23. See Eric Ebeling, *Keilschrifttexte aus Assur religiösen Inhalts* II (Leipzig, 1923) no. 421. This is a late Assyrian tablet from Assur containing, essentially, the same phraseology as the Uruk text cited above. A portion reads: "A prince will arise and will exercise kingship for 13 years. There will be an attack of Elam on Babylonia and the booty of Babylonia will be carried off. The shrines of the great gods will be ruined and Babylonia will be defeated. There will be chaos, upset, and trouble in the land, and the upper classes will lose power. Some other, unknown person will arise, will seize power as if a king, and will kill off the nobility." See Lambert (1978) 10.

24. I. J. Gelb, *A Study of Writing* (Chicago: University of Chicago Press, 1952) 152.

25. J. P. Hyatt, *The Treatment of Final Vowels in Early Neo Babylonian*, Yale Oriental Researches 23 (New Haven: Yale University Press, 1941).

26. Gelb, *Study* (1952) 152.

27. Weisberg, *Guild Structure* (1967) 108.

28. Weisberg, *Guild Structure* (1967) 108 and 112-117, where a lengthy bibliography is found containing references to studies dealing with the relationship of the Aramaic language to the writing of cuneiform.

Marvin A. Powell

Three Problems in the History of Cuneiform Writing: Origins, Direction of Script, Literacy

"Origins" suggests that cuneiform was invented in a short period of time around 3000 BC by a citizen of the Sumerian city of Uruk and that it arises conceptually out of the token system described by D. Schmandt-Besserat. "Direction of script" agrees with S. Picchioni that cuneiform was written and read vertically down through c. 2300 BC, but it emphasizes the use of reed patterns to demonstrate the manner in which the stylus was manipulated and sees this mode of manipulation as the motivating force behind the transition to horizontal script. "Literacy" argues that cuneiform was not as difficult as usually assumed, that the alphabet had no demonstrable effect on the level of functional literacy, and that the superiority of the alphabet over cuneiform has been exaggerated.

All of the problems discussed in this paper have long been pondered by cuneiformists. None of them have simple answers. Thus, their perennial attraction. I do not propose to solve these problems in a definitive fashion. However, I do propose alternative solutions which, in some cases, run counter to prevailing opinion, and here my intention has been to construct answers which are not less likely to be true than opinions generally held. Ideally, we would like to substantiate our hypotheses by at least two independent witnesses. Where this is possible we conventionally call these hypotheses "facts," but it must be borne in mind that such "facts" become progressively rarer as one moves back in human history from the present day. In this sense, much of mediaeval and ancient history is without "facts," and the third millennium, which forms a large part of our concerns in this paper, is almost entirely devoid of them. Bearing this real fact in mind, we now proceed to indulge ourselves in what a certain wit termed the prerogative of the living: to play tricks on the dead.

ORIGINS

The problem of cuneiform origins can be reduced to four interrogatives: *who, when, where,* and *why?* Given my caveat above, no one will expect me to suggest the name of the individual, the year BC, and the street address associated with the invention of cuneiform writing. However, publication of the papers by Margaret

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Green and Denise Schmandt-Besserat in this issue encourage me to believe that we are perhaps closer to a solution of the problem than we have been in the past.

By convention we say that cuneiform (by which we mean its genetic parent discussed by M. Green) was invented around 3000 BC. The probability that this conventional date could be off by as much as two centuries need not disturb us, because all Near Eastern chronology for this early period is tied together by a number of typological linkages. Thus, mostly what we are talking about is an eventual shrinking or expanding of the whole chronology, not of its individual parts. Consequently, our conventional date has no significance for the question of whether Mesopotamian or Egyptian writing was created first. That is a problem which we are simply not in a position to solve presently. In this context it is worth noting that a recent attempt by the archaeologist James Mellaart to develop a chronological system using radio-carbon dating and other evidence which would eliminate "the dubious claim that writing was invented in Mesopotamia earlier than Egypt" (1979:12) has already encountered very serious criticism (Weinstein 1980; Kemp 1980; Munn-Rankin 1980).

Other chronological problems confront us at the site of Uruk where the oldest, stratified Mesopotamian examples of pictorial writing have been found. Strommenger (1980) has recently reviewed the system used by the excavators to distinguish strata III and IV and has pointed out the circular nature of the argumentation, the script being used to date the levels, and vice-versa. Thus, at present, separating out stages of the script in relation to the archaeological levels seems uncertain at best. However, other considerations enable us to treat the Uruk IV-III archaeological strata provisionally as a unit, at least with regard to the origin of the pictorial script.

During the period in question, Uruk seems to have been not only the largest urban center in Mesopotamia but also the only one that could be called a city at all (Adams and Nissen 1972:17f.; cf. Powell ca. 1982). Now, we do not *know* that writing was invented at Uruk, but our job is not to multiply hypotheses. We must choose the more likely, and the present evidence suggests Uruk as the probable locus of invention.

If we are correct in this assumption, then the arguments about the emergence of the impressed tablet system marshalled by Schmandt-Besserat above enable us to make other probable inferences concerning when and why cuneiform was invented. Her work has, in my opinion, established beyond reasonable doubt the presence of this system in the fourth millennium, prior to any pictorial writing system, as well as its continuity down into the period when the pictorial ancestor of cuneiform was invented. For our purposes it is irrelevant whether the impressed tablet system disappeared in Uruk IVa or in Uruk III, because it could have continued in use for limited purposes even after the invention of the pictorial system. Thus, we may formulate our second hypothesis: the pictorial ancestor to cuneiform writing was invented as a conceptual whole during the time period represented by the Uruk IV-III archaeological strata.

This hypothesis rests upon the likely assumption that the pictorial system grows out of the accounting system attested by the impressed bullae and tablets, and it provides us with a further inference about the nature of early Sumerian writing. The impressed tablets are clearly mnemonic in nature. They form part of

an accounting system, the rules of which we do not know and are unlikely ever to recover. The same is essentially true of the early pictorial tablets. They contained the information necessary for the accountant to interpret the record, but there is nothing in them to suggest that they represent a systematic attempt to fix most or all of the language in a way that can be decoded by an outsider. Thus, the scepticism voiced by I.M. Diakonoff (1975:109f.) about the validity of the term *logograms* for the early pictorial signs — as advocated by Falkenstein (1936:32), Friedrich (1966:45), and Gelb (1952/1963:65) — finds solid support in Schmandt-Besserat's work. The conceptual background to the early pictorial script now stands revealed, and it is not, as often supposed, logographic, but mnemonic, with elements much closer to the ideography argued by Diakonoff than to any system of logography. Even the number of signs discovered (estimated by M. Green, above, at about 1200) militates against an assumption of logography and, therefore, encourages us to seek alternative conceptual frameworks for interpreting early cuneiform writing.

If, on the other hand, we look upon the early pictorial script as basically mnemonic and seek evidence for this in its later development, we find traces of its mnemonic character enduring to the very end of the Sumerian orthographic tradition. It is, in fact, so deeply embedded in the system that it inhibits development of either a phonetic or a logographic type of writing. By the twenty-fourth century BC when the structural paradigm for all subsequent Sumerian orthography has already emerged, one finds a repertoire of phonetic signs sufficient — had the phonetic principle been systematically applied — to represent Sumerian in a phonetic script. This, however, is never done in standard orthography. One finds, alongside signs that can be called true logograms (since they are normally used only to represent one word each), signs that can only be called ideograms (or some corresponding term), since they represent a number of different words related only in a conceptual — not a verbal — sense. These ideograms — and often the logograms — are delimited and made readable by a subsystem of semantic and phonetic markers, usually called "determinatives" and "phonetic complements" respectively, which make it possible — even for an outsider — to decode and understand the system.

A few words about this system are in order because, in my opinion, it has been misunderstood, not only by linguists and historians of writing, but also by specialists in the field as well. E. Reiner (1973) treated the subject of reading cuneiform texts written in the Akkadian language, for which she set up a fairly exhaustive set of rules from the standpoint of the modern scholar. One will eventually be able to set up such rules for reading cuneiform Sumerian from the twenty-fourth century BC on, but these, being intimately linked with the origins of the script itself, are ultimately rather different from those for reading the derived systems used to write Akkadian, Eblaite, Elamite, Hittite, Hurrian, and Urartian. There are, of course, certain visual-lingual-aural rules which enable one who has mastered them to transform visual symbols into language sounds. However, anyone who becomes really proficient at this is likely to find that he has, in the process, learned the language. Knowledge of the language is probably the primary factor in determining the ability to interpret visual symbols in a manner intelligible to other speaker-hearers of the language itself.

