



Review

Reviewed Work(s): *Alexandrie II: Inscriptions des factions à Alexandrie* (Centre d'archéologie méditerranéenne de l'Académie polonaise des sciences...dans la république arabe d'Égypte au Caire) by Zbigniew Borkowski

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BOOK REVIEWS

Zbigniew Borkowski, *Alexandrie II: Inscriptions des factions à Alexandrie* (Centre d'archéologie méditerranéenne de l'Académie polonaise des sciences...dans la république arabe d'Égypte au Caire), Warsaw 1981: 147 pages, 27 plates.

The ostensible purpose of this volume is to publish a small corpus of some 30 acclamations scratched on various parts of an early Byzantine theatre at Alexandria. In fact most of the long introduction and the two appendixes is devoted to elucidating what Borkowski argues to be the background to these acclamations, namely Heraclius' revolt against the unpopular regime of Phocas. Borkowski dates the beginning of the revolt to 608 and skilfully analyzes its antecedents, character, motives and development. His first four chapters represent a major contribution to the study of Heraclius' accession and the last stages of Byzantine rule in Egypt. They are certainly the fullest and best-documented account now available.¹

This review is mainly devoted to two basic points. First (Part I, by Bagnall), was Borkowski right to date the revolt to 608 rather than 609? Second (Part II, by Cameron), is this the right background? For the main flaw in Borkowski's learned and acute study is that he comes nowhere near proving that his inscriptions date from the period of Heraclius' revolt in the first place.

I

Rejecting the date of 609 in the *Chronicon Paschale* for the outbreak of revolt at Carthage, Borkowski nonetheless accepts the chronicler's synchronism between the revolt in Carthage and that in Alexandria, dating both instead to 608. The basis for the dating to 608 is twofold: Cedrenus' dating of the revolt to Phocas' sixth regnal year (27.xi.607-26.xi.608; Cedrenus' authority on such a detail is virtually worthless); and the attribution to the revolt by Grierson of a series of solidi bearing indiction numbers (as Grierson argued) of 11, 12 and 13 (respectively 607/8, 608/9, and

¹ Cf. also the recent article of G. Rösch, "Der Aufstand der Herakleioi gegen Phokas (608-610) im Spiegel numismatischen Quellen," *JÖB* 28 (1979) 51-62.

609/10).² The bulk of the issues of solidi are assigned to Carthage, but some (including all three years) to Alexandria. If the triple attribution (occasion, date and place) is correct, a very strong case can be made for 607/8 as the date of the revolt in both places.³

Three points about the relationship between Carthage and Alexandria should also be noted: (1) Phocas also minted in Carthage in indiction 11; therefore the revolt started some time after the beginning of the indiction (Borkowski 24); (2) the chroniclers indicate that the revolt prevented the African grain fleet from being sent to Constantinople, but that the Alexandrian one was dispatched in that year (Borkowski 23-24 with nn. 5 and 6); (3) Phocas did not mint any gold in Alexandria.

There is no great novelty in the above. But Borkowski goes beyond it to make some original observations about the Egyptian papyri for the period. These are, however, not wholly consistent between the historical summary and the Appendix (I) in which a detailed discussion is given, nor entirely correct. On page 24 we read "l'Égypte était aux mains des rebelles en été 608....D'autres spéculations sur une date plus exacte sont pour le moment stériles." A footnote tells us "On manque p. ex. jusqu'ici de papyrus de l'an 608, cf. infra, *Appendice I*, p. 134, note 29." In the note cited, however, one finds only the reference to the consular list of *CSBE*.⁴ There is only one reference there, *SB XII* 10798, and both Borkowski and Worp and Bagnall have independently dated this text now to 609.⁵ But that list includes only *consular* dates, which are very rare in the seventh century (the major point of this note of Borkowski's); it does not mean that there are no papyri dated by other means. In fact, there are three or four published texts with regnal dates--all mentioning Phocas. These are:

2 Borkowski 23-25; see Appendix II, pp. 137-43, on the coins.

3 Theophanes also gives year 6, as Borkowski points out (p. 23), but in the context of assigning Phocas seven years, which is wrong. Phocas' penultimate year was 608/9, which would agree with the Paschal Chronicle. Borkowski places little confidence--correctly, we think--in the choice of one chronicler's dates over those of another.

