

Late Bronze Age Troy: A Response to P. Jablonka and C.B. Rose*

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The opening section of the Jablonka–Rose response points out that the difficulties of excavating at Troy require that the excavators exercise great caution in examining the material culture of the site, implying that this has been done. But has it? While the question of the validity of the excavation methods, recording procedures, and techniques of dealing with the finds may be raised by other experts, it is obvious to me as a historian that the Troy excavation in presenting the results of its work mingles actual findings with mere assumptions and wishful thinking. Furthermore, Jablonka and Rose stress that they are going to provide access to the broadest possible range of information, implying that I have withheld information. In what follows it will become obvious that it is the other way round.

C.B. Rose's response (627–28) may be summed up in eight words: absence of evidence is not evidence of absence. Since he diplomatically maintains silence on the specific points of the controversy, further reply is not needed.

P. Jablonka's first specific counter-argument consists of an attack on views I expressed in nine (!) lines of *Die Stadt im Altertum* published 20 years ago. He prefers to represent an incomplete and distorted rendering of these few lines rather than answer the views contained in my contribution to this volume. Instead, he tries to depreciate these as "not . . . new" and unjustly maintains that they had already been presented "in a series of almost identical papers" (615), including an article, written by D. Hertel and me and published only very recently in *Anatolian Studies*,¹ whose content he did not know, when he composed his "reponse." This explains why Jablonka uses an article published by D.F. Easton, J.D. Hawkins, and A.G. and E.S. Sherratt² as the basis of his own argumentation, although the views expressed by those

authors have been thoroughly rejected in the Hertel and Kolb paper.

My article in this volume to which Jablonka responds deals with Troy VI. Yet, throughout his response Jablonka adduces evidence from other Bronze Age periods (even from Troy II), which has nothing to do with Troy VI. Although the question of LBA Troy's trade represents the core of my article, the response to it is extremely short (624–26), essentially repeating arguments refuted in my article, and contains hardly anything new, apart from some find statistics—most still unpublished—from the recent excavations. Unfortunately, they lack clear chronological distinctions and precision. For example, one would like to know how many of the "more than 2000 Mycenaean and Minoan sherds" (624 n. 38) belong to the different phases of Troy VI and VII respectively and to how many pots these sherds can be attributed? Furthermore, their definition as *Mycenaean* is problematic, since according to the present state of research most of them are local imitations. One would welcome a publication of imported (!) Mycenaean sherds from Troy. Indeed, the quantities of true imports at Troy have not increased sufficiently to change the picture presented in my article. In addition, there are no new facts with regard to contacts between the Aegean and the Black Sea in which Troy might have played "an important role in trade" (627). Moreover, recent contributions to this subject by A.G. and E.S. Sherratt and by O. Höckmann referred to in the response, offer nothing else but highly speculative arguments and no new data.³ Finally, decisive counter-arguments, such as the total lack of Mycenaean sherds in the Black Sea region and the absence of Black Sea imports at Troy and vice versa, are simply passed over. Instead, imported Mycenaean seals are falsely interpreted as evidence for economic administration at Troy (625).

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¹Hertel and Kolb 2003. See now also Hertel 2003 and 2003a.

For the rest, it would be frustrating to refute all of Jablonka's unjust polemical remarks.

²Easton et al. 2002.

³Easton et al. 2002, 101–6. Höckmann 2003.

