

Defining 'Neo-Elamite history'

Some thoughts on M.W. Waters, *A Survey of Neo-Elamite History*

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WATERS, Matthew W. --

A Survey of Neo-Elamite History. (State Archive of Assyria Studies, Vol. XII). State Archives of Assyria Project, Helsinki, 2000. (25 cm, XVIII, 139). ISBN 915-45-9044-9; ISSN 1235-1032. \$29.50.*

“To examine traditionally-held perceptions of Neo-Elamite history and their validity and to offer new interpretations as well as directions for further research” (p. 1): there can be no mistake about the goals set by Matthew Waters for his monograph on Neo-Elamite history, a revised edition of the author’s doctoral dissertation (University of Pennsylvania 1997). A separate survey of Neo-Elamite history has certainly been a desideratum, especially given the progress made and the new insights gained since the publication of *Elam, Surveys of Political History and Archaeology* (1984) by E. Carter and M.W. Stolper. The new survey is laudable for gathering and analysing the available textual sources and offering an update of the status quo in the field. The author’s ample use of the Assyrian royal correspondence (hence the inclusion of the monograph in the *State Archive of Assyria Studies* series) increases the value of this study. As such it will undoubtedly be welcomed as a convenient and well-documented tool, particularly in combination with the chapter on the Neo-Elamite period in the recent *The Archaeology of Elam* by D. Potts (1999).

Yet, considering the goals set by the author himself, the result is less gratifying. The opportunity to build an image that would do more justice to the vitality of ‘Elam’ in the first

* I am grateful to Amélie Kuhrt for her useful comments and suggestions on a previous draft of this text. The following abbreviations will be used in this review: ABC: chronicles in A.K. Grayson, *Assyrian and Babylonian Chronicles*, Locust Valley 1975; ABL: letters in L. Waterman, *Royal Correspondence of the Assyrian Empire*, 4 vols., Ann Arbor 1930-36; BIWA: inscriptions in R. BORGER, *Beiträge zum Inschriftenwerk Assurbanipals*, 1996; EKI: Elamite inscriptions in F.W. König, *Die elamischen Königsinschriften*, Graz 1975; MDP: Mémoires de la délégation en Perse; PF: Elamite texts in R.T. Hallock, *Persepolis Fortification Tablets* (OIP 92), Chicago 1969 (PF-NN: unpublished text from the same corpus).

millennium and the complex transformations it underwent has not been tackled successfully, notwithstanding various publications from the last two decades that could have provided the building stones for such a new approach. From this perspective this survey generally is what it explicitly strives not to be: a rather traditional view that does not cover the full extent of the Neo-Elamite horizon. This review is *not* intended to run down Waters' work - within the limits set to his study the author gives ample proof of his historical skills - but to indicate the directions that, in the opinion of the present reviewer, a new survey of Neo-Elamite history should have taken.

A Survey of Neo-Elamite History offers a chronological overview of the period roughly defined by 1) the end of the 'classical' Middle-Elamite empire in the 11th and 2) the gradual rise of the Achaemenid Empire in the second half of the 6th century BC. Elamite textual sources are confined to the period after *ca.* 750 BC, Mesopotamian sources are almost exclusively available for the period of *ca.* 747-643 BC. Notwithstanding gradual philological progress, it is hard to deny that many Neo-Elamite texts are still "en déchiffrement" (to use the term coined by Jean Kellens for Gatha-Avestan). Besides, it remains difficult at best to place Neo-Elamite texts on internal grounds (dynastic genealogy, palaeography and syntax) in a relative chronological framework. This results in an uncomfortable over-dependence of at times biased or ill-informed Mesopotamian sources. Waters is right in repeatedly stressing (*e.g.*, pp. 8-9) that this situation is methodologically undesirable, yet unavoidable. So, it does not come as a great surprise that most of his monograph is concerned with the period covered by the Assyro-Babylonian sources - a mere fifth of the Neo-Elamite period.

For a historian confronted with a situation like the one outlined above there are essentially two options. The first would be to accept the preponderance of the Mesopotamian documentation and use a critical analysis of these sources for a historical sketch that necessarily will focus exclusively on military and political history. Waters clearly favours this approach and even explicitly denounces the use of "specific archaeological and art historical considerations" (p. 1). Moreover, he does not discuss any of the Elamite texts *in extenso*, nor venture into independent philological analysis, which may come as a surprise in a survey of Neo-Elamite history. The content of *e.g.* the Elamite royal inscriptions does not receive full treatment; the texts are used mainly for the reconstruction of royal genealogies (cf. the author's remarks on p. 81). As stated before, much of the Elamite documentation is notoriously challenging, but the

main reason for renouncing the analysis of these texts may rather have been the selective criteria for historical relevance applied in this study. In his overview of sources Waters discusses the problems inherent in the biased Assyrian royal inscriptions. Elamite royal inscriptions “add little insight to these problems, and they often complicate matters. Most are dedicatory inscriptions, and they seldom contain details of the king’s military deeds” (p. 6). This is a perverse evaluation of Elamite sources, *i.e.*: they only possess historical value when they convey information on the political-military history, almost as an additional tool to the Mesopotamian documentation. Such a definition of ‘history’ is, in the eyes of the present reviewer, much too narrow. The exclusion of insights gained in the fields of archaeology and art history, the absence of extensive analysis of Elamite texts and - most disturbing - the exclusive focus on political and military events (mostly related to Mesopotamia) imply an unfortunate limitation of the historical horizon.

