

ADDENDUM TO “PROTO-CANAANITE SERPENT SPELLS IN THE
PYRAMID TEXTS” (REVISED FEB. 11, 2007)

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I have received many requests for an English version of my Hebrew lecture. There is no English version of the *lecture*, but, as I told the Associated Press, I am preparing an English *edition* of the Proto-Canaanite texts. The lecture was nothing more than an announcement of things to come; scholars are urged to reserve judgment until the English edition appears.

Regrettably, a debate has already begun on the Internet, between T. Schneider and Y. Sapir (see <http://groups.yahoo.com/group/ANE-2/message/3644>). It appears that Schneider has not read the Hebrew lecture in its entirety (see below), but that has not stopped him from circulating his allegations widely on the Internet and repeating them in a newspaper interview. It is therefore necessary to begin setting the record straight, even before the publication of my edition. On the Internet, Schneider raises three objections to what appears in the two-page handout that accompanied the lecture.

Schneider’s first objection: “As far as I can see, the author did not account for the most recent Egyptological treatment of the spells in question (C. Leitz, *Die Schlangensprüche in den Pyramidentexten*, Or 65 (1996): 381-427).”

Wrong. As will become obvious when my edition appears, Leitz’s article has been consulted extensively (as have all treatments published before and after it, with leading experts in the field providing additional insights). Indeed, it was Leitz’s article (which, like its predecessors, acknowledged that portions of PT

235-236 were unintelligible) that triggered R. Ritner's initial question to me: "Can any of this be Semitic?" In the absence of any evidence offered by Schneider, one may wonder how he could pretend to know whether or not one specific publication had been "account[ed] for" (whatever that means) in a selective lecture without footnotes. Finally, it must be noted that there are important treatments of the spells by Egyptologists that are more recent than that of Leitz.

Schneider's second objection: "Not all the lexemes pretended to appear in the spells can be assumed to have existed around 2500 BC within a single dialect (e.g. Hw 'snake' is Old Aramaic and more recent; with a chronological gap of 1500 years)." Schneider reformulated this objection when he was interviewed for an article in *Frankfurter Allgemeine Zeitung* (Feb. 4, 2007; Wissenschaft, p. 64). In that interview (whose original wording was provided to me by the newspaper), he mentioned Old South Arabian alongside Old Aramaic as the languages in which the word for "snake" is attested, but he continued to insist that a gap of "more than 1500 years" was "very problematic."

The reformulated objection reveals that Schneider has little understanding of the comparative method, the tool used by historical linguists to reconstruct forms of extreme antiquity. According to that method, cognates in related languages derive from the common ancestor of those languages. Such conclusions are especially persuasive when we are fortunate enough to find those cognates in the oldest extant texts—as is the case with the Old Aramaic word for "snake," attested already in the Sefire inscription. Anyone with training in historical linguistics will recognize that, in the realm of ancient languages, this case is as iron-clad as they come.

Even if we restrict ourselves to the two languages mentioned by Schneider, Old Aramaic and Old South Arabian, it should be obvious that the comparative

method takes us back to a time before the Pyramid Texts. And there is no reason to restrict ourselves to those two languages; indeed the South Arabian evidence is much weaker than the evidence from other Semitic languages.

The normal word for “snake” in Arabic, attested already in the Quran, is *ḥayya*. It is, of course, derived from **ḥawya*; cf. *taḥawwā* “it coiled itself up” (used of snakes) and *ḥāwī* “owner of snakes”; the assimilation *wy* > *yy* is normal in Arabic. Schneider, who refers only to my two-page handout, would certainly have mentioned this cognate (it is obviously not a loanword) had he read the entire lecture.

Additional evidence in the lecture overlooked by Schneider concerns the Biblical name *Ḥawwāh* “Eve.” Many scholars believe that this name is based on the Semitic word for “snake.” A related theory, espoused by many of the same scholars, is that the Punic underworld goddess *Ḥwt* is a snake-goddess. It is also commonly held that Ugaritic *yšthwy* “he prostrated himself” and its Hebrew cognate are related to Arabic *taḥawwā* “it coiled itself up.” It is surprising that Schneider makes no mention of the Ugaritic form, which is, of course, many centuries older than the Old Aramaic form he cites.