Curiously enough, my definition of a “commercial city” and “trading center” is rejected as too “narrow,” because of being “not helpful in trying to understand the role and importance of Late Bronze Age Troy,” and “then trade and places of trade would hardly exist during the Bronze Age” (624). It would appear, then, desirable and even obligatory to have as many Bronze Age places of trade as possible and among them, of course, Troy. Is this the lesson to be drawn from Korfmann’s “lack (of) terminological precision” admitted by Jablonka (627)? It seems that the Troy Project insists upon an arbitrary usage of terminology, and Jablonka cunningly attributes to me a definition of trade that is not mine (624). But it is important that B. Hänsel, a leading specialist for Bronze Age trade, in the case of Troy prefers to speak of *exchange* instead of trade.⁴

B. Hänsel also agrees with me that the archaeological evidence does not justify defining the Bronze Age settlements on the hill of Hisarlik as cities. His definition of the term *city* is quite similar to mine,⁵ which is equally criticized as “narrow” by Jablonka (627). In reality, our definition is broad, allowing it to be applied to settlements from prehistoric to modern times. It excludes, however, a site like Troy, which until now has generated—apart from the citadel—the remains of only a few houses at any given period, with no traces of a city wall, of differentiated public architecture, of urban economic structures, or of market activities. Although Jablonka focuses on the remains of the settlement and assumes that an important city is evidence for important trade, he does not present any new data in regard to the essentials necessary for a city. The presumed disappearance of almost all Bronze Age remains from the area of the so-called Lower City does not justify the assumption that those prerequisites once existed. Absence of evidence may not be evidence of absence, but it is certainly not evidence of evidence.

Instead, we are confronted with surprising information concerning the alleged extension of the “Lower City.” Now it is supposed to cover “between 25 and 35 hectares” (627). This figure is the product of stunning methodological “progress.” Since the excavations have practically come to an end, the Troy Project has started “a systematic survey,” and “the initial results . . . show a continuous scatter of Bronze Age pottery in the area south of the citadel” (620). This is the same area that is supposed to have been largely deprived of any Bronze Age strata by massive erosion (621). Obviously, pottery sherds from this area would have been carried down to the foot of the hill, while those

now discovered on the surface must originate from the citadel and its immediate surroundings. We are not told how many (Late) Bronze Age sherds have been collected on an area of about 35 hectares, nor how many of them might be dated to the *Late* Bronze Age (fig. 4 of the Jablonka and Rose article refers only to “Bronze Age Pottery” in general). However, we are told that sherds have been found on ground situated roughly as high as the citadel and, thus, could not have been carried downhill from the citadel and its immediate surroundings (620 n. 24). This argument fails to convince, considering the long history of the site with its frequent destructions, leveling operations, and discarding of rubble. Bronze Age material was certainly deposited all around the citadel and the settlement. Moreover, soundings conducted in squares M18 and O11, only about 25 m and 80 m further east than the citadel, have resulted in the discovery of a 20 cm thick Bronze Age layer without any architectural remains. The excavator has previously interpreted this fact as demonstrating that this area was situated beyond the limits of the settlement area.⁶ Furthermore, up to the year 2003, three more excavations have been undertaken even much further east, in squares X2, Y59, and FF5, without producing any remains of Bronze Age buildings in these areas.⁷ It is, of course, possible or even probable that there existed some kind of dispersed settlement, consisting for example in farmhouses, in the surroundings of the citadel. But there is no evidence of a densely built-up city area.

The response hardly mentions an even more important development. The alleged section of a city wall from Troy VI, found during the 1995–1996 seasons and celebrated as a sensational discovery, has disappeared and with it a city gate invented by the excavator. This wall is now conceded to be just covering slabs for a water channel dated to Troy VIIa at the earliest (617, with n. 14). In fact, a close look at photographs published right after its discovery,⁸ reveals an opening at the southeastern end of this wall. This means that its character as a water channel was clearly recognizable from the beginning.

It is astonishing how this fundamental change is commented upon by Jablonka (617 n. 14): “This does not influence the interpretation of the structure as a whole, as given in Korfmann 1997, 49–53.” In that article the excavator identified this section of wall as the “Lower City” wall of Troy VI.⁹ And what is the excavator’s interpretation after the “discovery” of its true function? Jablonka (617) is cautious enough not to comment on it. Instead of a city wall, we are

⁴Hänsel 2003.

