

ONCE MORE ON THE DESTRUCTION OF HASANLU IV: PROBLEMS OF DATING

BY

Inna N. MEDVEDSKAYA
Institute for Oriental Studies
USSR Academy of Sciences

In vol. XXVII of *Iran*, R.H. Dyson and O.W. Muscarella published a paper directed against the redating of the destruction of Hasanlu IV to 714 B.C., as proposed in my article “Who Destroyed Hasanlu IV?”¹. The authors rejected my attempt to revise a date proposed some thirty years ago, when excavation at the site was in its initial stage. But periodic revision is a perfectly normal and often unavoidable procedure in the interpretation of archaeological material.

I first felt a need to redate Hasanlu IV’s final destruction, when investigating the Eurasian and Ancient Near Eastern types of horse bridle, and local Hasanlu IV style ivory carvings with battle scenes². The chronology of these artifacts, as I tried to show, excludes the ninth century B.C. These categories of objects, unlike other artifact types, can be associated only with the eighth century. I will make bold to recapitulate here some of my positions, since it is clear from the arguments of Dyson and Muscarella that quite a number of my basic assumptions have been misunderstood.

To begin with, the presence in Hasanlu IV of *iron* monoring bits, in addition to the bronze ones³, is evidence in favour of a later date. Iron European-type bits with separate cheekpieces, emerging in the late HAB, are recorded as a leading feature of European sites of the HAC period, whose beginning is generally assigned to the mid-eight century B.C.⁴ This is the *ante quem non* date for their penetration into Iran. It should be noted that in Hasanlu IV, iron mono-ring bits form 43% of all the bits recovered: eighteen out of forty-two. Dyson and Muscarella regard the Halstatt bridle type as itself “derived from the east”⁵, although it is not quite clear with what precise region they associate its “eastern origin”. For most scholars dealing with the genesis of the European-type bridle — such as S. Piggott, C.J. Balkwill, M.-G. Hüttel, M.A. Littauer and J.H. Crowel, J.A.H. Potratz, alle referred to by my opponents, — the term “east” had different geographical connotations. In dating the Hasanlu IV bits, only definite proof of the Ancient Near Eastern origin of the Halstatt bridle might play a decisive rôle, but no such proof is afforded by any of the scholars named above. I would like the reader to recall that in my paper it was suggested that, in approaching this problem, two structurally distinct bridle types should be considered, each having a distribution area of its own. One of them, the type with the cheekpieces permanently fixed to each canon end, was in use in the Ancient Near East, whereas the other, comprising bits with separate cheekpieces, was characteristic of Eurasia⁶. Tracing the evolution of the Eurasian bridle,

Piggott and Balkwill show when and under what influences the Eurasian bridle type underwent fundamental changes, such as the replacement of organic materials by metal, and the subsequent ousting of one-canon metal bits by two-canon ones, the use of which materially facilitated control of the horse. Considering that in Europe this development took place later than in the Ancient Near East, Piggott — who used Balkwill's research — argues that “while independent translation from organic to metal bits in two unconnected areas is a reasonable assumption, the appearance of the two-canon bit in late HAB, with its *East European counterparts* (italics added), must be considered in connection with the ancient Near East *in its widest sense*” (italics added). Bij way of an example of Ancient Oriental bits sporadically reaching western regions in the latter half of the 2nd millennium B.C., he cites bronze bits from the hoard discovered in the acropolis at Mycaenae⁷. In this example the bits and cheekpieces are linked after the Oriental method, whereas in Europe metal cheekpieces were then mounted on the bits separately. In tracing a possible Ancient Oriental influence, Piggott had in mind only the structure of the two-canon bit. The question of any connection between Ancient Near Eastern bits and Halstatt mono-ring bits was not even raised.