4 R. S. Bagnall and K. A. Worp, *Chronological Systems of Byzantine Egypt* (Stud. Amst. 8, Zutphen 1978) Appendix D.

5 Borkowski 135; *BASP* 17 (1980) 111.

ZPE 31 (1978) 130-33	Arsinoite	9.ii
SB I 5266	Arsinoite	2.viii
CPR IV 23	Panopolite	29.viii ⁶
SB I 4505	Thinite	11.x (or 606?) ⁷

Even leaving SB 4505 aside, it is clear that regnal dates by Phocas were normal in the Arsinoite as late as early August, and in the Panopolite as late as the end of that month. The year is in fact comparatively well documented.

Borkowski's argument (pp. 134-36) is based on the important discovery of a pattern in the curious cluster of papyri which have a dating formula of the type ὑπαρχίας τοῦ αὐτοῦ δεσπότου in texts where no emperor has been mentioned by name. Two published texts of 609, both probably from Oxyrhynchos, have this peculiarity, PSI I 61 (8.v) (regnal rather than consular date) and SB XII 10798 (11.vi). In the next year, Borkowski points out, even this shred of imperial titulature vanishes and one published text (SB I 5270, 27.ii, Arsinoite) and four unpublished ones (all Oxyrhynchite) ranging from 25.vi to 23.x.610, omit all imperial dating formula (p. 134 with n. 27). Borkowski concludes, "l'explication la plus simple qui vient à l'esprit est que l'omission complète de l'*intitulatio* ne pût se faire que quand la chute de Phocas et la victoire de son antagoniste purent être considérées comme sûres (ou aussi, ce qui change peu, quand les autorités locales le considérant furent mises en place). La formule comportant une *intitulatio* incomplète devrait donc être un signe d'incertitude et de chaos que dut provoquer la guerre civile en Égypte. Il faut considérer une telle forme du prescrit comme de compromis, ou plutôt de louvoisement. Les deux remontent à une période quand les armées rebelles occupaient le pays et il était peu raisonnable d'utiliser le nom de celui contre qui la révolte était dirigée" (p. 135).⁸

All of this seems superficially to make very good sense. But Borkowski does not mention that an Arsinoite text of 29.v.609, BGU III 837, gives Phocas' titles in full. What are we to make of this fact?

First, Borkowski is surely right in reading *some* significance, political or otherwise, into these variations in regnal formula.

⁶ See ZPE 31 (1978) 135 n. 3.

⁷ See CSBE 66.

⁸ Borkowski elucidates similar behavior in the documents from the time of Phocas' displacement of Mauricius.

The total omission of a regnal formula from all of the documents of 610 is decisive. (Heraclius does not appear in a regnal formula until early 611, and his dating observed the death of Phocas as the starting-point.⁹) Secondly, it must be admitted that we should take the evidence from 608 seriously, namely that Phocas was still recognized in the areas from which we have evidence. Thirdly, the halfway phenomenon of omitting the imperial name is limited on our evidence to Oxyrhynchos.

This is true not only of the two examples from 609, but also of those from the end of Mauricius' reign. Borkowski points to five texts from 598 to 602 which omit Mauricius' name, as against one which includes it.¹⁰ But this is only the Oxyrhynchite documentation. We have also, all with Mauricius' names:¹¹

Memphite	<i>BGU</i> I 255	15.v.599
Arsinoite	<i>BGU</i> II 399	23.iii.599
	<i>SB</i> I 4858	2.vi.600 ¹²
	<i>P. Lond.</i> I 113 (5c)	8.viii.600
	<i>BGU</i> II 395	7.ix.600
	<i>SB</i> I 5317	600-601
	<i>BGU</i> I 309	20.iii.602
	<i>P. Grenf.</i> II 88	20.xii.602 ¹³
Antaiopolite	<i>P. Vatic. Aphrod.</i> 1	23.iv.599 or 598
Hermopolite	<i>P. Lond.</i> V 1899	18.vii.600
	<i>SB</i> VI 9586	12.xii.600
	<i>P. Grenf.</i> II 87	23.v.602
	<i>P. Lond.</i> III 1009	25.vii.602 ¹⁴
	<i>P. Ross. Georg.</i> V 42	602
Herakleopolite	<i>P. Köln</i> III 158	16.x.599
Panopolite	<i>P. Paris</i> 21 ter	13.vii.599
	<i>P. Paris</i> 20	14.vii.599 (or 600)

In all, 17 texts from 6 nomes.

9 *P. Lond.* V 1736 (Syene) is 25.ii.611; *P. Oxy.* XXIV 2420 is ii-iii.611. *P. Oxy.* I 138 can be dated only to the range of 5.x.610 and 29.viii.611 and thus could be somewhat earlier.

