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## Roman population size: the logic of the debate

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**Abstract:** This paper provides a critical assessment of the current state of the debate about the number of Roman citizens and the size of the population of Roman Italy. Rather than trying to make a case for a particular reading of the evidence, it aims to highlight the strengths and weaknesses of rival approaches and examine the validity of existing arguments and critiques. After a brief survey of the evidence and the principal positions of modern scholarship, it focuses on a number of salient issues such as urbanization, military service, labor markets, political stability, living standards, and carrying capacity, and considers the significance of field surveys and comparative demographic evidence.

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

Our ignorance of ancient population numbers 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 point in time. When I say ‘we do not know’ I do not simply mean that we lack numbers that are both precise and safely known to be accurate: that would surely be an unreasonably high standard to apply to any pre-modern 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 overall Italian population number 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, in turn, determine how we assess Roman conditions in relation to other, later periods of Mediterranean population history. Secondly, moreover, this is by no means an antiquarian issue, a case of wanting to know for 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: thus, a ‘large’ population (by pre-modern 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 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.

## **2. Purpose and method**

For all these reasons, a better understanding of Roman population size is a vital concern for ancient and indeed all of pre-modern history well beyond the comparatively narrow ambit of the present project with its focus on second-century BCE Italy.<sup>1</sup> At the same time, the Leiden initiative calls for a broader vision of Roman demography 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,

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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, 2004-2008.

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 historical general context. Ideally, this exercise ought to be performed by a disinterested party with no stake in ongoing 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 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 Roman population number: 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.

### **3. 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 refer to 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 25 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 (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.

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<sup>2</sup> See esp. Brunt 1987: 15-120; Lo Cascio 1994a.

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

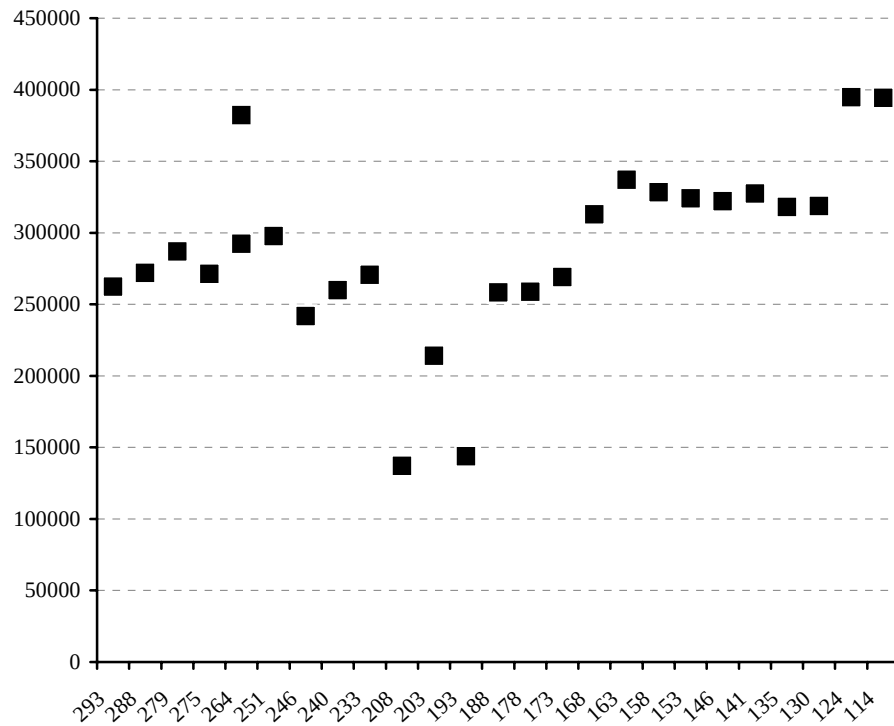


Fig. 1 Reported census tallies, 294/3 to 115/4 BCE

Source: Brunt 1987: 13

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 lowest figure, for 204/3 BCE, that is not completely incompatible with surrounding figures) and 395,000 (in 125/4 and 115/4 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

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

<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 chronologically adjacent figures. The two records cited in note 4 cannot be reconciled with much higher counts in the same periods. The result of 382,233 reported for 265/4 BCE can only be defended by considering all immediately preceding or following counts to be marred by massive underreporting, and is not deemed demographically possible here.

of the Italian allies after 89 BCE and of the 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 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 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.

