. 1.4.3–4; 1.7.1–4; Verg. *G.* 1.51–3; Cato *Agr.* 1.4; 4.2; and the sources cited by Frayn (1979, 53–6); cf. Plin. *Nat.* 18.7.28.

<sup>196</sup> See Gummerus (1906, 15–49); Kolendo (1980, 57–70; 85–128); Kolendo (1993); Dickson (1788, 1.502–4; 1788, 2.35–6; 75–80). Cato recommends more labourers to cultivate both his 100 and 240 *iugera* farms than would be employed on 1,000 acre farms in England. See Hasbach (1908, 81–2). See also Craigie (1887, 102) for a breakdown of the labour employed on English holdings, significantly lower than Cato's recommendations.

<sup>197</sup> See Kolendo (1980, 122 and n. 202).

slave *familia* under the supervision of a *uilicus*. It would be a mistake to conclude from this, however, that Roman smallholders would not have welcomed this ground-breaking work, very well crafted so as to portray Cato as precisely the sort of tough, frugal, acquisitive,<sup>198</sup> and hard-working farmer whom the Roman peasant-soldier would most admire.<sup>199</sup> Cato's work reflects the high standards of cultivation and the emphasis on livestock, wine, olive oil, and a wide range of crops typical of the most intensive nineteenth- and twentieth-century Italian or Dutch peasant farming.<sup>200</sup> It also constantly reveals, like the works of Columella and many other wealthy Roman farmers,<sup>201</sup> the sort of direct experience and careful study of farming eschewed by their English aristocratic counterparts, disinterested in lowly pursuits like agriculture, as Dickson complains.<sup>202</sup>

With a healthy market for meat, wine, vegetables, and olive oil, and a commitment to mixed farming informed by generations of practical experience, as reflected in the lore collected in Cato's *de Agricultura*, Roman *coloni*, like their Dutch counterparts, could make a respectable income even from a plot of a few *iugera*.<sup>203</sup> Not that Roman peasants will often have relied solely upon farming their own smallholdings.<sup>204</sup>

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<sup>198</sup> Gummerus (1906, 15) and Martin (1971, 81–93) see in Cato's attention to profit the type of the cold calculating businessman or capitalist rather than the peasant, but a keen interest in profit is characteristic of family farmers in many cultures. See the extremely sensible discussion in Astin (1978, 258–61) and compare the unsentimental attitude in the words of modern family farmers collected by Robinet (1973) and Scheuring (1983).

<sup>199</sup> For Cato's canny self-presentation as a farmer working with his own hands alongside his slaves see Astin (1978, 1–3). See also the encomium of the peasant farmer in Cato *Agr.* 2; 5.1–5.

<sup>200</sup> See Desplanques (1969); Pazzagli (1973).

<sup>201</sup> See e.g. Gummerus (1906); Kolendo (1980); Rinkewitz (1984); Kron (2004a); Kron (2008).

<sup>202</sup> See Dickson (1788, 1.94–5). Under the influence of Coke of Norfolk and George III there was to be a brief vogue for agricultural experimentation on the part of the English aristocracy, but the vast majority of the literature was written by men of much lower status, such as Arthur Young or William Marshall.

<sup>203</sup> Keppie (1983, 124 n. 113) cites the testimony of peasants in the Liri valley that a holding of 3 ha or 12 *iugera* with some wine and vegetables was sufficient to support a family in the early 1980s. For the viability of nineteenth-century Belgian peasant farms of as little as 0.5 ha, combining a range of subsistence and cash crops with one or two cattle, able to weather even under the extreme stresses of the 1845 potato blight, see Vanhaute (2007, 123).

<sup>204</sup> See e.g. De Neeve (1984a, 169–70). For the likelihood that the small 2 *iugera heredia* and other small plots given to colonists in the early Republic were to be supplemented by tenant farming see Frayn (1979, 90–3). *Pace* De Neeve (1984a) there is no warrant for underplaying the importance of the activities of Roman peasants, both the landless

The highly entrepreneurial activities of the ambitious Sienese peasant Benedetto di Meo Massarizia show how aggressively farmers with a bit of land of their own could buy, lease, or rent and then either work, sublet, or sell a range of farms, often scattered over a broad area.<sup>205</sup>

Although many assume, with Toynbee, that viticulture was overwhelmingly dominated by slave-staffed plantations<sup>206</sup> Roman peasants must surely have played an important role in wine production, as Tchernia has argued.<sup>207</sup> Even in the extremely poor agrarian economy of southern Italy today, strangled by poverty and the competition of international and northern Italian farming, a peasant can make more income from a thousand square metres of vineyard than from all the grain, livestock, and olive oil he can cultivate on eight hectares of land.<sup>208</sup> The illuminating diaries of a family of *contadini* in Renaissance Siena vividly illustrate how enterprising small peasants bought vineyard plots whenever possible and relied heavily on selling such cash crops in the market.<sup>209</sup>

De Caro's excavation of the Villa Regina at Boscoreale illustrates the sort of prosperity a good vine-dresser could achieve.<sup>210</sup> The owner worked a small plot of perhaps 3–8 *iugera*<sup>211</sup> planted with vines, fruit and nut trees, and a small irrigated garden, and there is also evidence for the keeping of some pigs. Nevertheless, the villa is furnished with a *torcularium*, *dolia*, and a *cella uinaria* with a capacity of approximately

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and those with some land of their own, as tenant farmers before the Gracchi, just as in the late Republic and Principate, when it is abundantly attested. See Capogrossi Colognesi (1996); Kehoe (1997, 139 ff. and 139 n. 5); De Ligt (2000).

<sup>205</sup> Balestracci (1999, 45–61). For the routine cultivation of a significant number of scattered plots by Umbrian *contadini* see Desplanques (1969, 199–202 and figs. II, 22; II, 23).

<sup>206</sup> Brunt (1972); De Neeve (1984a) and (1984b); Carandini (1988).

<sup>207</sup> Tchernia (1986, 114–5; 260–99). Italian vineyards remain relatively small to the present. In the early 1960s, for example, vineyards in Piedmont ranged from 1.64 to 10.38 hectares, with an average area of only around 3.75 hectares but an average value of 500,000 Lire. See INEA (1964, 46).

<sup>208</sup> Brøgger (1971, 37–8) summarizes the case of one peasant from the hamlet of Montevarese who makes each year 600,000 Lire from his vineyard, compared to 30,000 Lire from cereals, 100,000 Lire from livestock, 150,000 Lire from olive oil, and another 905,000 Lire from wage labour.

<sup>209</sup> Balestracci (1999, 25–6). For an excellent survey of the abundant Roman and comparative evidence for the energetic participation of small peasants in the market see also De Ligt (1990) and (1991). For the full integration of Renaissance Italian peasants in the market see also Epstein (1998, 96–7; 101–6).

<sup>210</sup> De Caro (1994).

<sup>211</sup> De Caro (1994, 127–8).

10,000 litres, and the modest but comfortable *villa urbana* is well decorated with frescoes and has been expanded to include a *triclinium* and *lararium* decorated in the fourth style.<sup>212</sup> De Caro's calculations suggest that the sale of wine could realize a gross revenue of up to HS 7,500.<sup>213</sup> As Jacopo Ortalli illustrates with a detailed survey of small farmsteads in northern Italy,<sup>214</sup> the modest prosperity of the farmhouse at Villa Regina is by no means atypical and would likely be much better represented were excavators more inclined to investigate surface scatters from smaller farmsteads, as Dominic Rathbone argues in this volume.

Given the lively market for meat, livestock farming, even more perhaps than viticulture or olive oil production, would have been very well suited, as it was in the Netherlands, for Roman small farmers eager to make a living and build up some capital.<sup>215</sup> Dairying and mixed farming, along with market gardening, often represented the only refuge for small farmers determined to survive the concerted onslaught of the great landowners in eighteenth- and nineteenth-century England.<sup>216</sup> Livestock farms would not have faced the additional labour or capital costs of harvesting and processing grapes or olives. Moreover, livestock are not only extremely valuable, they are the most easily and inexpensively transported of all agricultural products,<sup>217</sup> and so peasants need not have been as adversely affected by escalating costs for land located near cities or major transport arteries, as argued by Yeo and De Neeve.<sup>218</sup>

Market gardening is very labour intensive and virtually demands peasant proprietorship for maximum productivity.<sup>219</sup> The potential return and labour demands are such that plots of two or three hectares are generally beyond the ability of most peasant families. In most of early-twentieth-century Campania market gardening was carried out

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<sup>212</sup> See Jashemski's analysis in Jashemski (1987) and De Caro (1994, 95–114) as well as De Caro (1994, 115–30).

<sup>213</sup> Plin. *Nat.* 14.50 f. claims that Atticus' vineyards at Arretium and Nomentum could produce a revenue of up to HS 7,500 per *iugerum*.

<sup>214</sup> Ortalli (2006).

<sup>215</sup> Var. *R.* 2.4.3 has Tremelius Scrofa declare that no Roman who cultivates a farm fails to keep swine.

<sup>216</sup> Beckett (1990, 50–1).

<sup>217</sup> Kautsky (1899, 1.35–6). In the Roman context see Yeo (1946).

<sup>218</sup> De Neeve (1984b).

<sup>219</sup> See Sismondi (1820) and Mill (1906) for the importance of market gardening in Continental Europe and the contrast with England. For the development of Dutch market gardening see De Vries (1974, 153–5).

on plots ranging from one half to two hectares.<sup>220</sup> As we have already alluded, gardening was a particularly highly developed segment of Roman farming, in marked contrast to its limited role in English agrarian capitalism.

### VIII. *Conclusion*

Considering that most modern interpretations of the Gracchan political crisis are rooted in the hypothesis of an even more profound social crisis in Roman peasant agriculture, it is surprising how little serious critical scrutiny historians have given to the economic viability of the smallholder in Roman peasant agriculture. Rosenstein's important book is a welcome exception, exposing the weakness of many of the rationales offered for the alleged failure of Roman small farmers and their deracination.<sup>221</sup> Significantly, he is able to undermine such claims while working, for the sake of his argument, with a model of the productivity of Roman peasants based on Brunt's decidedly pessimistic views. Brunt, like many of the ancient historians who have offered estimates of Roman agricultural productivity since, assumes extremely low yields (consistent with little or no manuring, weeding, or hoeing), biennial fallow, and concentration solely on inexpensive cereal crops, grown for subsistence, neglecting any role for market gardening, wine, olive oil, tree crops, or livestock, even pigs or barnyard fowl. This is the crude extensive farming of the worst *latifondi* of the Italian Mezzogiorno, stripped even of rough grazing on fallow or stubble fields, not the sort of farming normally practiced by independent small farmers in any culture, except occasionally in conditions of extreme depopulation and isolation or exclusion from markets, or of the direst poverty and social injustice.

As I have argued above, a reading of the Roman agronomists, of our archaeozoological, palaeobotanical, and archaeological evidence, as well as the analysis of peasant farming in cultures comparable to late-republican and early-imperial Italy, reveals a picture markedly different from the grim scenario sketched by Toynbee and Brunt. Roman smallholders remained a vital force in Roman agriculture and society. They continued to own, not just work as tenant farmers or day

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<sup>220</sup> See Prampolini (1981, 92).

<sup>221</sup> See Rosenstein (2004).

labourers, a significant proportion of Italy's arable land. As in northern Italy and the Netherlands, many Roman peasants will have practiced intensive mixed farming, and there is little evidence from studies of English, Dutch, or Western European farming that smallholders need have been any less productive in applying these methods than larger 'capitalist' farms described by Cato, Varro, or Columella.

Given the evidence for the continued viability and vitality of peasant farming in Roman Italy, how does one explain the historiographical tradition of the demise of the Roman peasantry? I cannot address this question here, but we must remember that this is a modern and not an ancient consensus, and an increasingly controversial one. Most of our sources, particularly the Latin writers who were closest to the events, betray little awareness of the existence of an agricultural or social, as opposed to a political crisis. As Astin notes, the most knowledgeable contemporary source, Cato the Elder, seems oblivious to any revolutionary changes in the Roman agricultural economy.<sup>222</sup> Cicero's silence can perhaps be attributed to his ideological stance, but there is also remarkably little to be gleaned from Sallust, a historian of strong *popularis* sympathies and an eloquent critic of the reactionary oligarchy of Sulla. Even those ancient sources which allude most explicitly to a crisis in Roman peasant farming, primarily Appian and Plutarch, stop well short of endorsing the prevailing modern theories. Appian's account of the antecedents to the Gracchan reforms deals most fully with the plight of the Roman rural population, yet it contains little if any information which could not have been extrapolated from the legislation itself, or from the writings of the Gracchi or some of their embittered partisans. Nor is there much evidence elsewhere in his extant writings that Appian, a Greek-speaking professional orator from Alexandria in Egypt who turned to universal history, had any great interest in or knowledge of Roman agriculture or peasant life. References to a Roman agrarian crisis certainly fade from view in subsequent books of his Civil Wars.

Plutarch takes a broader and more sophisticated view, emphasizing the political nature of the crisis, and placing even less credence in a serious agrarian crisis as the underlying cause of the conflict. He does

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<sup>222</sup> See the discussion in Astin (1978, 240–76). Toynbee (1965, 2.296–310) attacks Cato for callously ignoring the plight of the small farmer in his *de Agricultura*, without allowing for the possibility that Cato's apparent complacency accurately reflected the rural economy of his day.

a much better job than Appian in showing how the Gracchi canvassed support from several segments of Roman society with grievances against the senatorial ruling elite. Consequently, in Plutarch's account the violence of the senatorial reaction is more comprehensible. The Gracchan program differed significantly from previous agrarian legislation in that it seemed systematically to attack important interests of the wealthy, rather than simply providing patronage for the poor. It was therefore more radical than any passed since the Licinian-Sextian laws of 367 BC. Nevertheless, opposition to land reform on the part of the rich was not the spark that led to social strife, any more than a desperate need for such reform among the poor. The senatorial elite recognized the serious challenge which a new model of politics based on the aggressive use of tribunician legislation represented, and when defeated through the normal mechanisms of government chose to use violence to reassert their control. The political ramifications of the Gracchan revolution were great, but we must be more cautious in inferring a broader social crisis still so tenuously attested by our ancient authorities.

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# URBANISATION AND DEVELOPMENT IN ITALY IN THE LATE REPUBLIC

Neville Morley

## I. *Cities and Development*

The aim of this paper is to sketch some parameters for future debate on the relationship between the phenomenon of ‘urbanisation’ and other developments associated with the transformation of Roman Italy and the ‘Gracchan crisis’ in the late Republic.<sup>1</sup> This is a surprisingly neglected subject, despite widespread agreement amongst historians that Roman society and culture should be characterised as ‘urban’, especially in comparison with other pre-modern societies.<sup>2</sup> It is clear that there were significant changes in both the numbers and fortunes of urban centres in Italy in the third and second centuries BC, not least as a result of the Romans’ treatment of conquered cities and the establishment of different types of colonial foundations, but this development plays at best a secondary role in accounts of demographic and economic change, which focus above all on the city of Rome. In Hopkins’ famous model, for example, the growth of towns is mentioned as a consequence of the influx of wealth and the displacement of the peasantry, resulting in the expansion of the market for the produce of new slave-run estates; however, apart from his efforts at making the figures for population change and migration add up—his estimate for the scale of Italian urbanisation seems to be based solely on the difference between the most plausible estimate for the size of Rome and the number of displaced peasants who, according to his model, need to be accommodated somewhere in the peninsula—there is no attempt

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<sup>1</sup> I am especially grateful to Luuk de Ligt and the other organisers of the conference for the stimulus to reconsider the subject of Roman urbanisation. I owe special thanks to John Bintliff, Guy Bradley, Michael Crawford, and Paul Erdkamp for their comments in the discussion. I have also benefited greatly from participation in a conference on *Religiöse Vielfalt und Soziale Integration* in Dresden, organised by Martin Jehne, Bernhard Linke, and Jörg Rüpke, which suggested some interesting parallels between the trajectories of economic, social, and religious developments in late Republican Italy.

<sup>2</sup> There is no index entry for ‘city’, ‘town’, or ‘urbanization’ in Flower (2004), for example. On the ‘urban’ nature of Roman society see Alston (2002).

at establishing the relative importance of towns and the metropolis or considering whether their growth might have had different effects.<sup>3</sup>

As key elements of the Hopkins model have been questioned, for example by emphasising the degree to which the villa was a geographically limited phenomenon and reinterpreting the scale and nature of migration, towns have fallen out of the picture, and are scarcely mentioned in recent collections of papers. At best they may be invoked, rather as they were by Hopkins, in the context of hypothetical calculations of the population dynamics of the peninsula; the estimated overall level of urbanisation is offered alternately as proof of the high estimate of population, since otherwise the proportion of Italians living in cities was historically unprecedented and implausibly high, and as proof of the low count, since the annual rate of increase required for the high count is rendered implausible if one takes into account the demands of urbanisation.<sup>4</sup> In such arguments urban centres are simply taken to echo the role of the city of Rome as population sink, just as the Hopkins model assumed that they echoed its role as market for villa produce. The sheer size of the metropolis, perhaps ten or more times larger than its nearest rival in Italy, might seem to justify giving it the lion's share of attention—but it should surely also raise the question of whether the impact of other towns on their immediate hinterlands was the same, on a smaller scale, as that of the capital, or qualitatively different. Equally, even the influence of Rome needs to be understood in the context of the broader phenomenon that was Italian (and Mediterranean) urbanisation and the elaboration of different kinds of networks.<sup>5</sup> While one can make reasonable generalisations about the impact of the capital at a national and even regional level, locally we must always be dealing with the complex interaction between different urban centres, of different natures, sometimes operating in harmony (as local towns played a role in the supply networks of the metropolis) and sometimes in competition.

There are two obvious problems in the study of Italian urbanisation in the late Republic, either of which might account for its relative neglect as a theme. The first and most obvious is the problem of evidence. Whether the size of a city or town is estimated from its built-up area,

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<sup>3</sup> Hopkins (1978).

<sup>4</sup> E.g. in Morley (2001); Lo Cascio (1999); Scheidel (2001) and in this volume, with full bibliography.

<sup>5</sup> Morley (1997).

or from the area enclosed by the city walls, or from the number of recipients of the largesse of local benefactors, the margins of error are considerable.<sup>6</sup> This is clearly illustrated by the degree of variation even in the cases of well-explored and well-preserved sites like Pompeii (from a low estimate of less than 10,000 to a high one of 30,000) or Ostia (from 20,000 to 60,000), and by the fact that, for an idea of the overall level of urbanisation in Italy, historians are still relying on a highly speculative model intended primarily to differentiate between the levels of the urban hierarchy.<sup>7</sup> More recent archaeological work, using sampling techniques to establish the likely population density of an urban centre, promises to narrow down the range of reasonable estimates for population sizes of different categories of centre. However, a critical point is that in most cases this will relate to a single moment in the history of any given site; without extensive excavation we shall continue to have to chart the development of most urban centres using the dates of construction of major public buildings rather than any more direct proxy for population size, while estimates of the overall level of urbanisation relate either to a hypothetical maximum, based on the total number of known cities, or to the second half of the first century AD, when the Elder Pliny listed over 400 cities in Italy. It is thus difficult to relate the process of the expansion of cities, let alone changes in their nature or function, to other processes of social and economic change. The same can of course be said of the growth of the city of Rome, where the rate of expansion up to the time of Augustus is a matter of pure speculation, but at least there the general trajectory is undisputed.<sup>8</sup> The history of Roman intervention in the Italian urban system, founding new centres (some of which flourished while others did not), punishing or restricting existing ones, and altering the conditions within which cities operated (for example, by building roads), means that we can safely assume that the development of urbanisation in Italy in the late Republic was not so straightforward, but we lack the material to chart the process in different regions in any detail.<sup>9</sup>

The second issue is theoretical and methodological, namely that of the definition of the city, or rather the tendency of the debate to

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<sup>6</sup> Nissen (1902, 36–9) and Duncan-Jones (1982, 259–77), but cf. De Ligt's speculative reconstruction of the urban population of Cisalpine Gaul in this volume.

<sup>7</sup> Namely Morley (1996, 181–2); still being employed by Scheidel in this volume.

<sup>8</sup> Brunt (1971, 383–4).

<sup>9</sup> Summaries of the process in Lomas (2004, 207–13) and Patterson (2006).

become bogged down in an entirely unhelpful understanding of the issues involved. An association between the growth of towns and the development of the economy in early modern Europe has long been identified, and this has led many historians to posit a direct causal connection.<sup>10</sup> If the emergence of towns or cities is at least a marker and perhaps also a promoter of economic development, then the identification of whether or not such centres are present becomes a critical question for historians of other periods and societies. However, attempts at developing a cross-cultural definition have foundered, being either too specific (taking the early modern European model as the sole template for urban status) or too general (encompassing centres that manifestly did not have a positive effect on the development of their hinterlands). It is worth noting in passing that the typical Italian city during the Roman period, with a population of a few thousand, would not qualify as a city in studies of early modern urbanisation, and so comparisons of rates of urbanisation in the different periods are entirely meaningless.<sup>11</sup> The alternative approach has been to recognise that not all cities are economically productive, and hence to seek to develop typologies, of ‘generative’ and ‘parasitic’, or ‘producer’ and ‘consumer’ cities, on the basis of more detailed studies of their economic or political structures and role. The debate about whether the ancient, or the Roman, city is appropriately described as a ‘consumer city’ of the ideal type established by Weber and Sombart is familiar in ancient history, and generally recognised to have run into the ground.<sup>12</sup> However, the difficulty of discussing the economic role of classical urban centres without getting drawn back into that debate—as recent discussions of non-agricultural production and of trade in classical antiquity have demonstrated—may account for the reluctance of historians to engage with the part played by urbanisation in the development of Roman Italy.<sup>13</sup>

Three problems can be identified with both the population threshold and the typological approaches to the definition of the city and its relationship with its hinterland. First, they seek to impose a binary

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<sup>10</sup> Theoretical and methodological discussion in Holton (1986). Studies of early modern urbanization in De Vries (1984) and Van der Woude, Hayami & De Vries (1990).

<sup>11</sup> De Vries (1984, 21–2), taking 10,000 as the threshold; cf. Horden & Purcell (2000, 92–3).

<sup>12</sup> Finley (1981), discussed in Parkins (1997) and Scheidel (2007, 80–5). On the historiography of the concept Morley (1996, 14–21).

<sup>13</sup> Compare Parkins & Smith (1998), Mattingly & Salmon (2001).

structure, in which a given centre either is or is not a proper city, on what is inevitably a continuum of urban forms and functions. Such an approach may be sustainable in developing a synchronic analysis of the level of urbanisation in a given society at a given period, but it offers a misleading perspective when attempting to chart development over time; the assumption that an urban centre suddenly becomes economically progressive when it reaches a certain population level, or that a society will take off in economic terms when it possesses a sufficient number of the right kind of urban centres, produces a narrative in which both continuity and discontinuity are hugely exaggerated.<sup>14</sup> Second, such approaches show a tendency to assume that the early modern European city is ideal in more than a Weberian sense, and that other cities can be expected to be economically and socially productive to the extent that they match that template. Hence Finley's insistence that the ancient city could not be productive because it corresponded to the 'consumer' ideal type, and the equally misleading insistence of his opponents that they could prove that it was productive by identifying the presence of urban crafts and trade; in both cases the argument is intended to establish the degree of similarity to the early modern template, on the assumption that this will determine the economic impact of the urban centre. This kind of urbanism, it has been suggested, is "a myth in the strictest sense", "an ideology of modernity ethnocentrically identified with the crystallization of the social forms of liberal capitalism".<sup>15</sup>

This leads on to the third problem with seeking to define 'the city', namely the underlying assumption that the city (or a particular type of city) is an independent social object acting upon the society in which it is located. "The town as a physical object is turned into a taken-for-granted social object and a captivating focus of attention in its own right."<sup>16</sup> We have a cultural predisposition, it is clear, to associate cities with modernisation and to regard them as agents of change, but in understanding economic and social developments, in early modern Europe or elsewhere, it makes more sense to focus on wider processes of change—the division of labour, economic specialisation, and the expansion of the market—rather than restricting our focus to the city or regarding it as the source, rather than as one manifestation, of these

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<sup>14</sup> Cf. Horden & Purcell (2000, 93).

<sup>15</sup> Castells (1976, 70).

<sup>16</sup> Abrams (1978, 9).

processes. Exclusive concentration on the ‘city’ is an example of the fallacy of misplaced concreteness; it is, as Whittaker argued for the ancient world, “only an imperfect way of studying the operations of power in society”.<sup>17</sup>

## II. *Processes of Urbanisation*

This might suggest that historians’ neglect of the place of urbanisation in the history of republican Italy has, inadvertently, saved us from a range of misconceptions; attention has focused on wider social and economic processes, such as demographic change and the reorganisation of the countryside, rather than being distracted by the epiphenomena of city foundations and development. However, the call from sociologists like Abrams for historians to abandon the town or city as a concept is not intended to suggest that towns and cities should henceforth be ignored altogether; rather the tendency to take cities for granted as real and important social objects should be “replaced by a concern to understand towns as sites in which the history of larger systems—states, societies, modes of production, world economies—is partially, but crucially, worked out”.<sup>18</sup> The city is not the only manifestation of and location for wider processes of change, but it may be a crucial location; its development is often one of the more visible products of change, and hence a useful barometer, but it may take on a still greater significance as the space where different processes come together and interact, whether to reinforce or oppose one another. Further, we need to consider how far the particular nature of urban space may in turn have influenced the trajectory of those processes, and the ways in which different social groups might seek to manipulate or control that space as a means of accumulating social power.<sup>19</sup>

This emphasis on the role of the city as a site where wider developments in late republican Italy are (partially but crucially) worked out might seem to invite further discussion of questions of definition, despite the problems discussed above. I want to focus rather on urbanisation as an ongoing and variable process that is the continually-reshaped product

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<sup>17</sup> Whittaker (1993, 15).

<sup>18</sup> Abrams (1978, 10).

<sup>19</sup> These ideas are heavily influenced by the sociology of Mann (1986) and Harvey (e.g. 1985; 2001).

of the confluence of a broader set of processes. The fact that we can, within more or less any given historical context, identify particular sites as being worthy of the label 'urban' is in part a matter of reading the past through our own prejudices; however, it suggests that there is at least a possibility that we can identify cross-cultural and transhistorical constants; relating not to the nature of the urban site itself but to the processes which it embodies. That is to say, urban centres do resemble one another, despite all the manifest differences between them even within the same historical context, because they come into being and develop as the result of similar developments within their society. This approach owes a great deal to recent archaeological discussions of urbanisation and the emergence of social complexity in early Iron Age societies, and suggests that it is appropriate to characterise the emergence of nucleated settlements of less than a hundred people and the growth of cities of tens and hundreds of thousands of inhabitants in the same way.<sup>20</sup> At very different scales, from the local to the regional and the supranational, and in quite different (and continually modified) contexts, we are concerned with the material product of the interaction of four critical processes.

*Concentration.* The concentration of people at a specific location, which might equally be termed 'nucleation', but also the concentration of resources, is manifested above all in investment in the built environment. In so far as this model of urbanisation might be employed for other periods and historical contexts, there is scope for argument about whether the concentration of population needs to be permanent or whether temporary cities of nomads, focused on oases and periodic markets, might hold the same significance. For the case of Roman Italy what matters is that concentration is always relative to its context; rather than focusing on a magic threshold of populations of 1,000 or 10,000 within a limited area, *any* shift in the distribution of population between nucleated centres and the countryside and/or between different nucleated centres is of interest. In addition to its interaction with other processes, the concentration of population has two clear implications for economic and social structures: the necessity of investing resources in transporting food and other supplies from their place of production to their place of consumption, even if the same individuals are consuming

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<sup>20</sup> E.g. Osborne & Cunliffe (2005).

what they produce, and the demographic consequences of increased opportunities for the spread of different diseases in a nucleated rather than a scattered population.

The measurement of concentration is, as discussed in the previous section, problematic; ideally we should require information on the numbers and the range of sizes of nucleated centres and the proportion of the total population living in nucleated centres at any given time. However, the advantage of an emphasis on concentration as a process is that absolute figures matter less than change over time; it is equally significant whether a previously scattered population is moving into villages of a few hundred people or the balance between small towns and the metropolis is shifting in favour of the latter, and either of these developments is more likely to be archaeologically visible, sufficient to allow us to make qualitative judgements on the relative extent of concentration between periods. We can hope in the future to be able to draw upon detailed studies of changing settlement patterns in specific areas, not least because the preliminary indications are that there are significant differences according to local circumstances; contrast the apparent disappearance of the colonial foundation of Cosa with the development of new centres in Samnium.<sup>21</sup> Across Italy as a whole there is an impression of increasing concentration during the late Republic, not only through the ongoing expansion of Rome but also through the development of new centres in areas, like Samnium, previously characterised by more scattered settlement patterns, along with evidence for increases in the size of some existing centres. Many significant questions remain, however, not least whether growth of centres outside Rome was proportionate to growth of the capital—as one might expect on the basis of comparative evidence showing that migration is usually stepwise—or whether this period sees changes not only in the overall level of concentration but also in its pattern, with the distribution of people and resources increasingly biased towards the centre of the urban system.

*Crystallisation.* Arguably this is a better term than the obvious alternative of ‘centralisation’, emphasising that the progressive concentration in specific locations of power and the institutions through which it is medi-

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<sup>21</sup> See Patterson (2006).

ated and exercised is not necessarily a directed or deliberate process.<sup>22</sup> There is a clear tendency in late-republican Italy for political, social, religious, and cultural institutions to become located in the same space at the heart of the urban centre, to the extent that cities can be seen as machines for establishing and wielding power—which would explain the willingness of elites to invest resources in developing them. The urban centre mediates power at different levels; it may serve not only as a means of exerting control (politically, economically, culturally, and symbolically) over the territory surrounding it, but also as a means for higher orders of power to control larger regions; seen above all in the way that Rome extends its influence across Italy through the network of urban centres of colonies and allies and the lines of communication established between them. This then suggests two different but closely related fields of enquiry: the means by which control is established over a territory via the institutions of the city (generally, of course, this is not a question of an urban elite dominating the countryside but of a single elite exercising its power above all through urban institutions), and the means by which Roman control is extended over wider regions and the empire as a whole, both formally and informally (for example, through networks of kinship, patronage, and other forms of obligation, maintaining links between different levels of the urban system).<sup>23</sup>

The most obvious consequence of the crystallisation of power in the urban centre is the investment of considerable resources by the elite in an elaborate built environment, both to enhance the effectiveness of the city as a means of control and influence and as a result of the city becoming the arena for competition within the elite. Thus crystallisation promotes the concentration of population and resources, attracted by the possibility of employment in the building and service industries and of gaining a share of the elite expenditure. In turn, the concentration of population may increase the influence of the elite, or at any rate influence the means employed in the exercise of power, as it proves more effective to work through mass patronage with banquets, donations, and public building works than through the individual patronage more characteristic of traditional rural social relations. The rewards for control of economic, political, and economic institutions are clearly greater the more individuals participate in them.

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<sup>22</sup> On crystallization see Eisenstadt & Shachar (1987, 68–74).

<sup>23</sup> Cf. Morley (1997) and references.

It is relatively straightforward to take the construction of public buildings in urban centres as a proxy for the centralisation of political and other institutions; thus the process of crystallisation is easy to chart in the late Republic in areas like Samnium, where the elite moved to adopt the conventional model of the urban centre as political centre.<sup>24</sup> It is less visible in regions that were already urbanised and where most centres already possessed a full complement of public buildings. In order to determine whether crystallisation had reached its limits in regions like Latium and Campania, or whether it was simply less dramatic, we need to look for alternative indications, for example the disappearance of terracotta votives and many associated rural sanctuaries after the third century BC, suggesting the crystallisation of religious power in fewer separate locations (many of which were now urban), or the inscriptions that suggest that the *decuriones* of Veii came to meet in the Forum of Caesar in Rome.<sup>25</sup>

*Integration.* This is closely related to the consolidation of elite control over territory and population, but is clearly a separate process. It can take a range of different forms, many of which are mutually reinforcing. Political integration, drawing ever larger numbers of people into participation in the same or similar political institutions, or at any rate subjecting them to the same laws and coercive forces, establishes similar relationships between mass and elite across Italy. Social and cultural integration brings about the erosion of differences of language, customs, and material culture, establishing similar habits of eating, dress, and behaviour; it fosters the gradual development of a social identity beyond that of kinship and, in later periods, an Italian or Roman identity rather than one focused entirely on the local area.<sup>26</sup> Economic integration, with the establishment of common means of exchange and legal frameworks and increased traffic between individuals and regions, led to increasing numbers of people dependent upon systems of redistribution rather than being primarily self-sufficient, and to an increased dependence of cities and even regions on wider market networks. The urban centre plays a key role in all of these developments, as the location of the main political, cultural, and economic institutions and the

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<sup>24</sup> Patterson (1991).

<sup>25</sup> On votives and cult sites, see Comella & Mele (2006); on the *decuriones*, Purcell (1983).

<sup>26</sup> See Dench (1995) on developments in the central Apennines.

place where individuals—visitors as well as permanent residents—were most likely to encounter, and to be encouraged to adopt, new customs, language, ideas, and norms. The city was clearly not the only institution promoting the integration of Italy—the army was at least as important, for example in the spread of Latin and the adoption of coined money—but it must have been an important one. Besides the declining number of inscriptions in languages other than Latin, and the evidence for the spread of Roman material culture, it is difficult to get beyond the literary evidence, which clearly shows (and promotes) the development of an integrated Italian elite but whose relevance to the mass of the population may be questioned.<sup>27</sup> It is easier to show the development of economic integration by charting the patterns of distribution of goods like imported pottery and wine amphorae, and also by using the evidence of the *nundinae* tablets from Campania, which shows that markets in different towns, including Rome, were increasingly interconnected.<sup>28</sup>

*Differentiation.* Again this is a broad category covering a number of different but related processes. Economic differentiation goes hand in hand with economic integration. Individuals become integrated into the market system and increasingly dependent on the economic activities of others because economic roles are increasingly differentiated: producers begin to specialise rather than concentrating solely on achieving self-sufficiency, most obviously in urban centres, where individuals may focus entirely on secondary or tertiary industry, but also in the countryside with increased involvement in the market and changing patterns of production in response to market incentives.<sup>29</sup> Limited regional specialisation also becomes possible as a Mediterranean-wide network of markets and information emerges, above all in response to the growing demands of the metropolis and the army.<sup>30</sup> Differentiation supports the increased concentration of population in larger centres, which might be prohibitively expensive if all urban inhabitants continued to farm and had to walk out to their fields every day.

Political differentiation sees the emergence of elites and their growing separation from the masses, with the elaboration of political institutions

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<sup>27</sup> See generally Lomas (1993; 2004).

<sup>28</sup> Morley (1996, 166–74).

<sup>29</sup> De Ligt (1990; 1991).

<sup>30</sup> Morley (2007).

providing a formal basis for exercise of power, and the development of a rhetoric and ideology that offers the less formal basis.<sup>31</sup> At a higher level there is the process of differentiation within the elite, broadly defined; as Italian politics becomes more integrated, hierarchies inevitably develop, dividing Roman and provincial, established family and new man. Such distinctions are reinforced by social differentiation, expressed through material practices in the form of luxurious residences and ostentatious lifestyle, performed above all in the urban centre. They are also reinforced by cultural differentiation, with the establishment of a divide between town and country and the appropriate forms of behaviour for each context, and the ever more elaborate sets of rules and expectations governing elite activities—including, as Habinek has argued, the practice of discrimination (differentiation) itself.<sup>32</sup>

Once again this process is fairly easy to chart for the political elite, whose writings discuss at length competition within and between different levels of the hierarchy and the relation between the elite and the masses; clearly, however, this is neither a complete nor a neutral perspective. Similarly, discussions of the cultural and ideological sphere are themselves fully implicated in the processes of social differentiation and competition for which they provide the evidence. The range of evidence for economic differentiation and specialisation is wider; lists of occupations recorded in inscriptions, not only from the capital, show increasing division of labour and specialisation of tasks in at least some urban centres, while there is extensive evidence for professional merchants and developing structures of trade and distribution, and a reasonable quantity for increased specialisation in agriculture, especially but not only from the farms of the elite.<sup>33</sup>

### III. *Contradictions and Conflicts*

In summary, a wide range of evidence suggests that late-republican Italy experienced significant developments in all four of the processes associated with and frequently located within urban centres: increased concentration of population and resources, ongoing crystallisation of political, social, economic, and ideological power, increasing integra-

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<sup>31</sup> Millar (1998), Mouritsen (2001), Morstein-Marx (2004).

<sup>32</sup> Habinek (1998, 34–68); on city and country: Braund (1989).

<sup>33</sup> See e.g. Kehoe (2007), Morley (2007).

tion, and increased differentiation in all spheres of society. There is a clear risk of this picture turning into the invariably positive view of urbanisation as a straightforward phenomenon of development, both manifestation and cause, but this time all-encompassing—the agent of political and cultural change as well as economic and social. The extent to which the different processes seem frequently to reinforce one another, as the growth of cities promoted integration and specialisation, and economic integration promoted crystallisation and concentration, increases this risk. It is important therefore to take into account various countervailing tendencies; and this may in fact offer ideas on how to relate this general model of urbanisation to the specific context of Roman Italy and the Gracchan crisis.

First, the processes of urbanisation are clearly not indefinite; indeed in a pre-industrial context they may rapidly run up against the limits of ecology, technology, and demography. Excessive concentration of population could become unsustainable as supplies needed to be sourced from ever more distant regions; even the growth of Rome eventually came to a stop, and no other centre possessed either the resources or the power to imitate its Mediterranean-wide hinterland. Similarly, the progress of economic specialisation and integration was limited by the productivity of agriculture and the speed and cost of transport, while the progress of crystallisation and integration in the political, social, and cultural sphere was constrained by the speed of communication and, arguably, by cultural resistance to the excessive dominance of Rome and its values.

Second, the four processes involved in urbanisation did not necessarily proceed in step with one another. Certainly it is possible to identify cases of the crystallisation of political institutions with only limited degrees of concentration or integration, or of concentration of population without obvious signs of crystallisation. This represents a clear warning against taking evidence for one aspect of urbanisation as proxy for others, for example taking changes in public buildings as a straightforward indicator of population change or growth, or the development of nucleated centres as evidence for increased division of labour. However, it is also possible that processes might be not only mutually supportive but also mutually dependent, so that limited development in one area acted as a brake on development in another. The Italian urban system remained one organised around a large number of relatively small centres focused on Rome, rather than developing into a more mature and integrated system; without wishing to assume that all

urban systems ought to develop along the same lines as that of early modern Europe, the roots of this history still seem worth exploring.<sup>34</sup> It seems equally possible that overdevelopment in one area might create problems elsewhere; the upheavals of the late Republic seem, at least to some extent, to be connected to the concentration of population in urban centres before either the political institutions or the economic structures had developed sufficiently to sustain and manage this.

Third, the different processes were not necessarily supportive at all, but on the contrary were permeated with the potential for conflict and contradiction. There is an obvious tension between the processes of integration and differentiation: the widening separation of mass from elite and town from country might work against the process of eroding differences in the adoption of a common way of life or political and social identity. The political integration of the peninsula turned individual local problems into a single national problem, creating conditions where a Gracchus could seek to win popular support through the same networks and structures that the traditional elite used to exert control, while the possibility of a more or less united Italian front against Rome was essentially created by the efforts of Rome to integrate Italy into a single system. Tensions were equally likely between developments in the economic sphere, where differentiation and integration clearly could work together, and the social or political sphere, where the consequences of economic change created problems for institutions. The system was built upon competition, not only within the elite but between different levels within the emerging system; different interests might prevail at local, regional, or national levels, while the growth of some centres would often be at expense of others (most obviously in the case of Rome), competing for the same resources or influence.

This leads to the fourth qualification of the optimistic perspective: these developments were not necessarily beneficial at all, but above all they were not consistently or universally beneficial. The transformation of Italy was clearly not a directed or planned process, but equally it was not a completely spontaneous process; it was, at least in part, the consequence of deliberate attempts by some sectors of society to exert and increase their power over others. Increasing economic differentiation and market integration can equally be seen as increasing the vulnerability to market uncertainties of an ever larger proportion

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<sup>34</sup> Cf. Rozman (1978–9).

of the population, with merchants and especially wealthy landowners benefiting at their expense. Social differentiation involved the articulation of the idea of lower status and poverty, so that certain groups were now defined in terms of their exclusion, shame, and vulnerability—a phenomenon, and political problem, that tended to be associated with urban centres rather than the countryside.<sup>35</sup>

There is a conventional historiographical contrast between the crisis-ridden late Republic and the stable Italy of the Principate. One interpretation of this contrast is that by the second century AD Italy had attained a functional equilibrium in which different parts of the system worked in harmony and were not greatly disturbed by exogenous factors, whereas republican Italy was undergoing far-reaching structural change promoted and aggravated by external factors. The Gracchan crisis stands then as the critical moment of a political response to barely-understood symptoms of change. The aim of this paper is not to establish a new model of the city as an independent social agent driving forward the development of the Italian economy, but rather to focus attention on these long-term structural changes, and above all the interaction and conflict between the different processes which both took place within and produced the Italian urban system.

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<sup>35</sup> Morley (2006).

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## THE POPULATION OF CISALPINE GAUL IN THE TIME OF AUGUSTUS<sup>1</sup>

Luuk de Ligt

In order to put the arguments of this article into a realistic perspective, I want to start by making it clear that we do not know and shall never know for certain how many people lived in Cisalpine Gaul during the late Republic and early Empire.<sup>2</sup> In theory, then, this could be the shortest article on ancient demography ever written. The reason why I have nevertheless decided to devote a short piece to this seemingly unpromising topic is quite simply that there are, in my view, many interesting things to say about the population of Cisalpine Gaul that have never been said before. It is also my contention that even though the new considerations that will be put forward in this paper do not in any way prove a low-count interpretation of Italy's demographic history to be correct, they at least highlight some difficulties in the high count that have not received the attention they clearly deserve.

My attempt to shed new light on these issues will concentrate on the shape of the urban network and on the size of the aggregate urban population. I shall begin by looking at the physical size of the towns of Cisalpina and by examining some of the variables that are likely to have influenced the number of town-dwellers per hectare. My next step will be to discuss briefly the problem of urbanization rates. In theory, if it were possible for us to recover both the approximate number of

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<sup>2</sup> For an interesting discussion of the demographic make-up of early-imperial Cisalpina from a high-count perspective see Kron (2005). For developments between 225 BC and 28 BC see Bandelli (1999).

town-dwellers ( $POP_{urb}$ ) and the overall urbanization rate ( $URB.RATE$ ) for Cisalpinia, the overall population of the North could be extrapolated from the urban population, using the following formula:

$$POP_{tot} = (100 : URB. RATE) \times POP_{urb}$$

If we could put an approximate figure or even a range of approximate figures on average urban population densities in the North ( $DENS_{urb}$ ), the size of the urban population could be calculated by multiplying the total number of urban hectares ( $HECT_{urb}$ ) by the average number of town-dwellers per hectare. The next step would be to look at the overall urbanization rate in Cisalpine Gaul. If it were possible for us to put an approximate figure on this third variable as well, the population of the North could be extrapolated from the urban population using the following formula:

$$POP_{tot} = (100 : URB. RATE) \times HECT_{urb} \times DENS_{urb}$$

Finally, if we also had a rough idea of the number of slaves in the North, the size of the free population could be calculated using the following formula:

$$POP_{fre} = PERC_{fre} \times (100 : URB. RATE) \times HECT_{urb} \times DENS_{urb}$$

In what follows I shall demonstrate that of the four variables contained in this formula the number of urban hectares can be reconstructed with a fairly high degree of confidence. Unfortunately, we have very little information on urban population densities, and even less on urbanization rates and slave numbers. It is precisely for this reason that the size of the population of the North cannot be accurately determined. However, it is my contention that a systematic discussion of the four variables just mentioned helps us to see more clearly the startling contrast between the two reconstructions of Cisalpine Gaul implied by the low count and the high count.

### I. *The physical size of the northern towns*

Let me start with what I have come to regard as the least problematic variable, the number of urban hectares in *regiones* VIII to XI in the age

of Augustus.<sup>3</sup> My first step in approaching this seemingly unrewarding topic was to create three different categories, one comprising the most important urban centres, another comprising centres of secondary importance, and a third comprising all remaining agglomerations of urban status. My next step was to assign each of the 78 towns of Cisalpina to one of these three groups. In doing so, I took into account various indications contained in the literary sources. Three examples of this are Strabo's use of the phrase *polis axiologos* in regard to Mediolanum and Dertona, and his well-known characterization of Patavium as a wealthy and populous city.<sup>4</sup> He also calls Verona 'a large city' (*polis megalé*), lists Placentia, Cremona, Parma, Mutina, and Bononia among the 'famous towns' (*poleis epiphaneis*) of the North, but uses the term *polismata* (small towns) for Opitergium, Concordia, Atria, Vicetia, Regium Lepidum, Claterna, Forum Cornellii, Faventia, and Caesena.<sup>5</sup> Unfortunately, we cannot always be sure that Strabo's classifications and descriptions are valid for the late Republic and early Empire. At least in some cases he can be shown to have missed or neglected recent developments, such as the expansion of Forum Cornellii and the establishment of veterans at Ateste after 30 BC.<sup>6</sup> However, even if some of the information provided by Strabo is demonstrably out of date, it remains the case that many of his classifications are accurate for the time of Augustus. I have also used some later sources, such as Pliny the Elder's list of the notable towns (*nobilis oppida*) of northern Liguria, and Tacitus' statement that Mediolanum, Novaria, Eporedia, and Vercellae were 'the strongest of the Transpadane towns' (*firmissima transpadanae regionis municipia*).<sup>7</sup>

Although these impressionistic clues shed some light on the relative importance of many northern towns, they do not of course allow us to put any figures on their physical extent. Fortunately, this problem can easily be resolved with the help of the many topographical studies on

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<sup>3</sup> For an excellent survey of the evolution of the urban network of Cisalpina up to the end of the Social War see Bandelli (2007).

<sup>4</sup> Str. 5.1.6, 5.1.11, 5.1.7.

<sup>5</sup> Str. 5.1.6, 5.1.8, 5.1.11.

<sup>6</sup> Chilver (1941, 54) infers that "Strabo is drawing on information about conditions in the region before the principate of Augustus, indeed before the colonizations of the triumviral period." Cf. the data assembled in Appendix I.

<sup>7</sup> Plin. *Nat.* 3.49: Libarna, Dertona, Iria, Vardacate, Industria, Pollentia, Potentia, Forum Fulvii, Augusta Bagiennorum, Alba Pompeia, Hasta, Aquae Statiellae; Tac. *Hist.* 1.70.

the cities of Cisalpine Gaul that have been published during the past fifty years. Many of these studies give a precise figure for the extent of the inhabited area or contain maps from which an approximate figure can be derived. In other cases we are given information only on the number of hectares enclosed by the town walls. In these cases I have assumed that the entire walled area was inhabited. One reason for this is simply that the scattered evidence we have does not support the view that a large proportion of the areas enclosed by the town walls of the North was not built-up. My personal impression is that in this respect the towns of Cisalpine Gaul were more compact than many of the cities of Etruria and Magna Graecia during the archaic and classical periods.<sup>8</sup> It seems significant that several towns in the North acquired suburbs in the early-imperial period.<sup>9</sup> If there were large empty spaces within the town walls, one would expect most of these to have been built up before sizeable extramural quarters started to develop. In any case, in assessing the merits of the high count for North Italy we should try to avoid minimizing the urban population by assuming—without good evidence—that a significant proportion of the areas enclosed by town walls did not have buildings. In other words, even in the absence of conclusive evidence, it seems advisable to assume that the entire walled area was built up.

If we apply these ideas to the extensive body of literature on the towns of the North, it is possible to put an exact or at least a rough figure on the size of 61 northern towns, making up some three-quarters of the total. In many cases the impressionistic indications supplied by the literary sources are confirmed. One example of this is Patavium. In his well-known article on the size and population of Greek and Roman cities Beloch gave Patavium 85 ha, on the assumption that only the area enclosed by the two branches of the river Meduacus was built up during the early Empire. Later research has revealed this assumption to be incorrect. In reality there was a substantial built-up area to the east of the central 'island' which may have comprised a further 40 or 45 ha. On this view, early-imperial Patavium would have covered some 130 ha, confirming Strabo's statement that it was 'the best of all cities'

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<sup>8</sup> Chevallier (1983, 149): "Même si les plans d'urbanisme, conçues largement, prévoyaient des extensions futures à l'intérieur des murs, la totalité de l'espace urbain à été en general garnie."

<sup>9</sup> Chevallier (1983, 149).

in the North.<sup>10</sup> Another important centre was Mediolanum, whose town wall enclosed some 80 ha in the late Republic and early Empire. This confirms Strabo's description of Mediolanum as an *axiologos polis*. It may be noted in passing that the figure of 133 ha given by Beloch refers to the area enclosed by the longer town wall of the late third century AD.<sup>11</sup> A third example is Bononia, which covered some 50 ha in the age of Augustus.<sup>12</sup> This time Beloch's estimate, 83 ha, turns out to be too high. However, even with 50 ha Bononia remains one of the largest centres in the North, confirming Strabo's statement that it was among the 'famous' cities of the North.

If we define the most important towns of Cisalpine Gaul as those covering 40 or more ha, we end up with 15 very important towns. It is striking to find that the existing archaeological literature permits us to put an exact or approximate figure on the size of all of these towns. The reason for this must be that archaeological research in the North has been biased towards the larger centres. The average number of hectares per town is 59.9, the total number of hectares is 898.4.<sup>13</sup>

My second category comprises those cities which are known to have covered or are likely to have covered between 20 and 40 ha. Interestingly, three of the four *firmissima municipia* mentioned by Tacitus fall into this category, suggesting that even towns half the size of Mediolanum were regarded as substantial. All in all, 29 towns can be assigned to this category on the basis of their physical extent. To these 29 towns I have added Ateste and Atria. Ateste is poorly documented but is known to have received a substantial body of colonists after the battle of Actium. Atria possessed a theatre and a substantial amphitheatre, suggesting that it was far from negligible. In estimating the physical extent of the towns making up my second category I have given each of these towns 27.6 ha, the average for the 29 towns for which we have secure evidence. The total number of hectares for my second category is 854.3.<sup>14</sup>

Finally, we get to the lowest tier in the urban hierarchy, for which I have used 19.9 ha as an upper limit. The sizes of 17 of these smaller centres can be determined with a reasonable degree of confidence. They range from 2 ha in the case of Forum Novum to c. 15 ha in the case of

<sup>10</sup> Str. 5.1.7.

<sup>11</sup> Beloch (1886, 487).

<sup>12</sup> References in Appendix I.

<sup>13</sup> Appendix I.1.

<sup>14</sup> Appendix I.2.

Feltria and Potentia. The average for the towns whose physical extent can be determined is 10 ha. If we apply this figure to those centres for which no data are available (sometimes because their locations are unknown), we end up with a total of 320 ha for the smallest towns.<sup>15</sup>

Before proceeding with my argument, I want to draw attention to the fact that the foregoing analysis refers solely to those settlements which were ‘towns’ in a juridical and administrative sense. It does not take into account the numerous *vici* which must have existed. In this context a comparison with the settlement system of North Italy at the beginning of the seventeenth century is instructive. In this period the region appears to have had *c.* 60 cities and towns having 5,000 inhabitants or over, but alongside these larger centres there were between 150 and 200 small towns having populations of between 2,000 and 5,000.<sup>16</sup> Similarly, there must have been several large *vici* and numerous smaller lower-order settlements in the territories of each of the 78 ‘towns’ of Roman Cisalpina. In fact, a considerable number of large *vici* covering between 5 and 15 hectares have been located.<sup>17</sup> If these centres had 150 persons per hectare (cf. below), their populations would have ranged between 750 and 2,250. One lesson to be drawn from this is that at least some *vici* were bigger than some of the ‘unimportant towns’ making up the third tier of my urban hierarchy. Another is that a functional understanding of the settlement system of the Roman North can only be achieved if we take these lower-order centres into account.<sup>18</sup>

<sup>15</sup> Appendix I.3.

<sup>16</sup> For the large towns of northern Italy in the early-modern period see Appendix II; for the number of small towns see Musgrave (1995, 254–255).

<sup>17</sup> See e.g. Zaccaria (1979), Strazzulla Rusconi and Zaccaria (1984), Gregori (1993), Maggi and Zaccaria (1994; 1999), Sena Chiesa (1995; 2003), Arnaud (2004; 2007), Grassi and Slavazzi (2007), Barra Bagnasco and Elia (2007), Ventura and Cividini (2007), Ambrosini (2007), Spagnolo Garzoli (2007), Janke (2007).

<sup>18</sup> I am grateful to John Bintliff for helpful comments on this point. For a useful discussion of what a fully functional settlement system may have looked like see Bintliff (2002). Unfortunately, the archaeological data presently available offer no sound basis for estimating the proportion of the ‘rural’ population living in the many lower-order central places that must have existed. It may, however, be noted that in the early-modern period the vast majority of the rural population of North Italy lived within walking distance of the 60 large and *c.* 200 small towns which provided ‘urban’ goods and services, including administration (Musgrave 1995, 255). If the total number of settlements performing central-place functions was roughly identical in the early Empire, there would have been some 214 (260 minus 46) small towns and large *vici*. Of these smaller settlements the 32 ‘unimportant towns’ of Cisalpina appear to have covered *c.* 10 ha on average. If the remaining 182 centres also covered 10 ha on average, and if they had 150 inhabitants per hectare, the total population of the hypothetical

At the same time my list of 78 towns is too long in the sense that it includes many settlements which would never be classified as 'urban' in a late-medieval or early-modern context. In his study of the urban network of early-modern Europe, Jan de Vries applies the label 'town' only to those centres having 10,000 inhabitants or over.<sup>19</sup> In my view, only a few towns in early-imperial Cisalpina fulfilled this criterion. In other studies focusing on late-medieval or early-modern towns the critical threshold is 5,000 or 3,000.<sup>20</sup> In the case of late-medieval and early-modern Italy we cannot go below the latter threshold, for the simple reason that there are no reliable lists of settlements having fewer than 3,000 inhabitants. If those towns meeting this threshold had urban population densities of approximately 150 persons per hectare, they would have covered *c.* 20 hectares.<sup>21</sup> It follows that if we want to make a rough comparison between the sizes of the urban populations in Roman and early-modern times, all Roman settlements which were towns in a juridical sense but covered less than 20 hectares must be classified as non-urban. If we apply this criterion, we are left with 46 towns in early-imperial Cisalpina which can be compared to the *c.* 63 northern 'towns' which had populations of 3,000 or over at the start of the seventeenth century AD.<sup>22</sup>

If we add up my estimates for these 46 towns, we obtain an estimate of 1752.7 urban hectares in Cisalpine Gaul. Since some of the underlying data are rough approximations, there can be no doubt that there is a considerable margin of error. It must, however, be emphasized that my estimate of the number of urban hectares in the North is almost certainly too high rather than too low. As I have already pointed out, I have consistently assumed that the areas enclosed by the town walls of the North were entirely built up. Second, although my analysis focuses on the size of the North-Italian population in 28 BC, my list of northern towns includes some urban centres, such as Augusta Praetoria, which

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214 lower-level centres would have been 321,000. This is only one sixth of the rural population implied by the low count for Cisalpine Gaul (cf. below). In other words, even in a low-count model for Cisalpina there is room for a very large number of non-urban central places.

<sup>19</sup> De Vries (1984).

<sup>20</sup> E.g. Bairoch (1988).

<sup>21</sup> Of course, it is precisely for this reason that I have chosen 20 hectares as the lower limit of my second category of towns.

<sup>22</sup> Appendix II. Since not all towns having populations of 3,000 or over appear in Bairoch (1988), the figure of 63 is to be regarded as a minimum figure.

either did not exist or did not have the citizenship in that year.<sup>23</sup> Third, my estimates refer to the physical extent of the towns of Cisalpine Gaul in 28 BC. However, since most of the archaeological data cannot be dated very precisely, all increases in the size of towns during the Julio-Claudian period are included in my estimates. For all these reasons it seems highly unlikely that the 46 towns which make up the first two tiers of my urban hierarchy covered more than 1752.7 hectares in 28 BC.

Regardless of the margins of error to which I have alluded, this crude figure is of considerable interest. As is well known, the high count for late-republican Italy assigns a population of *c.* 6 million to the North. This is higher than the figure for AD 1600, when the regions corresponding to Roman Cisalpina were inhabited by some 5.4 million people.<sup>24</sup> Of these 5.4 million inhabitants 1.17 million, or 21.7%, lived in cities and towns having a population of 3,000 or over. For the limited purposes of this section I want to draw attention to some data concerning the size of the northern cities in the early-modern period. From the mid sixteenth century onwards, Milan, Venice, and Bologna appear to have been the largest centres in terms of physical size, with *c.* 794, 600, and 419.5 ha respectively.<sup>25</sup> Alongside these giant cities there were many other substantial towns, such as Brescia, Cremona, Ferrara, Genova, Mantova, Padova, Piacenza, Verona, and Vicenza. Of this second group of cities Ferrara, Genova, Padova, Piacenza, and Verona covered areas of 340, 155, 450, 290, and 380 ha respectively.<sup>26</sup> The total for these five cities plus Milan, Venice, and Bologna is *c.* 3,428.5 ha. In other words, eight large cities in the North, accounting for roughly 45% of the urban population in the sixteenth century,<sup>27</sup> were twice as

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<sup>23</sup> Another possible example is Augusta Taurinorum, which may have been founded in 27 BC (e.g. Chilver 1941, 201).

<sup>24</sup> Beloch (1937–61, III.352), followed by Jongman (1988, 72) and by Lo Cascio and Malanima (2005, 14). Del Panta et al. (1996, 275) assume a total population of 12.5 million (instead of Beloch's 13.3 million) for Italy as a whole (including the islands) around 1600.

<sup>25</sup> Milan: 794 ha in 1565 according to Beloch (1937–1961, III.175); Venice: 600 ha according to Benevolo (1980, 600); Bologna: 419.5 ha in the early fourteenth century according to Beloch (1937–1961, II.91).

<sup>26</sup> Chandler and Fox (1974, 85): Ferrara 340 ha in 1500; Heers (1961, 45): Genova 155 ha in 1450; Benevolo (1980, 326): Verona 380 ha in the fourteenth century, Piacenza 290 ha in the fourteenth century, Padova 450 ha within the Venetian walls of the fifteenth century.

<sup>27</sup> Appendix II.

large as the combined 46 large and medium-sized towns of Cisalpine Gaul in the early Empire. These figures suggest that the total number of urban hectares in North Italy in the early-modern period must have been at least 7,000, which would be roughly four times higher than the estimated figure for 28 BC.<sup>28</sup> In fact, even if we assume (without good reason) that I have underestimated the total area covered by the large and medium-sized towns of Roman Cisalpina by as much as 500 hectares, the figure for early-modern Italy would still be more than three times higher than that for 28 BC. Unless we assume that urban population densities were three or four times higher in Roman times than they were during the Renaissance, it follows that the high count for the North can be maintained only by positing a Roman urbanization rate far below the early-modern level.

## II. *Urban population densities*

The second variable in my formula is the number of people per urban hectare. At first sight, any attempt to put a figure on this variable seems doomed to failure. The main difficulties can be summarized as follows. There can be no doubt that in many Italian towns urban population densities varied over time. This problem is especially acute if we base our calculations on the amount of space enclosed by town walls. As many scholars have observed, Greek, Etruscan, Oscan, and no doubt Roman towns often contained empty spaces which were built up during a later stage of development. Moreover, even if we assume that the entire area enclosed by the walls of an ancient town was indeed built up, it was of course entirely possible for urban population densities to increase over time, for example because new buildings were fitted in among existing ones, or because it became more common to build houses with two or three storeys.<sup>29</sup> A combination of these developments is known to have taken place in republican Pompeii, where we

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<sup>28</sup> It follows that Kron (2005, 474–5) is quite wrong in suggesting that the urban population of North Italy in Roman times may have equalled that of the later Middle Ages.

<sup>29</sup> This development is known to have taken place in many cities all over the world. See e.g. Chandler and Fox (1974, 5) for densities up to 200/ha just before the building of a new town wall.

observe an increase in the number of buildings within the town walls and in the number of buildings having an upper floor.<sup>30</sup>

Several factors which favoured such a development can be identified. To begin with, it does not seem far-fetched to suppose that many inhabitants of urban agglomerations preferred to live within the town walls, if only because they appreciated the protection provided by these walls. We should therefore expect urban population densities to have increased before large suburbs started to develop. This phenomenon is well attested in medieval Europe. In the specific case of Graeco-Roman towns we must also reckon with the fact that older towns were gradually boxed in by the cemeteries surrounding them, making it difficult for suburbs to develop.

Of course it was also possible for urban population densities to decline. It has been pointed out that decreases in the urban population, even drastic ones such as the population collapse caused by the Black Death, were usually not reflected in any change in the physical make-up of towns.

Although the ancient evidence relating to the issue of urban population densities is not exactly overwhelming, there are good reasons for thinking that the number of inhabitants per urban hectare varied enormously. A well-documented example is the Latin colony of Cosa, where 24 larger and 224 smaller houses were discovered.<sup>31</sup> The ratio between the two types is approximately one to ten, suggesting that the bigger houses belonged to *equites* and the smaller ones to foot soldiers. In any case, it seems reasonable to assign a family of five or six to each of the smaller houses and between 10 or 12 people to the remaining 24, which were approximately twice as big. This would give the colony

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<sup>30</sup> For the gradual filling up of empty spaces within the town walls of Pompeii see Pesando (1997, 13–15) and Schoonhoven (2003, 236–8). For similar developments in second-century BC Cosa see Brown (1980, 66). More two-storey buildings in late-republican and early-imperial Pompeii: Pesando (1997, 77, 207–8) and esp. Pirson (1999, 161, 171–3). For the provinces it is possible to point to the data from Hermopolis, which seems to have had *c.* 37,000 inhabitants on 120+ hectares (unfortunately the exact number of urban hectares is unknown; see Van Minnen 2002, 286 n. 8). If we take 190 hectares as an upper limit for the area covered by the four quarters of the city plus the suburbs (cf. Roeder 1959, 107; Bagnall 1993, 52), we end up with an estimate of between 200 and 300 persons per urban hectare in the 260s AD. It cannot be a coincidence that Hermopolis was a very old city in which almost all houses had two (and sometimes more) stories above ground. For discussion see Bagnall (1993: 49 and 52–53).

<sup>31</sup> Fentress et al. (2003, 24).

between 1,360 and 1,632 free and unfree inhabitants, or between 101 and 121 persons/ha.<sup>32</sup> It may be noted that this is the density for the entire area enclosed by the town wall. The density per hectare for domestic space only would be approximately 25% higher.

On the other hand, between 20,000 and 58,000 people are thought to have lived within the walled area of Ostia, which comprised 69 ha. The urban densities implied by these figures range from 290 to 840 persons/ha.<sup>33</sup> The latest estimates are close to the lower end of this band.<sup>34</sup> However, even a population density of 300/ha would be at least two and a half times higher than the corresponding figure for Cosa.

The densities implied by the most common population estimates for Rome are also high. Although some scholars have assigned Rome fewer than 500,000 inhabitants, most ancient historians continue to support the old figure of between 800,000 and one million. If the entire population lived within the area later enclosed by the Aurelian wall, the average number of people per hectare would be between 580 and 730. If we include the suburbs beyond the Aurelian wall, it becomes between 440 and 560.<sup>35</sup>

It appears therefore that the highest urban population densities in Roman Italy are likely to have been at least four or five times higher than the low density reconstructed for Cosa. How then can we put even a very approximate figure on population densities in the towns of Cisalpine Gaul?

In my view, some headway in this difficult field can be made by looking at urban population densities in Italian cities and towns of the late-medieval and early-modern periods. During the first half of the fourteenth century, when Italy's urban population reached a temporary high, most Italian cities appear to have had population densities ranging between 100 and 140 persons/ha. Cities falling into this category include Pistoia, Verona, Bologna, and Padova, with 103, 105, 119, and 133 persons/ha respectively. Later on, in the sixteenth century, Milan had 126 inhabitants/ha.<sup>36</sup> In some cities we find somewhat

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<sup>32</sup> In Roman Egypt the average size of households in the *metropoleis* was 5.31. The population structure of Egypt is thought to be similar to that of Italy: see Bagnall and Frier (1994, 68). Brown (1980, 18) gives mid-republican Cosa 1,100 inhabitants.

<sup>33</sup> Duncan-Jones (1982, 276).

<sup>34</sup> Storey (1997, 973–5): 319 persons/ha.

<sup>35</sup> Hopkins (1978, 97); Lo Cascio (1999, 165).

<sup>36</sup> Pistoia: 117 ha in the late thirteenth century according to Herlihy (1967, 74); Verona: above, note 26; Bologna: 419.5 ha in the early fourteenth century according

higher densities: early-fourteenth-century Arezzo is thought to have had some 168 inhabitants/ha, while a density of 175 persons/ha can be calculated for Florence in 1333.<sup>37</sup> Finally, it is possible to detect a small group of cities with much higher densities. The most striking case is late-medieval Genova, with some 65,000 inhabitants on 155 ha, or 419 persons/ha. Another is early fourteenth-century Siena, with 50,000 people on 165 ha.<sup>38</sup> In this case the implied density is 303 inhabitants/ha. It should, however, be noted that the area enclosed by the town walls of Siena was not entirely built up. In other words, in the built-up area the number of people per hectare must have been considerably higher than 300.<sup>39</sup>

How can these differences in urban population densities be explained? In the case of medieval and early-modern Italy the answer clearly lies in the architectural make-up of the cities and towns in question. Most medieval towns were agglomerations of one- and two-storey buildings.<sup>40</sup> The corresponding population densities hardly ever exceeded 150 persons/ha. There were, however, some notable exceptions. One instance of this was medieval Genova, which was boxed in not only by its city walls but also by the surrounding mountains. Precisely for

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to Beloch (1937–61, II.91); Padova: 300 ha in 1320 judging from the map in Hyde (1966, 36); Milan: above, n. 25. For all these cities I have used the population estimates of Malanima (1998): Pistoia 12,000 in 1300, Verona 40,000 in 1300; Bologna 50,000 in 1300, Padova 40,000 in 1300, Milan 100,000 between 1500 and 1600.

<sup>37</sup> Arezzo: 107 ha according to Cherubini (2003, 140); Florence: 630 ha in 1333 according to Herlihy (1958, 35 n. 1) (the figure of 512 ha given by Beloch 1937–1961, II.128, is too low). Again the population estimates are those of Malanima (1998): Arezzo 18,000 in 1300, Florence 110,000 in 1300. If Pisa, whose third circuit of walls enclosed *c.* 185 ha according to Herlihy (1967, 74) had *c.* 38,000 inhabitants around 1300 (thus Herlihy 1958, 36), it would have had 205 persons/ha. But the estimates for early fourteenth-century Pisa are controversial (Ginatempo and Sandri 1990, 259), with Malanima (1998) assigning the city only 30,000 inhabitants in 1300, implying an urban population density of 162 persons/ha.

<sup>38</sup> For Genova see above, n. 26. According to Benevolo (1980, 326) the fourteenth-century walls of Siena enclosed *c.* 180 ha, but I have used the lower figure of 165 ha given by Bortolotti (1983, 30). Cf. Piccini (2003). The figure of 101 ha given by Beloch (1937–61, II.150) and by Chandler and Fox (1974, 92) is far too low. For the estimated populations of Genova and Siena see Malanima (1998). For Genova's population cf. also Ginatempo and Sandri (1990, 69–70, 248–9).

<sup>39</sup> The Italian pattern is very similar to the pattern found in the northern Netherlands in the mid sixteenth century. In 1560 the average urban population density for all towns in the northern Netherlands was *c.* 130 persons/ha. But Nijmegen and Dordrecht had 200 people/ha, while the fast-growing city of Amsterdam had 300 inhabitants/ha. See the valuable collection of data in Visser (1985, 15–17).

<sup>40</sup> Pounds (1974/1988, 24, 275); cf. Pounds (1969).

this reason, Genova had a disproportional number of very high buildings, many of which had six or more storeys.<sup>41</sup> As we have seen, this is reflected in an unusually high urban population density. It cannot be a coincidence that Siena also had numerous high buildings, with four or five-storey buildings being the norm in the central urban area.<sup>42</sup> As a result of this, the streets of Siena feel like alleys, although they are not particularly narrow. A third example is medieval and early-modern Naples, where we again find a combination of high buildings and high urban population densities.<sup>43</sup>

In my view, this is the key to the enormous variations in urban population density that can be observed in Roman Italy. Even though we know disappointingly little about the domestic architecture of late-republican and early-imperial Rome, the existence of high buildings in this city is documented as early as the third century BC.<sup>44</sup> It is also possible to point to the Augustan regulation which established 70 feet as the maximum height of new buildings in Rome. If we assume that each floor had between 12 and 15 feet, it follows that Augustus was thinking of buildings having between five and six storeys.<sup>45</sup> High buildings are also characteristic of Ostia, where the average building-height for residential buildings was between 2.5 and 4 storeys.<sup>46</sup> In the light of these data, it is quite reasonable to assign Ostia and Rome population densities between 300 and 600/ha.

The other end of the spectrum is represented by Cosa, with no more than 100 or 120 persons/ha. From recent analyses of the domestic architecture of this town it appears that the houses of the ordinary colonists who made up the majority of the population had no upper storeys.<sup>47</sup> So far, the only domestic building in which traces of an upper floor have been detected is the so-called House of the Skeleton, from

<sup>41</sup> Heers (1962, 402); Chandler and Fox (1974, 5); Ginatempo and Sandri (1990, 69).

<sup>42</sup> E.g. Balestracci and Piccini (1977).

<sup>43</sup> In the 1530s Naples had c. 150,000 inhabitants on 350 ha (Benevolo 1980, 326; I. Quaresima, 'Napoli, città contesa' at <http://www.napoliontheroad.it/quaresimadomspagnola.htm>). The implied urban population density is c. 430 persons/ha. Cf. Beloch (1886, 409) on the very high densities in nineteenth-century Naples.

<sup>44</sup> Yavetz (1958, 506) and Patterson (2006, 353), both referring to Liv. 21.62.3.

<sup>45</sup> Yavetz (1958, 507), referring to Str. 5.3.7 and Suet. *Aug.* 89. For a good discussion of the archaeological evidence for multi-storeyed *insulae* in Rome see Wallace-Hadrill (2000, 204–8).

<sup>46</sup> Duncan-Jones (1982, 277).

<sup>47</sup> Brown (1980, 64–7, and figs 81, 83, 85–7); Bruno and Scott (1993, esp. figs 6–9, 11–12, 20–1, 25–6).

the early first century BC. It is perhaps no coincidence that this was a somewhat larger house which occupied the space of five former gardens.<sup>48</sup> Cosa is therefore a clear example of an Italian town having low buildings and a correspondingly low urban population density.

For the purposes of this paper the central question is whether the towns of early-imperial Cisalpina were more like Rome or more like Cosa. In my view, there are good grounds for thinking that the latter alternative is more likely to be correct. One of the reasons why Rome had many high buildings is that the oldest part of the city had been boxed in by the Servian wall for a very long time. At the same time the existence of cemeteries made it difficult for suburbs to develop to the West and South. A closely related factor was that the fast expansion of Rome's population had the inevitable effect of pushing up the price of land within and near the city walls. This means that for purely financial reasons the owners of rented apartments must have aimed at a high rental income per square metre. The main options were to build expensive apartments that could be rented to wealthy people or to build high *insulae* containing many cheaper domestic units. As several studies of the living conditions of the Roman plebs have demonstrated, many builders opted for the latter alternative.<sup>49</sup>

If we apply these ideas to the North, it is surely not without significance that many northern towns were recent foundations. Interestingly, many towns in Cisalpine Gaul appear to have acquired walls only in the final decades of the republican period or in the Augustan period.<sup>50</sup> Many other towns remained unwallled under the Empire.<sup>51</sup> Moreover, because most northern towns were young, they had not yet become locked in by their suburban cemeteries. So if these towns expanded

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<sup>48</sup> Brown (1980, 67–8); Bruno and Scott (1993, 142–3). Judging from Fentress et al. (2003, 34–42), another large house, the so-called House of Diana, had no upper floor.

<sup>49</sup> Yavetz (1958). As Wallace-Hadrill (2000, 205) points out, many *insulae* are likely to have had a heterogeneous population comprising tenants of different social categories.

<sup>50</sup> For examples see Conventi (2004): Mutina (42 BC), Alba Pompeia (Augustan), Brixia (Augustan), Comum (59 BC), Mediolanum (40–35 BC), Ticinum (Augustan), Verona (mid first century BC), Vicetia (second half of the first century BC), Concordia (Augustan), Tergeste (33–32 BC), Tridentum (Augustan), Augusta Taurinorum (Augustan), Augusta Praetoria (Augustan), Augusta Bagiennorum (*uallum* under Augustus). For a general discussion of the town walls of Cisalpina see Chevallier (1983, 104–6); Mansuelli (1971, 120–1); Tiusi (2002/2003, 82).

<sup>51</sup> E.g. Libarna (Conventi 2004, 89), Ateste, Caesena, Forum Livii, and most of the smaller towns listed in Appendix I.

during the late Republic and early Empire, one would expect them to have grown laterally rather than vertically.

As far as the archaeological evidence goes, it seems to support this inference. In a general survey of the domestic architecture of Cisalpina the Italian archaeologist Scagliarini notes that the ground plans of northern houses tend to be large, and that there is very little evidence for the existence of upper storeys in the North. His main finding is that the domestic architecture of North Italy was 'extensive' compared to that of Pompeii.<sup>52</sup> In other words, urban population densities in the North were low. In a more recent study Michele George notes that the houses of North Italy are in many ways more similar to those in North Africa and Southern Gaul than to those in Central Italy.<sup>53</sup> Since urban population densities in Roman Gaul are thought to have been lower than 150/ha, this observation points in the same direction as Scagliarini's earlier findings.<sup>54</sup>

Of course it may be objected that our knowledge of the make-up of the towns of Cisalpine Gaul is based on the excavated remains of only a few hundred houses. Part of my response to this would be that even if the material presently available does not *prove* that northern towns had low population densities, it is at least fully compatible with the drift of my argument. It seems also significant that during the extensive excavations carried out at Aquileia, a free-lying site, no traces of Ostia-type *insulae* were discovered.<sup>55</sup> This suggests to me that even in the larger towns of Cisalpine Gaul the average height of buildings was low.

Although any attempt to put a figure on population densities in the North must remain to some extent speculative, I am inclined to opt

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<sup>52</sup> Scagliarini (1983, 304): "Non vi è nulla che indichi concretamente una parcelazione delle *insulae* in piccole unità di abitazione con sfruttamento intensive dello spazio... Le planimetrie sono estensive, è scarsamente documentata la presenza di un secondo piano..." Cf. Chevallier (1983, 147): "Elles [les maisons] ont en général un seul étage, même à basse époque, mais de grandes pièces (50 à 70 m<sup>2</sup>, très tôt)." This impressionistic conclusion was based on a corpus of 501 private houses (*ibid.* 148), of which only a small proportion had been properly published.

<sup>53</sup> George (1997, 32–3). Cf. also Tosi (1992b, 382–3); Maioli (2000, 183); Ortalli (2003, 96–7); Cavalieri Manasse and Bruno (2003, 47).

<sup>54</sup> For Transalpine Gaul see Goudineau (1980, 310): "une densité de 150 à l'hectare constitue un seuil qui ne fut sans doute franchi par aucune ville de province", followed by Woolf (1998, 137 and n. 103). In my view this claim is broadly valid for towns in the western half of the Empire in the late Republic and early Empire, but not for all the provinces in the East, some of which had a long tradition of urbanism and urban population densities well over 150 persons/hectare. Cf. my remarks in note 30.

<sup>55</sup> Mian (2003, 84–6).

for an average density of between 120 and 150 persons/ha for the areas covered by the towns of Cisalpine Gaul (including those areas occupied by public buildings).<sup>56</sup> If we adopt this range, average urban population density in early-imperial Cisalpinga was somewhat higher than the density which can be reconstructed for mid-republican Cosa, but roughly comparable to those which are found in most Italian cities and towns of the late-medieval and early-modern period.

### III. *Urbanization rates*

In AD 1600 21.7% of the North-Italian population lived in cities and towns having 3,000 or more inhabitants, and 18.4% in urban centres with a population of 5,000 or over.<sup>57</sup> This made North Italy one of the most urbanized areas of early-modern Europe.

Did Roman Cisalpinga have a similarly high urbanization rate? Before trying to provide a tentative answer to this question, I begin by noting Lo Cascio's view that, if we ignore Rome, the overall urbanization rate in early-imperial Italy is likely to have been between 15% and 20%.<sup>58</sup> On this view, Roman Italy as a whole was almost as urbanized as Italy in the early-modern period.

Most of those who subscribe to a low-count interpretation of Italian demographic history operate with somewhat higher urbanization rates, although few attempts have been undertaken to explain them. One notable exception is Jongman, who constructed a model which explains high urbanization rates as a result of high elite income and

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<sup>56</sup> Hansen (2006, 61–3) operates with a density of 150 persons/ha of inhabited space in the Greek *poleis* of classical times. If we assume an average urban population density of 120 persons/ha for the towns of Roman Cisalpinga and assume that 20% of the area of towns in this region was occupied by public spaces and buildings, we also end up with a density of 150 people/ha for domestic space. For the Greek town of Halieis, Jameson, Runnels, and Van Andel (1994, 549–52) reckon with 250 persons/ha for domestic space. If 20% of the town's built-up area was occupied by streets and public buildings, the implied density is 200 persons/ha. In the case of Hermion the same authors assume that domestic space accounted for 75% of the built-up area.

<sup>57</sup> See Appendix II. The percentages are based on a northern population of 5.4 million. During the early-modern period North Italy also had between 100 and 150 small towns with populations over 2,000. Although some of these had more than 3,000 inhabitants, they do not appear in the lists of Bairoch (1988). This means that the proportion of the North-Italian population living in towns with 3,000 inhabitants or over was actually somewhat higher than 21.7%.

<sup>58</sup> Lo Cascio (1999, 165); Lo Cascio and Malanima (2005, 17).

expenditure. In his view, the combined annual incomes of the Italian elite, comprising senators, *equites*, and decurions, must have been enough to feed some 2.4 million people, or 32% of a hypothetical population of 7.5 million.<sup>59</sup> In this calculation the population of Rome is included. If we remove Rome, the urbanization rate drops to somewhat below 20%. Of course, the level of urbanization would rise again if there were 6 million rather than 7.5 million free and unfree Italians in the time of Augustus.

For our purposes the most interesting aspect of Jongman's approach is that it establishes a causal link between a high urbanization rate and a consistent pattern of elite residence in towns. This is in line with the findings of many specialists in medieval Italian history, who have identified the habit of Italian landowners to live in cities as a major force behind the high level of urbanization found in late-medieval Italy.<sup>60</sup> The existence of this causal connection provides us with at least one reason for thinking that urbanization rates in Cisalpine Gaul cannot have been very low. As Garnsey has noted, the modest size of the Latin colonies of the North and the extent of the centuriated areas surrounding them suggest that most *pedites* were expected to reside in the countryside. The other side of the coin is that the Roman government acted on the assumption that the larger landowners would be town-based.<sup>61</sup> In other words, in setting up the Latin colonies of Cisalpinia the Roman government extended to the North the central-Italian tradition of elite-residence in towns. As far as we can tell, the indigenous elites of Transpadana were also town-based by the end of the Republic. From the early Principate onwards they also took up the central-Italian model of urban euergetism.<sup>62</sup> The establishment of veterans from Central Italy in various northern towns between 44 BC and 25 BC can only have strengthened this pattern.

On the other hand, there are some grounds for thinking that Cisalpine Gaul was less urbanized than the Centre and South. Some twenty years ago Bekker-Nielsen calculated average inter-city distances for various parts of Italy. One of his findings was that the average distance between cities in the North was between 24.9 and 35.6 km (except in the Via Aemilia corridor). The corresponding figure for Latium and

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<sup>59</sup> Jongman (1988, 192–8).

<sup>60</sup> E.g. Jones (1974, 1679–81); Britnell (1991, esp. 26–30).

<sup>61</sup> Garnsey (1998, 123–6); cf. Gabba (1979, 34).

<sup>62</sup> Frézouls (1990).

Campania is only 11 km.<sup>63</sup> In principle we cannot rule out the possibility that urbanization rates in the North were similar to those in Central Italy, and that the towns of the North were simply bigger than those in Latium and Campania. One clue that points in this direction is Strabo's statement that Cisalpine Gaul had a large free population and many large and wealthy cities. According to him North Italy had surpassed the Centre and South in these respects.<sup>64</sup> On the other hand, the simple fact that northern towns were spaced more widely than those in other parts of Italy still suggests that urbanization rates in the North were somewhat below the average rate for Italy as a whole. One reason for this is that the town populations of Roman Italy almost certainly included some farmers.<sup>65</sup> In areas where the rural population was scattered over wide territories, such town-based farmers must have made up a relatively small proportion of the total farming population. For this reason alone higher inter-city distances are likely to have meant a somewhat lower urbanization rate.<sup>66</sup>

In the case of four republican and early-imperial colonies it seems possible to estimate the proportion of the total population that could be accommodated within the city walls. The two cities of Placentia and Cremona, both founded in 218 BC, had town walls enclosing 38.4 and 30 ha respectively. If we assume 120 inhabitants/ha, 4,600 and 3,600 people can be fitted into these towns. We also know that Placentia and Cremona each received 6,000 male colonists, a figure which increases to *c.* 21,000 if women and children are included. The urbanization rates implied by these figures are 22% and 17.1% respectively. If we assume 150 people/urban ha, these figures become 27.4% and 21.4%.<sup>67</sup> Of

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<sup>63</sup> Bekker-Nielsen (1989, 25).

<sup>64</sup> Str. 5.1.12.

<sup>65</sup> In the Latin colonies of the North there was room for a substantial group of *pedites*. As Garnsey (1998, 126) admits, plots of land in the vicinity of town could be worked by commuting peasants.

<sup>66</sup> Another factor influencing urbanization rates in Italy was the amount of land owned or controlled by the urban elites of the various regions concerned. Unfortunately, we know next to nothing about patterns of land ownership in the North. The only important clue is the land register from Veleia, which shows that large estates were common in this part of Cispadana. In the Latin colony of Aquileia *equites* received 140 *iugera* of land apiece (Brunt 1971/1987, 193).

<sup>67</sup> Although the colonists must have held most of the land belonging to Placentia and Cremona, there are reasons for thinking that some Celtic communities survived in pockets. See Garnsey (1998, 128). The actual rates of urbanization are therefore likely to have been somewhat below the levels indicated in the main text. My calculations in the final part of this section are based on an overall urbanization rate of 15%.

course, it may be objected that at least initially most of the colonists sent out to Placentia and Cremona must have lived within the town walls, if only because these two towns were situated near the territories of hostile Gaulish tribes. Although this alternative scenario makes some sense, there are strong indications that the Roman government did in fact expect most inhabitants of newly founded colonies throughout Italy to take up rural residence, even in potentially hostile areas.<sup>68</sup> Moreover, in the specific cases of Placentia and Cremona we must remember that Placentia was situated in the territory of the Anares, and Cremona in that of the Cenomani, two Gaulish tribes that maintained friendly relations with Rome before the Second Punic War.<sup>69</sup> The Roman government therefore had some reasons for thinking that the colonists sent out to these towns would not be exposed to frequent hostile attacks. In any case, even if initially most of the colonists of Placentia and Cremona lived within the town walls, the historical legacy of this can only have been an urbanization rate considerably *higher* than 20%.

Another example is the town of Comum, where Caesar established some 4,500 colonists. If these included the descendants of an earlier group of 3,000 colonists, as seems to have been the case,<sup>70</sup> the total free population of Comum and its territory would have been roughly 16,000. With 120–150 people/urban ha, between 3,000 and 3,750 of these could have been accommodated within the town walls, which enclosed 25 ha. The implied urbanization rate is between 18.8% and 23.4%. Of course this rate would have been significantly lower if the descendants of the original colonists were not included among the Caesarian colonists.

Finally, of the *c.* 10,500 free people who were settled in the Augustan colony of Augusta Praetoria in 25 BC between 5,000 and 6,300 can be accommodated within the 42 ha enclosed by the town walls. In this case the implied urbanization rate is as high as 50% or even 63%. We must, however, remember that the population of Augusta Praetoria's territory included an unknown number of indigenous *incolae*, so that the real urbanization rate must have been considerably lower.<sup>71</sup>

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<sup>68</sup> Garnsey (1998, 126). As he points out, "the practice of disposing of land in viritane allotments in newly conquered and therefore potentially hostile areas... is distinctly problematic, unless we accept that dispersed settlement could be viewed as a way of controlling an area on which the hold of the ruling power was insecure."

<sup>69</sup> Peyre (1979, 47–8).

<sup>70</sup> Brunt (1971/1987, 201). But cf. Duncan-Jones (1982, 267).

<sup>71</sup> Brunt (1971/1987, 171 n. 4).

These scattered data suggest to me that in many parts of Cisalpine Gaul urbanization rates were in the order of 15% or higher.<sup>72</sup> There is, however, a complicating factor. In my discussion of the physical extent of the northern towns I have distinguished between very important, important, and unimportant urban centres. The last of these three categories was defined as comprising all towns covering fewer than 20 ha. Although many of these smaller centres are poorly documented, it would appear that at least some of them were service centres for the surrounding countryside rather than centres of habitation for local landowners. One illustration of this is *Veleia*, which had a small amphitheatre and a basilica but few substantial houses.<sup>73</sup> Another is *Forum Novum*, which covered a mere 2 ha.<sup>74</sup> Judging from its small size, this town cannot have been inhabited by a substantial land-owning elite. At this point I refer back to my earlier argument that urbanization rates in many parts of Cisalpine Gaul cannot have been very low, because we are dealing with a society in which a large proportion of the land-owning elite was town-based. As I have just explained, this argument is invalid for many of the smallest towns of the North. For this reason it seems appropriate to use a lower nucleation rate for these towns. In my calculation I have used a nucleation rate of 10% for all centres covering fewer than 20 ha. In principle this figure could be lowered to 5%. At first sight this may seem to introduce an uncontrollable margin of error. It must, however, be remembered that small towns account for only one-seventh of all urban hectares in the North. For this reason alone even a radical downward adjustment of the nucleation rate for these centres would have little effect on the outcome of my calculations. At the same time many of these small centres are found in districts which are likely to have been thinly populated.<sup>75</sup> Some other small ‘towns’

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<sup>72</sup> In estimating the proportion of the colonial population that could be accommodated within the walls of *Placentia*, *Cremona*, *Comum*, and *Augusta Praetoria* I have ignored the fact that a certain percentage of the urban population is likely to have consisted of slaves owned by the elite. This means that the urbanization rates for the free populations of these four towns must have been somewhat lower than the rates given in the main text.

<sup>73</sup> Mansuelli (1971, 78–9, 90–1, 145).

<sup>74</sup> Appendix I.3.

<sup>75</sup> One thinks especially of *Liguria*, where urban territories were considerably smaller than in other parts of Cisalpine Gaul (Chilver 1941, 48–9). According to Bekker-Nielsen (1989, 25), the average distance between towns in *regio IX* was 26 km; but this region comprises *Cispadana*, where intercity distances were larger than in *Liguria*. The mountainous parts of *Liguria* seem to have been characterized by low urbanization rates and

seem to have been small simply because they had small territories. For all these reasons I do not think that the existence of such centres makes it impossible to reach any meaningful conclusions.

We have now reached a point where we can begin to feed figures into the formula discussed at the beginning of this paper. In doing so I have distinguished between two sets of conditions, one in which the average urban population density in Cisalpina is put at 120 persons/ha, and another which is based on 150 persons/ha. The results are as follows:

1. 2,072.7 urban ha, 120 people/urban ha, urbanization rate 15% for towns covering 20+ ha and 10% for smaller towns >>> 1.79 million people in Cisalpine Gaul
2. 2,067.4 urban ha, 150 people/urban ha, urbanization rate 15% for towns covering 20+ ha and 10% for smaller towns >>> 2.23 million people in Cisalpine Gaul

In other words, if we assume that urbanization rates in the North were between 10% and 15%, and if we assume that the number of people per urban hectare was roughly similar to that found in most Italian cities of the late-medieval and early-modern period, we end up with a total population between 1.7 and 2.3 million. It will be immediately apparent that these estimates fall far short of the population required in Lo Cascio's high count.

If we retain Lo Cascio's own estimates of urbanization rates in early-imperial Italy, the only way to arrive at a much higher population is to assume urban population densities matching those found in medieval and early-modern Genova and Naples. But, as we have seen, the archaeological data presently available, fragmentary though they are, make it difficult to offer a convincing alternative reconstruction along these lines.

Before taking leave of the topic of urbanization, I should like to point out that in extrapolating the total population from the urban population, I have classified all settlements which were towns in a juridical sense as urban. As we have seen, however, most analyses of medieval and early-modern towns ignore all centres having fewer than

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by low population densities. A large proportion of the rural population seems to have lived in villages. See Giorelli Bersani (1994, 187–90); Arnaud (2004, 484).

5,000 or 3,000 inhabitants. If we want to avoid comparing apples with oranges, we must therefore exclude all Roman towns covering fewer than 20 ha (cf. above). If we do this without altering our estimates for the total population of Roman Cisalpina, the number of urban hectares drops to 1,753, and the overall urbanization rate to *c.* 12%.

This finding sheds an interesting light on the traditional claim that the low count for Roman Italy implies an implausibly high urbanization rate.<sup>76</sup> In the case of Cisalpina this claim is demonstrably incorrect. In fact there can be no doubt that even a low count for the North implies an urbanization rate which is very much lower than the rate for the same area in the early-modern period.

#### IV. *Free citizens and slaves*

Although the foregoing discussion may already seem unduly speculative, I should like to add a few words about the quantitative importance of slavery in the North. Before proceeding with a brief discussion of this slippery topic, I hasten to say that we do not have any data which permit us to arrive at even a very crude estimate of the size of the unfree population of Cisalpine Gaul. My aim is simply to see whether it is possible to construct a more or less realistic *model* in which the findings of the foregoing sections can be related to the Augustan census figures.

Let me begin by noting that widely diverging estimates of the number of Italian slaves have been put forward during the past forty years. According to Brunt, for instance, early-imperial Italy had some 7 million inhabitants, of whom 3 million were of servile status. In his view, slaves made up roughly 40% of the Italian population.<sup>77</sup> In his *Conquerors and Slaves* Hopkins lowered these figures to 6 million and 2 million, in which case one-third of the Italian population would have been of servile status.<sup>78</sup>

In an article which appeared a few years ago I argued that the number of slaves needed on slave-run villas for the production of wine and olive-oil has been vastly exaggerated, and that Hopkins' estimate

<sup>76</sup> E.g. Lo Cascio (1994, 39; 1999, 164–5).

<sup>77</sup> Brunt (1971/1987, 124).

<sup>78</sup> Hopkins (1978, 7 n. 13). Cf. Andreau and Descat (2006, 80–2), who hold that slaves made up between 30% and 40% of the Italian population in the late Republic and early Empire.

can only be defended by assuming that many slaves were employed in cereal cultivation and on smaller farms.<sup>79</sup> More recently, Scheidel has suggested that there may have been no more than between 1 and 1.5 million slaves in early-imperial Italy.<sup>80</sup> Finally, Lo Cascio operates with between 2 million and 3 million urban and rural slaves, making up between 15% and 20% of his Italian population of 15–16 million.<sup>81</sup>

This rapid survey shows that it is extremely difficult to put a figure on the number of Italian slaves. However, it is generally agreed that slaves must have made up at least 15–20% of the Italian population.

As far as Cisalpina is concerned there is very little to go on. It has often been claimed that there were fewer slave-run plantations in the North than in the Centre and South.<sup>82</sup> In view of the fact that slave-run *uillae* in Central Italy were better placed to produce for the huge market in Rome, this seems a reasonable assumption. On the other hand, it does not seem far-fetched to suppose that the elites of the Latin and Roman colonies of Cisalpina set up slave-run *uillae* on the central-Italian model, and that many others followed their example.<sup>83</sup> Such *uillae* would have been oriented towards the growing towns of the North.<sup>84</sup>

As has already been noted, there is little evidence to back up this general argument. One of the few exceptions is the literary evidence concerning a slave-run estate of the Saserna family, which can be assigned to North Italy.<sup>85</sup> We also know that in various towns of Cisalpina slaves

<sup>79</sup> De Ligt (2004, 746–7).

<sup>80</sup> Scheidel (2005) and in this volume.

<sup>81</sup> Lo Cascio and Malanima (2005, 11–12); cf. also Lo Cascio (2002, esp. 62–3).

<sup>82</sup> Chilver (1941, 146–150: few large estates in the North; 151: more tenants than slaves in the North). Chevallier (1983, 206–7): slavery less important in Cisalpina than in the Centre and South. Duncan-Jones (1982, 273) reckons with 28.6% slaves in Comum and its territory. This seems too high for a northern town.

<sup>83</sup> As Gabba (1979, 34) points out, the holdings of 100 and 140 *iugera* assigned to the *centuriones* and *equites* of the Latin colony of Aquileia were similar in size to Cato's model estate of 100 *iugera*.

<sup>84</sup> Of course, there is much archaeological evidence for rural *uillae* in the countryside of North Italy. See e.g. Mansuelli (1957b); Righini (1979). Unfortunately, we have very little information on the labour force used on such estates. Note that there is no secure evidence for Tibiletti's suggestion that large landowners in North Italy tended to use "indigenous wage labourers" ("salarinati indigeni") rather than slaves: Tibiletti (1978, 93 n. 44).

<sup>85</sup> Var. *R* 1.18.6 and 1.19.1; Col. 2.12.7–8, on which see Kolendo (1973, 14–16) and Bortuzzo (1994). Cf. also Chevallier (1983, 207 n. 262), referring to Cic. *Mil.* 26 (rural slaves in the Apennine districts of North Italy) and *II* 11.592 (rural slavery near Pola).

and freedmen were used in commerce and manufacturing, though we cannot put any figure on this phenomenon.<sup>86</sup> All in all, it is impossible to go beyond the inference that in Cisalpina, as in Central Italy, slaves were employed not only as domestic servants but also as labourers and managers in all sectors of the economy.

Despite its meagerness, even this finding is not without interest. As the evidence from Roman Egypt shows, the proportion of slaves in the total population could easily be as high as 10% even in those parts of the empire in which most slaves were used either in households for domestic purposes or as personal agents for their masters' business dealings.<sup>87</sup> The north-Italian percentage is likely to have been somewhat higher than this.

In what follows I shall present the results of two calculations in which the share of the servile population has been set at 15% and 20% respectively. This does not mean that I attach any importance to these estimates.<sup>88</sup> My aim is merely to explore some of the ramifications of two more or less realistic reconstructions of the population of Cisalpine Gaul in the light of the debate between high-counters and low-counters. In other words, I am not implying that these are the *only* realistic scenarios.

If these estimates are combined with those offered in the foregoing sections, we can offer a range of estimates for the size of the free population of the North around 28 BC. However, if we want to compare the results of these calculations to the approximate sizes of the free population implied by the low count and high count for Cisalpine

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<sup>86</sup> *Liberti* in trade and manufacturing: Chilver (1941, 177, 181); Lazzaro (1985; 1989); Zampieri (2000). Cf. also Pelletier (1991): slaves and freedmen important in the urban economy. Most of the slaves referred to in inscriptions from the North are imperial slaves and slaves fulfilling various administrative tasks for towns or for private owners. See Chevallier (1983, 207–8).

<sup>87</sup> Bagnall (1993, 208–9); Bagnall and Frier (1994, 70–1). In a less affluent community in Upper Egypt slaves made up 7% of the urban population. See Bagnall, Frier, and Rutherford (1997). As Scheidel (2001, 61) points out, this suggests that Bagnall and Frier's earlier database need not be representative of Egypt as a whole.

<sup>88</sup> For my purposes the crucial question is not whether these percentages are accurate, but what happens to my estimates of the free population if the hypothetical share of the servile population is lowered or increased. For instance, if slaves made up 10% rather than 15% of the northern population, the share of the free population increases from 85% to 90%. This would increase my estimate of the size of the free population by 6%. In short, the hypothetical percentages that have been fed into my calculations are already so low that the effect of lowering the share of the unfree population on my estimates of the number of free inhabitants is negligible.

Gaul, we must also distinguish between those communities which had been given the citizenship before or in 89 BC and those which received it in 49 BC.

The first category comprises 22 towns, among which were Ariminum, Bononia, Aquileia, Mutina, Placentia, and Ravenna.<sup>89</sup> In the early Empire the 16 largest towns of this group of 22 covered some 599 ha, while the smallest centres covered a further 52.5 ha. This leaves 1,421.2 ha for those towns which received the citizenship in 49 BC. If we assign these towns between 120 and 150 persons/ha, put the nucleation rate at 15% for the large centres and at 10% for the smaller ones, assume 15–20% of the population to have been of unfree status, and put the share of adult male citizens at 28% of the total free population, we obtain the following results:

1. 120/ha, 10–15% urban,<sup>90</sup> 20% slaves >>> 278,647 new adult male citizens
2. 120/ha, 10–15% urban, 15% slaves >>> 296,062 new adult male citizens
3. 150/ha, 10–15% urban, 20% slaves >>> 348,309 new adult male citizens
4. 150/ha, 10–15% urban, 15% slaves >>> 370,078 new adult male citizens

It should be emphasized that these four calculations take into account all ‘towns’ in a juridical sense. If all agglomerations covering fewer than 20 hectares are classified as ‘rural’, the number of urban hectares added in 49 BC drops to 1153.7. After this modification the four models give the following results: 1. 206,743; 2. 219,664, 3. 258,429, and 4. 274,581. As is immediately apparent, the eight outcomes generated by these models, even by those which are based on a broad definition of ‘towns’, fall within a range that is entirely compatible with the low count, according to which roughly 300,000 new adult males in Transpadana acquired the citizenship in 49 BC.

Since these calculations are based on a number of assumptions that cannot be verified with the help of the surviving evidence, they do

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<sup>89</sup> See Appendix I, in which these 22 towns have been marked with an asterisk. I have followed Brunt (1971/1987, 170) in assuming that Ravenna was Latin after 89 BC.

<sup>90</sup> 15% for towns covering 20+ ha, 10% for smaller towns. Cf. section 3.

not *prove* the low count to be correct. They do, however, show that it is possible to fit the archaeological data concerning the towns of the North into various coherent models that makes sense from a low-count perspective.

#### V. *Some possible alternatives*

At this point we must ask whether it is also possible to construct an alternative model which is compatible with the high-count view of Italian population development.

As is well known, the corner stone of the high count is the assumption that the census figure for 28 BC, when *c.* 4 million *ciuium capita* were registered, refers to adult male citizens only. Since comparative evidence suggests that all premodern censuses were at least 10% defective<sup>91</sup> and because adult males must have made up *c.* 28% of the population, this implies a total citizen population of *c.* 15 million, of whom between 1.25 million and 2 million can be assigned to the provinces.<sup>92</sup> If we add between 2 and 3 million slaves,<sup>93</sup> we end up with a total Italian population of at least 17 million. In Lo Cascio's most recent reconstruction between 35% and 45% of the population is assigned to Cisalpina as early as 225 BC.<sup>94</sup> If we apply this percentage (which is likely to have increased during the late Republic) to 28 BC, we end up with at least 6 million people in Cisalpine Gaul in the early Empire.

We are therefore faced with the task of manipulating our formula in such a way as to obtain a northern population roughly three times higher than the total implied by the low count. Since the number of hectares occupied by agglomerations covering 20 or more hectares cannot have been much higher than 1750, this can only be done by varying the number of people per urban hectare and/or the rate of urbanization. Just by way of illustration, I present four scenarios in which these two variables have been adjusted in such a way that the requirements of the high-count model for Cisalpina are met.

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<sup>91</sup> Scheidel (1996, 167).

<sup>92</sup> 1.25 million citizens abroad: Lo Cascio (1999, 164); 2 million: Frank (1924, 333).

<sup>93</sup> Above at note 81.

<sup>94</sup> Lo Cascio and Malanima (2005, 9). Kron (2005, 458–9) suggests that early-imperial Cisalpina should have held at least 75% of the peninsular population, which works out as 42.9% of the total population of mainland Italy.

1. 2,000+ urban ha,<sup>95</sup> 450 persons/urban ha, and 15+0% in towns
2. 1,900 urban ha, 300 persons/urban ha, and 9.5% in towns
3. 1,850 urban ha, 240 persons/urban ha, and 7.4% in towns
4. 1,750 urban ha, 150 persons/urban ha, and 4.4% in towns

In other words, it is entirely possible to fit 6 million people into Cisalpine Gaul, but only at the cost of making this area very sparsely urbanized or by giving its towns a very high urban population density.

The former alternative is directly at odds with Lo Cascio's assumption that between 15% and 20% of the population of Augustan Italy lived in towns. As I have noted, it also sits very uneasily with the likelihood that a consistent pattern of elite-residence in towns resulted in a relatively high urbanization rate.

The second alternative raises the question as to why the young towns of Roman North Italy should have had population densities twice or three times as high as most of their counterparts in Renaissance Italy. One possible answer is that the towns of Roman Cisalpina had far more three- and four-storey buildings than their late-medieval and early-modern successors. As we have seen, however, the archaeological evidence relating to the urban make-up of the northern towns points in precisely the opposite direction.

By way of conclusion I can only repeat what I said at the beginning of this paper. We do not know and we shall never know how many people lived in Cisalpine Gaul in the late Republic and early Empire, and we shall never be able to disprove the high count for this area once and for all. Nonetheless it is my contention that my investigations into the towns of the North have provided ancient historians with some good reasons for thinking that the high-count interpretation of the republican and Augustan census figures is unlikely to be correct.

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<sup>95</sup> With 450 persons/ha all settlements covering more than 6.7 ha would have had more than 3,000 inhabitants. Since many *uici* were larger than 6.7 ha, the number of 'urban' hectares implied by model 1 is probably much higher than 2000. With 150 persons/ha only settlements covering 20 or more hectares can be regarded as 'urban'.

## APPENDIX I

### CITIES AND TOWNS IN EARLY-IMPERIAL CISALPINA

Note: most of the data for Aemilia, Liguria, Cispadana, and Venetia have been drawn from Brecciaroli Taborelli (2007), Calvani (2000a), Panero (2000), and Cavalieri Manasse (1987). Conventi (2004) is useful for towns founded in the final decades of the Republic but does not consider the development of suburbs around earlier foundations. Where multiple figures are given, the figures which are underlined have been used in my calculations. Communities which are thought to have received the citizenship in 90 BC are marked with an asterix.

#### I. *Very important towns (> 40 ha) (15 towns)*

Alatinum: according to Tombolani (1987, 324) Alatinum reached its maximum extent in the first century AD, when the *pomerium* enclosed an area of 120 ha. In addition to this there is evidence for a substantial suburb outside the northern gate, which began to develop in the early Augustan period (Tombolani 1987, 335–6). Up until the final years of the Republic, however, the town seems to have been bounded on its eastern side by the Sioncello canal (Tirelli 2003, 37; Cresci Marrone and Tirelli 2007, 62). If we assume that the area within the late-republican *pomerium* was entirely built up, it follows from this that Alatinum covered *c.* 80 ha in the late 30s BC.

\* Ariminum: according to Conventi (2004, 49) walled area 41 ha at the time of foundation (not 34 ha as suggested by Beloch 1886, 487). Judging from the map in Ortalli (1995, 470) there was no habitation outside the republican wall, within which room was found for a theatre and amphitheatre of early-imperial date.

\* Aquileia: according to Conventi (2004, 78) walled area 41 ha at the time of foundation. There can, however, be no doubt that the town had become considerably larger in the time of Augustus (Bonetto 1998, 183–4). It continued to expand in the early Empire, when it became a transit centre for the armies of the upper Danube (e.g. Chilver 1941, 57), and there are good reasons for thinking that already by the mid-second century AD the area covered by the suburbs had

- become considerably larger than that enclosed by the republican wall (Verzár-Bass 2001; Sotinel 2005, 13). In any case the wall of the third century AD enclosed *c.* 100 ha (Jäggi 1990, 163 n. 15). In Late Antiquity the town came to cover more than 125 ha (Verzár-Bass 2003, 74–5). In the light of these data it seems reasonable to give late-republican Aquileia *c.* 70 ha (cf. Beloch 1886, 487: 64 ha).
- Augusta Praetoria: 41.7 ha according to Conventi (2004, 149). Cf. Beloch (1886, 487: 41.4 ha). The town was founded in 25 BC.
- Augusta Taurinorum: 54.7 ha according to Conventi (2004, 145). It remains unclear whether the town was founded in 29 BC or 27 BC (Chilver 1941, 21). The figure of 47 ha given by Beloch (1886, 487) is too low.
- \* Bononia: according to Conventi (2004, 70) inhabited area *c.* 50 ha at the moment of foundation, but the detailed discussion by Scagliarini (1991, 88) makes it clear that the figure of 50 ha actually refers to the size of the inhabited area in the early Empire. Strangely enough, no trace of a wall of republican date has been detected so far. Beloch's figure of 83 ha (Beloch 1886, 487; cf. 1898, 272) is much too high. Cf. also the map in Ortalli (2000, 440) and Garnsey (1998, 127 n. 76), who gives Bononia 60 ha within the walls.
- Brixia: walled area 36 ha according to Beloch (1898, 272), but *ca.* 50 ha in the age of Augustus according to Conventi (2004, 101). Within this area the hill of the *arx* was not entirely built up. See the map in Büsing-Kolbe and Büsing (2002, 33). On the other hand the town seems to have acquired a substantial *suburbium* at an unknown date (Tozzi 1974, 37). I have therefore retained Conventi's figure. It is unclear to me why <http://www.bresciainvetrina.it/bresciastoria/epocaromana.htm> gives Brescia only 29 ha in the Flavian age, when the town is supposed to have reached its maximum extension. For Brescia's growth under the Flavii see Rossi (2003, 27).
- \* Cremona: walled area 30 ha at the time of foundation according to Conventi (2004, 55), but inhabited area *c.* 55 ha in the early Principate judging from the map in Passi Pitcher and Mariani (2007, 216). For Cremona's growth during the late Republic and early Empire cf. Tac. *Hist.* 3.34.
- Hasta: *c.* 42–45 ha according to Panero (2000, 98). Cf. the map in Mercado (2003, 12).
- Mediolanum: *c.* 80 ha in the late Republic and early Empire according to Ceresa Mori (1995, 471) and Conventi (2004, 182). For the development of suburbs from the time of Augustus onwards see Cortese

(2007). The much later wall of Maximianus Herculius enclosed 133 ha (Beloch 1886, 487).

- \* Mutina: according to Conventi (2004, 73) the inhabited area was 40 ha. at the moment of foundation, but according to the map in Giordani (2000, 424) *c.* 48 ha if later growth is included. These figures are considerably lower than the figure of 65 ha given by Beloch (1898, 272).

Patavium: as Beloch pointed out, the two branches of the river Meduacus enclosed an area of *c.* 85 ha (Beloch 1898, 272). However, judging from the location of the extra-urban graveyards and a number of boundary stones the area enclosed by the late-republican *pomerium* may have been as large as 200 ha (Gasparotto 1951, 83–91; Tosi 1987, 160–161; Ruta Serafini et al. 2007, 67 and 78), and there can be no doubt that a substantial part of the eastern part of this area was inhabited (Tosi 1987, 161; cf. Livy 10.2). If we assume that about two thirds of the area within the late-republican *pomerium* were built up in 28 BC, Patavium covered *ca.* 130 ha at this date.

- \* Placentia: according to Conventi (2004, 58) walled area 38.4 ha (cf. Mansuelli 1971, table iv; Dall’Aglio 2007, fig. 4), but inhabited area *c.* 43 ha in the late Republic and early Empire according to the map in Calvani (2000d, 378). For a brief discussion of Placentia’s growth see Calvani (2000d, 376).

(\*) Ravenna: *c.* 60 ha according to the map in Maioli (2000, 530).

Verona: walled area *c.* 46.5 ha in *c.* 50 BC (Conventi 2004, 115), but there appears to have been a substantial *suburbium* already under Augustus: see Cavalieri Manasse and Bruno (2003, 47). I have therefore followed Panero (2000, 205) in giving Verona *c.* 50 ha.

Total: 898.4 ha

Average: 59.9 ha

## II. Important towns (20–40 ha.) (31 towns of which 29 known)

Alba Pompeia: walled and inhabited area 38.6 ha under Augustus according to Conventi (2004, 96).

Albintimilium: inhabited area *c.* 24 ha according to Conventi (2004, 98).

Atria: although very little is known about Atria’s lay-out and size (De Min 1987, 259–262), the town had a theatre, an amphitheatre, and a basilica (Tosi 2003, 503–506). As pointed out by Fogolari and Scarfi

(1970, 47), these clues suggest that Atria was more important than some scholars have suggested. De Min (1987, 259 and 262) interprets the archaeological data as indicating that the town reached its acme in the first century AD.

Augusta Bagiennorum: area enclosed by the early-imperial *vallum* 21 ha according to Conventi (2004, 151). Cf. the map in Preacco Ancona (2007, 272).

Aquae Statiellae: inhabited area *ca.* 28–30 ha according to Panero (2000, 55).

Ateste: although Str. 5.1.8 classifies Ateste as a small town, there are reasons for thinking that it had a substantial population in early-imperial times. The exact size of the area enclosed by the (pre-Roman) *pomerium* cannot be determined, but the location of the suburban cemeteries suggests that it was large (Baggio Bernardoni 1987, 221–223). From the map at the end of Tosi (1992a) it appears that remains of houses and *tabernae* are scattered over more than 50 ha, but the town seems to have been unwalled (Baggio Bernardoni 1992, 310) and to have consisted of three densely built-up nuclei which were surrounded by a scatter of isolated houses. Many of the domestic and non-domestic buildings which have been excavated were constructed during the first century of the Principate. Part of the explanation for Ateste's growth during this period may be the arrival of numerous veterans in or about 30 BC (Keppie 1983, 195–6). There are indications that Ateste declined in the second half of the second century AD (Baggio Bernardoni 1992, 318).

Bergomum: according to Scalvini et al. (1987, 7) modern estimates of the walled area of early-imperial Bergomum have ranged from 18.48 ha (Angelini) to 23.2 ha (Degrassi). I have used the latter figure, even though Angelini's estimate seems to be based on more accurate information. Note that there is nothing to suggest that late-republican Bergomum extended into the plain below the 'città alta' (cf. Fortunati et al. 2007, 311).

\* Caesena: small according to Str. 5.1.11. Built-up area *c.* 20 ha. according to the map in Maioli (2000, 496).

\* Claterna: small according to Str. 5.1.11. *c.* 22 ha according to the map of Ortalli (2000, 456). *c.* 18 ha plus some habitation along the roads according to the website of the Soprintendenza per i Beni Archeologici dell'Emilia-Romagna (<http://www.archeobo.arti.beniculturali.it/claterna/claterna.htm>).

Comum: walled area *c.* 25 ha in 58/57 BC according to Conventi (2004, 104).

- Concordia: although Str. 5.1.8 classifies Concordia as a small town, its walled area covered *c.* 40 ha under Augustus; see Conventi (2004, 133) and the map in Croce da Villa (1987: 394).
- \* Dertona: walled area *c.* 29 ha judging from the maps in Panero (2000, 22) and Zanda (2007, 157).
  - \* Eporedia: one of the *firmissima municipia* of Transpadana according to Tac. *Hist.* 1.70. Walled area *c.* 26 ha judging from the map in Panero (2000, 187). The suburbs between the eastern gate and the amphitheatre are almost certainly to be dated to the second half of the first century AD. See Mercado (1990, 453).
  - \* Faventia: small according to Str. 5.1.11, but built-up area *c.* 22 ha according to the map in Guarneri (2000, 472).
  - \* Forum Corneli: small according to Str. 5.1.11. This fits the situation in the first half of first century BC, when the town covered *c.* 10 ha (Susini 1957, 95). But under Augustus it came to cover *c.* 32 ha. See Mansuelli (1957a, 142–3), and the map in Curina (2000, 464).
  - \* Forum Livii: inhabited area *c.* 30 ha according to the map in Guarneri (2000, 478).
- Genoa: *c.* 25 ha judging from Melli (2003, 133, 137). Cf. Heers (1979, 372): Roman Genua smaller than 30 ha.
- Industria: *c.* 25 ha according to the nineteenth-century map reproduced in Zanda (1990, 566). At least some of the assumptions on which this map was based were confirmed by later research. See Zanda (1990, 564 n. 8) and Mercado (2003, 10).
- Laus Pompeia: walled area *c.* 20 ha judging from the map in Tozzi and Harari (1990, 528).
- Libarna: inhabited area 23 ha under Augustus according to Conventi (2004, 89).
- Novaria: *c.* 35 ha according to Panero (2000, 205). Cf. the map in Mercado (1990, between pages 462 and 463) and the valuable discussion by Spagnolo Garzoli et al. (2007, 120).
- Opitergium: although Str. 5.1.8 classifies Opitergium as small, its built-up area covered *c.* 35 ha under the Empire. See Busana (1996, table 2).
- \* Parma: *c.* 30 ha in the time of Augustus according to the maps in Calvani (2004b, 394) and Catarsi (2007, 98). The figures of 16–18 ha and 21.6 ha given by Tozzi (1974, 55) and Conventi (2004, 76) are far too low for the early Empire.
- Pedo: inhabited area *c.* 22 ha according to Panero (2000, 229).

Pola: *c.* 20 ha judging from Letzner (2005, Abb. 16). The figure of 16.5 ha given by Beloch (1886, 487) seems somewhat low.

Pollentia: inhabited area *c.* 20 ha judging from the map in Panero (2000, 135), *c.* 16 ha according to the different reconstruction offered by Preacco Ancona (2007, 268), but *c.* 34 ha according to Conventi (2004, 92). Although Preacco Ancona's analysis of the topography seems sound, I have used the latter figure.

\* Regium Lepidum: small according to Str. 5.1.11, but the built-up area covered *c.* 21 ha under the Empire; see the map in Lippolis (2000, 412).

Tarvisium: judging from the map in Malizia (1987, 350) Tarvisium covered *c.* 25 ha in the first century AD. If Palmieri (1980, 170–2) is right in conjecturing that the NW quarter of the town was built in or after the Augustan period, Tarvisium covered a considerably smaller area in 28 BC, but the evidence supporting this hypothesis is fragile (Malizia 1987, 350–351). Since archaeological work carried out between 1987 and 1999 has brought to light traces of habitation at sites previously considered to be extra-urban (Ravagnan 2003a, 349), I have given early-imperial Tarvisium 30 ha.

Ticinum: walled area 38.4 ha under Augustus according to Conventi (2004, 111).

Vercellae: one of the *firmissima municipia* of Transpadana according to Tac. *Hist.* 1.70. According to some specialists the town covered only 11–13 ha in the early Empire, but Panero (2000, 219) suggests that it may well have been at least twice as large. Cf. Spagnolo Garzoli et al. (2007, 112) for the view that the area covered by the town must have been “più ampia...rispetto a quella definita dalla tradizione degli studi”. I have given Vercellae 30 ha.

Vicetia: although Str. 5.1.8 classifies Vicetia as small, the town wall of the second half the first century BC enclosed *c.* 28 ha; see Conventi (2004, 118) and the cautious discussion of the whereabouts of the late-republican wall and *agger* by Rigoni (1987a: 110–112).

Total known = 799.2 ha

Average for known towns: 27.6 ha

× 31 = 854.3 ha

### III. *Unimportant towns (< 20 ha) (32 towns of which 17 known)*

Acelum: if we accept the theory that Roman Acelum had a town wall, and that this wall followed exactly the same line as its medieval successor (Ravagnan 2003b, 432–3), the area within the wall was *c.* 10 ha. However, the lay-out of medieval and modern Asolo suggests that only part of this area was built up. Although these indications do not permit us to determine the area covered by late-republican and early-imperial Acelum (Furlanetto 1987, 427–428), the small size of its theatre (diameter of *cavea* = 54 m.) reinforces the impression that it was very small.

Albingaunum: area enclosed by the town wall of *c.* 80–*c.* 70 BC no more than 7 ha. See the maps in Massabò (2004a, 461–2; 2004b, 48). Cf. also Gambaro (1999, 87–8).

Auriate (Forum Germanianum): small according to Panero (2000, 257).

Bellunum: very little is known about the urban layout of early-imperial Bellunum, but if the northern section of the early-imperial *pomerium* coincided with the line of the medieval town wall running between the porta Dante and porta Doiona (thus Zanovello 1987, 446–9 with map), the town covered no more than 11 ha. If the *pomerium* ran along the northern edge of the Piazza dei Martiri, as indicated on the map on [http://www.webdolomiti.net/storia/storia\\_origini\\_romani.htm](http://www.webdolomiti.net/storia/storia_origini_romani.htm), this figure rises to 13 ha.

Beria

\* Brixellum: *c.* 12.5 ha according to the map in Calvani (2000e, 408).

Ceva

Cemenelum: between 15 ha and 20 ha under the Empire according to Arnaud (2007, 175). It seems reasonable to use the former figure for 28 BC.

Dripsinum

Feltria: according to Rigoni (1987b, 450–451) Roman Feltria was bounded by the river Colmeda to the west, by the Via Garibaldi and Via Nassa to the south and (probably) by Borgo Ruga to the east. This would give the town *c.* 15 ha.

\* Fidentia: inhabited area *c.* 8 ha according to the map in Calvani (2000c, 390). Judging from the map in Catarsi (2007, 99) the town's four *insulae* occupied only 3.5 ha, but some dwellings were located outside this area.

Forum Fulvii/Valentia: Forum Fulvii was originally a *vicus* which became more important than Valentia. Panero (2000, 253–256, 261) classifies both Forum Fulvii and Valentia as ‘minor centres’. Cf. Zanda (2007, 157–9) for the view that Forum Fulvii had a relatively small territory and did not have any large public buildings.

Forum Iulii: *c.* 12.5 ha according to Borzacconi et al. (2007, 304–5).

\* Forum Novum: *c.* 2 ha according to the map in Calvani (2000f., 548).

Forum Popilii: *c.* 6 ha. according to the map in Guarnieri (2000, 488).

\* Forum Truentinorum

Forum Vibii: small according to Panero (2000, 258–260).

Iria

Iulium Carnicum: very small according to Mansuelli (1971, 78–9). The republican finds on the map in Vitri et al. (2007, 46) are scattered over *c.* 5 ha, but the data are so sparse that it would be unwarranted to take this figure as representing the extent of the urban area. Note that Iulium Carnicum may not have become a *municipium* or *colonia* before the Augustan period (Vitri et al. 2007, 47).

Laumellum: according to Maccabruni (1990) the size of early-imperial Laumellum (classified by her as a *mansio*) was approximately equal to that of the area enclosed by the late-antique walls. This would give the town *c.* 9.5 ha; cf. the map *ibid.* 142. Gabba (1990) classifies Laumellum as a *municipium*.

Mantua: according to a brief article entitled ‘civitas etrusco-romana’ on the internet, which seems to summarize the main findings of Velardi (2003), Roman Mantua covered *c.* 7.5 ha. Cf. Mart 14.195: *parva Mantua*.

Nesactium: walled area *c.* 8 ha judging from Rosada (1999, Tav. 1) and Letzner (2005, Abb. 91).

\* Otesini

Parentium

Potentia/Carreum: according to Panero (2000, 75) the inhabited area was somewhat larger than the area bounded by the modern Via Palazzo di Città, Via Principe Amedeo, Via Silvio Pellico, and Via Vittorio Emanuele II. The area enclosed by these streets is *c.* 12 ha. If one accepts the rather different reconstruction of the urban layout offered by Zanda (2007, 161), the town covered *c.* 13 ha in the first century AD. I have given Roman Potentia 15 ha.

Segusio: The walls of the third century AD enclosed *c.* 15 ha (Panero 2000, 248). During the Principate the area of the medieval Deserto

(outside the third-century wall) was also occupied by buildings, but none of these predates the last-but-one decade of the first century BC (Barello 2007, 262–265). Since we do not know how much of the area enclosed by the wall of the third century was built up in 28 BC, the only conclusion which can be safely drawn is that Susa covered less than 15 ha at this date. Note that Segusio had a very small amphitheatre (arena: 44 × 36 m), comparable in size to those of Tridentum (62 × 48 m) and Veleia (34.3 × 24.9 m).

\* Tannetum: village with status of town

Tergeste: walled area not larger than 11 ha judging from the maps in Maselli Scotti (1990, 624) and Conventi (2004, 139). Note that Conventi's figure for the length of the town wall (3,000 m) is much too high even for medieval times, when the town perimeter was *c.* 1,700 m. Although the entire area between the Colle San Giusto and the sea appears to have been built up between the time of Augustus and the mid-first century AD (Morselli 2007, 190–1), there can be no doubt that Tergeste was a small town in 28 BC (cf. Maselli Scotti 1990, 626). I have assumed that only the area enclosed by the town wall was inhabited in 28 BC.

Tridentum: walled area 13 ha in the time of Augustus according to Ciurletti (2003, 37) and Conventi (2004, 142). For the growth of suburbs in the late first century AD, see Ciurletti, *ibid.*, and Bassi (2007, 55).

Vada Sabatia: although Cicero (*ad Fam.* XI.13.2) refers to Vada as a *locus*, the presence of *quattuorviri* suggests that it became a *municipium* in 89 BC (Bulgarelli 2007a, 331). A concentration of archaeological finds suggests that the centre of the Roman town was at or near the Piazza San Giovanni Battista (Bulgarelli 2007b, 183), but its layout is completely unknown. However, as pointed out by Bulgarelli (1997–8, 299), the extensive cemeteries of the first three centuries AD demonstrate that it was far from negligible in this period. This inference is supported by the presence of many farm buildings in the town's territory (Bulgarelli 2003, 11–2; 2007b, 187). The fact that Vada occupied a strategic position in the road system (especially after the building of the *via Iulia Augusta*) and possessed a good harbour (Bulgarelli 2007, 18) is likely to have contributed to its prosperity. Taken together, these indications suggest that Vada became one of the larger 'small towns' of Liguria in the early Empire.

Vardagate: according to Panero (2000, 265) Roman Vardagate was bounded by the modern Via Luitprando, Piazza Mazzini, Via

Duomo, and Piazza Rattazzi. This would give it *c.* 3 ha. There was, however, some additional habitation near this urban nucleus (*ibid.*).

I have therefore given this town *c.* 5 ha.

Veleia: very small according to Mansuelli (1971, 78–9). *c.* 10 ha judging from the provisional map in Calvani (2000a, 376: map 18).

Total known: 170 ha

Average for known towns: 10 ha.

× 32 = 320 ha.

Total: 2072.7 ha.

## APPENDIX II

POPULATION FIGURES FOR LARGEST  
NORTHERN CITIES, AD 1600

(figures taken from Bairoch 1988 and Malanima 1998)

Alessandria	14,000	Fossano	5,000	Reggio Em.	11,000
Asti	9,000	Genova	65,000	Rimini	8,000
Bagnacavallo	5,000	Giovazzino	9,000	Rovigo	4,000
Bassano	7,000	Gorizia	5,000	Sant'Angelo	4,000
Belluno	5,000	Imola	6,000	Savigliano	9,000
Bergamo	24,000	Lodi	14,000	Savona	10,000
Bologna	63,000	Lugo	7,000	Torino	22,000
Brescia	40,000	Mantova	31,000	Tortona	5,000
Busca	5,000	Milano	120,000	Treviglio	6,000
Casale Monf.	10,000	Modena	18,000	Treviso	13,000
Cento	5,000	Modica	16,000	Trieste	5,000
Cesena	7,000	Mondovi	11,000	Udine	14,000
Chieri	7,000	Monza	9,000	Valenza	5,000
Chioggia	9,000	Monselice	3,000	Venezia	140,000
Como	12,000	Nicosia	15,000	Vercelli	6,000
Conegliano	5,000	Novara	8,000	Verona	49,000
Cuneo	7,000	Padova	36,000	Vicenza	36,000
Crema	11,000	Parma	23,000	Vigevano	8,000
Cremona	40,000	Pavia	25,000	Voghera	5,000
Faenza	12,000	Piacenza	33,000		
Ferrara	33,000	Pinerolo	5,000	Total:	1,173,000
Forli	11,000	Ravenna	8,000		

Since North Italy had a population of *c.* 5.4 million in AD 1600, the urbanization rate implied by these figures is *c.* 21.7%. However, since at least some towns having populations of between 3,000 and 5,000 are not included in Bairoch's lists (cf. Musgrave 1995, 254), the real rate must have been somewhat higher. If we take no account of cities having fewer than 5,000 inhabitants, the urbanization rate drops to *c.* 18.4% (Lo Cascio and Malanima 2005, 108).

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II

CENSUS FIGURES AND POPULATION



# COUNTING ROMANS\*

Saskia Hin

## I. *Introduction*

What was the size of the population of Roman Italy? The enigmatic character of much of the information in the ancient sources prevents a satisfactory answer to this fairly basic question. The solutions put forward by modern scholars are extremely divergent. The estimates of those presenting a ‘high count’ and those favouring a ‘low count’ are of an entirely different order of magnitude, which has significant implications for our understanding of many aspects of Roman economy and society.

A factor of approximately three divides the low-count and the high-count interpretations of our main source, the Roman census figures.<sup>1</sup> This is the logical consequence of the consensus on the one hand that the republican census tallies represented, or aimed at representing, all adult male citizens, and on the other hand very divergent understandings of the Augustan figures: ‘high counters’ think that the Augustan figures were a continuation of this practice (in which case they would comprise all adult males),<sup>2</sup> while the ‘low counters’ prefer to interpret the steep increase in the Augustan figures as indicating a significant change in the way the census results were recorded or reported.<sup>3</sup>

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\* I should like to thank the members of the Leiden VICI project for their share in adding references, comments, and criticism. I am furthermore indebted to Walter Scheidel for his substantial contributions at various stages. John Rich kindly shared with me his 1978 unpublished draft in which he argues that the republican census figures are to be taken as referring to adult male citizens *sui iuris*, as well as a letter to him from Brunt. I have also profited from his comments on an earlier draft of this paper.

<sup>1</sup> There is little point in listing the sequence of figures here: one may find them in Brunt (1971/1987<sup>2</sup>, 13–14), Nicolet (1976b, 69), or Toynbee (1965, 438–40 [including the Claudian figures]). For a visual presentation of the figures see Scheidel in this volume.

<sup>2</sup> Frank (1924); Wiseman (1969); Lo Cascio in several publications (esp. 1994, 1999a, 1999b, 2001a); Kron (2005). Morley (2001) explores the potential of the high count in an attempt to open up debate.

<sup>3</sup> This view is set out most elaborately by Beloch (1886, 370–8) and Brunt (1971/1987<sup>2</sup>, 113–20).

Contrary to previous practice, Augustus is thought to have started to include women and children in addition to adult males. As it stands, the debate is locked into an either/or dichotomy between these opposing interpretations. It is perhaps fair to say that the low count tends to be favoured over the high count by most scholars. Traditionally, low counters have perceived the last two centuries BC as a period of demographic stagnation or decline in the free citizen population of Roman Italy. However, a tendency to view the political and social developments of this period as best explained against a background of population increase rather than stagnation or decline has recently emerged. Luuk de Ligt has sought to reconcile the notion of demographic growth with that of a low population: a reinterpretation of the Polybian figures for the number of allies or a lower alternative estimate for the number of inhabitants of Gallia Cisalpina allows the republican census figures to represent growth in the free Roman population.<sup>4</sup> Taken together, we are now faced with two competing scenarios of population size, and moreover three—or perhaps rather four—developmental trends: rapid growth, moderate growth, decline and/or stagnation.

## II. *A brief outline*

This paper will not repeat the extensive analysis of the implications of these competing scenarios of absolute and relative population size for our understanding of Roman and premodern history as presented by Walter Scheidel elsewhere in this volume. Instead there are two aims.

First, in sections 3–5 I shall examine the assumptions underlying current interpretations of the Polybian army figures and the republican census tallies in order to answer two important questions: can we consider the former as independent evidence corroborating the low-count interpretation of the census figures? And did the census figures in fact represent, or aim to represent, all adult male citizens? I shall argue that in assuming that they did, we accept a number of logical inconsistencies.

Second, in sections 6–10 I explore the possibility of an alternative reading that allows one to adopt an intermediate position between the current scenarios of small and large populations. The possibility of such

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<sup>4</sup> De Ligt (2004, esp. 731–7), based on his inaugural lecture (2003).

a ‘middle count’ is opened up by interpreting the republican census figures as representing adult male citizens *sui iuris*, and the Augustan ones as representing all citizens *sui iuris* (regardless of whether they were men, women, or children). In section 11 I argue that this interpretation allows us hypothetically to set a maximum level for the population of Italy under Augustus of about 10 million. At the same time, there are a number of reasons for suspecting that the actual population was considerably smaller than this hypothetical maximum. Section 12 examines some of the factors which are likely to have worked in this direction. However, because the nature of the surviving evidence does not permit us to gauge the quantitative impact of these factors, it is not possible to be more precise than to conclude that the number of people inhabiting Augustan Italy must have fallen somewhere between the current low count and the *c.* 10 million implied when interpreting the Augustan *capita civium* as citizens *sui iuris*. The explanation for this is that there is a range of multipliers which may be used to calculate the total citizen population, since the share of the *sui iuris* in the total population is not determined purely by the prevalent demographic regime. To a certain extent this holds for any interpretation. The phenomenon of under-registration is a well known variable, and has received much attention. However, as I hope to show, there are many more of these variables. They include migration, territorial expansion, overcounting, nuclearization of households, the inclusion of freedmen with a different demographic profile, and changes in the proportion of women who were not *in potestate*. Each of these factors affects the relationship between the number recorded in the census and the actual population; but it seems to me that they all pull in the same direction: they lower the multiplier needed to get from the census population to the total population.

