



Wolf Leslau during the first field trip to Ethiopia in 1946.

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WOLF LESLAU

On the occasion of his eighty-fifth birthday
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"trinken" denn auch überliefert. Für beide Wörter existieren aber auch Formen mit Vorschlagsvokal, nämlich *eštā* bzw. *eštī*, beide mit Explosiva.¹³ Beim Zahlwort ist offenbar Ausgleichung in Richtung Explosiva erfolgt. Warum sie beim Verbum nicht eingetreten bzw. nicht überliefert ist, kann man nicht wissen. Möglicherweise handelt es sich nur um Grammatikersystematik, also nicht um eine Frage der lebendigen Aussprache, sondern der Orthographie bzw. Punktation.¹⁴

Anhangsweise sei daran erinnert, daß auch das Zahlwort für "fünf" im Aramäischen eine Besonderheit aufweist, indem nämlich die Maskulinform gegenüber der Femininform *ḥamšā* nicht wie zu erwarten **ḥmeš*, sondern *ḥammeš* lautet. W. Diem hat in seinem Beitrag zur Festschrift für R. Macuch (*Studia Semitica necnon Iranica* 1989) "Syrische Kleinigkeiten" S.68 ff. *ḥammeš* richtig als Mask. zu einem als *ḥammšā* zu interpretierenden Fem. *ḥamšā* erklärt, wobei die Nachbarzahl "vier" *arbaʿ* bzw. *arbʿā* im Rahmen einer Proportionalanalogie das Strukturmuster liefert. Man kann hinzufügen, daß die Korrespondenz *ḥammšā* : *ḥammeš* durch strukturell gleichgelagerte, wenn auch einer anderen morphologischen Kategorie angehörende Fälle wie *ʿemmrā* : *ʿemmar* "Widder", *šebblā* : *šebbal* "Ähre", *šepprā* : *šeppar* "Vogel" gestützt wird.¹⁵

Addendum

Fast ein Jahr nach Ablieferung des Druckmanuskripts kam mir durch die Freundlichkeit von Prof. Rainer Degen ein Artikel von R.D. Hoberman "Initial Consonant Clusters in Hebrew and Aramaic", JNES 48 (1989) 25-29 zur Kenntnis, der sich ebenfalls mit den von mir besprochenen Problemen beschäftigt. Ich bedaure, nur mehr nachträglich auf ihn hinweisen zu können, freue mich aber, daß wir beide zum gleichen Endergebnis gekommen sind.

13 *eštā* "trinken" liegt der Ma'lūla-Form *išš(i)* zugrunde.

14 Man beachte, daß das Partizip *šāṭē* mit Spirans lautet; ebenso in Ma'lūla *šōṭ(ī)*.

15 Diems Feststellung, daß das Neuostaramäische die Form *ḥammeš* nicht erhalten hat, bedarf der Einschränkung, s. E.Sachau, *Skizze des Fellichi-Dialekts von Mosul* 1895, 28 und O.Jastrow, *Laut- und Formenlehre des neuaram. Dialekts von Mādin im ʿAbdīm* 1967, 245 Fn.1.

ADDENDA TO THE CASE FOR FRICATIVE-LATERALS IN PROTO-SEMITIC

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Fourteen works by Wolf Leslau are cited in *The Case for Fricative-Laterals in Proto-Semitic* (Steiner 1977; hereafter: *Fricative-Laterals*) - by far the most of any author. One of those fourteen, *Lexique Soqotri*, contains remarks on lateral fricatives which played a formative role in my thinking on that topic and contributed in no small measure to my eventual decision to write a doctoral dissertation on it. It is, therefore, very fitting that these addenda to *Fricative-Laterals* should appear in this Festschrift.

Chapter I ("Laterals: Definitions, Symbols and Typology").

Page 8. I now prefer the term "lateral fricative" to "fricative-lateral".

Page 9. For the Caucasian laterals, see also Lafon (1963, 1964 and 1966).

Chapter II ("Fricative-Laterals in Modern South Arabic"). For Modern South Arabian, see now also Johnstone (1977, 1981 and 1987).

Page 22, fns. 3-5. For the glottalic emphatics of Modern South Arabian, see now also Steiner (1982: 192).

