

MORTIMER WHEELER ARCHAEOLOGICAL LECTURE

SCHLIEMANN'S TROY—ONE HUNDRED
YEARS AFTER

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IN a report on his first excavation of Hissarlik, written on 24 May 1873 for the *Augsburger Allgemeine Zeitung*, Schliemann announced categorically: 'Since I consider my task to be fully accomplished, on 15th June of this year I shall bring the excavations here in Troy to an end for ever.' Nevertheless, five years later he was digging there again, and on New Year's Day 1880 he wrote to Harper's asking them to take on the American publication of his *Ilios*: 'No money and no pain has been spared to make this work settle and exhaust for ever the Trojan question'.¹ That 'for ever' had an even shorter run, a mere two years. On 15 April 1883, following his third excavation, he offered his *Troja* to another American publisher, Scribner's: 'I have now terminated for ever the excavations on the site of sacred Ilion. . . . This book will contain an account of the most important discoveries I ever made in my life and it will settle the Trojan question definitely for ever. . . . Troy is now entirely excavated. . . .'²

That proved to be a seven-year 'for ever'. The fourth excavation, in 1890, lasted for five months and produced the sensational discovery that Troy II, the city of the great treasure, far from being Priam's Troy, as Schliemann and all his followers had firmly believed, was very much older. Priam and the Trojan War had to be shifted to the level Schliemann had previously labelled 'Lydian', but now simply Troy VI.³ Schliemann died before the planned 1891 campaign, and it was 1893 before

¹ Schliemann corresponded in several languages. When the original was in English, as in this instance, I have quoted verbatim. I shall not indicate the language of the original in subsequent quotations.

² *Briefwechsel*, ed. E. Meyer (2 vols., Berlin 1953-58), i 230, ii 86, 159, respectively.

³ See W. Dörpfeld *et al.*, *Troja 1893* (Leipzig 1894), ch. 1-2, and the full account in his *Troja und Ilion* (2 vols., Athens 1902).

Dörpfeld could continue their joint work, which he completed the following year. Dörpfeld lived to help the Cincinnati team, who excavated for seven seasons between 1932 and 1938. By then, in truth, Troy was entirely excavated.

The final reports of the Cincinnati mission began to appear in 1950. In the fourth volume, published in 1958, which dealt with the relevant stratum, now known as Troy VIIa, Blegen felt able to write that the 'fundamental historicity of the Greek tradition', its 'basic solidity and reliability . . . can no longer be denied.' Five years later, in a popular account, he expanded that lapidary pronouncement: 'It can no longer be doubted, when one surveys the state of our knowledge today, that there really was an actual historical Trojan War in which a coalition of Achaeans, or Mycenaeans, under a king whose overlordship was recognized, fought against the people of Troy and their allies.'¹

I was not the only one whose capacity to doubt had not been destroyed quite so totally. What evidence, I and others asked, did you find, or did Schliemann or Dörpfeld find, in all the years of excavation at Hissarlik that points to—'points to', not 'proves', please notice—a coalition of Achaeans or Mycenaeans under a king whose overlordship was recognized? So far as I can discover, the answer is limited to a single bronze arrowhead found in Street 710 of Troy VIIa, about which Blegen commented in his final report, 'One may wonder if the arrowhead . . . was not perhaps a missile discharged by an invading Achaean.'² I could dwell on the question of whether such wondering is consistent with the prefatory statement of method—'we have aimed to present a factual, objective account of what we did and what we found. . . . Conjecture and speculation have been kept to a minimum'—but I shall go on to more important matters, chronology to begin with. Any war, the Trojan War too, takes place at a fixed point in time, not vaguely in the thirteenth century. How did Blegen provide a fixed point, by archaeological evidence?

The answer to that basic question turns out to be unstable and unsatisfactory. The dates rest, of necessity, on the pottery remains and on nothing else. In the 1958 report Blegen wrote that Troy VIIa was short-lived, that its duration cannot be determined 'precisely', that 'a century or less, possibly even

¹ C. W. Blegen *et al.*, *Troy iv* (Princeton 1958), p. 10, and Blegen, *Troy and the Trojans* (London 1963), p. 20, respectively.

² *Troy iv*. 12.

within a generation of men,' seemed plausible, a generation which he then tried to narrow to the period *c.* 1275–1240 B.C.¹ But in the later popular book he had Troy VIIa destroyed about 1250 B.C. 'if not a decade or two earlier' on one page, and three pages later, 'about 1260 B.C., if not indeed somewhat earlier.'² When I commented on the chronological instability of the 'undeniable' Trojan War, Professor Caskey, who was most closely associated with Blegen in both the excavations and the publication, replied, first, that to question the dating 'without re-handling the pottery itself is a waste of everybody's time', which was wholly unresponsive to my objection; second, it 'need not be doubted' that the beginning of VIIa was 'near 1300 B.C.' but that we 'as yet must allow a latitude of some twenty or thirty years toward the end of the thirteenth century' for the date of the destruction of VIIa.³ By no arithmetic known to me can 'a generation of men' be dated within those widely separated end-points, nor is 1260 or 1250 or even 1240 a date that one would normally identify as falling within 'a latitude of some twenty or thirty years towards the end of the thirteenth century'.

