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New Directions in Historiography (Mesopotamia and Israel)

William W. Hallo, Yale University¹

In the field of Assyriology, the term historiography is used in two very different senses. On the one hand it refers to the manner in which the ancients remembered their own past, on the other hand to the theoretical problems raised by our modern reconstructions of that same past. The former sense is implied in the classic study of H.G. Güterbock, who long ago wrote a doctoral dissertation under Benno Landsberger on what they called "die historische Tradition."² The same ground has been gone over many times since then, often in comparative perspective. I will mention here in passing only two titles, one on each side of the debate, and both heavily critiqued upon their appearance: John van Seters' *In Search of History* (1983), subtitled "Historiography in the Ancient World and the Origins of Biblical History," and a new collective volume edited by Millard, Hoffmeier and Baker under the title *Faith, Tradition, and History* (1994), and subtitled "Old Testament Historiography in its Near Eastern Context."³ The latter sense was introduced into the general field of history-writing by such philosophically minded figures as Giambattista Vico, Benedetto Croce, and R.G. Collingwood,⁴ and is reflected in Assyriology in some of the more recent literature, which it will be my purpose to assess here. But first I would like to review my own previous contributions to the debate, proceeding in chronological order not, however, of their appearance but of the topics dealt with.⁵

For my definition of history I turned to the Dutch historian Johan Huizinga,⁶ in this respect following my late colleague Finkelstein,⁷ though correc-

¹ The substance of this paper was presented to the Institut für Orientalistik of the University of Vienna, Prof. Hermann Hunger presiding, October 21, 1996, and to the Oriental Club of New Haven, February 13, 1997. It is here offered to W.H.Ph. Römer in fond recollection of our encounters in Leiden in 1950-51.

² Güterbock 1934, 1938

³ Van Seters 1983; Millard *et al.* 1994; cf. also Cancik 1976 and the reviews by Zevit 1985 and Brettler 1996.

⁴ Collingwood 1993.

⁵ For a spirited defense of some of my positions, see Millard *et al.* 1994, especially Averbek 1994.

⁶ Huizinga 1936.

⁷ Finkelstein 1963:462 and n. 4.

ting him in an important respect. "History," Huizinga had said in 1936, "is the intellectual form in which a society renders account to itself of its past" — not "of the past" which Finkelstein had quoted him as saying.⁸ In practical terms this meant, to me, "an attempt to write ancient history by taking the ancient documents seriously without taking them literally" as it was put in the preface to the history which I co-authored with my Egyptological colleague W.K. Simpson, and which was presented as "not only a history but a commentary on ancient history and historiography."⁹ The new edition of that work holds fast to this motto: it "treats the ancient sources critically but respectfully."¹⁰

The same principle guided my other systematic surveys of Ancient Near Eastern history, Mesopotamian, Egyptian and Israelite,¹¹ and some of its critical turning points.¹² And it was put to the test in shorter contributions as well, beginning with the Sumerian sources. Whether attempting to date the Fara Period, or assembling data on the Gutians, or reconstructing the history of an aristocratic family at Nippur, or setting the letter-prayer of Ninshatapada in its historical context, I invariably combined and collated the evidence of all available sources, archival, monumental and canonical.¹³ In connection with Assyrian historiography, I considered primarily the Assyrian King List,¹⁴ in reference to Babylonian historiography, chiefly the concept of eras.¹⁵ But it was in regard to Biblical historiography that I repeatedly enunciated the principle to which I wish to address myself here: neither to exempt Biblical historiography from standards applied to other Ancient Near Eastern data, nor to subject it to standards demanded nowhere else.¹⁶

When I first offered that formulation, the field of Biblical history was already polarized into two camps that I chose to label — as neutrally as possible — maximalists and minimalists,¹⁷ a terminology which I credited to W.G. Dever (i.a.),¹⁸ though Dever himself has since disavowed paternity,¹⁹ and

⁸ Hallo 1980:6 and 20, n. 27.

⁹ Hallo and Simpson 1971:vi.

¹⁰ Hallo and Simpson, 1997:vii.

¹¹ Hallo 1996 ch. 9 and see the bibliography in *Studies Hallo* (1993) xi-xvi, items 7,8, 10 (ch. 9) and 125.

¹² *Ibid.*, Item 146.

¹³ *Ibid.*, Items 60, 56, 58, 109 (and 141).

¹⁴ *Ibid.*, Items 17 and 81.

¹⁵ *Ibid.*, Items 108, 114, and 127.

¹⁶ *Ibid.*, Items 10:107; 59:4; 91:5; 136:193 and Hallo 1996:314f.

¹⁷ Hallo 1980:3 and 19, n. 13; 1990:193.

¹⁸ Hallo 1980:19, n. 14, referring to Dever *apud* Hayes and Miller 1977:77.