A second and preferable approach, given the problematical situation of the available documentation, would be to *maximize* the number of indigenous sources by including archaeological and iconographic data, precisely as a counterweight to the preponderance of the Mesopotamian documentation. The comparison with the Achaemenid period is instructive: here too the (Greek) documentation on a military conflict (the Graeco-Persian wars) is dominant; the corpus of royal inscriptions is equally small and (with the exception of Bisotūn) does not convey information on political-military history. Still, no scholar would nowadays even contemplate writing a history of the Achaemenid period that focused solely on the Graeco-Persian wars. Instead, it is considered obligatory to attempt to create an *Iranian* perspective by analysis of the royal inscriptions and the ideology they convey, by iconographic study, use of archaeological data and reconstruction of economic and socio-cultural systems. Although the historian’s hands are admittedly more tied when working on the Neo-Elamite period, he should at least try to follow the same lines of approach.

In his first chapter Waters explains his reasons for adopting a tripartite periodization (Neo-Elamite I-III). The NE I-II transition is marked by the accession of Huban-nikaš I, the first known Neo-Elamite king, in 743 BC. For the NE II-III transition Waters takes the year 653 BC, the first instance where there is “unambiguous evidence for contemporaneous Neo-Elamite kings” (pp. 3-4). Waters’ scheme is backed by authorities such as Steve, Vallat and Malbran-

Labat, who have advocated similar periodizations, but its validity may be questioned.¹ The mention of Huban-nikaš I's accession occurs in the Babylonian Chronicle I (ABC 71, I:9-10), but it does not name him as the founder of a dynasty. It should be noted that an earlier Elamite king could hardly have been mentioned anywhere as the first datable entry in Chronicle I is 745 BC and no other chronicles that could have mentioned Elam in the earlier part of the first millennium are extant.² The second transition line, 653 BC, is equally debatable. In this year Assurbanipal installed Huban-nikaš II as 'king of Elam' and Tammaritu I as 'king of Hidali' (BIWA 104 B VI 6-9). This does not, however, imply a kind of co-regency or a breaking apart into several kingdoms; in fact, Mesopotamian and Elamite documentation offer surprisingly little ground for this idea (see below). Thus, the tripartite periodization is based on an *argumentum e silentio* on the one hand, and an assessment that is at least questionable on the other. This situation nicely illustrates the trouble one runs into when relying too much on the Mesopotamian documentation. Besides, it shows the indispensable role of archaeology: Pierre de Miroschedji (followed by Elisabeth Carter) proposed a bipartite division with a transition of NE I-II at *ca.* 725/700 BC, the only periodization thus far that is primarily based on *Elamite* (archaeological) evidence.³ Most important, de Miroschedji pointed out that there was a continuity in the material culture from 725/700 BC onwards, notwithstanding the supposed instability of central government or the severe destructions of 646 BC claimed by Assurbanipal. Given the dearth of accurate sources, any structuring of Neo-Elamite history will have to remain hazardous, but in any case the apparent continuity in the material culture should be

¹ Cf. my remarks in "Persians, Medes and Elamites, acculturation in the Neo-Elamite Period", forthcoming in G. Lanfranchi *et al.* (eds.), *Continuity of Empire: Assyria, Media, Persia* (History of the Ancient Near East / Monographs series; 2003). A survey of other issues that will briefly be discussed in this review, the Kalmākarra hoard, Elamite-Iranian acculturation and 'Medes' in Susa, can also be found in this publication.

² Chronicle I also mentions the accession of Tiglath-pileser III to the Assyrian throne in 745 BC (*ibid.* I: 1-2), again without mentioning a predecessor. If it were not for other sources such as the Assyrian Kinglist, the same 'argumentation' could have presented this king as the founder of the Neo-Assyrian dynasty.

³ See (*i.a.*) P. de Miroschedji, Observations dans les couches néo-élamites au nord-ouest du tell de la Ville Royale à Suse, *Cahiers de la délégation archéologique française en Iran* 12, 1981: 143-67 and E. Carter in Carter & Stolper, *Elam ...* 182.

represented in it. Yet, de Miroschedji's periodization is not discussed by Waters and referred to only in a footnote.⁴

The idea of several concurrent or even rival Elamite kings and - closely related to that - the image of an increasingly fragmented and weakened Neo-Elamite state, was first advanced by Cameron in his *History of Early Iran* (1936, esp. 167-8, 186) and has been very persistent ever since. Several factors are at play: 1) There are two recorded cases of a 'king of Hidali' ruling concurrently with the 'king of Elam'; 2) A king known in the Mesopotamian documentation as 𐎶𐎠𐎺𐎠, is credited with a reign of six years in Babylonian Chronicle I (ABC 79, III: 7-8), but an economic text found in Nippur is dated to the fifteenth year of 𐎶𐎠𐎺𐎠 (A 33248), a discrepancy sometimes explained by assuming an earlier reign for this king in only part of Elam; 3) If the traditional equation of the Elamite king Tepti-Humban-Inšušinak with the Te'umman from Mesopotamian sources is followed, this necessarily implies that his predecessors Huban-haltaš I-II, 'kings of Elam' (known from the Mesopotamian sources) and his father 'king' Šilhak-Inšušinak II (known from Elamite inscriptions) were rivals in kingship; 4) There are indications of various local rulers, notably Hanni, the ruler of Aiapir, who left several inscriptions in two gorges near modern Īzeh. Each of these arguments will be briefly discussed below.