This, of course, functioned in the same way for the ancient Sumerian (and Akkadian) scribes who really understood Sumerian (as we assuredly do not). The system of noun "determinatives" is well-known—there are only about a dozen common ones. It consists of procedures like prefixing the names of divinities with the picture of a star and suffixing the picture of growing plants to certain types of plants or the picture of a fish to aquatic animals. These determinatives serve to delimit the range of meaning of the ideogram and therefore to "determine" its reading. There also exists a parallel system of phonetic "determinatives," which function with verbs as well as nouns.

In addition to the phonetic chains of signs defining the tense, voice, person (and certain other aspects) of the verb, Sumerian writing delimits the semantic field (and therefore the reading) of verbal roots by a system of phonetic indicators with regular rules. I have tried in another context (Powell 1978: 182f. n. 31) to give an account (unfortunately incomplete) of one aspect of this system, but it remains to be described as a whole. These verb indicators, except in a few special cases, follow the sign which stands for the verbal root. Nouns, on the other hand, can have these phonetic indicators before the signs in question (e.g., ad+REEDMAT = adgub = "matweaver"), after them (e.g., PLOUGH^{na} = apinaK = "of a/the plough," distinguished from ^{ab}PLOUGH = absin = "furrow," whereby the *b* of the determinative *ab* is not significant—it could theoretically be read *ap*—only the position vis-à-vis the sign PLOUGH), and both before and after (e.g., ^{nu}BREAST^{da} = nubanda = "lieutenant" or some such meaning).[1] All of these features point to an early cuneiform writing system that is conceptually neither phonetic nor logographic. Whether one calls it *ideographic* or not reduces itself to a matter of dispute over terms.

Turning to the question of *who* invented it, my answer to this question remains, with a minor addition, the same as that I proposed a decade ago: *literatus Sumericus Urukeus.* I would like to be able to call him Ur-Nanše, Ur-Enki or, better still, Ur-Inanna, but I readily confess that—barring a revelation by Inanna—we will never know, in this sense, *who* invented cuneiform writing. However, that it was an individual I have no doubt, certainly not a committee, and not a slow accretion of sign after sign from generation to generation: there is not a single instance in the history of writing for a communal-evolutionary invention of a script. Individuals invent. The community of users modify, adapt, elaborate, refine, add to, and take away, but they do not invent. That this inventor was both a Sumerian and a citizen of Uruk (that first great city of humankind about which we know so lamentably little) is inherently probable. And, if he was not a Sumerian, then a Proto-Tigridian, Proto-Euphratean, Hurrian, Subarian, Dravidian, or Neolithian who thought, felt, spoke, and probably looked like a Sumerian.

While I hold my individual inventor to be inherently more plausible than some vague "they," my identification of him as *Sumericus Urukeus* has a more specific basis to it, which I review here by way of summation.

1) The appearance of the pictorial script coincides approximately with the emergence of Uruk as the only major urban center in Mesopotamia. 2) This script appears in Uruk at the end of a long tradition of accounting by means of the token-system. The system of numerical notation (and probably some of the abstract signs) is clearly modeled on the system attested by the impressed bullae and

tablets. 3) No earlier examples of this script have ever been found, although Assyriologists generally assume that a long period of evolution lies behind the script attested in the levels IVa/III at Uruk (which partly accounts for the predilection for speaking of *inventors,* rather than a single *inventor*). Discovery of prototypes of the Sumerian pictorial script in well-stratified deposits of the fourth millennium would, of course, make the theory here advocated untenable, but the overall archaeological evidence, especially the parallel developments in western Iran,[2] make such a discovery highly improbable. 4) The system of numeration deducible from the notation present on Uruk IVa/III tablets makes it virtually certain that these tablets are written in Sumerian and, *ipso facto*, highly probable that the inventor of the pictorial writing system was also a Sumerian.

A few additional words on this last point. The Sumerian sexagesimal system of counting is unique. This system is implicitly present in the notation of the early Uruk tablets, as well as in the approximately contemporary tablets from Jemdet Nasr and others deriving from uncontrolled excavations. Since systems of counting, like grammatical structures, tend to be stable elements in a language, the presence of sexagesimal structures in the early tablets constitutes cogent evidence that the language in which they are written is Sumerian. I presented this argument about a decade ago (1972:165-172) in the context of a critique of the "decimal substratum" theory. Having reviewed the whole problem in the light of additional evidence, I see no reason to change my opinion. Moreover, Jöran Friberg (1978:9) has recently removed one of the last buttresses for this theory by correctly interpreting a notation for capacity which looks like a unit *ten* but really represents a unit *six* (illustrated above in Schmandt-Besserat's Figure 6).

Perhaps the future will bestow upon us some unexpected piece of evidence that will settle the question of who invented the Sumerian system of writing, even to the satisfaction of those who prefer to doubt. Meanwhile, my judgment of the situation is that the balance tips heavily in favor of a Sumerian speaking citizen of Uruk. Historical reconstruction must of necessity concern itself with probability, not possibility, and many another commonly accepted assumption from ancient history rests on little or no more evidence than that which supports the theory here advocated. I am content to believe I have found the solution, but perhaps some of the doubters will find some evidence to put in the other balance pan.

Appendix: Recent Criticism of Schmandt-Besserat's Work

I have argued that Schmandt-Besserat's work provides the real key to understanding both the invention and the character of early Sumerian writing. Since two critiques pertaining to her theories have recently appeared, I shall consider briefly how they affect the underlying assumptions of my argument.

Le Brun and Vallat (1978:30-40) as well as the more recent paper by Lieberman (1980) focus, firstly, on her hypothesis that the shapes of some tokens are the prototypes for some of the early pictorial signs in the Sumerian system and, secondly, on her argument that the tokens belong to a common system of accounting that functioned as a kind of lingua franca for the whole of the Near East going back even to the beginning of the Neolithic period. I would agree with her critics that the arguments and evidence mustered to support these hypotheses are problematic. However, her attempts to make sense out of the objects in question are worthwhile, if for no other reason than the fact that they constitute a pervasive, but much neglected, part of the archaeological record. Certainly archaeologists are now looking at this material much more closely, as the interesting paper by Le Brun and Vallat (1978) shows.[3]

On the other hand, I think we have to reject Lieberman's sweeping claim (1980:339) which deems her hypothesis "unjustified on chronological grounds, imprecise or incorrect in terms of many of the formal comparisons which have been made, inadequate as an explanation of the appearance of writing, and based on an error in classification." Only the objection to imprecision in "formal comparisons" is really valid. As we have noted, this is the chief point with which Le Brun and Vallat have also taken issue. Lieberman has also made a useful contribution in discussing the lexical evidence that might just possibly relate to abacus-like accounting techniques (1980:346-351)—a theme already broached (though in different terms) in his letter to *Scientific American* (November 1978:10-15) but this pertains only peripherally to his critique of Schmandt-Besserat's theories. I have noted various windmills in the landscape of Lieberman's article and have marked them out so as not to forget their location, but I am willing to leave them for other tilters. I would only add that, for a paper written in the name of logic, Lieberman's critique contains an amazing amount of illogic, and it in no way effects adversely the assumption underlying the present paper, namely, that Schmandt-Besserat's paper published above, which grows organically out of her previous work, enables us for the first time to understand the invention of cuneiform writing.

Finally, I would like to call attention to what I believe to be the essential point. Lieberman (1980:358) states that the "conventionality [attested in the pictorial signs of the Uruk IVa/III texts] can only have resulted from a long development." I would agree, but not in the way this is understood by him and others. The "long development" is a *conceptual* development, the nature of which is now possible to grasp as a result of Schmandt-Besserat's work. On the other hand, the theory advocated by Lieberman, namely, that a long evolutionary development of the *pictorial signs* preceded the sign repertoire of the Uruk IVa/III texts, does not have a shred of substantial evidence to support it. It is theoretically implausible, and the archaeological evidence is against it. However, now that I have started it in this bald-faced manner, perhaps some believer will come forward with a modicum of evidence in its favor. Otherwise, let us desist from presenting this theory as though it were logical and supportable.

