

## Epistemology and Sumerian Agriculture: The Strange Case of Sesame and Linseed

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... et malgré son importance cet aspect épistémologique de la recherche n'est pas le sujet central de ma communication.

(Miguel Civil 1980)

My original intention for this paper in honor of our admired friend and colleague Miguel Civil was to provide an overview, a map, as it were, of the ancient agricultural landscape. This proved impracticable. The bibliography itself grew like weeds in a sesame patch. So, instead, I turned my efforts to the sesame field, in which Miguel himself has put in good labor. The problem also attracted my attention, because if we cannot distinguish sesame from linseed, as some maintain, then all hope of knowing anything about Sumerian agriculture – and about numerous other things – is vain.

I owe Miguel more than I can adequately express for years of sharing with me his wonderful knowledge of things Sumerian. Some points in this little essay would surely have benefitted had I discussed them with him, which for obvious reasons I did not. So I send it on its way as a small token of the affection that all of us who know Miguel personally must feel.

The still debated identification of *Sesamum indicum* in cuneiform sources is as interesting for the epistemology of history as it is for the history of agriculture. I hasten to add that my primary concern is not epistemology, but, of course, *what* we know about the past is never really separable from *how* we know it. How do we *know* that Sumerian še.giš.i<sub>3</sub> and Akkadian *šamaššammū* mean sesame and not linseed? In principle, we know it by following standard procedures in historical method and by logical analysis. As long ago as 1976, Miguel Civil himself, using these methods, pointed out two reasons why še.giš.i<sub>3</sub> must mean sesame and not linseed. Nevertheless, doubts persists about whether *Sesamum indicum* has been securely identified in cuneiform sources.

I begin by summarizing my conclusions: (1) the probability that Sumerian še.giš.i<sub>3</sub> and Akkadian *šamaššammū* denote *Linum usitatissimum* is minimal; (2) the probability that these words denote *Sesamum indicum* is within the range of what is usually considered to be *fact*; (3) therefore, sound historical method requires that we operate with the hypothesis that še.giš.i<sub>3</sub> and *šamaššammū* denote sesame, not linseed.

Such a clear-cut affirmation will doubtless bring to the minds of cuneiformists the long debate about whether *an.na* and *annakum* mean lead or tin. Until recently, most of us assumed that this controversy had been decided in favor of tin through the arguments presented in 1965 by Benno Landsberger. However, in 1982 Helmut Freydank and Manfred Müller demonstrated that the problem had not yet been solved, and I was recently forced to conclude that "AN.NA when used as money cannot mean tin in Nuzi contexts" (Powell 1990:105-107 with lit.). How then, one may ask, can you be so sure that *še.giš.i<sub>3</sub>* and *šamaššammū* denote sesame and not linseed? That is what I propose to answer below.

We begin with the recent work on plant domestication by Daniel Zohary and Maria Hopf (1988:126), who come to the conclusion that sesame is not attested until the first millennium BC, that "most workers now contend that these terms [i. e., *šamaššammū* and *še.giš.i<sub>3</sub>*] denote 'oil plant' in general and do not provide clear evidence for the cultivation of *S. indicum* in 2nd millennium BC in Mesopotamia", and further that "[s]easonal considerations are not clear either". If I object that none of these propositions can be supported by the evidence, the botanists will quite reasonably point to the recent judgment by the *Chicago Assyrian Dictionary* (= CAD) that *šamaššammū* is "probably flax" (Š/I 1989:301; the volume became available in the USA only in late 1990) as evidence that I obviously do not know what I am talking about.

Of course, as rejoinder I could point out that Wolfram von Soden maintained the identity of *šamaššammū* with sesame throughout his *Akkadisches Handwörterbuch* (1958-1981). Moreover, the literature suggests that a majority of cuneiformists have never embraced the idea that *šamaššammū* means linseed. However, majority opinion – though it offers the comfort of company – does not guarantee the correctness of any position, and I would no more wish to convince our botanical colleagues by counting noses than I would accept as valid evidence – even if it were true – the botanists assertion that "most workers now contend that these terms denote 'oil plant' in general".

Let us go back to the origin of this "Babel of tongues". It all began with Hans Helbaek's novel argument (1966) that the Sumerian and Akkadian words *še.giš.i<sub>3</sub>* and *šamaššammū*, which had previously been interpreted as "sesame", really denoted "linseed". Up until that time, CAD had always translated these words as "sesame". However, the effects of H. Helbaek's theory began to manifest themselves in volume K (published 1971), where under *kakkū* (K:58) the word is interpreted as "sesame" but under *kasū* (K:249) as "linseed".

