On Writing the History of Southern Mesopotamia*

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In his book Babylonia 689–627 B.C., G. Frame provides a maximally detailed history of a specific region during a closely delimited time period, based on all available sources produced during that period or bearing on it. This review article critiques the methods used to derive the history from the sources and the conceptual framework used to apprehend the subject of the history.

Babylonia 689–627 B. C., the revised version of Grant Frame's doctoral dissertation, covers one of the most turbulent and exciting periods of Babylonian history, a time during which Babylon successively experienced destruction and revival at Assyria's hands, then suffered rebellion and siege, and lastly awaited the opportunity to overthrow Assyria and inherit most of Assyria's empire. Although, as usual, the preserved textual sources cover these years unevenly, and often are insufficiently varied in type and origin (e. g., royal or nonroyal, Babylonian or Assyrian), the years from Sennacherib's destruction of Babylon in 689 to the eve of Nabopolassar's accession in 626 are also a richly documented period. Frame's work is an attempt to digest all of the available sources, including archaeological evidence as well as texts, in order to produce a maximally detailed history. Surrounding the book's core, chapters 5–9, which proceed reign by reign through this history, are chapters focussing on the sources (ch. 2), chronology (ch. 3), the composition of Babylonia's population (ch. 4), the structure of the Babylonian state (ch. 10), and Babylonia's relations with Assyria and Elam (ch. 11). The whole is framed by brief introductory and concluding chapters, and topped off with six useful appendices.

This review comes too late to serve the purpose of assessing Frame's book for the benefit of other potential buyers and readers. Moreover, since the book's publication there have appeared several other studies

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having to do with the same region and period, as well as new texts or new editions of texts,[1] so that it is not now possible to consider Frame's work without having in mind these more recent works — which will no doubt be utilized in preparing the second edition now projected.[2] I shall therefore use the occasion of writing this critique to highlight problems of methodology, and problematic assumptions, which are little affected by the publication of additional data. It should be noted that although my criticisms specify Frame and his work as the objects of reference, I am primarily criticizing practices of history writing, not this particular practitioner.

Frame undertakes his historiographic enterprise unencumbered by either overt ideology or theory, and he is thus guided by many tacit assumptions but virtually no critical methodology. While it is plain that he means to write an objective history, he neither states what a "political" history is[3] — apparently taking the definition of his genre for granted — nor does he clarify how he conceives the historian's task. His principles for the interpretation and use of textual sources are nowhere articulated, despite the attention paid in the second chapter to the nature and reliability of each type of source.[4] He typically proceeds by assembling bits of data from all his various sources and sequencing them in chronological order, like beads of information on

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[1] Notable in the latter category is Frame's own Rulers of Babylonia from the Second Dynasty of Isin to the End of Assyrian Domination (1157—612 B.C.), vol. 2 of The Royal Inscriptions of Mesopotamia, Babylonian Periods (1995), hereafter RIMB 2. In the former category, part of the same ground Frame covers in the book under review is brought into closer focus in B. N. Porter, Images, Power, and Politics: Figurative Aspects of Esarhaddon's Babylonian Policy (1996). The material available for study has, further, been increased and enhanced by the continuing publication and re-edition of texts in the State Archives of Assyria series, while several volumes of the State Archives of Assyria Studies touch on matters relating to Frame's topic.

    I wish to thank M. P. Maidman for reading a preliminary draft of this review article and checking references not available to me in Colorado; in addition I am grateful to him for many stimulating conversations which have helped me hone my discussion of various points. No errors of fact or interpretation nor any opinions which may be found herein are to be laid to his charge.

[2] Information courtesy of G. Frame, personal communication (May 1997).

[3] A. Livingstone raises a similar query in his review of Frame's work (BiOr. 52 [1995] 447), but from a standpoint different from my own.

[4] To state that "criteria for the use" of one or another category of source "have been established" by other scholars (pp. 8, 10, with nn. 15, 24, referring to the works of J. A. Brinkman, M. Cogan, and A. T. E. Olmstead) cannot replace explicit articulation of the relevant criteria, especially when they are not clearly manifest in the author's own use of those categories of sources.

threads of time, as if optimal exploitation of sufficiently full sources might yield an unbroken necklace of historical narrative. He appears to be unaware of the critique voiced a generation ago by M. Liverani,[5] namely that inscriptions and documents are first and foremost sources of information about themselves, and evidence for the purposes they were produced to serve,[6] not − in the first instance − sources of information about, or evidence for, what they appear to record or communicate. That is, the text must be understood as an artifact, not simply read as a "record of the past."

To "assemble, assimilate, and coordinate the available evidence in order to create a history," as Livingstone puts it when praising Frame's book for doing just that (BiOr. 52, 447), is at once inadequate to accomplish the historian's task, and otiose as a mode of presenting the historian's results. Why should we read in Frame's history that Tammarītu and his fellow refugees from Elam "came before the Assyrian king, crawling naked on their bellies" (p. 185) when we can read that in the annals of Ashurbanipal cited in the footnotes? Why does the discussion of the siege of Babylon and other cities during 650−648 consist principally of quotations from several types of textual sources describing conditions during that siege (pp. 150−153)? This is merely repeating what the sources say; collecting such quotations may be necessary or desirable, but it is not sufficient. And the insufficiency cannot be made up by filling the pages with inadequately grounded speculations, as Frame tends to do − expatiating, for instance, on Nabû-bēl-šumāti's possible motives and goals (p. 177), or on what sort of person Kandalānu might have been and why Ashurbanipal chose to appoint a person of that sort as king of Babylon (p. 195). It is naïve to compose a historical narrative by paraphrasing the assembled sources, patching up the gaps and uncertainties by guessing at the missing episodes and at the actors' motives, and supplying causal explanations derived from the reported outcomes of events. But this, notwithstanding his awareness that texts may misrepresent reality by making false claims or by presenting a one-sided view (e. g., pp. 90, 109), and that a text's content may be determined by its standpoint or by its literary character (e. g., pp. 132, 140, 248), is what Frame has done. To his credit, Frame is usually careful to identify the sources for each "fact" and

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[5] Or. 42 (1973) 178−194. The theme has been taken up recently by M. Van De Mieroop in BiOr. 44 (1997) 285−305.

