
This book contains the contributions given in conjunction with two exhibitions: 1) Bilder und Bücher um Homer und Troia von der Ilias Ambrosiana zur Heinrich Schliemann in the Archäologischen Museum Altenessen, and 2) Heinrich Schliemanns Trojanischer Altertümer in the Essenner Ruhrlandmuseum, as well as within the context of the 100th anniversary of Schliemann's death in 1990. There are nine contributions by seven scholars. Two contributions by two additional scholars (M. Korfmann and W. Kukenburg) are published elsewhere.

In a short Introduction (9-12), Patzek, under the rubric, "Archäologie und historische Erinnerung," sets the tone of these contributions. Accordingly, in the popular mind, archaeology has in its achievements far exceeded what one can expect from history. Unfortunately, however, it has been elevated to the level of a modern myth. For this, Schliemann was chiefly responsible, so that in the popular conscience he is really the founder of what has actually become a mythological 'science'. As a child of the eighteenth-century, historicism led to a simplified view of what archaeology can achieve. Today, however, things are much more complex, and the ability to establish historical facts from
archaeological evidence is virtually, if not indeed totally, impossible. This applies in particular to Schliemann's naive object of attempting to prove that the Trojan War actually took place—namely, by discovering and excavating Troy. The manner in which this proposition is seen today by experts allegedly forms the object of the contributions in this volume.

In a second part (12-13), Patzek therefore gives an overview of the contents of the various contributors, attempting to extrapolate a common thread. A distinct common thread does not, however, emerge from a reading of the individual papers.

In his first contribution, on "zwischen Realismus und Romantik: Gedenken an Heinrich Schliemann" (15-29), Cobet proceeds to trace the history of the Schliemann myth. He provides an overview, albeit somewhat one-sided, of attitudes towards Schliemann and his work by a variety of parties, from the popular to the most specialized. What he bemoans in particular is Schliemann's naive belief that it is possible to solve historical problems by means of archaeology—i.e., that with the spade as "the sword of archaeology" it is possible to cut the Gordian knot of major historical problems (was Troy at Hissarlik? was there a Trojan War?). Those who reply in the affirmative, turn archaeology into a myth.1 By treating archaeology in this way, however, such people pay no attention whatever to the complex interrelations involved in interpreting the evidence, and thereby "disparage the multi-stratification of the narrativising as well as the objective, antiquarian nature of the historical sources which do indeed fall into various research branches but ought not to be read as non-interconnected or indeed without the interlinkage of the contexts of their various stratifications".2 Cobet may indeed have a point—up to a point, but it is rather overstated. There is, for instance, no evidence to suggest that at the end of his career Schliemann was primarily concerned with proving that the Trojan War actually took place. Much less is this, so far as I can tell, Korfmann's object. Cobet never refers to the whole of Aegean Bronze Age culture and Troy's place within it, which appears to be Korfmann's real object in the current excavations. In this respect, "the sword of archaeology" has already provided answers to major questions.

In his contribution on Schliemann's Mausoleum in the central cemetery in Athens (31-50), Hammer-Schenk explores the architecture and the various iconographic motifs of the frieze to show that Schliemann wanted to be regarded as a hero, and so while still alive made provision for an appropriate monument in which to be buried. This is an interesting study, but does not appear particularly relevant to the stated theme of the papers, and therefore has something of an anomalous character, similar to that of which Cobet complains in connection with the Schliemann Congress in Athens in 1990.

1 He includes here in particular also Manfred Korfmann, the current excavator of Troy, among those who continue to keep this myth alive.

2 This is, I believe, the thrust of the German, which reads as follows: "Sie misachtet die Vielschichtigkeit der erzählerischen wie der gegenständlichen, antiquarischen historischen Überlieferung, die zwar arbeitsteilig, aber nicht zusammenhanglos und nicht ohne Verzahnung auch der Kontexte ihrer verschiedenen Schichtungen gelesen werden will" (28).
This is followed by a thoughtful study by Easton on "Schliemanns Ausgrabungen in Troia" (51-70). It is based on work going back to 1979, and especially on his three-volume doctoral Thesis. Within the context of his title Easton poses four questions: 1) How good was Schliemann as an archaeologist? 2) What have his excavations at Troy revealed? 3) How do his achievements appear today? 4) Did he really find Priam's Troy?

