

Considerations about Some Hypotheses and Lexical Data in Vennemann's *Germania Semitica*

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Abstract

Theo Vennemann's contribution to linguistics since the early 1970s has been multifaceted, and his bold scholarly voice – courting controversy but arguably fruitfully so even as one disagrees – is heard, for good reason, in compartments of linguistics spanning generative phonology, syntax, Germanic diachronic linguistics, and what is a bone of contention, hypothetical ancient impact in Europe of Vasconic (a family survived by Basque), or then, which is our present concern, Semitic or Hamito-Semitic language impact in the context of what he calls “Atlantis Semitica”, considering it an ultimately Semitic impact on the Atlantic littoral. The languages affected, which he considers in the book under review, are Germanic and in particular German and English, as well as Celtic (the latter especially as a major influence on English), and the linguistic and cultural impact over these of a presumed Semitic contribution. The book has two foci: *Germania* and the British Isles. He identifies the conduit of the Semitic input partly as Phoenician or Punic contacts, and partly as earlier, even earlier than what he considers the arrival of the Indo-Europeans (dated according to Renfrew's Neolithic Diffusion Theory). He identifies influences on the Germanic pantheon, or then on runes of the futhark, and also proposes hypotheses concerning names for money, or then toponomastics. I differ from Vennemann both in details, and, mainly, in that he assumes (and has done much more explicitly so in the collected essays of his 2003 book *Europa Vasconica – Europa Semitica*) a late arrival of the Indo-Europeans (it is precisely that which forced him to fill the vacuum with Vasconic and “Semitidic” precursors), whereas I agree with Mario Alinei that such a late arrival is a modern myth and causes a telescoping of timescales. Vennemann was courageous in taking the risk, engaging in trial and error, and spurring debate. In his 2003 book (which along with the book of 2012, prompted others to publish an edited book to refute it: Udolph 2013), Vennemann explained that Afroasiatic (Hamito-Semitic) speakers colonised coastal regions of Western and Northern Europe beginning in the fifth millennium BCE (he is not explicitly repeating this in *Germania Semitica*). I am not averse to the idea of diffusion piggybacking vernaculars or, rather, sublexicons, or even just sporadic *Kulturwörter*, but it was not only on the littoral; it was the spread of early farmers. Moreover, this may make reconstruction harder, both because of the need to consider proto-forms also for the Semitic etymon at the time of contact (not just for Germanic), and because intermediate vernaculars to which the wave of early farmers acculturated along the route (the Balkans and Pannonia?) also entail phonetic processes of adaptation we do not grasp. In *Europa Vasconica*, Vennemann proposed that Vasconic (Basque-related) speakers preceded the Indo-European arrival in areas later Celtic and Germanic, whereas I consider Vasconic contribution, to the extent it can be proved, to have been adstratal (and heavier on France's Atlantic littoral) – and moreover, as I do not know Basque, I must take note of the refutations coming from Basque studies quarters on phonetic grounds – whereas (as Alinei maintains) Proto-Celtic and Proto-Germanic speakers were already in place. In discussing

Germania Semitica, I insist on early farmers as being the conduit of Semitic loanwords in the semantic domain of agriculture or (partly) social organisation, but to Vennemann, the Indo-European spread was precisely of farmers. Vennemann has perceptively seen that the Northwest Semitic theonym *Ba'al* entered Proto-Germanic twice, at different stages of the development of its phonology. Vennemann's etymological hypotheses are consistent with his assumptions (this is their main limitation). I claim that for part of those etymologies, we should rather recontextualise them within the spread of agriculture during the Neolithic, along with the linguistic and other impact of Northwest Semitic (and probably also non-Semitic Anatolian or perhaps, just perhaps, even Ubaidian/Euphratic) speaking agriculturalists or practitioners of animal husbandry, or intermediate carriers of their cultural legacy (including elements of the pantheon). This alternative interpretation on my part of a segment of Vennemann's data is compatible with Alinei's Continuity Theory (which denies Indo-European arrival later than the Palaeolithic), and with his longer periodisation, which also affects the timescale of phonological change in Proto-Germanic as well as for example the rise of Romance dialects: currently prevailing assumptions about the timescale appear to be telescoped into a shorter temporal span than was prehistorically the case. The *Germania Semitica* hypothesis is bound to be controversial, and so is Continuity Theory. The latter does and will meet with greater success with younger generations of linguists. Vennemann is more cogent when he deals with the futhark or with some late data, and his tracing Semitic lexical elements has merits although mostly not the way he conjectured contacts. But as for when we need to look back to the spread of agriculture from the Near East, and Continuity Theory is useful for accommodating this. A very similar approach is Agmon and Bloch's to Proto-Semitic and material cultures of the Mesolithic and Neolithic, and it, too, adopts a chronology going back to the Palaeolithic.

Keywords: Contact linguistics - Northwest Semitic - Germanic languages - Proto-Germanic - Neolithic propagation of farming - futhark

Theo Vennemann gen. [genannt] Nierfeld, *Germania Semitica*, edited by Patrizia Noel Aziz Hanna (Trends in Linguistics: Studies and Monographs, 259), Berlin: De Gruyter Mouton, 2012, xxi + 742 pages, hardcover – ISBN: 978-3-11-030094-9.

