

## REVIEW–DISCUSSION

### EXPLORING ROMAN IMPERIALISM

Paul Burton, *Roman Imperialism*. Brill Research Perspectives. Leiden and Boston: Brill, 2019. Pp. vi + 114. Paperback, €70.00/\$85.00. ISBN 978-90-04-40462-5.

Paul J. Burton has given us a readable, informative, and concise historiographical account, which offers readers a systematisation of the main theoretical and historical problems surrounding the study of Roman imperialism. In keeping with the spirit of the series in which the book features, *Brill Research Perspectives*, the author proposes a synthetic approach, in just over a hundred pages, and provides a general overview of one of the ancient historical problems that has most attracted modern scholars from the nineteenth century onwards.

The structure of the book is unpretentious and generally well thought through. In Section 1 ('Introduction', 1–9), Burton explains the origins of the historical reflection on imperialism, charting the main scholarly developments from the late nineteenth century to the publication of W. V. Harris' *War and Imperialism in Republican Rome* in 1979. The author also summarily mentions the general character of the main ancient literary sources available and establishes the scope of his work. Then, in Section 2 ('Imperialism', 10–18) modern theories of imperialism are briefly explored, and Burton proposes the perspective of M. Doyle as an important analytical tool for thinking about Roman imperialism. Sections 3 and 4 ('Roman Imperialism', 18–73, and 'The Diversification of the Field', 73–93) constitute the core of the book; they are followed by Section 5 ('Conclusion', 93–104). Through these sections a consistent picture of modern scholarship on Roman imperialism is built up, specifically, of those works appearing between the publication of Harris' *War and Imperialism* and Burton's own *Friendship and Empire* in 2011. In this discussion, the author has privileged theoretically informed historical approaches and they are classified as metrocentric, pericentric, and systemic perspectives, with an emphasis placed on studies that may be considered as paradigm shifts.<sup>1</sup> Finally, the considerable diversification in current studies is also addressed,

<sup>1</sup> For this classification into 'metrocentric', 'pericentric', and 'systemic' perspectives: Champion and Eckstein (2007) 6.

emphasising the broader syntheses related to four secondary topics regarding Roman imperialism: ‘Soft Power’; ‘Frontier Studies’; ‘Race, Ethnicity, and Romanization’; and ‘The End of Imperialism (?)’.

The book has many positive features. Novices to the field will profit from Burton’s critical examination of earlier scholarship. The argument is clearly presented, and the theoretical and historiographical discussion is broadly helpful, as is the identification of specific historical problems. Burton’s final assessment is very interesting. Probably one of the strongest points of the work is the attempt to bring historians closer to current theoretical debates on imperialism while trying to discuss critically the major scholarship of the last forty years (W. V. Harris, E. Gruen, A. N. Sherwin-White, R. Morstein-Marx, A. M. Eckstein, and Burton’s own contribution). The scholarship on the topic is classified, as already mentioned, into ‘metrocentric’ (whereby the aggressive impulse comes from Rome), ‘pericentric’ (the impulse for attack comes from the periphery), and ‘systemic’ approaches, the last of which is the approach advocated by the author of this book. Specifically, Burton defends Constructivism in International Relations, which maintains that ‘the internal discourse of a state culture, its language and ideas, including values, norms, ideologies, and practices’ are central ‘in shaping the structure of the international system’ (68).

As is explicitly stated at the outset (9), the bibliography discussed in this book comes exclusively from English-language scholarship. According to Burton, this is due to his own location within a certain academic tradition, but also to the fact that most innovative scholarship of the last forty years has been written in English. Agreement with the author is possible regarding the discussion in Section 3, but the assumption is less plausible when we come to Section 4. Otherwise, Burton does not limit himself to explaining the general theses of the different historians under close examination, but also discusses how they have dealt with different kinds of historical sources, carefully flagging the strengths and weaknesses of their approaches, and also drawing the attention of readers to the reactions prompted among other scholars.

In this vein, Burton explores the most insightful academic reviews—a good scholarly exercise, which also serves as a useful reminder for students of the important function of this type of scholarship. This is no minor contribution in a text designed to introduce scholars to the study of a controversial topic such as ancient imperialism. However, this helpful critical exercise appears to be employed in a rather unbalanced way, as is conspicuous from Burton’s discussion of *War and Imperialism*. Here, a page and a half are devoted to explaining Harris’ thesis (39–40), while later scholarly criticism of him takes up three pages (41–3), followed by thirteen pages of ‘responses’ to Harris from ‘pericentric’ perspectives under the leading title ‘Substantial Responses to Harris’ (44–56), where one might prefer ‘pericentric perspectives’ as being the more neutral.

