unaltered: if circumstances called for action, measures were taken, but this action was not structurally different from before. This part establishes the baseline against which the Italic case is evaluated in Part II.

Arguably the richest textual source for Italic religious ritual is formed by the Iguvine tablets, but they have equally often been discussed by conflating it with Roman evidence. L. builds a good case against this tendency, and underlines many particularities of the religious ritual at Gubbio compared to Rome: for instance the absence of the long Catonian *praefatio*, the interspersing of the *mola salsa* after the killing (rather than before it), and the emphasis on repetition, silence and murmuring. In other respects, however, L.'s emphasis on differences between Italic communities and Rome does not always seem warranted. For instance in the discussion (207–10) on oaths and in particular the Samnite oath at Aquilonia. If historical, this is certainly an extraordinary ritual and aimed at the élite legion only. Moreover, de Cazanove has recently argued that the described place refers to a Roman military camp, not a sanctuary. The same goes for the archaeological evidence. The set of sanctuaries that is regularly cited reflects a thoughtful and deliberate choice to include different types of Italic cult places; but to use the same sample to argue that the architecture of Italic cult places is varied overlooks neat regional patterns. However, this does not affect L.'s main conclusion that Italic, including Roman, religious configurations should be understood as homologous, not identical systems which could operate autonomously.

The last part, 'Vers une nouvelle harmonie religieuse?', seeks to investigate to what extent Roman religious patterns became a model for the rest of the peninsula, and the rôle of Hellenistic influences in the process. On the whole, L. follows recent downplaying of direct Roman intervention (not all arguments are beyond discussion, e.g. the definition of *tota Italia*, 273). The discussion on the spread of anatomical votive terracottas, often seen as indicators of Roman expansion (275–9), should now be read along with the criticisms of M. D. Gentili and especially F. Glinister. At the same time, this reviewer's analysis of the 'precocious romanization' of the Marsi might actually support L.'s case, that 'la romanisation des dieux et des pratiques ne précède pas l'établissement des lois ni l'octroi de la citoyenneté romaine' (272). In his conclusions, L. justly argues that the adoption of Hellenistic elements in both sacred performances and architecture should be understood as a locally-driven and conscious choice. Whether religious ritual remained basically unchanged cannot, however, be established on the basis of the evidence presented: 'changing to remain the same' is itself a form of change.

In the end, the largely text-based *Variations rituelles* is more successful in showing the homology of Italic (including Rome) broader religious *patterns* than in tracing cultural convergence or other diachronic or geographic developments in religious *ritual* as such. The building of this framework is an important accomplishment, although the significance of inter-Italic dynamics risks being minimized in this dual structure. Whereas the main strength of the book lies in discussion of ritual texts, little archaeology is used, and discussion of it tends to be less informed (for instance at 281, where two different sanctuaries with similar developments are noted at Casalbore and Macchia Porcara> Casalbore, loc. Macchia Porcara is one sanctuary; 297: Matese> Majella; the choice of Tricarico at 282 to illustrate 'la persistante vitalité religieuse des Osco-Umbriens' is unfortunate: this is a very exceptional and complex site). A major challenge now is therefore to reconstruct precise ritual actions using archaeological evidence to test, complement and refine the framework.

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A. BOWMAN and A. WILSON (EDS), *QUANTIFYING THE ROMAN ECONOMY. METHODS AND PROBLEMS*. Oxford: Oxford University Press, 2009. Pp. xvii + 356, illus. ISBN 9780199562596. £79.00.

This is the first volume published by the Oxford Roman Economy Project (http://oxrep.class.ox.ac.uk), directed by A. Bowman and A. Wilson, and it sets out to present their research agenda and discuss the methodological problems involved. By 'collecting and analyzing quantifiable documentary and archaeological evidence' (12), the project is aiming to examine the performance of the Roman economy in four key 'diagnostic areas': demography and urbanization, agriculture, trade, and, finally, metal supply and coinage (6). For this first volume, the editors have invited a number of specialists to assess the challenges, pitfalls and possibilities of such an endeavour. Seventeen contributions have been divided into six sections, each dealing with various aspects of the four main areas of research selected by the programme for investigation. The reader is presented with up-to-date discussions of the potential and limitations involved in quantifying the Roman economy on the basis of diverse bodies of evidence, from field-survey data to ship-wrecks, papyri, the metallurgy of Roman coins and price records. The character of the volume is both exploratory and searching: many of the contributions have been written in response to a lead-paper in each section, and it is also very much evidence driven. A fundamental inspiration has clearly been the trends in archaeology, admirably reflected in Parker's justly famous catalogue of *Ancient Shipwrecks of the Mediterranean and Roman Provinces* (1992), seeking to produce larger bodies of data open to statistical analysis. The prospect is alluring: to provide the historian of the Roman economy with some of the time-series evidence so badly missed in our discussions. Such an effort systematically to compile large sets of data is clearly to be applauded and it will be interesting to follow the results as they become available on the homepage of the project and in subsequent publications.

But from reading the volume, not least the contributions of the editors themselves, it also very quickly becomes clear that many problems conspire to make the rewards to be hoped for from the colossal effort required rather less than something akin to the time-series familiar from more recent periods of European economic history. Parker's shipwreck statistics serve as the emblematic illustration. Though based on vast numbers of wrecks, it is clear that the material is skewed in many ways, e.g. with cargoes of amphorae being heavily over-represented due to their higher visibility on the sea bed to archaeologist divers. Few today would wish to do Roman economic history without access to Parker's invaluable catalogue; on the other hand, it seems clear that one cannot really hope substantially to overcome these basic limitations in the evidence. Find patterns of amphorae and pot shards do not easily or unproblematically translate into trade or population statistics, as Mattingly observes (164). Wilson, for instance, suggests that barrels became increasingly important in Mediterranean transport of goods from the second century A.D. But these wooden containers go virtually undetected in the archaeological record and the declining number of shipwrecks particularly from the third century may be explained by changes in archaeological visibility rather than significant reduction in the level of sea-borne trade (219–21). To what extent, though, remains open to conjecture. This fundamental uncertainty, however, would seem to contradict one of Wilson's main programmatic statements: 'what we need to compare is like with like … a study covering the archaeology of say, 1200 to 1800, to facilitate comparison with the Roman world and see where in this period different facets of the traded economy might return to Roman levels' (244–5). The expectation that an exclusive focus on archaeological evidence should enable us to produce time-series which would facilitate direct comparison of levels of trade across the entire span of pre-industrial European history seems too optimistic. If the archaeological record is not even consistent within Roman history in terms of visibility, how can we then expect the opposite to be the case for this much longer stretch of history where the effects of changes in types of goods and technology in combination with varying patterns of archaeological interest and activity must have exercised a much more profound influence on the shape of our archaeological record. Moreover, simply looking at quantitative levels would miss the enormous structural changes in patterns of long-distance trade between the periods, with the shift of the economic centre to north-western Europe from the Mediterranean during the late Middle Ages and the early modern period, as well as the global expansion of trade links to the Caribbean and south of Africa to the Indian Ocean.

In short, in spite of this commendable and extremely useful undertaking systematically to compile quantifiable evidence, firm ground is likely to continue to elude us and much must remain tentative. Under these circumstances, rigorous, often hypothetical, model building and use of comparative examples will both be crucial as checks on our efforts to analyse and quantify the Roman economy. After all, all data require theory to make sense of them; and thanks to the energetic efforts of Bowman and Wilson's team much more will require theoretical scrutiny over the coming years.

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