### III. *Manpower in Polybius: a new scenario?*

The debate over Roman Italy’s population size is rooted in philological evidence. The numerical data provided by the census figures give us a time series, but are limited to the population with rights of citizenship, and are difficult to interpret. For these reasons much attention has been given to Polybius’ survey of Roman and allied manpower resources in 225 BC. This survey contains the only numerical evidence for the size of the non-Roman population of Italy. Unfortunately, there are

some compelling arguments for serious skepticism towards the figures Polybius gives us.<sup>5</sup> Even for those who wish to accept the tallies as broadly representative of a genuine historical situation, problems of interpretation remain. This is reflected in the way Polybius' account has been employed to buttress different views on Italy's population and demographic development over time. Here I focus mainly on the view upheld by the most prominent proponent of the low count, Peter Brunt, and on the recent reinterpretation suggested by Luuk de Ligt.

Polybius' rather vague description accompanying the population numbers given in 2.24.10 has often been interpreted as referring to 'men able to bear arms'.<sup>6</sup> But which Romans and allies would have been considered 'able to bear arms'? Brunt suggested that the figures, where they refer to allies, comprise *iuniores* only, whereas the figure given for Roman men includes all males.<sup>7</sup> Against this De Ligt has argued that we should take both of these categories, allied and Roman, as consisting of *all* free adult males.<sup>8</sup> This view has the advantage of allowing moderate population growth within the margins of the low count: since it suggests that the number of allies who were later enfranchised and included in the census was lower than formerly thought, one may adhere to the low count without having to accept an overall stagnation or even decline in the number of free Roman citizens between 225 BC and 28 BC.<sup>9</sup> In the context of the ongoing debate over population development in Roman Italy during the late Republic, such moderate growth at least fits the surviving census tallies for this period, which, if taken at face value, suggest a gradual demographic expansion of the citizenry. Equally importantly, it detaches the phenomenon of natural population growth

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<sup>5</sup> For comments on the suspiciously simple arithmetical relationships between the various figures see Scheidel (2004, 3–4; 2006, 209). Henige (1998, esp. *c.* 1 and *c.* 15) provides many examples that show how figures in the context of military conflict are highly susceptible to manipulation and are often little more than propaganda.

<sup>6</sup> Cf. e.g. Brunt (1971/1987<sup>2</sup>, 44 f.) and the translation given in the Loeb edition (1921).

<sup>7</sup> Brunt (1971/1987<sup>2</sup>, 52–3 with table V), following Beloch (1886, 363–4). Baronowski (1993, 188) shares his view.

<sup>8</sup> De Ligt (2003, 8), (2004, 732–7).

<sup>9</sup> Note that De Ligt not only advances a reinterpretation of the manpower figures given by Polybius but also contemplates the possibility that Gallia Cisalpina had *c.* 900,000 rather than 1.4 million inhabitants. As he points out, accepting only one of these possible revisions leads to a scenario of demographic stagnation for the free Italian population (with about 4 million free people of citizen status living in Italy both in 225 BC and in 28 BC) and of moderate overall growth for the citizen body as a whole if we take account of citizens living abroad.

from the historical implications of the high count. Since it is precisely the need to jump from one end of the population scale to the other that sits uneasily with the notion of population growth, the thesis that the allied figures are to be interpreted as comprising all adult males removes two improbabilities which many would like to discard: that of population decline, and that of an extremely large population residing in the Italian heartland. But, obviously, such a convenient outcome cannot in itself prove the validity of the interpretation on which this scenario of moderate growth is based.

#### IV. *Wanted: men for war. The allied forces in 225 BC*

In actual fact, it seems rather difficult to maintain that the allied figures mentioned by Polybius should each refer to all the free adult males of a specific ethnic group regardless of age. Brunt and others, notably Lo Cascio,<sup>10</sup> may have a case when they claim that the figures for the allied forces excluded *seniores*. Polybius tells us when bringing up his catalogue at 2.24.10 that these are the numbers of soldiers ‘the registers brought up’.<sup>11</sup> Clearly, he builds on his preceding account in 2.23.9, where he had explained who were to be found on the registers and why. The returned lists that he describes a little later as comprising ‘those able to bear arms’ (2.24.16)<sup>12</sup> had been requested by the Romans once they had learned that the Galatians were on their way. Fearing for their freedom, or lives, καθόλου δὲ τοῖς ὑποτεταγμένοις ἀναφέρειν ἐπέταξαν ἀπογραφὰς τῶν ἐν ταῖς ἡλικίαις, σπουδάζοντες εἰδέναι τὸ σύμπαν πλῆθος τῆς ὑπαρχούσης αὐτοῖς δυνάμεως—‘they ordered all those who were subdued to bring up registers of those who were *en tais hēlikiais*, as they were eager to know what the total of their forces amounted to’.

What evidence then do we have for the hypothesis that by *hoi en tais hēlikiais* (‘men able to bear arms’) Polybius means all adult men of the allied communities? To my knowledge the only passage in which we find the phrase ‘men able to bear arms’ in connection with a census registration of allied forces pleads against this inference. It tells us that

<sup>10</sup> Lo Cascio (1999a, 168).

<sup>11</sup> καταγραφαὶ δ’ ἀνηνέχθησαν Λατίνων μὲν ὀκτακισμῦρτοι πεζοί... etc., ‘the registers brought up were of Latins, 80,000 footsoldiers... etc.’.

<sup>12</sup> Plb. 2.24.16: σύμπαν πλῆθος τῶν δυναμένων ὅπλα βαστάζειν αὐτῶν τε Ῥωμαίων καὶ τῶν συμμάχων πεζῶν.

Caesar during his campaign in Gaul received a register of the Helvetii, written in Greek on *tabulae* that were found in their encampment. In it, those able to bear arms were registered separately from children, old men (*senes*), and women, as Caesar tells us: *quibus in tabulis nominatim ratio confecta erat, qui numerus domo exisset eorum, qui arma ferre possent, et item separatim pueri, senes, mulieresque*.<sup>13</sup> I should suggest that this indicates what we might expect the phrase ‘those able to bear arms’ to mean in a Roman context. If Caesar is describing a historical event, the passage tells us that, when translating the Greek documents, the Romans considered a group excluding the older men as ‘those able to bear arms’. If registration of the Helvetii, in Greek letters and on heavy tablets, was rather ‘a peculiar scenario’ of which ‘virtually nothing makes much sense’,<sup>14</sup> perhaps the most economical interpretation of this passage would be that Caesar’s story is based on the assumption that the age categories used by the Helvetii were identical to those used in Roman Italy.

More importantly though, the actual Greek terminology used by Polybius, *en tais hēlikiais*, occurs in a military context in some other instances which help to establish its meaning. From these parallels, mainly in Dionysius of Halicarnassus, three notions emerge: a) in a military context being ‘in the age’ means ‘being in the military age’;<sup>15</sup> b) those ‘having the military age’ form a group distinguishable from those under and those above the military age: *τοὺς ἔχοντας τὴν στρατεύσιμον ἡλικίαν ἀπὸ τῶν πρεσβυτέρων*;<sup>16</sup> c) the watershed between those of military age and those above it is explicitly set at age 45 by

<sup>13</sup> Caes. *Gal.* 1.29.

<sup>14</sup> So Henige (1998, 217), who puts forward a dazzling series of questions challenging the logic of this story in particular, as well as that of other accounts involving the reporting of statistics.

<sup>15</sup> E.g. Plb. 16.36.3; DH 3.65.4; 4.15.6; 5.75.4; 11.63.2; Thuc. 8.75; cf. also Lo Cascio (1999a, 168).

<sup>16</sup> DH 5.75.4. Brunt interprets this passage as showing that men of all ages are to be included in the definition ‘able to bear arms’. However, his conclusion is incorrect because he fails to make a distinction between the notions *hoi en tais hēlikiais* and *hoi en hēbēi*. We are told that *ἑπτακοσίοις πλείους εὐρέθησαν οἱ ἐν ἡβῆ Ῥωμαῖοι πεντεκαίδεκα μυριάδων. μετὰ τοῦτο διακρίνας τοὺς ἔχοντας τὴν στρατεύσιμον ἡλικίαν ἀπὸ τῶν πρεσβυτέρων* (‘the Romans who had arrived at the age of manhood were found to number 150,700. After that he separated those who were of military age from the older men’). Clearly, we should interpret this passage as telling us that the group of *hoi en hēbēi*, numbering 150,700 men, consisted of two subgroups: those ‘having the military age’ and those older than that. As such, rather than providing evidence for it, it pleads against the idea that Polybius refers to adult males of *all* ages when he refers to *hoi en tais hēlikiais*. Cf. Plb. 1.73.1, where those ‘in the age’ form only part of the citizens: *καθόπιζον δὲ τοὺς ἐν ταῖς ἡλικίαις τῶν πολιτῶν*.

Dionysius at 4.16.3: ‘here also he (i.e. Servius Tullius) distinguished between those who were over forty-five years old and those who were of military age’.<sup>17</sup> This is logically implied also by the combination of two other passages in Polybius, 6.19.2–3 and 6.19.5, which form part of his description of the Roman military system. Here, Polybius says first that soldiers must serve before reaching age 46; next he tells us that those *en tais hēlikiais*, being the ones liable for service, must present themselves for recruitment. Polybius’ concept of men ‘in the (military) age’ clearly does not include all men. Even though the men over 45 are ‘wall defenders’ in Dionysius, the definition of τῶν ἐχόντων τὴν στρατεύσιμον ἡλικίαν excludes the entire group of men over age 45. That standing on a wall to guard a city makes them soldiers by our definition—or even by the Roman one<sup>18</sup>—is irrelevant to the question as to what Polybius would mean by *hoi en tais hēlikiais*: even while doing so, these elderly men (the *presbuteroi*) are not considered by Polybius to be ‘in the military age’. Therefore in my view the οἱ ἐν ταῖς ἡλικίαις (‘those able to bear arms’) can be thought of more precisely as ‘men of military age’, i.e. excluding *seniores*.<sup>19</sup> As a consequence, we are back where we started: Brunt’s contention that the manpower figures given by Polybius for the allies are to be taken as referring to *iuniores*.

V. *A citizen a soldier, a soldier a citizen?*  
*The figures from 234 BC and 225 BC*

If Polybius’ figures for the allied forces do not include all men, but only men of a certain age, what about his figure for the Romans? In the recent past various views on this topic have been expressed. In this section I shall take a closer look at the merits and problems of the two most common interpretations. Of course, a re-examination of this issue leads us back to our main question concerning the census figures and their interpretation. For, as is well known, the Polybian figure for

<sup>17</sup> διελὼν δὲ καὶ τούτων τοὺς ὑπὲρ τετταράκοντα καὶ πέντε ἔτη γεγονότας ἀπὸ τῶν ἐχόντων τὴν στρατεύσιμον ἡλικίαν, δέκα μὲν ἐποίησε λόχους νεωτέρων, οὓς ἔδει προπολεμεῖν τῆς πόλεως, δέκα δὲ πρεσβυτέρων, οἷς ἀπέδωκε τειχοφυλακεῖν.

<sup>18</sup> Cf. DH 5.45.3. In the war against the Sabines the defence of the city of Rome is left to servants and those above military age: τοὺς ὑπὲρ τὴν στρατεύσιμον ἡλικίαν γεγονότας.

<sup>19</sup> This is in contrast to the concept of *en hēbēi*, which seems to refer to adult men (above age 17) in a general way, and occurs in the context of census reports. See e.g. DH 9.25.1 and above, n. 16.

Roman manpower in 225 BC is close to the census figure reported for 234 BC: Polybius records 273,000 Romans for 225 BC, while the census figure for 234 BC is 270,713. This is a neat fit. It is clear that however one wishes to interpret either figure, an explanation for the similarity of the other is required because of their numerical closeness and the short time span of only nine years between them. But all conceivable ways of reconciling these matters necessitate the introduction of subsidiary hypotheses that are ultimately arbitrary. This feature is shared by my own attempt to reconcile both figures, which I shall set out in the next section.

One way of accounting for the similarity between the figures is to interpret them both as giving us the number of *iuniores*. Since the figures for the allies most likely represent the *iuniores*, it is a natural assumption that the tally for the Romans in Polybius did the same, particularly because the figure for the ‘Romans and Campanians’ is included in the sequence of figures denoting the allies, and is not presented as referring to men belonging to a wider age group. Precisely for this reason Mommsen interpreted both the Polybian figure for the Romans and Campanians and the republican census figures as referring to *iuniores* only.<sup>20</sup> However, for those upholding the view that the census figures represent *iuniores* difficulties arise in regard to the interpretation of the Augustan census figures. If we read the Augustan figures in the light of the republican figures and interpret them as representing *iuniores*, we must assume an extremely large population under Augustus, since the figure of 4 million would need to be multiplied by an even larger factor than if it represented all adult males. If Augustus continued to register only men aged 17–46, his figures should be multiplied by a factor of 4.6, instead of 3.18, in order to calculate the total number of people of citizen status.<sup>21</sup> The population would in this case rise to 18,689,800, without considering the effects of assumptions made about the numbers of slaves, foreigners, and citizens overseas, the net balance of which would drive up even further the total of inhabitants

<sup>20</sup> Mommsen (1887<sup>3</sup>, 411 and n. 1).

<sup>21</sup> See table 1 below. One can derive from columns 1 and 2 that the share of men <17 is 37.06%. Therefore an all adult male assumption would imply that 62.94% of the male population was registered. This results in a multiplier of  $200/62.94 = 3.18$ . If *iuniores* (17–46) only are registered, the census presents 43.51% of the male population, and the multiplier rises to  $200/43.51 = 4.6$ . Cf. also n. 32.

of Roman Italy under Augustus.<sup>22</sup> The only alternative to this reconstruction would be to add the subsidiary hypothesis that Augustus did not take a conservative stance, but in fact changed the reporting basis for the census from *iuniores* to all adult men. Under this assumption the high count would result in 12,920,340 Romans (again exclusive of the net balance produced by adding slaves and foreigners and subtracting citizens living outside Italy proper).

For low counters the assumption that the republican census tallies counted only the *iuniores* entails an even larger decline of the free population during the last two centuries of the Republic than in Brunt's scenario. For if the republican census tallies counted only part of the adult male citizens, the initial total population must have been larger than assumed, while the estimate of the population under Augustus would remain unchanged. Moreover, as I shall argue in greater detail below, the Roman census had multiple aims, including keeping track of people liable for tax and registering men with voting rights—and these were groups wider than just the *iuniores*. Nor would a census register that included only *iuniores* suffice as a recruitment list.<sup>23</sup> More importantly, since the census formula states that *capita ciuūum* were counted, it is difficult to see how these would, without any further qualification, exclude the *seniores*, who were plainly *ciues* with *capita*. Therefore it seems difficult to maintain the thesis that both the Polybian figure and the census figure for 234 BC represent *iuniores* aged 17–46.

As an alternative the view that both figures must include *all* men has gained much support. This method, reconciling the two tallies and explaining their similarity in a straightforward manner, forces one to assume that Polybius made the distinction between those 'in' and those 'over' the military age in a state of confusion when he defined the latter category as consisting of men aged 45+, or else that he failed to clarify that *hoi en tais hēlikiais* meant something else for Romans than it did for allies. Both assumptions seem unlikely. An additional difficulty created by this interpretation is that one would need to assume also that when Polybius speaks of 'men able to bear arms', he considers all men as capable of doing so. It is true that *seniores* or older men in general were sometimes called up for service. References to the phenomenon

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<sup>22</sup> For a discussion of the problems associated with such a high population see Scheidel in this volume.

<sup>23</sup> Cf. below, section 9.

are found in several authors—among them those quoted earlier from Dionysius, and Livy 42.33.4.<sup>24</sup> But imminent danger does not turn all adult men into ‘men able to bear arms’. Military manpower cannot have been equal to the overall number of adult men with sufficient resources living in Italy. Given the difference between life expectancy and healthy life expectancy (HALE), we must take into account the fact that a substantial number of Roman men were physically unfit to serve. With rising age their numbers will have increased. And physical disability certainly was not limited to elderly males. The ancient world was disease-ridden, and many suffered either from (infectious) diseases or their lasting consequences. Since it has been estimated that the average individual spent up to one-sixth of his life in a state of disability or ill health, the number of men permanently unfit for service due to disease was far from insignificant.<sup>25</sup> Hansen’s recent—and in his words ‘conservative’—estimate is that we are talking in the order of 20% of all citizens for the age group between 20 and 49 years.<sup>26</sup> Among the elderly the percentage will undoubtedly have been higher.

If we are to think that the Polybian figures for military potential are lists of *all* adult free males, these lists would provide an overestimate of Rome’s own capacity.<sup>27</sup> I should prefer to assume that the Roman generals in 225 BC wished to have accurate information on the number of men (both Romans and allies) able to fight in defence of their territory. Some have inferred from the fact that the actual recruitment took place by means of a selection procedure (the *dilectus*) that we should conceive of the phrase *qui arma ferre possent* as expressing a purely theoretical capability. Only when the actual *dilectus* took place and the generals chose their new legionaries would incapable men be separated out. But this is unconvincing. It would surely have been highly inefficient to have all adult men show up at that point, and there is no reason why they

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<sup>24</sup> E.g. DH 4.16.3 and 5.45.3, where elderly men guard the city walls. Note that in 5.45.3 they are defined as τοὺς ὑπὲρ τὴν στρατεύσιμον ἡλικίαν γεγονότας, those beyond the military age. Livy 42.33.4 concerns the context of the Third Macedonian War, when the consuls proclaimed that *nec ulli qui non maior annis quinquaginta esset uacationem militiae esse*.

<sup>25</sup> On HALE, cf. e.g. Scheidel (2007a, 41).

<sup>26</sup> Hansen (2006, 5–6, 86) and, more elaborately, Hansen (1985, 16–21). Cf. also Beloch (1922<sup>2</sup> 3.1, 268), where he notes that army figures cannot be the same as male population figures since not all men are ‘waffenfähig’, and (1922<sup>2</sup> 3.2, 403), where he adds 25% to account for men unfit for service or unable to serve.

<sup>27</sup> Cf. Schulz (1937, 181): from a military perspective it would have been ‘sinnlos’ to include all men in the counts.

should have done so. Disabled men permanently incapable of serving and men exempted because they had already fulfilled their service obligations, or were too old, too poor, or indispensable for society<sup>28</sup> could already have been sifted out at an earlier stage, when they had to appear before the censors. It seems to me that when a list of ‘men able to serve’ was created, on the basis of the census declarations, it would already have been evident that some men would not be able to serve in any circumstances, or should not be called upon for service. Asking these people to show up for a *dilectus* would have been pointless, and would have added an unnecessary extra administrative and organizational burden. Limited to the assessment of cases that were not self-evident, the actual procedure of the *dilectus* could without wasting further time serve to decide which of *ei qui possent* in fact *poterant*. From a military perspective, the only manpower that counts is manpower that one may reasonably expect to be capable and available to serve. For this reason it is highly unlikely that the lists of ‘men able to serve’ in Polybius’ account represent all adult free males.

VI. *An alternative reconciling hypothesis:  
iuniores in Polybius and sui iuris in the census?*

I wish to propose a different scenario. Most current interpretations presuppose that the Polybian figure for military manpower and the census figure must represent the same population sample, because of their numerical similarity.<sup>29</sup> Lo Cascio has already pointed out that the apparent similarity between the census figure for 234 BC and the Polybian figure does not prove that both figures were *in pari materia*.<sup>30</sup> I agree with his objection to the traditional view, since it is methodologically unwarranted to interpret the mere fact that two figures are of the same order of magnitude as indicating that they denote a single entity. To begin with, the figures we have stem from divergent contexts:

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<sup>28</sup> Cf. Hansen (1985, 17–18) on exemptions for administrative and constitutional reasons among Athenian citizens.

<sup>29</sup> With the notable exception of Lo Cascio (e.g. 1999a, 167). He interprets the Polybian figures as referring to *iuniores* and holds that the census figure for 234 BC is of the same order of magnitude not because it also represents *iuniores*, but because large sections of the male population were not registered by the censors: *cives sine suffragio* were excluded, and only a small proportion of *proletarii* were registered.

<sup>30</sup> Lo Cascio (1999a, 168).

that of a population registration, and that of a military crisis. It may well be that these required or targeted different sections of the same base population that just happened to be similar in size. Of course, if one wants to hold that both figures denote *all* adult males, this argument does not work. But at any rate it is clear that censuses miss part of their target population whatever it might consist of, which implies that claims of totality are always false. Any population count needs to reckon with a margin of under-registration, which stands in negative correlation to the level of bureaucratic sophistication. Therefore it is far from impossible that the two figures both present us with only part of the adult male population, but each with different parts. I suggest that the Polybian figures might be *iuniores*, and the 234 BC figure adult males *sui iuris*. Since only this section of the adult male population was liable for taxation—adult men *alieni iuris* could not own property—and was responsible for declaring dependants, who did not need to appear before the censors themselves, the census figures might record this subsection of the population.<sup>31</sup>

How well does such a hypothesis work in reconciling the figures of 234 BC and 225 BC? Following life tables in order to establish the share of men in the age group of the *iuniores* (i.e. 17–45), the multiplier comes out at 4.6, and the total population in 225 BC should therefore be 1,245,280.<sup>32</sup> To estimate the share of adult males *sui iuris*, I assume that men became *sui iuris* upon the death of their fathers. Based on the kinship simulation by Saller, 50% of the total male population should consist of adult men *sui iuris* (cf. table 1 below).<sup>33</sup> Therefore the multiplier needed in order to reach a total population figure is 4. If the census figure for 234 BC presents us with adult men *sui iuris*, this implies a citizen population of 1,082,852. The discrepancy between the implied population sizes is 163,000, or a 15% increase from 234 BC to 225 BC. This is obviously too large to be the result of natural increase. Even if some of the difference could be accounted for in this manner, a discrepancy of at least 12% would remain.

<sup>31</sup> I argue for this interpretation in further detail below, notably in section 7.

<sup>32</sup> Coale and Demeny (1983<sup>2</sup>) West level 3 females,  $r = 0$ . Men aged 17–45 form 43.51% of the adult male population (as can be derived from columns and 1 and 2 in table 1 below). The multiplier required to calculate the total population should therefore be  $200/43.51 = 4.6$ .

<sup>33</sup> Saller (1994, 52 table 3.1c). Both Saller's simulation and my internal distribution of age groups are based on the mortality schedule as provided by Coale and Demeny (1983<sup>2</sup>) West level 3 (0% growth).

Table 1. Livy's census figure for 234 BC (270,713 males registered)

Age	Males (%)	% with living father	Prop. registered by household head	Prop. registered as <i>sui iuris</i> (?)	% of entire male population registered
0–16	37.06				
17–19	5.67	54	0.54	0.46	2.61
20–24	8.81	45	0.45	0.55	4.85
25–29	8.1	34	0.34	0.66	5.35
30–34	7.36	23	0.23	0.77	5.67
35–39	6.62	13	0.13	0.87	5.76
40–44	5.91	7	0.07	0.93	5.49
45–49	5.22	3		0.97	5.06
50–54	4.52	1		0.99	4.47
55+	10.73	0		1.00	10.73
<b>Total:</b>	100				50
<b>Multiplier:</b>	200/50 = 4				
<b>Total Roman population:</b> <sup>34</sup>	270,713*4 = 1,082,852				

A subsidiary hypothesis is required to explain why the census figure would have missed out an additional one-eighth or so—or perhaps even more<sup>35</sup>—compared with Polybius' figure. There seem to be several possibilities—perhaps the census figure is an underestimate, because it deliberately excluded the very poor (comprising only those *sui iuris* who were *assidui*, i.e. capable of payment), or because of a rate of under-registration that considerably exceeded that for the soldiers. In my view the most plausible or at least most promising explanation would be that it was not the census that missed comparatively more people, but that it was Polybius who overstated the military manpower figures. As stated above, figures that stem from a context of war are particularly sensitive to manipulation. We need not point to Polybius as the cause

<sup>34</sup> Note that adult males entirely missed by the census are not accounted for.

<sup>35</sup> That is, depending on the strength one wishes to attribute to the argument set out above that it is unlikely that all men of a certain age group were considered fit for service, the actual figure for the total Roman population implied by the number of *iuniores* listed should rise. With it, the discrepancy with the total population figure implied by the census figure should rise too, and thus also the divergence that needs to be explained.

of misrepresentation; it may well have been his sources in which the data were already skewed.

As Erdkamp has recently suggested, Polybius' portrayal of the situation in 225 BC as one of severe imminent danger may well be the result of his sources exaggerating in order to justify the Romans' passive stance towards Carthage in these years.<sup>36</sup> If Polybius' account is indeed affected by what in our days would probably be labeled propaganda, it is not unthinkable that our figures might be inflated by one-eighth or so. An inflated number of soldiers would fit the mechanisms with which Polybius explains history in general and especially the dominance of Rome. For, as set out in his preface, he considers the victory of the Romans to be the result of a 'striking and grand (...) spectacle', and wishes to demonstrate that the Romans had 'quite adequate grounds for conceiving the ambition of a world-empire and adequate means for achieving their purpose'. He explicitly tells his readers that he wants to explain 'by what counsel and trusting to what power and resources the Romans embarked on that enterprise which has made them lords over land and sea in our part of the world'.<sup>37</sup> At the same time he appears to connect the misfortune of the Greeks with their pretentiousness, avarice, and lack of morals—which are also the 'cause of evil' for their decline in population.<sup>38</sup> It does not seem unrealistic to hold that if Rome's success was ascribed partly to its manpower, the military statistics might have been massaged.

Even if the discrepancy between the two figures still causes unease, this interpretation seems as least as plausible as the two stated above. The mismatch between the figures is not so great that it cannot be explained in a reasonably convincing manner. More importantly, it is the only explanation that allows us to retain the most straightforward interpretation of the Polybian figures—i.e. that these all represent *iuniores*—without having to choose between either an improbably high population or a situation of extremely rapid population decline. To my mind, a still more significant advantage of this hypothesis is that

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<sup>36</sup> Erdkamp (forthcoming).

<sup>37</sup> Plb. 1.2 and 1.3 (transl. Loeb).

<sup>38</sup> Plb. 36.17.7. Throughout history one can find examples of a perceived causal relationship between population size and the strength of the state: see Stangland (1966) for numerous references, among others to Livy 1.9; Pliny *Ep.* 7; Thomas Aquinas *de Regimine Principum*, 4.9; several mercantilists; Henry IV of England; and Lord Bacon's statement that 'a kingdom's greatness consists essentially in population and breed of men, strong and able to bear arms'.

the assumption that the republican census figures present us with adult men *sui iuris* provides a more convincing explanation for some hitherto unexplained expressions used to describe the census figures (see section 7 below). It also leads to a population size for the Augustan era that falls between the current low and high counts and therefore does not suffer from some of the difficulties associated with them.

VII. *The Roman demographer's puzzle: who are the capita ciuium censa?*

What are the arguments in favour of a *sui iuris*-reading of the census figures, or, to be more precise, of the phrase *censa sunt capita ciuium* that accompanies these figures in the majority of cases? Modern scholarship has advanced various rival interpretations of the republican census figures, including the view that they are to be taken as comprising men *sui iuris* only, but the appearance of Brunt's *Italian Manpower* was followed by a broad consensus that these figures must be read as comprising all adult males—until Lo Cascio developed his high-count interpretation of late-republican and early-imperial history. Other views on the census figures and on the meaning of the phrase *censa sunt capita ciuium*<sup>39</sup> were eclipsed. The dominant 'adult males' theory rests on the view that *ciues* must be males, and that *caput* should be interpreted literally as 'head'. Since 'heads' equals 'persons', the census figures should include all men. However, despite its apparent strengths, this reading suffers from some serious weaknesses.

First of all, Romans did not define only men as citizens. In the legal works of both Cicero and Gaius the term *ciuis* is also used to denote women of citizen status, for example in the context of mixed marriages, punishments, enfranchisement, and lawsuits.<sup>40</sup> As for the use of the term in the context of the census, we cannot attach much literal significance to the sporadic addition of the masculine adjective noun *Romanorum* to *ciuium*. If this were a pregnant use to indicate that of all citizens the census counted the male ones only, it is difficult to see how the *ciuium Romanorum* in the three Augustan census figures could

<sup>39</sup> In modern scholarly literature one often finds *censa sunt capita ciuium tot*. The latter word substitutes the actual numbers (or the variable 'x'), as in 'so and so many'. Note that the word does not appear in any of the phrases accompanying the census figures in the ancient sources; it is a modern convention.

<sup>40</sup> Gaius *Inst.* 1.29 (twice); 1.30; 1.32; 1.33; 1.68; 1.71; 1.74; 1.77; 1.78; 1.80; 1.84; 1.88; 1.90. Cic. *de Orat.* 1.183.

include women and children. Moreover, women as well as men could be subject to the phenomenon of *capitis deminutio*, a reduction of legal status imposed upon citizens as a punishment.<sup>41</sup> A legalistic interpretation of *caput* as the ‘possessor of a legal personality’ therefore does not imply automatically that the *capita ciuium censa* were men only. It seems to me that the definitions of both *ciuis* and *caput* are too broad to be thought automatically to designate only adult males. In actual fact, since Augustus uses exactly the same phrase, *censa sunt capita ciuium Romanorum*, anyone who wishes to read this as referring to the entire free population will need to hold that the words *capita ciuium* do not naturally imply only adult males.<sup>42</sup> Still, for quantitative reasons it is beyond doubt that women (and children) were not included in the republican census figures which we possess.

Though scholarly attention has focused on the nouns *capita* and *ciuium*, there is no compelling reason not to focus instead on the verbal element, *censa sunt*. If it is the process of *censere* or census taking to which attention is drawn, this could suggest that the figures refer to the *sui iuris*, since they alone by their declaration before the censors are full participants in the census taking. Thus *censere* in the formula might be used here to refer to the registration of those persons who made declarations, as John Rich has observed. Such a hypothetical distinction between declarants and people registered finds corroboration in Livy 43.14.8, where he speaks about soldiers who were on furlough from Macedonia.<sup>43</sup> In this passage Livy distinguishes *censi*, who had to declare themselves, from those *in patris aut aui potestate*, whose names had to be given to the censors (*eorum nomina ad se ederentur*). Soldiers *alieni iuris* are not considered *censi* here, which seems to signify that those who did not declare themselves would not fall under the definition of *capita ciuium censa*, even though their names were known to the censors.

Concerning the interpretation of the phrase *censa sunt capita ciuium* one may also wonder whether the word *caput* should be an indicator of rank (as in *caput mundi*) since in a literal interpretation the word adds

<sup>41</sup> Gaius *Inst.* 1.160: *maxima est capitis deminutio cum aliquis simul et ciuitatem et libertatem amittit.*

<sup>42</sup> As held by Brunt (1971/1987<sup>2</sup>, 22).

<sup>43</sup> Rich (unpublished 1978) with reference to Livy 43.14.8. Cf. also Pieri (1968, 52–8), where he draws attention to the act of census taking and analyses the etymology and possible definitions of *census*.

nothing to the definition *censa sunt ciuium*.<sup>44</sup> If read in this manner, *caput* could refer to a heading in the sense of an entry in the registers.<sup>45</sup>

More importantly, there are two Latin clauses that have received little interest but constitute a major challenge to those who wish to maintain that the *capita censa* represent adult males only. To my mind these clauses strongly support the argument that the republican figures represent only adult male declarants, i.e. adult men *sui iuris*. Added to the ordinary formula of *censa sunt ciuium capita*, one of these clauses tells us that widows and orphans were excluded from the census total (*praeter <pupillos> pupillas et uiduas*).<sup>46</sup> The other may be taken to state either that orphans (of both sex) were excluded, or that orphans and widows were excluded: in *praeter orbos orbasque* the *orbae* might be orphaned girls or widowed women.<sup>47</sup>

Comments on these clauses have been limited to the suggestion that they show us that there was a separate list of widows and orphans. Though I do not doubt that this may well have been the case, this explanation seems unsatisfactory. For if the census totals traditionally represented all adult males, and these only, then why would Livy and his epitomator be inclined to point their readers to the fact that a certain group of women and children was not included? Surely, if the census figures represented only adult males, it should have been self-evident to their readers that members of the other sex were not amongst them. One might argue that Livy and his epitomator found it necessary to avoid confusion because census figures contemporary to their readers did include women and children. But since reference is made only to widows and orphans, this explanation does not work.

In my view these remarks can signify only that misunderstandings may have arisen because the census totals were based upon household registration and represented the *sui iuris* declarants, which may have led the reader to suppose that they also included those who were left without a *pater familias* or husband and were therefore *sui iuris*. That the phrase was added in order to clarify that only the adult men amongst them were meant seems to be the most logical explanation for the use

<sup>44</sup> A *sui iuris* interpretation, however, does not require such a reading.

<sup>45</sup> Cf. Goffart (1974, 43).

<sup>46</sup> Livy *Per.* 59.7 (131 BC). <*Pupillos*> add. Mommsen.

<sup>47</sup> Livy 3.3.9 (465 BC): *praeter orbos orbasque*; the same phrasing appears in the *Periochae* of Book 3. The point may be illustrated by the fact that the Loeb translator chooses 'besides orphans and widows' for 3.3.9 but 'besides male and female wards' for the *Periochae*. Cf. *OLD*; *TLL* 9.2 'orbus' A 1 b and A 2 b.

of these clauses. Suggestive as they are, these remarks constitute major evidence since they form part of the very scanty qualitative evidence and are directly attached to the numerical evidence in an explanatory clause. One might even say that the fact that the epitomators of both Livy's third and his fifty-ninth book chose to include these phrases in their brief summaries indicates that they considered them to be of prime importance. Therefore, since these phrases would not make sense if the census figures represented all adult males, they strongly favour the idea that the republican census tallies presented men who were *sui iuris*. The latter stance was in fact taken by Zumpt, Hildebrand, and Mommsen (who later changed this view for his *iuniores* only interpretation), and again by Bourne.<sup>48</sup> Rich is the most recent to have canvassed this view, in an unpublished paper of 1978.<sup>49</sup>

#### VIII. *Other definitions of the census population in the ancient sources*

Needless to say, the phrases suggesting that only adult men *sui iuris* are included in the census figures are not the only ones added to the standard formula. The phrase *praeter <pupillos> pupillas et uiduas*, which accompanies the figure for 131 BC, is the main variation on the standard formula accompanying the republican figures for the post-Hannibalic period. Other variations or phrases hinting at the meaning of census figures concern figures for the sixth and fifth centuries BC. However, these early figures do not form a consistent series, but fluctuate so dramatically that they cannot reflect real demographic developments. In my opinion, this is a good reason for treating them with extreme caution. It is therefore all the more surprising that, instead of exploring the significance of the phrase referring to the exclusion of wards and widows, most scholars have focused on the only other census figure which comes with some additional information—our first census figure, supposedly stemming from the reign of King Servius Tullius in the

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<sup>48</sup> Zumpt (1841, 19–20 [but only those of them who are *assidui*]); Hildebrand (1866, 86–8); Mommsen (1874, 371 [the first edition of his *Römischen Staatsrecht*]) and (1887<sup>3</sup>, 408); Nissen (1902, 116–18 [only from Augustus onwards]) and Bourne (1952a and 1952b).

<sup>49</sup> Rich (1978); see Rich (1983, 294 n. 34), where he remarks that 'the view deserves more attention than it has so far received'.

sixth century BC. In his report of the figure of 80,000 *ciuium censa*<sup>50</sup> Livy relied on Fabius Pictor, a late-third-century BC source. Fabius, Livy says, claimed that *eorum qui arma ferre possent eum numerum fuisse*—i.e. that it denoted the number of those able to bear arms. From this it has been inferred that the Roman manpower figures presented by Polybius also denote all men. But again this view is vulnerable to the objection that not every man was able to fight. Second, it seems questionable to assume that this comment on the sixth-century BC figure proves that both the later census figures and the Polybian figures—whether they be those of the allies or those for the Romans and Campanians—are to be understood in a similar way. Apart from the general objection that the intimate connection between census taking and recruitment does not in itself prove that the figures produced by each have the same basis, there is also a huge chronological leap which must be taken into account.

Servius Tullius' census figure is very early, and more than three centuries separate it from Polybius' account of army strength in 225 BC. It must be noted that the accounts of this early period lack a fixed census formula, and a variety of descriptions is used. In fact, when one compares the two figures Dionysius gives for 498 BC and 493 BC it is difficult to avoid the conclusion that we are presented with two different census populations. For 498 BC he reports that *ἑπτακοσίους πλείους εὐρέθησαν οἱ ἐν ἤβῃ Ῥωμαῖοι πεντεκαίδεκα μυριάδων* or 150,700 adult Romans.<sup>51</sup> Only five years later the number has dropped steeply to 110,000 *τιμησάμενοι*.<sup>52</sup> In 474 BC Dionysius records that the number of 103,000 *ἦσαν οἱ τιμησάμενοι πολῖται σφᾶς τε αὐτοὺς καὶ χρήματα καὶ τοὺς ἐν ἤβῃ παῖδας*, that is the citizens who had registered themselves, their possessions, and their adult children.<sup>53</sup> In this case, in contrast to the first figure, the number given by Dionysius denotes those who made the declarations and excludes adult men *alieni iuris*. It shows therefore that a single definition cannot be imposed upon the early-republican figures.<sup>54</sup>

<sup>50</sup> Livy 1.44.2: *milia octoginta eo lustro lustro ciuium censa dicuntur* (NB not *capita ciuium censa*).

<sup>51</sup> DH 5.75.3.

<sup>52</sup> DH 6.96.4.

<sup>53</sup> DH 9.36.3.

<sup>54</sup> For the early period no public records are known; whatever records there were must have been preserved by transmission through family archives of censors, the authenticity of which must be doubted. Brunt (1971/1987<sup>2</sup>, 26–7).

One may resort to different explanations for the inconsistency evident when all sources on the sixth and fifth centuries BC are taken together. Though after the Second Punic War the census figures are introduced with a single definition that has only minor variations in word order, and the figures for the later Republic themselves show a consistent pattern, it is still very likely that different lists or enumerations of various sections of the population circulated, each created to meet one of the various needs which the census was intended to fulfill.<sup>55</sup> If they had done likewise during the early period, Dionysius' contradictory descriptions and figures for the censuses could simply stem from different data in circulation, each reflecting the content of different (sub)lists of people registered.

Although I have assumed for the sake of argument that Dionysius' remarks are accurate and that he knew what he was talking about, a more cynical reader may hold that actually he did not. In fact it has often been argued that the figures for this early period, and indeed all census figures until the mid third century BC should be discarded as inauthentic.<sup>56</sup> The addition of the remark on Fabius Pictor by Livy can be read as testimony to his awareness of the unreliability or lack of clarity surrounding these remote figures. He must either have had in mind an alternative view; suspected that others might interpret them differently if he did not add an explanation; or found disagreement in his sources as to what this early figure meant. Scheidel now argues that in Livy's view the early regal census must have been dissimilar from that in his own era, and that he brings up Fabius Pictor as an *auctor antiquissimus* in order to explain this and to convince his reader that this must have been the case.<sup>57</sup> Livy's preferred interpretation may well have been quite different from what the census figure represented, or what others thought it did.

There is simply no way of knowing whether the sources on the early censuses reflect (an)other shift(s) in census methodology, different sublists, ignorance, or invention. Whatever the cause of the inconsistencies, it is clear that the figures fluctuate far too much to reflect any genuine population development; that they referred to a remote past for which the sources were as hard for historians to build on then as they are

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<sup>55</sup> See below, section 9.

<sup>56</sup> Cf. Brunt (1971/1987<sup>2</sup>, 26 f.).

<sup>57</sup> Scheidel (forthcoming 1).

now; and that the explanations of what they presented provided by ancient authors are inconsistent. I believe that this makes it difficult to use Livy's description of the earliest census, *qui arma ferre possent*, as the basis for interpreting all the figures for the later Republic. In fact, the little qualitative evidence directly connected to the census figures in our literary sources clearly supports a *sui iuris* interpretation.

#### IX. *Serve, pay, and vote: the aims of registration*

There is another way to approach the problem under consideration. We may ask what the purposes of the census were. In other words, why should the Roman state have wanted to register certain sections of the population; why did it make sense to list their names; and what purpose was served by recording a gross total? By re-examining the process of census-taking from this angle, I intend to shed some further light on the strengths and weaknesses of the dominant 'all males' interpretation.

It is generally accepted that the efforts made to register the Roman population served a threefold purpose: recruitment for the army, the recognition of voting rights, and the collection of taxes all depended upon being registered as a Roman citizen at the census, which was for much of the period under consideration held about every five years. This was clearly a major operation for the Roman state—a process for which censors were elected as magistrates. Of the actual collection and processing of the information we know little.<sup>58</sup> So, given the consensus on the multifold purpose of the census registration, it is all the more remarkable how easily ancient historians have accepted Brunt's statement that it would be 'incomprehensible that the Roman state should attach any importance to figures irrelevant to military strength', and therefore that the census figures represent all adult males.<sup>59</sup>

#### *Recruitment*

A major weakness of the all adult males view is that a list comprising all adult males could not be used directly for military purposes, as I have already pointed out. A list of all adult males would include

<sup>58</sup> For what we do know see also Northwood's contribution to this volume.

<sup>59</sup> Note that on the basis of the same argument Gabba (1952, 172) earlier concluded that the census figures must have included *assidui* only: others could not adequately serve in the military.

males too old to be eligible for service, as well as those physically unfit. Moreover, over a period between two censuses new seventeen year olds would have to be added to the lists.<sup>60</sup> For recruitment purposes a single list including all males would become less and less useful over the years that elapsed between two censuses. If census lists comprised adult males only, then how are we to envisage that those lists would have served to assess military strength? Such a list would have given the officers responsible for the levy no way to assess how large their pool of new young recruits was. One of their main target groups, those having just turned seventeen, would by definition have been out of reach. Clearly there needed to be frequent derivation from a list that comprised juveniles who would come of age before the next census.<sup>61</sup> Besides, the oldest age group, which was released from liability to active military service, as well as the permanently unfit, should logically have been removed from a list used to manage recruitment<sup>62</sup> but not from a list designed for taxation or voting purposes.

#### *Fiscal aims*

Facilitation of the assessment and collection of *tributum* was one of the main reasons for registration of assets in the census, for (allegedly since Servius Tullius) military taxes rose with increasing wealth. Land, property on land, and moveable assets (including slaves) as well as cash could be taxed. The use of the term ‘tax’ is somewhat misleading since payment of *tributum* was not a permanent imposition established by law but decided by senatorial decree according to circumstantial needs, and could be repaid when the state treasury was in surplus. Income and assets were not taxed at a fixed percentage; instead an estimate was made of the number of soldiers and amount of material needed, in order to determine how much money Roman citizens needed to pay.<sup>63</sup> A proportion of the adult males, however, were *alieni iuris*, and because they could not own property they owed no *tributum*. For this simple reason, a list of all adult males would not have served the state’s

<sup>60</sup> Cf. Mommsen (1887<sup>3</sup>, 407).

<sup>61</sup> Gellius 10.28.1 holds (based on Tubero’s *History*) that the Roman state kept track of boys under seventeen precisely to be able to recruit them once they turned seventeen.

<sup>62</sup> Cf. Suolahti (1963, 45).

<sup>63</sup> DH 4.19.

revenue purposes. In addition to those *alieni iuris*, the poorest *sui iuris* were also exempted from *tributum*.

We can infer from the two recorded instances when women contributed to the war effort by means of financial contributions that women were not subject to *tributum* in the Republic. The first instance shows that they contributed during the Second Punic War, but that their contributions were voluntary.<sup>64</sup> Much later the triumvirs wanted 1,400 of the richest women to pay *tributum*.<sup>65</sup> In response, a speech was made by Hortensia, who claimed that the charges were outrageous. She contrasted the voluntary character of the contributions of women during the Second Punic War with the obligatory charges put forward now, and held that women had never before been taxed for war, had nothing to do with it, and had no share in its honours. Therefore women should not be liable to pay *tributum*. The triumvirs were sensitive to her arguments, and most women were discharged from the payment of *tributum*.<sup>66</sup> Appian's formulation in an earlier section on the triumviral period also shows the extraordinary character of the imposition: he states that 'they (i.e. the triumvirs) levied heavy contributions . . ., finally even from women'.<sup>67</sup> We do hear of a different tax imposed upon widows and orphans, the *aes equestre et hordiarium*. It was meant to provide for the horses of the *equites* as well as the horses' fodder.<sup>68</sup> Much about this tax is obscure, and since references to it concern early periods, we are not even sure whether it was still paid during later times.<sup>69</sup> In so far as this tax remained in use, registration of the assets of widows and orphans would have been necessary. But at any rate, the *aes equestre et hordiarium* did not lead to the inclusion of widows and orphans in the census figures.

After the massive influx of wealth following the defeat of Macedon in 167 BC, *tributum* was no longer imposed upon those with Roman citizenship rights in Italy. But managing the state treasury proved to be problematic. Taxation for military purposes was reinstated in 43 BC, allegedly by the senate, since there was need of much money for the

<sup>64</sup> Livy 24.18.13–15.

<sup>65</sup> App. *BC* 4.32–3.

<sup>66</sup> So Mommsen (1887, 236–7), who considers this case the only exception to their exemption.

<sup>67</sup> App. *BC* 4.5.

<sup>68</sup> Livy 7.41.8 (341 BC), Plu. *Cam.* 2 (376 BC) and Festus 183L.

<sup>69</sup> Mommsen (1887<sup>3</sup>, 257).

war.<sup>70</sup> As an emergency measure during civil war,<sup>71</sup> the Roman administration temporarily fell back upon the traditional levy of *tributum*, the assessment of which had until 167 BC constituted a major aim of the census. Roman citizens outside Italy, on the other hand, continued to be subject to *tributum* just like their provincial non-Roman counterparts were, except when they were citizens of a colony, of a city which had been granted immunity, or when they had been granted individual *immunitas*.<sup>72</sup> For this reason their registration in provincial censuses was required.<sup>73</sup>

Notwithstanding the fact that Roman citizens in Italy proper were under ordinary circumstances during the late Republic freed from liability to pay *tributum* for the maintenance of the army, registration of assets still had a function. Men still needed to be registered in census classes in order to be allocated to a voting century, and this process depended upon registration of assets.<sup>74</sup> Moreover, as mentioned above, military *tributum* was not the only tax imposed upon the Romans. It is unclear to what extent the payment of *vectigalia* for the use of state land (*ager publicus*) was actually enforced.<sup>75</sup> But an indirect tax of 5% on the manumission of slaves, the *uicesima libertatis*, remained in use from the fourth century BC till the third century AD.<sup>76</sup> Owners had to register their slaves in the census.<sup>77</sup> Fiscal motives—i.e. the securing of the state's income—thus required registration of the assets of those sections of

<sup>70</sup> DC 46.31.3: ἐπειδὴ τε πολλῶν χρημάτων ἐς τὸν πόλεμον ἐδέοντο.

<sup>71</sup> Cf. Nicolet (1976a, 88 f.).

<sup>72</sup> In the form of *tributum soli* (land-tax) or *tributum capitis* (poll tax). See e.g. Neesen (1980, 151 f.), who points to regional diversity. Rathbone (2001, 107 f.) argues that Roman citizens were not required to register for the provincial census in Egypt, and were exempted from poll tax. However, this did not free them from their obligation to hand in a declaration: Bagnall and Frier (1994, 12).

<sup>73</sup> Cf. Brunt (1990, 329 f.) and Neesen (1980) on taxation of Roman citizens in the provinces.

<sup>74</sup> Though, as Nicolet (1991a, 128) suggests, voluntary recruitment increasingly diminished the need to update lists of those available for military service.

<sup>75</sup> For a skeptical view cf. Roselaar (forthcoming), *contra* Nicolet (2000, 75).

<sup>76</sup> On *vectigalia*, which could also consist of other revenues from the exploitation of public resources, such as the mines and the manumission tax (*uicesima libertatis*) see Nicolet (2000, 76). See also Neesen (1980).

<sup>77</sup> Cic. *Leg.* 3.7: the *familia* needed to be registered for the census, i.e. including slaves. Livy 39.44.3 informs us that all slaves that had been sold for 10,000 asses or more since the *lustrum* previous to 184 BC—i.e. the *lustrum* of the census of 189 BC—would be given an *aestimatio* (*aestimarentur*) or value 10 times as high as their actual value (*quanti essent*) and their owners taxed for that notional value at a rate of 3%. Cf. Brunt (1971/1987<sup>2</sup>, 15).

the population which were targeted by the various taxation measures: users of *ager publicus* and owners of slaves (theoretically anyone *sui iuris*), men *sui iuris*, widows, orphans, and, in the exceptional case recorded for the triumvirate, rich women.

The control of outward cash flows was a source of concern as well. During the late Republic military expenses fell in proportion to the amounts spent on populist measures, which started to absorb larger and larger shares of the state's income. Once distributions became a central feature of Roman politics, they came to bear heavily on the state's budget. In Rome census registration helped to control expenditure on the grain dole under Caesar. In order to facilitate this process, household heads including orphans were to be registered, according to the *Tabula Heracleensis*.<sup>78</sup> The list of declarants publicized as non-eligible to which the *Tabula* refers must by implication have contained both men *sui iuris* and orphans who owned property, regardless of sex. The prescriptions on the grain dole, however, must relate only to Rome, since only the city's proletariat was to benefit from corn distributed by the state.<sup>79</sup> That in Rome the registration of both groups of *sui iuris* citizens coincided with their display on a single list set up in several locations does not establish what happened in a different context—that of a general census of Roman citizens. What happened to that during Caesar's dictatorship we simply do not know. The best we can tell, as far as registration for taxation during the preceding republican period is concerned, is that women and children who were liable to taxation were not included in the census figures. Vice versa, if the census figures recorded men liable for taxation, it would not make sense to suppose that all adult men were included. In short, from a fiscal perspective it makes more sense to assume that the census covered all citizens *sui iuris* who were liable to taxation, rather than all adult males.

### *Registering the electorate*

The third purpose of the census, the distribution of voting rights, targeted yet another part of the population, since in this case it was not those of a certain age and/or with a certain amount of possessions who counted, but in principle all adult male citizens. Therefore, if a list

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<sup>78</sup> *Tabula Heracleensis* §2. Text, translation, and commentary in Crawford (1996, 355–91).

<sup>79</sup> Nicolet (1991a, 130).

of all adult males were to have had any direct purpose, it should have been in the context of registration for voting purposes. For those in the higher census classes such registration was important. The distribution of voting rights in the *comitia centuriata* was dependent upon property: the richer citizens, notably those of the first class, were allocated a proportionally much larger number of centuries, and thereby a greater share in the vote.<sup>80</sup> This established a voting hierarchy that ensured the predominance of the elite in the assembly which elected the most influential political functionaries: consuls, praetors, and censors. Whether or not those of lower status needed to be listed as voters depends on the view one takes of the importance of the *comitia tributa*, in which no property qualification was observed. Though some have considered this assembly as a democratic element which counterbalanced the power of the aristocracy, several recent analyses emphasize that the political agency of the Roman plebs should not be overestimated.<sup>81</sup> First, it is doubtful to what extent Polybius' description of the Roman political system as a mixed constitution<sup>82</sup> and various Ciceronian speeches and letters which reveal a concern with the opinion of the plebs reflect a historical reality. Discrepancy between ideology and practice may well be the case.

This holds equally where the application of voting rights is concerned.<sup>83</sup> How many of the Roman *plebs* might we expect to have been present at meetings of the popular assembly? The system of direct participation and the fact that these meetings were centralized in Rome certainly did not enhance participation rates. Situated far away from many voters, involvement in politics took much time and effort, and many citizens must have judged that the costs of participation were not in proportion to the gains.<sup>84</sup> The space available to voters was rather constrained. The Comitium in the forum, where informal

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<sup>80</sup> E.g. North (2006, 262); Taylor (1966, 5).

<sup>81</sup> North (2006, 273 f.) and Jehne (2006, 14–25) provide a further guide to the issues of citizen participation in Roman politics and the notion of 'democratic' elements. The discussion hinges to a large extent upon the role of the elite: its grip upon the political sphere, intra-elite competition, and the openness of the elite to newcomers. Cf. also Mouritsen (2001, ch. 1) and Turchin (forthcoming, c. 6, 12 f.).

<sup>82</sup> Mouritsen (2001, 5 f.) on Plb. 6.11.

<sup>83</sup> The issue is addressed e.g. in Millar (1998, 211 f.), who characterizes actual voting as the product of chance, circumstance, or organization by interested groups that determined which persons—always a tiny minority—would in fact be present at the Forum to perform the function of representing the whole *populus Romanus*.

<sup>84</sup> But cf. MacMullen (1980, 457) for an alternative explanation of low voter turnout.

*contiones* and—until 145 BC—legislative assemblies were held, was rather limited in size and allowed for the presence of perhaps a maximum of 3,600 people for voting and 5,000 for a *contio*,<sup>85</sup> the Augustan *Saepta* on the *Campus Martius* (used for the *comitia centuriata* and, during the late Republic, the tribal assembly) could accommodate considerably more people, but the figure of 70,000 suggested by Taylor is now held to be an overestimate.<sup>86</sup> Even if we accept a low count, these figures imply a dramatically low participation rate among voters.<sup>87</sup> A massive turnout of voters was not to be expected from the Roman citizenry, nor was it aimed at by the leading political elite, of which the censors were part. In Rome nothing was done to encourage the participation of a representative body of citizens in politics, be it through reimbursement of attendance, as happened in Athens, or by the establishment of a representative system.

All this raises questions concerning the value attached to meticulous registration for voting purposes. If there was indeed a procedure of formal identification of voters beforehand,<sup>88</sup> this would not necessarily have required a separate list of all voters, since the original declarations would have contained all the information necessary to settle the matter, but it could have helped to smooth such a process. A separate list of all voters could thus have served as a roster of the electorate, where one might expect citizens to have been organized by voting century. This again would have required frequent updating, since membership of the *iuiores* and *seniores* could change between elections, and so the organization in centuries was not very stable. Such a ballot list would seem to involve great effort and little gain from a practical point of view; but this does not disprove its existence *per se*. Listing the total number of voters is a different matter for which I do not see a purpose in the context of the voting system. It would have been useful if the aim was to distribute voters equally among several centuries within one class,

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<sup>85</sup> Mouritsen (2001, 19 and 25).

<sup>86</sup> Taylor (1966, 53 f.). MacMullen (1980, 454) suggests there was room for 55,000 but conjectures that voting totals never surpassed 40,000 (aisles provided maximum capacity in order to accommodate the largest voting centuries, but the average century was smaller than that). Mouritsen (2001, 30) estimates 30,000.

<sup>87</sup> On theoretical maximum rates of voter participation among the Roman citizenry see Scheidel (2006, 218–9 fig. 4 and 5, and 212–20).

<sup>88</sup> This is a much debated issue—cf. Nicolet (1976c, 708–10 [positive on identification procedure]); Viriouvét (1996, 883 f.) and Mouritsen (2001, 29), both negative. Evidence is scanty; controversy rests mainly on divergent interpretations of Varro *R.* 3.5.18 and Plu. *Mar.* 5.3–5.

since this goal could only be achieved by establishing a quota in order to determine how many voters each century should contain. Such a procedure, used in contemporary electoral systems of proportional representation, requires knowing the total number of voters first. But Roman sources state explicitly that men were distributed over centuries according to their membership of a *tribus*, and therefore the size of the centuries could vary dramatically both between and within census classes, and even men registered in one of the seventy centuries of the first class did not necessarily enjoy voting rights equal to their fellows.<sup>89</sup> The electoral system therefore reveals no interest in counting the total number of voters. Although not necessary for practical reasons, an ideological purpose remains a possibility—one might argue that showing how the Republic shared voting rights between all free men would be in line with the demand that representatives of every single tribe should participate in a vote in order for it to be valid.<sup>90</sup>

Two conclusions emerge from this brief analysis of the purposes of the census and its target populations. Indisputably, there must have been several lists derived from the census. No total figure could fulfill the threefold purpose of the census, for the obvious reason that these diverse functions did not coincide.<sup>91</sup> Moreover, of the target groups for each of those aims—fiscal, military, and electoral—only one consisted of all adult men: that of the voters. At best, this conclusion undermines Brunt's statement that the census tallies should represent all adult males because the military aim was the sole one that would have mattered to the Roman state. If one wishes to maintain that the sum totals given as *capita censa* are both directly linked to one of these three registration purposes and that they are to be all adult males, we should conceive of them as tallies based on the electoral roster rather than as a list of those available for military service. However, given the Roman voting system, there was little need to make such an effort. Alternatively, a *sui iuris* hypothesis is in perfect harmony with the fiscal purpose of census taking, because this section of the population contained all those owning property. Registration of assets continued to have a function, although

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<sup>89</sup> Inequality of voting weights within each class was not solely the result of unequal sizes of centuries due to different numbers of citizens in each tribe, but also because the much smaller group of *seniores* over age 46 voted in the same number of centuries as their younger counterparts.

<sup>90</sup> Mouritsen (2001, 24) with reference to Cic. *Sest.* 109 and the *Tabula Hebana* (AD 19) 33–4, to be found in Oliver and Palmer (1954, 229 [text] and 243–4 [commentary]).

<sup>91</sup> Cf. Suolahti (1963, 43–4).

it must be conceded that with the exemption from *tributum* for Roman citizens after 167 BC its importance must have declined.

### X. *The practice of census taking*

Analysis of the procedures of census taking may give a better insight into how registration worked in practice. If we could establish how declarations were made and how information was processed, this would allow us to speak with more confidence about what the census lists are likely to have represented.

Unfortunately, here too we face some major limitations of evidence. We cannot tell what happened in a censor's office (or whether the offices were always manned by the censors); whether it would always be offices at which one would appear;<sup>92</sup> and from when exactly the registration procedure was decentralized. But not all is lost. What we do know is that when a census was held in Roman Italy, persons who were *sui iuris* had to show up for registration by representatives of the authorities. Three ancient authors—Dionysius, Cicero, and Gellius—as well as the inscription known as the *Tabula Heracleensis* refer to a *professio*, an official declaration that needed to be made for the census.<sup>93</sup>

A *pater familias* had to state under oath his civic status, the names and ages of his wife and children as well as his property—the latter including slaves.<sup>94</sup> However, it was not just fathers in their own right who had to appear before the censors—adult men *sui iuris* without wives and/or descendants were equally liable to show up according to the *formula censoria*, the censorial law. By the same token, widows and orphans who did not belong to someone else's household also needed to be registered, since they too could own property. It was those who were not in their own *potestas*, but *alieni iuris*, who were registered not by themselves but by their household head, as they fell under the authority

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<sup>92</sup> In Roman Egypt this was not the case. Whereas *metropoleis* had permanent offices open for census registration, declarations from small communities are dated within a brief period, suggesting instead that the censors went to the village and called everyone to appear, or perhaps went knocking on people's doors—we can only speculate here. Bagnall and Frier (1994, 17–8).

<sup>93</sup> DH 4.15.6; Cic. *Leg.* 3.3.7; Gellius 4.20.3; *Tabula Heracleensis* l. 145–8. Cf. Bourne (1952a, 132).

<sup>94</sup> Bourne (1952a, 132). For the inclusion of a daughter in the *professio* of a Roman citizen in Egypt (presumably for the Claudian census) see Rathbone (2001).

of the latter.<sup>95</sup> This included adult men whose fathers (or grandfathers) were still alive and who had not been released from their power by means of a procedure of emancipation.<sup>96</sup> Property registered could, apart from slaves, include land in Italy, farming tools, clothing, jewelry, and presumably—for our sources are patchy—anything else that was valuable.<sup>97</sup> The censors were in office for eighteen months and seem to have registered the population in sections.<sup>98</sup> There are some sources that speak of punishment arising from failure to make a declaration before the censors,<sup>99</sup> but scholars disagree as to whether sentences were imposed during the early Republic only or continued into later times and provided an incentive to register.<sup>100</sup> Whether there was a grace period such as in Egypt, where one could register until three years after the closure of the census without repercussions, is unclear.<sup>101</sup> Moreover, although scholarly discussion revolves around the question of continuity v. discontinuity of evasion of punishments over time, in actual fact we do not know whether these laws were ever carried out.

<sup>95</sup> Suolahti (1963, 34–5).

<sup>96</sup> The term *duicensus* though, found in Festus 58L and explained therein as *dicebatur cum altero, id est cum filio census*, might suggest that adult sons were counted separately from their fathers (cf. Lo Cascio 2001b, 567 and 575). But enigmatic as both the term and its concurrent definition are, we cannot tell whether it refers to the shape of the census list, or to a stage in the census procedure. Perhaps it has something to do with financial privileges for men with sons, cf. App. *BC* 1.9, who holds that the Gracchan land laws allowed 250 *iugera* for each child in addition to the maximum of 500 *iugera* per person, though his claim may be doubted: Roselaar [forthcoming]). Moreover, if—as is commonly held—the practice of emancipation of adult sons before the death of their father and/or marriage grew more widespread over time (cf. below, section 11c), it may be that it refers to cases in which a father had emancipated a son who still lived in the household. Presumably, they were to appear together. All Festus allows us to conclude is that sons were registered by the censor in connection with their fathers. DH 9.36.3 seems to tell against their inclusion in the census figures: he refers to sons *alieni iuris* as being registered by their fathers, but as not being included in the final census figure (on this passage see above, at note 53).

<sup>97</sup> See Brunt (1971/1987<sup>2</sup>, 15) for further references.

<sup>98</sup> Cic. *Arch.* 11: *primis Iulio et Crasso nullam populi partem esse censam*—‘in the first year under Julius and Crassus no census of any section of the population was held’ (i.e. in 89 BC). Cf. also Livy 29.37.5 for separate registration of *equites*, apparently postdating the *lustrum* of 204 BC.

<sup>99</sup> Livy 1.44.1: *censu perfecto, quem maturauerat metu legis de incensis latae cum uinculorum minis mortisque*. Cf. DH 5.75.3 on the reign of Servius Tullius. DH 4.15.6: καὶ μέχρι πολλοῦ διέμενε παρὰ Ῥωμαίοις οὗτος ὁ νόμος. App. *BC* 4.32 (43 BC). Gaius *Inst.* 1.160.

<sup>100</sup> E.g. Parkin (2003, 184); Brunt (1971/1987<sup>2</sup>, 33–4). Suolahti (1963, 38). Cf. also Mommsen (1887<sup>3</sup>, 367–8) on the punishment of evaders.

<sup>101</sup> Bagnall and Frier (1994, 16–17) and Hombert and Préaux (1952, 77–9); there is a papyrus *P. Lond.* 261 which refers to delays of one, two, and three years (idem, 138–9)—but only one.

Therefore it is as difficult for the late Republic as it is for the early Republic to establish whether such laws acted as a strong disincentive to non-registration.

With regard to the problems of interpretation surrounding the population figures, how the censors archived or recorded the oral information given to them by the declarants is obviously what would be most interesting to know; but this is precisely what our sources keep silent about. Logic requires that the censors, or some other officials, must have composed several lists, as stated above. What we know about Roman Egypt is that here derivatives were made from a basic census list. As in Roman Italy, those *sui iuris* were liable for registration of both themselves and their dependents. Apart from the house-by-house registration and the lists of minors, scribes needed to draw up person-by-person copies of population lists, summaries of those lists, and lists of *katoikoi* and those excluded from the tax estimate.<sup>102</sup>

One of the reasons for creating derivatives from the main list was to keep track of those under the age of fourteen.<sup>103</sup> These children were on the record as members of their father's household, on his declaration; but even so the effort was made so that the cohort amongst them who had reached the age of fourteen and had thus become liable for taxation could be added to yet another list of taxpayers.<sup>104</sup> Lists were updated annually on the basis of documents such as notices of births and deaths received in the interim.<sup>105</sup> The main list, from which these subsidiary lists were all derived, consisted of the gathered household declarations, the *κατ' οικήων ἀπογραφαί*. They were glued together into composite rolls in which sheets were numbered sequentially, and as such formed the basic source of authoritative information. Grouped according to village or toparchy, the so-called *τόμοι* originating from different locations were sometimes linked to each other.<sup>106</sup>

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<sup>102</sup> This is stated in *P. Mich.* XI 603 (AD 131/133), a papyrus that seems to be a collective employment contract for a group of nine scribes and gives an account of the tasks they needed to perform.

<sup>103</sup> An elaborate description of the census procedure and census records in Egypt is provided by Bagnall and Frier (1994, *c.* 1) on the basis of the earlier work of Hombert and Préaux (1952).

<sup>104</sup> As can be derived from papyri such as *P. London* 257–9, lists of men liable to poll tax (AD 94).

<sup>105</sup> Parkin (2003, 142 f.).

<sup>106</sup> Bagnall and Frier (1994, 19, 27 f.). *P. Bruxelles* inv. E. 7616, studied by Hombert and Préaux (1952), consists of eighteen household declarations, carefully cut, glued together, and numbered. From this, it can be seen that each household declaration

Extrapolation from Egypt to Italy may obviously prove to be dangerous. The numbering and gluing together of the household declarations suggests the method the censors employed to organize their administration in Roman Egypt, but does not necessarily tell us anything about Roman Italy. Still, it is difficult to imagine how Roman censors at the moment of registration could have created three different lists simultaneously. It seems more plausible to assume that they simply noted down whatever information they needed on a file stamped as belonging to a certain declarant. How one would get from there to a total of *censa sunt capita civium* remains a matter of speculation. What is clear though is that were our census totals to reflect all adult males, it would have been necessary to create a new derivative file that split up the declarations of families, since adult males who were not *sui iuris* were headed under the entry of their fathers or *pater familias* by whom they had been declared. One may hold then that the figures presented to us are the sum totals of such derivative voting rolls; for they cannot have been anything else if we interpret them as representing all adult men. But it is at least equally plausible that the figures given as *capita censa* are simply totals of the number of census declarers, i.e. adult men *sui iuris*, who had appeared for the census to declare themselves and those in their power.

XI. *Inclusion of widows and orphans under Augustus:  
implications for population size*

As we have seen, the phrase ‘exclusive of orphans and widows’, which accompanies two of the republican census figures, strongly suggests that at a certain point they started to be included in the summarizing figures—otherwise this remark simply would not make sense. Are there any indications that the Augustan census figures were the first to include these categories?

Mere registration of widows and orphans would not constitute a break with the past: they had been registered during the republican period as

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was made on a separate sheet and given an individual number. The Brussels papyrus roll contains declarations from two villages, Theresis and Thelbonton Siphtha in the Nomos of Prosopite. As each of them contains its own numbering system, this roll is a later composition and the actual census must have been administered village by village. The households from Theresis had their declarations made approximately one month before those living in Thelbonton Siphtha. Cf. Parkin (2003, 142 f.).

well, as were, for that matter, other dependents.<sup>107</sup> What led Beloch to argue that under Augustus all citizens of whatever age and sex must have been included in the total was precisely the phrase mentioning widows and orphans. He took it that the addition only of widows and orphans would have served no purpose, and that their inclusion must have meant that in actual fact all other women and children were included in the total as well.<sup>108</sup> Many low counters, most notably Brunt, have subsequently reckoned with a change in the part of the population counted (not in those registered) under Augustus, when figures jump to 4,063,000 citizens registered for 28 BC; 4,233,000 in 8 BC; and 4,937,000 in AD 14 respectively.<sup>109</sup> The simple reason for assuming such a change is that it seems the most logical way of accounting for the discrepancy between the previous republican figures and the Augustan ones. But the fact that our meager extant documentation does not provide any further indications of such an alteration does not make life easier for those wishing to explain it, and has led others—high counters—to suppose that we must assume not change but continuity in the basis of recording under Augustus.

The alternative stance that widows and orphans alone started to be included has been defended by Bourne, but he related their inclusion to the pre-Augustan period. He thought it most probable that orphans and widows had been included throughout the republican period already, except for the two times their exclusion is explicitly stated.<sup>110</sup> This, however, is unconvincing. First of all, when he pointed to the fact that the census figures immediately following these two cases show unaccountably large increases and argued that this supports his view, he clearly failed to realize that his interpretation in fact forced him to explain why the *preceding* figures are so low.<sup>111</sup> Moreover, he underestimated the effects of the demographic regime on population distribution when thinking that it must be ‘highly improbable (...) that one-fifth of

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<sup>107</sup> E.g. De Ligt (2007).

<sup>108</sup> Beloch (1886, 376).

<sup>109</sup> *Res Gestae* 8; preserved on inscriptions found in Ancyra, Antiochia (Pisidia), and Apollonia (Pisidia). For 28 BC the Latin text is considered authoritative; the Greek ‘transcript’ records 4,630,000; Riccobono (1945). The *Fasti Ostienses* give 4,100,900 for AD 14. But cf. Seston (1954) and Nicolet (2000, 189–96), who both consider the figure inauthentic on the basis of their paleographic analyses of the epigraphic text. Cf. also Lo Cascio (1994, 29 n. 38).

<sup>110</sup> Bourne (1952a, 134–5).

<sup>111</sup> Bourne (1952a, 134).

the citizens *sui iuris* in the second century BC should be widows and wards'.<sup>112</sup> On the same grounds, Beloch rejected Nissen's thesis that *sui iuris* widows and wards were included in the census figures from Augustus onwards. He found that these could not have been many, so that 'in der Hauptsache also der *ciuium capita* des kaiserlichen census für Nissen identisch (sind) mit den erwachsenen Bürgern männlichen Geschlechtes'.<sup>113</sup> The share of the non-adult male *sui iuris* population was likewise misjudged by Mazzarino when he argued that the difference between the Augustan figure of 4,100,900 reported in the *Fasti Ostienses* and that of 4,937,000 in the *Res Gestae* showed that the former excluded widows and wards, whereas the latter included them, as did in fact, according to Mazzarino, the two other figures for 28 BC and 8 BC in the *Res Gestae*.<sup>114</sup>

In reassessing the strengths and weaknesses of Nissen's interpretation, we can now draw upon demographic models which not only provide us with better insights into population distribution, but also allow us to reconstruct demographic patterns on the micro-level of the family. We are now able to determine roughly what share of women and children had no fathers or husbands under a given demographic regime. One conclusion which emerges from these models is that their numbers were much higher than earlier scholars supposed when they rejected the thesis proposed first by Nissen and later by Bourne.

In establishing the approximate number of Roman women and children *sui iuris* we must reckon both with overall life expectancy and with Roman patterns of marriage. It does not need to be demonstrated here that the former was low, and mortality high. In as far as we can tell on the basis of source material that is far from ideal, Roman marriages were between younger women and older men—the size of the age gap may be disputed, but there is little doubt that there was one.<sup>115</sup> Fathers were likely to die first, meaning that wives and children were left behind. That *patria potestas* was often broken quite early due to these

<sup>112</sup> Bourne (1952b, 181).

<sup>113</sup> Nissen (1902, 116–18) and Beloch (1903, 481).

<sup>114</sup> Mazzarino (1962<sup>2</sup>, 35–6 n. 1). I am grateful to Elio Lo Cascio for this reference.

<sup>115</sup> Though exact details are lacking, on the basis of commemorative shift patterns on inscriptions the age at first marriage (AAFM) for women is usually placed at between age 15 and 20; and that for men around age 30: Saller (1987, 29–30). Lelis, Percy, and Verstraete (2003) propose a revision of the dominant view. However, the sudden and steep increase in commemorative shift inscriptions for males strongly pleads for placing male AAFM near age 30: see Scheidel (2007b).

demographic conditions and patterns is now widely accepted.<sup>116</sup> Saller's micro-simulation of the Roman family gives us an idea of the share of women and children who were not under someone else's control via *patria potestas* or marriage. On the basis of the marriage pattern assumed, a little over 11% of all women were not yet married but had already lost both their fathers and grandfathers (see table 2).

Lacking information on remarriage, it is more difficult to gauge what percentage of married women who had lost their husbands or got divorced did not find themselves a new husband and thus retained their *sui iuris* status. However, evidence from Egyptian census records and medieval Italian registers suggests that their numbers were considerable. This is in keeping with historical evidence for the early-modern period and modern observations that women tend to remarry less than men for a number of reasons related to both opportunity and desire.<sup>117</sup> Saller's assumption that remarriage of widows and divorcees should be set at 100% is therefore surely an overestimation.<sup>118</sup> A minimum percentage of widows of around 13% may be expected when judging from comparative evidence.<sup>119</sup> Other data—those from Roman Egypt included—suggest even higher numbers.<sup>120</sup> Besides widows and orphaned girls the third new group consisted of boys who became *sui iuris* through the premature deaths of their *pater familias*. They formed c. 5.9% of the total number of males (see table 2). Together the widows and wards thus form a group only two-fifths smaller in size than the group of adult men *sui iuris*, which made up 50% of the male population.<sup>121</sup> Clearly the idea that the number of widows and wards could

<sup>116</sup> Saller (1987).

<sup>117</sup> On the difference between men and women and the various factors affecting them see e.g. Grigg (1977, 194 f.); Schofield (1981, 213); Leridon (1981); Knodel (1988, 77 and 170); Buckle, Gallop, and Rodd (1996); McCants (1999, 451 [eighteenth century Amsterdam]).

<sup>118</sup> Saller (1994, 46). In basing his assumption on the Augustan marriage legislation that imposed fines upon those who had not remarried after one or two years, he fails to recognize the usual discrepancy between prescript and practice.

<sup>119</sup> Herlihy and Klapisch-Zuber (1978, 659 Appendix V table 1, and 663 table 2). In the Florentine *catasto* of 1427, of all women 17.5% were widows; this figure is somewhat higher than that for the Tuscan region altogether, which is 13.6%. The discrepancy is due to the age difference between husbands and wives, which was largest in Florence, and smaller both in the countryside and in smaller cities (207 table 24).

<sup>120</sup> Cf. Bagnall and Frier (1994, 113).

<sup>121</sup> 50%: derived from Saller (1994, 52) and Coale and Demeny's level 3 population distribution, growth rate 0% (cf. table 1). Widows and wards:  $11.37 + 13_{(\text{min. } \%)} + 5.91 = 30.28$ .

Table 2. Orphans and widows\*

a) Women unmarried and (grand)fatherless						
Age group	% fatherless (f)	Proportion of (f) grandfatherless	% (grand)-fatherless	Proportion unmarried	Group size as share of female population	Group size registered as <i>sui iuris</i> (share of female population)
0-4	6.5	0.645	4.2	1	12.74	0.53
5-9	18.5	0.765	14.15	1	10.53	1.49
10-14	31	0.86	26.66	1	10	2.67
15-19	44.5	0.925	41.16	0.7	9.46	2.72
20-24	57	0.965	55.01	0.25	8.81	1.21
25-29	69	0.985	67.97	0.05	8.1	2.75
<b>% of females unmarried and (grand)fatherless:</b>						<b>11.37</b>
b) Boys (grand)fatherless						
Age group	% Fatherless (f)	Proportion of (f) grandfatherless	% (grand)-fatherless		Group size as share of male population	Group size registered as <i>sui iuris</i> (share of male population)
0-4	6	0.62	3.72		12.74	0.47
5-9	18	0.75	13.5		10.53	1.42
10-14	31	0.85	26.35		10	2.63
15-16	40	0.92	36.8		3.78	1.39
<b>% of males (grand)fatherless:</b>						<b>5.91</b>

\* Age group distribution: Coale and Demeny (1983<sup>2</sup>) West Level 3 females,  $r = 0$ . Proportions (grand)fatherless derived from Saller (1994), p. 48 (females) and p. 52 (males).

not account for even one-fifth of the *sui iuris* population is a severe underestimation of the combined effects of the Roman mortality regime and marriage system.

If Augustus included both widows and wards as well as adult males *sui iuris* in his population figures, they made up about 40% of the entire free citizen population. That is, whilst adult men *sui iuris* and boys without fathers together formed 56% of all males (the rest of them consisting of boys and men in the *potestas* of (grand)fathers), *c.* 24% of females of all ages were *sui iuris* (11% not yet married but without living (grand)fathers, plus *c.* 13% widowed). This averages to 40% *sui iuris* in the total population.

The effects of this on our assumptions about the size of the population of Roman Italy under Augustus are considerable, and can be shown by a simple adjustment of the multiplier. With a share of 40% registered, we need a multiplier of about 2.5 (100/40) in order to calculate the entire population of Roman citizens (instead of 3.18 in the case of an all adult men hypothesis). Using this multiplier, the 28 BC figure suggests a population of around 10 million free citizens. Of course this figure cannot be regarded as representing the population of Italy in the time of Augustus. In order to estimate the size of the Italian population, we must make assumptions about the number of overseas citizens, which need to be subtracted, and also about the number of slaves and foreigners, which need to be added. Instead of proposing any new figures here, I shall simply round off my calculation on the basis of current assumptions in order to show their quantitative effects. If we reckon with Brunt's estimate that in 28 BC about 1.2 million of all citizens (or *c.* 375,000 adult male citizens) lived overseas,<sup>122</sup> and if we further assume that about 1.5 million slaves and aliens need to be added to the Italian population, this would put the total population of Italy at about 10.3 million in the early years of the Principate. By AD 14, when 4,937,000 million citizens are recorded, the total would have risen almost to 12 million, if we take on board Brunt's view that by this time some 2 million people of citizen status lived in the provinces.<sup>123</sup> Were Frank's estimate to be taken as more plausible,

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<sup>122</sup> Brunt (1971/1987<sup>2</sup>, 265 [1.2 million in 28 BC, or 375,000 adult men]), derived from Beloch (1886, 377), who gives 350,000 to 400,000 adult men for 28 BC. Note, however, that Beloch (1899, 615) seems to estimate the total number of overseas citizens at between 1.75 and 2 million already in 28 BC.

<sup>123</sup> Brunt (1971/1987<sup>2</sup>, 265 [1.87 million]).

the number of citizens overseas would be about 50% higher, which would create estimates for Italy of 9.5 million in 28 BC and nearly 11 million in AD 14.<sup>124</sup>

Needless to say, these estimates can serve only as a very rough indication of the quantitative effects of the hypothesis that Augustus included widows and wards in his figures, since they are affected by assumptions about the number of slaves and aliens in Italy as well as about the number of citizens overseas which may well be incorrect.

More important though is the observation that if the *sui iuris* registered formed a larger share of the population than the 40% on which this population estimate is based, the multiplier for the Augustan figures should be lowered to below 2.5. Accordingly, the estimated figure for the population size of Roman Italy would be lowered further. I believe that the 9.5 to 11 million figure is indeed bound to be an overestimation, as there are several factors that could have contributed to an increase in the share of the citizens that were *sui iuris*, or may have led us to overestimate the multiplier for other reasons. In the following section I shall give brief attention to each of these.

Before I continue to fine-tune my interpretation by adding some further modifications, a brief summary of the main argument seems appropriate. So far I have hypothesized that Augustus followed the republican practice of counting men *sui iuris*, but departed from the republican tradition by including women and children *sui iuris* in his reported census totals. This theory implies a total population figure lying somewhere between the estimates of the high counters and low counters. The reason for this is that low life expectancy in the ancient world resulted in a young population, and one in which many lost their fathers at a young age. As a consequence, there were many more children and women *sui iuris* than there were adult men *alieni iuris*.<sup>125</sup> This means that in my interpretation of the Augustan census figures a larger share of the total population was included in the census figures than in the high count interpretation, which holds that *all* adult men (i.e. both *sui iuris* and *alieni iuris* adult males) and *no* women and children were included. At the same time, the total population implied by my

<sup>124</sup> Frank (1924, 339 [about 1 million adult male citizens by AD 14]).

<sup>125</sup> Adult men *alieni iuris* as a percentage of total population: 6.47 (cf. table 1:  $100 - 50 - 37.06 = 12.94\%$  of men/2). Women and children *sui iuris* as a percentage of total population: 15.14 ( $11.37 + 13 + 5.9 = 30.28/2$ ) (cf. table 2, and p. 24 with n. 121).

hypothesis is larger than that assumed by the low counters, who hold that the Augustan census included all men, women, and children.

## XII. *A population category on the rise: the sui iuris*

In order to fine-tune my model, I want to introduce some modifications. There has been so much attention to the phenomenon of undercount that little argument is required here to show that records do not always include everyone they aim or claim to include. Such a discrepancy between aim and result, and the underlying causal mechanisms, are one type of distortion that must be regarded as a variable, because it may have changed over time. Second, increases and decreases in the number of people registered may reflect increases or decreases in the target categories—in absolute or relative terms, or both. In the case of the Roman census figures there is much room for potential shifts of this type, because we deal with two juridical categories: that of citizen and that of *sui iuris*.

In this section I analyze several factors which may have pushed up the census totals. Not resulting from natural growth, these factors simply added people to the category of citizens *sui iuris* counted, either by changing their status or simply by mistake. These matters are relevant because they affect the multiplier one should use in order to calculate a total population figure for Roman Italy: if we think that because of these factors the *share* of the total free population registered as *sui iuris* was on the rise over time, there must have been a significant discrepancy between real population growth and the rate of increase suggested by the census figures.

### *Overcount*

There is an opposite to the well-known phenomenon of undercount: overcount. Accidentally registering people twice is an acknowledged problem in modern censuses, where it may actually exceed undercount.<sup>126</sup> For our purposes, its importance lies rather in its correlation

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<sup>126</sup> Thanks are due to Heili Pals, Stanford University, who pointed out to me the problem of over-registration. In 2002 (after two years of evaluating the 2000 census) the American Census Bureau reported an estimated net census overcount of 0.5%, with about 5 million people missed, but 6 million counted twice. Cf. Sunshine Hillygus, Nie, Prewitt, and Pals (2006, 70 and 132 n. 4).

with migration. Over-registration is particularly high among temporary migrants. Due to insecurity about whether the ones left behind have or have not registered them, miscommunication, fear of the negative consequences of failing to register, or simply due to lack of awareness, many among this group are registered both by their families and by themselves, or even by their temporary household heads (e.g. landlords). Effort is needed to filter out these double counts even when modern data systems are available; without them doublets are likely to go unnoticed.<sup>127</sup> If, as Paul Erdkamp has argued, migrants to Rome were mainly young adult males who were temporary residents,<sup>128</sup> they may well have been registered twice: once in their city of origin, and once in Rome. That in reality they may have appeared only once in the census totals (being registered only as a family *member* in their city of origin) is irrelevant: the point is that with the growth of Rome an increasing number of people must have migrated, and therefore the scale of the phenomenon must have increased. Since the growth of Rome was concentrated in the first century BC, so also must have been the worsening of this phenomenon: an increasing number of young adult males hitherto not counted as *sui iuris* will now have been included in the census totals. So, while undercount by the census surely remained a factor of greater importance for the Roman census, double counts became a counterforce of increasing importance over time. The obvious effect would be an inflation of census figures not corresponding to any demographic reality.

#### *Emancipation of (grand)children*

The phenomenon of *emancipatio* entailed a voluntary release of (grand)-children from paternal power by those who possessed it, and made the recipients *sui iuris*. It was therefore not solely the demographic regime that determined the share of the population that was *sui iuris*. Changes in social practice could have generated fluctuations in the size of the *sui iuris* population relative to that of the total population. The number of

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<sup>127</sup> The *Tabula Heracleensis* from the Caesarian period gives an interesting insight into the possible prevalence of this phenomenon in Roman Italy when it states that 'it is not the intent of this law to require a person who has residence in several municipalities, colonies, or prefectures and who is entered in the census in Rome to be registered by this law in the census of the aforesaid communities as well' (§29). Whether this advice was followed is, of course, another matter.

<sup>128</sup> See Erdkamp's contribution to this volume.

people *sui iuris* was partly dependent upon the frequency of emancipation of (grand)children by their (grand)fathers. If the proportion and number of dependent children released from paternal power increased during the republican period, this would have effectively inflated the registered census population. We do not have sufficiently detailed information to enable us to pin down whether or when this practice became a major factor, but at least some specialists think that *emancipatio* of dependent children became important during the last phase of the Republic; and Gaius' *Institutes* imply that it occurred frequently during the Principate.<sup>129</sup> It has been suggested that the origin of the practice is to be attributed to colonization.<sup>130</sup> How geographical separation must have complicated the exercise of legal authority over a child is not too difficult to imagine, and if this indeed lay behind the phenomenon, increased migration caused by the rapid growth of Rome during the late Republic may well have encouraged the spread of premature release from *patria potestas*.<sup>131</sup>

*Women sui iuris in a sine manu marriage*

As Mommsen already remarked, with the rise of the *sine manu* marriage married women must have started to be entered independently in the census records.<sup>132</sup> Since this new type of marriage no longer placed women under the legal power of their husbands, the juridical status of women who entered this type of marriage would have changed from *alieni iuris* to *sui iuris* upon the death of their fathers or upon *emancipatio* prior to that. Over time their transition from the juridical category of the *alieni iuris* to that of the *sui iuris* must increasingly have followed the same pattern as that of men. Although a marriage *sine manu* seems to have been possible since the time of the Twelve Tables, this status was transitory rather than permanent: only during the first year of an *usus*-marriage did women not fall under the *potestas* of their husbands. During the early Republic there were therefore virtually no married women who were *sui iuris*. Conditions changed most probably only towards the end of the second century BC. From then on, marriages

<sup>129</sup> Gaius *Inst.* 1.16.18. Cf. Leonhard (1905); Costa (1901, 320).

<sup>130</sup> Leonhard (1905, 2478); Bourne (1952b, 182).

<sup>131</sup> I owe this suggestion to Luuk de Ligt.

<sup>132</sup> Mommsen (1887<sup>3</sup>, 365 n. 2).

*sine manu* became more widespread. By the time of Augustus, so it is commonly assumed, *manus*-marriage had mostly disappeared.<sup>133</sup>

Theoretically, women in a marriage who were *sui iuris* should belong to the same category as the orphans and widows who are explicitly referred to as being excluded from the census figures of 465 BC and 131/130 BC. Those women in a *sine manu* marriage whose fathers had died may have fallen under the category of *orbae*—like widows did in the phrase *orbi et orbae*.<sup>134</sup> However, they are not explicitly described as a group excluded from the census figures. In this they resemble manumitted female slaves, emancipated sons and *liberti* under the age of 17, emancipated unmarried women, women in a *manu* marriage who got divorced, and those who had belonged to the *familia* of a *pater familias* who had suffered *capitis deminutio* and were not adult males.<sup>135</sup> None of these categories is described as being excluded from the census figures; all of them were also *sui iuris*, but not adult males and in need of tutelage. We are simply uncertain as to what happened to them in the context of the republican census figures. All we can infer from our contextual evidence on the purposes of the census taking is that unless they had to pay taxes, their inclusion would not have served any practical purpose. As far as *sui iuris* women in a *sine manu* marriage are concerned, there is little reason to assume that they were taxed more heavily than orphans and widows. Since it is most reasonable to assume, as I have argued above, that orphans and widows were excluded from the census figures during the Republic but included under Augustus, there would be a certain logic in assuming that women *sui iuris* in a *sine manu* marriage followed the same pattern.

Acceptance of this hypothesis would have a substantial impact on the multiplier, since the implication would be that the Augustan census totals included most adult women. The percentage of women registered in the censuses of 28 BC, AD 6, and AD 14 would in fact have been nearly equal to that of men, given that by that time marriages *in manu* were uncommon (the *sui iuris* status of women being established in a similar manner to that of men). If we adapt our calculations in order to account for this factor, the multiplier would fall from 2.5 to a minimum

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<sup>133</sup> Evans Grubbs (2002, 21); Gardner (1998, 209–10); Treggiari (1991, 30–4); Looper-Friedman (1987).

<sup>134</sup> Above, at n. 47.

<sup>135</sup> Sachers (1943, 1498).

of 1.75.<sup>136</sup> We should then obtain an Italian population in the order of 7.5 million in 28 BC, and one of *c.* 8.1 million in AD 14 (if we follow Brunt's estimate of the number of citizens in the provinces).

*Freedmen: a demographically anomalous subpopulation*

The multiplier commonly used is also inflated due to the inclusion of enfranchised slaves who had received Roman citizenship. This is the case whether or not one wishes to adhere to a *sui iuris* hypothesis. What is important to realize is that freedmen are a group included in the census figures for which the usual multiplier works particularly badly. Because most women seem to have been freed only after their period of prime fecundity,<sup>137</sup> and their children born previous to that remained slaves, enfranchised slaves are rather unlikely to have produced many free children. So by multiplying their group by a factor equal to that of ordinary Roman nuclear families, we create ghost citizens, attributing to freedmen free children they did not have. How large a number is difficult to tell, but if the number of freedmen was even loosely related to the number of slaves, we can be sure that from the Second Punic War onwards they were on the rise. Thus the number of extra citizens added in error on account of the multiplier will also grow over time.

*Citizens outside Italy: registration and emigration*

The Roman census was by origin a census of citizens living in Italy. With territorial expansion, citizenship spread. For the debate on the size of the Roman population this is a phenomenon of major importance, as it raises the question of how many of the citizens recorded lived in Italy itself. The more citizens overseas were registered, the more we need to downscale our estimates of the number of inhabitants of Italy proper as inferred from the Augustan census totals.

It is generally thought that overseas citizens were registered rather unsystematically.<sup>138</sup> Two arguments have been adduced to support this view: the Roman state was not sufficiently developed to keep track of

<sup>136</sup> I assume here that *manus* marriages are completely absent. Thus the percentage of all women (of all ages) *sui iuris* can be derived directly from Saller's calculations (cf. above). It amounts to 57%, i.e. equal to the share of the male population *sui iuris*. Therefore the multiplier should be  $100/57 = 1.75$ , instead of  $100/40 = 2.5$ .

<sup>137</sup> Cf. Scheidel (1997) on the enfranchisement of female slaves in Egypt, and Scheidel (2005, 72–3; 75).

<sup>138</sup> Cf. Brunt (1971/1987<sup>2</sup>, 115).

citizens further afield, and there was little interest in Italy to register Romans living abroad. Therefore they were hardly registered for the census, if at all.

To my mind, the latter suggestion, that there was little interest in registering citizens overseas, entails a denial of the political concept of Empire. If Roman citizens left Roman Italy, they formed part of a flow of outward migration only from the perspective of a modern nation-state. For a Roman citizen as well as the Roman state this was still internal migration, and therefore it seems more likely that our figures for *cives Romani* were at least intended to include Roman citizens regardless of their geographical residence.

The argument that such a task—the registration of citizens living at a far distance from Rome—was beyond the capability of the Roman state seems undermined both by the notion that census registration in Italy must have involved more complex administrative processes than previously thought, and by the growing body of evidence that all kinds of lists were also created in the provinces.<sup>139</sup> All in all, it seems difficult to maintain that Rome was a society simply too underdeveloped to be capable of organizing such procedures. The number of citizens overseas that Brunt assumed to be registered is therefore likely to be on the low side, which leads one to think that the Italian population figure as implied by the middle count is artificially high and should be adjusted downward.

### XIII. *Measure and man: Augustus and the census shift*

Since I propose a shift in the meaning of the census figures under Augustus—albeit a shift different from that proposed by the low counters—I feel obliged to pay some attention to why this happened, even if a reconsideration of this problem produces no firm conclusion.

There has already been much speculation as to whether and why Augustus would or would not have changed the recording basis of the

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<sup>139</sup> See Brunt (1990, 345 f.) for a list of epigraphic evidence for provincial censuses. Nicolet (1991a, 133–9); Christol (2006, 31 f.). The *Fasti Ostienses* show that Augustus knew the assets of citizens in Cyrene: Nicolet (1991b), reprinted in Nicolet (2000, esp. 194 [cf. below]). Note also *CIL* 10.680, an inscription for T. Clodius Proculus, who was sent to the province of Lusitania to take the census there in the age of Augustus and one of the sources of evidence for provincial censuses quoted in Le Teuff (unpublished 2007).

census figures. This unsatisfactory state of affairs reflects the opaque nature of the surviving evidence. The truth is that we have in fact no independent evidence that can help us to explain what the mere figures reveal: that *something* was going on in the Augustan era. In the foregoing sections I have argued that Augustus started to include widows and orphans in his figures. That their inclusion should be dated to this period seems the most logical hypothesis: this is when the jump in our figures occurs, and the phrases added to some of the census figures suggest that widows and orphans were included at a certain point, but not during the Republic.

It seems, however, that we can push the argument further than that. We have only two republican figures for which Livy explicitly adds the clarification that they did not include orphans and/or widows: one in Book 3 and its *Periocha*, and the other in the *Periocha* of Book 59. Clearly Livy is trying to prevent confusion, but why did he do so on these two occasions, and why only then? The phrases are added to the figures for the censuses of 465 BC and 131/130 BC.<sup>140</sup> In itself this does not take us much further, as the historical records for these years seem to contain nothing that might have prompted Livy to give this kind of comment in regard to the censuses. However, as Walter Scheidel observes, there are indications that Livy composed Book 3 of the *Ab Urbe Condita* not long after the results of the census of 28 BC had become known, and Book 59 shortly after the Augustan census of 8 BC had been completed.<sup>141</sup> Therefore a fairly straightforward explanation for Livy's exegesis of the census figures in these books results from the hypothesis that Augustus started to include widows and orphans in his census counts. If the reported census totals indeed changed in character in Livy's days, it seems a reasonable supposition that he wanted to emphasize the different nature of the earlier censuses he was writing about when the Augustan census results had just been published. If contemporary events motivated him to introduce explanatory phrases, it is perfectly understandable why he did so only in these two instances.<sup>142</sup>

Why Augustus would have decided to go over to this new practice is a question of a different kind, and difficult to answer. His shift in focus

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<sup>140</sup> Livy 3.3.9 and *Per.* 59.7.

<sup>141</sup> Scheidel (forthcoming 1).

<sup>142</sup> Scheidel (forthcoming 1).

has been connected with his preference for family-oriented values and an interest in and concern for the entire family rather than just adult males. However, if his change in focus was indeed a conscious attempt to stress that for Augustus it was not just adult males who counted but rather families, he let slip some excellent opportunities to emphasize this ideological shift. In the *Res Gestae* he makes no connection between the census and his policy of propagating family life. The proclamation of a population figure that, when taken at face value, suggests a rise in population rather than a decline could hardly serve to express the ‘concern about the population problem’ (i.e. a decline) that Brunt argues lay behind an Augustan reform in the reporting of census figures. Nor can one hold that there were no concerns about population size, marriage, and childbearing earlier on, when the census totals did not include the entire population.<sup>143</sup> In that sense, Augustus’ new and ‘purely demographic interest’ was not so new.<sup>144</sup>

It would obviously make for a stronger case if we could instead attribute an alteration in the practice of recording census figures to changes in the practical goals of the census. This is not unproblematic either. The inclusion of all women and children cannot be linked to any of the threefold goals of registration analyzed above: just as before, they still were not given voting rights or expected to fight. An extension to include more types of citizens is therefore difficult to account for from the perspective of the census’ military and voting aims. In actual fact, over time the importance of both diminished as the army professionalized and the vote first came to be shared among a significantly larger number of people (with the extension of citizenship) and then lost all meaning as substantive decision-making passed to the emperor and the senate.

There remains the purpose of securing the state’s finances. But the one change in this realm connected with the Augustan era that could be relevant, the introduction of the *uicesima hereditatum*, postdates the changes in the census record. Moreover, when Augustus permanently installed this new tax of 5% on inheritances in AD 6, it came with a *lex Iulia uicesimaria*, which provided that all wills were to be opened before a magistrate and archived thereafter.<sup>145</sup> The amount of tax due by heirs,

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<sup>143</sup> E.g. Brunt (1971/1987<sup>2</sup>, 114).

<sup>144</sup> *Ibidem*.

<sup>145</sup> Wesener (1958, 2475).

*legatarii*, and other beneficiaries could therefore easily be established on the basis of these records, so that the introduction of the *uicesima* did not require census registration of groups that had previously not been targeted for fiscal aims.

Despite this, I should like to note in passing that although this tax was not permanently imposed until AD 6, Augustus—then still Octavian—had already made an attempt to reform the state’s taxation system in order to secure its solvency in 40 BC by means of an inheritance tax. We do not know if the later *lex Iulia uicesimaria* was part of this initial proposal. The accounts of Appian and Cassius Dio, which explicitly describe his determination to reinstate the inheritance tax after its initial failure as well as the scheming he resorted to in order to achieve his goal,<sup>146</sup> and the edict of the *Fasti Ostiensis* of 7/6 BC, which shows how Augustus counted people of all ages who owned property,<sup>147</sup> all testify to his ongoing attempt to secure the state’s finances by means of fiscal reforms. As late as AD 14 he still contemplated a change from inheritance tax to land tax (*tributum*) for all Roman citizens.<sup>148</sup> Taking into consideration that it was not clearly established from the outset which citizens and what kinds of property would be targeted for new taxation, it seems possible to hypothesize that the decision to include all property owners, i.e. all people *sui iuris*, in the census of 28 BC was in fact somehow connected with the fiscal purposes of the census.

<sup>146</sup> App. *BC* 5.67 f.; 5.130; DC 55.25.4–5; 56.28.4–6. His request to the senate to investigate all other possible sources of revenue first is regarded as mere lip service by DC 55.24.4–5, where he stresses that Augustus already had his own plans. Cf. also Günther (2005) on Octavian’s claim that his legislation went back to an idea stated in Julius Caesar’s memoirs.

<sup>147</sup> Nicolet (1991b), reprinted in Nicolet (2000, esp. 194). Text to be found in *SEG* 9, no. 8, 1.4–6: ἐπειδὴ τοὺς πάντας εὐρίσκω Ῥωμαίους ἐν τῇ περὶ Κυρήνην ἐπαρχίᾳ πέντε καὶ δέκα καὶ διακοσίους ἐκ πάσης ἡ<λ>ικίας δισχειλίων καὶ πεντακοσίων διναρίων ἢ μείζω τίμησιν ἔχοντας. Nicolet recently drew attention to the phrase ‘of all ages’, reading it as a firm indication that Augustus included all citizens in his census totals. He suggests that knowledge of the total number of citizens might have become important because of the *uicesima hereditatum*, a new tax of 5% on inheritances. However, I should suggest it shows that his interest was in those citizens, whatever their age, who had financial assets. Since Augustus speaks of people who possess a certain amount of money, he cannot refer to people *alieni iuris*: these could not have any money in their own right. Rather, the ‘people of all ages’ he speaks of must be those *sui iuris*—a group denoted both by their unlimited age range and their ownership status. Christol (2006, 36) holds that ‘la *uicesima hereditatum*, instituée par Auguste, réinsère fortement les citoyens romains dans la trame du *census*’ but does not address the problem of the interpretation of the *capita civium* recorded on the census lists.

<sup>148</sup> DC 56.28.4–6.

XIV. *Concluding remarks*

Rejection of the high count as an extreme hypothesis cannot by itself lead us to the size of the population inhabiting Roman Italy. I should agree with others in thinking that the high count in its present form creates more difficulties for our understanding of Roman history than does the low count. Even so, we have to pay equal attention to the problems associated with the so-called low count. To my mind, the logical inconsistencies involved in thinking that the republican census figures represent all adult males, which I have outlined above, force us to reconsider the—often implicit—acceptance of this dominant interpretation. In my view our enigmatic sources allow an alternative reading that was briefly suggested in earlier literature but received little attention overall: the hypothesis that the republican figures comprise only those adult males who were *sui iuris*.

This interpretation enables us to argue for a diversity of middle counts for the Augustan period, for the multiplier required to get from census totals to population totals becomes more flexible in this scenario. Given the scarcity and nature of the ancient evidence, the only hypotheses and variables of which the numerical implications can fruitfully be explored are those concerning the inclusion of adult males *sui iuris*, widows, and wards in the Augustan figures. Such an interpretation of the Augustan census figures would limit the size of the free Roman population of Italy to somewhere around 10 million.

The inclusion of widows and wards is, I believe, likely not to be the only factor that explains the apparent steep rise between the republican and the Augustan figures. Therefore the actual number of inhabitants of Italy was probably considerably lower than 10 million. The credibility of each of the hypotheses outlined above—that the multiplier for freedmen must be much reduced; that the census was perhaps more prone to over-registration than it had been before; that migrants to or in Rome or elsewhere in Italy who had not (or had to a lesser extent) been registered earlier as *sui iuris* were now in the records; that the number of overseas citizens was perhaps larger than Brunt assumed; and that married women might be *sui iuris* rather than *in potestate* will determine to what extent the multiplier should be lowered for the Augustan censuses.

Consequently, there is no single population figure to be presented. A range of potential combinations leads to one of several possible lower or higher middle counts. Quantification is essential if we are

to judge what the (combined) effects of these factors would be for our view on the size of the total population of Roman Italy. But given the nature of our evidence, I am reluctant to attempt to quantify any of these other potential factors: I cannot see a way to narrow further the range of possibilities by tightening up the lower and upper limits of a middle count. What is clear though is that all of the factors mentioned would push in the same direction, enlarging the share of the population registered under Augustus. Therefore, even if we cannot give a specific figure for the size of the Roman population under Augustus, we can go beyond complete agnosticism: the actual number of Roman citizens in a middle count would be closer to the current low than to the current high count.

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ROMAN CENSUS FIGURES IN THE SECOND  
CENTURY BC AND THE PROPERTY QUALIFICATION  
OF THE FIFTH CLASS

Elio Lo Cascio

Recent contributions (among others, by Rich, Lo Cascio, Rosenstein, and De Ligt)<sup>1</sup> have cast serious doubt on the traditional account of the agrarian and demographic history of the second century BC and on earlier reconstructions of the background to the *lex agraria* proposed by Tiberius Gracchus. This traditional scenario, built on the massive analyses by Toynbee and Brunt, has found its most elegant presentation in the first chapter of Hopkins' *Conquerors and Slaves*, where we also find a tentative reconstruction of the quantitative dimensions of the interplay between the various factors involved.<sup>2</sup> The recent contributions already mentioned, though differing widely in their interpretation of individual factors, all question two of the central tenets underlying this scenario. The first of these tenets is that the second century BC or its central decades witnessed a substantial decrease in Italian manpower generally or at least in the number of adult males liable for service in the armies which were fighting for the conquest of the Mediterranean. At the same time, those questioning this assumption have cast doubt on the traditional view that large parts of the Italian countryside came to be populated by imported slaves, replacing a dwindling free country-dwelling population. In offering various alternative reconstructions of the demographic history of the Italian peninsula in the second century BC, most of these recent contributions try to base themselves on the census figures for that period, which have been preserved chiefly in Livy and the Livian tradition. In this paper I shall first summarize the most important conclusions of these various contributions and then try to assess the reliability of these data as a basis for demographic reconstructions. Can the evidence of the census figures be taken to reveal real demographic change (that is, natural increase or decrease

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<sup>1</sup> Rich (1983); Rich (2007); Lo Cascio (1994; 1999; 2000; 2001a; 2004); Rosenstein (2003; 2007); De Ligt (2004; 2006; 2007a; 2007b); see also Morley (2001).

<sup>2</sup> Toynbee (1965); Brunt (1971); Hopkins (1978, *c.* 1).

of population) or is it to be interpreted rather as reflecting gains or losses of citizenship by certain individuals or groups, and changes in the efficiency of census taking?

More than twenty years ago John Rich opened the debate by ably questioning the orthodoxy of a supposed manpower shortage. In particular he objected forcefully to the idea that the reduction of the property qualification of the fifth class could have been the means used to solve the increasing difficulties in recruitment caused by a decrease in the number of *assidui*. He observed that, even if we admit such a decrease, the demand for recruits made on the *assidui* was lower after 168 BC than in the earlier decades of the century.<sup>3</sup> If I understand Rich's original position (which seems to differ from some of his later formulations) correctly, he did not dismiss the "contemporary anxieties about manpower", which are undeniable and must be connected with worries about the increase in the number of slaves, as being completely unfounded. Nonetheless he pointed out that the idea of a deracination or an uprooting of the peasantry was shown by the archaeological evidence of rural settlement in many areas of the Italian peninsula to be manifestly exaggerated.

In more recent years a more radical thesis of a substantial increase in the rural population of Italy has been advanced as part of a reassessment of the meaning of the Augustan census figures by the so-called high counters.<sup>4</sup> As far as the central decades of the second century BC are concerned, the high counters have adduced three arguments against the theory that this period saw a decline in the free rural population. The first of these is that the evidence collected during survey campaigns carried out in various parts of Italy points to the continued existence of a large free rural population.<sup>5</sup> Second, there is the economic logic of

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<sup>3</sup> Rich (1983).

<sup>4</sup> Lo Cascio (1994; 1999; 2000; 2001a; 2004); Lo Cascio & Malanima (2005); Kron (2005).

<sup>5</sup> See among many others the contributions in Lo Cascio & Storch (2001), which in any case show the extreme diversity of rural landscapes in the different regions of the Italian peninsula and across different periods of time. I must admit that I am not impressed by the argument put forward by De Ligt (2007b, 176 f.) against Frederiksen (1970–71), based on the allegedly wrong attribution to the second century BC of most of the black-glaze pottery found at the sites detected during the survey campaigns in South Etruria; see also, for example, Rich (2007, 158 f.) and Gualtieri in his contribution to this volume. I do not want to rehearse here the problem of the 'missing sites'; I notice only that the much greater difficulty involved in detecting small peasant farms may result in the underestimation of the free rural population. De Ligt is prepared to

the so-called villa system, which, in order to be really efficient, needed the seasonal labour provided by peasant smallholders.<sup>6</sup> Third, and most important, the theory that Italy's free country-dwelling population was declining is supported neither by Appian's presentation of the development of the legislation *de modo agrorum* nor by his analysis of the reasons lying behind the bill proposed by Tiberius Gracchus.<sup>7</sup> In fact, his presentation hints at a situation of clear and, I should say, structural population pressure in the Italian countryside. It is in this demographic context that we must situate not only the pre-Gracchan legislation *de modo agrorum* and the policy of colonisation (as in fact has become the standard view after the publication of Tibiletti's epoch-making articles on this theme),<sup>8</sup> but also Gracchus' *lex agraria* itself. In the decades preceding Gracchus population pressure may also have led a growing number of citizens to apply some of the Malthusian preventive checks, such as celibacy, late marriage, and family limitation, and this development may explain some of the comments and reactions referred to in our sources, such as the admonitions of men such as Metellus, the censor of 131/0 BC.<sup>9</sup>

Some of the low counters, in particular Luuk de Ligt, have objected to the interpretation of the Augustan census figures given by the high counters and therefore to their general reconstruction of the demographic development of Italy from the third century BC to the beginning of the Empire. But even if De Ligt refuses to accept the revisionist view, he nonetheless maintains that there was no population decline in the second century BC and even in the first century BC. In fact, he posits a long-term trend of population growth which would have been checked by substantial waves of emigration from the Italian peninsula towards Cisalpina and the provinces and which from the middle of the second century onwards BC, when population pressure began to be felt, could have furthered the development of tenancy as

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concede that the archaeological evidence as a whole does not support the idea of a generalized, strong decline of the rural population after the Hannibalic War all over the Italian peninsula.

<sup>6</sup> This observation can be traced back to Max Weber (see especially Capogrossi Colognesi (1990, 56 f.)) and has been repeated, in addition to Capogrossi Colognesi himself in several contributions, by Garnsey (1980); Skydsgaard (1980); and Rathbone (1981).

<sup>7</sup> App. *BC* 1.7–9: see especially Lo Cascio (1999, 231 f.; 2004, 113 f.).

<sup>8</sup> Tibiletti (1948/9); Tibiletti (1950), now conveniently reprinted in Tibiletti (2007).

<sup>9</sup> Livy, *Per.* 59; Suet. *Aug.* 89.2; Gel.1.6.

an “alternative survival strategy” for impoverished peasants.<sup>10</sup> However, instead of interpreting the testimony of Appian as pointing to a serious problem of population pressure, he simply dismisses it as wholly unreliable.<sup>11</sup> In a similar vein John Rich in his latest contribution to the debate states that “paradoxically the distress which Gracchus associated with depopulation may have been partly the opposite”,<sup>12</sup> implying on the one hand that there was population pressure, but also that Appian was wrong in his diagnosis. While maintaining that there is no secure evidence for population decline during the second century BC, De Ligt tries to counter the revisionist approach by developing some numerical arguments. One of these concerns a peculiar interpretation of the famous passage in Polybius about the events of 225 BC, with its account of the military strength of the Romans and their allies on the eve of the Gallic invasion of the peninsula,<sup>13</sup> a topic on which I cannot dwell here. For his reconstruction of developments during the second century BC he relies on a rather optimistic assessment of the reliability of the census figures for the period 203–114 BC, which he interprets as covering both *assidui* and *proletarii*, and as representing therefore more or less accurately the number of adult males. Despite the clear statements made by Cicero and Dionysius of Halicarnassus<sup>14</sup> (which can be traced back to their annalistic sources), he holds that the number of rural *proletarii* must have been low in the last quarter of

<sup>10</sup> De Ligt (2007a, 8).

<sup>11</sup> According to De Ligt (2007b, 177), “It is... difficult to understand how the idea that Appian refers to the onset of a Malthusian crisis can be reconciled with Lo Cascio’s view that the free Italian population continued to grow at a rate of 0.4 per cent annually for another century”. This criticism can be countered as follows: 1) a rate of 0.4% for two centuries (I assume that De Ligt refers to the estimates given for 225 BC and 28 BC in LoCascio & Malanima (2005, 208 tab. 2), which actually imply a *range* of rates of increase of between 0.31% and 0.49%) can conceal widely different rates of increase (or even decrease) in the shorter term; 2) population pressure or overpopulation can be experienced in some areas (say the peninsula or some regions of it) and not in others (the Po Valley). In my previous contributions I assumed that population pressure in the last decades of the second century BC was released by a large wave of emigration, chiefly towards Cisalpina, but also to other provinces (see e.g. Lo Cascio (2003)). The importance of emigration has been underlined by De Ligt himself: see De Ligt (2004) and also Crawford’s contribution to this volume.

<sup>12</sup> Rich (2007, 165).

<sup>13</sup> Plb. 2.23–4; but see Lo Cascio (1991/94) and (2000, 166 ff.). I intend to return to the interpretation of this passage elsewhere.

<sup>14</sup> Cic. *Rep.* 2.40; Dion. Hal. 7.59.6.

the second century BC, partly because he revives the old idea (going back to De Sanctis and Fraccaro and fully developed by Gabba)<sup>15</sup> of a lowering of the census qualification of the fifth class between 130 and 125 BC. This leads him to deny that the census figures for the second century BC, and especially those for 125/4 and 115/4 BC, are to be regarded as seriously defective. Moreover, he considers the overall rise in the census figures as reflecting a genuine demographic trend.

Nathan Rosenstein is also convinced that the censors managed to register most of the adult male citizen population and that the number of *proletarii* must have been very low. It is the latter view which led him to propose a radically new interpretation of Livy's testimony concerning the activities of the censors in 214 BC, from which Peter Brunt had tentatively deduced that on the eve of the Hannibalic War *assidui* made up slightly less than half of the adult male citizen population.<sup>16</sup> He also believes that the population was increasing during the second century BC. In his view, the resulting population pressure in the countryside was released not only by recruiting a very high percentage of adult males into the army, but also by the scale of mortality in war affecting the men serving in the Roman armies conquering the Empire. If I understand his arguments correctly, he thinks that this additional mortality had the paradoxical effect of increasing the birth-rate among the survivors, thus contributing to population growth.

In short, both De Ligt and Rosenstein want to revive the notion of population growth, or at least of the absence of decline, without correcting the general reconstruction developed by the orthodox school of Beloch and Brunt and without exploring the possibility of a causal connection between the "anxieties about manpower" which we find in our sources<sup>17</sup> and a situation of population pressure. In developing their alternative reconstructions they rely heavily on the evidence of the census figures, which lend a certain measure of support to a scenario of population increase. The problem, however, is that these figures can be considered to represent population change only to a limited extent, as Brunt realized. No one will doubt their general reliability (even if at least in one case—the census of 179/8 BC or the census of 174/3

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<sup>15</sup> Lo Cascio (1988) and references there.

<sup>16</sup> Rosenstein (2002).

<sup>17</sup> Rich (1983, 299).

BC—we can be certain that one of the transmitted figures is corrupt).<sup>18</sup> However, there can be no doubt that the sometimes huge fluctuations in the census figures cannot reflect a natural population trend, and that these discontinuities are much more easily and legitimately explained as reflecting the operation of a variety of factors affecting the procedure of census taking. In many cases the impact of these factors is clearly discernible, the most significant example being the sudden increase in the number of *ciuium capita* between 131/30 and 125/24 BC.

Year	Census figure	Source
265/4 BC	292,234	Eutropius 2.18
252/1 BC	297,797	Livy <i>Periochae</i> 18
247/6 BC	241,712	Livy <i>Periochae</i> 19
241/0 BC	260,000	Hieronymus Ol.134.1
234/3 BC	270,713	Livy <i>Periochae</i> 20
209/8 BC	137,108	Livy 27.36
204/3 BC	214,000	Livy 29.37
194/3 BC	143,704	Livy 35.9
189/8 BC	258,318	Livy 38.36
179/8 BC	258,794	Livy <i>Periochae</i> 41
174/3 BC	269,015	Livy 42.10
169/8 BC	312,805	Livy <i>Periochae</i> 45
164/3 BC	337,022	Livy <i>Periochae</i> 46
159/8 BC	328,316	Livy <i>Periochae</i> 47
154/3 BC	324,000	Livy <i>Periochae</i> 48
147/6 BC	322,000	Eusebius Armen. Ol.158.3
142/1 BC	327,442	Livy <i>Periochae</i> 54
136/5 BC	317,933	Livy <i>Periochae</i> 56
131/0 BC	318,823	Livy <i>Periochae</i> 59
125/4 BC	394,736	Livy <i>Periochae</i> 60
115/4 BC	394,336	Livy <i>Periochae</i> 63
86/85 BC	463,000	Hieronymus Ol.173.4
70/69 BC	910,000	Phlegon fragment 12.6
28 BC	4,063,000	<i>Res Gestae</i> 8.2
8 BC	4,233,000	<i>Res Gestae</i> 8.3
AD 14	4,937,000	<i>Res Gestae</i> 8.4

Roman census figures (265 BC–AD 14)

<sup>18</sup> The figure for 179/8 BC reported in Livy *Per.* 41 is 258,794; the figure for 174/73 BC reported in Livy 42.10 is 269,015, but Livy says that this last figure was much lower than the one of the previous census.

Before proceeding to analyse some of these fluctuations, however, I want to underline a point which seems to me important. Even if census figures are unlikely to comprise most adult men of citizen status, the series of census figures as a whole does at least indicate the *minimum* possible size of the population of a large area of the Italian peninsula (roughly 55,000 km<sup>2</sup> at the end of the second century BC according to Beloch).<sup>19</sup> What I mean by this is that, even for the high counters the proportion of *incensi*, in any particular case, cannot normally have been higher, or at least not much higher, than 50%. If we accept the census figures for the end of the second century as correct, that is, if we ignore the possibility that many citizens remained underregistered, they imply a population density in the *ager Romanus* of at least 24 inhabitants/km<sup>2</sup>, comparable to the density deduced by Brunt himself for the entire territory subject to the Romans in 225 BC (25.5 inhabitants/km<sup>2</sup>), though much lower than the density of the *ager Romanus* itself in 225 BC according to Brunt's calculations (36 inhabitants/km<sup>2</sup>).<sup>20</sup> It is only with the censuses of 86/85 BC and 70/69 BC that the two different views of Roman population history begin to diverge radically. In these years the area of the *ager Romanus* was 160,000 km<sup>2</sup>. If we assume a moderate rate of under-registration of around 10%, as the low counters do, and thus posit a total population of 3,000,000, we have to account for a drop in the density of the population from, say, 26 persons/km<sup>2</sup> to 18.75 persons/km<sup>2</sup>. How can we explain this drop? Several possible explanations can be put forward: that the territory of the ex-allies was much less densely populated than the territory of what was previously *ager Romanus* (respectively 15 and 26 inhabitants/km<sup>2</sup>); that the population did in fact decrease drastically, for example as a result of the Social War and of the first Civil War; or that the rate of under-registration increased dramatically. Of course, the correct solution might be a combination of these three possibilities. In any case the implications of this numerical exercise are quite interesting. It would seem that the low counters must either assume that the demographic effects of the Social and Civil Wars were utterly disastrous, or take on board the view that population densities within the territories of the new *civies* were very low in comparison with the old *ager Romanus*. If they are not prepared to defend either of these propositions, they must maintain that, precisely

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<sup>19</sup> Beloch (1880, 73 f.).

<sup>20</sup> Brunt (1971, 54, table V).

in consequence of the extension of the citizenship to the whole of peninsular Italy, under-registration increased drastically.

Whatever the situation after this extension, it seems to me that the set of census figures for the second century BC as a whole, for all their undeniable defects, at least indicates a rough order of magnitude for the *minimum* possible size of the citizen population of the Italian peninsula, even for the high counters.

In dealing with the fluctuations observed in the individual figures for the second century, I want to insist on two preliminary and general points which seem to me decisive. I think we have enough evidence to demonstrate the crucial importance of the registration of men under arms as a factor influencing fluctuations in the number of *ciuium capita* covered by the censuses. It appears that under normal circumstances men *sui iuris* serving in the army did not present themselves before the censors in order to make the *professiones* necessary for their registration. It follows that they were not counted, and in fact what we know about the censuses of 204/3 BC and 169/8 BC would remain inexplicable if we do not accept this conclusion.<sup>21</sup> The second point is that we have no evidence whatsoever to suggest that any of the sudden increases in the number of *ciuium capita* during the second century BC reflected a reduction in the property qualification for the fifth class, which would have, in a sense artificially, increased the number of *assidui*. One reason for this is that it is virtually certain that the numbers of the *ciuium capita* given by our sources refer to all adult males and not only to *assidui*. The opposite idea, maintained by Herzog, Gabba, Pieri, Earl, and other scholars,<sup>22</sup> has not won acceptance, and for good reason, as I have argued elsewhere.<sup>23</sup> Nor can it plausibly be argued that a lowering of the census qualification for the fifth class led to many former proletarians, now turned into *assidui*, being registered, for the simple reason that there is no evidence whatsoever to back up the theory that such a reduction actually took place during the second century BC.

It is not necessary for me to rehearse what I argued almost twenty years ago.<sup>24</sup> It seems to me highly significant that almost all the figures

<sup>21</sup> Liv. 29.37.5: *lustrum conditum serius quia per prouincias dimiserunt censores ut ciuium Romanorum in exercitibus quantus ubique esset referretur numerus*; Liv. 43.14. 5–10: see below.

<sup>22</sup> Herzog (1877); Gabba (1949; 1952); Pieri (1968, 177 ff.); Earl (1963, 35 ff.); see also Molthagen (1973, 439 ff.); Schochat (1980, 9 ff.).

<sup>23</sup> Lo Cascio (2001b, 570 ff.).

<sup>24</sup> Lo Cascio (1988); see also Rathbone (1993), following some of the main conclusions of my reconstruction.

for the property qualifications of the classes—and the figure for the fifth class in particular—that we encounter in the literary sources are explicitly referred to ‘Servian’ times, or at least to a much earlier period than the second century BC, and certainly to a period in which the monetary standard was still the libral *as* (or the reduced libral *as* of ten ounces).<sup>25</sup> The very few exceptions to this rule are the figures for the first and fifth classes given by Polybius<sup>26</sup> and, according to some scholars, the figure for the first class given by Gellius.<sup>27</sup> For this reason I suggested that the entire set of figures for the property qualifications given by Livy and Dionysius must be regarded as a translation, in terms of the later and much lighter *asses*, of an earlier set of census qualifications expressed in libral *asses*. I also suggested that the property qualification of 1,500 (or rather 1,100 *asses*) for *assidui*, which we find in a famous chapter of Cicero’s *de Republica*, and which claims to describe the Servian system,<sup>28</sup> refers to libral *asses* before the progressive reduction of the standard, which began, in my view, already during the First Punic War,<sup>29</sup> and according to Michael Crawford during the Hannibalic War.<sup>30</sup> That the system described by Cicero was actually the ‘Servian’ system and not the reformed one, even if it envisaged 70 centuries for the first class, has recently been convincingly reasserted by various scholars.<sup>31</sup> His source for the centuries could have been the annalist Vennonius, who is said by Dionysius to have attributed all the thirty five tribes to Servius Tullius.<sup>32</sup> This interpretation of the Ciceronian passage obviously rules out the theory that a reduction to 1,500 uncial *asses* must have taken place immediately before 129 BC, the dramatic date of the *de Republica*. The other reference to 1,500 *asses* being the property qualification for *proletarii* and 375 *asses* being the threshold for the *capite censi*, wrongly thought to be different from *proletarii*, that we read in Gellius and Nonius Marcellus must also refer to this early stage. The validity of this inference is not affected by the possibility that the origin of the mistake was the equivalence of four *asses* with a *sestertius*,

<sup>25</sup> Liv. 1.43.7; Dion. Hal. 4.17.2; Cic. *Rep.* 2.40; Gel. 16.10.10; Non. p. 227 L.; Fest. p. 100 L.; Plin. *Nat.* 33.43.

<sup>26</sup> Plb. 6.19.2; 23.15.

<sup>27</sup> Gel. 6 (7).13.

<sup>28</sup> Lo Cascio (1988, 286 ff.).

<sup>29</sup> Lo Cascio (1980/1; 1998, 187 ff.).

<sup>30</sup> Crawford (1974, 43, 595 ff.).

<sup>31</sup> Most notably by di Gennaro (1993).

<sup>32</sup> Dion. Hal. 4.15.1.

in force after the retariffing of the *denarius*, dated by most scholars (but against the testimony of Pliny) to the 140s BC.<sup>33</sup>

Even allowing for the allegedly conjectural character of many aspects of my reconstruction, it seems to me that the only conclusion we are entitled to draw from the whole of this intractable evidence is that the census qualification of the first class must have been twenty-five times the census qualification of the fifth class in the central decades of the second century BC, as suggested by the testimony of the sixth book of Polybius, whereas in an earlier period (the last decades of the third century BC—or maybe even before) it was eight times, if we accept Dionysius' presentation of the 'Servian' system, or roughly nine times, if we accept Livy's version. This can be explained by looking at the development of the monetary system and the census qualifications in the period after Cannae, when the standard of the *as* was rapidly reduced in several stages and the old monetary system effectively collapsed. The evolution of the census rating of the fifth class can be accounted for if we suppose that when the monetary system was rebuilt around the new sextantal *as* and the new *denarius*,<sup>34</sup> the property qualifications of all the classes were not readjusted to the new standard of the *as* to the same extent. This was perhaps the only time when the property qualification of the fifth class was strongly reduced in real terms, in order to increase the pool of possible recruits for the Roman army in an emergency.

Some other fluctuations in the census figures can be easily explained by looking at groups of people who lost or acquired the Roman citizenship. Of course, those who lost their citizenship included the colonists of the new Latin colonies, which were few but still important in the first decades of the century. Moreover, many Roman citizens were sent to old Latin colonies. Among those who acquired citizenship, albeit illegally, were the Latins who migrated to Rome and were registered there, provoking the bitter resentment of the magistrates of their communities over the loss of manpower resulting from this. This resentment expressed itself twice, in 187 BC and in 177 BC. The previous censuses therefore included them; the following excluded them (12,000 Latins were expelled after 187 BC).<sup>35</sup> With the census of 189 BC the Cam-

<sup>33</sup> Plin. *Nat.* 33.45.

<sup>34</sup> Crawford (1974, 3 ff.).

<sup>35</sup> Liv. 39.3.4–6: *legatis deinde sociorum Latini nominis, qui toto undique ex Latio frequentes conuenerant, senatus datus est. his quarentibus magnam multitudinem civium suorum Romam com-*

panians resumed their rights as citizens, a development which raised the problem of where they had to be registered. The senate's decision was Rome.<sup>36</sup> I deduce from this that before their defection they had their local census, as *ciues sine suffragio*. The question facing the senate was therefore whether they were to obtain once more the political and administrative machinery needed to hold the census in Capua. In my view there are very strong grounds for believing that after 338 BC, that is beginning with the census of 332 BC, the *ciues sine suffragio*, who were not registered in the tribes, were not included in the enumeration of the *ciuium capita* and were therefore expected to have autonomous censuses.<sup>37</sup> When the Campanians were reintegrated into the citizenship, they were still members of an *urbs trunca, sine senatu, sine plebe, sine magistratibus*, to use the words of Livy.<sup>38</sup> In other words, because they no longer had the administrative machinery needed for an autonomous census, they had to be registered at Rome. This must mean that their number was added (for the first time, in my view) to the number of *ciuium capita*. In the same census the *ciues* of some other communities of *ciues sine suffragio*—Formiae, Fundi, Arpinum—which had received the full citizenship, were also included in the total of the *ciuium capita*.<sup>39</sup> The

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*migrasse et ibi census esse, Q. Terentio Culleoni praetori negotium datum est, ut eos conquireret, et quem C. Claudio M. Livio censoribus postue eos censores ipsum parentem eius apud se censum esse probassent socii, ut redire eo cogeret, ubi censi essent. hac conquisitione duodecim milia Latinorum domos redierunt, iam tum multitudine alienigenarum urbem on<e>rante; Liv. 41.8.6–7: mouerunt senatum et legationes socium nominis Latini, quae et censores et priores consules fatigauerant, tandem in senatum introductae. summa querellarum erat, ciues suos Romae census plerosque Romam commigrasse; quod si permittatur, perpaucis lustris futurum, ut deserta oppida, deserti agri nullum militem dare possint; Liv. 41.9.9: legem dein de sociis C. Claudius tulit <ex> senatus consulto et edixit, qui socii nominis Latini, ipsi maioresue eorum, M. Claudio T. Quinctio censoribus postue ea apud socios nominis Latini censi essent, ut omnes in suam quisque ciuitatem ante kal. Novembres redirent; Liv. 42.10.1–3: Eo anno lustrum conditum est; censores erant Q. Fulvius <Flaccus A. Postumius> Albinus; Postumius condidit. censa sunt ciuium Romanorum capita ducenta sexaginta nouem milia et quindecim, minor aliquanto numerus, quia L. Postumius consul pro contione edixerat, qui socium Latini nominis ex edicto C. Claudii consulis redire in ciuitates suas debuissent, ne quis eorum Romae, et omnes in suis ciuitatibus censerentur; on this whole issue see now Laffi (1995).*

<sup>36</sup> Liv. 38.28.4: *Campani, ubi censerentur, senatum consuluerunt; decretum, uti Romae censerentur*; Liv. 38.36.5–9: *Campani, cum eos ex senatus consulto, quod priore anno factum erat, censores Romae censi coegissent—nam antea incertum fuerat, ubi censerentur—, petierunt, ut sibi ciues Romanas ducere uxores liceret, et, si qui prius duxissent, ut habere eas, et nati ante eam diem uti iusti sibi liberi heredesque essent. utraque res impetrata.*

<sup>37</sup> Lo Cascio (2001b, 577 ff.) and references there.

<sup>38</sup> Liv. 31.29.11.

<sup>39</sup> Liv. 38.36: *De Formianis Fundanisque municipibus et Arpinatibus C. Valerius Tappo tribunus plebis promulgauit, ut ius suffragii latio—nam antea sine suffragio habuerant ciuitatem—esset. Huic rogationi quattuor tribuni plebis, quia non ex auctoritate senatus ferretur, cum intercederent, edocti, populi esse, non senatus ius suffragium, quibus uelit, impertire, destiterunt incepto. Rogatio perlata est,*

inclusion of the Campanians and of these other communities following the bestowal of full citizenship can partly explain the big leap from the figure of 194–193 BC. As for the effect of manumission, I do not think that we have any means of calculating its impact on the increase in the number of *ciuium capita*, notwithstanding the ingenious attempts made by some scholars (such as Dumont) to estimate the proportion of *liberti* in the citizen body.<sup>40</sup>

It seems to me that the only other way to explain the fluctuation of the census figures is to suppose that the number of *incensi* was not always the same, not only because the censors were not uniformly effective in conducting the registration, but also because the real working of the procedure still defies our attempts at reconstruction. For example, from a famous passage in Livy we know that the censors of 179/8 BC, M. Aemilius Lepidus and M. Fulvius Nobilior, introduced an important reform which changed the way in which the citizen body was divided among tribes and centuries.<sup>41</sup> This reform may well have had an impact on the efficiency of census-taking, but on this we can only speculate. As noted above, we have solid evidence that serving soldiers who were *sui iuris* were not always registered and counted. One wonders what happened to their sons of military age *in potestate*. It seems to me that the care taken by the censors of 204 BC in registering soldiers overseas, explicitly noticed by Livy,<sup>42</sup> can explain why the figure for this census was much higher than the previous and the following figures. As for the *proletarii*, I have argued elsewhere that many or most of them did not register, notwithstanding the severe punishment originally established for the *incensi*.<sup>43</sup> This punishment, which was characteristically the same as that affecting those *assidui* who did not present themselves at the levy after having been individually conscripted, must have been obsolete by the second century BC, especially for men who were not obliged to serve. For the same reason there would have been no particular interest for the Roman authorities to insist on the registration of *proletarii*.

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*ut in Aemilia tribu Formiani et Fundani, in Cornelia Arpinates ferrent; atque in his tribubus tum primum ex Valerio plebiscito censi sunt.*

<sup>40</sup> Dumont (1987, c. 1).

<sup>41</sup> Liv. 45.51.9: *mutarunt suffragia, regionatimque generibus hominum causisque et quaestibus tribus discipserunt.*

<sup>42</sup> 29.37.5; see above.