DIRECTION OF SCRIPT

We normally read cuneiform tablets starting at the left-hand edge, reading left-to-right and down the column. If there is more than one column on the obverse of a tablet, we move over and read that just as we would a modern newspaper. We read the reverse side in a similar manner, except that we turn it over from the bottom (instead of the side) and start reading at the upper right corner, going down the column and reading lines left-to-right. At the bottom, we go back to the top of the next column immediately to the left and repeat our procedure until we reach the left-hand side of the back of the tablet. This is clearly

the way cuneiform was written and read from around the middle of the second millennium BC onward, and it is the way that most of us read even the early tablets treated above by M. Green. However, it is also immediately apparent that the early tablets assume their proper pictorial perspective only when turned 90° to the right from the way we customarily read them. This phenomenon has given rise to the questions *when* and *why* did one start writing and reading left-to-right instead of top-to-bottom? Aside from the intrinsic interest for the technique of writing, these questions touch upon other issues that have not been fully appreciated.

The question of *when* one changed over to left-to-right writing is pertinent for the history of Sumero-Babylonian mathematics. Kurt Vogel (1958) has pointed out that mathematical problems in Old Babylonian texts from the first half of the second millennium BC refer to what we call the "left edge" of the tablet as the "top." Two interpretations are possible. Either the left edge was still regarded as the "top" in the Old Babylonian period or this is an archaic terminology that has been handed down from the Sumerian period. If we could fix the change-over from top-to-bottom direction to left-to-right direction, it would also give us a much needed *terminus ante quem* for this mathematical terminology. I would like to believe with Vogel that it goes back well into the third millennium, since that would be splendid confirmation of one of my favorite theories (Powell 1976), but, unfortunately, the earliest unequivocal (and datable) evidence for left-to-right script is on Babylonian boundary stones of the fourteenth century BC (Brinkman 1976:37, 211, 265; likewise Edzard 1980:547). Thus, we must content ourselves with noting the implications of the *when* question and hope more evidence will turn up.

For the *why* I think we can offer more adequate evidence. The fundamental dynamics of the change in direction of script seem to be tied to the way in which the stylus was manipulated. This, too, has been much discussed, but never thoroughly investigated (bibliography in Driver 1976:19-26, 228, and Edzard 1980:545f.) Breasted (1916: 241-246) thought he could infer from Assyrian palace reliefs that the stylus was held in the fist, and this has been accepted by Driver (1976:22). However, a careful scrutiny of the reliefs cited by Breasted (1916:246 nt. 1), as well as others, leads me to two conclusions: 1) the depiction of scribes on Assyrian palace reliefs does not strive for representational accuracy in detail, only for symbolic representation; 2) some of the reliefs clearly contradict Breasted's assumption (e.g., Patterson 1915:P1. 40-41; Hall 1928:P1. XXXVII). Moreover, the Til-Barsip wall paintings of scribes (Thureau-Dangin and Dunand 1936: XXIVgh), though incomplete and available only in a copy by Lucien Cavro, do not support either the Breasted assumption or Driver's interpretation (1976:22) of the manner in which the stylus was held. Rather, all of these representations confirm the conclusion that emerges from careful study of stylus impressions: It was held between thumb and fingers approximately as shown in Figures 1-9.

Stylus impressions provide the key, not only for inferring the nature of the material and how it was shaped, but also how it was manipulated. The stylus is called in both Sumerian and Akkadian "reed," and this is entirely confirmed by the fibrous impressions left in the clay by the vascular bundles present in the reed itself. The only adequate study of this phenomenon was published many years ago by Messerschmidt (1906). Of all the theoretical reconstructions of the shape and manipulation of the stylus that have been published—Zehnpfund (1893), de Morgan (1905), Clay 1906:17-20, Falkenstein 1936:5-7—only Messerschmidt's rests on adequate evidence. This may strike Assyriologists as strange. The study is little appreciated: Edzard (1980:545) omits it from his bibliography; Driver's *Semitic Writing* (1976:19-30), which has been through three editions and which quite misunderstands the implications of Messerschmidt's work, has never been challenged on this point. I myself, being ignorant of Messerschmidt's conclusions, laboriously rediscovered the same principles that he had already deduced seventy-five years ago.

The most important conclusions which derive from his investigation concern the way in which the stylus was cut from the reed (Figure 11), and the way it was held in the hand (Figures 1-9). The stylus was cut from the outer section of a mature reed. It seems unlikely that it ever consisted of the whole stalk of the reed, as Falkenstein (1936:6) seems to assume. (This is another puzzling problem clarified by Schmandt-Besserat's demonstration that the iconography of early Sumerian numerical notation has no inherent connection with the natural shape of the reed.) Messerschmidt (1906: 304 n. 1) cut his stylus from a bamboo cane twelve centimeters in diameter, but much smaller diameters are quite sufficient, as I have discovered by experimenting. The cut reed is held in the hand so that the inner side is up toward the palm of the writer. The slick, outer surface, which creates the smooth, right-hand faces of the wedge, is held down toward the tablet. This surface may be up to six or seven millimeters in breadth (but is often much smaller). On the right, the stylus tapers off to a knifelike edge, while the left side, which is braced against the thumb (and may run up to about four millimeters in breadth), forms the left-hand writing surface. It is this surface that leaves behind the reed patterns found on the tablets.

These reed patterns show that the stylus was held in one basic position and that the surfaces of the stylus which made contact with the tablet were always the same in all periods from the mid-third millennium on. For the early third millennium Messerschmidt (1906:192f.) implies that he had at his disposal at least some archaic tablets and that the same principles apply there also. Those which I have examined from Uruk in the Orientalisches Seminar of the University of Heidelberg seem to confirm this. However, it must be emphasized that, because of the state of surface conservation, perhaps only ten to twenty percent preserve these patterns. Therefore, one needs to look for divergent features on large numbers of tablets using a magnifying glass of 7x or more. I have, in this way, examined about 4000, including about 500 Presargonic, about 300 each of Old Babylonian and Neo-Babylonian, less than a hundred from various periods over the three millennia (mostly Babylonian, but some Assyrian), and the remainder from the Ur III period.[4] Not what one could call a random sample, but, together with Messerschmidt's experience, which conforms precisely to mine, enough to allow the following deductions about the change in direction of script.

Since the reed patterns show that the basic position of the stylus vis-à-vis the hand and the tablet was fixed, experimentation also shows that a right-handed scribe writing his tablet in lines top-to-bottom and in columns right-to-left has to position himself in an angle of roughly 45° to his tablet and approach it from the left side, never from the right side. Thus, there must have been from the

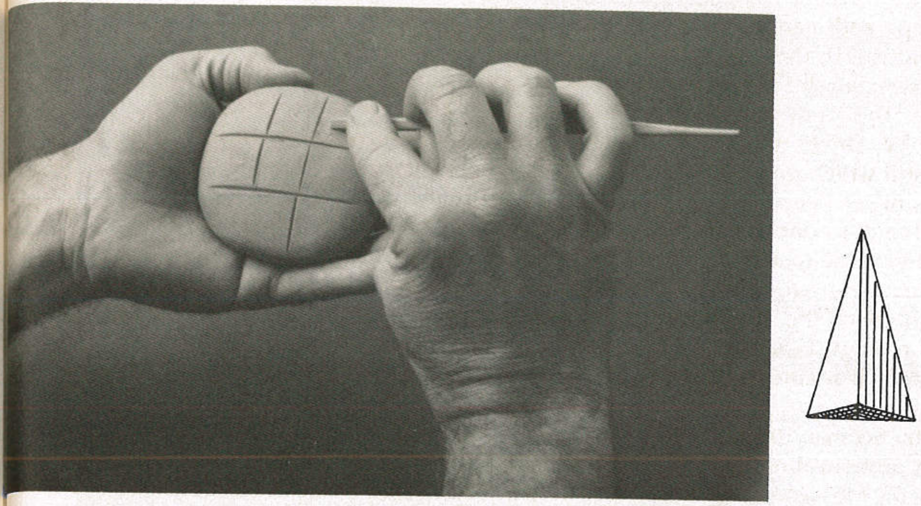
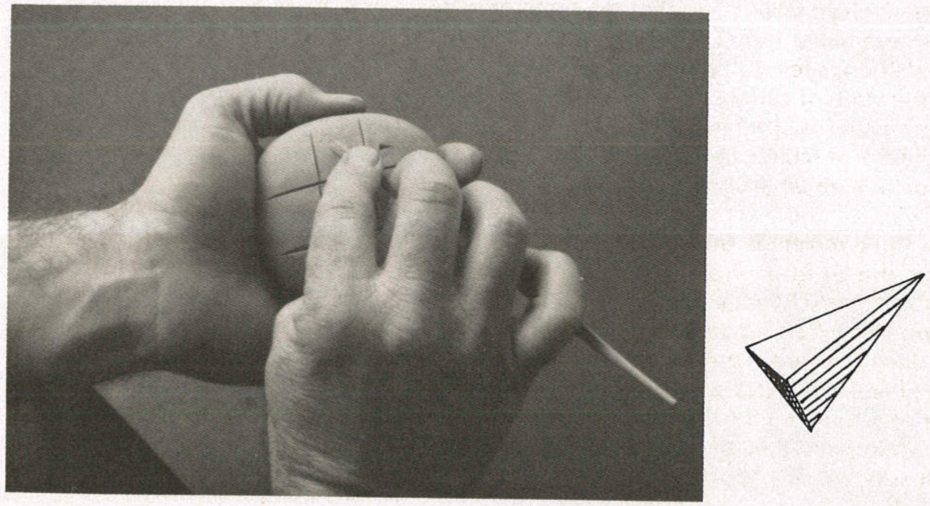