Chapter III ("Correspondence Sets Containing the MSA Laterals").

Page 29. The Akkadian cognate of the Semitic words for "hyena" is, as pointed out by von Soden (1981: 163), *būṣu*.

Page 34. For the correspondence between ESA /s²/ and Hebrew /š/, see now Beeston (1977).

Pages 38-41. Rosenthal's assertion that /ǧ/ was not merged with /^c/ in Old Aramaic is now confirmed by the much younger Aramaic text in demotic script which still distinguishes the two, rendering Aram. /ǧ/ with Eg. *ḥ/h* and Aram. /^c/ with Eg. ^c (Steiner and Nims 1984: 93, 1985: 67). The Uruk incantation, on the other hand, can no longer be cited in this connection, because it is now clear that there is no basis for Gordon's theory (1937-39: 111, 113) that /ǧ/ and /^c/ are distinguished there. Many of

the fundamental assumptions of that theory have been refuted by Blau (1982: 6fn18). Another crucial assumption made by Gordon - that the Aramaic word for "gate" was /tarġā/ rather than /tar^cā/ - seems to be contradicted now by the Aramaic text in demotic script (XVII/ 7,8,12,13; cf., for the present, Steiner and Nims 1985: 70). Indeed, it was never very convincing to begin with, given the equivocal nature of the comparative Semitic evidence (see *Fricative-Laterals*, p. 135, fn. 3).

The finding that */ġ/ was unmerged in Old Aramaic makes it very unlikely that /k²/, the Old Aramaic reflex of */d/, was realized [ġ], as generally believed¹. Had the reflexes of */ġ/ and */d/ been identical or nearly identical in Old Aramaic, they would not have been represented by different signs, viz., <^c> and <k>, respectively.

It is well known that, in the Achaemenid period, a spelling change occurred: <^c> came to be used to represent the reflex of */d/ as well as the reflexes of */ġ/ and */^c/. It is probable that this spelling change resulted from the merger of /k²/ with /ġ/ - not /k²/ with /^c/ as commonly believed - for the Aramaic text in demotic script renders the reflex of */d/ with h/h, the same rendering that it uses for /ġ/. It was not until later, when /ġ/ (which by that time was the reflex of both */ġ/ and */d/) merged with /^c/, that the reflex of */d/ completed its journey from one end of the vocal tract to the other.

If Old Aramaic /k²/ was not realized [ġ], how was it realized? What is the solution to this problem which, according to Macuch (1990: 227), "still may cause scholars many sleepless nights"? Jacobsen's solution, viz. [q']², no longer seems attractive to me, for it is based on an assumption which has now been shown to be incorrect, viz., that /k²/ shifted directly to /^c/. Moreover, it does not account for the important new evidence which has come to light since the publication of *Fricative-Laterals*.

We now have a sufficient number of Akkadian transcriptions of Old Aramaic /k²/ to notice that they exhibit a curious ambivalence, e.g., *Ra-ḫi-a-nu/ Ra-ḫi-a-nu* and *-ra-*

ḫi-i/ -ra-ḫi-i = רִי־י (Beyer 1984: 101)³. The same ambivalence as to manner of articulation can be detected in the later reflexes of /k²/; normally that phoneme merged with the fricative /ġ/, but in one instance it wound up merged with the stop /g/ (in /ghk/; see *Fricative-Laterals*, pp. 113-115).

To my mind, this ambivalence points to a realization of /k²/ as an affricate - either velar [kx'] or uvular [qχ']. This is not the first time that these phones have been mentioned in connection with /k²/. In my discussion of this problem (*Fricative-Laterals*, p. 40), I posited [qχ'] as the immediate ancestor of /k²/. Voigt (1979: 101-102) subsequently proposed [x'] as the value of /k²/. These proposals were based on the theory of Cantineau and Martinet that PS /d/ was a glottalic lateral affricate, viz. [tʃ]. I adduced parallels from a number of languages for [tʃ] > [qʃ] > [qχ'], parallels which are equally valid for [tʃ] > [kʃ] > [kχ'], if not more so. Now that there is independent evidence that /k²/ was an affricate, we can turn around and use it to corroborate the Cantineau-Martinet theory.

