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Troy in recent perspective

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Abstract¹

The historic series of excavations of Hisarlık-Troy have been continued over the last 15 years by a collaboration between teams from the universities of Tübingen and Cincinnati with fruitful results. Over the year 2001 however the director, Manfred Korfmann, attracted sharp criticism from colleagues, largely through the medium of the press, for his methods and publications. He was accused of exaggerating the importance of the site in the Late Bronze Age, particularly as a political capital and trading centre of Anatolia, and more specifically of unduly inflating the results of his investigations of the lower city. A symposium was convened by the University of Tübingen in February 2002 with a view to discussing these criticisms and the defence in an academic atmosphere. The four authors of this article attended the Tübingen symposium. After listening to the contributions it seemed to us that an assessment of the issues from our respective view-points would be timely: thus a detailed consideration of the archaeological questions, a review of the notable recent progress in Hittite sources firming up the historical geography of western Anatolia, and an evaluation of Troy's position in Late Bronze Age trade. In all these areas we conclude that the criticisms of Korfmann are themselves considerably exaggerated.

Özet

Hisarlık-Troya'da sürdürülmekte olan önemli kazıların son 15 yılı Tübingen ve Cincinnati üniversitelerine bağlı ekiplerin işbirliği ile sürdürülmekte ve verimli sonuçlar elde edilmektedir. Ancak, 2001 yılında kazı başkanı Manfred Korfmann, çoğunluğu basın yoluyla olmak üzere, uyguladığı yöntemler ve yayınlarıyla ilgili olarak meslektaşlarının keskin eleştirilerine maruz kalmıştır. Korfmann, Geç Bronz Çağ'da yerleşimin önemini abartmakla suçlanmıştır. Bu suçlamalara özellikle Troya'yı Anadolu'nun politik başkenti ve ticaret merkezi olarak sunması ve aşağı şehirde yaptığı incelemelerin sonuçlarını haksız olarak abartması neden olmuştur. 2002 yılı Şubat ayında, bu suçlamaların tartışılması ve akademik bir ortamda savunulması için Tübingen Üniversitesi tarafından bir sempozyum düzenlenmiştir. Bu makalenin dört yazarı da bu Sempozyuma katılmıştır. Katılımcıları dinledikten sonra, herbirimizin görüş açısından sorunların değerlendirilmesinin uygun olduğunu düşündük. Böylece, arkeolojik soruları detaylı olarak değerlendirdik, Batı Anadolu'nun tarihsel coğrafyasıyla ilgili bilgilerimizi sağlamlaştıran ve yeni yayınlanmış önemli Hitit kaynaklarını yeniden inceledik ve Geç Bronz Çağ dönemi ticaretinde Troya'nın durumunu yeniden değerlendirdik. Tüm bu alanlarda yaptığımız çalışmalar sonucunda esasen Korfmann'ı eleştirenlerin abarttığı sonucuna vardık.

The ruins of Hisarlık/Troy are without question one of the great archaeological sites of Anatolia. With a sequence of occupation spanning the entire Bronze Age, ca. 3000–1000 BC, and a history of investigation extending back to 1870, the site has few rivals, let alone equals: perhaps Boğazköy/Hattusa investigated since 1906 for the Middle-Late Bronze Age and Kültepe/Kanesh investigated since 1925 for the Middle Bronze Age. Other potential comparables, such as

Malatya-Arslantepe and Tarsus, have not received such intensive attention or yielded such results. Troy's high public recognition is obviously due partly, but not solely, to its literary associations. Its first full scale excavator, Schliemann, must for all his faults be reckoned the father of Anatolian archaeology, and probably remains the most publicly recognised of all Anatolian archaeologists, certainly in his native Germany. His excavations from the period 1870–1890 were extended and brought to a conclusion by Dörpfeld in 1893–1894. Thereafter a team from the University of Cincinnati under Blegen undertook a campaign of sober reassessment in the years 1932–1938.

¹ Professor Korfmann kindly read and commented on the manuscript, but the opinions expressed here remain our own.

More recently since 1988 a major international expedition has resumed work at the site under the direction of Manfred Korfmann of the University of Tübingen with the collaboration of a team from the University of Cincinnati and other specialists in the field. Korfmann came to Troy with an established reputation in Anatolian archaeology and an excellent record of scientific publication. Besides his funding received from his university and the German state, and the funds contributed by his collaborators, Korfmann has been successful in winning very substantial support from German industry, in particular from the firm Daimler-Chrysler. This has enabled him to run very properly funded operations for more than 15 seasons on a scale which less efficacious colleagues may well envy.

Korfmann's goals in his current round of investigations at Troy have been generally to apply modern methods and techniques to old problems as left by Blegen, and Schliemann and Dörpfeld. This has involved painstaking re-examination and reassessment of the excavated area along with scientific conservation and restoration. Environmental research and survey have also formed a prominent part of the effort. But one specific and declared goal has from the start been the intention to investigate the Bronze Age lower city.

A regular, not to say predictable, reaction of tourists visiting the site, particularly perhaps those knowing only Homer, has always been: 'Oh, but it is so small!'. But it has always been clear that the site as excavated by Schliemann and Blegen is only the citadel. These excavators themselves were well aware of this and did not doubt the probable existence of a contemporary lower town, though they hardly investigated this feature. The more that our knowledge of Middle-Late Bronze Age archaeology of Anatolia has expanded, the clearer it appears that Troy represents a typical citadel of the period, for which lower towns are a typical feature. Thus, before Korfmann's operations, no Anatolian archaeologist would have doubted the presence of a lower town. Its investigation then may well be understood as having been one of the principal goals.

Korfmann's campaigns of the 1990s have been very successful and what may be termed 'high profile'. They have generated substantial publications, scientific and popular. As example of the former, the annual *Studia Troica*, now boasting 11 volumes, records all the yearly work and technical reports. Korfmann and his lieutenants lecture widely in Europe and the United States, meetings which are always well attended and popular in the best sense of the word — making scientific results accessible to the public in interesting and intelligible form, and generating an atmosphere of excitement and support. This archaeological style may be contrasted

with an alternative: the reluctance to talk up and explain in context the significance of particular excavations, and the failure to publicise, sometimes alas even to publish the results. There can be no question as to which style is likely to win most support and the associated level of funding.

Public interest in matters Trojan aroused in Germany led to the mounting of a major exhibition under the title *Troia, Traum und Wirklichkeit* (*Troia, Dream and Reality*), as a companion to which a bulky and lavishly illustrated volume has been produced. This ranges well beyond the limits of the present excavations on to such matters as Schliemann's life and work, 'Priam's treasure' and its eventful history, and Troy in literature and art, Classical, medieval and modern. The text consists of over 50 essays by Korfmann, members of his team and other collaborators, and other specialist scholars, on the various aspects of the subject.

A notable feature in Korfmann's more popular publications has been reconstructions of various parts of the city, often set within its landscape, painted by the artist C. Haussner. Walls and houses are reconstructed from the surviving plans, and the appearance of the structures up to their roofs and battlements is suggested on the basis of archaeological evidence combined with a knowledge of traditional local building techniques. Thus far this is regular archaeological practice, and indeed notable advances are being made by the introduction of computer-generated images into this field. Where the practice may stray on to more controversial ground is in the additional restoration of buildings not attested by surviving remains in order to complete the picture. In the case of Troy, this results from the well established fact that the entire central area of the Troy VI citadel, its upper part where doubtless the most important buildings stood, was razed by Classical builders in order to level the site for the construction of the temple of Athena. Thus the only surviving foundations within the citadel enclosure are those of the large buildings immediately within the citadel wall. This total destruction of the most important part of the Late Bronze Age citadel, both buildings and contents, is a grievous archaeological loss. The painted reconstructions however show a hypothetical central palace and a tier of surrounding buildings for which no evidence survives.

This is even more marked in the case of the lower city. The excavators have found limited evidence for this, in the form of parts of a Late Bronze Age defensive system with a possible wall, a palisade and two ditches, enough to speculate on its probable course around the settlement, and remains of buildings both around the foot of the citadel wall and further off. On the basis of this very limited evidence, the paintings reconstructed an

... fortified lower city, complete with buildings. This may be defended as simply offering a representation of what may well have been the appearance of the Late Bronze Age city, it may just as well be criticised for greatly exceeding the available evidence.

A step further was taken in the exhibition, which presented a large model or maquette showing the entire walled citadel and lower town. This seems to have had the effect of bringing into the open a strand of academic dissatisfaction with Korfmann's work. This was articulated principally by a Tübingen university colleague of Korfmann, the ancient historian Frank Kolb, who in a book published in 1984 had characterised Troy as a 'miserable little settlement', which could not 'raise a claim to the designation as a city'. Interviewed on the subject of the Troy exhibition by the *Berliner Morgenpost*, he defended his position against the implications of Korfmann's reconstructions, describing them in such terms as 'fiction', 'figments of fantasy' and 'the media hot air balloon of the Troy excavations', and accusing Korfmann of deliberately misleading the public. His remarks found a ready audience in the same media at the start of a long hot summer, and Kolb was encouraged to sharpen his offensive and language, branding Korfmann the 'von Däniken of Archaeology', and employing other such unacademic barbs. All this took place while Korfmann was out of the country, on excavation at Troy.

Kolb claimed to be speaking for a significant number of German academics, who kept their views to themselves for fear of accusations of envy, clearly not a charge to which he felt himself vulnerable. His offensive was joined by Dieter Hertel, Privatdozent at the Institute for Classical Archaeology in Munich, who had worked with Korfmann at Troy and now published a booklet, *Troia. Archäologie, Geschichte, Mythos* (2001). This work plays down the significance of the site of Troy and of Korfmann's operations there.

Kolb's aggressive and intemperate language more or less speaks for itself. It did not find favour with the Rector of Tübingen University who demanded a public retraction and apology. The university further convened a 'scientific symposium' under the title 'The meaning of Troy in the Late Bronze Age', which took place on 15–16 February 2002 before a large and excited public audience, and was attended by considerable media coverage. There were 13 invited speakers, approximately paired to put the cases for the 'prosecution' and 'defence' in the spheres of archaeology and excavation, trade and the environment, the historical-geographical background of the Hittite texts and the Homeric problem. Theoretically at least, ample time had been allowed for audience participation and

comment after each paper, but as always this depends on the speakers keeping to time, which is of course the exception rather than the rule. The hopes of the Rector and convenors of the symposium for a calm academic debate of the important questions where personalities and invective would be set aside were only very partially fulfilled.

The site of Troy has the misfortune to stand on not one but two academic fault-lines, one on either side of the Aegean: the Homeric problem concerned with the historicity (or otherwise) of the *Iliad*; and the problem of Anatolian historical geography of the Arzawa lands as reconstructable (or not) from the Hittite texts. Both topics have been known to evoke strong emotions from those involved, and anyone working at Troy will have difficulty in keeping clear.

The authors of this article attended the symposium as invited observers, and had some opportunity to make their English voices heard amid the often heated German exchanges. Since each of us is a specialist in one aspect of the subject, we thought that it would be of interest to readers of this journal to see our assessment of the respective debates and our own views on the issues. We thus offer our presentations under the headings (with authors' initials):

The archaeology of the site: citadel (JDH) and lower town (DFE)

The historical geography of western Anatolia in the Hittite texts (JDH)

Bronze Age trade in western Anatolia (AGS/ESS)

While each of us has obviously drafted one section, we have each read the others' contributions and offered comments which have been incorporated as appropriate.

The archaeology of the site

The citadel

The 'prosecution' has devoted effort to denying that the site of Troy could represent a 'Residenzstadt'. Their argument is generally conducted by setting up criteria for such an entity and then demonstrating that Troy does not meet these.

The grounds on which Troy is denied this status are: (1) the size and character of the walls, gates and surviving buildings; (2) the lack of finds of materials expected of a palatial centre, such as written documents, seals and sealings, luxury goods, traces of wall paintings, sculpture etc. This line of argument is advanced principally by Kolb 2002b, and by Hertel in his paper at the Tübingen symposium (Hertel 2002). They are of course much aided in this argument by the total disappearance of the greater and most significant part of the citadel, which has been noted above.

To establish Troy's failure to qualify as a Residenzstadt, it is compared with the other palatial centres of the Late Bronze Age: Boğazköy, Alaca, Kültepe, Beycesultan, and outside Anatolia with Mardikh, Ras Shamra, Knossos, Mallia, Phaestos, Mycenae, Tiryns.

Considerable special pleading is evident in these arguments. In the context we must, for example, ask whether it is purely coincidental that Kolb's article prints all the plans offered for comparison at a larger scale, sometimes much larger, than that of Troy (fig. 1). Hertel 'cherry-picks' discoveries of the types noted above as criteria, and emphasises Troy's deficiencies in these respects. In general this line fails to compare like with like.

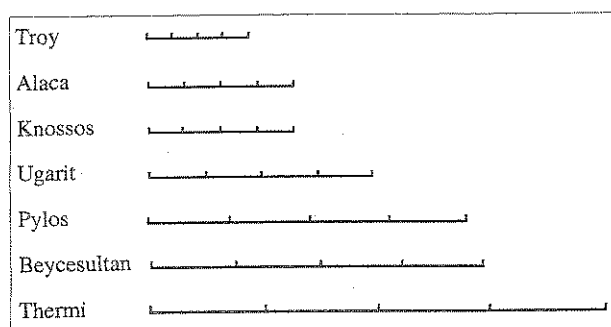


Fig. 1. Scales at which the various city plans adduced by Kolb (2002b) for comparison with Troy are reproduced. Each scale is 40m

By way of some corrective, we may consider the comparison of Troy with Boğazköy, with special attention to the two citadels Hisarlık and Büyükkale. It should be hardly necessary to emphasise that this is a comparison between an imperial Anatolian capital of the late 14–13 centuries BC and what would never have been claimed to be more than a regional capital. We should also bear in mind the shape and extent of Hattusa through all its second millennium history until it was overlaid by this imperial expansion, i.e. the citadel plus lower city, perhaps also some of the slope between, but without the vast circuit of the upper city (fig. 2).

Rather than following Kolb's practice, we shall reproduce the plans of the imperial citadel Büyükkale and the Troy citadel at the same scale (fig. 3). Making allowances for the contrast between imperial and regional, we note that Troy is not as inferior in size of walls, gates, houses etc. as has been suggested. The style of course is very different: construction of the walls, layout of the gates and the Troy free-standing megaron-type buildings as against the Büyükkale building units grouped round a series of courts. A more appropriate comparison for Troy VI than the Beycesultan level V

burnt palace (Middle Bronze Age) as offered by Kolb is the megaron complex of Beycesultan level II, the so-called 'Little Palace' (fig. 4). Indeed further comparable both in size and character is the Iron Age royal citadel of Gordion (fig. 5). Thus the comparison of these three citadels, Troy level VI, Beycesultan level II and Iron Age Gordion, suggests what we should expect of western royal citadels against those of central Anatolia and further east.

Hertel's 'proof' that Troy cannot be a Residenzstadt relies heavily on the absence of monumental sculpture, wall painting traces, written documents and seals or sealings. Granted that the absence of written material is a problem, we may well consider other explanations besides lack of status and importance. We also note the similar absence of such criteria of rank from Beycesultan (both levels V and III–II) and Gordion. Let us however persist with the Büyükkale comparison.

We note there that the uppermost terrace of buildings on the east side of the upper court has disappeared entirely leaving only the rock-cut beddings for masonry. (These buildings are however restored in the wooden model of Büyükkale currently in the German excavation house at Boğazköy.) Written material on Büyükkale comes only from the tablet archives of buildings A, E, K, and sealings only from the south corridor of building D. We may ask, what if these four sites, a small part of the whole, had been lost? Other material finds from Büyükkale level 3a, the imperial period, are notably sparse: a stele of Tudhaliya IV and other fragments of inscriptions, probably of the same king, and some fragments of lion sculpture, but no wall painting traces.

It would seem that the criteria assembled to define a Residenzstadt are less a coherent group of features of universal application than a collection of some of the finer recoveries from the Minoan-Mycenaean world on the one hand and the Hittite on the other, put together to deny Troy that character. Now it may well be that the civilization of western Anatolia did not reach the splendour or grandeur of the Minoan-Mycenaean or the Hittite worlds, but that is not really the point. The question actually is whether the archaeological site could represent the seat of an Arzawan king, as Wilusa is recorded to have been. Here it could be that the term 'Residenzstadt' might be taken to imply more than it actually means, if for example 'Residenz' conjures up the opulent display found in those palaces of the German Länder. 'Provincial/regional capital' is perhaps a less heavily charged term and may more exactly describe the site of Troy. For further consideration of what this might mean in Anatolian terms, compared with the recovered remains at Hisarlık, see below.