Figure 1. Stylus is held in normal position, but tablet is turned 45° to the right from normal writing position and 90° from the later (standard) reading position; i.e., it is in the orientation characteristic of the archaic script. Right: The wedge made, turned 90° to the left to the later (standard) reading position. This wedge occurs rarely and disappears from standard usage after c. 2300 BC. Note the position of the striated face created by the cut surface of the reed and the smooth face made by the outer skin of the reed.

Figure 2. Stylus and tablet in normal writing position. Note the angle of the tablet to the body and the stylus grip common to Figures 2-5. These are the wedges that survive after the middle of the 2nd millennium BC. Right: The wedge made, oriented in standard reading position.



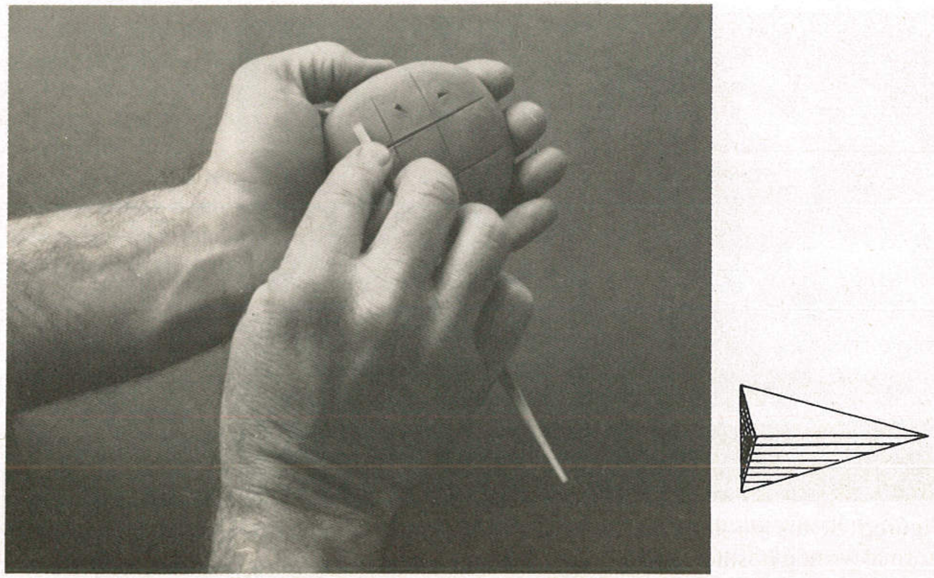
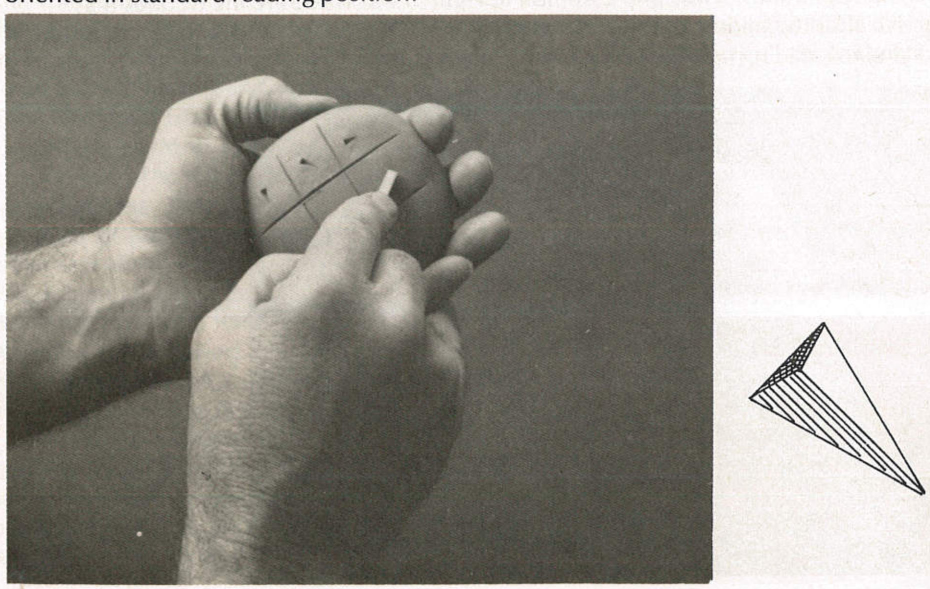


Figure 3. Standard writing position of stylus and tablet. Right: The wedge made, oriented in standard reading position.

Figure 4. Standard writing position of stylus and tablet. Right: The wedge made, oriented in standard reading position.



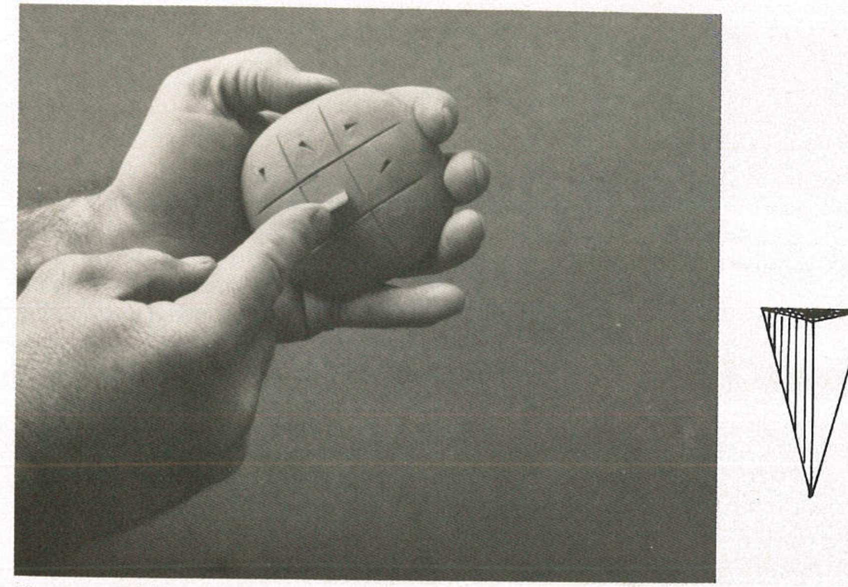
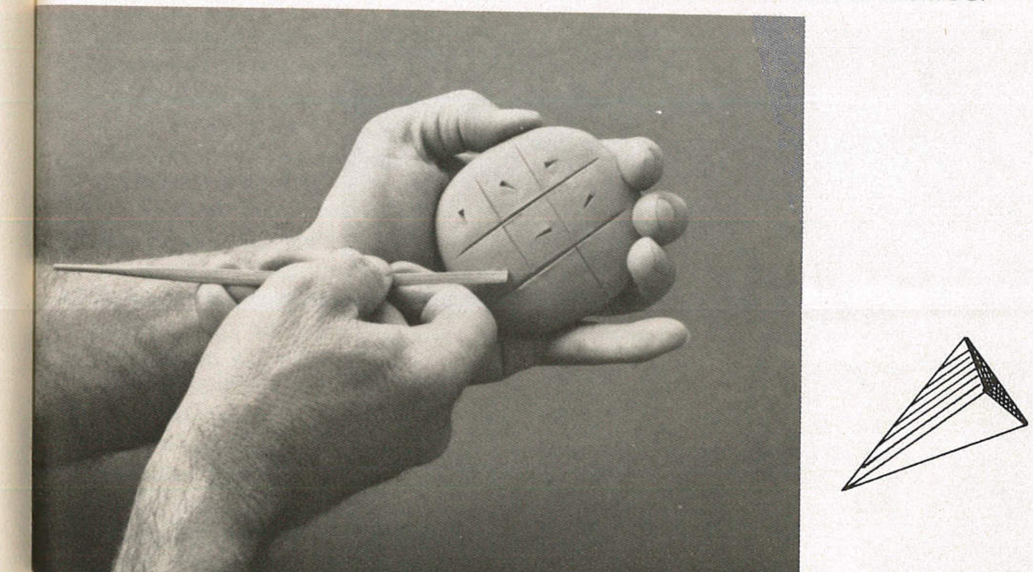


Figure 5. Tablet is in standard writing position, while the stylus cuts across the axis of the arm with the end of the stylus slightly to the left of the arm. Right: The wedge made, oriented in standard reading position.

