3

Response to Elio Lo Cascio

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What do we mean by urbanization? If I have a number of qualms about the resourceful and wide-ranging assessment that Elio Lo Cascio offers of the degree of urbanization of the Roman imperial world, in considerable part they stem from discomfort with his answers, explicit and implicit, to the question of what counts as urban in our calculations of the percentage of population living in urban centres. I do not see any sign of consensus on this subject in the fraction of the relevant literature that I have read. A lack of clarity on this point is also at the root of the inconsistency with which Italy and Egypt are treated in this chapter. Lo Cascio's opening remarks treat his position as self-evident, not needing justification. That seems to me unwise.

Lo Cascio's view is that size matters, and little else does. In particular, he is not attached to juridical definitions of the status of settlements as a discriminant. One can understand why this seems a reasonable approach. Just because a settlement of 2,000 people is called a city does not mean that it belongs in the same category with Ephesos. And for cross-period comparisons a size cut-off seems plausible. But in fact official status has an impact on the nature of a settlement and its relationship to its surroundings. In particular, cities provided or were expected to provide a certain range of services, and these helped to structure that relationship. Small or large, the presence of major cult centres, organs of justice, and markets (to name only the three most obvious) makes a great difference in the degree to which a settlement employs its population in non-agricultural occupations

and can draw on the surplus of others' agricultural activity. If we are asking about urbanization not in the abstract but as a pointer to (e.g.) the percentage of the population not employed in agriculture, then official status matters. For this reason, I do not see an alternative to the painstaking counting-up of cities.

We would agree, however, that ancient Roman cities were not in the main (or should not be defined as) what Lo Cascio calls agro-towns—or, at least, that in speaking of urbanization we should exclude these from consideration. For Egypt, on which I shall concentrate my remarks, the non-agricultural character of the nome capitals is put beyond doubt by the Hermopolite land registers of the fourth century, where on almost any reasonable estimate of the populations of the city and the nome one can hardly argue that the landowners (with their families) amounted to more than about 15% of the population; and those with enough land to live solely on rents from it can hardly be more than half that. It is true that the register omits the *pagus* nearest to the city, which will have contained intensively cultivated garden and orchard land. But much of that no doubt belonged to the same wealthy people who owned the largest share of urban holdings in the rest of the Hermopolite, and one *pagus* out of seventeen cannot alone change the picture greatly. It is also true that some urban residents undoubtedly worked as hired cultivators of one sort or another in the surrounding countryside, on land owned by the wealthy or by women. Practically speaking, however, considerations of distance make it unlikely that this was more than a modest percentage of the population. On any reckoning, then, a large majority of Hermopolis's population must have been supported by something other than landowning or agricultural labour.

That gives us a fourth-century marker. It does not tell us of itself anything about change during the previous several centuries, and in fact nothing in Lo Cascio's chapter really gets at the dimension of growth. This is hardly surprising. Even for Egypt, our data about city populations, however imperfect, are also scattered across the centuries. If recent attempts to deduce a structure to the distribution of metropolitan populations have had difficulty with the limited quantity and quality of data,[1] still less have they been able to trace change,

[1] Tacoma 2006, with earlier bibliography cited by him.

as we tend to have at best one data point for any given city. Probably the best we can do is to compare the metropolis of the Arsinoite nome in the third century BC, under Ptolemaic rule, and at somewhere in the third to fourth century. The census records from the second half of the third century BC suggest a population for Crocodilopolis of only about 4,000; estimates for the imperial peak range from 27,000 to 46,000,[2] with Tacoma recently estimating '44,000 or less'.[3] Although we should not underestimate the urban character of even the small city of the Ptolemaic period,[4] most scholars of Graeco-Roman Egypt would agree, rightly or wrongly, that most of the growth came in the Roman period, with the advent of the class of small notables living on agricultural rents. Since Clarysse and Thompson have estimated the population of the Arsinoite nome at 85,000 to 90,000 at the same period at which Arsinoe itself was in the vicinity of 4,000,[5] the early Ptolemaic period would apparently have had an urbanization rate of only 5% or so in this district.

It is perhaps worth another look at the ratio of urban to rural population in Roman Egypt, taking into account the work published since *The Demography of Roman Egypt*.[6] I remain convinced that the concept of carrying capacity will get us nowhere. Egypt in Hellenistic and Roman times always produced a substantial surplus for export, whether through the sale abroad of a royal tax surplus, private shipments, imperial tax wheat sent to Rome for free distribution, or some combination of these at various points. We have figures for the tax wheat sent abroad, but we have no idea how much was sold at any given point; there is thus no way of estimating what was consumed in Egypt.

