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What was the Roman empire?

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EMMA DENCH, *EMPIRE AND POLITICAL CULTURES IN THE ROMAN WORLD* (Key Themes in Ancient History; Cambridge University Press 2018). Pp. xv + 207, 5 figs. ISBN 978-0-521-00901-0 (pbk.). \$27.99.

“Key Themes” has at last reached empire. The series “Key Themes in Ancient History” commenced in 1992 with I. Morris’ *Death ritual and social structure in classical antiquity*; now E. Dench’s *Empire and political cultures in the Roman world* is the 30th volume. The series continues to provide superb and intelligent surveys of both key issues and historical literatures. Even in this context, Dench has set herself a remarkable brief, no less than “how can we understand the local experience of change attendant on empire in the Roman world” (1). Writing after a generation of revolutionary work in both empire studies writ large and in the study of ancient empire more particularly, the material available for study is vast. In what follows, I first trace the author’s delimitation of her topic and survey the book’s achievement chapter by chapter, with some commentary along the way. At the close I will raise some more broadly thematic and theoretical considerations.

The introduction seeks to survey with extraordinary compression 100 years of literature on the effects of empire, commencing with F. Haverfield and ending with G. D. Woolf and myself.¹ It is a pleasure to read Dench’s remarks on Haverfield’s revolutionary work. Whatever one might say in the early 21st c. about his interpretative and political commitments — and Dench offers highly perceptive criticisms of these —, Haverfield made it impossible to study provincial history responsibly without giving intense regard to material culture and the questions of agency and localism that it must raise.² But, despite Haverfield, Dench detects in mainstream scholarship in the mid-20th c. two underlying commitments (16): the first separates Roman history into a period of violence and conquest under the Republic and one of government under the “empire”; the second locates politics — and agency — in the metropole, with culture (if anything) in the provinces. These commitments were made unsustainable by a body of work in the 1970-80s that upended views of Rome as purposive and agentic and recovered a substantial view of both local politics in itself and the contribution made by provincial individuals and institutions to the directions and actions of Roman government. Paired with this transformation was another, which made a powerful claim for the historical importance of ideological systems, particularly in provincial contexts, as they served to mediate and naturalize Roman domination in local terms. The late F. G. B. Millar serves for Dench as the exemplar of this first transformation, while K. Hopkins and S. R. F. Price stand for the second.³

Dench caps her condensed survey by describing Woolf’s and my own work as

tak[ing] forward the focus on empire as a belief system and as continu[ing] to complicate the idea of the bipartite model that distributes power and agency neatly along the lines of ‘Romans’ and ‘subjects’ or ‘natives’ (12; see also 33-35).

1 The works in question are F. Haverfield, *The romanization of Roman Britain* (3rd edn., Oxford 1915); G. Woolf, *Becoming Roman: the origins of provincial civilization in Gaul* (Cambridge 1998); and C. Ando, *Imperial ideology and provincial loyalty in the Roman empire* (Berkeley, CA 2000).

2 Writing about Haverfield allows Dench the opportunity to remark (3-4) on the complex and contested issue of the politics of the term ‘Romanization’ and, for that matter, what sort of ancient politics it presumes. It might have been useful to observe, with J. H. C. Williams, that this anxiety in modern scholarship is largely restricted to Dutch and Anglophone scholarship — i.e., to persons writing in countries that were never meaningfully Romanized: J. H. C. Williams, “Roman intentions and Romanization: Republican northern Italy, c. 200-100 B.C.,” in S. Keay and N. Terrenato (edd.), *Italy and the West. Comparative issues in Romanization* (Oxford 2001) 91-101.

3 Dench cites F. G. B. Millar, *The emperor in the Roman world* (Ithaca, NY 1977); K. Hopkins, *Conquerors and slaves* (Cambridge 1978); and S. R. F. Price, *Rituals and power. The Roman imperial cult in Asia Minor* (Cambridge 1984).