Nor does that exhaust our chronological difficulties. Troy VIIa had two architectural peculiarities: the houses, of poor quality and small in size, were huddled in terraces, and the familiar large storage jars were sunk to the rims into the floors of the dwellings and covered with stone slabs so that they could be walked over. 'We may, with some degree of confidence,' writes Blegen, 'recognize the endeavour of the threatened community to lay up sufficient supplies of food and drink to withstand a siege.'⁴ 'Homer', everyone knows, stretched the siege of Troy into a ten-year operation but neglected to allow for Achaean replacements or supplies. Are we now asked to believe that for thirty-five years, and perhaps longer, the Trojans anticipated that siege, that when they resettled the city after the destruction of Troy VI by a violent earthquake, they said to themselves, 'One day the Achaeans will come under a recognized overlord; so let us prepare by huddling together and sinking *pitthoi* into the floors, in which we shall be able to store food and drink when the evil day arrives'?

In the year following the publication of Blegen's popular book, Vincent Desborough, who can scarcely be charged with not having handled masses of Mycenaean pottery himself,

¹ *Ibid.*, pp. 8 and 12 respectively.

² *Troy and the Trojans*, pp. 160 and 163 respectively.

³ *Journal of Hellenic Studies*, 84 (1964), 10.

⁴ *Troy* iv. 12.

labelled Blegen's efforts to pull the destruction of Troy back into the first half of the thirteenth century as 'rather tendentious'.¹ By that he meant that Blegen was motivated by reasons extraneous to his own archaeological evidence and judgement, namely, the wish to get Troy destroyed well before the destruction of Pylos. Desborough himself suggests, first, that a date as late as *c.* 1200 B.C. for the end of LH IIIB pottery and the transition to LH IIIC is 'perfectly admissible'—that is the key pottery sequence for the end of Troy VIIa; second, that the destruction of Troy VIIa should however be placed between 1250 and 1230: 'it is inconceivable that the Achaeans could have mounted any sort of combined attack after the catastrophic disaster that overcame many of the Mainland centres at the end of LH. IIIB *c.* 1200.'² Again the 'archaeological' dating has been sacrificed to extraneous considerations. Just a century before, on 18 October 1873, Schliemann wrote to the sceptical Max Müller, with the directness that characterized his correspondence and his books and articles: 'It was . . . beyond any doubt the treasure of the last king, of the king who reigned when the catastrophe happened and this king being called Priamos by Homer I call the treasure Priam's treasure and *have no other evidence of the correctness of the name*' (my italics).³

In one respect, then, 'Schliemann's Troy' remains unchanged one hundred years after. The archaeology is vastly improved in technique and the quantity of the documentation is incalculably increased, but the central question is being answered in exactly the same way and almost exactly the same language, though perhaps less 'naïvely'. I believe this to be a unique situation in the history of modern archaeology; not even Camelot can touch it. As J. M. Cook has recently written, 'If Homer, in Wilamowitz's phrase, was Schliemann's "Hausgott", to the next generation he was the Word. After Schliemann the emphasis was on the historical topography of the Trojan War; a reader may sense an underlying assumption that Strabo is human and liable to error but Homer is infallible and never nods.'⁴ As we shall see in a moment, Professor Cook should have written 'generations' (in the plural).

Schliemann began his twenty-two year career as a working

¹ *The Last Mycenaeans and Their Successors* (Oxford 1964), p. 164.

² *Ibid.*, pp. 240 and 249 respectively.

³ E. Meyer, 'Schliemann's Letters to Max Müller in Oxford', *Journal of Hellenic Studies*, 82 (1962), 75–105, at p. 83.

⁴ *The Troad* (Oxford 1973), p. 92.

archaeologist with the firm conviction that not only was Homer in the *Iliad* to be read as if he were a reliable 'war correspondent', in Myres' phrase, but that the *Odyssey* was a mixture of ordnance survey and log-book. So in a few days' scratching in Ithaca in 1868 he located the farm of Laërtes, the field where Eumaeus lived and the 'cyclopean ruins' of ten of his pigsties, he found cremation urns which 'very possibly . . . preserved the ashes of Odysseus and Penelope or their descendants', and he dug unsuccessfully for the roots of the olive tree from the wood of which Odysseus constructed his marriage-bed.¹ His rapid publication in book form of these discoveries (and others to which I shall soon return) did not meet a warm reception. Tozer, the leading authority on ancient geography, suggested in a short review that 'a little more criticism might have saved him a good deal of trouble'.²

Not many years were to go by before Schliemann himself would have agreed. In August 1873 we find him writing to Charles Newton, a few months after his discovery of the great treasure in Troy II, a stratum which was flat and without an acropolis: 'Homer is an epic poet and no historian. He never saw neither the great tower of Ilium, nor the divine wall, nor Priam's Palace because when he visited Troy 300 years after its destruction all those monuments were for 300 years couched with its 10 feet thick layers of the red ashes and ruins of Troy. . . . Homer made *no* excavations to bring those monuments to light, . . . Ancient Troy has no Akropolis and the Pergamos is a pure invention of the poet.'³

Such a rapid about-face represents an aspect of Schliemann's temperament that is perhaps less well known and merits a little of our time. It is, I believe, as important in his work as the more familiar and often tempestuous irascibility. The latter can be documented endlessly. I shall restrict myself to one example, which I have chosen because it involves Frank Calvert, the American consul in the Dardanelles who offered Schliemann free rights to dig on the half of the mound of Hissarlik which he personally owned, helped him throughout his career with the endlessly troublesome Turkish authorities and in many other ways, and after Schliemann's death, was equally helpful to Dörpfeld, as the latter warmly acknowledged.⁴ The published correspondence includes a number of quarrelsome letters

¹ *Ithaka, der Peloponnes und Troja*, ed. E. Meyer (Darmstadt 1973), pp. 39, 51-2, 31, and 28-9 respectively.

