

effet du déclin de l'esclavage. Si chaque aspect pris séparément peut encore être interprété autrement, leur coïncidence chronologique et convergence<sup>28</sup> nous amène à y voir la même cause primaire génératrice — le déclin de l'esclavage.

Ce qui importe ce n'est pas le nombre des esclaves mais leur rôle dans la production,<sup>29</sup> en agriculture, dans l'artisanat et le commerce. Dans ce domaine aussi le déclin est incontestable.<sup>30</sup> R.S. Bagnall accepte cette conclusion (n. 18) mais croit que le faible emploi de la main d'oeuvre servile dans l'activité économique proprement dite est compensé par le rôle joué par les esclaves dans les 'households' de leurs maîtres: 'the importance of slave assistance for the ability of a small elite to manage business, civic and military affairs should not be underrated' (p. 233). Mais cette assistance qui sans doute avait une certaine valeur pour les maîtres des esclaves néanmoins ne peut pas être mise sur le même pied qu'une participation massive à une activité productive.

Nos remarques ne diminuent en rien la grande valeur et l'importance du livre de R.S. Bagnall qui sera lu avec intérêt, profit et plaisir par tous les spécialistes de l'histoire ancienne, indépendamment de leur domaine particulier de recherches.

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*The Roman Army in the East*. Journal of Roman Archaeology, Supplementary Series Number 18, ed. by D.L. Kennedy, Ann Arbor, 1996, 320 pp.

Edward N. Luttwak's influential book *The Grand Strategy of the Roman Empire from the First Century A.D. to the Third*, Baltimore, 1976, was received favorably by not a few ancient historians, but criticized nonetheless in several important reviews (e.g. J.C. Mann, *JRS* 69, 1979, 175-83; cf. F. Millar, *Britannia* 13, 1982, 1-23). In his major study of the frontiers, C.R. Whittaker rejected Luttwak's two main notions, namely, that there existed a Roman grand strategy and that the Romans developed systems of defence on the frontiers (*Les frontières de l'empire romain*, Paris, 1989; *Frontiers of the Roman Empire. A Social and Economic Study*, Baltimore, 1994). But perhaps the most comprehensive attack upon Luttwak's reconstruction of Roman strategy and frontier systems was put forward in Benjamin Isaac's innovative and provocative study (*The Limits of Empire*, Oxford, 1990, rev.ed. 1992), whose main concern is with the military aspects of the Roman presence in the East while Whittaker concentrates on the social and economic

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seems hard to suppose that they (the landholdings — I.F.) did not include slaves' (ibidem, 124).

28 R. Bagnall lui-même remarque avec justesse que 'the presences or absences of document types have much to teach us about these societies' (R.S. Bagnall, *Reading Papyri*, 24).

29 Même à l'époque romaine qui nous a conservé un grand nombre de textes où figurent des esclaves ruraux, esclaves artisans etc. ils ne constituaient pas ni la main d'oeuvre principale ni le gros du personnel servile. Dans P.Oxy. XLIV 3197 (111) il s'agit de la division des esclaves appartenant à Tiberius Iulius Théon, membre de l'élite municipale alexandrine. Il est impossible de déterminer le nombre exact des esclaves (probablement une centaine) mais seulement 12 des 59 esclaves nommés ont une profession, et ces professions n'ont rien à faire avec une activité productive. Il s'agit d'esclaves domestiques, dans le meilleur cas de serviteurs spécialisés (*êpêtês, koureus, mageiros*), v. Fikhman, *Oksirinkh* (n. 22), 325-7.