The contrast with the lengthy development, for example, of Burton's own perspective as set out in *Friendship and Empire* is revealing in this regard: it takes up five pages (68–72), but barely more than one is devoted to scholarly reactions (and criticisms) (72–3). Of course, it is a relatively recent book, much less discussed by later academic critics, and even Burton concedes that it 'is not the appropriate forum for a lengthy response to *Friendship and Empire's* critics' (72). Even so, it would have been interesting, for the same didactic purposes set out above, to give readers a more explicit summary of the points questioned by different authors (many of them not coming from the 'Anglo-Saxon' sphere, such as P. Valdés Matías). I do not think it is helpful to readers to state only that there were those who 'were clearly motivated by enthusiastic interest in the work and sympathy with its general approach' (72). Among them, mentioned in a footnote, is P. Valdés Matías, who, while recognising the importance of the approach proposed by Burton, is quite critical. Starting from the examples of Roman policy regarding the Ilergetes and Hiero II of Syracuse, Valdés Matías concludes that 'muy a menudo los análisis de Burton se muestran incompletos o alejados de las evidencias que proporcionan las fuentes ... una parte importante de sus conclusiones se muestran inasumibles con la información omitida'.<sup>2</sup> The Spanish scholar also criticises some aspects of Burton's theoretical perspective on International Relations as well as his particular reading of Neorealism.

Aside from the uneven length of his discussions of individual authors and works, it seems to me that Burton fulfils the objective of systematising the main historiographical positions and, in addition, provides a good discussion of the systemic models of Roman imperialism, which, at least since Eckstein's seminal books, are at the heart of the debate among historians. Neorealism emerged as an issue that forced historians to think about the historical problems of the ancient world in light of a theory drawn from international relations (if not in light of contemporary interstate politics). Unfortunately, the debate that followed was frequently focused not on the conceptual problems deriving from the application of this modern theory to ancient realities, but was directly linked to personal attacks and derogatory political accusations. These attacks frequently leave the historical debate aside.

Indeed, the early scholarly reception of Eckstein's work was frequently harsh, even hostile; distrust towards the conservative political implications of the Neorealist approach was the main intellectual concern of reviewers. In the context of world events after the dissolution of the USSR, but especially after September 11, 2001, and the emergence of a more aggressive American imperial doctrine under the G. W. Bush Administration, there were many parallels between the place assigned to Rome and the USA. In many cases, the tone of the criticism was strongly derogatory. Some examples seem significant. Sartre (2007) 623, in his review of *Mediterranean Anarchy*, published in the French journal *Topoi*, accuses

<sup>2</sup> Valdés Matías (2012) 68.

Eckstein directly: ‘On l’a compris, l’essai d’Arthur Eckstein a autant à voir avec l’Histoire Ancienne qu’avec l’idéologie des néo-conservateurs américains au début du XXI<sup>e</sup> siècle’. Smith and Yarrow (2012b) 9, in their introduction to the edited volume dedicated to the memory of P. Derow, *Imperialism, Cultural Politics, & Polybius*, link Eckstein’s arguments with US foreign policy (in this case discussing his *Rome Enters the Greek East*): ‘that is an argument that drives the book towards a justification of a policy rather than an academic argument’. Eckstein (2012) 358, in a review published in *Histos*, answered this last criticism and meaningfully defended his theoretical model: ‘Realist theory is not a cover for aggressive policies; it is, in fact, a mode of academic analysis’. But again Harris (2016) 43, in his new book on the Roman Empire, attacked the argument of Neorealism as a cover for the advancement of a certain foreign policy: ‘it is not hard to see that the self-styled “realists” who write about Roman history are actually trying to provide coverage for the foreign policy of the contemporary United States—a policy that may possibly be defensible, but preferably not by means of pseudo-history’. Meanwhile, Hölkeskamp (2009) 213 talks about ‘the very American hidden agenda of this book’.