⁵Hänsel 1996. Kolb 1984, 15; 2003, 121–22.

⁶Korfmann 1991, 17; 2001b, fig. 1.

⁷For excavations during the 2003 campaign, see the exca-

vation areas entered in the plan in Korfmann 2004, 23, fig. 15.

⁸Korfmann 1996, 41–2, figs. 34, 35; 1997b, 48, fig. 45.

⁹Korfmann 1996, 41–2; 1998, 473. Easton et al. 2002, 82–94.

now confronted by Korfmann with a mudbrick bastion of Troy VIIa set right in front of the impressive Northeast Bastion of Troy VI Late.¹⁰ This is, of course, a completely new, and nonsensical, interpretation of the archaeological evidence. Neither did the strong Northeast Bastion need reinforcement by such a mudbrick structure, nor were any other Troy VIIa fortifications constructed in this way. Rather, like those of Troy VI, they were built with high stone walls and a mudbrick superstructure.

Further fundamental changes of interpretation are not explicitly identified as such in Jablonka's paper. Previously, several sections of ditches found in the south and northwest of the hill were regarded as obstacles against war chariots, with a gate protected by two short palisade sections in the interior southern ditch. A city wall was supposed to run about 100 m behind, since no trace of it could be found close to the ditch (see Kolb, 606). Instead, a more conventional system has now been introduced by Jablonka (616–17). A palisade is believed to run the entire distance (!) "parallel to the ditch." In addition, "a rampart behind the palisade" and a "city wall . . . at some distance behind the palisade" are invented; the palisade and rampart are represented like a full-blown city wall in Jablonka's (fig. 2) so-called reconstruction. It is evident that the ditch has changed its function and become a normal defensive ditch. With regard to the outer ditch, Jablonka does not explain if he thinks that it was part of a similar defensive system. In any case, regardless of Jablonka's assertions to the contrary, there is no direct evidence that the ditches encircled a settlement (see below), that there existed a palisade,¹¹ or a rampart, and a city wall. Moreover, Jablonka fails to produce a parallel for such a defensive system in the Late Bronze Age. Where did such a system exist?

The dimensions of the ditch sections have been magnified in order to enhance their importance. They are described as being 4 m wide and up to 2 m deep, and Jablonka (616, with n. 8) tenaciously insists upon these measurements, although they are obviously wrong, as is demonstrated by the sober report done by those who performed magnetometer measurements: In the south we have "a ditch of 2–3 m width and about 1.5 m depth," the ditch section in the northwest "was cut into the limestone with the same proportions

¹⁰Korfmann 2004, 34.

¹¹Jablonka draws a specious comparison between a palisade and gate, which he dates to Troy II and the hypothetical palisade and gate of Troy VI. In the former case, the rock-cut trench and postholes are clear evidence for a palisade; whereas there is no sufficient evidence for such a structure in Troy VI.

¹²Jansen and Blindow 2003, 330, 337. Cp. Becker and Jansen 1994, 106. Korfmann himself (2001b, 28) describes the ditch in the northwest as only 1 m deep.

as in the south."¹² Anybody with a ruler may check the width of the ditch by measuring the only ditch section, which has been drawn in a plan as shown in Jablonka's fig. 2. It is about 3 m wide. Moreover, Carl Blegen had already discovered a section of the inner southern ditch under the Late Bronze Age building, which he called Crematorium and dated to Troy VI (see Kolb, 602, with n. 214). Jablonka obviously accepts Blegen's dating of this building. It is entered as No. 11 in fig. 3 of his paper. Blegen defines the dimensions of the ditch as being 2 m wide and 1 m deep. This also demonstrates the absurdity of Easton's suggestion¹³ that the ditch originally was 4 m wide and 2 m deep, and that its dimensions have been reduced by massive erosion of the rock during the following centuries. With regard to the date and function of the ditch, it would require an explanation that Blegen's Troy VI Crematorium (no. 11 in Jablonka's fig. 3) was built across the ditch. It is strange that the Troy Project seems to have never discussed the consequences resulting from this fact.