One must at least credit CAD with consistency, because it has maintained "flax" or "linseed" in subsequent volumes published in 1977 (M/I:49, 244-245, 251 under *maḥāḥu*, *maqātu*), 1980 (N/I:286, N/II:56-57 under *napāṣu*, *našāpu*), and even in 1984 (S:160 under *sapānu*) and, as we have already seen, most recently under *šamaššammū* itself, although in 1982 (Q:285-286) CAD has deduced that the word for "flax plant" in Akkadian is really *qū*, corresponding to Sumerian *gu*, both of which also mean "thread/cord". In short, here CAD agreed with the conclusion already reached by W. von Soden in 1971 (*Akkadisches Handwörterbuch* pp. 924-925).

The significance of this conclusion has been overlooked by those who continue to doubt the identity of *šamaššammū* with *Sesamum indicum*, because it identifies the *qū*-plant with fiber production to make linen (Sumerian *gada*, Akkadian *kitū*), not with seed production for oil. Of course, the botanists could ask: how do you know that this *gu/qū*-plant means *Linum usitatissimum*? Here again, we could count noses, but, for the sake of agricultural epistemology, I shall summarize how we know it: (1) by the name, which denotes fiber; (2) by the presence of linseed in the archaeological record from the 8th millennium onward coupled with attestation of linen from before 6000 BC (H. Waetzoldt 1983:583; J. M. Renfrew 1985; Zohary & Hopf 1988:115-119); (3) by cultivation patterns attested in written sources, which tell us that it was not a field, but a garden crop, planted in the fall and harvested in the spring (Waetzoldt 1985:78; P. J. LaPlaca & M. A. Powell 1990:85-86, 90, 94, 102); (4) by the use of this plant to make linen cloth, the

meaning of which is assured by other Semitic words for linen that are related to Akkadian *kitû* as well as by numerous contextual passages (Waetzoldt 1983); (5) by the absence from the archaeological record of any other fiber plant with which it could conceivably be identified.

We may summarize the evidence for *Linum usitatissimum* in cuneiform sources as follows. The name for the plant itself – as well as for “thread”, “cord”, “net” – was *qû* in Akkadian, *gu* in Sumerian. In Sumerian the name for the unretted stalks was also *gu*, but in Akkadian everything from this point on up through the cloth itself seems to have been known only by the name *kitû* (CAD K 1971: 473-475). This word clearly denotes our word “linen”, i.e., cloth made from flax fibers, but there is reason to suspect that both this word and the Sumerian *gadu* go back to a still older neolithic word which denoted “something woven”. I cannot discuss this here, but this too fits the overall pattern of both written and archaeological evidence, which points to *Linum usitatissimum* as the primary fiber plant of the ancient Near East. Finally, a startling clue that *šamaššammû* cannot mean “linseed” is this: names for the seed of *Linum usitatissimum* are known, and they are called in Sumerian *numun gu*, “seed of flax/thread”, in Akkadian *zêr kitê*, “seed of linen”. Moreover, neither of these “flax seed” or “linseed” is ever entered in the ancient lists of synonyms as a synonym of *še.giš.i<sub>3</sub>* or *šamaššammû*.

How then, one must ask, if the evidence against H. Helbaek’s theory is so compelling, have cuneiformists and botanists alike gotten themselves into such a muddle? The answer: by not taking account of the evidence and by not thinking the matter through logically. I confess my own culpability when I wrote that “*še-giš-i/šamaššammû* may be a generic term for oilseed plants, including flax” (Powell 1984:65). This was hedging. And the same, I believe, is true of Nicholas Postgate’s article (1985; cited by Zohary & Hopf 1988:126) on something he called the “oil-plant” in order, as he put it, “to avoid committing myself in advance to a botanical definition”. However, our hedging has turned out not to be a logical option.

Part of the muddle has to do with cuneiformists’ own ambivalence about the evidential value of etymology. True, the evidential value of etymology is “limited”, but this does not say much, because it shares that characteristic with every other piece of evidence. More pertinent is that the evidential value of etymology is never *nil*, and, as a fundamental rule of scientific procedure, etymological identity always creates an *a priori* claim in favor of that identity. In other words, if sufficient etymological grounds exist to posit the identity of *šamaššammû* with *Sesamum indicum*, an alternative theory – in order to be logically valid – must first establish the likelihood that *šamaššammû* is indeed something else.