There is, of course, a level on which such a taxonomy makes sense, and these books are usefully assimilated to each other. But it also seems to me that the particularity of these works and their distinctive traditions does not come across in Dench's account. The contemporary landscape strikes me as more variegated, and its explanatory potential more varied and capacious, than this taxonomy allows. My own *Imperial ideology and provincial loyalty* was surely inspired by the work of Millar and Hopkins, more perhaps than I then knew — I do affirm, like Hopkins, that “a society's illusions are often the most interesting and potent aspects of its reality”.⁴ But the form of that book's argument nodded not to (supposedly) shared beliefs in a Benedict Anderson kind of way;⁵ instead, it sought to enact a form of post-Marxist ideology and discourse critique, the point being that successful ideologies sustain forms of domination, while discourse critique exposes both how that happens and where persons within those matrices of power had room and capacity to negotiate — and what they surrendered through such participation.

It is more surprising that Dench declines to position Woolf in succession to Haverfield. To my mind, what Woolf achieves in *Becoming Roman* (at least, what Woolf achieves in this work that makes it distinctive and useful within such a taxonomy) is, as did Haverfield, the writing of a history of provincial culture on the basis of material evidence. What is more, Woolf sought to exploit several of the signal possibilities granted historical inquiry by such evidence: for one thing, he located material evidence within topographies of economic and social relations and mappings of state infrastructure; for another, the focus on largely domestic material culture allowed him to remove imperial politics — both agency and words — from the explanatory régime. A great deal of superb work on the spatial distribution of cultural forms has followed. By declining to understand Woolf in these terms, Dench does not afford herself the opportunity to cite or evaluate that scholarship.

The introduction closes by establishing the parameters of the book. Three statements in particular are worth quoting in full:

This book is a thematic treatment of change attendant on the Roman empire that can be broadly classified as political, with its chronological focus between the early reception of Macedonian dialects of conquest and empire ca. 300 BCE and the “high” empire of the second to third centuries CE. [16]

This book seeks to bridge the gaps between the Republican military political actions of “the Romans” and the “government of Empire” traditions, on the one hand, and between both of these traditions and the “provincial cultures” tradition. Qualifying “cultures” with “political” centers, I focus on articulations of statehood, peoplehood, and grouphood rather than on looser questions of cultural identity or mapping broader changes in material culture. [16]

Each of the four chapters that follow, “Territory,” “Wealth and Society,” “Force and Violence,” and “Time,” explores a theme that raises a fundamental set of primarily political questions about how and by what means the articulation of sovereignty, constitution, and self-direction (e.g. around the payment of taxes, minting coinage, setting boundaries, and articulating territory, keeping an army, or having a common calendar) on the part of states and other groups changed at the interface with the Roman imperial state. [17]

The close of the Introduction thus narrows the field of inquiry from “the local experience of change attendant on empire” (1) to areas of communal life that are denominated “political”. In this light it is more clear why Woolf is grouped under the rubric “empire-as-belief” and why a category is not created for Haverfield and Woolf under a banner such as “local culture as material culture”. That is to say, the landscape of empire studies to which Dench responds and which she wishes to clarify is restricted to those areas of scholarship that focus on politics and government, and Woolf must be situated within them.

4 Ando (supra n.1) xiii; see Hopkins (supra n.3) 198, quoted by Dench (11).

5 Benedict Arnold, *Imagined communities: reflections on the origin and spread of nationalism* (London 1983; rev. edn. 2006).

The programmatic remarks that close Dench's Introduction deserve scrutiny for two further reasons that may usefully be clarified before we turn to the argument of the book. These concern the issue of political centers, on the one hand, and the aspirations and powers of ancient states, on the other.

First, the focus on "political centers" prompts disquiet. The Roman empire was what we might term a republican empire: the metropole penetrated the territory and population of the empire largely by instrumentalizing select "political centers" as nodal points for the transmission and translation of imperial priorities into local forms of symbolic and social action. These centers were deemed sites of politics — they were *poleis* or *civitates* or *municipia* — and were allowed what Cicero termed the *imago rei publicae* (the form of public life). The vast array of populations and population centers in their hinterlands were deemed villages (or some regional equivalent); local life there was perforce non-political, by virtue of being quite literally non-poliadic. Importantly, the communities deemed "political centers" by the empire were encouraged to regulate public power, and to operate their law-making institutions, by democratic means: magistrates were elected; there were votes for laws. Domination was local, but it rested on democratic norms of legal and public legitimacy. My concern is as follows. To the extent that this description captures the operation of interest between the imperial center and the local élites with whom it collaborated in the project of empire, any focus on "political centers" risks reproducing within modern inquiry the modes of aesthetic, ethical and normative evaluation that naturalized and sustained the political régime we hope to study.

