Troy in clearer perspective

Dieter Hertel[1] and Frank Kolb[2]

*[1]University of Cologne, [2]University of Tübingen**

Abstract

Was late Troy VI a large Anatolian palatial city, a hub for trade, a commercial metropolis or even the centre of a Bronze Age federation of cities (*hanse*), as the present excavator of Hisarlık, M. Korfmann, has claimed in numerous publications? Several German archaeologists and historians have maintained the opposite and declared Korfmann's view of Troy as unfounded and a fiction. In a recent article in *Anatolian Studies* (2002: 75–109) D.F. Easton, J.D. Hawkins, and A.G. and E.S. Sherratt state that they share the opinion of the excavator. In reality, they do not defend the above mentioned views, but offer a more restrained description of the role of Troy VI in the Late Bronze Age. Their arguments, though, can be shown to be untenable due to insufficient evidence. Thus, a thorough criticism of Korfmann's statements remains fully justified.

Özet

Acaba geç döneminde Troya VI, şu anda Hisarcık kazılarını yürütmekte olan M. Korfman'ın pekçok yayınında iddia ettiği gibi, görkemli bir Anadolu şehri, ticaret merkezi sayılabilecek bir metropol ve hatta bir Bronz Çağ kent federasyonu (*hanse*) merkezi miydi? Birçok Alman arkeolog ve tarihçi bunun tersini iddia etmiş ve Korfman'ın Troya üzerindeki görüşlerinin dayanaksız ve kurgu olduğunu açıklamışlardır. *Anatolian Studies*'de kısa süre önce yayınlanan bir makalede (2002: 75–109) D.F. Easton, J.D. Hawkins, ve A.G. ve E.S. Sherratt Korfmann'ın görüşlerini paylaştıklarını belirttiler. Aslında yukarda belirtilen görüşleri tümüyle savunmadılar, ancak Geç Bronz Çağda Troya VI nın rolüne daha ölçülü bir açıklama getirdiler. Ancak tezleri yeterli kanıta dayanmadığı için, kabul edilemez olarak düşünülebilinir. Dolayısıyla Korfmann'ın tezine yapılan yoğun eleştiriler halen geçerliliğini korumaktadır.

The site of Hisarlık in northwest Turkey, usually identified with Troy/Ilios, has been a focal point for heated debate since before Schliemann came on the scene. It derives its importance from the Homeric epics and its relevance as an archaeological site from being the only thoroughly investigated prehistoric site on the western coast of Asia Minor. Only in recent years has growing attention been paid to other prehistoric sites farther south, such as Panaztepe, Limantepe, Bademgediği Höyük, Ephesos and Miletos. These investigations promise to put the ruins on the hill of Hisarlık into a more sober perspective — probably as a site of reduced importance.

The alleged prominence of the settlement on the hill of Hisarlık and in particular of late Troy VI (14th century BC) was raised to new heights by its present excavator, Manfred Korfmann. He published his views not only in the volumes of *Studia Troica* and in scientific journals, but regularly provided for ample media coverage in popular journals, newspapers and on television. In these publications he apparently offered a completely new picture of Troy VI based on solid facts. This tendency culminated in the Troy expositions at Stuttgart, Braunschweig and Bonn in the years 2001–2002.

*Hertel participated in the Troy excavation from 1989 to 1991 and visited the site every year from 1997 to 2000. Kolb visited the site intensively in 1989 and 1997 (together with Korfmann). We are very grateful to Kirsten Gay (Tübingen), who essentially improved our English text.

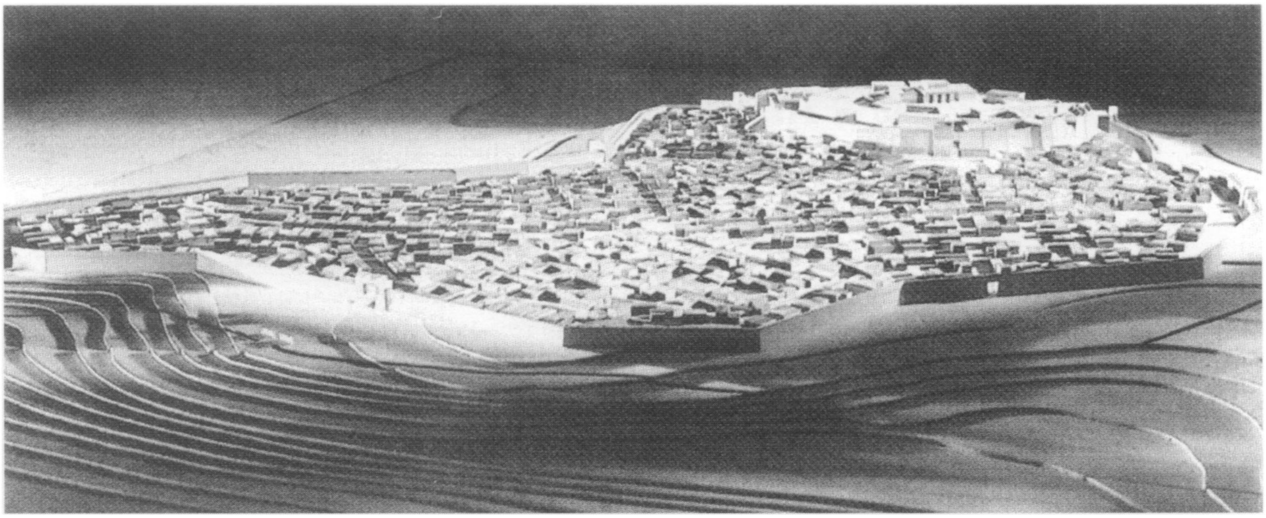

Fig. 1. Model of Troy VI presented as a reconstruction by M. Korfmann during the 2001/2002 Troy exhibit (Korfmann 2001c: 73, fig. 77)

Though Korfmann's 'reconstruction' of Troy VI met with increasing dissatisfaction among German prehistorians, archaeologists and historians, the usual and sometimes problematic discretion among scholars led to only occasional polite criticism (for example, Cobet 2001). These reservations were simply ignored by Korfmann. Combined with his predominance in the media, he succeeded in suffocating internal and public criticism until July 2001, when a journalist of the *Berliner Morgenpost* asked Frank Kolb for his opinion on Korfmann's Troy. This was the beginning of a heated debate which on Kolb's initiative culminated in the Troy Conference at Tübingen in February 2002.

D.F. Easton, J.D. Hawkins, A.G. Sherratt and E.S. Sherratt (2002: 77) introduced themselves to the readers of *Anatolian Studies* as 'invited observers'. This is a euphemistic description of their role at this conference. Easton has been a long time participant in Korfmann's excavations at Troy and has published his dissertation in Korfmann's series *Studia Troica*. Hawkins and A.G. Sherratt arrived at Tübingen with written statements and illustrations prepared in advance and took part in the discussions as staunch supporters of the excavator.

Consequently, the content of their article makes these 'observers' appear more like the 'lieutenants' they describe (76) as disseminating the excavator's opinions. Although they know and occasionally quote from the website of the Abteilung für Alte Geschichte at the University of Tübingen (Kolb 2002), they prefer to ignore the alternative version presented there and obediently follow Korfmann's distorted presentation of the preliminaries, causes and developments of the Troy controversy. They even repeat (76) the insidious suggestion of 'envy' on the part of his opponents. Reproaching us for 'unacademic barbs', they conceal from their readers the fact — well known to them — that it was Korfmann who introduced such 'barbs' into the academic discussion on Troy several years ago, when he called the geoarchaeologist E. Zangger a 'Däniken' (i.e. author of fictional history) and denigrated him as having never published an article in a good scientific journal. Korfmann was sentenced in court to pay half a million German marks in case of recurrence (see Kolb 2002). They also omit to mention that several renowned German archaeologists and historians have supported us in accusing Korfmann of 'Fiktionen', 'Anachronismen', 'Irreführung der Öffentlichkeit' (misleading the public) and forcing through his view of Troy with brute authority and efficient media activities ('Mit brachialer Autorität und effizienter Medienarbeit hat Korfmann sein Klischee von Troia in der Öffentlichkeit durchgesetzt' U. Sinn in *Stern* 1–2 August 2001; see Kolb 2002, with Anlage 1; also Cobet, Gehrke 2002).

It is even more unfortunate that the four authors' presentation of the scholarly discussion and the basic facts of the Troy question is so one-sided. First of all, they have not sufficiently informed their readers about the statements Korfmann made regarding Troy VI which provoked the criticisms mentioned above. The authors point to certain exaggerations (for example, 94), yet at the same time try to play them down. In this respect, they follow Korfmann's own defensive strategy. His assistant P. Jablonka (2002: 265) excuses Korfmann's feats as products of overenthusiasm ('Überschwang'). However, this 'Überschwang' has manifested itself for several years and has found its way into numerous publications. The statements caused by this enthusiasm form the core of Korfmann's view of Troy VI. Therefore, they have to be specified here. According to Korfmann (1997: 94–5; 1997a: 83–90; 1998: 369–85; 2001: 355–

68, especially 357, 359–60), late Troy VI was an Anatolian palatial city of up to 10,000 inhabitants, thus confirming the description of Troy in Homer's *Iliad*. Moreover, Troy VI was a hub for trade between the Aegean world, Anatolia, the Balkans and the Black Sea, a commercial metropolis and even the centre of some kind of Bronze Age *hanse* (a late medieval city federation), whose members were the Trojan allies mentioned in the *Iliad*. On a map in which all important trade routes of the 20–18th centuries BC cross at Troy (Korfmann 2001: 356, fig. 383), Korfmann attempts to illustrate these ideas. In reality, he has arbitrarily prolonged the routes in space and time and added new ones *ad libitum* without the support of any evidence.