Of course, one may have legitimate doubt about the validity of an etymology. But, in that case, methodological rigor requires that we examine our underlying hypotheses and spell out our suppositions. Otherwise, ill-founded doubts – which are a “cheap fix” anyway because they require no labor – are likely to lead one into the logically absurd positions represented by my hedging or by H. Helbaek’s theory that sesame was really linseed.

Let us turn now to H. Helbaek’s original argument. It can be reduced to four loosely related – but not logically interrelated – propositions: (1) *Linum usitatissimum* (linseed) is attested in the archaeobotanical record; (2) *Sesamum indicum* (sesame) is not attested in the archaeobotanical record; (3) *še.giš.i<sub>3</sub>* and *šamaššammû*, which have been translated by cuneiformists as “sesame”, really mean linseed; (4) the word “sesame” was transferred from *Linum usitatissimum* to *Sesamum indicum* about a thousand years ago.

The logical structure of Helbaek’s argument can be analyzed as three sets, each consisting of two propositions and a conclusion. The basic set is: (1) the oilseed linseed has been found in the archaeobotanical remains in the Near East; (2) the oilseed sesame has not been found there prior to ca. 1000 AD; (3) therefore, if an oilseed occurs in written documents in the Near Eastern or Mediterranean area prior to ca. 1000 AD, it must be linseed.

Strictly speaking, this conclusion is not logical, and Helbaek tried to deal with this in a second argument: (1) an oilseed does occur in cuneiform texts; (2) the cuneiform texts call that oilseed “sesame”; (3) therefore, the “sesame” in cuneiform texts must mean the linseed in the archaeobotanical record.

This, too, is not strictly logical, so Helbaek supported it with his third set of assumptions and conclusion: (1) the "sesame" in cuneiform records, which is really linseed, continued to be called "sesame" until about a thousand years ago; (2) about a thousand years ago when "real sesame" was introduced from India the name "sesame", which really denoted linseed, was applied to "real sesame"; (3) therefore, prior to ca. 1000 AD, "sesame" really denoted linseed.

H. Helbaek's argument can be rebutted by refuting any one of the propositions. F. R. Kraus (1968), as a philologist, focused on the weak spot in the third set of propositions and brought the following counter argument: (1) "sesame" words exist in various languages prior to and after 1000 AD; (2) even if we assume that they all meant *Linum usitatissimum* prior to 1000 AD, it is improbable that any two of these words would have independently undergone exactly the same shift in meaning from flax to *Sesamum indicum*; (3) "sesame" words do exist before and after 1000 AD in both Greek and Aramaic, therefore Helbaek's theory is improbable.

Stripped of its orientalist paraphernalia, it can be seen that F. R. Kraus's argument is no more logical than H. Helbaek's which perhaps accounts for why it has failed to convince a minority of the profession. At bottom, it still rests primarily upon etymology, for Kraus was neither able to *demonstrate* that the word meant sesame in Aramaic either before or after 1000 AD nor trace the development of the word in Greek. Essentially, this part of Kraus's argument is just a declaration of faith in the value of etymology as a heuristic tool.

Moreover, even an admirer of F. R. Kraus such as I must admit that he himself (1968:112) introduced confusion by conceding H. Helbaek's contention that "the philologist ... has mainly etymological criteria for guidance" and then promptly turning around and adducing *contextual*, not etymological, evidence. Kraus also went on to say that he was not going to attempt a botanical identification of še.giš.i<sub>3</sub>/šamašammū and then proceeded to do precisely that. If cuneiformists cannot present an accurate picture of what they are doing, it should not surprise us that the botanists are mystified.

Nevertheless, F. R. Kraus did show that there was good reason to doubt the validity of H. Helbaek's proposition that "sesame" words somehow miraculously all shifted from *Linum usitatissimum* to *Sesamum indicum* around 1000 AD. In fact, Helbaek never tried to demonstrate the truth of this proposition but merely stated it as *credo*. More important, Kraus also went on to point out that neither the Greek nor the cuneiform evidence favored identification of "sesame" words with *Linum usitatissimum*. This was sound procedure, because it strengthened the *a priori* etymological claim for sesame and reduced the *probability* that šamašammū could *accidentally* be linseed.

