

Rhodos, preoții lui Helios se regăsesc atât în inscripții, cât și pe ștampile de la sfârșitul secolului al IV-lea a.Chr. până la începutul epocii imperiale. Preoția lui Helios constituia cea mai importantă sarcină, fiind ținută pe rând de locuitori din Ialysos, Kamiros și Lindos. Așa cum arată autorul, unii dintre acești preoți fuseseră înainte și preoți ai Atenei Lindia sau/și *damiourgoi*. Lista preoților lui Helios prilejuiește lui N.B. un excurs privitor la istoricul cercetării ștampilelor de amfore produse în Rhodos. După N.B., începutul ștampilării amforelor rhodiene este de plasat în jurul anului 300 a.Chr. În discuție este adusă o inscripție descoperită în Rhodos, care amintește primii preoți ai lui Helios, dintre anii 407 și 368 a.Chr., și cei din perioada 332–298 a.Chr. Apoi, N.B. reușește să identifice preoți eponimi de pe ștampile cu persoane cunoscute din inscripții și din alte surse.

Concluziile (p. 201–202) sunt însoțite de un tablou sintetic, de un tabel cronologic al inscripțiilor rhodiene (p. 205–246) și de cinci anexe (p. 247–304): atestări ale adopției, eponimi, preoți ai lui Helios asociați lunii intercalare de pe ștampilele de amfore, sculptori și *stemma*.

Lucrarea este însoțită de un catalog excelent al inscripțiilor (p. 305–453), 72 la număr, cu un aparat critic detaliat, traduceri și comentarii pertinente și cu ilustrații de bună calitate. Urmează liste bibliografice pentru surse (p. 455–465), ștampile de amfore (p. 466–469) și o listă generală (p. 471–479). Concordanțele, un index detaliat și o listă a sursei ilustrației din text încheie acest volum deosebit prin calitatea cercetării.

Uneori, trimitere la catalog ale descoperirilor discutate în text sunt oarecum greoaie, adesea fiind făcute exclusiv în notele de subsol, iar lectura este, prin natura subiectului, mai degrabă dificilă. N.B. reușește însă nu numai realizarea unui capitol de istorie locală, ci, prin rolul jucat de insulă în epoca elenistică, și o contribuție majoră de istorie antică. Nu în ultimul rând, volumul va deveni un instrument de lucru util pentru arheologia epocii elenistice, ștampilele de amfore rhodiene fiind descoperiri comune atât în spațiul mediteraneean, cât și în cel pontic.

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Toni Naco del Hoyo, Fernando López Sánchez (eds.), *War, Warlords, and Interstate Relations in the Ancient Mediterranean*, Impact of Empire 28, Leiden – Boston, Editura Brill, 2018, xiv + 504 p. (ISBN 978-9-00-435404-3)

A volume having this title is clearly promising from the very beginning specifically for all the students of private ancient Mediterranean (and Pontic) war and warfare – brigands, pirates, mercenaries, ‘rogue’ generals etc. Likewise, ancient military historians, classical archaeologists interested in the archaeology of violence and researchers of ancient international relations are surely tempted to at least throw an eye on such a volume that explores the appropriateness, the benefits and the shortcomings of using the concept of ‘warlord’ for describing influential individuals leading private military operations in ancient global systems characterized by different distributions of power – multipolarity, bipolarity or unipolarity.

This volume represents the proceedings of an ambitious ICREA Conference held in Barcelona in 2013 that assembled many leading scholars in the fields of classical warfare and international relations, as well as promising young researchers in the same fields. It is surely a pity that influential scholars in the field of private warfare, like Philip de Souza and Vincent Gabrielsen, have not submitted or retreated their papers for publication in this volume (and this calls again into question the perspectives of publishing in proceedings volumes whose appearance lag for years after the conferences actually took place). Nevertheless, most participants to the Barcelona conference sent their contributions and, at the same time, the editors were very inspired both to keep in their introduction some of the remarks

made by the scholars whose papers are missing and to invite also two contributors who did not take part to the conference, but whose papers in the last, separate, part of the volume, outside the immediate scope of the other contributions, is highly welcome.

The result is a highly attractive mosaic of 22 papers, ranging in time roughly from the 4<sup>th</sup> to the 1<sup>st</sup> century BC (with the notable exceptions of the last two papers, dealing with Late Antiquity and modern international relations) and spatially covering all the Mediterranean, although the centre of gravity is represented as a matter of course by the Greek and Roman societies. Therefore, there is barely anyone interested in ancient military warfare in the Late Classical and Hellenistic periods who will not find at least one paper of immediate interest. The diversity of topics and approaches is indeed conspicuous: from the Persian Empire and Hellenistic kingdoms, through the Greek city-states, to the Roman and Carthaginian republics, and from general analyses like J.W. Rich's "Warlords and the Roman Republic" and N. Rosenstein's "Why No Warlords in Republican Rome?", through regional or factual surveys like M. Álvarez Martí-Aguilar's "The Network of Melqart: Tyre, Gadir, Carthage and the Founding God" and F. López Sánchez's "Galatians in Macedonia (280–277 BC): Invasion or Invitation?", to individual case studies like those dealing with Lysander (D. Gómez-Castro), Iphicrates and his *lochagoi* (N.V. Sekunda), Sulla (S. Zoumbaki) and Sertorius (T. Naco del Hoyo and J. Principal).