1) Hidali, probably a considerable city situated at the eastern border of modern Khūzestān, was of importance to the Neo-Elamite kings. Its location was strategic for various reasons: the city formed an outpost of the lowland Elamite state and, as it lay on the main route to the central plateau, it was instrumental for contacts with various tribes living there (cf. below). Furthermore, the Assyrian campaigns typically did not reach as far as Hidali and various Neo-Elamite kings found a safe haven in this city. For these reasons the governorship of Hidali must have been a prestigious position.

⁴ Steve and Vallat partly based their periodization on Elamite palaeographical evidence, but that only provides a relative chronology - their division-lines are also based on Mesopotamian documentation. See M.-J. Steve, *Syllabaire élamite. Histoire et paléographie*, Neuchâtel - Paris 1992: 21-3 and F. Vallat, Nouvelle analyse des inscriptions Néo-Élamites, in: H. Gasche & B. Hrouda (eds.), *Collectanea Orientalia. Histoire, arts de l'espace et industrie de la terre. Études offertes en hommage à Agnès Spycket*, Neuchâtel - Paris 1996: 385-95.

A certain Ištarandi, ‘king of Hidali’ was defeated by the Assyrians, probably simultaneously with Te’umman, ‘king of Elam’ (653 BC). Following this event Assurbanipal installed Huban-nikaš II as ‘king of Elam’ and his younger brother Tammaritu as ‘king of Hidali’. The position of the ‘king of Hidali’ has been explained variously. Pierre de Miroschedji saw it as a step in a Neo-Elamite tripartite *cursus honorum*, comparable to the *sukkalmaḥ*-system operative in Elam in the first half of the IIInd millennium.⁵ In fact, there is no convincing evidence for such an elaborate *sukkalmaḥ*-like system in the Neo-Elamite period and the idea has rightly been refuted by Waters (pp. 32-3) and others (see esp. Quintana, *NABU* 1996.4, note 109). Another option would be to explain the ‘king of Hidali’ as an indication of political fragmentation. This is not what Waters does in his careful discussion (pp. 54-6), but - rather surprisingly - his conclusion is much more outspoken: “If Ištarandi did rule concurrently with Te’umman, this reflects Elam’s split into separate kingdoms, an event that is not evident in the source material” (p. 107). Waters furthermore states that this event heralded increasing “factional strife” and finally “a complete breakdown in the Elamite political structure.” Consistent with this very outspoken perspective Waters marked 653 BC, the date of “unambiguous evidence for contemporaneous Neo-Elamite kings” (pp. 3-4), as the transition from NE II to III in his periodization (cf. above).

In fact the title ‘king of Hidali’ hardly supports the idea of a political split. Assyrian sources always acknowledge one single paramount ruler, consistently labelled as ‘king of Elam’. This also holds true for Te’umman and Huban-nikaš II: the Mesopotamian sources consider them as rulers of the Elamites, there is no indication of their power or area of influence being limited. On the contrary: a letter to Assurbanipal (ABL 1309, quoted on p. 60), probably dating to the reign of Huban-nikaš II, contains a reference of Huban-nikaš entering Hidali. Later, during the reign of Indabibi, Assurbanipal threatened this king that he would destroy and depopulate Susa, Madaktu *and Hidali* and put another person on the *Elamite* throne (BIWA 153-55 C IX 59-86, quoted on p. 66).⁶ These cases may not be particularly clear, but they are

⁵ La localisation de Madaktu et l’organisation politique de l’Élam à l’époque neo-élamite, in: L. de Meyer, H. Gasche & F. Vallat (eds.), *Fragmenta Historiae Elamicae, mélanges offerts à M.-J. Steve*, Paris 1986: 209-25.

⁶ One may add BM 79013, a Babylonian economic text drafted at Hidali and dated to “Tammaritu, king of Elam”. This has been explained as a reference to the Tammaritu (I) who was king of Hidali (see Leichty, *AnSt* 33, 1983: 153-5). This king, however, reigned concurrently with his brother Huban-nikaš II who was the acknowledged ‘king

obviously relevant to the Hidali debate and should have been discussed by Waters in that context. As far as the limited available evidence allows any conclusions, there seems to be no ground for assuming a split of the Elamite kingdom. Hidali was a strategically important city and the position of its governor would obviously have been related to this. From the case of Huban-nikaš II and his brother Tammaritu, it could be inferred that ‘king of Hidali’ was a position given to a member of the ruling dynasty.⁷ Such an arrangement would certainly not imply political weakness, but rather an attempt at consolidating the exclusive power of the ruling dynasty and to put an important city under a special and trusted governor. Again, a parallel with the Achaemenid Empire is instructive. Cambyses acted, briefly, as king of Babylon during the reign of his father, Cyrus; that arrangement too did not imply political weakness or fragmentation.

2) From the discrepancy between 𐎶allušu’s six years’ reign in the Babylonian Chronicle (698-693 BC) and the economic text A 33248 dated to the fifteenth year of ‘𐎶allušu, king of Elam’ D.B. Weisberg (*JAOI* 104, 1984: 213-7) surmised that a king 𐎶allušu started as the king of a smaller entity before he became king of all Elam. Waters follows this line: “If A 33248 is connected to the 𐎶allušu in question [...], its date formula may indicate a fractured Elam, organized around various political centres” (p. 28). The author cautiously considers the possibility of a scribal error, but seems inclined to interpret the discrepancy as an indication of a “decentralized Neo-Elamite state” (p. 105). A scribal error is indeed far from excluded given

of Elam’ in Mesopotamian official documentation. There are no references to rivalry between these two brothers. Hence it would be more logical to identify the king in BM 79013 as Tammaritu II who succeeded Huban-nikaš II as king of Elam. If this holds true, we have a text drafted in Hidali, but dated to the king of Elam - again indicating Hidali’s inclusion in the kingdom.