A few years later Miguel Civil, in a brief aside (1976:141), further reduced the probability of H. Helbaek's theory being *accidentally* correct by pointing out that Sumerian cuneiform sources from the Ur III period distinguish "sesame farmers" (engar giš.i<sub>3</sub>-ka-ke<sub>4</sub>-ne) from "flax farmers" (engar gu-ke<sub>4</sub>-ne), and, more important, he also brought the first direct refutation of Helbaek's argument by pointing to reported finds of sesame from ancient Urartu. Since Helbaek's whole theory stands or falls on the postulated *absence* of sesame from the archaeobotanical record, namely the failure of archaeologists to turn up "one single find of a sesame seed anywhere in the Near East", this constituted sufficient rebuttal of the theory as a whole.

More recently, sesame and flax were discussed extensively during the Third Sumerian Agriculture Group Meeting (Cambridge, 1984). Out of this discussion came endorsements of the arguments already advanced by F. R. Kraus and M. Civil together with additional evidence commending the identification of šamašammū with sesame and contravening the identification with linseed. Those arguments are published in the papers by Dorothea Bedigian (1985; cf. also D. Bedigian & J. R. Harlan 1986), an agronomist who has made a special study of the history of sesame and who adduces experimental evidence that carbonized sesame seed are much more fragile than carbonized linseed; by Hartmut Waetzoldt (1985), a Sumerologist with an unusually good knowledge of the realia embodied in the Ur III materials who has studied both sesame and flax and has specifically emphasized that the Ur III evidence for še.giš.i<sub>3</sub> is

commensurate *only* with a hypothesis of summer planting and fall harvest of še.giš.i<sub>3</sub>; and by Marten Stol (1985), an Assyriologist particularly conversant with the realia of the Akkadian material who has brought additional evidence "compatible" with sesame. The paper, already cited, by Nicholas Postgate (1985) points in the same direction; and, finally, T. W. Gallant (1985) has briefly discussed sesame and flax in Greek and Latin sources and pointed out that the different methods used for processing sesame (boiling, soaking) and linseed (roasting) could be one reason why so few sesame seeds have been found vs. so many carbonized linseed.

These conclusions were arrived at in the full knowledge that the archaeological evidence for sesame cultivation in ancient Iraq is *nil* (summed up by J. M. Renfrew 1985). However, the botanists D. Zohary and M. Hopf (1988:127) have now noted additional archaeological evidence for sesame in the part of Urartu near Lake Van, i.e., in a pre-600 BC horizon, a piece of evidence sufficient in and of itself to refute Helbaek's thesis. Finally, we should note that Egyptian finds of sesame seed were long ago reported from the First Intermediate Period and from the 18th Dynasty, and, even though Renate Germer (1985:171-172) seems a bit hesitant about the earlier find, she notes that sesame seeds are fairly easy to recognize and therefore concludes that the 18th Dynasty find from Deir el Medineh was probably correctly identified. It is also worth noting that the time period from which the first Egyptian find is reported (i.e., late 3rd millennium) fits with what we would expect from the cuneiform evidence.

In short, all is still well with the historical hypothesis that *šamaššammū* is *Sesamum indicum*: all of the known pieces of evidence fit the theory, and there are no valid counter indications. Precisely the opposite is true of the linseed hypothesis. No one, aside from H. Helbaek, has ever presented an argument in favor of identifying *šamaššammū* with *Linum usitatissimum*. Since the key assumption – absence of sesame from the archaeobotanical record – has proven factually false, this means that the only way Helbaek's conclusion could be correct is by accident. By the same logic, Hammurabi could *accidentally* be Samsuiluna or even Sargon of Akkad. Thus, sound historical method requires the assumption that *šamaššammū* is *Sesamum indicum*. All disciplines operate with "facts" like this. Otherwise we would spend all of our time proving the obvious.

This elemental principle of historical procedure does not, of course, exclude *legitimate doubt*. Serious doubt plays an important role in the scientific process of discovery, because it focuses attention upon weak spots in the dam, those "soft facts" in what is really an interlocking network of theory. H. Helbaek's theory was prompted by just this kind of serious doubt, which arose from the total absence of sesame in the archaeobotanical evidence then known to him, and he advanced an argument to explain it.