10 *PSI* III 239, *PSI* III 179 (but see p. 136, n. 33), *P. Oxy.* XVI 1991, and two unpublished texts, vs. one unpublished text of 7.iii.601.

11 We include only papyri from 11.xi.598 (the earliest without Mauricius' name) to the end of his reign.

12 See *ZPE* 46 (1982) 244-45.

13 See *RFBE* 60.

14 See *RFBE* 61.

The situation is thus parallel. Since Phocas did not launch a rebellion against Mauricius by capturing Egypt, what explanation in terms of rebellion and loss of imperial control can be offered? At any event, the Oxyrhynchite documents stand alone in this situation as in 609, and they caution us not to generalize to all of Egypt too hastily from the Oxyrhynchite omission of imperial names.

Still more curious is *P. Laur.* III 91, not known to Borkowski (it was published in 1979), in which the same type of consular formula "of our same pious master" is found with a Trinitarian invocation in a year 4, indiction 10, Choiak 4, or 30.xi.606. This occurrence, right in the middle of Phocas' reign, further undermines an interpretation which invokes a revolt. *P. Rainer Cent.* 125.21 gives ἡ αὐτῆ ὑπατία Μεσορή κς (νδ(ικτ(ονος) θ', which surely must be 19.viii.605. As this is not a contract, its lack of an invocation is not surprising, though its Arsinoite provenance is.

What we have, then, is a phenomenon limited to Oxyrhynchos (on available evidence) and continuous for the period 598-610: of the Oxyrhynchite texts with the pertinent kind of dating formula, only one mentions an emperor's name (Mauricius', in the event). No other place behaves in this manner. Can we find an explanation? Short of an unlikely find of the right sort of correspondence, any answer must of course be speculative. Could the Apion clan be in some way responsible? The bulk of our Oxyrhynchite documentation of this period depends on that Great House for its existence, and the notaries involved presumably took their orders from the Great House. Moreover, the family is not attested to have held any important official positions in these reigns, a fact which has suggested a hostile relationship to the emperors.¹⁵ The confessional stance of the clan under these Chalcedonian emperors is somewhat unclear: Apion III appears favorably in the correspondence of Gregory the Great as late as 603; but a Strategios was instrumental in the reconciliation of the Egyptian and Syrian monophysite churches in 616. His exact place in the family is a matter of uncertainty, but there is no reason to think he was not a member of

¹⁵ J. Gascou, *Les grands domaines, la cité, et l'État en Égypte byzantine (5e, 6e et 7e siècles)*, forthcoming, Appendice I; I am grateful to the author for the opportunity to read this in manuscript. Our information about high offices in this period is, however, lacunose: cf. E. Stein, *Studien zur Geschichte des byzantinischen Reiches* (Stuttgart 1919) 186.

it. To attempt a more detailed explanation of the situation in the Oxyrhynchite documents would be useless in the present state of our evidence; but it is difficult to imagine that the notaries would have acted as they did without the approval of the Great House.¹⁶

II

That most (if not all) the inscriptions were inscribed within a fairly short space of time is clear enough; for no fewer than seven of the charioteers acclaimed are named in more than one inscription, two of them in four. But Borkowski goes further: "le chercheur actuel est frappé par l'impression que toutes ces inscriptions furent exécutées en un court espace de temps et que personne n'y ajouta rien....Elles semblent provenir des derniers jours avant un déluge" (p. 75). This is pure fancy. Why should anyone have added anything to such modest graffiti? The fact that some are apparently unfinished (nos. 16-19) proves nothing. There is another unfinished factional acclamation at Didyma (*I. Didyma* no. 611). It is hardly surprising that such trivial, routine graffiti should be abandoned at the slightest distraction. And how could we possibly tell whether or not the other inscriptions and drawings Borkowski publishes from this same theatre are later?

Next he remarks that: "Le catalogue de Christophilopoulou, avec ses propositions de datation, montre que parmi 27 acclamations, 24 sont datées soit au règne de Phocas et le début de celui d'Heraclius, soit attribués a cette période par les philologues et historiens" (p. 77). It would be hard to imagine a more misleadingly formulated statistic--especially from so generally responsible a scholar. The unwary reader might be impressed by the implication that there were grounds for dating 89% of extant factional inscriptions to the first decade of the seventh century. After turning back in surprise to Christophilopoulou's list (*Char. ... A. K. Orlandos* II [1964] 350-58), I established the true statistics to be as follows. Of the 27 inscriptions included, only 5 are explicitly dated to the reigns of Phocas or Heraclius. Of these 5, two are a balancing pair on the same pillar in Oxyrhynchos and so really only count as one; the other 3 are all graffiti in the same theatre in Ephesus. Five more may be assigned to this period conjecturally, but it must at once be added that four of

