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Troy and Homer

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Abstract: This is a review of Joachim Latacz's book *Troy and Homer: Towards a Solution of an Old Mystery* (2004), focusing on the archaeological issues.

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*Troy and Homer: Towards a Solution of an Old Mystery.* By JOACHIM LATACZ. Oxford, UK: Oxford University Press, 2004. Pp. xix + 342. Cloth, \$45.00.

This is an excellent book by one of Europe's leading Homerists. In the 1840s, well before Schliemann set spade to soil at Hisarlik, George Grote suggested that while Homer's epics were excellent sources for the customs of eighth-century-BC Greece, we would never know whether the Trojan War he described really took place.<sup>1</sup> Schliemann and Dörpfeld shocked classicists out of this view, but since the 1980s positions like Grote's have returned to favor.<sup>2</sup> The Trojan War itself dropped out of historians' analyses of Homer, because it seemed that there was really very little we could say.

Joachim Latacz argues to the contrary that the late Manfred Korfmann's excavations at Troy since 1988 have changed the equation. Korfmann was the first archaeologist to explore Troy's lower town. Against those who insisted that Troy VI (c. 1700-1200 BC) was basically just a castle on a hill, inconsistent with Homer's account of windy Ilion, Korfmann concluded that the city covered some 20 hectares, with a population of 7,000-10,000 people. Its lower town was fortified, and its rulers grew rich by controlling trade between the Aegean and Black Seas. Korfmann insisted that there really was a Trojan War, although it was these trade routes, not the beautiful Helen, that the Achaeans came to seize.<sup>3</sup>

No excavation in the world generated such intense debate in the past fifteen years as Korfmann's. Only Troy could bring learned professors to fisticuffs at international congresses. L. takes his stand firmly on Korfmann's side, and focuses on explaining what this position means for how we read Homer. L. brings together archaeological, historical, and philological data in a fine illustration of classical scholarship's multidisciplinary traditions. Following Korfmann, L. sees Troy VI as a typical Anatolian city, with an acropolis and fortified lower town. He takes the hieroglyphic Luwian inscription found on a biconvex bronze seal in 1995 as evidence that Hisarlik was a royal center, probably that of a vassal king in the Hittite Empire. Given this premise, he suggests, the only possible conclusions are (i) that Hisarlik was the Wilusa mentioned in several Hittite documents, and (ii) that Wilusa was the city that Homer called Ilion. L. then presents newly discovered or reinterpreted Hittite texts suggesting that Achijawa refers to part or all of the Mycenaean world, and draws attention to the funerary inscription of Amenophis III (known since 1966, but generally ignored by classicists), clearly referring to a kingdom called Danaja, with major cities at Mycenae and Thebes. Achijawa/Danaja, he concludes, was a major power in the fourteenth-thirteenth century, contending with the Hittites in western Turkey; and half a millennium later, Homer knew this. The Hittite texts show that the king of Achijawa successfully shielded the brigand Pijamaradu, operating out of Millawanda (Miletus), from the Hittites. In August 2003 Frank Starke announced his reinterpretation of a Hittite text as a letter from the king of Achijawa to a Hittite king (probably Hattusili II, c. 1265-1240 BC), asserting Achijawan ownership of islands in the north Aegean that had been in their hands since the days of the king's ancestor—none other than Kadmos. Combining this name with the implication of Linear B tablets excavated at Thebes in the 1990s that this city controlled a realm reaching into Euboea, L. suggests that in the thirteenth century Thebes displaced Mycenae as the leading city in an Achijawan kingdom. Departing from Korfmann's thesis, L. then suggests that under Theban leadership Hittite-Achijawan tensions mounted. The Hittites

punished Achijawan aggression by capturing Millawanda; the Achijawan king responded by besieging Wilusa. Soon after the Trojan War both kingdoms collapsed, but Greek migrants to western Turkey in the eleventh century preserved a good deal of knowledge of the war, enshrining it in epic poetry. Stories about Troy probably entered the tradition at a time when the genitive of Ilion was *Ilioo*, midway between the Mycenaean form *Iliioio* and the Homeric *Iliô*. The basic storyline and certain set pieces—above all the Catalogue of Ships—then survived until the *Iliad* was written down in the eighth century.

Most Homerists, I suspect, feel comfortable with the idea that the Trojan War was already an old story when Homer sang about it in the eighth century, and that the story was based on some real event(s) in the Bronze Age. L. deserves our thanks for pulling together so much external evidence from archaeology and Hittite and Mycenaean texts to illustrate the plausibility of this view. Troy probably became a major topic for heroic oral epic in the twelfth or eleventh century, and many of the names and places in the story must have come down to Homer in more or less their Late Bronze Age form. That said, L. insists that we will be disappointed if we expect Homer to tell us about Mycenaean society. He suggests that the Trojan War was merely a vehicle that the greatest epic poets could use to tell stories about the burning issues of their own day. L.'s account of how the tensions among the aristocrats within Troy and in the Achaean camp might mirror the profound divisions in the rapidly changing societies of eighth-century Greece is masterful, and I cannot see how anyone could reasonably object to his conclusion that “when [an eighth-century bard] wanted to provoke debate about problems of his own time, there was no more effective method than to take this old story with its well-known characters—Agamemnon, Achilles, Paris, Helen, and others—and place the issues in the mouths of these characters” (p. 203).