Balkwill's research into the development of the bridle concerned solely the European region. His notion of “east” appears to have included an area extending as far as the Danube basin, and no farther. He believes that “the jointed bit” (i.e. Group 4 with separate cheekpieces, according to his classification) appears first in Western Europe in HAB₂/B₃: the origin of Group 4 may belong in this region, eventually being transmitted westwards via the Danube in HAB⁸”.

Evidently, Piggott's “East European” bit and the two-canon bit from HAB were “counterparts” in the sense that they had the same linkage of bits and cheekpieces: both had the cheekpieces mounted on the bits separately. The movement of this bridle type westwards, in the direction of Western Europe, is definitely recorded precisely for the eighth century B.C.⁹. The iron mono-ring bits found in the agora at Athens also reflect the influence of Halstatt culture¹⁰. I did not go into the problem of the indigenous origin of the Halstatt bit-and-cheekpiece complex (it is beyond the scope of the present study), but the iron bit first appeared in the Halstatt culture and may be called indigenous as far as that goes. The nomads (assuming that when saying “east”, my opponents mean the steppes of Eurasia) began to use iron bits not before the seventh century B.C. The most favourable conditions for the penetration of the Eurasian-type bridle into Iran existed in the eighth century B.C., when the predominant direction in the exchange of cultural elements between the North Caucasus and the Carpatho-Danubian world was from west to east (turning *contra versa* after the end of the century). We have good indicators of this eastward movement in the distinctive fittings for the cross-straps of the headstall, found in Iran. Only here, and in the Halstatt culture area, do they occur in combination with mono-ring bits¹¹. The significance of the western (more precisely, Danubian) component in the development of the “Pferd-Wagen-Komplexes” in a vast stretch of territory bordering on China, from as early as the 2nd millennium B.C., was specially noted and emphasized by Hüttel. Littauer and Crouwel do not discuss the genesis of either bridle type. There is only one passage which may be construed as an admission of their different origin: “whereas, previously, the outer ends of all *Near Eastern* (italics added) metal bits had passed through the cheekpieces, we now also find other methods of connecting canons to cheekpieces: ... cheekpieces passing through loops in ends of jointed canons or lashed to latter”¹². Neither does Potratz insist on the Ancient Oriental (notably Iranian) derivation of mono-ring bits and three-hole cheekpieces¹³.

In view of the foregoing, viz., the chronological priority of Western Europe in the use of iron mono-ring bits with separable cheekpieces; and the archaeologically documented fact of the eighth-century B.C. Halstatt culture being very dynamic — with its influence spreading both westwards, to Western Europe, and eastwards, to the North Caucasus and further south, reaching Iran (where it is evidenced by findings of mono-ring bits in combination with the fittings for cross-straps) — we cannot accept the opinion of Dyson and Muscarella concerning “an eastern origin for the Halstatt pieces” as valid, or admit it as a possible argument in the dating of the given bridle type from Hasanlu IV.

Two more artifacts may serve as evidence for the eighth-century B.C. dating of the bridle. They are the trapezoidal noseguard and the noseband. The presence of the latter type is attested in Hasanlu IV by examples found *in situ*¹⁴, and by representations on local style ivories carved with battle scenes. The noseguard was discussed by Dyson and de Schauensee in connection with the dating of the Hasanlu IV bridle as a whole. In an attempt to substantiate a ninth century B.C. date for the noseguards, and seemingly forgetful of their own eighth-seventh-century parallels for them, they turn to ninth century B.C. Assyrian reliefs where, they tell us, they can discern protective armour on the horses¹⁵. But Assyrian ninth century B.C. reliefs do not corroborate an early date. I have made a special study of the problem of horses' defence armour, discussing the noseguard as a functional element, and trying to find out when it first appeared, and what factors contributed to its development¹⁶. The dating, I believe, should be not before the eighth century B.C. Rather surprisingly (to me), Dyson and Muscarella regret that “so much time and space has been afforded to this simple and incomplete fragment”, meaning the noseguard¹⁷. They are probably no longer inclined to define the artifact in question as a noseguard, as before. This does not, however, invalidate my conclusions, particularly as regards the chronology of the noseguard and noseband complex.