Easton does not address these questions in turn, but proceeds as follows: How did Schliemann come to excavate Troy? (51-53). How good was Schliemann as an archaeologist? (53-55). Under the second of these, many of Easton's criticisms of Schliemann are correct, but at the same time also incorrect, because they lack adequate perspective. From the standpoint of 1992 they appear very correct indeed, but one needs to place Schliemann's work within the context of his contemporaries. One could take many examples, but one will suffice. Easton complains about the absence of a contour plan of Troy. But what other archaeologist produced a contour plan of a site in 1875 or 1880 or even later? One looks in vain, e.g., for a contour plan of Knossos in Sir Arthur Evans' Palace of Minos (published between 1921 and 1935). Nor does Easton anywhere explain that Schliemann was the first individual to excavate a prehistoric mound in that part of the world, and at the same time one with a very complex stratigraphy. It is true that already then Schliemann could have done some things differently, but on the whole he could, by the end of his career, hold his own with any contemporary excavator.

This section is followed, not by a direct reference to questions 2) and 3), but by turning to "Schliemann's Day-Books" (55-57) and "The Archaeology of the Site" (57-69). This latter, the longest and most interesting section, is not so much concerned with Schliemann's excavations, but, by using the information provided by Schliemann, Dörpfeld and Blegen et al., Easton attempts to establish refinements within the different phases at Hissarlik. These refinements constitute his most important conclusions, and as such they are significant, but at the same time appear to be a departure from what the title of the book implies.

Easton does not then consider his fourth question, but "Did Schliemann actually prove that the Trojan War took place?" (69-71). Clearly, the answer is 'no'! But in his "Conclusion" (71), he acknowledges that if there was a Troy at all, Hissarlik is still the best candidate. In other words, the discussion is conducted in such a way that Schliemann receives very little credit indeed.

Hertel offers a fascinating thesis on "die Vielschichtigkeit des Troianischen Krieges" (73-104; also published elsewhere). "An old layer of the legend," in which only Achilles and his companions are represented as fighting in the Troad and around Troy, takes one to a historical substratum in the legend of the Trojan War in which Greeks from central and northern Greece fight against the pre-Greek population of the Troad and around Troy—at first sustaining losses, but ultimately triumphing. This permits one to theorize that, if something like this actually occurred, such events must have taken place in North-West Asia Minor in the "post-Hittite-proto-Greek period". An Achillean Trojan War would accordingly "need to have taken place at a time when there were as yet no Aeolian, Ionian and Dorian Greeks, no longer any Hittites nor Mycenaeans, but already Phrygians in Asia Minor"—in other words, "in the 12th/11th century" (78).
This enables one to extrapolate that the Aeolian Greeks were the protagonists in the Trojan War. This finds further confirmation in the colonization of Asia Minor by the Aeolians and the Ionians. And then comes the crucial point. Hertel maintains that both these colonizations can be dated relatively accurately—namely, "on the basis of archaeological evidence". Accordingly, "the first Aeolian and Ionian colonies in Western Asia Minor, above all Smyrna and Miletus were founded in the 11th century" (79). In an accompanying footnote Hertel provides references for only Smyrna. What is therefore astonishing is that he apparently does not have a single piece of archaeological evidence upon which to fall back for a single Aeolian colony in the 11th century BC, to say nothing whatever about the Troad and Troy—at least not at this point in the discussion. None the less, Hertel proceeds to hypothesize from the single reference to Smyrna that such colonies were preceded by fighting, not always successful in the first instance, as we know (he does not tell us how we know this), and that such fighting could have taken place at the earliest in the 12th century. And on the basis of what happened at Smyrna and Miletus (Hertel does not tell us precisely what), one can extrapolate that something very similar happened in the Troad and in Troy. Hence the Trojan War! (79). In other words, Hertel's reconstruction is not anchored in any sound archaeological evidence, but in a process of "rationalisierende spekulieren" of the kind later employed by Cobet, that requires certain specific presuppositions and circular arguments.