However, from the standpoint of historical method, H. Helbaek made four serious mistakes. First, he offered an argument based primarily on absence from the archaeological record. The very fact that this argument was already in error when it was made underscores its limitations, a phenomenon with which all ancient historians are familiar. Second, Helbaek offered an etymological argument without troubling himself to examine the philological evidence, which, in fact, did not support his theory. Third, Helbaek posited that all "sesame" words prior to ca. 1000 AD denoted *Linum usitatissimum* without troubling himself to examine the contextual evidence for sesame and linseed in cuneiform, Greek, or Latin sources, none of which supports his theory. Fourth, Helbaek assumed as a methodological postulate that flax was being grown for oil without ever examining the basis of that postulate.

This last error accounts for much of the confusion that prevails. Originally I thought that H. Helbaek's linseed theory was just a passing thought that came to him in the course of writing up his account of the plant remains from Nimrud, the sort of thing we all do in off moments. But, as I worked deeper into the literature, a different picture emerged. It was, after all, Helbaek who investigated and interpreted the gut contents of the "bog people", which included sizable proportions of linseed (J. M. Renfrew 1973:124 with lit.). And it can hardly be anything but Helbaek's ideas that flax was grown for food, oil, and fiber that has led Jane Renfrew (1973:5) to believe that flax was "probably first cultivated

for its oily seeds" and to infer (1973:124) that the Latin sources support the idea that flax was grown for food. On the contrary, Columella (2.7.1) does not mention flax as a food plant (unless one is willing to take *cannabis* as food), and, in the midst of a large section dealing with flax as a fiber crop, Pliny has only a single nineteen-word sentence (19.3.16) mentioning medicinal, food, and religious use of linseed. It is hardly necessary to underscore the fact that this (and other) evidence from the ancient written sources agrees neatly with Gordon Hillman's reinterpretation of the gut contents of the "bog people" as "famine food" (Hillman 1981:156-158; A. J. Legge 1989:223). Archaeological evidence, like the cuneiform evidence, is after all merely *evidence* requiring hypotheses for its interpretation.

That flax was cultivated primarily for fiber emerges not only from cuneiform, but also from Egyptian (R. Germer 1985:100-102), Greek, and Latin sources. Medicinal, food, and other uses are incidental. Even Theophrastus (ca. 370-285 BC) fails to mention linseed oil, noting only that linseed are like the seed of the *paliouros* (*Zizyphus spina-christi*), which, says Theophrastus (*Hist. Plant.* 3.18.3), are brayed by physicians and used as a cough remedy because the seeds contain something sticky and oily like linseed. Renate Germer (1985:101 with ref. to Lucas *Materials*<sup>4</sup> 329) finds the earliest Egyptian reference to linseed oil in a Greek papyrus of the Graeco-Roman period. This suggests a date *no earlier than* ca. 300 BC for the use of linseed for oil in the ancient Near Eastern or Mediterranean area, and Pliny's failure to mention use of linseed for oil in his *Natural History* (completed 77 AD) points to a still later date. As we have seen, Pliny does mention incidental use of linseed for medicine, food, and ritual purposes, but the complete absence of any mention of linseed used for oil in the *Natural History* (i.e., 19.1-6 or elsewhere) is a fact which those who believe that linseed was grown for oil are obliged to explain.

In short, H. Helbaek's linseed argument is without foundation. He never attempted to apply the theory to the evidence. Only CAD has done that, and, predictably, it confirms the unreliable nature of the basic hypothesis, because one of the characteristic features of an erroneous theory is that, when applied, it leads to one contradiction after another.

Contradictions are apparent from CAD's first attempt to apply the theory. Under *kasû* (CAD K 1971:249) we find "millet" and "linseed" offered as translations for *duḥnu* and *šamaššammū* in Neo-Babylonian contracts from the 6th-5th centuries BC. Why does CAD translate *duḥnu* as millet? Answer: etymology. But the etymological reasons for identifying *duḥnu* with millet are hardly more compelling than those for identifying *kitû* with linen or *šamaššammū* with sesame.

If doubts of a botanist are sufficient grounds to reject the identity of sesame with *šamaššammū*, then, by the same logic, one ought also to reject the identification of *duḥnu* with millet. After all, the botanists D. Zohary and M. Hopf insist (1988:78) that "[s]olid signs of the cultivation of *P. miliaceum* in the Near East appear very late", namely in the finds of millet reported by H. Helbaek from 7th century BC Nimrud in Assyria. By Helbaek's logic, if one is going to reject the obvious etymological identity of *šamaššammū* with sesame, then one should also reject the obvious etymological identity of *duḥnu* with "millet", or argue that *duḥnu* somehow changed its meaning in the thousand years since it first turns up in Akkadian texts at Nuzi.