The Hasanlu IV carved ivories have important dating features which assign them to the eighth century B.C. In addition to the presence of the noseband in the bridle, they record meaningful details such as the attachment of the tassel to the rein, not to the collar as in the ninth century B.C. This is a point of difference which seems to have escaped my opponents. This position of the tassel in the eighth century was in line with the general improvement in the rider's equipment, prompted by the increasing role of cavalry in battle¹⁸. Tassels then came to be used as ornamental accessories on chariot horses as well. This was a single large tassel hanging under the horse's jaw, not suspended from the collar as was the case in the previous century. The connection of the tassel to the rein is recorded in Hasanlu IV ivories¹⁹. The finding *in situ* — a single large tassel, not several of them — fully accords with our view²⁰.

Referring to a ninth-century B.C. relief from Nimrud, Dyson and Muscarella expect us to believe that it shows a mounted archer without a squire²¹. This would support their contention that the practice of riding without an outrider — an important dating feature — is known before the eighth century B.C. The relief is certainly poorly preserved, but plain to be seen behind the archer, are the beard and coiffure of another rider, holding in his hand the reins of the archer's horse. His presence is further attested by the existence of three horses' forelegs, represented in the same way as in the couple of riders following the group in question, and very much like it in many respects²². References to eighth century B.C. Urartian bronzes, claimed to show mounted archers without outriders, are even less convincing. here two things ought to be remembered: firstly, that (as I noted in my paper) the invention of the *nauz*-tassel in the eighth

century B.C. made the military rider independent of the assistance of an outrider; and secondly, that Urartian bronzes show, not archers, but lancers carrying shields, and lancers used to fight on their own. The same is true of the rider from Zincirli: he bears a shield, and consequently is not an archer²³.

Altogether, I can not see why the carved ivories manifesting different chronological features should not be dated by the latest of these features. That is precisely the method followed by Mallowan in dating Nimrud ivories²⁴.

I still wonder if Muscarella accepts Mallowan's dating of North Syrian ivories to the latter half of the eighth century B.C. This is an important question and I regret that it has remained unanswered²⁵. To realize when the general situation was most propitious for the diffusion of North Syrian ivories as far as Urmia, one has to have a clear idea of the contemporary pattern of military, political and economic conditions. My "version of eighth-century B.C. history in northwestern Iran" is based on the work of such scholars as S.M. Batsiyeva, I.M. Diakonoff, N.B. Yankowska, M. Wäfler, and O.W. Muscarella. Relevant to the subject in hand are two phenomena: the emergence, in areas far distant from North Syria, of schools of ivory carving clearly showing the influence of North Syrian art; and the fall to its lowest ebb of Assyria's military, political, and economic hegemony in the earlier half of the eighth century B.C. That these two phenomena coincided in time was probably not a matter of chance: the latter would have been favourable to the development of the former. A linking factor was provided by Urartu's growing role in international affairs. During the decline of Assyria, Urartu practically (although we can not say precisely in what capacity) headed the Northern Syrian Alliance, and seems to have had the support of the Southern Alliance as well²⁶. I.M. Diakonoff has reconstructed the circumstances which impelled Mattiel, King of Arpad, to form an alliance with Urartu²⁷. In Argishti I's reign, Urartu may have succeeded in establishing control over the portion of the Iron Route which traversed the Upper Euphrates basin²⁸. In its relations with Syria, Urartu may have played the rôle of an agent of transfer, carrying North Syrian goods in a north-eastern direction, even as far as Urmia. Obviously, political domination does not necessarily presuppose the cultural prevalence of the stronger state. This is convincingly illustrated in the work by M. van Loon which is referred to, in this connection, in my paper. It contains inscriptional evidence for the fact that during Urartu's political hegemony in the eighth century B.C., Syrian-produced luxury articles reached north-eastern lands in the form of tribute, gifts, etc.²⁹. Analysis of "Urartian" cauldron attachments by Muscarella and van Loon has revealed the North Syrian provenance of these items, and suggested the conclusion that "Urartu seems to have been the *receiver* (italics added) of some goods made in the South-West (either through trade or the rough plunder or imposition of tribute) and *producer* (italics added) of other goods which found their way to the North-East"³⁰. This was corroborated by N.B. Yankowska who showed that in the eighth century B.C. trade routes shifted, by-passing Assyrian possessions and areas of possible military activity³¹. It was in the light of these scholars' results that I considered the appearance of North Syrian ivories in Hasanlu IV³². The state of affairs just described may have contributed to the spread of North Syrian cultural influence to Hasanlu IV at a time when its local schools of ivory carving were in the process of formation. My opponents' answer to the question, "Who acted as an agent of transfer for North Syrian goods or ideas to Iran?" seems to be that "historical sources