¹⁹ Shanks 1996:35: "How would you define the minimalists and the maximalists?" Dever: "I didn't coin those terms. I'm not sure who did." Shanks 1997 and 1997a still uses the term without attribution.

I know sometimes receive credit for it ²⁰ — or should I say blame. (Another early use of 'maximalist' was by D. Pardee in reference to what he called 'Dahoodic.') ²¹ Speaking very generally, the maximalists are willing to accept the Biblical version of events unless and until falsified by extra-Biblical sources, preferably contemporaneous, bearing on the same matters — a position stated with unusual candor by Bob Becking when he declared: "The dates in the Book of Kings can only be considered as untrustworthy when they can be falsified by contemporaneous evidence." ²² The minimalists, by contrast, demand that the Biblical version of any given event must have extra-Biblical verification, preferably again contemporaneous, before it can be regarded as historical. And they set themselves up as arbiters of what constitutes extra-Biblical verification, as we shall see. No wonder that most scholars prefer to place themselves in the golden mean between these extreme (and irreconcilable) positions, ²³ especially today, when this polarization has gone much further, with the very term 'Biblical history' under fire. ²⁴ What is more to the point here, however, is that today it is no longer so clear that the historiography of Mesopotamia and the rest of the ancient Near East still provides a methodological model for avoiding this kind of polarization. Let me illustrate.

My illustration will be taken from the Sargonic dynasty. As I already put it in 1971, the rise and fall of this dynasty is so much the stuff of later legend that the chief historiographic problem is to peel away the legendary accretions in order to get at the authentic core, the Sargonic kernel at the center. ²⁵ True to the principles already reviewed here, I applied this test to *all* the relevant sources in reconstructing the history of the dynasty. ²⁶ I even utilized glyptic evidence to justify a measure of credence in the traditional version of the death of three of its members as enshrined in the so-called "historical omens." ²⁷ In short, I applied my own dictum that "the literary tradition can be used to fill the lacunae of Sumerian history, but only where the contemporary monuments and archives have provided the framework." ²⁸ But the newer historiography, in

²⁰ Yamauchi 1994:6, referring to Hallo 1990:187 (correct to 1990:193). But see above, note 17, for the earlier formulation in Hallo 1980.

²¹ Dennis Pardee, JNES 40 (1981) 69.

²² Becking 1992:52.

²³ As did I (Hallo 1980:3) despite Yamauchi's characterization of some of my opinions as maximalist (1994:13 and n. 68).

²⁴ Whitelam 1996, and his paper at the SBL meeting, Philadelphia, 1995, for which see Shanks 1997:50f.

²⁵ Hallo and Simpson 1971:54f.

²⁶ See especially *ibid.* 54-68.

²⁷ The point was first made in Hallo 1962:13f., n. 107 (an item inadvertently omitted from the bibliography in my Festschrift) and subsequently elaborated on in ~~IBO~~ 45:773, 110:13f., 117:26, and 140:156.

²⁸ Hallo, Item 36:139, cited Averbek 1994:81, n. 6.

Items

part, rejects this approach. For some of its practitioners, the very term 'historical kernel' is anathema,²⁹ and the only valid sources are contemporaneous ones; the later ones are, at best, testimony to the concerns of the later age that produced them.

This point of view, so redolent of the minimalist position in Biblical historiography, is expressed with greatest force and clarity in the volume *Akkad the First World Empire* which appeared in 1993.³⁰ It is based on a symposium held in Rome three years before that (1990) at the invitation of Mario Liverani, who edited the volume and himself contributed two important articles to it. He summarized what he called Güterbock's "first principle" as contending that "information contained in a literary text could not be accepted unless it was confirmed by another source" and took issue with it as too "loose," implying that another source could confirm a later literary tradition only if it was contemporaneous and not likewise literary. Güterbock's second principle, that of the "historical kernel," was also rejected as leading to "very burdensome results."³¹ The contributors are by no means all of one mind on these issues, but most of them go a long way toward similarly narrow criteria of historicity. This is hardly surprising, since they were selected with that consideration in mind or, as Liverani puts it: "Recently ... new interests and more advanced positions are to be noticed, mostly by the participants to our conference."³²

The over-all result is a kind of sparse, not to say censored version of Sargonic history, almost as if a blue pencil had been run through the histories hitherto reconstructed. There is also, inevitably, a much heavier emphasis on social and economic developments than on purely political or military ones, given the greater reliance on contemporaneous documentation, and its greater abundance.

There is much of value in the book, as is to be expected from any project to which Liverani has put his name. Already in 1973, he had laid down a "memorandum on the approach to historiographic texts,"³³ and in the last decade, he has published a half-dozen syntheses on the history of Mesopotamia (and beyond), whether as author,³⁴ co-author,³⁵ or co-editor,³⁶ and including a massive history of the entire Ancient Near East.³⁷ In the last, I

²⁹ Liverani 1993:6, 42f., 51f.

³⁰ Liverani 1993.