² *The Academy*, 1 (1869), 22.

³ *Briefweschel* i. 238-9.

⁴ *Troja 1893*, p. 6.

to, or about, Calvert, but no other reaches the flights of an enormous letter written from Athens in February 1878 to Max Müller in Oxford. I quote an extract:

'I have to point out to you a libel in Frasers Magazine for February written by Wm C. Borlase, President of the Royal Institution of Cornwall and entitled "A Visit to Dr Schliemann's Troy", which for the number of its inculpations and the vehemence of its attacks leaves far behind it any libel that has been written against me before. First of all I must tell you that I have in the Troade a foul fiend of the name of Frank Calvert, who has given the text as well to the libel of Mr Gallenga as to that of Wm Simpson in Frasers Mag. of July last and to the libel now before us. That Calvert has been libelling me for years; I answered him in the *Guardian* three times, showing by my last answer that he is of bad faith and a liar, and therefore his further libels were refused by the Press. But, never daunted, he now enrages the English travellers against me by his ill representation and explication of the ruins of Hissarlik and persuades them to attack me.'¹

I must leave it to the psychologists to consider whether Schliemann's demonic qualities, of which this letter represents only one side, were not essential, whether he could have achieved what he did without them. My immediate concern, as I have said, is with something else about the man, which I am certain was at least as integral to his career. He retained to the end of his life a sense of inadequacy because of his lack of formal education, and there is genuine pathos in his innumerable appeals for assistance and advice, and in his expressions of gratitude. His letters to Bismarck and the Kaiser may grate, but that is a different matter. No one can fail to be touched by his relations with Virchow, with Müller, with Dörpfeld, with Sayce, Mahaffy, and a host of other scholars. And, what is decisive, he accepted advice on scholarly matters, and he could admit error. It was no small thing, after all, to appreciate almost instantaneously in 1890, at the age of sixty-eight, that Troy II, the Troy of the great treasure, was apparently not Priam's Troy, a view on which he had staked everything for so long.

Having quickly discarded, as we have seen, the Homer-the-war-correspondent prop (though he could never bring himself to abandon it totally when there was a glimmer of a chance to revive it), Schliemann held fast to two other supports. The

¹ 'Letters to Max Müller', p. 97.

first was the treasure. As he wrote to Newton in the letter I have already quoted, 'But my treasure shows that Troy was . . . immensely rich, and being rich it was powerful, had many subjects, large dominions . . .' Confirmation came soon after when he discovered more treasure in the shaft-graves at Mycenae. And the second prop was the topography of the Troad. Homer could not have seen buried Troy, but he could, and did, see the plain of Troy, and he described it with remarkable accuracy.

Then came 1890 and Dörpfeld's excavations of 1893 and 1894, which removed the treasure from the story. Troy VIIa was a pitiful poverty-stricken little place, with no treasure, without even any large and imposing buildings, with nothing remotely resembling a palace. The inhabitants of those remains were not, in Blegen's words, 'made up of the highest grades of society'. Happily, as he immediately goes on to say, the 'houses of the ruling class and of the well-to-do presumably stood on the upper ringed terraces in the central part of the site, whence they were removed without leaving a trace when the top of the hill was shaved off in Hellenistic and Roman times.'¹ Since there is nothing to be done with buildings that have been removed without leaving a trace, apart from presuming their existence, the post-Schliemann generation was left, as J. M. Cook correctly noted, with only the historical topography.

A problem in logic then arises though it is commonly ignored. Reviewing Schliemann's *Trojan Antiquities* in 1874, Max Müller, probably the first academic of high rank to appreciate fully the importance of the excavations at Hissarlik, wrote: 'The locality of the war, as described by the poet, may have some amount of reality, but that is perfectly compatible with the mythological character of the war itself.'² But then, we don't take Max Müller seriously these days, in view of his wild theories of myth. Let us instead turn to that paragon of erudition and sobriety, who never in his life permitted himself a flight of fancy, the historian Georg Busolt. In the first edition of volume 1 of his *Griechische Geschichte*, begun in 1879, completed early in 1884 and published in 1885, Busolt accepted without qualification Schliemann's dismissal of Bunarbashi and his identification of Troy with Hissarlik.

I must digress briefly about Bunarbashi, a village some ten kilometres south-south-east of Hissarlik, and correspondingly further from the sea. In 1785 Lechevalier identified a 'citadel' outside that village as Homer's Troy, and his identification

¹ *Troy* iv. 6–7.

² *The Academy*, 5 (1874), 39–41, at p. 41.

won majority support despite scattered opposition. Von Hahn's excavation there in 1864 drew a blank, and four years later Schliemann, fresh from his 'discoveries' on Ithaca, satisfied himself that Bunarbashi could not be Troy, partly by 'Homer-the-war-correspondent' arguments but also by the unanswerable argument that trial trenches which he rapidly dug showed no signs of any prehistoric habitation,¹ a conclusion which all subsequent excavations have confirmed. But unanswerable arguments are not necessarily persuasive. I cannot explain the magnetism of the Bunarbashi-Troy equation, for which there was never any good reason, but the fact is that neither Schliemann's work there nor his first excavations at Hissarlik persuaded enough of the archaeologists and philologists, and Schliemann responded to their deafness with his characteristic energy and anger.