indicate that this rôle was played in the ninth century by Assyria”³³. But this assertion involves a hidden contradiction. Muscarella holds that in the ninth century B.C. Assyria had a full monopoly of Iranian trade. At the same time, he says that “Assyria could keep North Syrians and other Westerns out of her eastern provinces and satellites, including regions in Western Iran”³⁴. The reason for this is not far to seek: the inflow of North Syrian goods might oust her own. How are we then to assess the ratio of North Syrian to Assyrian ivories in Hasanlu IV, which is 4:1 /21% to 5%/? Does it not indicate that Assyria could not be an agent of transfer? This ratio was very probably due to factors other than Assyrian mediation. In the ninth century B.C., Assyria was active in every sphere of international life, and we find evidence for this in Hasanlu IV³⁵. Now in the eighth century B.C. the situation changed, which is also reflected in the material from Hasanlu IV, particularly in the ratio of North Syrian to Assyrian goods.

Dyson and Muscarella attach the greatest importance, even surpassing that of archaeological and historical aspects of dating, to radiometric evidence. The data of radiocarbon analysis — by the way, first published in such detail, which alone would be enough to justify the appearance of my paper — are regarded as completely excluding the possibility of the town’s continued existence in the eighth century B.C., since the dates obtained by this method, with only a few exceptions, indicate a time no later than the ninth century B.C. But there seems to be a serious logical flaw in their reasoning. As a matter of fact, C14 dates cannot have anything to do with the fire which destroyed Hasanlu IV. They record the date of the tree’s death, i.e. when it was felled. The town was built no later than the ninth century B.C., or perhaps a little earlier. Beams, a highly valued building material, were apt to be reused. In the first fire, the houses were damaged; but they underwent only such repairs as the blocking of doorways, installation of additional columns, and the rebuilding of old stairways with fire-damaged bricks³⁶. Supposedly, the IVB structures might have incorporated ninth century B.C. or even earlier beams. The wide range of C14 dates seems to call for the use of archaeological and historical corrective factors³⁷. The radiocarbon dates published by Dyson and Muscarella in no way contradict the dating of Hasanlu IV within the ninth-eighth century B.C. time range. Moreover, they agree perfectly with such a dating.

Hasanlu IIIB. The principal arguments advanced by Dyson and Muscarella against a later date for the destruction of Hasanlu IV refer to the absence in Hasanlu IV: —

- a) characteristic Urartian objects among the 7,000 finds recovered from the stratum.
- b) diagnostic Urartian pottery.
- c) a fortification wall³⁸.

They question the identification of a number of artifacts as Urartian; this remains open to discussion. But is an abundance of Urartian cultural features really indispensable for proving that life at Hasanlu IV still continued in the eighth century B.C.? There were no Urartians among the inhabitants of the town. The Urartians, or *Herren*, to use the term suggested by S. Kroll, lived in fortresses that they built themselves, and it is with these sites that diagnostic Urartian pottery is associated³⁹. We may speak of a gradual growth of Urartian features in eighth century B.C. ceramics, and a spread of the fashion for ceramic forms brought to the area by the *Herren*⁴⁰; but there is no need to look for Urartian pottery on sites belonging to local people. Qualitative changes might, and did, occur in local ceramics, but this happened much later, viz. in the seventh century B.C.⁴¹. The indigenous population, having submitted to

Uartian rule, and taking care to pay the tribute imposed by the conquerors, was welcome to live in the places of their original habitation.