The real controversy, I think, should not be over whether the story of Troy goes back to the Bronze Age, but over how much of Homer's story line is accurate. L. himself seems equivocal on this issue. After developing his scenario at length, he closes the book by conceding that “it may be that we cannot yet say anything definite about the historicity of the ‘Trojan War’” (p. 286). My own hunch is that L.'s sense of the balance between genuine Bronze Age stories and bardic invention is roughly right, but nothing in the Hittite and Mycenaean documents or the new excavations *requires* this conclusion.

L.'s qualified confidence in the accuracy of Homer's account of the war rests largely on the way external sources (Hittite, Egyptian, and Mycenaean) confirm that Achijawa was a major power, and clashed with the Hittite Empire around Wilusa. But if the non-bardic evidence does show that Homer knew a lot about Late Bronze Age political history, it is then odd that he apparently did not know the basic fact that the war was a frontier clash between the Achijawan and Hittite Empires. It seems to me that there is a logical problem here. This worsens if L. is right that Achijawa attacked Wilusa in revenge for the Hittite sack of Millawanda. L. explains the absence of Miletus from Homer's story by pointing to the speculation of Wolf-Dietrich Niemeier (Miletus' excavator) that Tudhalija III captured the city in the later thirteenth century (p. 284). Homer's silence would then simply be faithfulness to the facts: Miletus took no part in the war. But if early archaic poets were silent about Miletus because they knew this, it strikes me as remarkable that the Trojan Cycle never mentions Miletus in the lead-up to the war, even as the setting for one of the clearly mythological events that precipitated the crisis. It might be more economical to assume that the poets' knowledge was extremely

spotty. Homer perhaps “knew” about the wars of the Late Bronze Age in the same way that the poet of *The Song of Roland* “knew” about Charlemagne’s campaigns, allowing him turn a Basque ambush of the Frankish rearguard at Roncesvalles 300 years before his own time into a major battle in which the Franks annihilated a vast Saracen army. I suspect that Homer, like the *Roland* poet, knew that great wars had been fought, knew the names of some of the leaders, and even knew where the battles had been; but he may not have known much about why the armies fought or what the outcome was. Trevor Bryce has interpreted the Hittite texts differently, seeing the epic story of a great siege as conflating numerous wars around Troy going back to the early fourteenth century, when Attarsiya, a “man of Achija,” led 100 chariots against the Hittites.<sup>4</sup> This is not so far from L.’s position, explains the epic tradition’s silence about Millawanda and the Hittites, and—to my mind—makes sense as part of a profound reinterpretation of the Mycenaean legacy in late eleventh-century Greece.<sup>5</sup>

Korfmann’s new excavations at Troy are every bit as suggestive as the Hittite documents, and their meaning is every bit as unclear. Magnetometer surveys and excavations since 1992 have traced parts of two broad ditches, dug early in the Troy VI phase (c. 1700-1500 BC), and silting up by its end (1200 BC). L. accepts Korfmann’s argument that they delimited a lower town of 17 hectares, in addition to the 2.3 hectares of the citadel. A typical Anatolian fortification would include a defensive wall behind the ditch, but none has been located. L. follows Korfmann in seeing this as the result of stone robbing in the Hellenistic-Roman Troy VIII-IX phases (pp. 29-33). But this is not a very satisfactory argument. Late Bronze Age fortifications normally had massive foundations. Even if every single stone was plundered, magnetometry should reveal the foundation trench just as clearly as the ditch, and conventional excavation by the very fine team of fieldworkers that Korfmann and his colleagues assembled should detect the robbers’ trench with ease. The absence of evidence for a fortification wall is of course not evidence of absence, and Korfmann’s argument that the settlement covered about 20 hectares remains the most plausible interpretation of the data. But until the excavation confirms the presence of the wall, or—better still—exposes larger areas of Troy VI habitation in the lower town, doubts will necessarily remain.

L. follows Korfmann in stating that the 20-hectare site had about 7,000-10,000 residents, but that strikes me as unlikely. Cross-cultural studies suggest that cities of this size rarely attain densities over 150 people/hectare;<sup>6</sup> it would be surprising if more than 4,000 people lived in Troy VI. Troy was not a trivial site—probably just half a dozen Aegean sites were bigger<sup>7</sup>—but it was hardly a major site by west Asian standards.<sup>8</sup> This certainly does not falsify L.’s arguments, but needs to be factored in more clearly.