#### 4. 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 2 to 1 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 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 underregistration, this reading is consistent with an Italian 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

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<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 4, the number of citizens would have had to double through natural growth, manumission of slaves, and enfranchisement of provincials 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 had been massively deficient. In other words, there is no way of accepting all of these tallies at face value.

<sup>8</sup> Beloch 1886: 370-378; Brunt 1987: 113-120.

<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: 456-457) 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, 376; Brunt 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: 178-181). The heart of the matter is that any of these claims are ultimately untestable: 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 1987: 97 (allies), 117 (with 198-203) (Transpadana), 121-130 (growth), 116 (census accuracy).

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 this interpretation of the evidence 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 the reading of Tenney Frank, Elio Lo Cascio and Geoffrey Kron that considers 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 certain multiplier 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 3,500,000 of the 4,063,000 adult males that he thought had been recorded in the census of 28 BCE to Italy, 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.

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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 (3 million, a pure guess: Brunt 1987: 124-125) or Beloch (2 million: 1886: 416).

<sup>12</sup> Beloch 1886: 507 posited a gross total of 6 million in 14 CE and 7 million in 47 CE (437; and see also Beloch 1903) but allowed for substantial growth all over the empire later on: Beloch 1899: 619-620. Brunt 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 per cent from 14 to 164 CE.

<sup>15</sup> Beloch 1899: 620.

<sup>16</sup> Cf. Lo Cascio and Malanima 2005: 210.

<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, 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: 340-341.

<sup>20</sup> Frank 1940: 1.

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 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 per cent lower than the 1994 estimate of 16.4 million), between 13.5 and 14.5 million of whom are thought to have resided in Italy proper (compared to 14.47 million in the 1994 estimate, or up to 6.7 per cent fewer than before). The inclusion of slaves raises the grand total to 15 to 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 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 overall Italian population size 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 per cent 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

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

<sup>22</sup> Lo Cascio 1994b: 116.

<sup>23</sup> Lo Cascio 1996: 292-293.

<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 the statements in this article that deal with details of Roman history to Lo Cascio. 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.

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 per cent 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 even of 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 per cent would add another million residents. Alternatively, an undercount of 5 per cent 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 might be the most plausible estimate.<sup>29</sup> Thus, it is hard to envision a minimum of fewer than 16 million Italians in 14 CE. The difference between the censuses of 14 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 per cent) and 1.25 million in the provinces (an increase of 62.5 per cent). This alone would be enough to raise the Italian total to 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 and 164 CE as it did between 14 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 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 per cent), compared to 1.8 million in the provinces (at a rate of 90 per cent). In fact, unless we are prepared to believe that a much larger share of all citizens resided outside Italy in the period of Augustus than is commonly surmised, there is no realistic scenario that would produce a final Italian population below 18 million. Within 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 the reproductive capacity of the Italian population.<sup>31</sup> Moreover, possible

<sup>28</sup> Ibid. 208 list 15-16 million in 1 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% drop during the epidemic (ibid. 204).

<sup>29</sup> Scheidel 2005a: 66-71.

<sup>30</sup> Lo Cascio and Malanima 2005: 204.

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



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 onward.

In general, the ‘high count’ logically implies significant net natural growth in late Republican Italy; very substantial underregistration of citizens in the same period but vastly improved coverage later on (to the extent that underregistration 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 (albeit 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 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 alone does little 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 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

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<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: 444-453 (underregistration) and 461-482 (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.