Figure 6. Stylus must be shifted to make this wedge, but tablet remains in the normal writing position. Right: The wedge made. It is less common than the wedges in Figures 2-5 and goes out of standard use in the first half of the 2nd millennium BC.



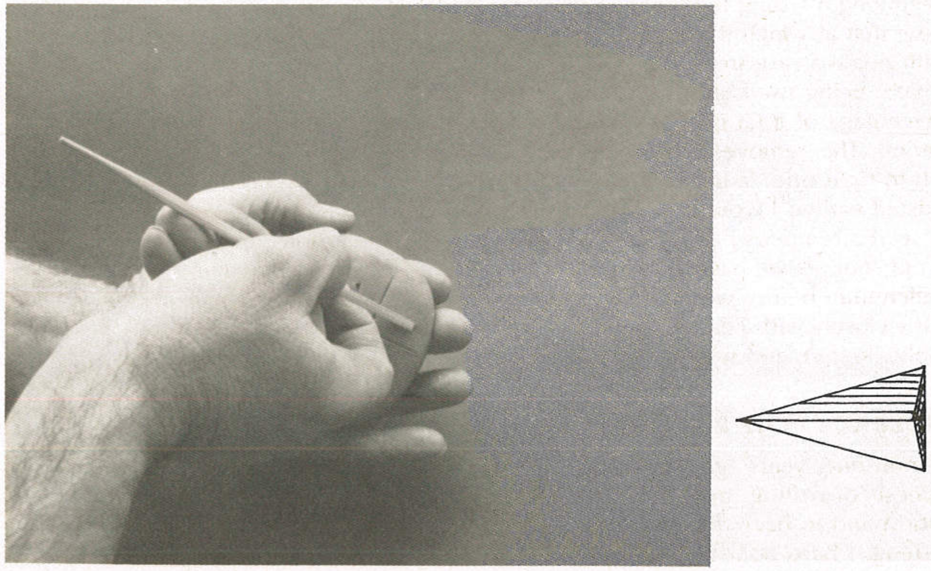
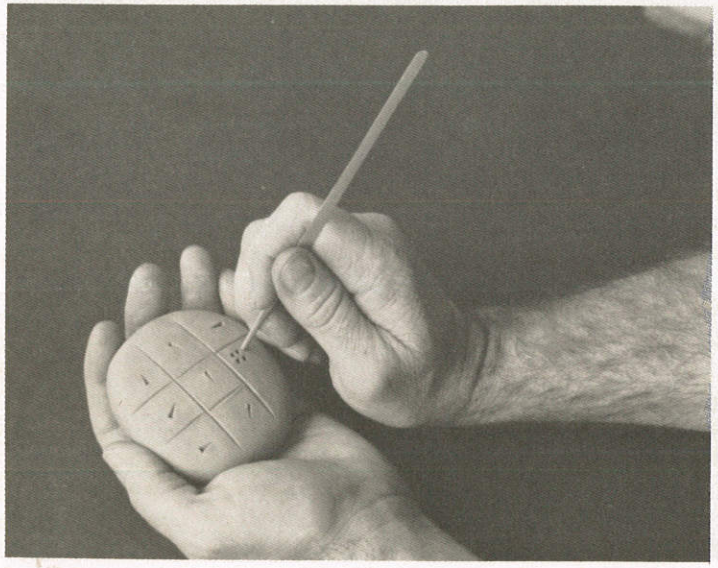


Figure 7. Stylus held in same position for making wedge in Figure 6, but tablet must be turned about 40° counterclockwise. Doing this by wrist motion alone creates an awkward writing position, thus the tablet itself must be shifted in the hand. Right: The wedge made. It is very rare and disappears from standard usage c. 2300.

Figure 8. Writing "tens" with the inverted reed, standard procedure down to c. 2300.



beginning a strong tendency to *write* the tablet at an angle rather differently from that at which it was read. The majority of stylus impressions can be made with relative ease from the left as shown in Figures 2-6. The other impressions which, being used only in special circumstances, always constituted a tiny percentage of total number of wedges are ultimately abandoned in the Akkadian period. This removes the last obstacle to perceiving the script in a horizontal, left-to-right orientation, but I see no justification for assuming this to have existed earlier. I would, therefore, see the Akkadian period (twenty-third century BC as the *terminus post quem* for a changeover in the direction of reading the script, but, as we have already seen above, this leaves us with almost a millennium before we arrive at a secure *terminus ante quem*. This coincides fairly closely with Edzard's appraisal of the situation (1980:546f., with bibliography), and with that we leave the matter, still largely in limbo.[5]

LITERACY: THE ROLE OF CUNEIFORM

About thirty years ago I. J. Gelb (1952/1963:212-220) made a case for the monogenesis of writing, by which he understood the Sumerian system to be the oldest and to have directly generated or inspired other systems of genuine writing. I have pondered this problem myself now for almost two decades and, looking at it from first one side then another, have come to the conclusion that any decision on the matter must be something like an act of intuition. Having done this, my intuition tells me that Gelb is more likely to be right than Friedrich (1966:173f.), who regards as improbable the assumption that the Sumerians made the primary invention of writing, with the Egyptian and Chinese systems being inspired by the Sumerian. While I would explicitly disavow "diffusionism," Friedrich's formulation of the matter is rather problematic. The chronology of early Chinese script is as difficult as that of early Sumerian script, but, like the latter, we have a pretty good idea of when it appears, and this time corresponds to the Near Eastern Late Bronze Age[6] (c. 1600-1200 BC). Given the contacts across the Iranian plateau that have been established by archaeological exploration for as early as the Early Bronze Age and perhaps even earlier, is it really conceivable that no *idea* of writing reached China from the Near East? I think not, but, for world literacy, that is not the central problem, since the Chinese system has not had a major impact outside of the Far East.

The other point that Friedrich raises, namely, whether the Egyptians received the impetus for the creation of a script from Mesopotamia, is more pertinent. I think we will eventually be able to demonstrate that, structurally, Egyptian writing has a lot in common with early Sumerian writing. Then, other arguments already advanced for Mesopotamian influence on Egypt about the time that writing is introduced will have a greater bearing on the problem (e.g., Emery 1961:30-42; Gelb 1952/1963:214f.). Under those circumstances, the arguments of Schmandt-Besserat (above) about the Sumerian character of the late phases of the token system will also appear in a different light. Archaeological evidence, as a whole, indicates a greater dynamism in external contacts for Mesopotamia than for Egypt in the late fourth and third millennium, and the spread of writing may well turn out to be part of this dynamism: to Egypt, to Elam, to Ebla, and perhaps to other areas of which we have as yet no evidence.

