

# A kinder, gentler Roman army?

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RICHARD ALSTON, *SOLDIER AND SOCIETY IN ROMAN EGYPT: A SOCIAL HISTORY* (Routledge, London and New York, 1995). viii + 263 pp. ISBN 0-415-12270-8. \$59.95.

Alston's book is the first general treatment of the Roman army in Egypt since Jean Lesquier's massive (586 pages) *L'armée romaine d'Égypte d'Auguste à Dioclétien* (Mém. IFAO 41, 1918), which covered the same chronological ground. Despite its notes, bibliography, and appendixes, Alston's work is an essay, not a manual or reference work; it is in no sense intended as a replacement for Lesquier. Its value comes from its consistent argumentation of a particular view of how the army should be studied and of what its rôle in society was. In a world where doctoral theses<sup>1</sup> are often reference works rather than essays, a change is not unwelcome.

Chapter 1 sets out the author's main assumptions and views about the two elements implicit in the title: the Roman army and Roman Egypt. On the first, he rejects any notion that the Roman army was the first modern professional force, with firm, regular structures and conscious strategic planning, an army which can be validly analyzed from a modernist viewpoint. Equally he denies that the Roman army properly constitutes a separate field of study from the rest of ancient history; he thus aligns himself explicitly against views quoted from M. P. Speidel, E. Birley, E. Luttwak, and others, and with the approaches of F. Millar, B. Isaac, and J. C. Mann. The army was, he thinks, quite unlike modern armies (at least in western democratic states) in being deeply embedded in society as a whole rather than deliberately kept uninvolved in civilian affairs. It is thus properly studied as part of ancient society. It is curious in all this discussion to miss any mention of the work (listed in the bibliography) that will surely occur to any ancient historian reading even the title of Alston's book, R. MacMullen's *Soldier and civilian in the later Roman empire* (1963). Even reading MacMullen's brief preface will suggest that Alston exaggerates the novelty of his approach, and since MacMullen (declining to organize history "up to Diocletian") covers the 3rd c., one might even expect some overlap.

The portion of the introduction dealing with Roman Egypt focuses on two questions. The first is the well-known difficulties of working with papyri: their analytic, often isolated and uncontextualized character. The second is the applicability of Egyptian evidence to the Roman empire as whole; not surprisingly, Alston supports a view that has in the last decade or so become a consensus among those who work on Roman Egypt, that Egypt was not in most respects any more peculiar than other provinces. In the case of the army, this view is reinforced by the consideration that the army will have differed less than almost any institution from province to province.

In chapt. 2 we are given a general treatment of the size, development, and placement of the Roman garrison of Egypt. The supporting Appendix 1 lists units and the references to them in the documents.<sup>2</sup> Egypt was relatively easily defended, being accessible by only a few routes by

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1 This is a revised version of Alston's thesis, London 1990. Although some material as late as mid-1994 appears in the bibliography, it appears that 1992 was the effective cut-off.

2 The 25 pages of this appendix on the auxiliaries will be a valuable reference tool, even if it does properly belong to that accumulative, positivistic, institutional tradition that Alston is uncomfortable with. (The non-Egyptian evidence, unfortunately, is not always clearly set out so that one can tell which shows the unit abroad, which not.) The list of units appearing in the *Notitia Dignitatum* (Appendix 1, 190-91), however, is unfortunately not to be used without checking the entries in Seeck's edition. Despite the remark on 191 that "the spelling of stations has been normalised only in cases in which the station meant is clear", there is no consistent relationship between the reading of the *Notitia*, Seeck's emendations in his apparatus from other sources, and what Alston prints. Nor is there any consistency in the rendering of the oblique cases used in the *Notitia*, some of which are turned into the

land (from Gaza, down the Nile from Nubia) and hardly at all by sea. Its internal transport system — the Nile — was the best of the ancient world. (The sketch map on 15 is inadequate, an inconsistent hodge-podge of ancient and modern names [Cairo — not Babylon—, Asyut, Edfu, Philoteras, Quseir, Myos Hormos, and so on], and not even an adequate support for the physical description of Egypt provided.<sup>3</sup>) After the earliest days, the Augustan garrison amounted to two legions and a dozen auxiliary units, a total on Alston's estimate of c.22,000 troops (Tables on 24-26 show the auxiliaries by unit and by period, but not by place, for which one must consult the appendix). The garrison seems to have shrunk in size, with two main reductions, each of a legion, one under Augustus, the other in the 2nd c. Alston thinks these were not compensated with more auxiliaries, but of course it must be recognized that we do not in most cases have secure evidence for the sizes of the auxiliary units. The total size after the early 2nd c. was probably in the range of 11,000-16,000.