<sup>43</sup> Cic. *Caec.* 99; Dion. Hal. 4.15.6; 5.75.4; Gaius *Inst.* 1.160; *tit. Ulp.* 11.11; on the reason why a punishment of comparable gravity is enacted by the *Lex Osca* of the *Tabula Bantina* (*Roman Statutes* 13) see Lo Cascio forthcoming.

They would have registered themselves only if they could draw a real advantage from registration. In this context it must be remembered that registering oneself was a very difficult job, especially if it still required the personal attendance of a *sui iuris* at Rome. I think that was still the case during the whole of the second century BC. In my view the decentralization of the census was introduced only by the Caesarian measure included in the *Tabula Heracleensis*.<sup>44</sup>

It is well known, however, that many scholars, from Brunt, to Galsterer, Humbert, and most recently De Ligt have used the extension of the *ager Romanus* and the relative autonomy of *municipia* and *coloniae* as arguments against the survival of a centralized census until the time of Caesar.<sup>45</sup> They have pointed to a passage in Livy which seems to prove the existence of a decentralized census as early as 169/8, even if it is not entirely clear to what extent, if any, they see this procedure as one of the factors behind the “greater efficiency”, to use the expression of Brunt, shown by the censors of these years. However, it seems to me that, instead of proving the existence of a decentralized census, the episode narrated by Livy actually confirms the existence of a centralized census based in Rome in these years.<sup>46</sup> In the context of the difficulties encountered during the levy for the Macedonian campaign, the new censors, Livy says, issued an edict which added to the customary oath—the oath, that is, required of a *sui iuris*—an additional one required of all *iuniores*. Every *iunior* had to swear that he was a *iunior*, that he had presented himself at the *dilectus*, and that he would do so in the future whenever there was a new *dilectus*, as long as these censors were in charge. Livy adds that there were many soldiers on leave from the legions of Macedonia without a valid justification, because

<sup>44</sup> *Roman Statutes* 24, ll. 141–63: see Lo Cascio (1990, 308 ff.).

<sup>45</sup> Brunt (1971, 33 ff., 522 f. and *passim*); Humbert (1978, 320 ff.); Galsterer (1976, 111); De Ligt (2007b, 173 f.).

<sup>46</sup> Liv. 43.14.5–10: *censores, ut eam rem adiuvarent, ita in contione edixerunt: legem censui censendo dicturos esse, ut praeter commune omnium civium ius iurandum haec adiurarent: ‘tu minor annis sex et quadraginta es tuque ex edicto C. Claudii T. Semproni censorum ad dilectum prodisti et, quotienscumque dilectus eris, quoad hi censores magistratum habebunt, si miles factus non eris, in dilectum prodibis.’ item, quia fama erat multos ex Macedonicis legionibus incertis com meatibus per ambitionem imperatorum ab exercitu abesse, edixerunt de militibus P. Aelio [C. Popilio] consulibus postea eos consules in Macedoniam scriptis, ut, qui eorum in Italia essent, intra dies triginta, censi prius apud sese, in provinciam redirent; qui in patris aut aui potestate essent, eorum nomina ad se ederentur. Missorum quoque causas sese cognituros esse; et quorum ante emerita stipendia gratiosa missio sibi visa esset, eos milites fieri iussuros. Hoc edicto litterisque censorum per fora et conciliabula dimissis tanta multitudo iuniorum Romam conuenit, ut grauis urbi turba insolita esset.*

their commanders were looking for popularity. Concerning these people another edict was issued, which enacted that all the soldiers who had been recruited since 172 BC were to return to their province within thirty days, having first appeared before the censors; it also enacted that the names of those still *in potestate* of their fathers or their grandfathers had to be reported to the censors. Livy finishes by saying that the edict and the letters of the censors were sent *per fora et conciliabula* and that as a consequence of this a big crowd of *iuniores* assembled at Rome. From the whole passage I should deduce: a) that normally those who were *sui iuris* presented themselves before the censors, that is in Rome; b) that those who were *sui iuris* registered their sons and grandsons *in potestate*; c) that in this specific case soldiers on leave had to return to the province within thirty days, having previously presented themselves to the censors; d) that in the case of the soldiers among them who were not *sui iuris* it was enough if their names were reported by their fathers or grandfathers. Put differently, the censors urged all *iuniores sui iuris* who were on leave from Macedonia and the fathers and grandfathers of those who were not *sui iuris* to come to Rome within thirty days. All other *iuniores* had more time to swear, but apparently always before the censors or their *iuratores*, that is in Rome. The only detail that could suggest a decentralized procedure is the link that Livy establishes between the diffusion of the edict and the letters *per fora et conciliabula* and the crowd in Rome, as if this proves that no *iuniores* could have come from *municipia*, *coloniae*, and *praefecturae*. One obvious counterargument against the inference that a decentralized procedure was used in 169/8 BC is that in Livy's narrative Rome is the only place where *professiones* are given and oaths are taken. The reason why Livy mentions only *fora* and *conciliabula* could simply be that in non-urbanised areas, some of which were far from Rome, it would have been both more difficult and more worthwhile to give publicity to the censors' enactment.

In any case the main conclusion I should draw from this episode is that the very high figure for the census of 169/8 BC must be explained, following Brunt, as a result of the greater efficiency with which this particular census was carried out, without necessarily supposing that this greater efficiency was the result of the introduction of a decentralized census procedure.

The last major leap in the census figures for the second century BC is that between the figure for 131/0 BC and that for 125/4 BC. I think that nobody will follow Beloch and Fraccaro in correcting this figure (and also that for 114 BC) or Carcopino in seeing in the leap the effect

of an increased number of manumissions or grants of citizenship.<sup>47</sup> I also think that most people will agree that the leap must be largely or entirely due to a decrease in the number of *incensi*. The problem is to understand why there were fewer *incensi*. In my view the vast majority of the newly registered citizens must be sought among the *proletarii*. I think that the link with the *lex agraria* is obvious: more *proletarii* wanted to be registered in order to be entitled to the Gracchan allotments (unless one is ready to accept Richardson's suggestion that the *Italici* were included among the beneficiaries of the law, and that those *Italici* who received land were also granted Roman citizenship).<sup>48</sup> Like Brunt I regard the objection that the effect of the *lex agraria* should have been felt already in the census of 130/1 BC as doubtful. As Brunt pointed out, the Gracchan commissioners must have spent a lot of time carrying out the preparatory work preceding the actual settlement of the beneficiaries of the law.<sup>49</sup>

In conclusion I should say that the idea of a serious manpower shortage during the central decades of the second century BC is not only unproven, but wildly implausible and counter-intuitive in the face of the economic growth experienced in the Italian peninsula as a consequence of imperial conquest. Contrary to the traditional view, the free population must have been large enough to create serious problems of population pressure, aggravated by an influx of slaves, which must have been bigger than during any other period of Roman history. This problem was alleviated by migration to the urban centres of Italy, chiefly to Rome, and also by migration towards provincial territories, chiefly towards Cisalpina. There are also good grounds for thinking that many of the variations that can be observed in the census figures for the second century BC are to be explained as the result of a variety of factors affecting the procedure and the aims of census-taking rather than as a direct reflection of natural population increase or decrease. For this reason these figures must be regarded as having limited value as evidence for Italy's demographic history.

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<sup>47</sup> Beloch (1886, 351); Fraccaro (1947); Carcopino (1929).

<sup>48</sup> Richardson (1980).

<sup>49</sup> Brunt (1971, 77 ff.).

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## CENSUS AND TRIBUTUM

Simon Northwood

In some recent publications on the republican census it is claimed that the property valuations recorded by the censors were notional or arbitrary and were not genuine market values.<sup>1</sup> This claim seems to me to be over-confident and in conflict with the evidence as I see it, and in this paper I hope to put forward significant counter-arguments. I also wish to draw attention to a forgotten problem, viz. whether all Roman citizens were eligible to pay *tributum*. At first sight both of these issues may seem abstruse, but, aside from the fact that we should understand Roman institutions and procedures for their own sake, whether the census was a genuine attempt to assess real wealth affects our appreciation of supposed changes in the census requirement for military service, and whether all citizens paid *tributum* may affect our view on which parts of Roman society were expected to be included in the census, and therefore on the demographic issues to which the census figures pertain.

### I. *Census*

I begin with a brief outline of what the census procedure actually was. For this we rely on scattered and partial references in historians and antiquarians, in various works of Cicero, and in surviving laws.<sup>2</sup> We must on a number of occasions rely on later practice being a continuation from our period, or earlier reports being a retrojection of genuine later procedures. There is a danger of course of presenting the census as an institution with no history, but at least in the initial stages of investigation this seems unavoidable.

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<sup>1</sup> E.g. Rathbone (1993, 145), with particular reference to valuations of small properties. Cf. also below, n. 18.

<sup>2</sup> Much but not all of the evidence is referred to in Brunt (1971, 15–16). The so-called Altar of Domitius Ahenobarbus, which has a representation of the taking of the census, is discussed by Nicolet (1980, 86–8) and Coarelli (1968), but it provides little material relevant to the present discussion.

It seems that every five years, and under the direction of the censors, who held office for 18 months in each five-year period, all male citizens who were *sui iuris* were obliged on oath<sup>3</sup> to declare themselves (including age, full name, tribe, and filiation),<sup>4</sup> their family (probably including ages),<sup>5</sup> and their possessions before the *iuratores* of the censors.<sup>6</sup> Absentees might be represented by officers of the tribes.<sup>7</sup>

<sup>3</sup> Gel. 17.21.44; 4.3.2: the oath of Sp. Carvilius Ruga that he married for the purpose of having children (235 BC or 231 BC: first divorce); cf. Suet. *Jul.* 52.3: *uti uxores liberorum quaerendorum causa quas et quot uellet ducere liceret*; Gel. 4.20.3 (cf. Cic. *de Orat.* 2.260: oath declaring wife); Tab. Her. 148: *ab ieiis iurateis accipito; iuratores*: Livy 39.44.2; Pl. *Poen.* 55–8; *Trin.* 872; Livy 43.14.5–6 (oath to appear at the levy added to normal oath); DH 4.15.6: ὁμόσαντας τὸν νόμιμον ὄρκον, ἢ μὴν τάληθῆ καὶ ἀπὸ παντὸς τοῦ βελτιστοῦ τετιμηῆσθαι.

<sup>4</sup> V. Max. 9.7.1–2: L. Equitius tried to be returned as the son of Ti. Gracchus (102/1BC). Tab. Her. 146–7: *eorumque nomina praenomina patres aut patronos tribus cognomina et quot annos quisque eorum habebit*; DH 4.15.6: name of father, and own age and tribe.

<sup>5</sup> Cic. *Leg.* 3.7: *ensoris populi aevitates, suboles, familias, pecuniasque censento*. For wives see also above n. 3 and DH below; Brunt (1971, 15) speculated that only wives *in manu* or *sui iuris* were declared by their husband, those *in potestate* appearing in their father's declaration; but the oath at least would not have distinguished between these different statuses. Elsewhere only DH explicitly mentions children: 4.15.6 (wives and children, no mention of ages); 5.75.3 (wives and children and ages of children); 9.25.2 (women, children, slaves, foreign traders, and artisans: no mention of ages). The ages of underage sons would need to be known in order for universal recruitment at 17, otherwise some might not have appeared on the levy list till age 21. Maybe DH was basing his description on Augustan practice (thus Pieri 1967, 15). De Ligt (2007, 179 n. 44) points out that the ages of women reported in Plin. *Nat.* 7.158 (ages 97–115) presumably derived from the census. The system which DH oddly presents as instituted by Servius immediately before the census allowed an annual calculation of numbers of men, women, and children by means of coins contributed at the Paganalia (DH 4.15.4), and also a calculation of the total number of men and the number of those entering manhood each year, using coins paid at three temples for birth, death, and reaching manhood (4.15.5: ἐξ ὧν ἡμελλε διαγνώσεσθαι καθ' ἕκαστον ἐνιαυτὸν ὅσοι τε οἱ σύμπαντες ἦσαν καὶ τίνες ἐξ αὐτῶν τὴν στρατεύσιμον ἡλικίαν εἶχον). It would be strange if DH's sources, including the later second century BC annalist Piso (fr.14P), had imagined an early system which was better at producing useful information than the historical census which they knew themselves. The implication is that the census fulfilled at least these basic functions, which would have required knowing the age of male children.

<sup>6</sup> Cic. *Leg.* 3.7 (above, n. 5).

<sup>7</sup> Var. *L.* 6.86 *de censoriis tabulis: curatores omnium tribuum, si quis pro se siue pro altero rationem dari volet*; Gel. 5.19.16 reports P. Scipio Aemilianus complaining *absentis censori iubere, ut ad censum nemini necessus sit uenire* (*ORF* 21.14); cf. Mommsen (1871–88, II, 367 n. 1). The circumstances in which one could be absent are unclear. Certainly military service abroad would count (unspecified agents were sent overseas in 204 BC) if absent for the whole period of assessment: Kubitschek (1899, 1914–15). Vell. 2.7.7: *matores...ciuis Romanos ad censendum ex prouinciis in Italiam reuocauerant*. Maybe the same excuses were allowed as for the levy. We know specifically of sickness, ill health, state business: Lex. Rep. 14.17.23, Cic. *Arch.* 11 (military service on the staff of a magistrate); Kubitschek (1899, 1914) points to Livy 43.14 as evidence for liberal granting

That only citizens who were *sui iuris* made declarations before the censor has a certain logic: those in *patria potestas* could not own property and therefore had nothing to declare. It remains the case, however, that the explicit evidence for this is less than we should like. The term *duicensus* may refer to someone whose declaration included an adult son; Livy describes those on leave from Macedonia in 169 BC *qui in patris aut aui potestate essent* as not being required to appear before the censors (43.14.8); and on one occasion Dionysius seems to suggest the assessment only of those *sui iuris* (9.36.3). But it is noticeable that the Tabula Heracleensis makes no such distinction, speaking of *quei ciues Romanei erunt*.<sup>8</sup>

Further lack of clarity arises in some quarters with regard to the place of *proletarii* in the census. Some have claimed that *proletarii* were not expected to make a declaration.<sup>9</sup> This is in fact an important issue, since non-declaration by *proletarii* would mean that the census totals we find in our sources might significantly underestimate the size of the male citizen population. But there is in fact no direct evidence for non-registration by *proletarii*, and such a view faces a serious logical

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of excuses, but this text refers to the discharge of soldiers by generals, not permission from censors to be absent from the census. Cic. *Att.* 1.18.8 (60 BC) refers to *negotiatores* being assessed *sub lustrum*.

<sup>8</sup> For the assessment only of those *sui iuris* see also Saskia Hin's contribution to this volume. Fest. 58L: *duicensus dicebatur cum altero, id est cum filio census*. DH 9.36.3: ἦσαν οἱ τιμησάμενοι πολῖται σφᾶς τε αὐτοὺς καὶ χρήματα καὶ τοὺς ἐν ἡβῆ παῖδας ὀλιγὸν πλείους τρισχιλίων τε καὶ δέκα μυριάδων (cf. 6.96.4: no mention of οἱ ἐν ἡβῆ); on other occasions the figure seems to be for all adult males (οἱ ἐν ἡβῆ: 5.20, 75.3; 9.25.2; 11.63.2: ὁ τ' ἀριθμὸς τῶν ἐχόντων τὴν στρατεύσιμον ἡλικίαν ἐγινώσκετο καὶ τῶν χρημάτων τὸ πλῆθος 11.63.2). All *equites* of course had to appear whether *sui iuris* or not, in order to be inspected. Livy 39.3.5 and 41.9.9 (cf. 42.10.3) on the well-known expulsion of Latins from Rome does not in fact help us with this issue (*contra* Sage's note on 39.3.5 (Loeb) and Kubitschek (1899, 1914), whose citation of Gel. 5.19.16 is irrelevant). Tab. Her. 145–6: *omnium municipium colonorum suorum queique eius praefecturae erunt, quei ciues Romanei erunt, censum agito*. That only those *sui iuris* made declarations does not of course mean that only they owed service or had voting rights, since the son took the same status as his father: *filius familias in publicis census loco patris familias habetur ueluti ut magistratum gerat ut tutor detur* (Dig. 1.6.9).

<sup>9</sup> Nicolet (1980, 73) claims that *proletarii* were not expected to give a property declaration because they were exempt from taxation and military service. Schwahn (1939, 56) had taken the opposite view. For what it is worth, Gel. 16.10.10 implies *proletarii* did declare, and note also Cicero's *populi* (*Leg.* 3.7). Whether or not the property of a *capite census* was recorded in the census (the term would imply not) is a different question. Suolahti (1963, 35) argues that information about *proletarii* (and strangers owning property in Rome) was gathered on another occasion: a rather unlikely proposition; elsewhere (44) he (rather problematically, given his previous statement) thinks the taxation list or one based on it constituted the voting list of the *comitia tributa*.

obstacle: the confirmation of one's status as a *proletarius* surely required an assessment before the censor, in order to confirm that one had not become an *assiduus* since the last census. Moreover the existence of the term *capite censi* (probably synonymous with *proletarii*), if taken literally, undermines the idea that there was a section of the population excluded from the census because of insufficient wealth.

For present purposes, however, we can put to one side the *sui iuris* and *proletarii* problems and concentrate instead on the nature of the property declaration. The citizen seems to have declared all land,<sup>10</sup> buildings,<sup>11</sup> cash (including money on loan),<sup>12</sup> moveable goods (including of course slaves),<sup>13</sup> and to have attached a value to these goods, which were itemised separately.<sup>14</sup> In 140 BC P. Scipio Aemilianus could therefore challenge his opponent Ti. Claudius Asellus:

*si tu in uno scorto maiorem pecuniam absumpsisti quam quanti omne instrumentum fundi Sabini in censum dedicavisti...* (Gel. 6.11.9; ORF 128)

if you have squandered on one harlot more cash than you declared at the census as the value of all the equipment on your Sabine farm...

So a citizen did not simply declare an amount in cash without itemising his property; nor did he declare property without giving it a value, for we know that censors were able to challenge the valuations made

<sup>10</sup> Fest. 50L: *ager Romanus priuatus: censui censendo agri proprie appellantur, qui et emi et uenire iure ciuili possunt*; cf. Cic. *Flac.* 80: *illud quaero sintne ista praedia censui censendo, habeant ius ciuile, sint necne sint mancipi, subsignari apud aerarium aut apud censorem possint.*

<sup>11</sup> Var. *L.* 5.160: *et omnes in censu uillas [in]dedicamus aedes.* I have not seen this text referred to in any modern work on the census.

<sup>12</sup> Cic. *Flac.* 80; Fest. 322L: *in aestimatione censoria aes infectum rudus appellatur.* Money or other possessions on loan could not be declared: Cic. *Flac.* 80. Livy 6.27.1–8 and 31.2 suggest that levels of indebtedness could be discovered by means of the census; it would certainly show the amount of money on loan (and maybe to whom) in the lenders' returns, but not in the borrowers', if we are to believe the other evidence already cited.

<sup>13</sup> E.g. farm equipment (Gel. 6.11.9); clothing, jewellery, transport (Livy 39.44.2); slaves (Cic. *Leg.* 3.7; Livy 39.44.3; Cic. *Flac.* 80).

<sup>14</sup> For valuation by the declarant see DH 4.15.6 (cf. 5.75.3): ἐκελευσεν ἅπαντας Ῥωμαίους ἀπογράφεσθαι τε καὶ τιμᾶσθαι τὰς οὐσίας πρὸς ἀργύριον; Fest. 51L: *censores dicti, quod rem suam quisque tanti aestimare solitus sit, quantum illi censuerint*; Tab. Her. 147: *rationem pecuniae*. Pl. *Trin.* 872: *census cum sum, iuratori recte rationem dedi*; cf. Dig. 50.15.4: *omnia ipse qui defert aestimet. censere and aestimare are not synonyms: censores proprio censu non aestimant bona sed censent quanti aestimanda sint; secundum eorum arbitrium quisque cuius rem suam aestimat.* Itemisation is implied in Gel. 6.11.9 and Livy 39.44.2 (note Cato's ability to discern whether a slave had been bought since the previous census).

by the citizen, and even to put their own values on particular types of property.

Livy's report of Cato the elder's censorship in 184 BC is significant for understanding valuations in the census:

*in censibus quoque accipiendis tristis et aspera in omnes ordines censura fuit. (2) ornamenta et uestem muliebrem et uehicula, quae pluris quam quindecim milium aeris essent, <decies tanto pluris quam quanti essent> in censum referre iuratores iussi; (3) item mancipia minora annis uiginti quae post proximum lustrum decem milibus aeris aut pluris eo uenissent, uti ea quoque decies tanto pluris quam quanti essent aestimarentur, et his rebus omnibus terni in milia aeris attribuerentur.* (Livy 39.44.1–3)<sup>15</sup>

Also in accepting assessments his censorship was stern and harsh towards all ranks. Jewels and women's dresses and vehicles which were worth more than 15,000 asses he directed the assessors to list at <ten times more than their actual value>; likewise slaves less than 20 years old, who had been bought since the previous *lustrum* for 10,000 asses or more, he directed to be assessed at ten times more than their actual cost, and he ordered that on all these articles a tax of three asses per thousand should be imposed. (trans. Loeb)

The exceptional revaluation of property for the purposes of a punitive surcharge reveals that valuations were normally at a market or near market rate. The declared valuations of the luxuries and vehicles were their market values, as was the initial valuation of the slaves at their cost price. This market valuation of property accords with the fact that the censor could challenge the citizen's own valuation and could even confiscate for under-assessment: there would have been no room for variation in valuation between owner and censor if conventional valuations prevailed, only disagreement over the amount of property owned. There would in fact have been no need for a citizen to present an *aestimatio* at all. Moreover, declarations included cash, which could hardly have been valued at anything other than its currency value, a situation which would have been anomalous if other items were valued at notional rates.

The aim therefore was to give a market valuation of a person's property. Technically of course even so-called market rates are notional, since the only true market price is the amount someone is prepared to pay at the moment of assessment. A better approximation would be the price for which something was actually sold, the more recent the

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<sup>15</sup> Cf. Plu. *Publ.* 18.2, where Cato imposes the special charges on luxuries above 1500 drachmas.

better, and this sort of valuation actually appears in Livy's description of Cato's census (slaves bought since the previous *lustrum* for 10,000 asses or more). With the caveat that all market rates in global valuations are in a sense notional, I think we can still make a distinction between a valuation which attempts to approximate a market value and one which is entirely notional. So we know in this context what we mean by a market rate. The difficulties created by such assessments must have necessitated a set of rough market values to be used as a rule of thumb in order to check declarations, and from which divergences must have needed justification, e.g. in the case of land, in regard to its quality, location, and use.<sup>16</sup> How often these values were adjusted will have depended on changes in the economy itself, but there was every incentive, given the importance of property valuations for taxation and military service, for these averages to be revised in line with real prices whenever necessary.<sup>17</sup> Changes in the level of wealth required for military service also seem to presuppose a roughly market valuation: including more former *proletarii* by increasing the notional value of, for example, land might have had the desired effect of increasing the pool of available military recruits, but it would also have distorted the membership of the higher census classes in a way that would surely have been unacceptable to the elite.<sup>18</sup>

The nature of the property declaration is not the only elusive element of the census. The *formula census* or *lex censui censendo* is something about which we know less than we should like.<sup>19</sup> It seems to have defined the form of census imposed on the disloyal Latins in 204 BC (Livy 29.15.9) and the municipal census (Tab. Her. 142–156), to have contained provisions for the punishment of those failing to declare (Gaius *Inst.* 1.160), to have prescribed the oath taken by the declarant (Livy 43.14.5),<sup>20</sup>

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<sup>16</sup> Dig. 50.15.4 reports that a declaration involved stating whether land was arable, pasture, timber, or hay-producing (and how much had been sown in the last ten years), or planted with vines or olives (and with how many vines and olive trees). We cannot know if this level of detail was carried over from the republican census or determined instead by the need to charge *tributum soli* and *capitis*.

<sup>17</sup> Unless of course there was deflation.

<sup>18</sup> But note Crawford (1985, 24): "I suspect that at this level the designation of a figure in asses and the assessment of property were largely arbitrary processes and sometimes wonder how much the property qualification for serving in the legions ever really meant." In other words, he believes that at the lower end of the scale property was not assessed at market rates.

<sup>19</sup> To be distinguished from the *censoriae tabulae*. Var. *L.* 6.86.

<sup>20</sup> Maybe the censors' oath too: Zonar. 7.19.

and to have in some way defined who and what should be declared. That the formula could be added to seems clear from the addition in 169 BC of a further (temporary) oath with regard to attendance at the levy and an instruction for soldiers on leave (and *sui iuris*) to attend the census (Livy 43.14.5–8).<sup>21</sup>

Some see the *formula* or censors' edict as a document which also gave refined instructions for the implementation of already legally defined requirements of the census, e.g. (variously) "advice to the tax payers, possibly even a table of taxation rates", "the principles they would apply in watching over the moral behaviour of the citizens", "to more precisely define what should be assessed and how", rules to be followed in the census, instructions on the valuation of different types of property, and the "tarif d'estimation", which might change in the case of luxury items.<sup>22</sup> We might want to argue about what was and was not already legally defined, and how much was *tralatitium*, carried over from one census to another, but it is obvious that if valuations of property were in fact based on notional rates, these would have been included in the *formula*, especially if they changed from census to census. I have argued of course that valuations were at a market rate, so I believe there were no such notional rates in the formula. If there were any rates, they will have been the rough market rates ('rules of thumb') which I have suggested might have been useful for the assessors when receiving declarations. But of course the censors may instead have preferred to keep these values to themselves.

The detailed assessment of the entire male citizen population was no doubt a significant undertaking, presumably accounting for most of the censors' eighteen months in office. I do not wish to discuss the location of the procedure, except to say that clearly at some point it became possible for *municipia* to perform their own census, and that I see no procedural difficulty in assessments being done in the localities, particularly since there had always been the possibility for tribal officials to represent citizens absent from Rome: large-scale declarations in areas distant from Rome may have been a logical extension of this dispensation, originally intended only for a minority. With regard to

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<sup>21</sup> Kubitschek (1899, 1916) and Humbert (1887, 994 n. 113) seem to suggest that additions could be made orally or in writing (they make a perhaps doubtful analogy with other censorial pronouncements in speeches or edicts).

<sup>22</sup> Taxation rates and principles: Suolahti (1963, 38); what and how: Kubitschek (1899, 1916); rules, valuation, 'tarif': Humbert (1887, 994).

the accuracy of the census, one might on the face of it expect a high degree of inaccuracy both in the number of people assessed and in the candour of declarations. But there were factors both encouraging and discouraging citizens from presenting themselves and declaring accurately. The benefits of avoiding or reducing liability to *tributum* (before 167 BC) and of avoiding military service accrued to those who failed to declare or who declared dishonestly. The negative effects were of course either total loss of voting rights (in both *comitia centuriata* and tribal assemblies) or at least diminution of the influence of one's vote in the *comitia centuriata*, and of course the potential penalties.<sup>23</sup> There was also the loss of social prestige that almost certainly accrued from being registered in a higher census class.<sup>24</sup> We have no evidence as to how these influences worked in practice, though it is common to assume that citizens valued their voting rights less than they did freedom from military service and *tributum*. But there are two significant features of the census procedure which may in fact have significantly assisted the completeness and accuracy of the returns. First, declarations were made in public. Declaring in public in front of one's neighbours must have made it much more difficult significantly to misrepresent one's property. Censors' officials will therefore have found it relatively easy, unless faced with sophisticated collusion by entire communities, to produce roughly accurate returns.<sup>25</sup> There will still have been under-registration: the truly

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<sup>23</sup> DH 4.15.6: punishment for non-declaration had once been scourging, forfeiture of property, and enslavement (cf. 5.75.3: loss of property and citizenship): μέχρι πολλοῦ διέμεινε παρὰ Ῥωμαίοις οὗτος ὁ νόμος; Livy 1.44.1 gives imprisonment and execution as the punishment; sale into slavery appears in Gaius *Inst.* 1.160; and Cic. *Caec.* 99 reports provisions in the Twelve Tables to sell into slavery those who refused army service or who were not assessed; according to Zonar. 7.19 the non-assessed were sold into slavery by the consuls and their goods sold by the censors. Gel. 20.1.47 gives death or slavery as the punishment for debt—presumably non assessment was seen as defaulting towards the state; in the Tabula Bantia those not appearing at the census without a valid excuse would be sold at auction with family and goods becoming state property; DC 47.16 reports confiscation for under-assessment in 43 BC; App. *BC* 4.32 and 34 has fines for false valuations, though these concern extraordinary impositions on selected wealthy citizens. When the severest penalties no longer applied, a person discovered to have failed to declare or to have falsely declared was vulnerable to the censors' discretion to impose a surcharge or to demote to the *aerarii*: Kubitschek (1899, 1915). When *tributum* was no longer levied, the penalties might have been fines or additional military service.

<sup>24</sup> The impulse to over-declare is seen in Cic. *Flac.* 79–80.

<sup>25</sup> I assume that the census was often conducted locally: thus Brunt (1971, 536–7), following Pieri (1967), who thinks Var. *L.* 6.86, where all Quirites must attend the *lustrum* in arms, is derived from an archaic source.

landless will always have been able to disappear, but those with any landed property at all will have found it difficult to avoid the census. Second, while we are ill-informed about the practicalities of record keeping, it seems probable that a new census was not begun entirely from scratch; instead each new census used the previous census as its model.<sup>26</sup> This meant that once on the census list it would have been difficult to get off it without collusion from neighbours and family, fleeing Roman territory, or faking one's own death. And suddenly appearing or being discovered for registration would expose one to the charge of non-registration in previous censuses.<sup>27</sup> This use of the previous census as a model will also have reduced the administrative burden of taking the census, which in any case can be overestimated.<sup>28</sup> All this should have aided accuracy and reduced avoidance. Brunt's estimate of 10% under-registration, though challenged by some, may not be too far wide of the mark.<sup>29</sup>

## II. Tributum

The citizen's liability to contribute *tributum* was calculated based on his census declaration. There is of course disagreement over the date of the introduction of *tributum*, which fortunately does not concern us directly here.<sup>30</sup> For our purposes a larger problem is who was required to pay,

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<sup>26</sup> This is implied in Cato's census quoted above: it would allow the censor to know whether a slave had been bought since the last census. The use of the previous census was noted by Mommsen (1871–88, 2.370 f.); cf. Suolahmäki (1963, 33). Another argument would be that if the census list was indeed compiled from scratch, and if serving soldiers were allowed not to attend the census, such soldiers would have disappeared from the census list, a situation which would hardly have been acceptable.

<sup>27</sup> A point made by Jeremia Pelgrom at a Leiden seminar.

<sup>28</sup> It does not seem to me an insurmountable task with the help of a small but skilled staff of public slaves and other assistants to keep tolerably well a rolling record of c. 300,000 adult male citizens.

<sup>29</sup> Brunt (1971, 33–5).

<sup>30</sup> It is logical to think that *stipendium* necessitated *tributum* and that both appeared at the same time, traditionally in 406 BC in the war with Veii. This link between *tributum* and *stipendium* is circumstantially supported by Var. *L.* 5.181: *ab hoc ea quae assignata erat (sc. pecunia) attributum dictum; ab eo quoque, quibus attributa erat pecunia, ut militi reddant, tribuni aerarii dicti; id quod attributum erat, aes militare.* Thus Nicolet (1976, 16–19), who thinks also that the census must have been established at the same time. But while Livy does associate *tributum* with *stipendium* (4.60.7, cf. 36.2: public land contemplated as source of *stipendium*; 5.20.5; 7.27.4; 10.46.6), he does not explicitly state that *tributum* was introduced in 406 BC, and we twice hear of *tributum* earlier in the fifth century BC (2.9.6: 508 BC; 2.23.5: 495 BC; cf. 6.14.3); other early republican *tributa* appear in DH 5.20.1:

since there are indications that not all citizens were liable. Dionysius of Halicarnassus is unequivocal in stating that in the constitution established by Servius Tullius the *proletarii* were exempt from both military service and taxes (4.18.2).<sup>31</sup> Livy mentions payment according to wealth, but he is not explicit about the exclusion of *proletarii* (1.42–3). There may, however, be a trace of Dionysius' version in Livy when in 508 BC the plebs are said to have been relieved of taxes: *portorii et tributo plebes liberata... pauperes satis stipendii pendere si liberos educunt* (2.9.6). Although Livy speaks here of the plebs as a whole, there is a suggestion that in fact only the *proletarii* were exempted: *pauperes* seem synonymous with *proletarii*, given the ubiquitous derivation of *proletarius* from *proles*.<sup>32</sup> When we come to Livy's account of the *tributum* raised for the first *stipendium* (many think this was the first *tributum*), we find that he does not mention exceptions among the plebs. His later narrative at 5.10.5 (401 BC) suggests that those actually serving did not normally pay: having been enrolled to guard the city, the *seniores* were reluctant to contribute, since although at home they were performing the function of soldiers.<sup>33</sup>

A further piece of possible evidence for payment only by *assidui* is the commonly reported etymology (from Aelius Stilo) of *assiduus* 'ab aere dando' / 'ab asse dando' (*as* + *do*).<sup>34</sup> I qualify this as only possible

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508 BC; 5.47.1: 503 BC; 8.68.2–3: 486 BC; Sal. *Hist.* fr.1.11: pre-494 BC). Crawford (1985, 21–3) suggests *tributum* was introduced at the same time as *stipendium* in 406 BC and that expenses not covered by plunder had previously been paid for by a liturgy system. Schwahn (1939, 7): *tributum* was regularly raised only after the introduction of *stipendium*. Harris (1990) accepts the possibility of payment in *aes rude* but prefers to believe in regular monetary payment from c. 280 BC and some irregular payments out of plunder from c. 400 BC onwards.

<sup>31</sup> Cf. DH 4.9.7, 11.2; 7.59.6. DH 4.19.1–3 explains that early taxation was in order to provide provisions and military supplies and not pay; hence in 503 BC repayment is made for τὰς κατ' ἄνδρα γενομένης εἰσφορᾶς αἷς ἔστειλαν τοὺς στρατιώτας (5.47.1).

<sup>32</sup> Cf. Ogilvie (1965 ad loc.): "a specious derivation of *proletarii*". No other derivations survive from antiquity: see the collection in Maltby (1991). Plut. *Popl.* 11.3: ὅ τὰ τέλη τῶν πολιτῶν ἀφεῖλε, but at the same time he mentions the temple of Saturn being made a treasury to hold contributions for war, i.e. *tributum* was not abolished. DH 5.22.2 (507 BC) has the plebs made free from all the taxes imposed under the kings and all contributions for military purposes; he gives military service as the quid pro quo: μέγα κέρδος ἡγούμενοι τοῖς κοινοῖς εἰ τὰ σώματα μόνον αὐτῶν ἔξουσι προκινδυνεύουσα τῆς πατρίδος. This suggests all plebeians and not only *proletarii* were exempt. In contrast we find military contributions in 503 BC (5.47.1) and implied in a speech of Appius Claudius in 495 BC (6.24.2), where the plebs are said to have been freed from the taxes paid to the kings.

<sup>33</sup> *inuitis conferentibus qui domi remanebant, quia tuentibus urbem opera quoque militari laborandum serviendumque rei publicae erat.* Thus Nicolet (1976, 33).

<sup>34</sup> Cic. *Rep.* 2.40; *Top.* 10; Gel. 16.10.15; Char. 95 Barwick; Paul. *Fest.* 9L.

evidence because we might see it as: i) a genuine etymology, maybe of a group which also exclusively owed military service, maybe not; ii) a false etymology but one inspired by knowledge that only those who owed service had paid tax; or iii) opportunistic nonsense.<sup>35</sup>

On the basis of evidence such as this, the connection between *tributum* and *stipendium*, and the evidence in Dionysius of Halicarnassus and Livy, it has been argued that *tributum* was “a tax payed by those who have a duty to serve, for the benefit of those serving”.<sup>36</sup> According to this view, not only *proletarii* but also serving soldiers were exempt from *tributum*.<sup>37</sup>

Other evidence, however, is equivocal. Varro’s *tributum... tributim a singulis pro portione census exigebatur* (L. 5.181), implies no exclusions; the *tributum* for the first *stipendium* is paid by *uolguus hominum* (Livy 4.60.7); and even where we see a trace of exemption for serving soldiers (see above), the tribunes claim that *stipendium* has been introduced *ut plebis partem militia, partem tributo conficerent* (Livy 5.10.6).<sup>38</sup> There are further references to payment of tax by the plebs (Livy 5.20.5–8), and by ‘people of scanty means’ (τεθλιμμένων... τῶν βίων: DH 8.73.5) and ‘each person according to his property’ (τῶν χρημάτων τὸ πλήθος ἀφ’ ὧν ἔδει... ἕκαστον τελεῖν: DH 11.63.2 [443BC]): note the potential internal inconsistency in Dionysius. But of course it may be that Livy and Dionysius were unable or unwilling to incorporate technical

<sup>35</sup> Etymologies of *assiduus* are collected in Maltby (1991); cf. Mommsen (1871–88, III,238): “Auf das genaueste passen alle diese Ausdrücke auf den Gegensatz der Steuerfähigkeit und der Vermögenslosigkeit und die tralatäische Herleitung des *assiduus* oder vielmehr *assiduus ab asse dando* ist sprachlich so verkehrt wie sachlich zutreffend”.

<sup>36</sup> Nicolet (1976, 29): “le *tributum* est un impôt payé par les mobilisables au profit des mobilisés”. Schwahn (1939, 57): *proletarii* not liable. Mommsen (1871–88, 3.237–8 cf. 3.297), followed by Kubitschek (1894, 426), accepted that only *assidui* paid tax, but defined *assidui* as taxpayers, not those eligible for service in the legions. He saw the census boundary between *proletarii* and *assidui* as 1,500 asses, as in Cicero and Gellius, but the census of the fifth class as 4,000 asses, as in Polybius, i.e. he did not accept a post-Polybian reduction in the census rating of the fifth class. For Mommsen therefore there was a section of the population assessed between 4,000 and 1,500 asses which did not owe regular military service but did pay *tributum*.

<sup>37</sup> Nicolet (1976, 33). According to this logic, *seniores* will also have been exempt since they too did not owe regular service, but this is not commented upon by Nicolet. We have no evidence that this was the case (Livy 5.10.5–9 presents *seniores* as paying *tributum*).

<sup>38</sup> *pars... pars* naturally suggests the whole plebs is being discussed (meaning 3b OLD). Nicolet (1980, 166) sees the problem with this type of picture: “this account may contain anachronistic traces of a much later situation; for the idea of a *tributum* weighing thus heavily on the plebeians is contrary to the original logic of the system”.

distinctions into their narratives of social struggle, just as they are almost never explicit even about the fact that a portion of the plebs did not owe regular military service.<sup>39</sup>

An argument can therefore be made that *tributum* was paid only by those in the five Servian classes and not by *proletarii*. There is no evidence which explicitly contradicts this, though there are numerous statements in the ancient sources which would be consistent with the opposite view.<sup>40</sup> A significant worry is our reliance on Dionysius as our starting point, since it is just possible that in this respect he was augmenting his source on the basis of speculation or Greek models which he was keen to see reflected in early Rome.<sup>41</sup> Perhaps one should be influenced in his favour by the etymologies given by Varro, but ultimately there seems little prospect of finding a solution to this problem, which must have been obscure even to the Roman annalists, most of whom wrote after the cessation of *tributum* in 167 BC, and given the fact that practice may have changed over time.

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<sup>39</sup> Exceptions: DH 5.67.5 (498 BC)—in a speech Ap. Claudius Sabinus remarks that there is no need to relieve the debts of the poor on account of threats to withdraw military service, since there are few left who genuinely have no property, and even those in the fifth class serve only in the last line as slingers (cf. DH 4.17.2); DH 13.12.2—in the levy against the Gauls the Romans include those sedentary citizens who have little experience of war (that these are *proletarii* is supported by the comment that they were more numerous than the regular soldiers, which fits Dionysius' earlier observation on the numbers in the final century of the Servian system).

<sup>40</sup> The category of the *aerarii* is interesting. One might be tempted to argue that if *proletarii* had paid tax then citizens could have been punished just as much by demotion to the *proletarii*, and there would have been no need for a separate category of *aerarii*; but of course demotion to the *proletarii* would not have deprived a citizen of all voting rights nor have exposed him to punitive rates of taxation as did demotion to the *aerarii* (Livy 4.24.7).

<sup>41</sup> This can be seen in Dionysius' description of how taxation was raised: the amount required was decided upon and divided equally among the 193 centuries. This will have placed a heavier burden on those in the higher classes, since their centuries had fewer members. Within the century, contributions will have been proportional to assessed wealth. This is not the system suggested by evidence for later times, but it is accepted by Nicolet (1976, 39–45) as a genuine reflection of an early system which was similar to the mid-fourth-century BC Athenian system of *symmoría*. Dionysius' programmatic purpose, viz. to present the early Romans as Greeks, must lead us in the opposite direction: "if he was not describing the system of the middle and late republic, he is much more likely to be inventing a system for early Rome on the basis of his knowledge of the Athenian system than to be possessed of any real knowledge of the institutions of the early republic": Crawford (1978, 189). Nicolet (1976a, *passim*) prefers to think that Livy, Dionysius, and Cicero shared the same ultimate source, which Dionysius represents more fully.

But let us suppose for the sake of argument that Dionysius was right and *proletarii* did not pay *tributum*: what effect would this have had on the inclusion of *proletarii* in the census? According to some it would have had serious consequences, since there would have been no reason for the censors to register citizens who owed no financial or military contributions. But this is only partially true: in crises all citizens, regardless of wealth, owed military service, and it seems unlikely that the state would have been comfortable not knowing the size of its pool of emergency manpower. We cannot of course know the attitude of *proletarii* themselves, but we can surmise that if they were illegible for *tributum* they would have had no reason not to declare themselves, except in order to avoid emergency recruitment, which may not have been uppermost in their minds. If under normal circumstances there were no pressing reasons for *proletarii* not to appear before the censors, and a military incentive for the censors to require them to do so, we should seek further arguments before assuming that the census regularly missed such a significant section of the population. Maybe the issue of who paid *tributum*, at least from a demographic point of view, is not so important after all.

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III

SURVEY ARCHAEOLOGY AND DEMOGRAPHY



# REGIONAL FIELD SURVEY AND THE DEMOGRAPHY OF ROMAN ITALY

Robert Witcher<sup>1</sup>

*Everyone knows that the models which inform most of the demography of the past decade are illusory, images of population which have never in fact existed, but are good to think with (Golden 2000: 32).*

## I. Introduction

Regional field survey has identified tens of thousands of settlement sites around the Mediterranean;<sup>2</sup> the potential significance of this evidence for demographic reconstruction has long been discussed.<sup>3</sup> This article considers some of the key debates concerning the demography of Roman Italy from the perspective of archaeological field survey. First, it addresses the question of whether or not the results of survey archaeology reveal the supposed decline of the peasantry during the second century BC; specifically, it examines the republican settlement evidence of the South Etruria survey. Second, it considers a demographic model of the early imperial *suburbium*<sup>4</sup> and evaluates its working assumptions; in particular, it considers the significance of this suburban population for the wider debate about the size of the early imperial population of Italy as a whole—the ‘high’ vs. ‘low’ counts. Finally, two models are developed to explore the implications of these high and low population figures for our assessment of the significance of the archaeological evidence; they imply two very different reconstructions

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<sup>2</sup> Barker and Lloyd (1991).

<sup>3</sup> Bintliff and Sbonias (1999).

<sup>4</sup> Witcher (2005).

of the socio-economic organization of Roman Italy. However, both models suggest that regionality is a critical consideration.

## II. *Archaeological survey and historical texts*

The South Etruria survey was one of the earliest systematic archaeological field surveys in the Mediterranean; instigated by John Ward-Perkins in the mid 1950s, it spanned twenty-five years.<sup>5</sup> The potential relevance of these survey results for the republican demography of Italy was rapidly identified; already by the late 1960s attention was directed specifically to the contribution the survey could make to discussion of the demographic changes implicit in the ‘Gracchan crisis’. Here the archaeology offered a (seemingly) independent check on the traditional text-based narrative largely built upon the works of Appian, Polybius, and Cato the Elder. Essentially, this narrative focused on the spread of capitalistic agricultural estates using slave labour, combined with the continuous wars of the republic, leading to a decline of the peasant class.<sup>6</sup> Field survey offered the settlement evidence (farms, villas, etc.) with which to assess this model.

One of the earliest discussions was Frederiksen’s (1971–2) *Dialoghi di Archeologia* article, in which he applied the results of the South Etruria survey to the Gracchan problem. In direct contrast to the prevailing text-based model, Frederiksen argued that the archaeological evidence presented clear support for an increase in settlement numbers (and implicitly, population) around Veii, Capena, and Sutri during the second century BC;<sup>7</sup> this was echoed by Nagle, who noted that farms continued to outnumber villas.<sup>8</sup>

However, some scholars were (and some remain) dubious about the potential contribution of survey data to such historical debates. In the same year that Frederiksen’s article was published—1971—Brunt reformulated and forcefully restated the traditional ‘Appian’ model; he

<sup>5</sup> Potter (1979).

<sup>6</sup> Most eloquently outlined by Hopkins (1978).

<sup>7</sup> Frederiksen (1971–2, 344–6).

<sup>8</sup> Nagle (1979, 424–8, 433); see also Nagle (1976, 487). Skydsgaard (1969, 28–34) covers the same ground, but is more cautious about the chronology of the emergent archaeological evidence and its demographic implications; see also Garnsey (1979, 3–4).

too made reference to the South Etruria survey results, but expressed broader concerns over survey evidence and its possible contribution.<sup>9</sup>

Despite Frederiksen's explicit recognition of the problems of survey data, he was in fact rather too optimistic about the ability to date the black gloss (*vernice nera*) pottery. Indeed, one of the key South Etruria survey reports, the Ager Veientanus survey,<sup>10</sup> which was still unpublished at the time he presented his paper, is rather more cautious. Whereas Frederiksen dated most of this material to the second century BC,<sup>11</sup> Ward-Perkins and his colleagues refrained from committing to any date more precise than the last three centuries BC.<sup>12</sup> Indeed, Potter's synthesis of the South Etruria survey material explicitly rejected any attempt to subdivide the republican period material into chronological subphases;<sup>13</sup> he argued that the Tiber valley demonstrated considerable conservatism in comparison with the coast, rendering existing chronologies inappropriate.<sup>14</sup>

Arguably, the contribution of the South Etruria survey—and regional survey more generally—to the demographic problems of the second century BC was poorly served during this period: some scholars were over-optimistic in their approach, others were too pessimistic. Interestingly, many (but not all) of the former were classicists and many of the latter were archaeologists.

Clearly, one of the key barriers to progress was the dating of the black gloss pottery; it was a preliminary restudy of a sample of this material during the early 1980s which reopened the possibility of identifying second century BC settlement and population decline.<sup>15</sup> The results suggested that 80% of the South Etruria survey black gloss pottery dated to the fourth and third centuries BC, and just 20% to the second and first centuries. However, it was not until the late 1990s that the first full

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<sup>9</sup> E.g. whether sites were in contemporary occupation, recovery rates (see below), the issue of agro-towns, etc. He explicitly discusses the issue of black gloss chronology and the problem of identifying Gracchan colonists. Nonetheless, he notes that the peasantry clearly survived during the late republican period, Brunt (1971, 352–3).

<sup>10</sup> Kahane et al. (1968).

<sup>11</sup> Frederiksen (1971–2, 345).

<sup>12</sup> Kahane et al. (1968, 11).

<sup>13</sup> Potter (1979, 95–6, 109–10).

<sup>14</sup> See also Taylor's comments in Kahane et al. (1968, 56–7).

<sup>15</sup> Liverani (1984).

Table 1. Number of Middle Tiber valley sites by period  
(based on Patterson et al. 2004, figure 3)

<b>Chronological period</b>	<b>Dates</b>	<b>Number of sites</b>
Mid Republican	350–250 BC	513
Late Republican 1	250–150 BC	199
Late Republican 2	150–30/1 BC	561
Early Imperial	30 BC–AD 100	1486

study of all the South Etruria survey material (not just the black gloss) was undertaken, involving *c.* 90,000 sherds of pottery.<sup>16</sup>

The results of this recent work support Liverani's pilot study, with most black gloss dating to the fourth and third centuries BC.<sup>17</sup> Using this material, the Tiber valley project has created a new chronological framework and redated each settlement site (Table 1).<sup>18</sup> The results suggest a significant dip in settlement numbers during the period *c.* 250–150 BC (Late Republican 1). It is, of course, important to note that even this new chronological framework is still rather coarse when compared with the resolution offered by the historical sources and it does not offer convenient pre- and post-Gracchan phases with which to gauge the context and impact of the Gracchan reforms. Nonetheless, it does present the possibility of identifying trends *within* the Republican period; unsurprisingly, these results have already attracted some attention in relation to the demography of late republican Italy.

A preliminary report on the findings of this restudy of the South Etruria survey material identified a series of 'crises', including an apparent mid-third to mid-second century BC decline.<sup>19</sup> A number of problems with the interpretation of these data were identified, and overall the authors were cautious about the ability to identify specific historical events and processes using such archaeological material. Similarly, in a more recent paper Di Giuseppe argues that the changing quantities of black gloss should be considered primarily in terms of production and consumption, and only secondarily in terms of historical processes of

<sup>16</sup> Tiber valley project: project outline, Patterson & Millett (1998); preliminary results, Patterson et al. (2004).

<sup>17</sup> For details of the black gloss pottery see Di Giuseppe (2005).

<sup>18</sup> See Patterson et al. (2004) for fuller discussion, especially table 1. In the following text specific periods as defined by the Tiber valley project are capitalized (hence 'Late Republican 1').

<sup>19</sup> Patterson et al. (2004).

settlement and demography; Roth makes a similar point about black gloss in Italy in general.<sup>20</sup> Nonetheless, with the growing intensity of the high/low population debate, speculation about the relevance of these findings to the traditional narrative is inevitable.<sup>21</sup>

Given the perceived centrality of the South Etruria survey data to the Gracchan problem, these new results offer a valuable opportunity to revisit the issue; more generally, they provide an excellent case study with which to consider the broader issue of the integration of textual and archaeological data.

The starting point must be an explicit recognition that the archaeological and textual records are created through fundamentally different processes; therefore they have their own particular strengths and weaknesses, and these differences should be respected.<sup>22</sup> Indeed, common sense should alert us to the problems of using archaeological data to identify or ‘materialize’<sup>23</sup> a *c.* 10% decline in the number of male Roman citizens over a period of a few decades given the chronological insensitivity of the ceramics, the problematic relationship between pottery, site definition and site population, the geographical dispersal of this population, and the fact that it formed just one (legally defined) group within a larger population. And all that before the motives of both historical individuals and writers have been considered. Here, however, I put aside the question of whether or not it is sensible or desirable to expect textual and archaeological records to correlate, and focus on the limited quantity of republican material culture (and specifically of mid-third- to mid-second-century BC black gloss) with which the traditional narrative can be explored.

In the understandable attempt to respond to historical questions, archaeologists continually refine chronological frameworks into ever shorter periods. However, as these periods become shorter, more and more material culture is rendered chronologically ‘generic’ (i.e. a sherd of 250–1 BC becomes ‘generic’ and is excluded from analysis of the sub-periods 250–150 BC and 150–1 BC). Hence of 6985 sherds of black gloss only *c.* 1900 sherds (*c.* 27%) can be dated to a specific

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<sup>20</sup> Di Giuseppe (2005, 49); Roth (2007, 77–94) with specific reference to second-century-BC demography at 190–6; Roth (2006) for discussion of black gloss from Capena in south-eastern Etruria, particularly at 138; for the same point, but on a broader scale, see Witcher (2006a, 49–52).

<sup>21</sup> Di Giuseppe (2005, 52).

<sup>22</sup> On the Gracchan issue specifically see Rathbone (1993).

<sup>23</sup> Porter (2003).

Table 2. Chronological distribution of black gloss pottery (based on Di Giuseppe 2005: appendix 2). NB the low Late Republican 2 total is supplemented with significant quantities of other datable artefacts, e.g. coarsewares, plain wares, and early terra sigillata italica forms; such additional dating material is very limited for the Late Republican 1 period

<b>Republican subperiod</b>	<b>Number of closely-datable black gloss sherds</b>	<b>% of total black gloss (6985 sherds)</b>	<b>% of closely-datable black gloss (1900 sherds)</b>
Mid Republican	1251	18%	66%
Late Republican 1	261	4%	14%
Late Republican 2	388	6%	20%

subperiod.<sup>24</sup> In other words, any chronological trends identifiable *within* the republican period are necessarily based on only a fraction of the total assemblage. However, as the vast majority of these closely-dated sherds are rims and bases, and the vast majority of the generically-dated sherds are body fragments, it is a reasonable assumption that the chronological distribution of the body sherds should broadly correlate with the datable rims and bases. It is therefore unlikely that all generically-dated black gloss relates to a single sub-phase—the Late Republican 1, for example. In other words, the inability to date the majority of black gloss sherds should not have significantly distorted the ratios of material culture over time.

The chronological distribution of the diagnostic black gloss sherds is heavily skewed towards the Mid Republican period (Table 2). Just 4% of sherds are datable specifically to the Late Republican 1 period. Such rarity is a problem for any sampling strategy.<sup>25</sup> The key question is: was there really no Late Republican 1 black gloss on a particular site, or is this material so rare that it is impossible to be certain that it was reliably recognized during survey? In terms of probability, it would be necessary to collect at least 25 sherds of black gloss to be confident that, if Late Republican 1 black gloss *were* present, a single representative sherd would be recognized.

<sup>24</sup> For comprehensive details of the black gloss pottery see Di Giuseppe (2005).

<sup>25</sup> For an ecological parallel see McArdle (1990).

The South Etruria survey was rather unsystematic by modern standards.<sup>26</sup> A representative ‘grab’ sample of sherds was collected from each site in order to establish occupation within a few broad chronological periods—Etruscan, republican, early imperial, etc. The primary concern was the achievement of a broad date for each site’s occupation, not the collection of a large and representative sample of material culture. As a result, sample sizes from individual sites are small; the average number of black gloss sherds collected at each site where it occurs is just six (median = 3; mode = 1). In other words, the sample sizes are far below the level necessary to be confident that any Late Republican 1 material present on a site was included in the sample. (In contrast, the average of six sherds corresponds to the minimum number necessary to recognize with confidence the presence of any Mid Republican material.)

The scarcity of Late Republican 1 material—combined with the South Etruria survey’s sampling method—makes it impossible to state emphatically that there was a reduction in the number of sites (and indirectly, population) during this period. A number of other observations support these conclusions. For example, over one-third of the sites abandoned in the Late Republican 1 period were reoccupied in the subsequent Late Republican 2 period. This could be taken as evidence for the abandonment and reoccupation of a site due to changing population levels, but it is also exactly the pattern to be expected when one period has notably less (diagnostic) material culture than prior and subsequent periods. In similar circumstances other surveys have assumed continuous occupation.<sup>27</sup>

Further, there is no clear spatial patterning in the distribution of continuing, abandoned, and reoccupied sites; for example, the abandonment of marginal areas or land around villas might be expected as a result of depopulation or estate agglomeration. Rather, continuing and abandoned sites are thoroughly interspersed with continuing sites across the whole area. This is exactly the pattern to be expected when mapping a random stochastic process based on low figures.

Hence the apparent decline in mid-third to mid-second-century BC settlement numbers is highly problematic. The rarity of material—and the particular methods used to sample it—mean that the definition of

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<sup>26</sup> Potter (1979, 10–14); Witcher & Craven (forthcoming).

<sup>27</sup> E.g. the Rieti survey: Coccia & Mattingly (1995, 114).

brief sub-periods will inevitably lead to erratic variation in the quantities of pots and therefore sites. Even simple adjustments to the start and end dates of the republican sub-periods can dramatically change the quantities of material included or excluded, and thus transform settlement histories and demographic trends. None of this is to argue that the chronological framework as defined is wrong—indeed it reveals interesting new patterns. But the decision to use this tight periodization comes with implications which should not be ignored. In this case, clearly there were significant changes in the quantities and forms of black gloss in circulation (e.g. a shift in the organization of production and consumption).<sup>28</sup> However, there are insufficient data to argue confidently for a decline in the number of sites (and population) during the Late Republican 1 period. It is important to stress that this critique does not mean that there was *no* settlement or population decline, simply that it is impossible to use the current data to support or falsify this scenario; the data could equally suggest material impoverishment (fewer pots) or population nucleation (fewer but larger sites). The improvement of dating resolution in pursuit of historical insights therefore comes with certain costs. There is a risk of creating patterns on the basis of ever less evidence and then interpreting these in terms of stock historical debates (e.g. demographic change) when they may well relate to a number of other possibilities, such as changing consumption.<sup>29</sup>

In summary, the South Etruria data cannot contribute directly to the specific issue of second-century BC settlement (and population) decline. More generally, caution should be exercised when applying archaeological data to any text-based narrative; improving chronological resolution is not the panacea it often seems. However, archaeology can feed into the historical issue under consideration. Even if it is impossible to identify short-term population shifts, the much broader, long-term population increase postulated by the high counters may well be more amenable to archaeological investigation. In other words, though it is impossible to falsify specific text-based models such as the second-century BC population decline using survey data, it should be possible to evaluate the validity of the alternative high-count model. The next section outlines some of the key methods used and problems raised by demographic models based on survey data.

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<sup>28</sup> Di Giuseppe (2005, 52–3); Roth (2007, 65–102).

<sup>29</sup> Witcher (2006a).

### III. *Archaeological survey and population figures*

Those who argue for the importance of field survey point to its usefulness for demographic reconstruction.<sup>30</sup> Often, however, this reconstruction has been implicit or rather simplistic. For example, in *The Changing Landscape of South Etruria* Potter entitled his chapter on the first millennium BC, 'The Population Explosion'; however, he goes no further than associating increasing site numbers with increasing population.<sup>31</sup> In contrast, over the past decade surveyors have become more confident about survey data and their ability to address demographic issues in particular.<sup>32</sup> Indeed, it has even been argued that it is *necessary* to go beyond the mapping of sites ('dots on maps') and to (re)populate these landscapes with people in order to appreciate their significance.<sup>33</sup>

The principal method of survey-based population reconstruction quantifies the number of site types per period and multiplies these figures with standard site populations, for example, five individuals (or one household) per 'farm'. The populations of nucleated settlements (villages and cities) are commonly reconstructed by multiplying their urban area (in hectares) by population densities derived from comparative studies (commonly 100–250 persons/ha). Elements of uncertainty or debate can be accommodated within these reconstructions by using minimum and maximum site population ranges (e.g. 5–10 individuals per farm) and/or the inclusion of probable/possible sites (e.g. 10 certain farms and 5 possible farms). Examples of this basic approach include surveys from Laconia,<sup>34</sup> Jerba and the Albegna valley,<sup>35</sup> and the *suburbium* of Rome.<sup>36</sup>

More elaborate versions of this methodology have refined the chronological resolution of site occupation down to individual centuries. For example, Perkins' reconstruction of the republican population of the Albegna valley considers the date range of individual sherds from each site; if half or more of a sherd's date range falls within a specific century,

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<sup>30</sup> See papers in Bintliff & Sbonias (1999).

<sup>31</sup> Potter (1979, c. 4, esp. 59, 72, 89). Most of the interim South Etruria survey reports, published in the Papers of the British School at Rome between 1955 and 1977, do not even mention the issue of population, and none discusses it in any detail.

<sup>32</sup> Osborne (2004).

<sup>33</sup> Sbonias (1999).

<sup>34</sup> Cavanagh et al. (1996).

<sup>35</sup> Fentress (forthcoming).

<sup>36</sup> Witcher (2005).

then the site is considered occupied.<sup>37</sup> This chronological refinement allows Perkins to break down an undifferentiated republican period into individual centuries and to make closer observations about the relationship between the archaeological evidence and key historical events (e.g. the foundation of the colony of Cosa). However, this approach suffers from the same problem as the republican material from the South Etruria survey; that is, in order to achieve ever finer chronological resolution it is necessary to work with ever less of the material and to make ever more assumptions of it. Arguably, this method appears to work because the shorter periods and smaller quantities of material culture combine to create erratic changes in settlement numbers which seem to fit well with the serial agricultural and political crises which litter republican history.

A slightly different approach to demographic modelling builds on ecological carrying capacity, using Geographical Information Systems. Goodchild uses textual and comparative evidence for estate size, sowing rates, yield ratios, etc. in order to model agricultural production as a proxy for population in the middle Tiber valley.<sup>38</sup> This detailed modelling involves large margins of error, but usefully encompasses urban as well as rural population by considering agricultural surpluses.

All of these approaches move the relationship between textual and archaeological data onto a new level. Instead of indirect comparisons of census figures with generalized settlement trends, a much more immediate comparison of absolute population numbers has emerged— $x$  million against  $y$  million. To what extent do these textual and archaeological population figures complement or contradict? And what are the assumptions which underpin such reconstructions?

In order to answer these questions, it seems appropriate to consider my own reconstruction of the early imperial population of the *suburbium*.<sup>39</sup> The principal aim of this model was to consider the social, political, and economic interaction of metropolis and *suburbium*. This was motivated by three considerations: 1. Existing population estimates for the *suburbium* appeared far too low—Beloch suggested the equivalent of 10 persons/km<sup>2</sup>;<sup>40</sup> most recently Blanton has argued for 31

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<sup>37</sup> Perkins (1999); see also Cambi (1999).

<sup>38</sup> Goodchild (2006; 2007).

<sup>39</sup> For fuller discussion of the model see Witcher (2005). See also Scheidel in this volume.

<sup>40</sup> Beloch (1886, 402–3).

persons/km<sup>2</sup>;<sup>41</sup> 2. Quilici's (rather neglected) observation on the high density of settlement, and therefore population, in the *suburbium*;<sup>42</sup> if corroborated, this would have wide-ranging implications for urban-rural relations, which archaeologists and historians had not yet fully explored; 3. If it really were possible to reconstruct past populations using field survey data, there could be no better-suited combination of period and place than the early-imperial *suburbium*: a wealth of survey projects, highly-dispersed settlement, abundant and varied material culture with many imports, and a short chronological span. If it were impossible to reconstruct the population of the early-imperial *suburbium*, then it would cast doubt on the whole enterprise of using survey data for demographic reconstruction in general.

Full details of the methodology are contained in my earlier article, but a summary is provided here. The basic model proposed standard site densities/km<sup>2</sup>, based on the results of various field surveys in the area up to 50 km from Rome and in the area from 50–100 km from the city. From these it was possible to calculate the number of sites in the two zones. Population ranges for each site type (farm, villa, etc.) were then reviewed and 'informed estimates' proposed. It was then possible to calculate the minimum, maximum, and informed estimate figures for the immediate and wider *suburbium*. The figures ranged from *c.* 195,000 to *c.* 650,000, with an informed estimate of *c.* 325,000 (the equivalent of *c.* 60 persons/km<sup>2</sup>) for the immediate *suburbium*, and an additional informed estimate of *c.* 385,000 for the wider *suburbium* (the equivalent of *c.* 42 persons/km<sup>2</sup>).

It is important to stress that such models are heuristic devices; in keeping with the opening quotation from Golden,<sup>43</sup> there was no suggestion that the early imperial population was either exactly or constantly 325,000. Rather, this was a working figure, in the right order of magnitude, with which to think through issues such as the relationship between urban and rural populations. Indeed, at double the previous highest estimate of the suburban population, and six times Beloch's figure, the model suggested that there were significant implications for existing assumptions about metropolis-hinterland relations.

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<sup>41</sup> Blanton (2004).

<sup>42</sup> Quilici (1974a, 424–5).

<sup>43</sup> Golden (2000, 32).

However, the success of the methodology for the reconstruction of the early imperial population does not presuppose that it can be applied universally. Reconstructing the population of successive periods raises considerable issues concerning the relative visibility of settlements over time. As the above discussion of the South Etruria survey black gloss pottery emphasizes, some periods, such as the republican, present particular problems of visibility because of their heavy reliance on a single diagnostic artefact class and/or long chronological duration. In other words, although it may be possible to apply the same basic methodology to reconstruct the size of the republican suburban population, this may be a relative underestimate and therefore difficult to compare directly with the early imperial figure.

In order to deal adequately with the many issues raised during the reconstruction of the population of a single period, the original article deliberately restricted itself to discussion of the early imperial period alone and did not address the issues raised by diachronic reconstruction. In reality, such problems of visibility may mean that it is possible to reconstruct ancient population only for a few specific periods and places;<sup>44</sup> the coincidence of large amounts of diverse and diagnostic material culture, dispersed settlement, and short chronological duration make the early imperial *suburbium* one of these rare cases. However, because the early imperial population must fit into a longer demographic history, it provides a valuable baseline from which to approach the demography of other periods and regions.

In identifying the large scale of the suburban population and considering its implications for interpretations of Roman society, my earlier article arguably achieved its stated aims. However, in passing it was noted that, were these population densities extrapolated across the whole of Italy (with due allowance for mountains, etc.), they would appear to support the low count for the wider Italian population. This presented something of a paradox; even though the reconstructed population of the *suburbium* had been at least doubled, across Italy as a whole this would still only be sufficient to match the low count. This situation is surely explained by the implausibly low level of previous estimates of suburban population density; this provides a useful demonstration of why it can be useful to “think with models”.

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<sup>44</sup> See Sbonias (1999, 9).

The model itself started with no ideological attachment to either the high or low count; the aim was simply to assess whether the scale and significance of the *suburban* population had been underestimated. (Whether or not I started with implicit assumptions which conditioned my choice of parameters only others can judge.) Having identified the paradox of a higher suburban population *and* a lower Italian total, the second half of this article considers whether or not it is possible to identify even higher population densities in the suburban and other landscapes and what these might mean for the high and low counts.

#### IV. *Models and assumptions*

All models inevitably involve assumptions. Which of the assumptions made by the *suburbium* model might be revisited in order to find more people in the suburban landscape? Two main issues will be discussed: number of persons per site type, and recovery rates.

##### *Number of persons per site type*

For each site type the model summarized minimum, maximum, and 'informed' population estimates; for farm sites, these figures were five, fifteen, and eight respectively. The decision to assign eight individuals to each farm, instead of the more conventional five,<sup>45</sup> was based on comparative evidence for extended rural peasant families. This has attracted criticism; Fentress has argued that eight is too high and that five may be a better reflection of rural family size *after* conscription, high mortality, etc.<sup>46</sup> Further, Rosenstein has demonstrated that such small nuclear families could be remarkably robust.<sup>47</sup> However, for current purposes (i.e. assessing whether it is possible to increase the population total further), it is sufficient to note that an informed estimate of eight persons per farm is already too high for many scholars and skewed towards producing a higher population total.

However, it is important to consider two related issues: site definition and population. Osborne has noted enormous variation in the cut-off

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<sup>45</sup> See Osborne (2004).

<sup>46</sup> Fentress (forthcoming). See also Wilson (2008).

<sup>47</sup> Rosenstein (2004).

point between farms (populated with a fixed number of individuals or families, or multiples thereof)<sup>48</sup> and larger sites, such as villages, where population figures are derived by multiplying site size by standard population densities. For example, for one survey, a 1 ha scatter may be a farm with a population of five individuals; for another survey, it would be a hamlet with a population of 100. This is clearly a critical issue, especially if population estimates are to be compared between surveys and regions. However, the survey evidence from the *suburbium* makes it difficult to tackle this issue directly. In the vast majority of cases scatter sizes were not recorded; indeed, the bulk of the evidence comprises 'legacy data' collected during the 1960s–80s, which inevitably fall short of modern survey standards.<sup>49</sup> Further, most ancient towns lie beneath medieval and modern successors, and town size is only reliably known in a few cases. Hence in the model standard site populations were used for all site types, including villages and towns. (It is worth noting, however, that where urban site size is known, the figures calculated on the basis of site size and population density are similar, e.g. the informed large town population figure was 5,000; Falerii Novi extends across 30 ha; at 100–250 persons/ha, this totals 3,000 to 7,500 persons.)

More broadly, this raises the issue of site type definition. How is a farm to be distinguished from a villa? Scholars have made great efforts to establish the appropriate archaeological criteria. However, Rathbone has questioned the near universal survey dichotomy which makes a rigid distinction between 'farm' and 'villa'; he argues that archaeologists have oversimplified reality and in the process created two bogus historical categories.<sup>50</sup> In order to address issues such as the decline of the free peasantry and the rise of the slave villa estates, scholars have sought pseudo-historical categories such as farm and villa in the archaeological record. In reality the evidence—especially in the *suburbium*—suggests a more complex settlement spectrum which can be resolved into two categories only with massive generalization.<sup>51</sup> However, it is important to appreciate that it is almost impossible to strip away these interpretative categories from existing survey data. These two categories are not simply overlaid on survey results; rather they permeate right down to practice in the field, conditioning what is recorded and how. Hence

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<sup>48</sup> Usually five individuals (or one family/household) per farm: Osborne (2004).

<sup>49</sup> For the problems and possibilities of such data see Witcher (2008).

<sup>50</sup> E.g. Rathbone (1993); see also Rathbone in this volume.

<sup>51</sup> Witcher (2006b).

in the current model the dichotomy between farm and villa as made by individual surveys is maintained on the basis that the distinction is 'reasonably robust'.<sup>52</sup> This does not mean that the dichotomy is real, but simply that this *archaeological* classification is one of many valid methods of dividing up this site spectrum; it is 'robust' in the sense that most surveys are clear about the *relative* distinction of farms and villas in their local context. Archaeologists have to work with the data available; attempts to erase such categories from these 'legacy data' are limited by the information recorded by earlier surveyors (e.g. size, artefact densities). Further, the prospect of new spatially-extensive, high-intensity surveys matching the scale and quality of evidence recovered by the South Etruria survey is, unfortunately, remote.

Returning to the population of individual site types, the informed villa site population was 25 individuals. This is rather lower than estimates in other regions (e.g. Albegna valley),<sup>53</sup> on the basis that many of these sites are relatively small, both in terms of scatter size and the size of their notional estates. For many of these sites 25 individuals may be too many; for others (for example, the Villa of the Volusii) it may be too few; it is, however, presented as an average figure which encompasses a broad range of villa sites.

Villages contribute only a tiny proportion of the total population, and the informed figure (100) could be doubled to the maximum population with minimal significance. It is possible that undiscovered villages lie beneath medieval and modern successors (indeed several inscriptions recovered from towns such as Riano and Rignano (*CIL* XI 3858–4080) may well indicate the presence of *uici*); however, the original model made generous allowance for such sites, assuming some 50 villages. In reality there is limited direct evidence for this number of sites. Certainly some very extensive scatters are known in the area south of Rome;<sup>54</sup> to the north of the city there appear to be fewer. It is very possible that acceptance of the farm/villa dichotomy led the South Etruria surveyors to identify a cluster of farms rather than a single village (however, the *demographic* significance of this error is less significant). Again it is sufficient for current purposes to note that even the generous figure of 50 villages contributes less than 2% of the overall

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<sup>52</sup> Witcher (2006b).

<sup>53</sup> Fentress (forthcoming).

<sup>54</sup> E.g. Quilici (1974b).

population total in the current model; increasing village populations from 100 to 200 and doubling the total number of sites (from 50 to 100) still contributes only 20,000 persons. Villages are a particularly understudied settlement type in Roman Italy, but in the *suburbium* their demographic significance was limited.

Moving on to urban centres, the figures used in the model are lower than the Italian average estimated by Brunt (*c.* 7,800);<sup>55</sup> the reason is that, despite the density of towns in this area, individual centres were relatively small in comparison with the larger cities of Umbria and especially of northern Italy. Again, however, even if the maximum urban populations are considered, this adds only *c.* 20% to the overall total. Leading on from this, if urban population is increased too far, the urban-rural ratio starts to exceed the widely accepted figure of *c.* 10–20%. This, of course, is another assumption which should be explored. But higher urbanization rates would require a fundamental reconceptualization of Italian urbanization and economy along the lines of the more heavily-nucleated Greek model. (It should be noted that the model already suggests a high urban:rural ratio of *c.* 30% including Ostia, or *c.* 21% excluding Ostia.)

Hence, in terms of the number of persons per site type, there is some leeway within the existing model to increase the overall suburban population. Assuming the maximum suggested figure for each site type doubles the total population to *c.* 644,200 (119 persons/km<sup>2</sup>). Whilst I believe that such site population figures are too high, a *c.* 20% increase on each site type is certainly feasible. However, there may be easier ways to find more people.

#### *Recovery rates*

All surveys should at some point consider recovery rates, that is, assessment of what percentage of sites has been identified and therefore what percentage of sites—and indirectly what proportion of population—is missing. It has long been recognized that survey recovers only a percentage of past settlement; a whole battery of depositional, post-depositional, and recovery processes operate to reduce the visibility of artefact scatters. Studies have considered factors as varied as the use of perishable materials, alluviation, land use, survey intensity, and indi-

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<sup>55</sup> See also Duncan-Jones (1974, 259–87).

vidual walker bias. Again the long history of fieldwork in the vicinity of Rome makes it difficult to assess the impact of such processes. However, it is highly likely that an atlas of all known sites would strongly reflect the significant variation in survey coverage and intensity, rather than genuine variation in past settlement patterns.<sup>56</sup> For this reason, rather than using known survey sites the model is based on a standard site density. The figure of one villa and two farms/km<sup>2</sup> in the area up to 50 km from Rome was based upon an assessment of the varied site densities recognized around the *suburbium*, but placed particular emphasis on more recent, high intensity surveys. Such surveys invariably recognize higher site densities and, though usually much smaller in spatial extent, provide a corrective to the older, more extensive surveys.<sup>57</sup>

Again, in terms of assessing whether or not the model can accommodate higher overall population totals, some allowance has therefore already been made for sites missed by older, less intensive surveys; the actual number of farms and villas recorded in the *suburbium* is far fewer than the hypothetical 16,000+ used in the model. However, the model does *not* make significant allowance for those sites missed for other reasons—e.g. those which are aceramic, undatable, destroyed, or deeply buried and undisturbed by the plough (to name but a few of the possible causes of low visibility). Of course, the problem is that there is no obvious means of assessing what percentage these missing sites might comprise—this is a (Rumsfeldian) ‘known unknown’.

There have been various attempts to estimate survey recovery rates by comparing figures from historical texts with settlement numbers.<sup>58</sup> In the third/second century BC colonial context of the Ager Cosanus, a figure of *c.* 20–33% site recovery has been suggested.<sup>59</sup> If this were also the case in the early imperial *suburbium*, then survey would have missed two-thirds or more of all sites. Clearly this would have profound implications for the reconstruction of population. However, it should be

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<sup>56</sup> E.g. see maps in Amendolea (2004); Torelli (1992).

<sup>57</sup> Both Fentress (forthcoming) and Wilson (2008) correctly observe that the results of the very small survey at Corese (Di Giuseppe et al. 2002), used in the original model, may well not be representative of the wider area and lack the inferential power of a probabilistic survey. However, whilst this may be true, the survey also provides an insight into the sites missed by the older and less intensive surveys of this region, following Cherry's (1983, fig. 1) observation of the strong connection between survey intensity and the density of sites recovered.

<sup>58</sup> For a summary of figures from 20%–50% see Sbonias (1999, 4).

<sup>59</sup> Cambi (2001, 140).

noted that the failure to identify, for example, two-thirds of sites does not equate with the failure to identify two-thirds of the population. This is because larger and/or richer sites are easier to locate than smaller and/or less opulent sites.<sup>60</sup> Villages and villas—and their larger populations—are therefore likely to be better represented than the inhabitants of farms. As a result, the percentage recovery of population is higher than the percentage recovery of sites.<sup>61</sup>

The model's baseline site density/km<sup>2</sup> was one villa (with a notional 50 ha/200 *iugera* estate) and two farms (each with 25 ha/100 *iugera* estates), totalling 41 persons/km<sup>2</sup> (or 60 including urban populations). This is already a dense site distribution in comparison with most other Italian regions; however, accepting that site recovery is incomplete, it is necessary to increase this density still further. Taking the 33% site recovery rate suggested for the Ager Cosanus, and assuming that it is easier to identify villas than farms, the baseline density might be increased to two villas (with 15 ha estates) and seven farms (with 10 ha estates) totalling 106 persons/km<sup>2</sup> (rising to 125 including urban populations).

However, taking *c.* 1 ha per person per annum in a biennial fallowing system (i.e. 100 persons/km<sup>2</sup>) as a baseline, then a population density of 106 persons/km<sup>2</sup> is moving towards the limits of self-sufficiency.<sup>62</sup> Further, it should be recalled that, as site densities increase, estate sizes decrease, so it becomes likely that the number of persons per site type also decreases; certainly it is difficult to imagine a scenario in which both maximum site population figures and a three-fold increase in site

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<sup>60</sup> Schiffer (1987, 347).

<sup>61</sup> See Witcher (forthcoming).

<sup>62</sup> Rural population densities can certainly exceed the 100 persons/km<sup>2</sup> threshold, but a number of associated developments might be expected. For example, the shift from a biennial fallowing system to a more intensive crop rotation system often leads to improved integration of arable and pastoral strategies and, in particular, manuring; archaeologically this is widely associated with extensive off-site scatters (Alcock et al. 1994; Bintliff & Snodgrass 1988). Alternative strategies for supporting extremely high rural populations include the significant importation of foodstuffs (evidenced through amphorae) or changes in labour arrangements as the balance between people and land leads to declining wages and therefore more exploitative forms of dependency (e.g. tenancy, bondage). Certainly the best evidence from early imperial Italy for manuring and large-scale imports comes from the *suburbium*; however, even here the evidence is far from comprehensive (e.g. off-site scatters are very discontinuous; the vast majority of import amphorae relate to wine). More importantly, as discussed below, even if such systems had evolved in the *suburbium*, the evidence that they pertained more widely across Italy is almost non-existent.

numbers would coincide. In addition, such consistently high density of sites makes no allowance for the common land needed to supplement the cultivation of individual properties<sup>63</sup> and little allowance for the surplus needed to pay taxes, buffer risk, and feed urban populations.<sup>64</sup>

How realistic then is the recovery rate of 33% of early imperial sites in the *suburbium*? Arguably, the third-century BC landscape of the Ager Cosanus was rather different from the early imperial *suburbium*. The former was a newly-founded (273 BC) and fragile colonial landscape (already in need of demographic reinforcement by 199 BC); in contrast, the early imperial *suburbium* was a mature and highly structured landscape closely integrated into the metropolis itself.<sup>65</sup> In particular, the abundance and diversity of diagnostic material culture, especially imported goods, suggests that early imperial suburban settlement is much more visible than the precarious colonial sites of the mid Republic. As a result, it is likely that survey has recovered more than 33% of early imperial settlement in the *suburbium*. Exactly what percentage, however, is open to question.

In summary, some of the assumptions of the original model could be revised—most obviously making further allowance for site recovery rates and perhaps slightly larger populations at villas and towns. However, although site recovery is an issue of critical importance for survey in general,<sup>66</sup> recovery rates in the early imperial *suburbium* are arguably better than in most other regions of Italy. Hence it might be possible to double the suburban population total of the original model, but to take the figure much higher would require a fundamentally different model of social and economic organization, for which the evidence is as yet limited.

But what about the wider picture? If the high count of *c.* 14 million for the total population of Roman Italy is to be achieved, population density needs to be consistently high (although not uniform) across the whole of Italy. In other words, in order to assess the feasibility of the high count, it is necessary to look beyond the *suburbium*.

In considering the wider Italian population total, a vital consideration is the percentage of land under cultivation (i.e. the percentage of Italy to which a standard population density should be applied). Beloch and

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<sup>63</sup> Evans (1980).

<sup>64</sup> Hopkins (1978); Horden & Purcell (2000).

<sup>65</sup> Witcher (2006b).

<sup>66</sup> Witcher (forthcoming).

Brunt suggested 40%; I have suggested an increase to 50%.<sup>67</sup> However, on the basis that survey has subsequently attested settlement in quite remote areas (e.g. Casentino, Cicolano, Monte Amiata), this figure could *perhaps* be still further increased. Importantly, for the high count, the higher the percentage of Italy under cultivation, the lower the number of persons/km<sup>2</sup> that is required to achieve a population of 14 million. For example, generously assuming that 75% of Italy was cultivated (i.e. 187,500 km<sup>2</sup>—discounting only the one-quarter of Italy's surface area over 750 m),<sup>68</sup> a population of 14 million would require an *average* density *c.* 75 persons/km<sup>2</sup> (including urban population). A more realistic 60% under cultivation would require population density to rise to *c.* 93 persons/km<sup>2</sup>. As demonstrated above, with small adjustments this density could be exceeded in the *suburbium*; it could even offset lower population elsewhere. But the physical extent of the *suburbium* comprised no more than *c.* 10% of Italy. Therefore the high count still requires high populations in non-suburban areas. How then do these figures relate to the survey evidence from other Italian regions?

### V. Regionalism

Field survey has now been conducted in nearly every Italian region, encompassing a wide variety of environmental and historical contexts. Taken at face value, the density and character of early imperial settlement demonstrates considerable intra- and inter-regional diversity.<sup>69</sup> The most obvious pattern is the marked difference in the density of settlement; beyond the *suburbium* (and a few other areas such as parts of Campania) settlement density is significantly lower. This contrast is further reinforced when site density is converted into population density. For example, if the same basic methodology as outlined for the *suburbium* is applied to the Biferno valley survey results, the early imperial population is the equivalent of just 3.4 persons/km<sup>2</sup>, or 7.4 including

<sup>67</sup> Beloch (1886, 439); Brunt (1971, 126); Witcher (2005).

<sup>68</sup> Certainly, elevation is a rather crude measure of the extent of cultivation, and in favoured areas settlement existed even above 1,000 m (e.g. Iuvanum; Bradley 2005); however, on a broader scale, cultivation becomes significantly more difficult above 750 m. By way of comparison, in 1997 only *c.* 47% of mainland Italy was under cultivation (Loscby 2000: table 18.3).

<sup>69</sup> E.g. for Etruria see Witcher (2006b).

Table 3. Demographic model of the Biferno valley in the early imperial period (based on data from Barker 1995). NB as towns were not identified as part of the survey, there is no need to correct for the 20% sample

	<b>Towns</b>	<b>Village</b>	<b>Village/ villa</b>	<b>Villa</b>	<b>Farm</b>	<b>Domestic site</b>	<b>Sporadic</b>	<b>Totals</b>
Sites identified (c. 20% sample)	3	5	2	14	34	34	19	111
Total number of sites (x5)	3	25	10	70	170	170	95	543
Persons per site type	2500	100	50	25	8	2	1	–
Total population per site type	7500	2500	500	1750	1360	340	95	14045

urban populations (Table 3).<sup>70</sup> A similar model for the Albegna valley recently proposed by Fentress calculates 19 persons/km<sup>2</sup>, or 21 including urban populations.<sup>71</sup> Further afield, in Laconia (Greece) the Roman rural population is calculated at 8.2 persons/km<sup>2</sup>,<sup>72</sup> and on the island of Jerba (Tunisia) the density is 22 (or 52 including urban).<sup>73</sup>

Although these demographic models all use the same basic methodology, the specific details vary (e.g. site type populations) and as a result the figures may not be strictly comparable (though there is no reason that there should be, for example, a standard Mediterranean-wide family of five). However, all of these figures (except those for Jerba) are much lower than the 60 persons/km<sup>2</sup> originally calculated for the *suburbium*. Indeed, in the case of the Biferno valley the difference amounts to a whole order of magnitude (c. 7 against 60 persons/km<sup>2</sup>). Obviously, it is possible to increase these site densities by adjusting these models' assumptions, for example, assigning larger populations for each site

<sup>70</sup> For a more sophisticated version of this exercise see Wilson (2008). Two obvious points of contrast between the figures for the Biferno valley and the *suburbium* (Witcher 2005, table 2) are the demographic importance of villages (18%) and towns (53%) in the former area; this may be of direct relevance to the discussion of low recovery rates (see below).

<sup>71</sup> Fentress (forthcoming).

<sup>72</sup> Cavanagh et al. (1996).

<sup>73</sup> Fentress (forthcoming).

type. However, in the case of the Biferno valley, farms would need populations more akin to villages in order for this method to increase the overall population density to suburban levels.

The alternative is to consider recovery rates in order to find more sites and hence higher population. In particular, if, as suggested above, sites are particularly visible in the *suburbium* because of the great abundance and diversity of early imperial material culture, it is valid to question whether or not the marked decline in site density with distance from Rome is real. In other words, are site recovery rates in areas such as the Biferno valley substantially lower than in the *suburbium*? This question cuts to the core of the high/low population debate, for it exposes what must be believed about the archaeological record in order for either of these text-based figures to be accepted. In order to comprehend the arguments and the implications more clearly, the following sections sketch two alternative models which characterize the uneasy ways in which the textual and archaeological evidence have been harmonized. The models are presented on the basis that discussion of the early imperial population of Roman Italy has polarized between either high (*c.* 14 million) or low (*c.* 7 million) and that an intermediate compromise figure is excluded by the terms of the debate.

#### VI. *Low count*

This model starts with an early imperial Italian population of *c.* 6–7 million.<sup>74</sup> In order to reconcile this low figure with the archaeological evidence, it is necessary to assume that survey recovers a significant percentage of sites. This is because the densities of sites (and population) directly attested by survey are already sufficient to achieve a population of several millions if extrapolated across Italy as a whole. By the time recovery rates are factored in, a population of *c.* 6–7 million is within easy reach. A recovery rate of *c.* 50% or more of sites would be sufficient. In this scenario, survey identifies the majority of sites and therefore this model might be characterized as “What You See Is What You Get”. This means that the patterns and trends in the data can be accepted more or less at face value. For example, the significant regional variation in site numbers should be accepted as represent-

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<sup>74</sup> Brunt (1971); Hopkins (1978).

ing a genuine contrast in settlement and population density—several, perhaps even ten, times higher in the *suburbium* than in areas such as the Biferno valley in Samnium. It follows that this model presupposes that a disproportionate percentage of Italy's *c.* 6–7 million population lived in the *suburbium*; in turn, this concentration of population is likely to have had important implications for the economic organization of early imperial Italy.

Another corollary of high recovery rates is the socio-economic status of rural populations. The ability of survey to recognize a majority of sites assumes that rural populations were well integrated into urban and regional economies through the consumption of finewares and other manufactured/imported goods on which survey relies to identify sites. These goods were presumably exchanged for agricultural surpluses at local markets. In other words, the assumption of high recovery rates requires a conceptualization of rural socio-economic organization akin to Horden and Purcell's Mediterranean peasantry.<sup>75</sup> Far from the independent and autarkic citizen-soldier eulogized in Roman literature, these rural populations were involved in agricultural production well beyond subsistence, the constant redistribution of surplus through regional social and economic networks, and the consumption of manufactured/imported goods.<sup>76</sup>

### VII. *High count*

The alternative model starts with an early imperial population in the order of *c.* 12–14 million.<sup>77</sup> In this case, the archaeological evidence is reconciled with the high count by assuming that survey recovers only a small percentage of sites, far below 50%, perhaps as low as 10–20%; this is because there are simply too few 'dots on the map' to accommodate a population of *c.* 14 million. For high counters, it is therefore necessary to believe that survey fails to recognize the majority of sites and therefore population.

In order for the high count to be valid, large swathes of land such as the Biferno valley simply cannot have been as thinly settled as they

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<sup>75</sup> Horden & Purcell (2000, 270–7).

<sup>76</sup> Witcher (2007).

<sup>77</sup> Lo Cascio (1994; 1999).

Table 4. Effects of hypothetical recovery rates on demographic model of the Biferno valley in the early imperial period (based on data from Barker 1995)

	<b>Towns</b>	<b>Village</b>	<b>Village/ villa</b>	<b>Villa</b>	<b>Farm</b>	<b>Domestic site</b>	<b>Sporadic</b>	<b>Totals</b>
Total sites (see Table 3)	3	25	10	70	170	170	95	543
% site recovery	100%	15%	15%	8%	2%	2%	2%	—
Multiplier	1	6.7	6.7	12.5	50.0	50.0	50.0	
Actual number of sites (rounded)	3	168	67	875	8500	8500	4750	22863
Individuals per site type	2500	100	50	25	8	2	1	—
Total population per site type	7500	16800	3350	21875	68000	17000	4750	139275

appear. Rather, to raise the calculated population of the Biferno valley to the notional high count average of *c.* 75 persons/km<sup>2</sup> (see above), it is necessary to assume that the survey recovered *c.* 15% of villages, 8% of villas, and just 2% of farms and other scatters (Table 4). These percentages can be redistributed between site types, but the general point is clear—on average the survey recovered less than 5% of sites and *c.* 10% of the population (just 14,045 of 139,275).

However, a compensatory tenfold increase across all Italian population densities would result in unacceptably high figures in, for example, the *suburbium* (i.e.  $10 \times 60 = 600$  persons/km<sup>2</sup>). It is therefore necessary to accept that, under the high count model, rather than regional variation in site/population *density*, survey reveals regional variation in site/population *recovery rates*. In other words, the people are there but the ability of survey to recognize them is regionally variable; the vast majority of these *c.* 14 million persons is invisible.

Why should some populations be more or less visible than others? The obvious answer is variation in evidence used to identify these populations, i.e. (principally) ceramics. Hence, whilst some rural populations (mainly those in the *suburbium*) conform reasonably well to the Horden and Purcell model (see above), a rather different model must have prevailed in more distant areas, such as the Biferno valley. Here the very low recovery rates imply extremely densely occupied landscapes but with very limited consumption of manufactured/imported goods. In contrast with the low count model, this suggests impoverished subsistence peasants with minimal economic contact with urban markets. Clearly the monumentalization of early imperial urban centres was not

paralleled by the social and economic integration of their rural hinterlands;<sup>78</sup> for some areas of Italy therefore the high count would presume a return to the consumer city model. In sum, for the high count, though there may well be some variation in regional population density, the implicit differences in social and economic organization are far more significant. The apparent failure of survey to recognize *c.* 80–90% of the population actually reveals fundamental socio-economic differences across Roman Italy.

### VIII. *Discussion*

The high and low counters have begun to explore the logical implications of their figures in order to identify proxies with which to support their positions. Arguably, however, these implications (e.g. levels of conscription, labour wages) have not explicitly considered the archaeological evidence and how interpretations of it must differ. The two models outlined above highlight that the high and low counts have profoundly different interpretations of the archaeological evidence. For example, in order to accept the low figure it is necessary to accept that survey recovery rates are high; in turn, this must reflect a socially and economically integrated, high consumption society. Importantly, therefore, these models connect the high and low count assumptions about survey data to the processes which generated the archaeological record; that is, the actions of individuals within particular social and economic systems.

Scholars have increasingly found it difficult to choose between an Italian population of *c.* 7 and 14 million;<sup>79</sup> both figures have useful explanatory power (e.g. the high count reduces the extremely high conscription rates assumed by the low count; the low count better explains the widespread use of slavery). Similarly, it is difficult to choose between the two ‘archaeological’ versions of the high and low counts presented here (e.g. the high count is based on the *absence* of evidence); the models do, however, encourage explicit assumptions and logical thinking. For example, it would seem difficult to sustain the simultaneous

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<sup>78</sup> For recent discussion of the close and shifting relationship of towns and their hinterlands during the early imperial period see Patterson (2006, 269–78).

<sup>79</sup> E.g. Morley (2001); Scheidel in this volume.

assumptions that population was high and that rural populations were economically integrated.

But how might we approach the archaeological evidence without the fixed points provided by the high and low count figures? In other words, how would we interpret the archaeological evidence independently of the texts? Unlike the text-based debate, these two archaeological models are not necessarily mutually exclusive. Arguably, an intermediate population total would make better sense of the evidence as we currently understand it. Hence, there were more sites closer to Rome of which a higher percentage has been recovered. Conversely, there were fewer sites further away from Rome, of which survey has recovered a lower percentage. This interpretation is also apparent when comparing, for example, the *suburbium* with Samnium: the quality and quantity of material culture and the form and distribution of villas are quite distinct and are plausibly interpreted as differences in both population *and* recovery rates. Further, these assumptions about the interpretation of the archaeological data might not only be compatible, they may also actually be correlated. For example, a small population might have an aggregate demand which is insufficient to maintain a specialist craftsman; this population might therefore be both smaller and *disproportionately* less visible.<sup>80</sup>

Such an interpretation is not just a case of dismissing the textual evidence and ‘splitting the difference’; we still have to acknowledge the census figures. However, it is important to question the assumption that the textual and archaeological evidence should neatly agree. As stressed above, these sources relate to phenomena (censuses, ceramic consumption, settlement patterns, etc.) which are only very loosely connected; the evidence is also received or recorded in quite distinct ways. Indeed, we might well be suspicious if the texts and archaeological evidence did conveniently agree given the wildly varying estimates of key variables (e.g. the number of citizens overseas, etc.). Hence, we should expect *some* relationship between the textual and archaeological evidence but not a straightforward one. Arguably, the fact that the archaeological figures are already within the same order of magnitude as the census figures should be considered worthy of remark in its own right.

The argument for an intermediate figure does not require us to reject the historical texts out of hand. Hin proposes a new reading of the

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<sup>80</sup> Witcher (2007).

historical texts which offers one possible alternative to the high and low counts.<sup>81</sup> Hin's middle figure provides a maximum population of 10 million, but she argues that the actual population was somewhat less. This sensibly avoids the temptation or necessity to quantify variables for which we have, in reality, limited information (e.g. census *overregistration*). I do not suggest that Hin's third way provides direct support for an intermediate archaeological figure and much less that it should coincide with such a figure; simply, that the current polarization of interpretation between high and low counts is increasingly inadequate. I suggest that there is no need to force the archaeological data to fit the high or low count figures, not least because intermediate figures are possible on text-based grounds as well. However, perhaps the key point highlighted by these models is not the scale of the population *per se*, but the fact that they all imply regional variability of either population and/or socio-economic organization across early imperial Italy. *Tota Italia* was not a uniform Italia.

### IX. *Conclusions*

Modelling absolute population figures with survey data is not undertaken in the belief that the resulting numbers are real (i.e. precise, conclusive, etc.). Rather, these are "models for thinking": they serve to highlight assumptions and logical problems which help to advance understanding; they also help to develop comparisons between different periods and regions. In this sense, survey data can most definitely contribute to the study of Roman demography.

The example of the South Etruria survey is instructive. Here a distinct decline in the number of Late Republican I sites would appear to support the historical model of second century BC (citizen) population decline; however, an assessment of the processes which lie behind this settlement trend (in particular, the deposition and sampling of black gloss pottery) reveals that this pattern is insufficiently robust to support such an interpretation conclusively. This is not to argue that it is impossible to address this issue archaeologically, but simply that the characteristics of this particular dataset mean that it cannot be used to address this issue with any certainty. This example helps to reveal the problems

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<sup>81</sup> Hin in this volume.

of stretching the (interpretation of) archaeological evidence to support the (interpretations of) textual data. More broadly, it also illustrates that the basic demographic modelling methodology outlined here cannot be used indiscriminately. Numerous issues of archaeological visibility (from variation in the consumption of material culture to differences in modern land use) mean that this technique may be applicable only to a few special combinations of period and place—arguably one of these is the early imperial *suburbium*. Fortunately, the special cases where it is applicable provide baselines from which to consider the evidence of other periods and places. For example, the large population of the early imperial *suburbium* did not appear from nowhere; it must relate closely and logically to the late-republican population.<sup>82</sup>

A re-evaluation of some of the assumptions behind a demographic model of the early imperial *suburbium* suggests that it may be possible to increase the population still further; particularly through greater allowance for recovery rates. Overall, however, the special conditions of the *suburbium* (most importantly its metropolitan-style consumption practices) mean that recovery rates are less problematic here than for many other regions of Italy. In order to assess the impact of recovery rates, models have been framed around the high and low counts. These models foreground a number of quite distinct and implicit assumptions made about the interpretation of archaeological survey data. In particular, the high count assumes very low recovery rates; in turn this suggests relatively unintegrated rural economies and low consumption. In contrast, the low count assumes high recovery rates; and this suggests a more integrated rural economy with extensive access to manufactured/imported goods. The models in themselves do not resolve the high vs. low count debate, but they do provide an archaeological perspective on (what has been to date) a primarily text-based dispute.

Beyond the issues involved in the high and low counts, the models stress further the growing evidence for the variability of early imperial Italy. Whichever model (or combination of models) one chooses, *regionality* is the theme which repeatedly emerges: either in terms of population (as evidenced in differences in settlement density) and/or in terms of socio-economic organization (as implied by variation in survey recovery rates). In turn, better recognition of the importance

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<sup>82</sup> Witcher (2006b).

of regionality for our understanding of the demography of Roman Italy should encourage new perspectives on old problems, such as the second-century BC population decline.

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## POOR PEASANTS AND SILENT SHERDS

Dominic Rathbone

### I. *Introduction*

The big issue to which this paper ultimately relates is the size, nature, and dynamics of the farms and rural population of Roman Italy of the Republic, especially in the second to first centuries BC.<sup>1</sup> The main potential evidence for this subject is the results of the many archaeological surface surveys of rural areas of Italy which have been carried out since the 1950s, and are still popular projects today.<sup>2</sup> That fifty years of research have not yet produced a generally accepted view of developments is due to technical problems such as the comparability of survey practices and publications, and changes in the typology and dating of black gloss pottery, the principal diagnostic fineware for republican Italy, and the interpretative difficulty of getting the data to answer the main historical question which stimulated their collection. That question was whether in the second to first centuries BC the free peasantry of Roman Italy was, as historical tradition has it, being pushed off the land by the expansion of slave-staffed estates, but in recent years it has taken a more demographic turn (as other papers in this volume illustrate) into whether we should interpret the Augustan census figures to indicate population decline or growth in later-republican Italy.

My narrow concern here is the typology of the apparent farm sites identified by these field surveys, especially the smaller sites.<sup>3</sup> In his masterly synthesis which made ancient historians take notice of the pioneering south Etruria surveys, Potter adopted from his work on the Ager Faliscus a tripartite division of findspots: extensive scatters (c. 3,500 m<sup>2</sup>) including luxury architectural components, medium scatters

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<sup>1</sup> I am grateful to the participants at the Leiden conference, especially John Bintliff, Rob Witcher, and Henrik Mouritsen for comments and information, and to the organisers and editors for patiently pushing me to publish. I also owe much to discussions long ago with Barri Jones and Tim Potter.

<sup>2</sup> Helpful lists and summaries in Witcher (2006, 91–6); Patterson (2006, 72–88).

<sup>3</sup> For villages see Pelgrom in this volume. I leave aside non-agricultural sites such as rural sanctuaries and road stations.

(1,000 to 1,400 m<sup>2</sup>, some up to 2,000 m<sup>2</sup>) including some decorative items, and small scatters (a few hundred m<sup>2</sup>) of tile and pottery, which he identified as villas, farms, and huts; some ‘huts’, he thought, were temporary shacks, as used by shepherds, some were outbuildings of large estates, and some were badly degraded farm sites.<sup>4</sup> Thus ‘huts’ was a portmanteau term for small findspots rather than a particular category of farm site. More simply, Ward-Perkins, in his report of the Ager Veientanus survey, noted that sites were very varied and difficult to classify, but reckoned that a basic division into ‘villas’ and ‘farms’ was justifiable.<sup>5</sup> Surveys since then have added their own twists, but the general trend has been to use a binary classification into ‘small’ and ‘large’ sites, as noted and endorsed most recently by Witcher, who also follows the trend to refer to the sites as ‘farms’ and ‘villas’.<sup>6</sup> The persistent conflation of archaeological description (large, small) and historical interpretation (villa, farm) betrays the influence on the survey enterprise of the historiographical tradition, begun by the Romans themselves in their disputes over late-republican agrarian laws (which the annalists then wrote back into early Roman history), of a constant conflict between small peasant farms and large slave-staffed estates. If we are to use the survey data to construct an independent picture of developments, it would be better not to start with this conflation of findspot description and site interpretation.

In this paper I will use evidence from written sources (section 2) and from some excavated farmsteads (section 3) to argue that this simple division of sites into two implicitly distinct and unitary categories is unjustified and unhelpful. There have been some, if unsatisfactory, attempts by scholars to distinguish different types of villa buildings and estates.<sup>7</sup> My focus is instead on the smaller sites, the possible farms of the Roman peasantry, who remarkably have never received a monographic

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<sup>4</sup> Potter (1979, 11–12, 122). Although huts comprised 35% of his sites in the Ager Faliscus, as compared to 43% farms, he excluded huts from his composite map of sites in South Etruria (fig. 35). In his composite tables of the survey results he sidestepped the problem of classification by just giving total sites per area/period. Potter (1992, 642–7) reintroduces a fourth category of ‘large villas’.

<sup>5</sup> Kahane et al. (1968, 153–6).

<sup>6</sup> Witcher (2006, 97–8), although he notes that this is problematic and adds (n. 29) that the *alimenta* tables indicate “an intermediary class of landowners” (whatever that might mean).

<sup>7</sup> Notably Rostovtzeff (1957, II 551–3, 564–5); Carrington (1931); Dohr (1965). Rossiter (1978) is a pioneering attempt at a general classification of Roman farm buildings.

study and are typically imagined, under the influence of old ‘peasant studies’, as a static and undifferentiated mass.<sup>8</sup> I will suggest that the smaller sites of field surveys represent a spectrum of farm types and sizes with a considerable band of overlap between family farms and slave-worked farms. I will also suggest that a substantial sector of the Roman peasantry had farms which were so small, physically insubstantial, and temporally transient that it is not clear how well surveys have been able to recognise their exiguous material traces. Previous airings of this picture of poor peasants and silent sherds (strictly, absent sherds) have provoked some disbelief from archaeological colleagues. Although I would have liked to research and think further before venturing into print, I have been persuaded to do so here, albeit in an unfinished form, in order to see what responses are evoked.

## II. *Small farms: written evidence*

From the written sources for Roman history we have a small, but precise and consistent, body of data for the sizes of smallholdings allocated on occasion by the Roman state to its citizens.<sup>9</sup> In a tradition which supposedly, perhaps genuinely, went back to settlement of the Ager Veientanus in the 390s BC, the archetypal citizen allotment was deemed to be 7 *iugera* (1.8 ha). Eight or perhaps all nine of the attested citizen allotment sizes, mostly colonial, of the early second century BC range from 5 to 10 *iugera* (1.3 to 2.5 ha), and the viridane allotments of Caesar’s scheme in the Ager Campanus were 10 *iugera* for men with at least three children and 12 *iugera* (3 ha) in the Ager Stellas, and there is some independent archaeological evidence for allotments of these sizes.<sup>10</sup>

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<sup>8</sup> Heitland (1921) is about the condition of labour, including on large estates; Frayn (1971) is about farming. There have been many useful studies of particular aspects, e.g. Evans (1980); Rosenstein (2004).

<sup>9</sup> I ignore here the miniscule allotments near Rome of the fourth century, such as 2.5 *iugera* at Satricum in 385 (Liv. 6.16.6), because I think these were given as a form of booty to *assidui* in addition to their existing farms.

<sup>10</sup> 184 Potentia and Pisaurum both 6 *iug.* (Liv. 39.44.10); 183 Modena 5 *iug.*, Parma 8 *iug.*, Saturnia 10 *iug.* (39.55.7–9); 181 Gravisca 5 *iug.* (40.29.1–2); 177 Luna 6.5 *iugera* (41.13.4–5, corrected from 51.5 *iugera* by Salmon 1933, 31 n.2, following De Sanctis); 173 Ager Ligustinus and Ager Gallicus 10 *iug.*, and 3 *iug.* for Latins (42.4.3–4); 59 BC Campania, as proposed 63 BC by Rullus (Cic. *Agr.* 2.78, 85 etc.). For archaeology, apart from the implications of centuriation grids, see the late first-century farms at Monte Forco and Grottarossa (section 3 below).

Since it is implausible that the state settled citizens on allotments which were below the minimum *census* qualification for *assidui*, that is for military service, allotments of 5 to 10 *iugera* must have been reckoned to meet the official threshold value for the fifth *classis*, which in the period 211–141 BC was (after being halved from its previous value) 4,000 sextantal asses, equal to 400 denarii.<sup>11</sup> Because actual property values must have varied considerably, the Romans presumably had some notional scale of landholding in mind which corresponded to the cash figures, and minima of 100, 75, 50 and 25 *iugera* for the first four *classes* seem plausible to me, which would imply a notional 4 *iugera* for the fifth *classis*. In 140 BC, when the cash figures for the first four *classes* were written across into sesterces, the figure for the fifth *classis* was instead reduced to HS 375, equal to under 94 denarii. This sum cannot have represented more than a hut and garden-plot, enough to claim that a man was not landless. This fits well with the story of Spurius Ligustinus, Livy's heroic veteran of 171 BC, father of eight children, owner of a paternal hut (a 'small *tugurium*') and a single-*iugerum* plot; spurious undoubtedly, but probably reflecting the reality of the later second century when, as Tiberius Gracchus acidly observed, the soldiers whom generals exhorted to fight for their ancestral property did not own a single clod.<sup>12</sup>

In contrast, we know that in some of the colonies with Latin status founded by the Roman state in the early second century BC the 'infantry' settlers were granted allotments of 15 to 50 *iugera* (3.8 to 12.5 ha).<sup>13</sup> There were also some more generous schemes for citizens in the late Republic. The probably Gracchan allotments east of Luceria seem to have been of 80 or 90 *iugera*. For what it is worth, Domitius Ahenobarbus in 49 BC promised allotments of 40 *iugera* to his soldiers, and more to

<sup>11</sup> See Rathbone (1993) for the levels and chronology of the *census* thresholds. Some scholars continue to doubt the post-211 minimum of HS 375, but it is attested as a registered census of the fathers in two out of six birth declarations by Roman citizens in early imperial Egypt, which is too odd to be chance.

<sup>12</sup> Liv. 42.34; our Spurius is Ligustinus, 'the Ligurian', and also from Sabinum, both stock homes of tough peasants (or was 'Ligustinus' invented with the settlement schemes of 177 and 173 BC in mind?). Plu. *TG* 9.5 cites Tiberius' observation, which some annalist then put into the mouths of the tribunes proposing the *lex Licinia de modo agrorum* supposedly of 367 BC (Liv. 6.36.10–11).

<sup>13</sup> 193 Copia 20 *iug.* (Liv. 35.9.7–8); 192 Vibo 15 *iug.* (35.40.5–6); 189 Bononia 50 *iug.* (37.57.7–8); 181 Aquileia 50 *iug.* (40.34.2–3). The text of Livy records allotments of 51.5 *iugera* for the 177 BC citizen colony at Luna, but the real figure may have been 6.5 *iugera* (see n.10 above).

centurions. Triumviral colonists at Volaterrae received 25 or 30 *iugera*, and centurions 50 or 60 *iugera*.<sup>14</sup> On my interpretation, allotments of these sizes would have qualified their recipients for the fourth to second *classes*. The Romans clearly knew that their normal citizen allotments were small, and must have deliberately kept them small. The main point here, however, is that the literary evidence for state allotments attests the creation of peasant-soldier farms in a range of sizes, from a minimum of 5 *iugera* up to 50 and even 80 *iugera*, although citizen allotments cluster at the low end of the scale.

Without going into detail, which would require another paper, and remembering that the quality of land will have varied considerably, I note that this range of allotment sizes implies a range of different agricultural possibilities. Farms of 5 to 12 *iugera* (1.3 to 3 ha) will have been minimum subsistence farms, worked horticulturally by the labour of the family.<sup>15</sup> Farms of around 15 to 25 *iugera* (4 to 6 ha) will have permitted the keeping (fodder) of a draught animal and production of a small marketable surplus, and maybe some leisure. Farms of around 30 to 50 *iugera* (8 to 13 ha) will have allowed the employment of a couple of slaves or the leasing out of some land, more specialisation in cash crops with a regular surplus for sale, and more opportunity for leisure. In 206 BC complaints from the Latin colonies of Placentia and Cremona (whose allotments were probably generous) spurred the consuls to encourage the people (*plebs, populus*) to return to their farms, despite protests about the lack of free labour, slaves, and animals, and the destruction of their farmsteads (*villae*).<sup>16</sup>

We should also recognise that peasant farming was a dynamic and unstable system. Within a few years of the initial allocation of allotments, a kaleidoscopic process of division and amalgamation of the privately owned plots will have begun as the result of partible inheritance and sales, of some families prospering and others failing, some

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<sup>14</sup> Luceria: see section 3 below. Ahenobarbus: Caes. *Civ.* 1.17.4. Volaterrae: *Lib. Col.* 168.16–20 (Campbell). The 30 *iugera* referred to in the Lex Agraria of 111 BC (*Roman Statutes* I 2.13–14) is not an allotment size; the clause grants private ownership to any occupation of public land by an individual citizen since 133 BC up to that amount.

<sup>15</sup> Farmers may have had some access to local *ager compascuus*, common grazing land, but the idea that they could also farm nearby unallocated *ager publicus* is a myth: see Rathbone (2003). Evans (1980) illustrates how a family could live off a subsistence allotment by exploiting varied foodstuffs rather than relying on grains (a common scholarly misapprehension).

<sup>16</sup> Liv. 28.11.9; the significance of this passage was noted by Heiland (1921, 141–2).

expanding and others dying out. Some will have increased their holdings and become employers of slaves and tenants, some will have had to resort to renting and labouring, others will have preferred to move off and take their chances as urban poor. There are echoes of this in the literary sources. In 209 BC twelve Latin colonies complained that their manpower and finances were exhausted and in 204 BC were punished by the senate with heavier levies.<sup>17</sup> In the early second century nine colonies are known to have needed heavy new drafts of settlers to replace losses through the Hannibalic war, native attacks, disease, desertion, and so on.<sup>18</sup> The list is certainly incomplete and probably represents only the worst cases of a chronic phenomenon. In the late Republic colonists were no more reliable, and veterans were notoriously flighty. The Gracchi tried to prevent alienation of the allotments in their scheme by granting them under leasehold with a peppercorn rent to the state; Sulla and Caesar banned sale of their allotments, in Caesar's case for ten years. This was apparently not what many settlers wanted. The Gracchan allotments were privatised fully by the *Lex Agraria* of 111 BC, the Sullan colonists allegedly sold up anyway, and in 44 BC Brutus and Cassius revoked Caesar's ban.<sup>19</sup>

### III. *Small farmsteads: excavated evidence*

I suspect that many Roman peasants who had subsistence farms or less lived in oval huts, very similar to those represented by archaic Latial hut-urns, built of timber posts, brush and mudbrick on crude stone footings, and sometimes with a tiled roof. Although prehistoric to archaic examples have been excavated, I know only—which may be my ignorance—of a single semi-published example from the third to first centuries BC, which is the late third- or early second-century oval hut, with rough stone footings, probably not much bigger than 50 m<sup>2</sup>,

<sup>17</sup> Liv. 27.9, 29.15.

<sup>18</sup> 200 Venusia: Liv. 31.49.6; 199 Narnia: 32.2.6–7; 197 Cosa: 33.24.8–9; 190 Placentia and Cremona: 37.46.9–11; 186 Sipontum and Buxentum: 39.23.3–4; 184 Cales: *CIL* VI 1283; 169 Aquileia: Liv. 43.17.1. In some cases resettlement is probable but not reported, such as Mutina in 176 following its capture by the Ligurians in 177 (Liv. 41.14.2, 16.7–8, 18.3); the case of Cales shows that the annalistic tradition omitted some resettlements altogether, and the full text of Livy breaks off in 167. See further Broadhead in this volume.

<sup>19</sup> Rathbone (2003, 162–4, 173); Keppie (1983, 95–6).

revealed under a later building near Matrice in Samnium.<sup>20</sup> I divert instead to some literary excavation. Roman writers of the late Republic and early Principate use the word *tugurium* to denote a farmer's hut, in historical works typically inhabited by a famous peasant leader on his 7 *iugera* farm, or the smaller round shack of shepherds, not unlike the *capanna* still used in the Roman Campagna in the mid-twentieth century, for which the writers also use the near-synonym *casa*.<sup>21</sup> Some poetic imaginings of rustic huts add detail. In their timber-frame *casa* in Bithynia, where they hosted Jupiter and Mercury, Baucis and Philemon had a willow bed with sedge mattress, a wooden bench and table, a bronze cooking pot, a beechwood platter and cups, pottery plates and an 'embossed' wine-mixer, plaited baskets, rough clothes, and some drapes. Simulus, the 'rustic tiller of a slender farm' and breakfaster on herb porridge, lived with a slave-girl in a *casula* with a lockable door; household equipment included a goatskin cape, truckle bed, table, wooden shelf, hearth made of tiles, handmill for grain, pestle and mortar, sieve, and lamp; Simulus also had a grainstore and stable for two oxen, a kitchen garden fenced with reeds, and sold surplus produce at the market in town, so he was not a subsistence farmer.<sup>22</sup> It is impossible to assess the realism of these vignettes, but some literary references suggest autopsy, for example Columella explaining how to protect drying figs with an arched covering of hurdles like the roof of a *tugurium*, and the elder Pliny noting that 'rustics' use bark to make the eaves of their *tuguria*.<sup>23</sup>

The smallest excavated Roman farmhouse which I know is site 154 on the Monte Forco ridge in the Ager Capenas in south Etruria.<sup>24</sup> The area had first been deep-ploughed in 1956, and the farm was excavated in 1961. The building was a plain rectangle of 11 by 5 m (55 m<sup>2</sup>), probably one-storey, with two doorways. It was very solidly built of concrete walls faced with *opus reticulatum*, partly cut into bedrock, and tufa quoins. Two dolia set into the ground outside with adjacent post-holes indicate some external storage and working areas partly covered by a

<sup>20</sup> Lloyd (1991, 182–4).

<sup>21</sup> E.g. Var. *R.* 2.10.6; cf. Potter (1979, pl. IIIb).

<sup>22</sup> Ov. *Met.* 8.629–88 (cf. *Fast.* 5.499–522); [Verg.] *Mor.*

<sup>23</sup> Col. 12.15.1; Plin. *Nat.* 16.35; cf. *Dig.* 50.16.180.1, with tiled roof. Admittedly Vitruvius 2.1.2–6 regards such huts as archaic, but his subject is elite housing.

<sup>24</sup> Jones (1962, 172–3; 1963, 147–58). I regret my failure to provide plans here. Plans of most of the sites I describe can be found in Rossiter (1978) and Flach (1990, 215–49).

home-made lean-to. The original construction seems to belong to the late first century BC; in the earlier second century AD a new partially paved floor was laid over a layer of detritus used as make-up, and other changes made; this was later covered by a thick layer of rubbish and manure topped with a beaten earth floor which had post-holes in it. Black gloss, terra sigillata, and coarseware sherds and a Domitianic coin were found by the survey; excavation added three terra sigillata and fifteen coarseware sherds. A small rock-cut tomb found 35 m to the west implies more than one generation of occupation, but not necessarily continuous through to the last phase when the building seems to have been reused for animals. The site was one of five or six located along the ridge at roughly 130 m intervals (with room for one more in a gap). The surface material of one other site also included black gloss and terra sigillata sherds, while the rest produced just coarseware and tile fragments. Each farm controlled not more than 10 *iugera* of land of which up to 6 (1.5 ha) were suitable for arable farming. These look to be veteran allotments, probably viridane, of a Caesarian or triumviral scheme.<sup>25</sup>

Although they were not excavated, I draw attention to two similar groups of sites identified in the region by survey at the same time. First, a few kilometres north of Rome on the via Flaminia, a construction project at Grottarossa revealed twenty-six sites with late-republican black gloss sherds at intervals of a few hundred metres, of which only five or six produced terra sigillata.<sup>26</sup> A cluster of small veteran allotments of the later first century BC is implied, most of them quickly abandoned. Second, the field survey around Sutrium recorded a number of rectangular structures of concrete faced with *opus reticulatum* of 9 by 4 m or thereabouts. Admittedly in the published list it is difficult to distinguish possible farmsteads from cisterns, but these may be another set of identical solidly built late first-century BC farmsteads for small veteran allotments.<sup>27</sup>

The second smallest excavated Roman farmstead which I know is the Nocelli farm near Luceria in north Apulia.<sup>28</sup> It was a rectangular

<sup>25</sup> Keppie (1983, 168) notes the lack of written evidence for a colony; he does not mention these sites.

<sup>26</sup> Jones (1963, 146).

<sup>27</sup> Duncan (1958, 98–131). Keppie (1983, 169–70) thinks triumviral settlement at Sutrium probable but unproven.

<sup>28</sup> Jones (1980, 92–8), only a preliminary notice. A full pottery report might clarify the periods of occupation. See now Volpe (1990, 209–13) on the centuriation.

single-storey structure of 10 by 15 m (150 m<sup>2</sup>), almost three times bigger than the Monte Forco farmstead. It too was plainly but solidly built, but with some internal divisions, and a Ctesibian pump set into the floor, which drew water from a well outside covered by the external veranda. The farm was one of a series built at regular intervals in a zone of two phases of centuriation to the east of Luceria; each farm seems to have controlled over 80 or 90 *iugera* (20+ ha) of land. The centuriation and associated buildings were first identified from aerial photographs. Black gloss sherds and the pump date the farm, and hence the second-phase centuriation, to the later second century BC. Aerial photographs show that trenches for vines, at roughly 2.15 m intervals, and pits for olive trees covered about two-thirds of the centuriated area, and a bulldozer revealed examples close to the excavated farmhouse. Bones from the excavation were mainly sheep/goat with some pig; the few cattle/oxen bones probably indicate use of a working animal or two. The pump will have been useful for supplying the animals with water in the dry environment. A second beaten earth floor was later laid over a thick deposit of household rubbish which blocked the Ctesibian pump, and a covered extension was added on one side of the farmstead. Subsequently a pair of tanks for settling olive oil from pressing were cut into the floor. The farm fell into disuse in the early first century AD, although some late first-century sherds were found outside. It seems that this was a colonial farm set up by the Gracchan commission around the 120s BC; it was semi-abandoned, then remodelled for habitation; later it became an oil-pressing facility for a farmer, small or large, who lived elsewhere, and finally a derelict shed. Unanswerable questions are where and how in the earlier phases the grapes and olives were pressed: somewhere else on the farm, or in a specialised facility run by the local farmers or a third party, perhaps linked to the itinerant merchants who bought up surpluses of grain, wine, and oil in Apulia and Calabria which they carried on donkeys to ports for maritime distribution?<sup>29</sup>

Slightly larger than the Nocelli farmhouse, but of cruder and presumably private build, is the site 9 farmstead in the territory of Luna in northern coastal Etruria.<sup>30</sup> It was one of six findspots of similar size and date located by a field survey on the hill slopes above the town. The typical scatter was of around 1,000 to 1,500 m<sup>2</sup> and included one

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<sup>29</sup> Var. *R.* 2.6.5.

<sup>30</sup> Delano Smith et al. (1986, 100–3 [survey]; 109–18: [excavation]).

fineware and five to ten coarseware sherds and some tile fragments; repeat visits in successive seasons were necessary to retrieve a “reasonable sample”. Findspot 9 itself produced one terra sigillata and, unusually, sixty coarseware sherds, three amphora fragments and some brick and tile. It was partly excavated in 1979–81 deliberately to see what a ‘small site’ really was. Under the levelling for the excavated building were exiguous traces of second-century occupation, perhaps from one of the 6.5 *iugera* farms of the Roman colony of 177 BC.<sup>31</sup> The later farmstead was of irregular rectangular plan, some 9–12 by 15+(?) m (150+ m<sup>2</sup>). A row of domestic rooms, perhaps with a second storey, was flanked by a part-roofed court for stabling animals and storage, with a lean-to covering a water dolium. The walls had rough stone footings, packed with rubble and clay, and mudbrick uppers. The floors were all of beaten earth. The “considerable” excavated pottery (not published) included black gloss sherds and was mostly of the later(?) second and first centuries BC; there is nothing later than the mid-first century AD. In the absence of archaeological clues we can say only that the location suggests polyculture including vines and olives (a press was identified at findspot 39), and that the possible stable and the size and plainness of the farmstead hint at a middling family farm of say 20 *iugera* (5 ha), perhaps rather more, whose parameters of occupation stretch over a century.

I know of no other excavated and published Roman farmsteads of less than 200 m<sup>2</sup>, but there are plenty with groundplans in the range of 400 to 600 m<sup>2</sup>. I give six examples, enough to illustrate the similarities and differences. I start with the Villa Sambuco near Blera in south Etruria, which was identified through visible masonry, and excavated in 1958–60.<sup>32</sup> It had a rectangular plan of 22 by 24 m (530 m<sup>2</sup>) laid out around a central corridor-court, with a second storey on one long side. The timber-frame and mudbrick walls rested on three fine courses of tufa blocks from a local quarry. This was a new build of the earlier to mid-second century BC which was destroyed by fire and abandoned in the mid-first century AD. The rooms were all plain: the ground floor included a stable and storerooms; bedrooms must have been upstairs, but the owners may have lived in town. Finds were “not especially numerous”, mostly coarsewares, also amphora fragments and loom-

<sup>31</sup> See n.10 above.

<sup>32</sup> Östenberg (1962).

weights, one denarius, and iron nails (from the staircase). This looks like a modest family farm, possibly colonial in origin, which engaged in polyculture. Although the building is over three times larger than the Luna farmstead, it did not necessarily have a commensurately larger farm, which at a guess may have been in the region of 20 to 40 *iugera* (5 to 10 ha).

My second example, Giardino Vecchio, is near Cosa, in southern coastal Etruria, in the area centuriated for the Latin colony of 273 BC.<sup>33</sup> It was identified by survey in 1978 as findspot C19, a 20 m diameter scatter of tile and pottery dated to the third to second centuries BC, with sporadic outlying material, and was part-excavated in 1981–2. Excavation revealed that it was built in the earlier second century on a new site in a rustic style similar to the Luna farmstead, and so was probably a private construction which may or may not have been related to the resettlement of Cosa in 197 BC. However, at around 20 by 24 m with up to twenty ‘rooms’ plus lean-to, it was three times as large (500+ m<sup>2</sup>) as the Luna farm, and it had far superior fittings. Two of the living rooms had floors of *opus signinum* (crushed and polished tile) and plain wall-plaster. There was a kitchen with an oven, a room with many loom-weights (which were common across the site), a room with a lever-press for wine, a lean-to stable, and possibly some second-storey rooms. Finds (not published) included black gloss and terra sigillata sherds, amphorae fragments, and several bronze and two silver coins. The site was abandoned in the late first century BC. This again looks to be a family farm, perhaps with a few slaves, practising polyculture, with some regular surplus production of wine and perhaps other crops, and also perhaps a significant textile output.<sup>34</sup> At a pure guess, I would put its farm in the range of 20 to 50 *iugera* (8 to 13 ha).

My third case is the farmstead of Posta Crusta in the territory of Herdonia in northern Apulia, which was excavated in 1972–4.<sup>35</sup> It had been located in 1969 by field survey using earlier aerial photographs in an area which had been centuriated with square 200 *iugera* blocks (each possibly divided into allotments of 67 *iugera*); the centuriation is said to have been Gracchan, later reused by Julius Caesar or Octavian to

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<sup>33</sup> Attolini et al. (1982, 383–5; 1983, 462–3); cf. Carandini & Cambi (2002, 142–3).

<sup>34</sup> Roth, U. (2007, 53–87) rightly complains that we underestimate the economic importance of rural textile production.

<sup>35</sup> De Boe (1975); redated by Volpe (1990, 111).

settle veterans.<sup>36</sup> Over traces of a smallish building, probably an original colonial farmstead, a new farmstead was built in the second to first century BC. It was of similar rectangular plan to the Villa Sambuco near Blera, 21 by 19 m (400 m<sup>2</sup>), laid out round a central corridor-court, with well built rubble walls, and possibly some second-storey rooms.<sup>37</sup> Two of the four or five domestic rooms had *opus signinum* floors with mosaic and *opus sectile* inserts. There were purpose-built rooms for the production of olive oil: a milling room floored with *opus spiccatum* (herring-bone pattern tiles), a press room floored with *opus signinum*, and a storeroom with a settling vat and dolia (number and size uncertain). It was abandoned in the second century AD, and the site was reused in the third century for a massive new farmstead. The second phase building looks like another family farm, perhaps using some slave labour, which produced olive oil as a specialised cash crop. The fine fittings imply a larger farm than the previous cases, perhaps comparable to the hypothetical Gracchan allotments of 67 *iugera* (17 ha).

My next three examples all come from the territory of Pompeii, from which we have the largest local set of excavated rural ‘villas’, even if the manner of clearance of those unearthed in the eighteenth to early twentieth centuries leaves us unclear about many points, including their occupation chronology (except that they have a common terminus ante quem of August AD 79).<sup>38</sup> First, the farm-cum-inn at Boscoreale Stazione, excavated in 1903.<sup>39</sup> This fascinating farmstead, of irregular plan, had a main agglomeration of rooms of up to 21 m by over 20 m (it was not fully cleared) and a stable wing of 20 by 6 m, making a probable total groundplan of close to 600 m<sup>2</sup>. It fronted a road north from Pompeii, perhaps to Nola. The walls were of mortared rubble, it seems, plainly plastered inside, and most floors of beaten earth. In the centre was a substantial kitchen with a large oven, a cooking-range, and an enclosed pantry. A connecting room contained a conical hopper grain-mill (possibly donkey-powered) and a sheep or goat in a pen, presumably for milk. Also adjacent was a large room in which a scatter of jewellery and other personal objects was found, probably the

<sup>36</sup> Volpe (1990, 50–1), with *Lih. Col.* 164.27–9, 200.27–8 (Campbell).

<sup>37</sup> Rossiter (1978, 10) suggests Greek influence for this building type, comparing the South Villa at Olynthus, but it is a very simple functional plan.

<sup>38</sup> Carrington (1931) remains the most readable introduction. I use the conventional numbering based on Rostovtzeff (1957, 151–3), as updated by Kockel (1985, 534 and fold-out).

<sup>39</sup> No. 28. Della Corte (1921, 436–42); cf. Oettel (1996, 41, 83, 231–2).

family living room. A couple of smaller rooms were perhaps bedrooms. Another room housed a lever-press for wine, and there was a large storage area containing dolia; empty amphorae were stacked in the kitchen-garden and the granary, which was under repair at the time. Skeletons and a graffito show that numerous chickens were kept. The large stable, which had a mezzanine floor, was also used to store hay and timber. The lean-to porch at the front entrance had benches and a hearth, and its pilaster bore graffiti such as 'Cerdo to Cerdos, cheers'.<sup>40</sup> The first room indoors was also for hospitality. Travellers who stayed at this rural inn could have slept on the mezzanine in the stable above their animals. The house had been hastily cleared before destruction, but the extraordinary miscellany of objects left, forgotten, or dropped deserves fuller study: it included figurines of deities, bronze and silver coins, jewellery, iron tools, bronze vessels, coarse and terra sigillata wares, lamps, and glass vessels. This looks like a polycultural farm of some size, perhaps 30 to 50 *iugera* (8 to 13 ha), run by a resident family who processed and sold its varied surpluses on site to the clients of their inn. The residual small finds point to a monetised business and a fair level of prosperity.

My fifth case is the farmstead at Villa Regina (Boscoreale), one km north-west of the Villa of the Mysteries, which was uncovered in 1978–83.<sup>41</sup> Occupation of the site apparently went back to the late fourth to early third century BC, but the earliest identified building is a two- or three-room farm of irregular plan, roughly 20 by 12 m (240 m<sup>2</sup>), perhaps of the Sullan colonisation of around 80 BC. This was extensively remodelled and expanded in the Augustan period into an irregular complex of up to 20 by 24 m (450 m<sup>2</sup>). The new farmstead, although crudely built, was laid out around a porticoed court and included a room equipped for a lever-press for wine, a wine store with eighteen embedded dolia of 1 *culleus* capacity (517 l) totalling almost 100 hl, a stable/barn, threshing floor, kitchen, dining room, and a few other living and storage rooms with hints of a second storey above them. The dining room was decorated with Third/Fourth-Style wallpaintings of the mid first century AD, perhaps when the building was again slightly modified and enlarged. In its last phase before AD 79 it seems to have become the subsidiary building of a larger farm: the hearth in

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<sup>40</sup> *CIL* IV 6869.

<sup>41</sup> No. 57. De Caro (1994).

the kitchen had long been out of use, and eighty-three curved rooftiles were stacked against a wall of the dining room. One storeroom had shelves bearing pottery and iron nails. Total finds included seventy-seven pieces of terra sigillata, lamps, and a few objects of iron, bronze, and glass; thirty-six black gloss sherds were found in the vineyard. Traces of pits around the farm attest various fruit trees and a hoed vineyard with a mixture of 20- and 3-year-old vines trained on trees and stakes at 1.2 m intervals. Wheat, pork, and sheep/goat were consumed and produced, while remains of two cartwheels imply the keeping of an ox or two. Graffiti of unknown date attest three slave names: *Masculus*, *Hilarus*, *Secundus*. The excavators propose a vineyard of 2 to 7 *iugera* and a couple of *iugera* of arable to meet domestic consumption. This seems far too small to me, given the oxen and slaves. I would suggest some 10 *iugera* of vines and 20 or more *iugera* of arable, say 30 *iugera* (8 ha) in total.<sup>42</sup> In its Augustan phase this was probably the farm of a resident family with a few slaves, but could have been a unit of a larger holding; conversely, in its last phase it was probably part of a large estate but could have still been the single or main farm of a family resident elsewhere, perhaps in town. I merely observe that either is possible.