¹⁶ I am indebted to K. A. Worp for several helpful discussions of the papyrological evidence.

them come from that same theatre at Ephesus. Two (from Constantinople) are quite certainly of later date, and the remaining 15 carry absolutely no indication of date: they could as easily be late fifth as early seventh century. If we add in the 11 new inscriptions included in my more recent list (*Circus Factions* [1976] 314-17, described on the preceding page of Borkowski as superseding Christophilopoulou), that makes 26 completely undatable inscriptions--or 31, if we also include five more unpublished acclamations (four to the Greens, one to the Blues) from the theatre and stadium of Aphrodisias kindly shown to me by Charlotte Roueché.

So the true figures are: (not counting Borkowski's new inscriptions) 43 factional graffiti, of which 5 definitely and 5 more possibly date from the decade or so after 602, for what it is worth at most 23%. Of these 10, 7 come from that one theatre in Ephesus. And even they are demonstrably not all contemporary. For example, H. Grégoire, *Recueil des inscriptions grecques-chrétiennes d'Asie mineure* (1922) nos. 113 *bis* and *ter* link the name of Phocas with the Blues, and someone subsequently erased the names, presumably a Green after Phocas' fall and death (5 October 610). But there is no way of knowing how long after, for Grégoire no. 114 is an acclamation to Heraclius and his son Heraclius Constantine, who was not proclaimed Augustus (still an infant) till 2 January 613. Here someone has added 'and the Greens' after the two emperors' names, at earliest more than two years and perhaps many years after Phocas' death. The same would apply to Grégoire 114 *bis*, *ter* and *quinqüies*, linking the Greens with "Christian Emperors" unnamed--but unmistakably plural. If they belong to this period at all, they are later than 613.

Borkowski's third argument is the phrase at the end of his no. 8 (p. 82), ἀπὸ κορυφῆς ὡς ὀνόχων ἔπεσεν τὸ βένετο, "The Blues are fallen from top to toe." He takes this quite literally, as a reference to the triumph of the Greens, with the victory of Heraclius and his lieutenant Nicetas, over Phocas and the Blues. This is a possibility--if we were sure on other grounds that the graffiti could be dated to within a year or so of 610. But it is a pretty slim basis for establishing that date in the first place. Borkowski does not even consider the obvious interpretation of the words, Green exultation at a decisive victory in the hippodrome. Sports fans and writers have never been conspicuous for the moderation of their language: words like crush, rout, demolish, slaughter and annihilate are used routinely to describe

any clear sporting victory. And whereas today this sort of aggression is (mainly) verbal, there is abundant evidence for its regular extension into real battles in ancient arenas, with stones and knives rather than fists. There must have been thousands of occasions in the century and a half between Anastasius and Heraclius when the Greens could have exulted in the (temporary) destruction of the Blues "from top to toe" whether inside or outside hippodrome or theatre.

There is also another objection. The only reason we can date the factional inscriptions we can to the reigns of Phocas and Heraclius is precisely because they *do* link the colours to the name of one or the other emperor. This seems to be a new development of the period, almost certainly a direct reflection of the involvement of the Blues and Greens in the struggle between Phocas and Heraclius. This is why, alongside graffiti explicitly linking Greens with the Heraclii and Blues with Phocas, and especially in the light of the erasure of Blue/Phocas graffiti, we may feel some confidence in attributing to the reign of Heraclius and Heraclius Constantine the other Ephesian inscriptions linking the Greens to "Christian Emperors" unnamed. If Borkowski's no. 8 had been found in this context at Ephesus, his interpretation might then have seemed plausible enough. But what is the context in the theatre of Alexandria?

Of fourteen graffiti described by Borkowski as 'factional' (I am omitting his nos. 15-19, of which too little survives to be sure), no fewer than ten acclaim charioteers without naming either colour. In three cases the point is underlined by elaborate accompanying drawings of charioteers, in and out of chariots. Of the four that do name the colours, two link them with the names of charioteers (8, 10), one is just an acclamation to the 'Young Greens' (9), and the last is too fragmentary to read (14). In no case is a colour linked to an emperor, named or unnamed, and in no case has any attempt been made to erase the names of either colour (only the name of the Green charioteer Kalotychos, in no. 3). Nor are the Greens styled 'orthodox', as on several other inscriptions, but simply (in no. 8) as 'noble' or 'splendid' (γεννεώτατοι).