Its disposition around the province is hard to describe in detail, although Alston makes a valiant effort. (Here as usual, however, he cites late-antique archaeological material but not post-Diocletianic documents, an inconsistency hard to understand.) The soldiers of individual units were usually dispersed to many locations (but not crossing the Upper/Lower Egyptian divide, it seems). In fact only at Alexandria was there a real massing. This exception was the result of politics, not strategy: Alexandria was the second city of the empire and seat of the prefect. The broader pattern had nothing to do with strategic thinking (the existence of which Alston denies); rather, soldiers were the basis of the civil authority of Roman government. The position, developed in detail later, is essentially that of B. Isaac in his treatment of the Near East, although Alston does not mention Isaac in this connection.<sup>4</sup> It seems to me essentially correct, although in fairness it must be pointed out that the strategic position in Roman Egypt was normally one of controlling communications routes, not defending against invasion, and the positioning of the army is consonant with that. Moreover, any invasion from Palestine, the most likely source, probably would not happen without warning and thus time to deploy the central reserves.

Next comes a discussion of the two ends of the military career, recruitment and veteran status (chapt. 3). Alston describes what he takes to be a commonplace of modern literature on the Roman army, which "emphasize[s] how the army gradually came to look to veteran communities for recruits so that many of the legionaries gave not a city or province as an *origo* but a camp, the *origo castris*." Thus the army is supposed to have become a distinct, self-replenishing caste, with recruitment increasingly from the provinces where units were serving, and where men retired after their service. The camps thus became military islands. Alston argues that the Egyptian evidence shows that legionaries were still being recruited outside the province throughout the first two centuries. Tables 3.1 and 3.2 (42-43) show the numbers. But it must be pointed out that these are based on just a few documents, and these may not be entirely

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nominative correctly, some put into the nominative incorrectly (Thaubastos for Thaubastis, for example), some left as they are (Precteos for Prectis [Πρήκτις]), and others mangled (e.g., Foenicon turned into Poenicon). The Ala I Quadorum is not merely in the Oasis but at Trimithis (in the Dakhleh Oasis, actually). Ala I Abasgorum is listed twice (as in the *Notitia*, XXXI 41 and 55), once as Ibeon, the other time as Large Oasis. It was at Hibis (the genitive Hibeos appears in XXXI 41), the capital of the Great Oasis.

- 3 Which perpetuates (17) the ghost-division of the Arsinoite, the 'Themistes', laid to rest (one thought) by J. Bingen in *ChrÉg* 62 (1987) 234-39.
- 4 B. Isaac, *The limits of empire: the Roman army in the east* (Oxford 1990, 1992<sup>2</sup>). The omission is characteristic; although Alston's bibliography includes many works offering parallel information for other provinces, he almost never cites them in connection with the discussions of these points in his chapters. Even the Vindolanda tablets (*Tab. Vindol.* II is one of the few works of 1994 to make it into the bibliography) are hardly referred to except in the conclusion, although *Tab. Vindol.* II 154 is exceptionally illuminating for the pattern of dispersal of soldiers from individual units.

representative. The 1st-c. evidence tells only about Leg. XXII Deiotariana. Alston admits the force of these doubts on 44 but argues them away. In any case, the legions made up a diminishing part of the Egyptian garrison, and there is far less evidence that significant parts of the auxiliary force were recruited outside Egypt. The names of soldiers in Egypt, as they turn up in papyri and ostraka, are in fact heavily Egyptian throughout the Roman and Byzantine periods. In any case this may be the wrong argument altogether if one wishes to attack the view quoted above, for the geographical origin of soldiers tells us nothing about their social origins: soldiers could perfectly well be recruited only from military camps even if they came from outside Egypt; and Egyptians need not come from a previously military environment.