My last example is a farmstead at Boscoreale Giuliana, excavated in 1904.<sup>43</sup> The core of the building seems to represent an earlier small farmstead, perhaps colonial, of 10 by 18 m (180 m<sup>2</sup>), only slightly larger than the Nocelli and Luna farms. At some point it was expanded to become a very plain building of some 16 by 33 m (530 m<sup>2</sup>) which had rubble and mortar walls, crudely repaired after the earthquake of AD 62, and beaten earth floors. The entrance court, which had a hand-mill and oven and adjacent latrine, was littered with amphora fragments. Of the four identical rooms to the right, one was used as a dining-cum-living room, the others perhaps as stores. There may have been bedrooms on the second storey above the old building. There was a barn full of hay, and chestnut stakes were stacked in the stable. Another room contained a large olive-mill. The mill attests production of olive oil as

<sup>42</sup> This is not the place for a debate about Roman wine yields. I base my estimates on Col. 3.3.7, 11: the normal range was 1 to 2 *cullei* per *iugerum*, though he aimed for 3 *cullei*, equivalent to 20, 40, and 60 hl/ha respectively. The vineyard was semi-intensive, so I use 2 *cullei* per *iugerum* here. For what it is worth, a farm of 30 *iugera* easily fits into the the location map of De Caro (1994, tav. A).

<sup>43</sup> No. 26. Della Corte (1921, 423–6); cf. Oettel (1996, 40, 84–5, 227).

a cash crop, while the stakes point to vines. The apparent absence of press-fittings is curious; possibly pressing took place at another unit of the estate or a moveable screw-press was used. In any case the overall impression is of a slave-staffed unit of a larger estate. The size of the farm was probably medium, say around 30 *iugera* (8 ha).

Lastly, to illustrate the overall range and variety of farmsteads, and to clarify which sites are clearly not peasant farmsteads, I briefly review four excavated large sites. First, the Selvasecca farmstead also near Blera in south Etruria, only 2 km away from the Villa Sambuco.<sup>44</sup> This substantial timber-frame and mudbrick building of 36 m square (1,300 m<sup>2</sup>) was laid out around a peristyle court with paving and Doric columns and capitals of stone. Some domestic rooms had mosaic floors and painted walls. There may have been a lever-press for wine. Two concrete cisterns for water were added nearby. In one or more stages several rooms were divided by partition walls of rubble and concrete, and the domestic rooms fell into severe disrepair and became littered with antique architectural terracottas, including a few moulds. Pottery and coins date occupation from the second century BC to the fourth century AD, according to the excavators, but it has since been suggested that the terracottas belonged to the original construction and date it to the fourth or even fifth century BC, which would make it, along with the Auditorium villa near Rome, one of the earliest examples of an Italian farmstead with large, separate, and fine living quarters.<sup>45</sup> In any case, the amenities suggest a prosperous resident owner who probably possessed other farms in the area, and was what we might call a 'gentleman farmer', one of the local elite.

Somewhat smaller (around 1,000 m<sup>2</sup>) but no less fine is the Pompeian villa at Boscoreale Pisanella excavated in 1876–7 and 1894–6, most famous for the 'treasure' hidden there.<sup>46</sup> Its design is a masterpiece of compact efficiency. There were two facing lever-presses for wine with gravity feed to the embedded dolia in the adjacent courtyard, and a mill and another lever-press for olive oil. Some arable farming was practised

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<sup>44</sup> Berggren & Andren (1969).

<sup>45</sup> Terrenato (2001, 22–3). It is not clear to me whether the architectural terracottas (and moulds?) came from collapse of the building or were dumped there from somewhere else. The stone blocks interpreted by the excavators as moulds for tile-making look like bases for press uprights; cf. Rositter (1978, 11).

<sup>46</sup> No. 13. Pasqui (1897); cf. Oettel (1996, 16–18, 183–92, 263–5).

(threshing floor, bean straw, sickles); there were draught oxen (wagon parts), and pigs and chickens were kept and eaten. The three horse skeletons in the entrance court may relate to refugees from Pompeii taking shelter (and who maybe hid the treasure?). There was a small but fine ground-floor suite for the manager (*uilicus*), plain rooms for the slaves who cooked for themselves (and so were unchained), while upstairs there were elegant rooms for visits by the owner's family. This farmstead was evidently purpose-built as the centre of an intensively farmed slave-staffed estate. The seventy-two dolia for wine of 2 *culleus* capacity suggest a vineyard of about 50 *iugera*; allowing another 50 *iugera* of olives and arable gives a guesstimate total of 100 *iugera* (25 ha).

Comparable in agricultural type but on a completely different scale is the well-known villa of Settefinestre, near Cosa in coastal south Etruria.<sup>47</sup> Built around 40 BC, it had a square central complex of 3,600 m<sup>2</sup> of which luxurious living quarters occupied a third, with more work-space in the cryptoporticus and a huge separate granary (500 m<sup>2</sup>). There were three lever-presses and perhaps over eighty 2 *culleus* embedded dolia for wine, and a mill and lever-press for olive-oil. There were rows of small rooms for storage and the slave workers. Around the turn from the first to second century AD the production of wine and oil stopped and a large separate piggery was built. Including the adjacent orchard and gardens and assuming moderately intensive viticulture, an estate of about 200 to 250 *iugera* (50 to 60 ha) is likely.

Lastly, by way of contrast, comes the Pompeian farmstead at Gragnano Carità.<sup>48</sup> Laid out around a large porticoed courtyard, this square complex of over 2,000 m<sup>2</sup> was half the ground size of Settefinestre, although it had some second-storey rooms and perhaps some outbuildings too, but it was a plain and purely functional building built of mortared rubble. A single wine-press and area with embedded dolia attest some wine production. One side of the courtyard was occupied by a spacious double-width stable in which "numerous" skeletons of cattle/oxen and horses were found, which indicate large-scale cultivation of grain and fodder. The use of some twenty to thirty chain-gang slaves is implied by a secure internal courtyard with one entrance containing eleven small rooms (an *ergastulum*), an adjacent bakery-kitchen

<sup>47</sup> Carandini et al. (1985).

<sup>48</sup> No. 34. Della Corte (1923); cf. Oettel (1996, 40–1, 75–6, 228–30).

for making their ready prepared meals, and a fourteen-slot leg-iron, presumably for punishment. The associated farm must have been of several hundred *iugera*, probably farmed extensively and deserving the term *latifundium*.

#### IV. *Observations*

A brief survey like this of some of the written evidence and a small selection of the archaeological data for smaller farms and farmsteads in mid- to late-republican Italy is likely to produce more questions than conclusions. However, it does show that an analytic approach to field survey results which rests on a basic distinction between small and large findspots or sites, interpreted as farms and villas, is flawed. The written sources and excavations reveal a broad spectrum in size of farmsteads, with much variety of fittings and use of space, and a wide range in size of farms, and that the relationship of building size to farm size is not always direct.

A fundamental issue is how to define a 'peasant' farm. The Romans had words such as *rusticus*, *agrestis*, and *paganus* which approximate to the economic and cultural connotations of 'peasant', but are equally imprecise, although their system of *classes* may offer some parameters. Here I make a first stab at a working definition, inevitably in the tradition of Humpty Dumpty. The written and archaeological evidence shows that not all Roman peasants were subsistence smallholders. At the bottom end I would include families with holdings below subsistence level or no land at all as long as they made their living primarily as agricultural tenants or labourers (i.e. excluding craftsmen and the urban poor). The reduction of the minimum *census* in 140 BC had recognised all but the landless as *assidui*, and recruitment long before Marius in 107 BC had included the landless. More problematic is the upper boundary where farm size and lifestyle took a family beyond peasant status. Attested state allotments to ordinary settlers did not normally exceed 50 *iugera* (12.5 ha). This quantity of land, with the concomitants of use of draught animals and some slave labour or tenants, and regular production of a marketable surplus, was not incompatible with a peasant lifestyle, as the pseudo-Vergilian *Simulus* illustrates. Some Gracchan settlers in Apulia apparently received allotments of up to 80 or 90 *iugera* (20+ ha), but this probably reflects the more extensive nature of agriculture in this

semi-arid region.<sup>49</sup> I would set the extreme upper limit at 100 *iugera*, the minimum qualification in the Republic for membership of the elite *prima classis* and for town councillors in the Principate. On a farm of between 50 and 100 *iugera* lifestyle was the decider: as long as some members of the family regularly worked on the land, in my view it was still a peasant farm.<sup>50</sup> This working definition is necessarily crude and vague at both ends, but so too was the historical reality.

The selection of farmsteads discussed above suggests a working categorisation for further research into small farmhouses of not normally above 250 m<sup>2</sup> groundplan, medium farmsteads generally of around 400 m<sup>2</sup> to 600 m<sup>2</sup>, and then a range of large buildings of over 1,000 m<sup>2</sup> whose varied typology is not my concern here. Further investigation would doubtless reveal farmsteads with groundplans of between 250 and 400 m<sup>2</sup>, and also between 600 and 1,000 m<sup>2</sup>, but perhaps not many. I note that the presumed original building at Boscoreale Giuliana had a groundplan of 180 m<sup>2</sup> and the predecessor of Villa Regina one of 240 m<sup>2</sup>; most other excavated sites I know, other than large villas, fall in the bracket of 400 to 600 m<sup>2</sup>. Most of the farm sizes attributed to the nine small to medium farmsteads are my guesstimates, but while the smallest farmstead (Monte Forco) fairly certainly had the smallest farm (10 *iugera*), the second smallest (Nocelli) had the largest farm (80 to 90 *iugera*), albeit in a region (Apulia) of extensive farming. For what it is worth, my guesstimates for the others, except Posta Crusta, another Apulian farm, all lie in the range of 20 to 50 *iugera*. Thus any one farm might have been twice the size of another while the farmsteads were similar; but equally, the smaller farm may have been more intensively and productively worked. We can only recognise the wide range of possibilities. However, we can say that this medium category of farmstead, large and substantial enough for Romans to call a *villa*, comprised both prosperous peasant farms, often with some use of slave labour, and units of slave-staffed estates, normally with a non-resident owner, themselves subdivisible into various types. Furthermore, a large estate, at least before the emergence in the later first century BC of 'super-villas' like Settefinestre and Gragnano Carità, will have been made up

<sup>49</sup> For example the Gracchan vine trenches around Luceria are spaced over 2 m apart, twice the normal planting distance in Pompeian vineyards.

<sup>50</sup> I would exclude the idealised image of the elder Cato as a young man (*ORF* fr. 28; *Plu. Cat. Ma.* 3.2), Roscius of Ameria acting as steward on his father's farms (*Cic. S. Rosc.* 18 etc.), and so on.

of a number of such medium units, like the thirteen farms at Ameria of the elder Sex. Roscius; of course some medium slave-staffed farms may have been the only property of a local notable. I find it unhelpful to call these rural buildings 'Hellenistic' farmsteads and to seek the origin of the slave-staffed villa elsewhere.<sup>51</sup> Even if we reserve the term 'villa' for the really large estates, Boscoreale Pisanella is an extrapolation from Villa Giuliana, and Settefinestre from Pisanella. There is no missing link. Even Gagnano Carità is only a variation on the theme. But here my concern is the peasant farms.

Granted the considerable written evidence for the small size of farms typical of citizen allotment schemes, both colonial and virgane, it is striking that so few small farmsteads of under 250 m<sup>2</sup> or even 400 m<sup>2</sup> groundplan have been excavated (and published). That they did exist is evident from the few excavated examples like Monte Forco and Nocelli and the evidence of field survey and aerial photographs for large numbers of similar unexcavated sites, especially in south Etruria and northern Apulia. In part this is because there has been no archaeological project, to my knowledge, specifically to investigate a selection of small sites in one area or across a number of areas. Of the six non-Pompeian small and medium farmsteads discussed above, all except Villa Sambuco were excavated deliberately to add detail to a regional survey. Small sites on their own normally do not attract research or rescue excavation. Another factor, probably less important, is that small farmhouses were sometimes expanded or built over, as at Luna, Posta Crusta, Villa Regina, and Boscoreale Giuliana, leaving little evidence for their occupation and use. However, I would suggest that the most important reason is that most small Roman farmsteads were too flimsy and materially poor to leave much of an identifiable archaeological trace. As a negative suggestion this is difficult to demonstrate, but there are pointers towards it.

Two of the three excavated small farmsteads, Nocelli and Monte Forco, were exceptionally well built, even when compared with the medium farmsteads discussed above, presumably because they were built by the state for settlers in virgane schemes of the late second and

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<sup>51</sup> For example Terrenato (2001). Nor do I find the concept of a 'Catonian' villa helpful; the miscellaneous agricultural compendium under Cato's name contains modules and advice for several different types of farm, not a coherent set of prescriptions. To me Selvasecca and the Auditorium site near Rome are residential villas with limited farming facilities (none in some phases).

later first century BC respectively.<sup>52</sup> As noted above, the unexcavated small buildings recorded by early field surveys in south Etruria and identifiable in aerial photographs of northern Apulia all seem to relate to settlement schemes of this period. Conversely, while the state-imposed centuriation grids of colonies of the fourth to earlier second centuries BC have often marked the landscape to the present day, traces of colonial farmsteads of that period are extremely rare. In the well surveyed Ager Cosanus, for example, some residences in the small town (only 13 ha) and some village agglomerations in the outlying hillier areas are insufficient to explain the general dearth of findspots with third-century material from a guesstimated 4,000 colonists settled on 16 *iugera* allotments in 273 BC.<sup>53</sup> The simplest explanation is that state provision of homes for settlers, using dressed masonry or concrete, began with the Gracchan scheme (whose costs required the revenues of the new rich province of Asia), while farmhouses of similar size constructed privately, including those of settlers in the middle Republic, were normally a hut (*tugurium*) built largely of perishable materials on thin rubble footings, sometimes with a tiled roof. Such buildings, if their remains survived intervening agricultural activity (and reuse of the tiles) through to modern ploughing, would leave little building material to register as a findspot in a field survey.

The material culture of poorer peasant farms also seems to have been slight. While medium farmsteads like Boscoreale Stazione and Villa Regina caught in the eruption of Vesuvius attest use of a wide range of ceramic and metal objects, three or four of the six non-Pompeian small and medium farmsteads produced some coarseware and a few fineware sherds and nothing much else (Monte Forco, Nocelli, Villa Sambuco, perhaps Posta Crusta).<sup>54</sup> Of course part of the explanation is regular cleaning, including before abandonment. Most sherds on non-Pompeian sites come from rubbish tips, sometimes reused as make-up for a new floor (Monte Forco, Nocelli) or dispersed by ancient manuring (the black gloss in the Villa Regina vineyard, perhaps the later terra sigillata at Nocelli) or by modern ploughing. Nevertheless the small quantities of

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<sup>52</sup> Compare, for instance, the standard state-built farmsteads of the mid-twentieth century *bonificazione* of the Maremma (which I saw when I slaved at Settefinestre).

<sup>53</sup> Carandini & Cambi (2002, 105–8, 121–3, 137–40); cf. Attolini et al. (1982, 369–70): none at all in the Valle d'Oro, the central and fertile valley.

<sup>54</sup> Giardino Vecchio produced many loomweights, but the quantity and type of pottery excavated, as at the Luna farm, is not specified. In fact pottery catalogues are only given for Monte Forco and Villa Regina.

discarded broken items must still imply limited possession of finewares. Field survey data tell the same story. The re-examination of the south Etruria material by the Tiber Valley Project has dealt in total with some 7,000 black gloss sherds from 1,137 findspots, an average of just over six sherds per findspot, implying just a couple per 'small' findspot.<sup>55</sup> The quantity of material must also in part (there are other factors) reflect length and continuity of occupation. The six non-Pompeian small and medium farmsteads discussed above had occupation spans of roughly 150 to 250 years, but the lack of discernible and potentially dateable occupation layers (cleaning again) makes it impossible to assess continuity of occupation within those spans. Monte Forco and Nocelli, the two smallest farmsteads, both were restructured twice, which may indicate significant breaks in occupation. Posta Crusta was built over an apparently short-lived predecessor, and Villa Regina was an enlargement of a relatively recent new build on a previously occupied site. For what it is worth, field survey data shows variable but sometimes considerable discontinuity in sites producing material across a major ceramic watershed (e.g. from late black gloss to early terra sigillata).<sup>56</sup> Direct archaeological evidence for discontinuities in the occupation of farmsteads is difficult to find, but that does not make it safe to assume continuity, which is itself difficult to demonstrate. The written evidence for the instability of settlers and the generally low quantity of material finds suggest to me that discontinuity in the occupation of small farmsteads was common.

Finally comes the question of the interpretation of field survey data. All survey reports begin with an admission of the problems of classifying findspots. Nonetheless, insofar as the usual binary classification is robust, the excavated sites discussed here would suggest as a general rule that large and rich scatters are probably indicative of substantial villa buildings (over 1,000 m<sup>2</sup> groundplan), and small less rich scatters of medium farmsteads (around 400 to 600 m<sup>2</sup> groundplan), whether prosperous peasant farms or modest slave-staffed villas. Some large scatters may in fact relate to medium farmsteads, and a certain

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<sup>55</sup> Patterson et al. (2004, 13–17); Rob Witcher gave me the number of findspots. Although it has been suggested that sherds were kept selectively, Jeremy Paterson recalls full collection of finewares in the Ager Faliscus, and the experience of later surveys in the Ager Lunensis and Ager Cosanus was similar (see section 3).

<sup>56</sup> For example see the diagrams in Patterson et al. (2004, 10 fig. 3); Potter (1992, 648 fig. 9).

percentage of small scatters to small farmsteads (mostly less than 250 m<sup>2</sup> groundplan), especially those of late-republican settlement schemes which were built with dressed masonry or concrete and rubble faced with *opus reticulatum*. Small farmsteads as a whole are likely to be drastically under-represented by survey findspots because of their relative simplicity of construction, material poverty of contents, and transience of occupation. In more recent surveys their slight traces may figure as off-site material, although in surveys not so long after the introduction of deep ploughing they seem to have been more visible as discrete findspots, like the 'hut' category of the south Etruria surveys.<sup>57</sup> While some practitioners of field survey assert the scientific repeatability of the data collection, the more general view is that repeated deep ploughing over decades degrades and disperses the material of poorer sites in particular.<sup>58</sup>

The bar chart summarising the provisional results of the Tiber Valley Project provides a convenient case for illustration of some other problems of interpretation of field survey data.<sup>59</sup> Note that the figures below are all my estimates derived from the bar chart which I use for methodological illustration. For the real figures we must await the full publication. I focus on the slump in the total of findspots producing material, mainly black gloss sherds, of the Late Republican 1 phase (250–150 BC), down to around 200, compared to the 500 plus findspots with Middle Republican material (350–250) and over 550 with Late Republican 2 material (150–30 BC). Note too that over 250 findspots produced only Generic Republican material (350–30) and another 250 and more only Generic Late Republican material (250–30 BC). Also there were almost 1,500 findspots with Early Imperial material (50 BC–AD 100). In cases like this, as others have noted, the generically dated findspots need to be distributed proportionately among the specific period totals, and the latter need to be weighted to smooth out differences in their chronological spans. Thus distributing the generic

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<sup>57</sup> Witcher (2006, 97) suggests that Potter's 'huts' would today be called off-site material, but still in the 1970s to 1980s in the Ager Lunensis and Ager Cosanus very small discrete findspots were identifiable as well as off-site material, which in those days perhaps was rather ignored.

<sup>58</sup> E.g. Barker (1995, 48–51); *contra* Patterson (2006, 15) citing work at Cures Sabini and Metapontum. The phenomenon is the implicit background to Potter (1992).

<sup>59</sup> Patterson et al. (2004, 10 fig. 3). These problems are variously noted in survey reports; for a synthesis see Patterson (2006, 9–24); for some ingenious, if not entirely convincing, responses to the problems see Ikeguchi (1999/2000, esp. 12–13).

totals to the basic totals of 500–200–550 findspots increases them to 600–307–843; weighting the Late Republican 2 total for its longer span (100/120 years) reduces it to 703.<sup>60</sup> Expressed as percentages of the grand total of findspots in all three periods, the distribution pattern of the data is adjusted from 40%–16%–44% to 37%–19%–44%, which shows that the basic period totals exaggerated the slump in the Late Republican 1 period.

These adjusted totals are still not entirely satisfactory, because no allowance is made for turnover, which is tacitly assumed to be constant across all periods. They are an indicator only of the known (sample) number of sites which were occupied at some point or points in the chronological span of the period, whereas to compare the overall levels of settlement in different periods we would need to multiply the number of sites for each period by their average number of years of occupation, whether continuous or discontinuous. For this the only possible clue is the quantity of sherds found per findspot, although, as we will see, this is also a product of the supply of the diagnostic material. To give a hypothetical example, if 100 findspots have material of century A and 100 of century B, and if the average length of occupation of sites in century B was 20% shorter than in century A, the overall known (sample) level of settlement would in fact have fallen by 20%. Of course when the findspot data are presented according to category (e.g. small and large), we would also want to differentiate average turnover according to category; for instance we might have grounds to believe that small sites had a higher rate of turnover than large sites.

The above refinements still do not address the often unmentioned but fundamental issue that the sherds recorded by field surveys are direct evidence for the rural distribution (i.e. supply and acquisition) of ceramic wares and only partial and potential evidence for settlement patterns.<sup>61</sup> Field survey data would be better represented by a diagram which showed the number of sherds found per ceramic period, and per category of findspot, as well as the number of findspots. In the case of south Etruria we would then see an even more dramatic slump in both Late Republican periods in the distribution of black gloss wares.

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<sup>60</sup> Note that the period spans used here are rather notional and may have varied somewhat between regions.

<sup>61</sup> Noted by Potter (1979, 18); Patterson et al. (2004, 16). Millett (1991) remains a rare case of systematic evaluation of a set of survey data with reference to varying pottery supply.

Precise figures are not yet available but, for illustration, if 80% of the 7,000 black gloss sherds recovered are Middle Republican, then the average findspot of 350–250 BC produced eleven black gloss sherds, whereas the average Late Republican findspot of 250–50 BC produced only two sherds.<sup>62</sup> Of course distribution may also have been different between different categories of site; for example, small sites may have been less able or willing to acquire a particular type of pottery. Although varying rates of site turnover will also have influenced the quantities of material, the primary explanation must lie in ceramic history, that is in the varying nature and quantity of production, distribution, and use of each ware. There is much to be clarified before we dare to extrapolate from the ceramic data to settlement patterns, let alone move on to demographic arguments.

As a provocation, I offer an alternative south Etrurian story. Throughout the Republic the typical peasant farmstead was so poor that it left little archaeological trace. Around 350–250 BC larger farmsteads of prosperous peasants and early ‘villas’ emerged in which considerable regionally produced black gloss pottery was used and which had quite long periods of occupation. From 250 BC onwards the inhabitants of these farmsteads preferred and could now afford metal and glass tablewares; black gloss production became more centralised and directed at overseas markets. The settlement schemes of the Gracchan to triumviral phases created massive new cohorts of smallholders with solid farmhouses but restricted means who revived the rural market for ceramic finewares. The paucity of average black gloss sherds in the late Republic reflects the initial instability of these settlers. The wider and denser distribution of early terra sigillata reflects growing stability of settlement under the early Principate, and suggests that early terra sigillata, especially if decorated, was a more generally acceptable alternative to glass and metal tablewares than late black gloss had been.

Ongoing research into the pottery types, especially coarsewares, which constitute most of the potentially diagnostic finds, will continue to make incremental improvements to our interpretation of field survey data.<sup>63</sup>

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<sup>62</sup> Patterson et al. (2004, 16). 80% of 7,000 sherds = 5,600, divided by 500 findspots = 11.2 sherds; 20% = 1,400, divided by 620 findspots (200 + 550 – 130 continuing findspots) = 2.3 sherds.

<sup>63</sup> Roth R. (2007) illustrates the potential of detailed regional studies for understanding better the complex relationships between production and consumption of finewares, whatever the value of his wrapping of ideological assertions.

Even more useful would be one or more projects to excavate at least a dozen small farmsteads with particular attention to the relation of surface traces to remains below ground, the type, quantity, and location of small finds excavated, the apparent continuity or discontinuity of occupation, and the faunal and botanical evidence for agricultural practices. Excavations in zones where written evidence, the topography (as at Monte Forco), or centuriation give external indications of the size (and number) of farms would be especially useful. The aim would be to produce a proper, solidly grounded classification of types of small farmstead and farm.<sup>64</sup> There are inevitably some questions to which archaeology will never be able to give an answer, or a full answer, such as the number and status of occupants or workers, and the changing extent of tenancy, a crucial topic in the agrarian history of Roman Italy, which may have grown enormously in the early Principate.<sup>65</sup> Also instructive would be to compare field survey results and small farms excavated or documented in the Greek and Hellenistic worlds, such as the Boiotia survey, the Black Sea colonial farms, and landholdings in Ptolemaic Egypt. Some preliminary reading has given me the impression that Greek and Hellenistic smallholdings and farmsteads were typically larger than Roman citizen allotments and farmhouses, and that field surveys record higher densities of findspots, both of which imply that Roman peasants were significantly poorer than their contemporary counterparts in the Greek and Hellenistic worlds. As noted before, the Romans clearly knew that their citizen allotments were small, and deliberately kept them small, partly to force sons into military service, and also recruit them to colonies with Latin status, and partly, as I will argue another time, to restrict the voting impact in the *comitia centuriata* of the beneficiaries of the land schemes by granting allotments which would confine them to the bottom (fifth) *classis*. Lastly, I suggest that we should not imagine the peasantry of Roman Italy as an inert, autarkic, and undifferentiated mass, on the analogy, for instance, of Carlo Levi's negatively static view of the southern Italian peasantry, which was curiously influential on British scholars in the later twentieth century. Instead our prospect should be of a diverse group, in complex and constantly

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<sup>64</sup> This might, incidentally, help to answer the question of how many 'hut'-sites represent the outbuildings of slave-staffed villas (cf. n.4), a possibility which Ikeguchi (1999/2000, 11) stretches to an implausible extreme: "a large proportion", in fact most.

<sup>65</sup> As argued by De Neeve (1984).

changing circumstances, some very prosperous, most precariously close to subsistence, dependent as a group for demographic and economic survival, let alone growth, on external resources such as rural tenancy and labouring, military service, urban employment, and state allocation of new farms to the young and landless, for which a new model might be Macfarlane's mobile, opportunistic, and individualist peasants of later medieval England.<sup>66</sup>

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<sup>66</sup> Levi (1945); Macfarlane (1979).

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SETTLEMENT ORGANIZATION AND LAND  
DISTRIBUTION IN LATIN COLONIES BEFORE  
THE SECOND PUNIC WAR<sup>1</sup>

Jeremia Pelgrom

I. *Introduction*

The foundation of a colony on recently conquered soil must have had an enormous impact on the indigenous landscape and society. Unfortunately, no contemporary reports of the mid-republican colonizing enterprise are transmitted, and such episodes must be reconstructed from scraps of information in the later sources and from archaeology. Several such attempts have been made, most notably Salmon's *Roman Colonization under the Republic*,<sup>2</sup> based largely on the written sources, and Frank Brown's *Cosa: the Making of a Roman Town*,<sup>3</sup> which combines historical and archaeological data. In these publications colonization is described predominantly as a well-organized state-directed and administered event that radically transformed the conquered landscape. The symbols par excellence of the new imposed order were the *oppidum* (the administrative and religious centre of the new community) and the *ager diuisus* (a rigid egalitarian division of the rural space).

According to these studies the urban centre of the colony was designed to resemble the mother city, a view based ultimately on Aulus Gellius (16.13.8–9), for whom the colony was a small image of Rome. The similarity with the *Urbs* was not only one of appearance but included the whole socio-political and religious organization of the colony, or at least reflected what Rome ideally should have been like. Besides the creation of a city and its civic institutions, the foundation of a colony also involved a meticulous reorganization of the rural space. The colonial commissioners, with the help of a small army of engineers and technicians, divided the conquered territory into various categories of land, among which were areas reserved for pasture, public

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<sup>1</sup> All dates are BC unless indicated otherwise.

<sup>2</sup> Salmon (1969).

<sup>3</sup> Brown (1980).

land, territory where the remaining indigenous population could live, and most importantly the fertile land around the *oppidum*, which was distributed to the arriving colonists and became private property. This last category in particular required substantial work and organization since the selected area had to be divided into equal plots. An impressive network of *limites* was created in order to achieve this; the principal axes were marked by roads and/or channels, the minor ones by walls, secondary roads, boundary stones, or natural markers.<sup>4</sup> This geometrically ordered landscape must have looked impressive, and according to various scholars it truly was a triumph of *ratio* over the chaos of nature.<sup>5</sup> It imposed agrarian order and erased the past; the centuriated landscapes “provided a public and highly visible demonstration of Roman power and the humiliation of the enemy; they announced complete Roman control of the disposal of the land, permanent occupation, and a probable intention to distribute the fruits of victory to her own citizens and soldiers”.<sup>6</sup>

According to this prevailing view the *oppidum* and the *ager diuisus*, the first in the urban and the second in the rural sphere, epitomized Roman power and lifestyle and dictated the new social and economic norm. The creation of a colony was not only an act of military aggression but also one of civilization. Especially in less developed areas the achievements of the Roman conquerors must have astounded the surrounding indigenous communities and possibly even aroused admiration. Colonization is also believed to have introduced urbanization and a more developed form of agriculture and husbandry into these regions.<sup>7</sup> The combination of these qualities made the colonization program an essential component in Roman imperial strategy: these new city-states controlled conquered territory but at the same time stimulated integration between Romans and the subjugated Italians.<sup>8</sup>

Recently, however, various pillars of this edifice have been undermined. In a series of provocative articles a number of scholars have argued that this state-organized understanding of the mid-republican colonization program is the result of anachronistic ideology, syncretism,

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<sup>4</sup> For a detailed study of the rural reorganization accompanying colonization see Gargola (1995, esp. ch. 4).

<sup>5</sup> E.g. Quilici (1994, 127, 130).

<sup>6</sup> Campbell (1996, 81). For a similar view see Gargola (1995, 87).

<sup>7</sup> E.g. Dyson (1992, 25) and Curti (1995, 210).

<sup>8</sup> For the role of mid-republican colonization in the Romanization process of Italy see Torelli (1999, 173–5, 186–7), Williams (2001, 3), and Coarelli (1992, 19).

and schematization.<sup>9</sup> Careful restudy of the archaeological evidence of several colonial *oppida* has shown that the urban, religious, and political organization of these towns was not in fact highly standardized and copied from Rome.<sup>10</sup> Elizabeth Fentress, for example, has argued convincingly that Frank Brown's reconstruction of Cosa as a 'little Rome' was strongly coloured by expectations based on the text of Gellius referred to above, and maybe even by his wish to 'excavate the inaccessible Rome'.<sup>11</sup> His identification of the houses at the forum as *atria publica* and the temple next to the *comitium-curia* complex as the sanctuary of Concordia are almost exclusively based on the assumed parallel with the urban topography of Rome. According to Fentress the available archaeological evidence does not support such a reconstruction but suggests rather that Cosa at the start of the second century looked like a hierarchically organized military camp.

Not only is the understanding of colonial *oppida* as little versions of Rome in doubt, but the whole idea that there was a blueprint of what colonial towns ought to look like, and that this was implemented under the guidance of a state commissioner and completed soon after the arrival of the colonists, is now being questioned. Again in Cosa house foundations found beneath the temples on the *arx* indicate that the topography of the town changed over time, and that the monumental and excavated town plan was not envisioned from the start.<sup>12</sup> Likewise, a critical restudy of the urban development of Paestum shows that the *forum* area of the Greek-Lucanian town was not radically Romanized immediately after the arrival of the colonists, but was transformed gradually, "without institutional or social change having any close causal relationship with architectural developments".<sup>13</sup>

These revisionist studies have concentrated on the urban aspects of colonization. But their general conclusion—that there was no central

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<sup>9</sup> The revisionary position is voiced in greatest detail by Bispham (2006); other fundamental contributions are Crawford (1995) and Fentress (2000).

<sup>10</sup> For a revision of the religious organization of colonies see Torelli (1988) and Bispham (2006). For a critical review of the evidence concerning the political organization of colonies see Mouritsen (2004) and the response of Coarelli (2005). The normative classification of different kinds of colonies is questioned in Crawford (1995) and Bispham (2006, esp. 80–5), but see Northwood (2008). About the ethnicity of colonists see Bradley (2006). For historiographical issues see Patterson (2006). The urban organization is discussed below.

<sup>11</sup> Fentress (2000, 19).

<sup>12</sup> Fentress (2000, 13–21).

<sup>13</sup> Crawford (2006, 67).

plan, nor a strong involvement of Rome in the creation of the new Latin communities—also forces us to rethink other aspects of the colonial enterprise. If Rome indeed did not directly interfere, is it then conceivable that well-organized city-states emerged soon after the arrival of the colonists, and that ambitious land reforms and rigid division programs were carried out?

In recent years several Latin colonial territories have been subjected to thorough archaeological examination. In many Latin colonies field surveys have been conducted, and in almost all remaining cases topographical studies of the extra-urban area are available.<sup>14</sup> Moreover, the study of aerial photographs mainly by French and Italian scholars has produced valuable information about Roman land division programs and the ancient road network.<sup>15</sup> In the following sections I shall discuss what these studies reveal about the early years of the Latin colonies. In doing so I shall concentrate on the question as to whether or not these findings are compatible with the conventional state-organized view of colonization.

## II. *Dots and colonists: the problem of the missing sites*

Various references in the written sources (mainly in Livy) about the foundation of colonies and the number of settlers sent to these newly conquered lands provide a good impression of the probable extent of the resettlement program. Nineteen Latin colonies are recorded for the period between the fourth century and the first half of the

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<sup>14</sup> For Caes: Compatangelo (1985); Fregellae: Crawford et al. (1986), Hayes and Martini (1994), Coarelli and Monti (1998); Luceria: Volpe (1990; 2001), Volpe et al. (2004); for work in progress in the Celone Valley survey: <http://www.archeologia.unifg.it/ric/ricognizioni/celone.asp>; Suessa Aurunca: Arthur (1991); Interamna Lirenas: Hayes and Martini (1994); Sora: Tanzilli (1982); Venusia: Marchi and Sabbatini (1996), Sabbatini (2001); Hatria: Menozzi and Martella (1998); Cosa: Carandini et al. (2002), Dyson (1978); Paestum: Skele (2002) with further references; Beneventum: Patterson (1988); Ariminum: Fontemaggi and Piolanti (1995); Brundisium: Cambi (1999), Aproso and Cambi (1997), Manacorda and Cambi (1994). The old Latin colonies which remained independent after the Latin War are not included in this paper. Despite the fact that they are in a juridical sense similar to the new colonial foundations (all inhabitants had Latin rights and formed independent *municipia*), their genesis in the early Republic clearly separates them.

<sup>15</sup> For the Italian school see Quilici (1994) with further references; French research in Chouquer et al. (1987).

third century, and in six cases the number of colonists is also provided (ranging between 2,500 and 6,000).<sup>16</sup> According to the conventional understanding all these colonists received a plot of land for private use, most probably located in the fertile areas around the colonial urban centre. Regrettably, no information is transmitted about the size of the holdings distributed to Latin colonists before the Second Punic War, but virgatae and colonial assignments in the early and mid Republic to Roman citizens generally varied between 2 and 50 *iugera* (see fig. 1), which suggests a size somewhere in between the two extremes.<sup>17</sup> Although the sources do not mention it explicitly, it is often assumed that the majority of settlers built their dwellings on their own allotted land-holdings, resulting in a dense and regularly inhabited rural landscape (see fig. 2 for an artistic reconstruction).<sup>18</sup>

Such densely populated landscapes, however, are not recorded in archaeological field surveys. In the Ager Cosanus, for example, during the large-scale survey of the territory carried out by the Wesleyan University only two sites attributable to the first generation of colonists were identified within the probable limits of the *ager diuisus*.<sup>19</sup> Based on an estimate of the farmland available in the area and the probable amount of land each colonist received, Dominic Rathbone has calculated that these findings represent only between 0.3% and 0.8% of the number of third-century farms that must have been present in the territory.<sup>20</sup> A resurvey of the area by a joint Italian and British team some years

<sup>16</sup> Cales (2500); Luceria (2500); Interamna Lirenas (4000); Sora (4000); Alba Fucens (6000); Carseoli (4000). The 20,000 colonists for Venusia mentioned by Dion. Hal. 17/18.5.2 is excluded as it is generally believed to be corrupt (e.g. Brunt (1971, 56)).

<sup>17</sup> Larger allotments were granted to *centuriones* and *equites* in the later Latin colonies.

<sup>18</sup> E.g. Rathbone (1981, 17). For a general discussion of the habitation preference of Italian peasants see Garnsey (1979–1980). Only the upper class of the new community (*equites*), a very small percentage of the total population (between 5% and 10%), is believed to have resided in the city. Their holdings are considered to be larger than those of the *pedites*, and located possibly at a considerable distance from the city (see also Gabba 1984, 23).

<sup>19</sup> Dyson (1978).

<sup>20</sup> Rathbone (1981, 17). His computation results from an estimate of the available arable land in the Valle d'Oro (the area immediately surrounding the fortified centre of the colony) and the probable amount of land each colonist received (between eight and thirty-two *iugera*). The expected settlement density is contrasted with the meagre two third-century sites identified by Dyson and his team in this area. He argues that, based on the 12,000 *iugera* of arable land available, there must have been between 375 and 1500 farms in the area (depending of the size of the land holdings).

Year BC	Place	Type	Nr. colonists	Size of allotments in <i>iugera</i>	Reference	Size of <i>ager divinus et adsignatus</i>	Density of allotments per km <sup>2</sup>
418	Labici	Colony	1500	2	Livy IV,47	7.5 km <sup>2</sup>	200
395	Volscian frontier	Viritate/Colony	3000	3 <sup>7/12</sup>	Livy V,24	27 km <sup>2</sup>	112
393	Vcii	Viritate	—	7	Livy V,30	—	57
389	Vcii	Viritate	—	4 or 28 <i>plethra</i>	Diod XIV,102,4	—	286 or 41
385	Satricum	Colony	2000	2.5	Livy VI,15	12.5 km <sup>2</sup>	160
383	Ager Pontinus	Viritate	—	—	Livy VI,21	—	—
339	Ager Latinus	Viritate	—	2 + 3/4	Livy VIII,11	—	145
339	Ager Falernus	Viritate	—	3	Livy VIII,11	—	133
329	Anxur	Roman colony	300	2	Livy VIII,21	1.5 km <sup>2</sup>	200
290	Sabinum	Viritate	—	7	e.g. Val Max IV, 3,5; Columella 1 <i>praef</i> 14	—	57
232	Ager Gallicus and Picenum	Viritate	—	—	e.g. Polyb 2,21	—	—
201	Samnium and Apulia	Viritate	—	2 for each year of service	Livy XXXI,4 and 49	—	—
193	Copia	Latin colony	3000 (ped)	20 (ped); 40 (cqui)	Livy XXXV,9	180 km <sup>2</sup>	20 (ped) 10 (cqui)
192	Vibo Valentia	Latin colony	3700 (ped)	15 (ped); 30 (cqui)	Livy XXXV,40	161 km <sup>2</sup>	27 (ped) 13 (cqui)
189	Bononia	Latin colony	3000	50 (ped); 70 (cqui)	Livy XXXVII,57	390 km <sup>2</sup>	8 (ped) 6 (cqui)
184	Poentia	Roman colony	2000*	6	Livy XXXIX,44	30 km <sup>2</sup>	67
184	Pisaunum	Roman colony	2000*	6	Livy XXXIX,44	30 km <sup>2</sup>	67
183	Mutina	Roman colony	2000	5	Livy XXXIX,55	25 km <sup>2</sup>	80
183	Parma	Roman colony	2000	8	Livy XXXIX,55	40 km <sup>2</sup>	50
183	Saturnia	Roman colony	2000*	10	Livy XXXIX,55	50 km <sup>2</sup>	40
181	Aquileia	Latin colony	3000+	50 (ped); 100 (cent); 140 (cqui)	Livy XL,33	375+ km <sup>2</sup>	8 (ped) 4 (cent) 3 (cqui)
181	Graviscac	Roman colony	2000*	5	Livy XL,29	25 km <sup>2</sup>	80
177	Luna	Roman colony	2000	51.5	Livy XLI,13	258 km <sup>2</sup>	8
173	Ager Gallicus	Viritate	—	10; 3 for allies	Livy XLII,4	—	40 133 (al)

\* = Number not provided by sources, but seems plausible based on constant number of 2000 colonists sent to the other Roman colonies of that period.

Fig. 1. Land distribution and allotment size in the early and mid Republic.

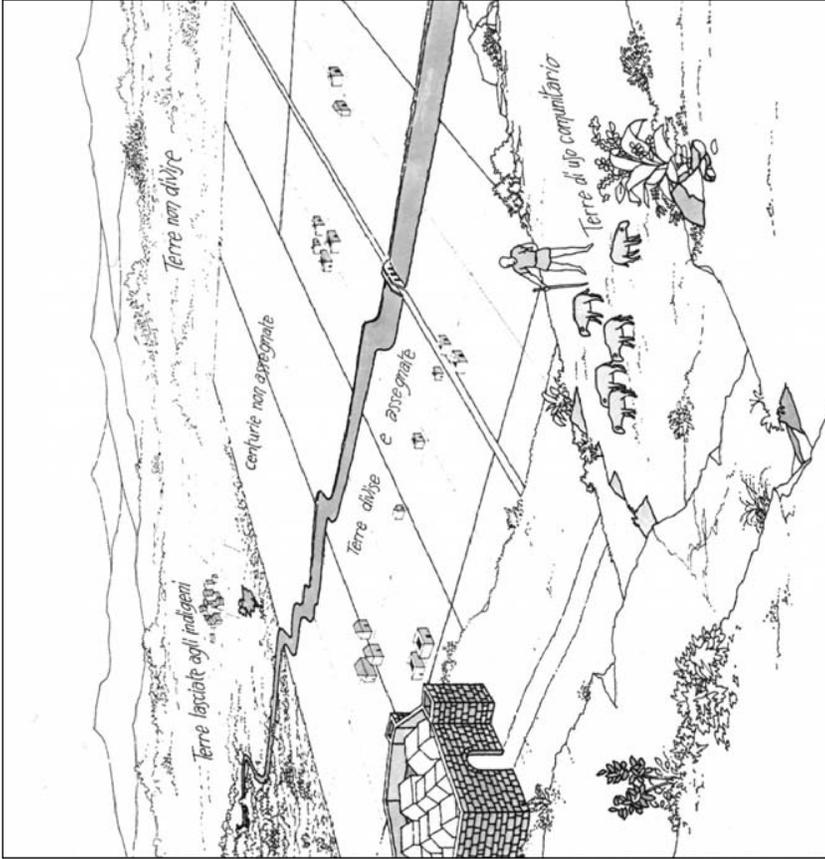


Fig. 2. Artistic reconstruction of a Latin colonial landscape (by G. Moscarà in: *Misurare la terra; centuriazione e coloni nel mondo romano*, 129).

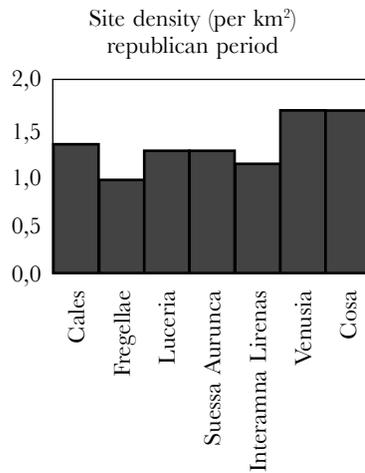
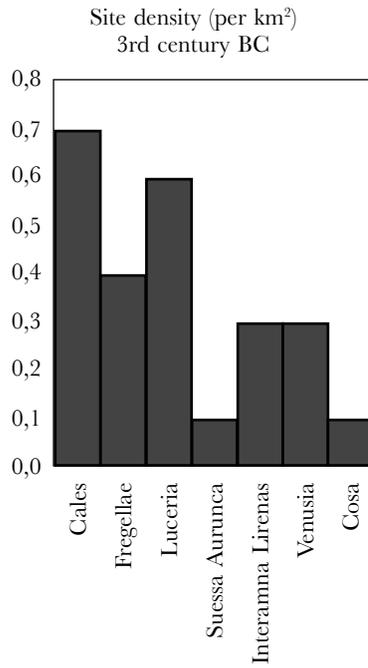


Fig. 3. Recorded settlement density per km<sup>2</sup> in Latin colonies.

later confirmed the conclusion of the Wesleyan survey that the early colonists of Cosa are virtually invisible in the archaeological record.<sup>21</sup>

This problem of missing sites is not confined to the Ager Cosanus. A glance at the various survey publications shows immediately that the low recovery rate of third century sites is symptomatic for all investigated colonial territories of the mid Republic (see fig. 3). The recorded settlement densities range from 0.1 to 0.7 sites/km<sup>2</sup>; this is clearly much lower than what ought to be expected (compare with fig. 1 last column). Even if one assumes that Latin colonists in general received large (50 *iugera*) plots of land, the compatibility between the number of farms recorded by survey and the expected farm density is 8.75 to 1.25%; if colonists received holdings of 8 *iugera*, the recovery rate would fall in a range between 1.4 and 0.2%.<sup>22</sup>

These findings can mean one of two things: either colonial peasants did not live on their allotted holdings, or archaeology is not able to recognize them. The latter suggestion is the more popular, as it is assumed that colonial dwellings were flimsy constructions that easily escape archaeological detection.<sup>23</sup> Colonists are supposed to have been recruited from the very poor and mostly landless populace, and their limited resources allowed them to build only very modest farms which, combined with a very low living standard and sober life style, hardly left any traces that can be recognized by an archaeologist.<sup>24</sup> Support for such a view comes from methodological advances in the survey discipline itself. Recent studies have shown that there is a clear relation between recovered site numbers and the intensity of the collection

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<sup>21</sup> Celuzza and Regoli (1982, 37) report that no certain site from the third century was found in the Valle d'Oro area. In a later publication, after a more profound study of the pottery, seven certain and sixteen possible sites from the early colonial phase were identified in the centuriated area (Attolini et al. 1991, fig. 2, and Cambi 1999, fig. 8.2.). In the final publication the settlement number rises to eleven certain and nineteen possible (Carandini et al. (2002, fig. 40)).

<sup>22</sup> If one includes all republican sites regardless of whether third-century occupation is attested, maximum compatibility rises to a maximum of 20% in the case of 50 *iugera* allotments, and 3.4% in the case of 8 *iugera* allotments (see fig. 3 for settlement density figures). However, such calculations assume an implausibly stable settlement system over roughly four centuries.

<sup>23</sup> E.g. Rathbone (1981, 17) and Scheidel (1994, 11).

<sup>24</sup> A slightly different explanation is that colonists lived in very simple houses because they used all their energy to build the necessary public structures (Celuzza and Regoli 1985, 51), or that as a result of natural attrition and the Gallic invasions and Punic Wars early settlements were short-lived and therefore difficult to recognize (Dyson 1978, 259).

strategy employed.<sup>25</sup> The fainter archaeological traces in particular, such as those of simple rural structures, are underrepresented in the datasets of the traditional large-scale site surveys under consideration here.

Undeniably, field surveys for all sorts of reasons often fail to produce reliable quantitative information about ancient landscapes, and there is a great risk that there are few recognizable traces from periods when construction techniques were simple and consumption of (diagnostic) ceramics was low. However, it is doubtful whether this potential methodological problem suffices as a solution to the missing site problem. The argument has one important weakness, namely that the assumption that many isolated farmsteads must have been present in the colonial territory is not backed up with any kind of literary evidence. The idea that colonists must have lived evenly dispersed over the countryside is based merely on derivative arguments such as the notion that they did not fit into the colonial town centre, on the ideological concept of the autarkic soldier-farmer, and on the parallel with the Greek colonial world, where densely populated *chôrai* are recorded archaeologically.

### III. *Where did colonial peasants live?*

An elegant solution to the problem would be to assume that the vast majority of colonists lived inside the colony's urban centre and travelled some distance to cultivate their fields.<sup>26</sup> However, the existence of so-called agro-towns and commuting peasants in republican Italy has been strongly challenged by Peter Garnsey in his influential article of 1979.<sup>27</sup> One of his most convincing arguments is that the size of Roman urban centres was usually too small to have contained a large population.<sup>28</sup>

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<sup>25</sup> For a good recent discussion of the relationship between survey intensity, survey area, and site productivity in Italian archaeology see Van Leusen (2001, ch. 4) with further references.

<sup>26</sup> For this explanation of the missing sites problem see Hayes and Martini (1994, 36), Attolini et al. (1991, 144), and Arthur (1991, 100).

<sup>27</sup> Garnsey (1979).

<sup>28</sup> Garnsey (1979, 13–15). His conclusion is based on the earlier studies of La Regina (1970–71, 451–2) on Aesernia and Alba Fucens, and Tozzi (1972, 16–21) on Cremona, who demonstrated that the number of colonists mentioned by Livy could never have fitted inside these colonial town centres. In the case of Cremona Tozzi calculated that about two-thirds of the colonists had to live outside the city proper, a figure that according to Garnsey must be raised since the assumed 320 persons/ha estimate used by Tozzi is probably too high; he argues that 150 persons/ha or lower is more plausible. See also the discussion below.

According to Garnsey the small size of colonial town centres proves that colonists “were expected to establish a rural base”,<sup>29</sup> and that Roman towns functioned predominantly as administrative centres which housed only the elite and possibly some farmers who had their lands in the immediate vicinity of the town. Against Garnsey’s conclusion two objections can be made: 1) his general conclusion is based on a very limited number of cases which are potentially unrepresentative; and 2) his estimated number of people per urban hectare could be too low.

After the appearance of Garnsey’s seminal article much more archaeological information about Latin *oppida* has come to light which makes it possible to test his hypothesis in a large number of cases. In fig. 4 I have calculated the percentage of the total number of colonists that could have fitted inside the various colonial towns using an urban population density of 120 persons/ha.<sup>30</sup> If this estimate is roughly correct, it follows that most colonial towns probably could not hold more than 20–30% of the total number of colonists. In some cases a much higher percentage (up to 100%) potentially fitted inside the town walls. It must be noted, however, that in the graph a maximum urban population percentage is given. This assumes that all space inside the city walls was occupied by the colonists. In cases where colonists were sent to previously existing centres such a scenario is implausible, as

<sup>29</sup> Garnsey (1979–80, 15).

<sup>30</sup> For colonies marked with an asterisk the number of colonists is provided by Livy. For the other colonies up to Aesernia the estimate proposed by Cornell (1995, 381 table 9) has been used. Only for Firmum have I used 2,500 instead of the proposed 4,000. For Brundisium and Spolegium I have used an estimate of 4,000. The following town sizes have been used: Caes: 58 ha (Sommella 1988, fig. 69.2); Fregellae 80: (Coarelli and Monti 1998, tav. II); Luceria 195 ha (Lippolis 1999, fig. 1); Saticula: c. 107 ha (Johannowsky 1998, fig. 149); Suessa Aurunca: 35 ha (Arthur 1991, fig. 7); Interamna Lirenas: c. 35 ha (Hayes and Martini 1994, 35); Sora: 71 ha (Coarelli 1982, 230); Alba Fucens: 34 ha (Sommella 1988, fig. 69.4); Narnia: c. 7 ha (Bradley 2000, 137 n. 116); Carseoli: unknown to me; Venusia: 42 ha (Marchi and Salvatore 1997, fig. 9); Hatria: 70 ha (Guidobaldi 1995, 199, and Azzena 2006); Paestum: 120 ha (Greco 1988, 82); Cosa: 13.25 ha (Brown 1980, 10); Ariminum: 41 ha (Sommella 1988, fig. 69); Beneventum: c. 45 ha (Giampaola 1991, tav. V), but if the Cellarulo area is included the size increases to c. 100 ha; Firmum: 10 ha (Gaggioli et al. 1980, 269); Aesernia: 10 ha (Sommella 1988, 228); Brundisium: c. 56 ha (Lippolis and Baldini Lippolis 1997, 310), for the town size in the Messapian period (104 ha) see Burgers (1998, 228) with further references; Spolegium: 30 ha (Morigi 2003, fig. 219); Placentia: 38.4 ha (De Ligt in this volume); Cremona: 30 ha (De Ligt in this volume); Copia: unknown to me; Vibo Valentia: c. 80 ha (Fischer-Hansen et al. 2004, 262); Bononia: 50 ha (De Ligt in this volume); Aquileia: 41 ha (De Ligt in this volume).

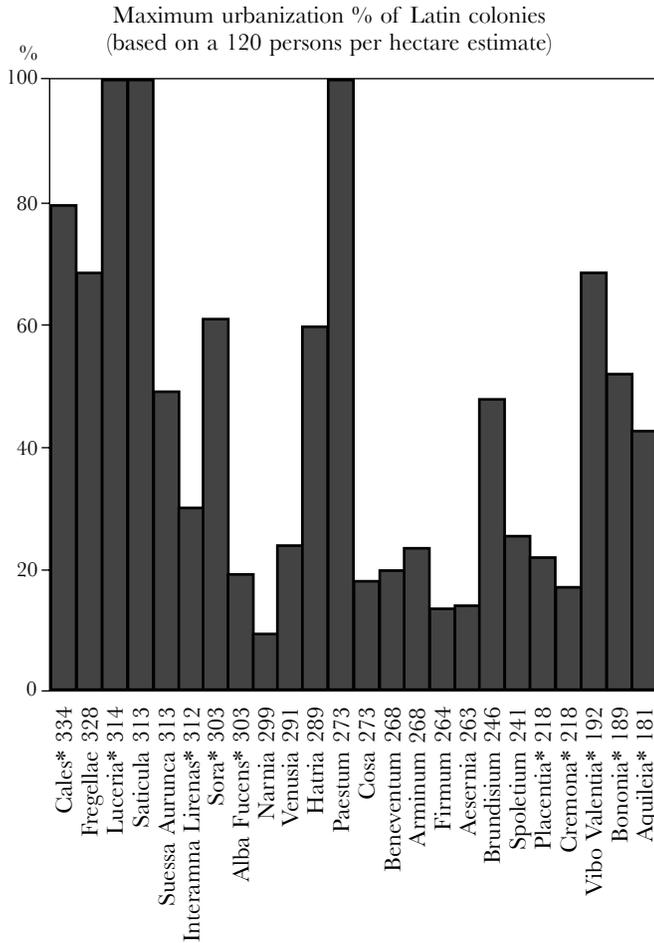


Fig. 4. Maximum urbanization % estimate of Latin colonies.

a substantial proportion of the indigenous population most probably continued to reside in the colony.

Paestum is a good example of this. Ample archaeological evidence suggests that there was a continuity in most parts of the town between the Greek-Lucanian phase and the Roman period, which makes it implausible to think that colonists occupied the whole urban area.<sup>31</sup>

<sup>31</sup> It has been suggested that the Greek/Lucanian town (70 ha) at the time of the foundation of the Latin colony was enlarged by *c.* 50 hectares. The colonists supposedly lived in this new part of the city, while the indigenous population continued to live in the old town (Greco 1988, 82, 80 fig. 1). In this case the urbanization rate drops to

More problematic is the situation in non-Greek towns. It is often assumed that Latin colonies were new foundations and that the size of the town was determined during the initial years of the colony and involved the well-known ritual ploughing of the primeval furrow (*sulcus primigenius*) around the city.<sup>32</sup> However, from reading Livy one gets the strong impression that the early Latin colonies in particular were established in the cities of the conquered people.<sup>33</sup> In some cases it has been argued that the new colony, even though it borrowed the name from the conquered town, was founded *ex novo* in a different location;<sup>34</sup> and this hypothesis is strengthened by the fact that little archaeological evidence of pre-Roman occupation is encountered in various colonial towns.<sup>35</sup> This solution, however, is not applicable to all situations. In some cases, such as Cales, Hatria, and Spolegium, clear traces of an indigenous phase have been recognized inside the colonial town centre.<sup>36</sup> Furthermore, recent excavation of the walls of the Latin colony of Saticula has clearly shown that the fortification

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*c.* 48%. Very little research has been done in this part of the town, which makes it for now impossible to test this hypothesis. A similar situation can be assumed for the colony of Vibo Valentia that was placed in the Greek city of Hipponion. Archaeological investigation inside the very large city walls demonstrates that Greeks continued to live inside the city. An orthogonal street grid identified in the lower part of the city is considered to have been the colonial settlement (see Ianelli and Givigliano 1989, 678).

<sup>32</sup> See Gargola (1995, 73–5) for a good description of this ritual.

<sup>33</sup> The name of the colony is often similar to the name of the conquered city. This policy changed over time and in the second century colonies were usually named differently (e.g. Copia and Vibo Valentia). The conquered settlements according to Livy were proper cities, often with walls. For the Ausonian phase of Cales see Livy 8.16. Livy speaks of an *urbs* with walls (§ 10). Similar descriptions can be found for most early Latin colonies.

<sup>34</sup> E.g. Filippo Coarelli, who in the case of Fregellae argues that the Volscian town of Fregellae mentioned by Livy was situated in a different location from the Latin colony, namely near the modern Roccadarce (Coarelli and Monti 1998, 47–8). For Venosa see Marchi and Salvatore (1997, 6–7) and Marchi and Sabbatini (1996, 47–8). It is suggested that Melfi or a village identified in località Canalini Sottana may be the pre-Roman Venusia mentioned in the sources. For Cosa see Brown (1980, 8), who proposes that the modern town of Orbetello was the original town, called Cusi or Cusia, which gave its name to the colony.

<sup>35</sup> In Venusia pre-Roman material has been found, but “I frammenti ceramici databile al pieno IV sec. a. C., rinvenuti in più luoghi dell’area successivamente occupata dalla colonia di Venosa, non appaiono, infatti, legati ad un contesto architettonico che possa confermare la presenza di un centro abitato preromano sull’altopiano” (Marchi and Salvatore 1997, 6). These scholars fail to note, however, that there is even less evidence for the early colonial phase of this colony. The situation at Cosa is similar (see discussion below).

<sup>36</sup> For Cales see Pedroni (1986) and Pedroni (1990); for Hatria see Guidobaldi (1995, 37–42) and Martella (1998, 48); For Spolegium see Sisani (2007, 92) with further references.

was constructed well before the Roman period.<sup>37</sup> Likewise in the case of Luceria it is implausible to suppose that the 2,500 colonists built a city wall enclosing an area of 195 hectares. Large fortified enclosures are a typical Daunian phenomenon, which suggests that the walls of Luceria belonged to the pre-Roman phase of the city.<sup>38</sup>

In theory it is possible to think that the 120 persons/ha estimate for urban population density is too low, and that first generation of colonists lived close together in very small houses. But such a view is difficult to defend for several reasons. In order to achieve high population densities per hectare, colonists needed to live either in very small houses or in multi-storey buildings.<sup>39</sup> For both requirements no archaeological evidence is found in mid-republican colonial cities. Excavations in Cosa, Fregellae, and Alba Fucens show that houses dating to the second century were relatively large (there was even room for gardens), and most had no second floor.<sup>40</sup> Admittedly, the second-century situation is not necessarily representative for the earlier phases of colonial towns. The problem, however, is that there is almost no archaeological evidence for third-century habitations which could give us some insight into the living conditions of that particular period. Only in the Latin colony of Fregellae do we find an (elite) residence which can be dated to this period, but in all other cases excavated urban residences date from the beginning of the second century or later.<sup>41</sup>

The lack of remains of fourth- and third-century dwellings within the town walls might be explained as a result of the monumentalization phase of cities in the second century, which obliterated all traces of previous habitation. However, the fact that many post-Hannibalic structures seem to have been built on virgin soil suggests that colonial town

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<sup>37</sup> De Vito and Di Maio (1998); the wall was enforced in the later third/early second century BC.

<sup>38</sup> For a discussion of the fortified settlements of pre-Roman Daunia see Whitehouse and Wilkins (1989, 117). Topographic studies inside the town walls of Luceria have furthermore suggested that only 90 ha of the town were actually inhabited. Moreover, there is a remarkable rectangular pattern (measuring *c.* 38 ha) recognizable in the current street pattern of the town, and it is suggested that this was the area where the Roman colonists lived (if true the maximum urbanization drops to 52%). Also it is argued that the early colonial settlement at Fregellae was confined to the south of the urban area (Crawford et al. 1985, 84 E).

<sup>39</sup> See De Ligt in this volume for a detailed examination of this argument.

<sup>40</sup> See Pesando (1997, 275–320) for a good overview of houses in Latin colonies.

<sup>41</sup> For the houses of Fregellae see Coarelli and Monti (1998, 62–5).

centres were not very densely populated in their earliest phases.<sup>42</sup> This conclusion is strengthened by two surveys carried out within the town walls of Interamna Lirenas and Cales. Both surveys demonstrate that the majority of BG ceramics found there are from the second century, and that most of the early material is found in restricted areas of the town, often in cultic contexts.<sup>43</sup> These observations are at odds with the idea that population densities in the earlier phase of the colony were much higher than later on. It is unclear how the colonial *oppida* in their early phase were organized, and much more research is clearly needed. The archaeological information presently available, however, suggests that colonial towns followed a development fairly similar to other, better investigated Italian towns. Recent studies in Pompeii have shown that the walled town (65 hectares) of the fourth and third centuries was sparsely populated, and that agricultural activities were carried out within the town walls. Only from the late third and early second century onwards did the town start to change rapidly, becoming fully built up within one generation.<sup>44</sup>

To sum up, archaeological investigations in the countryside and in colonial *oppida* thus far have found surprisingly few traces of the first generations of colonists. The theory that the rural emptiness apparent from archaeological survey is the result of an urban settlement preference of colonists must be rejected, since the limited size of colonial *oppida* and the scarce evidence for third-century occupation suggests that in most cases only a small proportion of colonial families could have had an urban base.<sup>45</sup>

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<sup>42</sup> Cosa is exemplary: no traces of habitations dating before the late third century have been identified during the many years of excavation. Only a very limited amount of black gloss ceramics dating to the early colonial phase (i.e. the third century) has been encountered. See Taylor (1957) for a study of the BG pottery of the early excavations; for the BG ceramics recovered during the recent excavations see <http://www.press.umich.edu/webhome/cosa/ceramics.html> (last accessed 20/12/2007).

<sup>43</sup> For Cales see Pedroni (1986; 1990). Of the seventeen locations where material is collected only five had clear late fourth and third century pottery. Three of those are clearly connected with cultic activity (around the temple area, on the *arx*, and in the votive area loc. Ponte delle Monache); two are secondary deposits (fill of the city wall). There is an interesting correlation between the find spots of pre-Roman and early colonial material. For Interamna see Hayes and Wightman (1984), Hayes and Martini (1994, 38, 138–45).

<sup>44</sup> Nappo (1997); Pesando (1997, 12–27).

<sup>45</sup> An additional problem with the assumption that the majority of colonists lived in the town-centre is that especially in the larger colonies such a settlement system implies that colonial peasants had to walk enormous distances to reach their fields. Wightman and Hayes calculated for Interamna Lirenas that if peasants lived inside

#### IV. *Early colonial settlement organization*

The archaeological landscapes of third-century Latin colonies are characterized by their emptiness. As we have seen, only a very limited number of rural sites have been identified, and these represent only a fraction of the expected number of rural colonists' dwellings.<sup>46</sup> The incompatibility of the historical expectation with the results from field archaeology is not limited to this quantitative issue only; the spatial distribution of recorded sites is also very different from the expected pattern of settlement. Third-century sites are not scattered evenly over the territory, as is often expected, but are predominantly clustered together and separated from each other by large tracts of unoccupied land.

Such a configuration, for example, is clearly visible in the territory of Interamna Lirenas (see fig. 5). Two clusters of third-century sites have been identified: one in the immediate vicinity of the colonial town centre, the other in the Gari river area near the modern town of Sant'Angelo in Theodice. Between them lies an area extending for 5 km in which no sites from this period have been found.<sup>47</sup> In the Ager

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the town some of them had to walk six to eight kilometres to reach their small fields of an estimated six *iugera*. If larger sized allotments were distributed, their daily trip would have been even longer.

<sup>46</sup> Franco Cambi (Cambi 1999 and Carandini, et al. 2002, 137–145) recently calculated that the ambitious 'Fra la valle dell'Albegna e il Fiora' survey project was able to recover between 20% and 33% of the probable third- and second-century colonists' dwellings in the territory of Cosa. But his assessment of the quality of the survey record proves on critical examination to be too optimistic. In his calculation of the recovery rate he compares the probable number of colonists living in the countryside (estimated at 4,000) with the total number of sites recognized during the survey project. The territory covered, however, is much larger than the probable Ager Cosanus since it also comprises the territories of the later colonies of Heba and Saturnia. His assessment is therefore valid only for the second century (in which case, however, the total number of colonists seems rather low). If one applies the general method of comparing sites with the expected number of colonists to the specific third century situation, the recovery rate drops significantly to 3.4–6.4%. A further complication with the above comparison is that it assumes that all sites in a colonial territory can be regarded as dwellings inhabited by colonists. This position can be defended only if one assumes that either all indigenous people were removed from their original lands (killed or resettled outside the colonial territory) or that all former inhabitants were enrolled in the colony and included in the number of colonists mentioned in the sources. There is, however, strong evidence that in at least some cases the indigenous communities were left on their ancestral lands but did not receive Latin rights (see discussion below). If one accepts such a reconstruction, and if it is assumed that a considerable indigenous population (without Latin rights) continued to live in the colony, a higher settlement density/km<sup>2</sup> must be assumed, thus reducing even further the compatibility percentage.

<sup>47</sup> Hayes and Martini (1994, 188 fig. 43).

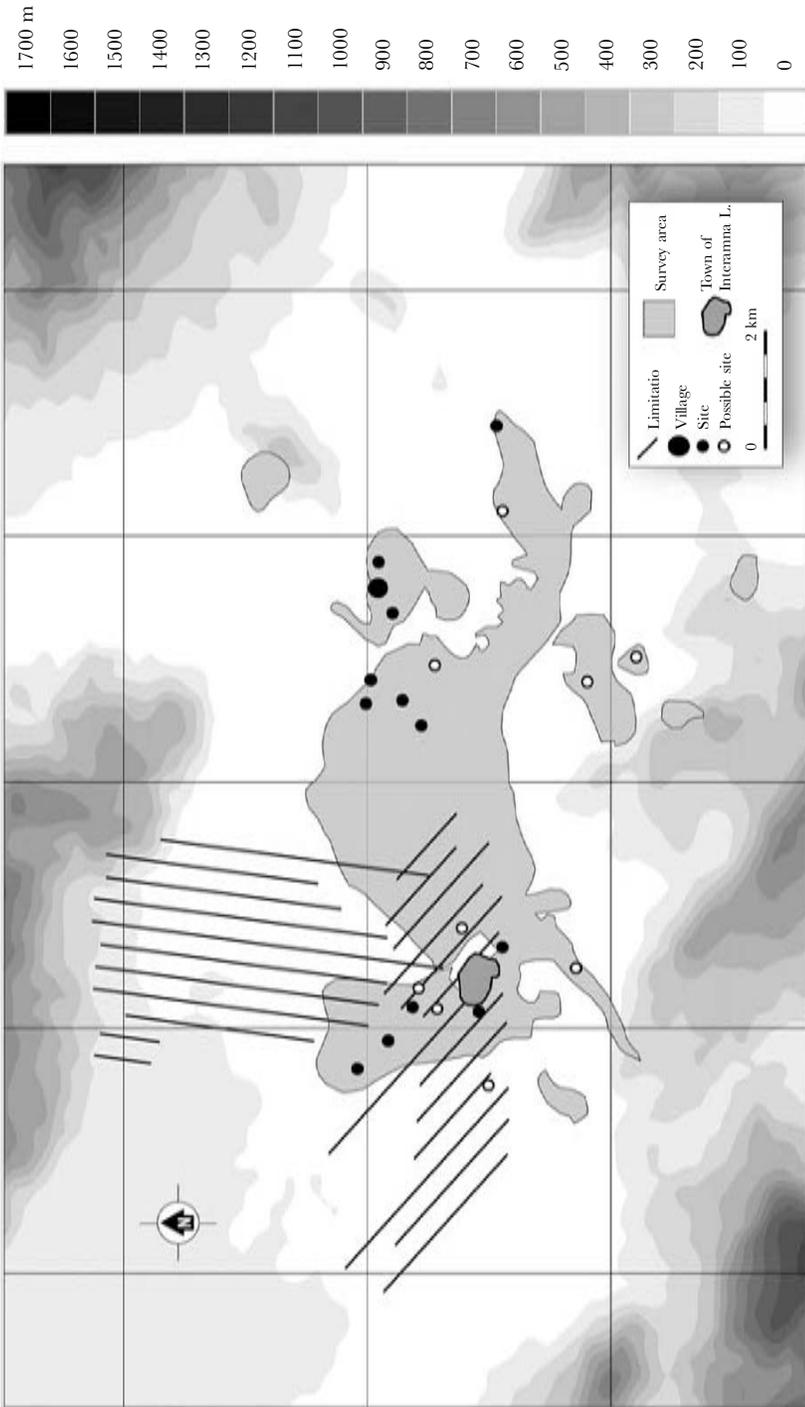


Fig. 5. Site distribution of 3rd century BC sites in the territory of Interamna Lirenas.

Cosanus a similar pattern is discernible: a large cluster of third-century sites is located in a restricted part of the Valle d'Oro in an area between località le Tombe, Poggio Sette Finestre, and Monte Alzato at the west bank of the Torrente Melone; other concentrations are found in the Valle Lunga and the area close to the coast, between the Fiume Chiarone and the Fosso del Tafone (see fig 6).<sup>48</sup> In the Ager Calenus only a limited area was surveyed, making it more difficult to recognize a clustered pattern of habitation. Two sample areas have been explored: one to the north of the ancient town, in which a substantial number of third century sites have been identified, another around Sparanise, where almost no sites of this period could be identified.<sup>49</sup> The marked difference between both sample areas suggests that a clustered pattern is also characteristic for this area. In the territory of Fregellae concentrations of sites have been identified to the north of the modern town of Ceprano<sup>50</sup> and in the Monticelli del Carmine area,<sup>51</sup> while large empty areas are recorded between the town of Fregellae and the river Melfa. The situation around Venusia is more complex: although concentrations of sites are located in the Masseria Casalini and Masseria Briscese area, the other third-century sites are scattered throughout the territory, and no specific clustered pattern can be recognized.<sup>52</sup>

The characteristic but unanticipated clustered configuration cannot easily be explained as the result of a bias in the archaeological record. In the territory of Interamna Lirenas thorough study of the geology, geomorphology, and modern land use of the area has demonstrated that the recorded void between the two clusters of settlement “cannot be explained either by the soil types or by possible recent obliteration of sites”.<sup>53</sup> It is significant in this context that field surveys in the Greek poleis of Italy, using a very similar research strategy, have in fact discovered dense and evenly distributed patterns of settlement dating to the fourth and third centuries.<sup>54</sup> This marked and consistent differ-

<sup>48</sup> Carandini et al. (2002 fig. 40 and tav. 14).

<sup>49</sup> Compatangelo (1985).

<sup>50</sup> Coarelli and Monti (1998, 97 [sites 33–8] and tav. XXXVIII).

<sup>51</sup> Hayes and Martini (1994, 181–2 [sites 86–93] and fig. 27).

<sup>52</sup> Marchi and Sabbatini (1996).

<sup>53</sup> Hayes and Martini (1994, 71) and ch. 3 for the results of the geologic and geomorphologic research.

<sup>54</sup> See Carter (2006, esp. ch. 5). See also Burgers and Crielaard (2007) for a preliminary publication of the results of a survey conducted in the territory of Taras. In this last case an intensive off-site survey strategy was employed.

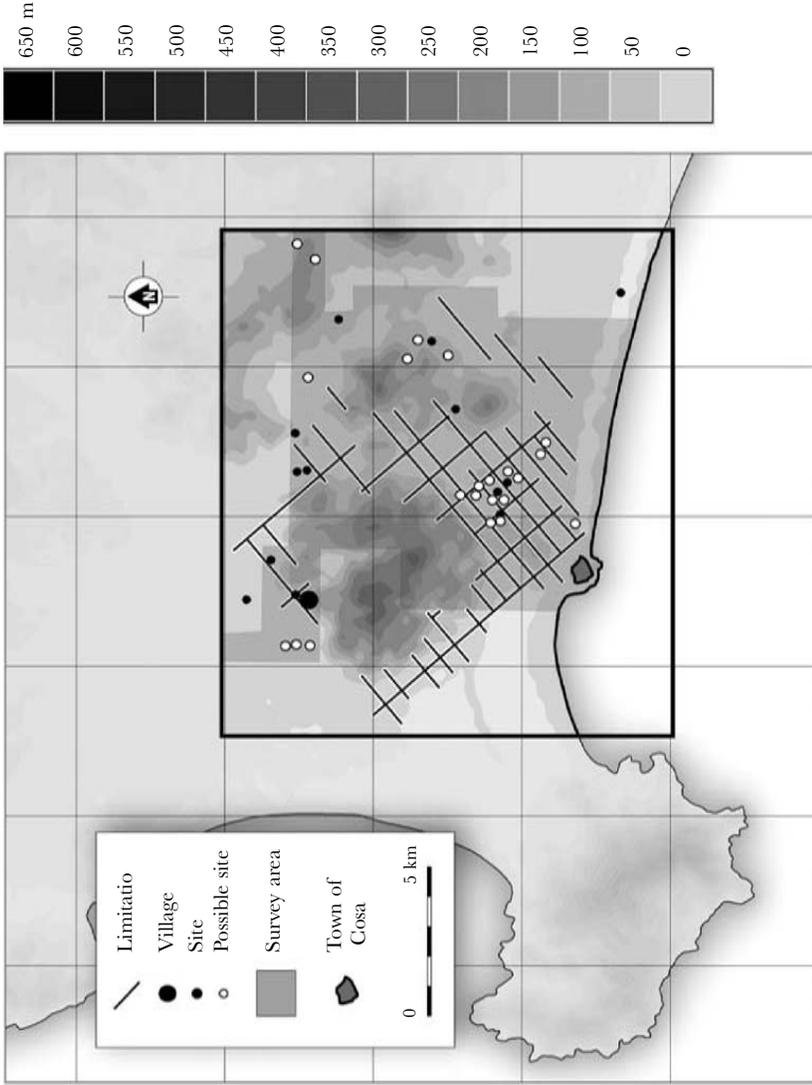


Fig. 6. Site distribution of 3rd century BC sites in the territory of Cosa.

ence between coeval landscapes strongly suggests that it is the result of very different settlement realities rather than of some bias in the archaeological record.

The scholarly debate about ancient rural settlement is often restricted to the question of whether people lived in agro-towns, characterized by large population centres and virtually uninhabited rural territories, or dispersed throughout the rural territory of the colony (i.e. farmers living on their holdings). This implied dichotomy, however, is an oversimplification of the complex and diverse nature of the reality of rural settlement. One especially interesting element of the rural landscape which is neglected in this bipolar model is the agricultural village. Even today a typical rural landscape in the Apennine region is dominated by a dense network of villages and hamlets often no more than a couple of kilometres from each other.<sup>55</sup> The vast majority of farmers live in these modestly sized rural population centres, while a limited number of isolated farmsteads can be found along the roads connecting the various villages; generally the further one moves away from the village, the fewer of these farms there are.

The Apennine village landscape in its outward appearance displays some interesting similarities to the configuration of sites recorded for Latin colonies. Both landscapes are characterized by clusters of settlement located a couple of kilometres from each other.<sup>56</sup> The physical correspondence at first sight is limited to the clustering of farmsteads, while villages (the focal point of the Apennine settlement system described above) are often lacking in the archaeological site distribution maps. These graphical reproductions of archaeological findings, however, are often misleading since the uniform dots on the map give the inaccurate impression of a landscape of equally sized settlements. The reality behind these dots is generally far more diverse and complex than at first sight appears and usually requires a close examination of the site catalogue (if published satisfactorily) in order to be understood. A further methodological problem is that villages are generally difficult to recognize during regular field surveys since they are often located in the least fertile areas, often on hilltops also offering natural protection. A survey archaeologist is dependent on the fertile areas for his research;

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<sup>55</sup> E.g. Frederiksen (1984, 31).

<sup>56</sup> The similarity of course is only one of appearance, and as both landscapes developed in very different political and economic contexts no more fundamental parallel between both landscapes is implied here.

only on fields that have recently been ploughed can one expect to find artefact assemblages making possible the identification of ancient settlements. If people lived in unfertile areas that even today are not being cultivated, a survey archaeologist will often pass by without noticing the traces of their settlements, or he will record only the few farms located along the roads leading to the villages. An additional difficulty is that the land favourable for building larger settlements has remained popular throughout history, as a result of which many ancient villages are buried beneath medieval and modern ones. An illustration of these problems is the fact that many of the (few) identified villages in these areas are known only from rescue excavations inside modern villages, or from accidental (or clandestine) discoveries.

Despite these graphical and methodological problems several villages have been recognized or can plausibly be reconstructed on the basis of the available information. A clear example can be found in the site catalogue of the territory of Interamna Lirenas: a site identified near the Gari River is described as a wide and heavy scatter of *c.* 6 ha without perceptible breaks.<sup>57</sup> Such a scatter is obviously too large to be interpreted as a farmstead and is probably best described as a village. Around it, probably along the roads leading to and from the village, a couple of isolated farmsteads have been located. In the territory of the Latin colony of Suessa Aurunca, near the modern town of Cascano, just 2.5 km to the east of the colonial town centre, excavations have revealed part of a late fourth/early third-century ashlar wall which is very similar to the early walls of Suessa.<sup>58</sup> These fortifications probably enclosed a nucleated settlement of the same period. A couple of kilometres to the south-west of Suessa, at località Ponte Ronaco,<sup>59</sup> another village has been identified from a large concentration of ceramics found in the area. In the territory of Cosa in the Valle Lunga<sup>60</sup> and the area between the rivers Chiarone and Fiora three villages have been recognized inside or near habitation clusters.<sup>61</sup> Finally, in the territory

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<sup>57</sup> Site 526 in Hayes and Martini (1994, 230). The site has a long history from the early republican to late imperial period, so it is not clear if the described size is relevant for the mid-republican period.

<sup>58</sup> Arthur (1991, 40).

<sup>59</sup> Talamo (1987, esp. 161, 177); Arthur (1991, 121 [site S12]). Around the villages identified in the territory of Suessa Aurunca various isolated farmsteads and tombs have been recognized. Only a few of them can be securely dated to the third century.

<sup>60</sup> Site Orb107 (Carandini et al. 2002, site catalogue).

<sup>61</sup> Site PR 9 (4 ha); PR 58 (1.2 ha); PR 80.1 (3 ha): *ibid.*, site catalogue. Another village surrounded by smaller sites is found at the border of the Ager Cosanus: LC 8

of Luceria recent research has located at least three republican villages covering areas varying between 2.5 and 11 ha.<sup>62</sup>

Regrettably, none of these villages has been properly excavated, so we know virtually nothing about these nucleated settlements.<sup>63</sup> The consequence of this gap in our knowledge is that it is impossible at present to estimate the number of people dwelling in these places, and more importantly that we know nothing about their ethnic and socio-juridical background. As we shall see below, the ethnic question is relevant because these villages are often associated with the indigenous population that continued to live in the colonial territory.

### V. Vicatim habitantes?

Most scholars agree that part of the conquered indigenous community continued to live in the colonial territory; their number and status, however, are more problematic issues about which no consensus has

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(1 ha), and one outside the survey transects MAR 150.1 (2 ha). The Etruscan town of Orbetello was also populated during the early colonial period. A reference in Castagnoli (1956, 157) is suggestive: he recognized a significant part of a wall structure that enclosed the west side of località le Tombe, the area where a large cluster of third-century sites has been identified.

<sup>62</sup> Volpe (2001, 344–5). This list includes only the villages located in surveyed areas. In other colonial territories villages are also known: in Acsernia a republican village is located at 6 km to the south-west of the colonial town centre near the modern village of Macchia d’Isernia (Pagano 2004, 78). Near Hatria several villages of the republican period have been identified, e.g. S. Rustico (Basciano c. 20 km to the north-west of Hatria), Valviano (c. 10 km to the west of Hatria), Case di Sante e Monteverde (Cellino), Penna S. Andrea, Guardia Vomano, Castilenti, and Città S. Angelo. Inscriptions found in two of these (S. Rustico and Valviano) reveal that in the late-republican period they were probably *uici* with their own form of administration. On these villages see Guidobaldi (1995, 264–77) and Menozzi and Martella (1998, 42). Just beyond the probable northern limit of the territory of Benevento John Patterson identified a large site probably to be interpreted as a village dating from the fourth to the second century (site 10). Around it several isolated farmsteads have been identified: Patterson (1988, 170–1). The remark in Crawford et al. (1986, 50) is also interesting: “It may also be that the pattern of settlement in the territory of Fregellae was not so much one of single farms, but something close to villages: sites 51, 52, and 54 lie very close to each other”; idem for sites 13 and 12.

<sup>63</sup> Besides the already mentioned unpublished excavation of the fortification walls of Cascano, the only properly excavated village site inside a Latin colonial territory of which I know is S. Rustico Basciano. A large number of evenly distributed houses have been found alongside a large road near a sanctuary of which the monumental phase dates from the late second century. Furthermore an inscription mentioning two *magistri* (probably *magistri uici*) has been found. The excavated structures, however, all date from the first century (see Menozzi and Martella 1998, 42–3 with further references).

yet been reached. Brunt, reasoning from military considerations, argued that the vast majority of colonists must have been of Roman or Latin origin.<sup>64</sup> In his view literary references to the enrolment of indigenous people are unreliable and are the result of an anachronistic retrojection by Livy and Dionysius (or their sources), who were influenced by the liberality with which the citizenship was bestowed in their own period.<sup>65</sup> Guy Bradley, on the other hand, pointing to recent studies on the complexity of identity and the polyethnic make-up of early Roman society, argues convincingly that these references are credible and illustrate that, especially in the archaic period, ethnic identities were not central to behaviour.<sup>66</sup>

References in Livy to the inclusion of indigenous people stop after the Latin War, which gives the impression of a policy change.<sup>67</sup> However, archaeological and to a lesser extent epigraphic evidence clearly indicates that indigenous people continued to live in colonial territories. What is less clear, however, is what their socio-juridical status was, and whether they were accepted as full citizens of the new community. Epigraphic evidence dating to the second century suggests that they had an inferior status (as *incolae*). This would mean that they continued to live in the colonial territory, and even in the colonial *oppidum*, without joining the Roman or Latin colonists in a political and juridical sense.<sup>68</sup> Full inclusion, according to Bradley, should not be ruled out as a possibility since there is “clearly a continuity in the archaic ethnic mentality that promoted the absorption of foreign people”.<sup>69</sup> Whether full inclusion also meant that some indigenous inhabitants were included among the number of colonists recorded in the sources is another matter. Cornell has argued from a demographical point of view that “it is unlikely that the Roman population on its own could have withstood such a drain on its citizen manpower (i.e. 70,000 adult males with their families)”.<sup>70</sup>

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<sup>64</sup> Brunt (1971, 540).

<sup>65</sup> See Bradley (2006, 171–2) for a detailed and critical discussion of this argument.

<sup>66</sup> *Ibid.*, 166.

<sup>67</sup> After the Second Punic War the situation changes again, and there are references to the inclusion of non-Latins in supplements sent to colonies.

<sup>68</sup> However, it is unclear if this inferior status was given to non-Roman/Latin newcomers that according to the sources migrated in great number to Latin colonies in especially the first half of the second century, or if this distinction was made from the start. See Coarelli (1991, 179) for arguments in favour of the former explanation (with further references).

<sup>69</sup> Bradley (2006, 179).

<sup>70</sup> Cornell (1995, 367).

And although he seems to suggest that the allied communities fighting alongside the Romans were the most likely candidates to join colonies, the argument could also be used in favour of the inclusion of native dwellers, possibly any pro-Roman faction in the subjugated community. On the other hand the terminology used in the sources to describe the enrolment of colonists (e.g. *deducere, missi*) suggests that they were sent out to the colony, and the common view is that they travelled as a group from Rome to their new homes.<sup>71</sup>

Despite the uncertainties surrounding the number and socio-juridical status of indigenous inhabitants living in the territories of colonies, it is often assumed in archaeological studies that the indigenous inhabitants can be distinguished from the Roman/Latin colonists on the basis of their settlement customs. While the colonists proper are supposed to have lived on their holdings distributed regularly over the colonial territory, the indigenous component is thought to have dwelled in villages (see again fig. 2 for a clear illustration of this view). This conception is rooted in a more general theory about settlement organization in which a clear distinction is supposed to have existed between the various non-urbanized Oscan people living in villages and the Greco-Roman world of city-states characterized by urban centres with rural territories settled in a regular fashion. The supposed difference in settlement organization is not a modern invention but can also be found in the writings of the various late-republican and imperial historians. Livy, for example, when characterizing the Samnites describes these rude highlanders as *uicatum habitantes* (9.13), contrasting them with civilized communities that knew an urban way of life.<sup>72</sup> This scheme is obviously an anachronistic and ideological construct that cannot be accepted at face value.

The ethnic or social status of the people inhabiting the various villages identified by archaeology will remain a mystery until more research has been done. In a few cases, for example in the Ponte Ronaco village near Suessa Aurunca, settlement continuity from the Iron Age into the Roman period suggests that the inhabitants were of indigenous origin. Their status remains unclear, but the discovery of a bronze coin of the third century with the legend SUESANO suggests that at least economically they were indeed integrated into the colonial com-

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<sup>71</sup> E.g. Brown (1980, 16), who gives a beautiful impressionistic description of such a journey.

<sup>72</sup> See also Tac. *Hist.* 4.64 for a similar opinion. In general on this issue: Frederiksen (1976, 341–2). See also Dench (1995, esp. 130–4).

munity.<sup>73</sup> Other villages appear as new foundations on archaeological maps, which makes them more likely to be the dwellings of migrant farmers.<sup>74</sup> The only epigraphic evidence from this period referring to *uici* (the *uici* painted on the *pocula deorum* of Ariminum and the *Veqo Esquelino* mentioned on a *patera* from Cales) is ambiguous since the term *uicus* may refer to an urban region, and it is in this sense that the inscriptions are usually interpreted.<sup>75</sup>

<sup>73</sup> Talamo (1987, esp. 161, 177); for the coin see Arthur (1991, 121, site S12).

<sup>74</sup> Generally, however, these new villages are interpreted as the settlements of indigenous farmers who were relocated by the Romans to these areas (for such explanation see e.g. Carandini et al. (2002, 110)). The question cannot be answered satisfactorily in the current state of research.

<sup>75</sup> E.g. Bispham (2006, 87–8) with further references. The evidence for the *uicus Esquilinus* comes from an inscription on a BG *patera* which reads: 'K.SERPONIO CALEB.FECE.VEQO ESQUELINO C.S. (CIL 1.416). The urban thesis is supported by another dedicatory inscription from Cales dating to the early imperial period which mentions a *uicus Palatius* (CIL 10.4641). Recently, however, Gaudagno (1993, 431–4) has questioned this urban reading and argues in favour of a rural location for both Calene *uici*. The rural location of the *uicus Palatius* is supported by medieval documents mentioning a toponym *Palaczu*, probably located at the western limit of the Ager Calenus, maybe alongside the via Faleria. *Esquelino* in his view could also be interpreted as opposed to *inquilinus*, thus referring to an extra-urban settlement. In his view the fact that none of the other inscriptions found at Cales contains any reference to these or other *uici* further undermines the urban hypothesis. For a critique of his arguments see Tarpin (2002, 87 n. 2), who is particularly sceptical about the *inquilinus* argument. The fact that the division of towns into *uici* named after the hills of Rome is a practice that is clearly attested in the late-republican period (see Bispham 2000, 158, n. 5, with further references) is considered a strong argument in favour of the urban thesis (see Bispham 2006, 87–8). Although admittedly the similarity of names between the mid-republican Esquiline *uicus* from Cales and the late-republican urban *uici* named after Roman hills strongly suggests a correspondence of practice (cf. also the unnamed *uici* of Ariminum, which might correspond to the urban *uici* referred to in later inscriptions (e.g. *uicus Cermalus* (CIL 11.419) and *uicus Aventinus* (CIL 11.421)), this is not necessarily true. There is a remarkable correlation between the distribution of urban *uici* named after the hills of Rome and triumphal colonization, which suggests that the practice is part of a late-republican reform (both Cales and Ariminum were recolonized in the late-republican period). The urban thesis therefore depends on one piece of evidence (the *veqo Esquelino* inscription) of which the provenance is unclear and the meaning of which can be explained differently. Paolino Mingazzini (1958), for example, proposes a different reading; in his view *Calebus* refers to the place of birth of the potter, while *Veqo Esquelino* is the place of production (i.e. the Esquiline in Rome). Moreover, he argues that adding one's ethnic identity is only meaningful if one works outside the place of origin. Interesting in this regard is another very famous potter of this period, Lucius Canoleius, who signs his vessels mostly 'L. CANOLEIOS L. F. FECIT CALENOS'; no *uicus* is added (see for numerous examples Pagenstecher 1909, 87–90). The thesis of Mingazzini is criticized by Lucia Sanesi (1978). Her main piece of evidence against the explanation of Mingazzini is the *uicus Palatius* of Cales. As discussed above, however, this could be explained as the result of the late-republican recolonization. Another argument is that there is archaeological evidence

VI. *Evidence for land division*

The existence of an *ager diuisus* is firmly attested for the late Republic and for the Empire, and in the *Corpus Agrimensorum Romanorum* we can even find drawings of this specific type of landscape.<sup>76</sup> For the mid-republican period such information is lacking, but two important pieces of evidence are often used to corroborate its existence in this period: 1) references in the sources that land was distributed in equal parts; and 2) traces of centuriation.

According to Livy land was distributed in the Latin colonies of Copia, Vibo Valentia, Bononia, and Aquileia. Furthermore, Livy informs us that a distinction was made between the size of the allotments of the *pedites* and those given to the *equites*: the latter generally received lots twice as large (see fig. 1).<sup>77</sup> All these references, however, concern colonies founded after the Second Punic War. For the earlier colonies the number of settlers is occasionally mentioned, but never is there any reference to allotment size. Is this silence meaningful, or do the sources simply neglect to inform us about it? The conventional view is that the latter alternative is correct. Since there are other references to the allocation of conquered territory in the pre-Hannibalic period in the form of viritane distributions (e.g. at Veii, in the Ager Latinus, and in Sabinum) and to the obscure allocation of very small parcels of land (2–4 *iugera*) in the so-called *Coloniae Priscae Latinae* of Labici and Satricum and in the *colonia maritima* at Anxur, it is presumed that this

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for the production of relief BG pottery in Cales. This fact does not necessarily prove that Kaeso Serponius worked at Cales (maybe he learned his trade there). During the survey of Cales discussed above various pottery sherds were found which were signed by the potter; among the many names no Serponius is recorded (they are mostly Atilii, Gabinii, and Paconii), nor any Canoleii (see Pedroni 1990, 177–183). The evidence from Ariminum does not reveal anything about the location of the *uici* or about the people inhabiting them. It is interesting to note that the *pocula deorum* refer not only to *uici* but also to *pagi* (see Franchi de Bellis 1995). An important argument against the urban thesis is the fact that colonial *oppida* were relatively small, so that the division of these centres into separate quarters is not easily explained (e.g. Mingazzini 1958). One can add to this argument the lack of archaeological evidence for the existence of developed and densely populated urban centres (discussed above). See also Stek (2008 esp. 132) for a religious argument in favour of mid-republican rural *uici*.

<sup>76</sup> See Campbell (2000, 278–316).

<sup>77</sup> In Aquileia there is a third intermediate class of *centuriones*.

must have been a general practice when people were sent to colonize recently conquered land.<sup>78</sup>

The historicity of these early allocations, however, is doubtful; and the 2 *iugera heredium* as the basic unit of virginate distribution, allegedly established by Romulus, looks particularly suspicious and could be a pseudo-historical reconstruction based on the fact that one *centuria* consisted of 100 plots of 2 *iugera*.<sup>79</sup> On the other hand the sometimes strange allocations of, for example, 3 7/12 *iugera* in Volscian territory and 2 *iugera* in the Ager Latinus supplemented with 3/4 *iugera* in the territory of Privernum are difficult to explain simply as ideological constructs (although the economic or social rationale behind such strange allotments is obscure). Whether fictional or true, the distribution of land in this early period does not necessarily imply that the same method was used in all colonial situations. The virginate assignments and the distribution of land in Roman colonies are from a socio-juridical point of view not comparable to Latin colonization. In these cases land was distributed in the Ager Romanus, where the colonists maintained their Roman citizenship, while in the Latin colonies settlers exchanged their former civil status for a new Latin one. Since the social position of Roman citizens was largely dependent on the ownership of land, it was important to regulate the amount of land each colonist received, in order to maintain the social and political status quo. However, the amount of land acquired by Latin colonists did not affect the Roman social balance since Latin colonies were independent communities.

Admittedly, the assignments recorded for the so-called *Coloniae Priscae Latinae* are more problematic because they were supposedly the model from which the later Latin colonies developed, and it is usually assumed that joining such a new Latin-Roman community involved the loss of Roman citizen rights. The acceptance and interpretation of this specific category, however, has been criticized, and it has been argued that these normative categories are probably anachronistic creations of historians from the late republican and modern period.<sup>80</sup> Quite apart from these doubts, Tim Cornell has pointed out that the traditional understanding

<sup>78</sup> The identification of Circeii as the location of land in *Volscis* distributed to colonists (Livy 5.24) is not certain; it has also been suggested that it refers to Vitellia or to another unknown colony: see Salmon (1937).

<sup>79</sup> See Gabba (1984, 20). This conclusion is strengthened by the fact that 2 *iugera* is insufficient land to sustain a family.

<sup>80</sup> Crawford (1995, 190); Bispham (2006, 81–4).

of these colonies as founded on the initiative of the Latin League is untenable, since the sources clearly describe the process as an exclusively Roman affair. He argues that the relevant decisions were taken by Rome, and that despite the fact that in most cases Latins were included, this was not a general rule. Moreover, it is possible that “some of the earlier colonial settlements never became Latin colonies”.<sup>81</sup> Land bordering on the *Ager Romanus* “may have been annexed and assigned *uiritim* to Roman citizens who were not formed into a new community but remained citizens and were directly administered from Rome”.<sup>82</sup> As an example of such a procedure he points to Labici; one could also add the colonies sent to the Volscian frontier, and to Satricum (the only other two instances in which allotment size is reported).

Likewise, the distribution of land in the later Latin colonies does not necessarily imply that this practice was as old as Latin colonization itself. It is remarkable that the references to land allotments in Latin colonies start just a couple of years before the well-known change in policy regarding Roman colonies which meant they were no longer restricted in size nor placed only in maritime locations.<sup>83</sup> Other indications suggest that the character of Latin colonization also changed in the post-Hannibalic period. For example, while the late-fourth and third-century colonies were settled on freshly conquered lands, often in the frontline, the Latin colonies of the second century (like the Roman colonies of this period) were created in a period in which the Roman frontline had moved beyond the Italian peninsula, and Roman hegemony in Italy was indisputable. Such a situation improved the safety of the colonists considerably. Furthermore, the sources report that there were serious recruitment problems for colonies after the Second Punic War; this may have necessitated a different policy of colonization which was more attractive for settlers.<sup>84</sup>

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<sup>81</sup> Cornell (1995, 302). See also Oakley (1997, 341–4) on the subject.

<sup>82</sup> Cornell (1995, 302).

<sup>83</sup> The policy of sending only very small numbers of settlers (300) to Roman colonies changed in this period (2,000 seems to have been the new norm). See Salmon (1969, 95–109). See also Mouritsen in this volume for an interesting explanation of this change in policy.

<sup>84</sup> The changed terminology used to describe the colonists is also suggestive. While Latin colonists of the post-Hannibalic period are always referred to in military terms (e.g. *pedites*, *equites*, and *centuriones*, except when colonists are sent as reinforcements), the colonists of the late fourth and third centuries are consistently referred to as *homines*. It is tempting to see here a change in the recipients of land in colonies, from a more general group to veterans or at least *assidui*, potentially as a reward for their services.

In any case the absence of references to the size of allotments distributed to Latin colonists prior to the Second Punic War remains remarkable, especially since in all other cases (earlier and later) where the number of colonists is specified the size of the distributed allotments is added. Of course this does not prove that land was not distributed in equal parts in Latin colonies before the Second Punic War.<sup>85</sup> But it does illustrate that the assumption that equally divided landscapes existed is conjectural and needs to be proved.

The second argument in favour of early land-division programs is the traces of centuriation identified in various Latin colonial territories. In the landscapes surrounding most Latin colonies detailed study of aerial photographs has revealed stripes separated by regular intervals which are generally interpreted as evidence for land division. However, the problem with these traces is that, since centuriation was practised at various moments in Roman history, they are notoriously difficult to date. From the Gracchan period onwards we have detailed descriptions in the *Corpus Agrimensorum Romanorum* about where, when, and how land was divided; for the pre-Gracchan period, however, evidence is sparse and vague. In order to establish if the identified *limites* in a Latin colony were put in place together with or soon after its foundation one therefore has to rely on other information.

One important and often used argument is that of typology. A specific form of *limitatio* called *per strigas et per scamna* is considered to be one of the oldest forms of land demarcation, a view that seems to be supported by the fact that the Greeks used a similar system already in the archaic period.<sup>86</sup> In contrast to the canonical 20 × 20 *actus* square blocks (*centuriae*) that dominated the Gracchan land reforms, this type of land division consisted of intersecting parallel lines which do not form squares (*strigae* have a longitudinal direction, *scamna* a latitudinal one). If such a system is recognized near a colonial town centre, it is often assumed that it must date from the early colonial phase. This argument,

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The practice of rewarding veterans with land is attested for this period in the land distributed to the veterans of Scipio in Samnium and Apulia (see fig. 1 for reference). The changed terminology of course could also be the result of the hierarchical system of land allocation, which required a distinction between different classes of recipients.

<sup>85</sup> The absence of references to allotment size could of course also be explained as the result of the source Livy used for his description of this period which, in contrast with the sources he used for colonization prior to the Latin War and after the Second Punic War, did not report this sort of information.

<sup>86</sup> Fundamental for this view is Castagnoli (1953/1955).

however, has been criticised. Campbell, for example, has argued that this specific type of *limitatio* was also in use in later periods, and that the decision to use it seems to have been determined by geographical considerations (it is more practical in mountainous areas).<sup>87</sup>

Some other *strigae* and *scamna* may predate the foundation of the colony. The typical Roman measurement unit used for land divisions is the *actus* (120 feet = 35.51 m); distances between lines that are multiples of this unit are plausibly dated to the Roman period. However, there are instances where the distance between the lines cannot be expressed in *actus*. Castagnoli, for example, noted in the case of Cales that the 480 m between the lines is not a distance measured in *actus* but is better understood as a distance of 16 *uorsus*, a unit that was used by the Umbrians and Oscans.<sup>88</sup> The difference between the *actus* and the *uorsus* is that the latter consists of 100 *pedes* instead of 120. Somewhat confusingly, the *uorsus* used in this scheme appears to be based on a foot of *c.* 29.57 cm. This unit is larger than the Oscan foot of *c.* 27.5 cm but identical in size to the Roman *pes*. A similar situation can be found in Suessa Aurunca. Initially a 20 × 21 *actus* grid was identified in the coastal area to the south-west of the *oppidum*.<sup>89</sup> However, a new study by Chouquer identifies in the same area a grid of square blocks measuring 8 × 8 *uorsus* (*c.* 240 m).<sup>90</sup> Again it is not the Oscan foot that is used, but a unit comparable to the Roman foot. To this list of land divisions using an 8 *uorsus* interval grid another example can be added: in Luceria a grid of 13.38 × 26.76 *actus* has been reconstructed.<sup>91</sup> This

<sup>87</sup> Campbell (1996, 86) and Campbell (2000, lx–lxi). The *limites* identified at Alba Fucens (12 *actus*), for example, are usually dated to the foundation of the colony (see Liberatore 2001, 186–7 for a recent discussion). However, there is a description in the *Liber Coloniarum II* which describes a land allocation program in Alba in the imperial period which consisted of *limites* at 1,250 feet intervals. According to Campbell “there are no clear reasons for dating this system to the foundation of Alba”, and he suggests that they belong to the imperial division program (Campbell 2000 429 n. 196, with further discussion and references). For the passage in the *Liber Coloniarum II* see Campbell (2000, 192 line 11–17). The 1,250 feet (*c.* 370 m) distance between the *limites*, however, corresponds to *c.* 10.4 *actus*.

<sup>88</sup> Castagnoli (1953/1955, 3). Chouquer et al. (1987, 191), however, claims that the lines are *c.* 470 m apart, which approaches 13 *actus*. But see La Regina (1999, 9) who argues that the measured 467 m distance between the *limites* corresponds to a 17 *uorsus* grid with Oscan foot.

<sup>89</sup> See Arthur (1982) with further references.

<sup>90</sup> Chouquer et al. (1987, 169–70).

<sup>91</sup> See Volpe (1990, 209–13) with further references.

strange pattern of land division corresponds exactly to a  $16 \times 32$  *uorsus* grid.<sup>92</sup>

An explanation for this rather strange unit of measurement can be found in the Greek world: 240 m corresponds to 8 *plethra* (the Attic foot is the same as the Roman one), but unlike the Roman *actus*, which consisted of 120 *pedes*, the Greek *plethron* (like the Oscan *uorsus*) comprised 100 feet.<sup>93</sup> In Metapontum, for example, a *strigatio* with intervals of 8 *plethra* has been identified, and it is dated to the archaic period.<sup>94</sup> The Greek origin of this particular system does not exclude the possibility that Roman land surveyors adopted it. However, the fact that *limitationes* based on the Roman *actus* are dated to the same period (e.g. in Anxur founded in 329, for example, a *centuriatio* measuring  $20 \times 20$  *actus* has been identified and dated to the foundation period)<sup>95</sup> is not easily explained, especially from the ‘statist’ point of view which presupposes a specialized Roman apparatus that created these landscapes. The view that these systems belong to the pre-Roman period is therefore at least as plausible; they may in fact represent Oscan land division programs<sup>96</sup> using a technique adopted from the neighbouring Greek towns.<sup>97</sup> A pre-Roman date for these *limites* is further strengthened by the fact that

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<sup>92</sup> The situation at Interamna Lirenas is less clear. The distance between the *limites* is irregular, but Chouquer et al. (1987, 124–5) argue that they probably correspond to 13 *iugera* (c. 461 m). However, the distance is often larger than that, which makes it also possible that a 16 *uorsus* module was used (c. 480 m). Moreover, a recent survey of the epigraphic evidence from the area has revealed that the identified *strigatio* covers large parts of the territory of Aquinum. If correct, this would render the hypothesis that this land division program belonged to the early colonial period implausible. Either the territorial boundaries changed drastically in later times (held improbable by Solin) or the identified *limitatio* does not belong to the colony proper (Solin 1993, 370, esp. n. 16).

<sup>93</sup> See Front. *de limitibus*, (Cambell (2000, 10 line 16 to 19)) on the 100 foot unit used by the Greeks, Umbians, and Oscans. Frontinus seems to suggest that the *uorsus* is older than the 120 foot unit used in the *actus*. See also La Regina (1999, 5–9).

<sup>94</sup> Carter (2006, 95–96) with further references.

<sup>95</sup> See Quilici (1994, 128) with further references.

<sup>96</sup> According to Campbell (2000, lx) a pre-Roman date is plausible, but “we cannot exclude the possibility that the Romans learnt from and copied the practices of other Italic people”.

<sup>97</sup> In the case of Suessa one could even argue that the land division belonged to the undiscovered Greek town of Sinope (Livy 10.21.7). The existence of this town is disputed (see Arthur 1991, 24). Also interesting is the belief of the Romans themselves that the technique of *limitatio* was of Etruscan origin.

traces of land division programs based on a 16 *uorsus* grid are known from non-Roman colonial contexts also.<sup>98</sup>

Another interesting case is the *limitatio* documented in the territory of the Latin colony of Paestum. In an extensive study Domenico Gasparri identified to the north of the city 22 division lines covering an area of 30 km<sup>2</sup>. In a first article he states that the lines are between 284 and 290 m apart, which corresponds to 8 *actus* (c. 284 m).<sup>99</sup> This pattern is especially interesting since near Cosa, founded in the same year, a *strigatio* of 16 *actus* (568 m) has been identified.<sup>100</sup> Excavation near Paestum has shown that the *strigae* represent channels flanked by roads, and a clear *ante quem* of AD 79 could be established (based on the volcanic stones deriving from the AD 79 eruption of Vesuvius that covered the feature). Ceramics found in the channel, dating mostly to the late fourth century, provide a *terminus post quem*. The combination of finds, according to Gasparri, strongly suggests a date around the foundation of the colony in 273.<sup>101</sup> After more careful measurement of the distance between the *strigae*, however, it appeared that the distance had to be adjusted to c. 275 m, somewhere between 7 and 8 *actus*.<sup>102</sup> Recently Michael Crawford has drawn attention to the fact that this distance is exactly 1000 Oscan feet or 10 *uorsus* (the Oscan foot being c. 27.5 cm), suggesting that it is connected with the Lucanian phase of the *polis*.<sup>103</sup> A similar grid can be found in the territory of Luceria. To the east of Luceria twelve *limites* have been recognized, of which the four most southerly ones are probably best understood as a 16 × 32 *uorsus* grid with a foot of c. 30 cm (cf. above). The six most northern *limites*, however, are spaced at different intervals, of c. 550 m, which is

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<sup>98</sup> In the neighbourhood of Cumae, colonized by the Samnites in the late fifth century (Livy 4.37.1–2), a 16 *uorsus* grid has been identified by Jean-Pierre Vallat (1980). A different module of 6 × 11 *uorsus* has been reconstructed for Allifae: Chouquer et al. (1987, 155). According to Adriano La Regina these systems are best understood as part of Samnite land-division programs: La Regina (1999, 9) (also for a discussion of the relevant epigraphic evidence for the existence of a developed Samnite agrarian regime).

<sup>99</sup> Gasparri (1989, 258).

<sup>100</sup> Castagnoli (1956).

<sup>101</sup> Gasparri (1990, 233–8).

<sup>102</sup> *Ibid.*, 238 n. 20. Gasparri (1994, 153) mentions 270 m as the average distance, but on the following page he gives 270/280 m.

<sup>103</sup> Crawford (2006, 65).

usually interpreted as 15.5 *actus*.<sup>104</sup> This distance, however, corresponds exactly to 2,000 Oscan feet or 20 *uorsus*.

Besides these possible pre-Roman grids based on the *uorsus*, there are also land division systems dated to the early colonial phase in which the *actus* was used as the unit of measurement. The most famous example is the 16 × 32 *actus* grid which has been recognized in the territory of Cosa.<sup>105</sup> An important criterion used in favour of its early date is the difference in the orientation of the *limites* and that of the via Aurelia, constructed in 241, which traverses the area. The idea is that it makes most sense to use the main road as the central axis of the division system. Since this is not the case, the *limitatio* is considered to pre-date the construction of the road.<sup>106</sup> Similar arguments can be found for the dating of the land division systems recognized in the territories of Spolegium (16 × 16 *actus*)<sup>107</sup> and Ariminum (20 × 20 *actus*);<sup>108</sup> both grids have a different orientation from that of the via Flaminia, which was built in 220. However, since there are plenty of examples of land division systems that post-date the construction of the main road but have a very different orientation, this line of reasoning is not convincing. A good example is the centuriation of the Ager Campanus, dated to the late-republican period, which is not orientated on the via Appia.<sup>109</sup> Moreover, it is clear (and also admitted by the same authors who claim that the different orientation of the road is a clear *ante quem*) that the orientation of the *limites* is determined by the geomorphology and watercourses and has little to do with the Roman road network (see also below).

Although there is ample evidence for the existence of networks of *limites* in colonial territories, none of these can be dated with certainty to the early colonial period. The ongoing debate about the date of these lines is a clear illustration of the problematic nature of such

<sup>104</sup> See Volpe (1990, 209–213) for a good description of the *limites*. The two middle *limites* are spaced irregularly.

<sup>105</sup> Castagnoli (1956). See also Carandini (2002, 121–23). See however, for the possibility of a second-century date of these *limites*, connected with the supplement of colonists Cosa received in the early second century, Celuzza and Regoli (1985, 49 f.).

<sup>106</sup> E.g. Sisani (2007, 93 note 43).

<sup>107</sup> Sisani (2007, 93 f.) with further references.

<sup>108</sup> Chouquer (1981, 843). See Bottazzi (1995) for some doubts concerning the reconstruction of the early 20 × 20 *actus* grid.

<sup>109</sup> See Chouquer et al. (1987, 199–206).

attempts.<sup>110</sup> The only clear argument for dating these land division systems to the early colonial period is the idea that land was divided in equal sized plots which therefore required some form of regular demarcation. As has been pointed out above, this argument is weak since there is no evidence for land being assigned in uniform plots. Moreover, the idea that the creation of a network of roads, channels, and ditches and vitrine land distribution are necessarily connected is not at all as self-evident as is usually presumed. Recently Franciosi has argued convincingly that the main function of such systems is drainage or irrigation of land, and that they are not necessarily associated with the demarcation of individual holdings.<sup>111</sup> On this view the *limites* might be regarded as evidence for land reclamation instead of land division. This also implies that we do not need to imagine a specific moment of land distribution. A more gradual development of these landscapes can be supposed, determined by the need and capacity of a community to exploit the more productive low-lying lands suffering from seasonal water disturbance.<sup>112</sup>

To cut a long story short, there is no convincing literary or archaeological evidence for the existence of a geometrically ordered *ager diuisus et adsignatus*, created by Roman engineers just before the arrival of the colonists, in any Latin colony founded before the Punic Wars. The assumption that all colonies must have had such centuriated territories

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<sup>110</sup> E.g. the 25 × 16 *actus* grid identified at Beneventum is dated to the Imperial period (Chouquer et al. 1987, 159), based mainly on a reference in the *Liber Coloniarius* see Campbell (2000, 164 line 20–21) or to the early colonial phase (Patterson 1991 and Torelli 2002, 74–7, who point to similar grids known for Vibo Valentia (*Liber Coloniarius* Campbell (2000, 164 line 16–17) and Velia (*Liber Coloniarius* Campbell (2000, 164 line 9)). Both Beneventum and Vibo, however, received colonists during the late Republic, which could explain the similarity. Similarly, the 15 × 15 *actus* grid identified in the territory of Fregellae is dated by Chouquer et al. (1987, 126–7) to the late-republican period, while Coarelli and Monti (1998, 109–11) think an early-second century date is more probable.

<sup>111</sup> Franciosi (1997).

<sup>112</sup> It is noteworthy that many of the identified *limites* are located in the lower-lying parts of the landscape, and that their orientation is clearly determined by geomorphology and watercourses. In other cases, as for example at Paestum, the systems of channels appear in marshy areas, clearly as an attempt at drainage. After its abandonment in the Roman period the area has only recently been used again for agricultural purposes, as a result of major drainage schemes in the 1930s and 1950s. The *limitatio* at Luceria is a good example of a system that grew gradually. The lines, as describes previously, are spaced at different intervals and also result from the use of different measurement units; this strongly suggests that the system was not created at once.

is conjectural and at present rests on a rather static and synthetic idea of how Romans controlled and organized conquered territory.

## VII. *Conclusion*

Latin colonization has long been considered one of the few cases in mid-republican history where archaeology and the ancient literary sources converged: both datasets in their own way seemed to illustrate parts of the same remarkable story. After critical restudy of the available evidence, however, this initial enthusiasm seems to have been over-optimistic, as it turns out that the perceived compatibility is largely the result of a one-sided interpretative process in which textual sources from the late-republican and imperial periods were used as manuals with which to decipher the archaeological findings. With regard to the rural aspects of Latin colonization, one can clearly see the influence of the *Corpus Agrimensorum Romanorum*. The powerful image of orderly divided landscapes which emerges from these imperial treatises is a very attractive model, especially as it fits nicely with other aspects of colonization and mid-republican society known from the sources, such as land division, the egalitarian ideal, and Roman military and political superiority. Archaeology also contributed to this image: surveys of Roman landscapes demonstrated that large numbers of modestly-sized isolated farmsteads were located in the countryside, and (most important of all) traces of centuriation could be seen on aerial photographs.

Closer analysis of this evidence, however, reveals the convergence between the written sources and the archaeological data to be more apparent than real. The density of recorded third-century sites is only a fraction of what must be expected based on the estimated size of the allotments which colonists received, and more importantly the clustered configuration of sites is very different from the evenly dispersed settlement that was anticipated. This discrepancy is often explained as resulting either from the inability of archaeology to record the colonists' dwellings, or from the choice of colonists to live in the urban centre. Since the latter explanation is implausible—in the majority of cases the recorded number of colonists cannot have fitted inside the colonial town centre—the first is regarded as the most serious candidate. The question, however, is whether we should use a hypothetical reconstruction of colonial rural organization, based mainly on late-republican

evidence, as a touchstone against which the quality of the archaeological sample can be measured.

If we release ourselves from the *idée fixe* that Latin colonies in their early years were organized according to the city-state model, consisting of an urban centre and a rigidly ordered hinterland, it becomes possible to recognize other forms of settlement organization. Although the archaeological evidence is still very flimsy, the information presently available seems to indicate that a multiple-core nucleated settlement system was the norm in most mid-republican colonial landscapes. Such a settlement system had its obvious advantages in an early colonial context. One has to remember the potentially hostile environment which the colonists were entering, their fields only recently conquered and surrounded by non-Roman people with possibly hostile intentions. It therefore made sense to live in larger and better defensible settlements and work the surrounding fields from there, rather than to live in isolated farmsteads spread out over the territory, or to leave the whole rural area unprotected by opting for residence in a single urban centre.

From an archaeological point of view it is only from the late third and early second centuries that colonial landscapes begin to approach the city-state model, with a more marked division between rural and urban space. It is only from this time that colonial *oppida* become proper urban centres, and the sources start to report land division programs. Much more research has to be done, especially and most urgently on the neglected secondary population centres, before we can truly start to rewrite early colonial history. However, the archaeological evidence presently available sits very uneasily with a reconstruction of Latin colonial territories which sees them as state-organized landscapes broadcasting a message of Roman supremacy.

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## POLYBIUS AND THE FIELD SURVEY EVIDENCE FROM APULIA

Douwe Yntema

### I. *Polybius' list*

The Greek writer Polybius admired the Romans, but not uncritically. He was also a very keen observer. His description of the burial rites of the Roman elite, for instance, is close to a well-informed anthropologist's view of a strange tribe.<sup>1</sup> Living as a hostage in Rome, he moved in high circles and was well informed on many things Roman. He was both an insider and an outsider to the Roman world. Polybius is therefore a very attractive source for the third century BC. One of the tantalizing passages in his *Histories* is the list of troops that could be supplied by the *socii* of Rome shortly before the Second Punic War (Plb. 2.24.10–11).

The lists of men... were as follows: Latins 80,000 infantry, 7,000 cavalry, Samnites 70,000 infantry, 5,000 cavalry; Iapygians and Messapians 50,000 infantry, 6,000 cavalry;<sup>2</sup> Lucanians 30,000 infantry, 3,000 cavalry; Marsi, Marrucini, Frentani and Vestini 20,000 infantry and 4,000 cavalry.... (translation DY)

This survey gives fairly specific quantities of troops for various regions of Italy. It should be noted that Polybius lists the able-bodied men. This suggests that the quantities given by the author can be supposed to be approximately the maximum quantities of foot soldiers and horsemen that could be mustered from a particular region. They probably include everybody who could bear arms and could be put into action in case of emergency, i.e. approximately every man between 18 and 45 years old.<sup>3</sup> Therefore Polybius' list is especially important: it might well serve as the basis for an estimate of the population figures for considerable parts of Italy in the later third century BC.<sup>4</sup>

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<sup>1</sup> Plb. 5.3.

<sup>2</sup> Polybius' text mentions 16,000 horsemen of the Iapygians and Messapians, but since this quantity is obviously disproportionate, the text is usually emended.

<sup>3</sup> For discussion of this topic, see Hin in this volume (section 4).

<sup>4</sup> The Greek *poleis* of southern Italy are, for instance, absent from this list.

The crucial question therefore is: are Polybius' figures reliable? Ancient authors often mention symbolically or propagandistically the size of military forces or how many perished in a battle. Did Polybius give accurate figures or did he follow the almost general trend by giving more or less symbolic quantities of troops, and must we read 'ten thousand' actually as 'very many'? Can we actually attempt to derive population figures for third century BC Italy from Polybius' list of troops?

## II. *Archaeological field surveys*

A reliable check on this type of demographic information in historical sources can usually be made by consulting archives. But this is not possible for the Roman Republic. In the case of Polybius' data, however, there is an alternative source of information that may assist us in establishing their value: the archaeological archives hidden in the Italian soil.

From the early 1960s onwards some of these archaeological archives have begun to be opened up. An early and important initiative was the South Etruria survey, carried out by the British School at Rome in the 1960s and 1970s.<sup>5</sup> Initially, the main objective of rural surveys was to get a better grip on the ancient countryside. Since pre-industrial economies were predominantly agricultural economies, insight into the rural world was believed to be crucial to the understanding of the societies of the past. The survey archaeologists located, among other things, prehistoric compounds, pre-Roman farmsteads, Roman villas, and medieval hamlets. They did so in order to discover settlement patterns and produced maps for various historical periods. These were thoroughly analyzed and the shifts in these patterns over time were explained. The most intensive variant of these is the urban survey, in which the method of field walking and mapping of artifacts collected from the surface was applied to abandoned settlements of urban character, revealing important data concerning the settlement's occupational history.<sup>6</sup>

Since the time of the first intensive field surveys (the 1970s), field survey methods have become increasingly sophisticated. This happened as the result of, among other things, the introduction of auguring,

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<sup>5</sup> Report in Potter (1979).

<sup>6</sup> E.g. Bintliff and Snodgrass (1988; 1991).

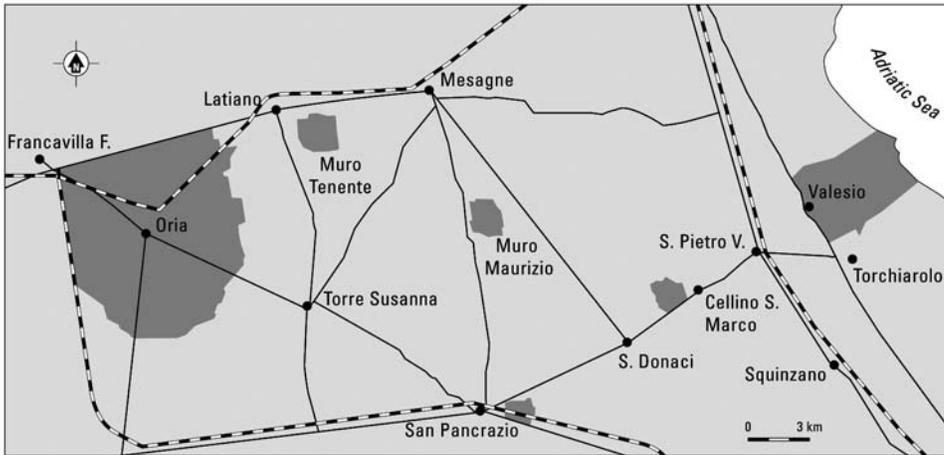


Fig. 1. Salento peninsula, Apulia. Field surveys areas (VU University Amsterdam, 1981–1995).

ground-penetrating radar, and GPS systems. But great progress has also been made in the interpretation of the results. Today the archaeological field survey is far removed from the romantic image of nineteenth-century prospection archaeology.<sup>7</sup> It has become a very useful tool for archaeologists. It enables them to generate many new data concerning a wealth of new aspects of the past. Whereas excavation supplies detailed information about a small area, field survey generates more generic data about large areas. The interpretation of these field surveys documents processes such as the human impact on the natural landscape, urbanization, ruralization, the birth of and the changes in regional settlement hierarchies, and abandonment of the countryside in favour of fortified towns.<sup>8</sup> It is particularly these specific characteristics of the field survey that are crucial in order to judge the value of the Polybian data on the quantities of troops in the passage quoted above.

In 1979 the Institute of Archaeology of the VU University, Amsterdam, decided to carry out a series of high intensity field surveys on the isthmus between Taranto and Brindisi (Apulia, Italy). This is the northern part of the territory of the tribe of the Messapians mentioned in Polybius' text. The area is known in Italian as the northern part of

<sup>7</sup> See e.g. Bintliff, Kuna et al. (2000).

<sup>8</sup> The rise of the archaeological field survey is closely linked to the rise of processual archaeology in the 1970s.

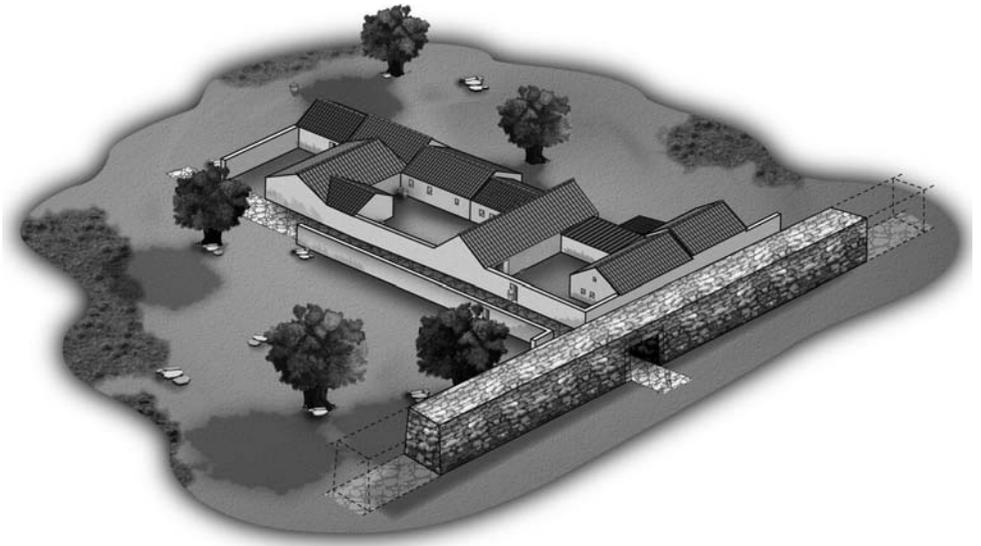


Fig. 2. Muro Tenente (Messapia): Quarter with urban farms and artisans' workshops, c. 280–230 BC).

the *Penisola Salentina* or simply '*Salento*'. After some initial reconnaissances in 1979 and 1980, three large campaigns were held in a 66 km<sup>2</sup> area surrounding the important pre-Roman settlement of Oria (the small Roman *municipium* of *Uria Calabria*) between 1981 and 1983. The most important aim of the Oria field survey was to reveal the dynamics of rural occupation in the survey area during the first millennium BC and the first millennium AD.<sup>9</sup> In the following two decades several other rural surveys were carried out near the pre-Roman settlements of Valesio (the Roman *Mutatio Valentia*; 1989–1992) and Ostuni (1999–2001), whilst the settlement areas of four pre-Roman walled sites were surveyed: Valesio (80 ha; 1985–1987), Muro Tenente (50 ha, 1992–1995), San Pancrazio (57 ha, 1994–1995), and a pre-Roman hamlet near Cellino San Marco (6 ha; 1995) (see fig. 1). Moreover, at the site of Muro Tenente two settlement areas were excavated in the years that followed the urban survey of the site (1996–2002). These digs enabled us to check the results and the interpretation of the survey (fig. 2).

These high intensity field surveys supplied vital data about a variety of ancient settlement types. Moreover, the survey areas were selected in

<sup>9</sup> Yntema (1993a).

such a way that insight was gained into human occupation on a wide variety of soils in a wide variety of landscapes.

While the Valesio field survey (20 km<sup>2</sup>) was carried out in a flat coastal area, the site of Oria is definitely inland, and the survey here (66 km<sup>2</sup>) was done in a gently sloping landscape. The Ostuni survey covered rugged areas and limestone soils with thin cover. The results of this fieldwork are therefore likely to offer a representative picture of human occupation in a considerable part of the territory of the tribal Messapians. All in all, an area of approximately 95 km<sup>2</sup> including all soil types and all types of landscapes was investigated very intensively.

The results of these field surveys have been published in various articles and two books.<sup>10</sup> The best dating evidence is supplied by the pottery fragments collected from the surface. These ceramics can often be closely dated for the first millennium BC and the first five centuries AD. The fine wares of the Hellenistic period can be dated with a precision of c. 30 to 50 years. It is possible therefore to construct an image of the occupation of the investigated areas for a particular period of time. Since the survey areas have been selected in a representative manner, this image is likely to hold good for the whole district inhabited by the Messapians.

### III. *The territory of the Messapians in the third century BC*

We have seen that Polybius recorded the number of troops that could be mustered in about the third quarter of the third century BC. The archaeological data indicate that conditions in this period were largely a continuation of the situation in the preceding half-century. Both the final quarter of the fourth century BC and the final quarter of the third century BC, however, were periods of substantial change.<sup>11</sup> In order to create an image of the human presence in the Messapian district at the time of Polybius' list, we have to look at the archaeological data

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<sup>10</sup> For most important reports on these surveys see Boersma et al. (1991: rural survey Valesio), Yntema (1993a: rural survey Oria; 1993b: urban survey Valesio), Burgers (1998: urban surveys of the sites of Muro Tenente, Muro Maurizio, San Pancrazio and Cellino San Marco), Burgers, Attema, & Van Leusen (1998: rural survey Ostuni).

<sup>11</sup> Cf. Yntema (2006). This situation began to change in the final decades of the third century BC. There was a truly dramatic change in southeast Italy in the first half of the second century BC (Yntema 2006).



Fig. 3. Muro Tenente (Apulia). Defenses of the Messapian town, *c.* 290/280 BC.

for the period between the end of the fourth century BC and the last third of the third century BC.

Both excavations and field surveys have demonstrated that there was a tremendous boom in urban and rural occupation in the Messapian district in the final decades of the fourth and the early years of the third century BC. Whereas only a few settlements were walled in the sixth or fifth centuries BC, all the larger centres were surrounded with impressive town walls by the early years of the third century BC (fig. 3). Moreover, urban settlements had by far their largest extent during the period under discussion.<sup>12</sup> The present evidence suggests that completely new quarters were constructed in the early years of the third century BC in areas formerly given to agriculture; these were mostly on the periphery of the intra-mural area. These new quarters close to the new walls contained urban farmsteads and artisans' workshops. In

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<sup>12</sup> E.g. Yntema (1993b); Burgers (1998).

the old central area of the settlement the dwellings were augmented and monumentalized. These are likely to have belonged to the local elites.<sup>13</sup> Between *c.* 330/300 and *c.* 250/230 BC the towns of the Messapians flourished greatly.

The same holds good for the countryside surrounding these towns. Whereas there was no permanent habitation here prior to the late fourth century BC, the rural areas appear to have been littered with farmsteads and hamlets from the final years of the fourth century BC onward.<sup>14</sup> At inland Oria some 30 farms were discovered on *c.* 40 km<sup>2</sup> of fertile soils,<sup>15</sup> and at least 48 rural habitations were discovered in the 20 km<sup>2</sup> area between the urban site of Valesio and the sea (fig. 4).<sup>16</sup> Both the architectural remains of the farmsteads and the adjoining rural necropoleis indicate that the farms were basically one-family units with a population of 8–12 persons.<sup>17</sup> The hamlets were nuclei of three to five farmsteads with *c.* 25–60 inhabitants. Ecological data suggest that both the urban and the rural boom coincided with a specialization in agriculture in which large-scale cultivation of corn, vines, and olives replaced the more subsistence agriculture of the fifth and earlier fourth centuries BC. It was also in the early Hellenistic period that the first series of transport amphorae were produced in the district under discussion.<sup>18</sup> This phenomenon, of course, supports the claim made on the basis of the ecological data.

#### IV. *Population figures on the basis of the field surveys*

We have seen that since these data were collected in the Messapian district from various geophysical units and from various types of soils,

<sup>13</sup> Cf. Burgers and Yntema (1999).

<sup>14</sup> Yntema (1993a); Boersma et al. (1991).

<sup>15</sup> The Oria field survey was carried out at a relatively early stage of our fieldwork in southeast Italy. That means that we may have missed a few scatters referring to farms and that the actual quantity of farmsteads may well have been larger in the Oria countryside during the third century BC than the 30 rural sites in the record.

<sup>16</sup> During the Valesio field survey only the rural area on the coastal side (east of Valesio) was investigated (see also fig. 3).

<sup>17</sup> In two cases in the Oria survey we were able to establish that a rural necropolis close to a farmstead contained about 25–30 graves, covering three generations. Since it is possible that not everyone living on a farmstead received a formal or archaeologically traceable burial (e.g. slaves, infant burials), at least 8–10 persons must have lived at a farm at the same time.

<sup>18</sup> Vanderersch (1994).