1. Features of the Volume *Germania Semitica*

Theo Vennemann (b. 1937), emeritus of the University of Munich, is a veteran linguist, and a visible one among linguists¹ in Germany, and controversial too, when it comes to historical linguistics, in particular to reconstructing the linguistic situation of prehistoric Europe.² His book *Germania Semitica* is structured similarly to an earlier collection of his,

¹ By formation he is also a mathematician.

² With Alfred Bammesberger, he has edited a volume entitled *Languages in Pre-historic Europe* (Bammesberger and Vennemann 2003).

his 2003 book *Europa Vasconica – Europa Semitica*, which was mainly concerned with traces of Vasconic (Basque) influence in Europe.³

In *Germania Semitica*, Vennemann does not always repeat his hypotheses explicitly, and in particular, he is not as explicit as in his 2003 about the antiquity of the earlier Afrasiatic⁴ presence on the North Sea

³ *Europa Vasconica - Europa Semitica* (Vennemann 2003a) comprises, as its Chs. 19 and 21, the articles “Germania Semitica: ⁺plōg-/⁺pleg-, ⁺furh-/⁺farh-, ⁺folk-/⁺flokk-, ⁺felh-/⁺folg-“ (Vennemann 1998a), and “Germania Semitica: *Biene und Imme*: mit einem Anhang zu lat. *apis*” (Vennemann 1998b).

⁴ Concerning the Afro-Asiatic or Afrasiatic or Hamito-Semitic language macrofamily, Jack Fellman (1978b) remarked: “In a previous article (Fellman 1978a), I discussed a language family termed Afro-Asiatic, which consists of five coordinate branches: Semitic, Egyptian, Berber, Cushitic, and Chadic. While this family is now generally recognized in the literature, its name still poses problems. The term ‘Afro-Asiatic’ was coined by Greenberg (1955) on the model of ‘Indo-European’. In contrast to the Indo-European case, however, where the languages are spoken through wide areas of the continents in question, the languages under discussion are not really representative of African and Asian languages and, indeed, are spoken in only small areas of these continents. A more traditional term in use for the language family under discussion is ‘Hamito-Semitic’ (or Semito-Hamitic’), formed on the basis of the Biblical genealogy in Genesis 10 of Noah’s sons, Shem and Ham. Nevertheless, as has been most forcefully noted by Cohen (1953) and elsewhere] and Greenberg (1955), the term ‘Hamitic’ in the compound name has no linguistic or anthropological validity in its own right, much less vis-à-vis Semitic. Further, Semitic is only one of the five language groups in the family and should not be given undue prominence in the family’s name. [...] I would advocate keeping the term ‘Afro-Asiatic’ with, however, the explicit proviso that this is an abbreviated form of ‘North Afro-Southwestern Asiatic’”.

Already Carleton Hodge (1971) entitled his edited book *Afroasiatic: A Survey*, but reviewing it, James Barr claimed (1973: 192): “since all such titles are conventional, and none can ever be found which exactly coincides with the reference intended, I would prefer to retain the traditional ‘Hamito-Semitic’. After all, what vistas of misunderstanding are not suggested by ‘Afroasiatic’?”. George Murdock [1964], answering – like also Greenberg (1964) – criticism levelled against them by Harold Schneider (1964) because of terminology they had introduced, stated among the other things: “*Afro-Asiatic*. ‘Hamitic’, my own suggestion, has not gained acceptance. ‘Hamito-Semitic’, though it has precedence, is misleading in suggesting that Semitic is coordinate with a unit consisting of the other four subfamilies of the group. The neologism ‘Erythraic’ has little to recommend it”.

In a study in which she proposed a common substratum for Cushitic languages, (also known at the time as Eastern Hamitic) languages, Margaret Bryan (1959: 10) remarked: “As [Marcel] Cohen has repeatedly pointed out (in Cohen 1924 and subsequent works), the term HAMITO-SEMITIC does not imply the existence of a unit which can be specifically designated as ‘HAMITIC’ (i.e., that part of HAMITO-SEMITIC which is not SEMITIC), but is merely a useful term covering four groups

littoral, so that that readers unaware of the 2003 may get the impression that Vennemann is fully ascribing the Semitic linguistic contribution in ancient *Germania* to Phoenicians, Gaditans, or Carthagians or Punic traders from Carthage's empire. The following is quoted from a review article by Baldi and Page (2006, in particular 2183–2184) of Vennemann's 2003 book, which is

a collection of 27 of Vennemann's essays. First, Vennemann argues that after the last ice age most of Central and Western Europe was inhabited by speakers of Vasconic languages, the only survivor of which is Basque. These speakers formed a substrate to the later-arriving Indo-Europeans. The primary evidence for the presence of Vasconic throughout much of Europe is drawn from the Old European hydronyms originally identified by Hans Krahe as Indo-European and reanalyzed by Vennemann as Vasconic. Second, Vennemann maintains that Afroasiatic speakers colonized coastal regions of Western and Northern Europe beginning in the fifth millennium BCE.⁵ According to his theory, these speakers formed a superstrate or adstrate in Northern Europe and had a profound impact on the lexical and structural development of Germanic. In the