The most important and most revolutionary aspect of his thesis, however, is that there is no break between Troy VIIb2 and Troy VIII—i.e., that the Aeolian colonization referred to above followed immediately upon the destruction of Troy VIIb at the end of the Bronze Age: this on the basis of a limited amount of pottery. In advancing this aspect of his interpretation, Hertel draws attention to three major points:

1) Troy VIIb was destroyed by fire.
2) The place was immediately colonized by Aeolian Greeks.
3) This could have taken place ca. 1000 B.C.

This aspect is not, however, without difficulties. In his discussion of 1) he assumes that the "Brandkatastrophe" was caused by human (foreign) agency. But such fire can be caused by various means. One therefore requires specific additional evidence to demonstrate foreign human agency. This Hertel does not do. Under 2) he attempts this by beginning with, not the archaeological evidence, as one should expect, but with the "literary tradition," from which he extrapolates eight points (81-90). There is, however, of course no way in which any of these have a direct bearing on what appears at Hisarlik immediately after the end of Troy VIIb. As Hertel accepts that much of what is otherwise found in Homer is poetic invention, so may many of the details of this literary tradition be.

In arguing his thesis, Hertel makes much of a number of "Proto-Geometric" (and Geometric) sherds, which he takes as proof of the arrival of Aeolian Greeks, and therefore the Greek settlement of Troy, namely as taking place by ca. 1000 BC. He acknowledges, however, that the same contexts produced quantities of Gray Ware, Gray Minyan Ware, Tan Ware, 'Barbarian' Ware and Knobbed Ware—all indicating continuation of traditions from the Late Bronze Age. This permits one to conclude that "elements of the older [BA] population survived the
destructions of Troy VIIb. Initially, they were conquered, but later lived together with the Greeks" (102). But since Hertel argues that the two types of pottery were found in the same contexts immediately after the end of Troy VIIb, it must follow that they lived together immediately. That foreign Greeks would have destroyed a site so thoroughly by fire, then taken it over completely, but at the same time have allowed the local population to live side-by-side with them, making their own pottery, seems most unlikely. Certainly Hertel does not offer any historical parallels. Furthermore, he does not point to any Protogeometric centres in Aegean Greece (i.e., on the mainland, and preferably in the Aeolian north) which would have inspired interest in Protogeometric pottery on the part of Aeolians who were presumably preoccupied with establishing a new settlement in difficult circumstances. Moreover, Hertel does not appear to make any allowances for a possible time-lag in new styles invented in Attica, apparently much less affected by the destructions at the end of the Bronze Age, and then spreading out from there. Consequently, his case for an (Aeolian) Greek takeover of Troy immediately after the destruction of Troy VIIb (i.e., in ca. 1000 BC) is far less secure than he would have one believe. It is equally possible that survivors from Troy VIIb continued to occupy the site and became influenced by Greek pottery, just as the inhabitants of Hissarlik in Troy VI and VIIa imported Mycenaean pottery and produced local imitations of it. This renders even more unstable Hertel's already shaky thesis about the "multi-stratification of the Trojan War".

It should also be noted that the demands which he makes on evidence for trade connections with Troy (80-82) are far more stringent than the demands he makes on the evidence for his own hypothesis. Moreover, as he demonstrates, the dating of the pottery (especially Mycenaean) of Troy VI (and also VIIa) is not precise in terms of absolute dates, and so is flexible enough to permit a number of different hypotheses. The same may be said of his own interpretation (as already noted). Consequently, he overstates the situation when he claims that "my interpretation of the archaeological evidence of Late Troy VI and Troy VIIa has demonstrated that a successful war by Mycenaean Greeks against Troy in the 13th century or the beginning of the 12th cannot have taken place" (88). The claim may be true, but not the interpretation. It may also be noted that in the same volume Easton does not seem to find the theory entirely convincing (67).