I will not provide an overview of all the papers, as this is already given by the editors in their Introduction (p. 7–11). Instead I will briefly address some questions and even possible shortcomings, pointing that they are not to be imputed to someone, especially given the fact that this volume is a pioneering venture in trying to adapt to classical military and interstate history possibly useful concepts drawn from modern social sciences, but should be highlighted in order to guide further attempts in the same vein.

*Primo*, I wonder if the chronological cut for the volume is meaningful. As I assume after reading it, warlordism is a phenomenon that might be envisaged only relative to states and a certain interstate global architecture (see below). Therefore, the chronological limits of the inquiry

should take into account the dominant type of political actors in the Mediterranean international relations and the distribution of power in the international system. The final emergence of a unipolar Mediterranean, conspicuously dominated by the Roman Empire, provides an undisputed ending point in the 1<sup>st</sup> century BC. The raise of Carthage, Syracuse and Rome as more than city-states in the Western Mediterranean, as well as the loss of the Greek *poleis* in front of Macedon in the East might also be considered as sufficient arguments for embracing the 4<sup>th</sup> century BC as the starting point of the inquiry, and probably this was also the reasoning of the organizers of the conference. Is this valid? Is this enough, when trying to adapt a modern concept for usage in ancient contexts?

I would have certainly preferred to oppose the warlords not only to the great empires of the last four centuries BC and to the Greek *poleis* in their last years of independence, but also to the Mediterranean city-states and tribal confederations before the clear domination of empires in the Mediterranean international system. The chronological starting point might have been raised at least to the last quarter of the 6<sup>th</sup> century BC, when for the first time an empire starts to compete for domination in the Mediterranean, followed quite rapidly in the 5<sup>th</sup> BC century by city-states ruling empires, as Carthage, Athens and Syracuse, if not to somewhere around 700 BC, when the Mediterranean begins to appear as a huge network woven through the activity of Phoenician, Greek and even Etruscan emerging city-states, without competition in their maritime ventures from land-based empires (Assyria, Egypt, Babylon, Media and Lydia are extraneous and quasi-totally land-minded). This would have helped not only in reducing the imbalance between the first part and the second part of the volume (seven papers on the 4<sup>th</sup> century BC and 13 papers on the Hellenistic period, see Introduction, p. 7), but would have certainly added value to the conceptual reflection on warlords, by extending it to political entities and international multipolar systems where the relationship between the private and the public power – at the core of the 'warlord' concept – was slightly different than in the last 4 centuries BC. The few remarks on possible occurrences of warlords in the archaic and early classical Persian Empire and Central Italy in C. Tuplin's, J.W.

Rich's and N. Rosenstein's contributions give some useful, but unfortunately insufficient, hints about how the debate could have been enriched by such a chronological and topical extension.

In order to show that this should not be simply taken as the resentment of an archaic-focused scholar, I may suggestively ask: are there any differences in the military possibly warlord-type actions conducted in Thrace and in the Hellespontic region by Peisistratus and the two Miltiades in the 6<sup>th</sup>, Alcibiades in the 5<sup>th</sup> and Iphicrates in the 4<sup>th</sup> century BC? Are the possible differences substantial or are they driven only by the different nature of Athens as a state and of the Northern Aegean as an international stage in these three consecutive centuries?

*Secundo*, I would have certainly expected one or two contributions of a more theoretical scope on the benefits or shortcomings of using the concept of 'warlord' in historical contexts that have never produced any equivalent terms (T. Năco del Hoyo and J. Principal, p. 385–386, 405; B. Rankov, p. 315), or at least a broader discussion in the editors' Introduction.

Surely, we are made aware by the editors that they did not develop the theoretical arguments briefly exposed in the Introduction, as the individual authors did "tackle most of these issues from multiple and varied perspectives in their own chapters" (p. 6). Indeed, there are valuable insights provided by most of the contributions and especially in those written by C. Tuplin, L. Rawlings, A. Coşkun, J.W. Rich, S. Zoumbaki, T. Năco del Hoyo and J. Principal. As well, R. Grasa's paper at the end of the volume is helpful as a summary of the conceptual debate regarding modern warlords. Last, but not least, readers are frequently informed on the lack of consensus between the participants to the conference with regard to the meaning and usage of 'warlord' (e.g. A. Coşkun, p. 206: "after days of lively debates in Barcelona, we were still far from agreeing on a clear-cut definition that was neither over- nor underdetermined"), therefore the reluctance towards an attempt to provide a general overview is wholly explainable.