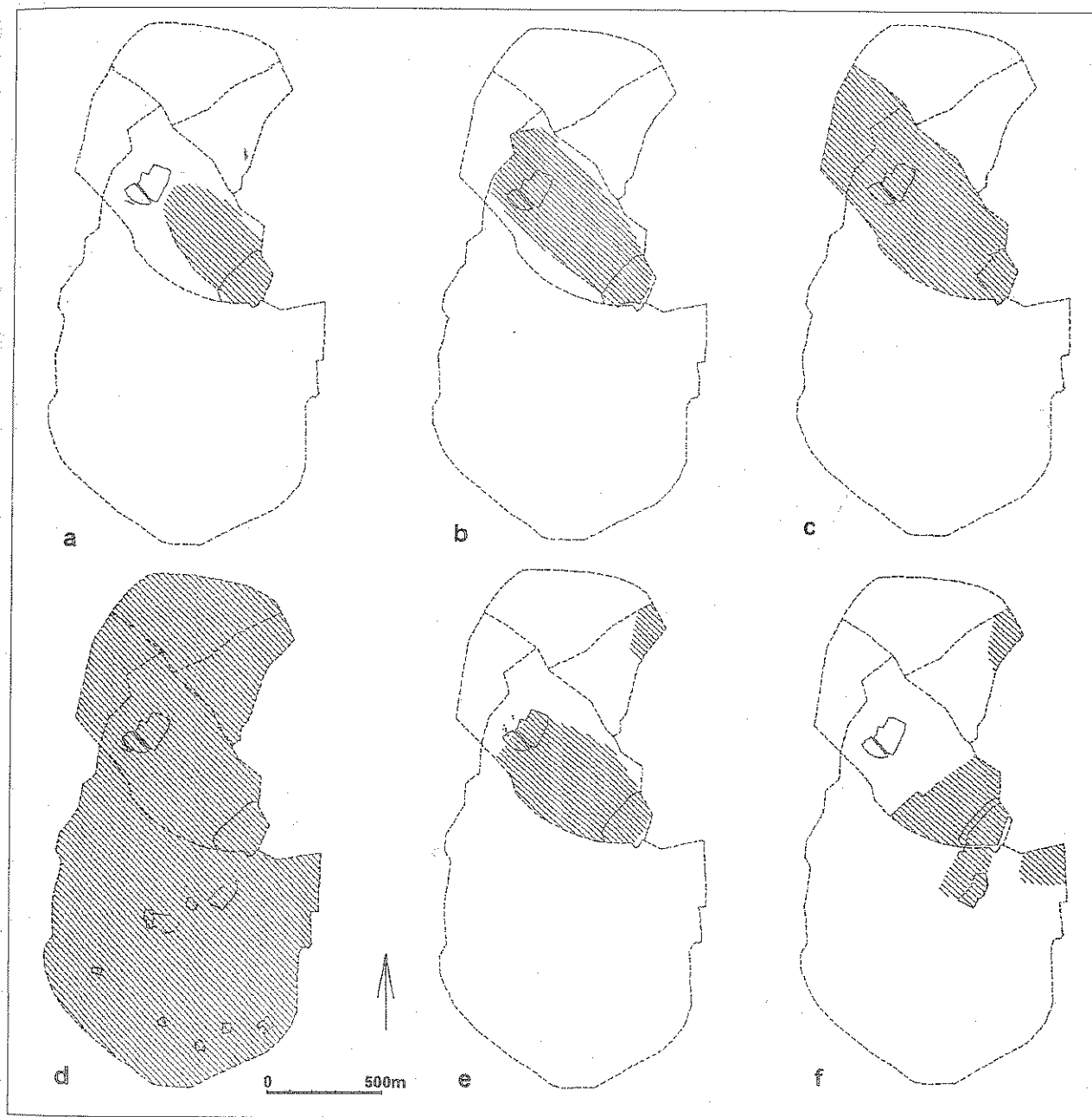


Fig. 2. Schematic site plans of Boğazköy showing areas of occupation in (a) pre-Hittite period; (b) Old Assyrian colony period; (c) Hittite Old Kingdom; (d) Late Empire (final Hittite phase); (e-f) early and late Phrygian periods (from Neve 1992: Abb. 15)

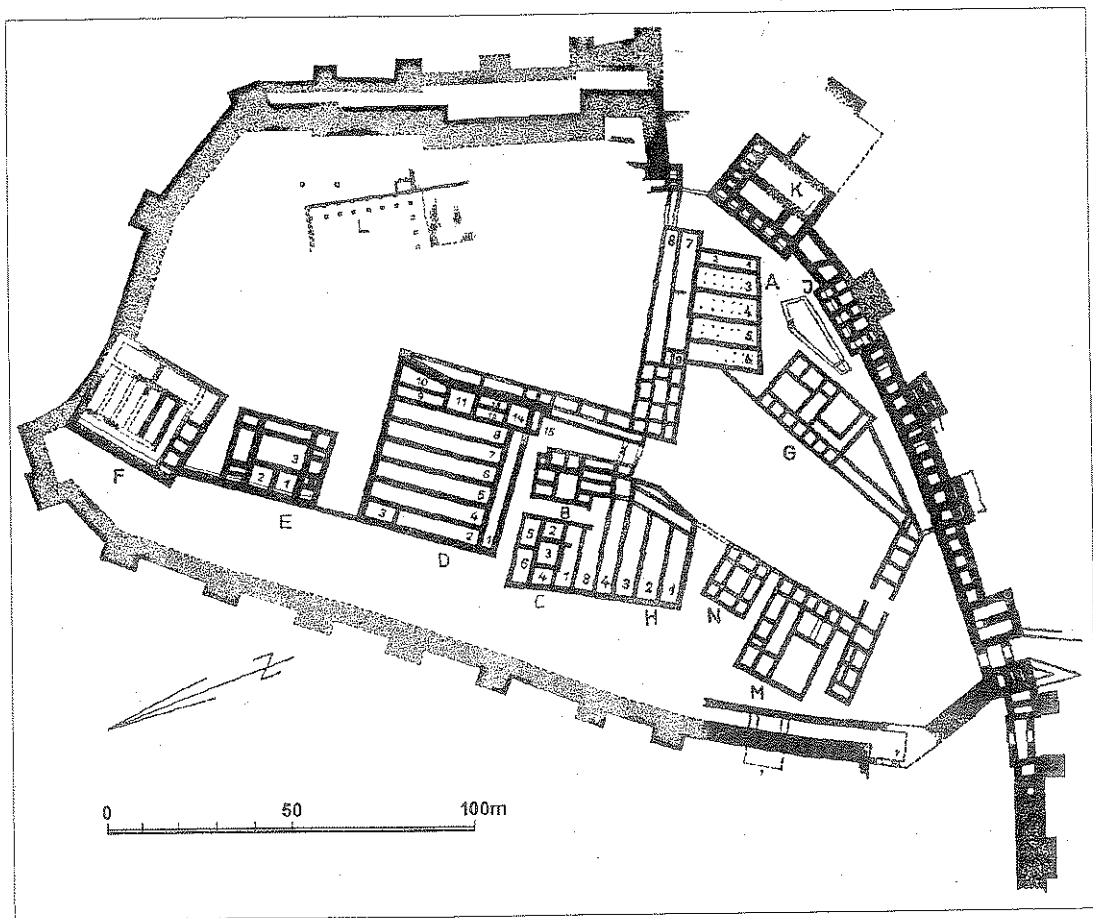
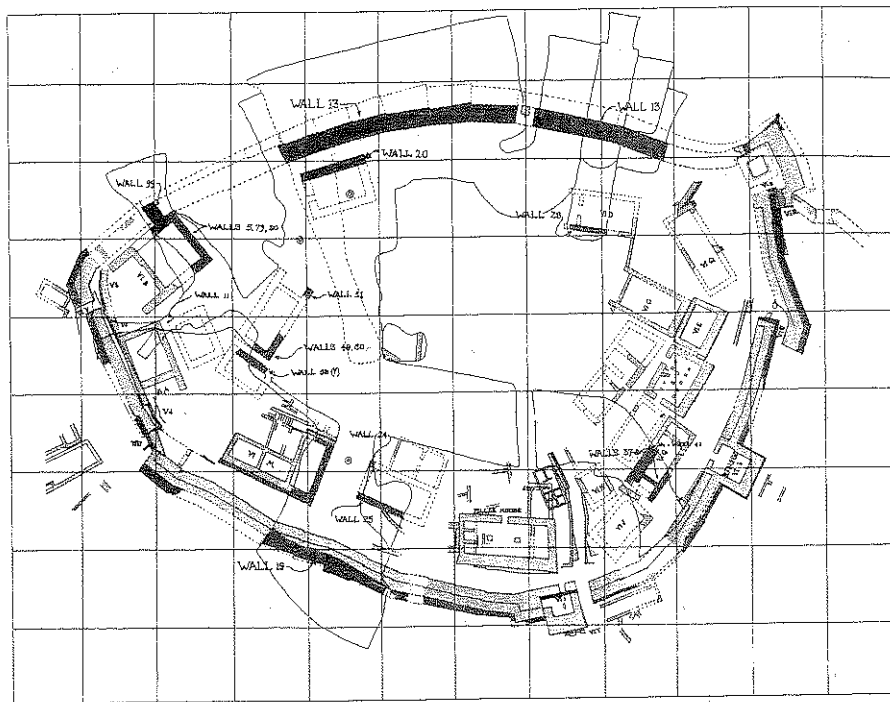


Fig. 3. Plans of Troy citadel and Büyükkale reproduced at the same scale (Easton 2002: fig. 202; Neve 1992: Abb. 18)



Fig. 4. Plan of Beycesultan, east summit level II (end of Late Bronze Age). The basic plan of the individual building units is the megaron. The lower figure, though unclear, shows the plan reduced to the same scale as fig. 3 (from Lloyd 1972: fig. 3)



Fig. 5. Gordion, citadel plan, reproduced at the same scale as fig. 3. Note the monumental gate and the megaron units which comprise the citadel (from Young no date: 5)

The lower city

Introduction

Since the discovery of the site by Franz Kauffer in 1793 it has been recognised that on the sloping plateau to the south of the citadel there were the remains of a lower city of Hellenistic and Roman date. Korfmann has now posited in quite concrete terms the existence also of a Late Bronze Age lower city on the same terrain. The

idea was first developed in extenso in 1992 (Korfmann 1992b) and has since been amplified in the light of excavation results. As presented in the exhibition catalogue it supposes a settled area covering ca. 270,000m² and stretching ca. 400m southwards from the citadel. The population is estimated at 5,000–10,000 depending on the degree of crowding and whether the houses were multi-storeyed. Haussner's reconstructions

and the model show a bustling, built-up city surrounded by a heavy, crenellated fortification wall and, about 100m further out, a defensive ditch bridged by periodic causeways. Each causeway is straddled on the inner side of the ditch by a short palisade with a central gate (Korfmann et al. 2001: 397, figs 23, 26, 77, 462, 465). The reconstructions depend partly on surface and geophysical survey, and also on excavated evidence from a number of areas: several trenches immediately around the outside of the Troy VI citadel, the largest being on the west side, two trenches ca. 150m to the south (H17, IKL 16–17), a trench ca. 400m to the south (yz 28–9) and a number of supplementary soundings on that southernmost fringe of the plateau (fig. 6; Korfmann et al. 2001: fig. 425; Korfmann 2001a: fig. 1).

It is partly the disparity between the comprehensive scale of the reconstructions and the limited size of the areas so far dug (2–3% of the lower city area) which has led Hertel and Kolb to characterise the former as ‘pure fantasy’, ‘a dream’, ‘fiction’ (Hertel 2001: 44; Walter 2001; Kolb 2002b: 8; 2002c: 3), accusations repeated at the symposium. They support this by contesting Korfmann’s interpretations at many points, maintaining that the middle part of the plateau was only sparsely occupied, the southern part not at all, that the lower town wall did not exist, and that the ditches and palisade were not defensive (Hertel 2001: 44–6; 2002: 17; Kolb 2002b: 13–21; 2002c). Korfmann’s population estimate should, they argue, accordingly be reduced to a maximum of 3,000 (Kolb 2002b: 19) or even 1,000 (Kolb at the

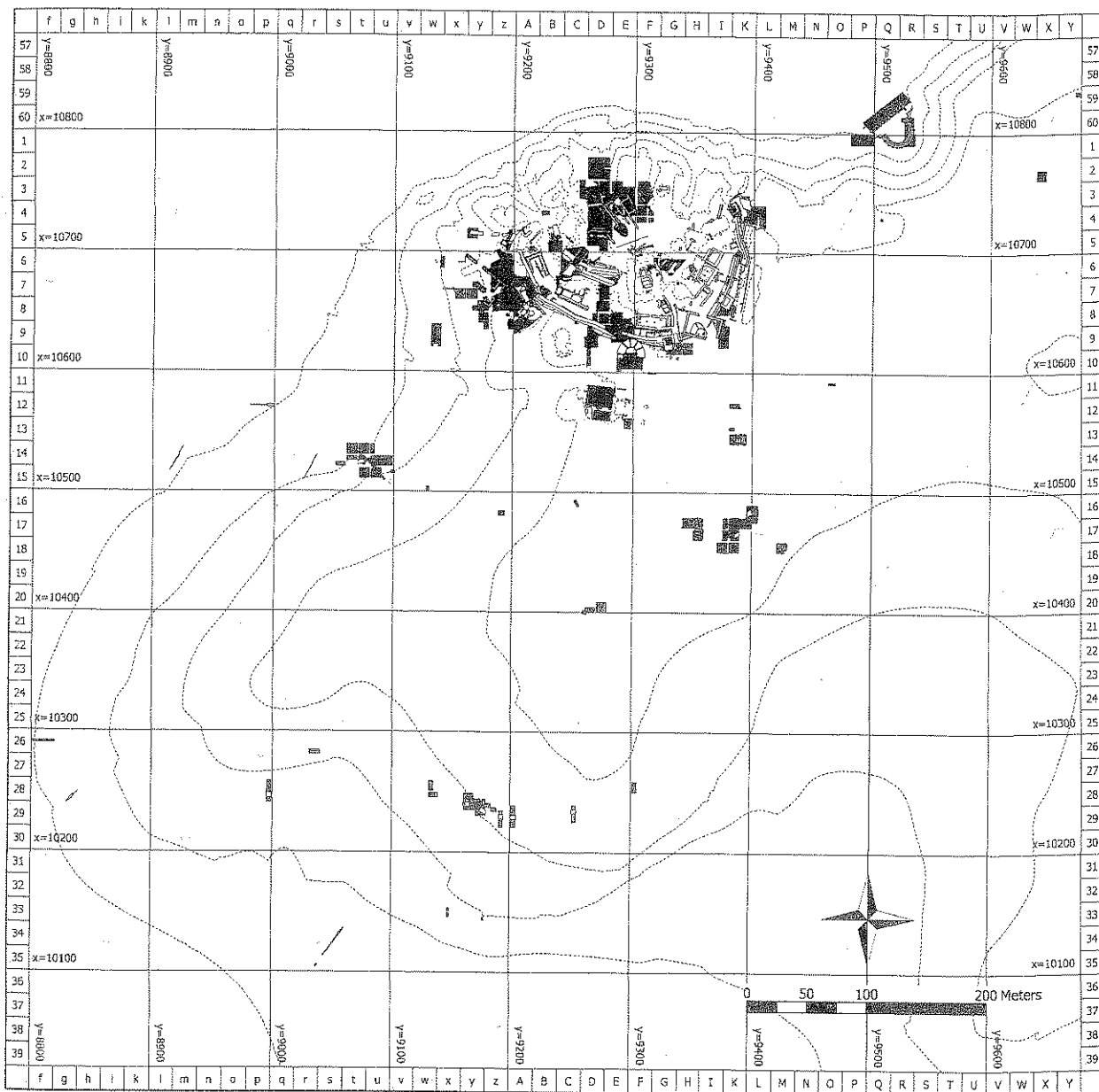


Fig. 6. Areas excavated, 1988–2002 (plan courtesy of Dr Peter Jablonka, Troia Projekt, Tübingen University)

symposium). But they go beyond such discussable matters to assert that the reconstructions represent a deliberate attempt by Korfmann to inflate the importance of his site with the object, Hertel suggests, of ensuring a continued flow of funding for his excavation (Walter 2001). To disguise the thinness of his evidence he has been 'confusing the layers' (*Berliner Morgenpost*; Kolb 2002a; Hertel 2002: 3, 7–8; Kolb repeatedly in the symposium) and is guilty of 'misleading the public' (*Berliner Morgenpost*; Kolb 2002b: 13; 2002c: 3).

At the symposium the attack in this area was led by Dieter Hertel in his lecture. There were reasoned replies from Peter Jablonka, who has himself excavated two of the critical areas in the lower city, and from Hans Peter Uerpmann, who is leading the bio-archaeological research at Troy. It was very unfortunate that Jablonka's paper, perhaps the most crucial of the whole symposium, was allowed only 20 minutes by the organiser. Support from the floor came notably from Brian Rose, who has been leading the post Bronze Age research at Troy, much of it in the lower city. It is evident from Kolb's website, however, that none of the arguments put forward have caused him to change his opinion in any material way; indeed all such contributions are there described as having been 'laughable' (Kolb 2002c: 7). This reaction contrasts starkly with the satisfaction generally felt by the prehistorians and Korfmann supporters that the criticisms had been thoroughly answered. Plainly there was no meeting of minds, and an exposition of the issues for a wider public seems called for.

Previous investigations

According to Kolb (*Berliner Morgenpost*) Korfmann has been trying to find evidence to substantiate an (irrational) conviction that Late Bronze Age Troy was a capital city.

Since 1988 he and his colleagues have been digging for traces of Bronze Age Troy. And in the meantime they have become convinced that the city of that period, in which people also chronologically place the Trojan War, was a metropolis, a great trading centre with everything that goes with it: a defensive installation on the top of the mound (which in the first years was further investigated) and, according to Korfmann, an enormous lower city (of which he has for the last five years been searching for evidence).