⁷ Compare the lucid remarks of Quintana on this subject (*NABU* 1996.4, note 109). There are parallels for a member of the ruling dynasty holding strategic positions, such as the case of Imbappi, brother-in-law of Huban-haltaš, ‘chief bowman’ of the Elamite army, who commanded the city Bit-Imbî (see p. 70 with references). See also J.M. Córdoba, *Die Schlacht am Ulaya-Fluß...*, in: H. Waetzoldt & H. Hauptman (eds.), *Assyrien im Wandel der Zeiten* (CRRAI 1992), Heidelberg 1997: 7-18. Córdoba also stresses the unity of the Neo-Elamite state, in his view a necessary element for understanding the Elamite strategy against the Assyrian military campaign of 653 BC (see esp. p. 15). His analysis (following von Clausewitz’ principles) of the Assyrian strategy in this campaign and the tactics deployed in the battle at the Ūlāya river is very illuminating; it is deplorable that his findings are absent from Waters’ discussion of the event.

the date of XII/15/15 (12th month, 15th day, 15th year). Moreover, a similar document drafted by Babylonians in Elam (PTS 2713, see Stolper in *Mélanges Steye*: 235-9) dates to the first year of Hallušu, 'king of Elam'. If the same dating-system as in A 33248 is used, this would have to imply that Hallušu, though reigning in only part of Khūzestān, claimed to be 'king of Elam' and was faithfully given this title in private Babylonian documents already in the first year of his reign (though Šutruk-Nahhunte II was at the same time recognized as 'king of Elam' in Babylon). These objections are at least problematical to Weisberg's theory. In the absence of additional evidence any speculation (even with cautious reservations) on a fragmented Elam during Hallušu's reign has to be qualified as hazardous and of little use.

3) Te'umman (664[?]-653 BC), known from the Mesopotamian documentation, used to be identified with Tepti-Huban-Inšušinak, a ruler who left several inscriptions in Susa (EKI 79-85). The former is known as brother of Urtak and Huban-haltaš II (who were sons of Huban-haltaš I); the latter claims to be son of 'king Šilhak-Inšušinak' (II), who himself is also known from a Susan inscription (EKI 78). As Waters explains, the equation implies that one or several of Te'umman's predecessors as 'king of Elam' ruled concurrently with this 'king Šilhak-Inšušinak'. Moreover, one would have to assume that Šilhak-Inšušinak II and Huban-haltaš I married the same wife, otherwise Huban-haltaš II, Urtak and Te'umman could not be (half-)brothers. Although the problem cannot be dealt with in full here, it may be clear that the problems raised by the equation of Te'umman with Tepti-Huban-Inšušinak lead to an uncomfortable amount of speculation. Moreover, Vallat, in his revolutionary analysis of Neo-Elamite inscriptions (see n. 4 above), placed both Šilhak-Inšušinak II and Tepti-Huban-Inšušinak in the period *after* the sack of Susa (646 BC), on the basis of palaeographical considerations.⁸ This would exclude the equation with Te'umman altogether. Unfortunately, the palaeographic developments in various kinds of Neo-Elamite texts may not have kept equal pace and a chronology cannot be established with absolute certainty. Waters is certainly to be credited for his acknowledgement and discussion of this problem. Generally, Waters does not seem to favour the Te'umman – Tepti-Huban-Inšušinak equation (pp. 40-1; 47-50), but this

⁸ This later date makes it possible to equate Ummanunu, the father of Šilhak-Inšušinak (known from EKI 78) with the king Ummanunu known from the so-called Acropole archive. This possibility is certainly more attractive than equating Ummanunu with 'king of Elam' Huban-menanu (692-689 BC) as Cameron (*History of Early Iran* 167-8) did.

position is not held to consistently. The discussion on Šilhak-Inšušinak is placed after that on the reign of Huban-haltaš II (681-675 BC), which is confusing and not in line with Waters' inclination (p. 96) to follow Vallat (placing this king in NE III). Likewise, when the author picks up the issue in his conclusion (p. 105), he only discusses "the ramifications of maintaining the identification of Tepti-Huban-Inšušinak with Te'umman" and does not refer to his own objections to this equation. Finally he places both Šilhak-Inšušinak II and Tepti-Huban-Inšušinak (admittedly with question marks) in his table of Neo-Elamite kings in NE II and not in his list of "rulers of uncertain date" as one would have expected. All of this gives the impression of an inconsistent attitude towards the problem and this is not beneficial to the clarity of the argumentation.

4) A last argument thought to support the idea of political fragmentation is a series of Elamite texts naming various 'kings'. Waters discusses all of these in his seventh chapter: the inscriptions of Hanni near modern Īzeh, the stele of Atta-hamiti-Inšušinak, the Persepolis bronze plaque naming Huban-Šuturuk, the Acropole texts mentioning Appalaya of Zari and Ummanunu, the Jerusalem cylinder seal with 'Huban-kitin, son of king Šutruk-Nahhunte' and the objects belonging to the Kalmākarra hoard bearing inscriptions of the 'kings of Samati'. All of these texts must probably be dated to the last century of the Neo-Elamite period, but it seems impossible at present to put all of them in one coherent framework, as Waters stresses. Again it comes as a surprise that according to Waters "the Acropole texts and Nineveh letters reveal renewed but splintered Elamite kingdoms..." (pp. 100-1). Those two groups of texts, however, *do not* give any decisive clue as to the splintered state of the Neo-Elamite kingdom, nor does Waters' survey of these sources (pp. 89-97) provide any explicit indication. An even stronger statement is found in the concluding chapter: "Analysis of the late Elamite sources indicates that Elam persisted in its fragmented state until it lost its independence to Persia" (p. 107). Such a conclusion is unacceptable. The sources are susceptible to diverging interpretations and - more important - Waters' survey does not include any analysis that could warrant such a statement.