As regards the aspirations and powers of ancient states, it seems to me worthwhile to interrogate the conceptual underpinnings, and comparative and normative implications, of the various powers that Dench associates with imperial government ("payment of taxes, minting coinage, setting boundaries, and articulating territory, keeping an army, or having a common calendar"), as well as the political theoretical cast with which she cloaks them (they relate to "the articulation of sovereignty, constitution and self-direction"). To the powers of government that Dench enumerates one might add others; to my mind, essential are the writing and aspiration to enforcement of criminal law, and the provision of law-writing and law-applying institutions. States and empires — political communities or, one might say, political centers — make law, and they (claim to) resolve disputes. What is implied by the unsurprising and uncontroversial listing of such powers is the existence of a very loose — perhaps even unarticulated — agreement about the functions that states should fulfill and therefore the powers that they should possess. A major historical problem then becomes how we should account for this. By this I intend that we need to ask aloud the question of how such an agreement came into being; how, when and why any such consensus was contested; as well as how real the powers were, and what purposes were served, and effects achieved, by accidental or strategic ambiguation regarding the gap between aspiration and reality in the exercise of state powers. Some potential responses to these questions are transhistorical in nature. These might focus on the legacies of statehood from one generation of states and empires to the next, as well as the Hobbesian question of how badly civil societies at any given level and period desired states and contributed to summoning them into being.⁶ Where the ancient Mediterranean is concerned, consideration of these questions must take account of the universality and near-exclusivity of empire as trans-regional political form, and the effects of this on local conceptions of power and the practices of the subordinate polities whom the empire instrumentalized to its own ends.⁷ Other interrogations of Dench's thoroughly uncontroversial list of state powers might

6 S. Richardson, "Early Mesopotamia: the presumptive state," *Past & Present* 215 (2012) 3-49; "Mesopotamian political history: the perversities," *J. Anc. Near Eastern Hist.* 1 (2014) 61-93; and "Before things worked: a 'low-power' model of early Mesopotamia," in C. Ando and S. Richardson (edd.), *Ancient states and infrastructural power. Europe, Asia and America* (Philadelphia, PA 2017) 17-62. Dench considers the possibility of comparative and historical study of forms of Mediterranean empire, but it remains a road not taken (21).

7 C. Ando, "Colonialism, colonization: Roman perspectives," in D. L. Selden and P. Vasunia (edd.),

drill down into a single context, using an array of theoretical literatures. To what extent does homeomorphy in the institutional and infrastructural elaboration of states, including empires and their enemies, as well as empires and their subordinates, result from the raw dynamics of power? The forms taken by resistance, as well as enforced collaboration, are often mimetic of the forms taken by the power that one resists.⁸ How powerful are network effects, or considerations of so-called efficiency, in the propagation and reduplication of standards, measures, and instruments — coinage, law, language, symbolic systems — across regions and within empires?⁹

These questions are provoked not simply by the programmatic claims made by the Introduction on behalf of Dench's project, but also by the very empirical richness on display in the chapters that constitute the body of the work. The less we take the Roman empire for granted, the more urgently we need to understand its reach, nature and impact in comparative and historical terms, and the greater the imperative to subject it to normative scrutiny.