[6] See M. W. Stolper, Cuneiform Texts from Susa. in: The Royal City of Susa (1992) 255, for a fuller statement of this point.

each conjecture at every step of the way, so that the reader can follow his path in constructing the historical narrative, without first having to reconstruct the process of deriving that narrative from inadequately specified sources as many authors make it necessary for the reader to do (some chapters of the revised Cambridge Ancient History are good [bad] examples of this). But there is, I think, a better way to write history, while still explicitly indicating where the data come from.

Part of the philological work that underlies the possibility of historical inquiry is to determine what the texts say, on the level of language and translation. And let it be noted that Frame's competence in reading the texts, as a cuneiformist and philologist, is not in question. Part of the historian's work, however, involves determining why a text says what it does, and moreover why the text exists at all.[7] Once having understood a text's message on the philological level, one should inquire not only whether what the text says is "true," or which way it may be biased, but why that particular message was communicated, by whom and for whose eyes or ears, and why it was conveyed in whatever specific form was chosen for it.[8] One should − let us say − "read between the lines," without of course reading into the sources what is not arguably there, and without indulging in groundless speculation; for logical inference is not identical to speculation. Inferences must be adequately grounded, and should possess convincing explanatory power. Ideally, they should be testable hypotheses. Thus, one uses the array of available evidence to infer historical reality from its manifestations; then, if possible, one checks those inferences against other types of evidence (or through different procedures of analyzing the same evidence).

Sibling kings

An example of how "reading between the lines" would somewhat alter the picture, perhaps revising it closer to reality, arises in assessing the relationship between Ashurbanipal and Šamaš-šuma-ukīn. Frame repeatedly observes that there is no evidence for conflict between the two brothers prior to Šamaš-šuma-ukīn's rebellion in 652 (pp. 95, 108, and 130); rather, both acknowledge each other with expressions of

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[7] Note M. Van De Mieroop's critique of prevailing tacit assumptions regarding the nature of the textual record, in: Why Did they Write on Clay?, Klio 79 (1997) 9.

[8] See Liverani's more subtle discussion, in Prestige and Interest (1990) 26, of the necessity to decipher the "code" (or codes) of values and ideas, through which communication is effected, in order to understand the messages encoded in the texts.

friendship in their inscriptions (p. 108, with n. 36), and extant correspondence indicates that Šamaš-šuma-ukīn accepted his brother's authority (p. 111). So Frame tries to identify features of the brothers' relationship that Šamaš-šuma-ukīn might have resented, and provocations that might have prompted his revolt, in order to explain why Šamaš-šuma-ukīn rebelled against Ashurbanipal (pp. 109–110, 114, 131). As if that were so mysterious! For him *not* to have rebelled would require explanation. Contrary to Frame's standpoint, the revolt itself is an "unequivocal indication that Šamaš-šuma-ukīn was discontented" prior to rebelling (p. 130), and that he found his subordination to his brother intolerable! It should be obvious that an effect, here for instance those expressions of brotherly friendship, need not be identical in nature to its cause. Instead of expecting that the texts should provide explicit evidence for hostile feelings between the two brothers before the outbreak of open hostilities, we should assume that inscriptions, letters, and documents will necessarily express the official, "correct" relationship – not anyone's real sentiments. In fact, perhaps the overt emphasis on harmonious, friendly relations is an effect of the brothers' rivalry and thus actually manifests their public suppression of mutual hostility.

This understanding of the evidence is suggested in particular by the pair of inscribed stelae found in the Ezida temple in Borsippa, one portraying Ashurbanipal and the other portraying Šamaš-šuma-ukīn.[9] Frame takes these two stelae to reflect good relations between the two brothers (p. 108). Indeed, each brother refers to the other in friendly terms, and Ashurbanipal's inscription includes good wishes for Šamaš-šuma-ukīn (RIMB 2, B.6.32.14:31, 46–48, and B.6.33.3:12, 27). But each brother claims to have accomplished exactly the same repair of the Ezida's enclosure wall (B.6.32.14:33–36 and B.6.33.3:13–16). If there were no rivalry between them, why would they vie for credit?[10]

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[9] The two stelae are illustrated in figs. 2 and 3 of the book under review. The texts of both stelae have now been re-edited by Frame in RIMB 2: Ashurbanipal's Ezida stela is B.6.32.14 and Šamaš-šuma-ukīn's is B.6.33.3.

[10] The fact that both brothers claim credit for one and the same achievement means they are vying for credit, not sharing it; in other words their twin claims are a symptom of rivalry, not of concord. This is a dynamic with which most close siblings and their parents are familiar.

What then would constitute evidence of concord? One example of an artifact which is broadly comparable to the two Ezida stelae, but evinces harmony rather than rivalry, might be the stela naming both Adad-nārārī III and his mother Semiramis, recently re-edited by A. K. Grayson (RIMA 3 [1996] A.O.104.3).