Furthermore, the disappearance of the alleged city wall has obviously intensified the temptation to reconstruct the ditch sections as continuous and encircling the "Lower City." Yet, it is simply not true that "all intermittent stretches" between the different ditch sections established by excavation are "clearly visible in the magnetogram" (618). The drawing of the ditches in Jablonka's figs. 3 and 4 is thoroughly misleading. In reality, the outer ditch has not been traced with certainty in square x=10100/y=9100. The interior ditch has been arbitrarily extended and bent towards the north at its eastern end, contrary to the Troy Project's own excavation results (see Kolb, 602, with fig. 2). Furthermore, the drawing obscures the fact that no connection has been found between the ditches in the south and the one in the northwest, as those who did the magnetometer measuring confirm.¹⁴ In Jablonka's fig. 1, an arrow has been entered in square x=10400/y=9000, pointing towards one of the irregularities visible in the magnetogram. This irregularity, known for several years, was not interpreted as a ditch section by those who did the measuring. It is wishful thinking to identify it as such. In reality, the result of the magnetometer measuring shows that at its western end the interior southern ditch appears to

¹³Easton et al. 2002, 87, 90.

¹⁴See my article, fig. 2.3. Korfmann 2001b, fig. 23; 2002, plan opposite p. 4. Jansen and Blindow 2003, 339, fig. 16. Moreover, Jablonka (618–19) unjustly denies the existence of interruptions in the Hellenistic and Roman ditch further south, and conveys a false impression of the nature of this ditch. It was not "formed of terracotta pipes or tiles," nor was it "narrow," but, on the contrary, broader than the Late Bronze Age ditch sections. See Jansen and Blindow 2003, 339, fig. 16. Jablonka 1996, 84–9, with fig. 14.

turn south and not north (see Kolb, 602, with fig. 2). The Troy Project's interpretations of the ditch sections stubbornly ignore this as well as the Bronze Age riverbed running along the western flank of the plateau, towards which the northwestern ditch appears to run (see Kolb, 603). The results of the much advertised interdisciplinary approach of the Troy Project are apparently not welcome as soon as they do not fit into the preconceived ideas of its director, who prefers to publish fanciful drawings of an uninterrupted ditch encircling the entire "Lower City."¹⁵ Although I made admittedly a mistake—insisted upon by Jablonka (616, but see n. 4)—in the use of the words *quarry* and *secondary*, this does not invalidate my contention that the ditch sections have no defensive purpose but are rather water reservoirs and water channels.

Wrong information on size and elusive arguments also characterize Jablonka's descriptions of the citadel and the "Lower City." The citadel, for example, is not 2–3 hectares (620), but only 1.8 hectares.¹⁶ Moreover, the argument that essentially more than 99% of the supposed architecture of Late Bronze Age buildings in the "Lower City" should have disappeared because of erosion and stone robbing does not become more plausible by its continuous repetition, since it is not plausible. Blegen's Crematorium mentioned above¹⁷ sufficiently demonstrates that solid Late Bronze Age buildings were not carried away by erosion. My own experience in field survey also shows that solid house foundations would not have been removed by erosion, and stone robbing to the extent envisaged by Jablonka and Rose would have been indeed unusual.