As stated before, this review is not the place to discuss all the issues involved, but an alternative reading of some of the relevant texts may at least be indicated here, if only to justify the criticism expressed above. The most important group is that of the 'Acropole' texts, an archive of the palace at Susa, dated sometime after Assurbanipal's sack of that city. The 'king Ummanunu' (MDP IX, 165:4-5) is often taken as the ruler of Susa. The extent of the Susan

monarchy is uncertain, but the scope of the palace's economic activities certainly encompasses much of Khūzestān. An 'Appalaya king of the people of Zari' appears in various Acropole texts. Zari is unattested elsewhere; it is unknown if it presented an independent entity or whether it formed part of Elam at any time. The name of Aplāia, the grandson of Merodach-baladan, who was extradited from Elam by Huban-nikaš II, may be the same as that of 'Appalaya'. Thus 'the people of Zari' could theoretically have been an Aramaic or Chaldaean tribe on the southwestern fringe of Khūzestān. The content of the 'Nineveh letters' remains largely obscure; all texts may be written to or by a certain Bahuri. Vallat, in his recent re-analysis of the texts, convincingly argues that Bahuri was an Elamite king.⁹ Apart from this no safe conclusions can be drawn.¹⁰ The kings of Samati, known from inscriptions on the large silver treasure found in Kalmākarra cave near Pol-e Dokhtar, probably ruled a small (tribal) entity in southern Lorestān, an area that may not have been under direct control of the Elamite kings throughout the Neo-Elamite period. These inscriptions do not attest to a fragmentation of the Neo-Elamite state, the entity we know from the Mesopotamian documentation, but to a proliferation of Elamite culture outside this state.¹¹ The same may hold true for Huban-Šuturuk (Persepolis bronze plaque), who seems to have ruled an area around Gisat, on the eastern border of Khūzestān or further into Fārs (cf. Waters pp. 87-9). Hanni, the '*kutur* of Aiapir', refers to 'king Šutur-Nahhunte son of Indada'; his subordination is expressed explicitly. It surely would not have been the first time that a petty king posed as independent

⁹ Le royaume élamite de Zamin et les "lettres de Niniveh", *IA* 33, 1998: 95-106. Vallat's arguments are only partially discussed by Waters.

¹⁰ Vallat believes that 'Zamin of Elam' is the name of the entity Bahuri ruled in the same period when Ummanunu reigned in Susa. Yet, the appearance of his name in a closely related letter found in Susa (MDP XXXVI, 1) suggests his immediate involvement in Susa (I find it hard to take the text as reflecting an 'acte d'espionage' as Vallat wants). In this context the recent suggestion of Julian Reade (*NABU* 2000.4, note 80) deserves further attention: Bahuri, 'founder of a royal house' (Vallat *o.c.*) may be identical to the Pa'e, who rebelled against Huban-haltaš III (648²-645²), claimed the title 'king of Elam' and subsequently had to flee and throw himself at Assurbanipal's feet (for references see Waters pp. 77-80).

¹¹ Waters does refer to, but does not discuss, the possible implications of the Kalmākarra hoard; the original version of his dissertation (1997, UMI 9727312) had more on this subject, as it had on the Elamite texts found in Armavirblur (ancient Argīštihenele, Urartu). The bibliography of the revised edition still contains, however, all the relevant titles.

ruler in the texts designed for his own local audience.¹² That Hanni abstains from this seems to underline the political *strength* of his overlord. For this reason it is all the more deplorable that it cannot be established whether this ‘king Šutur-Nahhunte, son of Indada’ headed the Neo-Elamite state at any time.¹³

The preceding remarks indicate that all the arguments used in favour of a fragmented Neo-Elamite state are open to different interpretations and need critical re-examination. It should be repeated here that Waters’ study has its own merits, especially the chapter (VII) on the ‘late and uncertain rulers’ that has much more discussion of the language and contents of Elamite texts and is laudable for its convenient presentation of this almost impenetrable documentation. This makes it all the more deplorable that the problematic idea of a fragmented state is not really discussed to its full extent and that all the various ‘kings’ are basically taken at face value without considering a more sophisticated model. Such a model should take into consideration that tribal groups living at the western, northern and eastern borders of first millennium Khūzestān probably fostered a wide variety of relationships with

¹² Compare Adad-it’i, who describes himself as *šaknu*, ‘governor’ (*i.c.* of the Assyrian king) in the Aramaic version of the Tell Fekheriye inscription, but as *mlk*, ‘king/ruler’ in the Aramaic version (see discussion in A. Kuhrt, *The Ancient Near East c. 3000-330 BC*, vol. II, London 1995: 397ff. and compare 491ff. on a similar issue, the ‘Sfire treaties’).