To sum up: Old Aramaic <ṛk²> "land" was probably realized something like [ʔarkx'] or [ʔarqχ'], while Official Aramaic <ṛ^c> was realized [ʔarġ] at first, later [ʔar^c].

Page 44. Garbini was not the first to suggest that the Masoretic distinction between *š* and *š̄* was introduced from Aramaic⁴. The very same notion was entertained nearly a century earlier by Nöldeke (1873: 121):

Es liegt freilich sehr nahe, die Trennung von *š* und *š̄* so zu erklären, dass in vielen früher mit *sch* gesprochenen Wörtern später die Aussprache *s* üblich geworden und durch eine abweichende Punctuation bezeichnet wäre...; natürlich habe ich mir die Sache früher zunächst auch so gedacht. Kein Gegengrund wäre noch, dass im Aramäischen für hebräisches *š̄* entweder auch *š* oder, später allein, gradezu *š̄* erscheint. Man müsste dann annehmen, dass sich jene hebr. Lautveränderung (wie vermutlich die Aspiration der כַּנְדִּכְפַּת) unter aramäischem Einfluss vollzogen hätte.

Nöldeke, however, rejected this idea, on the following grounds:

Aber entscheidend ist für die Ursprünglichkeit des *š̄* die Thatsache, dass dasselbe im Arabischen ganz anders reflectirt wird als *š̄*; jenes nämlich durch *ش*, dieses durch *س* oder *ث*. Mithin ist anzuerkennen, dass die alten Hebräer mit ihrem *š̄*

1 See most recently Macuch (1990: 227-228), who writes: "This is the standard explanation of the phenomenon, and due to the mentioned difficulty, we shall hardly learn much more about it". Incidentally, it appears that Macuch overlooked my discussion of Old Aramaic /k²/ on pp. 38-41, for he writes:

In a dissertation *The Case for Fricative-Laterals in Proto-Semitic* by Richard C. Steiner, dedicated to the fate of *d* and *š̄* [sic, for "š"], this problem was neglected. Although the author presented the following correct paradigmatic development (p. 115)... and treated at large the fate of Proto-Semitic *š̄* in different Semitic languages (pp. 111-120) or *d* > *š̄* in Pre-Aramaic (pp. 149-154), the problem of *d* > *q* in AA escaped his attention.

2 It should be noted that [q] is used here with its International Phonetic Association value, i.e., as a plain (= non-emphatic) voiceless uvular stop. Semitic *q̄* is transliterated as *q̄*.

3 Kaufman (1978: 105fn20), cited by Beach and Daniels (1980: 41), was not yet aware of this ambivalence.

4 Cf. also Tur-Sinai (1959: 677b) cited by Blau (1977: 100fn103).

zwei ähnliche Laute ausdrückten, von denen aber der eine mit der Zeit ganz den Laut des D annahm.

Brockelmann (1982 < 1908 >: 133) found this argument so convincing that he regarded the matter as closed:

Die ältere Ansicht, daß šm ursprünglich einen einheitlichen Laut darstelle, der sich später gespalten habe, dürfte heute kaum noch Vertreter finden.

Nöldeke's argument, recently upheld by Blau (1977) and Beeston (1977), suffices to refute Garbini's version of the theory of Aramaic influence but not the version put forward by Beyer (1969: 12) and Diem (1974). These scholars, unlike Garbini, do not deny that Proto-Semitic had a contrast between */š/ and */s/. Their claim is that the contrast was lost in ancient Hebrew through phonemic merger and reintroduced with the help of Aramaic⁵.

This version of the theory can be defended far more easily than Garbini's, and Diem has done a superb job in that area. Nevertheless, I find Blau's rebuttal (1977: 100-109) of Diem's arguments convincing. I wish to add only two points.

First, Diem's discussion leaves an important question unasked and unanswered. If Aramaic influence is the source of the distinction between šin and sin , why is there only one such distinction in the Tiberian, Palestinian and Babylonian systems? Why was no distinction introduced between šin and tin , zayin and dayin , šade and tade ? Why wasn't עשר read as תור, תורב, and קיץ as קיט in the same way that עשר was read as עטר?