This waspish accusation completely overlooks the fact that Korfmann, like all good archaeologists, is building on the work of his predecessors. In his earliest seasons Schliemann made numerous soundings on the plateau to the south of the citadel (Schliemann 1874: Taf. 213; 1875: Plan 1). In 1884, having studied the topography and the pottery scattered across the surface, he sketched out the possible limits of a 'Homeric' lower

town not so very different from Korfmann's (Schliemann 1884: Plan 3). He associated it at that time with the remains of Troy II (Schliemann 1884: 62–3). A massive stone wall leading away from the northeast corner of the Troy II citadel he took to be one end of a circuit wall surrounding the lower town (Dörpfeld 1902: Taf III, wall BC), an explanation which is still very plausible. After the discovery in 1890 of Mycenaean pottery in a building of Troy VI, and the consequent revision to the dating of all the prehistoric strata, he resolved to investigate the lower city of Troy VI in 1891 (Schliemann 1891: 24). Death robbed him of the chance.

Dörpfeld and his team did, however, carry out some modest investigations in 1893–1894. Soundings on the western part of the plateau, 140m and 200m south of the Troy VI citadel, produced strata of VI directly above bedrock (Dörpfeld 1902: Taf. III, points A and B. Both areas have been investigated again by Korfmann). Dörpfeld's opinion was that 'with regard to the VIth stratum, ... the settlement of a large part of the lower city is demonstrated' (Dörpfeld 1902: 238). Götze felt justified in concluding that the extent of the Troy VI lower city closely matched that envisaged by Schliemann in his *Troja* plan, if anything stretching further to the south (Dörpfeld 1902: 236–8). A limit appeared to be set by some Troy VI cremation burials found 400m to the south of the citadel, just beyond the Hellenistic city wall (Dörpfeld 1894: 123; 1902: 536).

Blegen likewise recognised the probable existence of a Late Bronze Age lower city of undetermined size, and exposed significant remains of it in areas around the outside of the citadel walls (in z5, A7, GH9, K6–8).

It has thus become clear that the area occupied by the inhabitants of the site at the end of Troy VI extended out beyond the limits of the fortress, and ... there can be no doubt that an extramural lower town of undetermined size really existed (Blegen et al. 1953: 351).

He did little to investigate it elsewhere, but did establish that the Troy VI cemetery found in 1893 outside the Hellenistic city wall was much more extensive than Dörpfeld had been able to show. He did not, however, discuss the relationship between the two (Sperling 1991: 155). Korfmann's own initial investigations showed grey Minoan and Mycenaean wares widely scattered over the plateau together with Hellenistic and Roman pottery. Systematic taking of cores along a north-south axis produced repeated indications of Late Bronze Age settlement just above bedrock — as Götze had previously found (Korfmann 1991: 26). Reconsideration of Blegen's unconvincing 'crematorium' 200m west of the Troy VI cemetery suggested much more plausibly that it might be a burnt Troy VI house cut by Byzantine pits (Korfmann 1992b: 128).

Thus when Korfmann began to excavate outside the citadel it was already established that a built-up area surrounded the citadel in Troy VI–VII, that soundings further south on the plateau repeatedly produced material of the same period just above bedrock, that Late Bronze Age pottery was widely scattered over the western part of the plateau, that there was a building of Troy VI 450m southwest of the citadel and that the late Troy VI cemetery, if it lay outside the settlement as might be expected, represented an outer limit. Although the eastern part of the plateau is relatively unexplored, a probable limit in that direction was also known in that Late Bronze Age material had failed to appear in excavations in square O11 (Korfmann 1991: 26). Only a very thin deposit of Troy VI–VII material has since been found in X2 (Korfmann 1999: 26). The notion that there might exist a large lower city was thus far from being vain or irrational, but arose logically from previous observation.

What then have Korfmann's researches actually revealed, and how far do they justify the reconstructions he has given us? For the area immediately around the citadel results up to 1994 can be seen in Elizabeth Riorden's magnificent plan published as a supplement to *Studia Troica* 4 (Hueber, Riorden 1994). For later findings and for areas further removed from the citadel one must consult Korfmann's annual preliminary reports and other studies in *Studia Troica* 1–11. In what follows I shall examine the relevant excavation areas each in turn, outlining Korfmann's findings and evaluating the criticisms levelled against him.

Area immediately outside the Late Bronze Age citadel

The new excavations have hugely increased the evidence for Late Bronze Age occupation in this area. On the east side, in IK8–9, work in 1991–1993 revealed a series of substantial buildings with stone foundations extending Blegen's sequence of late VI–VII back to early VI or even perhaps V. There are associated features such as a stone pavement, hearth and grain bin (Korfmann 1992a: 30–1; 1993: 21; 1994: 24). The area was thus not, as Blegen thought, first settled after the destruction of Troy VI. On the south side, in EF9–10, small excavations within the Roman odeion have produced a probable middle VI wall set directly on bedrock, clay-lined storage pits, a stone pavement also of middle VI date and the wall of a very substantial house of late Troy VI (Korfmann 1994: 22; 1998: 41–2; 1999: 14–15). It is true that a little further west, in a narrow trench hard against the citadel wall in D9–10, there is no evidence of buildings before VIIb1 (Korfmann 2001a: 22–7, correcting earlier reports). But the sequence here may be comparable to that in A7 and K4, where in Troy VI a road ran along the face of the citadel wall and was only built over in VIIb.

The most plentiful results have come from the area below the Hellenistic and Roman sanctuaries to the west of the citadel (fig. 7). A full account cannot be attempted here, but may be traced in Korfmann's preliminary reports from 1994 onwards. The area was already settled in Troy V. Architectural remains show that in the areas excavated all three phases of Troy VI were represented, and late Troy VI by at least nine buildings, although due to later disturbance and overlying features none has been fully recovered (Korfmann 2001a: fig. 12). All these walls had substantial stone foundations¹ and some remained standing to 1.20m high. A cobbled street led through the houses to gate VIU until the latter was closed in VIIa. A stratum of burning and collapse marks the end of Troy VI in this area, and there is evidence for seven ensuing phases of Troy VII (Korfmann 1999: fig. 14b). The remains of VIIa, with their surface 1.5m above that of Troy VI, include those of a large terraced house with at least five rooms built in part on the wall stubs of the preceding period. There are widespread signs of fire at the end of VIIa, followed by the construction of smaller buildings, some with cellars, in VIIb2 and a provisionally identified VIIb3. Ralf Becks, who has been primarily responsible for the excavation of this area, has put forward a sensitive and well-considered discussion of its layout in the Late Bronze Age, with suggestions as to its changing relations to the gate and citadel wall (Korfmann 2000: 21–8).

Kolb accepts that there was obviously some settlement immediately to the west of the citadel (Kolb 2002a; 2002b: 15), but he fails to draw attention to all the other points around the citadel where Late Bronze Age remains have been found. In fact, wherever excavations have been made here, they have unfailingly revealed a sequence of Late Bronze Age buildings, often substantial and, where the excavated area is wide enough to show it, set closely together.

Kolb and Hertel both complain that, in presenting the discoveries to the west of the citadel, Korfmann has fleshed out the meagre remains of Troy VI by adding in those of Troy VIIa (Kolb 2002b: 15; Hertel 2002: 7–8). This appears to rest on the fact that in one plan in the book accompanying the exhibition the buildings of late VI, VIIa1 and VIIa2 are all shown in shades of red which are hard to distinguish (Korfmann et al. 2001: fig. 74). The basic colour scheme (red for VI, green for VII) goes back to Dörpfeld, but here there are two innovations. First, the VIIa buildings are included in the red range. The evident purpose was to highlight the cultural continuity from VI into VIIa and to emphasise its difference from VIIb (shown in blue-green). This is hardly controversial. Second, a sub-division of both

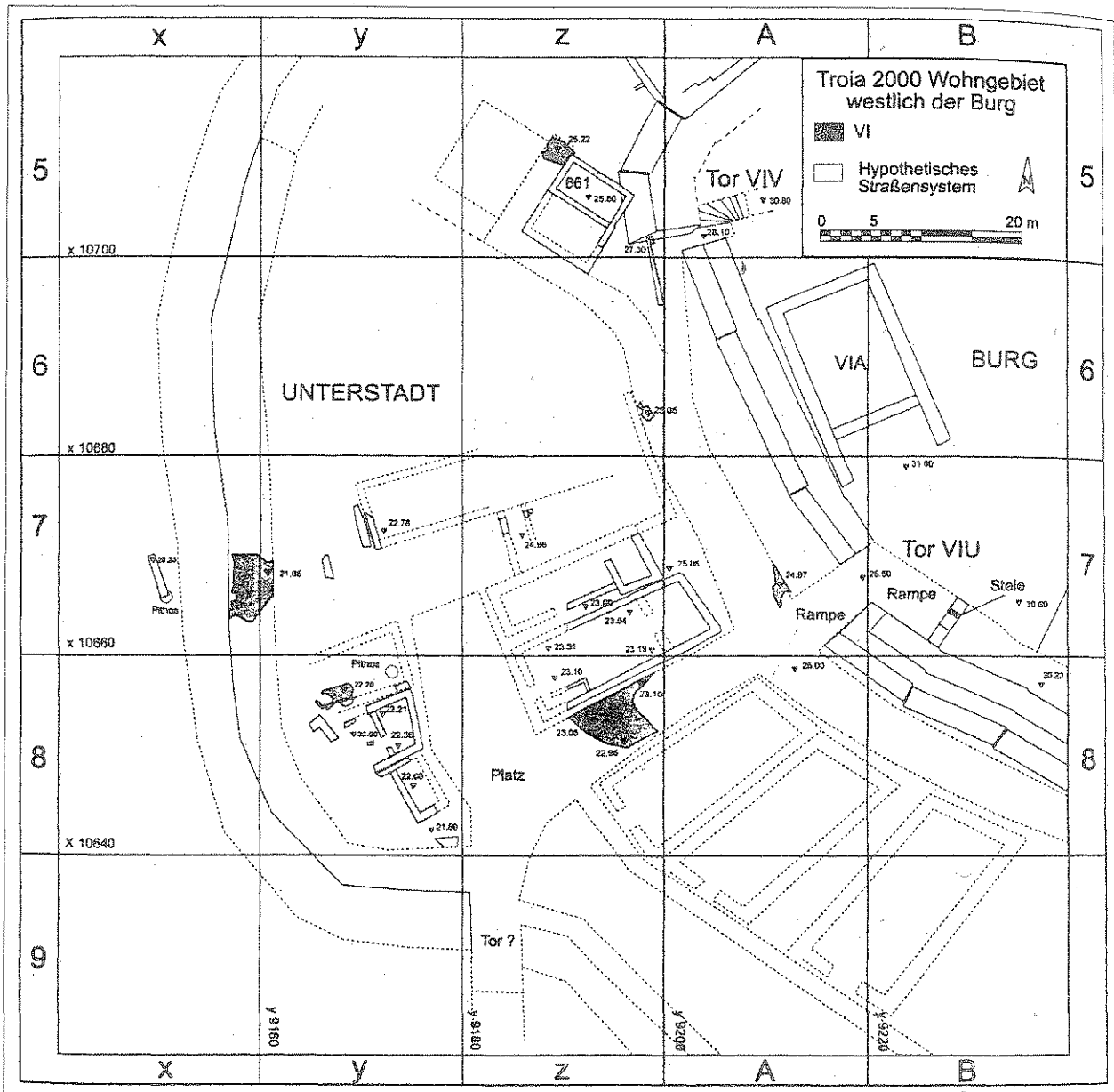


Fig. 7. Remains of Troy VI immediately to the west of the citadel (Korfmann 2000: fig. 12)

colours into multiple shades has been necessary because as a result of Korfmann's excavations more building phases are known. The reds are indeed too similar, and it is a pity that this was not taken up with the printer at proof stage. But far from conflating periods, the plan actually attempts a higher degree of differentiation than before. The known buildings of VI and VII in this part of the site are in any case clearly distinguished elsewhere in the book (Korfmann et al. 2001: fig. 480).

Most of the known structures in the area do admittedly belong to VII or later. This is because excavation has in most places gone no deeper. Where it has, remains of VI and also of earlier periods have come to light. One may confidently predict that, if the entire area were excavated, a built-up quarter of Troy VI and VII would be exposed. The cobbled streets speak for themselves and are plainly urban. It is obvious that the same sort of settlement probably extended all around the citadel except on the north side.

The middle plateau area²

Turning now to the area ca. 150m to the south, in the middle of the plateau, Korfmann's work has focussed on two neighbouring trenches in squares H17 and IKL16-17, in the latter investigating more closely an area first tested by Dörpfeld. H17 produced no complete structures, but 1.5m below the surface, in soundings in narrow areas between the walls of an overlying Roman glass factory, there were remnants of stone walls of late VI or VII, scattered mud-bricks, sherds of grey Minoan ware and a surprising amount of Mycenaean pottery of a quality equal to that found in the citadel. Numerous post-holes are mentioned in the report (Korfmann 1993: 25-6). Some later proved to be animal burrows (Korfmann 1997: 56), but others are genuine.

In IKL16-17 a larger area has been opened up (Korfmann 1994: 27-30; 1997: 53-62; 1998: 49-56; 1999: 20-2). Here the bedrock lies lower, and there are 3-3.5m of overlying deposits. Prehistoric, Hellenistic and even early Roman strata have all been hugely disturbed in this area by later Roman activity. Large Roman pits and foundations reach down to bedrock, and it is only in between these large intrusions that fragments of earlier deposits are preserved. Cut into the bedrock are the footings of a palisade originally thought to have belonged to Troy VI but now dated by sherds and C14 samples from its earliest fill to Troy I-II. There must once have been other deposits of Troy I-II in the area, but these were evidently removed in the Late Bronze Age for it is Troy VI or later deposits that now directly overlie bedrock. Preceding deposits are now found only where they have been left undisturbed in man-made cuts and natural depressions in the bedrock. A comparable phenomenon will be noted later in the southernmost part of the plateau. Troy VI buildings have been glimpsed in the northernmost quarter of the trench but lie mainly outside it — certainly to the north, west and east, and quite conceivably to the south as well (Korfmann 1997: fig. 54). In early and middle VI the area contained timber and mud huts, pavements, ovens, a threshing floor, pithoi, piles of murex and other shells, concentrations of bone needles and slag from bronze working. The impression is of an area where agriculture and crafts were pursued. In late VI and VIIa, by contrast, it contained houses with stone foundations. Remains of these have been found throughout the area, wherever later disturbance has not penetrated. All have a similar orientation (fig. 8; Korfmann 1997: fig. 54). Two phases are represented, and good quality Mycenaean pottery is found. In VIIb pits containing fragments of Buckel-