¹³ The matter is part of the frustratingly complex Šutruk-Nahhunte/Šutur-Nahhunte problem, which is conveniently presented and discussed in Waters’ appendix B (pp. 111-116). His suggestion, however, that Hanni’s overlord might be identical to ‘Ištarnandi, king of Hidali’ is not unproblematic: both Hidali (near or east of Behbahān) and Aiapir (Īzeh) were located in the mountainous region east of the Khūzestān plain, but a direct route between the two regions is unlikely to have ever existed given the inaccessibility of the intermediate area. In fact, Īzeh (which even today remains rather isolated), would probably have been reached quicker from central Khūzestān (Susa). Moreover, Waters’ proposition implicitly assumes an independent position for the ‘king of Hidali’, a view that I am inclined to oppose (cf. above). Vallat (*Nouvelle analyse...* 391 and *NABU* 1995.2, note 44) assigns to Hanni’s Šutur-Nahhunte the rule over Mālamīr (i.e. the Īzeh region). In this case, it is unclear, however, what Hanni’s function was and why the ruler of the area did not himself commission the inscriptions (EKI 75-6) at the two (obviously important) cultic centres near Īzeh. That ‘Šutur-Nahhunte, son of Indada’ was ruler of the central Neo-Elamite state should not be excluded, notwithstanding the genealogical problems. A solution may be found by interpreting in some cases *šak* not as ‘son (of)’, but as ‘descendant (of)’ (following Hinz and Steve; see Waters p. 18 for references).

the Neo-Elamite state.¹⁴ This alone renders obsolete the implicit axiom of a centrally governed territorial state ruled by one absolute king. Between old urban centres like Susa and areas where tribal (agro-) pastoralism was dominant a whole spectrum of variously defined loyalties towards the Elamite kings are likely to have existed. This certainly does not need to imply political fragmentation or even weakness, just as it does not imply it in the case of the Achaemenid Empire. The traditional and one-sided perception of the latter as a loose congregation of largely autonomous peoples headed by a powerless and distant king has been successfully challenged and replaced by a model that tries to underline the multiple and inventive ways in which cultural identities and local forms of organisation and control were used to maintain the Empire's internal cohesion. Especially relevant is the case of the Ouxioi, Kossaioi and other peoples living in the Central Zagros, on the borders of modern Khūzestān. Following the assessments of the Alexander biographers, the seemingly complete autonomy of these 'lawless brigands' (right in the heart of the Empire!) has long been taken to indicate the impotence of the central authority. Yet, a careful re-evaluation of the evidence has revealed that these tribes entertained a complex of mutual loyalties with the Great King and that these entities were indeed woven into the political texture of the Achaemenid state.¹⁵ One should at least consider the possibility that the Neo-Elamite state was essentially built upon a small-scale version of similar structures. For this purpose the historian will have to make the best of the scant evidence, but this is by no means an impossible task. Particularly the further exploration of the Acropole texts, which document exchanges with various (tribal) groups outside the Susiana, in combination with the still increasing archaeological evidence, will undoubtedly be rewarding.

The above remarks on the assumed fragmentation of Elam, are not intended to underestimate the effect of the Assyrian involvement in Elam. Still, from Waters' survey of this century-long involvement, one cannot escape the impression that the Assyrians achieved little more than temporarily limiting Elam's capacities to mount military campaigns. Until the reign of

¹⁴ The case of the Aramaeans living in southwestern Khūzestān is the most promising in terms of documentation. One may think of, e.g. the family relation between Tammaritu (II) and the Gaššal family or the attempts of the Elamite crown to appoint a leader of several tribes in the Sealand (ABL 282 and 576; see Waters pp. 38, 64).

¹⁵ See esp. P. Briant *État et pasteurs au Moyen-Orient ancien* (1982): 57-112 and *Histoire de l'Empire Perse* (1996): 747-53.

Assurbanipal both parties challenged each other with varying success. Assurbanipal may for a time have gained political control over parts of Elam, but he could not count on the loyalty of the officials he appointed as the epistolary documentation discussed by Waters (pp. 58-59) reveals. Even the puppet-king installed on the throne of Elam, Huban-nikaš II, turned against his benefactor. After the celebrated sack of Susa Elam did not become an Assyrian vassal-state and a Susa-based kingdom soon emerged (cf. Waters' discussion on pp. 102-3).

Perhaps the Assyrian campaigns were not, after all, the defining factor of the Neo-Elamite period. A better candidate for this would be the influx of Iranian tribes that increasingly populated the areas immediately north and east of the Neo-Elamite state. Waters mentions this factor several times (pp. 3, 35, 102), describing Elamo-Iranian relationships in Fārs as “one of the great conundrums of ancient Near Eastern history” (p. 16), but stresses that the Iranian migration does not appear in the written sources. This is not completely true: the actual migration may not have been documented, but the textual evidence does indeed betray an increasing Iranian involvement and influence. Iranian names, as well as *Parsip* (± ‘Persians’), appear in the Acropole texts as they do in Elamite cylinder seal inscriptions. Elements of Iranian syntax make their way into the Elamite language and even some Assyrian letters mention the presence of Persians in Hidali.¹⁶ Conversely, an Elamite influence is present in the documentation of the early Achaemenid Empire, especially in the (Elamite) Persepolis Fortification Texts, where we find a mixed pantheon of Iranian and Elamite gods - a situation that must have emerged from acculturation in the Neo-Elamite period. Most of these textual sources are mentioned by Waters, but not discussed in a coherent way.