Rather than Borkowski's impression of "les derniers jours avant un déluge" (p. 75), I am left with a distinct picture of sports fans whose main preoccupation was the heroes and battles of the hippodrome. In this context, there is nothing to suggest that the destruction of the Blues "from top to toe" is anything but a

hippodrome defeat. If so, then the inscriptions might be as early as the fifth century (unless there are independent reasons so far undisclosed for dating the theatre itself later than this).

Borkowski's publication of the inscriptions and drawings (all of which are illustrated) is meticulous. Every point of interest is fully elucidated. But there is one curious omission. The only way the texts are numbered is by the original inventory numbers, although the texts themselves are not presented in this sequence. This means that (e.g.) "inv. no. 39" is to be found between nos. 33 and 24. Yet numbers corresponding to the sequence in which the texts are presented are used in the indexes on pp. 144-46; for ease of reference, most readers will want to add them in the margin.

It is strange (but put beyond doubt by the drawings) that factional graffiti in a theatre should celebrate charioteers rather than pantomime dancers, the heroes of the theatre. On the extension of the former circus colours to theatrical games, see my *Circus Factions*, Ch. VIII. In Rome at least we know from the contorniates that pantomimes were just as popular as charioteers.

In his Ch. V, Borkowski takes issue with some of the views expressed in my *Circus Factions*, in particular my interpretation of the role played by the factions in the revolt of Heraclius. While in general agreeing with my approach he argues that I went too far in minimizing the political nature of their intervention in the struggle. On reflection, with some qualifications, I am inclined to agree with him. Since I propose to publish elsewhere before long a modified version of my overall interpretation of the factions, I will not raise the wider issues here. But I would like to repeat that we cannot argue back from the troubled times of Phocas to earlier centuries. These strange bodies we misname circus factions were constantly developing and changing.

I close on two small but perhaps significant details. In my book I stated categorically that the young men (as they are often described) who alternately chanted at the games and fought in the streets were the only real Blues and Greens there ever were. But one of the new inscriptions is an acclamation to the νεοὶ πράσινοι (no. 9, p. 86), and Borkowski rightly compares the 250 armed νεώτεροι πράσινοι mentioned in connection with the Nika revolt of 532 (*Chron. Pasch.* p. 625.12; *Theoph.* p. 185.6). The implication is that these "Young Greens" were a subgroup *within* the Greens. He advances the same argument in connection with the series of still formally unpublished place inscriptions for ἀράριοι (= claqueurs, cf. *Circus Factions*, p. 248) from the theatre of Miletus that I

quoted with the kind permission of Peter Hermann. My argument was that the τόπος ἀραρίων βενέτων dated from "a transitional stage when there were still claqueurs other than Blue and Green." But one might again infer that the claqueurs were a subgroup within the Blues. This might seem "l'hypothèse le plus probable" (Borkowski, p. 66) for this inscription taken by itself, but what of the signs τόπος ἐπινικίων ἀραρίων, τόπος φιλαγούστων ἀραρίων and the simple τόπος ἀραρίων that mark off the neighbouring blocks of seats? And Charlotte Roueché refers me to an unpublished τόπος ἀραρίων in the stadium and an acclamation in honour of a πρωταυράριος (? claque leader: cf. *Circus Factions*, p. 234) in the theatre of Aphrodisias. Here we do seem to find an assortment of different claques that were surely in due course absorbed into the all-consuming Blues and Greens.

Roueché's forthcoming book on Byzantine Aphrodisias promises a discussion both of the factional acclamations and a number of other important new acclamations found at Aphrodisias. There must be other groups of factional (and other) graffiti on the theatre seats (and elsewhere) in late antique eastern cities awaiting decipherment and publication.¹⁷ It is to be hoped that Borkowski's book will stimulate the search.

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17 It is interesting to note that there are no Blue and Green acclamations or τόπος inscriptions in theatres destroyed before the fifth century reorganization of public entertainments that introduced the former exclusively circus colors into the theatre (for this reorganization, my *Circus Factions*, Ch. IX); for example, at Stobi, where there are plenty of other τόπος inscriptions. I owe this information to Elizabeth Gebhard, who will shortly be publishing the Stobi theatre. G. W. Bowersock and C. P. Jones draw my attention to a nice example of "sporting exaggeration" on an unpublished graffito from the Odeum of Aphrodisias, where a pantomime is described as "melting the marble" (τήξας τὸ μάρμαρον).