But much might be gained if we acknowledge that Neorealism cannot be directly equated with the Neocon ‘imperial image’. Even though Eckstein (2016) 4, in an autobiographical and highly moving introduction to his recent book, acknowledged that he was shocked by the 9/11 attacks, but also that he was really disappointed by ‘the manipulative and dishonest official rationale for the Iraq War’, an identification between Neorealism and Neocon imperialism might be completely misleading. We can argue against some implications of Neorealist theory (or theories)—its focus on description without explanation, for example, or its underestimation of economic factors—but we should not transform the academic discussion into an echo chamber of political correctness. On the attempt (misleading and interested) to have the Neocon ‘imperial image’ overlap with the Neorealist model: Palacio de Oteyza (2003) 21–4. When the ‘imperial image’ claims to be the heir of Neorealism, as well as when it pretends to monopolise it, not taking into account either the ideas of the balance of power or moral and political prudence (which are present in Neorealism), it ‘damages the conception of Realism reducing it to a vulgar *cliché* of abuse of power and militarism’, because Neorealism was born indeed ‘as an attempt to manage power in order to avoid its excesses, and not as an apology of power dressed as moral universalism’: Palacio de Oteyza (2003) 24. On Neocon historians and classical antiquity, see now Olivera (2020).

In this sense, I think that there are two central points in Burton’s proposal here of (re)thinking in theoretical terms a possible convergence between both IR (International Relations) models, that is, the ‘layered’ IR Realist paradigm and the ‘moderate’ IR Constructivist one: first, to understand how ‘The international system “shaped and shoved” states in certain directions, but state-level internal culture determined the nature of state’s responses to those forces’ (103); and, in second place, to think about the practical consequences of a theoretically informed (and then conscious) explanation of Roman imperialism.

Thus, one can propose that we ‘shape’ the characteristics and the form of the Mediterranean interstate system and, at the same time, we examine the differing features of the internal culture of each state, as they are projected relative to the system and affected the way of one state relating to other states

(and Burton makes a good case for how the Roman system of *fides* and *amicitia* served to enhance Rome's international image). However, a not very convincing point of Neorealist systemic approaches is the much stronger emphasis on establishing the similarity of Roman military culture with those of other Mediterranean states against which Rome went regularly to war, as can be recognised in Eckstein's main argument. From a Constructivist approach, its emphasis on exceptional cultural traits of Rome (*fides*, *amicitia*) is highly dubious: it ultimately allows us to regulate the violence by invoking similar cultural notions shared with other Mediterranean states, as is suggested by Burton. Eventually, the hypercritical reading of Harris' 'exceptional' model, of Rome as an exceptionally brutal imperial predator, undervalues fundamental aspects of Roman society that indeed reveal an exceptional social inclination to violence and a distinctive way of dealing with foreign peoples. Likewise, an approach such as IR Realist or IR Constructivist, too heavily focused on power politics, power capabilities, and diplomacy, contributes to blunting the weight of the material incentives at stake in the expansion of, or at least for the interest in, resources derived from regular and predatory warfare.

Regarding the first of the points, on the one hand, as Burton argues (58–60), a central point of Eckstein's critique of Harris is that violence and militarism should be regarded as common features of ancient Mediterranean societies (i.e., Classical Greece, the Hellenistic world,<sup>3</sup> Iron Age Italy) and that Roman militarism and aggression cannot be considered a unique factor *per se* in such an interstate system. However, in his recent reconsideration of the problem, Harris, now well informed about new trends in comparative studies on social dynamics within ancient and modern empires (such as the Chinese or the British ones), has noted the largest 'military participation ratio', that is, 'the proportion of the citizens who were regularly under arms' as a distinctive feature of Roman expansion, especially in republican times.<sup>4</sup> It was greater than that of Qing China or the British Empire, not to mention the unthinkable comparison with any other ancient polity with which Rome could have gone to war. And, on the other hand, he has also observed that the 'high level of citizen participation in warfare' of the Romans was linked to a systematic policy of mass enslavement of prisoners of war (at a level impossible to compare with any other ancient or modern society). This comparative dimension of the social dynamics of empires gives new strength to Harris' thesis. It would have been interesting, from this point of view, to include the new variant of

<sup>3</sup> 'Hellenistic states were Schumpeterian war machines' (59). Burton credits Eckstein with calculating the temporal gap of only six years of peace in the Hellenistic world between 323 and 160 BC, although the idea belongs indeed to Lévêque (1968) 279 n. 108, as Eckstein himself duly recognised: Eckstein (2006) 83.

<sup>4</sup> Harris (2016) 67.

the ‘Harris model’ in Burton’s discussion, since it invites us to (re)think about the ‘exceptional’ character of Roman imperialism in an interesting comparative historical dimension.