Schneider’s third objection: “Most notably, the sound correspondences postulated by the author would in many instances not be possible in the Egyptian transcription systems of the 2nd millennium, and seem to be outright impossible for the 3rd millennium. These IMPOSSIBLE (sic!) correspondances (sic!) include:”

Those who have never attempted to reconstruct the phonetics of an ancient language are liable to be impressed by the certitude implicit in Schneider’s capitalized “IMPOSSIBLE.” But what is the basis for that certitude? Take, for example, the case of “<š> for emphatic sibilants/interdentals (line 18) (never possible).” It is difficult to understand why Schneider is so sure about this, when

J. E. Hoch (*Semitic Words in Egyptian Texts of the New Kingdom and Third Intermediate Period*, pp. 405-406) is not even sure that the phoneme in line 18 (probably an emphatic lateral fricative) still existed in Canaanite in the New Kingdom. If it was already merged with *š* in Canaanite (as it was already in Ugaritic), there is no way of knowing how it would or would not have been rendered in Egyptian unless we find a sufficiently early example. Happily, we now have *two* examples in the Pyramid Texts of Eg. <š> rendering the phoneme in question; had Schneider read beyond the handout, he would surely have mentioned my claim that the rendering in line 18 has a parallel.

Schneider's dogmatic certitude is likely to give non-specialists the false impression that his views are uncontroversial, shared by all students of Egyptian phonology. Take, for example, Schneider's assumption that "around 2500 the Eg. *Ayin*-grapheme still represented the voiced dental." This assumption is the source of two items in his list of "impossible" transcriptions. Schneider alleges that the use of Eg. <t> for Sem. /d/ and the use of Eg. <ʿ> for Sem. /ʿ/ are possible only from ca. 2000 B.C.E. He makes no mention of the view of prominent scholars (e.g., G. Takács, G. Conti, J. Osing, A. Loprieno and W. Schenkel) that Egyptian ʿ*ayin* was a pharyngeal in the Old Kingdom, as it was in later times. Some of these scholars reject the view of O. Rössler that ʿ developed from *d* (Takács, Conti, Osing), while others believe that there was such a development prior to the Old Kingdom (Loprieno, Schenkel). What they all have in common is that they reject Schneider's assumption that "around 2500 the Eg. *Ayin*-grapheme still represented the voiced dental."

The view of these scholars makes good sense. After all, how else is one to explain the fact that the dissimilation of ʿ to *i* in the vicinity of *h*, well known from Middle Egyptian, is attested already in the time of Teti? This change presupposes that ʿ, like *h*, was already a pharyngeal in the Old Kingdom.

Another scholar who does not accept all of the assumptions underlying Schneider's critique is, ironically, Schneider himself. Take, for example, another item on Schneider's list of "impossible" transcriptions: "<t> for /Tet/." Schneider must have forgotten that in his article on Mag. Pap. Harris (*Göttinger Miszellen* 112 [1989], 56), he assumes precisely that transcription when he equates Eg. *mtm* (no final *n*) with Canaanite *mṯmn*! It must be stressed that this papyrus is from the New Kingdom, the period from which we have most of our Eg. transcriptions of Canaanite. Thus, even if it were true that no transcriptions with <t> for /Tet/ are known (and it is not true, according to Hoch), this finding would be far more problematic for Schneider's own work on Pap. Harris than for my work on the Pyramid Texts.

Even more telling is Schneider's treatment of the semantics there. In his view, Canaanite *mṯmn* can refer to a pit with lions hiding (or confined) in it; however, since Heb. *mṯmn* means "hidden treasure/stores" (never "Vorratsgrube mit Vorräten"), he is forced to cite Arab. *matmura* (sic!) "underground granary" as evidence. But why doesn't Schneider discuss the date of the earliest attestation of his Arabic parallel? Why doesn't he warn us that it is roughly two millennia later than the Egyptian papyrus? After all, in *this* case we cannot appeal to the comparative method, because we are dealing with a meaning attested in a single language. Here the chronological gap is actually significant!

Let us conclude with a word about phonetic reconstruction. It is presumptuous to apply the label "IMPOSSIBLE" to Old Egyptian transcriptions that do not agree with one's own theories about Egyptian phonetics in the third millennium B.C.E. One of the main contributions of our Proto-Canaanite texts in hieroglyphic script is precisely in the area of Old Egyptian phonetics. Until now, it was necessary to resort to extrapolation and other modes of conjecture. All

students of Old Egyptian should rejoice that we now have direct evidence. If there are surprises here, so much the better.