The rate of recruitment comes next. Alston tries to figure how many were needed each year, using various types of demographic evidence, including computations from tombstones, which he recognizes are useless for the purpose. Good and bad demographic "data" (20-year survival percentages for adult males) are thrown together in Table 3.4 (p. 45) with no analysis. "It is notable how close these figures are", Alston says. Indeed they are, for those that do not fit have been discarded (quite rightly), and the rest are all either model life tables or heavily dependent on use of such tables. The model table figures for ages 15-40 and 20-45, invoked as separate witnesses, do not differ greatly because annual mortality does not change a lot between 15 and 20 (about a 10% drop in life expectancy). The models are not "pessimistic" (46), merely (one hopes) realistic, and it is certainly not true that at any higher a mortality rate the population could not have reproduced itself.<sup>5</sup> Battle mortality and discharge for disability are ignored.

The limited documentary evidence concerning the rate of recruitment to the legions shows fluctuations, which Alston takes to show that the legion "was forced to rely upon rapidly raised levies in time of crisis," a finding that he thinks "erodes the image of well-prepared professionalism which many historians [not identified] have propagated." Crisis recruiting then led to cyclical swings between large and small annual cohorts; this "suggests an irregularity about Roman practice." This conclusion surely goes beyond what the evidence can reasonably support. Larger annual intakes might be dictated by any number of events: military losses and epidemics come to mind. For example, Alston sees 173 as a year of heavy recruiting, and that could reflect the plague under Marcus.

The Romans made no apparent attempt to settle veterans in Egypt in colonies or in any kind of clusters. Karanis seems to have a large number of veterans, really quite disproportionate, for which Alston can offer no explanation, except that it was probably just the result of some chance individual decisions, leading to a cycle of supplying recruits and serving as a place of post-retirement settlement. This might seem to satisfy the requirements for the kind of inbred camp culture of the kind Alston argues against, except that soldiers and veterans never made up a dominant part of Karanis' population (see below on chapt. 7). It is worth considering (a) that Karanis may in fact have supplied a disproportionate number of soldiers, (b) that our evidence is unusually good for Karanis compared to that for most other villages, and (c) that Karanis was relatively more Hellenic in cultural make-up than most Fayum villages, a characteristic which may have been attractive to retirees and at the same time may have made recruitment easier.<sup>6</sup>

We turn next (chapt. 4) to soldiers' and veterans' privileges and disabilities, led by the overarching question whether soldiers were a privileged group. That is how they are portrayed by literary sources: highly privileged and out of control. Alston sees this picture as the

5 Indeed, one of the conclusions of Bagnall and Frier, *The demography of Roman Egypt* (Cambridge 1994) is the existence of a much larger reproductive potential than was actually used.

6 Note that on 49 Iulius Voltinus is an error for Voltimus; the text cited is reprinted as *M. Chr.* 90, where it is argued that the provenance is Alexandria.

product of élite literary sources hostile to the military because it competed with the senatorial aristocracy for power in the imperial system. The analysis leads us into thickets of detail. Marriage, for example, was for two centuries forbidden to soldiers, a disability leading to some harsh legal implications for property and penalties for dodgers. With wills, by contrast, soldiers enjoyed exemptions from some of the technical requirements imposed by law, a privilege Alston sees as a reasonable result of their frequent lack of access to legal specialists at times when wills needed to be made. Soldiers' entitlement to *restitutio in integrum* (because away on imperial service), the prohibition against new acquisition of land in the province of service, exemption from liturgies and summonses, and interference with *patria potestas* are all canvassed in turn. The underlying argument is that individual explanations of the specific provisions are insufficient; they must be seen as a package of measures aimed by Augustus at isolating and professionalizing the army after its turbulent development in the late Republic and the civil wars. If this is right, of course, one can hardly fail to note that Augustus's measures persisted long after their *raison d'être*; the provisions concerning wills and *restitutio* in particular are hard to defend in these terms once the army becomes mainly stationary. But slowness to change what has been established is not surprising in Roman law.

