

The bouletic merry-go-round

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LAURENS E. TACOMA, *FRAGILE HIERARCHIES. THE URBAN ELITES OF THIRD-CENTURY ROMAN EGYPT* (Mnemosyne Supplement CCLXXI; E. J. Brill, Leiden 2006). Pp. xiv + 353, figs. 6. ISBN 90 04 14831 0. EUR. 99.

When Septimius Severus gave the nome capitals of Egypt the privilege of having town councils, he provided them for the first time with a formal vehicle for the aspirations of a larger group of élite landowners than the handful who had held the civic magistracies of the 2nd c. This "bouletic class", as papyrologists tend to call it, occupies a large place in the documents of the 3rd and 4th c., particularly among the Oxyrhynchos papyri, where not until the A.D. 370s does the documentation seem to give out. Although the councils themselves as institutions were dealt with in A. K. Bowman's classic monograph,¹ the social history of their members has for the most part remained to be written. M. Drew-Bear's still-unpublished *thèse d'état* re-edited the Hermopolite council archive of the reign of Gallienus and discussed it in detail, and a number of her articles have offered important discussions of various subjects connected with it.²

L. E. Tacoma's new book sets out to fill a part of that gap.³ It is strongly focused on the theme of the title, the fragility that he argues was a principal characteristic of the bouletic élite: fragility in the sense of the inability of landowning families⁴ belonging to this élite to maintain their membership in it continuously, generation after generation. The book is a "slightly revised" version of a dissertation supervised by H. Versnel (the late K. Hopkins as outside examiner) defended at Leiden University in 2003. Except for a substantial appendix listing the members of the council of Oxyrhynchos, the changes from the dissertation are mainly cosmetic.

The strongest intellectual influence on the work appears to be that of Hopkins, particularly the affection for models; in fact, the book is an exercise in constructing such a model and exploring its implications. Its organization is straightforward. Part 1 (4 chapters) describes the élites as part of a highly urbanized society with strongly marked inequality of wealth. Part 2 (also 4 chapters) argues that the realities of demography and partible inheritance would have led to frequent failure of these propertied families to generate a member of the council in any particular generation or to remain for long part of this social stratum. There is also an introduction laying out the nature of the sources, the questions to be addressed, and the model to be employed, and a conclusion summarizing the results. There are 3 indices (places, sources, and subjects).

1 *The town councils of Roman Egypt* (Am. Stud. Pap. 11, Toronto 1971).

2 Remarkably, only one of these is cited in the bibliography of Tacoma's book, which for the most part ignores the Hermopolite documents in favor of the Oxyrhynchite. Among the most important are the following: "Le nome Hermopolite et sa métropole à l'époque gréco-romaine," *REA* 83 (1981) 21-33; "Les archives du conseil municipal d'Hermoupolis Magna," *Atti XVII Congresso Int. di Papirologia* (Naples 1984) 807-13; "Les athlètes d'Hermoupolis Magna et leur ville au 3e siècle," *Proc. XVIII Int. Congress of Papyrology II* (Athens 1988) 229-35; "Hermoupolis-la-Grande: une métropole d'Égypte d'après les papyrus grecs," *Cercle Lyonnais d'Égyptologie Victor Loret, Bulletin* 5 (1991) 33-40; "La culture grecque à Hermoupolis Magna," *Proc. XIXth Int. Congress of Papyrology II* (Cairo 1992) 195-204; and "Guerre civile et grands travaux à Hermoupolis Magna sous Gallien," *Akten des 21. Int. Papyrologenkongresses* (APF Beiheft 3.1, Stuttgart 1997) 237-43.

3 But only part, as Tacoma makes clear (13). Most of the life of the élite is left aside, and the physical record created by its competitive zeal for civic glory is not treated. He alludes only briefly (14) to urban archaeology.