Moreover, Ashurbanipal here forgoes the titles "viceroy of Babylon" and "king of the land of Sumer and Akkad," which he usually bears in his Babylonian inscriptions,[11] with the result that the symmetry between Ashurbanipal's Assyrian titulary and Šamaš-šuma-ukīn's Babylonian titulary is as close as it can get: Ashurbanipal bears his usual array of titles as king of Assyria in his Ezida stela (B.6.32.14:2−4), while Šamaš-šuma-ukīn, in *his* Ezida stela, is titled "[mighty(?)] king, [king] of Babylon, king of the land of Sumer and Akkad" (B.6.33.3:2−3)[12]; yet Šamaš-šuma-ukīn must still acknowledge Ashurbanipal as "king of the world" (B.6.33.3:12), and Ashurbanipal still represents his brother's kingship over Babylon as dependent on his own prerogative to grant it (B.6.32.14:32). Thus the texts of the stelae make the two brothers as equal as possible within the framework of their actual relationship. The images on the stelae correspond to the titulary, in depicting Ashurbanipal as an Assyrian king and Šamaš-šuma-ukīn as a Babylonian king.[13] Altogether it seems that the fabrication of one inscribed stela apiece crediting each ruler with the same minor repair, and adjusting their royal titles toward symmetry, was a means of appeasing two siblings in competitive conflict and sublimating their strife, not an expression of brotherly comity! Brinkman's characterization of the 16 years prior to Šamaš-šuma-ukīn's revolt as "*ostensibly* peaceful" (CAH² III/2 [1991] 48; emphasis added) is apt. The question is not so much why Šamaš-šuma-ukīn revolted − he evidently calculated that he could find enough allies to succeed, while being sufficiently fed up with Ashurbanipal's overlordship to risk failure − but why he waited until 652. The death of the queen mother

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[11] Compare, for example, RIMB 2, B.6.32.1:4−5. In Ashurbanipal's Ezida stela, the titles "viceroy of Babylon" and "king of the land of Sumer and Akkad" are borne by Esarhaddon (B.6.32.14:15−16).

[12] He bears these and other titles and epithets in various combinations in his other inscriptions (B.6.33.1, 2, 4, 5).

[13] See the description given by J. Börker-Klähn, Altvorderasiatische Bildstelen und vergleichbare Felsreliefs (BagF 4, 1982) nos. 225 (Ashurbanipal) and 226 (Šamaš-šuma-ukīn), on p. 216 (the statement there about the stelae's provenance should be corrected, in accord with Frame's commentary on both in RIMB 2, 217 and 252).

In this vein it may be noted that Šamaš-šuma-ukīn's stela appears to have been much less artfully wrought than Ashurbanipal's, though it is difficult to be certain due the damage inflicted (following the rebellion) on the former. Various asymmetries between the texts of the two inscriptions may be observed, as well as differences from other inscriptions in the relevant categories, which may or may not be significant.

Zakūtu, who had bound Šamaš-šuma-ukīn to Ashurbanipal by oath (SAA 2, 8:3), might have provided the occasion by removing an obstacle to rebellion.[14]

Quantifying dated archival documents

Just as taking what texts say at face value is an invalid interpretive approach, taking any body of evidence at face value is likely to yield invalid results when the factors that produced or preserved that evidence are not considered. Frame often uses the number of dated legal and economic documents extant for a given period of time as a gauge of economic activity and prosperity (pp. 13, 51, 62, 78, 114–116, 193, 198, 200, and 261; also Appendix A), a method developed by Brinkman.[15] This method surely has some validity in principle, but it must be recognized that the number of documents extant per year (especially for the period of concern here) depends on what factors resulted in those documents being preserved, deposited, or discarded over generations, even centuries, after the year in which they were written, not on what factors resulted in their being written in the first place. Frame acknowledges that accidents of preservation and discovery affect the reliability of the documents-per-year gauge of economic activity (pp. 13, 51 with n. 114, 198 n. 35). By and large, those "accidents of preservation" are "accidents" that occurred within the microhistories of the individuals and families (sometimes institutions) whose documents are at issue. For example, the archives of the Ea-ilūta-bāni family (with affines), studied by F. Joannès, contain documents going back to the early seventh century, but the processes that led to the deposition of these archives (as recovered and reconstructed) did not conclude until the early fifth century; similarly with the Egibi archives and

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[14] So far as I have been able to discover, the date of Zakūtu's death is unknown and it has not hitherto been suggested that the timing of Šamaš-šuma-ukīn's rebellion may have depended on this event. H. Lewy assumes that Zakūtu was alive in 652, but she has to assume so, in order to harmonize Assyro-Babylonian history with the stories told by Diodorus Siculus about Semiramis, who is translated (by Lewy) into Nitokris = Naqiʾa/Zakūtu for the purposes of working in the episode about the revolting son (JNES 11 [1952] 271 and 282–283).

[15] J. A. Brinkman/D. A. Kennedy, Documentary Evidence for the Economic Base of Early Neo-Babylonian Society: A Survey of Dated Babylonian Economic Texts, 721–626 B.C., JCS 35 (1983) 1–90; Brinkman, Settlement Surveys and Documentary Evidence: Regional Variation and Secular Trend in Mesopotamian Demography, JNES 43 (1984) 169–180 (esp. p. 177). See also Brinkman, ch. 21 in CAH² III/ 2 (1991) 6, 19, 29, 39, 50, 61–62, and 64.

numerous other groups of tablets.[16] A very different example is the early seventh-century hoard of tablets belonging to Bēl-ušallim son of Lēʾēa, of Babylon.[17] These tablets are preserved because, apparently, Bēl-ušallim stashed them away in a jar during the early years of Nabopolassar's struggle against Assyria, but never retrieved them; the tablets Bēl-ušallim put in the jar were promissory notes (which Frame incorrectly terms loans, pp. 115–116) for debts owed to him, which he no doubt hoped to collect when things settled down. His failure to retrieve his tablets and collect the debts, *not* the economic activity from which those debts arose, bequeathed this tablet hoard to Assyriologists. These instances illustrate that the relationship between the number of tablets actually produced during a given year and the number of *extant* tablets dated to that year is indeed largely accidental.