If soldiers' status is less privileged than often thought, that of veterans was in fact higher than that of most people around them, but in Egypt (and presumably in most other provinces) this was true mainly in the sense that they were Roman citizens; after 130 some Egyptian veterans were also Antinoites, with special rights granted by Hadrian at the city's foundation. It is not easy to separate out the effects of the various statuses when they were held concurrently. Effectively, Alston thinks, veterans differed from citizens at large mainly in being *honestiores*, with their immunities from beatings, beasts, mines, and *munera sordida*. Other liturgies emerge as the main area of contention, with the main guideline being that veterans were liable to patrimonial liturgies but not personal ones. All of their privileges, however, had to be constantly defended against the local authorities; we see them highlighted mainly in such conflict. Alston thinks that, if anything, the distinctive advantage of being a veteran declined over time rather than increased.

As for soldiers, however, Alston argues that the increased fixity of units, the prevalence of local recruitment, and the development of local ties undermined the distinctive status of military men in real life. The unpopular soldier-bully did occur, but against the will of the government. Soldiers were thus not a powerful élite in Egypt, nor were they outsiders. One difficulty with this view is that it is so radically at odds with the picture of the Roman military that we find in the one province from which we have non-Roman literary sources, Palestine (particularly the New Testament and the Mishnah). It may be possible to argue that the two provinces differed so much in character that the hostile portrait in Palestine is easily explicable, but one certainly cannot dismiss these sources as so much senatorial hostility to the military. Even if we grant the accuracy of Alston's analysis for Egypt (which is problematic<sup>7</sup>), how do we know which province was the exception? Here, perhaps more than anywhere else, Alston's desire to stick to Egypt throughout the detailed analysis makes it hard to claim the universal applicability for his results that his introduction and conclusions seek to establish.

Chapter 5 turns from the soldiers to the army itself, asking what its normal functions were.<sup>8</sup> Military campaigns, although occasional, were tolerably extensive: sporadic action on the southern frontier, trouble in the 3rd c. with desert tribes (ending, he thinks, with Diocletian; but the problems returned later); Corbulo's campaigns of 58-63; three Jewish revolts; the annex-

<sup>7</sup> See now A.J.B. Sirks, *Tyche* 10 (1995) 179-84.

<sup>8</sup> He briefly returns to the theme of friction with the civilian population, as it appears in literary sources, and to the "rather pessimistic" view of the relationship of soldiers and civilians that he thinks is dominant (see n.1 for MacMullen as the source of this train of thought). Although admitting that there was some tension, Alston sees this as a product of extreme rather than ordinary conditions.

ation of Arabia; campaigns in the east against Parthians and Persians on at least seven occasions; Zenobia. Egyptian units were variously involved in these wars, and the Leg. XXII Deiotariana disappears after 119. The suppression of internal revolts also produced considerable activity, the most important being the Jewish revolt under Trajan.<sup>9</sup> The troops generally fared poorly, especially in the *chora*, until externally reinforced, when faced with large revolts. Alston attributes this outcome to the relatively low technological edge of ancient troops over ordinary people, making it impossible for a small number of soldiers to defeat large numbers of civilians. The *Boukoloi*, like the Jews, defeated the army until reinforcements came from Syria. The conclusion is that most soldiers are likely to have seen some wartime service during their tour of duty, but probably relatively few died from it, and most spent a rather low percentage of their total time in service at it. This is of course hardly an unusual state of affairs.

We turn to more peaceable activities. After Augustus' first clearance of canals, there is no further sign of military involvement in Egypt's irrigation system; this was rather a civilian *corvée*. Construction of forts, roads, bridges, camps, and monuments is better attested, although not with much indication of who actually did what, and Alston is sceptical that this occurred very often for any one unit; on what basis, I cannot tell. There is more considerable evidence for soldiers as support and enforcement personnel for tax collection.