The botanists rely, however, on the two-edged argument from silence, and their rejection of the evidence of etymology has entangled them in the same web of contradictions in which H. Helbaek and CAD were caught. Thus, while ignoring Akkadian *duḥnu*, D. Zohary and M. Hopf are nevertheless prepared to affirm (1988:76) that *Panicum miliaceum* is "the Romans' *milium* and the Hebrews' *dokhan*", another example of how contradictions multiply themselves when one operates with erroneous theories. I cannot but wonder if the botanists of Helbaek's persuasion have considered what a large portion of the work on historical botany must be ripped out and thrown away if one rejects the validity of etymological evidence.

Some type of "millet", probably *Panicum miliaceum* to judge by the sparse archaeobotanical evidence, is attested in Akkadian as *duḥnu* together with sesame (*šamaššammū*) in a court protocol (not a letter!) from Nuzi (Kirkuk region, northeast Iraq) written in the 15th century BC – and in a context which



seems to imply simultaneous planting. This has been known for over half a century (R. H. Pfeiffer & E. A. Speiser 1936 no. 1). Nevertheless, H. Helbaek found the discovery of “[s]ome 800 cubic centimetres of millet” at Nimrud “most surprising” (1966:615).

Had Helbaek been well-informed about the occurrence of millet in Akkadian texts from roughly the 15th century onward, would he not have found the millet at Nimrud a *satisfying confirmation* of what he as a historian of agricultural botany should have expected? After all, the single impression of a millet seed which he had identified on a pottery lid from Jemdet Nasr must have struck deep root in his mind, because he specifically mentions it in the context of the “most surprising” find of millet at Nimrud as evidence for cultivation of millet in alluvial southern Iraq around 3000 BC (1966:615; see J. M. Renfrew 1984:38-39 for lit.). Moreover, D. Zohary and M. Hopf (1988:76-78, with lit.) cite 5th millennium finds of common millet stretching from the Ukraine into central Europe and as far away as Tepe Yahya in southern Iran, with 4th millennium finds as far apart as central Europe and China, but, as we have seen, they are skeptical about its cultivation in the Near East prior to the first millennium BC.

Given the well-established Mesopotamian connections evidenced at Tepe Yahya, the *real* picture of millet cultivation in the ancient Near East probably looked rather different from the current *archaeological* picture. In short, common millet (*Panicum miliaceum* L.) is another case, like sesame, where the archaeobotanical finds do not yet quite mesh with the written evidence, but, just as Sumerologists are now obliged to look seriously for millet in Sumerian sources, so are archaeologists and palaeoethnobotanists obliged to look for sesame in Ur III or Akkad period context. New techniques borrowed from soil science seem to offer the best chance of retrieving such evidence.

But let us return to *CAD*'s attempt to turn sesame into flax and to our demonstration that a bad theory hatches only trouble. Trouble is not far to seek in a translation like “of the ninety gur of flax [i.e., *šamaššammū*] ... before the rain I had crushed(?) [= the verb *napāsu*] forty gur of it, the rain did not touch it” (*CAD* N/I 1980:286). Mention of gur alone – a 300-liter capacity(!) measure – shows that flax cannot be intended, because, as we would expect, flax was measured in “bundles” and “hands”. Coupled with this metrological contrariety is the fact that real flax is harvested in the late spring when the chance of rain is minimal. That this passage fits sesame better than linseed was long ago pointed out by F. R. Kraus (1968:117), and this has been reiterated recently by D. Bedigian (1985:167; Bedigian & Harlan 1986:145).

The *CAD*'s attempt to apply Helbaek's linseed theory is, in fact, an excellent substantiation that *šamaššammū* is really sesame. In ancient Mesopotamia, both sesame and flax were harvested by pulling (Akkadian *nasāhu*), so this is not distinctive. However, in the case of sesame this had to take place when the lower pods were ripe but the others still green, because as sesame pods ripen they eventually open and scatter their seeds of their own accord, i.e., dehisce, Akkadian *maqātu*, which *CAD* (M/I 1977:244-245, 251) – trying to escape the obvious – translates “diminish”. After drying, the remaining pods must be thrashed, and this is the verb *napāsu* already mentioned above, which *CAD* has been forced to interpret – obviously with some misgivings – as “to crush” flax.