30 Voir Fikhman, *Egipet* (n. 22), 60-2; Fikhman, *Grundfragen* (n. 24), 154-6; Fikhman, *Oksirinkh* (n. 22), 197-9; Fikhman, 'Il lavoro servile' (n. 24), 252-6 etc.

aspects of the Roman frontiers in the West. In his turn, Isaac's book was received with much approval in some quarters, but came under heavy fire in others. That it has been the impetus of the present book, and that its sub-title, *The Roman Army in the East*, has been chosen as the theme and title for this collection of fourteen essays, testify to the stimulating and productive effects of Isaac's book. The editor has solicited contributions from various scholars aiming at furthering discussion and providing studies on varied aspects of the subject, rather than at reviewing the methodology, positions and conclusions adopted by Isaac. And yet many of the authors of this selection of essays on various topics relating to the history of the Roman army in the East, from the Late Republic to the 4th century C.E. (and in some cases even later), address themselves in greater or lesser extent to the views of Isaac. They all, however, provide substantial contributions which have to be assessed in their own right.

The collection opens with an introductory essay by the editor, D. Kennedy, entitled 'The Roman Army in the East' (9-24), which presents an overview of the subject. He avers his belief in the unity of the theme despite the vastness and varied geographical and cultural features of the areas concerned, from the Caucasus to Sinai, as well as the long time-span covered and the varied roles performed by the Roman army. The unity of the theme is based not only on the Roman perception of the eastern armies as a group, so Kennedy claims, but also on the geographically and strategically central location and role of Syria in the entire East, both in regard to Roman relations with Parthia and to the troublesome southern populations, the Jews and, to a lesser extent, the Arabs. Now, compared to the intensive research on the Roman army in Europe, the Roman East has suffered from neglect; it is only in the last generation or so that research on the Roman East, in particular archaeological surveys and excavations, has significantly increased. Kennedy provides a stock-taking survey of this research, and although he notes omissions and problems, he apparently considers the writing of a major history of the Roman Army in the East, on the basis of advances in research, as a real possibility. This may be doubted.

Kennedy himself qualifies such an optimistic expectation. He points out that the complexity of the subject might deter scholars from treating the entire East as one unit; they would tend rather to specialize in particular provinces or regions. But of course such special studies should not be regarded as a cause for concern, for those who will undertake the challenge to compose the history of the Roman army in the East will only benefit from them. The real obstacle, although this is not how he terms it, is implied in Kennedy's suggestions for future research. These are presented under three headings: approaches, methodology, and where and what (i.e. sites to be explored and excavated). Under the first heading he calls for problem-oriented investigations of material remains and for a total, integrated examination of sites explored, that is, of their full geographical, environmental, chronological and human context. Under the second heading he recommends the application of two techniques considered as the most productive for the discovery and recording of sites: surface survey and remote sensing. The *desideratum* that is urged under the third heading is simple: to acquire much more data about early legionary fortresses, late Roman forts, native military sites, in short to find more sites and to record fully those known. In delineating these *desiderata*, Kennedy brings to the fore the serious shortage of knowledge of basic facts and conditions, the enormous extent of the research that is needed (indeed badly needed, as Kennedy rightly points out, in view of the rapid modern development of the countries of the Near East). Therefore if one may be tempted

to write the history of the Roman army in the East, on the basis of the presently known materials, the shortcomings and limitations of such a work can be predicted.

Three authors continue the grand debate that started with the publication of Luttwak's book, and do not deal particularly with the Roman East. Astonished and, I suspect, amused by the passionate, vehement attack lashed out by E.L. Wheeler (*Journal of Military History* 57, 1993, 7-41, 215-40) against himself, Isaac and other scholars who dared to reject the main propositions of Luttwak, C.R. Whittaker (25-41) insists that a distinction should be made between basic military planning and strategy, let alone Grand Strategy. In answering the arguments raised by Wheeler, as well as those of A. Ferril (in P. Kennedy ed., *Grand Strategy in War and Peace*, New Haven, 1991, 71-85), Whittaker shows that while the relevant literary evidence and examples of Roman behaviour adduced by them do indicate battle plans, some planning of wars, even some strategic thinking, they do not testify to the existence of 'an integrated effort towards a political end', which is the essence of Grand Strategy according to modern theory. Warning against the confusion of war ideology with strategy, he re-examines the literary, epigraphic and archaeological evidence and affirms his conviction that there was never a reversal of the Roman ideology of expansion and of the claim to universal dominion (P.A. Brunt welcomed Luttwak's book but, in a recent analysis, has come to a similar conclusion, that is, that the right of Rome to rule the world was never renounced: *Roman Imperial Themes*, Oxford, 1990, 433-80). In short, a Roman traditional proclivity to aggressive expansion was not replaced by defensive strategy.