At the end of their article, the four 'observers' come to the conclusion 'that the criticisms raised against Professor Korfmann are unjustified' (106). In reality, they have not made a serious effort to defend Korfmann's 'enthusiasm' cited above, which can only mean that they share our opinion in this respect. With regard to their rescue operation in favour of the excavator, the authors have portioned out the burden: Easton (82–94) deals with the archaeological evidence of the lower city, Hawkins (77–81, 94–101) with that of the citadel and repeats his well known arguments for identifying Troy with Wilusa, and finally, the Sherratts (101–6) deal with 'the economic role of Bronze Age Troy'. This last section of the article is the shortest and most restrained defense of Korfmann's position. It will be discussed only shortly, since an extensive essay on this subject is in press (for the moment see Kolb 2002a).

Troy VI (late): a commercial city and trading centre?

The Sherratts expand for the most part upon general and geographical considerations which would be relevant only if it could be proven that Troy's potentially favourable position was utilised already during the Bronze Age. For this, however, they are not able to adduce even the slightest evidence. Their argument that 'the size of Korfmann's Troy indicates that its resident population found employment on a scale beyond the scope of a purely local economy' (102), is a circular one, since it presupposes the proven existence of a considerable population for Troy VI. Yet, this is not supported by archaeological evidence (see below, 'House remains in the so-called lower city').

Moreover, the Sherratts omit several decisive facts. There is hardly any evidence for exports from Troy. Imported objects in Troy VI levels are extremely rare. Very little imported Mycenaean and even less Cypriot pottery has been found. Copper oxhide ingots, which the Sherratts regard as so important in the context of Bronze Age trade, have not been discovered. Bronze Age stone anchors are also absent. There are no imports from Hittite Anatolia, from the Balkans and the Black Sea region positively attested from the hill of Hisarlık or the cemetery of Troy's supposed harbour in the Beşik bay. The Beşik bay is where Korfmann believes all the ships wanting to pass the Dardanelles and the Bosporus had to anchor until the arrival of more favourable winds. M. Basedow, who published the material from the Beşik bay cemetery in her Tübingen dissertation (Basedow 2000: 62–144, especially 125–6, 163–4), emphasises that nothing at the Beşik site points to commercial connections between Troy and the Black Sea region or to any other considerable trade relations involving Troy VI. Interestingly enough, the Sherratts do not quote this important book.[1]

The Sherratts make strangely contradicting statements on sea traffic between the Aegean and the Black Sea. On the one hand, they seem to approve (103) of Korfmann's opinion, on the other hand they admit that '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' (104). Essentially, this undermines the core of Korfmann's view and coincides more with our opinion. The Sherratts try to make good their argument by suggesting that 'alternatively, goods may have been conveyed overland to a Black Sea outlet'. However, the distribution maps for Aegean or Aegean type objects in the Balkans and the Black Sea region suggest that — apart from trade on the Adriatic Sea — Aegean goods were transported on overland routes which started from the north coast of the Aegean Sea and continued along the river valleys of Macedonia and Thrace towards the interior of the Balkans. The clearest indicator of overseas trade with Aegean goods in those times is Late Bronze Age Mycenaean pottery. The mere fact that not one single Mycenaean pottery sherd has been found until now on or near the shores of the Black Sea and north of the Rhodope mountain range which roughly constitutes the border between Greece and Bulgaria, definitely speaks against sea trade between the Aegean and the Black Sea. Obviously, Bronze Age commercial vessels were not able to overcome the adverse currents and winds in the straits. Thus, the conclusion drawn by the Sherratts that Troy was 'a gateway community on the edge of the world of urban settlements' and that its 'position in the trading networks of its day ... can fairly be described as pivotal' (103, 106), is entirely groundless (see also Hänsel 2003).

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[1] Basedow's dissertation, published as *Studia Troica Monographs* volume 1, was, until very recently, not available at Tübingen, not even in the library of Korfmann's own Institute.

The Wilusa question

The problems raised by Hawkins' paper on the Wilusa question (94–101) are different. His conviction that the state of Wilusa attested in Hittite documents was identical with Ilios/Troy, can be seen as fuelling the already existing circular reasoning. Korfmann refers to the alleged identity of Wilusa with Ilios to argue that the settlement on the hill of Hisarlık must have been important in size and population, while Hawkins uses Korfmann's conclusions from excavation as an argument for his location of Wilusa on the site of Hisarlık, where he identifies 'a significant regional power' (100, 106). Hawkins is certainly right in underlining the progress achieved during the last two decades toward reconstructing the political geography of western Asia Minor in the Bronze Age. However, he tends to exaggerate this progress. For example, the location of certain small states — and Wilusa was a small state too — as well as the borders between states in western Anatolia are by no means well defined. A comparison between Hawkins' map (95, fig. 11) and that of Korfmann's faithful follower Frank Starke (2002: 304–5) sufficiently demonstrates the uncertainties about the political geography, especially of northwest Anatolia. Contrary to Starke, Hawkins refrains from filling up the large 'empty' areas north of the Caicus and Maecestus rivers and east of the Troad. Indeed, both hesitate to locate the land Warsiyalla, which is mentioned in the treaty between the Hittite king and Alaksandu of Wilusa (Beckman 1999: no. 13, 11; Easton et al. 2002: 99) and may have been situated close to Wilusa.

Hawkins claims to have produced cumulative evidence which would lend his hypothesis a 'high degree of probability' (97, 98). In reality, he has made only a series of more or less plausible suggestions. The location of Wilusa rests on Hittite documents whose interpretation is controversial. The way in which Hawkins discards opinions different from his own is highly questionable. While he admits that the geographical proximity of Wilusa to other countries (Karkisa, Masa, Warsiyalla and Lukka) named in the Alaksandu treaty is 'one possible explanation', he continues 'that it would be unwise to give greater weight to this than to counter-indications'. A few lines further on he speaks of 'the unjustified inference of the proximity of Wilusa and Lukka' (99). There are more arguments of this kind which lend little credibility to Hawkins' conclusions, apart from the fact that he disregards certain difficult problems, for example, the possible location of Taruisa and Dardania in the Troad. By using a self-assuring language (for example, 'perfectly possible') with regard to his own hypotheses, he proceeds from an accumulation of mere possibilities to 'a high degree of probability' (99) and ends up (101) with the confident statement to have presented 'a formidable case', with the conclusion that 'the identity of Wilusa with Hisarlık-Troy is reaffirmed'. This is by no means the case, and the position of S. Heinhold-Krahmer (1977; 2003) whom Hawkins reproaches for evaluating 'the material from an agnostic standpoint', is not only shared by other distinguished scholars (for example, Röllig 1992: 194–5), but is also a more prudent position.

Troy VI (late): an Anatolian palatial city?

Unfortunately, those all too confident statements on the part of Hawkins and Starke have encouraged Korfmann to proclaim the identity of Ilios and Wilusa as a proven fact and to call the settlement on the hill of Hisarlık affirmatively Ilios/Wilusa (Korfmann 2001d: 1). While Hawkins admits that the so-called lower city of Troy VI is only 'sparsely attested' (100), the less critical excavator has amplified these sparse remains to a densely built-up city 'reconstructed' (fig. 1) in a model (Korfmann 2001b: 17, fig. 23; 2001c: 73, fig. 77). Korfmann's estimate of up to 10,000 inhabitants within an area of about 27ha, defined by an alleged defensive ditch (see fig. 2) is simply baseless. If this estimate were correct, the population of Troy VI would have exceeded even that of the Hittite capital Hattuşa which, according to its present excavator (personal communication), may have accommodated 3,000–6,000 inhabitants within a walled area of about 180ha. Moreover, Korfmann's model confines the densely built-up area to that within the contour of an alleged settlement wall running about 100m behind the ditch mentioned above, which means that he crowds together Troy's population in an area of about 11ha, an area where at most 2,000 people could have lived.

The four authors obviously feel the need to compensate for the pitfalls of the excavator's 'enthusiasm'. They concede that Korfmann's reconstruction of late Troy VI 'may ... be criticised for greatly exceeding the available evidence' and for 'a huge disparity between Korfmann's comprehensive reconstructions and the extent to which the lower city has actually been excavated' (77, 93). They admit (76) that 'a hypothetical central palace and a tier of surrounding buildings' is shown 'for which no evidence survives' and that 'this is even more marked in the case of the lower city'.

Their statement that at least partly 'regular archaeological practice' has been observed by Korfmann is too optimistic. They maintain that 'walls and houses are reconstructed from the surviving plans, and ... with the knowledge of the traditional local building techniques' (77).