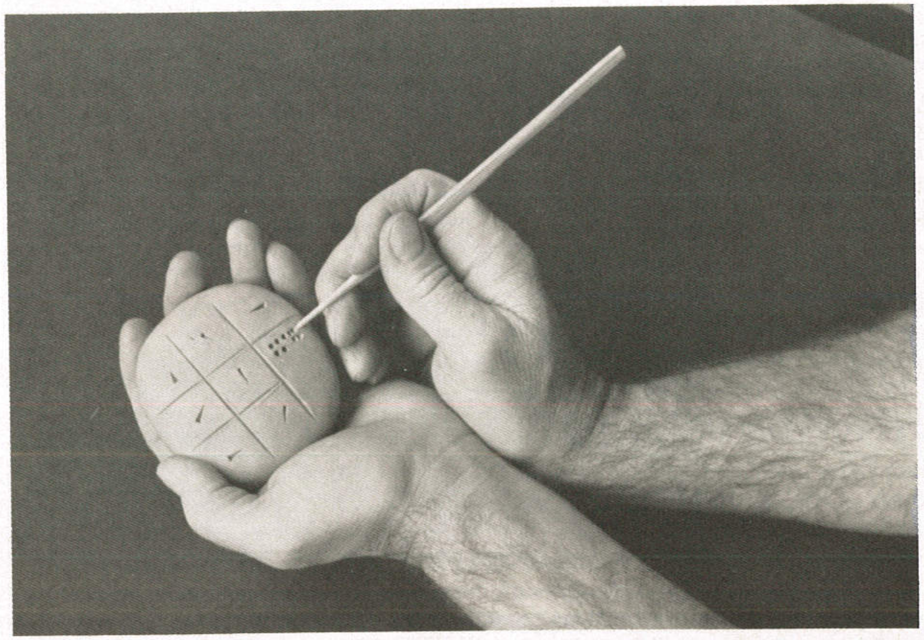
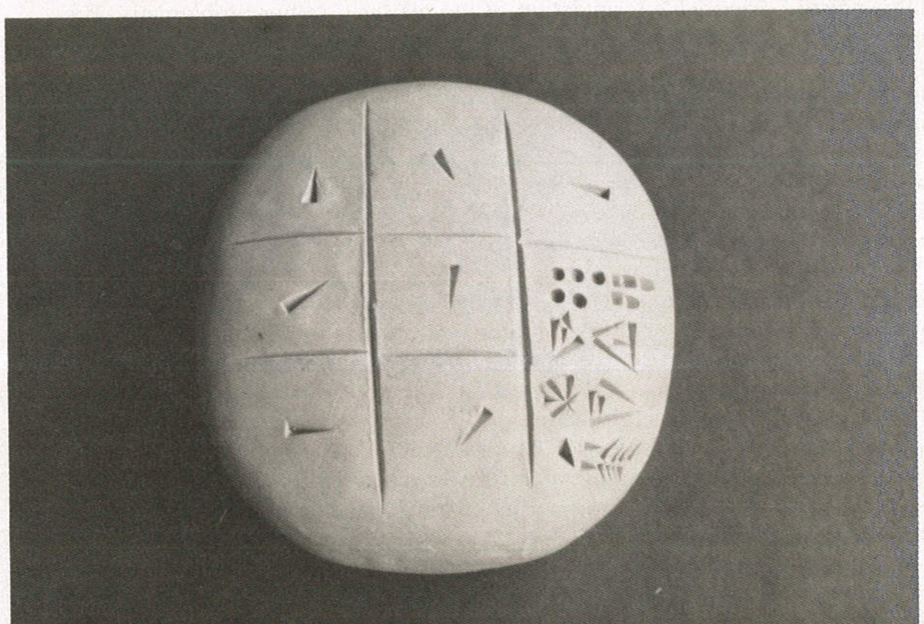


Figure 9. Writing "ones" with the inverted reed, standard procedure down to c. 2300.



The discovery of extensive palace archives at Ebla in northern Syria (recently G. Pettinato 1979; R. Biggs 1980) has forced us to reassess our understanding of the third millennium as a whole and of the spread of cuneiform writing in particular. What this discovery has shown us is a much wider spread of

Figure 11. The Reed Stylus. A. End of the whole reed showing section removed to make stylus. B. End section of the finished stylus. The acute angle formed by surfaces α and β is typical of the Presargonic stylus used to write the wedges in Figures 1-10. C. Side view of Presargonic stylus. α. Writing surface producing striated impressions in the clay. β. Smooth outer surface producing smooth face of wedge. γ. Inner surface of reed smoothed to a plane, never used for writing. δ. Sharp edge of stylus, not used for writing. ε. End of the stylus with a plane sloping downward toward edge δ and slightly toward surface γ.

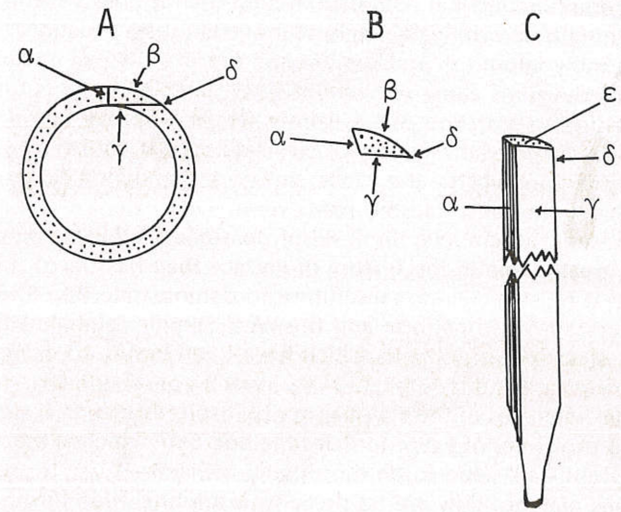


Figure 10. The impressions made in Figures 1-9, together with a brief text in the script characteristic of the state of Lagash in the 24th century. The text is in the lower right-hand section, oriented in the left-to-right perspective which characterizes the script of later periods. This is the orientation in which modern scholars normally read cuneiform, though it was not read this way in the period represented by this script. The text reads: 55 ninda ba-an-né 10 du$_8$, "55 flatbreads baked at [a ratio of] 10 per ban [a measure of c. 6 quarts]."

cuneiform in the third millennium than had previously been thought probable or even possible. Above all, it brings cuneiform much closer into the area where other ancient writing systems arose, in particular, close to the homeland of the alphabet. It would be premature at this point to attempt an argument in favor of cuneiform as inspirer of the alphabet, but it certainly puts cuneiform back into the front line as a contender.

In this context, it is worth noting that, in spite of objections to Gelb's theory concerning a syllabic character for the West Semitic "alphabet" (primarily by Egyptologists [Edgerton 1952] and Semitists who work with alphabetic scripts [Barr 1976:74f.]), Gelb has been able to respond to his critics (1958), and Diakonoff (1975:100f.) has recently noted another piece of evidence in favor of the syllabic theory. The significance of this is that, if it ultimately becomes clear that the West Semitic "alphabet" is, at bottom, a syllabary, cuneiform is the only likely model for it, for Egyptian writing represents an entirely different system, which has much more in common with the Sumerian system than it does with evolved cuneiform. Early cuneiform writing, if I rightly understand the situation, is spread in the early third millennium (or perhaps the late fourth) to Egypt and Elam, where it subsequently develops along indigenous lines. But, in Babylonia itself, it develops in the direction of a syllabary, which is spread by the Akkadians, whose dynamic role in the history of civilization can only be paralleled among other Semitic peoples by the Arabs, and to a slightly lesser extent, by the Amorites, the Aramaeans, and the Jews.

All of this remains, at present, little above the level of possibility. I think cuneiform played a much greater role in the history of literacy than has hitherto been thought, but it must remain for the future to demonstrate the relationship between Mesopotamian cuneiform and the West Semitic "alphabet." Nevertheless, one thing is clear: the arguments which have been based upon the suppositions that 1) the West Semitic "alphabet" is, in fact, consonantal in nature, 2) that "consonantal" features of the Egyptian script inspired this "alphabet," and 3) that the proximity of Egypt to Palestine and Syria cinches the derivation from Egyptian script —all need to be thoroughly reexamined.

Interesting as these issues may be, they are, at present, insoluble. Therefore, let us pass on to a matter which is perhaps not: the decline and extinction of cuneiform as a system of writing. It has generally been supposed that cuneiform died out because it was less practical and less easily learned than the alphabet. Proponents of this theory have often been Assyriologists themselves. This may, in part, be a function of Assyriologists being better scholars than they are pedagogues. In any case, they are hardly unbiased observers. There is definitely one aspect of the problem that is generally ignored in such formulations concerning the difficulty of the cuneiform system: learning cuneiform involves not only the mastery of a writing system, but of a whole body of knowledge associated with it. This may appear an elementary observation when stated in such plain terms, but I fear it is often overlooked. In any case, with regard to the relative ease of cuneiform *vs.* alphabetic scripts, the remarks of Diakonoff (1975:101) are worth noting: "everyone who has read Phoenician or Ugaritic, on the one hand, and Akkadian, on the other, knows that it is much more difficult to *read* and *understand* Phoenician and Ugaritic.... Reading Ugaritic is a process of

solving riddles (much more so than reading Akkadian, although there, too, an element of decipherment is always present)."