David Braund deals with a particular topic involved in the debate, which is whether rivers functioned as barriers (43-7). He concedes that Mann, Isaac, Whittaker and other scholars are right in their observation that rivers were means of communication, highways of shipping, and sometimes convenient lines of demarcation, and not natural frontiers. Against this reality, Braund seeks to show that in the environmental psychology of the Romans, rivers were perceived as powerful deities. To channel, bridge or sail a river might arouse the wrath of the deity; given their religiosity rivers were therefore considered as natural boundaries. It is not clear whether by this Braund means to demonstrate that the Romans recognized limits to their empire. However, he himself cites cases of how Roman generals (e.g. Lucullus, Crassus, Caesar) who were aware of the nature of rivers crossed them. Indeed coins of Trajan and a scene on Trajan's column point to the Danube's assistance in the Dacian campaign of that emperor. In other words, the religious nature of rivers did not pose a problem for Roman expansionist trends.

David Potter, who limits his discussion to the first and second centuries C.E. (49-66), argues that the Grand Strategy presented by Luttwak suffers from over-simplification; Isaac's contention that Rome persistently aimed at expansion is not born out by emperors' *mandata* that concerned foreign and border policies. Most of Potter's essay is devoted to evidence that weakens the positions of Isaac; one might infer that, in his view, the weakness of Luttwak's Grand Strategy is self-evident. According to Potter, the emperors' *mandata* show that Augustus' successors followed the famous advice he gave to Tiberius in a codicil to his will: *consilium coercendi intra terminos imperii* (Tac. *Ann.* 1.11; cf. Dio 56.33). The trouble, however, is that these instructions are not sufficiently known, and their reconstruction is open to different interpretations. Thus, for example, Potter claims that Tiberius' instructions to Germanicus to cancel plans for expansion in Germany may be gleaned from the recently found *tabula Siarensis* (J. Gonzalez, *ZPE* 55, 1984, 55-100; W.D. Lebek, *ZPE* 67, 1987, 129-48). It records the decision to construct

triumphal arches, at Rome, on Mount Ammanus and on the Rhine, to commemorate the victories of Germanicus in 16/17. This is just one of the honours bestowed on Germanicus posthumously as we know from Tacitus (*Ann.* 2.83), but the new evidence has been construed as implying defensive policy and a formal recognition of limits to the Empire (Potter, *ZPE* 69, 1987, 269-87; G.A. Lehman, *ZPE* 86, 1991, 79-96). The very argument and inferences are contested in this volume by Whittaker, and since it has been revealed that one arch was set up on the far bank of the Rhine (H.G. Frenz, *JRA* 2, 1989, 120-5), Whittaker claims that it conforms to Augustus' basic view, namely that the *fines imperii* included the unadministered but subject peoples, who lived beyond the administered provinces. In other words, the arch demonstrates the continuation of Augustus' claim to world dominion. Be that as it may, Potter is more successful in his insistence on the need to differentiate between foreign policy directed by an emperor, and behaviour and actions resulting from the local conditions of a frontier. He is also probably right in his contention, against Isaac, that the ideology of world conquest did not go unchallenged.