L. also follows Korfmann in seeing Black Sea trade as the source of Troy’s wealth, although one of the strongest points in Frank Kolb’s criticisms of Korfmann’s interpretation is that few items from the Black Sea have been found at Hisarlik, and all the indications are that traffic through the Hellespont was rather limited in the Bronze Age.<sup>9</sup> We need a clearer discussion of what would falsify the trading city thesis.

The most troubling archaeological problem is the absence of an archive comparable to those found at most west Asian royal centers. L. suggests that because of the Hellenistic leveling of the citadel and Schliemann’s onslaught “any ruins of the ‘state chancellery’, which must be regarded as firmly established at least for these periods [i.e.,

Troy VI-VII], were scattered to the four winds” (p. 115). The Luwian inscription found in 1995, he suggests, is just a tiny trace of what was originally there.

Like the arguments based on the ditch, this may be correct, but needs to be tested by more excavation. Schliemann dug badly, even by 1870s standards, but would even he have thrown away an entire archive without noticing? While hacking through the mound of Kuyunjik in 1849 Layard recovered thousands of Assyrian cuneiform tablets;<sup>10</sup> was Schliemann really so much worse than Layard? Possibly, as L. suggests, Schliemann’s spoil tips will yield inscriptions; but equally possibly there was no archive there for him to destroy in the 1870s. If Hellenistic builders had leveled such an archive and dumped the debris, it is rather surprising that no fragments of tablets have ever turned up. L. also suggests that the archive remains unexcavated, in the lower town. This is very possible, but again calls for more excavation. In the meantime, we can only conclude that the case for Hisarlik being the capital of a Hittite vassal state remains unproven. The single inscription we do have comes from a Troy VIIb<sub>2</sub> context, of the late twelfth century. L. insists that “The idea that the seal was kept for seventy or eighty years as a piece of antique decoration in the citadel only to be thrown away [or lost?] one day is less probable than that it continued in use as a seal in Wilusa even after the collapse of the central administration in Hattusa” (p. 119), but since we have no evidence for state writing in Troy VI, let alone Troy VII, I see no way to tell whether the seal was an heirloom brought from somewhere else in the Hittite Empire or the last remaining trace of a flourishing scribal bureaucracy.

Despite these concerns over the state of the evidence, I continue to suspect L. is right to think that Hisarlik was Wilusa, and was the scene of repeated clashes between a Hittite client kingdom and Achaean adventurers in the thirteenth century. Korfmann’s excavation did not settle the matter, though it did make the very great contribution of narrowing down the contested points. Above all, Korfmann identified archaeologically testable propositions, such as whether there was a (robbed) fortification wall, how extensive Troy VI housing was on the plain, and whether there was a palace archive. Korfmann’s untimely death is a great setback to scholarship, but there is every reason to think that future excavations will decisively settle some of these questions.

This is an excellent book. All Homerists, classical archaeologists, ancient historians, and Hittitologists should read it. Much of the evidence L. presents will be new to scholars in at least one of these communities, and all, I suspect, will be forced to think again by his powerful presentation. L. wrote the book for a non-academic readership in Germany, and whenever possible cited German scholarship. His bibliography provides an excellent overview of recent German Homeric and Hittite scholarship. L. repeatedly laments that even specialists have not taken the trouble to study the detailed annual excavation reports in *Studia Troica*. If his book encourages them to do this, it will have achieved much. Better still, readers might turn—as I did—back to the *Iliad* itself, with a new sense of excitement.

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<sup>1</sup> G. Grote, *History of Greece I* (London, 1846), 321.

<sup>2</sup> See, for example, the various papers in *A New Companion to Homer*, ed. I. Morris and B. Powell (Leiden, 1997), 511-713.

<sup>3</sup> M. Korfmann, "Troia, an Ancient Anatolian Palatial and Trading Center," *Classical World* 91 (1998): 369-85.

<sup>4</sup> T. Bryce, *The Kingdom of the Hittites* (Oxford, 1998), 392-404.

<sup>5</sup> I describe my own ideas about this reinterpretation in *Archaeology as Cultural History* (Oxford, 2000), 195-238.

<sup>6</sup> See particularly R. Fletcher, *The Limits of Settlement Growth* (Cambridge, 1995).

<sup>7</sup> Knossos, Phaistos, Mycenae, Mallia, Palaikastro, and Thebes. See T. Whitelaw, "From Sites to Communities: Defining the Human Dimensions of Minoan Urbanism," in *Urbanism in the Aegean Bronze Age*, ed. K. Branigan (Sheffield, 2001), 15-37.

<sup>8</sup> Cf. K. Branigan, "Aspects of Minoan Urbanism," in *Urbanism in the Aegean Bronze Age*, ed. K. Branigan (Sheffield, 2001), 38-50, Table 3.2.

<sup>9</sup> Frank Kolb, "Troy VI: A Trading Center and Commercial City?" *American Journal of Archaeology* 108 (2004): 577-613.

<sup>10</sup> M. T. Larsen, *The Conquest of Assyria: Excavations in an Antique Land* (London, 1996), 262-63.