It would be natural to ask why, in the ninth century B.C., being relatively independent politically, Hasanlu IV should have had no fortification wall. One of the probable explanations for the eighth century B.C. might be a ban laid and enforced by the *Herren*, who built numerous fortresses for their own safety, stationing their garrisons in them. It is to be noted that neither the façades nor the porticoes of the buildings constructed after the first fire, show any resemblance to ninth century B.C. Assyrian or North Syrian architecture. These façades and porticoes were precisely the features once associated by Dyson with the Uartian architectural tradition: an assumption which, as I have said, would be more likely for the eighth than for the eleventh century B.C.⁴³

Reasoning along the lines suggested by Dyson and Muscarella, how is it possible to regard the individual finds of Uartian origin from Hasanlu III, which they discuss in their paper, as evidence for an “apparent relationship with or cultural dependence on Urartu”? No more than two Uartian seals in a Uartian town! Of the hinged fibulae from Hasanlu IIIB, Karmir Blur and Toprakkale, as well as Bastam, Muscarella wrote some time ago: “... fibulae were not generally employed by Uartians but were in common use among the peoples of the Caucasus”; and “the fibulae from Toprakkale, then, may be tentatively considered as imports from the Transcaucasus or from Karmir Blur where “they may reflect local influences”⁴⁴. If then Muscarella thought that “whether it [the hinged fibula] can be referred to as an ‘Uartian’ import is uncertain”⁴⁴, how is it that now it has come to indicate, to him, an Uartian origin for the Hasanlu IIIB population? Hinged fibulae occur in Iran over a vast territory, greatly surpassing the area of the Uartian zone⁴⁵. The production centre for this type was supposedly located in Transcaucasia, in the southern part of the region. The fibulae might have reached Iran in two possible ways: either through Uartian mediation, or through direct contacts with Transcaucasian peoples⁴⁶. The problem of “Uartian” ceramics in Hasanlu IIIB and some other sites of the Uartian zone has already been dealt with⁴⁷. Dyson and Muscarella describe the wall encircling Hasanlu IIIB as a “classic and superb Uartian fortification wall system”⁴⁸. There is nothing to prevent it being “classic and superb” etc. within the newly proposed chronological range for Hasanlu IV. In 714 B.C., Uartian political presence was brought to an end; yet by the seventh century B.C., the Uartian heritage already formed part of the local culture of the area. The century-long activity of Uartian administrators, soldiers and builders, could not have passed without a trace. And if A. Gunter is right in asserting that all over the territory of the ancient Orient fortresses displayed many architectural features in common⁴⁹, why should not we suppose that the local Hasanlu builders might have learnt construction techniques from their Uartian colleagues during the long years of Uartian domination? Especially as architectural standards varied to the extent that no two Uartian fortresses were completely alike.

Hasanlu IIIB may have been constructed in the seventh century B.C., either by local men or — more probably — by the Mannaeans who evidently established their control over Lake Urmia’s western coast in the 670s; in the middle of the decade, the region was reached by the Scythians as well⁵⁰. Dyson and Muscarella think it is possible to

associate the new date for the fall of Urartian fortresses along the country's eastern frontier, in the mid-seventh century B.C., with the end of Hasanlu IIIB⁵¹. However, the two events may not necessarily have been connected. Prior to the fall of Assyria and during the subsequent redistribution of her possessions, the land of the Mannaeans remained an independent state; and the events of the mid-seventh century B.C., which led to the fall of Urartu's eastern outpost — probably as a result of the growing power of Media — did not affect the Mannaeans in any way. Hasanlu IIIB continued to exist even later. It was possibly built in the 670s (a dating based on the analysis of Scythian finds)⁵². The interval between the destruction of Hasanlu IV and the building of Hasanlu IIIB lasted no less than forty years. The “squatters' occupation” and the probable “agricultural activities” on the citadel mound only accelerated the ultimate levelling of the ruins of the once great city.