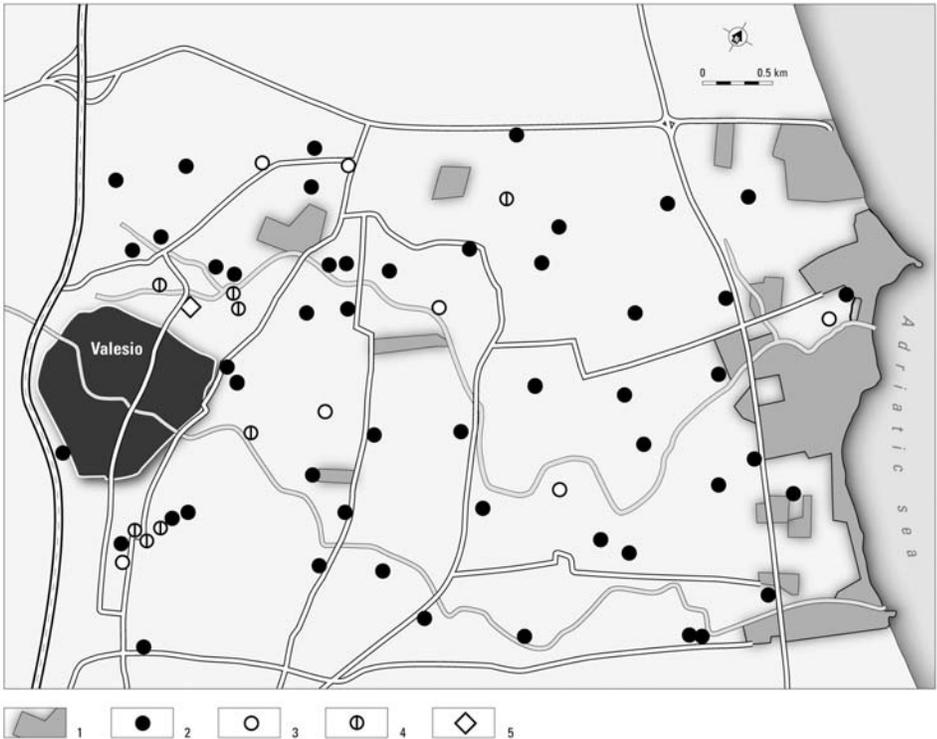


Fig. 4. Valesio (Apulia: field survey area of approximately 20 km<sup>2</sup>: rural sites occupied between *c.* 300 BC and *c.* 230 BC (1. plot not investigated; 2. farmsteads; 3. probable farmsteads; 4. scatters with amphorae, tiles, and storage jars; 5. extramural sanctuary).

they may well be representative of the whole district. These factors of course invite us to make a rough estimate of the population that lived in the district. Long before I came across the Polybian list of troops I simply wished to have an idea—generalized as it might be—of the number of people who had lived Messapia, and I therefore started to make calculations on the basis of our field surveys. We have seen that the territory of each walled settlement of the Messapians may have harboured some 30–60 farmsteads (average 45; based on an estimated recovery rate of *c.* 50%). Since there were between 26 and 28 of these fortified settlements in the area under discussion in the third century BC,<sup>19</sup> the actual number of Messapian farmsteads may have

<sup>19</sup> For lists of Messapian walled settlements see D’Andria (1993, 446–7) and Burgers (1998, 228–9).

been approximately  $45 \times 26\text{--}28 = c. 1,560\text{--}1,680$  farmsteads. Since these farms can be shown to have housed between 8 and 12 people, the rural population of the Messapian district may have amounted to approximately 10,000–15,000 individuals.<sup>20</sup>

A problematical factor in the demographic picture is the category of (probably small) coastal settlements. A few major settlements of the Messapians were on the coast (e.g. Otranto, Brindisi, Gnathia), but there were several fortified towns in the sub-coastal area: these were linked to small ports of call on the coast.<sup>21</sup> It is these ports that are badly known, either because they ended up in the sea as a result of coastal erosion, or because modern building activities on or near the seashore have destroyed them. There may have been between 9 and 12 of these. But since they were small, each of them may have had a few hundred inhabitants only, and their absence from the record therefore does not significantly affect the total number of inhabitants of the district under discussion in the third century BC.

But can we make an educated guess concerning the urban population of the Messapians? Most Messapians were town dwellers, although their towns were basically dispersed settlements consisting of spatially separated, intra-mural settlement nuclei. The extent of these walled settlements differed substantially: the fortifications of the smallest towns enclosed approximately 30 ha, whereas the large tribal centre of Oria may well have extended over *c.* 100 ha.<sup>22</sup> The urban surveys have demonstrated that often only 50–60% of the area within the late fourth/early third-century BC walls displays signs of habitation during the third century BC (fig. 5). This is a severe bias that should be taken into account. Moreover, excavations have shown that the habitation nuclei within the fortifications were fairly densely inhabited. A hectare of inhabited settlement could probably house between 80 and 120 individuals. Starting from these excavational data, the population of a fortified Messapian settlement can be estimated at between *c.* 3,000 and *c.* 6,000 individuals.<sup>23</sup> Judging by the results of the various urban field surveys, the population of a Messapian town on average may have amounted

<sup>20</sup> Minimum: 9,360 ( $8 \times 1,170$ ); maximum: 15,180 ( $12 \times 1,260$ ) individuals.

<sup>21</sup> For these small coastal settlements linked with much larger fortified inland settlements, see D'Andria (1976: on Torre Santa Sabina) and Yntema (1982, 111 n. 80).

<sup>22</sup> D'Andria (1993, 446); Burgers (1998, 228).

<sup>23</sup> The late fourth/early third century BC fortifications surrounding these settlements also suggest the presence of a substantial population: a considerable working force was needed in order to construct these highly conspicuous walls (between 2.5 and 4 km long; 5 to 6 m thick; and *c.* 6 m high).

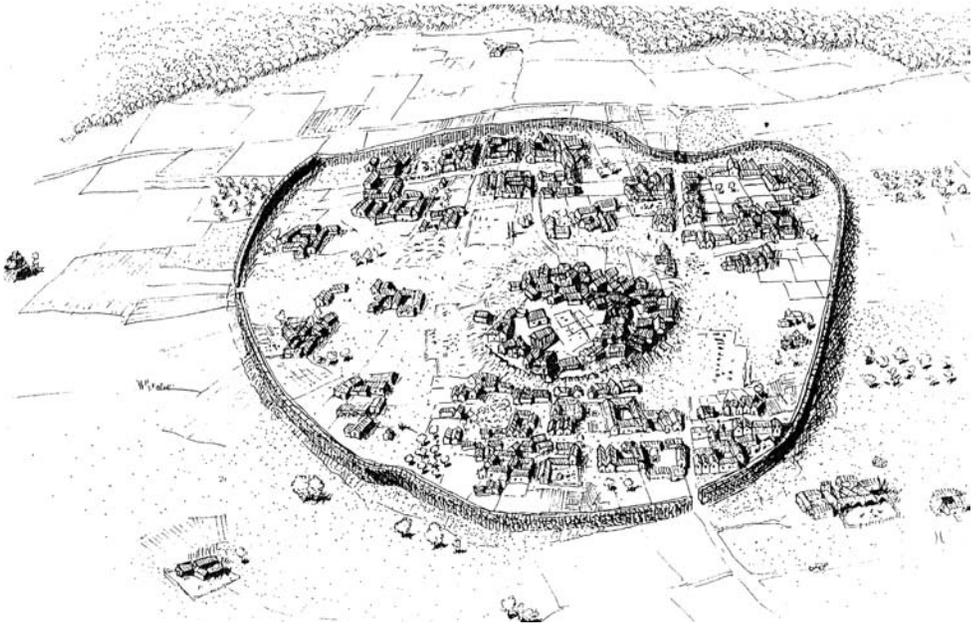


Fig. 5. Muro Tenente (Apulia). Artist's impression of the settlement in the third century BC, based on information from the urban survey, auguring, and excavations.

Table 1. Population figures in the tribal district of the Messapians on the basis of the results of the archaeological field surveys

	<b>Number of towns</b>	<b>Farms per town</b>	<b>Inhabitants per farm</b>	<b>Estimated rural population of Messapia</b>
Rural population	26–28	45	8–12	10,000–15,000
	Number of settlements		inhabitants	
Population of small ports	9–12		????	3,000–4,000?
	Number of towns		Inhabitants per town	Estimated urban population of Messapia
Urban population	26–28		4,000–4,500	104,000–126,000
<b>Total population of Messapia (according to the field survey data)</b>				<b>117,000–145,000</b>

to *c.* 4,000–4,500 individuals. Since there were 26–28 of these towns in the Messapian district, the urban population may have amounted to approximately  $26\text{--}28 \times 4,000\text{--}4,500 = 104,000\text{--}126,000$  individuals. To this quantity we must add some 10,000–15,000 farmers and their families. All in all, the lowest estimate for the Messapian population is *c.* 120,000 persons, whilst the maximum is approximately 145,000.

#### V. *Population figures based on Polybius*

In his list Polybius does not give us any quantities that correspond to the area in which the surveys were carried out. His text reads: ‘Iapygians and Messapians: 50,000 infantry, 6,000 cavalry’. We know that the Messapians lived in the southern part of Apulia with a population of approximately 120,000–140,000 individuals (estimate based on the field surveys).<sup>24</sup> The interpretation of the term ‘Iapygians’ is much more difficult,<sup>25</sup> but it was almost certainly used by Polybius as a Greek equivalent of the Roman term ‘Apuli’, i.e. the tribes living in present-day north and central Apulia.<sup>26</sup> Obviously Polybius combines in his list the tribal groups that spoke the Messapic language and that lived along the Adriatic shores of southern Italy. Since these tribes had many features in common, it is only natural that they are mentioned in the same breath in a general survey of Italic troops.

But how many Messapians were there among the 56,000 able-bodied men recorded by Polybius? It should be noted that the district of the Messapians was by far the most urbanized and most densely populated part of ancient Apulia. Whereas the Messapian district boasted 26–28 walled settlements, there were some 30–35 settlements of importance in the remaining part of Apulia. The ‘Iapygian’ settlements of North Apulia in particular were fairly dispersed, but there the population may well have been of the same order as that of the Messapian district: from settlements of 2,000–3,000 inhabitants to large settlements (e.g. Arpi and Canusium) of perhaps some 6,000–8,000 inhabitants. This means that densely populated Messapia with its 26–28 towns may well have supplied more than one third of the troops, perhaps some 40–45%.

<sup>24</sup> The names Calabri and Sallentini were used for the same population(s): see Yntema (2006, 93).

<sup>25</sup> Nenci (1978).

<sup>26</sup> The Polybian Iapyges were also known to the Greeks as the Daunioi and the Poiketioi (or Poidikloi).

Table 2. Population figures in the tribal district of the Messapians on the basis of Polybius' list

Polybius' Iapygians and Messapians	56,000 able-bodied men
Number of Messapians (= 40–45% of Iapygians and Messapians)	24,400–27,200 able-bodied men
<b>Total population of Messapia according to Polybius</b> (= 5 times 24,400–27,200)	<b>122,000–136,000 inhabitants</b>

Since Polybius records 50,000 infantry and 6,000 cavalry (= 56,000 men) for the Iapygians and Messapians together, the Messapians may have been obliged to supply approximately 24,400 (40%) to 27,200 (45%) able-bodied men to the Roman state in times of emergency.

The next question concerns the 'able-bodied' men. What is the percentage of this group in relation to the total population? Generally speaking, half of the population is female. Boys and elderly men should also be excluded. That means that the group of able-bodied men that could be called upon in emergencies may well have made up one-fifth of the total population.<sup>27</sup> If we multiply the estimated number of able-bodied Messapians which can be derived from Polybius (i.e. 24,400–27,200) by five, we end up with an estimate of between 122,000 and 136,000 for the entire Messapian population.

## VI. *Conclusions*

It should be noted that the calculations on the basis of the results of the field survey were made well before I read Polybius' celebrated passage cited above. Of course, there are many uncertain factors in these calculations. But the combination of high intensity *urban* surveys, high intensity *rural* surveys, and excavations in settlements which had also been subjected to field surveys has supplied the means to calculate approximate population figures in the past. They suggest that the total population of the Messapian district may have amounted to between *c.* 117,000 and *c.* 145,000 persons. Quite unexpectedly, however, this population figure appears to tie in well with the population figures

<sup>27</sup> Hin in this volume argues in favour of a multiplier of 4.6.

that can be derived from the Polybian list of troops, which suggests that some 122,000 to 136,000 Messapians were living in the southern part of Apulia in the mid to late third century BC. We may therefore conclude that the numbers of troops mentioned by Polybius are not purely fictitious. He may well have recorded the exact number of troops the allies (or subjects) of Rome could supply. It follows that if the manpower figures given by Polybius are multiplied by five, we obtain a rough estimate of the number of people that inhabited much of peninsular Italy in the third century BC.

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LUCANIAN LANDSCAPES IN THE AGE OF  
'ROMANIZATION' (THIRD TO FIRST CENTURIES BC):  
TWO CASE STUDIES<sup>1</sup>

Maurizio Gualtieri

*The strength of the material dimension lies in its presence everywhere, its weakness . . . in that we need much more than fieldwork, source criticism and analytical method to weight in societal and, not least, historical terms.* (Randsborg 1997, 194).

I. *Methodological issues and geographical background*

Since S.L. Dyson's epoch-making study of settlement patterns in the *ager Cosanus* (now almost 30 years old)<sup>2</sup> a growing number of scholars (I am referring particularly to archaeologists and historians of ancient Rome) have come to accept the relevance of survey data<sup>3</sup> and the related reconstruction of the ancient landscape, whether on a micro-regional or macro-regional scale, for an understanding of changes in the pattern of rural settlement, land-use, and not least of general demographic trends.<sup>4</sup> Thus, even in the face of a growing awareness

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<sup>1</sup> While thanking the University of Leiden and Prof. Luuk de Ligt for the invitation to participate, I also wish to express my deep gratitude to the organizers of this Conference, who have provided a truly interdisciplinary atmosphere for the discussion of a crucial topic in the economy and society of the Italian peninsula during the late republican period. For the general historical picture I would refer the reader to the still fundamental contributions by E. Gabba: Gabba (1974) (in particular the crucial observations at p. 138 on the significance of the 'mito della piccola proprietà contadina' in the context of the Gracchan reforms) and Gabba (1990).

<sup>2</sup> Dyson (1978). Dyson (1979) approaches the problem of the reconstruction of rural settlement patterns for the study of Roman agrarian systems from a more general perspective.

<sup>3</sup> In spite of the many interpretative difficulties and major limitations inherent in this particular type of archaeological evidence: see Bintliff (1997) and Dyson (2003).

<sup>4</sup> Fundamental discussion with numerous case studies in Bintliff and Sbonias (1999). A very recent discussion is Witcher (2005), who rightly points out that "There has been much debate about the use of survey (data) for modelling ancient demography" (125). He also includes a detailed analysis of the density of rural settlements in the *suburbium* and the possible demographic implications (125–7, with all previous bibliography). See also his contribution to this volume. For the specific case of the survey of rural settlements in Lucania, and possible demographic implications, see *Lucania romana* (31 and n. 46).

of the need for more systematic and controlled ways of collecting surface material and for a rigorous methodology in the interpretation of surface scatters (I prefer not to linger on these aspects of survey data in the presence of John Bintliff, who has written fundamental contributions on this topic),<sup>5</sup> surface survey and the study of ancient landscapes have played a growing role in historical reconstructions which aim to free themselves from the limitations of a purely text-based approach. I would add that, for the specific historical issues which are the subject of our Conference, landscape archaeology is providing major insights which may verify and/or refute a number of gross exaggerations (in some cases true misunderstandings) in what we have become used to labelling as the ‘conventional view’ of the transformation of Italy in the post-Hannibalic period and especially of agrarian change in the second century BC.<sup>6</sup>

The region under consideration is western Lucania (fig. 1), an area unquestionably targeted in the Gracchan land assignments,<sup>7</sup> as shown by the impressive concentration of inscribed boundary stones found in the fertile inland region of the Vallo di Diano (fig. 2), through which the important road from Capua to Rhegium (the *via Popillia*) was built in the second half of the second century BC (fig. 2). Kahrstedt’s classic study on *ager publicus* (fig. 3) took this region particularly into consideration.<sup>8</sup> Within it the two areas with which I am most familiar, thanks to numerous seasons of fieldwork, are the Mingardo-Bussento region immediately south of the Vallo di Diano, and the territory of modern Buccino/ancient Volcei, immediately to the north-east of the Vallo di Diano.

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<sup>5</sup> Of his major contributions to the interpretation of survey data I would refer the reader to Bintliff (1991; 2002) and Bintliff and Sbonias (1999). Many of the papers presented at the Conference held in Groningen in 2000, and particularly those in the session on “Comparative Settlement Archaeology” underline the major points of the most recent debate: see now Attema et al. (2002, 69–123). For more specific reference to southern Italy see the exemplary studies of Yntema (1993) and Burgers (1999).

<sup>6</sup> For a recent analysis of the problem, which also takes into proper consideration the archaeological evidence, see now the papers by Gabba, Lo Cascio, and Grelle in *Modalità insediative*. A restatement of the conventional view of decline and depopulation in southern Italy after the Hannibalic war is made by Cornell (1996). Crawford (2003) effectively points out that the weak point in Cornell’s argument is its total disregard of the archaeological evidence.

<sup>7</sup> The most recent summary with all previous bibliography is Franciosi (2002).

<sup>8</sup> Kahrstedt (1959).

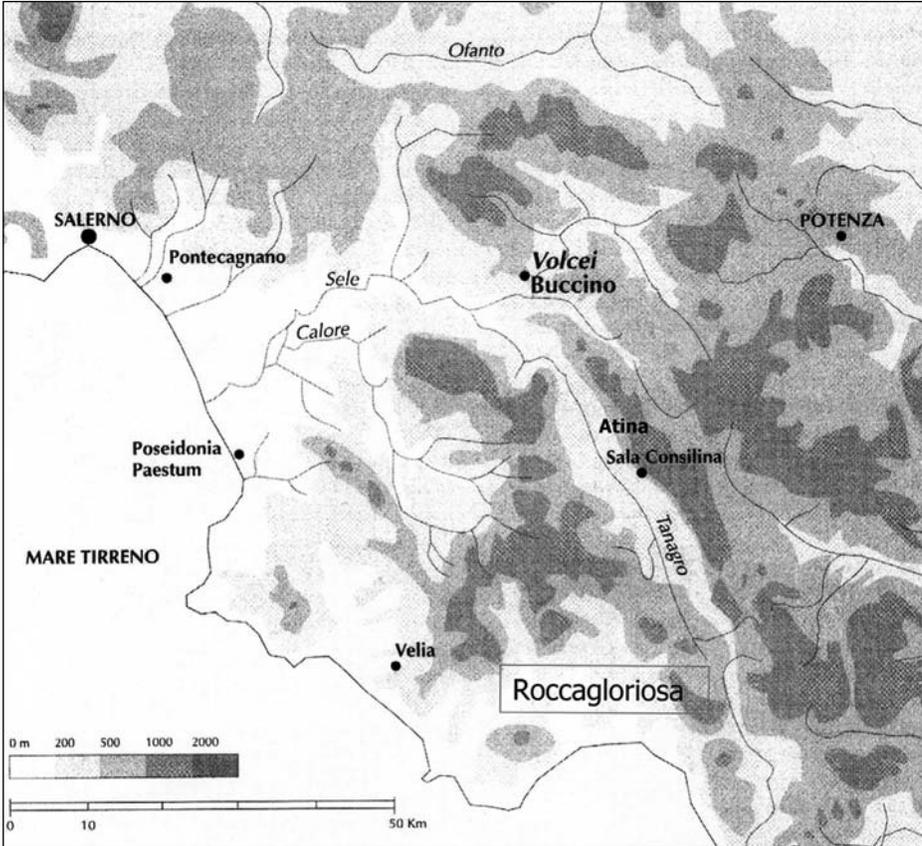


Fig. 1. Map of ancient Lucania with indication of major areas selected as case studies.

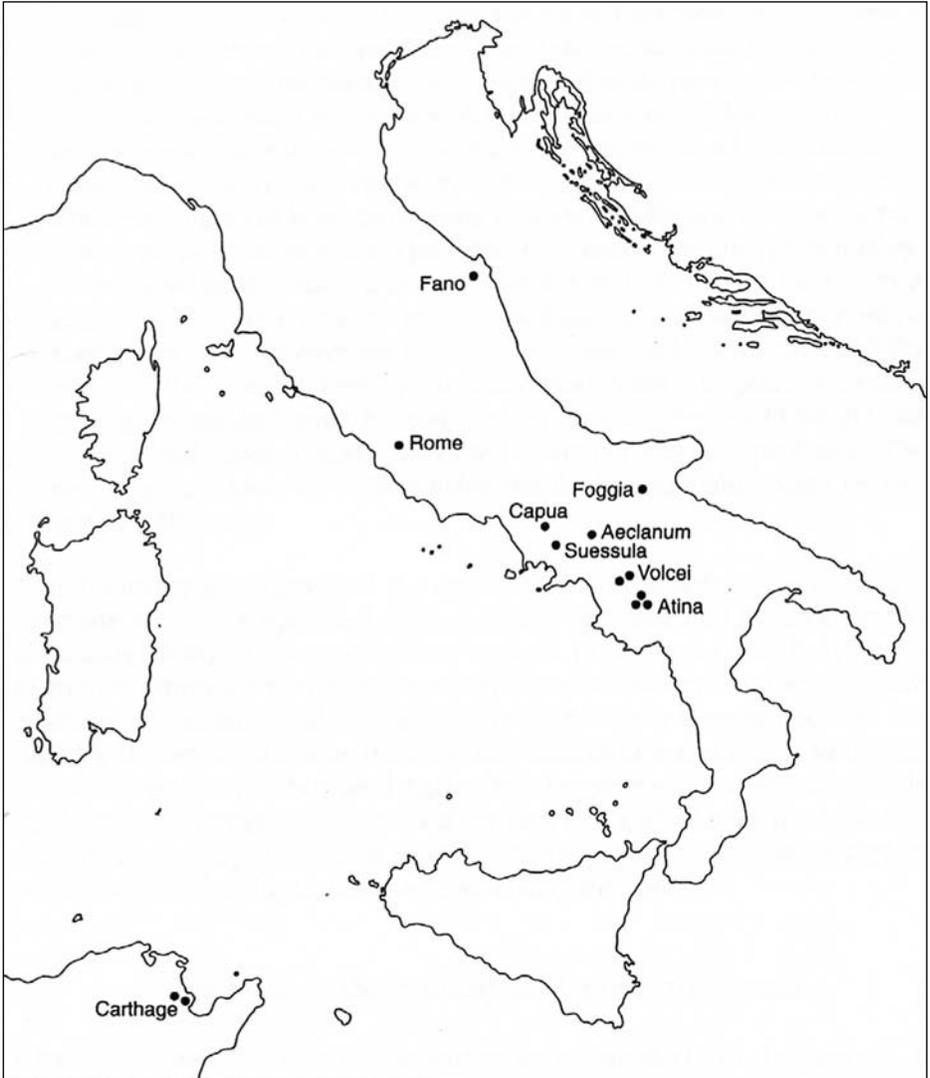


Fig. 2. Distribution of boundary stones of the Gracchan land commission found in Italy (after Bringmann 2007).



Fig. 3. Extent of *ager publicus* in Lucania and Bruttium (after *Roccagloriosa II*).

## II. *The Mingardo/Bussento region*

Of the two, the Mingardo-Bussento region, despite its rather peripheral location at the very margin of the mountainous Lucanian hinterland and its being made up of hilly terrain (fig. 4) with elevations between 200 and 400 m a.s.l., offers in many respects a better defined case study on rural settlement patterns and agrarian change between the third and first centuries BC on account of the fact that it has been the object of a systematic surface survey on a micro-regional scale, conducted between 1985 and 1990<sup>9</sup> in collaboration with F. de Polignac, then at the French School in Rome, H. Fracchia of the University of Alberta, and J.W. Hayes, to whom we owe much advice on the pottery dates (especially for the analysis of the survey finds) recently published in the volume *Roccagloriosa II*. The greater value of the Mingardo-Bussento survey, beside its systematic, intensive coverage of the area, lies in the fact that the survey on a micro-regional scale (an area of *c.* 100 km<sup>2</sup>) was conducted in conjunction with excavation of the major nucleated site in the region, the *oppidum* of Roccagloriosa, which functioned as a regional centre (political and administrative, to judge from the most recent epigraphic evidence) for the Lucanians. The stratigraphic and ceramic data from the extensive excavations conducted at the *oppidum* also indicate that it became a population centre at the peak of its development *c.* 325–250 BC. Furthermore, the economic data on primary production recovered from the excavations also indicate between the end of the fourth and third centuries BC an agricultural intensification which in many respects constitutes a sort of mirror image of the settlement pattern detected by the survey in the surrounding countryside,

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<sup>9</sup> A preliminary report with a first outline of changes in the settlement pattern of this region between the third and first centuries BC can be found in Gualtieri and De Polignac (1991). The later, analytical study of the pottery finds from the survey (H. Fracchia in *Roccagloriosa II*) has added many important details especially to the shady ‘transitional’ phase of the later third and second century BC. For a very recent summary of what still stands as “the third century gap” in Italian archaeology see Lippolis (2006), which is, however, a rather general overview without much discussion of the new evidence from a number of extensively excavated sites and systematic surface surveys conducted in Lucania (Lippolis bases his argument mostly on the evidence from Taranto). A more specific, lucid formulation of the problem is the one by Horsnaes (2004) with a discussion of the ongoing debate about the chronology of third-century BC pottery. A thorough reassessment of the later Gnathia production through the third century BC—which also points out the confusion often made between later black-glaze pottery and overpainted Gnathia—appears in Lanza (2007).

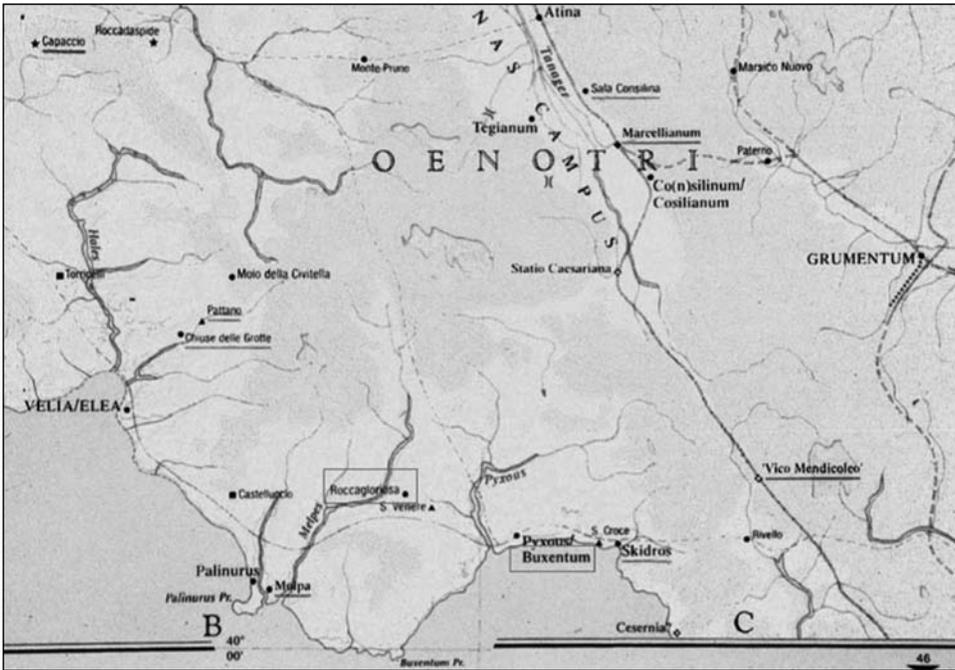


Fig. 4. The Mingardo (ancient *Melpes*)/Bussento (ancient *Pyxous*) region (adapted from Princeton Atlas of the Ancient World).

and may also help us to interpret some of the visible transformations indicated by the site distribution in the territory for the second and first centuries BC, when the Roman colony of Buxentum was founded (194 BC) on the adjacent coast. In order to gain a better understanding of the settlement pattern in the countryside after the decline of the fortified settlement in the final decades of the third century BC, we need to examine briefly the preceding situation. The territorial settlement of the Mingardo-Bussento region (fig. 5) between the second half of the fourth and the third century BC, within a core area of *c.* 80 km<sup>2</sup> surrounding the *oppidum*, was characterized by a dense distribution of scattered 'farms' and small settlement nuclei ('hamlets' or village communities).<sup>10</sup> As shown by the table showing distribution of the data on agricultural production (fig. 6), recovered in the form of charred

<sup>10</sup> On the problem of a hierarchical classification of surface scatters see Leveau (2002). Likely rural agglomerations are indicated on the map by a dot within a square in the plan (see fig. 5); triangles indicate rural cemetery areas associated with farms.

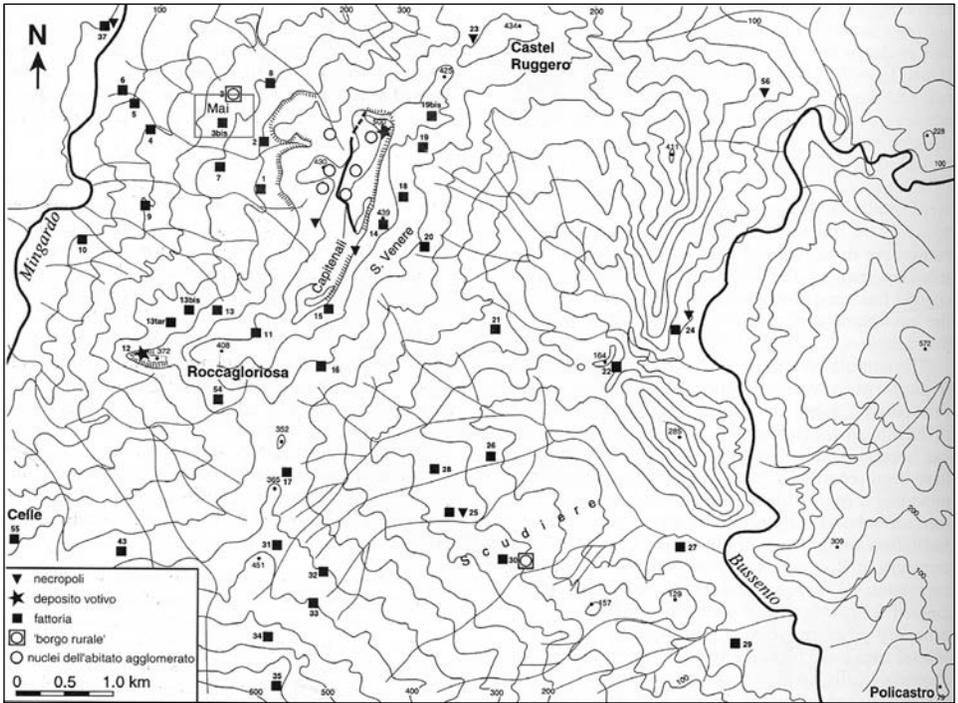


Fig. 5. Late fourth- to early-third-century BC settlement pattern in the Mingardo/Bussento region.

botanical remains from the excavated contexts of the *oppidum*,<sup>11</sup> the economic basis of the farms was a typical form of Mediterranean polyculture which saw an impressive growth in arboriculture in the early decades of the third century BC. Regarding this aspect, and in particular regarding the well-documented development of viticulture through the third century BC, I would underline the fact that in recent years scholars have argued that commercial manufacture of wine in Italy was certainly not unknown before the well-documented massive increase in the production of Italian wine in the final decades of the second century BC: to quote Chr. Vanderersch: “La romanisation n’effacera pas l’empreinte laissée par les viticulteurs grecs et italiens sur le paysage de l’Italie méridionale.”<sup>12</sup>

<sup>11</sup> A thorough and detailed analysis of this body of evidence appears in *Fourth Century BC* (281–305: Bokonyi, Costantini, and Fitt).

<sup>12</sup> Vanderersch (1994, 153–8).

PIANTE		VI-V sec.	IV sec. a.C.			III sec. a.C.			Totale
			I metà	ca. 350 a.C.	II metà	I metà	ca. 250 a.C.	II metà	
CEREALI	Triticum monococcum		1						1
	Triticum dicoccum (grains)			2	3	4			9
	Triticum dicoccum (chaff)			89	10	1			100
	Triticum compactum		1		1				2
	Triticum sp.	1		1	4	2			8
	Hordeum vulgare		2		8				10
	Hordeum sp.				4				4
	Panicum sp.			2					2
	Gramineae indet.		1	24	30	3	1		59
LEGUMI	Lens culinaris				1				1
	cf. Pisum sp.				1				1
	Vicia faba		3		31	9	25		68
	Leguminosae indet.				2	5			7
FRUTTA	Ficus carica			9	13	1	2		25
	Olea europaea				7	2			9
	Vitis vinifera	3	11	9	131	62	1	5	222
	Fragm.s (nutshell) indet.				2	1			3
PIANTE INFESTANTI	Cf. Avena sp.			1					1
	Bromun/Lolium				1				1
	Chenopodium album		1		2	1		2	6
	Euphorbia elioscopia			1	2				3
	Galium aparine		1		3				4
	Lathyrus aphaca				1				1
	Lathyrus sativus/cicera				2				2
	Polygonum sp.		3	1	5	1			10
	Silene sp.				5	1		1	7
	Weed seed indet.			16	124	6		1	147

Fig. 6. Archaeobotanical remains retrieved from the Roccagloriosa excavations.

Pottery production and metalworking may have been originally concentrated within the *oppidum* (where a probable potters' and coroplasts' quarter<sup>13</sup> and a large metalworking establishment<sup>14</sup> have been located), although it is becoming increasingly clear from the analysis of survey data published a few years ago<sup>15</sup> that some of the village agglomerations (in particular Mai on the flatlands along the Mingardo river) may also have housed artisan production (pottery, dolia, and amphorae) to judge from the extensive remains of kilns,<sup>16</sup> and that these village agglomerations lasted for a longer period (certainly through the third and second centuries BC).

Like other major Lucanian *oppida* (a case in question for continued habitation through the first century BC—presumably until the Social War—is the site of Civita di Tricarico, now in the course of final publication by O. de Cazanove),<sup>17</sup> the Roccagloriosa *oppidum* survived, although on a reduced scale and with major restructuring, the troubled period of the third century BC<sup>18</sup> with repeated Roman military operations in the south of the peninsula, and perhaps ceased to be a regional centre at the end of the Hannibalic war. However, small rural establishments on the plateau immediately outside of the fortification continued in use throughout the Imperial period (to judge from the late-Roman amphorae found on the site).<sup>19</sup>

Around what seems to have been a largely deserted *oppidum* the surrounding countryside continued to be inhabited through the second century and the first half of the first century BC (fig. 7). The distribution map (fig. 7) includes those sites whose continued existence bridges the period from the later third century to c. 50 BC, which have been labelled 'transitional' sites. Within the general picture provided by the analysis of survey sites published in *Roccagloriosa II* the transitional sites provide the link between the settlement pattern of the Lucanian period

<sup>13</sup> For a probable 'artisan' quarter see the discussion in *Fourth Century BC* (336–8: Gualtieri).

<sup>14</sup> For the metalworking establishment and related activity see *Fourth Century BC* (308–24: Wayman).

<sup>15</sup> See *Roccagloriosa II* (118–9, and figs. 92–3).

<sup>16</sup> General and fundamental considerations on this problem, although referring to a slightly later period, appear in De Ligt (1991). Some of the basic evidence from this sizeable and thick scatter with kiln remains is presented in Fracchia et al. (1983, 373–7 and figs. 13–5).

<sup>17</sup> Cazanove (2004).

<sup>18</sup> *Roccagloriosa II*, (ch. 2); *Fourth Century BC* (265–70: Fracchia).

<sup>19</sup> *Roccagloriosa I* (278–89: Arthur).

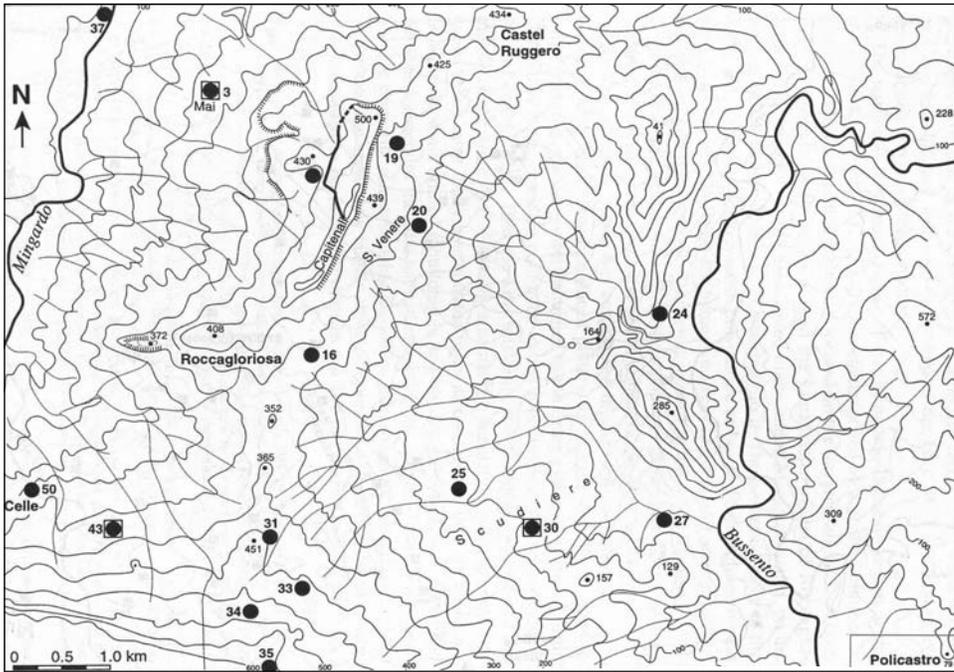


Fig. 7. Farms and villages in the Mingardo/Bussento region during the second and first centuries BC.

and the evident changes occurring in the course of the second century BC, at least in part related to the foundation of the new Roman colony along the coast at the mouth of the Bussento river, *c.* 10 km away from the Roccagloriosa *oppidum*.

All of the transitional sites were inhabited earlier in the third century BC, and most of them continued to be inhabited well beyond the first century BC. Thus in our view the transitional phase between the Lucanian settlement pattern and the transformation brought about in the age of 'Romanization' (I use the term in inverted commas since time does not allow us to discuss the many possible connotations of this term in the light of the heated debate raised by post-colonial theory)<sup>20</sup> in the Mingardo-Bussento region was marked by some degree of continuity

<sup>20</sup> On the general problem and also with some reference to the specific area in question see the detailed discussion in *Lucania romana* (58–61).

of rural settlement, although a numerical decline is evident in some categories present in the Lucanian settlement model.

To what extent this numerical decline was eventually coupled with the disappearance of artisan production in the area remains at the moment a matter of speculation. The arrival of new ceramic products indicates the accessibility of different market centres (in particular the areas around the Gulf of Naples) and the existence of new commercial and road networks for these sites which bridged the second and early first centuries BC.

The new road network also conditioned which sites survived in the transitional phase, and the location of new sites. When we consider (given the starting date of the early second century BC provided by Campana A wares, diagnostic plain wares, and Graeco-Italic/Dressel IA transitional amphorae) that the distribution of sites (fig. 7) is a likely picture of the rural landscape in which the foundation of Buxentum took place, it is clear that we can accept only with a good deal of caution the literary accounts about the difficulties encountered by the colonists of 194 BC, who allegedly were sent into a true wasteland. The specific information provided by Livy 34.45.2–3 and 39.23.3–4, that the consul of 186 BC found the new colony (as well as its twin colony of Sipontum) abandoned, does not necessarily imply that the hinterland of Buxentum (or, by the same token, the hinterland of Sipontum/modern Manfredonia, on the Adriatic side) is an area of southern Italy to be taken as proof of a post-Hannibalic ‘desertification’, as it has been in the conventional view. In a succinct note Nathan Rosenstein<sup>21</sup> has recently argued, with a good deal of common sense in my opinion, that the cause may not have been that the settlers could not make a go of it, but rather that they had more attractive options elsewhere.<sup>22</sup>

With the exception of the reuse of the extramural areas and some of the structures of the *oppidum* (note that the fortification wall has been left on the map, on account of uncertainty regarding the date—perhaps late third or early second century BC—and the extent of its actual destruction and/or reuse), the transitional sites were found on the hill-sides sloping toward the lower Bussento catchment basin and gravitating toward the coastal area, a fact which is not surprising in light of the

<sup>21</sup> Rosenstein (2004, 145–6 and n. 22).

<sup>22</sup> For example in the Po valley, a very appealing region to new settlers as argued by Broadhead (2003, 148).

possible impact of the foundation of Buxentum. As indicated on the distribution map (fig. 7), on the basis of the size and composition of the scatters we have very tentatively categorized the sites pertaining to the second/early first centuries BC as either: 1) 'hamlets'/probable village agglomerations, or 2) farms. It goes without saying that the nature of survey evidence, as previously indicated, makes these categories very fluid, but on the other hand, as already pointed out, the extensive combined evidence of survey and excavation for the earlier Lucanian period (late fourth and early third centuries BC) provides us with a firm basis for analogy. One hamlet site (no. 3 on fig. 5) and one farmstead (no. 37 on fig. 5) found on the side of the middle Mingardo valley disappear in the course of the first century BC, as they were cut off from the other sites gravitating toward the Bussento valley by an apparent change in the road system. On the Bussento valley side fourteen sites have been identified, which can be broken down typologically into one large village agglomeration, one smaller village, and twelve farmsteads.

The survey results indicate a visible change in the settlement pattern of the Mingardo-Bussento region in the decades immediately after the middle of the first century BC (fig. 8): the sites now identified by the presence of very diagnostic wares such as the earlier forms of undecorated Italian sigillata and Dressel 2/4 amphorae (starting around 40 BC) extend visibly toward the coast, in the lowlands around the mouth of the Bussento river.<sup>23</sup> Two new farm sites of this period identified a few kilometers from Buxentum itself (no. 29 and 29b on fig. 8) show a regular alignment and similarity in size of scatter, thus suggesting to the leader of the survey team of that moment, F. de Polignac, who first presented these sites in *Roman Landscapes*, a new form of planned land occupation. Unfortunately, administrative considerations interrupted the survey in the nearer territory of Buxentum, but further research in the territory of modern Policastro could provide additional evidence for this hypothesis. In any case the picture of a revitalized territory as indicated by the later study of the survey pottery (and highlighted by the

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<sup>23</sup> This 'revitalization' of the *Ager Buxentinus* in the second half of the first century BC has been analysed in detail elsewhere (Gualtieri 1996), and possible explanations have been put forward. In view of what happens a few km further south along the coast at *Blanda Julia*, we can now be fairly confident that this is due to the renewed strategic importance of the lower Tyrrhenian coastland between the second triumvirate and the war against Sextus Pompeius (for *Blanda Julia* see now La Torre and Mollo [2006]).

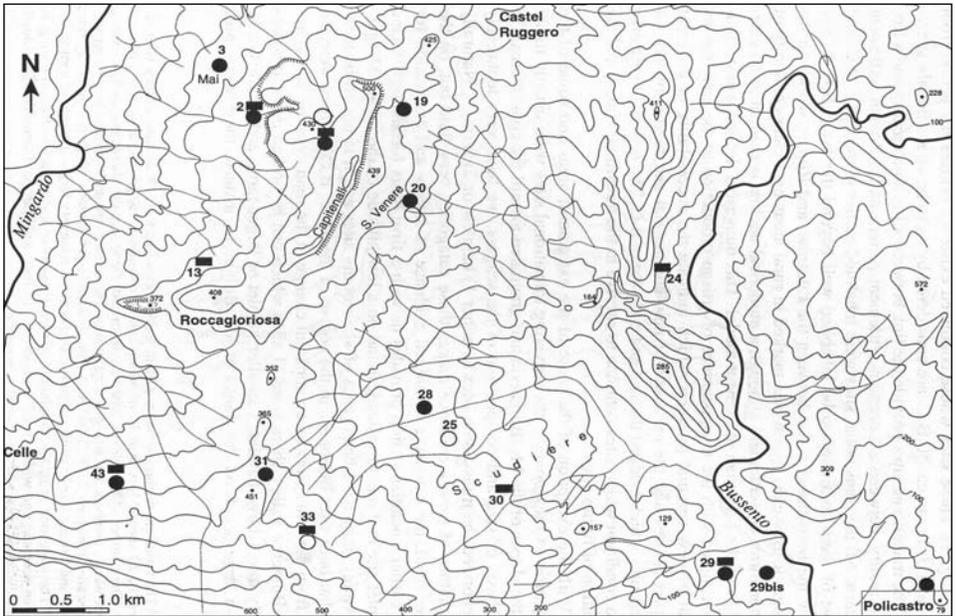


Fig. 8. Distribution of Dressel 1 amphorae (○), undecorated terra sigillata (■), and Dressel 2–4 amphorae (●).

distribution map in fig. 9) may be connected with the brief and somewhat ambiguous statement in the *Liber Coloniarum* that land assignments were made in the *ager Buxentinus*,<sup>24</sup> land allotments which Lachmann's reading of the text had already placed in the second half of the first century BC. The most recent publication of the evidence from the systematic and extensive explorations conducted by F. La Torre in the adjacent territory of Blanda Julia would seem to add new details to the picture of this part of Lucania on the Tyrrhenian seaboard around the Gulf of Policastro in the later decades of the first century BC.<sup>25</sup>

One last comment needs to be made on the two sites of this period which can be identified with certainty as villas (fig. 9: site no. 20, located on the very slopes of the ridge where the *oppidum* was once located, and the site at S. Croce, on the coastline east of Buxentum). The latter is a splendid example of a *villa maritima*, built in *opus reticulatum* (with all

<sup>24</sup> Gualtieri (1996, 543–6).

<sup>25</sup> La Torre and Mollo (2006, 472–4, 477–80); discussion of the new evidence on the Roman city and surrounding territory in the context of Roman colonization along the Tyrrhenian coast appears in *Lucania romana* (110–16).

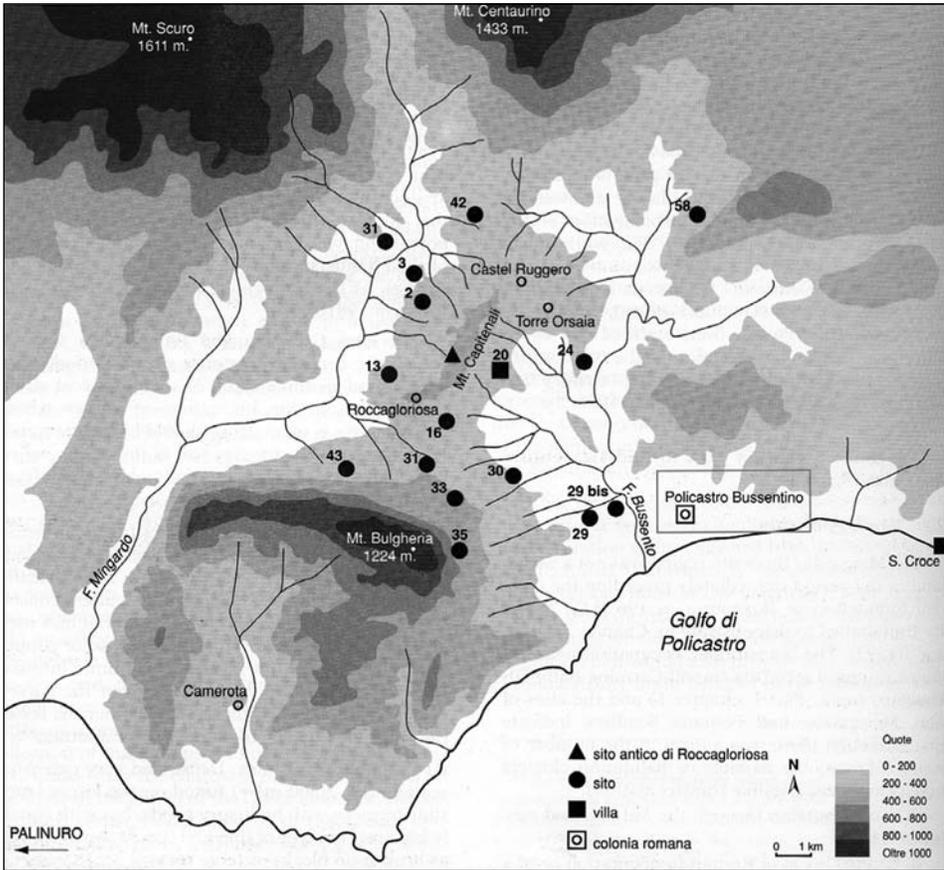


Fig. 9. Sites dated between the mid first century BC and mid first century AD in the Mingardo/Bussento Region.

the implications which this specialized building technique may imply)<sup>26</sup> and dated to *c.* 40 BC by the earliest mosaic decoration. It would seem reasonable then to argue that the two villa sites<sup>27</sup> so far sufficiently well documented in the Mingardo/Bussento region do not necessarily indicate structural changes in the local economy or a link to ‘the slave mode of production’,<sup>28</sup> but (to judge from their particular location in the one case, and the architectural layout in the other) would have functioned rather as status symbols and as expressions of elite control over the landscape.<sup>29</sup> Such a likely configuration of the few major villa sites in the region provides us with added archaeological documentation against the conventional view of a radical disruption in the local settlement pattern and agrarian systems between the second and first centuries BC.<sup>30</sup> It may also indicate some degree of slow transition in the rural settlement pattern and forms of agricultural exploitation in this corner of Lucania during the late-republican period. With this cautionary statement, however, I am certainly not implying a scenario of ‘changeless continuity’ between the Lucanian and the Roman settlement organization. On the contrary, I would point out again that what

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<sup>26</sup> Torelli (1995, 225–9) points out how constructions in *opus reticulatum* were the work of specialised équipes emanating from the *Urbs* and emerged as a form of ‘rationalisation’ of building techniques. They also aimed at “creation of a desired atmosphere of *urbanitas*” (228).

<sup>27</sup> The one at Santa Venere (Roccagloriosa) is discussed in detail, also on the basis of the evidence of a funerary monument associated with it, in Gualtieri (1996, 536–40). For the villa at Santa Croce (Sapri) see *Roccagloriosa II* (139–40 and fig. 110).

<sup>28</sup> To use the terminology adopted in Giardina and Schiavone (1981).

<sup>29</sup> Dyson (2003, 20–3); Dyson also reminds us, however, of the economic role that almost all Roman villas played, also in connection with the now generally accepted importance of commercial activities for maintaining the lifestyle of the upper strata of Roman society (*ibid.*).

<sup>30</sup> For a very concise and recent summary of the economic and demographic aspects of the debate, with ample bibliography, see Rosenstein (2004, 6–12). Frederiksen (1971) and Evans (1980) first questioned in detail traditional views about a severe decline of the free peasantry; Rathbone (1981) places greater emphasis on war and military service as the major cause of the decline of the free peasantry, rather than competition from the new slave-staffed crop and livestock farms (more often used by supporters of the conventional view, which sees a sharp decline if not total disappearance of small farms). In a recent assessment of Rathbone’s argument Lo Cascio (1999, 221–2) underlines the fact that “Sono gli stessi meccanismi di funzionamento della villa schiavistica a prevedere come dato strutturale il ricorso sia pure temporaneo ad una forza di lavoro libera, esterna all’unità produttiva.” De Neeve (1984) approached a number of questions relating to the extent and causes of the transformation and especially the possible effects on the free peasantry by utilizing principles of rural site distribution borrowed from von Thünen’s location theory.

indications we have (from a number of specific archaeological contexts)<sup>31</sup> of continued occupation in the countryside are to be considered in the light of an unquestionable phenomenon of 'destruction' of the pre-Roman settlement system, of which the collapse of a number of major fortified or defended sites<sup>32</sup> that functioned as centre-places may represent the most visible indication. All told, the continuity in the forms of rural settlement which our survey data allow us to postulate between the third and first centuries BC in the Mingardo/Bussento region is to be seen against a background of profound structural transformations in the agrarian economy and in the socio-economic organization of the communities involved.

### III. *The countryside of Buccino/Volcei*

The second case study, much better known to many of us, will require fewer detailed comments, in spite of the relatively abundant archaeological and epigraphic evidence pertaining to the second and first centuries BC and the many studies which have dealt with this territory, starting from V. Bracco's volume of the *Forma Italiae* dedicated specifically to Volcei and S.L. Dyson's publication on the villas excavated in the territory of Buccino. I have included it in my discussion because of some familiarity with the area, which, thanks to major funding from the European Community for post-earthquake reconstruction, has enjoyed in recent years a new wave of research both in the urban area of ancient Volcei (underneath modern Buccino) and in its territory, notably with the recent opening of a Parco Archeologico Territoriale under the direction of A. Lagi de Caro of the Department of Antiquities of Salerno. Mention should also be made of the very recent publication<sup>33</sup> of a volume by R. di Gennaro and A. Santoriello (two of her young collaborators) with the overly ambitious title, as I shall explain later, of *Settlement Dynamics in the territory of Buccino/Volcei*. It is unfortunate that Dyson's excavations and prompt publication of a number of important

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<sup>31</sup> A thorough review of the most recent evidence from southern Italy appears in Compatangelo-Soussignan (2004, 65–7).

<sup>32</sup> Cazanove (2004, 775). A recent overview of the situation in Lucania after the decline of the fortified sites is given by Isayev (2007).

<sup>33</sup> Di Gennaro and Santoriello (2004).

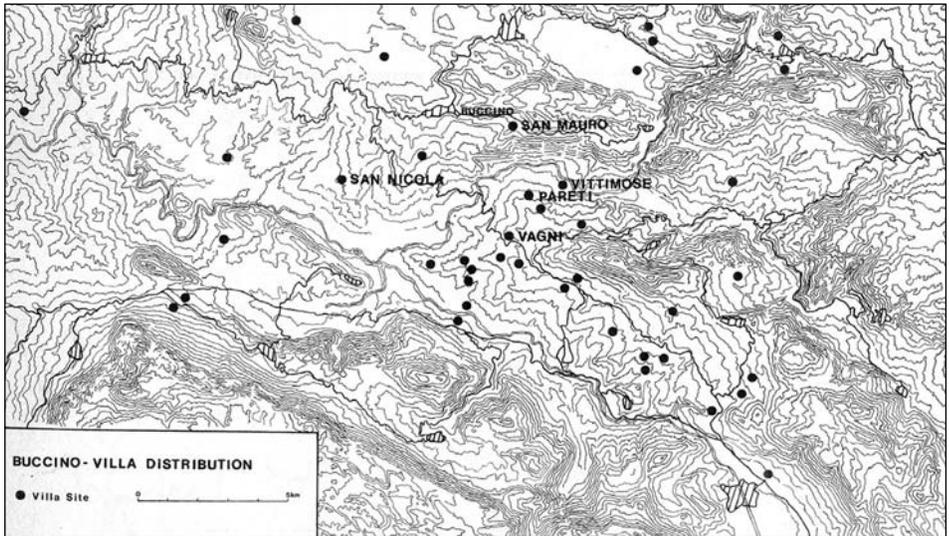


Fig. 10. Roman sites in the Buccino area (after Dyson 1983).

villa sites<sup>34</sup> do not allow clear chronological and functional distinctions of the earlier phases, which he pushes back in a couple of cases to the late third or early second century BC. Similarly the distribution map (fig. 10) of the sites surveyed in what can be presumed as the *ager Volceianus*, while showing with immediacy a densely settled territory, does not provide any chronological distinctions, and simply labels them as ‘villas’. Quite a number of these sites, to judge from the documentation provided for the ones excavated, may have had second-century BC or earlier phases. Dyson rightly points out that Buccino, with its cumulative experience of Hannibalic warfare, Gracchan land reforms, and destruction at the hands of the bands of Spartacus, should have been an ideal candidate for rural crisis. Yet the ensuing statement that “population levels remained quite high through the Republic” provides us with a fairly optimistic picture of the late-republican period which appears to be rather impressionistic and not easy to test on the basis of the published evidence from the territory. This is all the more regrettable when we consider the fact that the new project of urban excavation conducted by A. Lagi<sup>35</sup> has shown some interesting developments

<sup>34</sup> Dyson (1983).

<sup>35</sup> Lagi (1995).

in public building dating to the later third and second centuries BC. At Volcei the apparent prosperity of the countryside seems to correspond more and more to a picture of an ancient Lucanian settlement undergoing renewal along Hellenistic lines. Thus the possible picture which is emerging for the second and first centuries BC (and I should emphasize its appearance already in the course of the second century) is that of an 'urban' centre which was a major nodal point for social and economic life. Against this background, one would have hoped that the recent volume on settlement dynamics in the surrounding territory<sup>36</sup> might have provided some fresh approach and a settlement framework which would contextualize the rich archaeological and epigraphic data from the city and its territory<sup>37</sup> and especially the survey evidence, as the title would lead us to expect. But this is not at all the case. In what is therefore to be considered still as a preliminary and partial report on this important project the authors explain in the final chapter in a fairly apologetic manner that the analyses of finds so far carried out and the emerging picture of settlement patterns provide no secure foundations on which to base a deeper understanding of settlement dynamics in this very important district of the Lucanian hinterland. According to the authors the settlement dynamics outlined in this study of the countryside of Volcei are only a tentative interpretation of the available evidence: they represent merely a starting point for future research on the territory of Buccino. In the light of this it is all the more regrettable that not a single distribution map of identified rural sites is included. For a proper assessment of the rich archaeological and epigraphic documentation on the rural landscape of second and first century BC Volcei much remains to be done.

#### IV. *Some comparative evidence from the rest of Lucania*

A few elements may be added to the picture of settlement patterns between the third and first centuries BC based on the (still rather frag-

<sup>36</sup> Di Gennaro and Santoriello (2004).

<sup>37</sup> The sheer mass of (sadly sporadic!) archaeological data (Bracco, 1978) and the probable continuity between the Lucanian settlement and the Roman *municipium* are key elements for study of the 'Romanization' of an important Lucanian district. Furthermore, one should not overlook the additional evidence of the late-third-century BC coinage, most probably pertaining to this site, recently studied by Rutter (2001, 122 nos. 1341–5).

mentary) evidence available from other inland areas of Lucania. First and foremost the Vallo di Diano (i.e. the wide inland plain crossed by the river Tanagro, immediately south of the territory of Buccino) where, as mentioned above, the concentration of boundary stones set up by the Gracchan land commission is quite exceptional when compared to finds from the rest of the Italian peninsula (fig. 2). The Vallo di Diano is no doubt an important and fertile inland valley of western Lucania that had also functioned as a key transit area between the Ionian and Tyrrhenian coasts since early proto-historic times, and was later dotted with small urban centres which became sizeable *municipia* after the Social war.<sup>38</sup> Here most of the evidence on second- and first-century BC changes in the rural landscape, apart from the Gracchan boundary stones themselves,<sup>39</sup> is provided by a sizeable group of funerary monuments (recently labelled by H.G. Frenz as Lucanian *stelae*, considering the likely local manufacture), which are to be associated with farm or villa sites, although most of them are sporadic finds. A thorough and detailed study of the group<sup>40</sup> has dated them, on stylistic grounds and on the basis of the palaeography of the dedicatory inscriptions (when present), between the later decades of the second century BC and the early Julio-Claudian period. The coexistence of Oscan and Latin elements is the most interesting feature of this group of monuments and can be paralleled with analogous developments in the rural landscape of other inland areas in nearby Hirpinia in the second century BC, following the Roman conquest.<sup>41</sup> Not much can be said of the actual size and distribution of rural sites of which the epigraphic evidence in question provides good testimony, but, as pointed out some time ago by H. Solin, the Vallo di Diano nevertheless provides a vivid (if somewhat sketchy and impressionistic) picture of a dense, mixed population inhabiting the fertile countryside.<sup>42</sup>

For the inland areas of north-eastern Lucania we now have two partially published surveys which, not surprisingly, provide us with rather heterogeneous indications for the second and first century BC changes in settlement patterns. The Ruoti survey was conducted within a 5 km radius in the countryside surrounding the villa at San Giovanni

<sup>38</sup> Fraschetti (1981).

<sup>39</sup> For a general discussion see Catalano (1991) and Franciosi (2002).

<sup>40</sup> Coarelli (1981); see also further comments on the epigraphic texts in *Lucania romana* (146–9).

<sup>41</sup> *Basilicata* (330–1: Torelli).

<sup>42</sup> Solin (1983).

di Ruoti.<sup>43</sup> Although we are dealing with a systematic intensive survey in a fairly homogenous highland landscape, the picture for the second and first centuries BC remains somewhat blurred and of uncertain interpretation. As far as we can tell from the published data, there are signs of decline in the occupation of the countryside in that period, although, as pointed out in a recent overview on settlement patterns in central Lucania,<sup>44</sup> such results appear to be quite at odds with the evidence available for central Lucania. I would add that, apart from the limited number of sites in the sample, the analysis of chronologically significant ceramic finds for the earlier period ('black glaze') was based on a chronology which has recently been much revised. In any case some further analysis of the area in this period might clarify whether we are dealing with actual "segni di impoverimento in alcune zone.....della Lucania attorno alla villa di S. Giovanni di Ruoti",<sup>45</sup> or with a case that is anomalous when compared to the picture which emerges from a gazetteer of known rural sites in the area, compiled a couple of decades after the S. Giovanni survey.<sup>46</sup>

Finally, at least a brief mention should be made of the countryside around modern Oppido Lucano in the upper Bradano valley,<sup>47</sup> where, in conjunction with the large-scale excavations of a number of villas, a surface survey was conducted in the 1990s. Although the evidence pertaining to the last two centuries BC is still in the course of publication (major emphasis has so far been placed on the impressive developments of the mid- and late-imperial periods), it is noteworthy that at least one of the major excavated sites, the villa at Masseria Ciccotti, was built in the course of the first century BC on top of a pre-existing Lucanian site: the coexistence of villas and village agglomerations from an early date, which is implied for the Lucanian territory by a number of literary sources,<sup>48</sup> is another feature of the rural settlement pattern.<sup>49</sup>

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<sup>43</sup> Roberto (1984).

<sup>44</sup> *Leukania* (37: Terrenato). It is also to be underlined that we are dealing with a very limited sample of sites (*ibid.*).

<sup>45</sup> As indicated in a recent summary on second-century BC agriculture and population in Italy by Lo Cascio (1999, 220 n. 12).

<sup>46</sup> *Leukania* (37–8 and n. 30: Terrenato).

<sup>47</sup> Fracchia and Gualtieri (1999).

<sup>48</sup> Consider Sal. *Hist* 3 fr. 98 and 101, speaking of *uillarum atque uicorum uastatione* in the hinterland between Thurii and Metapontum at the time of Spartacus' revolt. See also App. *BC* 1.116. Pertinent comments in Crawford (2004). Extended discussion with ample bibliography in *Lucania romana* (177–83).

<sup>49</sup> See the general picture outlined in Carandini (1993).

*V. Concluding remarks*

To sum up, I would think that a balanced assessment of the contribution which landscape archaeology can make to the study of settlement patterns and agrarian change in Italy and especially (with more specific reference to the scope of this article) in southern Italy during the crucial period from the third to the first century BC (the period generally labelled with the comprehensive although somewhat ambiguous term ‘Romanization’) ought to take into full account the concise and lucid statement by Luuk de Ligt in the opening paragraph of his most recent overview of the agrarian economy in the Italian peninsula during the late-republican period: “The materials available to those aspiring to recover the principal outlines of Italy’s rural history during the second century BC are certainly less than ideal.”<sup>50</sup> However, such a necessary note of caution (which in many respects can be paralleled in Randsborg’s lucid methodological remark set forth at the outset) should not prevent us from acknowledging the relevance of archaeological evidence for the study of the social and economic transformations of this period, a fact which was first brought to the attention of archaeologists and ancient historians, in a rather tentative manner, by M.W. Frederiksen over three decades ago.<sup>51</sup> While fully accepting the limitations posed by the scarcity of the material evidence so far available, it might be added that increasing reliance on surface survey, the refinement of methods in the collection of data, and especially a more critical assessment of the growing body of field data, made possible by a rigorous archaeological methodology, have all provided in very recent years a fundamental contribution to the debate. To strike a more positive note, I would point out that, although outside of the specific area chosen for this paper, the recent reassessment of the *Ager Veientanus* surveys of the 1960s in the context of the new research being conducted in the Tiber valley and the most welcome recent publication of the extended *Ager Cosanus* survey conducted in the 1980s by A. Carandini’s team<sup>52</sup> (to mention just two large-scale projects) are providing much new evidence and fostering new debates on the specific problem of changing agrarian systems in Roman Italy, not least for the crucial

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<sup>50</sup> De Ligt (2006, 590).

<sup>51</sup> Frederiksen (1971); further comments on the basis of updated evidence in Frederiksen (1981).

<sup>52</sup> Carandini and Cambi (2002).

period under scrutiny. There is another important point to underline concerning the contribution which survey archaeology ought to make to the study of changing agrarian systems in Roman Italy. In spite of the many uncertainties in the interpretation of the evidence (and here I would specify not just survey evidence but also large-scale excavations of rural sites), most scholars would now agree that a good degree of regional differentiation might be accounted for, especially in the period between the late Republic and early Empire, with the help of a more realistic reconstruction of Roman agriculture.<sup>53</sup> Thus wholesale generalizations about the prevalence of an almost exclusively slave-based plantation economy in later republican Italy, in vogue just a couple of decades ago,<sup>54</sup> have been cast increasingly into doubt by a growing body of systematically collected archaeological evidence. Significantly, a recent and authoritative summary on the Roman countryside, which takes into full account the results of three decades of archaeological fieldwork in central and southern Italy, duly underlines the fact that "While the creation of the Roman political social and economic world did prejudice major changes in the countryside, many local and regional variations developed."<sup>55</sup>

I would like to conclude by returning to the kernel of the question concerning the profound transformations in the agrarian economy between the second and first centuries BC and point out that much still remains to be done,<sup>56</sup> especially in the direction of a more critical and balanced assessment of the new 'wave' of archaeological data on the ancient countryside. To quote from a very recent overview by J.-P. Vallat, we cannot ignore the fact that the literary and especially the archaeological evidence made available by systematic fieldwork both indicates a sort of contradiction, if not a true paradox, which

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<sup>53</sup> See the ample and detailed critical review by Rathbone (1993) of a comprehensive study on Roman agrarian history based almost exclusively on the literary evidence (in particular 26–7 and 35–6). Relevant comments on the necessity of taking into account regional differences in the agrarian economy of Roman Italy appear in De Ligt 1991 (esp. 53–6).

<sup>54</sup> The classic statement of this historiographic framework appears in the Proceedings of the Conference held in the late 1970s at the Gramsci Institute in Rome: Giardina and Schiavone (1983).

<sup>55</sup> Dyson (2003, 77).

<sup>56</sup> It is to be remembered that over two decades ago Rathbone (1981, 23) already pointed out that "Roman agrarian history—and especially the old problem of the 'decline' of the *assidui*—still remains very open to new approaches".

was first pointed out in a pioneer study by M.W. Frederiksen<sup>57</sup> but still frequently shows up in much of the research conducted on the countryside of the Italian peninsula between the second and first centuries BC by archaeologists<sup>58</sup> and ancient historians alike.<sup>59</sup> Very simply stated, “...au moment même où les Gracques décrivent une Italie en crise du fait du *latifundium* et du travail esclavagiste, l’essor phénoménal des petites fermes, le développement de l’économie de marché à l’échelle de l’Empire et la pression des villes sur les campagnes, montrent que des exploitations de taille modeste ont pu être rentables et s’intégrer à un tel système sans être les victimes obligées et reléguées de l’économie de plantation du type *villa*.”<sup>60</sup>

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<sup>57</sup> Perhaps in a somewhat provisional manner, as Frederiksen himself later conceded, given the state of ceramic chronology at the time. The ongoing restudy of the ceramic finds from the *Ager Veientanus* survey (in particular the ‘black glaze’ sites) has somewhat corrected and refined the chronology of sites identified. See also comments by De Ligt (2006, 598).

<sup>58</sup> Arthur (1991); Rathbone (1993); Fracchia (2001).

<sup>59</sup> De Neeve (1982); Dyson (1983 and 2003).

<sup>60</sup> Vallat (2004, 53).

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IV

ALLIED MANPOWER AND MIGRATION



# MOBILITY AND MIGRATION IN ITALY IN THE SECOND CENTURY BC

Paul Erdkamp

## I. *Mortality, fertility, migration*

The study of demography consists basically of three components: mortality, fertility, and migration, but only the first two have received much attention in modern studies of the ancient world. There may be good reasons for this: while ancient demography in general is plagued by a scarcity of quantifiable source material, the shortage of evidence is more easily circumvented with respect to mortality and fertility than with respect to migration. Biological and ecological factors have a larger and more predictable role in the birth and death of humans than in their mobility. Hence, selecting the appropriate ‘model life table’ solves a sufficient number of the ancient demographer’s problems to allow him to continue investigation. No such tables exist for migration. The extent and nature of mobility in each society is determined by economic, political, social, and environmental circumstances that are so changeable and interwoven that it is impossible to create a model of migration in the way one can of births and deaths. Few studies have been published on mobility and migration in Antiquity.<sup>1</sup> Of course, publications on Roman history have in the past dealt with mobility in the sense that many subjects naturally involve the movement of people. However, these publications have not addressed mobility *per se*, but colonization, the *ius migrandi*, or the Romanization of conquered areas.

This article aims to analyse the extent and diversity of mobility and migration among the freeborn population in Italy, limiting itself primarily to the second century BC, although evidence for other

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<sup>1</sup> An exception, dealing with Egypt, is Braunert (1964). Parkin (1992, 135 f.) acknowledges the importance of migration for ancient population studies, but sees no way to get around the lack of evidence. The brief section on migration in Scheidel (2001, 46 ff.) stresses mainly the problematic nature of the evidence. Frier (1999, 85–109) takes no notice of migration. Laurence (1999, 146 f.) devotes no more than one page to migration. One of the first full-scale studies of migration in the republican context is Broadhead (2002). Moatti (2006) announces a project on various aspects of mobility.

periods—ancient and later—will also be considered. In particular, the importance of migration for the growth of Rome will be assessed. Many people were forced to move as a result of slavery, but this aspect of mobility will not be covered, since the explanations for this phenomenon are quite unrelated to the forces and desires that motivated the movement of free people.<sup>2</sup> Slavery will be important though as a factor influencing the behaviour of the free population. Scholars of more modern migration may perhaps criticize my approach as one-sidedly material and economic, with little attention to the cultural and emotional aspects.<sup>3</sup> However, while I have sufficient evidence to construct an economic model of the motives and constraints of mobility in the republican era, I would be forced to rely on a speculative projection of possibly anachronistic ideas concerning cultural values, spiritual needs, and emotional attitudes to the family, if I were to address these issues. In short, non-material factors are acknowledged, but will play no significant role in this study.

The issue of definition is not of great concern to us. We are dealing with several broad categories of mobility whose essence is clear enough, although boundaries cannot be clearly marked. Migration is considered to be: (1) movement outside of one's community (whether town or city, village or group of villages, or dispersed habitation in the countryside), which results in (2) a shift in subsistence strategy, either between jobs or between farms. Given the nature of our discussion, this will suffice: we are not presenting a numerical analysis in which definitions greatly influence statistics and therefore outcome.

## II. *Human mobility in ancient history*

The most important study of Italy's population is undoubtedly Peter Brunt's *Italian Manpower* (1971), and it is indicative of the book's approach to mobility or migration that neither word occurs in its indices, though 'emigration' does. As far as the movement of people is concerned, the emphasis is on emigration, but only in order to deny that large numbers of migrants left Italy. Brunt's disregard of migration was determined by the then predominant view according to which

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<sup>2</sup> See recently Scheidel (2005a).

<sup>3</sup> Neo-classical economics are denounced as inadequate, e.g. by Hoerder (1997; 2002, 8 f.). On the importance of the family see for example the introduction to Davis Root and de Jong (1991, 221 ff.).

mobility in pre-industrial societies was restricted to short distances, and men—and women even more so—lived their lives within a short range. Brunt refers to a study of rural England in order to point out country-dwellers' general lack of knowledge of their neighbouring regions: "There was a deep ignorance of any kind of life more than 10 or 20 miles away."<sup>4</sup>

Already in 1991 Robin Osborne pointed out that scholars working on modern England realized that the populations of premodern societies were less immobile than previously thought.<sup>5</sup> (Interestingly, some of these conclusions were available before Brunt published his *Italian Manpower*.) This paradigmatic shift was based on two case studies in seventeenth-century England: Cogenhoe (Northamptonshire), for which evidence is available for 1618 and 1628, and Clayworth (Nottinghamshire), where the evidence is for 1676 and 1688. It turns out that many individuals and households appeared and disappeared in both communities within a short period, indicating a mean annual population turnover of roughly 5%. Osborne gives corroborative evidence from a variety of communities on the continent. Based on this evidence, he urges historians of Antiquity not to underestimate the degree of mobility in ancient populations. He writes: "it is quite likely that half the population will have arrived within the last ten years and that half the population will depart within ten years".<sup>6</sup>

In a much more recent publication Walter Scheidel explores what he terms 'the state-sponsored resettlement of citizens', i.e. colonization schemes and virgane distributions of land.<sup>7</sup> He rightly concludes that such schemes caused an exceptionally high degree of migration, in particular during the first century BC. Two of his main points may be emphasized here: (1) It was the coercive means of the Roman state that made possible population transfers on such a large scale. (2) Since the population of the city of Rome could not reproduce itself, between the Hannibalic War and the age of Augustus the city required a huge influx of immigrants in order both to maintain its population and to multiply its size. Two important estimates may be selected from his many calculations: 1–1.25 million individuals were resettled in colonies or virgane allotments during the last two centuries of the Republic, and

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<sup>4</sup> Brunt (1971/1987<sup>2</sup>, 159 ff.): quotation on p.161 from Chambers and Mingay (1966).

<sup>5</sup> Osborne (1991).

<sup>6</sup> Osborne (1991, 234).

<sup>7</sup> Scheidel (2004).

an equal number of people moved from the countryside to Rome or to any of the over 400 other cities.<sup>8</sup> In addition, two ‘negative’ results of his analysis may be mentioned: (a) private migration was not on a scale comparable to state-sponsored population transfers; and (b) movement from the countryside towards Rome left little scope for further migration within Italy.

Osborne suggests a degree of mobility on a scale quite different from Scheidel’s estimate of 2–2.5 million moving to the colonies, to Rome, and to the burgeoning towns in the last two centuries of the Republic. If we were to project Osborne’s rate of mobility onto republican Italy, the population of which Scheidel estimates to have been 4 million, we would arrive at 40 million individual migratory movements within the last two centuries BC! If valid, such numbers would dwarf Scheidel’s figures. Three observations may be made regarding the comparison of these two sets of figures: (1) Comparing Scheidel’s 2–2.5 million migratory movements to the much higher figures suggested by the early-modern parallels would be comparing apples and oranges, if only because the English figures include much shorter movements from one parish to the next. (2) Osborne’s figures for seventeenth-century England cannot simply be projected into the context of second-century BC Italy. (3) The movement of colonists receiving land from the Roman state and the migration of people towards Rome and other cities should be seen in the wider context of mobility and migratory movements. All these observations suggest that we should distinguish between various kinds of human mobility.

### III. *A spectrum of migration and mobility*

The theorizing and labelling applied in modern literature to mobility reflects the need to simplify and categorize individual behaviour in order to analyse and understand it.<sup>9</sup> The categories themselves may be misleading in the sense that they suggest clearly distinct types and patterns that are in fact absent in reality. Nevertheless, divisions are necessary. We may emphasize three criteria on which categories can be based: (1) A distinction between the movement of individuals and that of households. (It makes a difference to our understanding of migra-

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<sup>8</sup> Scheidel (2004, 19).

<sup>9</sup> See e.g. the categorizations in Moch (1992, 16 f.).

tion whether an individual leaves his/her household, or a conjugal unit leaves a complex household to settle elsewhere, or the entire household leaves its home.) (2) A distinction based on the urban-rural dichotomy, leading to four types of migration: rural-rural (i.e. from countryside to countryside), rural-urban, urban-rural, and urban-urban. (As the distinction between 'urban' and 'rural' is sometimes arbitrary, so are these types.) (3) A distinction, commonly made regarding mobility in later times, based on seasonal, temporary, and permanent migration. The various types of mobility should be seen as part of a spectrum; together they formed a mobility network, each part of which should be seen as interacting with the other parts. In less abstract terms this means, for instance, that individual and/or temporary mobility stimulated the movement of relatives, sometimes leading to the permanent migration of entire households.

#### IV. *Mobility as part of life*

Mobility starts with communication and information. Brunt suggested that people were generally ignorant of the land beyond a distance of twenty miles or so, and hence were reluctant to move. Therefore we should start by asking to what extent travel was a normal part of the lives of common people.<sup>10</sup>

First, the social and political functioning of the common citizens of Roman and allied communities meant that they visited festivals and assemblies, which often required some travel. Rome obviously attracted visitors even from afar. In 114 BC the Roman knight Publius Helvius was returning home to Apulia with his family after having attended the *ludi Romani* in Rome. Julius Obsequens (37) relates this fact because Helvius' maiden daughter was struck by lightning near Cales.<sup>11</sup> In 70/69 BC, Cicero (1 *Verr.* 54) tells us, many citizens from all over Italy had gathered in Rome for the sake of games, elections, and the census. Both passages can serve only as an illustration of a phenomenon that cannot be quantified or qualified. Festivals were quite frequent in Rome, often combining religious festivities with markets. The example of the Gracchans makes it clear that some assemblies and elections attracted voters from the countryside. Even better known, however, is the fact

<sup>10</sup> Cf. the chapter 'A mobile culture?' in Laurence (1999, 136-47).

<sup>11</sup> Discussed by Williamson (2005, 243 f.).

that rural voters were less reliable than urban ones when relied upon to attend assemblies.<sup>12</sup> Nevertheless, at least some of the Roman citizens living throughout Italy may have on occasion visited Rome in their role as citizens. One wonders whether Romans sent out to Latin colonies continued to participate in Roman religious and civic festivities. Roman citizens were exceptional in this regard, though. The focal points of the civic duties of the citizens of allied communities were much closer to home. The territory of the Paelignians, for instance, was much more compact than that of Rome, and one may wonder whether Umbrians or Lucanians did indeed have common festivities. Nonetheless, there were several important shrines that may have attracted people from various backgrounds, possibly from afar. In short, in the third or second centuries BC people were induced by civic or religious duty to travel far, Romans possibly more so than non-Romans.

Second, even smallholders were part of a larger economic world: they sold produce and bought products in various markets. Most daily transactions were obviously limited to a very short range. It is often pointed out that the catchment area for daily transactions was limited to some 10–15 km, reflecting the distance a peasant was willing to travel frequently.<sup>13</sup> However, for our purposes the question is whether trading activities regularly—once or twice a year—involved him in travel over a much wider distance. Country people needed farm implements, livestock, or items of modest luxury (after all, not all of them were poor), which they could not produce themselves. Hence, they travelled to urban or periodic rural markets in order to obtain such goods.<sup>14</sup>

The same applies to the sale of goods. In eighteenth-century Spain, for example, peasants would travel annually over distances of more than 100 km in order to sell crops and other products more profitably than at home. There is evidence regarding the ancient world (from Roman imperial times) indicating that much trade in grain, wine, or olive oil was in the hands of muleteers and petty traders, although there is little direct evidence to link small-scale trade to peasant proprietors.<sup>15</sup> Even so, it seems likely that smallholders would seek profitable markets for

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<sup>12</sup> Nagle (1971, 127) argues, for example, that in order to support Ti. Gracchus rural voters would have had to make as many as six trips to Rome; cf. Williamson (2005, 117 f.).

<sup>13</sup> Bintliff (2002).

<sup>14</sup> De Ligt (1993, 148).

<sup>15</sup> Erdkamp (2005, 139 ff.). Evidence linking small-scale trade with peasant proprietors is presented in De Ligt (1993, 138–9, 212–3).

their products, and would at the same time buy commodities that they could not produce themselves. Environmental differences in production have a large role to play in this regard: the inhabitants of the higher Apennine valleys probably visited the markets of the plains in order to exchange products such as wool and cheese for wine and olive oil. In short, many common people would on some occasions travel over distances of more than 100 km in order to buy and sell goods.

Third, transhumance involves the seasonal movement of livestock and people. The exploitation of seasonal differences between pastures in high- and low-lying areas may have been a feature of all times, but one should realize that transhumance is possible in Italy within fairly short distances.<sup>16</sup> The sources leave no doubt that large-scale and long-distance transhumance did eventually emerge, but long-distance transhumance was not imposed by the landscape. Varro (*R* 2.1.16) mentions the practice of driving herds of sheep from summer pastures in Samnium to winter pastures in Apulia in such a way that it must have been common in his own lifetime, i.e. the mid first century BC. There is no reason, however, to assume that the lives of the central-Italian peoples had for centuries revolved around long-distance transhumance.<sup>17</sup> Before the Roman conquest, Samnites and Lucanians undoubtedly kept herds of cattle and sheep even though they had no access to the plains of Latium, Campania, or Apulia. Even in imperial times the passing of hundreds of thousands of animals led to conflicts between farmers and shepherds. One cannot imagine such a system working in a world of political and administrative partition.<sup>18</sup> Long-distance transhumance, which flourished in the late republican and imperial periods (and again from the later Middle Ages onwards) is better understood as a feature of the political than of the natural landscape.<sup>19</sup> Therefore it is very likely that long-distance transhumance emerged only after the Roman conquest of central and southern Italy (and before the days of Varro).<sup>20</sup> Our sources stress wealthy ranchers as owners, but undoubtedly

<sup>16</sup> Waldherr (2001, 336, 343).

<sup>17</sup> Contra Williamson (2005, 135): "On the eve of Roman expansion, transhumance had become a way of life to one degree or another for most Italian peoples. [...] The peoples [...] of central and southern Italy [...] were especially dependent on transhumance for survival." On this basis she constructs a dichotomy between an urbanized, arable part and a transhumant, pastoral part of Italy.

<sup>18</sup> Brunt (1971/1987<sup>2</sup>, 723 [postscript]).

<sup>19</sup> Garnsey (1998); Horden and Purcell (2000, 198); Waldherr (2001, 337).

<sup>20</sup> Nicolet (1994, 615) dates the emergence of long-distance transhumance to the

smaller farmers were involved as owners of small herds and as wage-earning shepherds. From our point of view, the significant feature of transhumance is that it induced the seasonal movement of members of rural households who temporarily left their homes and with their herds crossed political and cultural boundaries.

Livy (27.38.5) tells us that when the Senate granted exemption from military service to the young men of Ostia and Antium, the latter had to swear an oath not to be absent from their colonies for more than thirty nights while the enemy was in Italy. Two points may be made. (1) This oath makes sense only if being away from home for more than a month was not unusual. (2) Even in 207 BC, when Hasdrubal's army threatened to join that of Hannibal, young citizens of Ostia and Antium had reason to be away from home for fewer than thirty days. Unfortunately, we do not know where they went and for what purpose. The point I wish to make is that we should not underestimate the frequency with which men from all levels of society travelled through Italy and thereby moved across cultural and political boundaries. It does not seem justified to claim that men would be entering an unknown world when travelling more than 30 km from their homes. The second century BC may in fact have been a period in which for many parts of Italy the thresholds for mobility were gradually lowering. The commercialization of Italy and the emergence of transhumance may reflect this situation. Men were probably more familiar than before with regions outside their own, and less hesitant to visit them.

#### V. *Seasonal mobility*

Mobility should be seen as part of a spectrum, at the one end of which we may situate some of the forms of travel we have just seen. Migration may be distinguished from travel by the change in subsistence strategy that it implies. Travelling to a market or festival—or even the seasonal movement of herds—does not imply a shift in livelihood in the same way as does the migration of individuals and households.

Migration is commonly differentiated as seasonal, temporary, or permanent. The principle behind seasonal mobility is that in certain seasons people can earn a higher income elsewhere than by staying

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second century BC. Likewise: Dench (1995, 118); Waldherr (2001, 344). See also Morley (1996, 155 ff.).

within their own household. In early-modern Europe seasonal migration gave rise to interregional networks of integrated labour markets. In Italy, for example, there were two such large networks, one of which connected the mountainous areas in central Italy to the cities and their hinterlands in the western plains, the other connected the western Apennines and the Alps to the prosperous cities and countryside of the plain of the Po. In the eighteenth century an estimated 100,000 people annually left their homes in central Italy to seek employment either as harvesters in the western plains and on Corsica and Sardinia, or in urban construction, harbours, and overland transport. The northern network involved an estimated 50,000 people seasonally employed in the rural and urban sectors.<sup>21</sup>

Economically, seasonal migration was a question of supply and demand for labour. On the supply-side we may make a distinction between structural and seasonal underemployment. Structural underemployment occurs when more labour capacity is available than can usefully be employed. If too many hands have to be employed on too small a farm, the farmer can seek a more labour-intensive exploitation of his land, but he may be restricted in his production strategy by the absence of markets and his primary aim to produce the food his household requires. Fluctuation in the requirement for labour within the year is caused by the seasonality of much of the work on arable farms. Farmers could reduce the seasonal peaks in labour demand by varying their crops, but again the peasant might be limited in diversifying the tasks on his farm by his primary production goal.<sup>22</sup>

Relative underemployment of peasant labour was characteristic of most of the pre-industrial world, but it did not automatically give rise to seasonal migration. Underemployment is in fact a misleading term, since it implies a definite level below which labour is not usefully employed. The overcapacity of labour is better understood as leading to gradually diminishing returns on the input of additional labour in farming. With declining income alternative employment strategies (or not working at all) become increasingly attractive. As a rule, households

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<sup>21</sup> On seasonal labour in early-modern Europe see Moch (1992, 40 ff. and 76 ff. [Italy]); Hoerder (2002, 288 ff.). On labour migration in modern developing countries see Alderman and Sahn (1989, 93 ff.).

<sup>22</sup> On (under)employment in ancient farming see Rosenstein (2004, 63 ff.) and Erdkamp (2005, 61 ff.). For a nineteenth-century example of intensification near an urban market: Martínez Carrión (1988, 94 ff.).

of smallholders rely on many diverse income-earning activities. Seasonal migration is governed economically by the seasonally shifting balance between various employment strategies. In other words, as wages rise in certain seasons, wage-labour becomes a more attractive alternative. Two elements are required for the emergence of seasonal migration: a seasonally fluctuating external demand for labour, and the presence of a large pool of available labour. Many regions in second-century BC Italy offered a large number of workers who were temporarily available and free to undertake any work they liked, unrestricted by lords or guilds.<sup>23</sup>

Both rural and urban seasonal labour undoubtedly occurred in Italy in late-republican times. The sources are sparse, but they explicitly show its existence at least in the rural context. Further considerations make the presence of seasonal labour a very likely possibility also in the urban economy. Many economic activities were seasonal, which is largely a reflection of the importance of agriculture in pre-industrial society. The agricultural calendar created peaks in labour demand, in particular when grain, olives, and grapes were harvested and processed.<sup>24</sup> Cato, discussing the proper payment in kind to wage-earners who harvest and/or thresh the estate-owner's grain, refers to seasonal labour on the estates of wealthy landowners (*Agr.* 136).<sup>25</sup> He also mentions a similar agreement concerning the harvesting and processing of olives (*Agr.* 144). Since the workers have to swear an oath not to steal anything, we are surely dealing with free labourers. Cato stipulates that the contractor has to provide fifty workmen (*Agr.* 144.4), but he does not indicate where they come from.

A reference to seasonal migration in relation to harvesting is found in a brief remark in Suetonius' *Life of Vespasian* (1.4), according to which the emperor's grandfather had been 'a contractor for the day-labourers who come regularly every year from Umbria to the Sabine district in order to till the fields'. Vespasian's grandfather's alleged involvement in the migration of labour should be dated to the second half of the first century BC, but the migration from Umbria to the Sabine country is mentioned by Suetonius as something still going on. Day-labourers

<sup>23</sup> Concerning the latter point, Temin (2004, 515).

<sup>24</sup> Spurr (1986, 134 ff.); cf. Xenophon *HG* 2.1.1: soldiers on Chios found work on the land in summer, but not in winter.

<sup>25</sup> On day labour at vintage and grain harvest see also Varro *R.* 1.17.2. Both passages are discussed in Krenkel (1965, 142 ff.).

were needed especially for such tasks as the grain harvest or vintage, but this need not have involved itinerant labour. Three elements may explain the bringing in of outside labour into Sabinum. (1) In Sabinum the work may have been earlier than in Umbria, so that Umbrian labourers were available while Sabine farmers were working on their own land.<sup>26</sup> (However, seasonal labourers in later times often left it to their wives to harvest their own crops.)<sup>27</sup> (2) In Sabinum commercial farming may have replaced peasant farming to such a degree that there were few local households seeking additional employment, creating the necessity to import day-labourers from Umbria. (3) The Umbrians may have accepted lower wages than the Sabine farmers. These explanations are not mutually exclusive.

We find a similar reference to the organization of migrant harvesters in another time and setting. In the famous inscription of Mactar (Tunisia), dating to the second century AD, a local landowner proudly mentions his humble start in life and his subsequent rise in wealth and status. He had worked for years as one of the farmers who travelled as far as Cirtae (Numidia) at harvest time. At one stage he became head of a team of harvesters who moved from estate to estate.<sup>28</sup> He ended his life as a villa-owner and member of the highest class. Cirtae is explicitly mentioned in order to emphasize the distances involved. We may explain the role of migrant harvesters in Africa in the same ways as in Sabinum.

There is good reason to believe that the annual cycle of expansion and contraction that governed the economy of early-modern cities also characterized their ancient counterparts. Some sectors of the pre-industrial economy were seasonal precisely because they depended on cheap labour, which was available in large numbers at certain times of the year. This did not involve only human labour, but also that of farm animals, which were primarily used to provide power in construction and transport.<sup>29</sup> Hence, much non-agricultural employment had to be timed in accordance with the agricultural calendar.<sup>30</sup>

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<sup>26</sup> Garnsey (1980, 42); Skydsgaard (1980, 69); Spurr (1986, 66); Dyson (1992, 135); Lirb (1993, 285); Laurence (1999, 147).

<sup>27</sup> Borges (2000, 188).

<sup>28</sup> Discussed in Krenkel (1965, 145 ff.); cf. Borges (2000, 191) concerning Spain: 'during the harvest months teams of harvesters moved from estate to estate'.

<sup>29</sup> Discussed in detail in Erdkamp (1999, 556–572).

<sup>30</sup> An idea of the number of animals involved in transport is given by records from Eleusis, which show that in Attica in the 320s BC it took thirty-three teams of oxen

Many non-agricultural activities depended on transportation. Shipping almost came to a halt in wintertime, and most of the shipping that did occur in winter was coastal and short-distance. Concerning the grain supply of Rome, for instance, Cassius Dio (60.11.2) observes that there were no stocks of grain for the winter apart from the grain that was shipped and unloaded in summertime. The slump in sailing during winter meant that ships' crews were unemployed at this time of year. Although the larger freighters did not use rowers and relied solely on the wind, they did require sizeable crews. Moreover, nearly all on-shore activities related to shipping and overseas trade closed down as well. Ships were loaded and unloaded by hand, sack by sack, or amphora by amphora, which in a large harbour provided work for thousands of stevedores.<sup>31</sup> Already in the fourth and third centuries BC the ports of Italy must have needed many workers to man their ships or to work in the harbour and related trades. The growth in the second century BC of the city of Rome and of the shipping in Puteoli and Ostia only increased the need for such labour.<sup>32</sup>

Activities that depended on overseas and river transport were seasonal too. To the extent that manufacture relied on overseas materials, it was also tied to the seasonal cycle of seafaring.<sup>33</sup> Some sources mention the sawing of imported timber and the cutting of marble at the harbour. In so far as building material was shipped from afar—and marble and timber often were—the building trade also experienced a wintertime slump.<sup>34</sup> Frontinus (*de aquis* 123) informs us that building was best done between April and November.<sup>35</sup> In short, urban economies were governed by an annual cycle of expansion and contraction. The seasonal

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three days to move a single column drum of about 7.5 tons from Mount Pendeli to Eleusis. The inscriptions from Eleusis also show that the transport of building stone was undertaken between July and September, which is precisely the time of year that oxen were not needed on the land (taken from Salmon [2001, 199 f.]). It also follows that not only poor peasants were attracted by such labour: only prosperous farmers employed a team of oxen (pointed out by Salmon [2001, 201]); cf. Erdkamp (2005, 19 f.).

<sup>31</sup> Casson (1965, 31). On the guilds in Ostia, Frank (*ESAR* 5.248 ff.); Meiggs (1959/1973<sup>2</sup>, 311–36); Aldrete and Mattingly (1999, 183).

<sup>32</sup> On the growth of commerce and other activity in Rome and Ostia from the fourth century BC onwards see Bispham (2000, 168 ff.). On the expansion of Ostia, Meiggs (1959/1973<sup>2</sup>, 30). On the unloading of cargo and its transportation to Rome, Aldrete and Mattingly (1999, 180 ff. with references to older literature).

<sup>33</sup> For a medieval example see Jacoby (1994, 551).

<sup>34</sup> Strabo 12.8.14 stresses the difficulty of the overseas transport of marble to Rome from Asia Minor.

<sup>35</sup> Cf. Jones (1964, 22 f.) concerning early-modern England.

expansion of urban economic sectors in summertime led to a temporary rise in employment opportunities at that time of year.

What implications does the cyclical nature of the urban economy have for the labour market? Work on ships, in harbours, and in construction was unsuited to slave labour for two reasons. (1) Wages for unskilled labour were nearly at subsistence level; therefore there was hardly a financial advantage to be gained by employing slaves rather than wage-earners. Scheidel has shown that in Roman Egypt and Italy the prices of slaves, the costs of their maintenance, and the wages of unskilled labourers were such that servile labour was hardly cheaper than free workers.<sup>36</sup> (2) Slave owners would have faced serious difficulties in finding meaningful employment for all their slaves in winter.<sup>37</sup>

Because of the short season of much of the economic activity, it has been suggested that the labour required in Ostia was provided by the plebs of Rome: "The sporadic nature of this demand required a large number of unemployed, or at least underemployed, people to be available whenever demand surged. The only group of potential labourers that appears to have met these two requirements of size and constant availability is the urban plebs."<sup>38</sup> However, Aldrete and Mattingly have overlooked the importance of the rural population for non-agricultural work. If we take this issue beyond the context of Ostia, the question is whether all the work in towns and cities was performed by the urban populace itself. If so, the fluctuation in employment must have meant that many labourers were without work and income for large parts of the year. This might fit the commonplace of an idle proletariat living off the dole, but in fact the urban masses needed to work for a living.<sup>39</sup> Apart from a few beggars and vagabonds, the urban populace

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<sup>36</sup> Scheidel (2005b), providing a contrast with classical Athens, where wage-labour was relatively expensive.

<sup>37</sup> Brunt (1966, 16; 1980, 84 f.).

<sup>38</sup> Aldrete and Mattingly (1999, 201). Thornton and Thornton (1989, 34 ff.) argue that in the first century AD there were no free peasants left in central Italy. Based on this doubtful assumption, they believe that in this period building programs did not attract temporary workers from the countryside and that all labour was urban-based.

<sup>39</sup> Thus Brunt (1966), pointing to Vespasian's famous dictum 'let me feed my masses' (Suet. *Ves.* 18). The alternative interpretation in Casson (1978) is refuted by Brunt (1980, 81 ff.), who refers also (p. 97) to Plu. *Per.* 12.5, where we are told that Pericles started his magnificent building program in order to offer work and income to those who did not serve in army or fleet. Livy 8.20.4 mentions that during an emergency in 330 BC all men were mobilised, including the artisans and 'sitting' craftsmen (*sellularii*), who were not fit for war. Interestingly, the urban plebs is not derided as 'idle'.

needed to work in order to have food, housing, fuel, and clothes. The introduction of the grain dole may have lessened the necessity to earn one's living throughout the year. However, a subsidy on subsistence in Rome may very well have lowered wages, because part of the cost of subsistence was shifted to the state. When Gaius Gracchus created the corn dole in 121 BC, this meant only that grain was sold cheaply, not handed out for free. The common people buy their food day by day, Tacitus (*Hist.* 4.38.2) writes, and this undoubtedly goes for the second century BC as well. Even if they earned enough in summer, the wages of the unskilled labourers were insufficient to build up reserves to tie them over prolonged periods of unemployment, the more so as prices tended to rise with the progress of winter. In short, if we reject the idea that large parts of the urban populace were unemployed in winter, we must assume the influx of labourers in summer, most of whom were attracted by the season's opportunities to earn an income on the ships, in the harbours, and in related trades.

The conditions that created labour mobility in early-modern Europe were also present in Antiquity. Labour capacity and employment opportunities were not evenly spread across space, while especially the latter were subjected to short-term fluctuations. In an integrated labour market supply and demand created movements of people between one area and another. Times of peak demand may have attracted men from smallholding households, who returned to subsistence agriculture as demand decreased. It has been estimated that in the late Republic and early Principate 4–6% of the total population of Rome could have been employed in the building industry.<sup>40</sup> I cannot judge the accuracy of this estimate, but surely fluctuations in the demand for labour must be taken into account. Unfortunately, there are hardly any sources that allow us to reconstruct the labour management in building. The best piece of evidence is provided by a ruling in the Digest:

The person who has contracted to build an *insula* should not hurry, mustering builders (*fabri*) from all quarters and providing a host of day-labourers, nor on the other hand should he be content with one or two, but he ought to avoid extremes in accordance with the rational practice of a careful builder, having regard to time and sites. (Venuleius, *Dig.* 45.1.137.3)<sup>41</sup>

<sup>40</sup> DeLaine (2001, 231); cf. Kolb (1985, 485).

<sup>41</sup> *item qui insulam fieri sponndit, non utique conquisitis undique fabris et plurimis operis adhibitis festinare debet nec rursus utroque aut altero contentus esse, sed modus adhibendus est secundum rationem*

The passage indicates a distinction between professional builders (*fabri*) and day-labourers. The professional builders could be servile or free—there is no way to tell. The majority of the day-labourers were probably free. According to Brunt, ‘the implication is that a builder might have no more than one or two permanent employees, no doubt slaves’, and that he would hire extra hands for large projects.<sup>42</sup> I see no compelling reason for the first half of this interpretation. In my view, Venuleius’ point is that a building contractor is not obliged to hurry a project by putting a lot of workmen on the job, but nor should he put only one or two men on it. Nothing is implied about the position of the ‘one or two’ workmen. The evidence suggests that building contractors employed a more or less permanent core of professional builders, among whom were probably many slaves, and an additional and more fluid workforce that consisted of free day-labourers and hired teams of slaves.<sup>43</sup>

Large projects offered much work for the urban population, but undoubtedly also attracted many unskilled workers from the countryside.<sup>44</sup> Nicolet identifies periods of heavy building in and near Rome in 194–174 BC, when the river port of Rome and the large warehouses were built, and in 144–136 BC, when the Aqua Marcia was constructed.<sup>45</sup> Increased demand for labour meant higher wages, which in turn strengthened the ‘pull’ of the city.<sup>46</sup> When large projects were finished, there was nothing to retain many unskilled workers, who therefore returned to the countryside.<sup>47</sup> Whether these migrants came for one season or for a couple of years depended on how much work was available throughout the year, and how much was strictly seasonal.

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*diligentis aedificatoris et temporum locorumque.* Translation quoted from Brunt (1980, 87). On public building contracts in the second century BC see Frank (*ESAR* 1.152 ff.).

<sup>42</sup> Brunt (1980, 87).

<sup>43</sup> On the hiring of day-labourers, Brunt (1980, 88 ff.). On slaves in construction, Schumacher (2001, 136 ff.).

<sup>44</sup> For the comparison with classical Athens see Salmon (2001).

<sup>45</sup> Frank (*ESAR* 1.183 ff.); Nicolet (1994, 626); also Boren (1957/58) pointed out that government spending on major construction projects was concentrated in particular periods. His hypothesis is that the ending of such projects resulted in urban unemployment and misery, but he may be overlooking the importance of temporary migration in response to government spending.

<sup>46</sup> Thus Brunt (1980, 93).

<sup>47</sup> Cf. Temin (2004, 518).

Not all cities grew in the second century BC,<sup>48</sup> and urban centres did not emerge in the interior districts of Italy before Augustan times. Much of the growth in Italy passed the mountainous areas by.<sup>49</sup> Many of the seasonal migrants in early-modern Italy came from the mountainous areas in the central and northern Apennines and from the Alpine region, the reasons for which have to be sought in local economic conditions. In the absence of nearby markets, there were few opportunities to intensify the cultivation of the smallholders' plots. At the same time the mountainous regions offered few alternative employment opportunities, again due to the absence of markets and intensive agriculture.<sup>50</sup> Exactly the same conditions applied to the peoples of the Apennines in Roman times, who therefore sought external employment opportunities. Just as the Swiss or Scots in early-modern Europe, the people of highland Italy were traditionally seen as mercenaries.<sup>51</sup> The central Apennines are praised for their soldiers.<sup>52</sup> The region also offered naval crews. Zonaras (8.11.8) mentions that during the First Punic War Samnites had turned up in large numbers to man the Roman fleet, which seems to imply naval experience. Likewise, Livy (28.45.19) says that Marsi, Marrucini, and Paeligni volunteered for the fleet in 205 BC. Dionysius of Halicarnassus (15.6.3 [c.327 BC]), moreover, says that the Samnites promised to furnish all the rowers that the fleet of Naples would need to fight against Rome.

Lack of land and seasonal unemployment on their farms had always plagued the smallholders of the interior, but in the second century BC they saw increasing opportunities to earn money in the cities and countryside of the prosperous plains. The city of Rome grew in size, and so did cities like Fregellae, Minturnae, Puteoli, and Ostia.<sup>53</sup> Agriculture

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<sup>48</sup> E.g. Cosa, Tarentum, or Naples. On Cosa, Morley (1996, 178). For a list and discussion of declining towns, Broadhead (2002, 43 ff.).

<sup>49</sup> Spurr (1985, 126 f.) notes that the development of market-oriented slave-based estates was closely linked to the availability of nearby markets, which in most parts of Italy were absent until the first century BC.

<sup>50</sup> Cf. Belfanti (1993, 260 f.). The economy of the mountains responded in two ways: with temporary and seasonal migration, and with specialization in rural manufacture. There is little evidence of rural manufacture for outside markets in second-century BC Italy.

<sup>51</sup> Dench (1995, 55); Horden and Purcell (2000, 228 and 387); cf. Gallant (1991, 134 ff.) on poverty driving men into service as rowers and mercenaries. On early-modern Europe, Hoerder (2002, 63 ff.).

<sup>52</sup> See references in Dench (1995, 68 n.4 and 129 n.78). This cannot all be due to their image of rusticity and uncorrupted morals (cf. in particular 113 ff.).

<sup>53</sup> On Minturnae and Fregellae, Broadhead (2002, 49 ff.).

in coastal regions intensified and commercialized, owing to the growth of internal and external markets for its products.<sup>54</sup> Because in parts of central and southern Italy commercial farming already existed in the third century BC, it is likely that conditions similar to those later seen in Sabinum and North Africa already gave rise to migratory flows of wage-earners. As slave-run farms emerged and local smallholders intensified their farming practices, the need for outside labour in peak times increased.<sup>55</sup>

### VI. *Temporary migration*

Employment opportunities encouraged people to leave their homes, sometimes for a season, sometimes for a number of years. While the reason for seasonal mobility is mainly to be sought in the fluctuation in income-earning opportunities within the year, the principle behind temporary migration lies largely in the life cycle of the families and individuals involved. In early-modern Europe many young adults (from their early teens onwards) would spend up to fifteen years away from home, either in cities or in rural areas offering wage-earning opportunities. Women would generally be employed in domestic service, while male migrants found work largely in manual labour, in particular construction, mining, canal or road building, and in handicraft, or they would serve in the army, navy, or commercial fleet. In another pattern of temporary migration young teens entered the households of more prosperous relatives who lacked children of similar ages. Both movements—young adults leaving their households and teens entering the households of relatives—were means to deal with the changes over time in the labour requirement and composition of households.<sup>56</sup> At certain stages in this so-called family cycle, many households could do without the additional labour of teens growing up. Seeking employment outside the household did not necessarily imply moving over a considerable distance, but for those growing up in regions offering little work, it usually did. Most temporary migrants returned when they had reached the age to form their own household. In western Europe this

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<sup>54</sup> Findings are summarized in Morel (1989, 496 ff.); Nicolet (1994, 614 f.).

<sup>55</sup> Morley (1996, 129 ff.).

<sup>56</sup> On this phenomenon in Spain, Martínez Carrión (1988, 96); Altman (1997, 258). In Japan: Hayami and Kurosu (2001, 308).

meant that many men and women returned after their mid-twenties. In the nineteenth century many migrants sent money to their families. The households left behind not only benefited from the migrants' wages, but also—and often mostly—from the withdrawal of excess labour and an extra mouth to feed. But again, temporary migration was also based on the consideration of income earning opportunities at home and elsewhere. The dividing line between temporary and permanent migration was obviously thin, as many men and women intended to return but never did, some because they found husbands or wives elsewhere, others because they died young.<sup>57</sup> Also the demarcation between seasonal and temporary migration is not clear-cut: in what may be seen as an inverse migratory pattern, many early-modern migrants returned home for a short time during slack periods and bad seasons.

With regard to temporary migration in Antiquity, a distinction should be made between men and women. The sparse evidence on the age of marriage of men points to an average in their late twenties or around thirty.<sup>58</sup> Admittedly, the sources are limited largely to town dwellers of at least moderate prosperity, and are of imperial date. Marriage patterns may differ in town and countryside and between periods. Nevertheless, the evidence that we have indicates that the age of marriage of rural men allowed them to leave their households and return in order to marry after a long period of outside employment.<sup>59</sup>

I want to stress that temporary migration does not mean that the employment opportunities were necessarily short-lived. It was rather the changes within the life cycle of the individual migrants that determined its temporary nature. Hence, many migrants found work that was more or less permanent, but decided to leave nevertheless. Domestic service in early-modern Europe provides a good example: both men and women left it because they had other plans, not because the need for servants stopped. The important point is that part of the male

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<sup>57</sup> In general, Hoerder (2002, 79 f.). Temporary migration in the Iberian peninsula: Reher (1990); Borges (2000, 184 ff.).

<sup>58</sup> On age of marriage of men, Brunt (1971/1987<sup>2</sup>, 137 f.); Parkin (1992, 125); Saller (1994, 25 ff.); Rosenstein (2004, 82 ff.).

<sup>59</sup> Reher (1990, 182) notes that the age of marriage of women migrating into Cuenca was higher than of those who were born there. See Hayami and Kurosu (2001, 309) for a similar phenomenon in Japan. Martínez Carrión (1988, 106) notes that the age of marriage was highest for those men whose work implied much mobility, such as merchants and muleteers. Solien de Gonzalez (1961, 1268) observes that the temporary migration of men tends to increase the age difference between husbands and wives.

labour pool in second-century BC Italy (just as in early-modern times) was very flexible and transitory, owing more to the needs and nature of rural households than to the uncertainty of the economy, which is not to deny that such labour was well suited to respond to short-term fluctuations in labour demand.

We have no real evidence for the labour mobility of adult males in republican Italy. However, military service may be seen as a kind of temporary migration, since it meant that young men left the household before marriage, and during part of their life found subsistence in the army. In earlier times mercenary service may have fulfilled the same function. Most recruits, who were seventeen or eighteen when first drafted, may have expected in the second century BC to serve for not more than six years or so.<sup>60</sup> Even if they did serve in several shorter campaigns, few conscripts would still be in the army when they had reached a marrying age. In the Roman army of Polybian times only a minority of soldiers (the *triarii*) would be in their late twenties or older.<sup>61</sup> Military service may therefore have fulfilled similar functions as did temporary migration. From this point of view, it did not matter whether enrolment in the Roman army was welcomed or not. It withdrew labour from smallholding families and thereby increased the value of the labour that remained, while at the same time reducing the food requirements of the household. Since conscription was not voluntary (which is not the same as unwanted), it was a factor in the family cycle that could not be controlled, although it could be predicted. In this sense military service was an important factor in establishing patterns of household formation. Moreover, we should not underestimate the role of voluntary service. In particular for the second half of the second century BC the sources indicate that more and more men served as volunteers in the Roman army.<sup>62</sup> Those too poor to serve in the infantry may have found similar employment in the Roman navy. The increasing desire of the rural poor to serve in the army may thus be an important factor in the proletarianization of the Roman army in the second century BC.<sup>63</sup>

<sup>60</sup> Smith (1958, 5 f.); Taylor (1962, 24); Brunt (1971/1987<sup>2</sup>, 399 ff.); Rosenstein (2004, 189 f.).

<sup>61</sup> Rosenstein (2004, 85 f.).

<sup>62</sup> App. *Pun.* 75; *Hisp.* 84; Sal. *Jug.* 86.2; Plu., *Mar.* 9.1, *Moralia* 201A; Plb. 35.4.14.

<sup>63</sup> In more detail, Erdkamp (2006).

The situation was different for women in Antiquity compared to early-modern Europe, when most migrant women were employed in domestic service. Ancient women would have found little work in the households of the rich, because slavery prevented the freeborn from entering domestic service.<sup>64</sup> Households that could afford to employ people would buy slaves. The preference for slaves was probably largely caused by the status they offered.<sup>65</sup> The only exception may have been wetnursing, the temporary nature of which was obviously better suited to wage-labour. Social values more than practical considerations or biological constraints meant that there were very few other employment opportunities for women. Female labour outside the household was either performed by slaves, or it was regarded as indecent.<sup>66</sup> Egypt has provided some evidence for craftswomen, in the form of apprenticeship contracts. Two elements are noticeable: (1) apprenticeship contracts for freeborn women are few in number compared to those for men: three against twenty-eight; (2) crafts were mostly practised in the domestic sphere.<sup>67</sup> This is not to say that the work of women was unimportant. Within the household they may have exploited wage-earning or commercial activities outside the primary subsistence activity.<sup>68</sup>

With regard to female labour one may point to a fascinating passage in Pausanias concerning textile production in the northern Peloponnesian harbour-town of Patrae:

The women of Patrae outnumber the men by two to one. These women are amongst the most charming in the world. Most of them gain a livelihood from the fine flax that grows in Elis, weaving from it nets for the head as well as dresses.<sup>69</sup>

There may be two plausible explanations for the imbalance between the sexes in Patrae, which are not mutually exclusive. (1) The employ-

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<sup>64</sup> There is actually one ancient passage lamenting the loss of domestic employment opportunities owing to slavery. Athenaeus *Deipn.* 6.264c, quoting Timaeus of Tauromenium, says that in the old days the Phocians did not have slaves: 'Mnason, the friend of Aristotle, who had acquired a thousand slaves, became obnoxious to the Phocians because he had deprived so many citizens of the necessary means of sustenance.'

<sup>65</sup> Just as servants became a symbol of status in early-modern England: Hoerder (2002, 288).

<sup>66</sup> See the survey of literary and epigraphic evidence in Treggiari (1979).

<sup>67</sup> Van Minnen (1998).

<sup>68</sup> Cf. Erdkamp (2005, 87 ff.).

<sup>69</sup> Pausanias 7.21.14; cf. Horden and Purcell (2000, 352 f.).

ment opportunities offered to women in or near this town, which apparently was a local centre for textile production, may have attracted female labour from the surrounding countryside or from neighbouring regions. (2) The large degree of male employment in shipping may have resulted in fewer adult men in Patrae, not unlike many coastal villages in early-modern times. In the first case, it would be a rare instance of independent female labour and migration. In the second case, we should have an interesting example of male labour migration.<sup>70</sup> The possible example of Patrae does not really contradict the hypothesis that work outside the household was primarily undertaken by men.

In short, temporary migration was not an option open to many ancient women, since this was precluded by the dominance of slave labour in the domestic sector. For that reason there was no point in postponing the marriage of daughters, a fact which agrees very well with our evidence for the age of first marriage of women. Although the evidence is to be treated with the same caution as that for the age of marriage of men, it points to a fairly young age. While in western Europe rural women married in their late twenties, women in the ancient world generally seem to have married much earlier, in their late teens or early twenties, while some girls may have married even earlier.<sup>71</sup> Such an early marriage precluded a previous phase of wage-earning outside the household.<sup>72</sup> In short, individual women were surely much less mobile than individual men, and in this respect the second century BC brought little change. Any female mobility was mostly in the context of the household they were (to be) part of.<sup>73</sup> Seasonal and temporary migration were therefore generally forms of individual male mobility.<sup>74</sup> The immobility of individual women has important implications for our understanding of the demography of the city of Rome.

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<sup>70</sup> On the textile industry in Naupaktos (on the other side of the Gulf of Corinth from Patrae), McCormick (2001, 535).

<sup>71</sup> Shaw (1987); Parkin (1992, 124); Saller (1994, 25 ff.); Frier (1999, 91); Scheidel (2001, 33).

<sup>72</sup> In contrast to the European family systems: Moch (1992, 32).

<sup>73</sup> Similar in sixteenth-century Spain: Altman (1997, 259). Because there was little work to be found for women in those places to which male labourers went, there was also no point for married men and women to seek temporary employment as a couple. For an example of employment migration of couples see Borges (2000, 182 f.).

<sup>74</sup> The permanent migration of families and households is discussed below.

VII. *The growth of the city of Rome*

While estimates of the extent of Rome's growth vary, most scholars agree that it could not have been sustained by the city's population itself.<sup>75</sup> The demographic models of other pre-industrial societies show that mortality levels in cities were higher than fertility levels, which means that immigration was necessary just to keep the population from declining. The causes of the 'urban graveyard effect' are still a matter of debate. Already in the eighteenth century scholars pointed to the phenomenon that the number of burials exceeded the number of births in almost all contemporary towns and cities. Two explanatory models have governed the modern debate. From the start, explanations focused on the high number of urban deaths. It was argued that high mortality resulted from infectious diseases doing well in the environment of large and crowded cities. The environment of the cities created urban disease pools that resulted in a significantly higher mortality level in towns and cities than in the surrounding countryside.<sup>76</sup> Sharlin made a vital contribution to the debate by stressing the crucial importance of urban fertility rather than mortality. While mortality levels were undeniably higher in larger cities, they were rarely so high that they could not be overcome by fertility. Sharlin argued that deaths exceeded births in cities due to the social circumstances governing levels of fertility there. In particular, migrants failed to reproduce themselves.<sup>77</sup> These models are not mutually exclusive, and it is important to note that both models accept that in virtually all premodern towns and cities the number of deaths was higher than the number of births.<sup>78</sup>

Some scholars argue that those people who were born and bred in the city built up a high resistance to the diseases that were endemic there, and that it was especially the migrants who died in large numbers. This would mean that the mortality figures for city-born people should be lowered and those of the migrants raised. However, regular and intensive contact between urban dwellers and the people of the countryside would diminish the imbalance between urban and migrant

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<sup>75</sup> Morley (1996, 39 ff.); Jongman (2003, 106 ff.).

<sup>76</sup> Wrigley (1967). Likewise, Wrigley (1990, 103 f.); also stressing mortality: Landers (1987).

<sup>77</sup> Sharlin (1978). See also the criticism by Finlay (1981) and Sharlin's response (1981).

<sup>78</sup> Galley (1995, 451) rightly emphasizes "the interplay between migration, fertility, and mortality". An example of such an approach: Galley (1995, 451).

mortality. In the case of republican Rome, one might argue that country dwellers visited Rome regularly, often for a considerable time. As a result of the contact between city and countryside, the rural population would be exposed to the infectious diseases carried by returning visitors and migrants. Hence, a model according to which migrants entered a completely alien disease pool may not be valid.<sup>79</sup>

In the case of second-century BC Rome we have to assume that an influx of immigrants was necessary merely to maintain the city's population, and even higher levels of immigration to produce the city's rapid growth. Livy informs us that in the 180s and 170s BC the Roman authorities on occasion removed Latin migrants from Rome, showing (according to Livy) that already at that time masses of foreigners swelled the city. However, the fact that the senate acted only in response to pressure from the Latin colonies shows that the authorities in Rome had no problem with migrants swelling the city's population. This is confirmed by the nature of the measures taken in 178/7 BC: only when the magistrates of the Latin towns could show that migrants living in Rome had been included in a local census held after 204 BC would the Roman authorities order their expulsion. When Livy reports that in 187 BC twelve thousand Latins returned home, this figure tells us very little, since we have no way of knowing how many Latins (and other Italian) migrants remained.<sup>80</sup>

In the absence of reliable figures, estimates of the growth of republican Rome are inevitably rough and hypothetical. Neville Morley estimates a population of 200,000 at the beginning of the second century and 500,000 in 130 BC. Walter Scheidel (following Brunt) more conservatively assumes that the city grew from 150,000 in 200 BC to 375,000 in 100 BC.<sup>81</sup> Moreover, the excess of deaths over births not only applies to the city of Rome, but also to the other cities of Italy, some of which—such as Ostia and Fregellae<sup>82</sup>—also appear to have experienced rapid growth in the second century BC.

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<sup>79</sup> *Contra* Morley (1996, 44 f.). Noy (2000, 18) notes that mortality may have been very high among imported slaves.

<sup>80</sup> Livy 39.3 (187 BC), 41.8–9 (178/7 BC), 42.10 (173 BC). See now Broadhead (in this volume).

<sup>81</sup> Morley (1996, 39); Scheidel (2004, 14); cf. Brunt (1971/1987<sup>2</sup>, 383 f.); Bispham (2000, 168 ff.); De Ligt (2004, 741 f.).

<sup>82</sup> Broadhead (2002, 49 ff.).

Neville Morley and Wim Jongman use Wrigley's figure for early-modern London, which had an excess mortality of roughly 10 per 1,000.<sup>83</sup> Scheidel uses the same figure in his calculations, which means that if Rome had 300,000 inhabitants, 3,000 more people would have died each year in the city than were born there.<sup>84</sup> Morley gives a figure of 7,000 migrants per year coming from the rest of Italy. The result, in his view, was a veritable drain of rural Italy, leading to overall population decline.<sup>85</sup> Preferring not to imagine rural depopulation, Jongman points to the importation of slaves as the element that made the massive growth of Rome and other cities demographically possible.<sup>86</sup>

Scheidel criticizes Morley's and Jongman's calculations concerning the number of immigrants involved in the growth of Rome. Their calculations assume that if 10 more people per 1,000 inhabitants died, 10 immigrants per 1,000 inhabitants were needed to compensate for the population loss. Scheidel, however, halves this figure, because the mortality figure includes infants and children, who would never have reproduced themselves. He argues that because adult immigrants imply much higher fertility, one does not need one immigrant for each death in excess of births: "In fact, 500 adults (who in terms of reproductive capacity equal 1,000 newborns) are sufficient to counterbalance a deficit of 1,000 births in this population."<sup>87</sup> On the basis of this assumption, and taking into account the involuntary influx of slaves, Scheidel calculates the numbers of immigrants required to sustain the city's size and its growth in the second century BC: "Based on the above estimates, the last two centuries BC would have witnessed the permanent transfer of the equivalent of some 1.8 to 2.2 million live births to the cities. Assuming that young adults dominated the movement, the actual number of migrants would surely have reached one million, but need not have surpassed this number by a very wide margin."<sup>88</sup> Scheidel's further estimates of various migratory movements imply that 500,000

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<sup>83</sup> Wrigley (1967, 46).

<sup>84</sup> Based on Wrigley's study of pre-industrial London.

<sup>85</sup> Morley's model is severely criticized by Lo Cascio (2001, 113 ff.).

<sup>86</sup> Jongman (2003, 109 ff.); cf. Brunt (1971/1987<sup>2</sup>, 387), who for the year 70 BC assumes that "slaves and freedmen accounted for well over two-thirds of the urban population". As De Ligt (2003, 24) shows, Jongman's calculations are based on an incorrect application of Wrigley's model, and his figures are therefore excessively high.

<sup>87</sup> Scheidel (2004, 17). So also Lo Cascio (2001, 117 f.); cf. Morley (1996, 53): 15,000 *births* 'earmarked for Rome' annually.

<sup>88</sup> Scheidel (2004, 19).

young adult men and 500,000 young adult women were involved in the above mentioned transfer to Rome and the other cities.<sup>89</sup>

Scheidel's use of the phrase "permanent transfer" tends to obscure the number of people who migrated to Rome (and other cities) but did not intend to stay there permanently.<sup>90</sup> To the extent that temporary migrants returned before marriage, their reproductive capacity did not benefit Rome.<sup>91</sup> The influx of temporary migrants to early-modern metropoleis was actually so large that their presence contributed significantly to the total population figures. The populace of London, for example, consisted for a significant part of men and women who for ten or fifteen years worked in the households of the rich. The conditions for the rural population in late-republican Italy (as discussed in previous sections) indicate a large influx of individual migrants who were attracted by the income-earning opportunities of the city. If we assume that only 1–2% of the adult population of rural Italy temporarily lived in Rome at any one time, this means that a significant percentage of the capital's populace consisted of temporary migrants.<sup>92</sup> Their presence boosted its population, although they did not contribute proportionally to the city's fertility.

Scheidel significantly reduces the number of migrants compared to Morley's and Jongman's estimates. However, I doubt whether we may actually halve the number of migrants in relation to excess mortality. On the one hand, the age structure of the city was indeed altered by the influx of migrants, who were probably mostly young adults. Migration therefore diminished overall levels of mortality. In other words, the demographic impact of migrants on the city of Rome was twofold: migrants added directly to the population of the city; and by changing the age structure of the city they lowered urban mortality and increased urban fertility. On the other hand, the children born to these migrants would also have been subject to a regime of high mortality. According to

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<sup>89</sup> Scheidel (2004, 17).

<sup>90</sup> Williamson (2005, 262); cf. Moch (1992, 44): "Unfortunately, we can never know how many people actually moved into and out of the early modern city. [...] We can only be certain that net figures of city growth [...] come nowhere near to measuring migration..." On the fluctuation in a city's population, Horden and Purcell (2000, 382 f.).

<sup>91</sup> Concerning eighteenth-century Reims, for example, it is pointed out that due to outward migration of immigrants "at best one urban marriage in three is 'useful' for the urban reproduction": Fauve-Chamoux (1994, 50).

<sup>92</sup> Assuming 2.5% of men and 0.5% of women and a rural population of 3 million, about 25,000 temporary migrants would be living in Rome.

Scheidel's own estimate, half of them would have died before reaching reproductive years. The children born to migrants are included in the unequal balance between births and deaths. If in eighteenth-century London the number of deaths exceeded the number of births by 10 per 1,000 inhabitants, this included the children born to the city's migrants. In short, while Morley and Jongman may underestimate the impact of the migrant's age structure, Scheidel overestimates it.

Another shortcoming of Scheidel's model is its assumption that an equal number of men and women migrated to Rome, which he has to postulate in view of the supposed high fertility among young adult migrants, on which his rejection of Morley's and Jongman's calculations is based.<sup>93</sup> The importance of the migrants' sex ratio has been noted by Lo Cascio, who acknowledges that "if the majority of migrants were male, their mean contribution to reproduction was lower than average".<sup>94</sup> However, he tries to circumvent this problem by arguing that "without significant female immigration, Rome could never have developed into a mega-city in the first place".<sup>95</sup> As Rome did indeed become a mega-city, his argument goes, the number of female migrants must have been sufficient. What his argument shows, however, is merely that we should be questioning some of the suppositions that are made concerning the demography of the capital.

Demographers express the growth rate of a population as the number of daughters born to each woman. In other words, men are demographically irrelevant in the sense that fertility is solely dependent on women. Early-modern cities attracted large numbers of women who sought employment in the households of the rich. In fact, a city like London counted more women than men, not only among its immigrants, but even among its entire population. (The large share of unmarried female servants also explains the huge number of foundlings in eighteenth- and nineteenth-century cities, whereas the children of female slaves in Rome simply had the same status as their mothers.) A large proportion of women characterizes early-modern cities in western

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<sup>93</sup> Cf. Scheidel (2004, 17): "the annual injection of (say) 500 male and 500 female young adults into a population of 100,000 that suffers from an annual shortfall of 10 births per 1,000 would overcompensate by 100 per cent."

<sup>94</sup> Lo Cascio (2001, 118).

<sup>95</sup> *Ibidem*.

Europe, in contrast to eastern Europe and Asia, where cities counted more men than women.<sup>96</sup>

Unfortunately, there is no evidence on the sex ratio in Rome, but there is good reason to assume that ancient Rome adhered to the eastern model rather than to the western European model.<sup>97</sup> We may begin with the sex ratio of those people born in Rome. At birth the sex-ratio must have been about equal, but gender specific mortality rates influenced the relative numbers of men and women. It has been observed over a wide spectrum of premodern societies that mortality among women during their reproductive years was higher than among men of the same age.<sup>98</sup> Adult women were more vulnerable to infectious diseases than adult men. Reasons are partly related—directly and indirectly—to maternity: pregnancy and breast-feeding put stress on the female body, which increased susceptibility to diseases, while the immune system was also weakened. It was infectious disease that caused high mortality in Rome, and which further increased the effect of differential mortality on the sex ratio of that city. The relatively young age of marriage in Antiquity may have increased the risks of childbearing. One particular study shows that “women who had their first birth under age 20 were 30 to 50 percent more likely to die”.<sup>99</sup> Part of the explanation has to be sought in the differences in entitlement to the household’s resources between men and women. Men were usually in control of these resources, and males were usually valued higher than females within the household. That such attitudes also prevailed in Rome is shown by the fact that the grain dole was limited to adult men and that *alimenta* benefited boys much more than girls. It is also likely that infanticide and exposure affected daughters more than sons.<sup>100</sup> An opposite effect may have resulted from military

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<sup>96</sup> Also York counted more women than men from the late seventeenth century onwards: Galley (1995, 457). In general, Moch (1992, 46, 56, 89). Statistics on foundlings in early-modern Europe cannot be used as comparative evidence for antiquity.

<sup>97</sup> An unequal sex ratio would also mean that the multiplier of 3.6 usually used to calculate the total population from a given number of adult men is not valid for Rome. Dio Cassius 54.16.2 claims that in 18 BC there were fewer women than men among the upper classes of Rome. The implications or indeed the truth of this statement are unclear. On this passage, Parkin (1992, 98 ff.); Rosenstein (2004, 94). Noy (2000, 60) assumes also that migrants entering imperial Rome were predominantly male.

<sup>98</sup> Alter et al. (2004).

<sup>99</sup> *Ibidem*, 355.

<sup>100</sup> On female infanticide in China, Campbell et al. (2004, 68); cf. Hayami and

service. However, the role of the urban populace in the Roman army seems to have decreased rapidly after the Hannibalic War. In short, it seems that gender specific mortality levels soon caused an unbalanced sex ratio among those born in Rome.

As we have seen, migration of free people into Rome probably aggravated the unequal sex ratio. The predominance of servile labour in domestic service in Antiquity ruled out good employment opportunities for rural women in a city like Rome. Also a young age of first marriage would not fit a protracted wage-earning phase in the lives of ancient women. It seems that the existence of a high number of female migrants is indissolubly connected to a high age of marriage in western Europe, in contrast to a low number of female migrants and a low age of first marriage of women in eastern Europe. (Incidentally, the lack of marriageable women in Rome may have been an added incentive for male migrants from the countryside to return home when seeking to marry.) Hence, an important element of rural-urban migration in early-modern western Europe is in fact absent from Antiquity. Even if we assume that as many as one-quarter or one-third of all Rome-bound migrants were female, the fertility levels among the migrants in general would still have been very low.

This leaves the influx of slaves, which may actually have had an opposite effect. It is likely that among the slaves who were brought into Rome the females outnumbered the males. Even if we assume that the sex ratio among the slaves sold into Italy was equal, it seems likely that many men were sold to the farms and mines in the countryside, leaving a smaller part to be employed in Rome and other cities. Whether as slaves or as freedwomen married to the freeborn and freedmen of Rome, their presence improved the sex ratio in Rome and increased the capital's fertility rate.

### VIII. *Conclusion*

The fact that the population of early-modern England was incredibly itinerant does not necessarily lead to the conclusion that the inhabitants of republican Italy were equally mobile. A smaller share of landowning peasants and, conversely, a larger share of landless rural labourers in

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Kurosu (2001, 307): "when couples without a son but with two or more daughters had another child, they were twice as likely to have a boy as a girl".

seventeenth- and eighteenth-century England raised levels of mobility compared to republican Italy.<sup>101</sup> Employment opportunities in Antiquity were constrained by the existence of slavery, which meant that certain types of employment in town and countryside were less available to freeborn people or not at all. This obstructed the mobility of women in particular, in contrast to England, where many thousands of girls found work in domestic service each year.

However, while mobility may not have been as high as in early-modern England, it certainly was not low. In particular, economic developments in Italy before and during the second century BC are likely to have boosted mobility as Rome and other towns and cities grew, and overseas trade, urban markets, and commercial agriculture expanded, thereby offering more employment opportunities than ever before. The growth of Rome reflects the attractiveness of the city to country-dwellers, since the growth in population cannot be explained without high levels of rural-urban migration. Less spectacular, but equally significant, is the growth of cities such as Minturnae, Fregellae, and Ostia. One employment sector that was of vital importance in early-modern Europe was not available to free-born people in Antiquity: domestic labour. Owing to the predominance of slaves in this field, there were few employment opportunities for women, who could hardly find proper work outside the household. Lacking the opportunity to earn a wage, women married young and participated little in individual migration. If they migrated, they did so as part of a household. Irrespective of colonization and land distribution, the existence of tenancy and a land market meant that the households of smallholders could acquire at a distance the means to continue farming their plots of land. In short, the population of Italy in the second century BC was far from immobile, inert, or stationary.

Roman demography depends on the use of models derived from better documented societies that can help modern historians to make quantitative and causal reconstructions of ancient society. However, we should be very careful in using comparisons, because similarities in one respect can hide differences in another. Mortality and migration are integral parts of all modern reconstructions of urban demography. However, conditions governing mortality in ancient Rome may have resembled the early-modern situation more closely than did the circumstances of migration in the ancient and early-modern world. Slavery in

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<sup>101</sup> Moch (1992, 10).

particular makes it hazardous to project early-modern situations onto the ancient world.