The other essays in the collection deal directly with the Roman East. I shall deal first with those that have some bearing upon the debate generated by the works of Luttwak and Isaac. Attempting to present the Parthian perspective, which has been neglected in modern studies, the editor reviews the relations and confrontation between Rome and Parthia (67-90). Kennedy's main conclusions are that Rome as well as the Parthians coveted the whole of the Fertile Crescent; mindful of the Roman defeats in the expeditions against Parthia, Augustus and his successors applied caution in handling Parthian affairs; thanks to its military superiority Rome became aggressive as from the time of Trajan, and extended its power over the eastern part of the Fertile Crescent, in stages, by the early third century. This review of the Roman-Parthian encounter offers, by implication, some support to Isaac's emphasis on the basic expansionist tendency of Rome. Kennedy, however, points out that Roman expansion ended with the assassination of Caracalla, and that the Roman gains in Mesopotamia seem to imply the unwillingness of even the aggressive emperors to expand beyond the Zagros.

Philip Freeman examines the annexation of Arabia (91-118), which reveals a confused, hesitant process (a term disliked by Freeman) of integration, rather than strategic planning. It was probably initiated by the governor of Syria, not by the emperor, and testifies to Roman reaction to some development in local conditions and not to Grand Strategy. Whatever the 'local event' that triggered off the Roman intervention, which is a moot point, it is in line with other cases of Roman expansion which may be explained by circumstances other than long-range planning. Thus far Freeman is in accord with Isaac, but he criticizes him for failing to appreciate the 'almost random nature of Roman warfare'. The validity of this criticism is doubtful although it is true that Isaac does not follow Freeman's view about the way provinces came into being (cf. Freeman in F. Freeman and D. Kennedy ed., *The Defence of the Roman and Byzantine East*, Oxford, 1986, 253-74), which is essentially a different problem. More to the point is Freeman's criticism, which is stated rather than fully argued, of Isaac's thesis that the Roman army in the East tended more to the suppression of local opposition to Roman government than to handling external threats.

In his contribution to this volume, Benjamin Isaac analyses the structure and information of Eusebius' *Onomasticon* (153-67). The regional cartography reflected in the *Onomasticon* is of the same quality as that of the global geography revealed in the work of Strabo, the Peutinger Table and the *Itinerarium Antonini*. A map drawn on the basis of

such geographical perception would not be drawn to scale, nor convey a true sense of topography, but be characterized by a linear approach to geography (cf. P. Janni, *La mappa e il periplo*, Rome, 1984). According to Isaac, Eusebius' source of information was material he found in the Roman governor's archive in Caesarea. The *Onomasticon* therefore indicates the geographical and cartographical shortcomings of the material at the disposal of Roman administrators and officers. It thus corroborates Isaac's argument that without a clear concept of topographical realities and advanced geographical insights the Romans were not capable of forming a Grand Strategy. There might be one flaw in the argument of Isaac, however. If the *Onomasticon* was written around 293 (cf. T. Barnes, *JThS* 26, 1975, 412-5), which Isaac seems to accept, it is quite doubtful that at that time Eusebius had access to the archive of the Roman governor of Palestine.

Shelagh Gregory's study of the architecture of Roman fortifications in the East, which is based on her research for a Ph.D. at Sheffield University (see now her *Roman Military Architecture on the Eastern Frontier A.D. 200-600*, Amsterdam, 1996), presents a devastating but instructive review of the early reports of Roman military installations in the East, including those of such prominent figures as von Domaszewski, Butler (Princeton Archaeological Expeditions to Syria), Musil, Stein and Poidebart. Misconceptions, inaccuracy, imagination and faulty methodology led these scholars all too often to wrong datings and identifications of so-called Roman fortifications. Time and again conclusions based on air photographs and surveys were belied by excavations: Roman buildings turned into Islamic or Sassanian and military structures into civilian. The overall conclusion is that the archaeological evidence does not show the construction or development of a fortified line; in this respect it supports Isaac's position. Gregory's main object was to ascertain whether changes in design of later Roman fortifications originated in the eastern frontier. Given the available information, the conclusions are, on the whole, negative: no typology of the eastern fortifications alone may be established; the abandonment of the 'playing card' forts in the West may be ascribed simply to reversion to the more 'normal' stationing of troops in cities; no new designs can be shown to have originated in the East.