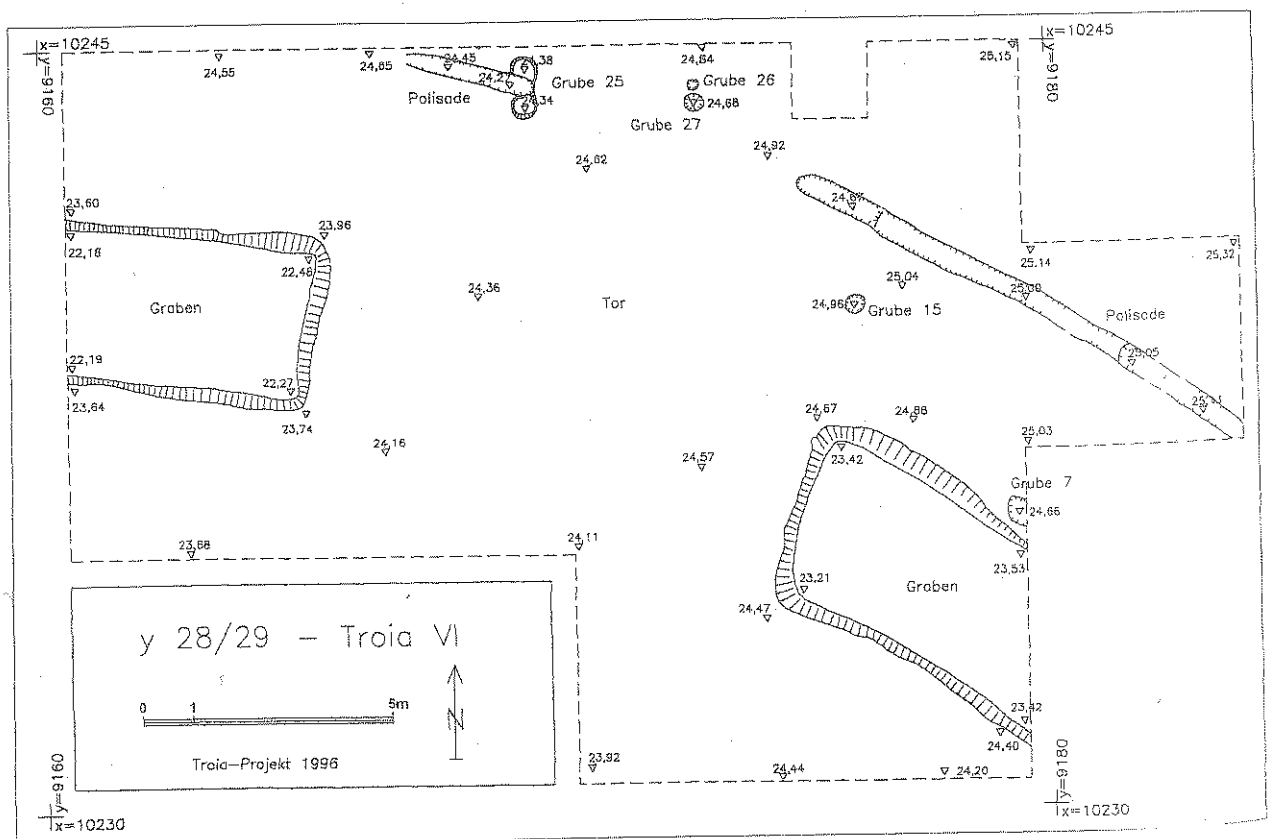
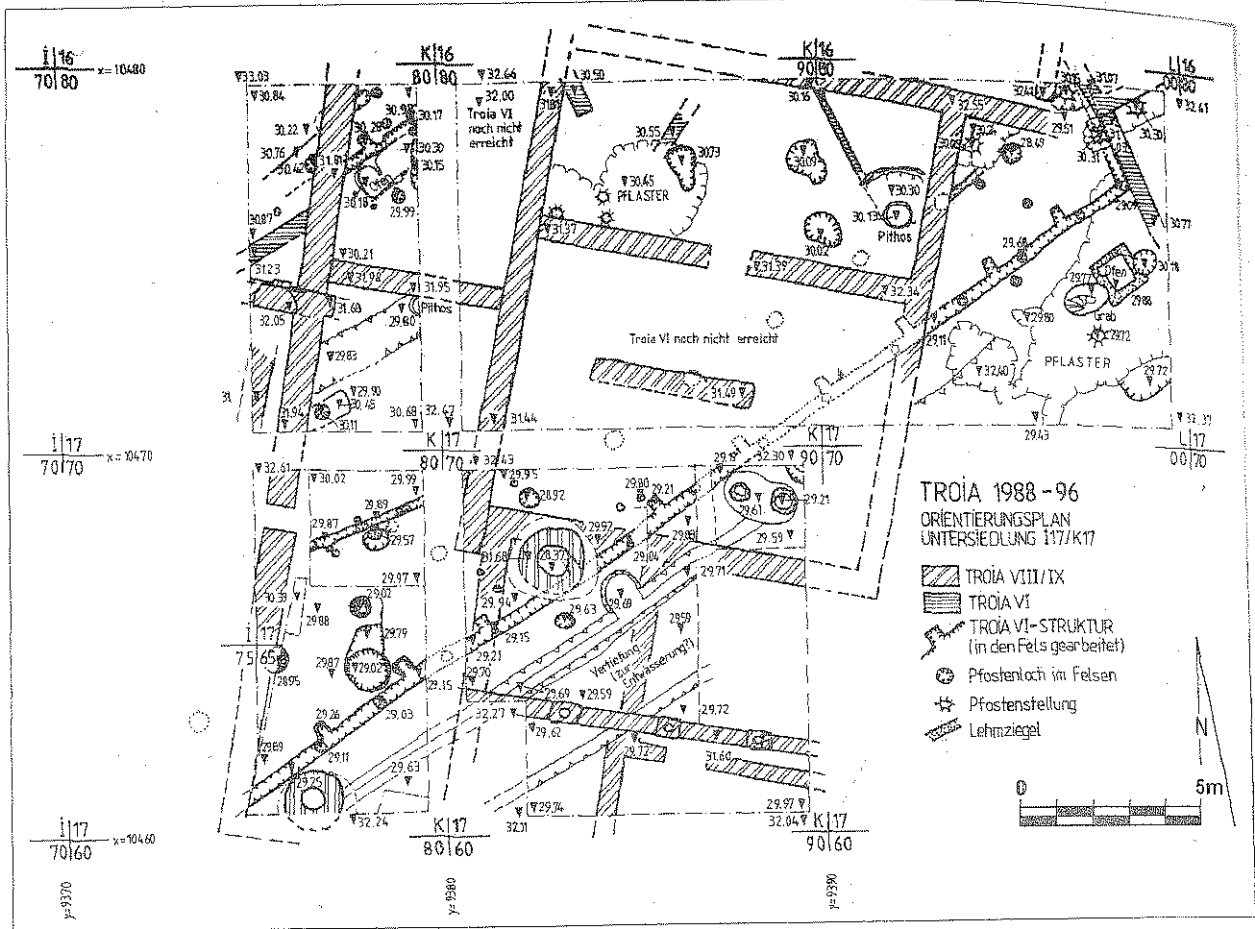
keramik and wattle-impressed clay show that the area was still settled. A putative street has been identified in the western half of K17.

Kolb describes the findings from IKL16-17 as amounting at any one time in the Late Bronze Age to no more than one single building with stone foundations plus some insubstantial huts, pits and open areas (Kolb 2002b: 13). Both he and Hertel argue from this that the middle part of the plateau was occupied only sparsely, with occasional solid buildings and outhouses set amongst gardens and farms (Kolb 2002b: 13-15; 2002c: 2; Hertel 2002: 8). They repeatedly cite with approval Korfmann's own, preliminary suggestion (Korfmann 1998: 52) that the area was thinly built with fairly large open areas between houses. The evidence as we now have it, however, suggests a degree of development: yes, insubstantial settlement with agriculture and crafts in early and middle VI, but followed by a fully built-up area in late VI-VIIa, with continued settlement, but a change of building methods, in VIIb. The reconstructions reflect the present understanding of the late Troy VI situation. It is worth remembering also that in late Troy VI there were more buildings only 20m to the west, in H17 — an area ignored by Kolb and Hertel. Occupation was certainly not so very sparse.

The southern half of the plateau

The most intriguing, and among the most disputed, discoveries come from the southernmost fringe of the plateau, ca. 400m to the south of the citadel (Jablonka et al. 1994; Jablonka 1995; 1996). Here in 1992 a 120m long anomaly showed up in the magnetometer survey (Becker et al. 1993). It was thought at first to indicate the presence of a buried, 6m wide, burnt mud-brick wall which, because of its position and orientation, might be the defensive wall of the Late Bronze Age lower city. This hope was in part disappointed when excavation proved the feature to be a ditch, but the same ditch has since been traced for a distance of ca. 400m from west to east, has been tested by excavation at six different points (Blindow et al. 2000: fig. 1; Korfmann 2001a: fig. 1), and has proved just as interesting. It was originally 4m wide. As preserved, the north side is now 2.5m high. It originated at the latest at the beginning of late Troy VI, for this is the date assigned after very careful study to the earliest fill in the ditch. (Initial impressions had placed it earlier, in middle VI.) At first the fill accumulated only gradually. A higher stratum of fill is a burnt destruction deposit of late VI date, after which the ditch was filled up with deposits dateable to Troy VI (Jablonka 1996: 80). It is likely that the ditch was no longer open in VIIa (Jablonka 1996: 73), but material washed in later from the surrounding area still includes sherds of VI and VII (Jablonka 1995: 76), indicating that occupation continued in the surrounding area.

² For clarification of some of the points here I am most grateful to the excavator, Peter Jablonka.



In y28–9 geomagnetic survey identified a 10m wide gap in the ditch. Excavation confirmed the finding, revealing what was effectively a causeway crossing the ditch (fig. 9). Three and a half metres to the north of this a second cutting in the bedrock has been found, running parallel to the ditch. This second cutting is much smaller — 50cm wide and sometimes as little as 14cm deep. It has been traced over a length of 18m but no more, and includes a 5m wide gap positioned just north of the causeway. It is suggested that it was the foundation trench for a wooden fence or palisade, and that the 5m gap was a gate. Two post-holes on the western side of the gap and one in the middle lend support to this, but there are no corresponding holes on the east side. An axis drawn along the centre of the causeway and through the middle of the putative gate, when extended northwards, runs up to the south gate of the Troy VI citadel. The magnetometer survey has suggested the presence of a second, similar gap in the ditch 220m to the west, in square n28.

A second ditch was identified in 1995 lying 100–150m further to the south and beyond the limits of the Roman lower city. This ditch was originally more than 3m wide, and the cut into bedrock is about 3m high on the north side. Its continuation has been found 230m to the southeast in square s34, and geomagnetic survey and additional soundings have traced it for a length of over 700m. The relative dating of the two ditches is not absolutely certain. They seem likely to be successive, for although the initial fill in the more southerly ditch may belong to the very end of Troy VI (scarcely distinguishable from VIIa), the remainder seems to have accumulated rapidly at some point during Troy VIIa (Jablonka 1996: 80). Thus the inner, more northerly ditch may belong to late VI and the outer, more southerly one to the ensuing period. The outer ditch was re-cut in Roman times, and the Roman deposits are quite distinct: water-laid bands of mud and silt.

Korfmann believes that the inner ditch and palisade, at least, were defensive, and that there may in addition have been a more substantial city wall at some point further north. No trace of the latter has been found on the plateau, but an upward step in the terrain ca. 70m to the north has been pin-pointed as a possible location (Korfmann 2000: 46). The model and Haussner's reconstructions show a built-up settlement extending all the way south across the plateau, bordered by just such a wall. Beyond, and all around the lower city, is shown a ditch with a number of causeways, each protected on the inner side by a short stretch of palisade.

Kolb and Hertel contest this entire reconstruction. They point out that neither cores nor soundings have produced Bronze Age strata across the southern half of

the plateau and suggest, on the basis of plant remains from the inner ditch, that this area was not built up at all but was used instead for the cultivation of figs and vines. Archaeologically it was a *tabula rasa* until the Hellenistic period (Kolb 2002a; 2002c: 2; Hertel 2002: 9). They emphasise that no trace of the city wall has actually been found around the southern part of the plateau (Hertel 2001: 46; Kolb 2002b: 16). The palisade, they say, would have been too weak to be an effective defence and could anyway have been skirted around at either end (Kolb 2002b: 17; Hertel 2002: 14–15). Kolb points out the absence of the additional post-holes which would demonstrate the presence in it of a double door, and doubts whether the shallow rock cutting represents anything other than a field drain or industrial channel (Kolb 2002b: 17). Hertel is equally sceptical (Hertel 2002: 15). The defensive character of the inner ditch is also strongly disputed. They argue that it could have been jumped across by a foot-soldier, bridged by planks, or filled in with earth, and that the causeway across it would have provided a fine highway for an enemy's chariots. In any case it has not been found around the east side of the lower city (*Berliner Morgenpost*; Hertel 2001: 45; 2002: 14; Kolb 2002b: 17). They both criticise its interpretation as a defensive work when the outer ditch is known to have been used by the Romans as a water channel (Kolb 2002a; Hertel 2002: 15), and Kolb has positively suggested that it could have been used to bring water from the cave on the west side of the site to irrigate the fields on the plateau (Kolb 2002b: 17–18). For dating the inner ditch Kolb strangely relies on the excavators' first impressions, ignoring the more considered judgements made later (Kolb 2002b: 17, n. 42).

The possible existence of a city wall will be discussed below. What evidence is there to support the reconstruction of a built-up area in the southern half of the plateau?

Korfmann's critics are right to point to the absence of Bronze Age strata, but this is not simply an absence of archaeological deposits; it is an absence in almost all excavated areas of any prehistoric surface at all (Korfmann 1992a: 33). This is partly attributable to erosion. Kolb has repeatedly dismissed this as though it were a convenient excuse (Kolb 2002a; 2002b: 13; and orally in the symposium), but the area is indeed a *tabula rasa* as he says — in the sense that it has been scraped clean. Around the inner ditch bedrock is in places only 20cm below the surface, and all strata of VI and VII are missing (Jablonka et al. 1994: 53; Jablonka 1995: 43, 48). That there has been a process of erosion which explains this is positively indicated by the presence in both ditch-fills of deposits, including Late Bronze Age

pottery, which were washed in during the Late Bronze Age and after (Jablonka et al. 1994: fig. 1 nos 5, 6). Further south, in g28, 2m of colluvium have accumulated over bedrock since it was exposed in Roman times (Jablonka 1996: 91). The valley to the south also contains an accumulation of erosion deposits which over the centuries have been washed down from the site (Kayan 1997). Byzantine and earlier inhabitants made an effort to halt the process by cutting terraces and building terrace walls (Jablonka 1996: 87–91).

Late Bronze Age deposits were not always missing, however. As far south as square s34 depressions in the bedrock still invariably contain residual clusters of Troy VI pottery which erosion has failed to sweep away (Jablonka 1996: 87). At one point just north of the inner ditch there are the remains of a series of pits cut into bedrock for pithoi, with the pithos bases still present. Thermoluminescence dating has confirmed that they are of second millennium origin. The pithoi themselves would have been sunk into house floors which must once have lain nearly 2m higher — a striking indication of the depth of deposit which has been lost. The Bronze Age fill of the inner ditch (Jablonka 1994: fig. 1 nos 3, 4) consists largely of refuse and destruction debris from a built-up area: there are bits of stone, some burnt; burnt clay; burnt mud-brick; mud-brick debris; ash; pottery of Troy VI; and animal bones (Jablonka et al. 1994: 60; 1995: 43; 1996: 45, 70). The fill in the outer ditch is similar (Jablonka 1996: 87–91). None of these earlier deposits in the inner ditch was washed in by rain, but all were tipped in from the north. Unless we suppose them to have been deliberately carried from a built-up area 200m or more to the north, we have to conclude that they, as well as the botanical evidence for figs and vines (Jablonka et al. 1994: 71), give us some indication of what was once in the vicinity. The burnt Troy VI house thought by Blegen to have been a crematorium may be a tangible remnant.

Hertel finds it incredible that all traces of Bronze Age structures should have been swept away by erosion (Hertel 2002: 9). But one has to allow for the fact that much may also have been removed by the builders of Hellenistic and Roman times. The Classical lower city extended across most of this area, as geophysical survey and excavation show, and Roman buildings were placed directly on bedrock. They even cut right into the bedrock which in places has had as much as 0.5m removed (Jablonka et al. 1994: 62; Jablonka 1995: 43; 1996: 45). Brian Rose emphasised at the symposium the extent to which the Roman builders pillaged earlier periods for stone. The result is that there are very few remains even from the Hellenistic and Augustan periods when it is quite certain that a lower city existed. Kolb points to the

preserved Late Bronze Age buildings on the west side of the citadel to show that the Romans did not always remove earlier strata (Kolb 2002b: 15), but the situation at the south end of the plateau was different. Here erosion may already have reduced the depth of soil to such an extent that only its total removal, and that of all earlier remains, could make solid building possible.

The absence of the Bronze Age surface is important when we come to consider the supposed palisade. One has to bear in mind that the shallow rock-cuttings which have been found must represent only the very bottom of what were originally much deeper features. The original cuts could have been made through a metre or more of overlying earth and perhaps bedrock, all since disappeared. If, therefore, the two channels as we know them peter out after a few metres in either direction (Korfmann 1997: 62) this does not mean that they never extended any further. It simply means that the evidence has disappeared. Similarly if on the east side of the supposed gateway there are two post-holes not attested, it may well be because they were not originally dug to quite the same depth as the others.

The purpose of the feature has of course to be inferred. Given its position parallel to the ditch, the coincidence of the gateway with the causeway, the direction of the axis through the two, and the real possibility that it extended far beyond the limits to which it has been traced, I think it is highly likely that it was, as Korfmann believes, a part of a defensive system. A continuous palisade of timbers 0.5m thick, set deep into the earth and the underlying bedrock, would not be the weak defence that Kolb and Hertel claim. Nor need its method of construction exactly replicate that of its Troy II predecessor as Kolb requires (Kolb 2002b: 17).

Equally the ditches will not have been as feeble a defence as Korfmann's opponents maintain. Each is situated at a natural drop in the terrain, so that the northern side is much higher than the southern. We must allow, too, for the added height on both sides of the ditch of the earth and bedrock which has disappeared in the interim. Thus an attacker would originally have been faced with ditches 3–4m wide, perhaps 2m or more deep on the south side, and towering 3 or 4m high on the north side, or perhaps more (Jablonka et al. 1994: fig. 1; Jablonka 1996: fig. 8). No-one could jump this, nor could any chariot easily cross it whether it were bridged with planks or filled up with earth. It is quite true that no continuation of the inner ditch has been found on the east side of the site — yet. Later disturbance and the porosity of the bedrock have so far made it difficult to follow here (Jansen et al. 1998: 276, 280). But it would be unwise to depend on this remaining the case. Since the feature was first identified by magnetometer in 1992, painstaking

work with fluxgate and caesium magnetometers and with ground-penetrating radar has nearly quadrupled its known length (Blindow et al. 2000: fig. 1). The latest work may even show a turn northwards at its most easterly end (Blindow et al. 2000: 129, fig. 8) and has now demonstrated the presence of a comparable ditch on the west side of the lower city in square p12 (Korfmann 2001a: 28, 42, figs 23, 24).

The suggestion that the ditch was intended as a water channel, on the model of the later Roman re-use of the outer ditch, founders decisively on two facts: (1) that it was interrupted at at least two points by causeways (Korfmann 2001a: fig. 23); and (2) that it undulates over its course by as much as 14m (Messmer et al. 1998). It also overlooks the nature of the deposits. The thin strata of mud and silt in the Roman cut were laid by slow-moving or stagnant water repeatedly over a long period (Jablonka 1996: 82). They are quite different from the Bronze Age strata in the inner ditch, although of course water will collect in any ditch in wet weather and this is reflected in the evidence here too (Jablonka et al. 1994: 60 stratum 3, 71). At the symposium Kolb's water channel theory was heavily criticised by Uerpmann — with, among other things, the trenchant observation that water cannot run uphill — and Kolb now denies that he ever advanced it (Kolb 2002c: 5). But he plainly did (Kolb 2002a; 2002b: 17–18).