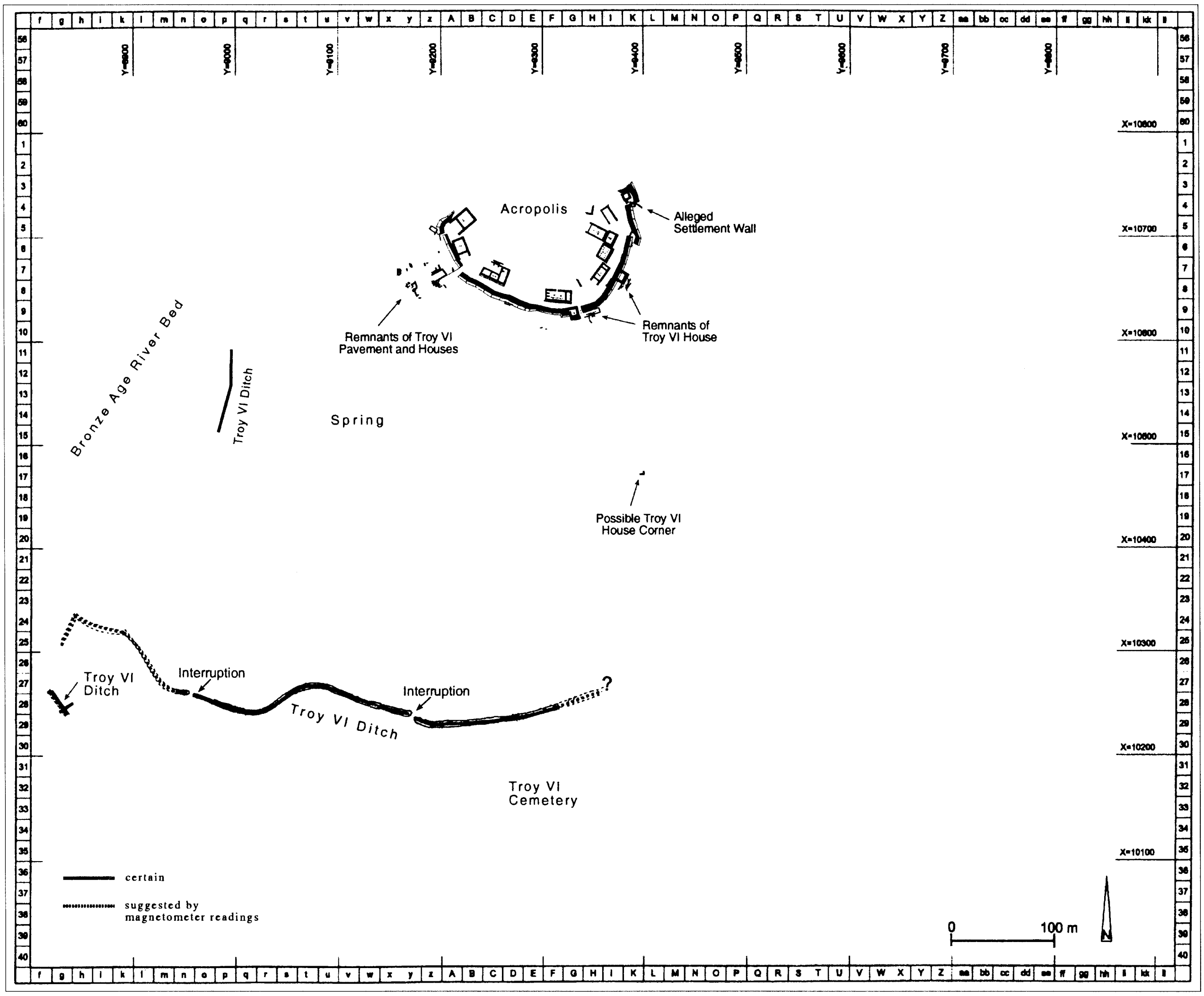

Fig. 2. Troy VI: schematic plan of actual remains discovered up to 2001, based on plans published by the Troy Project (drawn by F. Kolb, C. Drosihn, A. Thomsen)

Complete ground plans of houses have only been preserved for a few buildings within the acropolis area, while no complete ground plans of Troy VI houses outside the citadel have survived (see fig. 3). The reconstruction of the houses has been deduced from present-day Turkish houses built of sun-dried brick, regardless of the different social structures of Bronze Age Troy which must have influenced the interior organisation of the houses.

In spite of the sceptical remarks quoted above, the four authors try to defend the model of Troy VI as 'a perfectly valid and useful exercise to construct a well thought out, imaginative presentation' (91, see also 101). Easton (93–4) criticises Kolb (2003: 12) for demanding 'that a model or picture may show nothing whose existence is not firmly attested', and he maintains that 'the Büyükkale [i.e. the acropolis of Hattuşa] model ... suggests that the excavators of Boğazköy feel constrained by no such stringent code'. Both remarks are misplaced. The criticism of Korfmann's model concerned its claim to be a 'reconstruction', which by definition implies a precise knowledge of what once existed. In the case of the Troy model, there is no archaeological evidence for at least 95% of the buildings. The result resembles more an archaeological Disneyland than a reconstruction. A comparison with the Büyükkale model is simply out of place, since in this case almost all the reconstructed buildings are based on actual ground plans, with only a few gaps being plausibly filled (see Seeher 2002: 96–7, figs 3, 4). The same is true for the model of the Anatolian city of Kuşaklı-Sarissa (Müller-Karpe 2002: 178, 181). In the light of these examples, Korfmann's model clearly violates the rules of conventional archaeological practice.

The defense of Korfmann's reconstructions by the four authors is the more amazing given that Korfmann has already distanced himself from his model during the Troy debate.

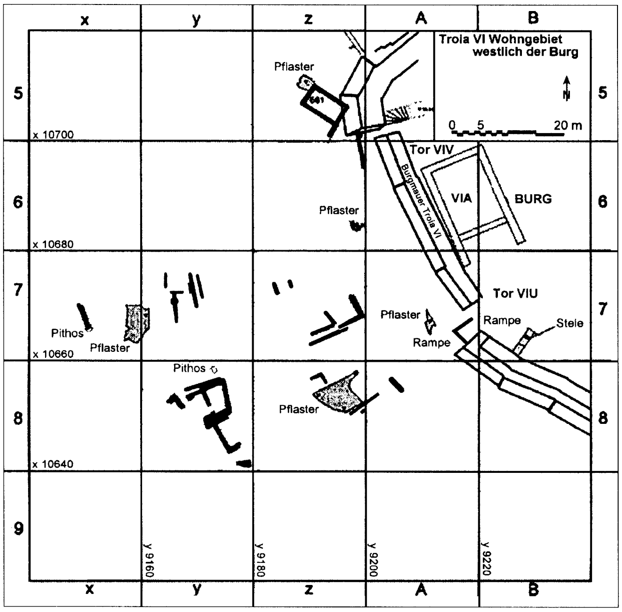

Fig. 3. Plan illustrating the Troy VI remains of pavements and houses found immediately outside the acropolis wall, as based on drawings of the Troy Project (Korfmann 2002: 12, fig. 10), drawn by F. Kolb, C. Drohsin

First, he revoked its designation as a reconstruction and claimed not to have been responsible for it, blaming its 'builders' for deviating from his original intentions. The accused former collaborators have now responded in an open letter declaring that Korfmann is guilty of 'brazen lies' (Kolb 2002: Anlage 6). Secondly, at the Bonn exposition, a CAD model of Troy VI was shown which contained a much reduced number of houses (about 180) and many fields interspersed between the buildings, especially in the southern half of the so-called lower city. This most recent version of Troy VI could hardly have acommodated more than about 1,000 inhabitants, the number tentatively proposed by Kolb (2003: 19) and criticised by Easton as 'decidedly minimizing' (94).

The citadel

Hawkins (77) starts his paper on the citadel (77–81) by suggesting that Kolb (2003: 21–6, figs 3, 5, 12, 18, 20, 21, 23a–25) deliberately printed city plans on different scales in order to let Troy appear small. This accusation is unjustified for two reasons. First, the plans were simply taken from other publications together with the scales. Second, they were not used to compare size but structure, i.e. to show the absence of a palace building of Anatolian or Near Eastern type on the acropolis of Troy VI (see figs 1, 2, 6). Kurt Bittel (1976: 138), excavator of Hattuşa and one of the 'big men' of Anatolian archaeology, had already characterised the structure of the citadel of Troy VI as different from the multi-roomed residential and administrative palaces of Hittite Anatolia. At the Tübingen Troy conference H. Hauptmann (Heidelberg), a renowned representative of Anatolian prehistory, took the same position in his paper. B. Hänsel (Berlin), a distinguished expert of Balkan prehistory, pleaded for seeing Troy VI rather in the context of north Aegean and southern Balkan settlements (see now Hänsel 2003: 112–19).[2]

Defending the quality of Troy VI as a *Residenzstadt*, Hawkins does not mention the fact that Korfmann did not simply characterise the settlement in that way but as an *altorientalische Residenzstadt*, and that he classed Troy with the 'größeren Handels- und Residenzstädten Kleinasiens und des Nahen Ostens' (Korfmann 2001: 397). Furthermore, it was Korfmann who allocated to Troy VI a place amongst cities like Alışar Höyük and Boğazköy-Hattuşa — without carrying out an exact analysis of the structural features of an *altorientalische Residenzstadt* (Korfmann 1995: 180–1). Hawkins, though, conveys the impression that Korfmann characterised Troy VI as a regional centre. Moreover, Hawkins ignores our most decisive argument against Korfmann's views, namely the absence of those architectural requirements necessary for supposing the existence of a palace area on the hill of Hisarlık. These are 'die Konsequenz des Planentwurfs, die Komplexität, Differenziertheit und Multifunktionalität' (Hertel 2002a: 5–6) which characterise Anatolian, Near Eastern and Mycenaean palace areas of those times. One finds such an architectural ensemble organised down to the last detail, with a clear functional structure serving the needs of the ruler, in the regional centres of Ras Şamra-Ugarit or Tell Açana-Alalach (Kolb 2003: 23, fig. 23a; Woolley 1953: 101–10, figs 14, 15, concerning level IV). Equally absent from the hill of Hisarlık are an orientation and subordination of the acropolis buildings to a central building as can be found in the palace area of Mycenaean Pylos (Kolb 2003: 23, fig. 25), which was a regional centre and not the capital of a big empire.

[2] Easton (84) stated inaccurately that Jablonka's paper at the conference found the approval of 'the prehistorians'. His remark that 'it was very unfortunate that Jablonka's paper, perhaps the most crucial of the whole symposium, was allowed only 20 minutes by the organiser', is a distortion of the facts. Korfmann was free to arrange the timetable of 'his side', and his strategy was clearly to divert attention as much as possible from his excavation to other subjects, i.e. Wilusa and the Homeric question.

