

## ECONOMICS OF RELIGION IN THE MYCENAEAN WORLD: RESOURCES DEDICATED TO RELIGION IN THE MYCENAEAN PALACE ECONOMY

BY LISA MARIE BENDALL. PP. XVI + 369, TABLES 72. OXFORD UNIVERSITY SCHOOL OF ARCHAEOLOGY, OXFORD 2007. \$80. ISBN 978-1-905905-02-7.

Bendall's *Economics of Religion in the Mycenaean World* is a dense, somewhat intimidating book, in particular if one is an archaeologist like this reviewer and not a Linear B specialist. But like many Mycenologists today, Bendall wants to bridge the gap between text and archaeology (xiii), and works hard in *Economics* to accommodate the nonspecialist reader. This review is written, therefore, from the perspective of an archaeologist who conducts fieldwork in Greece and who studies Mycenaean material culture but who is not a philologist; it is aimed at similar scholars who are trying to decide whether to commit to a lengthy and challenging, but ultimately rewarding, read.

*Economics* amplifies one chapter from Bendall's doctoral thesis (Cambridge 2000). It describes in exhaustive detail, based on strict criteria (ch. 2), the types of resources recorded in the Linear B documents that were destined for consumption in religious contexts. Religious text entries are those that (1) describe allocation of goods to ritual events, such as feasts (referred to throughout as "banquets"); (2) include unambiguous divine or sacerdotal names; or (3) indicate support for cult places or personnel, often by reassigning "ordinary" palatial resources. In chapters 3–6, Bendall compiles a catalogue of these entries, which allows her to measure (in Mycenaean units) the number and amounts of goods dedicated by the palace to religious purposes. The immediate goal of this exercise is to track palatial support for the religious sector—which was surprisingly weak—but the ultimate goal is to understand better the structure of the wider Mycenaean palatial economy and the priorities of its administrators.

In chapter 1, Bendall proposes (4–9), and in chapter 7 finally concludes (284, 292), that all Mycenaean palaces operated "command and control," "redistributive" economies (see also L.M. Bendall, "A Reconsideration of the Northeastern Building at Pylos: Evidence for a Mycenaean Redistributive Center," *AJA* 107 [2003] 181–232). Different palaces may have collected different kinds of goods in different amounts, for slightly different purposes, but they all employed redistribution as the means of acquiring these goods. Bendall's data indicate that some goods—a relatively small percentage of the total recorded—were acquired by the palaces and redistributed to priests, sanctuaries, and for festivals, but the majority was used in other ways. What exactly was done with all the excess material and finished items collected and manufactured by the palaces is rarely described in Linear B, and this is an outstanding, and fascinating, mystery: Were they buried with the dead? Shipped overseas? Gifted by palatial elites to loyal secondary elites? It is Bendall's tentative, sometimes provocative, answers to these questions (throughout and in particular in ch. 7) that form the meat of the book and make a long read worth the effort. That said, there are some potential problems with her application of theory and her interpretation of archaeological data.

Bendall's final conclusion—that Mycenaean palaces were redistributive centers—is unproblematic, but her ideas about political economy and the sources and extent of palatial "control" will confuse many archaeologists, especially those trained in North American departments of anthropology. Bendall argues

that Mycenaean palatial elite could not have fully controlled a state's territory and people but that the palace was, nevertheless, the dominant economic force in any Mycenaean region (8–9). Both of these assertions are debatable; some chiefdoms and archaic state systems did fully control territory and people—so, too, could the Mycenaean palaces, and if they were not in full control, there is no reason to believe they were the dominant economic force, in particular over the long term. What is potentially confusing here is that many archaeologists use Timothy Earle's discussions of redistribution as a reference point for understanding redistribution generally—and indeed Bendall does cite Earle's seminal paper on Hawaiian chiefdoms (in Earle and Ericson, eds., *Exchange Systems in Prehistory* [New York 1977]). But she scarcely cites any of his later, cross-cultural research on political economy, of the Inka state and Danish Bronze Age “warrior chiefs,” for example. She never specifically says that Mycenaean states functioned like Hawaiian complex chiefdoms, or that the Mycenaean *wanax* behaved like a Hawaiian chief, but since her only reference is to the paper by Earle, we must assume Hawaii is her primary theoretical model.

Hawaiian systems of redistribution were simple yet all encompassing; Hawaiian chiefs fully controlled Hawaiian territory and people, in large part through their control of all land and their ability to acquire agricultural surplus at will. Compared with Hawaii, the Mycenaeans employed a more complex and yet far less encompassing form of redistribution, based on limited systems of land tenure and selective taxation of those nonstaple materials needed for the manufacture of various prestige goods, such as textiles and perfumed oil. Staples, such as wheat, were grown on palatial lands in amounts necessary to support the palace and its retainers. Systems of mobilization like those of the Mycenaean palaces typically do not lead to, or require, widespread economic, and therefore overt sociopolitical, domination, as was the case in Hawaii, so why does Bendall cling to the idea that “the Mycenaean ‘palaces’ were the major [economic] players” (8 [emphasis original])? Archaeologists who study Mycenaean states from a cross-cultural, comparative perspective are usually struck by how little Mycenaean elites did control, and how precariously. In essence, Bendall treats the Mycenaean *wanax* like a Hawaiian chief, when in reality he appears to have run an elite household economy, one among many

in any given Mycenaean (secondary) state (see M.B. Cosmopoulos, “The Political Landscape of Mycenaean States: *A-pu<sub>2</sub>* and the Hither Province of Pylos,” *AJA* 110 [2006] 205–28, on excavations at Mycenaean Iklaina—probably Linear B *a-pu<sub>2</sub>*—near Pylos), all of which were networked, wealth financed, and probably highly competitive.