Busolt was one of those who was persuaded, but, he pointed out in a long footnote, Schliemann had merely confirmed what had been believed in antiquity from Herodotus on, despite the heresy of Demetrius of Skepsis repeated by Strabo, and no more. It is a fallacy to seek to check 'Homeric detail' on the ground. Troy II was obviously the seat of a great power, he continued, but we have no way of identifying that power, with Priam or with anyone else.² By the time a second edition was required, published in 1893, Busolt had to rewrite the volume completely, including a 125-page chapter on Mycenaean culture which displays in detail a remarkable knowledge of all the archaeological publications of the intervening eight years, among them the preliminary report of Schliemann's 1890 excavation which shifted 'Homer's Troy' from II to 'VI' (VIIa). He found it unnecessary, however, to alter the critical footnote, apart from the addition of further bibliographical references.³ As for the Trojan War itself, he simply ignored it as not an historical event.

So did Beloch, the first volume of whose *Griechische Geschichte* also appeared in 1893 (an *annus mirabilis* in the study of Greek history, for it also was the year of publication of a third major work, volume 2, the first Greek volume, of Eduard Meyer's *Geschichte des Altertums*). And that brings me to a puzzle. Schliemann was deeply and persistently upset by the resistance to his discoveries in German classical circles, and he seized on every hint of support, even though some of it was of the lunatic

¹ *Ithaka*, pp. 126-63.

² (Gotha 1885), p. 9 n. 1.

³ (Gotha 1893), p. 137 n. 5.

variety. Lunacy was not restricted to one side of the debate. In 1883 a German artillery officer, Ernst Bötticher, began a stream of publications in which he charged Schliemann (and Dörpfeld) with deliberately falsifying their reports in order to conceal that Hissarlik was nothing but a vast ancient crematorium. Schliemann was enraged, rejected the pleas of his friends to ignore the man, and in 1889 brought Bötticher to Hissarlik at his own expense—to no avail. So the following year, 1890, again at his own expense, he organized an international commission of experts, who met at Hissarlik and on 30 March unanimously issued a formal 'protocol' dismissing Bötticher's accusations and claims. Among them were von Duhn, Carl Humann, Charles Waldstein, and of course Virchow and Frank Calvert.¹

Characteristically, Bötticher's manic assaults drove Schliemann back to Troy, which he had left for ever, and to the fateful excavation of 1890. But I must revert to my puzzle: why did he not turn to his most obvious allies in Germany, the ancient historians? They, unlike the archaeologists and philologists, had promptly sided with him in dismissing Bunarbashi. The only exception, Ernst Curtius, does not count, not because, in Eduard Meyer's contemptuous words, he wrote history after the manner of Ephorus,² but because he and Schliemann had long had hostile and competitive relations: Curtius had been given the right to excavate Olympia, for which Schliemann yearned and pleaded. I have three other historians in mind. Busolt we have already considered. Then there was Adolf Holm, the first volume of whose *Griechische Geschichte* appeared a year later, in 1886. Unlike Busolt, still a young unknown, Holm was an established scholar who between 1866 and 1874 had published several pioneering works on Sicilian history, in which he revealed his knowledge and appreciation of contemporary archaeological work (and Schliemann had himself toyed with the idea of excavating in Sicily). Like Busolt, he now welcomed Schliemann's activity at Troy, accepted the equation with Hissarlik but remained sceptical of the historicity of the Trojan War.³

The third piece in the puzzle is also the oldest. In 1877, a year

¹ See briefly Ernst Meyer, *Heinrich Schliemann* (Göttingen 1969), pp. 356–61. Schliemann's correspondence in those years was filled with the Bötticher affair.

² *Geschichte des Altertums* ii (Stuttgart 1893), p. 30.

³ (Berlin 1886), pp. 27–8, 95–6, 145–6.

before Schliemann's second period at Hissarlik, there was published in Leipzig a 112-page *Geschichte von Troas*, based on personal autopsy of the site, massive knowledge of the ancient sources, and critical appreciation of Schliemann's publications. The conclusions on the points that concern us, though different in important respects from those of Holm and Busolt, were firmly on the same side in the controversy aroused by Schliemann: he had settled the Bunarbashi-Hissarlik dispute (though the caustic comment was made that one cannot understand the site from Schliemann's writings alone); there was a historical kernel to the tales of the Trojan War, but we can do nothing with them; that Priam's Troy of the *Iliad* never existed was *proved* by Schliemann's work (then, we must remember, dominated by a wholly false chronology). The author of this book was a young man of twenty-two. His name was Eduard Meyer. A child prodigy, he had become interested in Asia Minor as a schoolboy in Hamburg, embarked on his history of the Troad while a student at Leipzig but put it aside until he could find the opportunity to visit the region. That came when he was appointed tutor in the household of Sir Philip Francis, British consul in Constantinople. In September 1875 Meyer spent six days in the Troad under the guidance of Frank Calvert, and then completed his book.¹ Yet I can find no reference to it in any of Schliemann's published correspondence; it is ignored in the extensive hagiographical writing of the past half-century and in the accounts of professional archaeologists; it even escaped Professor Cook's close-meshed net.

A twenty-two-year-old budding scholar may have seemed too weak a prop, but by the time Schliemann organized his commission to crush Bötticher in 1890, Meyer was Professor of Ancient History in Halle, having previously held the chair in Breslau and published the first volume of his great *Geschichte des Altertums*. I must confess my inability to explain the puzzle satisfactorily. The reason cannot lie in the rejection by these historians of Schliemann's faith in the Homeric Trojan War or in their lack of enthusiasm for Schliemann's greater flights of fancy. They were rock-solid on what mattered to Schliemann most, Hissarlik, and Eduard Meyer, at least, was more willing to accept a historical kernel in the tales than was Max Müller, with whom, as we have seen, Schliemann maintained the closest relations. It appears that philologists, politicians, engineers,

¹ For these biographical details see briefly Karl Christ, *Von Gibbon zu Rostoutzeff* (Darmstadt 1972), pp. 286-9.

and architects have a measure of *auctoritas* on archaeological matters, but not historians, and in that respect, too, Schliemann's Troy remains unchanged after one hundred years.