In my view, the urban graveyard effect was very much a mixture of the two explanatory models that govern the modern debate. The factors that caused high levels of mortality and low levels of fertility both contributed to the natural decrease in premodern urban populations. However, these factors worked to different degrees in different cities. For example, excess mortality in small towns (like the Spanish town of Cuenca, which had 5,000 inhabitants in the mid nineteenth century) cannot be explained solely by urban disease-pools. It is this complex causality that makes it so very difficult for ancient historians to select the best premodern comparisons for ancient Rome. While it is very likely that more people died in Rome than were born there, this was a result not only of high mortality due to infectious diseases, but also of low fertility, partly due to an unbalanced sex ratio. If the environmental conditions that governed mortality in Rome differed from those in London, this is even more true of the social and demographic conditions that governed fertility and migration in both worlds.

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MIGRATION AND HEGEMONY:  
FIXITY AND MOBILITY IN SECOND-CENTURY ITALY

Will Broadhead

The political turmoil that plagued the Roman state in the age of the Gracchi has since antiquity been closely associated with demographic developments in Italy, and in particular with the relationship between Roman hegemony and Italian manpower. As modern accounts of the period have examined this association in increasingly sophisticated ways, they have tended to focus above all on what one might call the vertical aspects of demographic history. In other words, the primary lines of debate have been over the size of the free population of Italy, over the number of *assidui* within the Roman population, and over the size of the slave population of Italy. The questions that lie behind these debates are clearly central to any understanding of the history of the late Roman Republic, even if there remains uncertainty as to their answers. At the same time, however, the emphasis that most studies place on vertical demography, on the positive or negative growth of certain populations or subsets of populations, has tended to obscure the importance of what one might call the horizontal demography of Roman Italy. In an Italy that some of us believe was characterized by a high level of geographical mobility, it is just as important for us to focus on the dynamics of population distribution as it is to focus on population size. Put another way, it is just as important for us to ask *where* Italian manpower was as it is for us to ask *how much* of it there was.

One way to develop a horizontal reading of the impact of demographic trends on the history of the later second century BC is through the analytical framework provided by the concepts of fixity and mobility.<sup>1</sup> The way in which Rome organized its own citizens, its colonial populations, and its relations with the allied communities of Italy, and in particular the way in which soldiers were recruited to fight for Rome, implies an expectation of geographical fixity on the part of the hegemon. The manpower on which the Roman city-state relied for the

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<sup>1</sup> On mobility and fixity see Horden and Purcell (2000), esp. 342–400; Purcell (2004); and Horden (2005).

conquest of the Italian peninsula and of much of the Mediterranean arrived in the Roman army through a system of recruitment that was geographically determined. There is, furthermore, no indication that the system as we know it, at least as relates to the Latins and allies, was designed to respond to changes in the geographical distribution of population. The result was an inevitable tension between two conflicting phenomena: the fixity upon which Roman hegemony depended and the mobility that was a reality of life in Italy.

The surviving books of Livy's account of the second century BC record frequent examples of pressures exerted by the opposition between the expectation of fixity and the reality of mobility. The obvious problem for any attempt to chart the history of this opposition into the age of the Gracchi is the loss of Livy's account for the years after 167 BC. There is no reason to believe, however, that the issue of migration and its effect on the running of the Roman state somehow disappeared in the 160s BC. If the demographic trends that lie behind the tensions recorded by Livy for the early second century BC did indeed continue into the later second century BC, then the operation of Roman hegemony is likely to have continued to be affected by them, in ways that must constitute part of any explanation of the upheavals of the period from the Gracchi to the early first century BC.

### I. *Roman hegemony and the expectation of fixity*

It is often noted that the Romans were unusual among ancient states in not relying on the use of mercenaries as they established, defended, and began to project their hegemony in the later fourth and early third centuries BC.<sup>2</sup> The Romans relied instead on the provision of manpower by their own citizens and by the various colonial and allied communities of Italy. For citizens, the performance of *militia* was always the rule and was in emergency circumstances even expected of those who were ordinarily exempt, the *proletarii* and certain other individuals. For Latin colonies and the allied communities, the obligation to provide troops for the Roman army was at the core of their relationship with Rome. The recruitment of troops from all Italy is indeed frequently highlighted as a defining characteristic of Roman hegemony in the

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<sup>2</sup> E.g. Nicolet (1980, 89 ff.).

republican period, as well as being the key to Roman military success. What is less frequently highlighted is the degree to which the military demands that the Roman state made of her own citizens and of the colonial and allied communities of Italy imposed on the peninsula an expectation of geographical fixity. When and how any given individual performed his military service was determined by where he lived: citizens were recruited by geographical tribe, Latins and allies according to the demands of the geographically organized *formula togatorum*. High levels of mobility would therefore have made this key aspect of hegemony quite difficult to operate.

For the expectation of fixity to have played a significant role, it need not be the case that Rome intentionally and directly as a matter of policy prevented mobility among the citizen and subject peoples of Italy, though in the case of the citizen and Latin colonies that is precisely what happened. It is enough instead that the Romans simply considered geographical fixity the norm, and so expected the relative distribution of population to remain the same over time without the need for any intervention. Whatever was in the minds of the Romans who established the mechanisms of hegemony in the late fourth and early third century BC, the demand for manpower from the colonies and the allied towns forced at least those communities to worry about preventing net out-migration. As a result, a snapshot of Roman hegemony in the middle Republic is a picture of fixity imposed.

In the case of Rome's colonial foundations, both of the citizen and of the Latin variety, geographical fixity was directly imposed on the inhabitant population. Of the fixed nature of life in the small, citizen colonies of the middle Republic, Livy provides a general reflection in his account of the foundation of Minturnae and Sinuessa in 296 BC. Potential settlers were evidently reluctant to enroll in the two colonies because they were thought to be 'permanent garrisons' in hostile territory.<sup>3</sup> Beyond this generalization, we know more specifically from its suspension in 207 BC and again in 191 BC that inhabitants of the old-style citizen colonies had exemption from legionary service and that this *uacatio militiae* almost certainly corresponded to a requirement to remain in the colony.<sup>4</sup> Livy's account of the suspension of *uacatio*

<sup>3</sup> Livy 10.21.7–10: *in stationem se prope perpetuam infestae regionis non in agros mitti rebantur*.

<sup>4</sup> Livy 27.38.1 ff. (207 BC), where the colonists' *uacatio militiae* is described as *sacro-sancta*, and 36.3.4–6 (191 BC). See Brunt (1971, 40) for the likelihood that inhabitants

for the citizen colonies in 207 BC provides further details of how the restriction of colonists' freedom of mobility might have been expressed. In that year, because of the threatening presence in Italy of the armies of both Hannibal and Hasdrubal, and because of an already dwindling supply of manpower, the *uacatio militiae* of all but two of the citizen colonies—Antium and Ostia—was suspended. In Antium and Ostia, however, all men of military age were required to take an oath that they would not be absent from the colony for more than thirty days as long as the enemy armies were in Italy.<sup>5</sup> Though the swearing of a particular oath on this occasion does appear to have been an extraordinary wartime measure, it is nonetheless likely that a similar restriction was in place as a rule, with, one might speculate, perhaps a longer allowance of time away in less urgently defensive times. Whatever the details might have been, and however difficult it might have been to enforce,<sup>6</sup> fixity of personnel was an essential aspect of the nature of Rome's early citizen colonies.

Likewise in the case of the Latin colonies, Rome imposed on colonists an obligation to remain in the colony, or at least not to emigrate permanently. As I have argued elsewhere, there is no evidence to support the long-held view that Latin colonists had a special right to leave their colony and move to Rome—the so-called *ius migrandi*.<sup>7</sup> It is more likely that exactly the opposite was the case—that participants in Latin colonial settlements were obliged to keep their place in the colony and could only emigrate if they left behind a son. On this view, the frequently cited and much discussed passage of Livy at 41.8.9—*lex sociis nominis Latini, qui stirpem ex sese domi relinquerent, dabat ut cives Romani fierent* ('The law granted to the allies of the Latin name that those of them who should leave behind in their hometown an offspring of their own could become Roman citizens')—is not the expression of a privilege which might have been restricted in the second century BC, but is instead a paraphrasing of a regulation that formed part of the *leges datae* of

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of citizen colonies were not required to present themselves physically at Rome for the census, but were instead registered locally, precisely to avoid the depletion of manpower in what were for the most part coastal garrisons with specific defensive purposes.

<sup>5</sup> Livy 27.38.5: *earum coloniarum iuniores iure iurando adacti supra dies triginta non pernoctaturos se extra moenia coloniae suae donec hostis in Italia esset.*

<sup>6</sup> Demonstrated by the well-known episode of 186 BC, in which the consul Sp. Postumius reported to the Senate that he had found the colonies at Sipontum and Buxentum *desertas* (Livy 39.23.3–4).

<sup>7</sup> Broadhead (2001, 69–89).

Latin colonies. The aim of such a regulation would obviously have been to maintain the manpower of the individual colonies, both for local defense and as a regular supply of recruits for the Roman army. The latter connection emerges clearly from the well known episode of 177 BC, to which I return below, in which Latin embassies at Rome complained to the Senate that if emigration from the colonies were allowed to continue, ‘within a very few *lustra*, their deserted towns and deserted territories would not be able to produce a single soldier’.<sup>8</sup>

The view that colonial charters of the republican period included a provision imposing fixity is greatly strengthened by Chapter 14 of the *lex coloniae Genetivae*, which appears on the newly rediscovered fragment of the inscription recently published by Caballos Rufino:<sup>9</sup>

Quicumque in col(onia) G(enetiva) I(ulia) decurio erit, is decurio in ea colon(ia), intra qua aratro circumductum est, aedificium, quod non sit minus tegular(um) DC, qui colonus neque decurio erit, is aedificium, quod non sit minus tegularum CCC, habeto in biennio proximo, quo ea colon(ia) deducta erit.

Whoever will be a decurion in the colonia Genetiva Julia, that decurion in that colony, within the area around which the plow has been drawn, shall within two years of the deduction of the colony have a house of not less than 600 tiles; whoever will be a colonus and not a decurion, he shall within two years of the deduction of the colony have a house of not less than 300 tiles.

The requirement for decurions to own a house of a certain size is not new to the corpus of epigraphically attested charters: a similar provision appears in the *lex Tarentina* (26–31) while an indirect reference to such a requirement appears in a later chapter of the *lex coloniae Genetivae* itself (91).<sup>10</sup> The latter reference explains the purpose of the requirement, at least in the case of decurions, as being that local magistrates must have a property *unde pignus eius quot satis sit capi possit*. What is new in Chapter 14 of the *lex coloniae Genetivae* is the requirement not only for decurions but also for non-elite colonists to have houses in the colony and for them to do so within two years of the *deductio* of the colony. The primary concern here is not to ensure all potential magistrates own houses they can offer as security, but seems rather to be to ensure that all participants in the colonial foundation establish a tangible residence

<sup>8</sup> Livy 41.8.7 (see below).

<sup>9</sup> Caballos Rufino (2006), esp. 208–23.

<sup>10</sup> *Roman Statutes*, 15 and 25.

and so acknowledge and facilitate the fulfillment of their obligation to remain in the colony.<sup>11</sup>

Fixity was imposed on Rome's Italian allies no less than on her colonies, though fixity in the case of the allies was an *indirect* consequence of Roman hegemony. As is well known, the communities of Italy subject to Rome were required by treaty to supply manpower for the Roman army. Though there has been much debate over the exact details, it is generally accepted that some assessment of the manpower available in allied communities was compiled on a list known from the *lex agraria* as the *formula togatorum* and that this list was used by Rome to determine the relative burden of different allied communities.<sup>12</sup> There are two ways in which the demands represented by the *formula togatorum*, which were fundamental to the operation of Roman hegemony in Italy, imposed on the allied communities an expectation of fixity. First, if they had not before, the elite in the communities in question would from the moment of their alliance with Rome have felt the need to take regular stock of local manpower.<sup>13</sup> Indeed, it seems to be the case that a formal census was adopted in at least some of the non-Roman and non-Latin communities of Italy, and that the regulations adopted for these local censuses imitated the harsh punishments for *incensi* known from Rome.<sup>14</sup> Second, unless the *formula togatorum* was updated annually, which is hardly likely, those communities whose obligations to Rome were based on whatever assessment of local manpower appeared on the list will have had every interest in maintaining that level of manpower,

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<sup>11</sup> *Roman Statutes*, p. 310 already notes the possibility that the decurional housing requirement in the *lex Aventina* might reflect a desire to maintain the population of that city. Caballos Rufino (2006, 216) associates the provision with the contextually specific need to create civic spirit and a group identity among colonists who were ethnically and politically divided after Munda. I obviously put the emphasis instead on colonial manpower and suggest the requirement of Chapter 14 is not specific to Urso, but is precisely the kind of provision we should expect to have existed in colonial charters going back to the middle Republic.

<sup>12</sup> *Roman Statutes* 2, ll. 21 and 50. Cf. Livy 22.57.10 (*Item ad socios Latinumque nomen ad milites ex formula accipiendos mittunt*) and further references to a *formula* in the context of manpower from Latin colonies at 27.10.2–3 and 29.15.11–12. For a general account of the *formula togatorum* and various interpretations of the working thereof, Brunt (1971, 545–8), Ilari (1974), and Lo Cascio (1991–4) are essential.

<sup>13</sup> Lo Cascio (1991–4, 323–4).

<sup>14</sup> The borrowing into Oscan of the Latin terms *censere* and *ensor* suggests that the bureaucratic terminology of the census spread from Rome to much of the Oscan-speaking world before the Social War; see Untermann (2000), *s.v.* *O.censum* and *O.kenzsur*, 382–6; Gabba (1989, 228–9); Broadhead (2003, 133–6).

in order to be in a position, demographically, to satisfy the demand when it came.

The frequency with which the *formula togatorum* might or might not have been updated is one of many aspects of Roman administration that has eluded our literary sources. Common sense might suggest that the *formula* must have been revised with some regularity;<sup>15</sup> but there is no positive evidence of revision. There is in fact good reason to believe that Rome was reluctant to engage in regular revisions.<sup>16</sup> If the figures recorded on the *formula togatorum* did indeed remain static, then significant horizontal shifts of population would have created a lasting problem for the communities in question. Rome's regular demands for manpower from the allies will as a natural consequence have encouraged the allied elite to keep close tabs on their own populations.

The administrative relationship between the Roman state and its own citizens was also geographically specific, based as it was on membership of the geographically determined *tribus*.<sup>17</sup> However obscure the early history of the tribes might be, it is clear that at the latest by the middle of the third century BC each Roman citizen's participation in the political and military life of the state was determined by his membership in the tribe, and thus by his place of residence. It is possible that the tribe had always been the basis of the Roman census, though Lo Cascio has recently argued that the early census was based instead on the centuries alone, and that the census by tribe was an innovation of the years following the settlement with the Latins in 338 BC.<sup>18</sup> If that should be the case, it suggests that this moment, which is already so closely identified with the creation of the basic institutions of Roman hegemony, ought also to be associated with a significant shift at Rome from a citizenship whose primary enumeration was vertical, by centuries, to a citizenship whose primary enumeration was horizontal, by tribes.

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<sup>15</sup> Gabba (1989, 222): 'It seems unlikely that no provision was made for changes in the size of the citizen bodies in the allied states.'

<sup>16</sup> In addition to the episode, discussed further below, recorded by Livy for 177 BC, in which the Roman Senate seems openly to reject allied requests for a revision, Brunt (1971, 547) suggests that the general 'poverty of Rome's administrative machinery' militates against the possibility of frequent revision.

<sup>17</sup> Ignoring for the moment freedmen, who were assigned to the four urban tribes regardless of their place of residence, and those individuals who were removed from their tribe by the censors and relegated to the less prestigious urban tribes.

<sup>18</sup> Lo Cascio (2001), esp. 576 ff. See, among others, Taylor (1960, 3 ff.), and Cornell (1995, 173 ff.), for the view that the census was from the start based on the tribes.

By the middle of the third century BC, the tribes had also become the basis for the recruitment of Roman citizens via the levy.<sup>19</sup> Such a system would have functioned most smoothly if the tribes were of roughly equal size and remained of equal size as long as the levy by tribe remained the norm. In that sense at least, an expectation of fixity lies behind the theory of the levy by tribe.<sup>20</sup> In practice, however, we should probably expect the system of citizen recruitment based on the levy to be more readily flexible than the system of allied recruitment based on the *formula togatorum*. In both cases, common sense suggests the numbers of *iuniores* associated with each geographical unit (tribe, colony, allied community) should have been revised with some regularity to take account of changes in relative population size. As already noted, however, it is unlikely that the *formula togatorum* was regularly revised. It would, on the other hand, have been a much easier job for the Romans to adjust the levy by tribes to changing demographic realities. Provided the census was taken in its regular cycle, the Roman authorities responsible for the levy in any given year would have had at their disposal recently updated lists of *iuniores* for each citizen tribe, and could have adjusted the burden accordingly.<sup>21</sup> Nevertheless, the fact that fundamental activities of the Roman citizenship—recruitment to the army and participation in the tribal voting assemblies—were based on residence, on the geographical unit of the tribe, means that the balance and justice of the system depended on a certain degree of geographical fixity.

An expectation of fixity can thus be seen as central to the nature of Roman hegemony in Italy and especially of the system of recruitment that was so important to Rome's success in the middle Republic. As long as all the men in Italy who were liable for military service stayed where they were originally counted, the system of recruitment based on the tribal lists and the *formula togatorum* could have operated smoothly.

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<sup>19</sup> Gabba (1976, 53 ff.) = Gabba (1951, 144 ff.).

<sup>20</sup> There is no evidence of any positive restriction of the mobility of citizens from one tribe to another—so no fixity in that sense. For epigraphically attested examples of individuals who appear to have transferred from one tribe to another as a result of migration see Forni (1966, 139–155) = Forni (2006, 71–85).

<sup>21</sup> Here again, we must rely on probability, since Polybius makes no mention of such adjustments in his description of the levy by tribe at 6.19–21. On this and other problems of Polybius' description of the tribal levy as evidence for the procedure in place during the second century BC see Brunt (1971, 625 ff.).

## II. *Fixity and mobility in the early second century BC*

If we turn to Livy's account of the early second century BC, however, it quickly becomes clear that the individual inhabitants of Italy on whom Rome relied for manpower could *not* be counted on to remain in the place where they were originally counted for the purposes of the levy. In other words, the fixity upon which Rome's demands for troops depended was at odds with the mobility that, by the early decades of the second century BC at least, was carrying on regardless of Rome's schemes for recruitment.

One familiar passage of Livy should suffice to highlight the nature of the tension between fixity and mobility. In 177 BC, embassies from the Latins, who were losing citizens through emigration to Rome, along with embassies from the Samnites and Paelignians, who were losing citizens through emigration to Fregellae, appeared before the Senate at Rome and raised the issue of mobility:

Mouerunt senatum et legationes socium nominis Latini, quae et censores et priores consules fatigauerunt, tandem in senatum introductae. Summa querellarum erat ciues suos Romae census plerosque Romam commigrasse; quod si permittatur, perpaucis lustris futurum ut deserta oppida deserti agri nullum militem dare possent. Fregellas quoque milia quattuor familiarum transisse ab se Samnites Paelignique querebantur, neque eo minus + aut hos aut illos + in dilectu militum dare (41.8.6–8).<sup>22</sup>

The Senate was also moved by embassies from the allies of the Latin name, who had wearied both the censors and the previous consuls, and had finally been brought in to the Senate. The point of their complaints was that a great number of their citizens had migrated to Rome and had been registered at Rome; and that, if this trend were allowed to continue, within a very few *lustra*, their deserted towns and deserted territories would not be able to produce a single soldier. Samnites and Paelignians were also complaining that 4,000 families from their territory had gone over to Fregellae, and that neither of them as a result of this emigration furnished any fewer soldiers in the levy.

The details of the Latin and allied complaints are revealing: both explicitly draw attention to the negative impact of local emigration on their abilities to satisfy Roman demands for manpower in the

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<sup>22</sup> Though Briscoe (1986), *ad loc.*, believes the text of 41.8.8 to be corrupt, Laffi (1995, 50) has more recently followed earlier editors in accepting the text as is, reading *neque...dare* as a Livian gloss following the argument of the Samnites and Paelignians in *oratio obliqua*.

levy. In both cases, out-migration had led to a decline in the number of locally available *iuniores*; yet Rome's demands for manpower were clearly still based on figures derived from some previous assessment of Latin and allied populations. In other words, it is clear that there was at this stage no mechanism in place by which Rome regularly took account of changes in local population size in making demands of the Latins and allies: that aspect of Roman hegemony was still based on an expectation of fixity.

One obvious response to this manifestation of the conflict between fixity and mobility would have been for Rome to carry out a revision of the *formula togatorum* with the aim of adjusting relative demands on the Latins and allies to bring them into line with the new demographic reality. Such a course of action would have amounted to an acknowledgement that the logic of the levy needed to be more responsive to horizontal demographic change. What actually happened in response to the complaints of the embassies in 177 BC was that all Latins who had immigrated to Rome illegally—i.e. without leaving behind a son of military age—were forced to return to their home towns. Much has been made of the fact that, according to Livy, it was the Latin embassies themselves who asked that their emigrant citizens be repatriated and that the senate merely acquiesced.<sup>23</sup> For our purposes here, it does not much matter whether it was the Roman authorities, the Latin authorities, or both that initiated the repatriation of emigrants. What does matter is that given a choice between adjusting the demands of the levy and adjusting the distribution of population in Italy, Rome opted for the latter. Furthermore, that option, the policy of forced repatriation to restore the previous distribution of Latins in Italy, will not have been a straightforward operation. Latin emigrants at Rome had to be identified—which must have been done by comparing lists produced by the Latins with lists from recent Roman censuses—and physically located. They then had to be forced in some way to return to their Latin hometowns, where they would have to resume residence alongside those very compatriots of theirs who had demanded their involuntary return.<sup>24</sup> Such a process would have been socially disrupt-

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<sup>23</sup> See Frézouls 1981 for an alternative view, namely that Livy's account reflects senatorial revision of the episode to shift from the Roman senate to the Latin authorities all responsibility for the expulsion of Latins. For further discussion see Broadhead (2003, 141 ff.).

<sup>24</sup> For further discussion of the practical difficulties that must have accompanied forced repatriations see Frézouls (1981, 123) and Broadhead (2003, 144–8).

tive both at Rome and in the home communities. In short, the method chosen in 177 BC as a solution to the problem caused by migration reflects a strong adherence to the traditional fixity-based logic of the levy and a corresponding reluctance to rethink recruitment in such a way that it might take into account horizontal shifts in population resulting from mobility.

The episode recorded by Livy for 177 BC was not an isolated case. In fact, problems associated with mobility appear with some regularity throughout Livy's account of the late third and early second centuries BC. Early on in the period, at least four individual Latin colonies brought complaints of population problems on their own behalf. In 206 BC embassies from Placentia and Cremona complained to Rome that a large number of their colonists had abandoned the colonies out of fear of their hostile neighbors. On that occasion, a consular edict was issued ordering all such emigrants to return before a certain date. In 199 BC embassies from Narnia and Cosa complained of not having enough colonists. In 198 BC one of the two consuls spent the whole year rounding up further emigrants from Placentia and Cremona and forcing them to return to the colonies. In 190 BC Placentia and Cremona yet again sent embassies to Rome complaining of an *inopia colonorum* as a result of war, disease, and emigration; this time the senate recommended *supplementa* of 3,000 men each.<sup>25</sup>

After these individual cases, Livy records for 187 BC the first instance of a joint delegation from all the Latins—*qui toto undique ex Latia frequentes convenerant*—complaining at Rome that they had lost citizens through emigration. In response, the senate arranged for the expulsion from Rome of any Latins who had migrated since 204 BC. The result was the repatriation of 12,000 Latins, one of our few indications of the significant scale of the mobility in question. Emigration affected citizen colonies in this period, too, as became clear in 186 BC when Spurius Postumius reported to the senate that the colonies of Sipontum and Buxentum had been abandoned by their original settlers. In 177 BC as discussed above, a second joint delegation of Latins made the same complaint they had ten years before, this time joined by the Samnites and Paelignians, who complained of the loss of 4,000 families through emigration to Fregellae. Finally, at the census of 174/3 BC Livy records a consular edict declaring that the Latins who were required to return

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<sup>25</sup> Livy 28.11.8–11, 32.26.1–3, 37.46.9–11: emigration from Placentia and Cremona in 206, 198, 190 BC; Livy 32.2.6–7: Narnia and Cosa short of manpower in 199 BC.

to their colonies by the edict of 177 BC should not be registered at Rome, implying that a significant number of Latins who were supposed to have left Rome were in fact still there.<sup>26</sup>

For that part of the second century BC for which Livy's text survives, it is reasonable to say that tension caused by the opposition between an expectation of fixity and the reality of mobility was a regular occurrence and a frequent obstacle in the way of the smooth functioning of Roman hegemony in Italy. The Roman authorities frequently found themselves having to deal with a demographic problem in the horizontal plane: Latin and allied manpower was not always where it was supposed to be. With the loss of Livy's text after 167 BC we lose any similar references to mobility and the consequent creation of inequalities. There is no reason, however, to believe that there was suddenly a decrease in mobility in the middle of the second century BC or that continued mobility was any less problematic.

### III. *Fixity and mobility after 167 BC*

What would be the implications for our understanding of the period if the Latins and allies and indeed Roman citizens themselves were just as horizontally mobile in the middle and later second century BC as they appear from Livy to have been in the earlier second century BC? In attempting to answer this question, the study of mobility by necessity becomes a *mémoire perdue* type of exercise, both in the sense that the history of lost documents (the *formula togatorum* and the tribal lists produced by the census) plays a central role, and in the sense that one's view of the Roman response to mobility is likely to depend on one's adherence to the minimalist or the maximalist view of Roman administration.<sup>27</sup> Nevertheless, isolating the issues and thinking through some of the possibilities can help reveal whatever traces might exist of the conflict between fixity and mobility in the age of the Gracchi.

Roman hegemony in the middle Republic was all about manpower; and that manpower was supplied by citizens and subjects alike on the basis of geographical units of recruitment. The smooth operation

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<sup>26</sup> Livy 39.3.4–6: first joint embassy of Latins complaining of emigration to Rome in 187; 39.23.3–4: Sipontum and Buxentum in 186 BC; 41.8.6–12, 41.9.9–12: second joint Latin embassy plus Samnites and Paelignians in 177 BC; 42.10.3: Latins ordered home in 177 BC still resident at Rome in 173 BC.

<sup>27</sup> Demougin (1994); Moatti (1998).

of Rome's Italian hegemony therefore required careful management of those units. Increasing mobility across the boundaries of those geographical units over the second century BC, compounded by the likelihood that many migrants avoided registration at their destination,<sup>28</sup> made their management difficult. How did the Romans respond to this inevitable difficulty?

In the case of the recruitment of citizens by tribe, the Romans were of course in a much better position to respond to horizontal changes. The census was carried out with regularity throughout the second century BC and would have produced updated lists of *iuniores* available in each of the thirty-five tribes. Provided that the authorities responsible for the levy consulted the tribal lists, the number of men demanded from any given tribe could be adjusted accordingly. In that sense, the levy would have responded to changing realities and been more fair. Regular adjustment of demands for recruits would, however, have undermined the basic timocratic principle on which citizen participation in the state was based: individual tribes with increasing numbers of *iuniores* would have shouldered an increasing proportion of the military burden, but would have continued to count as a single vote in the tribal assemblies.

On this reckoning the crisis for Rome in the later second century BC was a horizontal one, requiring either the redistribution of manpower or the abandonment of the geographical logic—or fixity—of the traditional levy. How then might we develop a horizontal reading of the problems of the age of the Gracchi? What follows if, for example, instead of thinking of the Gracchan project as a redistribution of land with the aim of creating more *assidui*, we think of it as a redistribution of manpower with the aim of preserving the basic functionality of the geographically based levy by tribe and so the basic timocratic institutions of the Roman city-state?

One well-known aspect of the Gracchan agrarian program that seems open to such an interpretation is the inalienability of the land assigned by the commission.<sup>29</sup> Appian associates this inalienability with a desire on the part of Ti. Gracchus to protect recipients from wealthy landowners. It might also have served to eliminate the possibility of recipients selling up immediately and exploiting the program for some

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<sup>28</sup> Gabba (1989, 219–20).

<sup>29</sup> App. *BC* 1.10 and 27.

quick cash.<sup>30</sup> It is also possible, however, that the inalienability of allotments was designed to keep recipients in place—to preserve the distribution of population that would be established by the program. If the Gracchan program was intended to achieve a horizontal as well as a vertical redistribution, then it is to the Roman citizen tribes that we should look for an explanation. We do not possess any figures for the manpower of individual tribes; but it is reasonably certain that by the later second century BC the tribes were of unequal size.<sup>31</sup> For example, the Pollia tribe was the tribe to which was assigned much of Rome's expansion into the Po Valley, from Flaminius' distribution of the *ager Gallicus* in 232 BC down to the viritane distribution of the *ager Gallicus et Ligustinus* in 173 BC with the citizen colonies at Mutina and Parma in between. These extensions will already have swollen the ranks of the tribe. If, as some of us believe, there was also a high volume of immigration to the Po Valley in the second century BC, then it is easy to see how the Pollia might quickly have come to outstrip other tribes for manpower.<sup>32</sup> At the other end of the scale, the Romilia and Lemonia tribes appear not to have been extended at all before the Social War and are likely to have registered far fewer *iuniores*.

The Roman authorities would have had figures for relative tribal sizes to hand; and it is possible that imbalances of manpower between tribes played a part in assigning new citizens and new colonies to tribes. Taylor points to the example of Fundi, Formiae, and Arpinum, who, when they were granted full citizenship in 188 BC were placed in the Aemilia and Cornelia tribes rather than in the larger, but adjoining Oufentina and Teretina.<sup>33</sup> Other explanations for the tribal assignments on this occasion are of course possible; Crawford, for example, has argued that proximity to the major roads from Rome likely determined tribal assignments, with Arpinum going to the Cornelia because of proximity to the via Valeria and Fundi and Formiae going to the Aemilia because of proximity to the via Appia.<sup>34</sup> Whatever the case may be, we can say that after several occasions on which tribal numbers would have been adjusted by the addition of new members in the early second century BC, there was a gap of several decades. If there were indeed

<sup>30</sup> Stockton (1979, 56 f.).

<sup>31</sup> Taylor (1960, 79–100).

<sup>32</sup> Gabba (1989, 214); Gabba (1994, 108); Broadhead (2000).

<sup>33</sup> Livy 38.36.7–9; Taylor (1960, 93).

<sup>34</sup> Crawford (2002).

no new colonies or settlements in the middle decades of the second century BC, then a long time had passed since the last opportunity to address tribal imbalances when Ti. Gracchus began his tribunate in 133 BC. From those middle decades of the second century BC also comes the well known evidence for the unattractiveness of military service, particularly in Spain, and for resistance to the levy.<sup>35</sup> These added pressures would only have further highlighted any developing inequalities among the tribes.

If the Gracchan commission had the power *tribu movere*, then it could have accomplished an adjustment of tribal sizes in the process of assigning land, which on this view was made inalienable in order to preserve the new distribution of citizens across the tribes. The annalistic tradition preserves precedent, invented or not, for a similar redistribution of tribal memberships in the person of Appius Claudius Caecus, censor in 312 BC and ancestor of Ti. Gracchus' own father-in-law, as well as for hostile opposition.<sup>36</sup> Unfortunately, our knowledge of the history of the tribes across the second century BC is extremely thin, as is our knowledge of the relationship between Gracchan settlers and the tribes. It is tempting to suggest, for example, that Gracchan settlers in the Val di Diano were assigned to the Pomptina, which at the time was probably one of the least populous and which eventually became the tribe of Atina and Teganum; but the data currently at our disposal allow for little precision.

Turning to the non-citizen element of the levy and the *formula togatorum*, it is important to consider the Latins and allies separately. Despite the fact that they appear together in the context of the *formula*, it is clear the Romans took a more hands-on approach to Latin manpower than to allied. An early example is the punishment in 204 BC of the twelve colonies which in the heat of the Hannibalic War in 209 BC had claimed to be depleted of local manpower and so refused to send any more soldiers for the Roman army.<sup>37</sup> In 204 BC the twelve colonies were forced to provide twice the maximum number of soldiers they had ever provided. The twelve were also required in future to carry out their local censuses according to the formula determined at

<sup>35</sup> Brunt (1971, 391 ff.).

<sup>36</sup> Cornell (1995, 373 ff.) argues for the basic authenticity of the career of Appius Claudius Caecus as preserved in the sources; for skepticism and revision see the bibliography listed by Cornell at 375, n. 18.

<sup>37</sup> Livy 27.9.7–10.10; 29.15.1–10; 29.37.7.

Rome and to deliver their census returns to Rome to be kept in the public archives. If the delegates from the twelve colonies were telling the truth about the depletion of their *iuniores*, then the census returns would have revealed as much. The fact that Rome took no notice is consistent with the inflexibility that we have already highlighted in the early second century BC, when *supplementa* and disruptive expulsions were used to maintain colonial manpower and the fixity-based system of recruitment that relied on it. The clear precedent for direct intervention and redistribution of Latin manpower suggests we should not be surprised if, as part of the general concern for manpower in the age of the Gracchi, Rome again sought to redistribute Latins.

Given the prominence of the expulsions of Latins in 187 BC and 177 BC it is tempting to claim the expulsions of 126 BC and 122 BC as part of the same horizontal analysis. The expulsion of 122 BC however, is surely a case apart since Appian and Plutarch go out of their way to note that it was a temporary measure and associate it specifically with a desire to clear Rome of non-citizens in anticipation of the vote on C. Gracchus' citizenship bill of that year.<sup>38</sup> The expulsion of 126 BC on the other hand, is more open to interpretation as an attempt to repatriate manpower that Rome expected to be in place when called upon.<sup>39</sup> If the earlier precedents are anything to go on, then we should expect this expulsion at least to have included the Latins, if not to have specifically targeted them. The major difference now was that the Latin colonies no longer played the direct strategic role with which they would still have been associated at the time of the last expulsion, in 177 BC. The Romans had indeed abandoned the founding of Latin colonies altogether after 180 BC at the latest. A repatriation of Latins in 126 BC would thus have been nothing more than a strong statement of Rome's continuing commitment to the traditional system of recruitment that depended on an expectation of Latin fixity.

Pennus' expulsion law of 126 BC is often interpreted, much like the expulsion of 122 BC as an attempt to remove non-citizens from Rome so that they could not interfere with the voting on a major bill, in this case the citizenship proposal of M. Fulvius Flaccus in 125 BC.<sup>40</sup> On this view, three events are usually linked in linear progression: first, non-citizens were expelled from Rome in 126 BC in anticipation of

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<sup>38</sup> App. *BC* 1.23; Plu. *CG* 12; the case of 122 BC does not appear in Cicero's list of cruel expulsions at *de Off.* 3.47.

<sup>39</sup> Cic. *Off.* 3.47; Festus 362 L.

<sup>40</sup> E.g. Badian (1970–1, 388); Stockton (1979, 94–5).

M. Fulvius Flaccus' citizenship bill; second, the citizenship bill which was expected in 125 BC was withdrawn; third, in frustration at the withdrawal of the citizenship bill, the Latin colony of Fregellae immediately rebelled and was destroyed. Such an interpretation puts the cart before the horse and privileges the citizenship question over any other concern. If the expulsion of 126 BC was instead more like the expulsions of Latins in 187 BC and 177 BC though perhaps without the agreement of the Latin elites, then the expulsion itself ought to be considered sufficient to have provoked the kind of frustration manifested in the revolt of Fregellae, which is much more likely to have been part of wider Latin unrest than it is to have been an isolated episode related to Fregellae's unique history.<sup>41</sup> It is, moreover, worth noting that C. Gracchus appears to have expressed opposition to the expulsion law of Pennus in 126 BC and was after 125 BC accused of having been somehow involved in the conspiracy at Fregellae.<sup>42</sup> On this view, the citizenship proposals of Flaccus in 125 BC and of C. Gracchus in 122 BC represent an alternative sympathetic to the Latins, namely their permanent removal from the *formula togatorum* and the end of Rome's often heavy-handed imposition of fixity on their populations, in exchange for the softer fixity that came with registration in a Roman citizen tribe, for which the Latins would ultimately have to wait another generation.<sup>43</sup>

What of the Italian allies—the other side of the *formula togatorum*? We are even less well informed by our sources of the continuing story of fixity and mobility in their case. It is worth remembering that in the episode of 177 BC, as noted above, it was the delegates of the Samnites and Paelignians who were most explicit in complaining that they were expected to provide Rome with the same number of recruits despite the loss of significant numbers of *iuniores* through emigration. Despite the request implied by the allied delegates' complaint, there

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<sup>41</sup> On Fregellae and on general Latin unrest in this period see especially Mouritsen (1998, 118 ff.).

<sup>42</sup> Festus 362 L; Plu. *CG* 3.

<sup>43</sup> See Henrik Mouritsen's contribution to the present volume for the argument that the citizenship proposals of M. Fulvius Flaccus and of C. Gracchus had as their aim the enfranchisement of the Latins as a means of instantly increasing the proportion of Roman citizen manpower in the joint Roman-Allied army. Though our views might differ on certain details, they are not incompatible: if one of the aims of the Gracchan program was indeed the redistribution of the citizen population and the addressing of tribal imbalances, then the enfranchisement of Latins would have provided a large pool of new citizen communities whose assignment to tribes would have contributed to the achievement of that aim.

was no revision of the *formula togatorum* on that occasion, nor is there any reason to believe there were revisions later in the century either. Furthermore, Rome seems not to have taken any steps to stop or reverse the out-flow from Samnite and Paelignian territory: there was no forced repatriation of allies. Unlike Rome's hands-on approach to Latin manpower, the allies were left to their own devices to maintain a sufficient degree of fixity among their populations. Many allied communities will have failed to do so and will have felt the sting of Rome's demands more and more acutely, especially as reliance on allied manpower in general increased over the course of the second century BC. Insofar as the burden of military service contributed to the grievances of the allies in the Social War, as Velleius Paterculus makes clear that it did, we should not be surprised to see the areas of greatest out-migration among the instigators of the conflict in 91 BC.<sup>44</sup>

Telling the story of mobility across the second century BC is not easy: speculation abounds by necessity. Livy's many references to the impact of population movement in the early second century BC and to Rome's responses to such movement are not matched by the disparate and highly problematic sources for the age of the Gracchi. Yet it is not at all likely that the break in our sources reflects a break in the history of mobility; and it is therefore important to consider the possibility that the manpower crisis so closely associated with the period was a consequence of population distribution rather than, or at least as much as, population size, that it was, in other words, a horizontal problem rather than a vertical one.

An expectation of fixity in the distribution of the population of Italy—citizen, Latin, and allied—was built into the institutional structure of the Roman state. The timocratic justice of the levy by tribe and the vote by tribe depended on the citizen tribes remaining of roughly equal size. Likewise, the recruitment of Latin and allied manpower according to the *formula togatorum*, a document which described the distribution of *iuniores* in Italy at a certain fixed point in time, depended on the maintenance of that distribution for its smooth operation. The expectation of fixity on which the Roman hegemony depended was probably always in opposition to natural tendencies toward mobility, even if it did serve the Romans well from the later fourth century BC to the Hannibalic

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<sup>44</sup> Vell. Pat. 2.15.2; Salmon (1958, 159 ff.); Gabba (1994, 115 ff.).

War. That opposition was brought into ever sharper contrast by the developments of the second century BC and clearly challenged the Romans to rethink the nature of their hegemony, a process that began in the age of the Gracchi and ended with the abolition of the *formula togatorum* in 89 BC and the gradual disappearance of the geographical tribe as a meaningful administrative unit by the age of Augustus.

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## THE GRACCHI, THE LATINS, AND THE ITALIAN ALLIES

Henrik Mouritsen

My paper revisits the long-standing issue of the place of the Latins and Italians in the Gracchan reforms. I have already explored the broader issue of Rome and Italy in this period elsewhere, but the present paper takes a closer look at this aspect from a slightly different angle.<sup>1</sup> Inspired by the theme of the conference and its emphasis on manpower resources, I shall argue that the Gracchan initiatives dealing with the Latins and allies may indeed be better understood in that particular context, rather than as part of a distinct ‘Italian policy’.

Much has been written about the role of the Italians in Ti. Gracchus’ land reform, mostly focusing on the question of whether they were included among the beneficiaries or not.<sup>2</sup> The root of the dispute is the discrepancy between the statements in our two main sources: while Appian emphasised the ‘pan-Italian’ motive underlying the entire reform and the central role of the Italians in the planned revival of the peasant-soldier class, Plutarch made no reference to the Italians whatsoever and mentioned only Roman citizens as prospective beneficiaries. None of the other extant sources, Cicero, the Bobbian Scholiast, or the *Lex Agraria*, suggests that anyone but Roman citizens were to benefit from the scheme.

Despite the lack of corroborative evidence many scholars have nevertheless tended to favour Appian’s version of the reform. They have done so for two reasons. First, it has seemed easier to explain Plutarch’s version as a simplification which ignored the Italians for reasons of textual economy, rather than accepting that Appian invented an Italian element that was not originally there. Second, since the Italians contributed a vital part of the manpower resources on which Rome’s

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<sup>1</sup> Mouritsen (1998 and 2006a).

<sup>2</sup> In favour of allied participation: Göhler (1939, 70–131), Shochat (1970 and 1980), Brunt (1971, 76 n. 1). Contra: Nagle (1970), Badian (1972, 681), Molthagen (1973), Bleicken (1990), Kukofka (1990). Various compromises include Bernstein (1978, 137–59) and Richardson (1980). Cautious: Stockton (1979, 42–6). Much of the debate concerns the possibility that Appian confused the terminology and described Romans as Italians. This theory is hardly convincing, cf. Mouritsen (1998, 16 f.).

position relied, keeping the allies strong has seemed to many modern historians a logical Roman concern, just as valid as their concerns about Roman manpower.

However, on closer inspection it is Appian's text which turns out to be the weaker one. As I have argued in greater detail elsewhere, his conception of the period between the Gracchi and Sulla is highly original and must be seen in the context of the unique structuring principles that were applied to his Roman history in general. In the first book of the *Emphyilia* the main subject was the civil war between Marius and Sulla, and the preceding *c.* 50 years are construed as a gradually ascending curve of ever increasing violence. However, the Social War, being a conflict between Romans and non-Romans, posed a challenge to this concept and had to be integrated more fully into the domestic narrative of political unrest. It was fitted into the scheme as a stepping-stone between the political violence in Rome, out of which it supposedly grew, and the first civil war between Roman citizens, which in turn was presented as a direct continuation of the Italian conflict. This particular concept explains why the Italians were introduced from the very beginning as the central issue in Roman politics and were presented as the root cause of the first upheaval and outbreak of violence. Not only does this literary construction cast doubt on the historical value of Appian's account, a closer study of the text itself also reveals a number of inconsistencies which give away his authorial intentions.

In Appian's opening section the Italians were not just included in Gracchus' scheme; they were *the* beneficiaries. But very soon afterwards Appian changes tack and gradually introduces poor Romans into his account, where after a few pages they appear as the only recipients. This peculiarity may be explained by the need to reconcile the opening, all-Italian version of the agrarian scheme with the situation in 129 BC, when disgruntled Italians approached Scipio Aemilianus and asked for his help against the *triumviri*. In other words, Appian moves from a scenario of poor Italians opposed by rich Roman landholders to one of poor Romans opposed by rich Italian landholders. The heavy-handed way in which this shift is achieved, in my view, underlines Appian's schematic approach to the historical issues involved.

It may suggest that we are dealing with a purely literary construction and, when we start questioning Appian's version, it soon becomes clear that there is no reliable evidence to support the idea that Ti. Gracchus ever included Italians in his scheme. Further doubt is cast on this theory by the Lex Agraria, lines 2–3, which mentions only Roman citizens as recipients of the allotments by *sortitio*. The Italians feature only in lines

20–3, which deal with the privatisation of land given in exchange to Romans, Latins, or Italians as part of a colonial settlement. Moreover, Cicero only ever refers to Roman recipients.<sup>3</sup> Likewise, the Bobbian Scholiast described it as a distribution of public land among the Roman plebs: *ille, ut ager publicus Romanae plebi divideretur*.<sup>4</sup>

This takes us to the final argument for their inclusion, which is that the strength of Italian manpower would have been a natural Roman concern. This was indeed the argument on which Appian's version was based, but here we have to bear in mind that his account relied on the assumption that there was little to distinguish Italians from Romans at this time apart from increasingly spurious differences in status, which would soon disappear anyway: to Appian they effectively formed a single nation divided by obsolete, purely formal barriers. In later modern scholarship this particular perspective became embedded in what might be described as the 'convergence' model of Rome and Italy that saw the process by which Italy was unified under Roman leadership as a logical, indeed irreversible historical development. If we reject this nineteenth-century model, it becomes apparent that there is no reason to assume the Italians were ever part of the Gracchan scheme, which appears to have been concerned exclusively with the strengthening of Roman manpower resources.

This conclusion has wide implications for the overall place of the Italians in the Gracchan reforms. It suggests that they were not simply seen as one wing of a joint Romano-Italian army, let alone as Roman citizens-in-waiting. They were, I should argue, still seen as foreigners, closely allied to Rome but politically autonomous and culturally distinct.

<sup>3</sup> *Agr.* 2.10; 2.81; *Sest.* 103.

<sup>4</sup> *Bob.* p. 135 (on *Sest.* 103). Cicero famously also claimed that the Gracchi had violated the rights and treaties of the Italians, *Rep.* 3.41: *Ti. Gracchus perseveravit in civibus, sociorum nominisque Latini iura neglexit ac foedera*, cf. 1.31: *concitatis sociis et nomine Latino, foederibus uiolatis*. . . Richardson (1980) argues that Gracchus planned to enfranchise (some) Italians in order to qualify them for his land scheme. This supposedly triggered complaints about *iura et foedera*, since their treaties forbade grants of citizenship which would have undermined their manpower strength. It is unlikely that any treaty would explicitly ban this from happening, and the paradox remains that, rather than by employing illegal enfranchisements, Gracchus could simply have opened up his scheme to allies if he had wanted them to benefit. But supposedly they could not receive Roman land, so they must first have been enfranchised. However, the Italians do appear as recipients of land in exchange, which became their property by the law of 111 BC, seemingly unencumbered with any legal restrictions. More likely the violation of *iura et foedera* relates to their loss of territory, since Appian also describes the difficulties involved in distinguishing between Roman and allied land.

This approach would help to solve the otherwise puzzling question of why Roman manpower was such a concern at Rome in this period. Given that the legions made up only a minority of the whole army, and Rome had extensive allied manpower reserves at her disposal, one might wonder why the apparent decline in Roman manpower caused such anxiety at Rome.<sup>5</sup> But if we accept the points I have just made, the answer is, of course, that it was precisely the fact that the legions were in a minority that was the cause for their concern.

A decline of Roman manpower would have had two major implications. First, it threatened to undermine Rome's hegemonic status in Italy, which rested firmly on her military superiority and ability to enforce her will against recalcitrant allies. Second, Rome could not impose heavier burdens on the allies without jeopardising internal stability in Italy.<sup>6</sup> Rome's 'global' position had come to rely on regular supplies of Italian manpower in order to maintain and expand her empire, which in a sense reversed the original power relationship between Rome and her allies at the time the treaties had first been drawn up.<sup>7</sup> The latter now carried a major responsibility for the empire, but without any corresponding share in its governance or formal exploitation. This disparity would have created a natural tension between Romans and allies which any increase in the allied military burden could only have exacerbated.

As soon as we accept that the allies formed a separate category with interests distinct from Rome's, the internal balance of manpower between Romans and non-Romans emerges as a real political issue and a likely cause for concern. Presumably the Roman authorities were at all times acutely aware of the relative proportion of Roman to allied manpower, which would have made them even more sensitive to any signs of Roman decline.

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<sup>5</sup> Polybius (6.21.5) noted that the allies followed the same procedures of conscription as the Romans, but that does not allow us to conclude that they applied the same property qualifications as in Rome. Such formal constraints would often have been impractical; presumably the allies simply had to reach the numbers required of them by whatever means necessary. *Contra* Erdkamp (2007, 55), who may place too much weight on a passing comment by an author who showed little interest in the Italian allies.

<sup>6</sup> The sensitive nature of the relationship between Rome and her allies and the recognised need to keep the latter content and compliant are underlined by the Roman reactions to their complaints over the Gracchan land distributions and Caius' careful attempt to avoid a repetition; cf. Mouritsen (1998).

<sup>7</sup> Cf. Mouritsen (2006a).

I suggest that it is in the light of this issue that we ought to approach the first certain appearance of the Italian allies in the Gracchan reforms. This was Flaccus' proposal in 125 BC to extend Roman citizenship to all those who wanted it. The proposal came to nought, being withdrawn in the face of senatorial opposition before being formally promulgated. However, it still raises a number of questions of timing and political aims: in particular, why did Flaccus suddenly introduce what seems to be an entirely new policy element at this particular moment, moving the reforms in a radically different direction?

Again Appian is the only source to offer any context and explanation. He links the initiative to the effective stalling of the land distribution process that followed Scipio's intervention of 129 BC, when the Italian *possessores* had first raised their complaints (1.78 f.). To break the impasse Flaccus proposed that as compensation for the loss of Roman *ager publicus* the Italians should be offered Roman citizenship, which they enthusiastically accepted (1.87).

The account is evidently informed by Appian's unflinching perception of the Roman citizenship as an indisputable and universally coveted privilege. Moreover, his decision to highlight an abortive proposal which never even got to the vote must be understood as part of his overall narrative scheme, which sought to interweave the Social War and its alleged causes into the fabric of the political history of the previous generation.

Appian's version of Flaccus' bill raises a number of questions. The main problem is the idea that a general enfranchisement would compensate individual Italians affected by the redistribution of land. The connection between land and citizenship is central to Appian's version of the 'Italian issue' as a whole, and he used it again in his account of the events of 91 BC, where a similar trade-off is envisaged as part of Drusus' reform. There are fundamental flaws in this connection, however. First, there was no direct correspondence between those receiving compensation and those making the sacrifice, since the grant was a general one while the losses were highly specific.<sup>8</sup> It is also difficult to see how enfranchisement could have provided any effective compensation for those who surrendered public land, given that the economic

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<sup>8</sup> Alternatively—and rather less likely—the grants may have been made to individual beneficiaries, but since double citizenship was not accepted at this stage, those enfranchised would either have had to change domicile and move to Roman territory, or have seen themselves formally cut off from their local communities.

benefits from Roman citizenship were limited at this time and in many cases quite hypothetical. Thus Appian's account rests on the premise that Roman citizenship already in the second century BC represented a universally recognised privilege which could easily be translated into tangible economic gain. Or, in other words, that it held precisely the same status as it did in Appian's own lifetime, *c.* 250 years later.

The whole story is most likely Appian's own confection, and this suspicion is strengthened by the recurrence of the 'compensation' theme elsewhere in his work. Indeed, this type of political *quid pro quo* can be seen as a motif in Appian's first book. In 91 BC it is used as the key to explain Livius Drusus' legislative programme, which is presented as a grand compromise where all parties involved, the Roman people, the senate, the *equites*, and the Italian allies at the same gained and lost as a result of his reform (1.155–64). In each case their concessions were matched by an element of compensation, but this version of the reform is, in my view, best understood as part of Appian's general portrayal of Drusus as an idealistic reformer working for the common good. Moreover, on closer inspection most of the individual elements of the 'compromise' model are revealed as more or less spurious: the *equites* were not really compensated by an expansion of the senate, while the plebs received compensation without incurring any actual losses. Likewise the senate did not suffer any actual loss either, since the increase in its membership presumably was required by the judicial reform carried in its favour. And, as noted above, the Italian enfranchisement was supposed to 'compensate' them for loss of land, which affected only a minority of them. The motif was probably chosen partly for reasons of dramatic effect—the earnest champion of all good causes falling victim to the narrow-minded self-interest of those he sought to help—and partly because it allowed Appian to present the citizenship issue as the central element to which all the other measures were instrumental. In reality there can be little doubt that the programme was aimed at strengthening the position of the senate in general and particularly in relation to sections of the equestrian order.

Another version of the 'compensation' theme appears in Appian's account of the post-Gracchan land laws, where the plebs receive compensation for the discontinuation of the land distribution process through the imposition of a rent on the privatised land, the proceeds from which were to be distributed among the poor who lost out (1.122). The logic of this measure is not clear. The land distribution appears to have stopped well before this time, and it is far from obvious who was

to receive the money and how—through a general handout of grain to the Roman plebs?—in which case the notion of direct compensation becomes meaningless.<sup>9</sup>

These examples cast further doubt on Appian's 'compensation' motif, and returning to Flaccus' proposal we may conclude that as a means of kick-starting the land distribution process it lacks plausibility; there was no discernible relationship between the supposed aim of the proposal and the scope and implication of the measure. So why did Flaccus propose citizenship for all those who wanted it?

Here we first have to consider the implications. A complete incorporation of the whole of Italy can hardly have been anticipated for a number of reasons. This would have been a momentous step with enormous political, military, cultural, and economic implications, which certainly would have borne no sensible relation to the problem it was supposed to solve. Indeed, a full enfranchisement would itself have eliminated the recruitment issue that had inspired the Gracchan land reform in the first place, since Roman manpower resources would have been more than doubled. The only other evidence we have for Flaccus' proposal, that of Valerius Maximus, also implies that no general enfranchisement was envisaged, which is hardly surprising given the status of the Roman citizenship at this time.<sup>10</sup> It suggests it was an offer aimed at the minority of allies who wanted it, and only in hindsight did the proposal come to appear as a direct predecessor of the *lex Iulia*, which of course was Appian's intention and the reason why he gave it such prominence.

This leaves us with the question of whom it was aimed at. Here there are some indications that the most likely recipients would have been the Latins, Italian immigrants to Rome, and, as I have argued recently, the *cives sine suffragio*. This interpretation is strongly suggested by the reaction of the Latin colony of Fregellae and by C. Gracchus'

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<sup>9</sup> For the suggestion that the term *dianomai* refers to distributions of grain in this passage see De Ligt (2001, 132).

<sup>10</sup> Flaccus' alternative offer of *ius prouocationis*, reported by V. Max. 9.5.1, was, I believe, essentially separate from the offer of citizenship and aimed at different groups—simply because the implications were so fundamentally different. It is best seen as an attempt to reassure the Italians that their interests would be safe if the land commission, against whose decisions there had previously been no appeal, was to restart its work.

attempt to pass a slightly revised version which included only the Latins and presumably the *ciues sine suffragio*.<sup>11</sup>

The scope of the proposal was more limited but still hugely ambitious, and we have to ask what the motives may have been. There were undoubtedly political benefits to be gained by sponsoring a major enfranchisement bill, but they were neither instant nor perhaps entirely certain (since they partly depended on tribal inscription which was in the hands of the censors). Given the scale and structure of political participation in Rome, it would also seem somewhat excessive to react to a passing political difficulty by proposing a large expansion of the entire citizen body.

Faced with these complications, I suggest we return to what most scholars now agree was the original aim of the Gracchan reforms—namely a strengthening of the Roman military forces. Here we have to bear in mind that there were a number of ways this could be achieved. It could be done by lowering the property threshold for the fifth class. This may already have been tried, perhaps after the failed initiative of Laelius in 140 BC.<sup>12</sup> Another means of expanding the military base was to distribute land among the landless, thereby qualifying them for conscription. This was the original Gracchan plan, which had now been kicked into the long grass. This left one last option open, which was to enlarge the citizen body externally. And there happened to be one category of people in Italy who stood out as the obvious targets of such a policy, the Latins, who historically and culturally were closely tied to Rome.

The origins of this category went back to the foundation of Caesarea in 334 BC after the end of the Latin War. The new Roman colony was given a status as Latin, which referred back to the defunct Latin ‘League’ and reflected the colonists’ continued links to the mother city. It gave them certain rights in relation to Rome, which were partly practical and partly symbolic. The new status and its formal autonomy from Rome presumably reflected the unease felt in Rome about the creation of large communities which were geographically separated from the Roman heartland. But whatever the precise reasoning behind the new fictional Latinity, it became the preferred status for large Roman

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<sup>11</sup> Cf. Mouritsen (2006b). The close connection between Flaccus’ failed proposal, the revolt of Fregellae, and C. Gracchus’ proposal is also suggested by the apparent attempts to hold C. Gracchus responsible for the uprising (Plu. *CG* 3.1; *Vix. ill.* 65.2).

<sup>12</sup> Rathbone (1993).

colonies and in the next 150 years almost thirty settlements of this type were created in Italy. However, in the 180s BC this policy was reversed and for the first time large colonial settlements were granted full citizen status.<sup>13</sup> Salmon explained this shift as a result of a change in the status of the Roman citizenship after her victory in the Hannibalic War.<sup>14</sup> Supposedly it had then become far more valuable, making Roman settlers less willing to give it up in return for land. As Salmon stated: “By now Roman citizenship had become valuable as a result of the overwhelming predominance with which Rome had emerged from the long struggle with Hannibal, and Romans, even land-hungry Romans, were not disposed to relinquish it in exchange for the citizenship of a Latin colony.”

In fact there is little evidence to suggest that Roman citizenship in the early second century BC had become a privileged status, involving specific, practical benefits for the holders.<sup>15</sup> I should argue that the idea of a heightened value to the Roman citizenship after the Hannibalic War is essentially teleological, projecting an imperial concept of citizenship back to much earlier periods. For all we know, Roman citizenship was still simply the status of free Romans who lived in Roman territory, and for that reason the colonists who were settled far away from the heartland logically had to surrender their Roman status. There may always have been some unhappiness involved in giving up this status, which was an essential part of personal and civic identity, in favour of new allegiance to a ‘Latin’ settlement.

The end to Latin colonisation had important consequences. The status in a sense became politically defunct, since the rationale which had originally formed the basis for the creation of these communities was no longer considered compelling. When new large-scale colonies of Roman citizens could be founded far away from Rome, it made no

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<sup>13</sup> It is not entirely certain which colonies were the first to maintain their citizenship. Most scholars assume they were Parma, Mutina, and Saturnia, founded in 183 BC. However, Salmon (1969, 104 f.) argued that this honour belonged to Potentia and Pisaurum, both founded in 184 BC. The change in allotment size might support the latter view. Later only Aquileia was founded as a Latin colony in 181 BC, probably because of its exceptional distance from Rome: Livy 39.44.10, 55.6–9; 40.34.2.

<sup>14</sup> Salmon (1969, 100).

<sup>15</sup> Supposedly the increase in allotment size also served as a ‘sweetener’ to compensate for loss of citizenship, but that may easily be explained as the result of reluctance to move so far away, even beyond Italy, to distant and dangerous Gallic territory.

sense to maintain the political separation of the Latin colonies, which had lost their *raison d'être* as a distinct category.

The shift in colonial policy might be explained by Roman reluctance to reduce the citizen body further, presumably for recruitment purposes. As Salmon also noted in passing, Rome “may not have been willing to lose it [i.e. manpower] to Latin colonies”.<sup>16</sup> While the foundation of the colonies as independent communities originally may have served both practical and ideological purposes, it also meant relinquishing direct control over valuable manpower which, as shown by the events during the Hannibalic War, might complicate the supply of troops during a crisis.<sup>17</sup>

From the very moment Rome first accepted large colonies of Roman citizens, the status of the remaining Latin colonies became an issue that would have to be addressed at some point. The dearth of evidence for the years between 167 BC and 133 BC means that we cannot trace its progress in the sources, but most likely it had already emerged during this period. There were many good reasons for bringing an end to the anomalous position occupied by the Latin colonies, both from a Roman and a Latin viewpoint. The colonial elites would gain access to careers in Rome as well as public contracts; some colonists might have wished to return to their ancestral homeland; and economically they would all benefit from the suspension of *tributum* in 167 BC.<sup>18</sup> The Roman authorities in turn would gain direct control over Latin manpower, which would boost the Roman share of the joint Roman/Italian army: according to some calculations the Latins made up more than a quarter of the allied contingents after 178 BC.<sup>19</sup> Depending on how the enfranchisement was organised there might also be political and social benefits to be gained for members of the Roman elite.

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<sup>16</sup> Salmon (1969, 100).

<sup>17</sup> William Broadhead's paper in this volume further illustrates the complications involved in the separate statehood of the Latin colonies, including those caused by population movements.

<sup>18</sup> Enfranchisement would also have improved the Latins' precarious position *vis-à-vis* Roman magistrates. Stories of heavy-handed Roman officials are not uncommon, and the Elder Drusus suggested extending *provocatio militaris* to the Latins. The story told by Diodorus (37.12) about the Latin actor at Asculum in 91 BC also illustrates the anomalous position of the Latins. Faced with the threat from local Asculans, who took him for a Roman, the Latin defended himself with reference to the fact that he too was subject to the *fasces*.

<sup>19</sup> Ilari (1974, 174).

However, despite the valid arguments in favour, the senate remained reluctant, probably because of concerns about upsetting the status quo through large-scale expansions in the citizen body, which might have had both fiscal and political repercussions. Any decision to do so would also have had to be taken collectively in order to prevent individual politicians from gaining undue benefits in terms of patronage, status etc. For that reason the senate found itself paralysed over this issue, which lasted until 91 BC, when the cause finally found a suitable champion.

Flaccus took up the issue in 125 BC, and the motivation of the Gracchan political grouping may have been complex, since there evidently were benefits, both short- and long-term, to be gained from sponsoring Latin enfranchisement. The dramatic reaction of Fregellae underlined the urgency of the matter, and C. Gracchus therefore quickly tried again, slightly modifying Flaccus' proposal. But the idea probably grew out of the original concern about Roman manpower, which may have been focused more closely on the internal Roman/Italian balance than modern scholars usually have assumed.<sup>20</sup>

If, as has been suggested, the legions made up only between a half and a third of the army in the later second century BC, any signs of further decline in Rome's manpower resources could hardly have failed to set the alarm bells ringing.<sup>21</sup> If Rome was struggling even to keep up what was already a minority share, a solution to the problems (whether perceived or actual is irrelevant here) involved in raising sufficient legionaries would have become even more urgent.<sup>22</sup>

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<sup>20</sup> There is no reason to discount the idea that a wide range of options including enfranchisement may have been considered before the land distribution scheme was chosen. Extension of the citizen body may have been rejected in the first instance for political reasons, since enfranchisement naturally was a highly sensitive issue, while land distribution represented a more conventional feature of Roman politics. Already in 188 BC the extension of *suffragium* to three Volscian communities had been controversial, and the quick rebuttal of the idea in 125 BC may have confirmed the initial hesitation.

<sup>21</sup> Brunt (1971, 684–6) suggested the Italians might have contributed two-thirds in the later parts of the second century BC, although Velleius' comment at 2.15.2 can hardly be taken at face value; cf. e.g. Erdkamp (2007, 69).

<sup>22</sup> A concern about the internal balance of manpower does not, of course, imply that the Romans lived in permanent fear of an allied rebellion, although the extreme reaction to the Fregellan revolt might suggest an underlying anxiety about their position. Roman concerns reflected the fundamental disparity in status that existed between Rome and her allies, the former giving the orders and the latter obeying them. This unequal relationship was ultimately rooted in the potential use of coercion, a threat which had to be credible at all times.

In conclusion, I suggest that the Gracchan land reform and the underlying concerns with manpower make best sense if we accept that the Italians were perceived not as ‘us’ but as ‘them’. The army commanded by Rome was not seen as a single unit which happened to be made up of Roman citizens, Latins, and allies. It was the army of the Romans assisted by their colonists and their foreign allies. The internal balance within this body was important, which explains the attempts at increasing the share of Roman citizens.

The traditional narrative assumed that the distinction had become almost obsolete, and that the whole of Italy was rapidly on course towards full integration at all levels. Viewed from that perspective, already found in Appian, Roman concerns about upholding the strength of allied manpower seemed entirely logical and as important as the manpower of Rome herself. But if we accept that the Italians were not ‘Romans in all but name’, the weaknesses of this approach become apparent. The fact that they were subject to Roman hegemony, unilateral obligations, and certain forms of exploitation automatically defined the Italians as ‘them’ rather than ‘us’.<sup>23</sup> There was in this relationship a built-in tension over power, resources, and prestige, and any shift in the military balance, real or perceived, would have further highlighted an already sensitive issue. That in turn puts the spotlight on the one group that did fit this description, the Latins. Bringing them back into the citizen body would have dramatically altered the internal military balance in Rome’s favour.

Admittedly, the proposed enfranchisement could be seen as little more than a cosmetic change, reclassifying the Latins as Romans rather than allies, but psychologically the effect of this would have been significant, formally shifting the balance of manpower and entrenching Rome’s hegemonic position in Italy. If, as some have argued, the manpower crisis to which the Gracchan circle reacted was more imagined than real, such a move might have been precisely what the situation required.<sup>24</sup>

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<sup>23</sup> Erdkamp’s claim (2007, 73) that Polybius’ cursory treatment of the allies reflected a view of “the role of the allies as inherent in Rome’s harmonious unification of Italy” in my view goes too far. Polybius’ lack of interest in the allies is indeed striking, but it may be better explained as a consequence of his particular aims and methods rather than any distinct vision of Roman/Italian relations.

<sup>24</sup> Rich (1983), Rosenstein (2004), De Ligt (2004). Rome’s minority role in the army was nothing new at this time, but it may nevertheless have been seen as increasingly problematic. We have to remember that despite the fixed nature of the treaty system, Rome’s relationship with the allies was not static but evolved continuously under the

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influence of changing geopolitical circumstances. Despite the Roman attempt to fragment Italian opposition, the long-lasting peace and shared military obligations may have led to greater consensus among the allies, who were all subject to the same hegemonic power. Increased allied unity would have posed an obvious threat to Roman interests and possibly triggered concerns about the Roman share in the army, explaining why we suddenly find the initiatives to strengthen Roman manpower appearing at this particular moment.



V

AGER PUBLICUS



## THE GRACCHAN REFORM AND APPIAN'S REPRESENTATION OF AN AGRARIAN CRISIS

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This paper does not aim to clarify the social and economic history of second-century BC Italy. Such a project certainly cannot be carried out merely through the examination of a text. Instead, I intend to examine Appian's account of the background to the reform of Tiberius Gracchus, the history of public lands that is contained within it, and its relationship to the historian's depiction of the *lex Sempronia agraria* of 133 BC. This brief narrative displays features that are rare in classical historiography. The author set out in apparently objective terms a well-defined course of social and economic developments, he identified processes and motives that appear realistic, and he gave to developments consequences that follow naturally from the history he had just laid out. Perhaps for these reasons, his account has long attracted the attention of scholars interested in the social and economic history of republican Italy.

My study, then, is primarily historiographical.<sup>1</sup> It does, however, have implications beyond the interpretation of a text. Appian's account occupies a central place in many modern reconstructions of Roman economic and social arrangements, especially with regard to public lands. Students of the Gracchan reform have examined intensively the sources either to ascertain their reliability as evidence or to identify the texts that lie behind them. Appian's history and Plutarch's biography of Ti. Gracchus, the chief sources for the reform, have often been the object of these investigations.<sup>2</sup> However, studies often focus primarily on depictions of the political maneuvers of Ti. Gracchus and his opponents; and investigators have devoted relatively little attention to the accounts of a crisis beyond examining individual points within them. Here the apparent assumption has been that Appian's and Plutarch's accounts are either transparent or present no great difficulties other than in matters of detail, where error may serve as an explanation for

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<sup>1</sup> For studies of Appian as an historian see Gabba (1956); Hahn (1982); Goldmann (1988); Gowing (1992); Brodersen (1993); Magnino (1993).

<sup>2</sup> See most notably Cardinali (1912); Fraccaro (1914); Carcopino (1928); Gabba (1956).

problematic features. A systematic investigation of Appian's narrative should help to clarify both its organizing structure and the underlying principles that shaped its presentation.

### I. *Appian's Account and its Place in the Civil Wars*

In his *Civil Wars* (BC 1.7.26–8.34) Appian opened his narrative of Ti. Gracchus' tribunate with a forceful description of a developing crisis and the failure of an earlier attempt to resolve it. Throughout their conquest of Italy, the Romans seized land, which they then dispensed by founding towns to serve in place of garrisons. They immediately assigned some captured land to settlers or sold or leased it. Because they lacked the time to distribute the large amount of land that they had seized, the Romans also proclaimed that individuals might carve out estates for their own use, as long as they paid to the Roman people a portion of the crops and a tax on the animals they pastured there. The Romans intended these practices, or so Appian claims, to increase the numbers of *Italiôtai*, so that the Romans might have many allies in war. Matters did not turn out quite as the Romans intended. The rich first took possession of unassigned lands and, confident that no one would take them away, they then began to acquire by purchase and by violence the plots of the poor who lived nearby, creating in the process large estates, worked by slaves. The rich thus became richer and the number of slaves increased, as did the danger they posed to the Roman state, while the Italian population, the pool of potential recruits for the army, declined. Then some tribunes carried a law prohibiting anyone from holding more than five hundred *iugera* of public land or from pasturing on it more than a set number of animals, thinking that the poor would use the excess. Instead the rich either ignored the law or made but token efforts to comply with it. Thus the dire situation in the countryside remained unchanged until Ti. Gracchus entered office as tribune. This depiction of the use and abuse of public lands is, alongside a broadly similar passage in Plutarch's life of Ti. Gracchus (8), the only such account to survive.<sup>3</sup>

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<sup>3</sup> Although there are clear differences between Appian's and Plutarch's accounts, both probably derive from the same ultimate source; see Cardinali (1912, 45–92); Fraccaro (1914, 11–20); Sterckx (1969). Against this view see Carcopino (1928, 3–45) and Gabba (1956, 37 n. 1). In any case, Appian's account of the crisis is more detailed and systematic; it will be the primary focus of the present investigation.

Appian's portrayal of developments on public lands serves to introduce the reform of Ti. Gracchus. His account of the reform falls into a few discrete sections. After setting out the origins of the crisis, Appian (*BC* 1.9 35–11.47) outlined Ti. Gracchus' goals, the measure he proposed to deal with the situation, and initial responses to the law. He did this in the form of two short speeches attributed to the reformer, separated by brief descriptions of the measure itself and of reactions to it by rich and poor. These responses form dialogues with the orations: the complaints attack elements of Gracchus' first address or specific provisions of his law, while the second speech is basically a response to these protests. After setting out the reformer's aims in this fashion, Appian (*BC* 1.12.48–16.70) turned to the political maneuvers of Ti. Gracchus and his opponents.

The history of the use of public lands and the account of the reformer's goals, his law, and the initial response to the reform by others form an essential unity, sharing the same themes, attitudes, and assertions. These two segments depend upon and complete each other very economically. Now, Appian clearly and explicitly presented the *lex Sempronia agraria* as not only an appropriate response to the crisis, but also one that was perfectly in accord with traditional goals. The most marked unifying factor in his narrative is the long-term stability in goals that he assigned to all official attempts to determine the use of captured lands. After briefly acknowledging that the Romans founded colonies to serve as garrisons (a military function), Appian gave to every law, project, or regulation from the very beginnings of Roman expansion to the Gracchan reform one end (yet another military function). Thus the Romans allegedly intended by distributing, selling, or leasing land, and by accepting the legality of private exploitation of the excess, to increase the number of *Italiôtai* so that Rome might have numerous allies. After the development of large estates had led to a shortage of soldiers, the first reformers tried to correct the problem by proposing a law limiting holdings of public lands, 'thinking the remaining land would be sold immediately to the poor in small lots'. Finally, Ti. Gracchus himself introduced his legislation specifically to resolve the problem of poverty and the shortage of potential recruits that resulted from the misuse of public lands by once again seeking to limit holdings and divide the surplus in small plots to be given to the poor.

Appian also linked his narrative of developments on public lands and his depiction of the Gracchan law in other, more complex ways. Gracchus' first oration is a recapitulation of the developments Appian

had just set forth. In it the reformer placed his reform firmly and explicitly in the context of the crisis whose origins and nature had just been depicted, and he makes its resolution his clear goal. Ti. Gracchus, Appian wrote, spoke of the problems facing ‘the *Italiôtai*, excellent in war and related to the Romans, who were declining into poverty and lack of numbers’ and warned of the dangers presented by slaves ‘free from military service and hostile to their masters’. In the immediately preceding account of developments in the countryside, Appian presented the appearance of just these problems, and he did so in markedly similar terms. Thus the rich, having constructed large estates, allegedly preferred slave labor to free because the latter could be drawn from farming into the army, while the former would provide additional profit through their children, who increased freely because of their ineligibility for military service. As a result, ‘the rich became very rich, the number of slaves increased greatly . . . while the *Italiôtai*, worn out by poverty, taxes, and military service, were suffering depopulation and a shortage of men’.

Having proclaimed in this fashion that the reformer intended to resolve just the crisis whose origins he had previously depicted, Appian then turned to provisions of the law itself. Appian gave specific provisions of the Sempronian law in a very brief description of the measure immediately after Gracchus’ first address, in his account of the reactions of the rich that directly follows, and in the reformer’s second oration. After his first speech Gracchus ‘renewed the law providing that nobody should hold more than five hundred *iugera* of public land’, he added to it a provision that ‘the children of the occupiers might each hold one-half of that amount’, and he instructed that ‘the remainder should be divided among the poor by three elected commissioners’. Appian certainly wished Ti. Gracchus’ proposal to be seen as the continuation and indeed as the perfection of the earlier law *de modo agrorum*. He stated that Tiberius ‘renewed’ (*anekainize*) it, and he identified the same core feature, a five-hundred-*iugera* maximum. The framers of both, moreover, allegedly thought their law would achieve similar results: the authors of the first measure thought ‘the remaining land would be sold immediately to the poor in small lots’, while Ti. Gracchus made certain the poor would receive land by appointing triumvirs to make the distributions that his predecessors seemingly thought would take place without official intervention.

When Appian reported the reactions of rich and poor and the reformer’s response, he again repeated earlier assertions while justifying

specific features of the reform law. The poor, who complained about their poverty and childlessness while contrasting their military service to the exemption enjoyed by slaves, merely restated claims that Appian had made both in the opening narrative and in Gracchus' first speech. The lamentations of the rich are more detailed, and in recording and answering them Appian revealed specific, and important, details of the law. The first complaint is simple: the election of commissioners was 'very disturbing to the rich, because, on account of the triumvirs, they could no longer disregard the law as they had done before nor could they buy the allotments of the poor, because Gracchus had proposed to forbid sales'. Here, Appian again portrayed the Sempronian law as identical to the first attempt at reform; he also provides a very clear justification for the creation of the Gracchan triumvirate, and reveals a new provision that guarded against a resumption of estate-building by prohibiting what he earlier had shown to be a means by which the rich had acquired the land of the poor. Appian later would make removal of Gracchus' ban the first stage in the dismantling of the reform.<sup>4</sup>

The chief complaint was the loss of land, and it is echoed and answered both in Gracchus' second oration and in Appian's account of the crisis. Those fearing losses, we are told, pointed to the investments they had made in improving the land, or claimed that they had bought the land from their neighbors and now stood to lose both land and money, or asserted that they had received the land in the division of their fathers' estates, or they proclaimed that the land in question had been purchased with their wives' dowries or given as part of their daughters' dowries. In his second oration Ti. Gracchus responded to this protest and, while so doing, provided one more justification for a provision of the law: after confirming his support for the measure and exhorting the rich to end their opposition, the reformer then proclaimed that 'for any labor they [i.e. the rich] had spent they were receiving ample reward in the secure possession of five hundred *iugera* without payment, plus half as much again for each child in the case of those who had children'. Appian gave the framers of the pre-Gracchan law just this motive for permitting the rich to maintain a portion of their ill-gotten gains: its framers found it 'neither easy nor just to take away from so many men so much land they had planted for so long and built upon and prepared'.

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<sup>4</sup> *BC* 1.27.121–4; see also Gargola (1997).

This constant interaction between Appian's depiction of crisis and of reform continues at yet another level. In his history of the exploitation of public lands, the crisis that developed there, and the failed attempt at reform, Appian provided clear precedents or reasons for every provision of Ti. Gracchus' law that he would report. Appian identified four provisions of the law of 133 BC, and in his earlier account of the origins of the crisis he clearly and unambiguously justified each: the Gracchan law banned sales of the allotments that would be made under its authority, while earlier, purchase of holdings had been presented as a prime means by which the rich first had gained control of the land; the Sempronian law limited the size of estates established on public lands, while earlier, the development of just these estates was shown to be at the root of the problem—and an earlier law was presented as attempting just what Ti. Gracchus would attempt and with the same goal in mind; the law of 133 BC established a triumvirate to administer its provisions, and, when describing the first law, Appian also claimed that it failed to achieve its goal because the rich either had disregarded it or found ways to evade it; finally, the Gracchan law permitted large-scale occupiers to retain part of their illegal holdings, along with an extra portion for children, and this also finds its precedent in the earlier account.

Appian's account, then, was constructed in such a way as to make Gracchus' reform seem justified, perfectly in accord with traditional practices and goals, and an attempt to restore earlier conditions, long eroded by improper behavior. The law, however, was in fact a highly unusual measure. It had no clear and close predecessor, and, with the exception of C. Gracchus' re-enactment of it ten years later, it would have no clear and close successor. To illustrate this point, one should compare the known provisions of the Gracchan law with the brief characterizations, primarily in Livy's history, of other measures that had sought to control the use of public lands. In the first decades of the second century BC, laws that authorized colonies, *viridane* assignments, sales, or leases formed distinct legislative categories.<sup>5</sup> In other words, an individual legislative act authorized either colonies or *viridane* assignments or sales or leases, but not projects of more than one type.

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<sup>5</sup> For colonies, Mommsen (1887, 2.624–7); De Martino (1972–5, 2.102); Gargola (1995, 52–8); for *viridane* assignments see Gargola (1995, 103–6); for sale and lease see Gargola (1995, 116–19).

Similarly there is no evidence associating any law limiting the size of holdings, a kind of legislation known to scholars as a *lex de modo agrorum*, with formal and official land assignments, sales, or leases.

The authors of the *lex Sempronia agraria*, however, combined elements from hitherto separate legislative categories. They took a rule *de modo agrorum*, which had been enforced by aedilician prosecutions and fines, and applied its limit to a different end and possibly to lands differently defined (see below). To enforce the rules and to assign land to the poor, they created a special triumvirate. The history of the exploitation of captured lands contains many triumvirates, but they all founded colonies; they did not enforce rules *de modo agrorum* or make viridane assignments.<sup>6</sup> This combination of features may well be what Tiberius Gracchus' opponent, Ti. Annius Luscus, had in mind when he claimed that the Gracchan commissioners had gained their office *per saturam*—that is, through a law containing otherwise unrelated elements.<sup>7</sup>

Here, then, is the problem that lies at the core of the present investigation. Appian set out a history that in many ways is plausible, but he also embedded it in a narrative that presents Ti. Gracchus' law as an appropriate response to a crisis that was also perfectly in accord with traditional practice, and that is much less satisfactory. One should recall that Roman authors often presented innovative behavior by setting out real or imagined precedents. Here the Gracchan age was especially productive of alleged precedents for the activities of both the reformers and their opponents.<sup>8</sup> A goal of the present investigation, then, is to identify the ways in which Appian's depiction of the Gracchan reform has shaped his history of the use of public lands and his representation of the agrarian crisis.

## II. *The Chronological Stages of Appian's Narrative*

Appian's account of the crisis and its remedy has all the appearance of a historical work. He presented a sequence of developments in chronological order, and he made each successive development the clear result of what had taken place just before. Appian's description

<sup>6</sup> See Gargola (1995, 56, 105).

<sup>7</sup> Festus 416L: *imperium, quod plebes per saturam dederat, id abrogatum est*. In this passage, probably corrupt, Mommsen (1887, 2.634 n. 2, 3.336 n. 5) recognized a reference to the supposed illegality of the Gracchan triumvirate.

<sup>8</sup> For the traces of the Gracchi in later works see Béranger (1972); Rieger (1991).

of the crisis and his account of Ti. Gracchus' goals and his remedy moves through four broad chronological stages. In the first, Appian set forth the traditional regime governing public lands, the goal it was intended to achieve, and why this was deemed important. In the second, Appian reported the ways the rich subverted this system and the dire consequences this had for the state. In the third, he noted the passage of a law intended to correct the problems he just had outlined, justified the law's central provision, and explained its failure. Finally, he introduced Ti. Gracchus and his law, the culmination of a long course of development. The history, then, moves through four stages of development: a time when public lands were used for public purposes; a period in which original conditions were undermined and original goals subverted; a failed attempt at reform; and finally, a perfected reform, which would allow the resolution of a long-standing problem and the attainment of a long-held goal.

In each of the phases that center on some official action or actions, the emphasis lies on laws and legal forms. In the first, Appian set out certain well-known methods of exploitation: the assignment of plots to settlers in colonies or in *viridane* assignments and the sale or lease of tracts of captured land. In the late third and early second centuries BC these activities were frequently, and perhaps usually, authorized by a long series of laws enacted by popular assemblies.<sup>9</sup> Appian used the verb *epikérussó*, 'to announce' or 'to proclaim', to characterize the action that led to the formal opening of lands to private exploitation. This verb is often used, as in Appian, to indicate the proclamation of rules and penalties. The pre-Gracchan law establishing limits to the use of public land and the *lex Sempronia agraria* that supposedly renewed it were quite obviously individual laws, rather than categories of legislation. Perhaps this focus on laws and categories of legislation is what Appian meant when he identified the chief actors as 'the Romans'. Finally, Appian arranged each of the stages as a description of a law, a practice, or a process, to which he appended a declaration of its consequences or a proclamation of the goals or motives of the central actors. As we shall see, these attributions of context and motive are the source of most of the problems in interpretation.

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<sup>9</sup> For the connection between legislation and projects see Gargola (1995, 52–8, 103–6, 114–28).

### III. *Unity of Motive*

Throughout his account of the origins of the crisis and of the legislation Ti. Gracchus proposed to remedy it, Appian united the Gracchan law and all earlier official forms of regulation and exploitation into a single narrative by giving to all official acts the same purpose and by directing both attempts at reform against the same ills. By uniting a range of disparate practices around one issue and one goal, Appian created a single history.

This uniformity of motive clearly is dubious. Appian held that the Romans sought to govern the exploitation of public lands primarily so that the number of *Italiôtai* might increase and the Romans might have many allies in war. Two elements of this simple assertion are especially worrisome. First, Appian claimed that the Romans intended for *Italiôtai* to be the primary beneficiaries of their land policies, and later he would maintain that Ti. Gracchus intended them to be the recipients of his allotments. Although some scholars do accept the claim, it is probably incorrect. The sources for earlier land distributions often leave the legal status of the recipients unstated, but when they are explicit in this regard, the beneficiaries are Roman citizens and occasionally Latins.<sup>10</sup> In the Agrarian Law of 111 BC which attempted to resolve disputes arising out of the Gracchan reform, surviving clauses acknowledge the legality of allotments to citizens and to Latins.<sup>11</sup> Perhaps Appian was simply mistaken or confused—Italians, after all, were citizens in his day—or possibly he wished to bring forward all the issues of C. Gracchus' tribunate or the Social War, when the status of the Italians was a source of controversy, as soon as possible.<sup>12</sup>

The second element is more significant for the present discussion. For Appian, all practices had but one end: increasing the population and thus the number of potential recruits for the army by making it possible for the poor to farm land in small plots. Distributing land in colonies or in *viritane* assignments would certainly have helped to increase the number of potential recruits, although such settlements would also have served a range of strategic purposes—Appian himself noted that the Romans had founded colonies to serve as garrisons. And financial

<sup>10</sup> See, for example, Livy 34.42.5–6; 34.45.1–2; 42.4.3–4.

<sup>11</sup> See Crawford (1996, no. 2, lines 3, 21 and 31).

<sup>12</sup> For a discussion of the place of the *Italiôtai* in Appian's *Civil Wars* see Mouritsen (1998, 5–22) with extensive bibliography.

considerations would probably have provided the chief reason for the sale or lease of captured lands.<sup>13</sup> In the very first stage of his history, then, Appian assigned only one goal to a range of diverse practices, devoted to disparate ends.

This same combination of a reasonably accurate, if concise, account of a practice alongside a more dubious depiction of goals can also be found in Appian's representation of the first attempt at reform. A number of authors reported the passage of a pre-Gracchan law, usually as part of the Licinian-Sextian Rogations of 367 BC, that established a five-hundred-*iugera* maximum to holdings;<sup>14</sup> most do not mention any restrictions on the size of herds. Velleius Paterculus explicitly claimed this measure served as the model for the Gracchan law. The earliest source, a fragment from an oration that the Elder Cato delivered in 167 BC, notes the same five-hundred-*iugera* limit and also some unstated upper limit to the size of herds.<sup>15</sup> Cato's use of the law as an illustration confirms that the pre-Gracchan *lex de modo agrorum* was not merely a backward projection of the Gracchan reform by later historians.<sup>16</sup>

Appian gave to the first law three provisions. The most prominent, the limit on the size of holdings, provides the chief link between the Gracchan and pre-Gracchan laws. No other source mentions Appian's remaining provisions, and no source, not even Appian, includes these two provisions in the Gracchan law. The first required the swearing of an oath to follow the law, part of its *sanctio*; such a provision cannot be identified with any certainty in any legislation before the end of the second century BC.<sup>17</sup> The remaining provision required that landholders employ a fixed number of free laborers to watch and report. Here, Appian leaves unclear the objects of their scrutiny. Some scholars have suggested that these employees were to keep slaves under surveillance, reinforcing Appian's claim that this law, like its Gracchan

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<sup>13</sup> Officials who sold or leased public property often did so to meet specific and immediate needs or to support a single and limited public institution, such as a temple, a festival, or a priesthood; see e.g. Livy 28. 46.4; 31.13 1–9; 41.27.10; Fest. 204L.

<sup>14</sup> Livy 6.35.4–42.9; DH 14.12 22; Gel. 20.1.23; Plu. *Cam.* 39; Vell. 2.6.2–3; Var. *R* 1.2.9; Plin. *Nat.* 18.14.17; V. Max. 8.6.3; *Vir. ill.* 20.2–4; Col. 1.3.11. Columella makes the legal maximum fifty *iugera*, possibly a copyist's error.

<sup>15</sup> Cato fr.167 *ORF* = Gel. 6.3.37.

<sup>16</sup> Note, however, that Maschke (1906, 60–6) thought that both the Licinian law and Cato's fragment were later reconstructions, intended to provide precedents for the Gracchan reform.

<sup>17</sup> See Crawford (1996, 1.23–4).

successor, was aimed against large concentrations of slaves.<sup>18</sup> Roman laws sometimes contained provisions for informers to report violations and start prosecutions.<sup>19</sup>

Now, Appian made this law Ti. Gracchus' model in very specific ways. For him, both legislators aimed their measures against the same ills and both intended their laws to have the same results. The shared five-hundred-*iugera* limit to holdings occupies the central role in this presentation, for it would break up large estates, the root of all subsequent problems. Beyond this common feature, however, Appian noted primarily ways that the two laws differed. Thus, Appian claimed that Gracchus renewed the limit to the size of estates (he did not mention limits on herds), and to this, he added several innovations that he had earlier justified and would continue to justify: a provision permitting occupiers to retain in addition half the specified maximum amount of land for each child; another establishing triumvirs to implement land distributions; and a rule banning the sale of allotments. The Sempronian law certainly did establish a maximum size for landholdings, although the sources do present different maxima, some of which may derive from corrupt manuscript traditions.<sup>20</sup> There are, moreover, clear physical traces of both the Gracchan triumvirs and their land distributions.<sup>21</sup> Finally, Ti. Gracchus' law probably did include some ban on sales.<sup>22</sup> Despite Appian's explicit claim that Ti. Gracchus had renewed the earlier law, the details of his presentation show the reformer reshaping his model in quite distinctive ways.

The law's context and purpose are a different matter. Here one must clearly distinguish between elements that Appian claimed to be part of the law, and the goals, motives, or states of mind that he attributed to the tribunes who framed the measure and to the people who voted for

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<sup>18</sup> See e.g. Tibiletti (1948, 200–1).

<sup>19</sup> See Alexander (1985); Gargola (1995, 133–5).

<sup>20</sup> Thus Livy (*Per.* 58) claimed that the Gracchan law established a maximum of one thousand *iugera*, which some scholars have interpreted as the results of combining the basic five-hundred-*iugera* limit with the portions of a maximum of two children. Sic. Fl. (p. 102 Campbell) reported that a Gracchan law prohibited individuals from possessing more than two hundred *iugera*, but he did not identify which of the two brothers framed the law. The Agrarian Law of 111 BC notes the existence of such a limit but does not identify what it was; see Crawford (1996, no. 2, line 2).

<sup>21</sup> For signs of the activities of the commissioners see Gargola (1995, 155–63).

<sup>22</sup> Laws associated with Ti. Gracchus' younger brother, Gaius, did ban sales, and this may well have been derived from the legislation of the elder brother; see De Ligt (2007).

it. Appian's assertion that the Romans permitted occupiers to maintain a portion of their holdings as compensation for the effort and capital they had expended in improving them echoes a similar claim he would make about Gracchus' law. His announcement that the Romans thought the excess land 'would be sold immediately to the poor in small lots' provides the necessary connection between the law, which seemingly contained no formal provision for land distribution, and the measure's ostensible purpose in the form of an expectation allegedly held by those who put forward the law or voted for it.

Other Greek and Roman authors either are less specific about the law's context or they assign to it a different purpose. Some did place such a law in a context similar to Appian's.<sup>23</sup> Thus, Livy (6.35.4–42.9) located the *lex Licinia* in the midst of struggles over debt, land, and the election of consular tribunes, but he did not mention slavery or problems in military recruitment. Other authors situated the law somewhat differently, but not in a manner that is incompatible with Appian's account: Pliny (*Nat.* 18.4.17) thought the Licinian law covered estates established through the expulsion of neighbors; Dionysius of Halicarnassus (14.12.22) and Plutarch (*Cam.* 39) used the law to establish Licinius' credentials as a seditious tribune. Here, Gracchan echoes are possible, or even likely. Other passages place the measure—or one like it—in a much different context. When reporting the convictions in 298 BC of violators of what may be this law, Livy (10.13.14) noted that such prosecutions restrained immoderate greed. Later he (34.2.1–4.20) had the Elder Cato praise the *lex Licinia* along with the *lex Oppia*, a sumptuary law, as a check on avarice and luxury. And Aulus Gellius (20.1.22–23) included the law in a list of sumptuary laws necessary to restrain luxury.<sup>24</sup>

Here one should note that the mechanisms of the law may indicate that its framers never intended their law to achieve the goal that Ti. Gracchus set or to resolve the problems that he confronted. Primary responsibility for enforcing the law rested on the aediles, who prosecuted suspected violators.<sup>25</sup> The result of a successful prosecution was a fine, which the aediles often spent on ornamentation of the city

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<sup>23</sup> Plutarch (*TG* 8) assigns the law a purpose very similar to Appian's, but both authors probably followed more or less closely the same ultimate source; see n. 3 above.

<sup>24</sup> For a more detailed examination of these passages, which reaches similar conclusions, see Rich in this volume.

<sup>25</sup> For the aediles as prosecutors see Bauman (1974).

and its temples. There are no reported provisions that allowed excess lands to be seized or turned to other uses. One should recall that the Roman legislators had access to a range of practices that could more directly install settlers on the land in small plots, i.e. colonies and viri-tane assignments.

In Appian's history of the use of public lands the long-term stability of context and of goals serves most to unify the account into a single narrative and to unite it with the depiction of the Gracchan reform which immediately follows. It was, however, possible to write a history of public lands in a markedly different manner. Here the fate of captured lands in the *ager Campanus* provides a good example. The Romans had confiscated much of the land of Capua after the failure of its rebellion in the Second Punic War. Soon after, the consuls had leased the right to harvest the crops on certain lands, probably a temporary measure. Then in 205 quaestors sold a portion to finance Scipio Africanus' invasion of Africa, perhaps another temporary measure. After the war, triumvirs founded colonies at Volturnum and Liternum. Lands were also given to the temple of Diana on Mt. Tifata. By 173 BC the remaining land had come into private possession.<sup>26</sup>

In Campania, then, events seemingly moved through Appian's opening stages: the sale, lease, or distribution of captured lands by officials followed by the appropriation of the remainder by private citizens. The result again was the passage of a law, but not quite the way that Appian's rather schematic history would have it. At the beginning of the consular year 173 BC Livy (42.1.1–2) recorded that the senate sent one of the consuls, L. Postumius Albinus, to Campania, where he was to 'determine the boundaries between public and private lands, because it was well known that private persons, by gradually moving their boundaries outward, were occupying a very large part of it'. In the following year a tribune of the plebs, M. Lucretius, carried a law ordering the censors to lease lands that had been recovered in Campania.

Now, Livy (42.1.1–2) did not link the senate's assignment of the task to Albinus to any desire to turn the *ager Campanus* to a particular use. Instead he gave as a motive public awareness that individuals were moving their boundaries into public land. When he reported the enactment of Lucretius' law in the following year (42.19.1–2), he once again connected the measure to the failure of individuals to display

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<sup>26</sup> For the *ager Campanus* in these years see Rathbone (2003, 155–6).

sufficient concern over boundaries, and he claimed that leasing the land would remedy just this situation: ‘M. Lucretius, tribune of the plebs, put forward a measure providing that the censors should lease the fruits of the *ager Campanus*, which had not been done for so many years after the fall of Capua that the greed of private citizens had a free field in which to wander.’ Leasing the land, in other words, would install on it individuals who would defend its boundaries.<sup>27</sup> Livy said nothing about the legality of the possessors’ occupation nor of any limits to the scale of possession. The Roman elite seems to have regarded public lands in Campania as a reserve from which portions could be detached whenever a suitable purpose presented itself.

#### IV. *Appian’s Depiction of a Crisis*

When Appian attributed a common goal and context to the two efforts at reform, he also depicted both laws as directed against the same crisis, yet another unifying factor in his history. Conflicts over land are common in many agricultural societies; often they take the form of complaints over excessively large landholdings and include demands for their redistribution. Appian, however, described a very specific process, arising on lands of a precise legal character, and he connected them, by a sequence of causes and effects, to very explicit ills: the spread of slavery and the decline in military manpower resulting from increasing poverty and childlessness.<sup>28</sup>

Depictions of elements of this crisis are widely dispersed in Roman historical writing. The conviction that the rich were displacing or had displaced the poor from their lands is fairly widespread; some authors even attribute the practice to the earliest years of the Republic. The development of the praetorian interdicts may indicate a concern over rural violence (see below). Other aspects of Appian’s crisis can be paralleled in reports of events and activities that are close in time to the Gracchan reform. The Slave Wars in Sicily may well have convinced many of the danger that large numbers of slaves presented. And there

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<sup>27</sup> Such a strategy would not be unprecedented. Hyginus Gromaticus (p. 158 Campbell) recommended that officials making land assignments start at the outer limits of the allocated land so that the newly-installed landholders might ‘serve as boundary markers’.

<sup>28</sup> Whether or not there actually was such a crisis is a different and far more complicated matter; see e.g. De Ligt (2004).

are instances in the previous decades that illustrate resistance to conscription, if only for specific wars, and the dangers that the resulting unpopularity might have for ambitious officials.<sup>29</sup> A brief fragment from the historian Cassius Hemina, active around the middle of the century, may show that he claimed, in an unknown context, that the poor had been expelled from public lands.<sup>30</sup> Finally, while serving as censor in 131 BC, Metellus Macedonicus advocated, in a speech delivered to the people, that citizens be compelled to marry so that the number of children might increase.<sup>31</sup>

Although his claims are not unprecedented, Appian's account is rather unusual, for it unites these phenomena into a single sequence of causes and effects. Appian sets out the crisis in markedly legalistic terms. For him, the concentration of landholdings began on lands that were public—he characterized them as 'spearwon'—not private. Within this broad category he made those captured lands that were left over after the Romans had assigned, sold, or leased various portions the focus of the process. The Romans had earlier opened just these lands by proclamation to private exploitation on payment of a rent. The centrality of these surplus lands continues through the two attempts at reform. According to Appian, tribunes proposed the first law *de modo agrorum* to limit the size of holdings 'of this land' (*têsde tês gês*) and to restrict the number of animals that could be pastured on it.<sup>32</sup> Later Ti. Gracchus would renew this measure. Appian presented these lands not merely as a residual category—lands left over after officials had performed certain operations—but also as a legal one: the Romans intended their use to be governed by a single purpose, they placed on them rents assessed in a uniform fashion, and they eventually applied to them both the Gracchan and pre-Gracchan rules *de modo agrorum*.

<sup>29</sup> See Evans (1988).

<sup>30</sup> fr. 17 Peter (= fr. 20 Beck-Walter = Non. p. 217L): *Hemina in annalibus: quicumque propter plebitatem agro publico eiecti sunt*. The word *plebitas* is rare—Hemina's fragment and Nonius' explication of it are the only known occurrences—and its meaning is not entirely clear; Nonius (p. 217 L) associated the word with both poverty and plebeian status, which may have been synonyms for him. Forsythe (1990, 334) argues that the passage describes the background to the Gracchan reform and not the Struggle of the Orders.

<sup>31</sup> For the speech see Liv. *Per.* 59; Suet. *Aug.* 89.

<sup>32</sup> Here a textual problem could affect the argument. Some manuscripts lack the *têsde*, so that the restrictions would apply to 'land' rather than to 'this land'. The crucial word, however, is in the better manuscripts and it is the preferred reading.

Here, Sallust (*Jug.* 41) provides an instructive contrast. To set the stage for the political conflicts surrounding the war with Jugurtha, Sallust described briefly the division of the Republic into two factions, the nobility and the *populus*. Before the destruction of Carthage, he claimed, Senate and people had lived without strife, for fear of the enemy preserved harmony. Conflict began with peace, for the nobles abused their position and the people their liberty. Since the nobles were more powerful, they came to control the state, the treasury, and the benefits of empire, while the people were burdened with military service and poverty. At the same time, the parents and small children of soldiers, if they had a powerful neighbor, were driven from their homes. This unlimited greed, he asserted, set the stage for political conflict that began with the Gracchi and continued after their deaths. Like Appian, then, Sallust saw military service, poverty, and the expansion of the holdings of the rich by violence as threats to the political order. Unlike Appian, however, he did not characterize in any way the legal status of either the original holdings of the rich or of the tracts they would seize. Expulsion by violence and purchase, Appian's mechanisms of expansion, would have worked equally well on lands of any legal status, and the resulting poverty would have been just as dire for the state.

Appian's claim that public lands lay at the center of estate-building is not entirely implausible. Public lands that had not yet been assigned or converted to some fiscal purpose may well have been of considerable extent, so that any marked tendency of the rich to increase their holdings would have involved them in some manner. It is perhaps less likely that the process would have remained restricted to such lands. Violent confrontations in the countryside may have been relatively common in second-century BC Italy. At some time before 133 BC, innovative praetors introduced the edicts *unde ui* and *uti possidetis* to assist landholders in regaining control of lands from which they had been expelled; their use by the poor was not a realistic option. Here one should recall that the distinction between public and private permeated Roman law and set out distinct classes of remedies. Official projects, such as land distributions and sale or lease, were limited to tracts deemed to be the property of the Roman people. Abuses on private property generally were not subject to the direct action of Roman magistrates. Instead, displaced landholders, and not public officials, were responsible for seeking sanctions and remedies through private law procedures. In this, Ti. Gracchus did not break new ground. Scholarly discussion of

the background to the Gracchan reform is often influenced by the categories of Roman jurisprudence, although one may be justified in doubting whether actions in the countryside fit into these classifications quite so neatly. It is important to note that this rather legalistic way of approaching conditions is itself written into the single most influential ancient source.

Appian, then, placed the origins of the crisis on a single category of public lands, which he also made the focus of two attempts at reform. For this reason, scholars have long sought to find such a category in the writings of jurists and surveyors. A simple identification with *ager publicus* clearly is unacceptable, for Appian's category excludes lands that had been sold or leased, and tracts such as these still remained public in some sense. Specialized classifications of public lands, moreover, usually do not group lands suitable for cultivation with tracts used as pasture. In legal texts and in the *Corpus Agrimensorum Romanorum*, a range of terms denotes different kinds of arable public lands, while others, such as *ager scripturarius* and *ager compascuus*, refer to public pastures.

If one limits the discussion to arable lands, which would be most suitable for small-scale farmers, matters do not become much clearer. A survey of legal sources encounters a wide variety of terms that cover portions of such lands—i.e. *ager diuisus et adsignatus*, *ager quaestorius*, and *ager censorius*. These terms, however, refer to tracts turned to a single purpose. No single term or circumlocution contains all arable public lands not turned to some specific purpose. Now, scholars have often identified Appian's land with the *ager occupatorius* of the *agrimensores*.<sup>33</sup> The adjective *occupatorius*, derived from the verb *occupare*, 'to seize' or 'to take possession of', seems quite appropriate to designate lands whose exploitation was the result of private initiative. As we shall see, the *agrimensores* also reveal a range of uses for lands left over after land assignments, sales, and leases.

In an influential series of articles Tibiletti sought to link *ager occupatorius* with the application of rules *de modo agrorum*. Passages in the works of Columella (1.3.13) and some of the *agrimensores* seemingly limited *possessionses* to lands that the *possessor* actually cultivated or expected to cultivate (*in spem colendi*). Some, moreover, associate this rule with holdings explicitly formed either from *ager occupatorius* or by *occupatio*. These

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<sup>33</sup> For earlier views on *occupatio* and *ager occupatorius* see Bozza (1939, 9–70); Tibiletti (1948; 1949); Burdese (1952, 13–36); Stockton (1979, 206–16).

notices, Tibiletti suggested, marked the existence of customary limits, linked to a landholder's ability to utilize the land, in force before the imposition of fixed numerical limits in the fourth-century BC *lex de modo agrorum*.<sup>34</sup> This is certainly incorrect. In some cases the supposed restriction is virtually a definition of *possessio*: one could only claim to possess lands that one was actually using or those that one expected to use. Siculus Flaccus (p. 104 Campbell) used the phrase *sed quod aut excoluit aut in spem colendi occupavit* as a criterion to help surveyors establish the outer boundaries, otherwise unmarked, of a tract of *ager occupatorius* that a single *possessor* had seized. Others merely asserted that would-be *possessores* seized only lands that they could cultivate or expect to cultivate. There is no indication that this restraint was the result of any communally determined and imposed limits.<sup>35</sup>

In these discussions scholars have generally accepted that *ager occupatorius* was an extremely broad category, one that approximated fairly closely Appian's residual category. Paula Botteri, who re-examined the evidence, found a more narrow use of the term.<sup>36</sup> The *agrimensores* used *ager occupatorius* and *ager arcifinius* or *ager arcifinalis* interchangeably. For the *agrimensores*, such lands were formed when the victors expelled the defeated at the time of the conquest, and, under the authority of the commander, individuals then took control of portions. Botteri also identified passages in more literary works indicating that soldiers who took part in the conquest sometimes received lands from their commander as a form of plunder.

One incident may provide another example of this practice. In several scattered passages Livy noted a dispute over property in Syracuse in the years between Marcellus' conquest of the city and Scipio Africanus' arrival in 205 BC.<sup>37</sup> Marcellus had apparently given some lands that he had confiscated to individuals who had accompanied him. Livy had Marcellus defend this action in a speech in which he proclaimed that 'he took from individuals and he gave to individuals... according to the law of war'. In 210 BC some Syracusans complained to the Senate that their property had been confiscated improperly since they had always

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<sup>34</sup> Tibiletti (1948, 218–25). The passages in question are Sic. Fl. p. 104 Campbell; *Commentum de agrorum qualitate* p. 50 Campbell (note that Lachmann, whose text of the *agrimensores* Tibiletti followed, attributed this passage to Agennius Urbicus); Col. 1.3.13.

<sup>35</sup> For a detailed critique of Tibiletti's arguments see Mantovani (1997).

<sup>36</sup> See Botteri (1992).

<sup>37</sup> Liv. 26.30.10; 26.31.9; 26.32.6; 29.1.16–17.

been loyal to Rome. The Senate then decreed that the next commander, Laevinus (cos. 210 BC) should recover what property he could ‘without doing injury to the Republic’. The controversy apparently lingered. In 205 BC Africanus heard complaints in Syracuse about lost property that had still not been recovered.

Now, *ager occupatorius* defined as the surveyors did does not fit Appian’s history very well. It was not burdened with a tithe or rent. Indeed the *agrimensores* stress that such lands were not bounded in any formal manner, so that assessing payments, or even an occupant’s liability to pay them, would have been difficult.<sup>38</sup> Second, it does not fit well the relative chronology of Appian’s history. For him, the crisis developed on tracts that remained public after assignments had been made and portions sold or leased. According to the *agrimensores*’ definition, lands would have been seized at the time of the conquest, before any other arrangements had been made. Finally, the legal status of the victims differs greatly. For the surveyors, commanders and their soldiers drove off citizens of hostile communities as an immediate consequence of their defeat. For Appian, the rich expelled either Roman citizens or allies at some indefinite point after war had ended.

Lands described as *subseciua*, lands left over after the completion of a survey, provide the closest match between Appian’s lands and a surveyors’ category. Tracts of such land could be considerable. According to Livy (35.9.7–9), the triumvirs who founded a colony in the *ager Thurinus* in 193 BC left out of the assignment to colonists 36,000 *iugera* of arable land—one-third of the total amount surveyed—so that other colonists might be added later. Livy gave as the reason a shortage of colonists, not a shortage of time, as Appian would have it: the land, after all, had already been surveyed. Appian’s text may preserve signs that he did have *subseciua* in mind. In the opening stage of his narrative he set out the process of seizing, distributing, selling, and leasing captured lands with a long series of verbs in the imperfect tense, suitable for describing repeated actions as the Romans conquered Italy ‘region by region’. When Appian described the proclamation that allegedly opened excess lands to private use, he again used a verb in the imperfect tense, *epekêrutton*, indicating that he regarded the Romans as performing this act too successively in region after region. Lands that remained excess in a region after distributions or sales may best be described as *subseciua*.

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<sup>38</sup> For the unsurveyed character of *ager occupatorius* see Sic. Fl. p. 104 Campbell.

But here too there is a crucial distinction. Appian's land is both a residual category and a legal one. For the *agrimensores*, *subseciua* is primarily a residual category, for individual tracts could be held under a variety of rules. Here the actions of the magistrate or magistrates under whose authority the survey was made appear to have been crucial: there is no indication that these matters ever had been addressed in formal legislation. The *agrimensores* identified either with a fixed term or a circumlocution segments that had been sold or leased, tracts burdened with a tithe, portions left to the use of neighbors, segments turned back to the original inhabitants or put under the supervision of the magistrates of a newly-founded colony. It is quite possible, then, that some were occupied under terms similar to Appian's, although the whole class clearly was not. It should be noted that Appian elsewhere acknowledged the existence of some of these forms of exploitation. While recounting the attempts of the rich to block passage of the Gracchan measure, he (*BC* 1.10.41) included among the opponents colonists, inhabitants of *municipia*, and 'others who had a share in the lands and similar reasons for being afraid'. When later describing the actions of the Gracchan commissioners (*BC* 1.18.73–21.90), he presented them as active on lands that had been turned over to allied communities, a form of exploitation that he did not acknowledge when depicting the origins of the crisis. What united *subseciua* as a category may have been that tracts remained public in some fashion—*possessores* could not gain ownership through *usus*.<sup>39</sup> If Appian did intend to describe *subseciua* then once again he attributed a spurious unity of purpose to rather disparate methods of exploiting captured lands.

Our sources for the first *lex de modo agrorum* do not provide much help in the matter.<sup>40</sup> Here, Appian made the clearest claims; other authors are much less helpful. Most only identify the limit as applying to land without specifying its legal character, even whether it was public or private.<sup>41</sup> When reporting the passage of the *lex Licinia*, Livy (6.35.4.42.9) too did not explicitly identify the lands in question as public, although

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<sup>39</sup> Note, however, that Zancan (1931–2, 71–96) thought that *possessores* could gain ownership of public land through *usus* into the second century BC. He identified the recovery of the *ager Campanus* from *possessores* in 173 BC as the turning point. Against this see the arguments of Bozza (1939, 43–69) and Tibiletti (1948, 176–9).

<sup>40</sup> See also Rich in this volume, where he makes much the same point.

<sup>41</sup> Plu. *Cam.* 39; Vell. 2.6.2–3; Var. *R* 1.2.9; Gel. 6.3.37; 20.1.23 (and Cato's oration embedded in the first citation); Plin. *Nat.* 18.4.17; V. Max. 8.6.3; Col. 1.3.11; *Vix. ill.* 20.3–4.

his account of the background to the law indicates that he thought that they were. Reports of prosecutions do not help. When reporting the successful prosecutions of 298 BC, Livy (10.13.14) noted only that the condemned held too much land. Most notices reporting prosecutions of *pecuarii*, moreover, specify neither the nature of the offence nor the legal status of the lands on which they pastured their herds or flocks;<sup>42</sup> only Ovid (*Fasti* 5.283) noted that those convicted used public pastures. The vagueness of the accounts of other authors forms a striking contrast with Appian's apparent concern for legal forms.

### V. *Land and Legislation*

In the previous section I argued that Appian has presented a legalistic account of the origins of the crisis that sets out public lands of a specific type as the locus of the process of estate-building, that he made these same lands the subject of both the pre-Gracchan and Gracchan *leges*, and that these lands, as a legal category, have no close parallels in the known legal and technical terminology, although *subseciua* does perhaps come closest. In this section I would like to examine more closely the link between land and legislation.

Discussion of the legal categories into which public lands might be divided often takes place at a fairly high level of abstraction. However, lands acquired their status at specific moments through the actions of at least theoretically identifiable magistrates. Thus the category of *ager quaestorius* contained public lands that some quaestor had sold, *ager censorius* portions that a censor had leased, and *ager diuisus et adsignatus* tracts that some official in charge of a settlement project had surveyed and assigned to individuals. In the case of *subseciua*, the magistrate in charge of a survey assigned uses in his *lex data*.<sup>43</sup> Even *ager occupatorius* is connected with a magistrate's actions: those of the victorious commander. Jurists apparently formed these constructs after the enactment of the Sempronian law of 133 BC: the authors of the Agrarian Law of 111 BC did not write of *ager quaestorius* or *ager censorius*, but rather of lands that some quaestor had sold or some censor had leased.<sup>44</sup> Jurists and surveyors, it should be noted, were most concerned with lands

<sup>42</sup> See e.g. Liv. 10.23.13; 10.47.4; 33.42.10–11; Fest. 276L.

<sup>43</sup> See Gargola (2005, 146–9).

<sup>44</sup> See Crawford (1996, no. 2, lines 28, 86–9).

turned to some specific purpose, the most likely source of conflict; it is around them that most legal terms were concentrated.

Legislation authorizing official actions on public lands also specified in some fashion the lands in question. In the opening decades of the second century BC, laws and senatorial decrees often authorized specific magistrates to found colonies, make virgane assignments, or sell or lease public property.<sup>45</sup> No text of such a law survives, but, judging from the brief characterizations to be found in our sources, a common feature of these measures was the identification of the *ager* or the *agri* where official actions were to take place, a practice that is analogous to the declaration of a consul's or praetor's *provincia*.<sup>46</sup> When a law set out a magistrate's sphere of activity, then, it started a process that would result in the official creating within that space tracts that would acquire separate legal statuses: for example, *ager diuisus et adsignatus* and various forms of *subseciua*.

If we re-examine Appian's residual lands against this background, it is clear that they form, for Appian, a defined field over which officials were authorized to perform certain actions. Thus his *lex Sempronia* authorized its triumvirs to search out violators of the law in just these lands and then make land assignments from the excess. Here too the actions of these commissioners would have created tracts within this broad field of activity that would come to have a more precise legal character, most notably the plots given to the poor. Legislation authorizing the foundation of colonies or the making of virgane assignments named specific *agri* where the actions were to take place; a law like the *lex Sempronia* would probably have defined its sphere more abstractly and more broadly.

Against this background, it is important to identify a law or laws that might have defined a sphere of operation similar to Appian's. At this point we should turn to the law of 133 BC—the actual law and not just its representation—and examine the ways that Ti. Gracchus managed the task of defining the land covered by his law and the sphere over which his commissioners could lawfully act. Here the Agrarian Law of 111 BC provides the most certain evidence. In its Italian section the measure contains a recurring phrase which can be restored as 'whatever

<sup>45</sup> See, for example, Livy 28.46.4–6; 31.13.1–9; 32.29.3–4; 34.53.1–2; 42.4.3–4; 42.19.1–2.

<sup>46</sup> See Gargola (1995, 56–8; 103–6; 115).

public land that was in *terra Italia* in the consulship of P. Mucius and L. Calpurnius, except for that land whose division was excluded or prohibited according to the law or plebiscite that C. Sempronius, the son of Tiberius, tribune of the plebs, proposed? (*quei ager publicus populi Romanei in terra Italia P. Muucio L. Calpurnii consulibus fuit, extra eum agrum, quei ager ex lege plebeie scito, quod C. Sempronius Ti. f. tr. pl. rogauit*). The first part of this long passage, with its reference to the consuls of 133 BC, is almost certainly derived from the law of 133 BC, where it would have defined its commissioners' range of lawful action. The exceptions and exclusions are another matter. The epigraphic law acknowledged only C. Gracchus' re-enactment of his brother's law. The Gracchan commissioners did exclude some lands from their activities:<sup>47</sup> most likely the *ager Campanus*, *ager censorius*, *ager quaestorius*, and *ager in trientabulis* (lands turned over to state creditors during the Second Punic War).<sup>48</sup> These triumvirs, however, apparently largely ceased their activities in 129, with the intervention of Scipio Aemilianus.<sup>49</sup> Some of these exclusions, then, may well have been in the original legislation.

Thus, in practice and possibly in law, Ti. Gracchus defined a very broad sphere of activity both abstractly (public land) and geographically (*terra Italia*) and then identified exceptions again either abstractly or geographically. The result would have been a residual category very much like Appian's. Elements of this definition certainly were older than the Gracchan reform. The term *terra Italia* is rooted in those legal and religious concepts that affected and defined the powers of magistracies—like *pomerium*, it is augural in nature—and from the late third century BC it was used to delimit territorially certain magisterial powers and functions.<sup>50</sup> Thus a range of rules designated *terra Italia* as a magistrate's sphere of legitimate activity, and as an officially defined space it served for generations to delimit a range of official actions. In

<sup>47</sup> Appian himself (*BC* 1.21.90) noted that Scipio Aemilianus' intervention in 129 BC largely ended the distributions.

<sup>48</sup> For the evidence and a bibliography see Crawford (1996, 1.157).

<sup>49</sup> See Stockton (1979, 89–93).

<sup>50</sup> For *terra Italia* as a concept see Catalano (1961–2; 1978); Crawford (1996, 1.156–7). Catalano notes that rules apparently prohibited the *pontifex maximus* from leaving Italy, prohibited magistrates from renewing their auspices outside the peninsula, and barred consuls from appointing dictators beyond *terra Italia*. In the law of 111 BC certain *duumviri*, presumably those in charge of roads (thus Crawford 1996, 1.167) were assigned the task of keeping unobstructed roads on public lands in *terra Italia*. The extortion law of 122 BC used *terra Italia* to denote the area over which praetors conducting investigations under the law could have agents search for witnesses; see Lintott (1992, 125).

his own law Ti. Gracchus modified this space, which was essentially geographic in nature, by restricting it to public lands within the broader sphere.

At this point I would like to return to the pre-Gracchan *lex de modo agrorum*. For Appian, this law and its Gracchan successor both regulated the same lands. For present purposes it is a matter of some significance whether this was in fact the case, or whether Appian's claim represents a retrojection of a feature of Ti. Gracchus' law into the more distant past. Our sources generally place the original law in the mid-fourth century BC, no source provides any later date for the passage of such a measure, and Appian himself seemingly accepted the identification (see below). No law enacted before the mid-third century BC could plausibly have proclaimed its rules to be in force over *terra Italia*. If a fourth-century BC *lex de modo agrorum* did set out its sphere abstractly and then noted exceptions, those exceptions could not have included the *ager Campanus* or *ager in trientabulis*, both the results of actions during the Second Punic War, and probably would not have included lands sold or leased, which may not, as forms, have been older than the era of the Punic Wars.<sup>51</sup> If no later measure fulfilled an intermediary role, then Ti. Gracchus certainly modified, perhaps substantially, this aspect of the law that provided his model, just as Appian would portray him as adding or adjusting other features of the original regulation. It is quite possible, then, that the Sempronian law was the first measure to assert such a broad field of application.

Some conclusions, then, seem reasonably clear. Public lands left over after assignments, sales, and leases, which Appian made the locus of the crisis and the focus of the attempts at reform, cannot be identified as a formal category in the works of jurists and surveyors. The *lex Sempronia* itself, however, did identify the lands over which the Gracchan commissioners could lawfully act in much the same manner. Ti. Gracchus' language serves to establish quite clearly that a fourth-century BC law could not have provided his model in just this feature. Appian, then, may well have projected into the past a sphere of operations that closely resembled Ti. Gracchus', giving to it an invented history.

Although later agrarian laws did not closely replicate the Sempronian law, Ti. Gracchus' definition—public lands in Italy in 133 BC—would

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<sup>51</sup> The earliest reasonably certain instance in which public lands were sold took place in Sabinum, probably not long after the conquest; see Muzzioli (1975).

persist in Roman legislation. L. Flavius repeated it in his abortive agrarian law of 60 BC, where it again defined the measure's sphere of application.<sup>52</sup> The proposed law of P. Servilius Rullus, put forward in 63 BC, sought to define the powers of the officials who would implement its land distributions by an explicit reference to the rights and powers of Ti. Gracchus' triumvirs.<sup>53</sup> Gracchus' law, in other words, remained a matter of interest to some members of Rome's elite, and its technicalities were relevant to some authors of legislation.

#### VI. *Appian's Narrative as a Chronological Sequence*

It is in Appian's narrative as a chronological series that the single most intractable problem is to be found. Appian set out his narrative as a sequence of developments in chronological order, with each successive development the clear result of what had taken place just before. He did not provide any clear chronological markers except at its end, the tribunate of Ti. Gracchus, but he did set out an apparently clear and straightforward relative chronology.

In the first of the four chronological stages of his narrative Appian focused on certain forms of exploitation—land assignments, sales and leases, and the opening of the excess to private use—that the Roman state deployed in succession in each area that it conquered. If one were to view the account as a straightforward sequence in time, then the opening phase should have a reasonably clear conclusion. The most straightforward solution would end the opening phase with the conquest of Italy itself, either around 270 BC, when Rome had subdued all of peninsular Italy, or after the Second Punic War, when Rome defeated its rebellious allies. Neither fits well with our evidence for Roman land distributions. The majority of known colonies were founded after 270 BC, while the first three decades of the second century BC formed one of the most intense periods of colonization. In any case, it is difficult to place chronologically the very beginning of the narrative, at least in part because it is constructed not of events but of long-established practices that varied in frequency over time but never really ended.

Once again Appian's depiction of the first law *de modo agrorum* is central. As the focus of the penultimate stage of the narrative, the law

<sup>52</sup> See Cic. *Att.* 1.19.4.

<sup>53</sup> Cic. *Agr.* 2.12.31.

should be rather late, perhaps not long before the Gracchan reform. But the only identifiable and datable law of this type—and quite possibly the only pre-Gracchan law *de modo agrorum*—is far too early to be accommodated easily into the narrative. A number of authors made the law part of the Licinian-Sextian Rogations of 367 BC, a major turning point in the Struggle of the Orders; one, Velleius Paterculus, explicitly claimed this measure served as the model for the Gracchan law.<sup>54</sup> The earliest mention of such a law, a passage from an oration that the Elder Cato delivered in 167 BC, does not identify the measure.<sup>55</sup> Aulus Gellius, who preserved the fragment, cited the passage because he wished to contest the way Cicero's learned freedman Tiro had interpreted it. In the process, Gellius revealed that Tiro thought the law in question to be Stolo's, and Gellius in no way indicated any disagreement on this point.<sup>56</sup>

Scattered references to prosecutions seem to confirm the existence of some early law, while contradicting Appian's assertion that the measure was quickly ignored or circumvented. In 298 BC Livy (10.13.14) recorded that occupiers were convicted and fined for 'holding more land than the law allowed'. Prosecutions *de numero pecoris* may have been more frequent: Livy, Festus, and Ovid record convictions of *pecuarii* in 295, 293, 241, 196 and 193 BC, and while they do not identify the nature of the offence—Ovid seems to think it was simply using public pastures at all—violations of rules limiting the size of herds is perhaps the most likely possibility.<sup>57</sup>

The *lex Licinia* has long been at the center of a vigorous scholarly debate, where Appian's claims about the pre-Gracchan law's context and purpose have proven central.<sup>58</sup> To many, Licinius' law is far too early to have provided Ti. Gracchus with his model or to have been intended as a remedy for the same ills that the later reformer would address. Plausible contexts for the law can be found in the contemporary expansion of Roman territory, the first stages of the Roman conquest of Italy, and in the conflicts over lands that are often endemic in small-scale agricultural societies.<sup>59</sup> But one should recall that Appian claimed that

<sup>54</sup> Liv. 6.35.4–42. 9; DH 14.12.22; Gel. 20. 1. 23; Plu. *Cam.* 39; Vell. 2. 6. 2–3; Varr. *R* 1. 2. 9; Plin. *Nat.* 18. 14. 17; V. Max. 8. 6. 3; *Vir. ill.* 20. 2–4; Col. 1.3.11.

<sup>55</sup> Cato fr. 167 *ORF* = Gel. 6.3.37.

<sup>56</sup> Thus, Forsén (1991, 51–5). The passage in question is Gel. 6.3.40.

<sup>57</sup> Liv. 10.23.13; 10.47.4; 33.42.10–11; 35.10.11–2; Fest. 276L; Ov. *Fast.* 5.283–94.

<sup>58</sup> For recent surveys of the debate see Forsén (1991); Oakley (1997, 654–9).

<sup>59</sup> Hermon (2002, 143–69) has sought to place the law in the context of a proposed

the law was intended to address more specific ills, such as the excessive spread of slavery and the associated decline in the population that remained eligible for military service—and the fourth century seems too early for these developments. The simplest solution, then, is to reject entirely reports of a fourth-century BC law as yet another manifestation of the well-known tendency of Roman historians to project into the distant past more recent issues.<sup>60</sup> Others have found the existence of some fourth-century BC measure quite plausible, often pointing to the series of prosecutions.<sup>61</sup> Acceptance of the law, however, often involves, if only implicitly, modifying or rejecting the context in which Appian placed the measure or the purpose that he assigned to it. One should also recall that some ancient authors placed a law *de modo agrorum* in the context of sumptuary legislation.

Some scholars suggest that a later *lex de modo agrorum*, one not clearly noted in our sources, was aimed at just those ills that Appian claimed, providing a precedent for the later *lex Sempronia* not only in their shared provisions but also in their common contexts and goals, just as Appian presented them.<sup>62</sup> Scholars who propose the existence of an otherwise unattested law must find a date for the measure, if only an approximate one. Some have proposed identifying the law with the *lex Flaminia agraria* of 232 BC.<sup>63</sup> The only known provisions of this law, however, authorized viridane assignments in the *ager Gallicus* and Picenum.<sup>64</sup> A much more common solution has been to propose the enactment of a law early in the second century BC. There is some slight evidence to support this dating. Cato in an oration delivered in 167 BC mentioned a rule *de modo agrorum* while making the point that only actions should be punished, not desires.<sup>65</sup> The use of such a law as an illustration should indicate, so the argument goes, that the measure was passed only recently; but all it really shows is that the law was known to be in force, for memory of such a law could have been kept alive by prosecutions. This late

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struggle between patricians and plebeians over access to land. The absence of any reported mechanism for distributing excess land to some proposed group of recipients seems to militate against any such solution.

<sup>60</sup> Niese (1888) has been central here; see the discussion by Rich in this volume.

<sup>61</sup> See e.g. Allen (1889); Soltau (1895); Sterckx (1969); De Martino (1975).

<sup>62</sup> See e.g. Tibiletti (1948; 1949; 1950).

<sup>63</sup> Thus, Beloch (1926, 343–4); Nap (1935, 107); Valvo (1977). Against this view see the strong arguments of Gabba (1979).

<sup>64</sup> Plb. 2.21.7–9; Cato *Orig.* fr.2.14 Chassignet (= fr. 43 Peter = Var. R 1.2.7); Cic. *Sen.* 4.11; *Brut.* 14.57; V. Max. 5.4.5.

<sup>65</sup> Cato fr. 167 *ORF* = Gel. 6.3.37.

dating involves new difficulties. As we have seen, Tiro, Gellius' source, thought Cato's law was also Licinius'. B. Forsén quite properly notes that Tiro's view shows that memory of a second-century BC law, if indeed there was one, had disappeared by the late first century BC even among the learned, and he suggests that supporters of a later law must explain this development.<sup>66</sup>

For present purposes, however, the actual history of *leges de modo agrorum* is less important than their representation in historical works. Indeed, much of the debate surrounding the pre-Gracchan measure derives from the way that Appian depicted the law and its purpose, although claims about context and purpose are among the most dubious that he makes. While some scholars doubt whether an early law could have served as Ti. Gracchus' model, or question whether such a measure could have been a response to the same ills that Gracchus later faced, it is a matter of some significance that Greek and Latin historians found no obstacles to accepting a fourth-century BC law in these roles.

The tendency among Greek and Roman historians to project issues, actions, and contexts into the distant past is well known, and the practice has shaped depictions of the *lex Licinia*. Velleius Paterculus (2.6.3) thought that the Licinian Law first set the limits to the size of holdings that the Gracchi would later include in their own legislation. The Elder Pliny (*Nat.* 18.14.17) held that the measure was directed against estates that had been put together by forcible expansion at the expense of neighbors. Appian too seems to have had the Licinian law in mind. He attributed the measure to 'certain tribunes' and some sources identify two authors for the fourth-century BC law: C. Licinius Stolo and L. Sextius Lateranus. Appian, moreover, claimed that fictitious transfers of excess land to relatives were one of the ways that the rich had circumvented the law. In some accounts Stolo was one of his law's first victims when he transferred land to a son emancipated for the purpose.<sup>67</sup> Other authors assumed the long-term stability of the political context, of the persistence of roles and of situations. When recounting the strife around the Licinian-Sextian reform, Livy (6.35.6–7) reported that the patricians persuaded other tribunes to block the reading of the laws,

<sup>66</sup> For this important point see Forsén (1991, 53–4).

<sup>67</sup> Liv. 7.16.9; DH 14.12; Col. 1.3.11; V. Max. 8.6.3; Plin. *Nat.* 18.17; Plu. *Cam.* 39.5.

in much the same way that M. Octavius later would forbid the public recital of Ti. Gracchus' law.<sup>68</sup> Others presented the authors of the first law in the same role that Ti. Gracchus later would occupy: a seditious tribune.<sup>69</sup> Roman historians' sense of chronology was not a modern one, and Roman historical culture displays a high tolerance for both invention and anachronism.

Despite appearances, then, Appian's narrative does not set out a true chronological sequence. His first stage, which sets out traditional practices supposedly superseded by the estate-building of the rich, never clearly ends. The law that forms the center of the penultimate stage may well have been enacted at the beginning of the process of Roman expansion over Italy. Appian gave to these disparate measures and practices the appearance of a sequence in time by the roles that he assigned to them in his account of the origins of the crisis.

## VII. *Conclusion*

Up to this point the discussion has been technical, and it is perhaps desirable to state its conclusions more broadly. Appian set out a well-defined course of social and economic developments, he identified processes and motives that appear realistic, and he gave to developments consequences that follow naturally from the history he had just recounted. Except for a few of the details, the central issue is not the invention of the major elements, which can be paralleled or confirmed elsewhere, but rather the way that Appian assembled them into a unified history.

On closer examination Appian's account is riddled with anachronisms, for he read into the past the Gracchan reform, its context, its goals, and its legal categories. His depiction of the origins of a crisis and his history of the exploitation of captured lands embedded within it provides clear precedents for the Sempronian law and its most unusual features. Appian gave to all official activities on captured lands one goal and one context, obscuring the differing ends reached by various processes and making the overall administration of captured lands seem simpler, more unified, and more regular than it was. At the same time, he set out the agrarian crisis in a markedly legalistic manner and

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<sup>68</sup> On the anachronistic aspects of Livy's description of the tribunician intercession see Oakley (1997, 670–1).

<sup>69</sup> See DH 14.12.22; Plu. *Cam.* 39.

in ways that fit quite closely the provisions of the Gracchan law. The result is a depiction of an unprecedented reform that makes it appear both necessary and deeply embedded in traditional practices.

The pre-Gracchan *lex de modo agrorum* has perhaps suffered the greatest distortion. Appian assigned to this measure a different context, purpose, and sphere of operation, making it in virtually every way the model for the Sempronian law. He also assigned to it, if only implicitly, a chronological position that does not fit other evidence. Indeed, Appian's narrative, as a chronological sequence, is constructed largely of contemporary forms; the *lex Licinia agraria*, after all, was enacted at the very beginning of Roman expansion. His depiction of the law has created persistent scholarly controversy over its date and purpose.

The final result is an account that serves very imperfectly as evidence for Roman practices governing public lands and for social and economic conditions in second-century BC Italy, the use to which it has most frequently been put. Here the problem lies in the rather systematic fashion in which Appian has shaped practices and processes to fit a scheme. While the narrative may serve quite well as evidence for Roman perceptions of conditions in the countryside, it serves much less well as evidence for the actual conditions, their origins, and their consequences.