Let me now offer an overview of the work, chapter by chapter. Chapter 1, "Toward a Roman dialect of empire," raises with tantalizing speed an array of topics: the emergence at Rome of the project of empire in a world of empires; the relationship between soft power — the example is the language of "friendship" among peoples and between peoples and kings — and hard power in ancient diplomacy; the use made by Rome of institutions inherited from polities it had conquered; the modern topics of the "creation of provincial landscapes" and the reception and reproduction in local contexts of "the rituals, performance, or iconography associated with the power of the Roman state when this demonstrably has nothing to do with loyalty to Rome" (36). There is much here that is excellent, not least a caution as regards "closing the gap within imperial contexts" between feelings, beliefs and material culture (33). But even under the constraints imposed by the series' compression, there are, it seems to me, important gaps, having to do with evidence, bibliography and argument.¹⁰ Where data are concerned, the evidence of exploitation of pre-existing institutions is much more vast than the use made by Verres of the palace of Hieron II (26); the example seems to me to foreclose consideration of the important political consequences of what Hobbes would term "sovereignty by acquisition". Similarly, where the refashioning of local government is concerned, why focus on colonial and municipal charters generated at Rome instead of the remarkable array of material demonstrating local acts of self-fashioning in this regard (39)? Finally, and most importantly, the empiricism of the section on "reproducing Roman power" is not met, it seems to me, by adequate explanatory ambition. Why were symbolic forms associated with Roman power used in local contexts? What does it mean to say that such use in any given instance "demonstrably had nothing to do with loyalty to Rome?" What meanings might such use have? Elsewhere I have suggested that the symbolic languages of Roman power and the forms of Roman institutions became archetypal in many systems of Mediterranean thought, such that, for example, new claims to local or trans-regional power — even claims oppositional to Rome — were made in Roman terms, or Roman legal forms carried on even after the institutions that might have vindicated their claims on

The Oxford handbook of literatures of the Roman Empire (Oxford Handbooks Online, 2016, available at DOI: 10.1093/oxfordhb/9780199699445.013.4).

8 C. Ando, *Imperial Rome AD 193 to 284. The critical century* (Edinburgh 2012) 176-200 and 224-29; "Territoriality and infrastructural power in ancient Rome," in id. and Richardson (supra n.6) 115-48.

9 D. S. Grewal, *Network power: the social dynamics of globalization* (New Haven, CT 2008).

10 It would be invidious (and in any event impossible) to offer systematic supplements to what is an impressive bibliography. At this juncture let me simply cite, on the historical trajectory of soft power, two essays on Roman treaties: J.-L. Ferrary, "Traité et domination romaine dans le monde hellénique," in L. Canfora, M. Liverani and C. Zaccagnini (edd.), *I trattati nel mondo antico* (Rome 1990) 217-35; P. Sanchez, "'On a souvent besoin d'un plus petit que soi': le rôle des alliés de moindre importance dans la construction de l'Empire romain au II^e siècle av. J.-C.," *Cah. Glotz* 20 (2009) 233-47. On the symbolics and ceremonial of Roman power, the essays collected in A. Alföldi, *Die monarchische Repräsentation im römischen Kaiserreiche* (Darmstadt 1970) have no rival.

statal authority had ceased to exist.¹¹ Some explanation for why this happened and why it mattered, beyond the observation that “it has taken on a life of its own”, seems to be required (45).

Chapter 2 (“Territory”) considers two topics: the history of ideas and practices that comprised the notion of a frontier under Rome (“Frontiers”); and the Roman promotion of urbanism through the direct foundation of colonies in Italy, with brief reference to Spain (“Rome and the city”). This chapter is the least satisfying. For one thing, the ambitions of the Roman state changed profoundly over time: having planted colonies in strategic locations, and having absorbed several relatively coherent territorial units (especially islands), it came to conceive itself as a territorial empire, and to aspire to know and govern the totality of persons and territory within the frontiers. How this project was conceived and enacted is a major problem in the study of Roman power and the comparative history of the state, but the notion of “territory” on view here does not demand its treatment. As a related issue, this effort to backfill Roman control in the territories within the farthest reach of Roman arms (to put the matter crudely) required Rome to confront areas where neither by nook nor by crook could Rome foster the growth of Mediterranean-style city-states by which to extend its power: Cilicia, the Alps, Liguria, Pontus, the Beqa’a, great stretches of *Tingitana*. It is hard to see how “Rome and the city”, or a history focused on “political centers” more generally, could take the history of these regions on board. What might have been done to advance understanding of “the local experience of change attendant on empire” within the categories on view in this work is to study the sustaining or transformation of Roman cultural forms among implanted communities of Romans — in colonies — in landscapes of foreignness: how “Roman” did Roman colonies remain in the generations after their foundation? How did the foundation of a Roman colony affect the currency of particular symbolic and cultural forms, and trends in material culture, in surrounding regions?¹²