Generally, the Iranian factor is neglected in Waters' survey, not least because of the author's renouncement of discussing archaeological and iconographic evidence. A second factor is his decision not to include the early Achaemenid kings (p. 6), as those kings (especially Cyrus) showed a revealing sensitivity towards the Elamite heritage. It is certainly true that in

¹⁶ Iranian names: F. Vallat, *NABU* 1995.2, note 45; M. Mayrhofer, *Aus der Namenwelt Alt-Irans. Die zentrale Rolle der Namenforschung in der Linguistik des Alt-Iranischen*, in: M. Mayrhofer, *Ausgewählte kleine Schriften*, Wiesbaden 1979: 138-9. Elamite language: E. Reiner, *Calques sur le vieux-perse en élamite achéménide*, *Bulletin de la Société linguistique de Paris* 55, 1960: 222-7. ‘Persians(?)’ in Hidali: ABL 961 and 1309 (Parsumaš); E. Carter, *Bridging the gap between the Elamites and the Persians in Southeastern Khuzistan*, in: H. Sancisi-Weerdenburg, A. Kuhrt & M.C. Root (eds.), *Continuity and Change (Achaemenid History VIII)*, Leiden 1994: 75.

this case archaeology does not provide a clear image either, but progress is being made and some of the recent discussions have a direct bearing on Neo-Elamite history. One may refer to de Miroschedji's concept of an "éthnogenèse des Perses" (from the integration of Iranians and Elamites in Fārs), Carter's discussion of the sedentary zone in eastern Khūzestān (instrumental for contacts with the Iranian tribes), Sumner's re-evaluation of the ceramic evidence (perhaps indicative of Iranians settling in Elamite villages) or Boucharlat's assessments of Elamo-Iranian artistic continuities. Some recent discoveries also have a direct bearing on the issue as they indicate acculturation: the Arḡān tomb and the Kalmākarra hoard.¹⁷

It may be clear that, at least in the eyes of the present reviewer, (Mesopotamian) textual evidence alone cannot give a balanced image of the Neo-Elamite period. Without the evidence from archaeology and iconographic analysis, a historical survey will be incomplete unless one's definition of 'history' is extremely narrow. The period indicated by Waters as Neo-Elamite I (pp. 10-11) is not as dark with respect to the material evidence as it is in the written documentation. More important, dependence on the military-political history known from the available textual sources, must inevitably lead to a simplified perspective. This is the case when Waters speaks of "a declining Elam (splintered into small kingdoms) gradually subsumed by a rising Persian empire" (p. 104) or states (in the conclusion's last sentence) that "...continued Assyrian pressure wore Elam down into separate kingdoms still linked by way of language, commerce, and tradition but lacking the political cohesion and military strength to fend off Persian expansion" (p. 107). These statements do not leave any room for the complex processes of acculturation and integration that must have existed between Iranians and Elamites and that were undoubtedly instrumental to the formation of the Achaemenid Empire.

The three Elamite uprisings against Darius (two of which were headed by rebels with assumed Elamite royal names), as well as the importance of Elam's culture and territory in the Achaemenid Empire and the emergence of the independent kingdom of Elymais in the post-

¹⁷ P. de Miroschedji, *La fin du royaume d'Anšan et de Suse et la naissance de l'Empire perse*, *ZA* 75.2, 1985: 265-306; *idem* *La fin de l'Élam: essai d'analyse et d'interprétation*, *IrAnt* 25, 1990: 47-95; E. Carter, *Bridging the gap...*; W. Sumner in: H. Sancisi-Weerdenburg *et al.* (eds), *Continuity and Change* (Achaemenid History VIII), Leiden 1994: 65-95; 97-105; R. Boucharlat *ibid.*: 217-228. For discussion and bibliography on the Arḡān tomb and the Kalmākarra hoard see Henkelman (forthcoming).

Achaemenid period testify to both the vitality and the metamorphosis of ‘Elam’. *The Archaeology of Elam*, the well-documented survey on “the formation and transformation of an Ancient Iranian State” by D.T. Potts, describes ‘Elam’ as a concept that continuously renews and re-invents itself. It is a missed opportunity that Waters failed to adopt this perspective for the Neo-Elamite period to which it would be most applicable.

Some minor issues may be indicated as well:

-It is somewhat surprising that the map of “Mesopotamia and Elam in the Neo-Elamite period” (p. xviii) does not have the caption ‘Elam’ (though a provisional territorial definition is given on p. 3). On the other hand a caption ‘Hidalu’ appears in capitals, as do ‘Ellipi’ and ‘Gambulu’. This suggests that ‘Hidalu’ is thought to refer to a wider area or a people. Such may be in line with the author’s thought of a semi-independent ‘king of Hidali’, but it is certainly not backed up by the sources, that only speak of the *city* Hidali. Generally, the map is not very detailed, to put it euphemistically. The rendering on the map of southwest Iran lacks geographical details (the Iranian plateau, the Susiana plain, the central Zagros range, etc., etc.) as well as the many localities and areas known from both the Elamite and the Mesopotamian documentation. Many of these cannot be located precisely, but not all: the numerous studies on the geography of the area should have been reflected on the map; as it is now it hardly illustrates “Elam in the Neo-Elamite period”.