When we view the presumed palisade and the two ditches together, considering the character of each, and when we take into account the extent to which information may have disappeared or be as yet inaccessible, their interpretation as parts of two successive defensive systems seems entirely reasonable, indeed convincing. We can in fact now quite reasonably sketch out a picture of how the lower city may have grown over the centuries. The rock-cut palisade in HIKL16–17 may represent the outer limit of the Troy II settlement. By Troy V that same area had been covered by the settlement itself, as traces indicate, but Troy V graves were found not very much further to the south in D20 (Korfmann 1994: 31–4). Probably the limit of the Troy V settlement lay approximately in D19. In early and middle Troy VI IKL16–17 was still not very heavily settled, and may perhaps have lain towards the edge of the lower city. By late VI, however, thick settlement had spread across it and the lower city extended in some form as far as the palisade and the inner ditch with the southernmost houses probably interspersed by gardens and orchards. A little further to the south, outside the settlement, lay the cemetery of late Troy VI (VIh) excavated by Blegen. After some destruction in the lower city and the filling up of the inner ditch, the settlement probably expanded again further to the south perhaps at the very end of Troy

VI and certainly during Troy VIIa. A second ditch was cut whose contents witness to the presence predominantly of VIIa occupation in the vicinity. This is a fair indication that the area to the north of the inner ditch was already filled to capacity by the end of Troy VI, and justifies some of the higher population estimates. The cemetery belonging to this period may be presumed to lie yet further to the south (Becks 2002: 299). The resulting picture of a crowded lower city in Troy VIIa is consistent with what we already knew of the period from the citadel.

A lower city wall? *

Accepting, then, that we have quite convincing evidence for two successive Late Bronze Age defensive systems around the lower city, we must ask: were they supplemented by a city wall as Korfmann supposes?

While a city wall has not yet been identified on the plateau, possibly due to the depredations of the Hellenistic and Roman occupants, Korfmann reasoned that remnants of such a wall might still be found nearer to the citadel. In particular the area just east of the VI citadel walls seemed promising, for here the Hellenistic builders, far from removing earlier deposits, had been keen to pile up as much earth as possible behind the retaining walls IXN, IXM in order to support the platform of their splendid new temple. Korfmann further reasoned that the angle set into the southeast corner of the northeast bastion was the most likely place for a city wall to have run up to the citadel itself. A wall joining at this point would have been essential to prevent access from the outside via the cistern into the citadel itself, but would have continued to leave the water supply accessible to inhabitants of the lower city. In 1995–1998 he therefore dug around the southeast corner of the bastion (Korfmann 1996: 39–43; 1997: 49–53; 1998: 43–8; 1999: 16–17). It is a tight and complex area, still being studied, and the full picture is not yet available. Crucially, however, the stone footings of a very substantial wall did appear at exactly the right place, running off to the southeast as might be expected (fig. 10).

A length of 7m has been exposed, interrupted at the southeast end by what is interpreted as a gateway into the lower city. A road runs northwards at this point, bounded along its west side by a wall which antedates the presumed city wall. A circular group of stones set into the road is seen as a possible post-support on the west side of the gateway, and an upright stone 2m to the north could be a stele of the sort found outside other gates in Troy I, II and VI. The east side of the gate has not been found, nor has any continuation of the wall eastwards. To the west of the road a stone fill lies behind the north-south border wall. It is above this, on a mud-brick packing, thus on a sort of platform, that the stele is set.

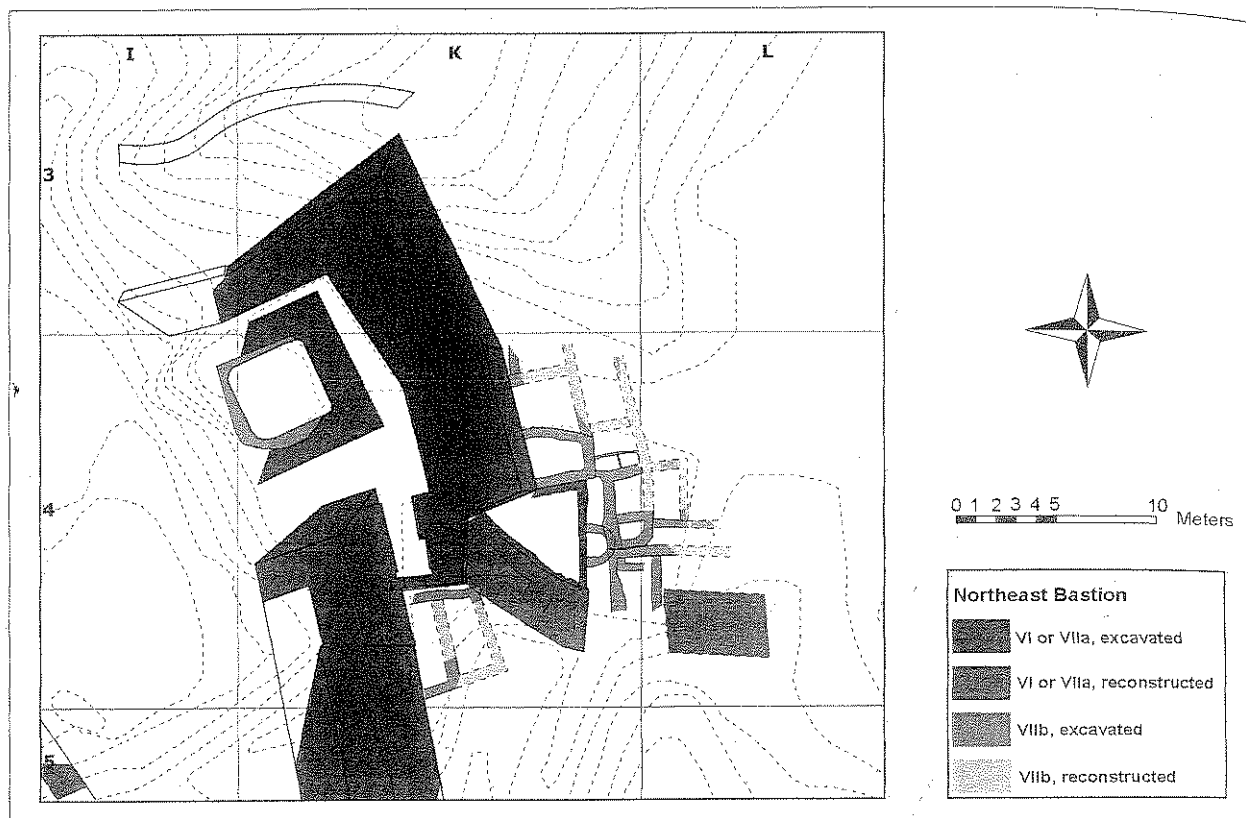


Fig. 10. Area around northeast bastion in K4 (plan courtesy of Dr Peter Jablonka, Troia Projekt, Tübingen University)

The picture is complicated by the presence of a 1m thick packing of horizontally laid mud-brick which covers the presumed city wall, the stele and the stone fill. In VIIb small houses were built over the road and around the mud-brick platform and bastion. These were in due course destroyed and filled up with rubble.

Hertel complains that the area is selectively and unsystematically described, and that the relevant pottery has not been published (Hertel 2002: 9–10). This is hardly a fair charge against reports which are only preliminary. The more substantial criticisms are that the wall is too low and too weak to have served as a city wall (*Berliner Morgenpost*; Hertel 2001: 46; 2002: 11), that it never really adjoined the bastion (Kolb 2002b: n. 23; Hertel 2002: 10), and that it may not have been contemporary with the ditch and may only have been built in VIIa not middle VI as Korfmann says (Korfmann 1996: 42; Hertel 2001: 46; 2002: 11; Kolb 2002a; 2002b: 17–18). The gateway is dismissed since in Korfmann's reconstruction it would have the unusual width of 6m and since no east side has been found (Hertel 2002: 13). The post-support is seen simply as part of the paving of what Korfmann (but not Hertel) regards as the street (Hertel 2002: 11). Both Kolb and Hertel take exception to a plan in the book accompanying the exhibition in which the hypothetical eastward continuation of the wall looks as if it is drawn in with a

continuous, instead of a dotted, line. Kolb implies that Korfmann did this deliberately 'in order to create the impression that there was a gate in a supposed city wall' (Hertel 2002: 13; Kolb 2002c: 2).

Some of these criticisms are astute and apposite. Because of the area's complexity and importance, the preliminary assessments have been thoroughly reviewed in Tübingen.³ The latest analysis of the pottery in fact confirms Hertel's view that the presumed city wall should be considerably later than middle VI and should post-date the building of the bastion. Although the ceramic data are not absolutely conclusive, it appears likely that the platform of stone fill, with the mud-brick capping into which the stele was set, was built up against the bastion in late VI. The city wall was built across it shortly afterwards. Then in VIIa the 1m thick mud-brick layer was laid across the top of the whole complex. This does not necessarily mean that in VIIa the wall was no longer in use, as the mud-brick layer could belong to some local repair or modification. At all events there was thus a century or two for the stones of the bastion to weather before they were obscured by the mud-brick structure (Hertel 2002: 10). The change to the dating means that, if this was indeed a part of a city wall, it will

³ I am most grateful here to Ralf Becks for up-to-date information and to Professor Korfmann for permission to publish it.

probably have been built while the inner ditch was in use, or the reconstructions depict, and could have survived into the period of the outer ditch.

The reconstructed gateway is indeed unusually wide (Korfmann 1996: fig. 33). But the photographs show that on the east side a cutting was made through the road by the foundation trench for the Hellenistic retaining wall (Korfmann 1997: figs 50, 52). Thus an east side to the gate could have been as close to the west side as 3.5m and been removed. Magnificent, if indirect, confirmation of its likely existence came when a gate in the Hellenistic city wall was found a few metres to the east in 1996 (Korfmann 1997: 52–3; Rose 1997: 96–101). The Hellenistic operations have obliterated or made inaccessible any remnant there of an earlier city wall.

It is unfortunate that in two of the published plans the hypothetical eastward extension of the city wall beyond the supposed gate looks as if it had been drawn in with a continuous line (Korfmann et al. 2001: figs 461, 480). Inspection with a magnifying glass suggests that the digital printing may have been to blame. At all events the criticism is over-particular since dotted lines are used elsewhere in the same book (Korfmann et al. 2001: fig. 368) and in all earlier publications (for example, Korfmann 1996: figs 33, 36, 37), making it pretty clear that it was unintended.

As with the palisade and the ditches, Korfmann's opponents dismiss too quickly the possible defensive value of the feature. The presumed city wall, where fully preserved, is 2m thick — and that is assuming we already know its full width, which we may not. Although this is nothing like as massive as the citadel walls, even a mud-brick wall of this size on a low stone footing would be a serious obstacle. In a stratified society it is perhaps not surprising that defences for the general population should not be as strong as those surrounding a royal residence.

It is true that at its west end the wall no longer abuts the bastion, and that there is a gap of about 1m between them. Hertel is confident that this gap must be original to the wall and was not caused when Dörpfeld exposed the northeast bastion in 1893–1894 because, he says, Dörpfeld 'demonstrably' dug no deeper than the semi-circular feature of Troy VIII which runs across the area (Hertel 2002: 10). This is a reasonable supposition to have made on the basis of Dörpfeld's records and, if he were right, the wall's defensive character would certainly be harder to believe in. In fact, however, the new excavations found otherwise although this was not made explicit in the preliminary reports.⁴ Dörpfeld certainly

did, at some points, dig to deeper than the Troy VIII feature. He appears to have cut a narrow, irregular trench along the face of the bastion in order to expose the lower parts of its walls, in the process slicing through a part of IXN (Dörpfeld 1902: Taf. III; Korfmann 1997: fig. 45 top). Korfmann's excavations found that it had cut through the mud-brick packing and, along the east face of the bastion, into the stone fill beneath it. It had also removed a part of the wall running eastwards from the southeast corner of the bastion, leaving a deeper hole where he had tried to trace the corner downwards to bedrock (for all this see Korfmann 1997: figs 45, 48). Dörpfeld's trench had filled up with loose earth and stones, and was identifiable when rediscovered. It had certainly cut through the wall in question, and it is entirely proper to suppose that the wall originally adjoined the bastion.

The area still presents some conundrums. In particular, the purpose of the later mud-brick packing is unclear. Of the presumed city wall we have only a short stretch, and one cannot yet be entirely confident that it is what Korfmann thinks it is. Equally one cannot say that the existence of a gateway has been established beyond all possible doubt. But the size, location and orientation of the wall are all compatible with its being the beginning of a city wall, and the later building on the same spot of the Hellenistic city wall may well speak for a continuity of tradition. The paved street (if such it is) and the stele on the mud-brick platform are points in favour of there being a Late Bronze Age gate at this point, and the presence of the later, Hellenistic gate again offers support. If some additional part of the Late Bronze Age wall could be found the case would be closed. Research is continuing on the west side of the citadel to see whether the wall can be picked up there, but has so far mainly met Hellenistic disturbance (Korfmann 1999: 16; 2000: 21–5, 27).

Conclusions

Everyone accepts that there was a lower settlement of some kind. The questions are: (1) is it permissible to reconstruct it so extensively; (2) do the reconstructions accurately depict its likely character and extent; and (3) has there been a deliberate attempt to mislead the public?

There is, as we have noted, a huge disparity between Korfmann's comprehensive reconstructions and the extent to which the lower city has actually been excavated. Kolb evidently feels that this raises an issue of principle. He enunciates his own view as a binding one, that a model or picture may show nothing whose existence is not firmly attested (Kolb 2002b: 12). To this one may simply respond, who says? The Büyükkale model alluded to above suggests that the excavators of

⁴ I am again greatly indebted to Ralf Becks with whom I have been able to discuss this question and who briefed me on the latest findings

Boğazköy feel constrained by no such stringent code, and no doubt there are in the world's museums many other models built on similar principles. It seems to me a perfectly valid and useful exercise to construct a well thought out, imaginative presentation of how an entire ancient site might originally have looked. It can be stimulating and educative. Children these days are well acquainted with such things as digital enhancement and computer reconstructions, and know what they are. There is little danger provided the basic data are also to hand. This was the case in the exhibition where there was a plan showing the extent of the excavations, and it is also the case in the book (Korfmann et al. 2001: fig. 425).

Then there is the question as to the accuracy of the reconstructions. There can be little doubt that the area immediately around the citadel was heavily built up on all sides. In the middle part of the plateau further excavation in additional areas could perhaps clarify matters. IKL16–17, however, was fully built up in late Troy VI and H17 has shown that these buildings were not isolated. In the southern half of the plateau there has been so much loss through erosion and Classical building activities that we shall probably never know how dense the occupation was. But Blegen's 'crematorium', the pithos bases set in bedrock and the ditch fills show that it was by no means so wholly absent as Kolb and Hertel believe. One may also reasonably ask whether the defences and the cemetery would have lain at such a distance to the south had there not been settlement stretching that far.

That in late Troy VI the lower city was defended by a palisade and ditch, and that in VIIa it was subsequently defended by a second ditch further south, is an entirely reasonable deduction from the evidence that we have, and the attempts to deny this seem very forced. It is true that the full course of the defences has not been recovered, but this is not unusual in archaeology. Had there been no controversy most archaeologists, one suspects, would have thought the 600m length of ditch so far documented by Korfmann to be more than sufficient. It is less certain, but still arguable, that a city wall also was a part of the defensive system in late VI and perhaps lasted into VIIa. Inclusion of the city wall in the model and the pictures was certainly very bold, but I would say not indefensible. It is difficult to conceive of the entry into the bastion being at any time unprotected by some such wall.

Hertel says with some justice that all attempts at reconstruction (i.e. on this scale) rest on fantasy (Hertel 2001: 44). This has not stopped either him or Kolb from offering his own. Korfmann's are certainly optimistic and lie at one end of a spectrum of possibilities. Those of Kolb and Hertel are decidedly minimising and lie at the other. The truth is much more likely to lie towards Korfmann's end.

Granted that the reconstructions have a legitimate aim and fairly extrapolate from what is known of the lower town, there is little to support the accusations that Korfmann has deliberately been misleading the public apart from three minor printing faults in the exhibition book. Errors of this kind are excusable when one takes into account the pressures of mounting such an ambitious exhibition on top of a full programme of university teaching and administration, not to mention the burden of running a major international excavation.

It is to be regretted that the accusations against Korfmann have been made so publicly and so repeatedly when their basis is so flimsy. Kolb has indeed withdrawn his earlier description of Korfmann as a 'von Däniken of archaeology' (*Stuttgarter Nachrichten*). Unfortunately he is now giving currency, without the slightest hint of disapproval, to the opinion of *Die Welt* that Troy is being excavated in the style of Indiana Jones (Kolb 2002c: 8). This is possibly the most offensive of a whole series of offensive remarks in his website. That it is untrue can be seen by anyone who reads the many preliminary studies in *Studia Troica* or who takes the trouble to visit the excavation.

The historical geography of western Anatolia in the Hittite texts (fig. 11)

Recent developments

Ever since the reading of the Hittite texts in the 1920s opened the window on the second millennium BC history of Anatolia, scholars have wrestled with the problem of placing the towns and countries named in the texts on the modern map. It has been recognised that western Anatolia generally was known to the Hittites as the 'Arzawa lands', a political term, which in the oldest edition of the Hittite laws appears under the ethno-linguistic term 'Luwiya'. The 'Lukka lands' too were seen as belonging to the west, but the location of the individual land and city names has remained highly uncertain, indeed controversial. We may take the map of Garstang and Gurney (1959: map facing 1, discussion in chapters VI–VIII) as a starting point. Arzawa is placed in the central west, the valleys of the Meander and Hermos, Lukka down towards Classical Lycia and Wilusa towards the Troad. Of the most important named cities, Apasa is at Ephesus, Millawanda at Miletus and the Seha River land on the Caicus by its association with Lazpa (= Lesbos). Troy is identified with the once attested (land of) Taruisa.