The ambitious chapter 3 (“Wealth and society”) sweeps from an overview of the economic rôle of the Roman state within the landscape of empire, including consideration of the relative weight of financial interests in the conquest phase, to the possibilities for profiteering by members of the empire’s administrative apparatus (under both Republic and Principate), to the rôle of census and the imperial coinage in local economies. Dench then inquires into Rome’s commitment to democracy, at home and abroad; this leads to brief reflection, following A. Heller, on the social effects of the spread of Roman-style councils in Greek cities, and on the emergence of practices of more-highly-elaborated social differentiation at public events, not least in seating in the theater. Here, and in the final section on *collegia*, Dench is well-attuned to the multiple commitments and “loci of power” that local élites negotiated as they sought to remain élite in highly particularized contexts.

Chapter 4 (“Force and violence”) pursues a set of interlocking themes related to the staffing, financing and functions of the Roman army. Regarding staffing, Dench (125-26) traces the

11 Ando (supra n.8) 17 and 228-29; see also id. (supra n.1) 209-12 and 268-69 and elsewhere, drawing directly on C. Geertz, *The interpretation of cultures* (New York 1973), but I have elsewhere discussed the importance in this regard of M. Black, *Models and metaphor* (Ithaca, NY 1962). Antecedents in ancient history for arguments of this kind include Price (supra n.3), which Dench does discuss, but also the remarkable essays by R. Gordon in M. Beard and J. North (edd.), *Pagan priests: religion and power in the ancient world* (Ithaca, NY 1990): “From Republic to Principate: priesthood, religion and ideology”; “The veil of power: emperors, sacrifices and benefactors”; and “Religion in the Roman empire: the civic compromise and its limits”.

12 As regards life within colonial foundations, see, e.g., B. Levick, *Roman colonies in southern Asia Minor* (Oxford 1967) and C. Brélaz, *Philippes, colonie romaine d’Orient: recherches d’histoire institutionnelle et sociale* (Athens 2018). On colonies as nodal points in the transmission of metropolitan fashions, see J. Edmondson, “Family relations in Roman Lusitania: social change in a Roman province,” in M. George (ed.), *The Roman family in the empire: Rome, Italy, and beyond* (Oxford 2005) 183-230; id., “The virginity of the soldier Zosimus and other family myths: terms of affection within and beyond the family at Augusta Emerita,” in J.-G. Gorges et al. (edd.), *Lusitânia romana — entre o mito e a realidade* (Cascais 2009) 249-79.

practices associated with particular phases of imperial power, in which Rome once recruited its own citizens and tithed its allies for specific numbers, but later turned to the system of auxiliaries drawn from provincial populations. Interrelated and interwoven to this tale is one concerning the rôles of violence and command in ideologies of politics and rule: the transition to monarchy furthered a developing set of messages that portrayed imperial violence as directed at outsiders, in benefit to provincials who need only pay for protection, while Romans were assured that the monarch's singular control over the military stood between them and civil war. In writing of the army's rôle in civilian life, Dench says too little, it seems to me, about the extent to which patterns of intermarriage between soldiers and local women could be highly localized, and underplays the extent to which religious practice bound soldiers together across great distances (*contra*, 128) — hence attesting the remarkable communicative apparatus and heightened mobility that made army a total institution or society.¹³ But although Dench cites C. Fuhrmann's work, she neglects to discuss the rôle of the army in the provision of violence in what we might term policing:¹⁴ when she asks "what soldiers typically did beyond warfare" (121), she identifies the military as simply the most highly-organized and ready-made workforce in the empire. What is more, in a work on "the local experience of change attendant on empire in the Roman world", we might well have expected some reflection on how, when and where local communities retained their own capacities for policing, and the statal use of force, not to mention some accounting of how rituals of manhood changed as (male) citizenship was divorced from the duties of soldiery.¹⁵