On the other hand, in this kind of systemic discussion, the economic benefits of imperialism are strongly downplayed when they are not largely missing from the discussion, either as concrete incentives or as ‘a welcome adjunct rather than a central ingredient’, as they were considered by Gruen some years ago.<sup>5</sup> In this sense, the economic discussion of imperialism is bluntly secondary in Burton’s book (except in the case of later imperial provincial administrative exploitation: 102–3). Notably, Burton draws attention, correctly in my view, to the fact that booty (land, bullion, slaves) could not be the primary reason for military campaigns, engaged year after year, given the generally perceived risk involved in ancient warfare (97–8). However, in order to defend a highly rationalised cost–benefit calculation in ancient decision-making on war, and to emphasise conversely that warfare was too expensive for the Roman *aerarium* during the Middle Republic, Burton resorts to T. Frank (1933), whose old figures show a chronic deficit between military expenditure and seized booty.

Recently, however, the financing of Roman warfare during the Middle Republic has been revisited, with the addition of important quantitative data within a novel interpretive framework. What is interesting is that the data systematised by P. Kay allow us to emphasise the importance of looting and war indemnities as a rudimentary financing mechanism within the ‘underdeveloped’ financial infrastructure of the Roman Middle Republic, whose only tool to keep the high rate of expenditure was to be quickly financed: ‘warfare became economically self-perpetuating’ (Kay (2013) 134–5).<sup>6</sup> Of course, it is difficult to assess accurately the share of Roman citizens in this appropriated (or indeed stolen, according to Harris’ fine correction) wealth.<sup>7</sup> At any rate, the scale of the resources gathered through warfare made it possible to finance the Roman *aerarium* for a long time during the first half of the second century BC, at least, when some 46,000 talents in silver bullion were collected between loot and war indemnities paid by defeated polities between c. 200 and 157 BC.<sup>8</sup> Moreover, Roman imperialist action had lasting consequences outside Rome, beyond the states that had suffered direct depredations. The enormous flow of silver bullion from the eastern Mediterranean to Rome, beginning in 188 BC at the latest, had visible economic consequences on Greek coinage throughout

<sup>5</sup> Gruen (1986) 315.

<sup>6</sup> Kay (2014) 21–42; id. (2013) 133–40.

<sup>7</sup> ‘Stealing’ (not appropriating): Harris (2015).

<sup>8</sup> Kay (2013) 136–7.

the following period.<sup>9</sup> These data, therefore, would make it necessary to rethink the Roman imperialist experience from the third to the second century BC, in which the level of rewards obtained through warfare was highly inconsistent and, therefore, its impact on public finances was also unstable.

I would also like to pause and think about the section devoted to ‘Race, Ethnicity and Romanization’. The discussion offered there is perhaps rather incomplete. Again, the bias for bibliographic selection is mostly linguistic, as suggested by the collection in which it is published. This leads us to ignore important previous works, which could have been helpfully contrasted, such as that of Y. A. Dauge’s *Le Barbare* (1981), which precedes the strong debate at the end of the 1980s on cultural ‘Otherness’.<sup>10</sup> At any rate, the author chooses some key works from earlier historians who have blazed the trail in studying the problem of ‘cultural interaction’ (Gruen, B. Isaac, with earlier readings by Sherwin-White, and D. B. Saddington). I fully agree with his assessment of the weaknesses of the work of Isaac, and also with the diagnosis of ‘overcorrection’ with respect to that of Gruen. One has the impression that the interpretation of the ‘Other’ in ancient Rome tends to be rather Manichean in current historiography (‘protoracism’ or ‘multicultural’ society?). And, from then on, it becomes difficult to find a middle way that is attentive to the many nuances in different kinds of sources (literature or material culture? That is the question). Possibly, in this case, the insightful discussion by K. Vlassopoulos would have been of great value here, although his book is not specifically focused on the Roman world. Vlassopoulos, far from proposing a reading of cultural ‘Otherness’ in the Hellenistic world in dualistic terms between chauvinistic ethnocentrism or unrestricted multicultural integration as irreconcilable poles, has pointed out, on the contrary, the existence of a certain balance between ‘Hellenocentrism’ (as an ‘intellectual system’, a way of thinking about the world), and ‘universalisation’.<sup>11</sup> It is clear that the ‘reality’—that is, the accumulation and intersection of relational and interactive connections in concrete social networks—was culturally affected by the ‘imagination’ (which is frequently perceived more clearly in literary texts), but

<sup>9</sup> Perhaps resulting from a massive withdrawal of silver bullion as booty to Rome from the Aegean area, together with the closure of the Macedonian silver mines between 167 and 158 BC (Liv. 45.18.3–4, 29.11; D.S. 31.8.7), there were massive bronze coinage issues in the Achaean League: Warren (2007) 164; Thonemann (2015) 74. This may also be behind the lightweight cistophoric coinage, coined from around 167 BC down to the principate of Augustus in western Asia Minor: Thonemann (2015) 80, with Kay (2013) 141–3. The latter author points out that the Attalids were experiencing similar problems to those faced by the Seleucids for the minting of silver coins. For the dating of the first cistophoric coinages in the 160s BC, see Meadows (2013) 175–81.