The main non-combat rôle of the army was policing. Alston argues that the military policing system extended far beyond the areas and routes for which we have evidence.<sup>10</sup> The literary sources certainly stress the prevalence of banditry in Egypt, and they receive some support from the documents (above all from the Thmouis documents for the 160s). The scale and severity, however, are hard to determine from the documents; Alston supposes that most of the country was secure most of the time. The district centurions (and other officers, too, although they get little discussion) were the key peace officials; Table 5.1 (88-90) has 56 references to them. Alston notes that petitioners to these officers tend to cite their own relatively high status, something he oddly finds "a rather undemocratic principle" (92), seemingly surprised that status should matter in a Roman province.

Unfortunately, centurions found in a policing rôle are never known from any other military document, and their unit is almost never given. It is thus difficult to make a connection between what we know of them in their police rôles and what we know of the army. Alston thinks that centurions were probably at least as high as *nomen strategoi* in social status, and he stresses the rôle of centurions as a major link between the central government and the deployment of power in the countryside: "linchpin of the Roman government of Egypt" (95). Important though the centurions were, all of this seems to me a considerable exaggeration.<sup>11</sup>

In sum, the military forces in Egypt were not large for the size of the province, and they were geared to ordinary rather than crisis situations: crises tended to overwhelm them. The security system was the most normal regular duty of these forces. Here again, Alston returns to his theme that soldiers were not physically separated from civilian society, and he cannot believe in the existence of an "atmosphere of mutual distrust or hatred," because "the deep

9 Little of the important work on this subject is cited; see J. Mélèze Modrzejewski, *The Jews of Egypt* (tr. K. Cornman) (Philadelphia 1995) 254-55 for the essential bibliography.

10 There is a discussion of the ostraka from the Eastern Desert and the Theban region on 81-82; failure to consult the *BL* (8.518) is noticeable in his reference to the son of Balaneos in *O.Florida* 2 (p.81).

11 On 93 Alston suggests that ἐπὶ τῶν τόπων, applied to a centurion, was "probably a Greek rendering of the title *centurio regionarius*." Perhaps in some sense it was the equivalent, but it would be a mistake to see this Greek phrase as having been created to translate a Latin word, for the Greek has a substantial history in Ptolemaic administrative language. *P.Enteux*. 27.9 refers to τοῖς ἐπὶ τῶν τόπων στρατηγοῖς, "aux stratèges de la région," as O. Guéraud (consistently referred to by Alston as "Guérard" throughout the book) translates it. Similar uses can be found in, e.g., *P.Dion*. 9.17 (*strategos*) and 35 (ξενικῶν πράκτωρ) and *P.Ath*. 5.13-14 (*chrematistai*).

involvement of the soldiery in civilian society and the level of integration demonstrated by the soldiery demand an absence of real hostility between the groups" (101).

In chapt. 6 we turn to the economy, with an agenda framed by two visions of the army found in the scholarly literature: (1) the army as the agent of economic change, by redistribution of resources, stimulation of trade and currency circulation; (2) the army as a drain on the economy of the empire, especially in late antiquity. Neither generalization finds much support in the Egyptian evidence deployed here.

The first and largest part of the chapter, however, is devoted to an attempt to describe the economic status of soldiers and veterans as individuals, to see if they could have, in a private capacity, had any significant impact on the economy. Soldiers' pay for the most part seems to have left a reasonable margin for savings — fortunately for them, as Alston argues that the Augustan retirement bonus disappeared very early.<sup>12</sup> Probably savings were the main source of retirement property. The diminution in value of pay produced by gradual inflation was in part perhaps offset by the *annona*, but this is not clear. Nor is the impact of changing methods of charging soldiers for their food. Overall, it is not clear that Diocletian's army was any better off economically than its predecessors, especially as a result of inflation from the middle of the 3rd c. on.<sup>13</sup>