³¹ Liverani 1993b:43.

³² Liverani 1993b:45.

³³ Liverani 1973.

³⁴ *L'Origine della città* (Rome, Riuniti, 1986); *Prestige and Interest: International Relations in the Near East ca. 1600 - 1100 B.C.* (Padua, Sargon srl, 1990).

³⁵ *La Palestina*, with Andrea Giardina and Biancamaria Scarcia (Rome, Riuniti, 1987).

³⁶ *I Trattati nel mondo antico: forma, ideologia, funzione*, with L. Canfora and C. Zaccagnini (Rome, "L'Erma" di Bretschneider, 1990).

³⁷ *Antico Oriente: Storia, Società, Economia* (Rome, Laterza, 1988).

particularly welcome his adoption of the chronological terminology which I had taken over from archaeology for my own history.³⁸ In the present book, I specifically endorse two points from his introductory observation: "[1] that Sargon is still 'pre-Sargonic' (only apparently a paradox!),³⁹ and [2] that the proper Akkadian experience is better represented by the short time lag of Naram-Sin and Shar-kali-sharri.⁴⁰ In this perspective, Naram-Sin with his wide range of enterprises and institutional innovations is no doubt the leading character."⁴¹ My purpose here, however, is neither to endorse nor to question specific details of his reconstruction of Sargonic history, but rather to challenge the volume and some of its individual contributions on the level of methodology, as we are indeed invited to do by its avowedly programmatic, even revolutionary, character.⁴²

Methodologically, I see four major problems with the approach championed by Liverani and followed, more or less, by some of his contributors. (1) The essentially exclusive reliance placed on contemporaneous sources threatens to attribute to them far more evidentiary value than they deserve. (2) The reluctance to use later sources unless verified by contemporaneous ones deprives the modern historian of potentially invaluable evidence from a time which, even though admittedly later than the events reported, is still millennia closer to them than we are. (3) The 'consolation prize' offered to those not ready to discard the later sources in their entirety consists of treating these sources as potential clues to the concerns of the times that produced them — but as often as not that time is here established on the basis of identifying the concerns expressed or implied, and placing them in the continuum of Mesopotamian history — however reconstructed — at the point where such concerns seem most appropriate. This certainly courts the danger of circular reasoning. (4) In general, the winnowing of the sources, and the reconstruction of Sargonic history from what is left, operates, not on the valid assumption that we can hope to know more than the ancient sources *told*, but on the questionable assumption — I would call it a conceit — that we can know more than they *knew*. This is a fallacy worthy of adding to the long list of 'historians' fallacies' catalogued by David Hackett Fischer a quarter of a century ago.⁴³ Let me justify my criticisms in some greater detail.

³⁸ Early/Middle/Late Bronze Age, Iron Age, etc.

³⁹ Cf. Hallo 1992:70, n. 5.

⁴⁰ Cf. my concept of the "high" or "classic" Sargonic period, most explicitly in Hallo forthcoming; previously: Item 90:191, Item 175:255, 1993:19, n. 26. Differently Zhi, 1989:4.

⁴¹ Cf. e.g. Hallo and Simson 1971:60-63, 1997:57-62.

⁴² Liverani describes the new historiography in terms of "a real 'Copernican revolution'," (p. 6 *et passim*). See also below, at n. 68.

⁴³ Fischer 1971.

(1) Contemporaneous written sources come in two of the three categories of cuneiform texts that have long been identified in my taxonomy, namely monuments and archives.⁴⁴ Of royal monuments — as against *private* votive, seal and weight inscriptions — it must be said at once that they are indubitably products of the royal chancery, and as such reflect the royal point of view. They are thus very far from being objective, disinterested accounts of any given reign. Liverani himself seems to admit as much when he notes that "the royal inscriptions are and have always been considered possibly affected by their celebrative purpose,"⁴⁵ or when he lumps them with canons and questions "How to 'read' (for the sake of historical reconstruction) a royal inscription or a later literary text."⁴⁶ Royal inscriptions thus become part of the pattern of 'literature as politics' which has been identified for many periods and cultures — Egyptian by Williams, Hittite by Hoffner, Assyrian by Machinist and more recently Barbara Porter, Israelite by Brettler⁴⁷ — and which is demonstrated for the neo-Sumerian period by Cooper in Liverani's volume.⁴⁸ Even when more or less contemporary with the events they describe, they are not unimpeachable witnesses to them.