In view of the considerations stated above, I cannot agree that my arguments in favour of Hasanlu IV's continued existence in the eighth century B.C. have been convincingly refuted by my opponents, and I do not believe it necessary to revise my conclusions.

Noten bij doc. medvesk

¹ DYSON R.H., MUSCARELLA O.W., Constructing the Chronology and Historical Implications of Hasanlu IV, Iran XXVII, 1989, pp. 1-27 (further referred to as Iran XXVII); MEDVEDSKAYA I., Who Destroyed Hasanlu IV? — Iran XXVI, 1988, pp. 1-15 (further referred to as Iran XXVI).

² I admit the justice of the assertion that not all the materials recovered are known to me: unfortunately, they have not yet been fully published. Nevertheless, I feel justified in believing that, however large the number of ninth century B.C. finds, the presence of even a small group of artifact types which, in that century, did not exist, imperatively demands that the eighth century B.C. be at least included in the Hasanlu IV time range.

³ In rendering my text thus: “mono-ring bits with separate, three-hole cheekpieces seem to have originated in the area of the Halstatt culture, in the *late HaB period*” (italics added), my opponents overlooked the fact that the passage in question referred to *iron* mono-ring bits, since bronze bits with separate cheekpieces were in wide use throughout the whole HaB period.

⁴ HBRKE H., Transformation or Collapse? Bronze Age to Iron Age Settlement in West Central Europe, in: The Bronze Age — Iron Age Transition in Europe, BAR International Series 483, Oxford, 1989, p. 184, fig. 35.

⁵ Iran XXVII, pp. 16, 24, note 50.

⁶ For a more detailed discussion, see MEDVEDSKAYA I.N., Horse Harness from the Sialk B Cemetery, Iranica Antiqua, XVIII, 1983, pp. 59, 60. These basic distinctions are far more significant than the internal morphological differences in details of the bits and cheekpieces of the European bridle.

⁷ PIGGOTT S., The Earliest Wheeled Transport, Thames and Hudson, 1983, p. 127.

⁸ BALCKWILL C.J., The Earliest Horse. Bits of Western Europe. Proceedings of the Prehistoric Society, vol. 39, 1973, pp. 425, 443, 445.

⁹ *Ibid.*, pp. 441, 445; PIGGOTT S., *op. cit.*, p. 126; HBRKE H., *op. cit.*, p. 185.

¹⁰ FOLTINY S., Athens and the East Halstatt Region. Cultural Interrelations at the Dawn of the Iron Age, AJA, 65, 3, 1961, p. 283, pl. 95 (8, 9).

¹¹ MEDVEDSKAYA I., Horse Harness..., pp. 71-77.

¹² M.A. LITTAUER and J.H. CROUWEL, Wheeled Vehicles and Ridden Animals in the Ancient Near East (Leiden, Köln, 1979), pp. 120, 121.

¹³ POTRATZ J.A.H., Die Pferdetransport des Alten Orient, Rome, 1966, p. 125.

¹⁴ MAUDE DE SCHAUENSEE and ROBERT H. DYSON, Hasanlu Horse Trappings and Assyrian Reliefs, Essays on Near Eastern Art and Archaeology in Honor of C.K. WILKINSON, New York, 1983, p. 67, fig. 9b.

¹⁵ *Ibid.*, p. 64. — As concerns the subject of note 58, here the fault lies with the translator. What I meant was certainly the date given in the publication, not my own.

¹⁶ Iran XXVI, pp. 2-8.