Tribes and ethnogenesis in southern Mesopotamia

The foregoing paragraphs address methodological issues regarding the use of sources and evidence. A similar issue involves the use of terms. Words and names encapsulate bundles of associations and assumptions connected with the entities they denote. Insofar as particular words and names are used as terms labelling entities that are the subject of investigation, it is incumbent on the investigator to examine each such term, identify the associations it evokes and the assumptions it tends to entail, and clarify its meaning for the purposes of the inquiry. Frame's subject is "Babylonia," an entity he terms a "state" though it was "not a unified, homogeneous state," and he divides the groups composing the population of this state into the categories "tribal and non-tribal," the former category consisting of entities labelled "tribes" and the latter consisting of "residents of urban centers," labelled Akkadians (p. 32). His fourth chapter is devoted to describing these diverse groups constituting "the Babylonian people," and he does so — appropriately — using indigenous labels and criteria for the

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[16] On the transmission and deposition of the Ea-ilūta-bāni archives, see F. Joannès, Archives de Borsippa: La famille Ea-ilûta-bâni (1989) 121–126, and my review of this work in Aula Orientalis 12 (1994) 105–120 (esp. pp. 108–111). Regarding the deposition of the Egibi archives and of one of their principal components, the documents of Iddin-Marduk, see C. Wunsch, Die Urkunden des babylonischen Geschäftsmannes Iddin-Marduk, Band 1 (Cuneiform Monographs 3a, 1993) 7–11 and 77–85.

[17] L. Jakob-Rost, Ein neubabylonisches Tontafelarchiv aus dem 7. Jh. v. u. Z., Forschungen und Berichte, Staatl. Museen zu Berlin 10 (1968) 39 ff.

most part (the use of the name "Babylonia" is a glaring exception; see further below). This portrait of southern Mesopotamia during the period in question as a disunited state comprising tribal and urban elements is fairly standard.[18] However, its validity for explaining the region's political history is compromised by its reliance on terms that are problematic because they involve unexamined assumptions, as well as on concepts that are problematic because they are unarticulated. Notable are the term "tribe" and the concept of ethnogenesis. Like so many who write on ancient Near Eastern history, Frame uses the word "tribe" rather promiscuously without adequately investigating its meaning and determining whether it actually applies to the entities labelled "tribes," and he discusses the emergence of distinct population groups without articulating what processes of ethnogenesis might have been involved.

It seems to be taken virtually as an axiom that Arameans and Chaldeans were "tribal" peoples. Proceeding from this assumption, since Chaldeans are identified in the texts as members of groups with names of the form Bīt-PN, any group called Bīt-PN is considered to be a "tribe"; likewise any group whose name is attested under the rubric "Aramean" is considered to be a "tribe." It may be correct to label these Chaldean and Aramean groups tribes, depending on how the term is defined. But it is not defined. Rather, the words "tribe" and "tribal" are used as if they *explained* the entities to which they are applied. These words are freighted with associations and assumptions regarding the nature of entities labelled "tribes" − which may not be true of the groups so labelled here − and therefore their use conditions how the evidence is perceived.

To Frame, the concept "tribe" evidently connotes a non-sedentary, non-agricultural mode of existence and a non-urban, non-state form of socio-political organization (pp. 32 ff.). These connotations mold his description of the Chaldeans and Arameans even as he adduces evidence contradicting such a characterization of those groups. So, for instance, he states that "from earliest times the Chaldeans were at least partially settled," and that some of them "turn[ed] to agriculture for their livelihood," although they "maintained their tribal structure" (p. 37). Turned to agriculture from what? Is there any evidence that the Chaldeans were ever predominantly *not* settled? Why put the case

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[18] It has been developed in detail by Brinkman in several works (see CAH² III/2, ch. 21, section I [pp. 1−23], and references there) and is repeated in textbooks, most recently A. Kuhrt, The Ancient Near East c. 3000−330 B.C., vol. II (1995) 575.

this way, unless it is because these people have been labelled "tribal" and tribal people are assumed to be intrinsically non-sedentary and non-agricultural?

Similarly, although tribal interests and identity are supposed to be distinct from those of the state (pp. 32, 237), Chaldean "tribes" emerge as the motor of Babylonian politics and policy (p. 43; see also the last sentence on p. 261). Indeed, all the evidence Frame adduces for Chaldean society and lifestyle bespeaks a settled, urbanized people who practice agriculture, animal domestication, and commerce, and who are deeply involved in the affairs of the "state" (the Babylonian polity or polities). Either the concept of tribe that is used here is wrong, or the Chaldeans are not tribes – or both. While less information is available for characterizing the more numerous Aramean groups, the same discrepancy between tacit assumptions and explicit evidence is occasionally manifest in Frame's description of the Arameans (some of whom too are settled in "towns and cities of their own" [p. 45]).[19]

Kinship is another concept underlying Frame's description of Chaldean and Aramean tribes. As noted above, Frame correctly uses indigenous labels and criteria to identify distinct groups, and those that indicate kinship, or genealogical descent, are the following.[20] Arameans were grouped into named descent groups, membership in which is indicated by qualifying an individual's name with the gentilic form of the descent group's name, thus "so-and-so the (name of descent group)-*aya*". Chaldeans were grouped into descent groups called by names of the form Bīt-PN, and individual members of these descent groups are called "so-and-so son of PN", where PN is the name of an eponymous ancestor (not of the individual's father). And "Akkadians," described by Frame as the non-tribal "descendants of older groups" who were mostly "residents of urban centres" (pp. 33, 32),

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[19] Incidentally, in the context of describing the emergence of new population groups, expressions such as "part(ial)ly settled" (pp. 37, 45) are rather odd, producing a vague impression of tribal people gradually settling on the land, illuminated sporadically by texts produced by the states on whose territory they settle, like dust in the sunshine.

[20] See Frame, pp. 34, 37–38, 46. Brinkman emphasizes the necessity of analyzing the population groups in terms that correspond to those provided by the sources; see his description and distinction of Aramean and Chaldean groups, on the basis of kinship organization and political organization, in "Notes on Arameans and Chaldeans in Southern Babylonia in the Early Seventh Century B.C.," Or. 46 (1977) 306–307.

were grouped into descent groups called by ancestor names of various types; descent from such groups was indicated by appending "son of PN," where PN is the ancestor name, to an individual's name (or to his name and his father's name). How different is the mode of identifying Akkadian descent group membership from the mode of identifying Chaldean descent group membership?[21] How different in kinship structure were Akkadian descent groups from Chaldean ones? If the Chaldean and Aramean descent groups are "tribes" because they are organized and identified on the basis of kinship, are not the Akkadian descent groups also "tribes"? Clearly kinship-based identification and organization do not constitute a sufficient criterion for determining whether a particular group is a tribe, or whether a particular society is tribal.