<sup>10</sup> Dauge (1981). But even clearly ‘colonial’ perspectives such as that of Haarhoff (1948) are overlooked. See Moreno (2016) 2–6.

<sup>11</sup> Vlassopoulos (2013) 328–9.

the imagination of the cultural ‘Other’ also had to open real space for concrete integration within an increasingly interconnected world (in the Roman case, even in political terms of a whole empire). Central to Vlassopoulos’ proposal, then, are the modern notions such as ‘globalisation’ and ‘glocalisation’ of cultural phenomena, which have not been taken into account in Burton’s work (even though it has been a much-disputed topic in ‘Anglo-Saxon’ scholarship in recent years).

From there, Burton goes on to discuss specifically the concept of ‘Romanisation’, focusing on the late Republic up to AD 200 (86 n. 406). The intellectual roots of this concept are summarised (Mommsen, Haverfield), and their strongly racial-colonial components are critiqued, taking into account the coming of a ‘post-colonial’ reaction in the 1990s, which is linked to the work of R. Hingley. It is a pity that highly controversial Francophone studies from the 1970s, which centred their historical explanations of cultural contact in the Roman Empire on the idea of ‘resistance’ to the colonial power, are left out of Burton’s discussion of this ‘post-colonial’ historiographic moment.<sup>12</sup> However, it features a good, if limited discussion of the English-speaking scholarly contributions between 1990 and 2010 that have focused on the western part of the Roman Empire and have begun to rescue the agency of the provincials in the cultural processes operating through different areas of the Roman Empire, mainly from an archaeological perspective (e.g., Hingley, M. Millet, G. Woolf, J. Webster, L. Revell). There is also a good argument advanced by Burton about the necessary dialogue between archaeology and the close reading of literary sources, which is sometimes rather underestimated in current approaches highly focused on material culture; literature is instead dismissed as a kind of negligible ideological upper-class testimony.

Burton’s conclusion on the crisis of the ‘Romanisation’ paradigm is provocative. There, he recognises that even if the attitudes of local elites, non-

<sup>12</sup> I am thinking of the controversial monograph of Bénabou (1976), but also of the debate that followed with Thébert (1978), or Leveau (1978), in a dossier published in the influential French journal *Annales: Économies, Sociétés, Civilisations*, in which the Roman imperial presence in Africa was at the forefront. Again, it is clear that the Anglophone bias of the collection in which *Roman Imperialism* was published ends up inducing certain historiographic bias, but it is sometimes perceived that this short-sighted direction leads to small distortions in the appreciation of current scholarship outside the English-speaking world. For instance, in the section on ‘Frontier Studies’ (78–83), otherwise very well thought out, C. R. Whittaker’s book (Whittaker (1994) is placed after those of Isaac and Millar, even though it is recognised that Whittaker’s appeared first in French five years earlier, as the product of four lectures delivered at the Collège de France in 1987. Isaac (1990) 439, for his part, admits not having been able to consult Whittaker’s French edition, and so does Millar (2001). Most notably, Burton reveals his Anglophone bias when he attributes the inquiry about ‘the persistence of indigenous elements in art, architecture, and religious practices as a form of protest’ (88) to Anglophone studies of the 1990s, even though it is one of the features already studied by Bénabou in the mid-1970s in his very polemical book.

local elite groups, and conquerors can be apprehended through a notion of strategy, agency, and adaptive strategy, that agency at the same time was strongly ‘limited’, due to the lack of ‘real’ available options in front of the ‘reproducing imperial authority and power’ (90), or, that is, ‘Roman-ness (or some version of it) was reproduced and reinforced ... through the pragmatic workings of hierarchy and deference’ (89). On the one hand, it is an important observation, because it invites the reader not to forget that there is an empire, a political framework, with its power structures and its social model, even in its diversity, which strongly limits adaptive strategies and modes of integration of the elites and non-elite groups. In this respect, Burton’s argument is clear and sound.<sup>13</sup>