The lower city

Introduction

In his paper at the Troy conference, Hänsel stated that Korfmann had not proved Troy VI to be a city at all, given that the hill of Hisarlık has shown no evidence for a diversified urban architecture, for a settlement and population of considerable size, and for Troy's function as a central place for economic, religious and cultural (for example, writing) activities. The existence of a settlement of unknown character outside the citadel had already been discovered by Schliemann, Dörpfeld and Blegen. Easton (84–5) is right in noticing that Korfmann's excavation has only reaffirmed this fact. We have pointed this out as well, underlining that the new excavations have not essentially enriched our knowledge about the settlement structure of Troy VI (Hertel 2002: 44; Kolb 2003: 12, 32), and we criticised Korfmann's unfounded statement that he had discovered a large lower city. It is enigmatic why Easton calls this a 'waspish accusation' (84). In the *Festschrift* for Korfmann, his former collaborator B. Kull (2002: 1182), characterises the lower city as largely invented ('zu großen Teilen erfunden'). Korfmann's arguments for the existence of a large lower city of late Troy VI are the following: (1) scatters of pottery sherds over parts of the area; (2) an alleged settlement wall; (3) a supposed defensive ditch; (4) the existence of houses with stone foundations and mud-brick superstructure in certain areas. The first argument can be readily dismissed. The settlement is situated on a hill, and it is quite normal to expect rubbish material, in particular pottery sherds, to be washed down the slopes forming a more or less extensive halo around the settlement. This is not a reliable indication of the extent of the actual settlement area.

The settlement wall

The existence of a settlement wall might be a valuable indicator for defining the limits of the settlement. An attentive reader, though, will easily perceive that even Easton remains unconvinced that the piece of wall (7m long and, according to the stone plan in *Studia Troica* 1996 [40, fig. 33], about 1m wide)[3] excavated by Korfmann east of the northeast bastion of the acropolis wall (figs 4, 5, 8) and identified by him as the settlement wall of Troy VI, actually served this purpose. He speaks of 'a possible wall' (76), a 'presumed city wall' and concludes that 'we cannot yet be entirely confident that it is what Korfmann thinks it is' (93). Nevertheless, he goes to great lengths to disguise the weaknesses of Korfmann's arguments and in particular Korfmann's manipulation of the published plans.

[3] In Klinkott, Becks 2001 (409, fig. 462) part of the wall has falsely been prolonged up to the bastion.

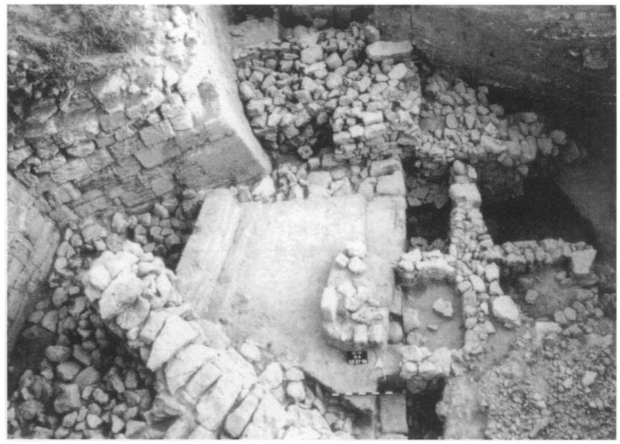

Fig. 4. Proposed Troy VI settlement wall and part of the mud-brick platform showing a re-used Troy VI block at the northern end of its eastern face (Klinkott, Becks 2001: 414, fig. 469)

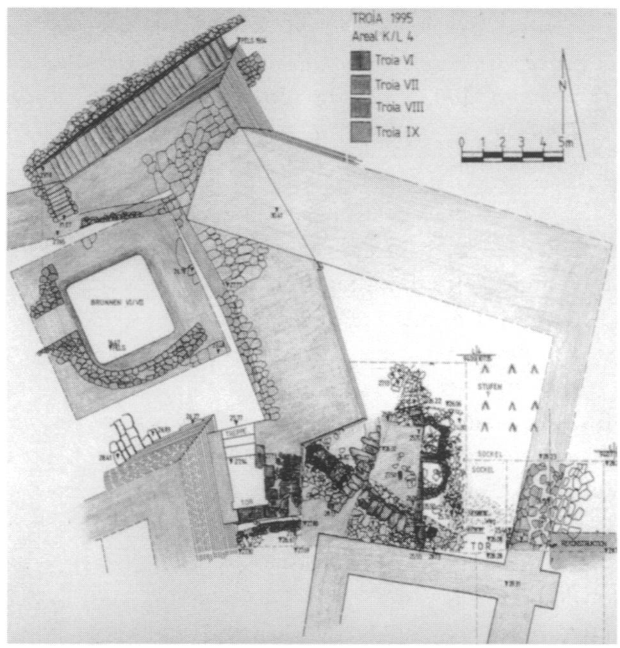

Fig. 5. Plan of excavation area around the northeast bastion and a drawing of the proposed Troy VI settlement wall (Kolb 2003: 20, fig. 17 = Korfmann 1996: 40, fig. 33)

When considering the complexity of the findings in this excavation area, it is necessary to quote Easton's (91–2) statements at some length. According to him, the wall is,

> 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. 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 Korfmann's plans a wall extending eastwards of the presumed gate has been entered at first as 'reconstructed' (fig. 5), then as a 'supplement' and, finally, for example, in the companion volume to the Troy exposition in 2001, as really existent (fig. 6) (Korfmann 1996: 41, fig. 34; Becks, Thumm 2001: 420, fig. 480). During the Troy conference, this was excused as a blending of red colours due to a draughtman's error. Unfortunately, the same misfortune happened in a plan drawn in green colours (see Becks, Thumm 2001: fig. 480; Kolb 2003: 18, fig. 17.5.9; 2003a: 135). Easton's (93) statement, that 'inspection with a magnifying glass suggests that the digital printing may have been to blame', strains credulity. Our own magnifying glasses do not disclose any difference between this one and other continuously drawn lines in the plan. Moreover, Easton's explanation decisively founders on the fact that those misleading plans had already been published for the first time three years before the publication of the companion volume mentioned above (see Korfmann, Mannsperger 1998: 32, 36, figs 48, 56). One wonders why Korfmann did not correct these serious errors in the meantime. In any case, a reconstruction of something for which there is no evidence is methodologically inadmissible. It is obvious that this piece of wall is of paramount importance to Korfmann's wish to have a gate and a settlement wall at this point. Again he shifts the blame for inaccuracies and distortions on to his collaborators.

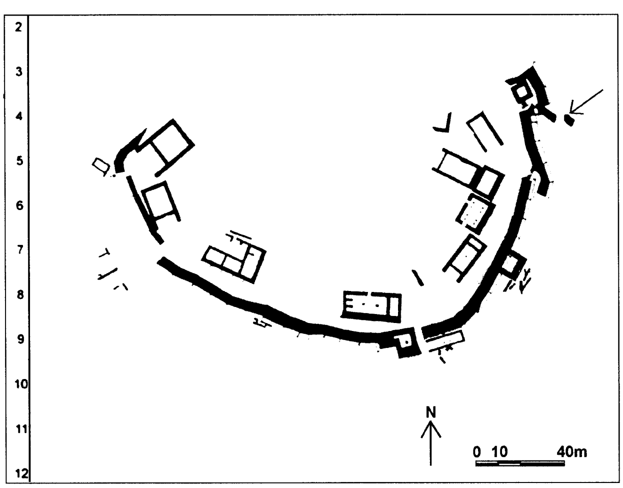

Fig. 6. Plan of Troy VI: acropolis and immediate surroundings including the proposed Troy VI settlement wall and the wrongly entered stretch of wall east of it, marked with an arrow (Becks, Thumm 2001: 420, fig. 480)

Concerning this so-called city wall, initially dated by Korfmann to middle Troy VI, we pointed out that the wall must have been built after the construction of the northeast bastion, in other words, not before the latest phase of Troy VI, since the bastion has been dated securely to phase VIg (Blegen et al. 1953: 20, 109–11) and the 'city wall' does not join into the bastion's masonry, as is erroneously maintained by Klinkott and Becks (2001: 411, figs 5, 8, note 3). Our dating is confirmed — as Easton (92) admits — by the latest analysis of the pottery found in the area of the wall. It is now dated to a time 'considerably later than middle VI and should postdate the building of the bastion'. Unfortunately, there is no information about where precisely the pottery came out: whether from the platform of stone fill or from the mud-brick capping into which the stele was set, from the 'city wall' itself or from the mud-brick layer covering the whole complex (Korfmann 1996: 42; Easton et al. 2002: 92).