Alan Rushworth's contribution is a comparative study of the deployment and functioning of the Roman army on the deserts of the East and North Africa (297-316). He highlights differences in strategic conditions, common problems of security and similarities in patterns of troop deployment and army activities. His two main conclusions broadly accord with those reached by Isaac: in both regions linear deployment of troops along roads was intended to secure communications and did not mark frontier lines or served as preclusive barriers; the army was very much occupied in internal policing of rugged terrain. He emphasizes, however, the need to study more thoroughly some important questions, for instance the use of client states, and to carry out detailed surveys and excavations of known structures before firm conclusions can be established.

The remaining five essays are not related to the great debate. Ruprecht Ziegler examines the connection between local production of bronze coins and imperial campaigns in the East (118-34). Following his own previous studies and those of several other scholars, he rejects the thesis of M. H. Crawford (*ANRW* II.2, 572-4) that the increased minting by cities resulted from their need to meet fiscal burdens imposed by the Roman government. Rather, cities turned to intensive minting when they anticipated large-scale troop movements, as a result of rising tension between Rome and its eastern neighbours; soldiers would need small change which the imperial mints did not supply. The interpretation

looks attractive, but it is questionable whether every peak in municipal issuing should be construed as indicative of a planned campaign or a rise in political tensions.

John Eadie reviews the involvement of the eastern army in politics in the years 175-272 (135-50). His main findings are that until the rise of the emperor Philip in 244, army rebellions in the East were the work of commanders or expeditionary troops who arrived from the West. In other words, they do not testify to local resistance to Roman rule, nor to the unreliability of the army of the East. However, the failure of the central government to provide security against the growing power of Sassanid Persia and its devastating raids into Roman territories, caused the emergence of Philip and other local candidates. They were able to exploit the general dissatisfaction to their own advantage for they were considered more attentive and reliable in handling the external threat than a remote emperor. But only the rise of Zenobia can be regarded as a truly indigenous rebellion.

The relationships between the army and the civilian population in Dura-Europos are the subject of the essay of Nigel Pollard (211-27). In contrast to the widely held view that the army acted as an agent for the process of Romanization, Pollard follows B. Shaw in focusing on phenomena that reveal separation of soldiers from civilians (*Opus* 2, 1983, 133-59). The study is based on the concept of 'total institution' used by the sociologist E. Goffman (*Asylums*, Chicago, 1961) to characterize institutions which function as closed societies. He examines social, economic and religious relationships, concluding that the official role of the troops (notably policing and tax-collecting duties) brought about tensions and conflict with the civilian population. Separation rather than integration should be stressed, and the concept of 'total institution' is thus proved right to describe the conditions in Dura-Europos. The conclusion seems to me to go beyond the evidence. Pollard admits that various cases of close relationships between civilians and soldiers are attested in the economic and social life of Dura-Europos. He prefers to draw attention to contradictory cases and possibilities, but the truth is that the available evidence is not comprehensive enough to decide whether separation was more typical and significant than integration.