One final point should be made. Throughout this essay, Appian has been treated as the author of the passage in question. Like all historians, however, Appian was dependent on his sources. The similarities between Appian's account of an agrarian crisis and Plutarch's (*TG* 8) show quite clearly the existence of a common source, which almost certainly described developments on the land and connected them to Ti. Gracchus' reform as Appian and Plutarch did. The degree to which Appian modified or distorted this underlying source is an important question, but probably an unanswerable one. The partisan nature of his account and its concern for legal forms may well indicate the presence of another agenda. If so, then Appian's history may well preserve another example of the polemics that long surrounded the reform.

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LEX LICINIA, LEX SEMPRONIA:  
B.G. NIEBUHR AND THE LIMITATION OF  
LANDHOLDING IN THE ROMAN REPUBLIC

John Rich

I. *Introduction*

The agrarian law passed by Tiberius Sempronius Gracchus as tribune of the plebs in 133 BC brought back into force a provision of an earlier law. That law had imposed a limit of 500 *iugera* (about 126 hectares) on landholding, on pain of a prosecution by the aediles and a fine if convicted, but over time the limit had ceased to be observed. Gracchus' law provided for the enforcement of the 500 *iugera* limit, with an additional allowance for children, and the establishment of a three-man commission to distribute the excess land recovered in allotments to the poor. So much at least is universally accepted. This relationship between Gracchus' law and the earlier law limiting landholding is asserted by both Appian (*BC* 1.8.33, 9.37) and Plutarch (*TG* 8.2, 9.2). Appian reports two further provisions of the earlier law: pasture rights were restricted to 100 larger and 500 smaller beasts, and the workforce was to include a minimum number of free men who were to report on what was going on. He also states that an oath was sworn to observe the law (*BC* 1.8.34).

It is generally assumed in modern scholarship that both the pre-Gracchan and the Gracchan limits did not affect private land and applied exclusively to the occupation (*possessio*) of public land (*ager publicus*). On this view the earlier law sought to limit the amount of *ager publicus* a man could occupy, and Gracchus' law sought to reclaim for distribution *ager publicus* held in excess of the limit.

The main point of controversy in respect of the earlier law has been its date. Cato's reference to the law in a fragment of his speech for the Rhodians, delivered in 167 BC, gives us a *terminus ante quem*. Many sources refer to a law establishing a 500 *iugera* limit passed by the tribune C. Licinius Stolo, and Livy names L. Sextius as co-author of the law and represents it as part of an anti-patrician package of laws passed after a long struggle in 367 BC. The identification of Licinius

Stolo's law as the one mentioned by Cato, Appian, and Plutarch and enforced by Tiberius Gracchus was first called into question by Niese (1888). Niese observed that Appian's account does not suggest such a long interval between the two laws and implies that the first law was passed after the conquest of Italy, and he argued that not enough *ager publicus* will have been available for occupation in the early fourth century BC for holdings of 500 *iugera* to be feasible at that date. He proposed instead a dating in the late third or early second century BC for the law introducing the 500-*iugera* limit and rejected the tradition of a fourth-century BC Licinian law as fictitious.

Niese's article started a debate which has continued to this day.<sup>1</sup> Niese's sceptical position, rejecting a fourth-century BC law outright, was at first widely followed, but in recent years has rightly found comparatively little support.<sup>2</sup> A crucial difficulty for this view is Livy's report (10.13.14) of successful prosecutions by the aediles in 298 BC of men who 'occupied more land than was permitted by law'. This passage is a bald notice of an isolated administrative event, which appears likely to derive ultimately from an archival source, and there is no reason to doubt its authenticity. Against Niese, traditionalists have continued to hold that the 500 *iugera* limit was introduced around 367 BC, and this view retains strong support.<sup>3</sup> These scholars regard Appian's account as too vague to constitute strong evidence for a later date, and insist that enough *ager publicus* would have been available for occupation in the early fourth century BC to make it possible for some individuals to amass holdings in excess of 500 *iugera*. In this regard they rely particularly on the confiscation of the territory of Veii after its capture, traditionally dated to 396 BC: much of this was distributed to individual Romans, but, they argue, a substantial portion must have remained public land. Other scholars opt for a compromise position, holding that a limit on holdings of *ager publicus* was introduced by a fourth-century BC Licin-

<sup>1</sup> For a good summary of the controversy see Forsén (1991, 13–28).

<sup>2</sup> Early followers of Niese include Pais (1899, 141–3); Maschke (1906, 52–67); De Sanctis (1907, 216–7); Fraccaro (1914, 71–4 n. 2); further bibliography at Forsén (1991, 15–18). For more recent support see Von Fritz (1950: 28–9); Meyer (1975, 286–7); Flach (1990, 32–3; 1994, 285–94).

<sup>3</sup> Early defenders of a date *c.* 367 BC for the 500 *iugera* limit include Allen (1889, 5–6); Cardinali (1912, 139–53); Vančura (1924, 1164–8); Last (1928, 538–40); Frank (1933, 27–8); further bibliography at Forsén (1991, 18–22). For more recent support see especially Burdese (1952, 52–70); Brunt (1971, 28–29 n. 5); De Martino (1979a, 183–93; 1979b, 25–9); Cornell (1989, 328–9; 1995, 328–9); Gargola (1995, 129–46); Serrao (1999, 160–5); Hermon (2001, 143–70); Forsythe (2005, 265).

ian law, but raised to 500 *iugera* only in the late third or early second century BC, when the limits on pasturage as reported by Appian were also introduced. This view, already outlined by Beloch, was most fully developed in an influential study by Tibiletti.<sup>4</sup>

All parties to this chronological controversy agree on one point which in fact forms the basis for the whole dispute, namely the assumption that both the pre-Gracchan and the Gracchan limits applied only to holdings of *ager publicus*. It has come to be generally overlooked that this claim is itself merely a modern hypothesis which was championed in the early nineteenth century by Niebuhr in his epoch-making *Römische Geschichte* and as a result came during the nineteenth century to win general acceptance. In this paper I shall be arguing that for the pre-Gracchan limit Niebuhr was wrong and has led most of the subsequent discussion astray. The principal basis for this contention is that Niebuhr's doctrine flies in the face of the bulk of the evidence. The Niebuhrian orthodoxy has been challenged in two recent but brief contributions, by Kunkel and Rathbone.<sup>5</sup> The matter is of such importance that a fuller discussion is required, which will examine the history of the question and seek to explore its implications for the history of both the fourth century BC and the Gracchan period.

## II. *Niebuhr's Doctrine in Context*

Barthold Georg Niebuhr (b. 1776) was of German family, but spent his childhood and youth in Danish territory.<sup>6</sup> His father Carsten won

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<sup>4</sup> Beloch (1926, 343–4); Tibiletti (1948; 1949). Scholars who follow Tibiletti or remain undecided between his view and the traditionalist position include Toynbee (1965, 554–61); Gabba (1967, 20–21); Sterckx (1969); Stockton (1979, 208–11); Neumann (1987); Forsén (1991); Oakley (1997, 654–9). Many of those who accept a fourth-century BC dating for the 500 *iugera* limit suppose that the limits on pasturage reported by Appian were introduced later, and a number of scholars hold that the law described by Appian was measure, which revived the 500 *iugera* limit first introduced in the fourth century an early-second-century BC and added the pasturage restrictions (a view first proposed by Soltau 1895).

<sup>5</sup> Kunkel (1995, 493–7); Rathbone (2003). Elster (2003, 365–7) notices Kunkel's view favourably.

<sup>6</sup> The primary sources for Niebuhr's life are the biography by his sister-in-law Dore Hensler (Hensler 1838–9), translated into English in an abridged and adapted form by Susanna Winkworth (Winkworth 1852), and his letters (Niebuhr 1926–29, 1981–84). Walther (1993) is a detailed intellectual biography; earlier studies include Rytkönen (1968); Witte (1979); Wirth ed. (1984). Heuss (1981) is fundamental for the aspects of Niebuhr and his work considered in the present paper. For assessments of Niebuhr's

fame through his journey of oriental exploration and then spent the rest of his career as provincial secretary for Dithmarsch in the Holstein region. Although he had aspired from an early age to write great historical works, Barthold opted for a career in the Danish civil service, assisted by the patronage of the reformist Finance Minister, Count Schimmelmann. After periods as Schimmelmann's private secretary (1796–97) and studying in London and Edinburgh (1798–99), he held a series of financial posts. In summer 1806 he transferred to high office in Prussia, just at the moment when King Frederick William III unwisely decided on war with France, and, only days after his arrival in October, Napoleon's crushing victory at Jena obliged Niebuhr to join the Prussian government in flight. He was, however, able to contribute to the reform measures by which Stein and Hardenberg sought to revive Prussia, both before and after the Peace of Tilsit (1807). In 1810 a sharp disagreement with Hardenberg led to Niebuhr's withdrawing from public service.

The years 1810–12 were the highpoint of Niebuhr's creative life. He had been appointed to the titular post of Historiographer Royal and elected to the Berlin Academy. The latter distinction entitled him to lecture at the newly founded University of Berlin, and he proceeded to give courses of lectures on Roman history to rapturous acclaim. Having been previously somewhat isolated from the scholarly world, he now formed notable academic friendships with, among others, the great jurist Savigny. Niebuhr's original aim was to continue his lectures on Roman history down to the end of antiquity. By the end of the second year's course (winter 1811–12), the lectures had reached 241 BC. During this period he also reworked the lectures for publication as his *Römische Geschichte* (hereafter, *RG*). Two volumes appeared successively in 1811 and 1812, taking the story down to 338 BC.<sup>7</sup>

The extraordinary novelty of Niebuhr's achievement was immediately recognized and was to have a lasting impact on the writing of Roman history. His principal contribution is often thought to have been a new depth of critical engagement with the sources. But perhaps

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career and historical achievement see also Christ (1972, 26–49); Momigliano (1994, 225–36); Smith (2006, 81–5); Nippel (2007, 207–9). The composition history of the *Römische Geschichte* is well analysed by Heuss (1981, 95–111); Walther (1993, 298–308, 556–9).

<sup>7</sup> Niebuhr (1811–12). English translation by F.A. Walter: Niebuhr (1827).

more important was the way in which he brought his wide knowledge of history and contemporary societies to bear as a comparative tool, enabling him to pose new questions and to intuit new constructions of the early Roman past. This intuitive boldness in reconstructing early Roman history and society was to be a very influential model, whose impact can be seen in the works of such later scholars as Mommsen, Pais, and Beloch. It was a powerful tool, but, deployed with Niebuhr's characteristic arbitrariness, it also had great potential to lead astray.<sup>8</sup>

Napoleon's debacle in Russia in 1812 had an unfortunate consequence for Roman historical studies: it tempted Niebuhr back to public life and distracted him from the composition of his history. From 1816 to 1823 he was Prussian Ambassador in Rome. Finally in 1825 he settled at Bonn and resumed university lecturing, and it was only then that he felt able to return to the *RG*. By this time he had amassed much additional material and had become much better acquainted with his scholarly predecessors, and so before continuing the work he produced a radically modified edition of the first two volumes, which now reached only 380 BC. Niebuhr had never been the most lucid of writers, and the accumulation of detail in this version (nearly twice as large as the first edition) sometimes makes the argument hard to follow. At his death in 1831 this was all that had appeared, and the posthumous third volume presents a partial treatment of the period down to 241 BC compiled by J. Classen from Niebuhr's papers.<sup>9</sup> Further posthumous publications included two versions of Niebuhr's Bonn lectures on the history of Rome down to the fall of the western empire, reconstructed from the extempore and reportedly not very coherent originals on the basis of students' notes by respectively M. Isler (Niebuhr 1846–48) and L. Schmitz (Niebuhr 1870, published in English).

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<sup>8</sup> Momigliano (1952) gives an excellent assessment of Niebuhr's method and its shortcomings, and of its impact in Germany and England. On the reception of Niebuhr see also Heuss (1981, 11–33); Walther (1993, 573–83). For Mommsen's ambivalent attitude to Niebuhr see Heuss (1968).

<sup>9</sup> Niebuhr (1828–32) (the second edition of Volume I was published in 1827, and a third edition, with some further revisions, in 1828; the second edition of Volume II was published in 1830). This version of the *RG* was translated into English by J.C. Hare and C. Thirlwall (Vols. I–II) and W. Smith and L. Schmitz (Vol. III); this translation is cited here from the 1851 edition.

It was a special interest in the problems of the Roman public lands which first led Niebuhr to focus on Roman history, and this aspect of his early scholarly career has received a good deal of attention, above all in a monumentally erudite monograph by Heuss.<sup>10</sup> In Copenhagen in 1803–06 he undertook a major study of the topic, and two extended manuscript treatments survive from this period, which have been partially published by Heuss.<sup>11</sup> Ms. A, composed in 1803–04, comprises a discussion of issues relating to *ager publicus* and Roman land settlement, followed by a historical narrative of Roman land settlements and a conclusion relating the theme to land issues of Niebuhr's own day, particularly Ireland.<sup>12</sup> A statement in the conclusion reveals that Niebuhr intended this treatise to form part of a larger general work on land law: its title, modelled on Montesquieu's celebrated *De l'esprit des lois*, was to be *Geist der Gesetze des Landeigentums*, and he had been encouraged to undertake the project during his visit to Scotland by James Grant, an expert on Indian land tenure.<sup>13</sup> Ms. B, composed in 1805–06, presents a new treatment of the general issues of *ager publicus* and land settlement, without a following narrative, and shows some development in Niebuhr's thinking.<sup>14</sup>

Niebuhr incorporated a good deal of this material into the *RG*, but evidently felt some difficulty about how to work it in. In the first edition he introduced it as an extended excursus in the course of his discussion of the Licinio-Sextian Rogations.<sup>15</sup> In the revised edition he inserted the (somewhat abridged) excursus at an earlier point, à propos of the agrarian proposal of Sp. Cassius (traditional date 486 BC), with a mere

<sup>10</sup> Heuss (1981). See also Rytkönen (1968, 48–142); Momigliano (1984; 1994); Whitman (1990, 154–8); Walther (1993, 152–216).

<sup>11</sup> Heuss (1981, 34–46) gives detailed information on the manuscripts and their date of composition.

<sup>12</sup> Heuss (1981, 501–5, 530–51) reproduces in full from Ms A the introductory section, the account of Tiberius Gracchus, and the conclusion (his 1A, 10A and 11A), and he gives a summary and analysis of the remainder with short extracts (46–79).

<sup>13</sup> See Heuss (1981, 551). On Niebuhr's relations with Grant see Momigliano (1984; 1994, 233–4); Walther (1993, 124, 152). As Momigliano showed, the (controversial) interpretation of the Indian *zemindar* system which Niebuhr felt helped him to understand *ager publicus* (Niebuhr 1828–32, II.151–2 = 1851, II.135) was drawn from Grant.

<sup>14</sup> Heuss (1981, 505–12) reproduces the introductory section of Ms B in full (his 2A), and he gives summary, analysis, and extracts from the rest (79–90). He also reports on related fragments from the same period (90–3), publishing some in full (512–30, his 3A–9A).

<sup>15</sup> Niebuhr (1811–2, II.349–94).

reference back in his account of the Licinio-Sextian Rogations.<sup>16</sup> The same arrangement was adopted in his Bonn lectures.<sup>17</sup>

Niebuhr's interest in the Roman public lands was thus part of a broader concern with land issues throughout history and in his own day across various societies from Ireland to India. It was also informed by contemporary concerns, and especially the movement for serf emancipation (*Bauernbefreiung*). The serfs had been freed in Denmark itself in 1788, but it was only in 1796 that emancipation was proposed for Niebuhr's home region of Holstein, which was in the personal rule of the Danish king as its duke. The Holstein aristocracy put up protracted resistance, and it was not until 1804 that the government succeeded in carrying the reform through. Although his primary government responsibilities lay elsewhere, Niebuhr had strong feelings on this issue and made a contribution to policy formation. He was highly critical of the aristocratic opposition, and believed strongly both in the emancipation and in the importance of ensuring that the emancipated serfs could prosper as farmers. In Prussia in 1807 he took a similar stance in favour of the October Edict proclaiming the emancipation of serfs throughout the kingdom. Thus for his own time Niebuhr was hostile to aristocratic landowners guarding their privileges and a strong believer in the importance for all states of a numerous and prosperous peasantry. These beliefs underpinned his conception of Roman agrarian issues, and also help to account for his hostility to the patricians and sympathy for the *plebs*, whom he conceived as originally a different population group from the patricians and their clients.<sup>18</sup>

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<sup>16</sup> Excursus: Niebuhr (1828–32, II.146–76 = 1851, II.130–55). The Licinio-Sextian Rogations: Niebuhr (1828–32, III.1–36 = 1851, III.1–30; although posthumously published, this section was revised by Niebuhr before his death). Part of the earlier excursus, dealing with the classification of landed property and with *limitatio*, now became an appendix (Niebuhr 1828–32, II.694–710 = 1851, II.620–33). The first edition also included an appendix on the *agrimensores* (Niebuhr 1812, 532–62), omitted in the revised edition, but reprinted in Niebuhr 1843, 81–107 (translated selections included as a further appendix in Niebuhr 1851, II.634–44).

<sup>17</sup> Niebuhr (1846–8, I.251–9 = 1870, 151–5).

<sup>18</sup> For Niebuhr's involvement in and attitude to serf emancipation and his belief in the importance of a strong and numerous peasantry see especially Heuss (1981, 39–47, 396–432); Degn (1984). On Niebuhr and the developments in Holstein see also Walther (1993, 130–51). Niebuhr's conception of the Roman plebs in the *RG* is succinctly analysed by Richard (1978, 7–11) and Heuss (1981, 125–30). In his Bonn lectures Niebuhr remarked on the connection between his *ager publicus* studies and his concern with the Holstein serfs (Niebuhr 1846–48, I.253–4 = 1870, 153), but his memory played him false when he claimed that it was the fate of the serfs after

The two Copenhagen manuscripts and the agrarian excursuses in the two editions of the *RG* and in the Bonn lectures all start by referring to the erroneous view that tribunician agrarian laws (*Ackergesetze*) were an attack on private landed property (*Landeigentum*).<sup>19</sup> In all of these accounts except the relatively brief version in the first edition of the *RG*, Niebuhr, who had been consistently hostile to the French Revolution, refers scathingly to extremists during the Revolution who called for agrarian laws and envisaged them as the equal partition of landed property.<sup>20</sup> In every version he singles out for criticism Machiavelli and Montesquieu as great writers on Rome who had interpreted the agrarian laws as applying to private landed property and yet, as he somewhat unfairly claims, not condemned them. In the second edition of the *RG*, displaying his now more extensive erudition, he adduces further authorities:

This misconception is as old as the revival of philology. Neither Sigonius nor Manutius doubted that the tribunes had limited landed property to 500 jugers, and had assigned the excess to the poorer citizens: nor had Beaufort any other notion, nor Hooke; though they all had before their eyes the reference to the conquered lands, which the Greek historians insist on as so essential a point. They only mention this by way of explaining how such vast estates could have arisen. That there was a kind of landed property to which no limit had been set, they had no conception. Yet every one of them must have been aware that there was a riddle to be solved here: but they tacitly gave it up. Ferguson on the other hand never perceived there was one.<sup>21</sup>

Thus, when, near the end of his life, Niebuhr composed this passage, he had come to think that until the French Revolution there had been

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emancipation which drew him to the topic, since his research was in fact well under way before the emancipation in 1804 (cf. Heuss 1981, 396–8).

<sup>19</sup> Copenhagen MSS., cited in Heuss (1981, 501–12); Niebuhr (1811–12, II.349–50); Niebuhr (1828–32, II.146–9 = 1851, 130–2; 1846–8, 252–3 = 1870, 152).

<sup>20</sup> On Niebuhr's attitude to the French Revolution see Rytkönen (1968, 31–35); Heuss (1981, 153–4, 337–8, 361); Walther (1993, 27–41).

<sup>21</sup> Niebuhr (1828–32, II.147 = 1851, II.131) (Hare and Thirlwall's translation, but I have made one correction). The only one of the writers named here to be mentioned also in the Copenhagen MSS. is Ferguson (*op. cit.* Heuss 1981, 511). Niebuhr, however, does there refer to one other authority, namely W. Goes, whose edition of the Agrimensores was then still the best available, complaining that his reconstruction of the Licinian law did not specify restriction to public land (Goes 1674, 348–9; Heuss 1981, 71–2, 241–4). On Niebuhr's limited knowledge of his predecessors when composing the first edition of the *RG* and his amends in the second see Momigliano (1957); Heuss (1981, 113–5); Walther (1993, 308–14).

a general consensus that the agrarian laws, and with them the Licinian and Gracchan limits, applied to private property. As we shall see below, he went on to claim that Heyne in 1793 was the first to challenge this doctrine and show that the laws applied only to *ager publicus*. This account of his predecessors' views has been widely believed, for example by Momigliano,<sup>22</sup> but it is in fact seriously misleading.<sup>23</sup> We must therefore devote some time to considering earlier writers' treatment of the agrarian laws and in particular the Licinian and Gracchan limits on landholding.

Equality of landed property as a political ideal, a notion which can be traced back to Plato's *Laws* (5.737–41), occurs in the works of numerous thinkers from the sixteenth to the eighteenth centuries, from utopian writers like More and Harrington to Enlightenment figures, particularly in France, such as Montesquieu and Mably.<sup>24</sup> Lycurgus' Sparta and Romulus' Rome provided the two ancient models of primitive states realizing this ideal: for Rome, the evidence was provided by the antiquarian tradition that Romulus allocated land to his new citizens in lots of two *iugera* (a *heredium*), although this was in conflict with other authorities' claim that the unequal division between patricians and plebeians went back to the foundation.<sup>25</sup> A rather loose conception of the 'agrarian law' came to be developed, as aiming to establish or re-establish equality or at least limit inequality.

Niebuhr focused on two such writers, Machiavelli and Montesquieu. This emphasis is not surprising, for Machiavelli's *Discorsi sopra la prima deca di Tito Livio* (published posthumously in 1531) and Montesquieu's *Considérations sur les causes de la grandeur des Romains et de leur décadence* (published 1734) were the two most notable discussions of Roman history by major political thinkers which had yet appeared. Niebuhr had given these works, and their errors, similar prominence in the preface to the first edition of the *RG*.

<sup>22</sup> Momigliano (1994, 231–2).

<sup>23</sup> So rightly Ridley (2000). However, Ridley's claim that writers before the late eighteenth century generally took the agrarian laws and the Licinian and Sempronian limits to apply to public land is also an over-simplification.

<sup>24</sup> Heuss (1981, 188–233, 269–308) provides a good overview.

<sup>25</sup> Two *iugera*: Var. *R* 1.10.2; Plin. *Nat.* 18.7. Romulus establishing patricians and plebeians: Cic. *Rep.* 2.33; Dion. Hal. 2.8; Plu. *Rom.* 13. (Dionysius did, however, claim that Romulus divided conquered land equally: 2.28.3). For modern views of the two *iugera* tradition see Gabba (1978); Crawford (1985, 24); Momigliano (1994, 225 ff.).

Machiavelli (*Discorsi* I.37) certainly supposed that the legal limit applied to all landholding. His ‘agrarian law’ seems to be a composite of the Cassian and Licinian laws:

This law had two provisions: the one provided that no citizen could possess more than so many *iugera* of land; the other provided that lands taken from the enemy were to be divided among the Roman people.<sup>26</sup>

Machiavelli approved the law in principle, since “well-organized republics have to keep the public treasury rich but their citizens poor”, but held that through various defects it had always led to conflict and eventually was the cause of the destruction of the Republic. He also criticized the Gracchi for introducing retrospective legislation. However, he qualified these negative judgements by reverting to his earlier view (I.4) that the conflict between the plebeians and patricians had served to keep Rome free for much longer than would otherwise have been the case.<sup>27</sup> This nuanced assessment hardly amounted to the approval of “the sacrifice of all private rights to the hope of good for the community” which Niebuhr imputed to him and to Montesquieu.<sup>28</sup>

In a brief chapter of the *Considerations* (ch. 3) Montesquieu remarks that the ancient republics were able to achieve a much higher rate of military participation than the monarchs of his own day, because their founders (the reference must be to Lycurgus and Romulus) had divided the land equally and by so doing had produced a powerful people and a good and very large army. In the case of Rome, the equal distribution of land made possible the growth of their power, which was later undermined by the increase in wealth. The chapter closes with words put into the mouth of Tiberius Gracchus:

“Tell me,” said Tiberius Gracchus to the nobles, “who is worth more: a citizen or a perpetual slave; a soldier, or a man useless for war? In order to have a few more acres of land than other citizens, do you wish to renounce the hope of conquering the rest of the world, or to place yourself in danger of seeing those lands you refuse us snatched away by enemies?”<sup>29</sup>

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<sup>26</sup> “Aveva questa legge due capi principali. Per l’uno si disponeva che non si potesse possedere per alcuno cittadino più che tanti iugeri di terra; per l’altro, che i campi di che si privavano i nimici, si dividessero intra il popolo romano”. (Machiavelli 1984, 140; translation, Machiavelli 2003, 100.)

<sup>27</sup> As Lintott (1999, 242–3) notes, the passage shows awareness of Appian, available to Machiavelli in Latin translation.

<sup>28</sup> Niebuhr (1828–32, II.147–8 = 1851, II.131).

<sup>29</sup> ““Dites-moi”, disait Tiberius Gracchus aux nobles, “qui vaut mieux, un citoyen

In what is otherwise a paraphrase of Appian (*BC* 1.11), Montesquieu has introduced a reference to unequal holdings, apparently indicating that he took Gracchus to be seeking to restore the primitive equality.

The theme recurs in Montesquieu's masterpiece, *De l'esprit des lois* (first published 1748). Here he asserts the desirability of equal partition of land, as enacted by Lycurgus and Romulus, in a newly founded state (V.5), and he even maintains that laws for a repartition of land are salutary in their nature, and dangerous only if implemented suddenly (VII.2).<sup>30</sup> However, elsewhere he concedes that equal partition of land is not always practicable and may be dangerous (VII.2), and defends property rights, citing Cicero's condemnation of agrarian laws with approval (XXVI.15).<sup>31</sup>

The antiquarian and historical writers of the sixteenth to eighteenth centuries who refer to the agrarian laws and the Licinian and Gracchan limits present a much less uniform account than Niebuhr suggests. To a considerable extent this diversity results from the contradictions in our sources, to be examined in the next section. In respect of the agrarian law of Sp. Cassius and the extended agitation which the ancient tradition reported as its sequel, these writers are in general agreement that the law sought to reclaim and distribute what was held to be public land occupied by patricians. This agreement is natural enough, since this is what Livy and Dionysius clearly state. However, on the application of the Licinian and Gracchan limits there is much inconsistency between their versions and sometimes within individual writers' works. This can be observed both in the writers cited by Niebuhr himself and in other comparable writers of the same period.<sup>32</sup>

The earliest of the works cited by Niebuhr is the treatise on Roman laws by Paolo Manuzio (Paulus Manutius), first published in 1557. Several other writers composed works of a similar kind about the same

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ou un esclave perpétuel, un soldat ou un homme inutile à la guerre? Voulez-vous, pour avoir quelques arpents de terre plus que les autres citoyens, renoncer à l'espérance de la conquête du reste du monde, ou vous mettre en danger de vous voir enlever par les ennemis ces terres que vous nous refusez?" (Montesquieu 1925, 28; translation, Montesquieu 1965, 41).

<sup>30</sup> Montesquieu (1768, I.197): "Les lois du nouveau partage des champs, demandées avec tant d'instance dans quelques républiques, étaient salutaires par leur nature. Elles ne sont dangereuses que comme action subite."

<sup>31</sup> Similarly, V.8 on moderating excessive aristocratic wealth: "il faut des dispositions sages et insensibles; non pas des confiscations, des lois agraires, des abolitions de dettes, qui font des maux infinis" (Montesquieu 1768, I.109).

<sup>32</sup> See the surveys of Heuss (1981, 234–68) and Ridley (2000) (minimizing the inconsistencies).

time, namely Antonio Agustín, J.U. Zasius, and François Hotman.<sup>33</sup> All these works report the law of Licinius Stolo, in terms corresponding to those of Livy and other sources, as limiting to 500 *iugera* the amount of land a man might ‘have’ (*habere*) or ‘occupy’ (*possidere*), and then add the further provisions on pasture attributed by Appian to the pre-Gracchan law. Agustín, Zasius, and Manuzio represent Tiberius Gracchus as re-enacting the Licinian limit with a concession for sons. However, Hotman, influenced by the wording of the Periocha of Livy Book 58, states Gracchus’ law as providing that “no one should occupy more than 500 *iugera* from public land” (*ne quis ex publico agro plus quingenta iugera possideret*).<sup>34</sup>

Manuzio’s treatise was originally planned as part of a larger work dealing with all the political institutions of the Roman Republic,<sup>35</sup> and such a project was in fact accomplished by his friend, the great Carlo Sigonio, in the studies which he eventually brought together as *De antiquo iure populi Romani libri undecim*.<sup>36</sup> Sigonio twice refers to the Roman laws limiting landholding in this work, in a discussion of *uetigalia*, and in a section on colonies and other land assignments.<sup>37</sup> His accounts of the laws’ provisions are similar to those of the treatises just considered. In both sections he gives the terms of the Licinian law in virtually identical language, reporting the land provision in the precise words used by Livy (6.35.5), but inserting Appian’s pasture provisions: *ne quis plus quingenta iugera agri, centum pecoris maioris capita, quingenta minoris possideret*. In each account Sigonio represents Tiberius Gracchus’ law as renewing the Licinian land provision. However, whereas in the second passage the phrasing is exactly as for the Licinian law, in the first, the additional phrase “from public land” appears, drawn from the Periocha, and Sigonio there states the Gracchan prohibition in exactly the same words as Hotman. However, Sigonio’s earlier statements about

<sup>33</sup> Agustín (1583), Zasius (1555), Hotman (1558). The bulk of Agustín’s work was composed in 1544–5; it was revised up till 1557, but not published until 1583. Zasius’ work was first published 1551, and the 1555 reissue includes additions by Loys Le Caron (Charondas). On Agustín’s treatise and its relation to the other works see Ferrary (1993). Earlier works on Roman laws are discussed by Ferrary (1995).

<sup>34</sup> Agustín (1583, 147–8, 188–9); Zasius (1555, 11, 15); Manuzio (1557, 27); Hotman (1558, 49, 81). Hotman has chosen to disregard (or emend) the Periocha’s discrepant figure for the limit.

<sup>35</sup> Ferrary (1997).

<sup>36</sup> On Sigonio and his achievements see McCuaig (1989).

<sup>37</sup> *De antiquo iure civium Romanorum* I.16: Sigonio (1574, 79–80), and *De antiquo iure Italiae* II.2: Sigonio (1574, 217–20).

*ager publicus* in both sections show that he in fact (like Niebuhr himself) interpreted both laws as applying not to all landholding, but just to *ager publicus*. This emerges most clearly from the section on land allocations. Here he tells us that the land used for allotments was always public, but could be of various types. One such type was land which was “public indeed, but stealthily usurped by powerful private citizens” (*publicum quidem, sed furtim a privatis potentibus usurpatum*), and it was proposals for distributing land of this kind which led to opposition. Such a distribution, he tells us, was first proposed by Sp. Cassius and finally, after many struggles, carried into effect by Licinius and Sextius (*L. Sextius et C. Licinius... legem Cassii agrariam tandem pertulerunt*). For Sigonio then, the Licinian law represented the eventual implementation of the Cassian law, and it therefore follows that he understood it as relating to holdings of public land.<sup>38</sup>

From the seventeenth century Roman histories began to be written in vernacular languages. The first such works tended to cleave closely to the ancient sources’ narratives, and this explains the contradictions in their accounts of the Licinian and Gracchan limits. Thus Laurence Echard, author of the first Roman history in English, reports Licinius’ law as “a law that no man should possess above 500 acres of land”, but, without commenting on the discrepancy, describes Gracchus’ law as “forbidding any man to possess above 500 acres of the publick lands”.<sup>39</sup> Comparable inconsistencies can be found in the first histories of Rome in French, by Scipion Dupleix, published as early as 1638, and by the Jesuits Catrou and Rouillé.<sup>40</sup>

The next such work in English, by Nathaniel Hooke, dealt just with the Roman Republic, and in much greater detail than Echard’s. His treatment of the Licinian and Gracchan laws is not fully coherent, and he does initially report the Licinian law as simply “forbidding any

<sup>38</sup> Sigonio (1574, 217–8). Sigonio’s meaning is correctly interpreted by Long (1846, 84–6), whereas Heuss (1981, 237–41) makes unnecessary difficulties. Ridley (2000, 461) cites only Sigonio’s edition of the *Fasti*, disregarding the *De antiquo iure populi Romani*, to which Niebuhr was evidently referring. Niebuhr’s reference to Louis de Beaufort is to his *La république romaine*, a comprehensive account of Roman republican institutions, comparable for its day to what Sigonio had produced two centuries earlier. However, this work’s references to the Licinian and Gracchan land laws are too imprecise to show how the author interpreted their limits (Beaufort 1766, I.369, II.415–7; cf. Heuss 1981, 252–3).

<sup>39</sup> Echard (1697, 145, 237).

<sup>40</sup> Dupleix (1638, I.334, 526, II.271–2); Catrou and Rouillé (1725–37, II.385, IV.370, XIII.297–304); cf. Heuss (1981, 246–9).

Roman citizen to hold more than 500 acres of land".<sup>41</sup> However, he introduces it as "relating...to the conquered lands", and elsewhere refers to it and to Gracchus' law as a restriction on holdings of "the publick lands".<sup>42</sup> He views Gracchus' law as entirely justified, since the nobles were usurping these lands illegally, and applauds him as "the most accomplished patriot that ever Rome produced".<sup>43</sup> Thus, Hooke, like Sigonio, is in fact closer to Niebuhr's own view.<sup>44</sup>

Some years before Hooke, the Abbé de Vertot, author of a history of the revolutions in the government of the Roman Republic, achieved a consistent statement of the view which Niebuhr was later to adopt: he states clearly at every point that both the Licinian and the Gracchan limits applied to the public lands acquired by conquest.<sup>45</sup>

The opposite view was, however, taken by the next notable historian of the Roman Republic writing in English, Adam Ferguson, a prominent figure in the Scottish Enlightenment. As with Montesquieu, his interpretation is framed in terms of the supposed desirability of equal property, with the purpose of the Licinian law being stated as "to limit the extent of estates in land". However, he regards Gracchus as misguided:

Actuated by these dispositions, or by an idea not uncommon to enthusiastic minds, that the unequal distribution of property, so favourable to the rich, is an injury to the poor; he now proposed in part to remedy or to mitigate this supposed evil by reviving the celebrated law of Licinius, by which Roman citizens had been restrained from accumulating estates in land above the value of five hundred jugera. . . . This project, however plausible, was extremely unseasonable, and ill suited to the state of the commonwealth. The law of Licinius . . . , though properly suited to a small republic and even necessary to preserve a democracy, was, in that condition of the people, received with difficulty. . . . Its renewal . . . was become in a great measure impracticable, and even dangerous in the present state of the republic. The distinctions of poor and rich are as necessary in states of considerable extent, as labour and good government. . . . The

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<sup>41</sup> Hooke (1738, 444; 1745, 522).

<sup>42</sup> Hooke (1745, xxvi, 523–4, 530–8).

<sup>43</sup> Hooke (1745, 538). He also (532, 536) cites with approval an anonymous writer's characterization of Gracchus' law as "conducting to the equality so necessary in a free state".

<sup>44</sup> Niebuhr acknowledged in his lectures that he had slight acquaintance with Hooke's work: Niebuhr (1846–8, I.72 = 1870, 54).

<sup>45</sup> Vertot (1734, II.270, 289, 342–8): the Licinian limit applied to "terres de conquête"; Gracchus revived it, and his measure was directed against "les usurpateurs des terres publiques".

project seemed to be as ruinous to government as it was to the security of property.<sup>46</sup>

Thus, in sum, the accounts given of the Roman agrarian laws and the Licinian and Gracchan limits before the French Revolution certainly do not present a consensus that their application was to private property, as Niebuhr alleged. They in fact exhibit wide variations, partly deriving from the contradictions in the ancient sources, but also influenced by philosophical considerations about the desirability of equality of property, and some of these writers, such as Sigonio, Hooke, and Vertot, held views not far removed from Niebuhr's own.

The 'agrarian law' was certainly much talked of, in the clubs and in print, during the French Revolution.<sup>47</sup> What was meant by it was far from clear, but it was commonly understood as implying the redistribution of private property in the interest of equality. The slogan was most often used negatively, as a weapon against opponents. Particularly in 1791–2 some radical spirits were prepared to argue openly for the agrarian law in journals, pamphlets, and books, for example Rutledge, Maréchal, Cournand, and Bonneville. However, their practical proposals were comparatively restrained, for example the distribution of common lands or limits on inheritance. At no point did the possibility of an agrarian law become practical politics. Robespierre in particular repeatedly inveighed against the agrarian law as an "absurd bugbear". In September 1789 the Declaration of the Rights of Man had proclaimed property rights to be natural, imprescriptible, sacred, and inviolable, and their protection was reasserted in subsequent constitutional declarations. It is true that Dufour and Momoro, despatched by the Paris commune in August 1792 as emissaries to the departments of Calvados and Eure, promised a revised version of the Declaration in which only "les propriétés industrielles" would be guaranteed and inviolable and "ce qu'on appelle faussement propriétés territoriales" would have such protection only pending legislation. However, this announcement led to protests even from radical journalists, and nothing further came of it. On 18 March 1793, on the proposal of Barère and amid much enthusiasm, the Jacobin-dominated National Convention decreed the death penalty for anyone proposing the agrarian law. The measure was

<sup>46</sup> Ferguson (1783, 64, 300–3).

<sup>47</sup> On the 'agrarian law' in the Revolution see Jaurès (1968–72, II.449–73, III.201–225, VI.109–40); Heuss (1981, 153–88); Rose (1984).

clearly designed to reassure property holders and deprive opponents of a propaganda weapon. Only one prosecution is known to have taken place under the law.

François-Noel Babeuf, who in 1791 had shown interest in the agrarian law, was eventually to go much further. In May 1793 he changed his forename to Gracchus, and in October 1794 he renamed his recently founded journal *Le tribun du peuple*. From 1795 he argued for the abolition of private property and the establishment of communism. The abortive conspiracy of Babeuf and his fellow 'Equals' in 1796 was rapidly suppressed by the Directory, and he was executed in May 1797.<sup>48</sup>

In both editions of the *RG* Niebuhr acknowledged that certain recent writers about the Gracchan disturbances had preceded him in observing that the agrarian laws applied only to the public lands. In the first edition he opened his agrarian excursus with the following statement:

Not long since, it would have been requisite in every work written not exclusively for those skilled in philology, in order to prevent egregious misconception, to shew with the utmost solicitude, that the agrarian laws of the tribunes did not affect landed-property. At the present day, well-known accounts of the Gracchian commotions, derived from Appian and Plutarch, enable us to assume as demonstrated, that no tribunitian agrarian law interfered with this sacred right.<sup>49</sup>

In the second edition the corresponding passage occurs a little later, and, before mentioning the writers about the Gracchi, he here inserts a reference to a work by Heyne, for which he makes large claims:

On the breaking out of a revolution, which nobody would have thought compatible with the tameness of modern times, the agrarian laws and the Gracchi were much talked of. This led Heyne to do history a service, by pointing out that the laws of the tribunes related simply and solely to the public domain; and guided by this remark accounts of the Gracchic troubles were written, before the revolutionary frenzy had quite spent itself, acquitting the Gracchi of the charge of having shaken property. It is to Heyne's essay that I myself owe my conviction of this truth, which I have firmly retained ever since I began my researches on Roman history.<sup>50</sup>

Thus, in this version published in 1830, Niebuhr represents Heyne as the first writer to have observed that the laws applied only to public

<sup>48</sup> On Babeuf see Rose (1978); Birchall (1997).

<sup>49</sup> Niebuhr (1827, II.353), translated by Walter from Niebuhr (1811–12, II.349).

<sup>50</sup> Niebuhr (1851, II.133), translated by Hare and Thirlwall from Niebuhr (1828–32, II.149).

land (a claim also made in his lectures),<sup>51</sup> and credits him with convincing both the unnamed writers on the Gracchan troubles and Niebuhr himself of this truth. Modern writers who have considered the question have taken account only of this formulation, and have taken Niebuhr's claims about Heyne's contribution on trust.<sup>52</sup> However, once again we shall find that in this version Niebuhr misrepresented the facts.

Christian Gottlieb Heyne (1729–1812) was from his appointment as Professor of Poetry and Eloquence in 1763 until his death the dominant figure at the University of Göttingen, then the leading centre of classical learning in Germany. Along with his chair, he held the offices of director of the University Library, secretary of the Göttingen Academy, and editor of its journal. His voluminous writings made important contributions not only to classical philology, but also to the study of ancient mythology and art (the latter under the influence of his friend Winckelmann), and he was a pioneer of the integrated study of all aspects of classical antiquity. One of the duties of his chair was the delivery of Latin orations (*Programme*) on formal university occasions, and in many of these speeches Heyne discussed an aspect of the ancient world as a means of throwing light on some topic of contemporary interest. Some ten of his orations, delivered between 1789 and 1795, relate to the French Revolution, on which they take overall a moderate stance.<sup>53</sup>

On 1 March 1793 at the installation of a new prorector, Heyne gave an oration with the resounding title *Leges agrariae pestiferae et execrabiles*. As with the other orations, the speech was published the same year in various locations, including *Göttingische Gelehrte Anzeigen*, and republished later in Heyne's *Opuscula Academica*, where it can now be most conveniently consulted (Heyne 1796). As he announced at the outset, Heyne's speech was directed against French extremists (*homines inter Francos fanaticos et furoribus civilibus lymphatos*), who, he heard, were calling for agrarian laws which would establish equality in landed property and claiming the Roman agrarian laws as their model. This was a highly topical theme, and Heyne's stance would have been congenial to the leaders of the

<sup>51</sup> Niebuhr (1846–8, I.253 = 1870, 152): “the first who expressed an opinion that [the agrarian law] referred to the *ager publicus* was Heyne”.

<sup>52</sup> E.g. Cardinali (1912, 93–94); Rytönen (1968, 82, 106); Heuss (1981, 131–2, 322–8); Momigliano (1984, 160; 1994, 231–2); Whitman (1990, 156); Capogrossi Colognesi (2000, 276).

<sup>53</sup> On Heyne and his contribution to ancient history see now Heidenreich 2006, with discussion of the *Programme* on the French Revolution at 20–2, 220–52.

French government: as we have seen, the National Convention decreed the death penalty for advocating the agrarian law just eighteen days after Heyne delivered his speech. Heyne devoted the bulk of the speech to Roman agrarian history, concluding that the proposed agrarian law to establish equality of property bore no relation to any Roman agrarian legislation, and that its advocates were motivated by personal greed and thus comparable to the profiteers from the Roman proscriptions. He passes over the Licinian and Gracchan laws quite rapidly. Assuming (like many earlier writers) that the Licinian law provided for the distribution of land held in excess of its limit, he states that this was not unfair because the land in question was public.<sup>54</sup> When he reaches Tiberius Gracchus, he merely states that he revived the Licinian law, and then sets out what he takes to be the terms of Gracchus' law.

Heyne's treatment of Roman agrarian laws, and in particular of the Licinian and Gracchan limits, far from being the epoch-making innovation which Niebuhr claimed in 1830, in fact contained no novelty. It is true that the view that the Licinian and Gracchan laws applied to landed property had gained prominence in recent writing about Roman history, for example through Montesquieu's remarks and Ferguson's history, but, as we have seen, earlier writers like Sigonio, Hooke, and Vertot had held them to apply just to public land. In any case, Heyne's brevity on this point shows that he was not primarily concerned with this scholarly disagreement, but rather with the current conception of an agrarian law which would establish equality of property. That this conception did not correspond to the realities of the Roman agrarian law will in fact have been widely recognized. Indeed, Levasseur intervened in the debate in the National Convention later in the month to make this very point, observing that the Roman agrarian law shared out conquered land, but the agitation now being prohibited was for sharing out property.<sup>55</sup>

While he mentioned Heyne only in the second edition of the *RG*, in both editions Niebuhr referred to recent accounts of the Gracchan disturbances without naming their authors. As a footnote added by the

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<sup>54</sup> Heyne (1796, 362): *Lex haec Licinia de modo agrorum, ne quis plus quingenta iugera agri possideret, scilicet ut quicquid agrorum supra praescriptum modum superesset, inter plebem aequo pretio divideretur, hactenus non iniqua videri poterat, quandoquidem agri erant publici, quos possidebant privati.*

<sup>55</sup> "La loi agraire était chez les Romains le partage des terres conquises; ici, il ne s'agit point de cela, il s'agit du partage des biens" (cited by Jaurès 1968–72, VI.110).

translators in the English edition confirms, the works in question are A.H.L. Heeren's *Geschichte der Staatsunruhen der Gracchen*, which was first published in 1795 and reissued in revised form in collected editions of his historical works (Heeren 1803), and D.H. Hegewisch's *Geschichte der Gracchischen Unruhen in der römischen Republik* (Hegewisch 1801). Both authors wrote prolifically on a wide range of historical subjects, although Heeren was to win the greater acclaim. A former pupil of Heyne, Heeren (1760–1842) became his colleague at Göttingen and his son-in-law, and was to write his life. Niebuhr had a low opinion of him: in letters of 1812 he speaks of Heeren's "easily won celebrity" and "false reputation", and the following year he published a scathing review of one of his works.<sup>56</sup> By contrast, Hegewisch (1746–1812), who was professor of history at Kiel, was well regarded by Niebuhr, who had attended his lectures as a student, and wrote warmly of "the good old Hegewisch" in a letter at the time of his death.<sup>57</sup>

It is a curious coincidence that, within a few years, these two scholars should have written books on the Gracchi with such similar titles and contents.<sup>58</sup> Each used the term *Unruhen* in their titles, and Niebuhr used the same word when referring to their works in his first edition (rendered by his English translator as 'commotions').<sup>59</sup> Both works were evidently prompted by the contemporary relevance which the French Revolution had given to the Gracchan episode and provide a narrative account which is broadly favourable to the tribunes. Both argue that Tiberius Gracchus' limit applied not to landed property, but only to public land (Hegewisch 1801, 3–5, 28–29, 71–76; Heeren 1803, 178–82). Hegewisch in particular regards himself as in polemic on this point with the established view, citing Ferguson as its representative. His is in general the more careful account, and in an appendix he mounts an effective defence from the ancient sources of this interpretation of Gracchus' law. Unfortunately, he leaves his view of the Licinian law obscure (Hegewisch 1801, 9). Heeren is clear that the Licinian as well

<sup>56</sup> Niebuhr (1926–9, II.278, 282); Walther (1993, 308–10). Similar comments in later letters: Niebuhr (1981–4, III.106, 108, 121). Heeren's monograph on the Gracchi is overlooked by Heuss (1981) and other recent studies of Niebuhr.

<sup>57</sup> Niebuhr (1926–9, II.266). On Hegewisch's study of the Gracchi see Heuss (1981, 328–30).

<sup>58</sup> It would seem that Hegewisch wrote in ignorance of Heeren's 1795 publication. In his 1803 republication Heeren (p. ix) wrote generously of Hegewisch's work.

<sup>59</sup> In the second edition Niebuhr opted for a different term, 'Bewegungen'.

as the Gracchan limit applied only to public land, but supplies only perfunctory argumentation.

Niebuhr's claim in the second edition of the *RG* that Heeren and Hegewisch were following the lead set by Heyne in his 1793 oration seems without foundation. Neither author refers to Heyne, and, in view of the small part played by the Licinian and Gracchan laws in Heyne's speech, it seems unlikely to have been a significant influence on their work. Hegewisch probably arrived at his view independently. Heeren may well have imbibed his view that the two laws applied to public land from his teacher Heyne, but the 1793 oration itself is not likely to have been a significant influence on him. He does not mention it in the section of his biography which he devoted to Heyne's speeches on political subjects (Heeren 1823, 200–1), and so, unlike the later Niebuhr, cannot have regarded it as of great significance.

In the Copenhagen MSS Niebuhr refers to Hegewisch's and Heeren's histories of the Gracchan troubles.<sup>60</sup> In a sketchy early fragment Niebuhr reminds himself to consult Hegewisch's book: this passage shows that he had already formed his own view of the application of the agrarian laws before he had read Hegewisch's account, and the same is probably true for his knowledge of Heeren's.<sup>61</sup> In the Copenhagen MSS, as in the first edition of the *RG*, Niebuhr makes no reference to Heyne's oration, which he mentions for the first time only in the versions composed in 1828–30, namely the Bonn lectures and the second edition of the *RG*. This must cast doubt on his claim in the second edition that it was Heyne's work which first convinced him that the tribunician laws applied only to *ager publicus*. If Heyne's work had really played so important a part in the evolution of his thought, he would surely have alluded to it in the versions composed in 1803–12. It seems more likely that Niebuhr became aware of Heyne's contribution only at a late stage, perhaps even after the composition of the first edition of the *RG*, and when, many years later, he returned to the topic, his memory played

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<sup>60</sup> Hegewisch's work is discussed by name at MS B, *ap.* Heuss (1981, 511–2). Fragment 6A, *ap.* Heuss (1981, 522–3) refers, without naming their authors, to histories of the Gracchan *Unruhen*. Here, as in the comparable passage in the successive editions of the *RG*, the reference is evidently to Hegewisch and Heeren (not Heyne, as Heuss, unaware of Heeren's work, supposed).

<sup>61</sup> Hegewisch: Fragment 7A, *ap.* Heuss 1981, 525 (“Ich muss Hegew. ansehen, um zu erfahren ob er, wie zu glauben, den Begriff des römischen *ager publicus* richtig hat”). Niebuhr may well have become acquainted with Heeren's work only from the 1803 edition, rather than the original 1795 publication.

him false as to Heyne's significance.<sup>62</sup> Thus Niebuhr probably arrived at his own view of the application of the agrarian laws independently of all these writers.<sup>63</sup>

Whatever their individual contributions and their interdependence, Heyne, Heeren, Hegewisch, and Niebuhr were at one in opposing what had recently become the dominant view of the Roman agrarian laws and in particular the Licinian and Gracchan limits. It was, however, Niebuhr who made the decisive impact on the subsequent discussion, through his firm insistence that both limits applied only to public land, and as a result of the enormous impact made by his history and especially by this part of the work.

Niebuhr did not, however, feel it necessary to present a fully argued demonstration that the Licinian and Gracchan limits applied only to holdings of *ager publicus*. In all his treatments of the topic he assumes that this conclusion required no further demonstration beyond what his immediate predecessors had provided. After his observations on earlier, erroneous views of the agrarian laws, he invariably proceeds directly to an extended discussion of problems in the tenure of *ager publicus*, which seemed to him to arise once it was recognized that it was to this land that the agrarian laws applied. In his view, his predecessors had failed to deal adequately with these questions and he had been able to resolve them himself after protracted struggle. The problems centred on the nature of *possessio* in relation to *ager publicus* and in particular on how it could be that, although the jurists sharply distinguished *possessio* from *locatio*, *possessores* holding *ager publicus* were liable to pay dues, as Appian and Plutarch tell us in conflicting formulations. Although flawed, the wide-ranging treatment which Niebuhr provided in response to these difficulties has formed the starting point for all of the subsequent modern discussion of *ager publicus* and of the holding of such land in *possessio*.

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<sup>62</sup> In his lectures he showed a similar confusion over the part played by the Holstein emancipation in the evolution of his thought (above, n. 18).

<sup>63</sup> Niebuhr had probably already reached his view that the Roman laws applied to *ager publicus* and begun his long struggle with its implications when he discussed a projected book on land law with James Grant in 1799 (above, n. 13). In 1803, writing to his sister-in-law Dore Hensler about his work on *ager publicus*, he observed that he had already been interested in such matters as a student in Kiel, at the time of their first acquaintance (Niebuhr 1926–29, I.310), and a letter from that time to his parents shows him entertaining radical views about agrarian law as a solution to current problems: at a party in the Hensler family home in 1794, “mischte ich über die Aufteilung der Ländereien, oder ein Ackergesetz wodurch der Ankauf mehrerer, als einer bestimmten Zahl Äcker untersagt wurde” (*ib.* I.62).

Since he nowhere made them explicit, Niebuhr's reasons for holding that the Licinian and Gracchan limits applied only to *ager publicus* have to be inferred from his incidental observations. Two considerations seem to have been particularly decisive for him. He evidently took this to be the clear implication of Appian and Plutarch: the scholars who took the opposite view are criticized for disregarding "the reference to the conquered lands, which the Greek historians insist on as so essential a point".<sup>64</sup> Secondly, he appears to have felt that the frequency with which the word *possidere* and its cognates figures in the relevant sources confirms that the limits related to occupation of public land rather than private ownership.<sup>65</sup>

When he was working on his Berlin lectures and their subsequent publication in the first edition of the *RG*, Niebuhr's new friendship with Savigny helped him to develop his thinking on the questions relating to *ager publicus* and to carry it beyond what he had achieved in the Copenhagen drafts, particularly in relation to the legal aspects, and Niebuhr made handsome acknowledgement of Savigny's contribution.<sup>66</sup> The topic was close to Savigny's own interests: the foundation of his great reputation had been his treatise on the Roman law of possession, *Das Recht des Besitzes*, first published in 1803. Niebuhr himself made a key contribution to the legal question, with his suggestion that the possessory interdicts had originated as protection for occupiers of *ager publicus*. Savigny accepted this, and included a new section to this effect (12a) in the third and subsequent editions of his treatise.<sup>67</sup>

Two other aspects of Niebuhr's conception of the Licinian law should also be noted. First, he regarded it as a *lex agraria* providing for land distribution, and assumed that this law, like that of Gracchus, established a commission to take over land held in excess of the limit and distribute it in small allotments.<sup>68</sup> The same view had in fact been taken in most earlier discussions of the law. We shall revert to this point in the next section.

Second, Niebuhr held that it was by this law that the plebeians acquired the right to occupy public land, which in his view had previ-

<sup>64</sup> Above, at n. 21.

<sup>65</sup> See especially Niebuhr (1828–32, II.161–3 = 1851, II.142–4).

<sup>66</sup> Niebuhr (1811–12, II.371 n. 488; 1828–32, II.169 n. 316, 172–3 = 1851, II.149 n. 316, 152).

<sup>67</sup> Savigny (1837, 215–24) with acknowledgement to Niebuhr at 216–7. In general on Savigny and his significance see Whitman (1990, chs. 4–5).

<sup>68</sup> Niebuhr (1811–12, II.399–400 = 1828–32, III.19–20 = 1851, III.16–17).

ously been restricted to patricians.<sup>69</sup> Though not as universally accepted as his interpretation of the Licinian limit, this notion too has been very influential and has been advocated particularly by Tibiletti.<sup>70</sup> To my mind, it is quite unfounded. It is indeed alleged in the ancient accounts of the agrarian conflict from the time of Sp. Cassius' proposal that the patricians had monopolized the occupation of public land, and it is plausible enough that they predominated in its early occupation. However, no source states that they enjoyed a monopoly of it as of right, nor has any good reason been offered for postulating such an arrangement.<sup>71</sup>

Niebuhr concluded his treatment of the Licinian land law with a paean of praise for what he regarded as a wholly admirable measure, holding that it was initially effective and would, if not frustrated, have maintained a free and prosperous peasantry and averted the fall of the Republic: "Happy the state, in which it was possible in accordance with the constitution to restore by a Licinian law a nation of free countrypeople!"<sup>72</sup>

From its first appearance Niebuhr's *RG* provoked wide and sometimes passionate discussion, especially in Germany and England. Along with many other features of the work, particular attention was paid to his insistence that the 500-*iugera* limits of the Licinian and Gracchan laws applied only to *ager publicus*. A different solution was proposed by Hüllmann and, more fully, Huschke, namely that the Licinian and Gracchan limits had not had the same application: the Licinian limit had applied to all holdings in land, both public and private, but Gracchus revived it just as a limit on holdings of public land.<sup>73</sup> This view

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<sup>69</sup> Niebuhr (1811–12, I.451–2, II.363–5, 395–6). Nothing corresponds to the first two passages in the agrarian excursus as revised for the second edition, which suggests that by then Niebuhr may have come to doubt his theory of the exclusive patrician right to *ager publicus*. However, he retained the statement that the Licinian law abolished this right in his revised account of the law's terms: Niebuhr (1828–32, III.15 = 1851, III.13).

<sup>70</sup> Tibiletti (1948, 216–7; 1949, 28–9; 1950, 246–7).

<sup>71</sup> The hypothesis of a *de iure* monopoly of *ager publicus* by the patricians is rejected by e.g. Last (1949, 433); Cornell (1995, 328); Gargola (1995, 143); Serrao (1999, 165–6). Our earliest source for the extrusion of plebeians from public land is Cassius Hemina fr. 17 Peter (*quicumque propter plebitatem agro publico eiecti sunt*); the context of this fragment is unknown. Capogrossi Colognesi's view that *ager publicus* was held on a gentilicial basis provides a different model of patrician exclusivity: on this see below at n. 114.

<sup>72</sup> Niebuhr (1811–12, II.402). For the second edition he added the qualification "though only for a century" (Niebuhr 1828–32, III.22–23 = 1851, III.19).

<sup>73</sup> Hüllmann (1832, 272–6); Huschke (1835, 2–21). Niebuhr's view of the Licinian

initially won much support among writers on Roman law: the greatest German jurists of the generation after Savigny, Puchta, Jhering, and Rudorff, all sided with Huschke against Niebuhr in their treatises, with Puchta opting for the modification that the Licinian limit applied only to private land.<sup>74</sup> Elsewhere, however, Huschke's view attracted little support. Schwegler in his massive history of early Rome presented an account of *ager publicus* which, while a considerable advance in lucidity and thoroughness, followed essentially Niebuhr's lines.<sup>75</sup> A number of writers argued the case for Niebuhr's view as against Huschke's.<sup>76</sup> Quite soon also the tendency developed to take the truth of Niebuhr's view for granted. Thus Mommsen in his *Römische Geschichte* flatly asserted that the Licinian limit applied to *ager publicus* without reference to any contrary view, and Niese in the ground-breaking article in which he for the first time pointed out the implications of this interpretation for the credibility of the Licinian law merely indicated his support for Niebuhr's view at the outset without further discussion.<sup>77</sup>

For most of the twentieth century, doubts about the Niebuhr doctrine virtually disappeared, apart from the odd isolated individual such as the Soviet scholar Utčenko.<sup>78</sup> Those like Heuss and Momigliano who explored the intellectual origins of Niebuhr's view took it for granted that it represented the truth.<sup>79</sup> A few of those who wrote on Roman agrarian questions explicitly acknowledged, without further argument, that they were following Niebuhr's view of the application of the Licinian limit.<sup>80</sup> Most writers, however, merely asserted without comment that

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limit was also rejected by Schultz (1833, 476 ff.), in the course of a wide-ranging and largely perverse critique of Niebuhr, on which see Heuss (1981, 330–3).

<sup>74</sup> Puchta (1841, 202–4, 272–3; and 1846); Jhering (1854, 157, 247–8); Rudorff (1857, 38–39). On these writers and their significance see Whitman (1990, ch. 6).

<sup>75</sup> Schwegler (1856, 401–55). However, Clason, in his continuation of Schwegler, sided with Huschke and rebutted his critics at length (Clason 1873, 196–218).

<sup>76</sup> Long (1845a; 1845b; 1846); Lewis (1855, 137–8, 383–94); Sundén (1858: *non vidi*); Lange (1876, 673–6); De Ruggiero (1892, 768–81). Further bibliography at Schwegler (1856, 401–2 n. 1); Clason (1873, 200); De Ruggiero (1892, 768 n. 1); Rotondi (1912, 217–8).

<sup>77</sup> Mommsen (1861, 294–301 = 1877, 304–10); Niese (1888, 410).

<sup>78</sup> The views of Utčenko and other dissenters are reported by Forsén (1991, 29–31).

<sup>79</sup> Heuss (1981); Momigliano (1984; 1994).

<sup>80</sup> E.g. Cardinali (1912, 93–95); Zancan (1935, 8–9); Tibiletti (1948, 176 n. 1): “condividiamo naturalmente la classica teoria del Niebuhr secondo cui le leggi agrarie vertevano esclusivamente sull'*ager publicus*”; Cornell (1995, 328): “Niebuhr's epoch-making work... established once and for all that movements of agrarian reform during the Roman Republic... were concerned solely with the manner of disposal and use of the *ager publicus*”.

the Licinian and Gracchan limits applied to *ager publicus*, and indeed appear to have been unaware of any other possibility or of the fact that most of our sources for the Licinian limit do not specify that it applied only to public land. Even standard commentaries on the sources in question pass over this feature without comment.

It is only very recently that this consensus has been challenged, by the contributions of Kunkel and Rathbone. Kunkel holds, like Hüllmann and Huschke, that the Licinian limit applied to all landholding, public and private, and, like them, he supposes that the Gracchan limit was more restricted, applying only to public land.<sup>81</sup> Rathbone prefers to suppose, like Puchta, that the Licinian limit applied only to private land, and he holds that this was also the case with the Gracchan limit, although it was to be enforced only in respect of holdings of public land, which were made private under the law.<sup>82</sup>

This section has exposed the flaws in the account of earlier scholarship on the land limits which Niebuhr gave in the second edition of the *RG* and which subsequent writers have taken on trust. Niebuhr claimed to be following Heyne, who, he asserted, had exploded an earlier consensus that both the pre-Gracchan and Gracchan limits applied to all landholding. In fact, as we have seen, Heyne's contribution was of little significance; a wide diversity of views on the application of the limits is to be found in the earlier scholarship, reflecting the contradictions in the ancient sources, and some writers held positions not far from that which Niebuhr himself was to adopt. Niebuhr did, however, succeed in establishing a new orthodoxy, that both the pre-Gracchan and the Gracchan limits applied only to holdings of *ager publicus*. We must now consider whether this view is well founded, or a different interpretation should be preferred, such as that proposed by Hüllmann, Huschke, and Kunkel, or the alternative offered by Rathbone.

### III. *Niebuhr's Doctrine Assessed*

This inquiry must centre on the ancient sources for the Roman limits on landholding, and the appropriate point of departure will be Appian and Plutarch, the only two sources to discuss the law establishing the

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<sup>81</sup> Kunkel (1995, 493–97, 639). His treatment of the Gracchan law is very brief, and he does not comment on the postulated restriction in its application by contrast with the Licinian law.

<sup>82</sup> Rathbone (2003, esp. 143–9, 159–62).

pre-Gracchan limit in the same context as Tiberius Gracchus' law. In both cases their treatment forms part of an overall account of the Roman public land and the long-running problems relating to it which, the authors claim, ultimately led Gracchus to introduce his measure. These accounts are on broadly similar lines, although Appian's is fuller and there are significant divergences (App. *BC* 1.7.26–8.34; Plu. *TG* 8.1–4). The two versions probably derive from a common (unidentifiable) source, with the discrepancies mainly or wholly the result of Appian's and Plutarch's own contributions.<sup>83</sup>

Both writers open by describing the Romans' practice of confiscating a portion of the territory of their defeated enemies and the various uses to which they put the confiscated land, with land not assigned in other ways being made available for individuals to occupy. Both agree that the occupation of this unassigned land attracted a charge: Appian (*BC* 1.7.27) states that a proportion of the crops was payable and a toll levied for pasture, while Plutarch (*TG* 8.1–2) says that a rent was charged. As we have seen, these statements were central to Niebuhr's difficulties over *ager publicus*; modern scholars, however, dispute whether any charges were in fact payable.<sup>84</sup> The two writers differ also over the purpose of this provision for the occupation of unassigned land: according to Plutarch, the intended occupiers were the poor, whereas Appian states that the arrangement was designed 'to increase the numbers of the people of Italy, so that they would have their kin to fight alongside them'. They are, however, agreed that this intention was frustrated by the rich. Plutarch tells us that they began to drive out the poor. Appian, much fuller at this point, explains that 'the rich, having taken over most of the unassigned land and in time . . . acquiring the adjoining and other smallholdings of the poor, partly by purchase through persuasion and partly by seizure through force, came to cultivate vast tracts instead of single farms', and then goes on to describe the exploitation of these estates through slaves and the resulting impoverishment of the Italians and the decline in their number. It was in reaction to this development, both writers tell us, that the law was passed establishing the 500-*iugera*

<sup>83</sup> A common source is postulated by, e.g., Cardinali (1912, 45–70); Fraccaro (1914, 11–29, 66–76); Tibiletti (1948, 192–209); Forsén (1991, 55–59). *Contra* Gabba (1956, 37 n. 1; 1967, 10 ff.).

<sup>84</sup> E.g. Cardinali (1912, 99–106); Kaser (1942, 28–29); Tibiletti (1948, 183–9); Burdese (1952, 63–68); Gargola (1995, 140–1, 235); Campbell (2000, 473).

limit, and Appian records further provisions (pasture restrictions and a minimum number of free workers). However, the law was either evaded or disregarded: Appian reports this briefly, but Plutarch is fuller, conceding that the law did keep rapacity in check for a time, but then going on to describe the expansion of slave labour and the hardship and depopulation of the poor.

Appian and Plutarch then pass to Tiberius Gracchus and his attempt to provide redress. Appian in due course makes the following statement about Gracchus' provisions in respect of the limit (*BC* 1.9.37): 'he proposed to renew the law that no individual should have more than 500 *iugera*, but added half of this again for their children over and above the old law' (ἀνεκαίνιζε τὸν νόμον μηδένα τῶν πεντακοσίων πλέθρων πλέον ἔχειν. παισὶ δ' αὐτῶν ὑπὲρ τὸν παλαιὸν νόμον προσετίθει τὰ ἡμίσεια τούτων). Appian confirms this and adds a further detail in a subsequent speech attributed to Gracchus, where it is said that those who were to lose land under his law would receive as compensation for the work they had expended on it 'the undisputed tenure, without charge and secure for ever, of 500 *iugera* each, and half of this again for children, for each of those who had children' (1.11.46: τὴν ἐξαίρετον ἄνευ τιμῆς κτήσιν ἐς αἰεὶ βέβαιον ἑκάστῳ πεντακοσίων πλέθρων, καὶ παισίν, οἷς εἰσὶ παῖδες, ἑκάστῳ καὶ τούτων τὰ ἡμίσεια). Plutarch does not specify the terms of Gracchus' law, but he does remark on its mildness, since 'those who should have withdrawn with a fine from the land which they were enjoying contrary to the laws' were to receive compensation (τιμὴ) when they surrendered 'what they had unjustly acquired' (Plu. *TG* 9.2). After Octavius' veto, he tells us (10.4), Tiberius withdrew this generous law and substituted one which simply ordered the wrongdoers to leave the land which they held 'contrary to the previous laws'.

Appian (*BC* 1.8.33) reports the pre-Gracchan limit in the following terms: 'they decided that no one was to have more than 500 *iugera* of this land, nor pasture more than one hundred larger or 500 smaller beasts' (ἔκριναν μηδένα ἔχειν τῆσδε τῆς γῆς πλέθρα πεντακοσίων πλείονα). The word τῆσδε is omitted by some manuscripts, but these are now regarded as the inferior group, and so the word should probably be retained. If it is omitted, we should translate 'the land' rather than 'this land', but the sense is hardly affected. What Appian understood by 'this land' is not wholly clear. He had earlier told us (1.7.29) that the 'vast tracts' which the rich had come to work comprised both the unassigned public land in their occupation and the lands of the poor

which they had acquired by purchase or seizure. The phrase ‘this land’ in the statement about the 500-*iugera* limit could therefore be taken as covering both forms of land, and so in effect all landholding, and Appian’s following statement (1.8.34) that it was expected that land held above the limit would be sold to the poor might also support this conclusion. Similarly, in his later account of the complaints of the rich about Gracchus’ law, he reports some as adducing the price (τιμή) paid to neighbours (1.10.39), which presumably refers to the land allegedly purchased from the poor. However, he represents the poor as complaining (1.10.40) that they were being deprived of their share of ‘the common property’ (τὰ κοινὰ), and later makes Gracchus urge that ‘the common property should be shared in common’ (1.11.44). On balance, the usual view is probably correct that Appian took the limits in the pre-Gracchan and Gracchan laws as applying just to holdings of the public land on which his whole discussion had been focused.

Plutarch’s report of the pre-Gracchan measure states simply that ‘a law was passed forbidding anyone to have more than five hundred *iugera* of land’ (*TG* 8.2: ἐγρόφη νόμος οὐκ ἔων πλέθρα γῆς ἔχειν πλείονα τῶν πεντακοσίων). The lack of the definite article or any other qualifier before the word ‘land’ (γῆς), together with the overall brevity of Plutarch’s account, leaves open the possibility that he took the limit to apply to all landholding, and therefore also that this may be how it was understood by his source.

Thus, even if it is conceded that Appian took the pre-Gracchan, like the Gracchan, limit to apply only to public land, we cannot be sure that this was how it was interpreted by the common source of Appian and Plutarch. Appian himself was capable of considerable individuality of interpretation, as Mouritsen has recently demonstrated in respect of his treatment of the Italian allies,<sup>85</sup> and such a claim in respect of the pre-Gracchan limit could well be his own contribution. However, even if it were the case that the common source of Appian and Plutarch took both laws’ limits to apply just to public land, it would not necessarily follow that it was correct to do so. The common source presented the history of *ager publicus* and Roman agrarian developments from the standpoint of the Gracchan crisis itself and in terms of the motivations of the protagonists in that crisis. The picture which results contains some evident distortions, for example about the spread of slave-run

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<sup>85</sup> Mouritsen (1998, 11–22).

agriculture and *latifundia*, and the source's account of the pre-Gracchan law may have undergone similar distortion.<sup>86</sup>

We have numerous and diverse further sources for the pre-Gracchan limit. All these writers speak of it simply as restricting the amount of land a man might 'have' (*habere*) or 'occupy' (*possidere*) to 500 *iugera*, without any indication that it applied only to a particular category of land.

The earliest such passage is a fragment from the Elder Cato's speech *For the Rhodians*. Speaking in 167 BC against a proposal to declare war on Rhodes, Cato seeks to refute the proposition that the Rhodians had wanted to make war on Rome and so should be punished by arguing that a mere wish should not be punishable, and illustrates his point by observing that there was no penalty just for *wishing* to contravene legal restrictions such as the limits on the amount of land or the number of pasture-animals a man might have:

What? Is there any law so harsh as to say: 'if anyone shall have wished to do that, let the fine be a thousand asses less than half his property; if anyone shall have wished to have (*habere*) more than five hundred *iugera*, let the penalty be so much; if anyone shall have wished to have a greater number of animals, let the punishment be so much'? In fact, we wish to have more of everything, and we are not punished for it.<sup>87</sup>

Cato did not specify which law the land limit derived from, but Tiro, in his comments on Cato's speech, assumed that the reference was to the law of Licinius Stolo:

(Tiro) says that it cannot be concealed that wishing to have (*habere*) more than 500 *iugera*, which was prohibited by Stolo's plebiscite, and wishing to make unjust and impious war on the Roman people are not equal and similar matters.<sup>88</sup>

Tiro's is probably the earliest direct reference to the Licinian law, with the next being that of Varro in his *De re rustica*, usually supposed to have been composed in 37 BC. This work is written as a contempo-

<sup>86</sup> So rightly Gargola (1995, 139 ff., and in this volume).

<sup>87</sup> Cato, *ORF*<sup>4</sup> fr. 167, cited by Gell. 6.3.37: *quid nunc? ecqua tandem lex est tam acerba quae dicat 'si quis illud facere uoluerit, mille minus dimidium familiae multa esto; si quis plus quingenta iugera habere uoluerit, tanta poena esto; si quis maiorem pecuum numerum habere uoluerit, tantum damnas esto? atque nos omnia plura habere uolumus, et id nobis impune est.*

<sup>88</sup> Gell. 6.3.40: *dissimulari autem non posse ait, quin paria et consimilia non sint plus quingenta iugera habere uelle, quod plebiscito Stolonis (Bentley: colonis codd.) prohibitum fuit, et bellum iniustum atque impium populo Romano facere uelle.*

rary dialogue, with a C. Licinius Stolo as one of the interlocutors. When introducing this character, Varro (*R* 1.2.9) represents his family as one of his qualifications for discoursing on agriculture: ‘C. Licinius Stolo...whose ancestors carried the law about a limit of land—for that law which forbids a Roman citizen to have [*habere*] more than 500 *iugera* was Stolo’s’ (*C. Licinium Stolonem...cuius maiores de modo agri legem tulerunt—nam Stolonis illa lex, quae uetat plus D iugera habere ciuem R.*).

Our only full account of the passage of the Licinian law is by Livy, according to whom it was first proposed in 376 BC by C. Licinius Stolo and his fellow tribune L. Sextius, along with two other laws, one on debt and the other prescribing that consuls should be elected rather than consular tribunes and that one consul should always be a plebeian; a protracted dispute followed, which was only resolved in 367 BC, when (although Livy fails to make this explicit) all three laws were finally carried (Livy 6.35–42). Livy (6.35.5) defines the terms of the land law at the outset as ‘about a limit on lands, that no one should occupy more than 500 *iugera* of land’ (*de modo agrorum, ne quis plus quingenta iugera agri possideret*). Having stated the terms of the three proposed laws, Livy permits himself an authorial comment: the senators, he tells us, were ‘terrified by the proposed simultaneous threat to all those things for which there is an unlimited desire among mortals, land, money and honours’ (6.35.6: *omnium igitur simul rerum, quarum immodica cupido inter mortales est, agri, pecuniae, honorum discrimine proposito conterriti patres*). The land issue is touched on again in several passages in the following narrative of the dispute over the laws: these will be discussed below.

The Licinian law is mentioned explicitly by Livy twice in the rest of his work. The first occasion is under 357 BC, when Livy (7.16.9) recounts the probably apocryphal tale that C. Licinius Stolo himself was fined 10,000 asses under his own law, ‘because he occupied 1,000 *iugera* of land with his son and by emancipating his son had defrauded the law’ (*quod mille iugerum agri cum filio possideret emancipandoque filium fraudem legi fecisset*). The second passage occurs in the (fictitious) speech which Livy wrote for the consul Cato in 195 BC unsuccessfully opposing the repeal of the Lex Oppia, which restricted feminine display. Here Cato is made to appeal to the Licinian law as one of a number of established checks on greed, asking: ‘What prompted the Licinian law about 500 *iugera* other than a huge greed for extending landholdings?’ (Livy 34.4.8: *quid legem Liciniam excitauit de quingentis iugeribus nisi ingens cupido agros continuandi?*).

Livy refers to the limit on landholding in one further passage, under the year 298 BC, without explicit mention of the Licinian law:

In that year numerous men were prosecuted by the aediles, because they occupied more land than was permitted by law; virtually no one was acquitted and a huge restraint was placed on excessive greed.<sup>89</sup>

As we noted at the start of this paper, this notice records an isolated administrative event, whose authenticity there is accordingly no reason to doubt. It is thus particularly notable that Livy here too speaks of the law then being applied simply as limiting the amount of land a man might occupy (*possidere*).

Thus Livy consistently speaks of the limit just as a restriction on the occupation of land. Moreover, both in his own voice (6.35.6, 10.13.14) and in Cato's (34.4.8), he repeatedly represents the limit as a restraint on greed. This confirms that he envisaged the limit as applying to holdings of all types of land: a limit applying only to one category of land would hardly be an effective check on cupidity, particularly if private landownership were excluded from its effect.

A number of later sources which refer to the Licinian law, and often also to Licinius' conviction, invariably describe the law as restricting how much land a man might 'have' or 'occupy'. (None mentions Sextius, Licinius' colleague in the Livian narrative, in this connection.) Velleius (2.6.3), in his account of Gaius Gracchus (possibly confusing his agrarian law with his brother's), states that 'he forbade any citizen to have more than 500 *iugera*, which had once been prohibited by the Licinian law' (*vetabat quemquam ciuem plus quingentis iugeribus habere, quod aliquando lege Licinia cautum erat*). Valerius Maximus (8.6.3), perhaps following Livy, reports that Licinius became the first victim of his law, because 'when he had prescribed by law that no one should occupy more than 500 *iugera* of land, he himself acquired a thousand' (*cum lege sanxisset ne quis amplius quingenta iugera possideret, ipse mille comparauit*). Plutarch, in his life of Camillus (39.5), reporting Licinius' legislation and later conviction, perhaps also following Livy, gives the terms of the law as 'that no one should hold more land than 500 *iugera*' (*μηδένα πλείθρων πεντακοσίων πλείονα χωράν κεκτηῖσθαι*). A notice of Licinius' law and conviction in

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<sup>89</sup> Livy 10.13.14: *eo anno plerisque dies dicta ab aedilibus, quia plus quam quod lege finitum erat agri possiderent; nec quisquam ferme est purgatus uinculumque ingens immodicae cupiditatis iniectum est.*

the *De uiris illustribus* gives (if the transmitted text is correct) eccentric versions of the amounts of land involved, but still represents the law as limiting how much land a man could 'have'.<sup>90</sup>

Several passages speak of the Licinian law in the moralizing terms we have already noticed in Livy. Thus Columella (1.3.11) adduces it as part of an argument for moderation in landholding:

Later on, even though our victories and the annihilation of the enemy had desolated vast stretches of country, it was still a criminal matter for a senator to occupy (*possidere*) more than fifty *iugera*. C. Licinius was condemned under his own law when, with an unrestrained passion for occupying land, he had exceeded the limit which he had set by legislation proposed when he was a tribune; and this not only because it was a mark of arrogance to hold so much land, but quite as much because it seemed the more scandalous for a Roman citizen, by extending the amount he occupied in an unprecedented fashion beyond the sufficiency of his inheritance, to leave unworked those lands which the enemy by their flight had abandoned. Therefore, as in all matters, so too in the acquiring of land, moderation shall be exercised.<sup>91</sup>

Columella shows some confusion here: the Licinian limit (if the manuscript reading is correct) is given as 50 *iugera*, and the law is dated after Curius Dentatus' land assignment of 290 BC. He envisages the excess holdings as being of land acquired through conquest, and for this reason the passage has been thought to tell in favour of the view that the Licinian limit applied only to public land.<sup>92</sup> However, the limit is specified as just on holdings of land, and there is no suggestion that there might be some forms of landholding to which it would not apply.

The Elder Pliny's reference to the limit clearly envisages it as checking the size of all estates. Commenting on high yields recorded from the early Republic, Pliny (*Nat.* 18.17) remarks:

Nor was this the result of the *latifundia* of individuals who ousted their neighbours, inasmuch as by the law of Licinius Stolo the limit was

<sup>90</sup> *Vir. ill.* 20.4: *idem lege cauit, ne cui plebeio plus centum iugera agri habere liceret. et ipse cum iugera quinquaginta centum haberet, alia emancipati filii nomine possideret...* On the problems of this passage see Forsén (1991, 69–73).

<sup>91</sup> *nox etiam cum agrorum uastitatem uictoriae nostrae et interneciones hostium fecissent, criminisum tamen senatori fuit supra quinquaginta iugera possedisse, suaque lege C. Licinius damnatus est, quod agri modum, quem in magistratu rogatione tribunicia promulgauerat, immodica possidendi libidine transcendisset, nec magis quia superbum uidebatur tantum loci detinere quam quia flagitiosius, quos hostis profugiendo desolasset agros, nouo more ciuem Romanum supra uires patrimonii possidendo deserere. modus ergo, qui in omnibus rebus, etiam parandis agris habebitur.*

<sup>92</sup> So Forsén (1991, 30).

restricted to 500 *iugera*, and he himself was convicted under his own law because he occupied (*possidere*) a larger amount, held under his son's name instead of his own.<sup>93</sup>

Finally, Gellius (20.1.23) lists the Licinian law (without precision as to its terms) along with other measures checking luxury: 'What seemed more salubrious than that law of Stolo's about the fixed number of *iugera*?' (*quid salubrius visum est rogatione illa Stolonis iugerum de numero praefinito?*).

Thus Cato, Tiro, Varro, Livy, and numerous later authors all present the pre-Gracchan limit just as a restriction on the amount of land a man could 'have' (*habere*) or 'occupy' (*possidere*). This is a remarkable consensus, made even more so both by the early date of some of these authorities and by the diverse character of the accounts, including not only writers narrating or commenting on the passing of the Licinian law and its author's alleged conviction, but also incidental references to the limit, some of which do not mention the Licinian law itself. The consensus is further reinforced by the fact that several of the authors envisage the limit in moralizing terms as a restraint on greed.

The natural interpretation of the pre-Gracchan limit as formulated by these numerous and diverse sources is that it applied to all forms of landholding. It is an extraordinary fact that all those scholars who so confidently assert without argument that the pre-Gracchan limit applied only to *ager publicus* do not trouble to note that this is in conflict with the overwhelming consensus of the sources, at least in their natural interpretation. The case for supposing that the limit applied only to *ager publicus* rests heavily on the evidence of Appian and of Plutarch in his life of Tiberius Gracchus, but, as we have seen, both the nature and the value of their testimony is actually a matter of doubt.

Niebuhr and the later scholars who argued his case do, of course, have lines of defence, to which we must now turn.

Niebuhr himself, like most of his predecessors, held, as we have seen, that the Licinian law was an agrarian law, which not only set a limit to landholding, but also provided, like the later law of Gracchus, for the establishment of a commission to recover land held in excess of the limit and distribute it in allotments. Since he believed that such agrarian laws always concerned public land, it therefore followed that

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<sup>93</sup> *nec a latifundiis singulorum contingebat arcentium vicinos, quippe etiam lege Stolonis Licinii incluso modo quingentorum iugerum, et ipso sua lege damnato cum substituta filii persona amplius possideret.*

it was to this that the Licinian law applied. However, there is in fact no evidence that the pre-Gracchan measure included any mechanism for recovering and distributing the excess land.<sup>94</sup> The only sanction which we hear of as arising from the law was the imposition of a fine by the aediles. No ancient source speaks of the measure as a *lex agraria*. As Huschke first observed, the law is correctly spoken of as a *lex de modo agri* (as by Varro) or *agrorum* (as by Livy 6.35.5).<sup>95</sup>

A central element in the case presented by Niebuhr and his supporters is the claim that it is implicit in our sources' statements, with their emphasis on the term *possidere* (translated above by 'occupy'), that the limit applied to *ager publicus*. Terms like *possidere*, *possessores*, and *possessionses* were so frequently spoken of in connection with *ager publicus* that, we are asked to believe, all would have understood that the reference here was to *ager publicus* without its being explicitly stated. Now it is true that *possidere* and its cognates were often used of holdings of *ager publicus*, and could be so used without explicit reference where the context made it clear. However, they could also apply to private land, and so we cannot assume that *ager publicus* is meant unless further contextual evidence is present.

In the developed law, *possessio* was a concept clearly distinct from ownership (*dominium*, *proprietas*). A man 'possessed' a thing if he had control of it—if he 'had' it, with or without ownership. The praetor protected the rights of the *possessor* through the possessory interdicts, providing he held possession *nec vi nec clam nec precario* ('not by violence or force or on sufferance'). If someone else wished to claim the thing as owner, he must bring an action against the *possessor* (*uindicatio*). For private land, the *possessor* would usually be the owner. Tenants, for

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<sup>94</sup> Scholars before Niebuhr sometimes inferred that Licinius Stolo's law provided for a viritate distribution of seven *iugera* from the reference in Columella 1.3.10 to *post reges exactos Liciniana illa septena iugera, quae plebis tribunus uirum diuiserat* and the comparable statement of Pliny *Nat.* 18.1.18 (so e.g. Goes 1674, 348). Niebuhr (1828–32, III.19–20 n. 19 = 1851, 16–17 n. 19) still hankered after this conclusion, although aware that Columella's and Pliny's statements must be garbled from the report of Var. *R* 1.2.9, that C. Licinius Crassus, tribune in 145 BC, *post reges exactos annis CCCLXV primus popululum ad leges accipiendas in septem iugera forensia e comitio eduxit*. Varro's obscure statement must be connected with Cicero's remark (*Amic.* 96) that Crassus *primus instituit in forum uersus agere cum populo*, and, however it is to be explained, cannot relate to land distribution. So rightly Huschke (1835); Tibiletti (1950, 236–9); Coarelli (1985, 130–1); *contra* Rathbone (2003, 147–8).

<sup>95</sup> Huschke (1835, 14–21). On laws *de modo agrorum* see especially Tibiletti (1948; 1949); Gargola (1995, 129–46).

example, renting land on contract, did not have *possessio*. Moreover, ownership might be acquired by *usucapio*, that is continued possession, for two years in the case of land, and this right can be traced back to the Twelve Tables (VI.3). For public land, the situation was different: the Roman people was the owner, while the occupiers of the land had *possessio*, but without right of *usucapio*.<sup>96</sup>

As we saw above, Niebuhr and Savigny held that the possessory interdicts originated as protection for occupiers of *ager publicus*. However, this hypothesis, which has provoked continued controversy, is a mere conjecture, and therefore can give no support to the interpretation of the pre-Gracchan limit as restricted to the *possessio* of *ager publicus*. At least one of the interdicts was already in existence by 161 BC, when Terence (*Eun.* 319) parodied the wording, but their origin remains unknown.<sup>97</sup>

Thus, even where our sources speak of the pre-Gracchan 500-*iugera* limit as the amount of land a man might ‘occupy’ (*possidere*), normal usage implies, in the absence of any contrary contextual indication, that this represented the amount of land of all kinds that he might occupy, comprising the private land in his ownership and further private or public land of which he was *possessor*. This is even clearer where the term used by our sources is ‘have’ (*habere*), as is the case with many of the sources reviewed above, including the three earliest, Cato, Tiro, and Varro. *Habere* has no particular association with the tenure of public land. It is often used of landholding in a sense close to *possidere*, as are *uti* (‘use’) and *frui* (‘enjoy’), and all four verbs can appear together as a composite formula, as in the inscribed agrarian law of 111 BC (Crawford 1996, no. 2, lines 9 ff.). *Habere* does, however, have a further ambiguity, since it is also used specifically of ownership. As Ulpian remarks, ‘we use “have” both of he who is owner of a thing and of he who is not the owner but holds it’.<sup>98</sup> It is thus perverse to interpret Cato, Tiro, and Varro in their references to the pre-Gracchan limit as referring just to the occupation of public land: the natural meaning of

<sup>96</sup> On *possessio* and ownership see briefly Berger (1953, 636–7); Nicholas (1962, 107–15). More fully, Kaser (1956; 1971, 140–3, 384–439).

<sup>97</sup> On the interdicts see conveniently Jolowicz and Nicholas (1972, 259–63). Supporters of the Niebuhr/Savigny hypothesis include Kaser (1956, 243 ff.); Labruna (1971); Falcone (1996); Capogrossi Colognesi (1997).

<sup>98</sup> *Dig.* 45.1.38.9: *nam et eum habere dicimus, qui rei dominus est, et eum, qui dominus quidem non est, sed tenet.* Cf. Paul. *Dig.* 50.16.188 pr.

their statements is that 500 *iugera* was the most land that a man was permitted to hold, whether as owner or as *possessor* by other means.

Livy's early books contain very numerous accounts of proposed agrarian laws, all thwarted, from that of Sp. Cassius in 486 BC on.<sup>99</sup> He would naturally have expected his readers to construe his account of the Licinian law *de modo agrorum* with this earlier narrative in mind. It has been maintained that, in the light of what had gone before, he intended them to interpret the Licinian law as applying only to public land. A careful reading will compel the opposite conclusion.

The lands proposed for division in Livy's fifth and early-fourth-century BC narrative are of two kinds: public land recently acquired by confiscation from defeated enemies, and land said to be public but occupied by private individuals, often referred to as *possessores*. Some allotments are said to have been actually made from land of the first type, none from the second. Throughout this narrative, Livy generally takes pains to avoid taking an authorial stance about the accuracy of the legislators' claims about the land in private hands. Thus, when reporting Cassius' original proposal, he tells us that the land to be divided was in part confiscated from the Hernici, and then adds the following:

He proposed to add to this gift some land which he alleged was public and occupied by private individuals. This alarmed many of the senators, occupiers themselves, at the danger to their wealth.<sup>100</sup>

Thus Livy avoids passing judgement about whether the land in question was actually public (let alone whether it was right for it to be reclaimed): he reports Cassius' claim that it was public, but in styling the landholders *possessores* he makes no claim about the ownership of their land. In other passages too which relate to agrarian proposals he speaks in his own voice just of *possessores* without specifying whether the land they occupied was in fact public.<sup>101</sup>

However, in his reports of their words and feelings, Livy portrays the *plebs* and its champions as maintaining eloquently that public lands were being unjustly occupied and newly acquired land withheld from

<sup>99</sup> References at Oakley (1997, 433–4).

<sup>100</sup> Livy 2.41.2: *adiciebat huic muneri agri aliquantum, quem publicum possideri a privatis criminabatur. id multos quidem patrum, ipsos possessores, periculo rerum suarum terrebat.*

<sup>101</sup> Thus Livy 3.1.3 (where he reports the *possessores* as complaining that the proposer was making himself popular *largiendo de alieno*) and 4.36.2. However, at 2.61.2 he describes Ap. Claudius as *causam possessorum publici agri...sustinenti*—the hardliner Claudius, it seems, was happy to defend the occupation of public land.

distribution. In all these passages, the claim that the lands being denied to the people are public is given explicit stress. In 413 BC Livy reports popular disaffection, and comments as follows (4.51.5–6):

The time would have been most opportune, now that the mutiny had been punished, for offering the distribution of the land of Bola to soothe men's spirits. By doing this, they would have reduced the desire for an agrarian law which sought to drive the patricians from what was felt to be their unjust occupation of public lands. As things were, this particular grievance heightened the resentment: not only, it was felt, did the nobles persist in retaining the public lands which they held by force, but they would not even distribute to the people the empty land which had lately been captured from the enemy—land which would soon, like the rest, be the spoil of the few.<sup>102</sup>

In 410 BC the tribune M. Menenius proposes an agrarian law and vetoes the levy, declaring that he would lift his veto 'if unjust owners would withdraw from their occupation of public land' (4.53.6: *si iniusti domini possessione agri publici cederent*). In 388 BC tribunes complained that the Pomptine land, which had recently been taken from the Volsci and should be distributed among the people, was being occupied by the nobility: 'noble individuals were encroaching into the occupation of the public land' (6.5.4: *nobiles homines in possessionem agri publici grassari*). In 385 BC at the height of his demagogic agitation, M. Manlius Capitolinus alleges that the patricians have embezzled public funds, 'no longer content with occupying public lands' (6.14.11: *nec iam possidendis publicis agris contentos*).

In Livy's narrative of the Licinio-Sextian laws, a very different pattern can be observed. As we have seen, in his initial report of the laws' promulgation he describes the law *de modo agrorum* as prescribing just that 'no one should occupy more than 500 *iugera* of land', and this is followed by Livy's characterization of the law in his own authorial voice as a threat to unlimited desire for land, a claim which he later reiterates.

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<sup>102</sup> *aptissimum tempus fuerat, uindicatis seditionibus, delenimentum animis Bolani agri diuisionem obici, quo facto minuisent desiderium agrariae legis quae possesso per iniuriam agro publico patres pellebat; tunc haec ipsa indignitas angebat animos: non in retinendis modo publicis agris quos ui teneret pertinacem nobilitatem esse, sed ne uacuum quidem agrum, nuper ex hostibus captum, plebi diuidere, mox paucis, ut cetera, futurum praedae.* Cf. Livy 4.48.2, where a tribunician proposal 'that land captured from enemies should be distributed individually' (*ut ager ex hostibus captus uirtim diuideretur*) is represented as threatening a majority of the nobles, since almost all Roman land had at some time been acquired by capture.

At three points in his narrative of the ensuing struggle over the proposed laws Livy permits Licinius and Sextius to express their own view about the land law. First, they interrogate leading senators at a *contio* (6.36.11):

Would they dare to demand that, when two *iugera* of land apiece were being distributed to the *plebs*, they themselves should be permitted to have more than 500 *iugera*, so that each of them might occupy the land of nearly three hundred citizens, while for a plebeian his own land would barely suffice for a roof over his head and a place for his burial?<sup>103</sup>

They go on to assure their audience that, unless one consul a year was a plebeian, ‘there would never be any limit on the patricians’ seizing of lands or butchering the *plebs* with usury’ (6.37.2: *nec agros occupandi modum nec fenore trucidandi plebem alium patribus unquam fore*). At a later meeting, they promise the *plebs* that ‘it would be able to have the city and the forum free from creditors, the fields free from unjust occupiers at once if it wished’ (6.39.9: *liberam urbem ac forum a creditoribus, liberos agros ab iniustus possessoribus extemplo, si uelit, habere posse*).

Subsequently, Livy permits a patrician spokesman—as usual, an Ap. Claudius—to present the opposing view of the land and debt proposals (6.41.10–11):

Let Sextius and Licinius reign in the city of Rome like Romulus and Tatius, since they are giving away others’ fortunes and lands. Is it so sweet to plunder others’ fortunes that it does not come to mind that by one law vast solitudes are being created in the fields through the expulsion of owners from their lands, and by the other trust is being overthrown and, as a result, the bonds of human society are being destroyed?<sup>104</sup>

In the tribunes’ speeches the landholders’ right to their lands is called in question by the use of *possidere* (‘occupy’) and still more *occupare* (‘seize’) and finally by the reference to ‘unjust *possessores*’. In response, Claudius insists that the measure will drive owners from their lands and so create a desert (thus impudently inverting the *topos* attributing this effect to

<sup>103</sup> *auderentne postulare ut, cum bina iugera agri plebi diuiderentur, ipsis plus quingenta iugera habere liceret ut singuli prope trecentorum ciuium possiderent agros, plebeio homini uix ad tectum necessarium aut locum sepulturae suus pateret ager?* The reference to two *iugera* relates to allotments in early colonies: cf. Oakley (1997, 676–7).

<sup>104</sup> *Sextius et Licinius tamquam Romulus ac Tatius in urbe Romana regnent, quia pecunias alienas, quia agros dono dant. tanta dulcedo est ex alienis fortunis praedandi, nec in mentem uenit altera lege solitudines uastas in agris fieri pellendo finis dominos, altera fidem abrogari cum qua omnis humana societas tollitur?*

slave-run *latifundia*).<sup>105</sup> However, in marked contrast to the rhetoric of earlier Livian tribunes, Licinius and Sextius make no reference whatever to public land and at no point suggest that their measure is aimed at redressing its unjust occupation.

This silence, across several utterances attributed to the tribunes, cannot but be significant. Livy, it seems clear, is portraying a change in the popular movement's strategy. Earlier tribunes unsuccessfully sought to carry agrarian laws whose purported aim was to reclaim and distribute public land; by contrast, Licinius and Sextius succeed in carrying a law which limits holdings of all land, irrespective of ownership. As for the law itself, Livy could not but approve: he had shown both sides indulging in exaggerated rhetoric, but, as his own repeated statements assert, the result was a check on cupidity, the vice which he had identified at the start of his work as having led ultimately to the downfall of the Republic (*Praef.* 11–12).

Thus our examination of the sources for the pre-Gracchan limit has yielded powerful support for the view of Hüllmann, Huschke, and Kunkel that it applied to holdings of all land, whether owned or merely occupied, public or private. This is the plain meaning of all the numerous sources for the limit, except perhaps Appian and Plutarch. It is also the clear implication of Livy's extended narrative.

We must now turn to Tiberius Gracchus' law. This might be thought to provide the strongest argument for the Niebuhrian view. With good reason, no one since Niebuhr's day has sought to defend the view, earlier adopted by, for example, Ferguson, that Gracchus' 500-*iugera* limit was to apply to private property in land, let alone all landholding. Such a restriction would by that time have been wholly unrealistic, and it is inconceivable that Gracchus would have attempted it. In any case, we have it on the authority of Cicero that 'Tiberius and Gaius Gracchus established the *plebs* on public lands, which were formerly occupied by private individuals' (*Leg. Agr.* 2.10: *Ti. et C. Gracchos plebem in agris publicis constituisse, qui agri a priuatis antea possidebantur*). Thus, if we follow Hüllmann and Huschke in holding that the pre-Gracchan limit applied to all landholding, we must, like them, suppose that, when Gracchus sought to enforce the limit, he restricted its application just to public land. This hypothesis requires us to reject the implication

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<sup>105</sup> Livy had recently alluded to this theme, at 6.12.5: see Kraus (1994, 162, 326); Oakley (1997, 506).

of Appian (*BC* 1.9.37, 11.46, cited above) that Gracchus reapplied the existing law, conceding just the secure tenure of land held within the limit and the extra allowances for children. Heuss, one of the few recent scholars to have noticed Huschke's view, dismissed it, with its implication that the pre-Gracchan and Gracchan limits did not have the same application, as "grotesk" and as "eine Absurdität, die wieder einmal zeigt, wie problematisch in den historischen Wissenschaften der Fortschrittsbegriff ist".<sup>106</sup>

By comparison with the rich attestation for the pre-Gracchan restriction, the sources for Gracchus' limit are meagre. Appian's statements, the vague references by Plutarch (*TG* 9.2, 10.4), and the statement of Velleius (2.6.3) purporting to refer to C. Gracchus have all been cited above. The most substantial further item occurs in the Periocha of Livy Book 58. The epitomator opens his summary of the book by reporting Ti. Gracchus' proposal of a *lex agraria* which provided *ne quis ex publico agro plus quam ∞ iugera possideret* ('that no one should occupy more than 1000 *iugera* from public land'), and, after reporting the passage of the law and appointment of the land commission, he adds: *promulgavit et aliam legem agrariam, qua sibi latius agrum patefaceret, ut idem triumviri iudicarent, qua publicus ager, qua privatus esset* ('In order to put more land at his disposal, he also promulgated another agrarian law, that the same triumvirs should judge which was public and which private land'). Two further items remain. The *De uiris illustribus* states (64.2) that Tiberius carried a law *ne quis plus mille agri iugera haberet* ('that no one should have more than 1000 *iugera* of land'). Finally, Siculus Flaccus states the following of a Gracchus, who may be either Tiberius or Gaius (*de condicionibus agrorum*, 102.31–33 Campbell):

Moreover, he passed a law to prevent anyone in Italy from occupying (*possidere*) more than two hundred *iugera*, for he realized that it was a harmful custom that anyone should possess a greater area of land than could be cultivated by the occupier himself.<sup>107</sup>

These passages yield puzzling divergences over the specified limit, which cannot be eliminated by emendation, as by Sigonio's proposal to correct the Periocha's 1,000 to 500 or Mommsen's emendation of Siculus Flaccus' 200 to 1,000. Siculus Flaccus' variant remains an isolated eccentric-

<sup>106</sup> Heuss (1981, 333–4).

<sup>107</sup> *praeterea legem tulit, ne quis in Italia amplius quam ducenta iugera possideret: intellegebat enim contrarium esse morem, maiorem modum possidere quam ab ipso possidente coli possit.*

ity.<sup>108</sup> The figure of 1,000 *iugera* given by the Livian Periocha and the *de uiris illustribus* represents a genuine and perhaps preferable alternative tradition to Appian's 500. Niebuhr, in his lectures, sought to reconcile the two traditions by supposing that the additional allowance of 250 *iugera* for children (or perhaps just sons) reported by Appian was limited to a maximum of two.<sup>109</sup> This neat solution has been widely accepted, but, as Badian observed, such a restriction was hardly consistent with Gracchus' declared aim of encouraging child-rearing.<sup>110</sup>

As to the application of the law, Velleius, the *De uiris illustribus*, and Siculus Flaccus all present it, in the same terms as the numerous sources for the pre-Gracchan limit, as restricting the amount of land a man might 'have' (*habere*) or 'occupy' (*possidere*), and this is reinforced for Siculus Flaccus by the moralizing comment which he appends. However, these are unreliable writers, whose testimony on such matters can carry little weight and is in no way comparable to that of the numerous and authoritative sources for the pre-Gracchan limit.<sup>111</sup> Much more significant is the statement of the Livian Periocha that Gracchus' law established the maximum a man might hold 'from public land' (*ex agro publico*). There is no reason to doubt that this accurately represents Livy's own account, and confirmation that he specified Gracchus' limit as applying to *ager publicus* is provided by the following statement about the additional law empowering the land commission to adjudicate on what was public land.

It therefore follows that Livy, who, as we have seen, represented the Licinian limit as applying not just to *ager publicus* but to all landholding, portrayed Gracchus as enforcing a limit only on *ager publicus*. We may reasonably infer that Livy's full account made it explicit that Gracchus was reviving the Licinian limit but only as applying to *ager publicus*.

Thus the view of Hüllmann and Huschke, which Heuss dismissed so scornfully, turns out to be supported by the authority of Livy himself, and we can therefore accept it. Given the poor quality of our sources for the terms of Gracchus' law, we need not be concerned that we have no explicit attestation that Gracchus restricted the application of

<sup>108</sup> See Campbell (2000, 369).

<sup>109</sup> Niebuhr (1870, 500).

<sup>110</sup> Badian (1972, 702–3).

<sup>111</sup> One may compare the widely variant statements in such sources about, for example, the late-second and early-first-century BC proposals on the franchise and on jury composition.

the existing limit from all landholding just to holdings of *ager publicus*. Both Appian and Plutarch attest that he sought to conciliate the landholders by not enforcing the existing law in its full rigour. Each writer only imperfectly reflects their common source, and so it is possible that the source did state that this modification had been made to the land affected. Certainly, Appian's presentation of Gracchus' law as, apart from the concession to children, a straightforward renewal of the earlier limit may in part be his own contribution. However, the common source was clearly concerned to present the pre-Gracchan limit in the context of a narrative in which the history of the public lands and agrarian developments would lead up to Gracchus' remedial actions, and this may have led the writer to elide the difference between the application of the two laws.

We must now consider the alternative suggestion of Puchta and Rathbone that the pre-Gracchan limit applied only to ownership of private land, with Rathbone's further thesis that the Gracchan limit had the same application, but was enforced only in respect of holdings of public land, which were now made private. These proposals do not seem to me attractive. A restriction of either limit to ownership of private land is not supported by the sources, being in particular difficult to reconcile with their widespread use of *possidere*, and the arguments offered in favour of the hypothesis are not convincing. It is not clear how Gracchus' law could have been drafted along the lines Rathbone suggests: what wording could have provided for the implementation of a limit on private ownership in respect of newly privatised public land, but not for pre-existing privately owned land? In any case, as De Ligt has argued, 'the undisputed tenure, without charge and secure for ever' which, according to Appian (*BC* 1.11.46), Gracchus granted to holders of public land up to the limit, is best interpreted not as privatisation, but as *possessio perpetua* of *ager publicus*. It was not until the post-Gracchan legislation that holdings of public land up to the permitted limit were made private.<sup>112</sup>

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<sup>112</sup> De Ligt (2001); App. *BC* 1.27.122–3; *Lex Agr.* of 111 (Crawford 1996, no. 2), lines 1–10 (the limit: line 2). The relationship between the inscribed law and those reported by Appian remains a matter of dispute.

IV. *Conclusion*

This paper has sought to show that the pre-Gracchan limit related to all landholding, but, when Tiberius Gracchus revived the limit, he applied it only to holdings of *ager publicus*. This view respects the strong consensus of our sources on the pre-Gracchan limit, including such comparatively early authorities as Cato, Tiro, and Varro and the extended narrative of Livy. It involves rejecting some aspects of the account given by Appian and perhaps also Plutarch, but this is acceptable in view of the distortions evident in Appian's narrative and perhaps also in that of Appian's and Plutarch's common source. It is my contention that this solution is preferable to the usual account according to which both limits related just to *ager publicus*: the latter interpretation fits well with the narratives of Appian and Plutarch, but fails to take adequate account of the consensus of the numerous other sources on the pre-Gracchan limit.

Niebuhr and his predecessors esteemed Appian's authority highly and sought to reconcile his account with the evidence of the other sources. The resulting tensions are evident in the careful treatments of writers like Sigonio and Hooke. Another factor was a belief that the Roman agrarian laws and limits on landholding aimed at restoring a primitive equality: this can be seen in the discussions of, for example, Machiavelli, Montesquieu, and Ferguson, and helped to shape the concept of the 'agrarian law' which was current at the time of the French Revolution. Heyne, Heeren, Hegewisch, and Niebuhr all reacted (perhaps independently) against this development, and Niebuhr was also led by his interest in contemporary land issues to his examination of the Roman *ager publicus* and its *possessio*, which formed the starting point for the modern discussion of this still arcane subject.<sup>113</sup> The impact of Niebuhr's history placed the issue of the land limits and his view that they both related to *ager publicus* at the forefront of discussion and prompted Hüllmann and Huschke to develop what in my view is the correct solution. Unfortunately, the authority of Niebuhr and his supporters soon foreclosed the discussion. As a result, modern scholars have generally taken Niebuhr's thesis for granted and overlooked the

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<sup>113</sup> Notable modern treatments include Zancan (1935); Bozza (1939); Kaser (1942); Tibiletti (1948; 1949; 1950); Burdese (1952; 1985); Botteri (1992); Gargola (1995); Serrao (1999, 378–401).

resulting source conflict. Kunkel and Rathbone have reopened the debate, and this paper seeks to carry it forward.

The conclusions for which I have argued in respect of the pre-Gracchan and Gracchan limits have wider consequences of some significance for the history of the Roman Republic. These cannot be explored in full within the compass of this paper, but the following remarks seek to set my contentions in this broader context and point out some of the implications.

Private property, including the individual private ownership of land, was central in early Roman society and law, as the evidence of the Twelve Tables confirms. Mommsen supposed that property was for long held in common by the *gentes*, and Capogrossi Colognesi has maintained that, until the Licinio-Sextian legislation, occupied *ager publicus* was held communally by the (in his view, exclusively patrician) *gentes*. These doctrines have no support in the sources and should be rejected.<sup>114</sup>

There was little expansion of the territory of the Roman state in the sixth and fifth centuries and, although there will have been common grazing lands, there was probably little public land available for occupation at that period. How much truth, if any, there may be in the reports of fifth and early-fourth-century BC agrarian agitation has been a matter of debate since their authenticity was first impugned by Niese.<sup>115</sup> Some of the Latin colonies said to have been founded in that period must be authentic, and it may be that such disputes as there were in the fifth century BC centred on recently acquired lands at the periphery of Roman and Latin territory rather than the holdings of the elite.

The major Roman successes of the early fourth century BC yielded substantial acquisitions of public land: all the territory of Veii (estimated by Beloch as some 562 km<sup>2</sup>),<sup>116</sup> and significant gains in the Pomptine area at the expense of the Volsci. Survivors of the old inhabitants continued to occupy some Veientine land, and there were virgane distributions of land in both regions. However, a good deal of this public land may have remained available for occupation, and it is not unlikely that this

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<sup>114</sup> Mommsen (1888, 22–29), rebutted by Pöhlmann (1925, 327–41); cf. Momigliano (1994, 228, 237). Capogrossi Colognesi (1980, 1983) is followed by Hermon (2001, 1–170), but see the critiques of Serrao (1999, 166–70) and Smith (2006, 235–50).

<sup>115</sup> Niese (1888). Optimistic judgements: Cornell (1995, 268–71); Oakley (1997, 433–4).

<sup>116</sup> Beloch (1926, 620).

became a matter of dispute, with resentment developing at what was suspected to be excessive occupation by elite members. This would provide a plausible context in which the issue of limiting landholding may have first been mooted.

Although much of the detail of Livy's elaborate narrative of the Licinio-Sextian Rogations must be fictitious, it is likely that it contains a core of authentic material and that at some time around 367 BC tribunes did indeed succeed in carrying through a package of legislation which addressed both constitutional and socio-economic concerns.<sup>117</sup> One of these laws will have dealt with land, and will have set a limit of 500 *iugera* on all landholding, comprising both the ownership of private land and the occupation of other land, public and private, on pain of a fine to be imposed by the aediles. Since, outside Livy's narrative of the crisis, only C. Licinius Stolo is associated with this law, he was probably its sole author.

There is no need to doubt this context and (approximate) date for the introduction of the 500-*iugera* limit. The argument that the limit is too high for this dating lapses once it is realized that it applied to all holdings of all land, not just *ager publicus*: it is plausible to suppose that the upper end of the range of elite landholdings may then have fallen around that point, and therefore that it would have been an appropriate choice for a maximum.<sup>118</sup> The later date which Appian appears to imply for the introduction of the limit is not a difficulty: Plutarch surely reflects the common source more accurately in placing his account of developments like the spread of slave labour after the introduction of the limit. There is no good reason to dissociate the measure from the context assigned to it by the tradition, as does Rathbone, proposing a dating around 300 BC.<sup>119</sup>

Concerns about the occupation of *ager publicus* may perhaps have given rise to the law, and it may have been expected that more land would become available for the poor to occupy, as those holding land in excess of the limit came into compliance. But this cannot have been the primary purpose of a law applying to all forms of landholding, whose only sanction was an aedilician fine and which made no provision for subsequent distribution. Such a law must reflect a belief

<sup>117</sup> See Cornell (1995, 327–40); Oakley (1997, 645–61).

<sup>118</sup> Cf. Kunkel (1995, 495).

<sup>119</sup> Rathbone (2003, 146–9).

that the concentration of too much land in the hands of particular individuals was not to the good of the community, and so, like other forms of misconduct, should attract aedilician sanction. As we have seen, several of our ancient sources for the law interpret it in moral terms: Livy regarded it as a restraint on greed (6.35.6, 10.13.14, 34.4.9); Gellius (20.1.23) includes it in a list of salutary measures; and Columella (1.3.11) comments on the law as checking an ‘unrestrained passion for occupying land’. These remarks show that in later times the law was often understood in moral terms, a conception which will go back to the time of its passage.<sup>120</sup>

As Gargola has argued, the law enforced a moral standard, like the later sumptuary laws.<sup>121</sup> The passing of such a measure is significant evidence for Roman society, beliefs, and attitudes in the fourth century BC, that crucial period which saw the beginnings of Roman expansion and the formation of the patricio-plebeian nobility.<sup>122</sup> The reform may perhaps be associated with the funerary restrictions of Table X of the Twelve Tables.<sup>123</sup> It is certainly linked with other checks on moral misconduct enforced by the aediles, including the contemporary restrictions on usury.<sup>124</sup> Similar attitudes were displayed later by censors and others when Roman conquest led to enhanced luxury, as (to take a famous early example) when C. Fabricius Luscinus as censor in 275 BC expelled P. Cornelius Rufinus from the senate for possessing ten pounds of silverware.<sup>125</sup> The legends that accrued about the modest means of men like Fabricius and his colleague Curius Dentatus are not to be wholly dismissed: behind these tales may lie authentic traces of at least some contemporaries’ attitudes.<sup>126</sup>

The limits on the number of animals to be pastured are mentioned only by Appian and Cato, and only Appian affirms that they came from the same law as the landholding limit (the Cato passage, often

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<sup>120</sup> The attempt of Tibiletti (1948, 219–25; 1949, 20–7) to deduce a primitive restriction of landholding to what the holder could cultivate himself from this passage of Columella and from Siculus Flaccus’ statement on Gracchus’ law, is mistaken, as Mantovani (1997) has shown, but the passages nonetheless reflect traditional ethical assumptions.

<sup>121</sup> Gargola (1995, 143–5).

<sup>122</sup> On these themes see especially Hölkeskamp (1987, 1993).

<sup>123</sup> But see Toher (2005).

<sup>124</sup> For aedilician prosecutions see Bauman (1974); Kunkel (1995, 490–504); Oakley (2005, 259–61).

<sup>125</sup> Torelli (1978, 199–202) reproduces the sources for this episode. On Roman regulation of private conduct by sumptuary and other measures see Baltrusch (1989).

<sup>126</sup> *Contra* Harris (1979, 65–7, 264–5).

taken to imply this, need not in fact do so). It must remain uncertain whether they formed part of the Licinian law or were introduced on another occasion. These limits too may have applied to pasturing on all land, not just public land, and, if so, could well date to as early as c. 367 BC.<sup>127</sup> Aediles are reported as fining *pecuarii* in 296, 293, 241 (or 238), 196, and 193 BC.<sup>128</sup> The notices in general do not state the herdsmen's offence, which was not necessarily pasturing beasts in excess of the limit, although that is the most likely explanation.<sup>129</sup>

The other provision of the pre-Gracchan law mentioned by Appian, that a minimum number in each work force should be free men, is surely fictitious, perhaps colouring introduced by his source. It recalls the dictator Caesar's ordinance that those engaged in pasturage should employ free men for at least a third of their herdsmen (Suet. *Iul.* 42.1), and evidently reflects the concerns of the Gracchan and post-Gracchan period. Similarly, Appian's claim that an oath was sworn to the law must be later fiction: other evidence for such a requirement is no earlier than the end of the second century BC.

Attempts were made for some time to enforce the Licinian law's restriction of landholding to 500 *iugera*. Although the story of Licinius Stolo's conviction is *ben trovato*, Livy's notice of prosecutions in 298 BC is authentic, and this is unlikely to be the only year in which such prosecutions took place. However, as elite wealth grew, the restriction will have become increasingly unrealistic, and prosecutions will have ceased: it is significant that we hear of no prosecutions for this offence after 298 BC, whereas prosecutions of *pecuarii* continue into the early second century BC. Thus, when, in 167 BC, Cato chose to allude to the restriction when urging the senate not to vote for war on Rhodes, he was adducing a statute which remained in force but was no longer implemented and of which many of his hearers will have been in breach. However, many may still have paid lip service to the principle that excessive landholding was undesirable, and not a few senators' holdings may have been within the limit. It is indeed likely that Cato himself took care to stay within the limit: he would hardly have chosen to mention it in his speech if he did not. Plutarch's tale of his varied

<sup>127</sup> See also Skydsgaard (1974), criticizing Tibiletti's objections to an early date for the restrictions.

<sup>128</sup> Livy 10.23.13, 47.4; 33.42.10; 35.10.11–12. The Publicii in 241 BC or 238 BC: Festus 276 L; Ovid *Fasti* 5.283–94 (Broughton 1951, 220 n. 3, for the date).

<sup>129</sup> Cf. Forsén (1991, 75–6). Ovid represents the Publicii as fining herdsmen just for using public land.

investments suggests strategies by which an enterprising but principled senator might enrich himself while respecting the Licinian prescription, as well as other constraints such as the Claudian law limiting senators' shipowning.<sup>130</sup>

When Tiberius Gracchus promulgated his agrarian law in 133 BC, he was impelled by developments in respect of land and manpower and concerns (whether real or imagined) which were altogether different from those which the Licinian law *de modo agrorum* had sought to address.<sup>131</sup> Acting, as he believed, in the national interest, he boldly set out to enforce the Licinian limit, probably for the first time since the early third century BC, using the new mechanism of the land commission, in order to obtain land for distribution. He and his eminent advisers sought to conciliate opposition by a range of concessions. Some details of these concessions are preserved in the scrappy notices in our sources which are all that survives of a document which was surely at least as long and complex as the extant agrarian law of 111 BC. The limit was raised, whether to 1000 *iugera*, as the Livian tradition has it, or by granting additional allowances for children, as Appian reports, or both. There would be secure tenure of public land up to the limit, and, although holdings of public land over the limit must be surrendered, no fine would be imposed (as under the old procedure) and instead, at least initially, compensation was offered. Our inadequate record has, however, preserved no explicit mention of the most important concession of all. The limit was to be applied, not, as the Licinian law prescribed, to all landholding, but just to holdings of public land. Very likely Gracchus' law included an express provision removing private land from the application of the limit.

Even with these concessions, Gracchus and his supporters will have expected strong opposition, including a tribunician veto. However, they could reasonably have expected that, given the range of concessions that had been offered, and once popular enthusiasm had been mobilized for the bill and distinguished senators had deployed their authority in its support, the vetoing tribune would back down, as had happened with the Cassian ballot law four years earlier.<sup>132</sup> However, M. Octavius was

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<sup>130</sup> Plu. *Cato* 21.5–8. For senatorial landholding in the pre-Gracchan period see Shatzman (1975, 11–18, 241–61).

<sup>131</sup> For these developments see Rich (2007) and the papers in the present volume.

<sup>132</sup> See Badian (1972, 690–701).

obdurate and Gracchus responded by deposing him, so unleashing the chain of events which would end in the Republic's fall.

Gracchus and his supporters must have believed that substantial amounts of *ager publicus* were in occupation over the legal limit and would thus be released for distribution under this law. It may well be that in fact most such *ager publicus* was outside central Italy and in allied occupation, as Saskia Roselaar argues in this volume, and the rapidity with which the land commission came into conflict with the Italian allies lends support to that view. However, the strength of the elite opposition does indicate that a significant element in the citizen body stood to lose under the law.

Both Appian and Plutarch attest to the eloquence with which Tiberius Gracchus had advocated his law (App. *BC* 1.9, 11; Plu. *TG* 9.4–6). Since his law enforced the old limit specifically on holdings of public land, Gracchus will necessarily have laid emphasis on that land, arguing that observance of the limit was especially appropriate there. This may have led him to make claims about the original purpose of the public lands and their occupation, explaining them in terms of the same concern for manpower which prompted his own law, and he may also have interpreted the earlier, Licinian measure in the same way.

It has long been recognized that the Gracchan episode had a huge impact on the Roman historiographical tradition and in particular on its depiction of agrarian issues. This is apparent in the tradition's account of agrarian agitation in the early Republic: the extant narratives of Sp. Cassius' law are transparently influenced by the Gracchan legislation, and, as has been supposed by numerous scholars from Niese on, the bulk of the narrative of plebeian agitation in the fifth and early fourth century BC against patrician occupation of *ager publicus* is probably a post-Gracchan confection. Similarly, as Gargola (cited above, n. 86) has observed, the account of the history of the *ager publicus* and of agrarian developments given, following a common source, by Appian and Plutarch is evidently conditioned by the Gracchan crisis to which it leads up, with the purpose of both the *ager publicus* itself and the pre-Gracchan limit being interpreted in terms of Gracchan preoccupations. In essence, this interpretation probably derives ultimately from Tiberius Gracchus' own advocacy of his law. Appian reinforced this interpretation with his own contributions, including the, as we can now see, erroneous claim that Gracchus' law had the same scope as its predecessor. Niebuhr's interpretation of the pre-Gracchan limit, which has so long held the field, derived from excessive respect for

Appian's authority. Fortunately the substantial body of other evidence that survives for the limit established by C. Licinius Stolo shows that, as Hüllmann and Huschke saw, it applied, unlike Gracchus' limit, not just to public land but to all landholding. Our accounts of both the fourth century BC and the Gracchan crisis must be revised to accommodate this finding.<sup>133</sup>

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# REGIONAL VARIATIONS IN THE USE OF THE *AGER PUBLICUS*

Saskia T. Roselaar

## I. *Introduction*

It has long been recognized that the *ager publicus* played an important role in the economic and social developments of the second century BC.<sup>1</sup> However, many time-honoured conceptions about the role of this category of land have recently been questioned. According to the traditional view, which seems to be supported by the ancient sources, a small group of wealthy people occupied the *ager publicus* and used it to establish large slave-staffed estates aimed at market production. This then resulted in the exclusion of small farmers from the land and a decrease in the free Roman citizen population, and this was the problem which Tiberius Gracchus wished to address. However, the assumptions on which this reconstruction is based have recently been challenged. Most notably, Rathbone has argued that the amount of *ager publicus* was actually very limited because most of the confiscated land had been privatized.<sup>2</sup> It has also been argued that competition for land among the elite was not yet very great in the second century: urbanisation was not as substantial as it would later become, and the market for agricultural products was correspondingly smaller.<sup>3</sup> This has serious implications for the idea of aristocratic competition for land: if there was no market for the products of a great number of large estates, there was no reason to accumulate large tracts of land, whether public or private.

The aim of this paper is to review the validity of the objections that have been raised against the traditional reconstruction of second-century developments. First, I shall examine the extent of the *ager publicus* in the second century. Then I shall consider the size of the market and the possibilities for elite competition. Finally, I shall discuss the role played

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<sup>1</sup> All dates are BC unless indicated otherwise.

<sup>2</sup> Rathbone (2003).

<sup>3</sup> De Ligt (2004); Jongman (2003).

by the *ager publicus* in the economic developments of the second century. In so doing, I hope to show that the role of the *ager publicus* displayed significant regional variation.

## II. *The amount of ager publicus in Roman Italy*

The idea that *ager publicus* played an important role throughout republican history has never been seriously questioned. Recently, however, Rathbone has argued that the amount of *ager publicus* was actually quite limited. He states that “*ager publicus* was essentially a transient category in which conquered and annexed land rested pending its transfer to private ownership”.<sup>4</sup> He admits that “from the late fourth century . . . as the scale of annexation mushroomed, more pasture, woodland and wetland was retained in state ownership as *ager publicus populi Romani*, and left open to almost unfettered use by Roman citizens.”<sup>5</sup> But the retention of arable *ager publicus* in state hands did not occur, according to Rathbone, until after the Second Punic War: “most of these lands [confiscated in the Second Punic War] . . . are normally, and plausibly, assumed to have remained occupied by their previous owners or possessors”.<sup>6</sup>

This view does not take into account various attestations to the presence of *ager publicus* in areas of Italy that had been conquered by the Romans before the Second Punic War. In many areas land appears to have remained in the hands of the state for a very long time after its confiscation. The most striking examples can be found in the Ager Gallicus and in Etruria. The Ager Gallicus was confiscated by the Romans in 290.<sup>7</sup> Two colonies were founded fairly soon after its confiscation: Sena Gallica in the 280s, and Ariminum in 268,<sup>8</sup> which is actually already twenty-two years after the confiscation of the land. In 232 Gaius Flaminius carried out a distribution of land in this region,<sup>9</sup> and

<sup>4</sup> Rathbone (2003, 175).

<sup>5</sup> Rathbone (2003, 149).

<sup>6</sup> Rathbone (2003, 150).

<sup>7</sup> Liv. *Per.* 11.6; Fron. *Str.* 1.8.4.

<sup>8</sup> Liv. *Per.* 11.7, 15.5.

<sup>9</sup> Plb. 2.21.7–8; Cic. *Brut.* 14.57; *Sen.* 4.11; *Inu.* 2.52; *Ac.* 5.13, Cato fr. 43 Peter (Var. *R.* 1.2.7). There has been much discussion as to which land exactly is meant as being distributed. Cicero consistently refers to the *ager Gallicus et Picenus*, which has led scholars to believe that both the Ager Gallicus and the *ager publicus* still available in Picenum were distributed in 232: Marcone (1997, 144); Humbert (1978, 237). Polybius, however,

in 184 the colony of Pisaurum was established.<sup>10</sup> Moreover, there is some evidence for Gracchan activity in the *Ager Gallicus*: a boundary stone referring to the measuring of land by the Gracchan land commission has been found in Fanum Fortunae.<sup>11</sup> It is clear that there was much arable *ager publicus* in this area, and that some of it had remained the property of the Roman state for almost a hundred and sixty years after its confiscation.

In Etruria something similar can be observed. Large areas of southern Etruria were confiscated in 281. Some colonies were established after the confiscation: Cosa in 273,<sup>12</sup> and the maritime colonies of Fregenae, Pyrgi, and Alsium during the First Punic War,<sup>13</sup> which is again already several decades after the confiscation of the land. It appears from events during the Second Punic War that there was still *ager publicus* in Etruria: in 210 part of the population of Capua was deported to the region beyond the Tiber, where they were 'forbidden to acquire or to hold either for themselves or their posterity landed property anywhere except in the territories of Veii, Sutrium, and Nepes, and in no case was such a holding to exceed fifty *iugera*'.<sup>14</sup> This does not mean that the deported Capuans were actually provided with land in a regular

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calls the distributed land 'Picenum, the land from which they had ejected the Senones when they conquered them', which seems to refer to the *Ager Gallicus* alone. Cato says 'the land lying this side of Ariminum and beyond the district of Picenum, which was allotted to colonists, is called the Roman *Ager Gallicus*' (*ager Gallicus Romanus uocatur, qui uirum cis Ariminum datus est ultra agrum Picentium*). Notwithstanding Cicero's use of the words *ager Gallicus et Picenus*, it would therefore seem more likely that Flaminius distributed only the *Ager Gallicus*. Since Polybius and Cato are the older sources, it is more likely that they are correct and that Cicero was mistaken. Cf. Oebel (1993, 31–2); Beloch (1926, 476).

<sup>10</sup> Liv. 39.44.4, Vell. 1.15.2.

<sup>11</sup> *CIL* P. 719 = *CIL* XI.6331 = *ILS* 26 = *ILLRP* 474. Campbell (2000, 452).

<sup>12</sup> Liv. *Per.* 14.8, Vell. 1.14.7.

<sup>13</sup> Liv. *Per.* 19.5 (Fregenae), Vell. 1.14.8 (Alsium). Pyrgi is only known to have been a colony by later references (Liv. 36.3.6). There has been some discussion as to the location of the colony Castrum Novum, which Livy *Per.* 11.7 records for the 280s, but Vell. 1.14.8 for 264. Beloch (1926, 429, 452) assumed that the colony was the town in Picenum of the same name. Hermon (2001, 229) sees Castrum Novum in Picenum as a colony founded in 290 as a partner to Sena Gallica; however, colonies were not always founded in pairs, and if they were they were not necessarily located close to each other. Others, e.g. Salmon (1963, 21), doubt therefore that the colony was located in Picenum, and assume that it was founded in Etruria during the First Punic War. However, since Velleius is notoriously unreliable when it comes to dates, it is likely that Livy is right about the foundation of the colony in the 280s. In that case it would be more likely that the colony was indeed established in Picenum.

<sup>14</sup> Liv. 26.34.10.

settlement scheme, but it is likely that there was some *ager publicus* on which they could live.<sup>15</sup>

In 200 the *ager in trientibus* was created: ‘many of the applicants had stated that there was land everywhere for sale and they wanted to become purchasers; the senate accordingly made a decree that they should have the option of taking any part of the public land within fifty miles of the City’.<sup>16</sup> The fifty-mile radius includes southern Etruria as far as Graviscae, suggesting that there was still some *ager publicus* here. The distribution of previously confiscated land continued with the foundations of the colonies of Saturnia in 183<sup>17</sup> and Graviscae in 181. This last town is explicitly stated to have been founded on land ‘captured from the people of Tarquinii’.<sup>18</sup> which can only have happened in the early third century. The enigmatic colony of Heba may also have been founded in the second century on land that had been confiscated in the third century.<sup>19</sup> Apparently, some public land even survived until after the Gracchan era, since in 91 the Etrurians and Umbrians protested against Livius Drusus’ plans for land distribution ‘because they thought that the Roman *ager publicus*, which was still undivided and which they were cultivating, some by force and others in secret, would at once be taken away from them’.<sup>20</sup> However, it is not stated that this *ager publicus* was located in southern Etruria, and it may have been situated further to the north, where fewer distributions of land had taken place.

In both the Ager Gallicus and Etruria there had been no land confiscations after the conquest of the territory in the early third century, and we must therefore conclude that much arable land here had remained public for a long time after its confiscation. It is therefore certainly not the case that all arable land was distributed as private property while only pastures and woodland remained *ager publicus*.

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<sup>15</sup> Liv. 31.31.4: ‘though we deprived the survivors of their city and territory, we gave them land and a place to dwell in’. However, this does not mean that the state assigned each individual an allotment, and certainly not that they each received 50 *iugera* (thus Rathbone 2003, 142 note 25), which would have been an unexpected reward for their infidelity. Cf. Liverani (1984, 39).

<sup>16</sup> Liv. 31.13.5–6.

<sup>17</sup> Liv. 39.55.6.

<sup>18</sup> Liv. 40.29.1, Vell. 1.15.2.

<sup>19</sup> It is unknown when Heba was founded. The date 128 was suggested by Salmon (1969, 114–5), but there is no evidence for this. It is only known to have been a colony during the Imperial period.

<sup>20</sup> App. *BC* 1.36.

It is important, however, to distinguish between various legal forms of *ager publicus*. Until its distribution by the state the land in the Ager Gallicus and Etruria had been *ager occupatorius*. This meant that this land could be occupied by any Roman citizen who wanted to use it, and that it was not administered or managed in any way by the Roman state.<sup>21</sup> Unfortunately, it is exactly this category of public land whose extent and location is the most difficult to reconstruct. The sources usually mention only the confiscation of land in a certain area, without further information. Livy, for example, records that two-thirds of the land of Privernum was taken in 340.<sup>22</sup> However, we do not know how large the actual amount of confiscated land was, since we do not know how much the defeated community possessed in the first place. Moreover, such a general proportion is often not specified. For instance, Livy describes how the Marsi in 302 ‘were compelled to surrender a portion of their territory’,<sup>23</sup> without specifying an amount or proportion of the land that was taken. Sometimes confiscations of land are not recorded at all, and we can conclude that land was taken only because *ager publicus* in the area is mentioned at some later time. For example, Livy frequently reports that a city, e.g. Caes in 334,<sup>24</sup> was settled as a colony, without any mention of the previous confiscation of land. Yet, colonies could be founded only on *ager publicus*, since the state could not distribute land which it did not own. It is clear that our source material is insufficient to allow a reliable reconstruction of the *ager occupatorius* in Italy.