On 110-12, Alston assesses the impact of the supply system on the provincial economy, finding it "remarkably difficult to distinguish any trends in the supply of the army" (112). The discussion is vague at times, and surely mistaken in thinking that the supplies "appear to have been organised by unit with an officer from the unit approaching local officials to obtain the necessary goods" (110). It appears, for example, that some of the supplies for the garrison at Philae came from the area of Thebes, more than 200 km distant.<sup>14</sup> Alston wrestles with the question of how far the army paid for its requisitions. After noting a payment for leather, he offers a counter-example: "yet, the shafts of spears obtained from Soknopaïou Nesos in the third century, presumably items of some value, do not appear to have been paid for" (111). He cites *SPP XXII* 92, with no indication that there are any problems with the text. In fact it received a full re-edition in 1986 by R. Daniel in *P.Freib.* IV, pp. 61-62 (see the preliminary remarks in *BASP* 16 [1979] 44-45), changing the text dramatically. To quote Daniel's translation (slightly modified), "I have received from you the javelins of palm wood imposed upon you, for which I paid to you the fixed price from public funds."<sup>15</sup> From being a counter-instance to

12 This may be correct, but the argument is based mainly on the fact that one veteran's family in Karanis did not have the landholdings that Alston thinks could have been bought with such a bonus. Now that argument is in itself dubious; we do not have a complete roster of the family assets ("... can only be shown to have had assets ..."). But the mean land-prices used in the argument are also bogus, for they are derived in a circular argument. One could just as well argue (using the same data in Drexhage that Alston uses) that 5 ar. of olive grove should have been worth about 8,000 dr. (omitting one outlier from the averaging), the remaining 2.75 ar. of other types of land another 1,000 dr. or so, and a house 2,000 dr., thus adding up to the retirement bonus of 12,000 dr. I must emphasize that I do not claim either that this is a correct assessment of this particular family situation (the evidence will not support such an argument) or that this veteran had received a bonus; but Alston's argument here simply has no substance. Just as odd is the claim (226 n. 18) that "we know little about the basics of Egyptian agriculture".

13 Throughout the book, Alston takes for granted a traditional view of inflation in the 3rd c. as having been continuous and severe. It is now clear that in fact it was much later, less continuous, and less truly inflationary than usually assumed: see D. Rathbone, "Monetisation, not price-inflation, in third-century Egypt," in C. E. King and D. G. Wigg [edd.], *Coin finds and coin use in the Roman world* (Studien zur Fundmünzen der Antike 10, Berlin 1996) 321-39.

14 Cf. *O.Leid.* 349.2n.

15 I render δημοσίᾳ "from public funds" rather than Daniel's "publicly", which he took to mean through the public bank. The use of δημοσίᾳ to mean at public expense is cited by LSJ (s.v. δημόσιος, V.1) as far

reimbursement, in other words, the text has come into conformity with the general model of official payment for requisitions.<sup>16</sup>

Overall, then, the argument is that soldiers were not numerous or rich enough to have had a big overall impact on a province of this size and wealth; nor was the cost of the garrison material against the total revenues of Egypt (which were not mainly used for this purpose). The spatial dispersal of the army meant that any positive economic impact was also very diffuse and probably unimportant, with the only large concentration of troops being near Alexandria, where its potential impact would have been least. Alston notes that the two halves of the empire may well have been quite different, and the implications of the Egyptian situation may be limited to the East. Overall, veterans were well off, but well off by the standards of the villages, not of the metropolitan élite.

The village of Karanis, in the Fayum, has already made a number of appearances in the argument so far. Chapter 7 is devoted to a more detailed study of the village, where a considerable number of veterans are attested, in order to use it as a test case for the place of veterans in society.<sup>17</sup> Alston thinks that it was a relatively poor village, not very socially stratified, and lacking in much of a non-agricultural economic base. It is hard to tell if Karanis differed greatly from most villages in stratification, and there is no evidence that it differed in the extent of non-agricultural activity. It is therefore difficult to see why it should be considered poorer than others. This entire discussion of land is confused. For example, Alston quotes H. Geremek as showing "that in the second century few landholders [at Karanis] had more than 10 *arourai* and the largest attested holding was 15 *arourai*" (123). He compares this unfavorably to average holdings of 13-14.5 *arouras* of grain land at Philadelphia in the early 3rd c. But the holdings at Karanis that Geremek was discussing are of garden and orchard land, not of grain land;<sup>18</sup> we have no basis for estimating the size of individual holdings of grain land at Karanis in this period. Garden land was worth several times what grain land was, and an olive grove of 10 *arouras* was a valuable holding. The remarks on the 4th c. are equally confused: "The grain land under cultivation at Karanis had declined by this period to about 4,200 *arourai* of which about 2,210 *arourai* was private land, the rest being state owned." On the contrary, it was all privately owned, despite the survival of the old terminology categorizing some of it as βασιλική.