4 After a long discussion (70-88), Tacoma rejects the possibility that the members of the élite derived much of their wealth from economic activities outside agriculture or para-agricultural enterprises related to their estates, a view he variously characterizes as "modified primitivist" or simply as a "middle ground". Given the centrality in the papyri of land records and taxation, this view seems to me insufficiently grounded. Activities like the weaving enterprise of the family of the *strategos* Apollonios are not taken sufficiently into account.

The main lines of the argument can be summarized succinctly. Egypt had a high level of urbanization, between 20 and 30% of the population living in urban centers.⁵ These formed a hierarchy (chart on 54-55), with an average size somewhere between 14,000 and 22,000 inhabitants and a handful of larger places (like Arsinoe and Hermopolis) perhaps twice that size. Only a small fraction of the population of these towns actually owned land and, within that fraction, ownership was highly stratified. Even the larger towns probably had a hard time finding more than a hundred adult male residents with more than 100 arouras of land, and much of the land was owned by a tiny subset of that élite.⁶ This 100+ arouras group was the main source of council members. Those with Alexandrian citizenship and office constitute a regional élite above these local ones.

Given the realities of ancient demography, which probably were much the same for élites as for other urbanites, the élites were vulnerable to failure to provide adult male heirs to a father's élite position. At the same time, partible inheritance, with a strong bias for equality of division, meant that too much success in reproduction would also cast a family out of the élite, unless it was part of the tiny group of families with very large landholdings. Families like that of Sarapion alias Apollonianos, with 5 known generations, are the exception rather than the rule.⁷ Even élite endogamy would not suffice to offset these facts. Elite continuity is thus improbable. The most likely source for replacements would be not veterans or freedmen, as sometimes thought, but rising landowners from the group just below the élite.

What is new in this argument?⁸ The major novelty, in Tacoma's own view, is the argument that cyclical mobility was necessary to replenish the ranks of the élite. The debt to Hopkins is obvious. But it is difficult to escape the feeling that we are encountering a straw man. Tacoma describes (156) what he takes to be a widespread view that Roman urban élites in general

dominated the local scene for generations, and monopolized the more honourable offices .. According to this theory social mobility was limited, and the occasional wealthy outsider had great difficulty in penetrating the upper ranks of the urban hierarchy.

No references for "this theory" are offered. Tacoma proceeds to say:

Although explicit statements are few, similar thoughts seem to exist with regard to the urban élites of Roman Egypt. The élite is taken to form a coterie of important families that managed to stay in power for generations on end.

Again, no citations. The same opponents are played in the concluding arguments (245):

Those who want to argue for widespread élite continuity have a lot to explain.

This page also has no footnotes. If everyone who has ever tried to reconstruct families in the bouletic class is taken to hold such views, then of course there are many such. But it would be fairer to say that the question simply has not occupied those who have written about the subject of the 3rd-c. élites.

Given the way in which Tacoma formulates his conclusion, one can hardly avoid discussing his concept of the élite (11):

The élites of the *metropoleis* formed a cohesive oligarchy, a ruling élite. For practical purposes, the élites are defined as the members of the councils and their families. This is a definition based on an institutional criterion. As we will see, the social and economic élite could be both larger and smaller.

5 Tacoma (11) calls the nome *metropoleis* "towns", reserving ("by convention") the term city for Alexandria and the Greek *poleis*. That does not stop him from using "urban" to refer to the *metropoleis*.

6 Tacoma does not try to decide if smaller *metropoleis* had smaller councils, a point on which there is no good evidence.

7 See the Leuven database of archives at http://lhpc.arts.kuleuven.ac.be/archives/sample_arch.php?id=210 for references and bibliography.

8 In the manner of dissertations, this one tends to exaggerate its own novelty. Although in general Tacoma scrupulously cites the scholarly literature underlying the various stages of his argument, he tends to begin sections by sounding as if he will have important new conclusions to offer, only to arrive at much the same positions he quotes from others, often by a slightly different route that contributes little new to the discussion. Many of these sections could have been shortened considerably by a more critical editor.

