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## Response to Alan Bowman

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It is hard to be sure whether this survey of the bases of our quantitative knowledge of the agricultural economy of Roman Egypt should be regarded as optimistic or pessimistic. Bowman sides with those who, looking from outside, think that actually we have quite a lot of information; but his analysis of the available data for large-scale parameters like population and land on the whole emphasizes more the gaps in our knowledge and the squishiness of some of the numbers we frequently use than it stresses their utility. Those two points of view can of course be reconciled by falling back on micro-level analyses and case studies, as the latter part of the chapter seems to suggest. We might, for example, understand in fine detail the balance sheet and income statement of an individual without having any means of escaping microhistory in favour of the quantification of a village, nome, province, or empire. But it is hard to see that this would constitute a positive outcome for a project aiming at quantification.

In my view we do not need to surrender to a negative assessment of our prospects. The principal reason is that I do not see the goal as a set of numbers on paper but as an interactive model in which every user is free to choose different values for particular variables, and which is created with as plausible a set of values as we can deduce. This model then allows us to examine the consequences of changing the value of a particular variable. If we adopt a particular percentage of urbanization, and let that drive the calculations, do we arrive at unacceptable results somewhere else? *Some* values for *some* variables will definitely produce results capable of falsification, and at that

point the model will begin to become bounded in some dimensions. Relationships between variables are likely to be more controllable than some of the macroeconomic values, but that does not mean that the latter can be invented freely.

An important element in making such a model cogent is the process of inventorying our ignorance as assiduously as our sources. Production requires inputs. Bowman recognizes this in several instances, most importantly perhaps transportation. We can take this process into areas for which the papyri are lamentably lacking in information, of which the most salient is probably energy deriving from sources other than human and animal power. These gaps cannot be filled with evidence, at least not at present, but they can be modelled. How much energy did it take to produce an amphora or a cooking pot? How much labour was required to make a sack? How many new items of this sort did an average person need per year? We will not arrive at precise knowledge, but we can probably build an approximation that is much better than nothing. And the process will be strongly recursive: that fuel needed animals to get it to the kiln, and the animals had to eat, and their food had to be grown by someone. Each major element in the economy needs to be traced through in this fashion.

A big gap of a different sort from the unreliability of macroeconomic indicators is the absence of information from the Delta, as Bowman points out. This also is not to be despaired of. There is in fact a lot of information about agriculture in the carbonized papyri from Thmouis, which have been subjected to detailed tabulation and analysis in the recent dissertation of Katherine Blouin.<sup>1</sup> We can recover critical information like the percentage of land allocated to different crops and other uses, thus helping to provide a set of values for the range of land use to help us contextualize the figures we have from other nomes. They are not perfect, of course, but the amount of information is likely to be surprising to anyone raised on papyrological pessimism about this region. We need to try to sketch rough borders for the Delta nomes and calculate their sizes, a project with plenty of its own hazards but again capable of being carried out within certain margins of error.

<sup>1</sup> See Blouin 2007a; 2007b; forthcoming.

I would suggest that it is at the nome level that we have the best chance of building a first-round model, which might then be extrapolated to the entire province. Extrapolation will carry with it a significant decline in the level of confidence, naturally, but that does not mean that it will not be worthwhile. (If one accepts the estimate that the Hermopolite amounted to about 4% of all of Egypt, straight-line extrapolation is particularly easy in that case, cf. my response to Lo Cascio; no doubt wrong, too, but by how much it remains to be seen.) I tried in the article of 1992 cited by Bowman to offer the merest sketch of what such a model for the Hermopolite nome might look like. It could be deepened significantly with other information, most notably the *vestis militaris* codex (*P. Col.* IX 247) to which Bowman refers. With this list of villages for two-thirds of the nome together with tax assessments (in garments, but capable of being turned into cash with only modest approximateness) we can in effect build a model of the entire list of villages for the nome, with their relative assessments. That is already a significant step beyond the aggregate numbers I used. It can then be linked to the sixth-century tax codex from the Hermopolite village of Temseu Skordon, admittedly outside the boundaries of this project. That codex (*P.Lond.Copt.* 1075), which is in preparation for publication by James Keenan, Leslie MacCoull, and me, allows an estimate of the number of taxpaying landowners in this village. One could then extrapolate the number of landowners in each of the other villages in the fourth-century codex and the model derived from it. Temseu Skordon was responsible for 3.37% of the total nome taxes in that codex; since the codex preserves about  $\frac{2}{3}$  of the total for the nome, we may extrapolate from this that it was liable for about 2.25% of the total taxes of the nome at that date. We do not know if it may have changed in relative or absolute size between the fourth and sixth centuries, but in any event it should be an example of a relatively large village. By my reckoning there are 359 individual taxpayers in the Temseu Skordon codex.

The last link in building the first stage of the model would then be the number of persons in the population per landowner. The number of landowners in the 1992 model was derived by dividing the arable land supposed to have been owned by villagers by an average holding extrapolated from the Karanis data. That figure could be given much

more work from other sources; it might be too high. If Temseu Skordon had 2.25% of the Hermopolite cultivable land owned by villagers, using the estimate that Bowman quotes, it would have had about 4,050 arouras. That comes to about 11.3 arouras per landowner, or somewhat less than half of the figure that I used in the Hermopolite model. The figure would be much larger if one calculated based on total arouras rather than those owned by villagers. My point is not to push for a particular figure here, only to say that we have materials from which to build a model.

The larger point here is that attempting to quantify the economy of Roman Egypt is not best thought of as a process with an end point consisting of a set of answers. Rather, it is better conceived as an ongoing, iterative exercise in the building of an open model that can be used by many scholars experimenting with different values and incorporating new evidence as it becomes available. It will have various components, including an input-output table and a set of sub-tables for different activities linked to it. With all of its gaps, our evidence is certainly sufficient to allow the building of enough components of this model for the whole to be capable of excluding some answers and helping us define ranges for others. Agriculture is the largest part of this model, of course, but we will get wrong answers for many values if we do not build it into a model that asks what the range of the possible is for non-agricultural activities. We have far less data for most of those domains, of course, but much can be obtained from comparative evidence.

One final point about modern Egypt as a comparand may be worthwhile. The landscape of Egypt has changed greatly as a result of the two Aswan dams, barrages elsewhere, the introduction of perennial irrigation, and the reconfiguration of the canal system. Survey work today can never hope of itself to recover the landscape of antiquity. But maps and accounts from the nineteenth century and before, including the account of the Fayum by an-Nabulsi which James Keenan has done so much to bring to our attention, can help fill the gap. Particularly worthwhile, I think, would be attempts to do just what Bowman suggests, build up a picture of the area and nature of some of the nomes we know relatively well from documentation, starting at the toparchy level. The results can be compared with our scattered information for these districts in the documents. I would

guess that at least for the Hermopolite, Oxyrhynchite, Arsinoite, and Herakleopolite nomes some real progress might be made in this fashion, providing a test of the kind of model-building that I have suggested.

### Bibliography

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