In the succeeding 30 years, a number of widely differing locations were proposed by scholars in different contexts: see, for example, the maps and accompanying articles of Macqueen (1968: 169–85); Bryce (1974: 103–16); Koşak (1981: 12*–16*). Notably attempts were

made to find a location for the highly controversial land of Ahhiyawa on the Anatolian mainland, especially by Steiner, following the original view of Sommer (see below). But in general the ease with which quite different locations could be plausibly proposed gave rise to a commonly expressed view that the situation was more or less hopeless — Hittite geography as a 'guessing game' (Mellaart, cited by Koşak 1981: 12*).

All this has changed dramatically in the last 15 years, 1988 being the effective turning point, the year in which the Boğazköy Bronze Tablet was published (Otten 1988; recent translation, Beckman 1996: no. 18c; bibliography, van den Hout 1998: 326), as was the hieroglyphic Luwian inscription YALBURT (Özgüç 1988: 172–4, pls 85–95; edition, Poetto 1993; Hawkins 1995: 66–85), and the

hieroglyphic Luwian inscription BOĞAZKÖY-SÜDBURG was discovered (Hawkins 1990: 305–14; 1995). Briefly summarised, the Bronze Tablet established the size and location of the kingdom of Tarhuntassa now seen to extend from Kizzuwatna/Plain Cilicia in the east, through its border with Hatti in the south Konya plain, to the city Parha on the river Kastaraya in the west, i.e. to Perge on the Kestros in Pamphylia (Otten 1988: 37; 1989: 18). Beyond this lay the Lukka lands, thus occupying all of or more than Classical Lycia (Hawkins 1995: 54, n. 194). The YALBURT inscription, narrating a Lukka campaign of Tudhaliya IV, named as conquered places besides Lukka the cities Awarna and Pinali, Patara, Talawa and Wiyanawanda, which correspond unmistakably to Classical Lycia, Xanthos (= Lyc. Arnä,

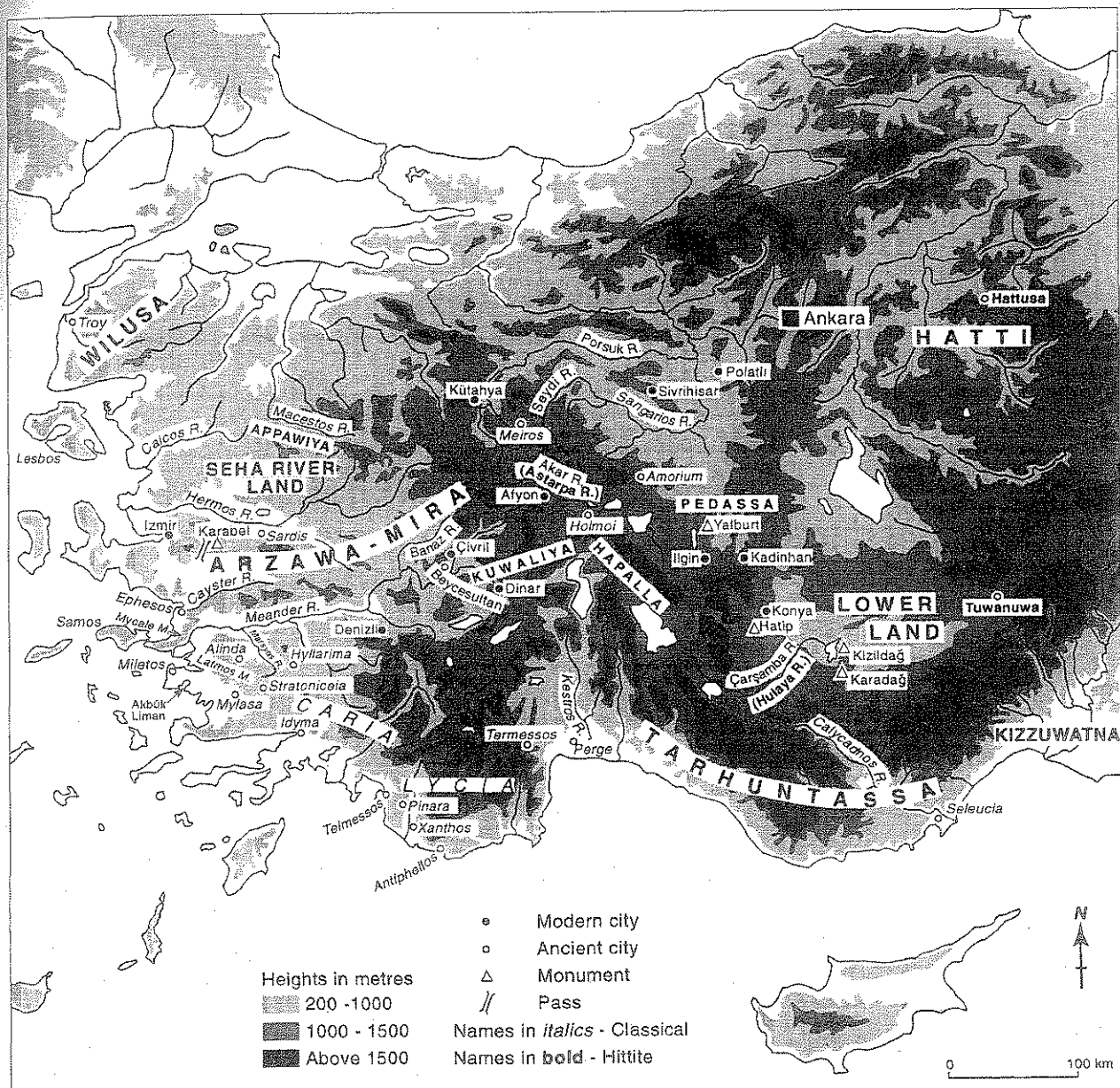


Fig. 11. Central western Anatolia (Hawkins 1998: fig. 11)

Aram. 'wrn) and Pinara, Patara, Tlos and Oenoanda (Poetto 1993: 74–82, La toponomastica; Hawkins 1995: 49–57). Thus suddenly the historical geography of south and southwestern Anatolia came into focus, and the Arzawa lands with their associated toponyms were definitively pushed back into the central west and northwest.

Decisive for the geography of these Arzawa lands was the establishment of the reading of the KARABEL inscription in the Karabel pass which carries one of the two routes from Ephesus in the Cayster valley across the Tmolus range to Sardis in the Hermos valley (Hawkins 1998: 1–31). The reading of this as an inscription of Tarkasnawa king of Mira, known also from his digraphic silver seal 'TARKONDEMOS', confirmed the suggestion already put forward that this was the northern boundary of the land of Mira leading to the Seha River land (Houwink ten Cate 1983–1984: 48, n. 38; Starke 1997: 451, nn. 40, 41) and that the kingdom of Mira included the rump of Arzawa proper (as originally argued by Heinhold-Krahmer 1977: 136–47, 211–19; Hawkins 1998: 15) with its capital city of Apasa, thus Ephesus. Remarkably the almost contemporaneous discovery of the HATIP inscribed rock relief southwest of Konya provides a very similar fixed point on the Hatti-Tarhuntassa frontier as described on the Bronze Tablet (Dinçol 1998a: 27–34, with earlier bibliography; 1998b: 159–66). As with Tarhuntassa, we are now in a better position to estimate the extent and importance of the kingdom of Mira in the late Hittite Empire. It probably stretched from the Tmolus range in the north to include the Meander valley in the south, and impinged on the city of Millawanda, the identification of which with Miletus is now hardly to be doubted. The general geographical scheme of western Anatolia by Garstang and Gurney looks to be triumphantly confirmed.

In the background to the textual evidence for western Anatolia, archaeological knowledge of the Bronze Age of western Anatolia has been slowly expanding since the Second World War. Before that few sites other than Troy had yielded significant information. The 1950s excavations of Beycesultan on the upper Meander produced good Middle-Late Bronze Age levels on a site intended to link archaeologically the plateau and the west (Lloyd, Mellaart 1965; Lloyd 1972; Mellaart, Murray 1995). Intermittent operations since before the war on the Bronze Age levels at Miletus, difficult of access because of the water table, are now bearing fruit (most recently Niemeier 1999), as is also Iasos (preliminary reports, Momigliano 2000; 2001) and the little published Turkish work at Limantepe and Panaztepe near Izmir (short reports in Mellink, 'Archaeology in Anatolia' and Gates, 'Archaeology in Turkey' *American Journal of Archaeology*; Panaztepe annually from *AJA* 91 [1987]; Limantepe annually from *AJA* 98

[1994]; cross references also to reports in *Kazı Sonuçları Toplantısı*). More recent are very promising discoveries at Ephesus itself (most recently Büyükkolancı 2000). All these coastal sites are now producing increasing evidence of Mycenaean settlement and influence, especially in the central west area from Izmir to Bodrum, in the form of Mycenaean pottery imported and locally manufactured, and Mycenaean tombs. Thus a more concrete archaeological background with which to connect the textual evidence is becoming available.

The Symposium

Symposium papers dealing with the textual sources were those of Starke and Heinhold-Krahmer, and additionally that of Niemeier offered a combination of archaeological and historical data. The three scholars represent rather different approaches to the subject: Niemeier as an archaeologist working in the area, who though not a Hittitologist himself makes full use of the recent publications in the field (Niemeier 1999; Niemeier, Niemeier forthcoming); Starke, an Anatolian philologist specialising in Luwian studies, whose recent publications have concentrated heavily on the historical geography of western Anatolia (Starke 1997; 1998–2000; Lukka, Miletos, Mira); and Heinhold-Krahmer, whose fundamental study of the Arzawa texts remains an indispensable handbook to the subject, but now 25 years old was written well before the break-through in our knowledge outlined above (Heinhold-Krahmer 1977; for her more recent contributions, see Heinhold-Krahmer: 1983; 1994a; 1994b).

The reading of the KARABEL inscription and my interpretation of the historical geography of Arzawa based on it followed hard on Starke's first foray into this field (I received the off-print of Starke 1997 while writing Hawkins 1998), and it is fair to say that our views originating from different standpoints broadly converge. Gratifyingly this common view is also that followed by Niemeier, with whom I have had the privilege of discussing the question at Miletus, Karabel and elsewhere. It is thus of interest to compare Heinhold-Krahmer's current views on the subject.

As noted, her great contribution on Arzawa was published (1977) at the time when western geography lacked any points of attachment beyond the easily dismissed toponym identifications Millawanda-Miletus, Apasa-Ephesus, Lazpa-Lesbos, Wilusa-Ilion and Taruisa-Troia. This severe uncertainty is well reflected in her book, which extraordinarily does not even offer a map. She faithfully reports all previously proposed locations, but herself carefully abstains from supporting any of these or attempting to put together even hypothetically a coherent scheme. How in 2002 would she adjust her views to the new evidence, textual and archaeological?

In the context of the symposium of course, the question bears principally on the Wilusa-Ilion identification, since the line taken by the Kolb camp has been either to deny this (the line taken by Hertel 2001: 60) or to talk down its significance (Kolb 2002b). But this certainly cannot be discussed in isolation from the general picture now emerging, since all identifications are more or less closely interlocking. Heinhold-Krahmer's current attitude seems to be a continuation of the scepticism of her earlier work, namely that the new overall picture remains unproved. In fact she does not deny the possibility that it is correct, but prefers to emphasise the negative points. Such scepticism is beginning to look somewhat out of place.

We may question whether her criteria for absolute proof and certainty are not pitched higher for the west than elsewhere in Anatolia. One such criterion is the discovery of written documents (cuneiform tablets). Thus she allows certain identification for Boğazköy-Hattusa, Maşat-Tapikka, Ortaköy-Sapinuwa and Kuşakli-Sarissa (identifications: Maşat, Alp 1980: 58; Ortaköy, Süel 1999; Kuşakli, Wilhelm 1995: 37–42; 1997: 9–15), also Adaniya-Adana and Tarsa-Tarsus, although these apparently lack her criteria of definite proof (see below). But one may suggest that other criteria not fully accepted by her should be admitted. A Hittite toponym apparently ancestral to a Classical or even modern one may not count for much in isolation, but if geographical information attaching to it supports the location, or if it is linked to one or more other such toponyms, or even if there is appropriate archaeological evidence with which to connect it, then a combination of these factors must carry weight. Absolute proof may be lacking, but accumulation of these other criteria may lead to a high degree of probability, which is often as much as students of antiquity can expect.

For example: Hittite Ikkuwaniya looks like the forerunner of Ikonion-Konya, and the Bronze Tablet reference to it as one of the neighbours of the land of Tarhuntassa supports this (Hawkins 1995: 29–51); the cities Adaniya and Tarsa juxtaposed in the *hisuwa*-ritual (*Keilschrifturkunden aus Boğazköy* XX 52 i 19; see Goetze 1940: 54–6) are much more likely to represent Adana and Tarsus than either name occurring in isolation might be; Parha on the river Kastaraya, named as the boundary of Tarhuntassa on the Bronze Tablet, has already been mentioned (Otten 1988: 37; 1989: 18) — it requires a very resolute scepticism to doubt the identification of this combination with Perge on the Kestros, which is in a highly suitable location, though lacking the support of archaeological evidence for Bronze Age settlement. Yes, this may not be actually proved, but is this a useful approach? Likewise the cluster of Lukka-

land city names compared with the similarly close group of Classical Lycian city names (Poetto 1993: 74–82; Hawkins 1995: 49–57) is very hard to dismiss as a pure coincidence, and this too even though the absence of identified Middle-Late Bronze Age remains in the area has been thought to raise difficulty in locating Lukka here.

Millawanda and Apasa

These newly acquired clusterings of toponyms, Parha on the river Kastaraya and the Lukka cities of Awarna and Pinali, Patara, Talawa and Wiyanawanda, added to the long known pair Adaniya and Tarsa, must surely prompt a more receptive viewing of Millawanda-Miletus and Apasa-Ephesus, paired by Mursili's Annals, year 3, Millawanda in a fragmentary context along with the kings of Arzawa and Ahhiyawa before the commencement of the campaign proper, and Apasa as the royal city of the king of Arzawa, the main goal of Mursili's campaign (Goetze 1933: 36–9, 46–51. Apasa is not elsewhere attested). Both Millawanda and Apasa are placed on the coast by the reports that Arzawans flee from them by boat across the sea to 'the islands' (king of Arzawa from Apasa, Goetze 1933: 50; Piyamaradu from Millawanda, Tawagalawa Letter, *Keilschrifturkunden aus Boğazköy* XIV 3 i 61. Note that *gursawananza* has been shown to mean 'to the islands', Starke 1981: 143). But here of course we have a great deal more than simply a pair of Bronze Age + Classical toponyms, more even than their location on the coast: we have the archaeological establishment of Miletus as a major Middle-Late Bronze Age site (Niemeier 1997; Niemeier, Niemeier forthcoming), and the recent limited but unmistakable suggestion of a Late Bronze Age citadel and lower town at Ephesus (citadel finds: Büyükkolancı 2000: 37–41; lower city, Artemision sondages: Gates 1996: 319). Beyond even this we have now the fixed point of the KARABEL inscription, at the northern exit from the territory of Ephesus, indicating that this is the frontier of the kingdom of Mira, which would incorporate as argued the core of Arzawa proper with its capital at Apasa (Hawkins 1998: 22).

There is yet more, even beyond the identified paired city names, the archaeological evidence and the fixed point of a rock inscription: we may add a recognisable ancient topographical description, that of Mursili II (Goetze 1933: 54 [mount Arinnanda], 60–7 [Puranda]). When Mursili entered Apasa and the king of Arzawa fled across the sea to the islands, the population also fled, some up mount Arinnanda, some into the city Puranda, and some across the sea with their king. Mursili turned at once to reduce mount Arinnanda, which he describes in sufficient detail (Hawkins 1998: 22, with earlier references), that if it is to be sought in the environs of

Ephesus, it may confidently be identified as Classical mount Mycale, modern Samsun Dağ, as scholars knowing the area have been quick to perceive.

After withdrawing to winter quarters Mursili returned the following spring to reduce the city Puranda, into which a son of the Arzawan king had entered. Puranda was high (people go up into it and come down out of it), and Mursili was able to besiege it and cut off the water. After reducing it, he proceeded against the Seha River land to deal with its unreliable king Manapatarhunda. As had been suggested before, and was confirmed by the reading of the inscription, the Karabel pass probably marked the northern frontier of Mira on the way through to the Seha River land, the most obvious route for Mursili to have taken. The modern, doubtless also the Classical road runs north from Ephesus-Seçuk to modern Torbalı, north of which it divides, the northwest branch going to Izmir, the northeast direct to the Karabel pass and through to the Hermos-Gediz valley. A few kilometres to the west of Torbalı is a prominent hill, Bademgediği tepe, the east side of which is somewhat cut by the new motorway. From its summit there is a good all round view over the plain and directly across to the Karabel pass. Investigations by Recep Meriç of Izmir University have revealed the presence of a massive circuit wall halfway up the hill and further remains on the top, plausibly dated to the Late Bronze Age. What is observable of the remains on the hill characterises it as a refuge rather than an actual city. Further, circling the foot of the hill to the north side are the remains of a rock-cut ascent road, in which ruts of chariot wheels may still be seen. This hill must surely be a convincing candidate for the site of Puranda, as proposed by Meriç (Gonnet 2001; Greaves, Helwing 2001; Meriç, Mountjoy 2002 forthcoming).