Nevertheless, at the end of the reading, I feel a little bit uncomfortable with the lack of some thorough discussions and possible unified views regarding the results of so many interesting case studies, where different authors used slightly (or even largely) different definitions of the concept

of 'warlord', in order to describe diverse historical facts and situations (or to refuse the possibility of making such descriptions). Is it useful to apply this modern concept to ancient reality or not? When? How? Are there conspicuous differences between its modern use and the tentative use in ancient contexts? Readers might well answer themselves these questions after crossing through all the particular cases shown in the book, but more expanded guidance than that briefly sketched in the Introduction would have been welcome.

I come up here with my own insights, based on the reading of the contributions to this book.

Establishing a *genus proximum* is rather simple, being based on two conditions that are consensually accepted: (1) the overwhelming part of the strength of warlords is military – they are able to attain their goals through use of the sheer force of significant armed groups, even armies, which are kept loyal to them through permanent redistributions of the wealth acquired as a result of military victories, plunder and military control of some territories producing resources; (2) they do not obey the central formalized institutional power of a typical political actor on the international stage and they are not themselves such a centre (they are not monarchs ruling through other institutions than the army).

Some authors end their conceptual quest here, although they recognize the unsatisfactory analytic usefulness of such a definition (e.g. J.W. Rich, p. 269: "To embrace these widely varying current applications, the word 'warlord' must be regarded as a broad, generic term to denote any individual non-state agent with military force at his control and able to act with effective autonomy. Such individuals may occur because central state control has weakened or because it has not yet fully developed or (as with condottieri) because it suits state authorities to co-operate with them. So defined, its breadth necessarily limits the term's analytic value, but with care it may still prove serviceable"). This might trigger confusions between warlords and usurpers, rebelled generals, leaders of political factions in a civil war, leaders of liberation movements etc. (which is the case in some contributions to this volume and in many other pieces of ancient historical literature).

Others authors try to establish a meaningful *differentia specifica* and most of the time the

discussion is revolving around legitimacy as the possible key to the conceptual conundrum (e.g. L. Rawlings, p. 155; A. Coşkun, p. 206–207). I think that such attempts are close to giving the correct solution for coining a truly useful analytic tool. However, even here much more precision has to be sought, as legitimacy is a central issue also to usurpers or leaders of factions caught in civil wars and is a highly subjective matter.

I assume that true warlords are those who do not seek any political legitimacy, ascension or power and are interested only in material gains for them and their bands/armies, extracted through the direct use of violence, through the threat of violence when they managed to establish their control over some areas and/or through selling their services to political actors of the international system. Therefore, in its most restricted sense, this conceptual category might encompass leaders of warbands, especially when they are excluded from their communities after they lose the internal competition for power, 'admirals' of piratical fleets, leaders of extended networks of brigands, mercenary captains of the *condottiere* type, rogue generals. They truly deserve their name when their military strength can somehow compete with that of typical political actors of the international system, either because of its size, in symmetrical confrontations, or because of the asymmetrical tactics they employ in contexts which favour such approaches.

Although I like to emphasize the apolitical dimension as the specific criterion that differentiates warlords from other autonomous charismatic military leaders, it should be born in mind that they are marginal pawns in a game where political actors are the dominant pieces. Exclusively or at least overwhelmingly exerting military power cannot be a lasting activity: warlords competing with political players either evolve, or perish. As R. Grasa (p. 464–469) astutely notices, warlordism should be envisaged as a process. People and territories over which political control is partially or entirely lost become subjects for the exclusive military control of warlords, but afterwards they come back under political control, either through the defeat of warlords, or through the progressive change of the very nature of the warlords' power: they

might institutionalize their power and begin a process of state formation or they might usurp the political power in already existing polities (scenario that occurs mostly in the cases of mercenary captains). In both situations, they cease to be warlords any more.

I think that the explanation for the great confusion reigning in the attempt to clearly conceptualize the idea of 'warlord' rests exactly in this processual dimension of warlordism, which renders it apparently so similar to other phenomena as civil wars, rebellions, secessionist movements, outright usurpations. Although the beginnings and the final results might be the same for all the phenomena, warlordism should be differentiated because at a certain stage of its development there is nothing political about it, like in the other cases, but only private military power used for acquiring material gains.

The fact that, in the end, the reading of this book kindles such reflections on the concepts of 'warlord' and 'warlordism' and their relation to the international system, shows that no matter the editorial preferences of certain very exigent readers, the volume is a great achievement, both for the study of ancient war and warfare, as well as for the research of the interstate relations occurring in the very remote past. It certainly represents food for thought for the scholars dealing with these research fields in Mediterranean ancient history. The great diversity of case studies collected in the book (happily complemented by a very well-built index) is a good incentive to expand the reflection not only to other periods, as I have already suggested, but also to other regions, like the Pontic area, where there are plenty of cases which await a closer scrutiny.

The topic itself and at the same time the approach followed in this volume in order to test the usability of modern concepts in the ancient world are praiseworthy. Although consensus with regard to the concept of 'warlord' has not been achieved, a great step forward has been made.

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