As for archival texts, most of these are not, it is true, products of the royal chancery, or instruments of royal propaganda. But they suffer from another disability, their laconic character. It is only the rare archival text which throws explicit light on courtly ceremonial, on diplomacy, on warfare and on other broad affairs of state. As a shining exception we may cite the example of the two letters of Ishkun-Dagan, one invoking (though not naming) the king and queen, the other mentioning the depredations of the Gutians who, according to the historic tradition, were destined to topple the great Sargonic Empire. They are duly cited by Aage Westenholz in the Liverani volume⁴⁹ but they turn out to be the exceptions that prove the rule, for though they have been repeatedly cited and anthologized since they were first published in 1926 and 1932 respectively,⁵⁰ their like has not recurred among the considerable number of letters of Sargonic date available by now.⁵¹ The proverbial character of the first⁵² has even tended to cast doubt on its contemporary status. And though the figure of Ishkun-Dagan has acquired additional reality by the discovery of an indubitably contemporaneous monument, namely his seal impression, in the Yale Baby-

⁴⁴ Cf. simply Hallo and Simpson 1971:154-158; 1997:154-157.

⁴⁵ Liverani 1993b:41.

⁴⁶ Liverani 1993a:7.

⁴⁷ Williams 1964, Hoffner 1975, Machinist 1976, Porter 1993 and 1996; Brettler 1989.

⁴⁸ Cooper 1993.

⁴⁹ Westenholz 1993:158f.

⁵⁰ Thureau-Dangin 1926; Smith 1932. Cf. e.g. Michalowski 1993:27f.

⁵¹ Kienast and Volk 1995. For the Ishkun-Dagan letters see pp. 53-55, 89-94.

⁵² On which see Hallo 1990a:209 and nn. 46-48.

lonian Collection,⁵³ one would hardly want to base the history of the fall of the dynasty on his 'Gutian letter,' at best an ambiguous piece of contemporary testimony - on the contrary, one needs to use it with utmost caution.⁵⁴

Archival texts, of course, are more revealing of management and administration, especially of the royal lands and enterprises, than they are of affairs of state as such. No matter how laconic, here their sheer numbers provide valuable insights, as fully documented in Benjamin Foster's two contributions to Liverani's volume. Of these the first deals with "Management and administration in the Sargonic period," and does so without noticeable concession to any particular philosophy of history.⁵⁵ The second is a bibliography of the Sargonic period running — for all its ostensibly 'select' character — to twelve pages; what is particularly noteworthy about it is that it devotes only half a page to 'historical studies' and almost ten times as much space to 'archival sources and studies,' 'letters' (also archival in my taxonomy), and 'society and economy.'⁵⁶ To the extent, then, that one chooses to equate history with social and economic history, one is justified in exploiting these sources to that end.

(2) But the reverse of that proposition is equally valid: to the extent that one thinks of history as embracing more than just social and economic phenomena, one is required to resort to other than only 'social and economic' sources, i.e., in particular, to later sources. Not to belabor the obvious, I will confine myself here to a single illustration of this point, the very concept of a 'Sargonic period.' How would modern historians have ever arrived at such a concept without the promptings of the native historiography and chronography?⁵⁷ One looks in vain for it in histories written before 1925 by such early synthesizers as Hugo Radau,⁵⁸ R.W. Rogers,⁵⁹ Stephen Langdon,⁶⁰ or even L.W. King.⁶¹ Except for the last, these are the very authorities whom Liverani faults for their indiscriminate utilization of late and early sources.⁶²

And no wonder, given the piecemeal recovery of the Sumerian King List and the relatively belated publication of a first working edition. To quote Thorkild Jacobsen, "The first fragment of the Sumerian King list of any impor-

⁵³ Hallo *apud* Buchanan 1981:445. Cf. the comments of Westenholz 1993:159, n. 3.

⁵⁴ Glassner 1986:40, 50.

⁵⁵ Foster 1993.

⁵⁶ Foster 1993a.

⁵⁷ For the latter concept see most recently Hallo, Item 127:178 and nn. 26f.

⁵⁸ *Early Babylonian History* (London, 1900), esp. pp. 154-175: "Kings of Agade."

⁵⁹ *A History of Babylonia and Assyria I* (London, 1902), esp. pp. 363-367.

⁶⁰ In: *The Cambridge Ancient History I* (Cambridge 1923) 402-434; (2nd ed., Cambridge, 1924) 402-436: "The dynasties of Akkad and Lagash," "The dynasty of Sargon."

⁶¹ *A History of Sumer and Akkad* (London, 1916), esp. pp. 216-251.

⁶² Liverani 1993b:42, n. 3. Cf. also Boscawen 1903:127-132.

tance was published by Hilprecht in 1906, the second by Scheil in 1911..., and lastly, in 1923, came the magnificent Weld-Blundell prism, which in many respects was to close the earlier phase of the study of our document."⁶³ That document, best dated in its present form to the end of the Isin I Dynasty,⁶⁴ postdates the Sargonic period by several centuries. If the fall of Akkad is dated about 2150 BC⁶⁵ and the death of Damiq-ilishu to 1794, it is at least three and a half centuries later. But our own vantage point is more than forty centuries later. Unless we want to go back to some of the wild speculations of the earliest stages of Assyriology,⁶⁶ we have little choice but to begin our structural outline with the help of the native historiography, and then to refine the results in the light of newly recovered contemporaneous documentation. In the process we may well find that the despised literary sources deserve a better reputation as we fathom their true meaning.

