

of local divine heritage as a means of expressing a new colonial identity on the other; all this within the context of interactions with other Phoinikian cities (the discussion of iconography here cried out for illustrations, however).

This rich volume, with its wealth of case studies, offers much to stimulate, and opens up some new avenues for understanding the rich and varied processes by which *coloniae* became *poleis*. It is much more than a *bilan*, rather an important study in Graeco-Roman acculturation.

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John D. Grainger, *Syrian Influences in the Roman Empire to AD 300*. London and New York: Routledge, 2018. 273 pp. ISBN 9781138071230.

The subject of John D. Grainger's latest book should be, in theory, easy to define. According to the title, it is supposed to show the impact of Syria on the Roman Empire. However, it is not clear what Grainger's Syria is. From the first pages, it seems that Grainger defines Syria according to the idea of Greater Syria, which includes many territories such as Arabia and Judaea (pp. 1-2). But this position is problematic as he often describes those territories as not being part of Syria (e.g. p. 35). Moreover, not only does he alter his definition of Syria, he does this frequently. The most obvious example is on page 60, which begins with him claiming that the legions posted in Palaestina should not be counted as part of the "Syrian Legions", while at the bottom of this page, he defines Judaea as part of Syria (the author flips randomly between "Judaea" and "Palestine"). On page 63, he counts the legion in Jerusalem among those included in the list of legions posted in Syria, whereas on page 64, Arabia and Palaestina are again considered separate from Syria. There are many further examples but the point is clear.

Through the book, Grainger often provides us with sentences that would leave an academic reader puzzled. For example:

As a geographical region Syria was comparable with Italy, Spain, Gaul or Asia Minor in importance, though in size it was smaller than any of these. And yet its influence on the life of the Empire was out of all proportion compared with these and was infinitely greater than any of them. (p. 1)

There are some problems with this sentence. Firstly, he never tries to prove the "infinitely greater" influence of Syria. Secondly, it is puzzling to find an assertion that Greater Syria, which includes Palaestina and Arabia, was smaller than Italy, by someone who wrote numerous books about the area.

The book's first chapter introduces the land, its inhabitants and history. This chapter is filled with many inaccuracies and gross mistakes. One of them is Grainger's claim that the Assyrian system of mass deportations "reduced most of Syria outside Palestine and Phoenicia to an impoverished rural society" (p. 14). This statement is not only utterly wrong but also demonstrates that Grainger misunderstands the scholarly works that he relies upon in his footnotes. Astour, in a book review of an earlier publication by Grainger, highlighted the same problems and mistakes.<sup>1</sup> Unfortunately, Grainger does not absorb corrections offered to him by book reviewers. He

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<sup>1</sup> Michael C. Astour, Review of, *The Cities of Seleukid Syria* by John D. Grainger, *Journal of the American Oriental Society*, 114/2 (1994), pp. 267-270.

continues to repeat factual mistakes and dubious assertions, which reviewers have noted, and yet, he has published dozens of books in the last three decades.

Grainger's questionable representation and interpretation of evidence is demonstrated in the following chapters. The second chapter is supposed to discuss Syrians who belonged to the equestrian or senatorial classes, or became Emperors. But he concludes this chapter by saying Syrians did not want to be in the equestrian or senatorial classes, because he found relatively few, and that they only wanted to be Emperors. This is an illogical conclusion, since no one could become an Emperor without a proper background i.e. being part of the equestrian or senatorial classes.

In chapters 3 and 4, Grainger assesses the military importance of the Syrian province to the Empire, and describes the location and number of military units deployed in the area. His main stance is that Roman units serving in Syria had to recruit locals; thus, most of the legion would gradually become Syrian. Similar claims and ideas have been raised before, usually for the late Empire and less for the middle Empire. Such claims are generally logical, yet not totally founded for lack of evidence. Grainger's case is toeing this path, as he does not manage to find many Syrian recruits in those legions. These two chapters are better than the two previous ones, but do not provide an adequate summary of even a meagre portion of the existing scholarly studies relevant to the subject.<sup>2</sup>

The fifth chapter, about a third of the book in length, is supposed to present Syrians serving anywhere in the Empire, and *auxilia* units recruited in the area of Syria. The chapter is filled with contradictions and completely ignores important data, other evidence and recent research, since these are not in line with how the author wants to see the ancient world. Most contentious is the way he deals with Jews. For example:

One reason for the violence of the Jewish revolts may be that, since Jews did not join the Roman army, there were plenty of young men in Judaea to join the rebellions. (p. 77, n. 1)

This statement is in direct contrast to a dozen or more articles from the last two decades which deal with Jews serving in the Roman army, including during the revolts. The ferocity of the Jews' rebellions is more probably related to their first-hand knowledge of Roman tactics and capabilities. Moreover, in order to affirm his attitude towards the Jews, Grainger ignores others who suggest that inscriptions RIU1189 and RIU1202 belong to Jewish soldiers and prefers to emphasise their Syrian origins (pp. 91-92). It could also explain why he omitted from his book altogether the inscription RIU1161 which might have belonged to a Jewish soldier serving in the same unit as the individuals mentioned in the other inscriptions. In addition, he claims the Samaritans were a sect in Judaism and then points out two Samaritan *auxilia* units, completely contradicting his claim that "Jews did not join the Roman army" (p. 119). On pages 124-125, he mentions the *Agrippiana* units and brings forward the possibility that they were recruited from King Agrippa's disbanded army, only to adamantly declare that they were anything but Jewish. Lastly, on page 129, he presents a list of all the Syrian units, with possible times of recruitment. Unfortunately, the Samaritan and *Aggripirana* units mentioned before, have disappeared from this list.

The sixth chapter, which is similar in length to the fifth one, deals with religions originating and spreading from Syria across the Empire. In this chapter, Grainger talks about different faiths:

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<sup>2</sup> For example, Grainger refers neither to any excavation report from Dura-Europos nor to any of Simon James's writings which portray the Roman army stationed there.

“Palmyrene gods, Jews, Jupiter Optimus Maximus Heliopolitanus, Dea Syra, Jupiter Optimus Maximus Dolichenus, other Syrian deities and Christianity”. He lists them in this order according to their influence and spread, from the least to the most, which is, of course, Christianity. But he rates Judaism, which was second only to Christianity, as one of the least popular religions. One might think that Grainger made an honest mistake by switching between the “other Syrian deities” and “Jews”, but it is most probably a “Freudian slip”, derived from his anti-Jewish attitude. This attitude is demonstrated most clearly where he portrays Judaism and modern Jewish historians who are trying to study the subject:

Some of the practitioners of the study have been far too keen to identify dubious items of evidence as Jewish in their search for as wide a spread of Jews — usually their co-religionists — as they can find throughout the Empire; a certain degree of exaggeration is present in all this. (p. 149)

Firstly, Grainger counts only five books in the footnote which probably relates to this statement, the majority of them written by non-Jews. His animus leads him to make elementary mistakes when writing the names of the authors.<sup>3</sup> Lastly, he does not provide evidence for his claims, from these books. He continues his anti-Jewish line by saying that the only ones exaggerating more than modern Jewish historians do about ancient Judaism, were ancient Christian writers depicting Christianity. He also says that many people identified as Jews were actually Christians (p. 201, n. 267) without providing any evidence. Throughout the entire chapter, Grainger tries to prove that there were less Jewish communities than what has been claimed before. According to him, the Jewish community of Venusia, which was considered large because of the sizeable number of bodies in the catacombs, was not that large, as the numbers should be spread over a century or two, which might indicate the existence of just a handful of Jews at any given time. While ignoring or diminishing the existence of Jews, he declares the existence of any other sect or belief in a certain geographic area, whenever he gauges any vague hint even from a single inscription. Another of Grainger's problematic sentences is:

The political terrorism of the Maccabees had been responsible for several expulsions, of Greeks, Idumaeans, and Jews, from Judaea and Palestine generally. (p. 153)

His target is almost certainly the modern state of Israel, as this sentence has nothing to do with historical research.

The seventh chapter is supposed to bring evidence about Syrian civilians living in the Empire outside Syria. The author does not manage to find many of those. Lastly, the eighth chapter speaks about the findings of Syrian influence in each province of the Empire (in a 20-page chapter, Grainger provides only 13 footnotes).

To conclude, Grainger's book is a mish-mash of facts and fairytales, sometimes even a political manifesto. There are countless contradictions and wrong footnotes. In many cases his footnotes refer to whole books instead of mentioning the exact page the argument relies on. One other glaring problem, which everyone who possesses knowledge of the English language can see, is the staggering amount of grammar and spelling mistakes. It demonstrates outright disrespect to his readers, after he was already rebuked on the same grounds for his earlier publications.<sup>4</sup> Most probably any translation of this book to any other language will be better than the original,

<sup>3</sup> For example, R.M. Smallwood instead of E. Mary Smallwood.