Mira and the Seha River land

The kingdom of Mira as constituted by Mursili after his defeat of Arzawa has been argued (Hawkins 1998: 21–3; Starke 2000) to have included: (1) the core of Arzawa proper with its capital at Apasa, that is the Cayster valley and the territory of Ephesus; (2) the inland province of Mira bordering on Hatti in the neighbourhood of the rivers Astarpa and Siyanta, that is the western plateau in the neighbourhood of modern Afyon, whence run the main passes to the west; (3) the land of Kuwaliya, probably the headwaters of one or more branches of the Meander, for which the site of Beycesultan is a good candidate as capital or other city. I have also argued that such an extended political entity as Mira-Arzawa would have required good communications to hold it together, which would have best been secured by the inclusion of the Meander valley, the main pass from the plateau to the west.

The Seha River land, long recognised as a coterminous neighbour of Mira (Houwink ten Cate 1983–1984: 48, n. 38), is placed to the north of Karabel, thus identified as the Hermos (Gediz) valley. No evidence is available on the location of its capital or other cities, whether at possible Bronze Age predecessors of Sardis or one of the big coastal sites. Explicitly added to it by Mursili was Appawiya, plausibly identified toponymically and geographically with Classical Abbaitis, the headwaters of the river Macestus (modern Simav; Garstang, Gurney 1959: 97; Hawkins 1998: 23).

It is perfectly possible, though evidence one way or the other is lacking, that the Seha River land may have extended northwards to include the Caicus valley (modern Bakır), an actual identification with which was considered by Garstang and Gurney, on the grounds of the interest shown by its king Manapatarhunda in a letter in the land of Lazpa 'across the sea' (Hawkins 1998: 23; the Manapatarhunda letter is the subject of a detailed reconsideration by Houwink ten Cate 1983–1984). The same letter shows that Wilusa was reached from the Seha River land (see below).

Wilusa

The foregoing evidence on Millawanda and Apasa, and Mira and the Seha River land has been reviewed here in this detail in order to suggest that even in the absence of definite proof of geographical locations, continued total scepticism on the political geography of the central west is no longer appropriate. The approximate location and extent of the two main Arzawa kingdoms as outlined may now be claimed to have a high degree of probability. The third and least prominent Arzawa kingdom, Hapalla, is certainly inland, reached from the 'Lower Land' (Konya plain) and does not directly affect the present argument (placed in different areas on the maps of Starke [1997: 449] and Hawkins [1998: 31] — the crucial evidence is that Hapalla was reached from the Lower Land via the city Lalanda, which could be placed at Classical Lalandos [Starke] or Laranda [Hawkins]; see Frantz-Szabó, Ünal 1983). What does concern us is of course the fourth Arzawa kingdom, Wilusa, which was not mentioned by Mursili in any of his preserved documentation (Annals or Arzawa treaties), thus apparently not of direct military or political concern to him at the time. A reasonable inference is that it was more remote than the others and beyond his Arzawa purview. It becomes prominent explicitly as the fourth Arzawa kingdom in the reign of Muwatalli as known from his treaty with its king Alaksandu, which has a long but damaged historical preamble giving a survey of Hatti-Wilusa relations (recent translation, Beckman 1996: no. 13, with bibliography, 173).

The two principal questions to be reviewed in the present context are: (1) the identity (or not) of Wilusa and the site of Troy; (2) on the assumption of a positive answer, what the historical sources on Wilusa may tell us of the status of the site of Troy and its relations to the Hittite Empire.

The inferences on Wilusa already noted, that it was a more remote Arzawa land reached through the Seha River land, can now only point northwest to the Troad, since the locations of Lukka, Mira and the Seha River land hardly leave another geographically recognisable western Anatolian country. Also significant in this context is its earlier appearance as the 'land of Wilusiya' beside the 'land of T(a)ruisa' among the places defeated by Tudhaliya I/II in his campaign against the land of Assuwa following his Arzawa campaign (*Keilschrifturkunden aus Boğazköy* XXIII 11, 12; translated by [Garstang and] Gurney [1959: 121]; see now Starke 1997: 455. Note that the sword from the booty of the Assuwa campaign is identified as a Mycenaean type [Niemeier 1999: 150]). Since Assuwa is not again mentioned as a political force, its importance was probably terminated at this point. It is notable that at this date Wilusiya is one of the (incomplete) list summarised as the 'land of Assuwa' rather than one of the (even more incomplete) list of Arzawa lands of the previous campaign, which includes besides Arzawa itself the Seha River land and Hapalla.

Thus in the reign of Tudhaliya I/II Wilusa is regarded as being part of a kingdom of Assuwa, but after the dissolution of that power, at least by the reign of Muwatalli II, is specifically categorised as an Arzawa land. A further indication of Wilusa's location is the information that it was once a bone of contention between the Hittite king (Hattusili III?) and the king of Ahhiyawa (Tawagalawa Letter, *Keilschrifturkunden aus Boğazköy* XIV 3 iv 7–10; for the reading, Güterbock 1986: 37). Since there is no longer any question of locating Ahhiyawa on the Anatolian mainland, and it must be recognised as an Aegean power (see below), this pulls Wilusa also into a western coastal location.

These then are the main indications for the location of Wilusa, not it may be thought very precise but significant since the clearer recognition of the location and extent of Mira-Arzawa and the Seha River land, especially if the physical geography of western Anatolia is considered. There is however a reference frequently cited as evidence for the location of Wilusa, which cannot safely be utilised. The inference that Wilusa must be near Lukka was drawn from the passage of the Alaksandu treaty.

If I, My Majesty, shall campaign from that land, either from Karkisa, Masa, Lukka or from Warsiyalla,

you (Alaksandu) will campaign with me with your troops and horse (Beckman 1996: 84, 11).

The location of Lukka in northwest Anatolia goes back to Otten 1961: 112 (although later given up by him; see Röllig 1988: 3). Since geographical proximity of Wilusa to the named places is only one possible explanation of this provision, it would be unwise to give greater weight to this than to counter-indications. Thus it may well be that these places are named as being prominent western countries which are not 'Arzawa-lands', and are without kings and perhaps with a mobile and generally uncivilised population. The unjustified inference of the proximity of Wilusa and Lukka has led either to the placing of Lukka up near the Troad (for example, Macqueen 1968: 176; also Mellaart 1968: 187), or, especially, since the firmer establishment of Lukka in the direction of Lycia, to the placing of Wilusa in Caria (for example, Hertel 2001: 55; also Steiner, at the Tübingen symposium, in a paper circulated after the proceedings).

The reference cited above as showing that Wilusa was reached from the Seha River land has alternatively been interpreted to show that Wilusa lay on the return journey from the Seha River land to Hatti (Hertel 2001: 56; this interpretation had already been expressly rebutted by Houwink ten Cate 1983–1984: 42). The relevant lines from the Manapatarhuntha letter read:

3. [Gassu ...] came and brought the Hittite army,
4. [... wh]en they went *APPA* to smite the land Wilusa,
5. (I was ill ...).

The understanding hinges on the sense of the preverb *appa*, 'back' or 'again', modifying either the main verb pair, 'they went (back/again)', or the infinitive *walhuwanzi*, 'to smite (back/again)'. Thus the sense 'they went back to smite the land Wilusa' is only one of a number possible, and to understand 'they went back (from the Seha River land to Hatti) to smite Wilusa', and thus infer that contrary to any other indication Wilusa lay between the Seha River land and Hatti, is to impose an improbable interpretation on the passage. It is much more naturally understood, whatever the precise significance of *appa*, that Gassu arrived in the Seha River land with the Hittite army en route for a campaign against Wilusa. Incidentally this is one of the very few passages to suggest that a Hittite army might actually have reached Wilusa (other possible occasions: the Assuwa campaign of Tudhaliya I/II [see above], but the army may have gone no further than Assuwa; the Milawata Letter of Tudhaliya IV seems to envisage a military expedition to restore Walmu as king, Hoffner 1982: 131, figs 40/38–46/44). Otherwise the general impression given by the

texts is that it lay beyond Hittite military reach though within the political range. Geographically the Troad, protected to the south and east by the Ida massif, suits this picture well.

Proceeding from the evidence of the location of Wilusa to the question of the identity of the site of Hisarlık as the capital of Wilusa, we need only align the textual arguments which push Wilusa into the Troad with the archaeological presence there of that rare item, a typical Late Bronze Age city, which can be evaluated as a regional capital. The evidence of its citadel, impressive even in its partially destroyed state (see above), together with that of its lower city, now adequately if sparsely attested by the limited sondages (see also above), is sufficient to suggest the seat of a local ruler of the Troad of the period. While the textual evidence points to Wilusa as a land (it is always determined by KUR, except where this is replaced by other logograms, INIM, DINGIR^{MES}, as is regular), it would be normal to find the capital city with the same name.

This then leads directly on to the second question: what would the textual sources have us expect of the capital city Wilusa, and are these characteristics compatible with the archaeological features of the site?

As one of the four Arzawa-lands ruled by men acknowledged by the Hittites as kings, albeit vassals, Wilusa cannot have fallen below certain minimal requirements for the status of 'kingdom', whatever those might have amounted to in terms of wealth, population and extent. Not many kings and kingdoms are attested in Hittite Anatolia of the Empire period (see Klengel 1990). Of the other Arzawa kingdoms, Mira may now be recognised as having been very large, extending from the Hatti frontier at the western plateau all the way to the coast incorporating the core of former Arzawa, the Cayster valley with its capital at Apasa-Ephesus, and with Kuwaliya, the Meander valley. The Seha River land too could have been extensive, including the Hermos valley and with Appawiya the upper Macestus (modern Simav), and with the probability that it stretched at least as far north as the Caicus (modern Bakır) valley. Hapalla on the other hand though poorly attested and of uncertain location, does not seem to have been so important and could have been much smaller. In fact the texts give also very little indication of where Wilusa might have ranked between Mira and Hapalla. But it is surely unrealistic to deny that the Troad with its capital city at Hisarlık could not fulfil the requirements of what we know of the Arzawa kingdom of Wilusa. The combined evidence of the citadel and the lower town, incomplete as each may be through destruction and limited recovery, certainly suggests an at least middle ranking regional power of Anatolia.

Location of Ahhiyawa

In this discussion of the location of Wilusa, mention has been made above of the land of Ahhiyawa, in the context of the hostilities over Wilusa between the Hittite and Ahhiyawan kings. Little further need be added here to the long and controversial debate over the location of Ahhiyawa, beyond the observation that with the firming geographical locations Lukka = Lycia, Mira = Cayster and probably Meander valleys, Seha River land = Hermos and probably Caicus valleys and Wilusa = Troad, no plausible geographical space can be proposed on the Anatolian mainland for a 'great kingdom' of Ahhiyawa. Indications have always suggested that Ahhiyawa lay 'across the sea', reached by boat via the islands, particularly in connection with the nautical escapes of Uhhaziti and Piyamaradu, both understood to have sought refuge in Ahhiyawa. It does remain however to deal with one counter argument to this, first advanced by Steiner in 1964, and unfailingly repeated by him on every possible occasion since (Steiner 1964: 371; further, for example, 1998: 170, and recently at the Tübingen symposium): namely that since the writer of the Tawagalawa Letter (Hattusili III?) sent Dabalatarhunda the charioteer (LÜKARTAPPU) to fetch Piyamaradu from Ahhiyawa, Ahhiyawa cannot have been across the sea and must have been on the Anatolian mainland. This argument, which at best might be characterised as simplistic, is without substance and should be removed from the discussion forthwith. It is well established that by the late Hittite Empire 'charioteers' served as confidential agents (Singer 1983: 3-25, esp. 9), not simply as 'drivers', and in the cited context, contrary to what the unwary may have been led to believe, there is no reference to Dabalatarhunda bringing Piyamaradu from Ahhiyawa by chariot. This may be contrasted with an earlier passage in the same letter, where the Hittite king observes that he sent the crown prince to fetch Piyamaradu from Millawanda with the instructions: 'Go, drive over, take him by the hand, mount him in a chariot with you and bring him before me' (i 68-70). Millawanda was on the mainland, but Ahhiyawa was not.

Conclusion

The attitudes of Kolb and his supporters to the Hittite sources for Late Bronze Age western Anatolia are mutually contradictory, thus do not add up to any co-ordinated critique. Their spokesman, Heinhold-Krahmer, as noted above, emphasised the negative, 'unproven' approach to western Anatolian geography without really engaging with the cumulative character of current evidence, though she explicitly refused to rule out the Troy-Wilusa equation. Kolb himself, being mainly concerned to denigrate the significance of the site of

Troy, insofar as he considered the Hittite sources at all, denigrated also the significance of Wilusa, specifically arguing that the Hittites concluded treaties with a number of insignificant places or population groups (Kolb 2002b: 30, n. 109 — reference inaccurate, should read *Keilschrifturkunden aus Boğazköy* XIV 1 [...] obv. 66–74 [vgl. A. Goetze ... , S. 18 f.]). He supports this point by the quite unfounded supposition that the Hittites might have made a treaty with Talawa, which has not been preserved. In so arguing he displays scant regard for the sources, and in particular fails to address the implications of recognition of Wilusa as an Arzawa kingdom.

Hertel's views on the matter may be taken as represented in his booklet *Troia. Archäologie, Geschichte, Mythos* (2001). His section 'Troia und die hethitischen Quellen' (53–60), concludes definitively that Troy was not Wilusa (60), yet he cannot be said to take proper account of recent developments or to demonstrate much control over the sources. While emphasising the fragmentary nature of much of the evidence and the controversial nature of the geographical locations (referring principally to the pre-1988 state of the discussion), he nevertheless acknowledges recent locations for Tarhuntassa and Lukka, and even of Mira, but without mention of Karabel in spite of the fact that he uses the map from my article (49, Abb. 7). Yet discussing the location of Millawanda, he prefers to the Miletus-location that of Milyas which he explicitly places in Caria (!), revealing a geographical grasp that hardly inspires confidence.

For the location of Wilusa, he advances first (55, also 49, Abb. 17) the supposed proximity of Lukka (accepted as Lycia) rebutted above, which leads him to locate it vaguely north of Caria (the Milyas for him!) or Lycia. He then pairs this with the argument, also rebutted above, that Wilusa lay on the return route from the Seha River land to Hatti (56). In so doing he ignores or rejects the most important recent statement on the subject by Houwink ten Cate, though it is not clear, since he does not cite him by name, whether his entirely inappropriate remarks (56, end of last complete paragraph) are directed at the distinguished Dutch scholar. Perhaps prudently, Hertel does not attempt to mark his location of Wilusa north of Caria or Lycia and east of the Seha River land on the return route to Hatti on a map: he would be hard pressed to do so. In general, his argument may be not unfairly summarised from his statements that the identification of Wilusa with Troy is unassured ('ungesichert') and doubtful ('zweifelhaft') (56), therefore Troy was not Wilusa (60).

Thus the prosecution's handling of the Hittite historical and geographical background to Wilusa-Troy does not add up to a coherent case. Their verdicts may be summed up as 'unproved and uncertain' (Heinhold-

Krahmer), 'unimportant' (Kolb), and 'non-existent' (Hertel). Only the first scholar has an extensive knowledge of the subject, and she evaluates the material from an agnostic standpoint which has been substantially eroded by recent discoveries. The defence on the other hand makes full use of the new information to present a formidable case. The identity of Wilusa with Hisarlik-Troy is reaffirmed, as is its position and status as a regional capital, the seat of an Arzawa king. Our knowledge of the political geography of southern and western Anatolia has been transformed in the last 15 years, even if this advance has escaped the notice of those who continue to deny the possibility of constructing a plausible historical map for the Arzawa lands.

The economic role of Bronze Age Troy

Much has been made, in the criticisms recently raised in the wake of the popular exhibition *Troia, Traum und Wirklichkeit*, of two aspects of the way in which the site was presented there. The first, which has been considered in full in the preceding sections, is the degree of reconstruction involved in presenting a realistic image of an ancient site from archaeological evidence. Little more need be said of the pioneering use of computer-based techniques in creating a meaningful picture of Bronze Age architecture from archaeological traces within a long occupied and much altered citadel and urban settlement, except to emphasise the value of the work now taking place on this aspect of the site (and to ponder the irony of archaeologists being criticised for presenting their work in so immediate and attractive a form for popular exposition). More significant for current interpretations of Bronze Age economic history, however, is criticism of the second aspect, considered by Professor Korfmann in the chapter contributed to the catalogue under the title 'Troia als Drehscheibe des Handels' — criticism which raises once again the shadow of a 'minimalist' view of early trade propagated by followers of the late Professor Sir Moses Finley's views on the nature of the ancient economy, and which sets itself against some of the more promising current avenues of understanding patterns of regional development at this time. (For a recent survey of such views about the ancient Mediterranean, see Horden, Purcell 2000, and for the Bronze Age see Sherratt, Sherratt 1998.)

Was Bronze Age Troy a 'hub of trade'? The answer to this question depends partly on the standard of comparison — whether from the standpoint of second millennium Europe or Mesopotamia, say, or in the context of the much larger urban installations which emerged during the first millennium BC — but it also involves larger, theoretical questions about the role of

trade in pre-modern contexts, and its importance in the spread of urban life and economic activity. While there is an inevitable tendency to exaggerate the significance of sites whose names have become famous in epic poetry and have taken on a mythical dimension in popular imagination, and it is undoubtedly a useful exercise to look critically at the physical realities behind their later representation (see for instance Sherratt 2001 on the nature of the Mycenaean centres), such deconstructive re-appraisal should not obscure the more general point that human communities do not exist in isolation, and that the enlargement and elaboration of any settlement beyond that of its neighbours is symptomatic of a role within wider networks of contact and exchange. The architectural prominence and material wealth of Schliemann's Troy was already evidence of an unusual concentration of resources at a single site; the size of Korfmann's Troy indicates that its resident population found employment on a scale beyond the scope of a purely local economy. These two features, together with the longevity of the site and the effort put into defending its location, indicate that it played a significant role in urban history; and indeed its position on the edge of the Bronze Age urban world, and at a gateway to the territories beyond, gave it a particular prominence in the skein of Bronze Age economic relations.