To return to the Sumerian King List, it was initially accused of presenting its dynasties as successive, in part because the formula for the change of dynasty came at the end of each dynasty and in part because, in its Nippur recension, the King List added all the regnal years of all the dynasties together to come up with a grand total of regnal years since the Flood.⁶⁷ But I have long argued that the native scribes knew better: compositions like Gilgamesh and Agga or the History of the Tummal show clearly that the first dynasties of Kish, Uruk and Ur were thought of as contemporary even though entered in succession in the King List. The transfer of kingship, though listed formulaically at the end of each dynasty, was clearly *not* implied to have taken place (necessarily) at the end of that dynasty, nor to have correlated (necessarily) with the beginning of the next dynasty; rather, the implication was that the transfer might have taken place anytime *within* both dynasties. But, having once rated inclusion in the King List (by the possession of Nippur or whatever criterion proves to be determining), the dynasty was then treated to a complete record of its members - both those who reigned before the dynasty assumed the hegemony of Sumer and Akkad and those who reigned after that hegemony had been lost again. When seen in this light — and there is nothing inherent in it to militate against this interpretation — the King List gains considerably in credibility.

⁶³ Jacobsen 1939:1.

⁶⁴ See below, n. 69.

⁶⁵ Glassner 1986:41, 53, who accepts my dating of the succeeding Gutian period for which see Hallo Item 56.

⁶⁶ The second (1901) edition of Rogers (above, n. 59) is a good example. Its Sargonic kings are confined to "Shargani-shar-ali cir. 3800" (a.k.a. Sargon), his son Naram-Sin, and his grandson Bingani-shar-ali (pp. 337, 361-367). Rimush and Manishtushu figure but not as members of the Sargonic dynasty (pp. 359-360).

⁶⁷ Hallo Item 29.

(3) The notion that historiographic literature is a valid clue to the period that produced it — indeed that the search for this clue is the only valid reason for studying it — is put in admirably candid fashion by Liverani when he speaks of the veritable 'revolution' in historiography that focused attention on "the search for the author and the environment of the text itself, its purpose, its audience, and the historical knowledge that was really available at that time."⁶⁸ We can see the pitfalls in this position if we revert once more to the Sumerian King List. Even to speak of 'the Sumerian King List' is to beg the question, for a major problem in arriving at its date is to decide whether it was composed by stages over an extended period of time (as, e.g. both the Assyrian King List and the Babylonian Chronicle are widely assumed to have been), or whether it is the product of a single 'author' who composed it at the end of its last dynasty, or at any rate in the course of its last dynasty. The dates proposed for it have therefore diverged by as much as 325 years, from the reign of Utu-hegal (so Jacobsen) to the accession of Hammurapi (so Hallo).⁶⁹ How then do we search for the author, environment, purpose, audience, and historical knowledge of the time of composition of the King List? Do we date the composition on the basis of our assumptions about these factors, or do we reconstruct these factors on the basis of our assumption about its date?

If this example seems unduly fatal to Liverani's programme, let us consider one of his own. He cites five well-known compositions in which the principal Sargonic kings serve as vehicles, in his opinion, for the views espoused by their authors. The first is *šar tamhāri*, "The king of battle." Liverani dates this text to the reign of Shamshi-Adad I, more specifically to a time when the resumption of the Old Assyrian trade with Anatolia, interrupted by Naram-Sin of Assur (and Eshnunna) was a matter of debate. By comparing Shamshi-Adad to Sargon and showing how the difficulties of the trade had been overcome by Sargon, the text was designed to lend support to those who favored its resumption now. Although he acknowledges that all this is no more than a hypothesis, he "believe(s) that the logical procedure of this analysis is the right one."⁷⁰

I beg to differ. This analysis piles assumption on assumption to arrive at a most debatable conclusion. It presupposes a degree of "political debate" at the time which remains to be demonstrated;⁷¹ it appeals to Sargon's inscriptions including their later copies as evidence that he did not cross the Euphrates, and to Naram-Sin's inscriptions as evidence that Naram-Sin was the first to do so, thus (a) ignoring his own strictures against the monuments, (b) treating Sargon's contacts with "lands further far-away in the north-west" as "only indirect or mediated,"⁷² and (c) taking Naram-Sin's claim at face-value in spite of its

⁶⁸ Liverani 1993a:6.

⁶⁹ Hallo Item 29:55, 127:179, 181. For an over-all survey see Chavalas 1994:111, n. 47.

⁷⁰ Liverani 1993b:52-56.

⁷¹ Liverani 1993b:52 and n. 26.