Alston estimates that about 14% of the population in the poll-tax account is Roman, not all of them veterans (although perhaps most originally related to veterans). Table 7.1 (124-25) gives a list of known soldiers and veterans who can be clearly identified. Of the 71, only 19 are identified by their unit, attesting 11 different units; if all had given a unit, nearly every unit in the garrison of Egypt would probably have been attested.

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back as Herodotos, and it appears also in Preisigke, *Wörterbuch*.

- 16 Both of Daniel's discussions of the text are cited in *BL* 8 (1992) 483. As to the shafts being of value, it was noted by M. P. Speidel (cited by Daniel, *P.Freib.* IV, p. 61, n. to ll.8-9) that palm wood was probably too light for standard shafts and perhaps was used for practice or parade weapons.
- 17 This discussion is preceded by an irrelevant opening about reaching Karanis by bus from Cairo (not the usual ancient route) and a general description of Karanis and its excavation history. Alston does not seem to know that the Michigan excavations which he describes were provoked in part by a flood of papyri reaching the market in the early 1920s, like the archive of Aurelius Isidoros. For the chronology of the occupation of Karanis, see now P. van Minnen, "Deserted villages: two late antique town sites in Egypt," *BASP* 32 (1995) 41-56. Alston's date for the abandonment of Karanis (119: c.500) is, to judge from van Minnen's discussion, probably some decades too late.
- 18 H. Geremek, *Karanis: communauté rurale de l'Égypte romaine au IIe-IIIe siècle de notre ère* (Wrocław 1969) 54: "le document ... nous donne des renseignements assez détaillés, mais seulement sur les jardins."

The chapter then proceeds to case studies of individual families.<sup>19</sup> The conclusions<sup>20</sup> are that soldiers and veterans were tied into the military scene very broadly, not just in a few units; their careers were heavily affected by personal connections. Despite the need to have fellow Roman citizens around for some legal transactions, their social circles were not simply Roman. Indeed, although these people were Romans, there was little specifically Roman about them. "The settlement of a large number of troops in an Egyptian village community may have led, therefore, not to Romanization but to Hellenization of the local culture" (138). The veterans fell towards the wealthier end of village society, but did not possess vast amounts of land. This distribution towards the top could reflect the rewards of service, but it could also reflect inherited wealth and the social milieu from which soldiers came in the first place. Clearly the material from Karanis votes for the hypothesis of full integration, except that military men tried to use their status for advantage wherever possible. There is no sense of ethnic division or conflict, and no evidence that these Romans had any significant cultural impact on the village.

The choice of Diocletian as a dividing-point is conventional, Alston says, but it reflects a whole host of changes that define late antiquity as a period with its own characteristics, political (more absolute monarchy, division of *imperium*), military (increasing importance of central campaign troops), cultural (Christianization), economic (concentration of wealth, larger estates<sup>21</sup>), and so on. So the division is, he thinks, justifiable. But it is nonetheless useful to take a summary look (chapt. 8) at the transformations that occurred in the army after this point. The roots of change lie in a series of military crises affecting Egypt: the Palmyrene takeover, southern tribal difficulties, the revolt of Domitius Domitianus, Diocletian's expedition, and the siege of Alexandria. The shape of revised military provisions is visible in the *Notitia*, which Alston dates in the early to mid 4th c.<sup>22</sup> The number of units had grown greatly, and 41 of the 70 were "cavalry". There were many more bases, not the old desert stations but mainly located in the inhabited land. This period is the archaeological context of much of the surviving remains, but they are not necessarily witnesses to more than a large expansion of fortifications<sup>23</sup> — certainly not evidence for a change in the army's rôle, either in explicit purpose or in social functioning. The military was formed largely from conscription,

19 "The papyri have not, in general, been preserved in archives ..." (126). Reality is much more complicated. A very high percentage of published papyri do come from archives (in the broad sense of the word), more in the Ptolemaic period than in the Roman. At Karanis, because there were formal excavations, this is less true than in some other contexts. There are many other questionable assertions in the course of these case studies that I cannot go into here.