<sup>37</sup> Kron 2005b: 485-486 (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: 120-121; Frier 2000: 811-816; 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: 442-443 conveniently gathers references to earlier secondary scholarship.

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 has come to be seen.<sup>41</sup> Saskia Hin has begun to argue that this position suffers from considerable logical inconsistencies, which will be set out in greater detail in forthcoming work.<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 at all, and some men at any age would be unfit for service), and of no obvious budgetary 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 that would not otherwise readily be 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. Thus, if we were 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 approximately 2.5 to the reported census figures, for totals of maybe 10 million Roman citizens in 28 BCE and 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: 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.

## 5. 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 per cent of Italy's population resided in cities (excluding Rome itself), or almost 40 per cent 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 pre-modern

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<sup>41</sup> Brunt 1987: 15-25.

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

<sup>43</sup> Liv. 3.3.9 and *Per.* 3; *Per.* 59.

<sup>44</sup> For 28 BCE, 11 million citizens overall (allowing for a 10 per cent undercount) might translate to some 11 million residents of Italy (if the number of overseas citizens roughly equaled that of slaves and aliens in Italy), and an eventual peak of perhaps around 13 to 15 million.

<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: 68-69 does not strictly speaking propose an urban percentage but argues for a non-agricultural population amounting to 32 per cent of the total or (approximately – this is not quite clear from the text) 18 per cent excluding Rome. For the relationship between the urban and the non-agricultural population, see below.

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 thus 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 to 18 per cent of the regional total, and for 38 to 43 per cent of the number of regional urban residents.<sup>48</sup> By comparison, in the late seventeenth century, London is thought to have comprised 70 per cent of the residents of all English cities with a population of 5,000 and over, and to have housed 9.5 per cent 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 explained as a function of its position as a pan-Mediterranean capital bloated by coerced transfers of food and other resources.

Secondly, 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 – in a sense – 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 of 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 insulated 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 numbers or to our understanding of urban hierarchies.<sup>50</sup> As an exceedingly rough guess (and applying the parameters of the 'low count' scenario), 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 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

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<sup>47</sup> E.g., Lo Cascio 1994a: 39; 1999a: 164-165; 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.

<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 catches the eye that while Lo Cascio and Malanima 2005: 222-223 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.

the 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 amount of 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> Thus, 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 inevitably 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’ model 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 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>

<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 per cent, compared to 20 per cent rural population growth: Wrigley 1987: 162.

<sup>55</sup> 1300 data from Malanima 1998: 110-116.

<sup>56</sup> Duncan-Jones 1982: 339, after Nissen; Bekker-Nielsen 1989: 21-22.

<sup>57</sup> For urbanism in Roman Egypt, see Tacoma 2006: 21-68.

<sup>58</sup> Rathbone 1990: 124-137.

<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 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

The aggregate urban population of 2,231,000 estimated for the 161 cities of 5,000+ residents in mainland Italy *circa* 1300 is roughly of the same size 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 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 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 per cent 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 per cent smaller 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 an 'high count' Italy of anywhere from 16 to 20 million (see above, section 4), an urbanization rate of 25 to 28 per cent would put some 4 to 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. Thus, if we took medieval urbanization rates to be the standard for Roman Italy as well, in the 'high count' scenario 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 aggregate urban population number in Roman Italy far exceeded Morley's estimate, this finding would not automatically translate to a much larger 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

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rounding practices, as for example when he (488-489) 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: 238-256; Scheidel 1996b; Duncan-Jones 1997.