I spoke earlier of a problem in logic. How was it that Schliemann and Meyer came to diametrically opposed judgements of what Schliemann had *proved*, though they agreed on what he had *found*? This is no mere antiquarian question, for one can replace the two names by contemporary ones, Schliemann by Blegen in particular. The stratification of Troy, the architecture and the pottery, and so on, are as settled in all essentials as they are ever likely to be. But these were not what Schliemann set out to discover. He was after something far greater, the truth about an ancient and famous historical question. And that is still the central question, one hundred years later, to which I shall now devote myself. What do we know about the Trojan War and its background that was not, and could not be, known a century ago before Schliemann and his successors excavated Hissarlik down to virgin soil? First, however, I want briefly to run through five points on which there can be no serious disagreement, outside the lunatic fringe that has always infested this subject. I do so in order to clear the ground of marginal or no longer debatable questions.

1. Schliemann was epithet-prone and still is: from one recent article I have culled the following—'pseudo-truth', 'fantasy-life', 'clever fraud', 'vulgar', 'dilettante', 'lack of conscience', 'psychopathy', 'egoistic, romantic, tortured, infantile'.¹ No doubt, but Schliemann was also the father of Greek prehistoric archaeology he is usually acclaimed to have been. He may, as has been said, have dug a site as if he were digging potatoes, but he was also the first man in this field, and virtually in any field of archaeology, to stress stratigraphy and the primacy of pottery for relative chronology. He also appreciated—the significance of which is not often acknowledged—that the highest aim of archaeology is to answer questions. When offered Chiusi as a site in 1875, he refused. 'There are no problems to be resolved', he wrote, 'and I shall not be able to find anything that every museum does not already possess.'² His judgement may have been right or it may have been wrong, but the sentiment was impeccably correct.

¹ W. M. Calder III, 'Schliemann on Schliemann: a Study in the Use of Sources', *Greek, Roman and Byzantine Studies*, 13 (1972), 335–53. This article is an important exposé of the hagiography that is current in the guise of biography of Schliemann.

² *Briefwechsel* i. 302.

It is the same, easily neglected sentiment with which Sir Mortimer Wheeler closed his autobiography, *Still Digging*: 'it was . . . my conviction that research should proceed, not fortuitously, but on a rigidly selective scale of values. Those values necessarily change from age to age and mind to mind; the prime point at issue is not their individual character but the necessity for their presence.'¹

2. Ancient Troy was at Hissarlik, as virtually the whole of antiquity believed, from Homer on, and the Troy which was besieged and captured by the Achaeans, *if there was a Trojan War*, was the city we now call Troy VIIa, violently destroyed late in the thirteenth century B.C.

3. The poet or poets we call Homer came at the end of a long oral tradition. Schliemann was firm on that, and virtually everyone else who has discussed the 'Homeric problem' since his time. We have a far clearer grasp today of the mechanism of oral poetry and its transmission, thanks to Milman Parry and his successors, but that new—I might say 'revolutionary'—knowledge has made little difference to the question with which I am concerned today.

4. Because of the oral tradition, and because the *Iliad* and *Odyssey* were neither histories nor war correspondents' reports, errors in scale and in the details are to be expected and do not constitute significant arguments, one way or the other, about the essential historicity of the account of the Trojan War. Granting that, I wish the 'other side' would abandon their dance of triumph on such rare occasions as the discovery that a boar's tusk helmet, mentioned once in the *Iliad*, actually existed in Mycenaean Greece. Who has ever pretended that every single word in the *Iliad*, every object, every description of the sea or a hill or a river in the Troad, is as imaginary as the mermaids and the abode of the gods? It becomes ludicrous when Schliemann, Schuchhardt, Dörpfeld, and countless others seriously hold up as powerful evidence in support of the identification of Hissarlik with Troy the long closing scene in the *Iliad*: Priam is driven by Hermes in a chariot to the Achaean camp, arrives in time to drink and sup with Achilles, gets up in the night and, still accompanied by Hermes, drives with Hector's corpse to the Scamander River, where the god leaves him as he continues to Troy, arriving with the dawn. The distance of this round trip from Hissarlik to the Achaean camp and back is roughly the same as that from this building to Tower Hill and back. Even

¹ *Still Digging* (Readers Union ed., London 1956), pp. 238–9.

without the help of a god, they could have managed Bunarbashi as well as Hissarlik in that time span.

5. Although it is almost embarrassing to do so, long experience compels me to say explicitly that I accept the proposition that the historical and archaeological problems which exercise us have no great relevance to the literary merits of the poems, or to their entertainment value. In return, I must insist that the literary merits have no relevance to matters of historicity.

And now, may I return from the red herrings to the central question? In his struggle to obtain official Turkish permission to launch his first excavation in 1871, Schliemann wrote repeatedly, in one form of words or another, that he had 'the purely scientific aim of showing that the Trojan War was not a fable, that Troy and the Pergamos of Priam existed in reality'.¹ Did he succeed? Do the ruins of Troy confirm the historicity of the war which Homer recalled and in part recounted? We know Schliemann's affirmative answer, repeated by Dörpfeld after him and by Blegen still later, by the three men, in other words, who were responsible for this undeniable (and undeniably great) archaeological triumph. Blegen's exact words bear repetition: 'It can no longer be doubted . . . that there really was an actual historical Trojan War in which a coalition of Achaeans, or Mycenaean, under a king whose overlordship was recognized, fought against the people of Troy and their allies.'² Blegen then went further. From Troy he moved on to Pylos, and the final reports on that excavation were not called simply *Pylos*, as the earlier ones had been simply *Troy*, but *The Palace of Nestor at Pylos*.