In connection with papers from a Symposium on Schliemann a contribution on "die Mauern von Jericho" (105-115) will doubtless come as something of a surprise—and rightly so. To be sure, Ussishkin at the outset does point to some alleged parallels, which he claims are very evident—e.g., both Troy and Jericho are deeply imbedded in Western tradition; both have become controversial in modern discussion. On closer examination, however, these parallels turn out to be rather superficial, which is even truer when Ussishkin cites Helen and Rahab. It is presumably for this reason that, after these initial statements, one hears no more about Troy in Ussishkin's discussion.

It is left for Cobet, in the contribution which follows (his second), on "Troia, Jericho und die historische Kritik" (117-135), to draw closer parallels. And he does draw many, which appear illuminating and indeed compelling—until one takes into account the manner in which he sets up his thesis of
Schliemann-Troia-Archäologie which he proceeds to argue within the context of his comparison of Troy and Jericho: namely, in a narrow and simplistic way. He employs two major components. At the outset, within the context of the hundredth anniversary of Schliemann's death, he cites a number of journalists in various newspapers on attitudes towards Schliemann (not the views of specialists on the significance of the data that have come to light at especially Hissarlik, but also at all other Aegean Bronze Age sites). Secondly, he cites Schliemann—but which Schliemann? It is essentially the Schliemann of 1874, i.e., Schliemann at the very beginning of his career, not the Schliemann who by the time of his death, after 20 years of experience, had greatly matured in respect of his methods and interpretation of the evidence. From the Schliemann of 1874 and journalistic comments of 1990 Cobet is able to extrapolate what he calls the "Schliemann Effekt"—i.e., the idea of "the spade as the sword of archaeology". "Der Spaten als Schwert der Archäologie" in the hands of Schliemann (of 1874) at 'Troy' produces an "Archäologie" that naively attempts to prove the 'unprovable'—that there was a historical Trojan War, as reflected in the Iliad.

Two major points may be made about Cobet's approach. First, although at the end of his discussion he cites Schliemann's well-known statement in his Trojanischer Altertümer (26-27) (1874), to the effect that, should he not find Troy, he would at least "penetrate to the deepest darkness of the prehistoric period and thus enrich science by discovering at least some interesting pages of the earliest history of the great Hellenic people," he attempts to play down its significance. Nor does he make much allowance for the fact that by the time of his last campaign in 1890, when significant new evidence came to light prompting Schliemann to plan the longest campaign ever for 1891, his approach to archaeological evidence was significantly different from what it had been in 1874 (not that he had given up his belief in the Trojan War or that the new evidence would not have strengthened that belief).

The second major observation has to do with Cobet's post-modern view of history: that the complexity of multi-stratified information does not enable one to reconstruct events but only cultural contexts. For Cobet, "griechische Geschichte" still begins as it did for Grote in the middle of the last century. In other words, for him there does not appear to be such a thing as Greek Vor- und Frühgeschichte.

The ultimate irony of his approach emerges in his discussion of Jericho. Since it is necessary to reject the biblical 'account' (as it is necessary to reject Homer's 'account' of the Trojan War), he finds it equally necessary to attempt to explain what actually happened (i.e., what were the 'events' of Jericho?). His method of "rationalisierend spekulieren," however, does not produce any more certainty about what 'happened' at Jericho than Schliemann's excavations at Hisarlik did on the question of the Trojan War. In other words, Schliemann's "Spatten" as the "Schwert der Archäologie" is just as valid as Cobet's "rationalisierend spekulieren". In connection with Jericho, Cobet at least ac-

1In much the same spirit in which Cobet rejects the biblical account of Jericho and at the same time just about everything in Keller's popularizing book, The Bible as
knowledges that at some point the Israelites did take possession of the land, which could have given rise to a later account in which Jericho could have played a significant role. But he does not allude to any early significant activity by the Greeks at the Hellespont that could later have given rise to the legend of the Trojan War. His discussion of Jericho does not therefore illuminate the "Schliemann Effekt" in a really meaningful way.