⁷² Liverani 1993b:53.

propagandistic cast. I am not insisting that the attribution of the composition to Sargon of Akkad is necessarily valid; I could as easily, for example, imagine that the composition originally dealt with Sargon I of Assyria and was subsequently transferred to Sargon of Akkad.⁷³ But to assume that the composition dates to the reign of Shamshi-Adad and then to write the history of that reign based on such a dating and such an assumption seems to me to defy logic.

Much the same could be said for the attempts by Liverani to associate the other four compositions with specific dates of composition and political contexts or purposes: "The Curse of Akkad" with the reign of Ishme-Dagan of Isin, "The General Insurrection" with that of Sumu-la-El of Babylon, the Naram-Sin Legend with Hammurapi or perhaps Samsu-ditana, and the geographical treatise generally known as "The Empire of Sargon of Akkad" with Esarhaddon or Assurbanipal of Assyria. In each case his objections to an uncritical and literal reading of the texts are valuable, but his total rejection of any 'historical kernel' leads him to new hypotheses about the compositions that are if anything even more difficult to justify.

(4) The proposition that we cannot aspire to know more than the ancient sources knew, only more than they told was put forward by me long ago in an utterly obscure book review,⁷⁴ but I have repeated it often if briefly⁷⁵ and continue to stand by it. It is of course only a working hypothesis, ready to be abandoned whenever, in a specific instance, it can be disproved. But Liverani turns the whole proposition on its head, effectively implying that we cannot even know — or reconstruct — more than the ancient sources *told*! In his own words: "If the Old Babylonian scribes knew more or less what we also know about the kings of Akkad, if they had access to the same kind of data (namely, the celebrative monuments) that we also have, then the search for the 'historical kernel' must be abandoned."⁷⁶ He seems to be saying that the historical tradition is based solely on the monumental texts and their later copies; that we have already recovered all these texts; that therefore there is nothing more to be said! This position can best be dealt with by confronting it, however briefly, with some alternative interpretations of Sargonic historiography in some other, equally recent publications.

The first of these actually antedates Liverani's by a few years. It is Glassner's dissertation on the fall of Akkad which appeared in print in 1986.⁷⁷ True to its subtitle "*L'événement et sa mémoire*," it makes an attempt to write two entirely separate narratives, one based on contemporaneous data, the other on the tradition, a distinction elsewhere somewhat invidiously labelled as "history

⁷³ Hallo and Simpson 1971:94; 1997:89.

⁷⁴ Hallo Item 149.

⁷⁵ E.g. Item 10:41.

⁷⁶ Liverani 1993b:51.

⁷⁷ Glassner 1986.

and tradition." ⁷⁸ The attempt is a gallant one, but doomed to failure because even the 'historical' narrative has constant reference to elements of the 'tradition.' Like the very concept of a Sargonic period (above, p. ②), the putative regnal lengths of the dynasty, and the notion of "the fall of Akkad" (*šulum Agade*), are borrowed from the tradition as preserved chiefly in the Sumerian King List and in a later monument of Shamshi-Adad I respectively. The inscription of Utu-hegal is used as a significant source ⁷⁹ though clearly a secondary one by his own definition, along with all other copies of royal inscriptions no matter how faithful to their originals. ⁸⁰ In contrast to Liverani, however, Glassner does not attempt to utilize such 'secondary sources' to rewrite the history of their presumed date of composition, nor to rewrite Sargonic history entirely without their help. His act of 'source criticism' must be hailed as a brave attempt to put theory into practice, to see what can actually be achieved when the sources are split into more and less reliable ones. It is thus comparable to those few attempts that have been made in Biblical criticism to actually present the text of documents identified by one or another documentary hypothesis, of which one of the best to my mind remains the effort of Pfeiffer and Pollard to reconstruct the early source in Samuel. ⁸¹

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A completely different approach is taken by Giorgio Buccellati in his study of a single Sargonic inscription, or what he argues persuasively is a single inscription. ⁸² As is true of much of his best work, his study combines archaeology and philology, and it does so here to focus on an inscription of Rimush, son and successor of Sargon, as preserved in Old Babylonian copies from Nippur. Virtually for the first time, ⁸³ and certainly for the first time systematically, he tries to reconstruct the physical appearance of the statue of Rimush from which the late copies of his inscription were presumably made. In this effort he is greatly aided by the scholarly notations inserted in the ancient copies as to where precisely the respective texts were located on the monument. The results of his research over many years are presented in the form of actual drawings as well as schematic transliterations and translations. I would differ with him on some details, notably I would take *mûš* to be a circular base not a plaque given its other attested meanings. ⁸⁴ But the overall result is an important step in the direction of a realistic appraisal of the Sargonic inscriptions and their late copies: the inscriptions are powerful instruments of royal propaganda, and their copies are faithful to an extraordinary degree, even displaying a kind

⁷⁸ Cf. Redford 1970; van Seters 1975.

⁷⁹ Glassner 1986:45.

⁸⁰ Glassner 1986:2f.

⁸¹ Pfeiffer and Pollard 1967.