This definition has a practical side, in that élite individuals can be identified unambiguously by the presence of magistracies or council membership in their description in documents.⁹ But in fact to a considerable degree it excludes their families, because they can be identified only where the name and titles of the council member also appear, which would be more commonly true of a man's children than of his wife. In fact, Tacoma does not use the term "élite" as consistently as his definition would suggest, as we can see in the remark (14-15) that "the limited degree of literacy implies almost automatically that the papyrological universe is very much the world of the local élites". That statement is true only if one uses an entirely different definition of "élite" than the one laid out 4 pages earlier. The vast majority of the papyri do not come from the bouletic stratum of society.

Perhaps a more serious question arises from the notion of a model, which Tacoma discusses in the introduction (15-18). He never actually says what he thinks a model is, but embraces the view (16) that models should be testable with data rather than challengeable only with logic:

Ideally, models should be verifiable and testable, and a major way to achieve that is by using data derived from ancient sources.

Indeed, that testability is the main reason that this book uses evidence from Egypt (15). The chapters do a fair job of setting out the main parameters of the model. Only two of these really matter, however, in evaluating the conclusions reached. One is the demographic functions — mortality, marriage, fertility — as we see them in the converging figures from demographic model tables and from the census declarations.¹⁰ The other is the distribution of wealth, which in this model is mainly taken from the Hermopolite land-registers of the mid-4th c.¹¹ From these two quantitative measures one could calculate by simulation a plausible rate at which families would fail to produce a male heir or would divide their property sufficiently to fall below the 100-aroura threshold (which I agree is probably a fair cut-off to use). In Part II chapt. 4, Tacoma proceeds more or less as if he had done just that. But in fact he never actually quantifies the probability of such discontinuity in the élite; he simply asserts that, given these parameters, it must be substantial. Given his precise definition of the élite, this seems likely enough; but I find it hard to see that it represents the sort of model for which he argues in the introduction, and it does not really tell us whether such turnover would have amounted to 10%, 20%, or 30% per decade, for example. Even a rough estimate of such probabilities would be of great interest.

There is an important respect in which the approach taken here, using the Hermopolite registers, could give a misleading impression of the probability of turnover in the membership of the council of Hermopolis. It is a factor which Tacoma discusses at some length (140-50, cf. 115), but apparently without recognizing its implications. This is the overlap in council membership between Alexandria and the metropoleis. We have no real idea how large this overlap was, because the Hermopolite registers cover only the residents of one quarter of Hermo-

9 Tacoma repeatedly calls these descriptions "self-identification". That is only sometimes the case. Much of the time it is a matter of documents addressed to these individuals, and we do not know how the writers of such texts decided what information to include. In the case of public documents, there was probably a rule. Because titles instead of patronymics were usually given, our ability to identify family connections between *bouletai* is fatally compromised. Tacoma recognizes the problem (235) but does not draw the full conclusion that seems to me necessary.

10 This chapter is based principally on R. S. Bagnall and B. W. Frier, *The demography of Roman Egypt* (Cambridge 1994; 2nd edn. with addenda 2006), supplemented by debate since the appearance of that book, particularly several contributions of W. Scheidel. Tacoma's remark at 168 n.18 suggests that he does not know what "statistical significance" means.

11 This section is mainly based on the discussions by A. K. Bowman in *JRS* 75 (1985) 137-63 and by myself in *JRS* 82 (1992) 128-49. Despite many pages devoted to other bodies of evidence for inequality, it is the Hermopolite registers that furnish Tacoma's parameters. Little of the evidence (and none from a *metropolis*) actually comes from the 3rd c.; any potential problems arising from this discrepancy with the stated subject of the book are left aside. There are many problems in the discussion that cannot be dealt with here, but one error (93 n.64) must be rectified: the problem in models of property ownership posed by the part of the population with zero property is dealt with in my article on 138-39, cf. also *Egypt in late antiquity* 71; however, the point (like so much in his book) is not directly relevant to his model.

polis, excluding Alexandrian residents (along with villagers and those of other *metropoleis*). Tacoma points out this problem (97) in discussing the property distribution curve.¹² These Alexandrians in general will have been considerably richer than the lower levels of the Hermopolite councillors, and thus presumably less vulnerable to the threat to continuous council membership posed by partible inheritance.