In his contribution on "die sichtbaren Beweise. Wahrzeichen, Denkmäler und andere Realien" ("the visible proofs: signs, monuments and other facts") (137-156), Röhrich offers something very different. Beginning with the local legend in Ankershagen, which Schliemann claimed to have made such a deep impression on him during his childhood, Röhrich attempts to explore how legends come about. "What gave wings to Schliemann's imagination were, so to speak, 'signs,' and by means of these the recollection of legends is essentially kept alive" (140). He then chooses a number of German legends to demonstrate this principle. Such 'signs' have various functions. They can emphasize the truth claims of a story, or they can also serve to keep alive the memory of an extraordinary, even legendary, event. Röhrich's examples are indeed interesting, but it would have been helpful had he extrapolated directly from his examples and shown which 'signs' in the legend of Troy and the Trojan War they illustrate. In any event, his discussion does not appear to be directly related to the question of the mythology of archaeology and the interpretation of archaeological evidence.

This is left for Patzek, in her second contribution, on "der Mythos vom Schicksalstag Troias" ("the myth of the fateful day of Troy") (157-169). She offers a fascinating interpretation, in which in particular the walls of Troy are seen as the 'sign' (there are also other 'Zeichen'), around which the multi-faceted legend of Troy emerged. Her interpretation, however, requires several assumptions, which themselves raise problems that are not addressed. For instance, virtually the whole of the legend was allegedly created by Homer, i.e., in the eighth century. In other words, no allowance is made for the possibility that parts of the Iliad and Odyssey may go back much earlier, perhaps even to the 16th century BC—which would of course affect the ultimate interpretation. And in connection with the "Schicksalstag" of Troy it is not the demolition or destruction of the walls that produces the "Ruine," but the fire which destroyed the buildings within the walls. Furthermore, there ought to be some explanation why Homer, coming from the Ionian region much further south, should have chosen Troy as the very centre-piece of his epic, situated not only in an Aeolic district but indeed at the very outer edge of the Greek presence in Asia Minor in the

History (1956), critics early in this century questioned whether Pontius Pilate was actually a historical figure—until excavations in Caesarea in 1956 turned up an inscription with the words Pontius Pilatus Praefectus Iudae. With, as it were, one stroke of "the sword of archaeology" a major question was answered, and this without any "rationalisierend spekulieren". Of course, not all questions can be so easily answered, but it is equally misleading to imply that none can. The question of whether there was a Trojan War could be so answered.

If anything, the wooden horse should become the 'Zeichen' in the legend, but this is never mentioned by Patzek.
eighth century. After all, there must have been similar conflicts with the local inhabitants in attempts to establish settlements further south. Is there any Greek site anywhere in Asia Minor (including Troy) dated to the eighth century that answers to the proportions represented in the *Iliad*? By contrast, there is a much closer correspondence with Troy VI at Hissarlik than any Greek site in Asia Minor. In other words, Patzek's discussion of the question suffers as much from failing to take the archaeological evidence (or lack thereof) into account as those who are accused of approaching it in a simplistic manner. This last contribution of the series probably gives the best idea of the book as a whole. Although it purports to be on *Archaeology and Historical Recollection* in terms of Heinrich Schliemann after 100 Years, in a discussion of over 7000 words there does not appear to be a single reference to either archaeology or to Schliemann.

While these contributions do offer some interesting individual investigations, they could have profited from a more balanced approach to what Aegean archaeology has achieved and has to offer 100 years after Schliemann.  

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1 The evidence for Greek activity at Hissarlik that can be dated securely to the eighth century BC is very limited.

2 While there is a useful Bibliography running over more than 11 pages, there is no Index, which would also have been useful.