Easton maintains — without enlisting any further arguments — that it was in Troy VIIa that the 'mud-brick layer was laid across the top of the whole complex'. Following Korfmann, he separates the platform of stone fill, 'with the mud-brick capping into which the stele was set', from the 'city wall', the first complex being slightly earlier than the latter. But why must there have been a chronological gap between these two constructions? At no point, either in the text or plan, have Easton and Korfmann (Korfmann 1996: 40, fig. 33; Easton et al. 2002: 90–3) been able to demonstrate a join between the two complexes. A look at the photographs shows that nothing indicates the existence of a join. One rather gets the impression that the platform and the 'city wall' were part of a coherent building project (see fig. 4; and especially Korfmann 1997b: 48–53, figs 45, 48, 50). Furthermore, Easton does not take into account the argument (Hertel 2002a: 13) that the whole complex may have been built in VIIa. The quality of the masonry is poor in comparison with the lower courses of the northeast corner of the bastion, and the wall bordering the platform of stone fill to the east was constructed with re-used Troy VI material. This is substantiated, for example, by the somewhat protruding penultimate stone in the platform's eastern face (marked with an arrow in fig. 4). Stones of this shape served the purpose of interlocking stone courses that formed the outer faces of the Troy VI walls. This is illustrated by the fourth stone from the right in the sixth

Fig. 7. Dörpfeld's photograph of the northern face of the northeast bastion (DAI Athens, Troja 158)

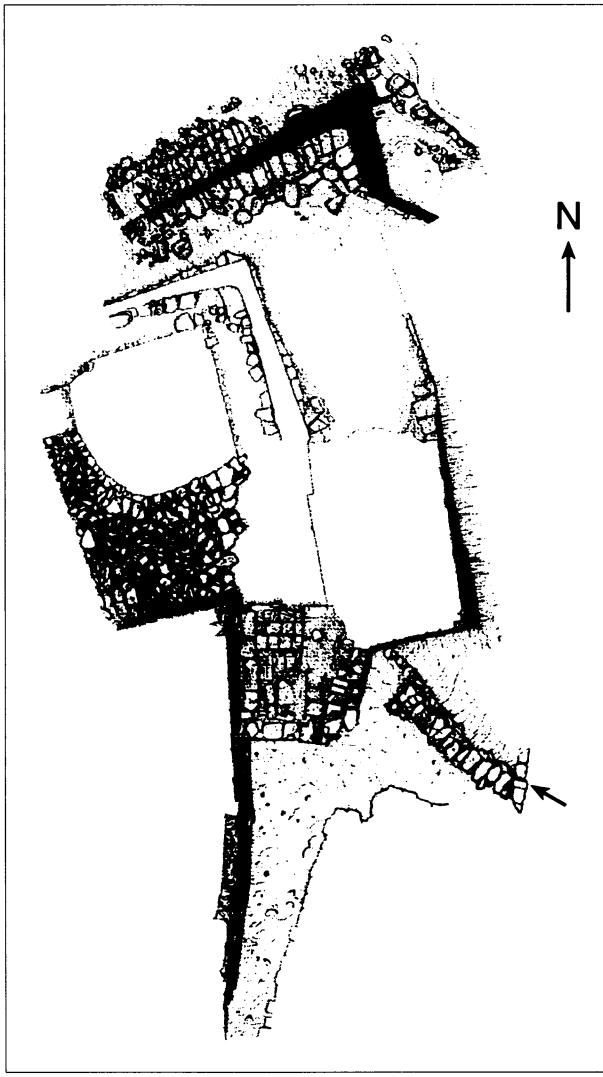

Fig. 8. Plan of excavation area around the northeast bastion and drawing of the proposed Troy VI settlement wall with specially worked stone at its southeastern end (Klinkott, Becks 2001: 409, fig. 462)

preserved course from the top of the northern face of the northeast bastion (fig. 7). In the platform's eastern face, however, such a stone is used in a context where it does not fulfill that function. Troy VI blocks also appear to have been re-used for the construction of the 'city wall' itself. The southeasternmost stone from the upper preserved course of the 'city wall' is worked in a manner which makes no sense within the context of this wall (fig. 8) (see Korfmann 1996: 41, 42, figs 34, 35; 1997b: 48, 50, figs 45, 48). According to Blegen (Blegen et al. 1958: 7), the re-use of stones of Troy VI is typical of Troy VIIa and b. Thus, the whole complex was probably built in VIIa at the earliest, at a time when the alleged defensive ditch was already out of use (see below, 'The defensive ditches').

Furthermore, the 'city wall' never had a mud-brick superstructure as would have been appropriate for a defensive wall. In photographs showing an earlier stage of the excavation (Korfmann 1996: 41, 42, figs 34, 35; Brandau 1997: fig. 34), it is evident (see fig. 4) that behind the northern face of the wall is a mud capping whose surface — in its western part at any rate — has the same height as that of the stone face of the wall. The mud surface and the surface of the stone face obviously formed the original surface level of the whole complex. This also results from the fact that flat stones which were set partly or completely into the mud capping (some stones may have fallen onto it from above later on) form an irregular stone ensemble in a similar way as the group of stones in the mud capping of the stone fill platform. This analogous construction confirms the impression that the mud surface of the 'city wall' formed the original level of that construction.

Easton offers neither a satisfactory description of these circumstances, nor does he present a convincing reason for the fact that a mud-brick layer was laid across the top of the whole complex (i.e. the platform and the 'city wall') in Troy VIIa. If there actually existed a mud-brick defensive superstructure of the 'city wall' before the mud-brick layer was added, one wonders what happened to this superstructure. Where are its remains? Was it completely removed and if so why? And why did the people who covered the whole complex with the mud-brick layer not use the lower part of the presumed superstructure as a back wall for this new layer? Incomprehensible is Easton's statement (92) that the mud-brick layer on top of the whole complex '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'. What could have been the purpose of enlarging the 'city wall' by several metres towards the north and then only up to a height of 1m?

The northern stone ensemble in the mud capping has been interpreted as a cult place by Korfmann (1998a: 473), because of the stele standing upright in this area (fig. 4). Its southern counterpart (on top of the 'city wall') might be interpreted in the same way, although no stele seems to have been found there. However, it could have been removed when cult activities came to an end. It is also possible that the broad, flat stone in the shape of an overturned top of a 'T' which slid into the mud capping at the foot of the Hellenistic wall that runs south of the 'city wall' (Korfmann 1996: 41, fig. 34) had functioned as a stele. This interpretation as a sacred place close to an acropolis gate[4] would explain why a 1m thick layer of mud-brick was laid across the top of the whole complex. Cult activities ceased presumably with the arrival of the new people of VIIb from the Balkan region, who built small rooms on top of the platform. The two cult sites, then, were not destroyed but integrated into the new construction.

The supposed gateway in the 'city wall', as reconstructed by Korfmann (fig. 5), would have been about 6m wide — much too wide for a city gate. Easton (93) tries to amend this flaw. According to him, a cutting through the supposed road on the east side of the 'city wall' indicates a foundation trench dug for the Hellenistic retaining wall. The construction of this retaining wall could have removed the east side of the gate. But if this really existed, what happened to the blocks? Where are the stones of the 'für Troia VI typischen Fundamente' (Korfmann 1996: 42; 1997b: 48, 50, figs 45, 48) of the 'city wall'? Neither Korfmann himself nor the leader of the Post Bronze Age Group, Brian Rose, mention any re-used Troy VI or VII blocks in the nearby Hellenistic constructions, and nothing similar can be seen on the photos (Korfmann 1997b: 48–53, figs 50, 52; Rose 1997: 95–101, figs 22, 23). In contrast, during the course of his excavation of the Hellenistic city wall west of the acropolis and of the trench at the nearby Troy VI citadel wall in square zA5, Blegen noticed that the stones of the latter had been re-used in the new fortification (Hertel 2003: 64, 81, note 107).

Between the corner of the northeast bastion and the 'city wall' the excavators came across an almost 2m wide gap (see fig. 4) filled with earth and stones. Easton (93) explains this as a hole dug by Dörpfeld into the 'city wall', and he argues that Dörpfeld 'certainly... did ... dig to deeper than the Troy VIII feature'. When we look at Dörpfeld's notebook, however, we find the following statement, illustrated by our fig. 9.

> Auffallend gut ist die Ecke des Turmes [i.e. of the northeast bastion] erhalten, die deshalb auch besonders photographiert ist. Sie verdankt diesen Erhaltungszustand der Thatsache, daß sie schon früh durch eine runde Mauer fg [i.e. the face of the Greek round bastion] verdeckt und geschützt worden ist. Es scheint sogar noch früher eine Mauer ef [i.e. the small wall in the north] erbaut worden zu sein, die gerade die Ecke conservierte (Dörpfeld 1893: 43, fig. on 43a).

That is all Dörpfeld tells us about his excavation in the area of the corner of the northeast bastion and of the Greek round bastion built later on top of the mud-brick capping which covered the platform and the 'city wall' (Korfmann 1996: 40–1, figs 33, 34, pls 1, 4). Nothing else can be seen either in our fig. 9 or on Dörpfeld's

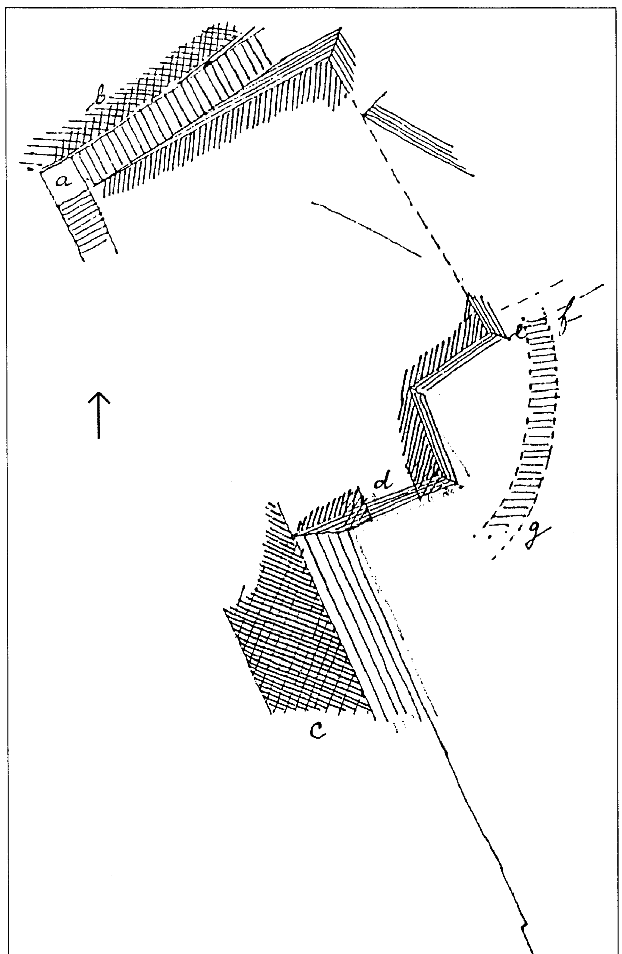

Fig. 9. Sketch plan of Dörpfeld's excavation near the corner of the northeast bastion and the Greek round bastion (Dörpfeld 1893: 43a)