As the term appears to be understood by Frame and many other Assyriologists, all the defining features of "tribe" other than kinship-based organization are negatives − non-sedentary, non-agricultural, non-urban, non-state, as discussed above. Thus, the concept of tribe functions in structural opposition to the concepts of city and state. Such an opposition between types of peoples, and between forms of socio-political organization, certainly emerges in many of the textual sources produced by cities and states of the period, and therefore possesses some validity at least as a feature of contemporaneous ideology. That does not obviate the necessity of defining the terms of analysis, of course. Regarding the term "tribe", in the estimation of Morton Fried, this word barely misses being the "single most egregious case of meaninglessness" in anthropological vocabulary.[22] Fried delivered this assessment in his introduction to a book of essays, published over three decades ago, devoted to elucidating the problems both of defining the term "tribe" and of defining particular "tribes" according to

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[21] Frame himself suggests that the "Akkadian" mode of identification with a "clan" called after an ancestor's name was developed "in imitation of the tribal structure" (p. 34). This idea was also put forward by Brinkman, in Prelude to Empire: Babylonian Society and Politics, 747−626 B. C. (1984), with n. 38. Brinkman suggests there that the "evolution of larger kin-based units" in the settled population during the 7th century is analogous to the "development of tribal organization under circumstances of contact with politically more advanced peoples" (n. 38) with reference to the work of M. H. Fried; this interesting proposition (see also n. 26 below) begs the question of what "tribal organization" is.

[22] "On the Concepts of 'Tribe' and 'Tribal Society'", in: (ed.) J. Helm, Essays on the Problem of Tribe (Proceedings of the 1967 Annual Spring Meeting of the American Ethnological Society (1967) 4−5. (The winning word is "race.")

reliable and consistent criteria. Indeed, these essays, together with the entry on tribal society in the approximately contemporary International Encyclopedia of the Social Sciences,[23] yield no clear consensus on what tribes are, and the term does not seem to have entirely shed its meaninglessness during the generation since they were published.[24] Some elements of usage and meaning do become clear from perusal of those discussions, however. First, tribes are not by definition non-sedentary and non-agricultural; this notion, which appears to be commonly held among those who study ancient Near Eastern history, presumably is an unconscious extrapolation from the existence of nomadic pastoralist "tribes" in more recent Near Eastern history. Second, kinship or shared descent (whether biological or consensual) is normally central to tribal identification and organization[25] − though this is not a feature exclusive to tribes. Finally, the opposition between tribe and state, on the real as well as on the conceptual level, has received empirical support and a degree of objective definition, but in the following dynamic sense: tribes come into existence and acquire definition in response to state formation, especially in response to state aggression; thus tribes and states are "alternative products of a shared history and so counterparts in ... a single social system."[26] That idea suggests a fruitful direction for investigating the emergence of distinct Aramean and Chaldean groups in late second and early first millennium B. C.

And, though that direction cannot be pursued here, this consideration leads us to the problem of ethnogenesis. How do distinct groups like the Chaldeans come into being?

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[23] I. M. Lewis, in the International Encyclopedia of the Social Sciences (hereafter IE-SocSci.), ed. David L. Sills, vol. 16 (1968) 146−150.

[24] See G. Emberling, Ethnicity in Complex Societies: Archaeological Perspectives, Journal of Archaeological Research 5/4 (1997) 295−344, esp. 297, 306.

[25] See for instance G. E. Dole, Tribe as the Autonomous Unit, in: Essays on the Problem of Tribe (1967) 83−100, esp. p. 95, and Lewis, Tribal Society, in: IESocSci. 16, 146.

[26] N. L. Whitehead, "Tribes Make States and States Make Tribes," in: (ed.) R. B. Ferguson/N. L. Whitehead, War in the Tribal Zone: Expanding States and Indigenous Warfare (1992) 129. Note also the fuller statement on the same page, " 'tribes' may be conceptualized as a recurrent and universal residual political phenomenon of both state formation and state expansion, achieving identity and forming boundaries only as a consequence of this external force." This concept was first formulated by M. Fried, in the essay cited above: after a detailed critique of prevailing definitions of tribe, he concluded that tribes seem generally to be "secondary phenomena" and that tribalism is "a reaction to the formation of complex political structure rather than a necessary preliminary stage in its evolution" (above n. 22, p. 15).

Interestingly, Frame avoids the term "ethnic group", preferring the more neutral "population group." In one instance when he does use the term "ethnic," it is to define "Akkadians" as a term with "socio-cultural" rather than "ethnic" implications (p. 34); while this may be correct, it would be helpful to explain to the reader why he conceives that ethnicity lacks socio-cultural implications, when it is usually understood to have them![27] In fact, the problems of defining what an ethnic group is, and what specific ethnic groups are, are of the same type and order as the problems of defining the term tribe and specific tribes. One of those problems is stability or continuity through time of ethnic groups inasmuch as such groups are characterized, and distinguished from other similar groups, by a specific inventory of cultural traits. Groups that may be called "ethnic groups" do not exhibit diachronically persistent sets of cultural features: whatever cultural features, including language or dialect, may be taken as diagnostic of the group change over the course of time, as well as from place to place if members of the group migrate.[28] This problem obviously has to be borne in mind when considering the emergence of Arameans and Chaldeans in southwest Asia, when no Arameans and Chaldeans were there before, whether or not one calls these population groups "ethnic."

While cultural, as well as biological, criteria fail as a means of defining both the concept of ethnic group and particular ethnic groups — although ethnic group membership does have cultural correlates — two criteria appear to be reliable: 1) self-ascription of identity on the part of group members, together with ascription of identity by non-members; and 2) the predication of that identity on the belief that members of the group share common descent (though not necessarily traceable genealogical relationships).[29] Both criteria seem to apply in the case of Aramean and Chaldean groups. The members of each such group evidently conceived of themselves as being related through shared descent, and they identified themselves and were identified by others as members of that group for the purposes of social action and interac-

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[27] See for instance the essays by Fried, D. Hymes, and Dole in Essays ... (above n. 22), and Emberling (above n. 24) 297 and 310−311.