Yet the plain fact is that Blegen found nothing, literally nothing, at either place to warrant his *historical* conclusion. Not a scrap was uncovered at Troy to point to Agamemnon or any other conquering king or overlord, or to a Mycenaean coalition or even to a war. For that blunt assertion I have the highest, if reluctant, authority, that of Professor Caskey, who wrote: 'the physical remains of Troy VIIa do not prove beyond question that the place was captured at all. An accidental fire, in unlucky circumstances, on a day a strong wind was blowing, might account for the general destruction that is known to have occurred. Furthermore, if this citadel was not sacked—and indeed if it was not sacked by Greeks under Agamemnon—we are left without a compelling reason even

¹ *Briefwechsel* i. 179; cf. pp. 178, 186. ² *Troy and the Trojans*, p. 20.

to go on calling it Troy.’¹ The ‘if’ clause is, of course, what the archaeologists have sought to demonstrate, and they have now turned it into a premise. The position at Pylos is even worse, for there, unlike Troy, Blegen found documents, tablets in Linear B, from which Dr. Chadwick has now drawn the firm conclusion that ‘Ana Englianos . . . cannot be Nestor’s palace’.²

None of this is said in disrespect of Blegen, master of his craft that he was, or of any of the other archaeologists who have made the same slide into a circular argument. Normally, material evidence without documents cannot answer the question Schliemann first posed. The most that can legitimately be said one hundred years after Schliemann is that, if there was a Trojan War at all like the Homeric one, Hissarlik is the sole fortress in that part of Asia Minor which could have been under siege, and Troy VIIa is the one stratum which could have been relevant. That is something no doubt—at least we hear no more of Bunarbashi—but it is not much. And there is a fundamental sense in which the intensive and increasingly sophisticated archaeology of the past hundred years has made the position worse, that is to say, it has reduced, rather than increased, the possibility of finding an answer to the key question without the discovery of new documents, in Hittite, for example.

This paradox, that the more we know the worse off we are, deserves further consideration, and chronology offers a good test. Homer of course provides no chronological foundation whatever, nor do the ancient chronographers from Herodotus to Eusebius, since they perforce made their calculations from oral traditions, which, it has now been demonstrated beyond rational disagreement, are invariably misleading in chronological matters. Not every tradition is as distorted as that of the *Nibelungenlied*, which manages to combine into a single complex of events the Ostrogoth Theoderic, who ruled most of the western Empire from 493 to 526, Attila the Hun, who invaded Italy in 452 and died in 453, forty years before the accession of Theoderic, and a certain Pilgrim, who was bishop of Passow from 971 to 979. But for the historian a 100-year error is not significantly less vicious than a 400-year error. There are those among us, I concede, who insist that, or at least behave as if, the Greeks by some mysterious process performed chronological miracles in their oral tradition, miracles of which no other

¹ Op. cit., p. 9.

² M. Ventris and J. Chadwick, *Documents in Mycenaean Greek*, 2nd ed. (Cambridge 1973), p. 415.

known people have been capable. Such a faith does not fall within my category of 'rational disagreement'. The only possible rational conclusion from the evidence has been summed up in the subtitle of an important book on chronology and oral tradition published this year, 'Quest for a Chimera'.¹

In the absence of dated written documents, archaeology, and archaeology alone, gives us a chronological framework. The difficulty faced by Schliemann, his contemporaries and immediate successors, was that they had too much leeway. It is sufficient to consider the 1893 volume of Eduard Meyer's *Geschichte des Altertums*. Meyer had studied all the available archaeological publications, possessed as penetrating a mind as anyone in the field, and had the further advantage of his knowledge of both hieroglyphics and cuneiform. Yet the best he could offer, apart from fixing the *floruit* of Mycenaean civilization in the fifteenth century B.C. from Egyptian synchronizations, was a very vague pattern which looked like this: the Stone Age was followed on both sides of the Aegean by a common culture he called 'Trojan', which was replaced on the mainland, in particular, by the Mycenaean. The latter was so advanced in technique and 'civilization' that we must allow a long gestation period, starting perhaps about 2000 B.C., and a long declining period until the appearance of what we now call 'geometric pottery'. Homer he placed in the tenth century B.C. and he expressed bewilderment, tinged with indignation, that neither Niese nor Busolt nor Wilamowitz nor Beloch saw the obvious point that Homer was post-Mycenaean.²

Meyer was too disciplined and historically minded to mislead himself into thinking he knew more than his evidence permitted, at least about the Trojan War. He accepted the historical kernel, and no more, as we have seen, and he complained specifically that 'the expedition against Troy cannot be set into a firm historical context'.³ Others, lacking the qualities I have just mentioned, could not resist the freedom offered by centuries among which to roam and they provided such contexts and combinations as suited their fancy. Today the margin has been reduced drastically, so that few contexts or combinations are any longer available. I need not catalogue all the advances of the past hundred years that have brought about this result. I shall mention merely Furumark's establishment of a chronology of Mycenaean pottery, not merely because it was perhaps

¹ D. F. Henige, *The Chronology of Oral Tradition* (Oxford 1974).