¹⁷ Iran XXVII, p. 24, note 67. Here the whole of my reasoning is confined to two conclusions of only secondary importance (Iran XXVII, p. 17, a, b), which in addition were not properly understood. Nothing was said of the diffusion of trapezoidal noseguards south of Transcaucasia. On the contrary, it is perfectly obvious that horses' defence armour evolved in the Ancient Near East; and as for the type of noseguard under discussion, it may have emerged in a Urartian milieu in the middle/latter half of the eighth century B.C. It was a basic type from which various modifications developed later on (hence the differences in the details of noseguards represented in fig. 4), as it spread over a territory including northern areas where its further evolution was associated with the Scythians.

¹⁸ Iran XXVI, pp. 8-10.

¹⁹ Iran XXVI, fig. 5; MUSCARELLA O.W., The Catalogue of Ivories from Hasanlu, Iran. University Museum Monograph 40, Philadelphia, 1980, Cat. No. 34 and probably Cat. No. 6.

²⁰ WINTER, I., A Decorated Breastplate from Hasanlu, University Museum Monograph 39, Philadelphia, 1980, p. 4, fig. 12.

²¹ Iran XXVII, p. 18, note 81. For a better reproduction than the one given by Winter, see WALLIS BUDGE E.A., Assyrian Sculptures in the British Museum, London, 1914, pl. XVIV, No. 16B.

²² Littauer gives a similar definition of the “squire’s” function in the ninth and eighth centuries B.C.; also, her interpretation of the subject represented in the relief is the same as mine: see LITTAUER and CROUWEL, *op. cit.*, pp. 134, 135, note 126.

²³ Iran XXVII, p. 18, note 82. Vieyra's work was indeed not available to me; but representations in the publications which I consulted suggested the same conclusion.

²⁴ Iran XXVI, p. 8, note 35.

²⁵ The authors' rendering of my text contains an error: "Medvedskaya", they say, "*chides*" (italics added) Muscarella for not questioning Mallowan who, she says, dates the *Hasanlu* (italics added) ivories to the latter half of the eighth century. Muscarella did not question Mallowan... because on p. 477 Mallowan dated the *Hasanlu* ivories to the ninth century B.C." (Iran XXVII, p. 24, note 87). Now the passage referred to reads: "He [Mallowan] dates *Syrian* [italics added] ivories within the chronological span of from the latter half of the ninth century B.C. — 720 B.C., referring the North Syrian ivories paralleled by Nimrud finds to the later part of the period" (Iran XXVI, p. 14, note 44). And here is what Muscarella himself once wrote: "North Syrian ivories from *Hasanlu* have parallels in Nimrud where they were either locally made or brought from North Syria" MUSCARELLA O.W., *The Catalogue...*, p. 213. That is why I attach so much importance to the answer to my question.

²⁶ DIAKONOFF I.M., *The Prehistory of the Armenian People*, New York, 1984, pp. 84-88.

²⁷ I need not be told that "Urartu never conquered North Syria"; this is self-evident. Neither could I assert that "Urartu controlled North Syrian trade" (Iran XXVII, p. 19).

²⁸ *A History of the Ancient World*, Leningrad, 1989, 3rd ed., p. 56 (in Russian).

²⁹ LOON VAN, M., *The Place of Urartu in First Millennium B.C. Trade*, Iraq XXXIV, 1977, p. 230.

³⁰ *Ibid.*, p. 229; MUSCARELLA O.W., "The Oriental Origin of Siren Cauldron Attachments", *Hesperia* XXI, 4, 1962, pp. 317-329.

³¹ Iran XXVI, pp. 11, 14, note 50.

³² Note 91, where I am supposed to hold the contrary opinion, in spite of my clearly expressed acceptance of both the authors' views, may be due only to a misunderstanding. Neither did I suggest anything like "eighth century North Syrian presence at *Hasanlu*" (Iran XXVII, p. 19). So far as I know, none of the scholars who has written of the emergence of schools of ivory carving in the area stretching from Greece to Urartu, has ever meant or implied Syrian presence in this territory. Neither have I given any grounds for ascribing this to me.