20 Moderately ambitious, given the inconclusiveness of most of the specific family discussions.

21 Alston claims (146) that I have shown (*JRS* 82 [1991] 128-49) "that there was an increasing level of inequality in Egyptian society from the second to the fourth century." What I in fact argued was that differences in measures of inequality had to do mainly with the category of landowner being measured and the nature of the evidence, not with development over time.

22 He offers no evidence for this date, and no citation of arguments published elsewhere. This is not the date usually accepted, and indeed is strictly impossible, as the list in its present form is later than the creation of the province of Arcadia in 383. It is, of course, quite possible that some of the information is much older than that and was even obsolete by the time the text we now have was put into final form. It would have been useful to know Alston's views on the matter.

23 The evidence is set out in Appendix 2. Despite its title ("The archaeology of the army"), this is about military architecture, and no coverage of other aspects of archaeology is provided. A number of plans are printed (no sources for these given), and much bibliography in the notes. The material is in fact heavily late-antique. There are odd bibliographical gaps: there is nothing post-1989 on Abu Sha'ar or on Quseir (he could have known S. E. Sidebotham, "University of Delaware Archaeological Project at 'Abu Sha'ar: the 1990 season," *NARCE* 153 (Mar. 1991) 1-6, for example; he could then have given a correct date for its construction), or on other Eastern Desert sites.



and had rather a low status; perhaps this has to do with increased civilian-military hostility visible in the Abinnaeus archive.

The effectiveness of the Egyptian army is hard to judge, Alston says; it did maintain control of Egypt for three more centuries until the Arab conquest (he seems to have forgotten the Persian conquest). The dispersal of troops argues for their continued use to control the Egyptian population. The main change of late antiquity seems to be in "increased formality in the manner of the army's dispersal". That is, the official units now tend to coincide with the actual dispersed detachments, as against the earlier imperial tendency to create detachments with men from several units. The identification of units with particular areas "encouraged integration of units into local communities".

The conclusion (chapt. 9) is devoted to the general applicability of the results to the empire as a whole. One area allowing detailed comparison is policing, thanks to the Vindolanda tablets, which can be compared to the ostraka from the Eastern Desert, showing the pattern of dispersed staffing. The Dura papyri, too, confirm this picture, as do epigraphic sources elsewhere. It is harder to compare social standing; and the economic position of soldiers is not likely to be much different elsewhere. Probably the overall social standing was much the same in other provinces.

The great benefit of the papyri, Alston argues, lies in the ability to see the interaction of the garrison with the population in daily life, uniquely in the entire empire. Soldiers were of marginally higher than routine socio-economic status and in continual contact with civilians. The papyri thus refute both the view of soldiers as an isolated caste, the terror of civilians, and that of them as a privileged class. Personal contacts and the vertical organization of society, with patronage, made soldiers the agents of the ruling élite. The political and administrative rôle of the army, as the local agents for Roman power in the *chora*, was one of its principal functions.

Much in the views that Alston sets forth seems to me eminently sensible, although he is too optimistic about the workings of Roman power at times. The scholarly underpinnings of the study, however, have at times proven shaky; too many details have been got wrong for the reader to feel confidence that the author is in control of his material. I have checked only a sampling, where something in the text or notes awakened suspicion, and I have cited only a selection of these concerns in this review. But the reader will be well advised to check the references before citing Alston for any particular point. It is particularly worrisome that papyri seem to be used without any systematic attempt to see if their texts or interpretation have undergone subsequent change, as they often have.<sup>24</sup> All the same, it would be rash to conclude from these blemishes that the principal arguments of the book are mistaken. Readers will have to form their own opinions about the likelihood that the occupying army of an imperial power was a largely benign and well-liked part of the human landscape of Roman Egypt.

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<sup>24</sup> This is usually not difficult to trace, thanks to the *Berichtigungsliste der griechischen Papyrusurkunden aus Ägypten*, of which the eighth volume, covering work published up to 1986, appeared in 1992, seemingly the year when Alston effectively closed his manuscript.