One final example of *CAD*'s attempt to apply Helbaek's erroneous hypothesis to the Akkadian evidence will suffice. Under *maḥāhu* (M/I 1977:49), *CAD* interprets the phrase *še.giš.i, adi šukūdam tammaru lā tamahḥah* from an Old Babylonian letter as “do not soak the flax seeds before you see Sirius”. The sighting of Sirius is assumed to refer to the heliacal rising of the Dog Star, a reasonable assumption, since it marked the beginning of the inundation in Egypt and occurs in the Mediterranean agricultural writers from Hesiod through Columella as a sign of high summer. Dorothea Bedigian (1985:171) gives the date for the rising of Sirius supplied by her informants as June 22 in 3000 BC. This agrees with calculations for the Old Babylonian period in the latitude of Babylon generously made for me by Phyllis Pitluga, Senior Astronomer at the Adler Planetarium in Chicago. On July 1, 2000 BC, Sirius would have been visible with the sun at 9° 55' below the horizon, and visible on July 4, 1600 BC, with the sun at 9° 54' below the horizon. Though the precise date of visibility would have varied according to

atmospheric conditions, it is clear that, for the Old Babylonian period, the heliacal rising of Sirius means roughly the beginning of July.

"Soaking" flax seed in midsummer does not make sense from any point of view: soaking linseed in water produces prussic acid, which is poisonous. What conceivable purpose could be envisioned in "soaking" flax seed in high summer when daytime temperatures are apt to be 40°C? F. R. Kraus, who first discussed this passage (1968:116) tried to make sense of it by assuming that the sesame was being "soaked" prior to planting to stimulate germination. The *CAD* has borrowed Kraus's idea and has come to the conclusion that *mahāhu* means "to soak seeds preparatory to planting", but this leads to another contradiction, because linseed germinates easily (I. Arnon 1972:395).

Moreover, F. R. Kraus's idea is a good example of how the GIGO (Garbage-In Garbage-Out) principle applies not merely to computers but also to the answers one gets from professional colleagues. Kraus read in Pauly-Wissowa's *Realencyclopädie* that, according to Theophrastus (*Hist. Plant.* 8.6.1), sesame had difficulty germinating and inferred that *mahāhu* must mean soaking the sesame seed before planting to stimulate germination. Kraus thought he had found support for his idea from his Leiden colleague H. Gloor who informed him that certain seeds which germinated with difficulty, such as some pulses, were soaked up to three days before sowing.

Alas, Kraus fell into one of those snares that lie in wait for all of us when we are too pressed for time to check the evidence. What Theophrastus really says is: most crops germinate well if they get a rain after sowing, except in the case of some, among which he mentions sesame (*sēsamon*). And, of course, this accords precisely with what the modern literature says about sesame (e.g., Arnon 1972:381). So, Kraus's idea about soaking sesame was erroneous from the beginning, which means that *CAD*'s inference that "soaking" refers to linseed has only an *accidental* possibility of being correct. Considering the fact that none of Theophrastus's references to *sēsamon* can mean linseed, as Helbaek's theory would require, but only sesame, as description of the unusual nature of the pod in *Hist. Plant.* 3.18.13 (tetracarpellate!) and 8.5.2 makes explicit, and adding to this the fact that "soaking" either sesame or linseed also rests on misconceptions, the accidental chance that *CAD* could be correct is very remote indeed.

Before taking a final look at *mahāhu*, let us review a few of the characteristics of sesame noted in I. Arnon's handbook *Crop Production in Dry Regions* (1972:381-384): "Sesame has high heat and light requirements ... is extremely sensitive to excess soil moisture ... is considered an extremely desirable preceding crop for wheat, as it does not exhaust the soil, but improves soil structure .... In irrigated crop rotations, sesame, with its short growing season, can be a useful catch-crop, that will make efficient use of soil moisture left by the preceding crop .... Sesame should not be sown until soil temperatures are at about 20°C. The seed must be sown in a moist seed-bed .... In the Near East, sesame is sown in late spring ... through a funnel attached to the native 'nailplough', which moves aside the dry top-soil, opening a shallow furrow so that the seed is placed in the moist layer underneath .... Weeds are a major problem in sesame production because the young plants at first develop very slowly and cannot compete with weeds .... When irrigation is needed, the first one should be applied before sowing, followed by two to four light irrigations during growth".