<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 per cent. At that rate, an adjusted urban population of 1.8 million in Roman Italy would translate to a total population of 6.8 to 7.7 million (i.e., 6.3-7.2 million plus the 0.5 million in Rome who were supported by 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: 488-489.

have raised average ‘urbanization’ levels to historically very high levels of approximately 50 per cent.<sup>65</sup> It goes without saying that for the centers of many of these *poleis*, ‘urban’ is a questionable attribute. 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 pre-modern 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 *comuni* 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 the non-agricultural share of the total population, and one in which the majority of all inhabitants 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 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 which proportion of the population of Roman Italy outside the capital lived in nucleated settlements that legally enjoyed urban status: 10, 20, 30 per cent? None of this 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’ scenario, 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 of the exploitation of agrarian assets (which would have benefited from rural residence) and thereby limited population growth. Conversely, a ‘high count’ scenario 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

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<sup>65</sup> Hansen 2006: 24 (and 73-74 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: 98-99 notes that if agro-towns are considered ‘urban’, Sicily boasted an ‘urbanization’ rate of two-thirds at the beginning of the early nineteenth century. See also Malanima 1998: 102-103.

<sup>69</sup> Nothing in Garnsey 1998: 107-131 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.

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.

## 6. 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 per cent of the entire citizenry in 215 BCE, 11.8 per cent in 214 BCE, and 12.6 per cent 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 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 per cent in late 215 BCE, 9.4 per cent in 214 BCE, and 11.9 per cent in 212 BCE.

Similarly high rates re-appear in 83-81 BCE, at 8.3 per cent, and again in 43 BCE, at anywhere from 5.9 to 8.6 per cent.<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 per cent.

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' scenario requires us to accept that in a few years in the 210s BCE, the Roman state temporarily managed to mobilize between 50 and 75 per cent of all *iuniores* (male citizens aged 17 to 45) for the war effort, are meant to discredit the assumptions of the 'low count'.

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<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.

<sup>71</sup> Lo Cascio 2001: 122-137.

<sup>72</sup> Long-term rates are lower, around 4-5% of the citizenry, or approximately one-fifth of all *iuniores*, 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 1987: 418 table X.

<sup>75</sup> Lo Cascio 2001: 136.

<sup>76</sup> Lo Cascio 2001: 137.

There is no question that the recorded peak levels in particular represent a formidable challenge to any ‘low count’ scenarios. 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 by 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 per cent 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 per cent of the Prussian population served in the army, as did 7.7 per cent 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 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 preindustrial 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’ scenario, the highest mobilization rates occurred in the 210s BCE, on rare occasions approaching or even exceeding 10 per cent 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 had to 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) per cent of all adult men, compared to 50 per cent 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 percent for the ‘high count’, compared to 11.9 per cent for the ‘low count’. Thus, the ‘high count’ scenario 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 catches the eye that Rome and (on a much bigger scale) the Confederate States (unlike Prussia

<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.

<sup>80</sup> Lo Cascio 2001: 112-113,

<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.



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 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 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 share of Italians in all legionaries whose provenance is known dropped from 62 per cent in the period from 30 BCE to 41 CE to 37 per cent in the years from 41 to 68 CE, to 22 per cent in the years from 69 to 117 CE, and to 2 per cent 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 not more residents of Italy 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>

## 7. Labor markets

The significance of labor relations as an indicator of demographic conditions has not received any 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 merely meant to be indicative of 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.,

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<sup>83</sup> Military service of slaves was an additional benefit: cf. most recently Hunt 2006; Rosenstein forthcoming.

<sup>84</sup> Forni 1953: 159-212, supplemented by Forni 1974: 366-80. Cf. Scheidel 1996a: 95-96 n.18.

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

<sup>86</sup> Scheidel 2005a: 76. My figures supersede Brunt’s guess of a corresponding rise from 500,000 to 3 million (1987: 67, 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.

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 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 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’ scenario 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 that was 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 pre-modern economies to shame (see below, section 13).

The same logic applies to military labor: it catches the eye 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 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

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<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: 75-78, a net increase by 1 million slaves over 200 years required the importation of some 3 to 4 million slaves overall. Scholars have long reckoned both with a larger slave population and higher sex ratios, a combination of features that would necessitate even larger imports.

<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 that 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.

demographic stagnation (as envisioned by the ‘low count’) or even stronger economic growth that coincided 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.