[4] There was a gate in the neighbourhood, the south door (VI R) of the northeast bastion. Furthermore, the platform could have been accompanied in the east by an ascent or road leading from the Simoeis valley up to the area of the lower settlement (see Dörpfeld 1902: pls II, III; Korfmann 1997b: 49–52).

photos (DAI Athens, Troja 147.422.400 = Hertel 2003: figs 17, 18). This strongly suggests that Dörpfeld did not dig deeper than the lowest layer of stones of the Greek round bastion. This explains why the stones at the corner of the northeast bastion are heavily weathered down to the stone fill of the platform (Korfmann 1997b: 48, 50, figs 45, 48; Klinkott, Becks 2001: 414, fig. 469). A similar weathering can be observed along the western side of the south tower of late Troy VI (in square G9; Dörpfeld 1902: Beilage 18, opposite 128) where it was not covered until the Hellenistic period. Thus, the strong weathering of the stones at the corner of the northeast bastion is due to its having been exposed to the elements for a very long time. The gap between the bastion and the 'city wall' was presumably not filled until the round bastion was constructed around 400 BC (Hertel 2003: 55–7). In his report about the excavation campaign of the year 1894 Dörpfeld (1894: 34) writes, 'Auch an der Burgmauer zeigten sich keinerlei Merkmale, die für das Vorhandensein einer Mauer der Unterstadt in jener alten Zeit [i.e. Troy VI] angeführt werden könnten'.

In conclusion, the 7m piece of wall never joined the northeast bastion and therefore cannot have been a settlement wall. Korfmann's fortification wall of the lower city of Troy VI is a fiction defended in vain.[5]

The defensive ditches

Several sections of Troy VI ditches hewn into the rock have been discovered by magnetometer survey and partly investigated by excavation (fig. 2). The so-called inner ditch has been traced over a length of about 400m running in an east-west direction along the south slope at a distance of about 400m from the citadel. A short piece of a similar ditch has been identified ca. 100m further south and another short section has been documented in the northwest (Becker et al. 1993: 122; Korfmann 1994: 4–5; Jablonka et al. 1994: 51–66; Becker, Jansen 1994: 106–10; Korfmann 1995a: 28–9; Jablonka 1995: 39–49; Korfmann 1996: 2–3, 44–9; Jablonka 1996: 65–96; Korfmann 1997b: 62–3; Jansen, Blindow 2002).

Easton (92–3) maintains that if the above-mentioned piece of wall 'was indeed a part of the city wall, it will probably have been built while the inner ditch was in use'. Obviously, Easton concedes that the 'Late Bronze Age defensive system' which Korfmann reconstructs for Troy VI, is by no means a proven fact. Indeed, he admits (87, 91) that the inner ditch was filled up already at the end of Troy VI, i.e. around 1300 BC according to Korfmann's chronology. This agrees with the conclusions drawn in Kolb 2003 (17).[6] Since the alleged city wall was obviously not built before about 1300 BC, a contemporaneous existence of this wall and the inner ditch is not probable, as Easton maintains, but highly improbable.

Korfmann's defensive system also does not withstand basic logic. The ditch was not found in the rather large excavated areas immediately west and east of the citadel (Korfmann 1995: 29) and, therefore, cannot have run parallel to and in proximity of the alleged settlement wall as one would expect. For that reason, Korfmann postulates its course at a distance of 70–120m from the wall (Korfmann 1996: 46–9; Blindow et al. 2000: 127–8). This would be the most singular defensive system in all of antiquity.[7] To be sure, the ditch could not have been defended by warriors posted on the wall. The excavator has interpreted the ditch as a defensive work to prevent war chariots from approaching the settlement area. It could not fulfill this purpose, though, because it could easily have been bridged or filled up with its average width of only 2–3m and a depth of up to 1.5m (Jansen, Blindow 2002: 330). An effective Late Bronze Age defensive ditch against war chariots can be seen at Syrian Emar for example, where an earthwork 500m long, 30m wide and 15m deep was discovered (Chavalas 1996: 14).

Massive erosion of the rock around the ditch is Easton's main argument in defending Korfmann's defensive system. He strangely argues that the ditch was 'originally 4m wide' or '3–4m wide' (87, 90). Erosion, however, would hardly have narrowed the ditch — whose sides rise up almost vertically — but rather widened it. Moreover, he suggests that the inner ditch running along the south slope originally was 'perhaps 2m or more deep on the south side, and towering 3 or 4m high on the north side, or perhaps more' (90). This is mere speculation, and a highly improbable one (it contradicts the excavation report: Jablonka et al. 1994: 53), because the ditch section found in the northwest has the same dimensions and runs on a plain level, where such an amount of erosion could hardly have occurred.

[5] For further arguments see Hertel 2002a.

[6] In Kolb 2003 (35, note 42) the excavator's different datings of different sections of the ditch are listed. Therefore, it is hard to understand why Easton criticises this as relying 'on the excavator's first impressions, ignoring the more considered judgements made later' (89).

[7] A recently discovered and not yet published ditch at Hellenistic Halicarnassos seems to run about 20m from the city wall. Like the Troy ditches, it is hewn into the rock, but is broader.

Easton and the excavators also need this huge amount of erosion to substantiate a proposed palisade which was to defend a 10m wide gap in the inner ditch on the south slope (figs 2, 10). A causeway of this width in a defensive work would again be singular in ancient times. Two rock cuttings, one of them about 10m, the other 2.5m long, both about 40cm wide and less than 30cm deep, run north of the ditch and parallel to it at a distance of about 3.5m. The two cuttings are separated from each other by a 5m wide gap (Jablonka 1996: 71–2). Their interpretation as the foundation for a palisade construction with a gate (see also the reconstruction in fig. 1) presupposes a huge degree of erosion which would have removed all traces of post-holes for the timbers of the palisade, as they are preserved in the case of a palisade structure about 200m further north (see Kolb 2003a: 127–8). Moreover, a 5m wide gate would again be unusual, even compared with the gates of the Troy VI citadel which are only 1.3–3.6m wide. Furthermore, there are no traces of a gate construction. Easton (86, fig. 9) refers to 'two post-holes on the western side and one in the middle' of the 'gateway'. However, though there are several rock cavities in the area, none of them is situated exactly in the middle of the 'gateway'. Moreover, two post-holes on one side and none on the other are hardly useful for a gate construction, but perhaps more effective for tying up animals at a watering place (see below, end of section).

Finally, the ditch has up to now not been found where it would have been most needed as a defensive ditch against war chariots: in the east and especially in the northeast, where the terrain was flat, offering the best opportunities for war chariots to approach the settlement. Easton writes (91), 'The latest work may even show a turn northwards at its [i.e. the ditch's] most easterly end'. On the contrary, a test trench at this point has not confirmed this assumption (Korfmann 2002: plan opposite 4). At its westerly end too, the ditch appears not to turn towards the north, i.e. towards the citadel, but to the south towards the outer Troy VI ditch (see fig. 2). During the Tübingen Troy conference a slide was shown which suggested that the ditch had been discovered as an almost uninterrupted line along the west side of the hill plateau. The publication of *Studia Troica* 11 (see also Jansen, Blindow 2002: 339, fig. 16) a few months later demonstrated that, in reality, only a short section had been found in the northwest (see fig. 2). There it runs through the plain, at a distance of at least 120m from the alleged circuit of Korfmann's settlement wall. This ditch then turns away from the settlement hill, breaks through the line of the Hellenistic-Roman settlement wall and runs in the direction of a Bronze Age river bed which has been discovered fairly close and parallel to the western

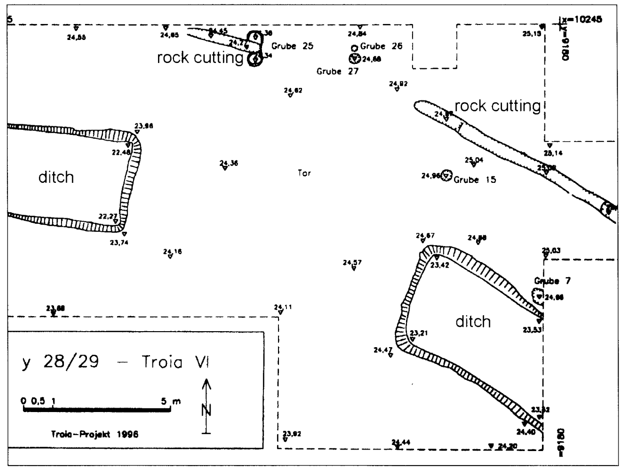

Fig. 10. Plan of Troy VI ditch with the 10m wide gap and the rock cuttings north of it (based on Kolb 2003a: 144, fig 4 = Jablonka 1996: 69, fig. 2)

flank of the hill (Kayan 2002: fig. 5; Jansen, Blindow 2002: 337). This makes no sense for a defensive ditch against war chariots which, in any case, would have been superfluous in this place because of the river bed and the steep west slope of the hill.