Chapter 5 ("Time") is the best in the book and a pleasure to read. It is capacious in its conception of its topic as the author moves from provincial negotiations over synchronization around a new year that commenced with the birthday of Augustus, to the influence of the Julian calendar, to the paradoxical specificity and (brief) portability of the Roman calendar as document, to the way in which calendars shape historical memory, and to the functions of memory in creating communities through establishing possibilities for inclusion and exclusion of persons and narratives. Dench also gestures to comparative studies, citing trends in the imposition of calendars and the superimposition of imperial time in the Near East and China. She indicates an openness to multiple forms of causation, acknowledging the efficiencies that follow on standardization, while tracing multiple forms of political and cultural effects.

The epilogue ("Becoming Roman?") offers two observations on which I would like to comment before turning to broader reflections inspired by this work. First, Dench cautions us not to be seduced by those manifestations of power that led Gibbon to denominate Rome "a polite and powerful empire":¹⁶

Recognizing that, even at the height of empire, Roman power was thinly stretched, coexisted with competing systems of power and authority, and fostered opportunism on the part of local states and groups, should encourage us to reintegrate within the appraisal of empire as

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- 13 A. Collar, "Military networks and the cult of Jupiter Dolichenus," in E. Winter (ed.), *Von Kummuh nach Telouch. Historische und archäologische Untersuchungen in Kommagene* (Bonn 2011) 217-45. The argument is further contextualized in ead., *Religious networks in the Roman empire* (Cambridge 2013) 40-78. On social solidarity within military society, see, e.g., the specialized study of the *beneficiarii*, drawing on the remarkable epigraphic material at Osterburken, by J. Nelis-Clément, *Les beneficiarii: militaires et administrateurs au service de l'Empire (I^{er} s. a.C. – VI^{ème} s. p.C.)* (Bordeaux 2000). On local patterns of intermarriage, see D. Cherry, "Marriage and acculturation in Roman Algeria," *CPh* 92 (1997) 71-83.
- 14 C. J. Fuhrmann, *Policing the Roman empire: soldiers, administration, and public order* (New York 2012).
- 15 Local capacities for policing: C. Brélaz, *La sécurité publique en Asie Mineure sous le Principat (I^{er} – III^e s. ap. J.-C.)*. *Institutions municipales et institutions impériales dans l'Orient romain* (Basel 2005).
- 16 E. Gibbon, *The history of the decline and fall of the Roman Empire* (ed. D. Womersley; London 1994) vol. 1, 70: "And yet, even the majestic ruins that are still scattered over Italy and the provinces would be sufficient to prove that those countries were once the seat of a polite and powerful empire". Womersley's edition reproduces the last text that Gibbon himself approved, in an effort to remove more than 200 years' worth of editorial interventions.

a system the conditions most clearly visible in the making of empire, at its edges and in times of crisis. (157)

What Dench seems to call for is a more robust inquiry not into state power but state weakness. I have argued that the Roman state not only penetrated more widely across its territory and more deeply through its population than did earlier states, but that it developed a notable conceptual apparatus for articulating this ambition; I have further urged on these grounds that the High Roman Empire constituted a notable moment in the history of governmentality. But Dench (and others) are right to push the pendulum back. Second, Dench urges (159) that scholars understand the Roman empire not to have “worked” through a totalizing shift in perspective on the part of persons — through their having “become Roman” — but via the delegation of functions to, or the assumption of functions by, local agencies and institutions. This is surely correct. A version of this argument is constituent of the claim I made earlier in this review on behalf of classifying Rome as a republican empire. The arguments with which the book closes — concerning time, weakness and localism — come together in a peculiarly ancient nexus in the Late Empire. At that time, more or less all residents of the empire were Roman, and they might have assumed that their shared juridical status had entailments at the level of culture. But the conditions of mobility and communication in an ancient technology régime meant that such assumptions were never going to be cashed out. Even at its most unified, the actual diversity of the empire’s forms of daily life was functionally invisible. In this same period, the imperial government appears to have devoted notable resources to organizing political ceremonies such that they occurred (or were advertised as occurring) at the same time. By this means, the empire surmounted through simultaneity in time the geographic dispersal of its community in space.¹⁷

In closing, I turn to three additional issues that have to do with historical change; the historiography of localism; and the value of comparison and related acts of theory.

