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URBAN MANUFACTURING IN ROME

HAWKINS (C.) *Roman Artisans and the Urban Economy*. Pp. xii + 307, figs, ills. Cambridge: Cambridge University Press, 2016. Cased, £64.99, US\$99.99. ISBN: 978-1-107-11544-6.

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In a pre-industrial economy, the scope and complexity of urban manufacturing are important for two reasons. The first is that they reflect an economy's purchasing power beyond subsistence: only prosperous societies can have a large manufacturing sector, particularly with regard to consumer goods (capital goods are a somewhat different matter). The second is that (in)efficiencies in manufacturing, whether technological or managerial, provide an important window on a society's economic performance. A crude summary of this book would be that the author is relatively pessimistic about the performance of the Roman urban economy. The book relies extensively on comparisons with medieval and early modern European urban economies. Here, H. is often well informed, but faces a difficult challenge. On the one hand his argument often relies on (very) elaborate discussion of comparative cases, and the logic of the argument is that the not very well-known Roman situation must have been similar. Conversely, the main line of his argument is that the Roman urban economy was different because it was less successful than the early modern one. Navigating between similarity and difference is a hard act. Moreover, many of his early modern examples are in fact from the period immediately before the Industrial Revolution. Here, he would also have done well to differentiate between England and the Netherlands on the one hand and other early modern urban economies on the other. The second strategic choice has been to ignore largely the archaeological evidence for the production and consumption of manufactured goods.

The book consists of four interlocking chapters that gradually work to its quite pessimistic conclusion. In the first chapter H. argues for the fundamental fragility of the demand for manufactured goods, creating great business vulnerability. Demand was fragile because it was subject to seasonal variability due to the seasonal nature of incomes from agriculture and due to unpredictable interannual fluctuations of incomes and hence of demand. This vulnerability was exacerbated by the generally low standard of living. Given the low income elasticity of demand for subsistence goods, demand for manufactured goods had to take up the variability of incomes. Here, H. clearly sides with pessimistic assessments of incomes of the mass of the population, glosses over alternative estimates and almost completely ignores the arguments from archaeology in favour of high levels of material culture enjoyed by quite large segments of the population in both Italy and the provinces (see W.M. Jongman, 'Re-constructing the Roman Economy', in L. Neal and J.G. Williamson [edd.], *The Cambridge History of Capitalism*, Vol. I [2014], pp. 75–100 for the counter-argument).

The second chapter then investigates the consequences for business strategies. The problem H. addresses here is that of the organisation of vertical business integration. The fragility of demand, he argues, made it unattractive to incur the substantial fixed costs of vertical integration within one firm. Instead, entrepreneurs opted for subcontracting, using professional *collegia* to reduce transaction costs. Their members would know their fellow *collegiati*, and deceiving these would incur a heavy reputation cost (see now also T. Terpstra, *Trading Communities in the Roman World: a Micro-Economic and Institutional Perspective* [2013]). Yet, H. views the success of these *collegia* as ultimately limited because of their supposed exclusivity compared to medieval guilds; he argues that women and slaves were rarely admitted. As for slaves, there are in fact quite a few

examples, but more importantly, successful slave craftsmen could expect to be manumitted, and freedmen are well represented in *collegia* and among known craftsmen. If anything, I would argue that the Roman case offered more upward mobility.

The third chapter, on manumission and labour hoarding, elaborates the implications of the idea of a fragile demand for urban manufactures by arguing that urban artisans had a big demand for flexible labour rather than the fixed supply represented by a permanent slave workforce. Where possible, they had to turn the fixed cost of investment in slaves into a variable cost. Hence, he argues, the not infrequent hiring out of slaves, either by their masters or by the slaves themselves working semi-independently while paying their masters a periodic sum. In addition, H. argues that manumission could also serve as a means to make labour supply more flexible, by hoarding the patron's entitlement to *operae* after manumission. Whether such *operae* were frequent remains contested, however (H. Mouritsen, *The Freedman in the Roman World* [2011]). There is no doubt that manumitted craftsmen continued to work for their former masters – the urban epigraphy is full of examples. Those inscriptions do not demonstrate that these freedmen had been manumitted for their *operae* rather than, for example, as an incentive for good behaviour (see e.g. G. Dari-Mattiacci, 'Slavery and Information', *The Journal of Economic History* 73 [2013], 79–116). Manumitted former slaves could just as well be a reliable source of trained wage labour.