I would begin by the best figure we have, namely that for the number of Hermopolite households, which probably amounted to about 7,000. What multiplier to use for individuals is debatable, but it seems unlikely that a number per house is relevant; the number per household is more useful, and that for urban households we estimated at 5.3. That is where a population figure of 37,000 came from (a figure per hectare is only derivative, not the source of the

[2] Clarysse and Thompson 2006, vol. 2: 100. [3] Tacoma 2006.
[4] Bingen 2007: 114–21. [5] Clarysse and Thompson 2006, vol. 2: 95.
[6] Bagnall and Frier 1994, esp. 53–7.

calculation). It may of course be too high or too low, for all sorts of reasons we cannot know.

The Hermopolite nome amounted to about 4% of the arable land of Egypt; the percentage might be slightly higher, but probably not lower, as some recent writers have tended to more conservative rather than more expansive estimates of the surface under cultivation (see Chapter 7 for further discussion). If the number is approximately correct, it was about double the size of the average nome, on the assumption that there were about fifty nomes. We do not know if the relationship between the size of a nome and the population of its metropolis was relatively constant. Tacoma's analysis tends to support a fairly stable relationship, but it has to be said that our data are scanty enough that it would be hard to exclude absolutely the possibility of something closer to a power law, in which the metropolitan population relative to rural population was greater in the largest nomes. The villages do seem to exhibit a power law distribution, to judge particularly from the *vestis militaris* codex *P.Col.* IX 247 (also Hermopolite). The Hermopolite probably had something like 15 to 20 sizeable villages at the head of a number four to five times as large of smaller settlements. But there is no obvious reason to believe that the distribution of metropolis populations followed a similar pattern.

If we leave that wrinkle aside for the moment, the average metropolis would on Tacoma's reckoning have had a population smaller than the 25,000 average that Frier and I used in our analysis, perhaps more like half the estimated Hermopolite population, or 18,500. Fifty such cities would yield an urban population (not counting Alexandria) of 925,000; add a half-million for the capital, and the total is 1,425,000. It is unlikely that we can do much better than that with present data. But even that is better than we can do for the rural population, where different positions are staked out by various scholars without really much basis. If the cities represented a quarter of the non-Alexandrian population, the *chora* would have had a population of 3.7 million; if a fifth, 4.6 million. Of course, these are static numbers. Both may have been true at one point or another, if (as we suppose) there was slow population growth over the first two centuries of Roman rule, down to the Antonine plague. Either seems more plausible than the possibility of an urbanization rate lower than 15%, which a total population figure for Egypt of 8 million would imply.

On almost any reckoning, however, we remain faced with the conundrum that Lo Cascio poses: Why did Egypt have more centres with populations greater than (say) 20,000 than Italy, with also a high urbanization rate and a population probably on the same order of magnitude? One might add the question why Egypt also probably had more settlements in the 2,000 to 5,000 range than Italy did, as an extrapolation from the Hermopolite *vestis* register would seem to me to suggest. One could juxtapose two possible answers. One is that Egypt's administrative structure itself, with its nomes and toparchies, later *pagi*, provided a means of concentrating agricultural surpluses via rents, taxes, and provision of services. It did not leave entirely to the chances of historical development the attribution of land to cities. The other is that Egypt's agriculture was simply that much more productive than Italy's. I suspect that both of these are true, but it would take us very far afield to try to quantify either.

In concluding it is worth returning to the problem of change, which is largely lost in Lo Cascio's attempt to arrive at general levels of urbanization—not surprisingly, given how difficult it is to answer the question for any time, let alone for several. It is only the very long-term data for the Arsinoite nome that may give us some sense of the scale of change. Even on a fairly conservative assessment, urbanization in that region in the middle to late Roman period is unlikely to have been less than 15%, and if the 5% estimate for the third century BC is anywhere near the mark, then a tripling of the concentration of population in the city over a half-millennium seems plausible. If one believes, as I do, that the urban population was more nearly a quarter of the total, then a quintupling is likely. How much of that took place under the Ptolemies and how much under the Romans, it seems impossible to say. At all events, this is the sort of direction in which I would look for attempting to quantify change.

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