[28] This is a distillation of the issue as formulated by F. Barth, Introduction, in: (ed.) F. Barth, Ethnic Groups and Boundaries (1969) 9−38, esp. pp. 12−13. See also Fried (above n. 22) 14; Hymes, ibid. 39−42; and C. F. Keyes, Towards a New Formulation of the Concept of Ethnic Group, Ethnicity 3 (1976) 202−213, esp. pp. 202f.

[29] Barth (above n. 28) 13−15; Keyes (above n. 28) 205ff.; Emberling (above n. 24) 299−304.

tion; the larger groups termed Arameans and Chaldeans each comprised several distinct segments to which these criteria apply as well.[30] Although it must be admitted that we should like to have more evidence of these groups' self-identification to substantiate the proposition that their distinct identity was self-ascribed, on the basis of the available evidence in both Assyrian and Babylonian sources it would appear that Arameans and Chaldeans were indeed "ethnic" groups by the above two criteria.

Frame does not explicitly address the question of how these groups came into being, but apparently accepts the common view that both Arameans and Chaldeans were intrusive in Mesopotamia.[31] Why not see them as indigenous? No ethnic group has any "primordial" existence. Any ethnic group (or any population group, for that matter) must come into being in some particular place at some particular time, through differentiation within an already existing population, and through the development of self-awareness and identification as distinct from other sections of that population.[32] There appears to be no good reason to believe that the Chaldeans did not emerge on southern Mesopotamian soil from the population already inhabiting that land. The proportion of West Semitic names attested for individuals identified as Chaldeans is too small to justify identifying the entire group as of West Semitic origin,[33] while the observed degree to which Chaldeans were "Babylonianized" (Frame, p. 37) probably indicates that they were as "Babylonian" as anyone else to begin with, not that they became so. Whatever factors prompted the differentiation of this group within the population of southern Mesopotamia, at some point they were perceived by themselves and others as possessing a distinct identity, and − wherever their group designation came from − they

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[30] In Keyes's words, "ethnic groups belong to a hierarchy of nested segments which are in opposition to segments of the same order" (above n. 28, 206).

[31] This view is manifest especially in the contrast between the Akkadians, who are said to consist of "older groups" resident in southern Mesopotamia for a long time, and the "tribal groups" (the Arameans and Chaldeans; pp. 32, 33); in the suggestion that the Chaldeans may have been West Semites (p. 36); and in the statement that the Arameans' "presence in Babylonia preceded the Chaldeans'" (p. 46). On the problem of Aramean and Chaldean origins and characteristics see also Brinkman, A Political History of Post-Kassite Babylonia, 1158−722 B. C. (AnOr. 43, 1968) 260 ff.; and D. O. Edzard, s. v. Kaldu, RlA 5 (1976−80) 291−297.

[32] As phrased by Emberling, "ethnicity is best seen as a process of identification and differentiation, rather than as an inherent attribute of individuals or groups" (above n. 24, 306).

[33] For the names, see Brinkman (above n. 31) 265−266.

started being called and calling themselves Chaldeans (or, within the larger group called Chaldeans by outsiders, calling themselves by the lineage names Amukānu, Dakkūru, etc.). The same, in principle, may be said for the Arameans, although modifying the formulation to take account of both the Arameans' West Semitic affiliation and their progressive geographical spread. Of this group's origins P. Dion suggests that, like the Amorites of an earlier age, the Arameans may have lived for a long time "dans l'obscurité de l'existence villageoise et d'une transhumance à faible rayon, avant de s'imposer dans l'arène politique du Proche-Orient."[34]

The "state of Babylonia"

It remains to examine what occupies the position "state" in the conceptual opposition between "tribe" and "state" outlined a few paragraphs above (with n. 26), wherein the position "tribe" is occupied by kinship-based segments of the Aramean and Chaldean ethnic groups (whether or not these are correctly termed tribes). Both the Assyrian empire and the Babylonian cities and kingdom may be considered to occupy the position "state" in this conception. While the Assyrian empire can be seen to play a significant role both in actual tribe-vs.-state dynamics and in developing the ideology of opposition, this is not central to the work under discussion. Rather, in the conceptual opposition between tribe and state that underlies Frame's description of the Babylonian people and Babylonian political structure (chs. 4 and 10), the position "state" is occupied by Babylonia. But was there, during the period under discussion, a state that could properly be called "Babylonia"? What kind of entity does the name Babylonia denote?

"Babylonia" is a Greek designation (later used in Latin as well) for the region of Mesopotamia, therewith naming that region after its capital city.[35] It is not a native designation.[36] The Greek designation

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[34] Les Araméens à l'âge du Fer: histoire politique et structures sociales (1997) 18.

[35] The name Babylonia seems not to be attested prior to the 4th century B.C.E., according to the 1996 edition of H. G. Liddell and R. Scott's Greek-English Lexicon. It is not used by Herodotus, who considers Babylon to be in "Assyria" (1.178); but Xenophon does use it, in the Anabasis (1.VII.1, II.II.13).