Second, there is extensive evidence from Qumran which cannot be ignored, since the distinction between š and ś is already attested there⁶. Diem (1974: 244) believes that the Aramaic reflex of PS */š/ (viz. /s/) had more prestige than the Hebrew reflex (allegedly /š/), but the overwhelming preference for the spelling with š in the Hebrew Qumran scrolls (Qimron 1986: 24) shows that the opposite was the case. So does the fact that the spelling of PS */š/ with D is significantly more common in vulgar texts with a large admixture of Mishnaic Hebrew like MMT (Qimron 1986: 24) and (to an

5 Diem (1974: 244-245) leaves open both the question of whether or not the contrast was reintroduced in a phonetically altered form and the question of whether or not it was introduced while the language was still alive.

6 In these documents, the reflex of PS */š/ "is generally written š (about 1000 times) but very occasionally D " (Qimron 1986: 24). There are also cases where the reflex of PS /s/ is written with š instead of D , which, following Ben-Hayyim (1978: 284-285) against Qimron (1986: 29-30), should probably be viewed as hypercorrections. The single case of PS */š/ written with D (Qimron 1986: 29) cannot affect our conclusions.

even greater extent) the Copper Scroll⁷. The pattern of scribal corrections in the Hebrew Qumran scrolls points in the same direction: all of the corrections involving */š/ are from D to š (Qimron 1976: 80, 1986: 24)⁸. Thus, the pattern at Qumran exhibits the normal scribal preference for conservative historical spelling found after a sound change - not the preference for an innovative Aramaizing spelling which Diem's theory requires⁹.

Pages 45 (bot.). A fine list of D - š doublets in the Bible was compiled in the tenth century by Dunash ben Labrat (Sáenz-Badillos 1980: 119*-120*).

Page 54, fn. 23. For palatalization in Šheri, see now Steiner (1982: 190-191).

Chapter IV ("Evidence from the Arab Grammarians for Lateral خ in Arabic").

Pages 58-59. The first scholar to conclude that the dād described by the Arab orthoepists was an "emphatic assibilated l" was, as I wrote, Richard Lepsius. However, while *Fricative-Laterals* was in press, I discovered that this conclusion was not quite as revolutionary as I had believed. Twelve months before Lepsius presented it to the Royal Academy of Sciences in Berlin, M.E. Brücke (1860: 316-317) had suggested in a lecture before the Imperial Academy of Sciences in Vienna that the pronunciation described by the Arab orthoepists, "if it in fact existed", could "perhaps" be that of an "emphatic lām ", although he also admitted the possibility that it was an emphatic [d].

Chapter V ("Evidence from Loanwords for Lateral خ in Arabic (I)").

Pages 68-72. On the use of Spanish *ld* to render Arabic خ , see also Giese (1964).

Chapter IX ("Evidence from *Ruldāyu* - *Rdy* for Lateral خ in Arabic").

Page 92. The rendering of /d/ with l+d/ṭ in cuneiform *Ru-ul-d/ṭa-a-u* = North Arabian *Ruḏā* (*Rḏw*, *Rḏy*) is discussed by Teixidor (1977: 69) as well: [Ruldāu in Ešarhaddon's Annals] is to be identified with the god Orotalt of the Arabs, mentioned by Herodotus (3.8)... Orotalt was the god Ruḏa (*rḏw*) but at the time of Herodotus the dād of Ruḏa was very likely pronounced with a lateral

7 In the edition of Luria (1963), I count at least fifteen certain examples of */š/ written with D but only three written with š !

8 By contrast, all of the cases involving PS /s/ go in the other direction (Qimron 1986: 28).

9 Cf. also Blau's argument (1977: 107) that it was Hebrew, "serving as the sacred tongue of the synagogue", which was the prestige language after its demise, whereas Aramaic was merely the vernacular.

lamedh, thus the name could have easily been understood by a Greek as *Rodl*, which becomes *Rodal* or *Rotal*.

Teixidor goes on to compare this rendering with Spanish *alcalde* = Arab. *al-qādī*, one of the many parallels I cited in *Fricative-Laterals*, pp. 92-93.