³³ Iran XXVII, p. 19.

³⁴ MUSCARELLA O.W., *The Catalogue...*, p. 213.

³⁵ I can not see anything "paradoxical" in my reference to Muscarella (Iran XXVII, p. 25, note 94). Perhaps I was not explicit enough, but I by no means exclude the ninth century B.C. from my dating of *Hasanlu* IV.

³⁶ Iran XXVII, p. 1; Iran XXVI, p. 14, note 61. It is probably due to an inadequate translation that I seem to underrate the extent and consequences of the first fire (Iran XXVII, p. 22, note 4). It was certainly not a small calamity, but it did not utterly destroy the city.

³⁷ The authors themselves tend to consult corrective factors as a means of refining the results of radiocarbon analysis: Iran XXVII, p. 8.

³⁸ Iran XXVII, p. 20.

³⁹ KROLL S., *Keramik Urartäischer Festungen in Iran*, AMI, *Ergänzungsband* 2. Berlin, 1976, S. 174.

⁴⁰ *Ibid.*, S. 172, 174.

⁴¹ MEDVEDSKAYA I., *The End of Urartian Presence in the Region of Lake Urmia*, *Archaeologia Iranica et Orientalia, Miscellanea in honorem L. Vanden Berghe*, Gent, 1989, vol. I, pp. 439-446.

⁴² DIAKONOFF I.M., *A History of Media*, Leningrad, 1956, p. 170 (in Russian).

⁴³ Iran XXVI, p. 15, note 63.

⁴⁴ MUSCARELLA O.W., *A Fibula from Hasanlu*, *AJA*, vol. 69, 1965, p. 236.

⁴⁵ VANDEN BERGHE L., *Les fibules provenant des fouilles au Pusht-i Küh*, *Luristan, Iranica Antiqua* XIII, 1978, pp. 68, 69.

⁴⁶ POGREBOVA M.N., *Transcaucasia and Its Contacts with Hither Asia in the Scythian Period*, Moscow, 1984, p. 156 (in Russian).

⁴⁷ See note 39.

⁴⁸ Iran XXVII, p. 20.

⁴⁹ GUNTER A., *Representations of Urartian and Western Iranian Portress Architecture in the Assyrian Reliefs*, Iran XX, 1982, pp. 103-112.

⁵⁰ MEDVEDSKAYA I., *The End...*, pp. 445-446.

⁵¹ Iran XXVII, p. 21.

⁵² Scythian artifacts found to the south of Transcaucasia belong exclusively to the Second, or Early Kelermes Phase of the Scythian Archaic Period. Characteristic of this phase were, among other artifact types, bone cheekpieces with zoomorphic terminals, which fell out of use in the Third, or Late Kelermes Phase / MEDVEDSKAYA I.N., *Periodization of the Scythian Archaic Period and the Ancient Orient. A Linguistic Reconstruction and the Earlier History of the Orient*, *Abstracts of the International Conference in Moscow*, 29 May — 2 June 1989 (in Russian). The Second Phase is now supposed by some to be datable within the earlier half of the seventh century B.C. (*ibid.*; KOSSACK G., *Zaumzeug aus Kelermes*. Halstatt Kolloquium, Veszprém, 1984, Budapest, 1986, S.134). In the 670s, Scythians were allies of the Mannaeans and the Medes, and consequently adversaries of Assyria. Since the 660s, they are no longer mentioned in cuneiform sources, probably because, led by Partatua, they had changed sides, and gone over to Assyria, and become her ally. So in the 660s Scythians would have been unlikely to stay in the land of the Mannaeans who still continued hostile to Assyria. Theoretically, then, Scythian artifacts from *Hasanlu* IIB, *Ziwiye*, *Qaplantu*, and *Nush-i Jan* ought to be datable to the 670s.