-The traditional identification of Ištar-ḫundi, sister-son of Huban-nikaš I (ABC 74-5; I:39-40) with Šutruk-Nahhunte II (EKI 72-3) is defended by Waters (p. 16). The latter king claims Huteluduš-Inšušinak, Šilhina-hamru-Lagamar and Huban-immēna as predecessors, rightly explained by Waters as a way of legitimizing Šutruk-Nahhunte’s descent (p. 17). Yet, the omission of Huban-nikaš I is somewhat mysterious. If, as Waters proposes (see his reconstruction on p. 26), Huban-immēna was from another, possibly rival dynasty and married the sister of king Huban-nikaš (cf. the Babylonian Chronicle), one would have expected his son to take full advantage of this dynastic concord, claiming both Huban-nikaš I and Huban-immēna as predecessors. Alternatively, if Huban-immēna was just a remote predecessor (as Huteluduš-Inšušinak and Šilhina-hamru-Lagamar were), it would still be remarkable that Šutruk-Nahhunte did not mention his father’s name or that of Huban-nikaš. Whatever the

solution to this problem (compare also Vallat *IA* 32, 1997: 65-6), it should be admitted that the Babylonian Chronicle and the Elamite inscriptions do not fit well together.¹⁸

-The passage in Assurbanipal's annals of four captured kings pulling his carriage, has a variant in which the Arabian king Uaite' is replaced by the Elamite 'Huban-nikaš' (see Hallo, *Israel Museum Journal* 6, 1987: 33-7 with further references). Waters (p. 80, n. 59) takes the latter to be Huban-nikaš II (653²-652² BC), but mentions at the same time that this king was killed by Tammartu II. One might consider the Huban-nikaš, son of Ademirra, who rebelled against Huban-haltaš III in the 640s as a possible alternative (ABL 280; see Waters p. 73). In any case, it seems clear that the variant text was inspired by the wish to have a full quartet of captured Elamite kings.

-For "BM 123793" (p. 80) read: "BM 124793".

-Waters' treatment of the stele of Atta-hamiti-Inšušinak (pp. 85-7) is laudable, if only for the thought provoking suggestion that this late Neo-Elamite king might be identical to the rebel Aθamaita in Darius' Bīsotūn inscription (DB V §71). Previously, it was always assumed that 'Aθamaita' was taken from the Neo-Elamite king as a resonant throne-name used to stir up nationalistic feelings among the rebellious Elamites. Waters' suggestion touches upon the possibility, seldom raised, that, during Cyrus' and Cambyses' rule, Elam (or part of it) was not under (direct) Persian control. Cyrus' march and victory in Mesopotamia (539 BC) does not need to imply the annexation of Elam, as is usually thought. In any case, the material evidence for Persian rule in Susa does not start before Darius. It is not unthinkable that Atta-hamiti-Inšušinak was a semi-independent Neo-Elamite king, who took his chance during the tumultuous early years of Darius' reign to re-establish a completely autonomous state and was mentioned as the rebel Aθamaita in Bīsotūn (note that the text does *not* state that Aθamaita was a throne-name as it does in many other cases). This would certainly be in line with the "grand aspirations" (Waters) expressed in Atta-hamiti-Inšušinak's inscription.

-According to note 36 on p. 87 the name 'Huhpir' occurs in EKI 89:4 and "has been identified with Huhnur". In fact the form ^{AS}*hu-uh-pir* does not exist at all: it represents an old reading (also used by Scheil in MDP IX), which should be corrected to ^{AS}*hu-uh-nur(u)* (see J. Duchene, in: *Mélanges Steve*: 66). Besides, the name does not occur in EKI 89:4 but in 88:4.

¹⁸ See also the more extensive critical remarks by F. Vallat in his review of Waters' monograph (to appear in *OLZ* 97, 2002) where Waters' reconstruction is refuted.

-The Acropole texts (MDP IX) mention various groups of Iranians, “often specifically Persians, but also Medes as well” (p. 94). The accompanying footnote does not refer to W. Hinz, the promotor *par excellence* of the idea of ‘Medes’ in Susa in several publications.¹⁹ If ‘Medes’ were mentioned in a Susa archive, this would obviously have an important bearing on Neo-Elamite history. Even a casual inspection of the relevant texts reveals, however, that the evidence for ‘Medes’ is rather meager and hardly convincing.

-Considering Neo-Elamite titulature, Waters states that Šilhak-Inšušinak (II) “claimed only the general title ‘king’ (p. 105). He then states that other rulers “bore the same title in other inscriptions”, listing Šutur-Nahhunte son of Indada, Šutur-Nahhunte, father of Huban-kitin, Huban-Šuturuk, Appalaya, Ummanunu and the kings of Samati. This presentation is a bit misleading. The title of the ‘kings of Samati’ is obviously ‘king of Samati’, not ‘king’. Appalaya is ‘king of the people of Zari’ (MDP IX, 71:1-2). Ummanunu is indeed called ‘king’ in the same archive (MDP IX 165:4-5), but if he is indeed the ruler in Susa there would be no need for the writers of these primarily economic texts to give his full title.²⁰ Šutur-Nahhunte, father of Huban-kitin, and Huban-Šuturuk are named ‘king’ in seal inscriptions; again it is perfectly possible that this title is just caused by the nature of the (necessarily short) inscription. Finally there is Šutur-Nahhunte son of Indada, coined ‘king’ by his subordinate Hanni; it remains uncertain whether this is a description or a formal title. Thus, the only case where ‘king’ may plausibly be taken to be a formal title is the inscription by Šilhak-Inšušinak.

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¹⁹ See e.g. W. Hinz, Zu den Zeughaustäfelchen aus Susa, in: G. Wiessner (ed.), *Festschrift für Wilhelm Eilers*, Wiesbaden 1967: 85-98 and *idem*, Elams Übergang ins Perserreich, in: *Transition Periods in Iranian History. Actes du Symposium de Fribourg-en-Brisgau (22-24 mai 1985)*, Leuven 1987: 125-134.

²⁰ Compare the case of queen Irtaštuna (Artystone) in the Persepolis Fortification texts. Out of 27 occurrences her name is accompanied by the title *dukšiš* (‘royal woman’) in only two texts (PF 1795 and PF-NN 3099).