Thus, in the light of the discussions of sesame by H. Waetzoldt (1985) and D. Bedigian (1985), "do not *mahāhu* the sesame until you see Sirius" probably refers to the first irrigation after sowing. Once we realize that *mahāhu* denotes some type of irrigation, a number of loose ends fall into place. The *CAD* (M/I 1977:49) came very close to recognizing this by isolating a meaning "to soak soil", but its commitment to Helbaek's theory prevented it from seeing the obvious: the "soakings" of soil, sesame, leeks, and seed quoted under *mahāhu* (meanings 1.a, 1.b, and 4), probably all refer to some form of "light irrigation", perhaps furrow irrigation.

This brings us another small step forward in the study of Sumerian agriculture, for  $e_3$ , one of the Sumerian synonyms of *mahāhu*, is probably identical with the  $e_3$  that occurs in the late 3rd millennium irrigation term  $a_{e_3}$ -a (H. Sauren *Topographie Umma*, Diss. Heidelberg 1966:230; P. Steinkeller



*BulSumAgr* 4 1988:74-75; H. Waetzoldt *BulSumAgr* 5 1990:14). This means that we must eventually reexamine the sequence of agricultural activities *mahāhu*, *pašāru*, *šullušu*, because these probably do not have the meanings posited by B. Landsberger (*Materialien zum sumerischen Lexikon* 1 1937:186) and, in general, accepted by the dictionaries. In particular, *pašāru*, "loosen", may refer to weeding (to judge by the Sumerian synonym *bur<sub>2</sub>*, and *šullušu*, "do a third time", probably denotes whatever agricultural activity came third in this sequence.

The long debate about the identity of sesame and linseed encapsules both the limitations and the challenge of cuneiform sources. H. Helbaek (1966:618) was partly correct in saying that cuneiform sources lack "specific descriptions" enabling "correlation of the Sumerian or Babylonian names with the specific Latin ones". One sentence in a reliable narrative can tell us what a thousand documents will not, and the paucity of narrative sources in cuneiform means that, reconstruction of Sumerian agriculture – and of many things Mesopotamian – will always entail much labor and leave many questions unanswered (so do other ancient – and modern – sources!).

However, contrary to Helbaek's idea that cuneiformists are at the mercy of etymology, cuneiform sources do indeed contain other kinds of evidence. As we have seen, etymology itself is not to be despised, because, after all, it pointed the way to the correct identification of sesame. True, we have no Theophrastus or Pliny, and no single cuneiform source tells us that *še.giš.i<sub>3</sub>/šamaššammū* is planted in the late spring, harvested in late summer or fall, is irrigated and weeded in the summer, that from its seeds (sometimes of a white color) a versatile oil usually amounting to about 20 percent of their volume was extracted, and that ants probably did not like it. Nevertheless all of this is either implicitly or explicitly there in the cuneiform sources. Moreover, they also tell us that sesame appears for the first time around the end of the 3rd millennium BC, that is, in an era when both archaeological and written evidence points to contacts with the Indus Valley, and that there are philological reasons to suspect that sesame was introduced from some other country.

Cuneiform sources also tell us about flax: it was planted in the fall, harvested in the spring, measured in "bundles" or "hands" in the raw state, and used for making thread, cord, nets, and linen cloth, which is measured by weights and lengths. They also tell us that the seeds of this plant were called "seed of thread" and "seed of linen". Moreover, from early Sumerian sources, we learn that this plant was grown in gardens, probably on the levee slopes, and apparently only in small plots, which suggests that the Sumerians were already familiar with the natural sensitivity of flax to salinity and probably also with Flax Wilt (*Fusarium lini*), which makes it necessary to constantly change the plots in which flax is grown (cf. Arnon 1972:393-399).

To sum up: the identity of *še.giš.i<sub>3</sub>* and *šamaššammū* with *Sesamum indicum*, on the one hand, and the identity of *gulqū* and *kitū* with *Linum usitatissimum*, on the other, are as certain as most of the "facts" we know from antiquity. To deny this is to paint ourselves into a Cartesian corner from which the only escape is the *deus ex machina*. Perhaps the most important lesson to be learned from this little episode in agricultural epistemology is that both cuneiformists and palaeoethnobotanists have to make more effort to understand the epistemological bases of the other discipline.

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