## 8. 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 a scenario 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 re-interpretation 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’ scenario developed by Beloch and Brunt.<sup>97</sup> Luuk de Ligt, in recent work as well as in the present project, likewise argues that even the relatively moderate demographic growth associated with the ‘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 farm land. Strong segmentation of the labor market may go some way in 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

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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: 118-169; 2005; Turchin and Korotayev 2006; Turchin and Nefedov forthcoming.

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

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

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 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 in both ways, in support of the ‘low’ or the ‘high’ counts. It is therefore inconclusive with regard to the question of absolute population numbers.

## 9. 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 in at least parts of this period.<sup>102</sup> These trends match late Republican and early monarchical 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 of an expansion of 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.

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 pre-modern standards. This scenario 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 monarchical period, and has noted resultant vulnerability to later epidemic

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

<sup>100</sup> Jongman 2007: 613-614, 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.

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 scenario 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 of farming and husbandry that supposedly enabled the Romans to square the circle and contain Malthusian pressures.<sup>107</sup>

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 of 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 case like 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 this point in time, the available information about living standards is still insufficient to provide a reliable guide in identifying the most likely scenario: too much

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

<sup>105</sup> Lo Cascio and Malanima 2005: 214-224.

<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.

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

<sup>109</sup> Goldstone 2002.

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

remains unclear about the representative character and the precise chronology of the material evidence. What I have meant to show instead is that our reconstructions of Roman population number 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.

## 10. 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 are 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 that had been 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 people, or 35.7 to 119 people per km<sup>2</sup> of cultivable land. Witcher opts for an estimate of 326,000 people. 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 per 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 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 remained undiscovered, the farther upwards we would 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.

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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.

<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 (of 60,000) rather than the ‘informed estimate’ for Ostia (of 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 people per 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 people per km<sup>2</sup> of cultivable land.

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 persons, 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 persons, 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 people 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 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 than 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 per cent 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, of 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 overschematically – accounted for this entire shortfall by the existence 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 per cent 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

<sup>115</sup> Witcher 2005: 127, 129.

<sup>116</sup> Population: above, section 4. Italy: Jongman 1988: 67; cf. already Beloch 1886: 439-440, quoting Nissen.

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' scenario 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.

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' scenario.

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 per cent for the inner zone of the *suburbium* and of 50 per cent for the outer ring cannot carry a lot of weight:<sup>120</sup> although they might seem consistent with the ('low count') notion of strong nucleation discussed in section 5, they might 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: thus, 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

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<sup>117</sup> For the former, see e.g., Terrenato 2004: 39-40 figs. 4.1, 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' count scenarios. 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 also Witcher's chapter in this volume.

<sup>119</sup> Mattingly and Witcher 2004, esp. 181-183 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), 130 n.67. The same is true (e.g.) of the 42 per cent urbanization rate for the territory of Jerba in Fentress forthcoming.



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 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 numbers.

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.

## 11. Carrying capacity

Given the right mix of favorable guesses about cultivated area, grain yields, fallowing and crop rotation, Roman Italy would probably have 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 monarchy coincided with a climate optimum of relatively high temperatures that would have been conducive to farming and elevated

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<sup>121</sup> Sbonias 1999b: 223-5, esp. 225 figs. 16.6-7.

<sup>122</sup> See now esp. Patterson 2006: 6-7, 72-88.

<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 pre-modern 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.

<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.

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 number.

## 12. 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 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 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 packed as Italy, up to four-fifths of the total imperial population had to be 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

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<sup>129</sup> E.g., Greene 1986: 81-85; Schmidt and Gruhle 2003; Fagan 2004: 189-212; Koepke and Baten 2005b: 152 fig.3; Malanima and Lo Cascio 2005: 218-219; Sallares 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.