The same applies a fortiori to the public pasture lands, the *ager scripturarius*. Whereas we can sometimes reconstruct the approximate location of *ager occupatorius* by reference to later distributions, many

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<sup>21</sup> App. *BC* 1.7 states that ‘they announced that this (land) could for the moment be worked by anyone who wished at a rent of one tenth of the produce for arable land and one fifth for orchards’. Many, therefore, assume that rents were demanded from those occupying the *ager occupatorius*, e.g. David (1997, 198); Lintott (1994, 54); Nicolet (1994, 622). This implies that an administration was needed in order to determine who occupied land and how much harvest was obtained from it. However, one of the characteristics of *ager occupatorius* was that it was not measured in any way; Appian describes how the state ‘did not have the leisure’ to allot it. The administration of a rent on the *ager occupatorius* would have created more work for the state than a distribution to individual citizens would have done. Therefore it is likely that, even if rents were demanded in theory, in practice they were not collected: Rathbone (2003, 153); Gargola (1995, 140); Stockton (1979, 214–5); Tibiletti (1948, 183).

<sup>22</sup> Liv. 8.1.3.

<sup>23</sup> Liv. 10.3.5.

<sup>24</sup> Liv. 8.16.13.

pasture lands were never distributed, and it is therefore quite impossible to determine where they were located.

What we do know is that after the Second Punic War many communities in southern Italy—in Samnium, Apulia, Lucania, and Brutium—were punished for their defection to the Carthaginians with confiscations of land. Furthermore, the first decades of the second century saw the final submission of Cisalpine Gaul, where further extensive confiscations took place.<sup>25</sup> When we look in more detail at the distribution of *ager publicus* throughout Italy, some interesting observations can be made. The most important is that while there was much *ager occupatorius* in many areas, especially in southern Italy and Cisalpine Gaul, this was not the case in central Italy. In the *suburbium* of Rome—Latium, Campania, southern Sabinum, and southern Etruria—there remained only a very limited amount of public land. Most of the land in these regions had become the private property of Roman citizens at a fairly early date.

In Latium most of the public land had been distributed as private property to Roman citizens in virgane distributions or in colonies after the Latin War; most of the rural tribes were located in Latium itself.<sup>26</sup> Furthermore, many towns in Latium had been granted the Roman citizenship without losing any land.<sup>27</sup> The same goes for Sabinum, where most local inhabitants had also received Roman citizenship without loss of land, and where the land that had been confiscated was distributed to Roman citizens in virgane distributions.<sup>28</sup> In Etruria, as we have seen, some land had remained public, but this was gradually turned into the private property of Roman citizens with the establishment of colonies in the early second century.<sup>29</sup> Finally, whatever *ager occupatorius*

<sup>25</sup> Liv. 36.39.3, 41.16.8.

<sup>26</sup> The tribes Maecia, Scaptia, Pomptina, Publilia, Oufentina, and Teretina were all located in Latium, and together they occupied much of the land that did not belong to colonies, *municipia* with full citizenship, and allied towns.

<sup>27</sup> Liv. 8.14.9 records the loss of land by Tibur and Praeneste, which remained allied towns; since there is no reference to the later distribution of this land, it may have remained *ager publicus* for quite a long period. The Liber Coloniarum refers to distributions *lege Sempronia* near Velitrae, but there is no other evidence to support this claim. Chouquer et al. (1987, 98) report a land distribution grid in Velitrae, which may date to the Gracchan period. Others, however, are more sceptical; cf. Campbell (2000, 426–7).

<sup>28</sup> The rural tribe Quirina was established in Sabinum in 241: Liv. *Per.* 19.15.

<sup>29</sup> Cicero mentions land being measured for distribution in the territory of Veii and Capena in 46 (*Fam.* 9.17.2: *Veientem quidem agrum et Capenam metuntur; hoc non longe abest*

had been left over in Etruria, Sabinum, and Latium within fifty miles from Rome had been turned into *ager in trientabulis* in 200. This boundary runs from Graviscae via Narnia, Reate, and Alba Fucens to Circeii, and it is therefore likely that most of the remaining *ager occupatorius* within this area had become *ager in trientabulis*, and thus was held with a secure title of possession by its occupiers.

In Campania as well much land was privatized during the third century by means of colonies or viritane distributions.<sup>30</sup> However, in Campania new land was confiscated during the Second Punic War: the territory of Capua, known as the *Ager Campanus*, was confiscated as punishment for Capua's disloyalty. Most of this land was disposed of quickly: new colonies were established along the coast,<sup>31</sup> and the rest of the *Ager Campanus* was disposed of in other ways, as we shall see. This means that the amount of *ager occupatorius* available in Campania was limited. Later references to distributions of land in Campania are scarce; some Gracchan activity is recorded in the *Liber Coloniarum*, and three Gracchan boundary stones have been found in various parts of Campania.<sup>32</sup> However, this activity seems to have been limited to the northern and south-eastern edges of Campania, and to have involved only a small area. The largest remaining tract of *ager publicus*

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*a Tusculano; nihil tamen timeo*), which has been seen by some, e.g. Keppie (1983, 52), as evidence for the continued presence of *ager publicus* in Etruria. However, Cicero alludes to the possibility that his own land in Tusculum might be confiscated, which would indicate that the land in Veii and Capena had also been acquired by confiscation. We cannot therefore use these passages as proof of the continued existence of *ager publicus* in southern Etruria since 396. The same goes for the veterans settled by Caesar and Octavian in the colony at Lucus Feroniae.

<sup>30</sup> The colonies Cales, Fregellae, Interamna, Suessa Aurunca, Minturnae, and Sinuessa were founded in Campania, as was the tribe Falerna.

<sup>31</sup> The colonies of Salernum, Puteoli, Liternum, and Volturnum were all located on land that had been taken from Capua.

<sup>32</sup> The *Liber Coloniarum* records distributions *lege Sempronia* or *lege Graccana* in Abellinum, Aefulae, Caiatia, and Suessa. Gracchan boundary stones have been found at Sant'Angelo in Formis (ancient territory of Capua) and Arienzo (ancient territory of Capua or Abellinum). Chouquer et al. (1987, 150, 168–9, 174, 188–91) report several land distribution grids in these towns that may be Gracchan. Campbell (2000, 382–3, 413–4, 418, 424), however, points out that it is very difficult to date centuriation grids to the actions of specific magistrates, and is reluctant to accept this evidence for Gracchan activity in Campania. He postulates that the Gracchi may only have measured the land and not distributed it. However, since the Gracchi were in principle only interested in public land, the presence of Gracchan boundary stones strongly points to the presence of *ager publicus* in Campania. However, if there was *ager publicus* in Campania, this was limited to the southern and eastern borders of the region.

in Campania was the *Ager Campanus*, and this was not distributed until 59.<sup>33</sup> It is therefore unlikely that during the second century a large amount of *ager occupatorius* was located in central Italy.

This means that the only forms of *ager publicus* still available in central Italy were some special kinds of public land: the *ager in trientabulis*, the *ager quaestorius*, and the *ager censorius*. The amount of each of these types of public land seems to have been small. *Ager quaestorius*, land sold by the quaestors, technically remained *ager publicus*. The buyer had secure title of possession, which would of course have been very welcome to those wishing to invest in production of cash crops. There are only two recorded cases of the sale of land as *ager quaestorius*: the land around Cures Sabini, which was probably sold in the third century,<sup>34</sup> and some land in Campania, where in 205 ‘the quaestors received instructions to sell that part of the Capuan territory which extends from the Fossa Graeca to the coast’.<sup>35</sup> Unfortunately, we do not know exactly where this

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<sup>33</sup> Plu. *Cat. Mi.* 33.1, Var. *R.* 1.2.10, App. *BC* 2.2.10, Vell. 2.44.4, D.C. 38.7.3, Cic. *Att.* 2.16. The presence of Gracchan boundary stones, combined with the statement in Plu. *CG* 8.3 that Gaius Gracchus was planning to found a colony in Capua, may indicate that he distributed the *Ager Campanus* itself, or at least intended to do so; see Badian (1972, 705). However, Cic. *Agr.* 2.29.81 and Gran. Lic. 28.36 state that the Gracchi did not distribute this land; moreover, if the *Ager Campanus* had been distributed by the Gracchi, it would have been privatized, but this did not happen until the distribution of 59.

<sup>34</sup> There has been much debate about when the *ager quaestorius* was first created. The Agrimensores mention the sale of the *Ager Sabinus* several times but do not give dates (ed. Campbell 2000, 102.35–104.3, 119.26–8, 257.12–3). Burdese (1952, 44) admits that the sale of land in Campania in 205 is the first secure instance of the sale of land, but suggests without proof that it may in fact have originated in the fourth century. Hermon (1997, 41–2) argues that the land in Sabinum was not sold until the time of Sulla, but this is very unlikely. The land distributions of Sulla were fundamentally different from those of earlier periods. The most likely solution is therefore that *ager quaestorius* originated with the sale of the *Ager Sabinus* shortly after its confiscation in 290; cf. Rathbone (2003, 151), Chouquer & Favory (1991, 73), Muzzioli (1975, 228). Other references to the sale of land are too vague to be credited: Rathbone (2003, 151) points to DH 20.17.1–2, where there is a reference to land in Samnium being sold, but it is unclear which land was concerned and to whom it was sold. Liv. 26.11.6 mentions ‘the sale by auction of the spot on which he (Hannibal) had fixed his camp, and the fact that, in spite of his occupation of it, there was no reduction in the price’. See also Zonar. 9.6. It is not explicitly said that this was done by the quaestors, but the fact that it was done by auction may indicate that it was similar to *ager quaestorius*. However, this seems to have been more of a propagandistic action than a genuine measure to raise money.

<sup>35</sup> Livy 28.46.4. Chouquer & Favory (1991, 127) are confused on this issue: they say that in 205 Calatia and Atella were sold, but this is never stated in the sources; in 174 there is a reference to ‘the money which they received from the sale of portions of the

land was located; the amount left over as *ager quaestorius* cannot have been great, since most of the Capuan territory became *ager censorius*.<sup>36</sup>

*Ager censorius* was *ager publicus* leased out through the agency of the censors. Again, those who leased the land gained secure possession of it. The leasing out of land as *ager censorius* is recorded only for the Ager Campanus: 'when [P. Lentulus] was *praetor urbanus* [in 165], the senate authorized him to buy up the Campanian territory, which was occupied by private individuals, in order that it should become public land. The owners of the land agreed to let Lentulus set the price, and being a just man he did not deceive them. Such was his moderation that he both served the interests of the Republic and restricted private ownership, and he used public money to buy 50,000 *iugera* of land. He brought the Campanian territory, which had been divided amongst private individuals, into public ownership, and let it out at a fair price. Put in charge of an investigation, he recovered much other land, and left a plan of the territory on a bronze tablet in the temple of Liberty, which Sulla later despoiled.' Although the state had tried to lease out this land twice before, in 209 and 173, it apparently did not succeed until 165.<sup>37</sup>

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State domain' in Auximum and Calatia (Liv. 41.27.10). Atella is not mentioned at all; Auximum and Calatia are reported to have been sold by the censors, not the quaestors. Moreover, these places were not sold in their entirety, only certain *loci publici*.

<sup>36</sup> Vallat (1981, 89)

<sup>37</sup> Gran. Lic. 28.29–36. The state had repeatedly tried to lease out the Ager Campanus. After the land had just been made *ager publicus* in 209, 'a measure was adopted by the plebs, with the sanction of the senate, authorizing [the] censors to let the territory of Capua to individual occupiers' (Liv. 27.11.8); but apparently this decision was never carried out. In 173 a first attempt had been made to return the Ager Campanus to the control of the state: 'during this year a large part of the Campanian district, which had been in many places appropriated by private individuals, was by the survey of the consul Postumius recovered for the State, and M. Lucretius, one of the tribunes of the plebs, gave notice of a proposal that the censors should let out the Campanian land for cultivation, a thing that had not been done through all the years since the fall of Capua, and as a consequence, the greed of private citizens took its course in the unoccupied land' (Liv. 42.19.1–2), but this had apparently not succeeded. There are two other references to the sale of land: in 199 the censors 'sold the land belonging to Capua which lay at the foot of Mount Tifata' (Liv. 32.7.3), and in 174 some public places in Calatia were sold, as we have seen. It is remarkable that Livy for 209 and 173 uses the term *locare fruendum*, while in 199 and 174 he uses *uendere*, a term that is usually connected with the *ager quaestorius*. The statement that the motion to let out the land caused protests in 173, since this had not happened 'since the fall of Capua', shows that in 199 and 174 land was not leased out, but sold. Apparently the censors could not only lease out, but also sell *ager publicus* (Burdese 1952, 48), but the specifics of these sales, and the differences, if any, between such land and *ager quaestorius* escape

It is more difficult to estimate the size of the *ager in trientabulis* sold in 200, since the only information about its location is Livy's statement that it was located within fifty miles of Rome. However, we have seen that in this area most of the land had already been made private very early on, and the size of the *ager in trientabulis* cannot therefore have been very extensive.

Since the acquisition of *ager quaestorius* and *ager in trientabulis* required a fairly large amount of money, it is to be expected that it was mostly the wealthy who obtained this land. It offered them a welcome means to acquire land with a secure title of possession. Security of possession in the case of the *ager censorius* cannot be established with certainty, since we do not know how the rent for this land was established. Cicero states that the Ager Campanus was occupied by the 'very humble plebs',<sup>38</sup> but this may be an exaggeration in keeping with his political goals. In any case, the amount of these three kinds of public land was limited and located only in some areas of central Italy, and it is therefore unlikely that a great number of rich people could have profited from them. More importantly, once the land had been obtained by its first possessors, it could be treated by them as private land in the sense that it could be sold or bequeathed to heirs. It was not comparable in nature to *ager occupatorius*, and was therefore not threatened by the Gracchan land distributions, which involved only *ager occupatorius*. The *ager in trientabulis* is mentioned in the Lex Agraria of 111 as an existing category of land, and its possession is protected by law: 'which is in the *trientabula*, [whatever of that land—] has or shall have passed, for whomever before [the proposal of] this [statute] it was lawful to have rented, to exploit, possess or defend [land or a piece of land,] apart from that land or piece of land [—] or it shall be appropriate [—it is to be lawful] for him to have, [exploit, possess and defend] it [after the proposal of this statute] just as anything was lawful to anyone before the proposal of this statute...'<sup>39</sup>

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us. The overall impression is that the state did not succeed in executing the proposal of 209 until 165.

<sup>38</sup> Cic. Agr. 2.31.84: *plebs optima et modestissima*.

<sup>39</sup> Lex Agraria, lines 31–32, ed. Crawford (1996, 116–7): *qui in trientabule[is est, quod eius agri—ob]venit obveneritve, quibus ante h(anc) [l(egem) rog(atam) agrum locum con]ductum habere frui possidere defendere licuit, extra eum agrum locu[m—] nve oportebit, id, utei quicquid quoeique ante h(anc) l(egem) r(ogatam) licuit, ita ei habere [frui possidere defendere post h(anc) l(egem) rog(atam) licet]*

In short, we can draw two conclusions: first, notwithstanding recent doubts, the amount of *ager publicus* was indeed very large; second, most of the *ager occupatorius* was located not in central Italy but in the more peripheral regions: southern Italy, Picenum, and Cisalpine Gaul. There was therefore almost no *ager occupatorius* in central Italy of which the rich could have taken possession by expelling the poor. This will have had important implications for the role of the *ager publicus* in the developments of the second century, as we shall see.

### III. *The importance of market production in Italy*

It is widely accepted that the Roman aristocracy gained the bulk of its income from agriculture.<sup>40</sup> The traditional reconstruction of events in the second century holds that the rich monopolized the land, and especially the *ager publicus*, by establishing large slave-staffed estates which produced foodstuffs, mainly grain, wine, and oil, for the ever growing market of urban dwellers. The exclusion of the poor from the *ager publicus* led to an increasing number of landless proletarians, who flocked to the cities, thus increasing the size of the market for the products of the rich. These proletarians were reluctant to have children, which led to an absolute decline in the free Roman citizen population. This picture is derived from the literary sources, mainly Appian: 'the rich gained possession of most of the undistributed land and after a while were confident that no one would take it back from them. They used persuasion or force to buy or seize property which adjoined their own, or any smallholdings belonging to poor men, and came to operate great ranches instead of single farms. They employed slave hands and shepherds on these estates to avoid having free men dragged off the land to serve in the army, and they derived great profit from this form of ownership too, as the slaves had many children and no liability to military service and their number increased freely. For these reasons the powerful were becoming extremely rich, and the number of slaves in the country was reaching large proportions, while the Italian people were suffering from depopulation and a shortage of men, worn down as they were by poverty and taxes and military service. And if they had any respite from these tribulations, they had no employment, because

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<sup>40</sup> E.g. Hopkins (1978, 48–54).

the land was owned by the rich who used slave farm workers instead of free men.' This interpretation has found its way into many modern textbooks.<sup>41</sup>

However, in recent years many scholars have argued that it is impossible to reconcile this picture with the reality of the second century. The main argument is that the market for which the rich could have produced their goods was actually very small. Jongman calculates that the urban population of Roman Italy in 28 BC amounted to some 1.9 million people.<sup>42</sup> Based on the average nutritional needs of adults and the estimated yields of crops, he calculates that to produce the grain, wine, and oil to feed these 1.9 million people, only 20,800 square kilometres of land were needed, or a little over 20% of the arable land in peninsular Italy.<sup>43</sup> Since the population of the Italian cities was much smaller in the second century,<sup>44</sup> the amount of land needed to fulfil the demands of the urban market was even smaller in this period. Other scholars have therefore pointed out that there was not much point in having great estates if the products could not be sold on the market. Since the needs of the urban population could be met by a relatively small part of the Italian countryside, the competition for land among

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<sup>41</sup> App. *BC* 1.7 (translation J. Carter, 1996); cf. Plu. *TG* 8.1–3. David (1997, 88–9); Cornell (1996, 110); Gargola (1995, 148); Brunt (1988, 73); Potter (1987, 98); De Neeve (1981, 76); Stockton (1979, 10); Gabba and Pasquinucci (1979, 36–8); Hopkins (1978, 1–3 and 11–5); Crawford (1978, 102); Brunt (1971, 121–31); Dilke (1971, 181); Salmon (1967, 317); Toynbee (1965, 251).

<sup>42</sup> Based on Hopkins (1978, 68–9 and 96–8).

<sup>43</sup> Jongman (2003, 112–6). He assumes that one person consumed 100 litres of wine, 20 litres of oil, and 200 kilograms of wheat per year, and that one hectare of land produced 2000 litres of wine, 440 litres of oil, or 400 kilograms of wheat: cf. Jongman (1988, 81 note 1 and 132–5). Of course, production was subject to regional and even local variations and was different for each species of grape or olive, of which there were many kinds. Consumption varied according to social class, age, sex, and occupation. Cato *Agr.* 11.1 estimates the result of five harvests at 800 *cullei* (416,000 litres) for an estate of 100 *iugera*; this would suggest a production of 832 litres per *iugerum* per year, or 3,328 litres per hectare. This may, however, be an exceptionally good harvest, and average yields may well have been lower. Cato *Agr.* 56–7 informs us on the amount of grain and wine given to slaves, who received at least 420 litres of grain (4 or 4.5 *modii* per month, depending on the season) and 160 litres of wine per year. Of course, children and women ate less, so the average consumption was much less than the figures given by Cato. Cf. Erdkamp (1998, 29–30); Morley (1996, 146–7); Purcell (1985, 13); Rathbone (1981, 12–3).

<sup>44</sup> It is estimated that the size of the population of Rome in 130 was 375,000, or 500,000 in 100; see Morley (1996, 113); Garnsey (1988, 191); Hopkins (1978, 68–9); Brunt (1971, 384). Cf. De Ligt (2004, 742), who estimates that the adult male citizen population of Rome in 133 amounted to about 100,000.

the rich must have been limited. After all, if there was only a limited market for their products, why would they compete so fiercely for extra land in order to produce even more for the market? In short, since the urban population of Italy was not very large, a large market for the products of the estates of the rich did not exist. Therefore there was no reason for the rich to accumulate large tracts of land, and competition for land cannot have been intense.

As a result of this, the number of slaves should also be reduced. Earlier estimates put the number of slaves as high as two to three million in the early empire,<sup>45</sup> but there is no secure evidence for this. Ancient sources state that only a relatively small number of slaves was needed for the production of wine and olive oil, and even fewer for grain production.<sup>46</sup> So, if only 20,000 square kilometres were used for market production, it is unlikely that more than a few hundred thousand slaves were employed in market production in Italy even in the late first century BC.<sup>47</sup> On the other hand, slaves were not only employed by large landowners. Many middling farmers owned a few slaves with which they worked their moderately small estates.<sup>48</sup> To these must be added the numerous slaves employed in crafts and services, many of which were located in cities.<sup>49</sup> Therefore the number of slaves is likely to have been higher than only a few hundred thousand, although certainly lower than the millions that are commonly assumed to have been present.

Of course, the land was put to many uses other than the production of grain, wine, and olive oil. Many other products are mentioned in Cato's work, such as fruit and vegetables, but also non-food products

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<sup>45</sup> The traditional picture assumes that a great many slaves were present in Italy. If there were one million slaves, or even two or three million, in other words one-third of the total population in 28, as Hopkins (1978, 68) estimates, this cannot have been achieved quickly. The number must have grown gradually, and therefore there must already have been many slaves in the second century; cf. David (1997, 87); Rathbone (1981, 22); Garnsey (1980, 35); Finley (1980, 148); Hopkins (1978, 9), Nicolet (1977, 83); Brunt (1971, 124). Bradley (1989, 19) even assumes that already in 225 one third of the population were slaves, owing to enslavements during the Italian wars.

<sup>46</sup> Cato 10.1 and 11.1: 16 slaves for a vineyard of 100 *iugera* and 13 slaves for an olive yard of 240 *iugera*; Col. 3.3.8: one slave per seven *iugera* of vines; Plin. *Nat.* 17.37.215: ten slaves for a vineyard of 100 *iugera*. De Ligt (2004, 746).

<sup>47</sup> De Ligt (2006, 600; 2004, 746). Cf. also Scheidel (2005, 71).

<sup>48</sup> Rosenstein (2002) with reference to Liv. 24.11.7–9.

<sup>49</sup> Jongman (2003, 106) states that "slavery was mainly an urban phenomenon". Although it is impossible to quantify the number of urban slaves, it is likely that a large number must be added to those employed in agriculture. Cf. Scheidel (2005, 67).

such as raw materials for clothes and wood for building.<sup>50</sup> Moreover, we must not underestimate the importance of animal husbandry. Some of this would have taken place in mountainous areas which were not suitable for agriculture, but there were also pastures in areas that were perfectly suitable for agriculture, such as the luxury pastures mentioned by Cato.<sup>51</sup> Agriculture and animal husbandry are not mutually exclusive categories, and much of the stock-raising would have taken place on land that was also suitable for agriculture. Working the land and transporting agricultural products to market must have required an enormous number of draught animals, such as oxen and mules, and all these animals needed fodder crops, which must have been cultivated on land that could otherwise have been used for the commercial production of other foodstuffs. Furthermore, animal manure was essential in order to maintain the fertility of arable land. The integration of animals into arable cultivation is attested in Cato's repeated advice about fodder crops and the collection of manure.<sup>52</sup> Apart from the products for the market at Rome, there was also a considerable export of Italian products, especially wine, to other parts of the Mediterranean.<sup>53</sup>

If we take into account all the functions that the Italian countryside was supposed to fulfil, then it becomes clear that demand for land was actually larger than might seem at first sight. However, it is important to take into account regional variation in the opportunities for commercial production. According to the Von Thünen model, if there is a central market, the land closest to this market will produce perishable goods,

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<sup>50</sup> Cato *Agr.* 1.7 gives a list of the various crops that must be planted on an estate, including not only grain, wine, and olives, but also a garden, a willow wood, a meadow, trees for fuel, an orchard, and an acorn wood. Scholars have disputed the purpose of this list, and many (even in antiquity, e.g. Var. *R.* 1.7.9–10 and Plin. *Nat.* 18.6.29) assumed that it was a list, in descending order, of profitable crops. However, it is more likely that it is a list of essentials that each farm should have; this is especially clear from the direction that there should be a vineyard 'or an abundance of wine (*uel si uino multo est*).<sup>51</sup> If there was wine that could easily be bought nearby, there was no need for the estate to make its own. Cf. Dalby (1998, 55–7 note 7).

<sup>51</sup> Cato *Agr.* 150 gives directions for the speculative sale of the produce of sheep, specifically cheese. These were obviously luxury products, cf. Dalby (1998, 213 note 269). Cato *Agr.* 50.1 mentions irrigated meadows which were carefully weeded. Clearly not all animals were kept on barren mountain areas. See Var. *R.* 2.2.18 for delicate 'jacketed sheep'.

<sup>52</sup> Cato *Agr.* 5.8, 10.1, 10.4, 36.

<sup>53</sup> Tchernia (1983, 91–2) estimates the export of Italian wine to Transalpine Gaul in the second century at 50,000–100,000 hectolitres per year, or about 40 million amphorae. However, according to Jongman's production estimates this amount could be produced on 50,000 hectares or 500 km<sup>2</sup> of land. Cf. Morley (1996, 113).

such as flowers, fruit, and vegetables; farther away there will be arable cultivation and arboriculture; while cattle breeding will take place at an even greater distance. This is remarkably like the developments we can see happening in Italy from the second century onwards.<sup>54</sup> Varro explains: 'And so it is profitable near a city to have gardens on a great scale; for instance, of violets and roses and many other products for which there is a demand in the city; while it would not be profitable to raise the same products on a distant farm where there is no market to which its products can be carried.'<sup>55</sup> In the immediate surroundings of Rome, therefore, *pastio uillatica* produced perishable goods. An endless range of exclusive foods was produced, such as pigeons, cranes, peacocks, dormice, fish, wild game, hares, and snails, which could all fetch enormous prices.<sup>56</sup> Although the peak in this kind of agriculture was not reached until the first century, Cato already gives the following recommendation: 'close to the City be sure to grow all kinds of vegetables; all kinds of flowers for wreaths'.<sup>57</sup>

Slave-run farms producing wine and olive oil for the market in Rome were located mainly on the coast of Latium, Campania, and Etruria, a little farther away from Rome.<sup>58</sup> At longer distances from the market, especially in southern Italy, an increase in cattle production is visible from the second century onwards. Of course, not the whole of southern Italy was turned into large cattle ranches; and conversely, there were also areas in central and northern Italy that were used for cattle, such as Umbria, Sabinum, the Po valley, and Liguria. Each region specialised in a certain kind of animal: Apulia and Sabinum were famous for horses, while the Po valley produced pigs.<sup>59</sup> Of course the Von Thünen model cannot be directly transferred to Italy: each city in Italy had its own *suburbium* where perishable goods were produced. Moreover, some regions of Italy specialised in the supply of markets outside Italy. For example, the area around Brundisium produced goods for the Roman armies stationed in Greece and Asia.<sup>60</sup> The amount of regional and

<sup>54</sup> Cf. Morley (1996, 143–158).

<sup>55</sup> Var. *R.* 1.16.3.

<sup>56</sup> Var. *R.* 3.2.14, 3.3.2–3 on the various kinds of goods produced in the *suburbium*. Prices are mentioned at several points: *R.* 3.2.15: 60,000 *denarii* for 5,000 thrushes; *R.* 3.2.18: fish ponds for HS 40,000; *R.* 3.7.10: HS 1,000 for one bird.

<sup>57</sup> Cato *Agr.* 8.2: *sub urbe hortum omne genus, coronamenta omne genus.*

<sup>58</sup> Tchernia (1993, 283–4).

<sup>59</sup> Var. *R.* 2.1.2; 2.10.11; Str. 6.3.9. See Gabba (1977, 279).

<sup>60</sup> Guzzo (1991, 84); Boersma (1990, 91).

even local variation in Roman Italy was therefore larger than would be suggested by the model.

Notwithstanding the ample opportunities for market production in central Italy, it remains unlikely that the size of the market would have been sufficient to warrant the transformation of Italy into a land dominated by large slave-staffed farms. Archaeological evidence supports this inference. An increase in the number of villas devoted to the commercial production of foodstuffs is certainly already visible from the early second century, and in some areas even from the late third century. Although there are many variations even within central Italy, the archaeological record shows an increase in the number of medium-sized estates during the second century in many places in central Italy as the century progressed.<sup>61</sup>

The buildings associated with these estates were not as large as the later luxurious villas: many of them consist of only a few rooms. Very large and luxurious villas appear only at the very end of the second century, and only in the first century in significant numbers. For most of the second century the size of individual buildings, and probably also of the estates with which they were connected, was limited.<sup>62</sup> Still, the second-century buildings are markedly larger than would be necessary for a small subsistence farmer, and many show architectural elements that indicate the production of cash crops, such as *dolia* for the storage of wine or olive oil, and olive presses. There is little reliable evidence for the disappearance of small farms; instead, both the literary sources

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<sup>61</sup> For commercial production in southern Etruria see Valenti (2003, 55); Carandini & Cambi (2002, 126, 142, 145, 176); Terrenato (2001, 27); Greene (1986, 107); Rathbone (1981, 21); Morselli (1980, 16). In northern Etruria, however, production was mainly aimed at the local market; see Terrenato (1994, 472). For Sabinum see Di Giuseppe (2005, 13–15); Alvino & Leggio (1995, 203); Coccia & Mattingly (1992, 274). For Campania see Arthur (1991, 64); Arthur (1991, 155); Panella (1980, 253). For Latium see Torelli (1995, 6); Mari (1991, 30, 36). On market production in central Italy in general see Rosenstein (2004, 155); Morley (1996, 147–151); Lafon (1993, 274); Purcell (1985, 7); Liverani (1984, 47); Garnsey (1988, 190); De Neeve (1981, 79); Stockton (1979, 14); Potter (1979, 122).

<sup>62</sup> Accardo (2000, 43); Torelli (1995, 13); Mansuelli (1988, 143); Evans (1980, 23); Potter (1979, 116). Terrenato (2001) criticizes the equation of such small villa sites with the ‘Catonian villa’, the large estates described by Cato. He argues that not one ‘Catonian villa’ has turned up in the archaeological record until the late second century. Instead, from the third century onwards there appear what Terrenato calls ‘Hellenistic farmsteads’, small sites that show some evidence of commercial production. However, Cato’s work gives no clear description of the building that should be associated with the estate. A fairly small building would be sufficient to accommodate the functions of Cato’s villa. The ‘Hellenistic farmsteads’ that Terrenato describes might in fact be identical with the Catonian villa.

and the archaeological record show a large degree of interdependence between large and small farms: large farms always depended on the seasonal labour of free workers, many of whom owned their own small plot of land.<sup>63</sup>

The *suburbium* of Rome is the only part of Italy where competition for land is likely to have been significant. In central Italy the possibilities for profitable market production were greatest: the city of Rome formed the largest market in Italy, and was continually growing. It was important for commercial farms to be located in the vicinity of the market since transport costs over land were very high, and so if products had to be transported over long distances, profits would have been severely reduced.<sup>64</sup> If land (private or public) was easily available in Cisalpine Gaul or in southern Italy, this was not of much use to someone living in central Italy and wanting to profit from the growing market there. It is likely therefore that, in central Italy at least, arable land was in high demand among those wanting to profit from the growing market. Therefore demand for land was most likely much stronger here than in the rest of Italy. It is entirely possible that in this part of Italy demand outstripped available resources, and that this led to increasing expulsion of smaller farmers from the land. If this was the case, the social problems identified by the Gracchi were real; but they were limited to central Italy, where already in the mid second century demand for land was greater than the amount available.<sup>65</sup> However, even if demand for land was indeed the most important cause of the proletarianization of the small farmer in central Italy (though to a lesser extent than has been generally thought), it seems impossible that competition among market producers was the sole reason for the increased competition for land.

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<sup>63</sup> Cato mentions free workers in many places: *Agr.* 1.3, 2.6, 4.4, 14–6, 21.5, 22.3, 144–5. See Var. *R.* 1.16.4, 1.17.2–3, Plin. *Nat.* 14.3.10. Cf. Erdkamp (2005, 58 ff.); Lo Cascio (2001, 221); Garnsey (1988, 44); Marcone (1997, 135); De Neeve (1984, 31); Rathbone (1981, 15); Garnsey (1980, 36); Finley (1980, 149); Evans (1980, 135); Stockton (1979, 19); Garnsey (1979, 2); Gabba and Pasquinucci (1979, 35); Hopkins (1978, 9).

<sup>64</sup> Cato *Agr.* 22.3 lists some prices for transport, such as moving a crushing mill, which would take six days from Suessa with the help of oxen and six labourers, and cost HS 72; if the same mill were to be transported from Pompeii (with apparently the same amount of labourers), this would increase to HS 280. Cf. Rosenstein (2004, 15); Erdkamp (1998, 62 ff.); Dyson (1992, 34); Jongman (1988, 141).

<sup>65</sup> Several scholars have argued that Italy reached its carrying capacity in the first century AD, e.g. Frier (2001, 142); Lo Cascio (1999, 123); but for central Italy this may even have been the case in the late second century BC.

IV. *Population growth and ager publicus*

Another factor which may have contributed to the increased pressure on the land in central Italy is population growth. This has been the subject of heated scholarly debate in the course of the last two centuries. Whereas until recently many scholars accepted the picture presented by the ancient sources that the Roman population was declining, lately several new views have been brought forward. If we look at the census figures preserved for the second century, we see at first a steady increase. It is likely that immediately after the Second Punic War conditions for population growth were favourable: the population was low, and land was available to anyone who needed it.<sup>66</sup> However, when this population growth continued into the middle and later second century, the situation changed, and the shortage of land led to increased economic difficulties for those who did not have access to the land, and were thereby excluded from basic subsistence. A maximum was reached in the census of 164/163; after this the figures slowly declined until at the census of 136/135 they were 20,000 lower than at their peak almost thirty years before. Many scholars have presented these figures as proof that the second century was a period of decline for the free Roman citizen population. This would correspond neatly with the sources, which state that people became more reluctant to raise children because of their poverty.<sup>67</sup>

However, the census of 125/124 shows a sudden increase of almost 75,000 compared to the previous figure. This makes it very difficult to maintain that the population had declined in the period leading up to 125. We should therefore seek another explanation for this sudden growth. Recently a gradual proletarianization of Roman citizens has been suggested as the cause of the stagnation and sudden rise in the census figures. Whereas most people immediately after the Second Punic War possessed enough to be counted as *assidui*, many of them became impoverished during the following century. Proletarians were officially counted in the census, but in practice they were not registered as carefully as *assidui*: the censors had less reason to record their numbers precisely since they did not play as important a role in the

<sup>66</sup> Rosenstein (2006, 236).

<sup>67</sup> App. *BC* 1.7, Plu. *TG* 8.3. Stockton (1979, 6); Gabba (1977, 279); Brunt (1971, 142–3); Bernstein (1969, 45); Toynbee (1965, 36–105).

military as did the *assidui*. At the same time, proletarians were increasingly unwilling to register themselves, since the wars fought in the later second century were not as profitable as they had been earlier, and they therefore preferred to avoid military service.<sup>68</sup>

It is therefore no longer possible to maintain that the second century was a period of decline for the Roman population.<sup>69</sup> In fact population growth fits the economic developments of this period much better. One of the usual effects of population growth is the proletarianization of the poor. If populations grow, more people become dependent on the same amount of land. This leads to fragmentation of smallholdings through inheritance, until they are insufficient to provide enough income for their owners.<sup>70</sup> This means that more and more small farmers need to find some other way to support their families, and so become dependent on wage labour. In Roman Italy some farmers who still held land of their own remained in the country and tried to supplement the income from their own farms with seasonal labour on the lands of the rich, as we have seen. These rural poor were those most eager to acquire new plots of land on which they could start again their lives as small independent farmers. This is shown, for instance, by the fact that many of Tiberius Gracchus' supporters were poor country-dwellers: when he stood for re-election, Tiberius 'summoned the country people to come to vote. But they were busy with the harvest, and so, under pressure because of the short time still remaining before the day fixed for the election, he resorted to the city population.'<sup>71</sup>

Other small farmers found it impossible to remain in the countryside. These left their land and moved to the cities in the hope of sustaining themselves by wage labour. The growth of Rome and other Italian cities in the second century shows clearly that many people were forced to seek their fortunes in the cities instead of working in the country on the estates of others.<sup>72</sup> Apparently, there were many small farmers who

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<sup>68</sup> De Ligt (2004, 742–4); Lo Cascio (1999, 234); Rich (1983, 303). Cf. Rosenstein (2004) for the importance of military service as a supplement to peasants' incomes.

<sup>69</sup> De Ligt (2006, 603); De Ligt (2004, 737); Rosenstein (2004, 12–3 and 146–7); Lo Cascio (2001); Garnsey (1988, 66).

<sup>70</sup> Garnsey (1988, 49). For early modern examples see Goldstone (1991, 73); Cooper (1985, 153–154); Grigg (1980, 22, 142, 194); Brenner (1976, 24).

<sup>71</sup> App. *BC* 1.14. Cf. Rosenstein (2004, 155). It is by no means the case that most of the poor farmers moved to the cities, as is often assumed, e.g. Boren (1958, 892); Hopkins (1978, 13). See Crawford (1978, 107); Bernstein (1969, 7).

<sup>72</sup> Marcone (1997, 134); De Neeve (1984, 37); De Neeve (1981, 76); Gabba (1979,

could no longer survive on the land. But even in the cities they could not earn enough money to support themselves, as can be seen from the fact that it was necessary for the state to dole out cheap grain to large numbers of city-dwellers. That the first such distribution was made by Gaius Gracchus may indicate that it was not until the late second century that many of the poor were unable to support themselves by any means at all,<sup>73</sup> and that it was now that population pressure began to have serious consequences.

It is likely that rural to urban migration was strongest in central Italy: it was here that demand for land was greatest, because people who were looking for land wanted to acquire it as close as possible to the market in Rome. At the same time, population growth caused the fragmentation of peasant landholdings. The growth of Rome was not only caused by natural population growth, but also by immigration of people from other parts of Italy, which only intensified the population growth already occurring in Rome and its surroundings.<sup>74</sup> The high level of pressure on the land in the vicinity of Rome made sure that it was in central Italy that most farmers became landless: the land had to be shared by so many people that more and more of them lost out completely.

Another problem now presents itself: if there was indeed an increase in market production, and this was located especially in central Italy, centred on the urban market at Rome, then which land was used by the rich in order to establish their large estates? We have seen that in these areas there was not a great amount of *ager occupatorius* available. It is therefore more likely that the large estates, which were undoubtedly developing in central Italy in this period, were established on private land, or on those (limited) tracts of *ager censorius*, *quaestorius*, and *in trientabulis* that still existed in Latium, Campania, and Etruria. It is remarkable that Cato in his 'standard work' on agriculture never speaks of *ager publicus*. He explicitly gives advice on how to buy land, but the possibility of working public land never occurs. Even the pastures he describes are apparently private.<sup>75</sup> This may be attributed to the fact

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42); Hopkins (1978, 67–8); Salmon (1967, 317); Toynbee (1965, 165). Brunt (1971, 59) argues that urbanisation was not yet an important factor in republican Italy.

<sup>73</sup> App. BC 1.22.

<sup>74</sup> Liv. 39.3.4–6; 41.8.6–12; 41.9.9–12 and 42.10.3 mention the expulsion from Rome of people who had migrated there from Latin and allied towns.

<sup>75</sup> Cato *Agr.* 149: leasing of private pasture.

that his estate was located in the vicinity of Rome, probably in northern Campania, an area in which hardly any *ager publicus* was available.<sup>76</sup>

If the land around Rome was predominantly private, then the land lost by the poor was not public land, but their private land. For many small farmers the money they could gain from the sale of their privately owned plots must have been a sufficient incentive to give up farming and try their luck in the city. The desire of many richer men to acquire new land ensured that poor people would have no difficulty in finding buyers for their lands, which had become insufficient to support their families.<sup>77</sup>

This seems to contradict Appian and Plutarch, who insist that the small farmers were driven off the *ager publicus* by the greed of the rich. Is it possible to reconcile the sources with the lack of *ager publicus* in central Italy? In order to answer this question we must look at the influence of Gracchan speeches on our sources. It is a widely acknowledged view that our sources were influenced by the propaganda of the Gracchi, and this is often seen as a problem. However, the presence of elements taken directly from Gracchan speeches or writings tells us much about the way they perceived and presented the problems of their time. As we have seen, it is very likely that there were problems in central Italy, the area about which the Gracchi had the most information, but that these involved expulsion from private land and not from *ager publicus*. However, in order to make their arguments more convincing, and also because they wanted to redistribute tracts of state-owned land, the Gracchi presented the problem as an issue concerning *ager publicus*. Already in the early Republic the struggle for access to land between patricians and plebeians was presented as a struggle for *ager publicus*, and it may be that the Gracchi presented their views in terms that were familiar to their audiences. Even if many of the sources concerning the early Republic were themselves influenced by the events of the Gracchan period, the existence of the Lex Licinia points to the fact that even before the Gracchan period the occupation of *ager publicus* by the rich was seen as a problem.<sup>78</sup> It is also possible that the Gracchi themselves

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<sup>76</sup> Dalby (1998, 22–4) locates it in Venafrum, and emphasizes that Cato did not wish to write a general handbook on agriculture, but only to give advice on farming in his own region.

<sup>77</sup> The possibility of the sale of private land by the poor is often neglected. It is mentioned briefly by Perelli (1993, 21); Hopkins (1978, 2–3), and Yeo (1948, 281).

<sup>78</sup> The Lex Licinia is much debated; many scholars do not accept the traditional limit of 500 *iugera* of *ager publicus* that could be possessed by one individual. Tibiletti

did not have an accurate grasp of the situation. Being familiar with the discourse on *ager publicus*, Tiberius may have thought that the estates of the rich were indeed established on public land. Similarly, it may well be the case that Tiberius genuinely thought that the population was falling, since the census figures known to him did show a continual decline.<sup>79</sup> It does not seem far-fetched to suppose that he failed to see that this downward trend was caused by proletarianization and not by population decline. The sources therefore give an indirect view into the minds of the Gracchi and show us how they perceived and presented the problems of their time, but they do not necessarily give an accurate picture of the situation in central Italy during the Gracchan period.

This is not to say that there were no holders of *ager publicus* that were injured by the Gracchan plans. According to Appian, Tiberius Gracchus' plan to distribute the *ager publicus* caused complaints from the current holders of *ager occupatorius*, who 'gathered in groups, deploring their situation and supporting their case against the poor by pointing to the work they had put in over many years, their planting, their building. Some had bought land from their neighbours—were they to lose the money as well as the land? Some had family tombs on the land or said that holdings had been treated as fully owned and divided up on inheritance. Others claimed that their wives' dowries had been invested in such lands, or that it had been given to their daughters as dowry, and moneylenders could show loans made on this security.'<sup>80</sup> It is clear that *some* people had occupied *ager publicus* and established commercial estates on it. However, Appian does not give any indication as to the location of this land. It is to be noted that the only reference that actually records the location of large estates, namely the account

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(1949, 6–14) argues that the relatively small amount of *ager publicus* available in 367 would make it unlikely that everyone would have been allowed to possess so much public land on top of their private holdings. Many therefore favour a date somewhere in the early second century for the introduction of the 500 *iugera* limit, e.g. Gargola (1995, 146); Lintott (1994, 55); Gabba and Pasquinucci (1979, 39); Gabba (1977, 275); Toynbee (1965, vol. 2, 556); Tibiletti (1948, 175 and 223). However, there is no evidence whatsoever for the passing of such a law in the second century. See Forsén (1991) for a comprehensive discussion. It may be the case that the Lex Licinia limited not only the possession of public land, but of public and private land combined, in which case the limited amount of available *ager publicus* would be compatible with the upper limit of 500 *iugera*.

<sup>79</sup> As De Ligt (2004) points out, examples of similar concerns about the perceived decline of populations that were actually growing can be seen in early modern England. See e.g. Neeson (1993, 13).

<sup>80</sup> App. *BC* 1.10, cf. Flor. *Epit.* 2.1.7.

of Tiberius Gracchus' journey through Etruria,<sup>81</sup> does not say that the estates he witnessed there were situated on *ager publicus*.

It may be therefore that those holders of *ager occupatorius* who complained against the Gracchan land law were those holding *ager publicus* outside central Italy, where, as we have seen, large amounts of *ager occupatorius* were still available. In southern Italy, for example, much confiscated *ager publicus* was not distributed until the Gracchan period.<sup>82</sup> It is often claimed that this *ager publicus* was monopolized by the rich, who then established large cattle farms on it.<sup>83</sup> This claim is certainly exaggerated. Archaeological evidence shows that in most areas of southern Italy arable farming was still the most common form of land use in the second century. Just as in central Italy, in the south larger farms aimed at market production became more common during the second century, although they were still fairly limited in size before the Gracchan period.<sup>84</sup> References to long-distance transhumance involving large herds moving from southern Italy into the Apennines appear only in the first century.<sup>85</sup>

Nevertheless, there are some sources that indicate the existence of stockbreeding enterprises in the second century as well. Appian for example refers to the use of slaves as shepherds, and cites the passing of a law aimed at limiting the number of animals one person could herd on the *ager publicus*.<sup>86</sup> The same stipulation occurs in the *Lex Agraria*,<sup>87</sup> where fifty small animals and (probably) ten large ones were allowed

<sup>81</sup> Plu. *TG* 8.7.

<sup>82</sup> For the locations of *cippi* set up by the Gracchan commission see Campbell (2000, 542–3), whose commentary on the *Liber Coloniarius* also gives much information on the other allegedly Gracchan distributions of land. See Cornell (1996, 113).

<sup>83</sup> David (1997, 90); Torelli (1995, 3); Gabba (1994, 159–60); Lomas (1993, 87); Simelon (1993, 44 and 69); De Neeve (1984, 9); Giardina (1981, 88); Gualandi (1981, 161); Stockton (1979, 12); Gabba and Pasquinucci (1979, 143–4); Gabba (1977, 278); Toynebee (1965, 155); Yeo (1948, 293–301).

<sup>84</sup> For Apulia and Calabria see Bonora Mazzoli (2001, 71); Volpe (2001, 323); Marchi & Salvatore (1997, 75); Marcone (1997, 135); Battista Sanguineto (1994, 568 and 583); Desy (1993, 111 and 281–5); Boersma (1990, 41); Jones (1980, 90–1); Delano Smith (1978, 163). For Lucania and Bruttium see Accardo (2000, 42–5); La Torre (1999, 110 and 140); Carter (1994, 180); De Siena & Giardino (1994, 200–9); Lomas (1993, 119); Simelon (1993, 49–57). For Samnium see Tagliamonte (1996, 249); Barker, Lloyd & Webley (1978, 44–8).

<sup>85</sup> Var. *R.* 2.1.16–17, 2.2.9, Hor. *Epod.* 2.27–28.

<sup>86</sup> App. *BC* 1.7–8. We do not know when exactly the stipulation about a maximum number of animals to be grazed was set, but it was certainly in force in 167, when Cato referred to it in his speech *Pro Rhodiensibus* (Gell. 6.3.37).

<sup>87</sup> *Lex Agraria*, lines 14–15.

to be grazed without payment. Larger flocks could be grazed for a fee. Moreover, already in the early second century reports occur of rebellions by slave shepherds: 'there was a widespread movement amongst the slaves in Apulia this year [186]. The herdsmen had entered into a conspiracy and were making the highroads and public pastures insecure through acts of brigandage.'<sup>88</sup> In Lucania a decline in agricultural production can be detected, and it may be that here some increase in animal husbandry took place.<sup>89</sup> Moreover, the subject of the famous Elogium Pollae, found in Polla in Lucania, prides himself on forcing the shepherds to give up their land to farmers.<sup>90</sup> It may be that here agriculture temporarily lost some ground in favour of stockbreeding. Since the amount of *ager publicus* in Lucania was large, especially in the Tanagro valley, in which Polla is located, it is possible that this occurred on public land. It must be emphasized, however, that we do not know who these stockbreeders were. It may be that they were not Romans, but Italians who used the Roman *ager publicus* for their own purposes.<sup>91</sup>

<sup>88</sup> Liv. 39.29.8–9. See also 39.41. In the first century shepherds were still an (at least alleged) danger, as illustrated by the shepherds that supported Catilina (Cic. *Sest.* 5.12, *Catil.* 3.6.14). There was a continuous fear that shepherds would be used as private armies: Cicero accused C. Antonius of selling his flocks but keeping the shepherds so that he could use them as an army (*Asc. Tog.* 87).

<sup>89</sup> Carter (2005, 243–5) gives archaeobotanical evidence for the growth of animal husbandry in the territory of Metapontum from the second century onwards. Terrenato (2001, 23) points out that some larger farms in southern Italy may have been used mainly for cattle breeding, such as the villa in Moltone in Lucania, which is located next to an important transhumance route. For cattle breeding in the south see La Torre (1999, 109); Morley (1996, 151–8); Desy (1993, 59–60); Lomas (1993, 120); Guzzo (1991, 84); Garnsey (1979, 10).

<sup>90</sup> *CIL* 1.551 = *CIL* 10.6950: *primus feci ut de agro poplico aratoribus cederent paastores*. The question to whom this inscription refers has been widely debated. Many scholars attribute it to various enemies of the Gracchi, such as P. Popilius Laenas, T. Annius Rufus, or T. Annius Luscus. It is, however, very strange that an enemy of the Gracchi would pride himself on having given out land to farmers. Others have therefore claimed that the inscription refers to actions of the magistrate while he was in Sicily, e.g. Burdese (1952, 102); that the reference was to the creation of *viasii vicanei* in connection with the building of the Via Popilia, e.g. Franciosi (2002, 212–3); or have proposed an identification with a pro-Gracchan individual, for example Ap. Claudius Pulcher, e.g. Verbrugge (1973). This is not the place to go into the details of this debate, but at least the stone does show that some *ager publicus* had been occupied by stockbreeders. See Pobjoy (2006, 58–9).

<sup>91</sup> It is a widely accepted view that much of the *ager publicus* in southern Italy was not only held by Roman citizens, but also by allies. However, more research into the patterns of landholding in non-Roman territory is necessary. Rathbone (2003, 150); David (1997, 144); Tagliamonte (1996, 249); Lintott (1994, 44); Gabba (1977, 276); Gabba (1979, 51); Salmon (1967, 332).

In any case, it is clear that the use of *ager publicus* outside central Italy followed very different patterns from that in central Italy, and that population growth and competition for land is unlikely to have been an important issue here. The sources, however, fail to distinguish between patterns of landholding in the different parts of Italy, and present the occupation of *ager publicus* by the rich as a pan-Italian problem.

### V. Conclusion

I have argued that there are notable differences in the use of land in the various regions of Italy. In central Italy a combination of population pressure and competition for land may have been responsible for the proletarianization of the small farmer, which led to the gradual monopolization of the land in this region by the rich. As far as central Italy is concerned, the sources are correct in their depiction of increased poverty among the poor, and increased competition for land among the rich. However, in central Italy it was not *ager publicus* but mainly the private land of the poor that was monopolized by the rich, since the amount of *ager publicus* remaining in this region was limited.

In other areas of Italy there was still a large amount of *ager publicus*, some of which had been public since the early third century. This may have been used partly for market production by the rich, not only for stockbreeding but also for the production of cash crops. It may be suggested, however, that most of the *ager publicus* in the peripheral regions was exploited by local Italian inhabitants who continued to occupy the land they had always held, even before its confiscation as *ager publicus*.

The most important lesson to be learned is that any study of the Roman *ager publicus* and economic developments in Italy in general must take care to distinguish between developments in the various regions of Italy.

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VI

DEMOGRAPHY AND THE END OF THE REPUBLIC

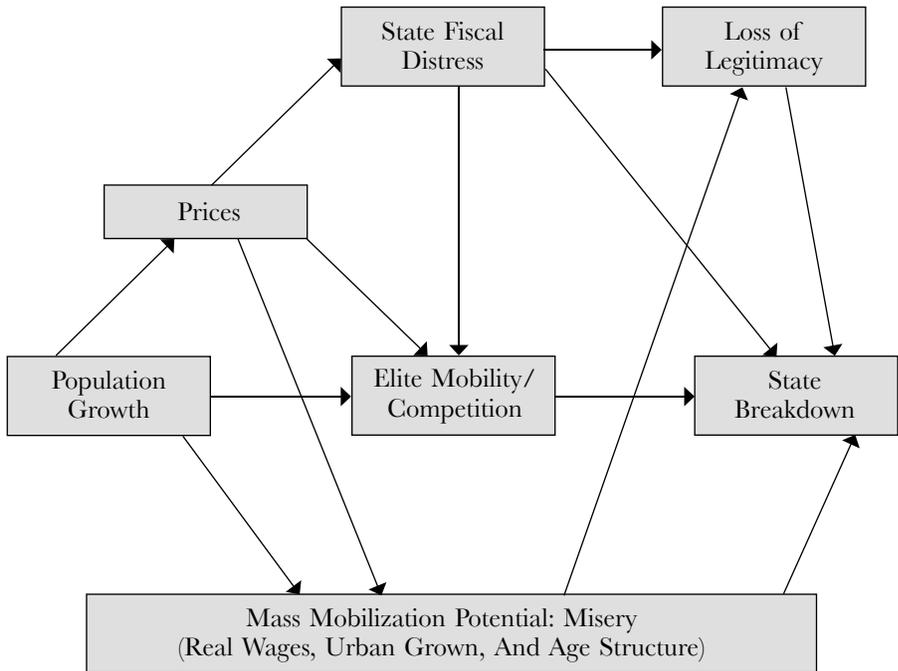


REVOLUTION AND REBELLION IN THE LATER SECOND  
AND EARLY FIRST CENTURIES BC:  
JACK GOLDSTONE AND THE 'ROMAN REVOLUTION'

Nathan Rosenstein

It should no longer need saying that the standard model of social and economic developments in second-century BC Italy stands in need of a major revision, and the question now is, what will take its place. Over the past few years, population increase among free Romans and Italians has emerged as a promising alternative in the minds of some scholars, myself included. Too many people competing for a finite amount of farmland could just as well have been the source of the second century's landlessness and rural distress as the displacement of smallholders through military service and the growth of large slave-estates. Yet population increase in and of itself does not explain *how* the distress it engendered could eventuate in political crisis beginning in 133 BC and finally the collapse of republican government in 89 BC. Demographic expansion in a context of limited agricultural resources occurred at many times in the pre-industrial world, yet it did not inevitably lead to state breakdown. What is needed is a model of how population pressures affect pre-industrial societies and governments, one that can show how they might have caused things to go so badly wrong for the Republic in the early first century BC.

Fortunately, one is lying ready to hand in Jack Goldstone's classic study, *Revolution and Rebellion in the Early Modern World*. What makes it so alluring is not simply that population growth lies at its center but the complexity and extent of the ramifications comprised within it. Also attractive is its wide applicability. Goldstone develops his model primarily with reference to the two great revolutions of early modern Europe, England in 1642 and France in 1789, but he also seeks to apply it to a variety of contemporaneous upheavals in Europe, Ottoman Turkey, and even China in order to show in what ways the processes he identifies in England and France were or were not fully played out in other states that felt the effects of the two great waves of population growth during this period. Regardless of whether or not Goldstone's analysis is correct in any particular case, the versatility and sophistication of



Goldstone Schema

Fig. 1

his model invite us to consider whether and to what extent it could be usefully applied to an analysis of developments during the Roman Republic in the later second and early first centuries BC. For those who do not know Goldstone's book, I will describe briefly his schema before turning to how it might help to understand the impact of Italy's second-century population growth.<sup>1</sup>

<sup>1</sup> Some might object that this paper compares apples and oranges. That is, events in England in 1642 and in France in 1789 led to genuine revolutions, while the civil war that broke out in 88 BC ultimately re-established only a somewhat modified version of the Republic's traditional form of government. However, this is to misunderstand the aim of Goldstone's analysis, which is to examine the causes of what he terms "state breakdown... a condition of grave disorder, with a collapse of state authority": Goldstone (1991, 10). He sharply distinguishes this from revolution, which he reserves for "those cases where state breakdown is followed by substantial changes in political and social institutions and in the ideology used to justify those institutions": *ibid.* It is entirely in keeping with the Republic's "crisis without alternative" that its state breakdown in the period 88–82 BC failed to eventuate in any substantial alteration to the "political and social institutions and in the ideology used to justify those institutions".

## I

The effects of population growth, in Goldstone's view, make themselves felt in four broad areas, as illustrated in Figure 1. The first involves prices. Obviously, in pre-industrial economies any long-term increase in the number of people on the land will eventually lead to a shortage of it. As plots are subdivided into smaller and smaller units from one generation to the next, an increasing number of them become so small that the food they produce can no longer support the families that occupy them. These families are forced to find work, borrow, or sell their land in order to purchase the food they need, which increases the demand for it and leads inevitably to a rise in the prices of grain and other basic foodstuffs. In pre-industrial economies, agricultural productivity is limited, and as population rises, increases in the supply of food soon fail to keep pace with the increasing number of mouths that need to be fed. The price increases that result affect not only the rural poor but urban populations as well, which are swollen by migrants from the countryside looking for work. More importantly, as the population continues to grow while the amount of farmland remains fixed, the proportion of people without enough land to feed themselves increases at a significantly faster pace than the population as a whole, driving up demand still more and so further inflating prices. The effects of rising food prices in turn ripple through the rest of the economy. Since those who must buy food usually spend a very large portion of their incomes on it, they are left with less to spend on other things, causing demand for non-food items to drop. This decline means less work for those who are employed to produce these items, and as a consequence wages stagnate or fall. Employers, faced with declining sales and profits, hire fewer workers and are under little pressure to increase the wages of those they do employ, because population growth has produced an excess of labor. However, non-food producers, too, eventually begin to raise prices in order to keep up with the rising cost of living. When food and other prices rise while wages decline or even remain steady, the result is a drop in real wages, that is, what the money one earns will actually buy. The combination of rising prices, declining real wages, and an oversupply of labor in its turn leads those without enough land or work to feed themselves to turn to vagabondage, begging, or crime in order to survive.

Still, some people benefit. Those in a position to sell grain or other foodstuffs profit enormously from the run-up in prices. In England,

for example, this led to enclosures aimed at turning forests and other commons into arable farmland to the benefit of landholders, who sought thereby to increase the size of their harvests and profits. These individuals were as a rule not only the rich but also freeholders and even tenants with secure, long-term leases, who cooperated in carrying out enclosures. All prospered from rising food prices. Those in the countryside who were hurt by enclosures were the economically marginal: cottagers and squatters who survived by dairying, wood-gathering, poaching, and metal, wood, and textile work. Enclosure deprived them of their homes and a good portion of their livelihood, while rising food prices and falling wages pressed them from the other side. Riot and disorder among this segment of the population resulted with increasing frequency in the later sixteenth and early seventeenth centuries. However, as Goldstone notes, poverty and popular distress in and of themselves seldom have serious political consequences in “a strong state supported by a unified political elite. The crucial factors that lead to state breakdowns are state fiscal crisis, elite opposition, and intra-elite competition.”<sup>2</sup> Hence rural suffering caused by rising prices is far less important to the workings of his model than their effects in the arena of elite competition.

In Goldstone’s scheme an increase in one’s economic prosperity brings with it the desire for a commensurate elevation in social standing. So as middling farmers grew wealthier and larger landowners became rich, along with merchants and others able to cash in on the rise in food prices or otherwise benefit from the economic conditions of the time, their desire for offices and honors to enhance their prestige grew apace. In and of themselves their aspirations would have been benign but for two further factors. The first was the declining fortunes of those among the traditional elite who could not profit from the rise in prices. For example, aristocrats whose fields were let to tenants on a long-term basis at fixed rents saw their incomes stagnate while their expenses continued to escalate. Worse, not only did they fail to increase their wealth, but rising prices for food and other goods eroded the purchasing power of what income they did receive. Second, the effects of population increase made themselves felt among aristocratic families as well as those of the poor. Aristocratic parents now found themselves with more children to establish in social and economic positions com-

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<sup>2</sup> Goldstone (1991, 86).

parable to their own, yet decreasing financial resources with which to do so. And to make matters worse, they faced growing competition for honors and royal patronage from the families of the newly rich, who themselves had more children to place. The result was fierce competition within the elite for access to royal favor, exacerbated by the third consequence of population growth that Goldstone identifies, its effects on the operation of the government.

In his model rising prices eroded the ability of early-modern governments to function effectively. Their costs rose along with everyone else's as population pressures drove up prices, particularly because the great majority of governments' expenditures went to the costs of waging war. So as the price of food went up, so did the cost of the armies governments were forced to maintain. Moreover, larger populations made possible the levying of ever larger armies, further adding to the financial exigencies that warfare imposed. And because warfare was chronic in this era, the fiscal pressures caused by the rising costs of war were unrelenting. To meet these demands on their treasuries, however, governments found themselves saddled with tax-structures and other revenue sources that simply could not keep pace with rising costs. In England, for example, ordinary crown expenses were supposed to be met from rents collected on royal lands. But these were generally leased out for long periods on fixed terms and so gradually brought in less and less in real terms as prices rose. Caught between declining real income and steadily rising costs, governments were driven to a variety of short-term expedients. In England the crown sold off assets, often took the lead in enclosing royal forests that it then leased out to tenants, and frequently resorted to borrowing. The French monarchy sold offices and likewise borrowed so heavily that finally it was seeking loans just to pay the interest on its debt. Measures like these could solve governments' financial problems in the near term, but in the long run they seriously weakened the monarchies' fiscal underpinnings. One consequence of this weakening was to further exacerbate tensions both with and within the elite. On the one hand, the governments' chronic shortages of money forced them to any number of expedients aimed at squeezing more money out of the rich, who resented the crowns' encroachments on their traditional immunities and privileges. On the other, a lack of funds made it impossible to satisfy all of the demands for patronage and offices that both members of the traditional elite and the newly rich were making upon their monarchs. Their governments' failure on this score further inflamed intra-elite competition at a time

when more and more aristocrats were seeking the civil and ecclesiastical posts and sinecures that would not only satisfy their quests for honor but bring with them the financial emoluments they often needed to buttress their declining fortunes.

However, the financial houses of cards that the monarchies had built for themselves were ultimately bound to collapse. When they did, governments faced the need to persuade an alienated and divided elite to agree to new taxes, a challenge made even more difficult by Goldstone's fourth factor, a crisis of confidence in the governments. By the time matters were coming to a head on the fiscal front, it had become abundantly clear to broad segments of the populations that something was very wrong in their societies and that their governments were incompetent to solve the problems of the age. In England the vagabondage, begging, and petty crime arising out of population growth strongly indicated to 'the middling sorts' and others that something needed to be done. The resulting sense among many Englishmen that things were spinning out of control led to the transformation of Puritanism, which began as a general movement for reform of the Church of England, into a crusade against corruption and Catholicism, which were widely associated with the Crown. The movement drew its power from the evident fact of the widespread social and fiscal problems its followers saw all around them. Although Charles' government tried to offer remedies, it was handicapped in providing poor relief by its chronic lack of funds, which only increased the sense that something would have to be done about the government. In France a similar perception of widespread social ills and government incompetence led many Frenchmen to adapt Enlightenment rhetoric to revolutionary ends. The cumulation of all these factors, then, combined to produce the collapse of state authority and paved the way for short-term events (and I would not want to minimize their importance) to bring about the collapse of state authority and ultimately a radical restructuring of each government.

I fear that I have not done justice to the very sophisticated and highly detailed analyses of state breakdown that Goldstone offers, but I hope I have provided enough description to convey the gist of his thesis, which is that the ramifications of population growth play out in complex ways. Equally important, all of these factors have to work together in order to bring about a collapse of state authority and revolution. Absent any one of them, and early modern governments in Goldstone's view were able to ride out rural unrest or elite dissatisfaction or fiscal problems until population growth stopped and pressures eased.

## II

Now obviously Italy is not northern Europe, and the second century BC differed in important ways from the seventeenth and eighteenth centuries AD. But it is important to emphasize that Goldstone's model simply describes a set of relationships among several factors arising out of population increase, relationships that ought to be applicable to the analysis of other cases of pre-industrial state breakdown. So the question we need to ask is not in what ways events in the later second century BC were similar to or differed from those in England on the eve of civil war or France prior to the revolution, but to what extent the factors and relationships that Goldstone's model identifies help us to understand the ramifications of population growth following the Hannibalic War. In my opinion, they prove remarkably useful.

In the first place, Goldstone emphasizes that population growth itself is not a monolithic phenomenon but rather a cluster of developments in fertility and mortality that can lead to the enlargement or shrinkage of certain age groups relative to others. This factor is important in his model because the revolutionary potential of a population is a function of its youthfulness. As the proportion of young people increases, radical action becomes more likely. The young are more readily mobilized in support of radical programs than their elders, while those sympathetic to such programs but hesitant to participate are more inclined to take an active part in supporting them once they see that large enough numbers are already involved.<sup>3</sup> Goldstone of course has the kinds of demographic and other data for seventeenth century England and eighteenth century France he needs to support this claim, data which are completely lacking for our period. Still, it is worth asking whether we can find support for a similar increase in the proportion of young adults in Roman Italy during the second century BC, and I would argue that what evidence we have suggests that this was indeed the case.

For Goldstone, as well as for others, disease acted as the principal brake on the populations of early modern Europe. In his analysis the return of plague during the sixteenth century caused growth to slow, while its disappearance in the seventeenth led to rapid increase. Without entering into the debate over the relative importance of disease as opposed to famine in regulating population growth, it is worth pointing

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<sup>3</sup> Goldstone (1991, 136–38).

out that after a cluster of references to pestilence in Italy in the period 187–174 BC, only two occur down to the century's end—one in 165 BC and another in 142 BC.<sup>4</sup> The absence of subsequent notices in Livy's, Obsequens', and Orosius' accounts therefore might suggest that serious outbreaks were in fact largely absent during the middle and later decades of the century.<sup>5</sup> While this is an argument from silence, it gains in credibility from the fact that famine and pestilence often caused the senate to authorize a religious response on behalf of the *populus Romanus*, precisely the sorts of events likely to be noted by Livy and the other authors.

In my view, however, an equally if not more important factor in controlling Italy's population growth was mortality due to war. As I have argued elsewhere, a surprisingly high proportion of the young Roman and Italian men the Republic sent to war in the century's early decades never returned. Their deaths eased competition for farmland, discouraged the application of preventive checks on fertility, and encouraged survivors to have more children.<sup>6</sup> Even more significant, however, was the sequel. Beginning in the late 170s BC, following the pacification of Spain, Gaul, and Liguria, Roman military activity fell off precipitously. Figure 2 graphs Roman warfare by year in terms both of the number of legions levied and the extent and results of the fighting those legions engaged in. It shows quite clearly what most of us familiar with the period understand impressionistically: for about sixteen years the Republic called upon its citizens and allies for significantly less military service compared to previous years, while those who did serve saw very little combat. It is a reasonable inference therefore that the cohort of draft-aged men, those aged 17 to about 28 or so, who lived through this period (indicated by the lines above the years 172–157 BC) would have suffered far fewer fatalities as a consequence of their military service than those drafted in earlier years. Consequently, more of them will have survived to marry and father children. It is also reasonable to assume that these men did not try to limit the size of their families—to the extent that this was possible prior to the modern era—since we

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<sup>4</sup> Livy 38.44.7; 40.19.3, 26.5–6, 36.14, 42.6; 41.31.5–6 and 10–11; Obseq. 13.22.; Oros. 5.4.8–11. Cf. Wiseman (1969, 74).

<sup>5</sup> However, Duncan-Jones (1996, 109–11) argues that outbreaks of plague in this period would have been more frequent than their appearance in Orosius would suggest. Cf. Syme (1979–, 6.232–33).

<sup>6</sup> Rosenstein (2004, 107–40).

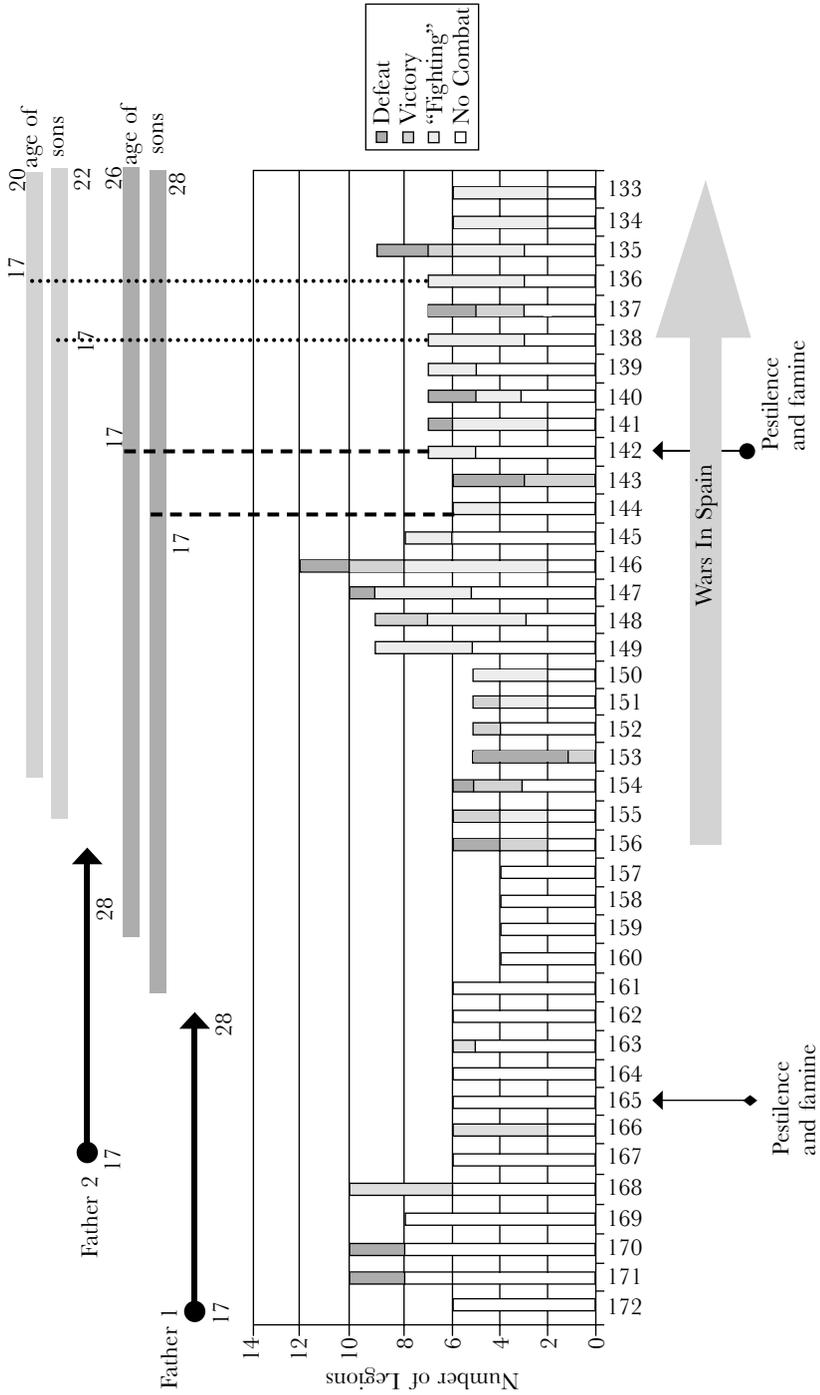


Fig. 2

know that the citizen population continued to increase between 168 and 124 BC. I would argue on that basis that the children of these men, especially their sons (indicated by the shaded lines above the years 161–133 BC), will have been significantly more numerous than previous generations of young men and women. These sons, it is true, reached military age towards the latter stages of the Spanish wars, and certainly those who served there suffered their share of casualties. But Spain was the Republic's only significant military theater in the late 140s and 130s BC, unlike the situation earlier in the century when Rome was often actively engaged on two or more fronts simultaneously. Consequently, casualties overall are likely to have been fewer than forty or fifty years earlier. And since there were more potential draftees to recruit from, casualties will have represented a smaller proportion of their total numbers. This bulge in the age-structure will have persisted throughout the period of the Spanish wars, and its members will have been reaching their mid- to late-twenties when its most famous representative, Tiberius Gracchus, born *c.* 163 BC, entered upon his tribunate. Their numbers and comparative youth will have considerably augmented the potential for radical action during that year. This increased potential will have been due not simply to the fact that these men were young. Many of them will have been reaching that stage in their lives when they ought to have been looking forward to marriage and children. That meant they had to acquire the means to support a family, a need that will have greatly enhanced their concern over acquiring land and their willingness to act boldly in order to secure it.

We can also look briefly at the end of our period in order to consider the impact of war on population and the contribution that demographic developments might have made to political turmoil and state breakdown at that point. Figure 3 presents a graph similar to Figure 2, depicting Roman military effort and the level of combat that legions sustained in the period from 132 to 91 BC. The generally peaceful character of the decade leading up to the Social and Civil Wars that the graph reflects is well known. The generation of men of military age in that period (indicated by the shaded lines above the years 118–91 BC), particularly those turning 17 in 100 BC, will have suffered very few deaths due to warfare compared to those reaching their majority a decade or two earlier. The crucial question is how many of them there were. The extent and intensity of warfare that their fathers (indicated by the lines above the years 129–114 BC) experienced was certainly not as

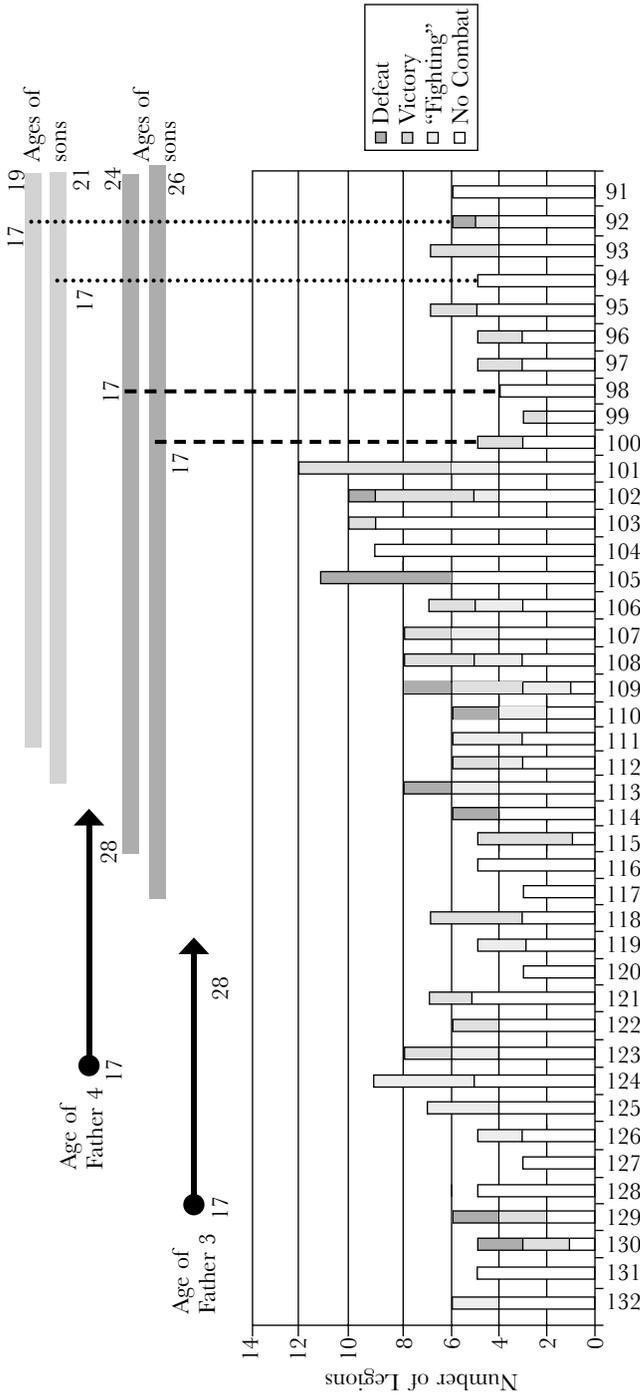


Fig. 3

limited as that men of draft age between 172 and 156 BC underwent, but neither was the fighting as severe as in some earlier periods. Nor was Rome's military effort between 129 and 114 BC as extensive as the mobilization it would undertake over the next dozen years. Here again, a reasonable case can be made that a fairly high proportion of men of military age during the years between 129 and 114 BC survived and spent the years that followed fathering children, so that the cohort of sons who came of age beginning around 100 BC was fairly numerous. And if Rome faced few serious military challenges in the years that followed, the rate of survival among these young men would have been high, resulting in a large number of men in their mid- to late-twenties by 91 BC.

### III

Quite possibly, then, the demographic contribution to the turmoil in the period from Tiberius Gracchus to Livius Drusus went beyond simply too many people on too little land. An unusually large number of young men within the population may have increased the 'revolutionary potential' of the situation during these years, particularly at the beginning and end, in just the way that Goldstone's model would predict. However, Goldstone also emphasizes that while economic distress among a large number of young people might lead to riots or other forms of disorder, in and of themselves they do not cause governments to collapse. For that, elite leadership is necessary. This factor, too, results in his model from population growth. Since demographic expansion affects the rich as well as the poor, a bulge in the under-thirty segment of the general Roman population ought to imply a similar enlargement of that same segment among the aristocracy. Once again, though, we have nothing like the sorts of demographic data that would allow us to verify this development in the later second and early first centuries. The census figures for the second century BC tell us nothing about the age structure of Rome's senatorial class. However, we can, I think, make a few guesses. Thanks to the pioneering work of Keith Hopkins and Graham Burton, we know that middle Republican aristocrats were generally not deliberately limiting their families to only one or two children. Hopkins and Burton argue that fertility among the political elite was high in this era, well above an average birth rate of 4.1 children per family, with perhaps 60% of all families having at least one son surviving to the

age of 40.<sup>7</sup> We do not, unfortunately, know if the senatorial class, that is not only senators themselves but also siblings, children, and cousins, was growing along with the general population. But in the absence of evidence or strong arguments to the contrary, we may at least suppose that this was the case. More important will have been the effects of military mortality on its age structure. As is well known, young men of this class as well as other sons of rich families with political ambitions had to acquire a reputation for *virtus* if they wanted to advance up the *cursus honorum*. Anecdotal evidence from the Hannibalic War and the campaigns from the early second century BC illustrates not only their service as officers, companions of generals, or cavalrymen, but their readiness to enter the thick of the fighting.<sup>8</sup> How many died in combat or as a result of disease or the accidents of military service we have no way of knowing. But in view of the high mortality among soldiers generally, the numbers cannot have been negligible. Periods of comparative peace therefore will have affected the survival rate among young aristocrats just as much as—if not more than—that of draft-aged men generally. Possibly, too, they were growing somewhat less bellicose, at least if we can credit Polybius' account of their reluctance to come forward to volunteer for service in Spain in the 150s BC, and so perhaps less ready to expose themselves to danger.<sup>9</sup> We ought to expect, then, that the number of young aristocrats was increasing, particularly towards the beginning and the end of our period. If this surmise is correct, then it ought to imply that the intensity of political competition was on the rise as well.

As Hopkins and Burton have shown, there were never enough high offices to accommodate the ambitions of all sons of consuls or even of praetors.<sup>10</sup> Rivalry was always keen even under the best of circumstances, and it would have become all the more so as the number of young aristocrats seeking to equal or better the achievements of their ancestors grew. Equally important was the inelasticity in the supply of offices. In Goldstone's model, the enlargement of the upper class only creates problems if the number of positions of honor and sinecures fails

<sup>7</sup> Hopkins (1983, 99–107, esp. 103–4).

<sup>8</sup> Note e.g. the frantic efforts of the son of Cato the Censor and a group of companions to recover his sword, lost in the thick of the fighting at Pydna in 168 BC: *Plu. Cat. Ma.* 20.7–8; *Aem.* 21.1–2. See further McCall (2002, 83–96).

<sup>9</sup> *Plb.* 35.4.1–14; other sources in Broughton (1951–2, 1.455–56); cf. Astin (1967, 45).

<sup>10</sup> Hopkins (1983, 55–69), see also Badian (1990).

to keep pace with demand. In pre-revolutionary England and France this occurred because of the financial straits in which governments found themselves. The Roman Republic did not confront similar sorts of financial problems. But as Brennan's study of the praetorship makes abundantly clear, the senate was strikingly reluctant to enlarge the number of praetors elected after the increase from four to six in 197 BC, despite its increasing difficulty in meeting the administrative needs of the Republic with that number of magistrates.<sup>11</sup> And of course more than two consuls was out of the question, while prorogation, the usual solution to the need for more administrators or generals than magistrates elected in a year, simply rewarded the prior year's already successful officeholders rather than creating new opportunities for others.

The growing wealth of Rome's second-century BC conquests and the uneven distribution of the profits of empire among the senatorial class also added to the heat of aristocratic competition. This story is familiar to everyone. Money had always been an important factor in enabling aristocratic self-advertisement by means of lavish display and benefactions. It would only become more so as the second century BC wore on and the Republic's conquests and other contacts in the Hellenistic East multiplied. The spoils of victory were not spread evenly, however, and the fortunate few able to make grand gestures such as giving games or building temples were able to enhance their stature dramatically in the eyes of the public, while their peers without rich victories, or without any victories at all, could not. They were forced to resort to the various surreptitious or illegal means of making money that enabled generals and governors to turn a profit in peaceful provinces. As the scale of the public and private expenditure by which aristocrats increasingly defined themselves grew over the course of the second century BC, magistrates' efforts at self-enrichment grew apace as they struggled to keep up with their richer peers, further intensifying the struggle to win the offices that would enable them to do so.

Things will only have gotten hotter after 123 BC and Gaius Gracchus' law placing the collection of the Asian taxes in the hands of the *publicani*. This step constituted a major change in the relative access to the spoils of empire that senators had up to that point enjoyed. For the regularity of tax collection and consequently of the profits both licit and illicit that *publicani* could derive from it contrasted markedly with

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<sup>11</sup> Brennan (2000, 623–29, esp. 628).

the episodic opportunities for enrichment available to magistrates and their staffs, which might come around only once or twice in a lifetime. As some members of Rome's equestrian class grew richer and so able to mount increasingly lavish displays, the pressure increased on aristocrats not simply to keep up but to surpass them in order to demonstrate the superior lifestyle and benefactions that their elite status required. More importantly, a significant improvement in one's economic condition ought to have led to a desire for a commensurate elevation in social rank in a culture that measured personal worth more in terms of honor than wealth. Although our record of new men who reached the senate in this period is by no means complete, Wiseman's list of them suggests a significant increase in their appearance after 123 BC.<sup>12</sup> Few reached the consulate, of course—although there is a significant cluster of them in the last decade of the second century—but our record of praetorships in this period is poor, so it is impossible to be certain whether or not they were making inroads here. But perhaps it was in elections to the tribunate of the plebs that they enjoyed their greatest success. In any event, the simple fact that there was now a growing number of equestrians with wealth enough to fuel political ambitions is likely to have increased the sense within the senatorial class that an already packed field of competitors for public office was growing even more crowded. And of course we have every reason to expect that equestrian families, like all others, were generally producing an increasing number of sons in these years.

#### IV

So far, then, it seems as if some of the conditions in our period fit relatively well into Goldstone's paradigm. However, the effects of population pressure on prices, especially food prices, play a key role in his schema, and these developments find no real analogy in the middle or late Republic that I can see. Generally speaking, the economies of early modern England and France seem to have been much more monetized than that of Italy in the second and first centuries BC.<sup>13</sup> A much larger proportion of early modern Europe's population, particularly in the countryside, worked for wages, so that increases in the price of grain and

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<sup>12</sup> Wiseman (1971, 182).

<sup>13</sup> On rural monetization in Roman Italy see now Hollander (2007, 122–33).

other foods coupled with a drop in real income produced widespread misery. Rising prices also made the cost of governing, particularly of waging war, increasingly expensive, and as these rose, they collided with an inflexible tax-structure that prevented governmental revenues from rising in step with inflation. The consequences in turn forced governments to adopt increasingly desperate expedients in order to raise money, expedients that ultimately caused widespread dissatisfaction and a crisis of confidence in the legitimacy of the government itself.

We have no data that would enable us to estimate average food prices for Italy, even in Rome itself, during the middle and late Republic, much less chart their course over time.<sup>14</sup> It is at least conceivable, though, that if we did have such data they would show a long-term increase for our period.<sup>15</sup> However, the urban, non-agricultural component of the population, or at least that portion of it resident in Rome, was supported to a considerable degree by grain imported from Sicily, Sardinia, and North Africa. The abundant agricultural resources of these provinces must to some extent have insulated Rome's population from the upward pressure on prices resulting from population increase (although not from the cyclical swings produced by the harvest cycle and from year-to-year variations in the harvests themselves).<sup>16</sup> And after 123 BC, of course, food prices in the capital were to some extent subsidized by the government. On the other hand, the vast majority of Italy's population is commonly assumed to have consisted of small-scale, independent farmers who had little direct involvement with the marketplace, especially

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<sup>14</sup> Harl's claim (1996, 272–73 cf. 212) of a three-fold increase in the price of wheat at Rome, from 4 to 12 asses per *modius*, between the early second and early first centuries BC rests on a misinterpretation of Livy 30.26.5–6, where in 203 BC the aediles sold grain at four asses per *modius*. However, as Livy makes clear, these prices were the result of an abundance of grain on the market in that year; hence they cannot be taken as normal. Other evidence that Harl cites to demonstrate a "subsistence wage" of 3 asses (1996, 454 n. 4) similarly fail to convince: *trium nummum* at Pl. *Mos.* 357 probably means 'three denarii', while the nine obols that Plutarch reports, at *TG* 13.3, that the senate allotted to members of the Gracchan land commission in 133 BC for their daily expenses seem to represent HS 6 if 1 drachma (i.e. 6 obols) = 1 denarius. Finally, the price of a *modius* of wheat in 73 BC, 12 asses, is based on prices in Sicily, not Rome: Cic. 2 *Ver.* 3.163. See further Duncan-Jones (1990, 147–48).

<sup>15</sup> Crawford (1985, 177) suggests that the rate of inflation during the second half of the second century BC was "remarkably slow", while Howgego (1995, 122) sees the progressive abandonment of small denomination coins as indicative of a "general, but not necessarily continuous" inflation from the second century BC onwards. Burnett (1987, 108) estimates that prices approximately doubled between the second and the first century BC.

<sup>16</sup> See in general Erdkamp (2005).

when it came to food. They raised most of what they ate and bartered for what they could not grow. This picture almost certainly exaggerates the primitiveness of the small-holder economy.<sup>17</sup> The widespread presence of mass-produced black-glaze and other pottery and various ceramic votive offerings suggests that the ordinary Romans who purchased them were not altogether strangers to the getting and spending of money. And to the extent that citizens in the lower census classes in fact paid *tributum* prior to 167 BC, this, too, probably required payment in cash rather than kind. Still, these expenditures do not imply a need to buy the food they lacked. That may have changed when a growing population, the cessation of colonization, and partible inheritances and dowries resulted in children receiving smaller and smaller subdivisions of their parents' holdings, until they found themselves at last with not quite enough land to feed themselves and their families. These unfortunates will have had no alternative but to find work in order to make up the difference by purchase, and when there were enough of them, the result will have been to drive up prices and depress wages.

Nevertheless, even if prices for city dwellers and some in the countryside did rise as the population increased, their impact on Rome's government is likely to have been far smaller than what we would expect according to Goldstone's model. Unlike early modern England or France, the Republic's empire largely protected it from the vagaries of the marketplace, particularly when it came to feeding its armies. As Erdkamp has shown, Sicily and Sardinia, through the taxes they paid in grain, furnished much of the food the legions and allies required.<sup>18</sup> The rest came from plunder and the contributions—voluntary or otherwise—of allies. True, Gaius Gracchus' grain law got the Republic into the business of providing a subsidized food supply for the urban plebs. But by that time Rome had annexed Carthaginian North Africa, and Asia Minor would soon be coming on line, furnishing ample resources with which to meet this new obligation. After the suspension of *tributum* in 167 BC, the Republic had to meet the cost of paying its legionaries' *stipendium* without taxing its citizens, but the *stipendium* appears to have remained fixed until Caesar doubled it, so inflation will not have affected the cost of the soldiers' pay.<sup>19</sup> And while Roman armies may

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<sup>17</sup> See Hollander (2007, 122–35).

<sup>18</sup> Erdkamp (2000, 53–70); *idem* (1998, 84–111).

<sup>19</sup> Suet. *Jul.* 26.3.

have grown larger over the course of the later second century BC, that increase occurred within the contingents supplied by Rome's *socii*, who were responsible for paying them.<sup>20</sup> Otherwise, the republican government was remarkably cheap to run. There will have been the expense of raising and equipping armies (although the costs of some soldiers' weapons and armor and every legionary's food were deducted from their pay) and of transporting them and their supplies, but these will not usually have been excessive. Magistrates received no payment beyond expenses for themselves and their staffs, and no extensive bureaucracy that needed to be paid existed beyond the simple functionaries who were attached to the various magistrates.<sup>21</sup> But since the magistracies themselves were few, the number of *apparitores* who assisted them was likewise small, and the overall cost of administration limited, particularly since this was met largely by taxing the Republic's provincial subjects, who were in no position to resist these exactions. Goldstone's emphasis on the deleterious effects of inflation on the ability of the institutions of governments to function therefore helps us not at all here.

## V

Indeed, it is from just this institutional perspective that Republican Rome appears to diverge radically not just from Goldstone's model and the governments of early modern Europe but from the governments of nearly every other large pre-industrial state that I can think of. Monarchies ruled almost all of them, monarchies supported to a greater or lesser extent by structures, organizations, and personnel that in various ways provided the instruments of coercive power essential to their continued existence. All governments, whatever their form, must have ways of exercising the compulsion essential to their ability to function. Without some institutionalized form of coercive power over its subjects, even if this takes the most benign form, no government can govern. From this perspective the Roman Republic appears stunted compared with the governments that ruled other pre-industrial states. As noted above, Rome at this time possessed no bureaucracy to speak of, paid or otherwise. It had no standing, professional army or other police force that could be counted on for loyalty in times of crisis. No

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<sup>20</sup> Brunt (1971, 677–86).

<sup>21</sup> On the *apparitores* see Purcell (1983, 125–73), mainly dealing with the Empire.

well-developed body of normative law embodied in a criminal code existed through which to discipline the population; a sophisticated corpus of civil law was only beginning to come into being in the later second century BC as a result of the work of the Roman jurists. And although civic religion was firmly in the hands of the senators, this did not constitute an authoritative body of moral and ethical precepts and dictates that could serve as a vehicle for social control. Instead, the senate's position as the Republic's governing organ rested until Sulla only on tradition and consensus. The strength of senatorial government and its ability to meet a crisis therefore was really only as strong as its cohesiveness and the collective respect in which its members were held, along with the perception that its leadership was beneficial to the *res publica* and acceptable to its citizens. There was really nothing else propping it up. Clearly, there were obvious reasons why the Republic's government had evolved along these lines, and for a very long time it had served the interests of Rome's leaders and ordinary citizens very well. But beginning around the middle of the second century BC and continuing into its later decades, that perception changed as respect for the senatorial order began to erode.

As wealth came to play an increasingly important role in aristocratic rivalry, and particularly as a consequence of the fact that some senators benefited enormously from rich conquests while others did not, the imperative grew for senators to turn a profit from their wars or tenures as the governors of peaceful provinces. This need will only have become more urgent after 123 BC and Gaius Gracchus' law on the Asian taxes, which as mentioned above altered dramatically the relative access that senators and *publicani* had enjoyed up to that point to the profits of empire. The growing wealth of some among the *equites* only added to the pressure on senators to keep up in the competition in generosity and display that was increasingly coming to define the aristocratic lifestyle. The consequences of these developments were several, but among the most pernicious was a growing perception of corruption among members of the senatorial order. This development began, as is well known, with occasional charges of maladministration early in the second century BC, particularly against several governors in Spain, but accusations against the Scipios, M. Acilius Glabrio, and others also contributed.<sup>22</sup> The establishment of the *repetundae* court in

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<sup>22</sup> Governors in Spain: Livy, 43.2.1–12; on Glabrio and the Scipios see Gruen (1995, 59–90) for sources and discussion.

149 BC marked an important milestone, but the notorious reluctance of senatorial jurors to convict their patently guilty peers enabled Gaius Gracchus to rally support for his law that placed equestrians on the juries instead.<sup>23</sup> Yet by this time the willingness of the public to believe—or at least of *populares* to charge—that corruption went far beyond provincial administration was widespread. Gaius himself, in a speech to the people during his first tribunate, could claim of his fellow senators that all of them had been bought.<sup>24</sup> A decade later accusations that various senators had taken bribes from Jurgurtha led to the Mamillian *quaestio*.<sup>25</sup> Finally, in 103 BC Saturninus passed legislation establishing a standing court to hear accusations of treason (*maiestas*) against senators, a charge destined to have a long and unhappy history at Rome.<sup>26</sup>

This is all a familiar story. What needs fresh emphasis here is the role these developments played in eating away at the foundations of senatorial control. The senate had always exercised that control mainly through ideology, through the *populus*' acceptance of the legitimacy of senatorial authority and that of individual senators in the conduct of public affairs. *Auctoritas*, as Galinsky puts it, “comes from special insight and is so weighty that the person seeking advice will almost certainly accept it... It is acquired... by an individual's superior record of judgment and achievement”.<sup>27</sup> Implicit in the concept of *auctoritas* therefore is the premise that the advice is beneficent and sound: the achievements, insight, and judgment of the person offering advice are so superior to those of the person seeking it as to render it in the mind of the latter the best course of action available. Transferred to the *res publica*, a course of action backed by the *auctoritas* of the senate represented what in the circumstances was likely to prove to be in the best interests of the Republic. *Auctoritas*, then, amounted to a kind of guarantee endorsing a particular course of action, and essential to that guarantee was an aristocratic ethos predicated on a selfless devotion to the interests of the Republic and its citizens.<sup>28</sup> The *populus* could trust any advice backed by the *auctoritas* of a senate that only had its best interests in view. Even when individual senators differed in the courses of action they urged,

<sup>23</sup> Gruen (1968, 8–15, 86–91).

<sup>24</sup> *ORF*<sup>4</sup> C. Sempronius Gracchus 44 = Gel.11.10.2–6.

<sup>25</sup> Sal. *Jug.* 40.1–3.

<sup>26</sup> Sources in Broughton (1951–52, 1.563).

<sup>27</sup> Galinsky (1996, 13–14).

<sup>28</sup> Cf. Rosenstein (2006, 365–81).

their listeners could be sure that they aimed only at winning honor and esteem through their advice. Genuine *auctoritas* in effect therefore entailed a kind of mutuality on the parts of the giver and the seeker of advice. It was, as Galinsky concludes, “something . . . granted not by statute but by the esteem of one’s fellow citizens”.<sup>29</sup> That mutuality was especially manifested and embodied in the citizens’ regular, almost ritual approbation, in annual elections and elsewhere, of individual aristocrats’ claims to have lived up to the elite ideals that both Rome’s ruling class and the public embraced.<sup>30</sup>

The collective ascendancy of the senatorial order therefore depended to a very great extent on perceptions, and charges of corruption inevitably altered those perceptions. In so doing, it ate away at the ideological foundations of senatorial rule. Accusations of bribe-taking, provincial extortion, and the like began to cast doubt on the motives not just of individual aristocrats but of the whole senatorial order. Aristocratic ambition and the public good could easily coincide when senators competed principally for honor and esteem. They diverged sharply, however, where money was the end senators had in view, and that divergence eroded the trust that senatorial *auctoritas* depended on. The fact that the accusations arose from among the *patres* themselves further weakened that trust, for how could one rely on the *auctoritas* of the senate when the senators were trading charges of bribe-taking and extortion? Such charges exacerbated an increasing bitterness in aristocratic rivalry already overheated by the growing number of young aristocrats entering the political arena and by the growing disparities in wealth among the *patres*. All this in turn made it more and more difficult to maintain the cohesiveness essential to senatorial control of the state.

This erosion of confidence in aristocratic leadership occurred within a broad spectrum of citizens, including those whom one might term the ‘middling sorts’ for want of a better term. These were men in the upper and middle range of the census classes, whose votes were essential to the continuity of Rome’s leading families in the Republic’s highest office. Their growing disenchantment was fed not simply by suspicions about the senators’ ulterior motives but by a growing sense that the social bargain that had underpinned aristocratic government

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<sup>29</sup> Cf. Galinsky (1996, 14).

<sup>30</sup> Cf. Morstein-Marx (2004, 204–78).

for so many generations was being contravened. This compact entailed an implicit understanding that ordinary Romans would accept elite leadership and fight the Republic's wars in exchange for a substantial share of the profits that resulted from the conquest of an empire. This arrangement was in effect the material foundation on which the ideological legitimacy of senatorial rule rested: the *patres'* advice could be seen to be sound and advantageous because Roman soldiers were enriched by the wars the senate urged them to undertake and in which individual senators led them. Although it is not possible here to explore fully this point, I believe that Romans in the middle Republic were not sharply divided between rich and poor but were broadly prosperous, at least by pre-industrial standards.<sup>31</sup> The source of their wealth and the mechanism for its wide distribution was war: soldiers profited greatly from plunder, while the formation of a large class of very poor citizens and allies was obviated by regular distributions of conquered land. In effect, these pay-offs constituted a tangible validation of the senate's *auctoritas* in the conduct of the *res publica* and Rome's foreign affairs. Yet as tax collection, money-lending, and various forms of provincial extortion came more and more to represent how the profits of empire were being extracted, ordinary citizens who had no share in these activities could easily come to perceive that the empire's riches were not being shared out fairly. This impression would only have been increased during a period in which the wars that were fought were proving less lucrative to Rome's soldiers than had earlier victories, particularly when those wars went badly. At the same time, the various schemes advanced by ambitious politicians to mitigate the plight of the urban and rural poor—Gaius Gracchus' grain law, for example, and various land distribution schemes in Italy—would only have enhanced a sense that the fruits of earlier victories were not being handed out equitably. It could easily seem that these sorts of measures pandered to the demands of a well-organized and vocal minority, while those in the middle ranks of Republican society were gaining little or nothing. Moreover, the distribution of *ager publicus* may well have raised additional concerns in the minds of many. Empowering a panel of aristocrats eager to serve the needs of the poor to go out and hunt for public land could quite easily have struck many of the 'middling sorts' as an excuse to expropriate private land under the pretext that it was public property, particularly at a time when a rising population was putting pressure on existing

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<sup>31</sup> But see now the contributions of Kron and Rathbone in this volume.

supplies of farmland and senatorial integrity was already suspect. It will have mattered little that land commissioners quickly turned their attention to allied lands or those overseas; the damage had already been done. And finally, the violent episodes that punctuated the last decades of the century will, despite their suppression, only have raised further questions about the senate's fitness to govern.

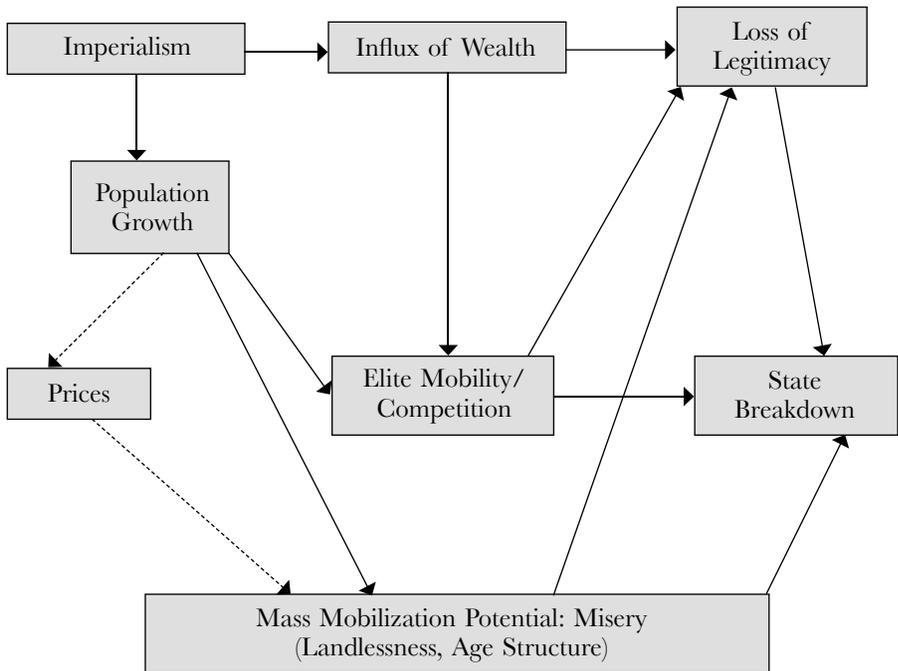
## VI

By the time crisis quite unexpectedly enveloped the Republic in 91 BC, key elements of Goldstone's model were largely in place. Overpopulation made Drusus' proposal to revive his father's colonial scheme attractive to the Roman plebs and anathema to the Italians, whose livelihoods were threatened not only by the bill but by the growth of their own populations. The presence of a large cohort of younger men whose hopes for a family depended crucially on obtaining a farm and whose youth disposed them to take bold measures to secure one made the situation explosive. The elite, too, was divided. Roman aristocrats, facing increased competition because of the growth of their own numbers, resented the inroads Italian notables were making into the republican citizen-body and alienated them by rebuffing their aspirations. The rising of their allies that followed temporarily unified the Romans, for the one thing they all could agree on was the advantage of retaining their privileged positions vis-à-vis the *socii*. But the aftermath found the Republic still struggling to cope with problems of overpopulation that now embraced the entire peninsula. The Republic's leadership remained deeply divided by bitter political competition. Cut-throat ambition marred the consular elections for 88 BC and the struggle over the Mithridatic command. Suspicion of the senators' motives and loyalty was endemic: charges of treason and betrayal flew even before the war ended, while debt was believed to be widespread among the senators.<sup>32</sup> And finally, to cope with all this the senate lacked any effective means of coercion when widespread distrust undermined the ideological bases of its authority.

The crucial elements of Goldstone's model, it seems to me, are all reasonably well represented; the main divergences are two, as illustrated

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<sup>32</sup> See, conveniently, *CAH*<sup>2</sup> 9.114–15, 165–70. Note, too, the murder of the praetor A. Sempronius Asellio by creditors in 89 BC when he sought to give relief to debtors: *Livy Per.* 74; *App. BC* 1.54.



Modified Goldstone Schema

Fig. 4

in Figure 4. First, imperialism is one of two exogenous drivers of the system (and perhaps the sole driver, if we discount disease as a factor in population growth). In other words, the creation of an empire was not significantly affected by the other elements of the system but exerted a critical influence in three different directions. It facilitated general population growth and enlarged its younger segments; it weakened aristocratic cohesion and increased aristocratic competition both through population growth and the unequal distribution of the spoils of conquest; and it indirectly undermined the foundations of the government as the motives of aristocrats became suspect and the share of the profits of conquest going to those in the middle range of Roman society diminished. Secondly, price increases failed to impede the effective functioning of government. But as I have tried to suggest, the unusual institutional character of republican government made it uniquely vulnerable to a crisis of confidence that could fatally weaken the ability of senatorial *auctoritas* to control the *res publica*.

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STATES WAITING IN THE WINGS:  
POPULATION DISTRIBUTION AND THE END  
OF THE ROMAN REPUBLIC<sup>1</sup>

Michael Crawford

No one could accuse Peter Brunt of having chosen easy historical problems to deal with, whether the demography of ancient Italy in *Italian Manpower* of 1971 or the end of a political system that had lasted nearly half a millennium, in the essay ‘The fall of the Roman Republic’, which forms the Introduction to the collection of articles, published and unpublished, of the same name, of 1988. The two problems, however, are not linked in the second contribution, although the view we take of the population of Roman Italy affects our view of almost every aspect of Roman history, as Walter Scheidel observes at the opening of his piece in this volume. What I hope to show here is that a view we ought to take of the end of the Roman Republic has a substantial impact on our understanding of Roman demography.

The essay of 1988 is shot throughout with passion, as we shall see, but also with pessimism, a pessimism that I do not remember as an undergraduate and that was perhaps not thought appropriate in a didactic context: the essay ends with the words, “The historian of Rome can be likened to a man standing at the entrance of a cavern of vast and unmeasurable dimensions, much of it impenetrably dark, but here and there illuminated by a few flickering candles.” Why should an intelligent person place themselves in such a position?

I shall come in a moment to justifying the view that the Roman revolution was indeed a revolution; accepting for the moment that it was, no power so large was overtaken by revolution between the end of the Ancient Near East and the Russian revolution, and whether or not our judgment is sharpened by comparative history, there is an undoubted fascination, for an age living with the consequences of the

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<sup>1</sup> This paper was originally given at a meeting on 15 September 2006 in London, in memory of Peter Brunt, organised by the Roman Society and the Institute for Classical Studies; some of its conclusions were presented in discussion at Leiden, and I am grateful to Luuk de Ligt for agreeing to include it in this volume.

Russian, German, Chinese, and Islamic revolutions, in the analysis of the Roman revolution.

The essay of 1988 concludes, rightly but not really surprisingly, that the Roman revolution was caused by a mix of social factors, personal ambition, and mere chance, of long- and short-run factors, factors whose analysis forms the central dilemma of anyone who tries to teach the late Republic.<sup>2</sup> The essay does not deign to mention Erich Gruen's bizarre view that it was only by accident that the Republic met its end when it did; and what I should like to do here is to offer another nail for the coffin of such a view, by drawing attention to a factor that is at once cultural, social, and economic, and that I have come to think is central to understanding the last couple of generations of the Roman Republic.<sup>3</sup>

The essay does reject as absurd Syme's view that Roman history is the history of the governing class, and with little more ceremony his view that Augustus set out to destroy the old Roman nobility, which *was* the Roman revolution. For Peter Brunt, it was the loss of freedom that was the Roman revolution; and, as a corollary, politics in the free Republic was about real issues, the rights of the people and their access to the rewards of empire. A fine defence of the centrality of freedom is to be found already in the speech of Brutus on the proposed dictatorship for Pompeius (Quint. *Inst.* 9.3.95 = *ORF* 158.1.16), and in his contemporary coinage. Brunt's passion comes out in the pages where he points out that it was the Senate that began in 133 BC the resort to violence.

A not dissimilar view emerges from Arnaldo Momigliano's review of *The Roman Revolution*, in the *Journal of Roman Studies* for 1940,<sup>4</sup> a review that I was certainly instructed to read as an undergraduate, but with which there is no explicit dialogue in the essay of 1988: the end of rule by a Roman oligarchy and by a Roman people *was* the Roman revolution, *because* they had not shared their freedom, in the first instance with the men of Italy. It was for Brunt "Italian voters and Italian swords (that) destroyed senatorial supremacy", because of senatorial rapacity and arrogance.<sup>5</sup>

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<sup>2</sup> Brunt (1988, 91–2).

<sup>3</sup> See Crawford (1976, 214–17).

<sup>4</sup> Momigliano (1940, 80).

<sup>5</sup> Brunt (1988, 69).

The failure to pursue this remark is the result, I think, of Brunt's emphasis on the continuity of Roman elite values—my phrase, not his—despite the admission of Italians and in due course provincials to the elite; and of his argument that, despite the loss of freedom, the *fundamenta* of *otium cum dignitate* were in fact secured by Augustus.<sup>6</sup> As Brunt remarks, the climate of admiration of Augustus and Tiberius on these grounds is well captured by Velleius (2.89, 126).

The essay is in fact, I think, much more an attempt to establish what changed between Republic and Empire than to explain the end of the Roman Republic. The Senate failed “to solve the problems that arose from Rome's expansion”; but the essay says little about the period between 70 BC and 50 BC, except in very general terms.<sup>7</sup> It accepts that problems with the Italians and the Equites were resolved in 70 BC; and, as Henrik Mouritsen has rightly observed in discussion, it is indeed well-nigh impossible to detect thereafter anything resembling an Italian agenda.

The peasants wanted access to land, the city poor wanted grain, the Senate failed to satisfy the soldiery, the first and the last being of course essentially the same constituency, as Brunt made clear already in the great article of 1962, ‘The army and the land’;<sup>8</sup> the identification of the factors—whose importance is of course undeniable—owes much, I think, to the plausibility of the claim that it was Augustus who sorted them out.

In contrast, in arguing—along with one of the ancient sources, who was nobody's fool—that the old Republic had disappeared by 49 BC, I shall not go beyond 42 BC, and only minimally even as far as that.

Before I do so, I should like to acknowledge my debt to Keith Hopkins' essay ‘Structural differentiation in Rome’: as the Roman Republic became more complex and its institutions more differentiated, so conflict arose; and the breakdown of consensus was the result of the failure of the community to absorb these conflicts.<sup>9</sup> This is in my view the first attempt ever to break away both from a narrative approach to the explanation of the end of the Roman Republic *and*

<sup>6</sup> Chaplin (2000) uses Livy as an example of the use of the past to justify change.

<sup>7</sup> Ancient accounts, in retrospective post-Gracchan apportionment of blame, blamed moral decline on imperial expansion: see Lintott (1972).

<sup>8</sup> Brunt (1962).

<sup>9</sup> Hopkins (1968).

from the explanatory categories of the ancient sources. A version of it is rather uneasily tacked on to the first chapter of *Conquerors and Slaves*, of 1978, most of which is about something else, namely the fate of the peasantry between Republic and Empire, for which indeed the chapter is cited by Brunt.<sup>10</sup>

The civil wars between 49 BC and 42 BC, then, killed some 100,000 men; just before the battle of Philippi, the portrait of Brutus the liberator appeared even on his coinage; and even if it is only to 19 BC that Dio dates all the elements of monarchy, a revolution was by 42 BC surely irrevocable. Let us go back first twenty years, then ten years. In 62 BC any Roman, not just Cicero, might have looked round contentedly and reflected that Rome had seen off Lepidus, Sertorius, Spartacus, the pirates, Mithridates, and Catiline, with varying degrees of effort. In 52 BC Pompeius was sole consul, and governor of Spain without ever going there, either before or after, the *noua genera imperii* characterised by Caesar as the catalyst of his invasion of Italy. What had gone wrong?

Well, Brunt is of course right to say, however briefly, that the response to the crisis of 67 BC—and one might add of 66 BC—was at any rate one of the *causes* of the ambition of Caesar in 59 BC, from which much else flowed.<sup>11</sup> Whoever it is who lies behind a rather muddled note on Lucan 1.87, he had a similar idea, that Pompeius, Crassus, and Caesar had the whole world divided up between them.<sup>12</sup> But there was much more going on, and that is what I should like to explore.

First, the last generation of the Roman Republic sees a changing process of decision-taking at Rome, with decisions sometimes not taken at the centre, but controlled from the periphery. What can be seen on one level as an attempt to adapt Roman political institutions at any rate to their Italian dimension can be seen on another level as control slipping away from the centre. I refer to the passing of decrees by the councils of the cities of Italy, first attested in 63 BC, decrees which were then forwarded to Rome, and presumably influenced opinion

<sup>10</sup> Hopkins (1978, 74–96); Brunt (1988, 68 n. 47).

<sup>11</sup> Brunt (1988, 76).

<sup>12</sup> *Supplementum Adnotationum super Lucanum* 1.87: *isti tres... totum orbem inter se diuisum habebant, Crassus quidem Parthiam atque orientalem plagam, Pompeius Aegyptum ac meridianam plagam, et Iulius Galliam atque omnem paene Europam inter se sortiebantur*; the remark about Pompeius presumably reflects a moment when it was thought that he would restore Auletes.

there. And just as every schoolboy used to know that Gaul was divided into three parts, so every undergraduate used to know that Pompeius and Crassus trailed to Luca, within the province of Caesar, to fix the elections for 55 BC. What Dio tells us—and it is not the least of his services to the study of the history of the late Republic—is that the three men did not stop there, but arranged for P. Crassus, the younger son of the consul-to-be, serving with Caesar, to bring soldiers to Rome to influence the voting in those elections (39.31.2).

But not only did the periphery directly influence what happened in Rome; much of it, admittedly for different reasons, was outside the control of the Senate for long periods between Sulla and 49 BC: Spain under Sertorius and Perperna, the east under Pompeius from 66 BC to 63 BC, Gaul under Caesar from 58 BC to 50 BC, Spain under Pompeius from 54 BC to 50 BC, the *noua genera imperii* that I have already mentioned. The attempt of Sertorius was obviously made in full awareness both of the state created by the peoples of Italy in 91 BC and of the attempt in 82 BC to create a state to resist Sulla based on northern Etruria and Corsica.

And all of the areas in question in effect had their own governments. Sertorius had his own senate and magistrates (App. *BC* 1.108, 507), both he and Perperna had their own *scribae* (Sal. *Hist.* 3.83): ‘so they sat down at their places for dinner, Sertorius in the lower position on the central table, and above him L. Fabius Hispaniensis, a senator who had been one of the proscribed; on the top table Antonius and below him Versius, the scribe of Sertorius, and the other scribe Maecenas at the bottom table, between Tarquitius and Perperna, whose scribe he was’ (for the government of Sertorius see also Livy, fr. 22). The war between Sertorius and Rome was regarded retrospectively as a civil war (Plin. *Nat.* 7.96; compare Flor. *Epit.* 2.22 = 2.10.9); and I should argue that when Sertorius corresponded with the *dunatôtatoi* in Rome (Plu. *Pomp.* 20.4), he was writing as the head of a government, even if he described himself as a proconsul.

Pompeius controlled the finances of the areas he ruled between 66 BC and 63 BC—I use the term advisedly—which seems to me much more important in the present context than his settlement of the east after the defeat of Mithridates without inviting the Senate to send *legati* to advise him; this has attracted disproportionate attention because of the fuss made by Lucullus and duly recorded by our sources; it is perhaps this background that leads Fergus Millar to emphasise Pompeius’

personal, rather than his institutional position.<sup>13</sup> Caesar recruited freely, as if he was a state, in the areas he ruled between 59 BC and 50 BC; and what he met in 49 BC in the Spain governed by Pompeius was a state ready for war.

These alternative states, for that is what they were, also provided a whole career structure that was alternative to the normal *cursus*: most of the men who had begun their career with Sertorius in an alternative state were quietly reinserted into the Roman political structure; the *legati* of Pompeius against the pirates acquired *imperium* not as a result of the vote of the people, but on his nomination; men as different as Cicero's younger brother Quintus, Crassus' younger son Publius, whom we have already met, the jurist Trebatius, all chose to look to Caesar in Gaul for the furtherance of their careers, long before anyone thought that there might be political and military choices to be made.<sup>14</sup> An emblematic figure is D. Albinus Bruti f., one of the organisers of support for Clodius in the autumn of 57 BC, who was then with Caesar in Gaul for the whole of the period from 56 BC to 52 BC.<sup>15</sup>

What underpinned all these alternative states, of course, was the scale of Roman (and Italian) settlement overseas from the middle of the second century BC onwards. I shall return to them later, but their existence meant that all of the dynasts had on the spot in the provinces men who could be recruited as their assistants and advisers, as their supporters, as their soldiers: few men left Italy with Sertorius;<sup>16</sup> and it was not at Venusia that the poet-to-be Horace was recruited to fight at Philippi.

Nor did the men who had served an alternative state always show much interest in returning to the Roman state. Deserters from the Roman armies there had always been, attested at least since the widow of Agron was forced to surrender them to Rome during the Illyrian wars (App. *Ill.* 7.20). But it was surely a new phenomenon when some of the former soldiers of Sertorius settled among the Aquitani and, much to his surprise, fought with them against Caesar (DC 39.46.3).

<sup>13</sup> Millar (1977, 611–12).

<sup>14</sup> Harries (2006, 116) seems to think that it was a change in the status of jurists that led Trebatius to seek patronage from Caesar.

<sup>15</sup> See Cic. *Dom.* 50; *Att.* 6.3.2, with Wiseman (1968, 299–302) against Shackleton Bailey.

<sup>16</sup> See J.-M. Roddaz, cited in n. 34 below.

Let us return to the rulers of these alternative states, and indeed to the elite as a whole of the last generation of the Republic. Their euergetism and their building is on a Mediterranean scale, no longer limited to Rome and Italy. The first benefactor outside Italy, as far as I know, is L. Caesar, consul in 64 BC, benefactor of Athens. In the following year Pompeius gave 50 talents, or 300,000 denarii, to Athens (*Pomp.* 42): the sum is about half of what it cost to maintain a legion for a year. These benefactions were followed by the monument of C. Memmius at Ephesus,<sup>17</sup> the building programme of Ap. Claudius Pulcher at Eleusis,<sup>18</sup> and by the munificence of Caesar, in Suetonius' words already in 50 BC *Asiae quoque et Graeciae potentissimas urbes praecipuis operibus exornans* (*Jul.* 28), embellishing also the most important cities of Asia and Greece with outstanding building works.

These men, as well as lesser figures, also negotiated with other states for support in their bids for power as if *they* were states, a process acidly characterised by Sallust in the *Oratio Lepidi* as *arma ab externis in nosmet uersa* (*Hist.* 1.55.17). The Samnites had of course negotiated with Mithridates,<sup>19</sup> the younger Marius with Iampsas of Numidia (*Plu. Mar.* 40), Cn. Domitius Ahenobarbus with Hiarbas of Numidia; but Sulla in effect decided to let Mithridates off the hook, ensuring in his memoirs that his action was duly whitewashed (*Plu. Comp. Lys. Sull.* 5.2; the parallel of Lysander turning to Persia is all too obvious). Sertorius negotiated with the pirates and with Mithridates, though unlike the Samnites he probably refused to renounce the province of Asia.<sup>20</sup> Catiline negotiated with the Allobroges (*Sal. Cat.* 40.1), Q. Metellus Scipio promised to surrender Africa to Juba (DC 43.4.6), Q. Caecilius Bassus in 44 BC was willing to accept assistance from Pacorus the Parthian. Nor are we dependent on literary sources alone: in the long sequence of inscriptions recording embassies from foreign states to Rome, there is one recording an embassy from Tragurium, modern Trogir, in 56 BC, not to Rome, but to Caesar in Aquileia (*RGDE* 24). It is not surprising that Mithridates of Pergamum cheerfully raised an army to relieve Caesar in Alexandria.

Members of the Roman elite had always turned to a *consilium* of their friends and family for advice; but the *consilia* of the dynasts are those

<sup>17</sup> *IGKS Ephesos* 403.

<sup>18</sup> *ILLRP* 401; Sauron (2001).

<sup>19</sup> See Crawford (2004).

<sup>20</sup> See *Plu. Sert.* 7.3; 21.5; Gelzer (1963).

of rulers of states, drawn from the whole of the Mediterranean world they aspired to control. To begin with Pompeius, perhaps modelling himself on Aemilianus and Polybius, a central figure in his entourage was not by origin a Roman, though he was in due course rewarded with citizenship, but Theophanes of Mytilene; he acquired also great wealth, whence flowed major benefactions to his native Mytilene; the reward for those was a plethora of honorific decrees and eventually deification (Tac. *Ann.* 6.18.5). His family remained influential in the Greek east long after his death (Suet. *Aug.* 56; Str. 13.2.3 = 617–18);<sup>21</sup> and Strabo saw *him* as the central influence on the conduct of Pompeius. At Caesar's side in Gaul was the father of the historian Pompeius Trogus, of Vocontian Gallic origin, described by Jürgen Malitz as in effect his *ab epistulis*; later there were Artemidorus of Cnidus and Balbus of Gades, both also richly honoured in their home towns for the power their association with Caesar brought. Of Caesar, Malitz indeed goes so far as to talk of his 'Kanzlei', his chancellery.<sup>22</sup> And Momigliano was right to observe that one aspect of the Roman revolution was an Augustan counter-revolution: at his side were Agrippa and Maecenas, not a Theophanes or a Balbus; nor did he ask a Posidonius to write about him, but Vergil and Horace.

The dynasts also had productive capacity at their finger-tips, in one case of a particularly sinister kind. A recent article by Daniele Manacorda has drawn attention to the amphorae of Cn. Pompeius Magnus;<sup>23</sup> they are Dressel IA in type, were made in the Vesuvius area, and date to 75–50 BC; examples are attested near Civitavecchia in one direction, on Malta, ancient Cossyra, in the other; these stamped examples are presumably proxy for larger numbers of unstamped examples. I do not want here to get into the argument about senatorial involvement in trade, but to foreground Manacorda's suggestion that the wine in the amphorae was for the fleets and armies of 67 BC and 66 BC; and to recall the suggestion that much of the wealth of the late republican elite came from supplying the armies of the period, paid for of course out of the revenues from the provinces.<sup>24</sup> The productive activity of the elite was in fact parasitic on empire; but the productive capacity was

<sup>21</sup> *EE* 2, 19; Anastasiadis & Souris (1992, with bibliography); White (1992, 210: his descendants).

<sup>22</sup> Malitz (1987).

<sup>23</sup> Manacorda (2005).

<sup>24</sup> See my remarks in Giardina (1981, 271–283).

there nonetheless.<sup>25</sup> My second example of productive capacity takes us into a more dangerous world: and it is from Dio that we learn that in 50 BC Caesar possessed not simply the wealth to finance his invasion of Italy, but the capacity to manufacture weapons (40.60.1).

If one asks oneself how organised these alternative states were, various things come to mind. It is surely significant that Crassus chose to define wealth in terms of ability to pay troops, something one would have thought characteristic of states.<sup>26</sup> And just as the Italians in 91 BC had begun immediately to produce their own coinage, so Caesar struck coinage immediately after invading Italy in 49 BC. And in his case, the massive scale of the coinage documents the nature of the organisation that lay behind it.<sup>27</sup> Nor did he hesitate in due course to double the pay of his troops.<sup>28</sup>

But, you will say, these reflections are only possible with the benefit of hindsight, with precisely the knowledge of what came with Augustus. Well, no. There has been preserved for us the letter written by Dolabella in 48 BC before Pharsalus, in the camp of Caesar, to Cicero—unfortunately for him also the father-in-law of Dolabella—in the camp of Pompeius (*Fam.* 9.9 = 157 SB, 2–3). Cicero has evidently been attempting to remind Dolabella of his duty to the *res publica*; Dolabella's reply is chilling, *reliquum est, ubi nunc est res publica, ibi simus potius quam, dum illam ueterem sequamur, simus in nulla* ('What matters is that we should rather be where the *res publica* is now than end up in none while we chase after the *res publica* of old').<sup>29</sup> A tragic, not cynical, assessment of the situation was offered by Matius in August 44 BC: wanting the *res publica* (of old) to be *salua* was seen by its guardians as incompatible even with expressions of regret for the death of Caesar by a minor figure who had been his personal friend (*Fam.* 11.28).

What are the demographic implications of these alternative states? The high count implies the existence of (at least) *c.* 13–14,000,000 Roman citizen people, men, women, and children, in 28 BC. This figure results from holding that *capita* in pre-Augustan, Augustan, and post-Augustan censuses means adult male citizens and applying a

<sup>25</sup> See also Nonnis (2003).

<sup>26</sup> See Whitehead (1986).

<sup>27</sup> See *RRC* 443.

<sup>28</sup> See Brunt (1950).

<sup>29</sup> In an earlier age, conflicting claims of *uindicatio in libertatem* had of course testified to conflicting claims to represent the *res publica*.

plausible multiplier. I do not propose to discuss either the details of this calculation or the possibility that *capita* might mean something different, except to say that the evidence collected in *TLL*, s.v., cols. 404.3–408.7, does not encourage belief in such a possibility. As has been recognised for a century and more, the nub of the problem consists in the values we assign to the following categories, which have to be added to the Roman adult male citizens counted before 90 BC: Italian allies, Latins, Transpadani (and probably Ligures, ignored alike by our sources and by modern scholarship) enfranchised in 49 BC, provincials enfranchised in the age of revolution, and Romans living overseas and imperfectly included in censuses before 90 BC.<sup>30</sup>

Brunt was perfectly well aware that, for the low count to work, low values had to be assigned to all of these categories. The category that interests us here is that of Romans living overseas, to whom Frank assigned 10–20% of the total, say 15%; Brunt, deliberately minimising, assigned 7.5%, a figure accepted by Lo Cascio, in order to avoid inserting a higher figure more favourable to his case into his calculations. In contrast, I see nothing implausible in holding, against Scheidel, that half of all Roman people lived overseas after 90 BC.<sup>31</sup> Such a proportion would have permitted the functioning of the alternative states I have discussed above, and I certainly do not see how they could have functioned with a proportion as low as 10%.

In favour of a proportion closer to 50% than to 10%, I draw attention to three pieces of anecdotal evidence. First, the 80,000 Romans allegedly killed on the orders of Mithridates in 88 BC is obviously a figure intended to convey, on the basis of what evidence we do not know, that there were an awful lot. How far the ultimate source of this figure thought, as Keith Hopkins was to recommend, in coordinates, we do not know; but it suggests that Romans of the late Republic assumed that a large proportion lived overseas. Second, Cicero in the *pro Fonteio* 11, remarked that ‘Gaul was packed with *negotiatores*, full of Roman citizens: no Gaul conducts any business without the involvement of a Roman citizen, no coin changes hands in Gaul without appearing

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<sup>30</sup> *Contra* Scheidel (2004, 5) most of Liguria is not in any normal sense of the word mountainous; it is unfortunate that most writers in English on Roman demography have little sense of the physical, economic, social, cultural, or institutional specificities of ancient Italy.

<sup>31</sup> See Scheidel (2004, 6–7 n. 23); here pp. 26–28.

in the accounts of a Roman citizen'.<sup>32</sup> Again, I do not suppose that Cicero or his audience put any kind of figure on how many Romans there needed to be for Cicero's picture to be true; but again, for the claim to be plausible, Romans of the late Republic must have taken it for granted that very large numbers of them lived overseas. Third, just as Catullus in Verona in the last generation of the Roman Republic lived in an elite, Latin, literary environment, so also the Romans living in Corduba lived in a society where local-born poets gave recitations in Latin.<sup>33</sup> If fifty men went to such recitations, and 1% of Roman men were lovers of poetry, there were perhaps 20,000 Roman people in Corduba in the 70s BC.

I also return to the case of Sertorius, and to the likelihood of the creation of new citizens in substantial numbers even before the outbreak of the Civil Wars of 49 BC onwards. Sertorius is a rare *nomen* in Italy, and the substantial numbers of Sertorii attested overseas after him are likely to be the result of enfranchisements by him.<sup>34</sup> We cannot use the commoner *nomina* Pompeius and Iulius in the same way, but they are surely likely to have dispensed patronage in the form of citizenship on an even greater scale.

If half of all Roman people lived overseas after 90 BC, that would mean for 28 BC 7,000,000 Roman people living in Italy. Before going on to the next stage, I should like to make it clear that I do not claim that that was the position; but simply that, for every further 10% above 10% that one assigns to the proportion of Roman people living overseas, the nature of the problem of the population of Italy changes radically.

Let us suppose then that in 28 BC there were 7,000,000 Roman people living in Italy, 1,000,000 of them in Rome. Again, *contra* Scheidel, I see nothing implausible in supposing that half of the rest, 3,000,000 lived in Cisalpina.<sup>35</sup>

<sup>32</sup> Compare 13, supplementing *negotiatorum* rather than *equitum*; 46.

<sup>33</sup> See Cic. *Arch.* 26.

<sup>34</sup> See Roddaz (2006, 11): the article is marred by the usual muddle between *gens* and 'famille' (107, n. 66). Note also the suggestion of Momigliano (1940) that the father of Sex. Afranius Burrus had received the citizenship from L. Afranius, cos. 60 BC.

<sup>35</sup> Scheidel (2004, 4–5); here pp. 22–23, 59–60; at least Scheidel recognises, unlike Morley (2001), that Cisalpina is a completely different story.

Two points about rhetoric. In 2004 Walter Scheidel poured scorn on the Claudian census figure: “barring otherwise unknown mass enfranchisements of millions (!) of provincials” under Tiberius, Gaius, and Claudius.<sup>36</sup> Well, of course, that is exactly what happened. Maybe not millions, but if 2,500 auxiliaries retired every year, and were enfranchised with a wife and two children, that makes 100,000 new Roman people every decade. In 2005 Geoffrey Kron attempted to argue for the high count by overwhelming his readers with instances of density of settlement.<sup>37</sup> But we can never know how complete a picture of settlement has been recovered by archaeological exploration. Or can we? The specifics of part of the ‘Piana di Lucca’, dry, flooded, dry, flooded, now dry again, mean that we might be able to recover a total settlement pattern for a limited period; and the ‘cento fattorie di Lucca’ might take us beyond rhetoric.<sup>38</sup>

To return finally to Cicero and to break for a moment the undertaking I gave not to go beyond 42 BC, it is notorious that the only precedent for the powers of Augustus was created by Cicero in the powers given to Brutus and Cassius in the last moments of the *res publica* of old; but it had been Cicero much earlier who had been in large measure responsible for encouraging Caesar to believe—and no doubt to tell Dolabella—that he was the *res publica*. After Luca, Cicero not only proposed a *supplicatio* of fifteen days in honour of Caesar (Prou. cons. 26); he also claimed that he could not be the *inimicus* of someone whose conquests were so extensive (ibid., 22), that he could not not be the *amicus* of anyone *bene merenti(s) de re publica* (ibid., 24). I see no reason to suppose that Caesar’s memory was any less good than Cicero’s; and I am sure that when Caesar penned chapter 13 of Book 1 of his *Bellum civile*, on the decurions of Auximum explaining to Attius Varus that they could not exclude from their city someone who had achieved so much, someone *bene de republica meritum*, Caesar remembered—and knew that his readers would remember—what Cicero had said a few years earlier. The alternative state that was in the end successful—I assume continuity from Caesar to Augustus—was in part created, in the 50s and in the 40s, by someone who would have described himself as its greatest enemy.

<sup>36</sup> Scheidel (2004, 6–7, n. 23).

<sup>37</sup> Kron (2005): 453 and nn. 65–8 are a mess; at 490–3 Toynbee seems to have metamorphosed into Taylor.

<sup>38</sup> See Ciampoltrini (2005).

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