So, was the Mycenaean *wanax* at all like a Hawaiian chief? The Linear B tablets are not much help in answering this question; they are relatively mute when it comes to the precise mechanisms whereby raw materials were moved to the palace, and the disposition of most finished prestige goods. Archaeological data must be used to fill the gaps in the tablets, which is what Bendall essentially does. In fact, these data lead away from the Polynesian redistributive model, but this is not how Bendall interprets them. With regard to land tenure at Pylos, Bendall argues that, although the tablets only record palatial control of land in the hinterland of *pu-ro*, in fact the palace must have controlled large tracts of land elsewhere in the region (76). However, according to Earle (*How Chiefs Come to Power: The Political Economy in Prehistory* [Stanford 1997]), Hawaiian chiefs controlled commoner land through their control of irrigation systems. Mycenaean elites had no equivalent means of controlling commoner land, and probably did not do so. The Linear B gap in this case is probably a real gap. The question is not whether a Mycenaean palace could have controlled all land but whether this was necessary. The answer is no. Hawaiian chiefs operated systems of staple finance (Earle 1997), which required control over all land and all surplus agricultural production. Mycenaean states, especially on the mainland, did not require collection of agricultural surpluses to finance their operations, and so none was collected. So the Pylian “grain harvest” gap (261) is a real gap. If in fact the Mycenaeans were collecting huge wheat surpluses (as they appear to have done at Knossos), we would expect much larger storage facilities at palaces, which are generally missing (Gla is the exception that proves the rule). One interesting additional question is the degree to which the Mycenaean state may have tried to control industries outside their direct purview. For example, did Pylos control the regional ceramic system? Bendall says no (282–83)—thus the gap in the Linear B evidence—and I would generally agree; I reached a similar conclusion in my book *Nestor's Wine Cups* (Oxford 1999).

But she goes on to say that “[t]here is no reason to believe that any palace was interested in (or would benefit from) producing pottery for the entire polity it controlled” (282). This is just plain wrong. In fact, we can again cite Earle (D’Altroy and Earle, *CurrAnthr* 26 [1985] 187–206; see also D’Altroy and Bishop, *AmerAnt* 55 [1990] 120–38), this time with regard to the Inka empire, which did try to control pottery production entirely and probably largely succeeded. The proper question again is not whether a Mycenaean palace could have controlled pottery production but whether it was necessary.

Bendall attempts to fill additional gaps in Linear B based on the archaeological data (primarily in ch. 7), but the archaeology works against her conclusions. Her analysis indicates that at Pylos, of all manufactured goods, perfumed olive oil was most likely to be used as a religious offering, 71–100% of that recorded in the tablets (102). Bendall then argues that significant amounts of additional perfumed oil must have been produced at Pylos and disbursed but went unrecorded in Linear B (102–4, 138–39). This argument is based partly on a comparison of the oil tablets from Pylos and Knossos, the latter of which record both nonreligious and religious disbursements, and of which 12–46% were offerings (139). Why this discrepancy, and how was nonreligious perfumed oil used at Knossos and (if it existed) at Pylos? Bendall argues that excess perfume was traded (270–71). If so, the “final use” of oil for trade is not recorded in Linear B, nor is international trade in general, at Pylos or Knossos. We do have, however, the Linear B–inscribed stirrup jars that were traded throughout the Aegean and beyond. These jars held perfumed olive oil, so it is not unreasonable to assume that excess oil was indeed traded. All these jars are from Crete, though; none is from Pylos. In fact, of all the Mycenaean pottery chemically and petrographically characterized in the Eastern Mediterranean, none of it comes from Pylos. It seems unlikely, therefore, that Pylian perfumed oil entered the international market in large quantities, if at all. Another explanation would be, again, that the differences—the gaps—in the Linear B records at Pylos and

Knossos are real and meaningful, and point to two different economic systems. The Mycenaeans at Knossos could trade the bulk of their perfumed oil (and textiles, too), because their state was staple financed. Thus, as compared with mainland Mycenaean states such as Pylos, Knossos appears to have moved huge amounts of grain to the palace, displays a very different regional settlement system, employed a much larger number of “collectors,” and owned giant flocks of sheep. A staple-financed Mycenaean Knossos makes good sense, given that the earlier Minoan system used staple financing and was coopted by the Mycenaeans. The Pylian state was wealth financed. Perfumed oil, textiles, and other prestige items were consumed within the state itself. Much of the wealth went into elite graves, and some of it may have trickled down to the secondary elite. Whatever the case, once again the *wanax* is not much like a Hawaiian chief. In fact, he behaves a lot like the Danish chiefs Earle ([1997] 97–102) studied at Thy, whose power was weak and based almost entirely on access to, and control of, wealth items. Mycenaean-dominated Knossos is reminiscent of Inka-dominated Mantaro, where a subject population was forced to contribute to a staple-financed system that fed a specialized, highly managed, extraregional political economy (Earle [1997] 96–7).

What makes *Economics* an important piece of research is not Bendall’s interpretations of her data but the data itself. What allows Bendall’s interpretations, and mine above, is her brilliant ability to catalogue and clearly organize and present huge amounts of Linear B data. In this publication, she has done Aegean prehistory a huge service. We may disagree on the meanings of the data (and the gaps revealed therein), but anyone who reads Bendall’s book will agree on its value, meticulous attention to detail, and engaging reanalysis. I very much look forward to the next installment.

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