Easton has not informed the readers of *Anatolian Studies* about these important facts. They support a view which, as he maintains, 'founders decisively on two facts' (91). This refers to the suggestion in Kolb 2003 (19) that the Troy VI ditches served as a water drainage system. Contrary to Easton's statement (91) that 'Kolb now denies that he ever advanced it', this view has not at all been retracted.[8] On which argument does it founder? According to Easton (91) it founders because the ditch 'was interrupted at at least two points by causeways' and because it 'undulates over its course by as much as 14m'. The first argument can be dismissed at once, since the Hellenistic and Roman ditch running about 100m further south equally shows two interruptions doubtlessly serving as causeways (see fig. 11). This ditch system is regarded as a drainage system ('römische Wassergräben') by the excavators (see Blindow et al. 2000: fig. 1 opposite 124). Another common feature of both ditches is the low gradient. Together with the interruptions it kept some water in the ditches for agricultural and perhaps industrial purposes (see below, end of section).

[8] Easton (91) praises Korfmann's palaeobiologist, Uerpmann, for his criticism of Kolb's view, but Uerpmann's paper was based on a confusion of the German words *Entwässerungskanal*, i.e. water drainage channel, and *Abwasserkanal*, i.e. waste water drainage channel, and therefore completely missed the point. In Kolb 2003 (19) only the use of the Troy VI ditch as *Entwässerungskanal* was put forth.

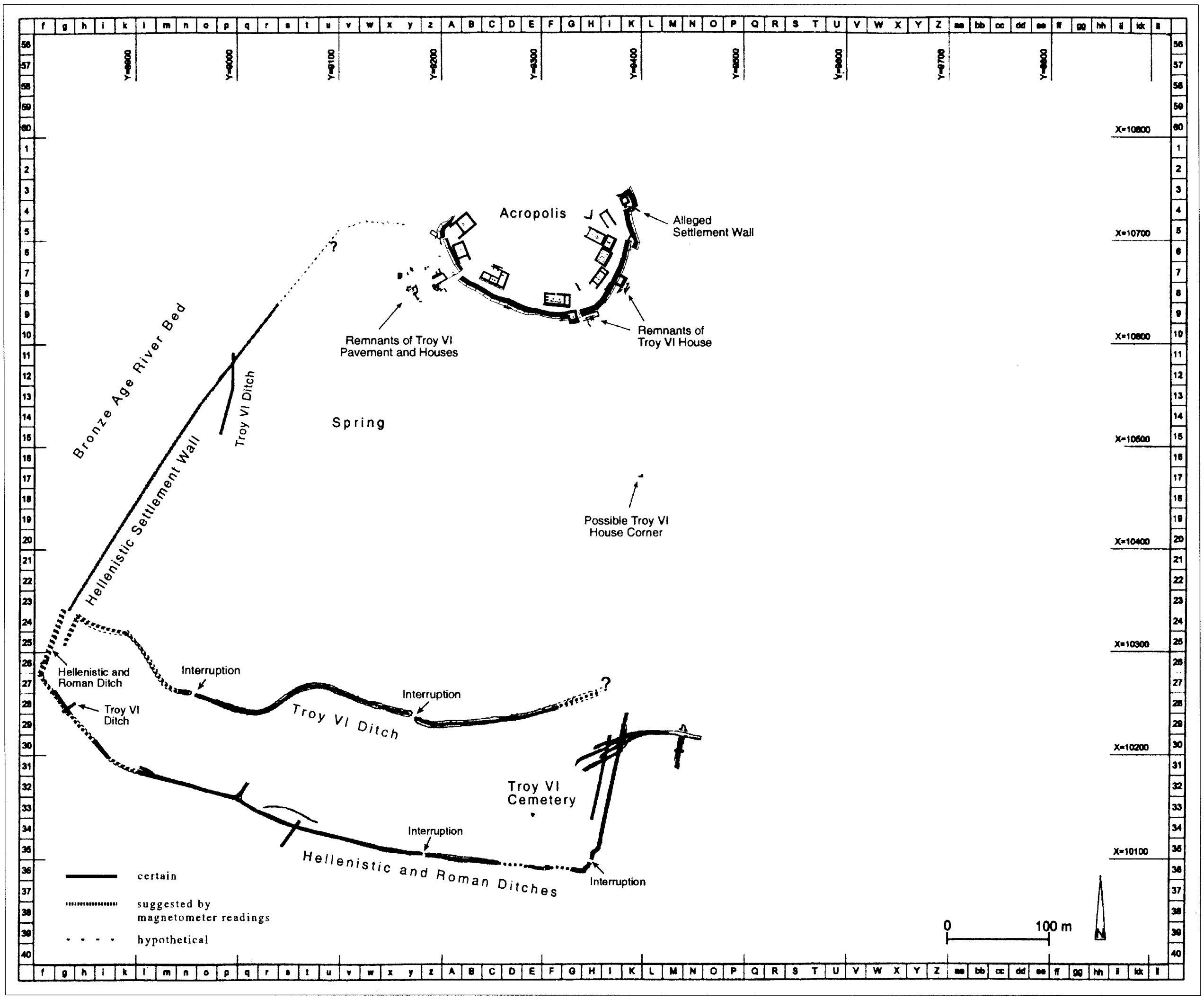

Fig.11. Troy VI: schematic plan of actual remains discovered up to 2001, including the Hellenistic settlement wall and the Hellenistic/Roman series of ditches, as based on drawings of the Troy Project (drawn by F. Kolb, C. Drosihn, A. Thomsen)

Easton's second argument overlooks that the ditches initially served yet another purpose. They were quarries for building material (Jablonka et al. 1994: 66; Jablonka 1995: 44, 54; 1996: 87; Korfmann 2001d: 36), and the quarrying was most easily done along the upper edge of a natural terrace. The undulating course of the inner ditch is in fact due to its following the course of such a terrace line.

The Hellenistic and Roman ditch appears to have been at least partly cut into another Late Bronze Age ditch, the so-called outer ditch (see figs 2, 11). It is dated by the excavators somewhat later than the inner ditch, towards the end of Troy VI/beginning of Troy VIIa (Jablonka 1996: 86) and was interpreted tentatively as another defensive ditch signaling an extension of the settlement area. It was not integrated into the model of the Troy exposition. Easton (91) now revives this interpretation, again without citing sufficient evidence. A test trench at the western end of the ditch has not confirmed the assumption that it turned northwards (Korfmann 2001d: 29, 33–6; Jansen, Blindow 2002: 336). Contrary to Easton's opinion, the presence of Troy VI and VII rubbish material in both ditches does not prove the existence of a settlement in the immediate vicinity of the ditches. No remains of Bronze Age buildings have been found near or between the ditches, apart from Blegen's 'crematorium'. In fact, vegetation remains in the inner ditch and artificial terraces to the south of it point to an agrarian use of the area north of the inner ditch in a direction towards the citadel and of the area between the two ditches (Jablonka et al. 1994: 60, 70; Jablonka 1996: 90).

The inner ditch doubtlessly was intended to protect the Troy VI cemetery (see fig. 2) from becoming swampy, and the outer Troy VI ditch must have served the same purpose as the later Hellenistic/Roman ditch (see fig. 11), that is, to protect the area at the foot of the hill from becoming marshy. The two Troy VI ditches may have been connected with each other (see fig. 2; Jansen, Blindow 2002: 336), thus forming a coherent drainage system. In addition, both ditches were partly used as water reservoirs for agrarian and possibly industrial purposes, while the rock cuttings mentioned above immediately north of the inner ditch may have served as a watering-place for animals (see Jablonka 1995: 54 for evidence for later activities of this kind in this area). Outside the walls of Kuşaklı-Sarissa artificial ponds served as water reservoirs for the inhabitants, their cattle and for agrarian purposes, as the excavator assumes (Müller-Karpe 2002: 179). The economy of Troy VI rested largely upon cattle and horse raising (see Uerpmann, Uerpmann 2001: 315–18; Kolb 2002a). Therefore, water reservoirs were important. The causeways in both ditches had to be rather broad in order to let wagons as well as herds of horses, cattle, sheep and goats pass into the area close to the settlement.

In sum, the evidence clearly points to the Troy VI ditches as having functioned as a water drainage and reservoir system, similar to other systems with this purpose in other Bronze Age contexts. In other words, nothing is left of Korfmann's defensive system. This also means that the ditches do not give any clue to the contour and size of the settlement area and cannot be used for a calculation of population numbers.

House remains in the so-called lower city

What about the alleged densely built-up settlement area? Before the start of the excavations, cores and soundings taken systematically in the area of the so-called lower city generated disappointing results (Korfmann 1991: 26–8). Apart from the area immediately outside the citadel, only a site about 200m distant from the acropolis promised to produce architectural remains from the Bronze Age, while in the southern half of the plateau no Bronze Age strata were found. This was explained as a result of erosion and of the removal of Bronze Age foundation walls by the builders of Hellenistic and Roman Ilion (Jablonka 1996: 90–2; Korfmann 1997b: 55).

Easton (Easton at al. 2002: 89–90) believes he has discovered a 'striking indication' of massive erosion.

> At one point just north of the inner ditch there are remains of a series of pits cut into the 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 have lain nearly 2m higher.

Unfortunately, Easton gives no information about where this evidence has been published. We did not find it in the excavation reports, and it is difficult to assess unpublished evidence. Easton's presentation of it permits different interpretations. For example, the position close to the ditch could imply the use of (bases of broken?) pithoi sunk into shallow cavities as watering vessels for animals or as industrial installations for fullers, whose activity in this area might be suggested by animal remains in the inner ditch (Jablonka et al. 1994: 60–1; Jablonka 1996: 71). Finally, the vague dating of the pithos remains to the second millennium does not allow for placing them reliably into the context of Troy VI let alone late Troy VI, which is the settlement we are dealing with.

Erosion certainly caused earth and rubbish material to be washed down the slope, but the relatively low incline does not permit an assumption that solid stone foundations were swept away (see Hertel 2002a: 9; Kolb 2003a: 125). Survey experience in the mountainous landscape of central Lycia has demonstrated that even on much steeper slopes stone foundations of buildings have usually been preserved even when these buildings have not been in use for more than two millennia (Thomsen 2002). Finally, a certain amount of pillaging of Bronze Age buildings may have happened in later periods. It is a fact, however, that Troy VI foundation walls were found almost exclusively underneath Hellenistic and Roman buildings and almost none in those rather large areas left unbuilt in these later times (Kolb 2003a: 125–6).