Some doubts may arise, on the other hand, when Burton refuses to question what we understand by ‘Roman-ness’, and to what extent it is the result of multiple contacts, as a product of the dynamic process of circulation of people, things, ideas, and technologies through the Mediterranean sea and beyond, which precede the very process of Roman imperial conquest. The Roman Empire only catalysed growing Mediterranean connectivity and globalisation in the making since at least the first Iron Age (after the big material collapse of palatial societies during the Late Bronze Age, *c.* 1200 BC).<sup>14</sup> Without its relational dimension, culture becomes an unintelligible phenomenon. There were multiple spaces of contact and exchange open in the empire, of the ‘middle ground’ type, prior even to the military and political conquest.<sup>15</sup> Therefore, there was neither such thing as a ‘pure’ Roman culture, which had absorbed and re-signified multiple elements since its expansion in Italy, nor any kind of closed cultural package imposed on local elites, and non-elite groups, but a really creative model of cross-cultural fertilisation.<sup>16</sup> The resulting multiplicity, within a political structure and system of marked social

<sup>13</sup> Although necessarily moving away from ‘totalising’ schemes of what ‘becoming Roman’ might mean, in a world in which ‘systems of power and authority, along with the opportunism engendered by the Roman empire’s substantial reliance on local states and groups’ really mattered: Dench (2018) 157.

<sup>14</sup> Pitts and Versluys (2015).

<sup>15</sup> On the concept of ‘middle ground’ see Gosden (2008).

<sup>16</sup> As was seen by Wallace-Hadrill (2008) 13, in his monumental and influential work, where he defends a cardiac model in which cultural change in the Roman empire is brought about. Wallace-Hadrill understands Rome as a big heart with two phases: first, the diastolic phase pumping ideas around the empire (i.e., ‘Hellenisation’), and, in second place, the systolic phase as ‘oxygenated ideas’ pumped back (‘Romanisation’): Wallace-Hadrill (2008) 27. Adopting the proposal of Gosden (2008), esp. 125–35: “The Roman empire becomes a great “middle ground”, not simply of the “Roman” versus the “other” (in various degrees assimilated to Rome), but an enormous multi-sided exchange across a vast territory, in which “influences came from everywhere and flowed to everywhere”. The absence of Wallace-Hadrill’s book in the final bibliography is very surprising.

hierarchies, can give a particular meaning to the idea of various glocalisations within a wider globalisation, or ‘Romanisation’ (with a full understanding of the historical issues attached to the use of this term), but an understanding also that in the centre of all this is the complexity of earlier entangled phenomena.

In the end, these objections amount to little more than quibbles. I would like to emphasise that Burton has written a useful and thoughtful little book, which, in a few pages, allows the modern reader to have a clear overview of the main issues relating to Roman imperialism. Without a doubt, he has undertaken a challenging task. The scale and complexity of earlier historiographical discussions could have made any scholar give up, but the result in general terms is effective. *Roman Imperialism* presents real rewards for its readers. Unlike other texts, more orientated towards an audience of undergraduate students,<sup>17</sup> Burton proposes his own interpretations on the basis of a critical discussion of earlier historical perspectives. In addition, and I believe that this is the strongest point of the book under review, it establishes a dynamic dialogue between ancient history and various theoretical approaches to understanding the dynamics of interstate conflict and imperial formation. Undoubtedly, this has great potential when thinking about the historical objective of Roman imperialism from another perspective, but also in proposing comparative approaches to other empires, both ancient and modern.<sup>18</sup> However, these great advantages are accompanied by some minor weaknesses, mainly related to the Anglophone bias of the book, in which important French, Italian, and German scholarship is lacking, and last, but not least, to the superficial treatment of some central problems (as it happens, in the case of ‘Romanisation’, or the economic dimension of Roman Republican expansion). Of course, all these remarks should not obscure the fact that *Roman Imperialism* is a valuable addition to the scholarship on the topic, which every scholar will be obliged to take into account when deciding to immerse himself in the study of this fascinating, difficult, and controversial historical topic.

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<sup>17</sup> Erskine (2010); Champion (2007). These books add maps, tables, and above all a selection of literary and epigraphical sources in translation (not needed in the case of Burton’s book).

<sup>18</sup> This line of studies, directly linked to the challenge posed by M. Detienne of ‘comparing the incomparable’ has been very fruitful during the last two decades, notably through the comparison between the ancient Roman and Chinese empires: cf. Scheidel (2009). Regrettably Burton does not express a view on this important front of investigation.

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