² *Geschichte des Altertums* ii. 120-33.

³ *Ibid.*, p. 207.

the most important single contribution in the struggle for a chronology but also because it best exemplifies the difficulties today.

Even archaeologists have constantly to be reminded, though they of course know it, that a pottery chronology such as Furumark's is not the equivalent of a chronology of the kings of England. What Caskey said of the date of the destruction of Troy VIIa, that we 'must allow a latitude of some twenty or thirty years toward the end of the thirteenth century', applies to every other archaeologically based date. Professionals appreciate that when they see such dates as *c.* 1260 scattered throughout an archaeological publication; the layman does not, and it is astonishing how often the professionals proceed to forget it themselves. They forget it because it is an intolerable obstacle: a latitude of twenty or thirty years is too wide when one is seeking a historical context for a war, in our case for the Trojan War. But it is too narrow for the freedom to roam which Schliemann and his immediate successors enjoyed, and that is why I spoke of the paradox of increasing knowledge.

We find ourselves in this predicament because Homer provides no context. Homer's war, the war of the poems and of the tradition, is a timeless event floating in a timeless world, and, in the sense in which I have been using the word, in a non-contextual world.¹ The story of Paris and Helen and Menelaus is the proximate cause, like the assassination at Sarajevo in 1914, but it is not a context. Other peers have been assassinated and other noble ladies have been abducted without embroiling half the world in a great war. What I have been calling a context, after Eduard Meyer, is the complex social and political situation, in and between the 'nations' involved, which led to war in one or another case, not in others; not to war in general, but to a specific war, waged by specific combatants on a specific scale, and so on.

The archaeology of Troy has added nothing. But the archaeology of mainland Greece, Asia Minor, Cyprus, and Syria, which produced important new documents, offers hitherto unknown or unappreciated possibilities. We know that the end of the thirteenth century saw widespread devastation throughout the Peloponnese and in central Greece, the break-up of the Hittite empire, destruction in Cyprus and in northern Syria.

¹ See my 'Myth, Memory and History', *History and Theory*, 4 (1965) 281-302, reprinted with revisions in Finley, *The Use and Abuse of History* (London and New York 1975), ch. 1, esp. pp. 14-17.

Documents from Cyprus and from Ugarit in northern Syria, tantalizing and fragmentary though they are, leave little room for doubt that some form of massive marauding activity lay behind these almost simultaneous devastations, and more and more experts are coming to link that activity with two Egyptian texts, long known, from which the appellation 'Sea Peoples' is derived. And the temptation grows to place the destruction of Troy VIIa in the same context.¹

So long as the obdurate silence from and about Troy persists in contemporary documents, that can be no more than an hypothesis. But it is an hypothesis which provides a recognizable and plausible context; one, furthermore, which arises from the ground, so to speak, not from the stratosphere, unlike recent suggestions that the Trojan War was an 'unsuccessful attempt to restore a falling empire',² or an 'expedition to secure the Hellespont' made necessary because Troy had become an 'untrustworthy guardian'.³

All such suggestions, the plausible as well as the implausible, contradict substantially the Homeric picture of the Trojan War, and that brings me to a methodological principle. Before stating it, I must stop to stress that it is not only archaeology which has advanced enormously since Schliemann's day. Our understanding of oral traditions, and of heroic poetry as a form of oral tradition, has advanced equally, and I am unable to understand why this new knowledge does not receive the same welcome as the archaeological (when its existence is noticed at all, which is not always the case). To argue against the comparative evidence, with Caskey, that "'faith" in the value of early Greek tradition is a quite respectable possession',⁴ is to abandon historical inquiry for a quasi-theological concept. Without faith, for which no foundation is offered or can be offered, reason suggests that when Homer (or any other oral tradition) conflicts directly with written documents (in our case the Linear B tablets) or archaeological finds, with respect to the past which Homer appears to be, and no doubt believed himself to be, narrating, Homer must be abandoned.

¹ Finley, 'The Trojan War', *Journal of Hellenic Studies*, 84 (1964), 1-9; on the more recently discovered documents, see G. A. Lehmann, 'Der Untergang des hethitischen Grossreiches und die neuen Texte aus Ugarit', *Ugarit-Forschungen*, 2 (1970), 39-73.

² G. S. Kirk, 'War and the Warrior in the Homeric Poems', in *Problèmes de la guerre en Grèce ancienne*, ed. J.-P. Vernant (Paris and The Hague 1968), pp. 93-117, at pp. 105-9.

³ Caskey, *op. cit.*, p. 11.

⁴ *Ibid.*

That is my methodological principle, and I shall briefly illustrate how little remains of Homer today as a witness for the world in which the Trojan War is traditionally held to have occurred, at those points for which either of the two kinds of evidence I have just mentioned is available. It would almost be enough to compare the relatively lengthy and optimistic list of Homeric–Mycenaean parallels in the material remains to be found in Helen Lorimer's *Homer and the Monuments*, published in 1950, with the paltry half dozen or so that survived by the time Kirk's *Songs of Homer* appeared in 1962.¹ Since then, the Homeric palace and the Homeric war-chariot have been jettisoned, together with their accoutrements.² And, finally, the worst blow of all: the surrender of the last bastion, Homer's 'Mycenaean geography'. In the second edition of *Documents in Mycenaean Greece*, published last year, Dr. Chadwick summed up his present conclusions on this subject: 'I believe the Homeric evidence to be almost worthless. . . . One major reason is precisely the complete lack of contact between Mycenaean geography as now known from the tablets and from archaeology on the one hand, and from Homeric accounts on the other. The attempts which have been made to reconcile them, as on p. 143 [of the first edition], are unconvincing.'³

This collapse of Homer on the witness-stand, it will surely have been noticed, is restricted to material objects, and the cross-examination was based on the discoveries not at Troy but at a host of places in Greece and elsewhere, other than Troy. We are then left with large areas of human behaviour in which either archaeology or contemporary documents, or both, fail to offer any controls for the Homeric tales, at least in the present state of knowledge; areas, furthermore, in which archaeology alone, without documents, is never likely to do so. How are we to judge our ancient witness when there is no common ground, when the other types of evidence neither conflict nor contradict nor corroborate? The range of topics to which that question applies runs the gamut from religion and sexual relations to the Trojan War itself, the one topic with which I have been con-

¹ pp. 110–12.