Furthermore, the "architecture" entered in fig. 3 of the Jablonka and Rose paper comprises remains covering roughly 800 years of settlement history. For the rest, there is nothing which has not been taken into consideration in my article, except for some misleading statements made by Jablonka: The Late Bronze Age dating of the not excavated architecture in no. 9 of his fig. 3 is just wishful thinking. Previously, the date and interpretation of this architecture was expressly left open.¹⁸ But if this was indeed a Late Bronze Age building, it would again refute the argument of massive erosion and stone robbing as causes for the disappearance of the so-called Lower City. Together with Blegen's Crematorium (see above), it would point to a dispersed settlement structure of Late Bronze Age Troy. Moreover, mere postholes in the rock are included in the list of architectural remains (e.g., no. 7 and most symbols for "buildings" in no. 6

of fig. 3), though they do not attest to the existence of Late Bronze Age dwelling houses, which were usually built of mudbrick with solid stone foundations. The postholes indicate fences, sheds, stables or storerooms. In reality, not even a dozen of the excavated houses in the so-called Lower City can be shown to have existed at any given period.

No more convincing is the interpretation of "the rectangular grid of pits to support pithoi" in Jablonka's fig. 3, no. 11. Jablonka¹⁹ had previously dated the grid of pits as probably Hellenistic and added that in case of a Bronze Age date the pits would have to be interpreted as part of a cemetery. Thus, if the new Troy VI/VIIa date for the pits given by Jablonka is correct, this would imply that the burial area of Late Bronze Age Troy started already north of the inner ditch, in a distance of about 400 m from the citadel, and this, in turn, would exclude an extension of the settlement down to the inner ditch.

Instead of finally following good excavation practice and presenting detailed, separate plans for the different LBA settlements with the actual ground plans of the buildings found, we are confronted again with a "plan" (Jablonka's fig. 3) that shows only symbols and numbers marking finds from all strata of Troy VI and VII. Unspecified "architecture" and mere deposits are listed side by side as if they would represent archaeological evidence of equal value. Obviously, the Troy Project obstinately refuses to convey a clear idea of what its excavation results really mean with regard to our knowledge of the Late Bronze Age settlements on the hill of Hisarlik.

CONCLUSION

Jablonka (627) admits "that some of Prof. Korfmann's more far-reaching interpretations on the role of Troy lack terminological precision." But he tries to play these interpretations down as merely "hypothetical, especially where he [i.e., Korfmann] addresses a non-specialist audience." Yet, apart from the fact that hypotheses should be serious, the excavator's interpretations were presented as proven statements and disseminated in scientific journals, as well as in other scholarly publications. Criticism of the present Troy excavation by archaeologists, prehistorians, historians, and philologists²⁰ has already had some salutary effect on the published opinions of Troy project members and their few sympathizers.²¹ This development is also evident in the Jablonka paper. Although its conclusion (627) that Late Bronze Age

¹⁵ Korfmann 2004, 30, fig. 25. Cp. Brandau et al. 2004, 144–45.

¹⁶ Korfmann 1996, 43.

¹⁷ It is, by the way, the only building with stone foundations missing in my plans figs. 2 and 3, because previously I was not able to locate it precisely.

¹⁸ Becker and Jansen 1994, 111.

¹⁹ Jablonka 1995, 55–6.

²⁰ See Hertel 2003. Hänsel 2003. Cobet and Gehrke 2002. Heinhold-Krahmer 2003, 2004, and so on.

²¹ See, for example, Easton et al. 2002.

Troy was a city and played an important part in trade still contains considerable exaggerations, it is worlds apart from Korfmann's presentation of Troy VI as a large Anatolian palatial city with up to 10,000 inhabitants—in keeping with the description of Troy in the Iliad—moreover as a commercial metropolis and hub of trade, and even as a center of a Bronze Age Hanseatic League (see Kolb, 578). Therefore, Jablonka's statement is surprising that "the substance of his [i.e., Korfmann's] claims holds firm when tested against the available data" (627).

My position formulated in my article (599) that "Troy was a political, military, and administrative center for at least a great part of the Troad," but not a city, has not at all been disproved by Jablonka. On the contrary, it is shared by leading specialists of settlement history in Bronze Age Anatolia, the Balkans, and the Aegean, such as Bernhard Hänsel and Harald Hauptmann.²²

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²²Hänsel 2003. Kolb 2003, 120 (for Hauptmann's opinion).