Herodotus' use of τ(+λ) rather than δ(+λ) to render North Arabian /d/ casts doubt on the customary normalization of *Ru-ul-d/ta-a-a-u* as *Ruldayu* rather than *Rulṭayu*. The first question to be asked is whether North Arabian /d/ - the reflex of what was probably, like the other emphatics, a voiceless phoneme in Proto-Semitic - was already voiced in Esarhaddon's day. It is true that Greek and Latin renderings of South Arabian /d/ in the place name *Hḏrm(w)t* show δ (already in Theophrastus) alongside τ/t (Olshausen 1879: 572-573), but this evidence bears on a very different time and place. Moreover, even if North Arabian /d/ was already voiced in the seventh century BCE, there is no reason to assume that an Assyrian transcriber would have given more weight to that feature of the sound than to its emphatic feature.

Chapter X ("Evidence from *Qišda* - *Qilda* for Lateral ش in Arabic").

Pages 95-97. My theory that the *qišda-qilda* doublets are to be connected with *iqṭaja^ca-iltaja^ca* and *jadd-jald* is confirmed by the variants *iltaqaṭa-ištaqaṭa-iḏtaqaṭa* cited by Corriente (1976: 76) from Ibn Jinni.

Chapter XII ("Evidence from Arabic ض - ش Doublets for Lateral ش in [Pre-] Arabic").

Pages 102-106. Cf. now Corriente (1978a), where ض - ل doublets are collected.

Chapter XIII ("Evidence from Incompatibility for Lateral ش and ض in Proto-Semitic").

Pages 108-109. In his review of *Fricative-Laterals*, Beeston (1979: 267) writes: Mathematical theory is something of which, as applied to linguistics, I have a rooted suspicion. Steiner has here (p. 108) worked out, by calculations which I am not numerate enough to comprehend, that according to pure statistical theory there ought to be forty roots containing both *l* and *š* [*sic*, for "š"; RCS], whereas the ones attested amount only to twenty [*sic*, for "nineteen"; RCS]. He concludes from this, on the basis of the well-known reluctance of Semitic languages to tolerate roots containing two similarly articulated consonants, that *l* and *š* did have some similarity of articulation. But this proportion between the incidence theoretically expected (if the two consonants were not in any way similar in articulation)

and the actually attested one would have had to show a very much greater numerical disparity than this in order to convince me of Steiner's conclusion. Statistics are extremely useful on a broad scale and where there is a very marked disparity between expected and actual occurrences, but I doubt whether abstract statistical theory about expectation of occurrences can be pressed into use in a case like the present; as H.M. Hoenigswald remarks, "Languages observed in the field show great statistical and distributional disparity of phonemes and distinctive features". Beeston is apparently under the impression that I have attempted to calculate, based on "pure statistical theory" and nothing more, the expected frequency of *š-l* co-occurrence in *any* language. In fact, however, my calculations are based on data from Arabic and pertain only to that language. The only "abstract theory" involved is elementary probability theory, according to which the probability of finding a root with, say, *š* in initial position and *l* in medial position can be calculated by multiplying the percentage of roots with initial *š* by the percentage of roots with medial *l* - assuming, of course, that *š* and *l* have no aversion for each other. The probability thus obtained is multiplied by the total number of roots in the language to find the expected frequency. It should be obvious that this method, employed by J. Greenberg in one of the classic articles of our field, takes into account the facts described by my teacher and dissertation supervisor, H.M. Hoenigswald¹⁰; thus, Beeston's objection is without foundation.

Beeston is also suspicious of my use of the chi-square test to calculate the significance of the disparity between expected frequencies and observed frequencies, and he refuses to be convinced by a chi-square value high enough to send a man to the electric chair. For Beeston, the only number which has any relevance is "the proportion between the incidence theoretically expected... and the actually attested one", and the only test which has any validity is his own intuition. One wonders how Beeston would react if a colleague in another field refused to believe that Epigraphic South Arabian has been deciphered and found to be a Semitic language.

One can only regret that Beeston saw fit to express his unqualified suspicion of "mathematical theory... as applied to linguistics". If Beeston is "not numerate enough to comprehend" the elementary calculations presented in my discussion, it is clear that he is not qualified to evaluate the use of statistics by such linguists as Henrietta Cedergren, David Sankoff, William Labov and Gregory Guy. Nevertheless, I fear that, due to Beeston's stature in our field, even his views on statistics will carry weight,

¹⁰ Unfortunately, the context of the latter's remark cannot be checked, since Beeston's reference is inaccurate.

lending an aura of respectability to the numerophobic attitudes of lesser scholars. This is a great pity, for it means that our field will continue to be plagued by studies whose results have no statistical significance.