[36] So far as I have been able to discover, during the relevant period there is no attested instance of a designation such as māt Bābili that could be understood to denote "Babylonia" in the sense of either a territorial state or simply a territory of the extent intended by either the Greek or the modern usage of the name Babylonia. In the two Middle Babylonian attestations listed by K. Nashef, RGTC 5 (1982) 48, s. v. Bābili, māt Bābili does not seem to denote such a state or territory, and these at-

presumably originated from the Achaemenid Persian usage of the
name Babylon (Bābiru-) to denote not only the city but the satrapy
governed therefrom.[37] Modern historians have adapted the Greek and
Latin usage, and mean by Babylonia the region of southern Mesopota-
mia, from the midsection of Mesopotamia where the Euphrates and
Tigris approach each other southward to the head of the Persian Gulf.
The only Mesopotamian designation that may embrace this entire re-
gion is Karduniaš, and that name (besides being Kassite and therefore
perhaps not truly a native Mesopotamian name) was rather antiquated
by the 7th century.[38] Normally, in both Babylonian and Assyrian usage
southern Mesopotamia is called Sumer *and* Akkad — or, bringing us-
age up to date with political and demographic reality, "Akkad, Chal-
dea, Aram, and the Sealand."[39]

Was there not, one might then ask, even though the geographical
region we call Babylonia evidently was not considered to be a unit by
those dwelling there in antiquity, was there not nonetheless a political
entity, a "state," whose authority embraced this region; was there not a

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testations in any case date much earlier than the 7th century B.C.E.; on the
Achaemenid-period occurrences of the designation *māt Bābili* see the next note.

[37] For the Achaemenid usage, see R. Zadok, RGTC 8 (1985) s.v. Bābili 2 (including
two attestations of *māt Bābili*), and R. Kent, Old Persian: Grammar, Texts, Lexicon
(1953) s.v. Babiru-. To give an example of the city's name being used to denote the
satrapy, Bābiruš is listed among the lands, *dahyāva*, subject to Darius I, in a passage
of the Behistun inscription (DB I.13–17; Kent, p. 117).

[38] Brinkman, RlA 5, 423. It should be stressed that the existence in the Kassite period
of a territorial state (Karduniaš) which was equivalent to "Babylonia" in modern
usage (see A. Kuhrt, The Ancient Near East c. 3000–330 B.C., vol. 1 [1995] 338)
does not entail the persistence of such a territorial state after that period. The usage
of the name Karduniaš in Assyrian inscriptions of the 7th century is an archaism,
not a reflection of current political reality. (Incidentally, it is unclear whether Kar-
duniaš is distinguished from or identified with Chaldea in the passage from Ashur-
banipal's inscriptions quoted by Frame, p. 183.)

[39] Quoted by Frame (p. 33) from Ashurbanipal's annals. Since Frame actually made
some of the foregoing observations about the native Mesopotamian terminology,
noting "the fact that no single term existed in common usage for 'Babylonia' as a
whole" (loc. cit.), and since his book is about an entity called Babylonia, why did
these observations not suggest to him the questions posed here?

À propos of the statement quoted above, this is one of many instances where
Frame's phrasing in the book under review and Brinkman's phrasing of the same
points in Prelude to Empire are strikingly similar (and in some cases the similar
phrasing can be traced to Frame's 1981 dissertation). To the quotation above com-
pare Brinkman's "there was no single native term to express 'Babylonia' as a whole"
(Prelude to Empire, p. 16, n. 62). This is no doubt altogether innocent, but citation
would be appropriate when the wording is so close.

kingdom centered at the city Babylon from which the name Babylonia derives? True, most of the time there was a ruler who took the title "king of Babylon," and this title could be imagined (then and now) to imply sovereignty over the whole of southern Mesopotamia. But the real situation often failed to conform to that ideology. Frame himself repeatedly emphasizes Babylonia's lack of political unity, as a corollary of its population's heterogeneity.[40]

During the period from 689–626 B. C. E., in fact throughout much of the early Neo-Babylonian period up until Nabopolassar's reign, the political history of southern Mesopotamia is characterized by a high degree of local and "tribal" autonomy, to the point that rulers of distinct territories and groups within this region not only operated independently, outside Babylon's jurisdiction, but were sometimes accorded recognition on the same level as the ruler of Babylon. Šamaš-ibni, ruler of Bīt-Dakkūri, was referred to as "king," and Nabû-bēl-šumāti, ruler of the Sealand, was ranked among kings in Assyrian inscriptions. Documents were dated by the years of Ningal-iddin, governor of Ur; and Ningal-iddin's son Sîn-balāssu-iqbi, who succeeded him in the same office, produced his own building inscriptions (though acknowledging Ashurbanipal's suzerainty). City governors and "tribal" leaders alike — for instance, Nabû-ušabši, governor of Uruk, and Bēl-iqīša, ruler of the Gambūlu — interacted directly with the Assyrian ruler, and with foreign powers; their communications and their policies were not directed through or by the king of Babylon or his officials.[41]

The state of Babylonia that encompassed all of southern Mesopotamia, with Babylon as its seat of government, seems to be more a creation of the historian's mind — including, perhaps, native Late Babylonian historians (see below) — than a real, functioning political entity that would have commanded the recognition of the people who were supposedly part of it in the period under discussion. If the term "Babylonia" is used to denote a political entity (as distinct from a geographi-

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[40] E. g., pp. 32–33, 50–51, 247, 252, 257.

[41] These observations too are found in Frame's work, to which sample page references are given in the order of the foregoing statements: local and "tribal" autonomy, pp. 38, 45–46; rulers of Bīt-Dakkūri and Bīt-Yakin ranked with kings, pp. 39, 79, 99, 176; texts dated by years of Ningal-iddin, p. 285, and building inscriptions of Sîn-balāssu-iqbi, pp. 110, 125–126; Nabû-ušabši and Bēl-iqīša's independence of action, pp. 119, 127. The independence of Bēl-iqīša and the Gambūlu "tribe" are sufficient to make Frame question whether this group and their territory were indeed "part of Babylonia," p. 118.

cal region), it must refer to the realm controlled by the king of Baby-
lon; and, while it varied in scale over the course of the early first
millennium, that realm did not normally (if ever) correspond to the
entire territory of southern Mesopotamia. Clearly the Assyrians had
to deal not with one "state" encompassing this territory, but with a
multitude of polities of various orders, one of which was the realm
under the jurisdiction of the king of Babylon. As king of Babylon,
Šamaš-šuma-ukīn ruled just that one polity, and as a rebel he had to
mobilize support among as many of the other southern Mesopotamian
polities as possible.