<sup>4</sup> For the most blatant case, see: Joseph B. Scholten, Review of, *The Roman War of Antiochos the Great* by J. D. Grainger, *The Journal of Roman Studies*, 96 (2006), pp. 219-221.

because no professional, in his or her field, would ever allow a book like this to be published the way it has been.

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Christopher B. Zeichmann, *The Roman Army and the New Testament*, Lanham, Bolder, New York, London: Lexington Books/Fortress Academic, 2018. XXI + 183 pp. 10 fig./illus. ISBN 9781978704022.

Zum historischen und politischen Kontext und der Vielfalt der Welt des Imperium Romanum, in der das Neue Testament entstand, gehört in sozialer, wirtschaftlicher, religiöser und kultureller Hinsicht ohne Zweifel auch das römische Militär, das länger schon auch in seiner wichtigen Funktion als Instrument der Herrschaftssicherung und der Verwaltung in den Provinzen mit vielfältigen Aufgaben erkannt worden ist.<sup>1</sup> Die hiermit zusammenhängende, bisweilen strittige Frage nach den Beziehungen und der Qualität dieser Beziehungen, Kontakte, Austausch- oder Abgrenzungsprozesse zwischen Militär und Zivilbevölkerung in den Stationierungsprovinzen, Garnisonen und Einsatzorten, blickt ebenfalls auf eine längere Forschungsgeschichte zurück.<sup>2</sup>

Das hier angezeigte Buch von Chr. B. Zeichmann (nachfolgend CBZ), Neutestamentler an der University of Toronto (Emmanuel College) und der Ryerson University, ist nicht sein erster Beitrag zum römischen Militär in Palästina.<sup>3</sup> Die vorliegende Publikation wird ergänzt durch 'The Database of Military Inscriptions and Papyri of Early Roman Palestine' (=DMIPERP; <http://www.ArmyofRomanPalestine.com>), die Texte aus der Zeit zwischen 63 v. Chr. und 135 n. Chr. versammeln will (aber noch im Aufbau befindlich ist vgl. CBZ S. XI) und sicher einmal in der Zukunft als ergänzende Quellengrundlage für den Gegenstand des Buches dienen kann: Auf die Datenbank und die dort gesammelten Belege wird im Text auch immer wieder verwiesen, die Hinweise führen aber noch ins Leere, s. etwa S. 13 zu §§ 83-91 etc.; viele Einträge sind in der Datenbank noch nicht auffindbar, was überaus bedauerlich bzw. auch ärgerlich ist: s.a. dazu die Liste der über die Datenbank zitierten Quellen auf den S. 181-183. CBZ interessieren allgemein die Sozialgeschichte der römischen Armee im Nahen Osten, die Rolle und Funktion des Militärs in der Region Palästina, vor allem während des 1. Jh. n. Chr., die Beziehungen bzw. Interaktionen

<sup>1</sup> B. Palme 2006: 'Zivile Aufgaben der Armee im kaiserzeitlichen Ägypten', in: A. Kolb (ed.), *Herrschaftsstrukturen und Herrschaftspraxis. Konzepte, Prinzipien und Strategien der Administration im römischen Kaiserreich*, Berlin, 299-328.

<sup>2</sup> Um nur zwei sehr gegensätzliche Ansätze für den römischen Nahen Osten zu nennen vgl. N. Pollard, *Soldiers, Cities, & Civilians in Roman Syria*, Ann Arbor 2000 und O. Stoll, *Zwischen Integration und Abgrenzung. Die Religion des Römischen Heeres im Nahen Osten. Studien zum Verhältnis zwischen Armee und Zivilbevölkerung im römischen Syrien und den Nachbarprovinzen*, St. Katharinen 2001.

<sup>3</sup> Vgl. etwa CBZ 2018. 'Military Forces in Judaea 6-130 CE: The *status quaestionis* and Relevance for New Testament Studies', *Currents in Biblical Research* 17.1: 86-120. Bald wird zudem folgen: CBZ 2019. 'Romans Go Home? The Military as a Site of Class Struggle in the Roman East and New Testament', in: R.J. Myles (ed.), *Class Struggle in the New Testament*, Lanham, 53-65.