Chapter XIV ("Evidence from Doublets that PS *D* and *Š* Were Phonetically Similar: Semitic Words to "Laugh").

Pages 116-117. On *šḫḫ-šḫḫ* in the Bible, see now Gevirtz 1990: 156-157.

Chapter XVI ("Evidence from *Balsamon* - *Bšm* for Lateral *Š*").

Pages 124-126. For additional discussion of the *l*-glide of /š/, see Steiner (1982: 189-190). The *l*-glide may also be represented in the cuneiform transcriptions of Aramaic /š/ collected by Fales (1978), e.g., NB-LB ⁴*il-te-eh/te-ri* = *štr*¹¹ and *il-ta-gi-b+* = *šgb*. Since cuneiform *t* is regularly used to render [θ]¹² in Iranian (Zadok 1976: 216, 218), Aramean (Abou-Assaf, Bordreuil, and Millard 1982: 18-19) and Arabian (Eph'al 1982: 89, 114) names, it seems clear that the use of cuneiform *lt* to render Aramaic /š/ indicates that the latter was heard as [θ] (see also Fales 1978: 96fn24). One is reminded of the use of *thl* and *lth* by modern explorers and medieval scribes to render [θ] in MSA and Welsh and the use of [θl] by Europeans to approximate Zulu [θ] (see *Fricative-Laterals*, p. 124). Now, it is obvious from Fales' discussion that he views cuneiform *lt* as being associated primarily with /š/; nevertheless, he is forced to admit that there are apparent examples of cuneiform *lt* used to render

11 Cf. Nabonidus' boast in the Nabonidus Verse Account (V, 10-11): "I do not know the stroke of the stylus, but I have seen se[cret things]; the god Ilte'ri has made me see a vision, he ha[s shown me] everything" (Pritchard 1969: 314 revised based on a personal communication from J.A. Brinkman). So far as I know, the sociolinguistic aspects of this passage have not been noted. The priests of Marduk, angry at Nabonidus' neglect of Babylonia's religious traditions, seem to have fabricated a quotation which would portray the Syrian-born king as abandoning its linguistic traditions as well. Their point seems to be that he is illiterate in Akkadian but not in Aramaic (the former written with a stylus, the latter with a pen) and that he calls the moon-god by his Aramaic name, *Ilte'ri*, instead of his Akkadian name, *Sin*. J.A. Brinkman comments (personal communication): "Note that the verse text apparently contradicts...the Nabonidus Cylinder II 5 i 10 (CT 36 21; dupl. RA 11 90 & 110 i 10) where it is stated... 'Nabû, bestower of all (gifts), gave him (Nabonidus) (knowledge of) the scribal skills.'" Both he and E. Reiner point out, however, that Nabonidus' illiteracy in Akkadian would not have been remarkable, since the vast majority of Mesopotamian kings were unable to read or write cuneiform. For the struggle between Aramaic and Akkadian, see CAD s.v. *sep̄ru*. (I am indebted to E. Reiner for this reference.)

12 Indeed, it is not impossible that Akkadian /t/ developed a spirantized allophone in the first millennium (see *Fricative-Laterals*, p. 147, fn. 8), possibly under the influence of Aramaic (or vice versa).

Aramaic /š/. Until these examples are somehow explained away, the cuneiform evidence for lateral /š/ in Aramaic will not be completely convincing.

Page 125. Egyptian and Hungarian also use digraphs to represent simple sounds. In Hungarian, where *s* has the value [š], [s] is represented by *sz*; in Egyptian, *nr* is sometimes used to write [l].

Page 129, fn. 27. For the possibility that /š/ was preserved in Phoenician (or, at least, Pre-Phoenician) and that it was realized as a lateral fricative, see Gevirtz (1990: 153-158).

Chapter XVIII ("Evidence from *Kaldu* - *Kašdim* for Lateral *Š* in Hebrew").