² H. Drerup, *Griechische Baukunst in geometrischer Zeit* [*Archaeologia Homerica* ii, ch. O (Göttingen 1969)]; P. A. L. Greenhalgh, *Early Greek Warfare* (Cambridge 1973). Kirk, 'War and the Warrior', admittedly more optimistic on this score than in his earlier *Songs of Homer*, does not convince me that he has made out a case in defence of the *Iliad* as a valuable source for the 'history of late Bronze Age warfare'.

³ p. 415.

cerning myself. Neither the war nor Troy itself is mentioned in any contemporary document in any language, from any excavation, so far as I know. The question thus narrows to archaeology and the Homeric Trojan War.

Schliemann, we remember, began his archaeological career by digging for the palace of Odysseus, the hut of Eumaeus, and so on, and he was promptly rebuked: 'a little more criticism might have saved him a good deal of trouble'. He then moved on to Troy, seeking Priam's palace and Hector's grave and the camp of the Achaean besiegers. But this time, not only was he not rebuked, he was followed with growing enthusiasm by a century of archaeological efforts and claims. Yet, I submit, the questions he posed at Troy were as unreal as those he had posed in Ithaca, unreal in the sense that archaeology cannot be expected to produce answers (unless it turns up documents).¹ What we call the Trojan War was, after all, only a single siege supposed to have occurred more than 3000 years ago, of a fortified city that remained in continuous occupation for at least another 1500 years thereafter, during which there were two massive earth-moving operations apart from the normal year-by-year constructions and demolitions. How is it imagined that archaeology can confirm the following matters of fact, which I repeat once again from Blegen's statements of what he believes the excavations to have confirmed (accepting for the sake of the argument that Hissarlik is Homer's Troy): (1) Troy was destroyed by a war; (2) the destroyers were a coalition from the mainland of Greece; (3) the leader of the coalition was a king named Agamemnon; (4) Agamemnon's overlordship was recognized by the other chieftains; (5) Troy, too, headed a coalition of allies?

The only answer I am able to discover to my question in all the outpouring on the subject is that archaeological discoveries have not contradicted Homer on those five points. But that is not an answer, it merely stands the question on its head. If archaeology cannot confirm such 'facts', it cannot, for the same reason, falsify them (unless it produces some such extreme evidence as that mainland Greece was unoccupied at the time, or similar improbabilities). The Trojan War is not unique in this respect. No one turns to the spade to test the account of Attila in the *Nibelungenlied* or the South Slav epic version of the

¹ This has most recently been argued with great vigour by R. Hachmann, 'Hissarlik und das Troja Homers', in *Vorderasiatische Archäologie . . . Anton Moortgat . . . gewidmet*, ed. K. Bittel *et al.* (Berlin 1964), pp. 95-112.

battle of Kossovo, not merely because it is unnecessary to do that, given the availability of documentation, but also because it would be an acknowledged waste of time. The fact that we have no documentation for the Trojan War does not alter the limits of archaeology in the slightest, it merely injects a large element of melancholy into the situation, and of desperation. There is at present a strong current among non-classical archaeologists to divorce themselves from what they have called 'counterfeit history' in one or another, equally pejorative, synonym. I have elsewhere expressed my disagreement with their approach and their arguments¹ but I must confess that Schliemann's Troy provides them with powerful ammunition.

Even Schliemann conceded distortions and fictions in the Homeric tale. What are the stigmata, I ask (not for the first time), which expose a distortion, an anachronism, or an outright fiction so as to distinguish it from a supposed 'reminiscence' or historical fact? In 1878, Charles Newton, Keeper of Greek and Roman Antiquities in the British Museum, reviewing Schliemann's book on Mycenae at great length for the *Edinburgh Review*, wrote: 'How much of the story of Agamemnon is really to be accepted as fact, and by what test we may discriminate between that which is merely plausible fiction and that residuum of true history which can be detected under a mythic disguise in this and other Greek legends, are problems as yet unsolved, notwithstanding the immense amount of erudition and subtle criticism which has been expended on them.'² Ninety-six years have gone by since Newton wrote that, and his conclusion remains the least sceptical one we have a right, on the evidence, to hold today. Some of us are more sceptical: Homer's Trojan War, we suggest, must be evicted from the *history* of the Greek Bronze Age.³

¹ 'Archaeology and History', *Daedalus*, 100 no. 1 (Winter 1971), 168-86, reprinted with revisions in *Use and Abuse of History*, ch. 5.

² Reprinted in his *Essays on Art and Archaeology* (London 1880), p. 249.

³ Homer on post-Mycenaean social institutions is, in my view, another matter, as I have argued most recently in my presidential address at the 1974 meeting of the Classical Association, 'The World of Odysseus Revisited', to be published in the Association's *Proceedings*.