The next step in H.'s argument is a discussion of the economics of slavery, where he argues that keeping slaves (and manumitting them and using their *operae* as hoarded labour) was far more expensive than wage labour. His explanation is high transaction costs in a badly functioning labour market. The problem is with the estimates that go into the equation, and with the wage estimate in particular. Outside Egypt we really have too few datapoints to feel at all confident, and even for Egypt the number of observations is pitiful, though showing a clearly rising trend in wages until the Antonine Plague (K. Harper, 'People, Plagues and Prices in the Roman World: the Evidence from Egypt', *The Journal of Economic History* 76 [2016], 803–39). A more conservative methodology would be to turn the argument around and use the large numbers of documented high slave prices to argue for a high wage level (see W.M. Jongman, 'The Early Roman Empire: Consumption', in W. Scheidel, I. Morris and R.P. Saller [edd.], *The Cambridge Economic History of the Greco-Roman World* [2007], pp. 601–2; E.D. Domar, 'The Causes of Slavery or Serfdom: a Hypothesis', *The Journal of Economic History* 30 [1970], 18–32 for the theory).

The final chapter takes the argument into the families of the craftsmen, by first arguing that many fathers seem to have arranged for their sons to serve as apprentices in other workshops either in the same trade or in another one. This intergenerational discontinuity is perhaps also evidenced by the pattern of commemoration in funeral inscriptions, although the evidence is painfully thin. Here, the argument is that the market for manufactured goods was too fragile to employ one's own sons. The final part of this chapter discusses the role of women. H. claims that Roman lower-class women would work more inside the household and less outside than their early modern counterparts, thus limiting the volume of market exchange. This may be true, but here the evidence for a proper comparison is extremely thin. The argument also misses the possibility that women may have worked in services rather than crafts. Looking at the archaeology of Pompeian houses, it is obvious that many workshops are an architectural part of elite homes. It is not hard to reconcile the prominence of a male workforce in the shops with domestic female servile labour in the elite household.

This is an important work that addresses some big issues. It shows a wide-ranging erudition and conceptual sophistication, with a welcome injection of comparative argument

and economic logic. At the same time, that also shows the work's weakness: for this reviewer's taste there is too much 'must have been' argument from historical comparison. Moreover, the archaeology of craft production receives some attention, but not nearly enough. Compared to the limited written evidence, the abundant and rapidly increasing archaeological evidence for the scale and technology of Roman manufacturing could have provided a useful antidote against H.'s pessimistic minimalism (cf. J.P. Brun, 'Techniques and Economies in the Ancient Mediterranean', Inaugural lecture, Collège de France [2012]).

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TRADE AROUND THE INDIAN OCEAN

COBB (M. A.) *Rome and the Indian Ocean Trade from Augustus to the Early Third Century CE.* (Mnemosyne Supplements 418.) Pp. x + 355, colour ill., maps. Leiden and Boston: Brill, 2018. Cased, €116, US\$140. ISBN: 978-90-04-37309-9.

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C. presents a comprehensive account of Indian Ocean trade during the Principate from a Roman perspective in nine main chapters (and a summary chapter) based on written (literary, epigraphic, papyrological) and archaeological evidence. Topics range from the origins of the monsoon to the ethnic identification of Yavanas and the role of the State, and are thoroughly presented with counterarguments outlined, followed by C.'s own interpretation. The text is clear and easy to read, the bibliography is diverse, and there is a useful index.

The introduction (Chapter 1) defines the chronological parameters, outlines the methodology, reviews the development of the subject and importantly, establishes the key themes to be addressed, comprising: development of trade; barter and bullion; the peak period of Roman trade; schedules, practicalities and Roman diasporas; and Indian Ocean goods and Roman society. C.'s aim is to 'provide a series of overreaching arguments and new perspectives and to challenge a number of long-standing theories' (p. 6).

Chapter 2, 'The Ptolemies and the Erythra Thalassa', is substantial and, unlike most treatments of the subject, provides a very welcome discussion of the pre-Roman trade. It questions whether Roman trade represented a new development resulting from the annexation of Egypt as a province or a continuation of Ptolemaic period activity. Reviewing evidence for Ptolemaic activity from the Egyptian Red Sea, South Arabia, East Africa and India (particularly from Arikamedu), C. argues for an increase in the volume of trade during the Roman period, which he sees as a logical development of the existing Ptolemaic trade. While this conclusion is sound, in my view he overstates the amount of exchange that can be identified archaeologically during the Ptolemaic period and overinterprets the chronology of some artefact types. For example, the amphora sequence at Arikamedu is key to his argument, citing in support a progression from Koan (the prototype of the Roman double-rod handled Dressel 2–4) to pseudo-Koan to Italian Dressel 2–4 amphora imports (p. 59). A misunderstanding of these categories sets up a false evolution: some