⁸² Buccellati 1993.

⁸³ But see Hallo Item 2:28, cited by I.J. Gelb in Kraeling and Adams 1960:320 n. 13.

⁸⁴ Hallo Item 50:59.

of scholarly interest in the physical details of the original. This is not as surprising as it might at first seem, given what we now know about the copying of royal monuments, presumably from their originals in Nippur, Ur and perhaps other places, as a portion of the scribal curriculum.⁸⁵ If Buccellati is correct, then the skepticism displayed by the new historiography towards the late copies of Sargonic inscription needs to be tempered.

In a recent article, Steve Tinney confronts the Old Babylonian traditions about the Great Rebellion against Naram-Sin with the evidence of the contemporaneous monuments. Like Liverani he concludes that the traditions "may be used to illuminate the socio-political background of the Old Babylonian period itself, but have no place in the reconstruction of the events of the Old Akkadian period."⁸⁶ However, he rejects any a priori "separation of literary and historical texts on the basis of apparent veracity," implying that each case must be judged on its own merits.⁸⁷

The most recent addition to the list deserves more notice than it has so far received. The Groningen dissertation by Gerdien Jonker is by far the most systematic and ambitious attempt yet to assess the Sargonic period not only in its own right but in the total context of Mesopotamian historiography including the ritual remembrance of the dead.⁸⁸ It succeeds admirably in this purpose, reviewing a huge mass of literature along the way. In brief, its conclusions can be summarized as follows: Memory is of necessity selective; since we cannot remember everything, it is essential that much be forgotten. Within the family, the ancestral cult provides for memorizing up to four previous generations at the most, and if a particularly illustrious distant ancestor is to be included among the honored dead, as e.g. in the case of the second millennium (Kassite) period eponyms of the first millennium (neo-Assyrian and neo-Babylonian) scribal families, then the intervening generations are readily dropped by means of "telescoping." In the royal houses, a comparable process was at work, but the availability of scribes and written records made possible the construction of very lengthy and detailed genealogies beginning in Old Babylonian and Old Assyrian times (from my point of view on the basis of the Amorite or Akkadian/Amorite interest in family relationships).⁸⁹ They were pressed into service in what were explicitly or by implication cultic invocations of the dead in connection with the *kispu*-ritual, the coronation of new kings and possibly other occasions. The *kispu*-ritual and possibly others were conducted in front of the statues of the deceased, and in the case of Sargon and Naram-Sin, the cult of their statues is attested as far away as Mari and as late as neo-Babylonian times. Historiography may thus be said to have followed ritual: to the extent that the veneration of

⁸⁵ Sjöberg 1976:166 and nn. 26f.; Klein 1986; Yoshikawa 1989.

⁸⁶ Tinney 1995:14.

⁸⁷ Tinney 1995:2.

⁸⁸ Jonker 1995.

⁸⁹ Hallo, Item 127:180-183.

royal predecessors and ancestors was constantly winnowed out to meet the limitations of memory, so was the retelling and recopying of narratives about them, with lesser royalty either forgotten entirely or their tales reattributed to the more enduring names.

Thus Jonker's thesis, like Liverani's, casts serious doubt on the historicity of the historical tradition, or at the very least on the accuracy of its particular attributions. Like Liverani's, it proposes an alternative context for that tradition. But whereas Liverani's alternative has a suspiciously modern ring to it, in that it presupposes a political atmosphere of spirited debate among an educated citizenry about the major issues of the day,⁹⁰ Jonkers' is much less dependent on hypothesis. Rather it rests on the surer ground of the Mesopotamian cult in general, and the cult of the dead in particular, the latter subject well illuminated thanks to such recent studies as those of Tsukimoto, Lewis, Scurlock and others.⁹¹

In conclusion, a few words may be ventured about the comparable situation in Biblical historiography. Here the United Monarchy and particularly that portion of it which belongs to the Davidic Dynasty may well be said to play somewhat the same role as the Sargonic Dynasty in Mesopotamian historiography. As long as the Hebrew Bible was the only source for reconstructing Biblical history, the historicity of David and Solomon was not a subject for debate. Even after the rediscovery of Near Eastern antiquity, that situation continued unchanged for a long time in spite of the total silence of the epigraphic sources with respect to these two kings. But skepticism in this regard grew in tandem with that about the historiographical validity of the Biblical text. One can perhaps read it off best in the work of J. Alberto Soggin, who has gradually moved the starting point of Israelite history, and of Biblical historiography, from the period the United Monarchy⁹² to the Exilic period,⁹³ though retaining a more flexible position in works directed at more general readerships.⁹⁴

As noted at the outset, the demand for extra-Biblical verification has replaced the test of inherent plausibility where Biblical historiography is concerned. In this light, it would appear that even this severe test had recently been passed with the discovery of an inscription mentioning "The house of David" in parallelism with "the king of Israel," and in a context which clearly seemed to point to a triumph over both of these dynasties by an Aramaean opponent around 800 BC,⁹⁵ most likely to be identified as Hazael.⁹⁶ It provided yet

⁹⁰ Liverani 1993b:46-48 et passim.