Page 140. I erred in accepting the claim of Dougherty and Artzi that the territory of the Chaldean tribe of Bit Yakin extended along the Persian Gulf as far south as Dilmun. J.A. Brinkman (personal communication) informs me that this claim is based on a misinterpretation of the following passage from Sargon's Khorsabad Inscriptions (Luckenbill 1927: 48):

... beginning with Iatnana (Cyprus) which is in the midst of the sea of the setting sun, to the border of Egypt and the land of Mushki, - the wide land of Amurru, Hatti in its entirety, all of Gutium, ..., all of Chaldea's (cities), as many as there were, the land of Bît Iakin on the shore of the Bitter Sea as far as Dilmun's border, - ...

In this passage, the phrase "as far as Dilmun's border" is not part of the parenthetical insertion (as Luckenbill's punctuation implies), for it does not modify "Bît-Iakin on the shore of the Bitter Sea" (as assumed by Dougherty and Artzi) but rather continues "beginning with Iatnana (Cyprus)... to the border of Egypt and the land of Mushki".

It remains to be seen whether I also erred in accepting the thesis of Albright (1952) that the Chaldeans migrated to Mesopotamia from Arabia. Beeston (1979: 266) labeled this assumption "astounding", and insisted that Albright's article "does not venture to do more than advance a hypothesis that the [Chaldean] *script* 'was brought north [into lower Mesopotamia] from an undetermined part of East Arabia, to which it had spread from Oman'".

It appears that Beeston did not read Albright's article very carefully, for he overlooked the reference it contains to "the migration of the Chaldeans from the south into Babylonia" (Albright 1952: 45). Even the passage which Beeston did cite ("It has generally been supposed that the Chaldeans came from East Arabia... However, there was no evidence other than intrinsic probability") shows that Albright had more in mind than the diffusion of a script. His point is that before the discoveries described in the article, there was (note the past tense) no evidence other than

intrinsic probability. The only debatable issue is whether Albright believed that both stages of the diffusion of the Chaldean script (Oman to East Arabia and East Arabia to Babylonia) were the product of an ethnic migration or only the second one.

In arguing against what he takes to be my theory, Beeston appeals to the authority of J.A. Brinkman, but here again he has read his own views into the works of others. Beeston cites Brinkman's statement (1968: 266) that, of the eighteen individuals known to have been Chaldean, "four names (Zabdi-il, Abdi-il, Jadi'-ilu, and Adinu) seem to be West Semitic, as is the tribal name Jakin", without realizing that Brinkman uses the term "West Semitic" very differently than he does. Brinkman informs me (personal communication) that, in his usage, "West Semitic" means "non-Akkadian"; it does not stand in contrast to "South Semitic" but includes it. His point is simply that phonological considerations (e.g., initial *ya-* has shifted to *ī-* in Akkadian) and/or lexical considerations (e.g., the Akkadian word for "servant" is (*w*)*ardu* rather than *abdu*) make it impossible to classify these names as Akkadian.

Is there anything about these names which precludes their being South Semitic? If there is, why didn't Beeston point it out himself? Why should Beeston, who is an eminent authority on ancient South Semitic, have to rely on the judgment of an Assyriologist in this matter? Indeed, given Beeston's understanding of Brinkman's statement, it is surprising that he did not attempt to correct it, pointing out, for example, that the name *Zabdi-il* is identical not only to the name *זבדיאל* borne by Israelites (Zadok 1988: 60, 179, 225, 274) but also to the name *Zbd'l*, *Ζαβδιρυλ* borne by ancient Arabs (Müller 1982: 25, 24).

Ironically, on the very page of Brinkman's book that Beeston cites, the following statement appears in a footnote (Brinkman 1968: 266fn1715):

In BASOR CXXVIII (1952) 44-45, Albright presented arguments for Chaldean infiltration into southern Babylonia from eastern Arabia along the shores of the Persian Gulf in the tenth and ninth centuries B.C.

A very similar summary of Albright's article is given by Edzard (1976-80: 292):

W.F. Albright, BASOR 128 (1952) 39-45 nimmt, auf Dougherty 1932 zurückgreifend, ostarabische Ursprünge der K. an....