Perhaps by reason of constraint of form, *Empire and political cultures* devotes surprisingly little time to the description and analysis of change. By this, I intend two things. For one, the arguments in the big thematic units tend to unfold by first describing structures and operations of the Roman state and subsequently describing local conditions in the aftermath. A ‘before’ picture might be implied by the ‘after’, but we almost never observe a locality — a case-study — in its pre-Roman particularity and trace the dynamics of social and material conditions into some post-conquest phase. In consequence, it is not that change is not observed; it is that differences within reactions to Roman rule are often ironed out and not explained.

My second concern with regard to historical difference concerns change at Rome itself. I have emphasized that Dench herself challenges, rather than respects, a distinction between a Republican period of conquest and an “Imperial” period of government. But empire was not merely something that Rome inflicted on others. In multiple ways, the project of empire distorted the conditions of possibility of politics at Rome. The Social War, the collapse of the Republic, and the recursive imposition of imperial forms of domination on the Roman people itself were three fruits of this process. These themes were central to the extraordinary work of A. J. Toynbee in *Hannibal’s Legacy*. When Dench established the start of her period with “the early reception of Macedonian dialects of conquest and empire”, I expected some discussion of his work (perhaps especially at 16 and 30). It deserves consideration in any volume of this kind.¹⁸

17 On the actual diversity of forms of daily life in the unified population, see my “Making Romans: democracy and social differentiation under Rome,” in M. Lavan, R. E. Payne and J. Weisweiler (edd.), *Cosmopolitanism and empire. Universal rulers, local elites and cultural integration in the ancient Near East and Mediterranean* (Oxford 2016) 169-85. On simultaneity and politics, see my “Triumph in the decentralized empire,” in J. Wienand and F. Goldbeck (edd.), *Der römische Triumph in Prinzipat und Spätantike* (Berlin 2016) 397-417.

18 A. J. Toynbee, *Hannibal’s legacy. The Hannibalic War’s effects on Roman life* (Oxford 1965); C. Ando, “Hannibal’s legacy: sovereignty and territoriality in Republican Rome,” in K.-J. Hölkeskamp,

As regards the historiography of localism, it would have been helpful to have some account from Dench herself of what she regards as the various ways in which local political cultures might be distinct from one another. One might construct such a list with respect to structural features that are of interest to us: e.g., neo-Aristotelian political form; political economics between empire, city-state and those denominated non-political; ecology; and so on. One might also construct a merely ostensive list of the primary features of local cultures that were visible and salient to ancients: cuisine, language, titles and forms of magisterial power, legislative authority, jurisdiction. Consideration ought to be given to matters such as these because, in the conditions of ancient empire — which is to say, given the limited infrastructural reach and profound technological limitations of imperial powers —, it should be possible to identify patterns in local conditions that issued in patterned local impacts of imperial power.

Finally, it would have been helpful for Dench to lay her own theoretical commitments on the table. Empire studies writ large, as well as scholarship on individual ancient empires more narrowly, are enormously diverse fields. Our understanding of “empire and political cultures in the Roman world” can only be enhanced by a clear-sighted account of what “empire and political cultures” looked like in some other world. What are the analytic axes along which others have studied Dench’s topic, as it were, in other contexts? If one assesses the experience of local change under Inca or Aztec rule differently than we do the experience of local change under Roman rule, does this have to do with the particularities and technologies of Roman and Inca power? Or the interface of imperial power and local symbolic systems? Or problems of ecology and terrain? Perhaps pre-modern imperial power is everywhere the same, but evidentiary régimes differ? One would like to know.

These questions are raised by Dench’s work because of its very richness of documentation and generosity of spirit. It deserves careful reading and passionate engagement.

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