Concerning the northern half of the so-called lower city, Easton maintains (85, 94), 'The new excavations have hugely increased the evidence for Late Bronze Age occupation' in the 'area immediately outside the Late Bronze Age citadel' and 'there can be little doubt that the area immediately around the citadel was heavily built up on all sides'. Yet, there can be considerable doubt whether this was the case. For confirmation of his statement Easton (85) recommends his readers consult 'E. Riorden's magnificent plan published as a supplement to *Studia Troica* 4'. They should in fact do this and, above all, consult the more recent version published as a supplement to *Studia Troica* 8. They might fail to recognise the faint traces of Troy VI remains which are limited to three small areas in the plan of 1994 and to four areas in that of 1998. In the latter plan, in particular, they are presented in such a way that makes them virtually indistinguishable from Troy VIIa remains. This is also a characteristic feature of the plan printed in Troia 2001 (Korfmann 2001c: 71, fig. 74). Easton (86) admits 'that the reds are indeed too similar', but as usual he tries to excuse Korfmann's questionable methods with no less questionable arguments: 'Far from conflating periods, the plan actually attempts a higher degree of differentiation than before'. This is hardly fathomable, since, as Easton continues (86), 'most of the known structures in the area do admittedly belong to VII or later'. More to the point is his remark (85): 'The evident purpose was to highlight the cultural continuity from VI into VIIa... This is hardly controversial'. We think it highly controversial that two settlements which chronologically succeed one another, are presented as one and the same. The most recently published plan shows the salutary effect of the Troy debate — and the relative paucity of Troy VI walls (Korfmann 2002: 12, fig. 10).

In fact, it is, or at least was, Korfmann's aim to conflate Troy VI and VIIa into one settlement, by adding the houses of Troy VIIa to compensate for the small number of Troy VI houses. Easton's (86) excuse for the paucity of Troy VI houses — that 'excavation has in most places gone no deeper' than Troy VII — is simply wrong. The same applies to his further statement (85), that 'wherever excavations have been made' around the citadel, 'they have unfailingly revealed a sequence of Late Bronze Age buildings'. Obviously, there were areas without Late Bronze Age buildings dating to Troy VI/VIIa (Korfmann 1998b: 41–2; 1999: 26; 2000: 27, 30–2; 2001d: 19, 23–4; see Blegen et al. 1953: 350).

Easton identifies at least nine late Troy VI buildings in the area of the Hellenistic and Roman sanctuaries, the so-called Weststadt. It is difficult to verify this on the basis of the few Troy VI walls entered in Korfmann's plans (see figs 2, 3) and impossible on the basis of the plan printed by Easton (86, fig. 7), where most of the buildings and walls are in reality non-existent, hypothetical ones. Furthermore, it should be made clear that some of the datings are hypothetical. Finally, in these plans Troy VI house remains of different periods of Troy VI (early, middle, late) have been entered side by side without distinction (this is obvious in square z7; see Korfmann 1998b: 37). In other words, the actual number of late Troy VI houses is even smaller than these plans and Easton's questionable statistics suggest.

Moreover, this small living quarter must surely be regarded as exceptional. It was situated at a point where an important road leading from the Scamander plain up to the citadel entered one of the two most important gates of Troy VI. This area was not only protected against the cold northerly winds by the acropolis wall, but also offered a panoramic view across the plain towards the sea. It was a topographically privileged quarter which cannot be considered typical even for the total area near the citadel, not to mention the rest of the so-called lower city.

An examination of the excavation area on the middle plateau (squares H17 and IKL16/17) confirms our view that Korfmann's densely built-up 'lower city' is a fiction.

Easton's (87, 83, 88, figs 6, 8) comments on this excavation area situated about 200m to the south of the citadel convey the impression of a densely inhabited quarter of late Troy VI, with houses built on solid stone foundations. However, it is obvious that the wall remains in this area cannot be dated precisely. Easton speaks of 'houses' and a 'fully built-up area', respectively, 'in late VI and VIIa' or 'in late VI–VIIa', thus conflating two successive strata and building periods. In the plan of the squares IKL16/17 (88, fig. 8), small remnants of stone walls can be recognised and a more massive house corner. Somewhat to the east of this corner was another house corner found on a lower level. In his excavation reports Korfmann talks of walls or houses of the 'Unterstadt' of 'Troy VI/VII' (Korfmann 1997b: 53–62; 1998b: 49–56; 1999: 20–2). Of the two house corners mentioned above the more recent one cannot be dated securely to late Troy VI. According to the excavator, it belongs 'an das Ende von Troia VI oder an den Beginn von Troia VIIa' (Korfmann 1998b: 52). This dating depends on a LH III B stirrup jar found on the house floor. Since this house had only one floor (Korfmann 1998b: 51) and according to P.A. Mountjoy (1999: 258) the destruction of Troy VI had already occurred in LH III A 2 (i.e. at the end of the 14th century), the house should be dated to VIIa. The somewhat earlier house corner might belong to late VI (see fig. 2). This dating is based on its position on a somewhat lower level and on pottery of Troy VI late/VIIa (Korfmann 1998b: 52).

The other walls in IKL16/17 obviously have not been dated by the excavators more precisely than within the long period of Troy VI (ca. 1700–1300 BC). They, too, could just as well belong to VIIa. The date assigned to the remains in H17 is similarly uncertain. These remains do not comprise stone foundations. Moreover, it has to be stressed that Blegen's excavation already established that large areas outside the citadel were only sparsely inhabited in late Troy VI. When Blegen cut two long north-south trenches into the area south of the acropolis, beween Korfmann's Weststadt and squares H17/IKL16/17, he found only 'little trace of preclassical occupation' (Blegen et al. 1950: 10; Blegen et al. 1953: fig. 445, C).

The CAD model of the actual remains of Troy VI presented by the Troy team at the Troy exposition in Bonn in 2002 showed one single house in H17/IKL16/17. It is therefore enigmatic why Easton (86, fig. 7), upon showing an excavation plan from 1997, reproaches us for citing Korfmann (Korfmann 1998b: 52): 'that this area of the lower city was thinly built, with fairly large open areas between the houses'. What and where is 'the evidence as we now have it' which would suggest 'a fully built-up area in late VI–VIIa'? Until 2002, at least, this 'evidence' was obviously not known to the excavators themselves.

Conclusions

It has been shown that Korfmann's interpretation of late Troy VI as a large Anatolian palatial city with up to 10,000 inhabitants, as a hub for Bronze Age trade, a commercial metropolis and a centre of a Bronze Age *hanse* is unfounded. While Easton, Hawkins and the Sherratts appear to share Korfmann's opinion, they have in fact offered a much more restrained view. They maintain (106) that Troy VI was identical with Wilusa and therefore 'a significant regional power in western Anatolia', that it occupied a 'pivotal' position 'in the trading networks of its day', and that a citadel and a lower city existed on the hill of Hisarlık.

We have always maintained that late Troy VI was an aristocratic residence, with probably a *primus inter pares* as a leading political figure, and that it was a regional centre involved in some trade. We also have demonstrated that its identification with Wilusa is only hypothetical and that not the slightest evidence exists to suggest a prominent role in trade. In fact, Korfmann has not even proved that late Troy VI was a city at all.

Easton (84) has reproached us for suggesting that Korfmann deliberately attempted 'to inflate the importance of his site with the object ... of ensuring a continual flow of funding for his excavation'. Indeed, this is the wide-spread opinion among German prehistorians, archaeologists and historians of Classical antiquity. Another motive, though, for Korfmann's assertions has been suggested. B. Kull (2002: 1182) thinks that Korfmann's labeling of the Trojan grey ware pottery as 'Anatolian' might be politically motivated ('wohl politisch motiviert'). A. Ünal (1999: 139) accuses Korfmann of not only exaggerating his findings but also that 'the excavators of Troy with all those false claims ... intended to do a favour for their Turkish colleagues, liberating pre-Greek and prehistoric Troy from its Greek context, and placing it into the Asiatic ... context'. Both motivations could explain Korfmann's contradictory characterisation of Troy VI. On the one hand, he raises Troy into the position of a hub for international Bronze Age trade; on the other hand, he characterises it as a purely Anatolian site with at best negligible connections with the Aegean and Balkan regions (Korfmann 2001: 357–61; 2001a: 397–99). All this suggests that Korfmann, as far as historical interpretation of the archaeological evidence is concerned, operates on an insufficient methodical level. This coincides with the picture conveyed during the Tübingen Troy conference, when Korfmann's collaborator Uerpmann, without the slightest trace of self-irony, expressed the opinion that the historians were envious, because they dealt only with written sources whereas the excavators were in possession of the truth. Korfmann himself declared in his final statement that it did not matter to him if Troy VI was a metropolis, a city, a village or whatever, the main thing being the continuation of the Troy excavation. He concluded: 'The caravan moves on' (see Kolb 2002). This prompted the newspaper *Die Welt* (18 February 2002, 15 March 2003) to call Korfmann a 'borderline historian', his methodological approach a 'scandal' and to interpret it as indicative of an 'excavation in the style of Indiana Jones'. The *Stuttgarter Zeitung* (18 February 2002) recommended to replace at least the camel-driver of the caravan with a younger one. Is one really obliged to contradict, as Easton (94) demands?

Addendum

Recent excavations have now determined that the remains formerly identified as the fortification wall of the 'lower city' of Troy VI are, in fact, cover slabs of a water channel, dated to Troy VIIa.

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