In short, Beeston's quibble is more with Albright than with me. Indeed, had Beeston published his critique of Albright's theory earlier, my discussion of the problem might well have been different.

Page 141 (bot.). For the possibility that the name of the Chaldeans contained a lateral sibilant in the language of the Chaldeans, see also Edzard (1976-80: 296). Neither Edzard nor I noticed the evidence for this hypothesis provided by an Assyrian transcription of another Chaldean name. The account of Sennacherib's first campaign

contains a list of eighty-eight walled cities of Chaldea, including *Bît Iltamasama*⁷ = *Bît Šamaš-šama*⁸. This toponym contains the personal name *Šamaš-šama*⁸ (lit., "the sun-god has heard"). The use of cuneiform *lt* to render the initial sibilant of the word for "sun-god" - presumably /š/ as in South Semitic and early Aramaic (?) - shows that that sibilant was a lateral (see above).

Chapter XIX ("Evidence from a Conditioned Sound-Change for Lateral Š in Akkadian").

Pages 144-147. Following Gumpertz and Diakonoff, I suggested that Akkadian /št/ > /lt/ is to be interpreted phonetically as [t] > [l]. Subsequently, Swiggers (1980), apparently unaware of my discussion, offered a somewhat different phonetic interpretation of the shift: [št] > [t]. Swigger's proposal (1980: 6fn6) takes [t] as an allophone of /l/ rather than /š/. It is, therefore, uneconomical in that it fails to take advantage of the distinct possibility that [t] was a realization (possibly a conditioned allophone) of Old Babylonian /š/ inherited from Proto-Semitic /š/.

Another interpretation of the Akkadian shift is given by Fales (1978: 97fn24), who writes that "the presence of a specific positional allophone of /š/, possibly *[s^l], may be responsible for the graphemic shift from <št> to <lt>...." This statement is rather vague, but it seems to refer either to a purely graphemic shift or to a subphonemic shift. If so, one may ask why an allophone of /š/ would be written with <l> rather than <š>. Moreover, Fales talks of the "lateralized" allophone of /š/ existing in the NB-LB period, failing to mention that the shift which that allophone is meant to explain is attested already in the OB period. It is unlikely that the allophone in question survived until the NB-LB period. If it had, there would have been no reason to write Aramaic /š/ with cuneiform *t* and *lt* in that period (see above)¹³.

Page 147, fn. 8. I proposed two possible explanations of the Neo-Assyrian merger of /lt/ with /ss/. According to the first of them, /l/ had a voiceless allophone before /t/ which came to be produced with friction in Neo-Assyrian, yielding [t]. Subsequently, a reciprocal assimilation of these two adjacent phones coalesced them into /ss/.

Fales' explanation (1978: 97fn24) of this shift differs in two respects: (a) instead of a reciprocal assimilation, he posits "an assimilation of *t* to the preceding (lateralized) sibilant", viz., [s^lt] > [s^ls^l], and (b) instead of taking the lateral fricative to be an

¹³ On the other hand, the voiceless allophone of /l/ may well have survived, if Aram. *𐤏𐤍𐤏𐤍* and *𐤏𐤍𐤏𐤍* are renderings of Akk. *iltānu* and **iltartu* rather than *ištānu* and *ištartu* and if Kaufman (1974: 141) is right in assuming that the *ṯ* in these forms goes back to *ṡ*.

allophone of /l/, he takes it to be an allophone of /š/ (see above). The problem with this explanation is similar to the problem we noted above in connection with his interpretation of the earlier shift *š* > *lt*. If Fales' [s'] is an allophone of /š/, why is [s's'] written with cuneiform <ss>? Since /šš/, despite its change in realization, continues to be represented by <šš>, one would expect [s's'] to be represented by <šš>, as well.

Chapter XX ("Evidence from a Conditioned Sound-Change for Lateral *D* in Pre-Aramaic").

Pages 149-153. On /š/ as a reflex of */d/ in Aramaic, see now Diem (1980: 83-84).

Appendix ("Directions for Further Research").

Page 157. For another argument against deriving Aramaic *rḳ(t)* "sandalstrap" from an original **ḳrḳ(t)* related to Hebrew *śrk* and Arabic *šrāk*, see now Gevirtz (1990: 157).

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