I would like to suggest that the derived cuneiform scripts used to write Akkadian and other languages were not nearly as difficult as is usually supposed—certainly not as difficult as Chinese and perhaps only slightly more so than Japanese. These two scripts have survived into the twentieth century as a result of favorable political and cultural conditions and show no signs of expiring. I think we must approach the demise of cuneiform with these facts in mind. The era in which cuneiform died (first century AD) was not only politically, but culturally, hostile to the culture which created cuneiform and the body of knowledge associated with it. That is a point I cannot argue here, but the evidence in favor of it is manifold. Had the political and cultural situation been otherwise, cuneiform might have lived on indefinitely. Still, combined with this hostile environment there was another factor that has been over-looked in considering the withering away of the cuneiform system.

Clay is a very bulky item. The cuneiform system depended on clay. It is true that some examples of cuneiform written on materials that produce a two-dimensional effect have been discovered (Driver 1976:30f.; Edzard 1980:567f.), but anyone who has either read or written cuneiform must have become at once aware of the radical aesthetic difference: cuneiform is a three-dimensional script. It began on clay, and there it ended—probably precisely for that reason.

Assyriologists have often observed that cuneiform literature does not contain works like those of Herodotus or Thucydides, and they usually attribute this to cultural causes. Other conclusions are possible. One of the longest cuneiform works known to us is the bilingual dictionary known as á = A = *nâqu* [a (pronunciation in Sumerian) = (the sign) A = (the Akkadian) "to cry out"]. It consisted of forty-two tablets. Without trying to be overly precise, I have calculated that this number of tablets would hold *no more than half* of Herodotus' *Histories*. This is an aspect of cuneiform writing that has been much ignored by both Assyriologists and historians of culture, namely, the extent to which our literary record has been affected by the writing medium itself. I think this effect has been profound, and the authors of the early Islamic period, who do not manifest the laconic character of our cuneiform sources, support me here.

Cuneiform is a beautiful system of writing that appeals very much to the senses. It is a matter of profound pleasure just to watch the signs appear under one's stylus and to feel the plasticity of the clay in one's hand. However, even the most ardent devotee might well change his mind, were he required to carry a cuneiform copy of Herodotus from Babylon to Damascus on his back. In short, I think it was primarily the hostile cultural environments that spelled the end of both cuneiform and Egyptian hieroglyphic (which lasted on a for a few centuries longer), but with cuneiform the great irony is that the one feature which insured its death in antiquity just as the codex was being born—being written on clay—is what has preserved it for us to the present day.

One final point remains: to what extent did the replacement of cuneiform by the alphabet affect literacy as a whole? The answer to this has been a common sense one. The alphabet is supposed to have opened up whole new worlds and to have had an unparalleled impact upon human literacy. David Diringer's work on the alphabet (1968) is perhaps the best known example of the point of view, but it is to be met with everywhere. Somehow the theory does not hang together very well. The Japanese system hardly seems to have inhibited the development of literacy (to say nothing of technology!), where the mid-century illiteracy rate was estimated by UNESCO (1957:41) at 2-3%, less than that of the United States. On the other hand, Iraq, the homeland as I would believe of Near Eastern writing and acquainted with the alphabet for over two-thousand years, was found by the same UNESCO survey (1957:34) to have an illiteracy rate of 89.1%. Not a very good batting record for the alphabet.

Even more disturbing for the theory of the alphabet's importance are recent developments in China. Gelb (1952/1963:237f.) painted a dark picture of the "selfish bureaucratic clique at the top" of Chinese society maintaining the traditional Chinese script even in the face of rationality and utility, but, as French (1976;116) points out, this is a view which is characteristically "non-Chinese" and that "all currently available evidence contradicts [the] view" that the Chinese are about to abandon their traditional system in favor of a more "rational" one. In short, there are many problems associated with the traditional view that the alphabet represents a turning point in the history of mankind. There is definitely no evidence that it had a major impact upon literacy. Definition of literacy is itself a problem, as is determining who falls into the literacy categories once they have been set up (e.g., with bibliographies: Goody 1968; Lockridge 1974). These problems are simply ignored by those who argue for Athenian literacy from the fact that decrees of the boule and people were published on stone or from the well-known institution of ostracism. The election signs at Pompeii belong to this same category of undigested and absolutely inconclusive evidence: how many literate people out of a hundred do there have to be to make such signs effective? Hammurapi, King of Babylon (1792-1750 BC), also published his laws proclaiming:

> The man who has been wronged, who has a grievance, let him come before my statue, "King of Justice," and read forth my inscribed stele and hear my precious words, and let my stele show him the grievance; let him see/read his case/judgment: let his heart breathe freely...(Driver and Miles 1955:96f.)

But who would wish to deduce from this that most Babylonians were literate?

To sum up. Really sound statistics for literacy prior to the nineteenth century do not exist. Every estimate for literacy prior to this time must be partially a kind of intuitive process, i.e., putting together everything one has learned and experienced and, after weighing it all in the balance, reaching a conclusion. My conclusion is that, prior to the invention of the printing press in the fifteenth century, at least ninety percent of the populations of *literate* cultures were functionally illiterate. That is to say, some of the "illiterates" in some periods may have been able to write their names, but few of them will have been able to really *read*. As the experience of modern Third World societies has shown, such minimal skills as being able to write one's name *mit Müh und Not* is economically and technologically of little significance. The inescapable conclusion is that the introduction of the alphabet, by itself, has had little effect upon reduction of functional illiteracy, and thus, its importance in the history of human development has been overestimated, whereas that of cuneiform has probably been underestimated.

1. These examples can be multiplied. The last is a curious one. The sign which I have symbolized by BREAST is, in fact, not used in the Old Babylonian phase of Sumerian orthography to mean "breast" (Landsberger 1951:98f.). Rather a compound sign derived apparently from a goat's udder is used for this purpose; whereas the sign which I have transcribed BREAST is, from at least the middle of the third millennium, used to denote a noun (dumu = "child") and two adjectives (tur = "small" and banda = "little"). It is for this reason (and many others) that I must affirm the necessity of reconstructing the pictorial prototype of Sumerian signs. Misuse of this technique has brought it into disrepute. However, if the assumptions argued in this paper are correct, it should be clear that we will never understand the underlying structure of Sumerian writing unless we are willing to investigate these kinds of features.

2. The significance of the important sequence described by Le Brun and Vallat (1978:31) was called to my attention by D. Schmandt-Besserat (see her formulation of the situation above).

3. Ideally one should be an archaeologist, philologist, metrologist, mathematician, and socio-economic historian all rolled into one, but, alas, who is? Two small points from the critique by Le Brun and Vallat (1978:34) illustrate the pitfalls: 1) SU *never* means shekel; the sign in question is GÍN, which in some periods has an appearance similar to SU: they are not, however, identical; 2) the sign NINDA does *not* have "une valeur numèrale"; even its use in a metrological sense is attested only for the Neo-Babylonian period (over two thousand years later).

4. Since the evidence on which my inferences are based has been collected over the past eight years, I am indebted to a number of institutions and individuals for facilitating my work in various ways: the British Museum, Bryn Mawr College Library, the Free Public Library (Philadelphia), Harvard Semitic Museum, the Hermitage (Leningrad), the Louvre, New York Public Library, the Oriental Institute (University of Chicago), the Pushkin Museum (Moscow), the World Heritage Museum (University of Illinois, Urbana), and Yale Babylonian Collection; V. K. Afanasieva, G. van Buitenen, K. Deller, I. J. Gelb, W. W. Hallo, S. I. Hodjash, N. B. Iankovskaia, W. Moran, H. Nissen, H. Waetzoldt, and C. Wilcke. The International Research and Exchanges Board, the National Endowment for the Humanities, and Northern Illinois University all supported other work of mine, during which the evidence for the present paper also gradually accumulated. To all I express my thanks.