The structural position of Troy in inter-regional contacts

In assessing how singular that role may have been, it is advisable to look at a spectrum of undiscovered possibilities. Archaeology depends in part on accident, and it would be naive to pretend that we know more than a fraction of the contemporary settlement network. Moreover the truly international role of Byzantium/Constantinople/Istanbul from late Roman times onward may have obscured the traces of any settlement which preceded it, however substantial, so that Troy may not have been alone as a significant Bronze Age site on the Sea of Marmara. This point is of obvious relevance to the identification of Hittite place names, not least the problem raised by the apparent existence of Wilusa and Taruisa as separate entities, and the possibility that Hittite rulers may have had as close an interest in the eastern end of the Sea of Marmara as in the western end. Two separate sites (with their own legendary histories) may have been deliberately identified with one location as part of the creation of Homeric epic in the late eighth century (in which there are many other striking examples of dual proper names, and a conspicuous concern to marshal a variety of legendary names into a single epic tale). The possibility that Troy may not have been the only significant Bronze Age site in the region does not diminish its importance at this time, however,

though it does warn against any claim of a wholly unique status. What is very evident is the significance of this maritime passageway throughout history, and the prominence of Troy itself is a sensitive indicator of this traffic.

This prominence arose naturally from the position of the site at the constriction point of patterns of long distance contact. Like Corinth, it was located both at the narrowing of a land route and between two bodies of water important for maritime traffic; like Corinth, too, it experienced an early prosperity and an eclipse by faster growing neighbours with locational advantages in an enlarged arena and increased volume of trade. From this perspective, Troy was to Constantinople as Corinth was to Athens (or Viking Hedeby to modern Copenhagen, to take a Baltic analogy). There are many examples of nodal points at critical locations, which have shifted their name and location but fulfilled similar functions within an expanding urban network. In Mesopotamia, for example, Babylon may be considered as the predecessor and functional equivalent of Seleucia/Ctesiphon-Baghdad as a central nodal point at the crossing of major routeways, and the shift from a location on the Euphrates to one on the Tigris reflects the changing importance of these waterways (Adams 1981). Few sites retain their primacy for ever, and Bronze Age Troy shone more brightly than its successor settlements on that spot. Understanding its importance during this time requires situating it within a growing but still shifting network of contacts. Fundamental to this is its position in relation to Black Sea and Aegean traffic, as well as to intra-Anatolian linkages. The Sea of Marmara forms a corridor between the Aegean and Pontic maritime interaction spheres, with constrictions at either end which form land bridges between Europe and Asia. The fact that the Dardanelles was apparently more important than the Bosphorus in the Bronze Age may indicate that the Aegean links weighed more heavily in determining its prosperity than Black Sea ones, though this position was reversed in the Christian and Islamic eras when the Black Sea became a gateway to trans-continental routes (across the Black Sea to central Asia and Scandinavia) as well as to earlier used coastal routes to the Danube corridor, the Pontic steppes and the Caucasus. This widening eastern outlook may have been responsible for the shift in emphasis from the Dardanelles to the Bosphorus, together with the military importance of the land link followed by the Via Egnatia. From the viewpoint of long term urban history, therefore, there is every reason to regard Troy as a prehistoric Byzantium. It was the renewed importance of Troy's location (albeit with a religious, and touristic, as much as a secular emphasis) in Hellenistic and Roman times which resulted in the degree of disturbance which hinders the recon-

struction of the Bronze Age lower town; though it was the growth of Byzantium into Constantinople as the nodal settlement on the Sea of Marmara which finally reduced its significance, and by preventing further build-up of occupation debris has thus allowed extensive archaeological access to the Greco-Roman and earlier Bronze Age remains. In this respect the site offers a unique window into urban settlement in these periods.

Archaeological evidence

Although Troy was a major centre throughout the Bronze Age, the evidence for this takes different forms. In the third millennium, it is striking for the character of its contents — Schliemann's 'treasures' with their fine craftsmanship and command of exotic materials, and also in its early adoption of wheelmade pottery (in shapes which often echo the metal vessels of the treasures). This indicates membership of a class of elite sites, of which Alaca Hüyük is an obvious parallel in inner Anatolia. During the second millennium the recovered artefactual material from Troy is less spectacular, and its external links are as often shown by imported pottery forms or evidence of large scale production as by truly elite objects. While this has been read by minimalists as a lessening of its importance, a more sophisticated interpretation — in conjunction with its evident increase in size and productive capacity — is that the nature of the archaeological record changed as its economic and political role matured, much as the showy splendour of Mycenae's shaft graves was succeeded by the more solid prosperity of palatial architecture: the propensity to put convertible wealth in tombs, or to hide hoards in times of insecurity, leads to archaeological fame but is less telling evidence of a leading economic role than the substance of the site itself. The flashlight images afforded by these early Bronze Age elite assemblages, however, give no doubt about the range of its contacts, in many directions; already during the later third millennium: amber from Scandinavia, lapis lazuli from Afghanistan (which reached northwest Anatolia via the Caucasus, perhaps through Alaca where the closest similar examples are found, to judge from the typology of the battle axes from Troy II [Treister 1996: 219–22]), silver, gold and tin from unidentifiable but distant sources, even the more humble bone artefacts such as the occurrence of a bone plaque of Castelluccio type, show far flung links to all points of the compass, to the edges of the known world (Korfmann 2001b: 355–7). It is evident that even before 2000 BC this was an unusually well connected location, and that the crucial position which it occupied retained its strategic economic importance through the succeeding Middle and Late Bronze Age phases, down to the last few centuries of the second millennium.

The reasons for this early prominence were well identified by the present excavator some 16 years ago (Korfmann 1986): the difficulty for Aegean shipping in penetrating the Hellespont and Dardanelles against adverse currents and winds, necessitating a longer or shorter wait in a sheltered position before penetrating further. It was control of this critical location which gave the inhabitants of the third millennium Troad their decisive advantage. Their prosperity, therefore, was a direct reflection of the importance of access to the Black Sea in the movement of materials along this corridor; and although the absolute volume of such traffic was undoubtedly small, the effects of these contacts in spreading Near Eastern innovations are being increasingly recognised by Bronze Age archaeologists in eastern and central Europe (Sherratt 1993). The appearance of tin as an alloying material in Early Bronze Age Hungary (O'Shea 1992), for instance, in the cemeteries at the confluence of the Maros and Tisza circa 2500 BC, falls within the horizon of early tin use in the eastern Mediterranean and Near East (and pre-dates the use of Bohemian tin in central Europe by several hundred years), indicating that the communities of the middle and lower Danube were linked to the Anatolian/Black Sea community in matters of elite technology. Similar considerations apply to the early occurrence of daggers with arsenic enriched surfaces at Usatovo as early as the late fourth millennium, or the long recognised analogies in the early second millennium between the goldwork of Troy and Transylvania (Kovacs 1999).

That all these examples concern the movement of small quantities of relatively precious items of material or ostentatious technology is a reflection of the importance of such long distance traffic both in spreading new modes of production and consumption, and in creating the initial advantage which made possible the emergence of complex sites such as Troy, which has no analogy (either in the third or second millennia) on the western Black Sea coast or in the Danube catchment. It thus had the additional advantage of being a gateway community on the edge of the world of urban settlements, at the interface between the east Mediterranean zone of urban economies capable of organising mass production (as shown by its wheelmade pottery) and the resource rich but less organised hinterland to which it gave access (Sherratt 1997: ch. 18). It is not surprising in this context that its artefactual repertoire has suggested to specialists (Barber 1991: 54) that the site had a special role in textile production, since this commodity is the major export in which such a settlement would have a decisive advantage, exchanging a manufactured product for raw materials. The appearance of textile derived motifs on the third millennium pottery of Early Bronze Age

cultures of the middle and lower Danube (for example, Nagyrev) would fit well with such an interpretation (for example, Kovacs 1977: fig. 4; see Bona 1975). The relatively small volume of this traffic (and the inherent difficulty of recognising both raw materials and textiles in the archaeological record, for which see Sherratt 1995) in no way diminishes its importance in the cultural context of the time.

It is not clear (at any stage in the Bronze Age) whether Aegean vessels passed along the Dardanelles and the Sea of Marmara into the Black Sea, or whether Troy acted as a trans-shipment point or port of trade between independent carriers (as Late Bronze Age Rhodes seems to have done, linking partly separate western Aegean and Levanto-Cypriot cycles of shipping). Even in the second millennium, it is most unlikely that vessels the size of the Uluburun wreck penetrated any further than Troy itself, though smaller craft may have continued the journey. Alternately, goods may have been conveyed overland to a Black Sea outlet, especially in the third millennium when quantities were small. This was probably accomplished by routes on either side of Marmara itself: either along the Gelibolu peninsula to reach the Black Sea coast via Kanlıgeçit, or along the southern side by way of Iznik and Izmit. Either of these routes, and the Bosphorus too if it was used at this time, would have provided an interface with coastal traffic around the Black Sea, which even in the second millennium probably took place in canoes rather than sailing vessels.

This Black Sea coastal traffic was of some antiquity, with its roots in Chalcolithic maritime exchange cycles or even earlier littoral adaptations (Price 1993); but its vitality from the later fourth millennium onward is likely to have been a response to the spread of ultimately Near Eastern innovations carried to a wider cultural sphere by new routes both to the Caucasus and to central Anatolia in the Early Bronze Age (Sherratt 1997: fig. 18.1). The location of Alaca Hüyük (presaging that of nearby Boğazköy-Hattusa) is symptomatic of north-south as well as east-west links within Anatolia, and the rich cemetery at İkiztepe near Bafra indicates one of the potential points of articulation between the central Anatolian and Black Sea trading networks. This coastal traffic is likely to have preceded any direct Aegean input, and indeed the foundation of Early Bronze Age Troy probably marks the beginning of a linkage between Aegean trading networks (themselves stimulated in a similar way by links from central Anatolia along the Meander valley route via Beycesultan) and this Black Sea cycle. The importance of the central Anatolian north-south route, from Cilicia or Malatya to the northern coast via Kültepe and the bend of the Halys, was both an incentive for the foundation of Old Assyrian trading

colonies in this region, and one explanation for the eventual emergence of the Hittite heartland as a centre of political power in the middle of this economic axis. The linkage between the Aegean and the western Black Sea through the Sea of Marmara opened up a second axis where this coastal traffic could be tapped.

The scale of Aegean maritime transport was revolutionised in the second millennium by the introduction of ships with sails, plying the route from the Levant along the south Anatolian coast, and penetrating as far as Crete and probably also up the west Anatolian coast as far as the Hellespont. At the same time, vibrant Bronze Age cultures with chariots, fortified settlements and an elaborate bronze metallurgy appeared in a zone from the Carpathian basin to the Pontic steppes, with related groups as far as the Urals and on the steppes beyond. This marked a new scale of activity in the lands around the Black Sea. Troy's rôle at this time continued to be that of a principal maritime gateway between the urban world and its immediate northern periphery. At the same time, its linkages within the Aegean and east Mediterranean were strengthened and to some extent re-oriented, especially in the Late Bronze Age when the Greek mainland saw a proliferation of palatial centres and an extension of maritime routes to Italy. The growing volume of production in western Anatolia is reflected in its increase in area in Troy VI, in parallel with the appearance of sites of comparable size and complexity elsewhere in the Aegean — even though only Knossos was to reach similar absolute dimensions (Whitelaw 2001: with comparative charts figs 2.10, 2.11). Troy continued to be a major player in the inter-regional exchanges of the time, a participation arising both from its coastal location (like that of contemporary Millawanda-Miletus) and from its unique role in relation to Black Sea access.

Nevertheless the scale of inter-Aegean exchanges should not be confused with the bulk transport of processed organic liquid products — principally oil and wine — in the east Mediterranean at this time, indicated by the distribution in quantity of Canaanite jars in the Levant and Cyprus, and on the south Anatolian maritime route plied by the ship wrecked off Uluburun (Pulak 1995). These routes articulated with the major artery across the Syrian saddle, which was the prime target of early Hittite expansionism, and whose major fortified centres make Troy seem relatively provincial. There is an evident zonation in the volume and intensity of Bronze Age trading activity, in which Troy joins the rest of the Aegean as an outer circle of participants arrayed around a core area of more advanced economies whose semi-processed products circulated in much greater quantities than in the area of their more distant trading-partners.

The apparent absence of trade in these bulk liquid commodities at Troy, however, does not preclude the manufacture of other organic goods there on an industrial scale, and the continuing importance of textile production (Blegen, Caskey, Rawson 1953). Part of this specialised production was probably exported to the Black Sea coastal areas (since no comparable production centres are known there, and where Troy retained its advantage), and some part within the Aegean: references in Linear B tablets from Knossos and Pylos to female textile workers from Lemnos, Cnidos, Miletus and Aswija (Assuwa?) (Chadwick 1976) suggest that western Anatolia had a particular expertise and reputation in this craft, and that its products would have been valued even in areas which now supported their own textile industries.

The Black Sea link gave access to other areas of specialist expertise: the importance of chariotry and horses is reflected in the occurrence of steppe types of horse gear for instance at Mycenae in the shaft grave period (Penner 1998), and such contacts must have been channelled through eastern Thrace and the Troad (using land routes as well as maritime transport in this instance). At the same time, Trojan consumers imported pottery (not in itself a valuable commodity, and thus indicative of routine exchanges which probably involved other materials as well) both from the Mycenaean area of mainland Greece, other parts of the Aegean and from Cyprus. Imported wares included both open shapes used as consumption vessels and closed container shapes such as stirrup jars for processed commodities such as olive oil; and the Mycenaean painted ware was also locally reproduced, in a process of import substitution (Mommsen et al. 2001).

All of this testifies to regular engagement in maritime traffic, carrying shipments which included quantities of everyday products as well as more valuable items. By the later 13th century, Trojan pottery itself — small quantities of grey wares probably travelling as part loads in other cargoes — was reaching the east Mediterranean, including Cyprus and sites in the coastal Levant (Allen 1991). Such cheap but identifiable items act as tracers for patterns of trade whose principal incentive was undoubtedly in the movement of more valuable materials. Occasional finds in exceptional circumstances indicate the character and direction of such traffic. The Uluburun wreck, with its tons of copper ingots, its glass, ivory, tin, textiles and terebinth resin, demonstrates not only the volume of traffic along major maritime arteries but also the catchment area of smaller items, which include a characteristic bronze axe of Lozovo/Pobit Kamyk type otherwise known only from the lower Danube area, and implying transmission via the Sea of Marmara and thus most probably through Troy itself (Buchholz 1999).

These patterns evolved over the course of the second millennium — increasing in scale after the 16th century when mainland Greece became a more active participant, growing to a peak in the 14th century, and changing character during the 13th century when less centralised forms of trade (including a lively commerce in scrap metal) threatened to upset established systems of distribution (Sherratt 2000). Coastal communities such as Troy benefited from this enlarged scale of maritime activity in the same way as the coastal cities of Cyprus, and the increasingly uncontrollable flows of material along the southern and western shores of Anatolia, form the background to Hittite military activity in the west. One underlying structural change from this time onward was the growing importance of links to central Europe via Italy and the Adriatic — an alternative access route to resources previously reached by the Danube corridor and part of a more direct linkage between the east and central Mediterranean. This resulted in a more pronounced east/west axis of maritime trade, in which the Black Sea played a gradually diminishing role, until its importance was revived with Greek penetration of this sphere in the seventh century BC. These changes in the final centuries of the second millennium were directly mirrored in the fortunes of Troy, whose reduced importance at this time is a sensitive barometer of this declining traffic.

It is possible, therefore, both to exaggerate and to underestimate the role of Troy as *Drehscheibe des Handels*. By comparison with some of its Bronze Age contemporaries, it was not metropolitan; but in the context of its regional urban partners it was a place of substantial importance, and in the eyes of its northern neighbours it must have been the brightest light on the horizon. The Sea of Marmara was not, during the Bronze Age, the entry point to routes which reached all the way across Eurasia; but it nevertheless gave access to the Danube, the rivers of the Pontic steppe and to the Caucasus, which provided important resources notably in stone and metal. When other routes to the central Mediterranean and central Europe gave alternative ways of acquiring such resources, towards the end of the second millennium, this importance slackened; and this phase lasted through the early centuries of the first millennium BC, after which its significance revived and in the seventh century Greek colonists penetrated along its coasts.

The economic importance of the Black Sea was decisively enlarged in the Hellenistic period when extensive eastern contacts were opened up; and by the fourth century AD it had reached new heights as the terminus of a northern Silk Road. The sites of Troy, Byzantium and its enlarged successor Constantinople mark these three stages of growth and together form an interrupted succession of major settlements where the

urban Mediterranean met a wider world, and where wealth accumulated from the encounter. Like the Venetian organisers of the Fourth Crusade in 1204, the legendary Greek warriors marshalled by Homer into a consolidated national epic in the late eighth century knew a site worth sacking — at the entry point to the Black Sea and the wealth to which it gave access.

Conclusions

We have approached the questions raised at the Tübingen symposium from our differing perspectives as specialists in Anatolian or Aegean archaeology and in Hittitology. Our findings, however, are entirely congruent: that Troy in the Late Bronze Age had a citadel and lower city appropriate to the capital of a significant regional power in western Anatolia; that it can most probably be identified as Wilusa; and that it occupied a position in the trading networks of its day which, in its context, can fairly be described as pivotal. Consequently we think that the criticisms raised against Professor Korfmann are unjustified.

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