Everett L. Wheeler presents a comprehensive, thoughtful study of the proverbial laxity of the Syrian legions (229-76). In a thorough analysis of the relevant sources from Tacitus to Ammianus Marcellinus, he shows that the subject of lax discipline was used by ancient writers as a literary *topos* which had no basis in reality. Throughout the whole period under discussion, the Syrian legions were not inferior to other legions of the Roman army, neither in their discipline nor in their efficiency. The origins of the *topos* go back to the third century B.C.E. when Roman armies faced Greek and, later on, eastern cultures. Exposure to these wealthier and more refined cultures was considered to threaten traditional Roman moral values and particularly to corrupt military discipline. The assumed Greek and eastern characteristics, such as softness, laziness, luxury etc., were attacked in public and in writing, and very soon were exploited and manipulated in internal politics. By the time Augustus secured his sole rule, the *topos* of the corrupting luxury and loose morals of the East had been well established. Wheeler's analysis is quite persuasive. What one misses is how this traditional perception of the East became attached to the Syrian legions. Was Tacitus the first imperial writer to ascribe laxity to the Syrian legions? This seems to be implied by Wheeler in his remark on Tacitus' intimate knowledge of Livy. At any rate, some writer must have been responsible for the application of the traditional image of the East to the Syrian legions, some time after the Augustan period.

Edward Dabrowa's contribution, too, concerns Syria. It is a prosopographical study of the legates of four of the Syrian legions: *III Gallica*, *VI Ferrata*, *XII Fulminata*, and *XVI Flavia*. A comprehensive prosopographical study of the officers of another legion associated with Syria was provided by Dabrowa in a former work (*Legio X Fretensis*, Stuttgart, 1993), and the officers of the legion *IV Scythica* are dealt with by H. Devijver and M.A. Speidel in another supplement of *JRA* (D.L. Kennedy ed., *Zeugma Archaeological Project*, forthcoming). Together these studies form an up-to-date revision of the lists of the commanders of Syria published by E. Ritterling in 1925 (*RE*, XII, cols.1529-30, 1561-2, 1575-7, 1594, 1708-9, 1766). A brief, informative history of every legion opens the list of its legates, following which Dabrowa discusses the family origin and career of every one of them, with the relevant epigraphic evidence cited in full. Altogether 31 legates are listed (*III Gallica* — 17; *VI Ferrata* — 7; *XII Fulminata* — 2; *XVI Flavia Firma* — 5). Given this limited evidence, the inference that many commanders of the Syrian legions played important roles in the political life of Rome may be hasty, and so is the claim that appointment to the post was determined by specific criteria.

In sum, this collection presents an illuminating cross-section of many of the sub-themes which make up the central theme. The various subjects treated emphasize most of the key problems with which the historian of the Roman army in the East has to deal. The scarcity and problematical character of the sources, underlined by the editor in the introductory essay, is demonstrated time and again almost through all the essays. Controversy over interpretation is thus inevitable. None the less, the editor may be satisfied with the results, for the essays not only constitute a useful, substantial contribution to scholarship but also provide a stimulus to further research on the history of the Roman army in the East.

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J.E.H. Spaul, *Ala<sup>2</sup>: The Auxiliary Cavalry Units of the Pre-Diocletianic Imperial Army*. Andover, 1994, 327 pp.

This is a useful, informative and succinctly presented book, defined by the author as a revision and updating of the article which C. Cichorius wrote on the same subject (*RE* I, 1893, cols.1224-70). Cichorius' article is indeed the starting point of almost every discussion, whether on individual units, questions of methodology or particular topics. No bibliography is given, although the author provides, in the introduction, an instructive (but not exhaustive) survey of works relating to the subject which have been published since Cichorius' *ala*. References to the works mentioned in this survey are used throughout the book. Some omissions seem odd, e.g. H. Wolff and W. Eck, eds., *Heer und Integrationspolitik. Die römischen Militärdiplome als historische Quelle*, Cologne, 1986; and even more so, the volumes of M.M. Roxan, *Roman Military Diplomas*, London, 1978, 1985, 1994 (of course the more recent volume appeared in the same year as Spaul's book). Not less curious is the fact that Roxan's editions of diplomas are not mentioned in the list of abbreviations of 'major printed sources', but the abbreviation *RMD* does appear, rightly, throughout the book. It is fair to add that many more relevant publications are exploited and mentioned where appropriate. In addition to somewhat affectionate appreciation of Cichorius, the introduction also provides general remarks and explana-