5. This problem has recently been treated from another point of view in a paper which became accessible to me after the present one was written: Sergio A. Picchioni, "La direzione della scrittura cuneiforme e gli archivi di Tell Mardikh-Ebla," Orientalia 49 (1980) 225-251. Picchioni's conclusions are similar to, though not precisely identical with, my own. I had the opportunity to discuss the problem with him recently at the Rencontre Assyriologique Internationale in Vienna (July 1981) and we are in agreement that cuneiform was read vertically down into the Akkad period (c. 2300 BC) and that the earliest irrefutable evidence for left-to-right reading is in the Kassite *kudurrus*. I subsequently made an additional search for earlier evidence among the published documents, but the earliest datable document I have found which incontestably shows left-to-right script is that listed by Brinkman (1976:265) as U.2.18, which is from the time of Nazimaruttaš (c. 1307-1282).

6. D. O. Edzard recently passed on to me the oral communication of a professional Sinologist to the effect that some examples of Chinese writing now seem to go back to about 2000 BC. This would, of course, fit the theory of monogenesis better than a date in the Late Bronze Age.

WORKS CITED

Adams, Robert M., and Nissen, Hans J. 1972
The Uruk Countryside: The Natural Setting of Urban Societies. (Chicago: University of Chicago Press)

Barr, James 1976
"Reading a Script Without Vowels" in W. Haas, ed., Writing Without Letters (Manchester: Manchester University Press), pp. 71-100

Biggs, Robert D. 1980
"The Ebla Tablets: an Interim Perspective," Biblical Archaeologist 43:76-87

Breasted, James H. 1916
"The Physical Processes of Writing in the Early Orient and Their Relation to the Origin of the Alphabet," The American Journal of Semitic Languages and Literatures 32:230-249

Brinkman, John A. 1976
Materials and Studies for Kassite History, vol. I (Chicago: The Oriental Institute of the University of Chicago)

Clay, Albert T. 1906
Documents from the Temple Archives of Nippur. The Babylonian Expedition of the University of Pennsylvania, Series A: Cuneiform Texts (Philadelphia: Department of Archaeology, University of Pennsylvania), pp. 17-20.

Diakonoff, I.M. 1975
"Ancient Writing and Ancient Written Language: Pitfalls and Peculiarities in the Study of Sumerian" in Stephen J. Lieberman, ed., Sumerological Studies in Honor of Thorkild Jacobsen on his Seventieth Birthday, June 7, 1974 = Assyriological Studies 20 (Chicago: University of Chicago Press), pp. 99-121

Diringer, David 1968
The Alphabet: A Key to the History of Mankind, 3rd ed., completely revised with the assistance of Reinhold Regensburger, 2 vols. (New York: Funk and Wagnalls)

Driver, G. R. 1976
Semitic Writing from Pictograph to Alphabet, 3rd ed., ed. by S. A. Hopkins (London: Oxford University Press)

Driver, G. R., and Miles, John C. 1955
The Babylonian Laws, vol. 2 (Oxford: At the Clarendon Press)

Edgerton, W. F. 1952
"On the Theory of Writing," Journal of Near Eastern Studies 11:287-290

Edzard, Dietz O. 1980
"Keilschrift" in D. O. Edzard, ed., Reallexikon der Assyriologie, vol. 5 (Berlin: Walter de Gruyter), pp. 544-568

Emery, Walter B. 1961
Archaic Egypt (Baltimore: Penguin Books)

Falkenstein, Adam 1936
Archaische Texte aus Uruk. Ausgrabungen der Deutschen Forschungs gemeinschaft in Uruk-Warka, vol. 2 (Leipzig: Kommissionsverlag O. Harrassowitz)

French, M. A. 1976
"Observations on the Chinese Script and the Classification of Writing-Systems" in W. Haas, ed., Writing Without Letters (Manchester: Manchester University Press), pp. 101-129

Friberg, Jöran 1978
The Third Millennium Roots of Babylonian Mathematics. I. A Method for the Decipherment and Metrological Analysis of Proto-Sumerian and Proto-Elamite Semi-Pictographic Inscriptions (Department of Mathematics, Chalmers University of Technology and the University of Göteborg, No. 1978-9)

Friedrich, Johannes 1966
Geschichte der Schrift unter besonderer Berücksichtigung ihrer geistigen Entwicklung (Heidelberg: Carl Winter, Universitätsverlag)

Gelb, I. J. 1952/1963
A Study of Writing, 1st ed. 1952, 2nd ed. 1963 (Chicago: University of Chicago Press)

Gelb 1958
"New Evidence in Favor of the Syllabic Character of West Semitic Writing," Bibliotheca Orientalis 15:2-7

Goody, Jack R., ed. 1968
Literacy in Traditional Societies (Cambridge: At the University Press)

Hall, H. R. 1928
Babylonian and Assyrian Sculpture in the British Museum (Paris: Les Éditions G. van Oest)

Kemp, Barry 1980
"Egyptian Radiocarbon Dating: A Reply to James Mellaart," Antiquity 54:25-28

Landsberger, Benno 1951
Die Serie Ur-e-a = *nâqu*, Materialien zum sumerischen Lexikon, vol. 2 (Roma: Pontificium Institutum Biblicum)

Le Brun, A., and Vallat, F. 1978
"L'origine de l'écriture à Suse," Cahiers de la Délégation Archéologique Française en Iran 8:11-59

Lieberman, Stephen J. 1980
"Of Clay Pebbles, Hollow Clay Balls, and Writing: A Sumerian View" American Journal of Archaeology 84:339-358

Lieberman, Stephen J. 1978
Letter to the Editor in Scientific American 239 November: 10-15

Lockridge, Kenneth A. 1974
Literacy in Colonial New England: An Inquiry into the Social Context of Literacy in the Early Modern West (New York: W. W. Norton and Company)

Mellaart, James 1979
"Egyptian and Near Eastern Chronology: A Dilemma?," 53:6-18

Messerschmidt, L. 1906
"Zur Technik des Tontafel-Schreibens," Orientalistische Literatur-Zeitung 9:185-196, 304-312, 372-380

de Morgan, J. 1905
"Note sur les procédés techniques en usage chez les scribes babyloniens," Recueil de Travaux relatifs à la philologie et à l'archéologie égyptiennes et assyriennes 27:234-249
Munn-Rankin, Margaret 1980
"Mesopotamian Chronology: A Reply to James Mellaart," Antiquity 54:128-129
Patterson, Archibald 1915
Assyrian Sculptures, Palace of Sinacherib (The Hague: Martinus Nijhoff)
Pettinato, Giovanni 1979
Ebla: un impero inciso nell'argilla (Milano: Arnoldo Mondadori Editore)
Powell, Marvin A. ca. 1982
"The Sumero-Babylonian City" forthcoming in Ronald T. Marchese, ed., The Ancient City
Powell 1978
"Ukubi to Mother . . The Situation is Desperate: A Plaidoyer for Methodological Rigor in Editing and Interpreting Sumerian Texts with an Excursus on the Verb taka:da_x-da_x(TAG_4)," Zeitschrift für Assyriologie 68:163-195
Powell 1976
"The Antecedents of Old Babylonian Place Notation and the Early History of Babylonian Mathematics," Historia Mathematica 3:417-439
Powell 1972
"Sumerian Area Measures and the Alleged Decimal Substratum,"Zeitschrift für Assyriologie 62:165-221
Reiner, Erica 1973
"How We Read Cuneiform Texts," Journal of Cuneiform Studies 25:3-58
Strommenger, Eva 1980
"The Chronological Division of the Archaic Levels of Uruk-Eanna VI to III/II: Past and Present," American Journal of Archaeology 84:479-487
Thureau-Dangin, François, and Dunand, Maurice 1936
Til-Barsib, Album (Paris: Librairie Orientaliste Paul Geuthner)
UNESCO 1957
World Illiteracy at Mid-Century: A Statistical Study. Monographs on Fundamental Education, No. 11 (Paris: UNESCO)
Vogel, Kurt 1958
"Ist die babylonische Mathematik sumerisch oder akkadisch?" Mathematische Nachrichten 18:377-382
Weinstein, James 1980
"Palestinian Radiocarbon Dating: A Reply to James Mellaart," Antiquity 54:21-28
Zehnpfund, Rudolf 1893
"Neu-Babylonisch-Assyrische Tafelschreibung" in Actes du huitième congrès international des Orientalistes, tenu en 1889 à Stockholm et à Christiania, Deuxième partie, Section Sémitique I B (Leiden), pp. 267-272

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