How important was Babylon, really? Can the fate of Marduk's im-
age, for example, or the celebration of the Akitu festival of Babylon,
have mattered so much to the majority of southern Mesopotamia's
several population groups as it did to the scribal and priestly elite of
Babylon?[42] The idea of Babylon's central importance seems to imply
an image of Babylon as the capital of a "nation," for it is connected
with the concept of Babylonian nationalism. But how can the politi-
cally autonomous and ethnically diverse regions and peoples of south-
ern Mesopotamia be conceived to constitute a nation? Babylonian na-
tionalism is invoked by Frame (pp. 258, 261), and more recently by
B. N. Porter (Images, Power, and Politics, e. g., pp. 41, 153), as a mo-
tive force in the region's political history, but the concept of national-
ism requires demonstration rather than assertion in the context of the
seventh century, especially prior to Nabopolassar's accession. Nabopo-
lassar succeeded where Šamaš-šuma-ukīn failed, and (re-)created the
"state of Babylonia" on the foundation of unity in opposition to As-
syria. How exactly he accomplished that would be a story worth tell-
ing, if only we had the data wherewith to reconstruct it. Perhaps his
success resulted in part from, or had the effect of, forging a sense of
Babylonian "nationalism" among the diverse population groups and
political entities of southern Mesopotamia. Nabopolassar's accom-
plishment may be manifest in the "Babylonio-centric" historiography
that developed in the wake of his success. Berossus, the only native
Babylonian historian whose work, albeit in the form of partial and
inexact quotations and synopses, entered European tradition, wrote
about "Babylonia."[43] Although his usage of this name reflects the fact

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[42] Cf. Frame's statements regarding Esarhaddon's restoration of Babylon and the re-
turn of the statue of Marduk, p. 69 and elsewhere.

[43] See for instance the passages quoted in Josephus, Contra Apionem I.137, 138, 150,
153.

that he wrote in Greek, and therefore called his land by the name by which it was known to his Greek readers, Berossus was also heir to the native historiographic tradition that includes the Babylonian chronicle series. The chronicles, which are written from the standpoint of the realm centered on the city Babylon (a realm they call Akkad), certainly present a "Babylonio-centric" perspective on the history of Mesopotamia, and might to that extent be considered "nationalist."[44] But the reality of the seventh century as known from the multitude of sources assembled by Frame belies this perspective, and we need not be in thrall to the image purveyed by the sector of society responsible for the historiographic tradition.[45]

Conclusion

The history Frame has produced is suspended between the philological and the historical levels of inquiry. This characteristic is widely shared in the discipline of Assyriology, as well as related disciplines wherein serious investigation depends on reading and processing large volumes of textual source material in defunct languages and scripts. Another characteristic Frame's work shares with the discipline is the retention of obsolete or inappropriate terms and concepts, some developed within Assyriology and some inherited by Assyriology from older disciplines (such as classical history) and from non-scientific ethnography of the Near East. Thus the foregoing critique, though naming the author of the work under review, applies to the field at large. It behooves us to begin our investigations by querying the terms and assumptions with which we work, then to define them and justify their application to the subject of investigation, and to develop appropriate methods which must likewise be explicated and justified. Thus we may build and rebuild, on valid but not immutable premises, a coherent

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[44] To apply this characterization does not deny the unusual objectivity of the Babylonian Chronicles. On the "parochialism" as well as the impartiality of the chronicles, see A. K. Grayson, Assyrian and Babylonian Chronicles (1975) 10–11.

[45] A counterweight to the historiographic tradition's Babylonio-centric perspective may be found in S. Cole's discussion of the history of Nippur and its neighbors in Nippur in Late Assyrian Times c. 755–612 B. C. (SAAS 4, 1996), though this study focusses principally on a period earlier than that studied by Frame. Cole emphasizes the political autonomy and independence from Babylon of the several regions and peoples of southern Mesopotamia (pp. 17, 45, 69 ff.), as well as illustrating how powerful the ruler of one such autonomous region, the šandabakku of Nippur, could become (pp. 50 ff.).

body of knowledge. In addition, it is incumbent on specialists in an field to communicate their results to non-specialists in a fashion tha does not hide the investigative process. It is a credit to Frame that h work, which is intended in part for the "general reader" (p. 3), ain to accomplish this, although non-Assyriologists would hardly be a enlightened as they might be impressed by the thicket of references i which his discussions and narrative are repeatedly anchored. Clarit and openness about where our information comes from and how w generate historical knowledge are just as essential for the gener, reader as for the specialist.

How might a work of history look, then, if undertaken with tl methodological criticisms enunciated above in mind, and if at the sam time the sources are to be kept in full view? For though one does n(wish to leave the texts behind or obscure the stages of interpretatio: exposition laden with repeated and redundant citations is inefficie1 as well as being tedious to read. One possibility is to compartmentali; the work thus: first presenting all relevant sources (in translation or : the original language as necessary, or in pictures, insofar as the sourc(are artifacts), selected or summarized according to the investigatot judgment (the criteria for which should be plain wherever questio1 might arise), and arranged in – probably – chronological order; the explaining the investigator's methods of evaluating and interpretir those particular sources, of construing them as evidence and co1 structing knowledge from that evidence; finally, presenting the resul of these investigative and interpretive processes: the history – ar here a system of abbreviated references to guide the reader back 1 the primary sources will replace the majority of the footnotes. Lastl regardless of how the work is organized and presented, let us write tl history well. One would expect the story of southern Mesopotam during the seventh century B. C. E. to convey some sense of the excit ment and terror of resisting Assyria's might, the agony of failure whe revolt was crushed, and the suspenseful anticipation of success.

Indeed we have no Assyrian Thucydides, as Livingstone complaine (see above n. 3). Must we have one, though, to gather the "facts" in a coherent and nuanced narrative for us? It ought not to be necessa to depend on an ancient witness to evaluate the evidence while ma shalling it, derive therefrom the progression of events and the phenor ena on which they are predicated, and tell the history as it may, belie ably, have been experienced.