<sup>132</sup> Population in 1850: 10.6 million (Del Panta *et al.* 1996: 277); in 1881: 12.2 million (Belletini 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 only defended a moderately high count for Egypt (Lo Cascio 1999c), which is however far below the adjustment necessitated by an empire-wide ‘high count’ scenario: 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: 484-485; cf. already Frier 2000: 814; Scheidel 2001a: 242-248), 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

most serious challenges to the ‘high count’, if not indeed – as I am inclined to believe – its single most serious handicap.

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’ scenario 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 of the order of 10 to 11 million (as explained below), the fact remains that without Brunt’s indefensible *deus ex machina* device of additional millions of slaves, the ‘low count’ tally 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 to 25 per cent 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, of 12.5 million, “ci pare verosimile”.<sup>137</sup> In other words, the final total is the result of questionable estimates or outright guesses regarding urban population numbers 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 per cent 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

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Roman empire for the 1870s (2005b: 485-486), which is likewise not implausible and consistent with the ‘low count’: see already above, n.37, and below, n.151.

<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.

<sup>138</sup> It merits attention that Malanima’s studies treats 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 to 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).

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, once 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 alone, none of these figures are 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> Upon 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 in the peninsula in 1300, 1,244,000 million people lived in cities of 5,000+ inhabitants,<sup>145</sup> and that they accounted for 20 to 25 per cent of the overall population,<sup>146</sup> the total population of the peninsula 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 to 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 per cent of the overall total,<sup>147</sup> and hence indicative of a regional tally of some 6 to 7 million. The discrepancy between this number and a putative Roman population of 1.5 to 2.5 million that can be accommodated by the ‘low count’ is undeniably quite dramatic.

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<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 (Beloch 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 Pantà *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 Pantà *et al.* 1996: 275 for the early sixteenth century.

<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 Pantà *et al.* 1996: 277 assign 900,000 inhabitants to Sicily and Sardinia in 1300.

<sup>145</sup> Tallied up from Malanima 1998: 112-116.

<sup>146</sup> As assumed by Pinto 1996: 43.

<sup>147</sup> Malanima 1998: 110-112; Pinto 1996: 43.

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 might 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 per cent of that of the peninsular population, whereas comparative evidence from the early modern period consistently suggests a respective value of closer to 75 per cent.<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 of the debate. In view of this, it is all the more important that De Ligt’s contribution in this volume succeeds in making a strong case that Roman northern Italy may indeed have been as sparsely populated as predicted by the ‘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 very roughly resembled 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 do in any case 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

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<sup>148</sup> Kron 2005b: 461-482. 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.

<sup>152</sup> Frier 2000: 814. See also Scheidel 2001a: 242-248 for Egypt, and more generally McEvedy and Jones 1978: 139, 143, 151, 227. Cf. also Hansen 2006: 87-91 for Greece.

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 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 whilst simultaneously lowering 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 in 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, do in no way establish that such alternatives 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.

### 13. 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 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

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<sup>153</sup> See above, n.42.

<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 to 30 per cent. 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 Panta *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 to 40 per cent. To be sure, following Malanima's own method, the medieval peak for the mainland would have to be put at 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 to 50 per cent. 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 per cent. Even the lower end of this range would push total Roman population size 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: 127-128 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.