⁹¹ Cf. Hallo, Item 144 with previous literature.

⁹² Soggin 1977, 1978.

⁹³ Soggin 1991.

⁹⁴ Soggin 1993.

⁹⁵ Biran and Naveh 1993.

another independent extra-Biblical witness to the Divided Monarchy of Israel and Judah to add to the many previously available. More significantly, for the first time it furnished epigraphic evidence that the southern dynasty could be designated after its founder and that this founder was not a figment of a greatly posterior imagination but already firmly entrenched in the terminology of the late ninth century. The chance that he was an invention of this century, and not a reality two centuries earlier thus became ever more remote.⁹⁷

The minimalist opposition was not quite silenced by this discovery, but the quandary in which it found itself can be gauged by the lengths to which it went to avoid drawing the obvious conclusions from the new evidence. It was suggested that the fragmentary nature of the monument made any interpretation of its over-all significance hypothetical or at least premature - a point considerably weakened by the discovery, the following season, of a substantial new fragment which clearly belonged to the same monument even if it did not actually join it.⁹⁸ It was argued that since the words for "House" and for "of David" were not separated by a word-divider, the reference had to be to a toponym, an argument hardly worthy of refutation. In utter desperation, it was hinted that the monument had been 'planted' in the excavation — if not by the excavator himself then behind his back. This gratuitous insult was answered in a most convincing way when André Lemaire, the respected epigrapher of the École Biblique in Jerusalem, found the identical idiom in another monument by the simple device of restoring one missing letter.⁹⁹ The monument in question is the stela of Mesha, king of Moab, contemporary with the Tell Dan stela though from the other side of the Jordan. It has been known since 1868 and on display in the Louvre for all to see since 1873.¹⁰⁰ No one could possibly suggest that it was a recent forgery.

What I am suggesting then is this. Methodologically, it continues to make sense to treat Mesopotamian history and Israelite history alike — to exempt neither from criticism, to expose neither to unreasonable tests of authenticity. Absent an overabundance of documentation such as applies to some much more recent periods, the historian of antiquity has no alternative but to use every scrap of evidence available — making allowances for its biases, for the intentions of its presumed authors and the expectations of its presumed audiences in order to reconstruct a remote past. To do otherwise is to commit and compound the very error of which the ancient historiographers and chronographers stand accused by the skeptics, namely injecting the concerns of our own time into the recital of past events.

⁹⁶ Margalit 1994.

⁹⁷ Cf. now similarly Rainey 1996:546.

⁹⁸ Biran and Naveh 1994. See also the excellent photo in Shanks 1996:34, and the discussion *ibid.* 35f.

⁹⁹ Lemaire 1994.

¹⁰⁰ See the translation by W.F. Albright in ANET 320f. and the recent study by Stern 1991:19-56.

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Abbreviations

Hallo Item	1, 8	=	Heritage; Civilization and the Jews
	2	=	Early Mesopotamian Titles.
	6	=	The Ancient Near East: a History.
	8	=	s. unter Nr. 1
	10	=	The Book of the People.
	17	=	JNES 15:220-225.
	29	=	JCS 17:52-57.
	36	=	JCS 20:133-141.
	45	=	JAOS 88:772-775.
	50	=	JCS 23:57-67.
	56	=	RLA 3:708-720.
	58	=	JNES 31:87-95.
	59	=	<i>Perspectives in Jewish Learning</i> 5:1-12.
	60	=	Or 42:228-238.
	81	=	<i>Eretz-Israel</i> 14:1*-7*.
	90	=	AnSt. 30:189-195.
	91	=	SIC 1:1-26.
	108	=	Bulletin of the Society for Mesopotamian Studies 6:7-18.
	109	=	Tadmor and Weinfeld 1983:9-20.
	110	=	Gorelick and Williams-Forte 1983:7-17 and pl. xii.
	114	=	JANES 16-17:143-151.
	117	=	Bible Review 1/1:20-27.
	125	=	History of the World, vol. I, ed. John W. Hall.
	127	=	Studies Sachs 175-190.
	136	=	JAOS 110:187-199.
	140	=	Studies Tadmor 148-165.
	141	=	Studies Garelli 377-388.
	144	=	Studies Talmon 381-401.
	146	=	Hallo 1992a.

- 149 = *Studies in Bibliography and Booklore* 3:71-73.
175 = *JAOS* 101:253-257.

Addendum

The important new work by Joan Goodnick Westenholz, *Legends of the Kings of Akkade: The Texts* (= *Mesopotamian Civilizations* 7) (Winona Lake, IN. Eisenbrauns, 1997) appeared too late to be taken into account here.