Tacoma is well aware that "it is intellectually unsatisfactory to assume that members of the élite remained mere passive victims of the contingencies of the demographic regime and the rules of partible inheritance. The capacity of such families to remain in power should not be underestimated" (232). But he does not actually spend much energy or space on strategies that these families as actors might have employed. He discusses (195-96, 200-1) the possibility that adoption might have been used to fill gaps in succession caused by the lack of a male heir, but he sees little sign of it and dismisses the idea. It is certainly true that the direct evidence for adoption in the papyri is scanty, and not only for the élite. But it must be recognized that Greek naming practices in the papyri do not favor the disclosure of adopted status, because an adopted son would give only the name of his adopted father as his patronymic. Not even census declarations indicate adoption; only the occasional direct reference or adoption agreement does. Thus there is every possibility that the papyri contain numerous adopted individuals whom we cannot identify. This is indeed argued in a forthcoming article by S. Hübner.¹³ It would not, of course, solve the problems created by partible inheritance, but it is a powerful tool for bringing more distant family members into an agnatic line and thus preserving the household and its public rôles across generations. It is also possible that marriage strategies could have played a rôle. Tacoma does not value that contribution highly either (241), but once again the evidence is slight because we rarely have recorded lineages beyond the generation of an individual's grandfather.

This book takes on a good subject. Its arguments certainly would have to give some pause to anyone who thought that family continuity in the *boule* was to be taken for granted. But it is not news to historians working with papyri that social mobility in Roman Egypt was significant, and one would like greater precision in the results of the analysis. More time and pages spent on a detailed simulation of turnover in a council, and fewer pages devoted to repeating, with variations of little import, the results of earlier scholarship would have made this a more compelling book. It is much to be hoped that in the future Tacoma will pursue the question with greater quantitative rigor and at the same time broaden his scope to say more on towns other than Oxyrhynchos and on questions about the towns in the 3rd c. besides those relating to the specific issue of the rate of turnover in the councils.¹⁴

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- 12 Criticizing me in n.74 for the fact that "this problem is ignored in the nome model". He does not say how one would include in the nome model something for which there is not a shred of evidence. The village ownership structure built into the nome model was based on village evidence from elsewhere, mainly Karanis, but there is no counterpart to allow even a guess at Alexandrian holdings.
- 13 The article in question argues that the phenomenon of apparent brother-sister marriage (discussed by Tacoma at 183-84 and 189-93) involves adopted rather than biological siblings. Although Tacoma periodically refers to problems of evidence in the papyri, he does not always seem to understand their consequences. Brother-sister marriage is a good example; although he notices that only one census declaration involves a member of the bouletic class, and almost all of our evidence for brother-sister marriage comes from census declarations, he does not seem to be aware that as a result one can say nothing at all about the phenomenon among the council élite. The remark (86) in relation to élite non-agricultural investment, "the only correct interpretation of the silence of the sources must be that the silence is real: there was no such involvement", is symptomatic of his failure to understand the importance of the gaps in the papyrological documentation.
- 14 One question not taken up in the chapter on "elite formation" (115-52) is precisely the formation of the councils when they were created. The bibliography does not cite the brilliant and suggestive article of P. van Minnen, "A change of names in Roman Egypt after AD 202? A note on P.Amst. I 72," *ZPE* 62 (1986) 67-92, which I discuss in *Reading papyri, writing ancient history* (London 1995) 36-37.