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>

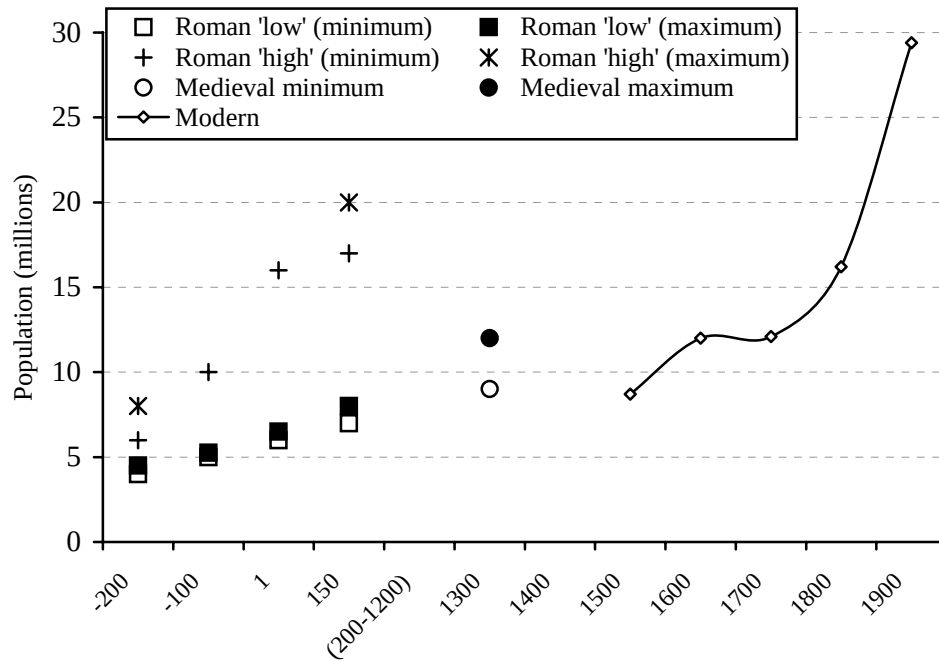


Fig. 2 Population estimates for mainland Italy, 200 BCE to 1900 CE  
Source: Sections 4 and 12, and n.154

<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 Pantà *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 Pantà *et al.* 1996: 277.

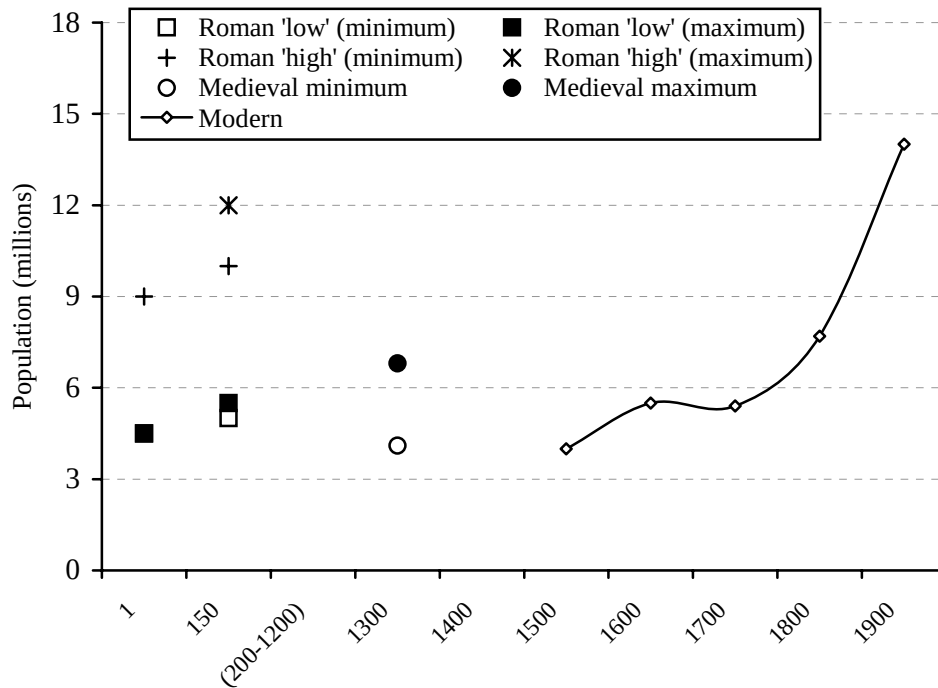


Fig. 3 Population estimates for peninsular Italy, 1 to 1900 CE  
Source: Sections 4 and 12, and n.154

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 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

<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.



largely insensitive to overall population density.<sup>158</sup> On the other hand, our understanding of the driving forces of 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 Michael Rostovtzeff's and Moses 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 pre-modern history in general.

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

<sup>160</sup> Elvin 1973: 113-199; Maddison 2001: 42.

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

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