of languages". In an obituary by Stefan Strelcyn (1975: 617) for Marcelo Cohen (b. 1884, d. 1974), who was a specialist on Ethiopia, like Strelcyn himself, the latter remarked that "the comparative study of Semitic languages led him very early to consider the larger linguistic family of which they constitute a part. In the monumental review of all known languages, *Les langues du monde* (Paris, Champion, 1924), a collective work initiated by Meillet and edited by him and by Marcel Cohen, for the first time in history the Hamito-Semitic languages were described as a coherent family with a well-defined linguistic system ('Langues chamito-sémitiques', *ibid.*, 81–181). The second edition of this book, completely revised, was edited by Marcel Cohen alone in 1952, although the names of both the original editors were preserved".

⁵ Note that whereas such are the timescales that some conjecture for Proto-Semitic speaking people to get out of Africa, Agmon (2010) instead, whose method is so similar to that of Alinei's Continuity Theory for Europe, shows that the Afro-Asiatic dispersal from Africa to Asia's Levant was as early as the Palaeolithic. This probably strengthens Sheynin's (2013: 197) critique when he argues: "If V.'s idea that Atlantic people migrated from Africa along [the] Atlantic coast holds true, he should present us etyma from the level of Hamito-Semitic, i.e. Afroasiatic, corresponding to the fifth millennium B.C." rather than from much later languages. Or then, the migration would be postulated from the Levant, of Proto-Semitic or Early Semitic speaking people, but this is really far-fetched, as it projects so far back in time a pattern of travel associated with much later Phoenician or Punic polities. It is one thing to propose that there was maritime immigration to the Balkans in the Neolithic, and it is quite another to claim that such travellers could master long voyages as far as *Germania*'s northern littoral. Clearly Vennemann was pushed into such conjectures by the postulate that before the supposed arrival of the Indo-Europeans (which like Renfrew3, he associates with the spread of farming), there was a vacuum he seeks to fill with relatives of the Basque and Hamito-Semitic peoples.

British Isles the language of these colonizers, which Vennemann calls “Semitidic” [i.e., para-Semitic] (also “Atlantic”), had a strong substratal influence on the structural development of Insular Celtic.⁶

Baldi and Page (2006, pp. 2184–2185) further summarise as follows:

In general terms, the ideas which underlie the two main theses of *Europa Vasconica-Europa Semitica* are the following (summarized from the Introduction): After the last ice-age, which ended about 11,000 years ago, Indo-European agriculturists, possibly originating in the Pannonian Basin of central Europe, migrated further into Europe in the sixth millennium BCE, arriving in Scandinavia beginning around the fourth millennium BCE. The migrating Indo-Europeans encountered other, non-IE people, who had started to settle there already in the eighth millennium BCE, i.e. several millennia after the last ice-age, and had already named the European rivers, lakes, mountains and settlements. Thus the oldest water names are probably the oldest “linguistic documents” in Europe north of the Alps. The structure of these names betrays an agglutinating language with initial accent, no vowel quantity and a predominant vowel a. The language family responsible for these names is called by V[ennemann] “Vasconic”, whose only surviving descendant is the Basque language of the Pyrenees. Additionally, there are toponyms on the Atlantic littoral which are neither Vasconic nor Indo-European. The prehistoric language responsible for these names (and other linguistic effects) is called by V the “Semitidic” (also “Atlantic”), group of languages, i.e. languages related to the Mediterranean Hamito-Semitic languages, which were spoken along the European Atlantic seaboard from the fifth millennium BCE until the first millennium CE. These languages are held to have influenced the Indo-European languages of the northwest littoral from the fifth millennium BCE onward.

Germania Semitica deals with prehistoric and early historic Semitic influences in European languages. In particular, it investigates two points in space and time at which this influence is especially conspicuous: (1) the British Isles, (2) ancient Germania. Though Semitic in both cases, the nature of this influence is quite different. It is substratal to the Celtic languages of the Isles but superstratal to Germanic” (vii). In the volume under review, one can find all articles which Vennemann pub-

⁶ Cf. in Vennemann’s “Atlantis Semitica: Structural contact features in Celtic and English”, on p. 58 in *Germania Semitica*: “There is some evidence that parts of the Atlantic littoral were linguistically Hamito-Semitic. [I do not believe that. – E.N.] For insular Celtic an Hamito-Semitic substratum has been demonstrated e.g. by Morris Jones 1900, Pokorny 1927–30, [...] For Proto-Germanic I have collected evidence that it developed under a Semitic superstratum [...] The general theory of stratal language contact [...] predicts on the evidence of case studies that more structural Semitic influence should be found in Celtic than in Germanic but more lexical Semitic influence in Germanic than in Celtic [...]”.

lished on those two topics in the period 2000–2010, and a few previously unpublished studies as well.

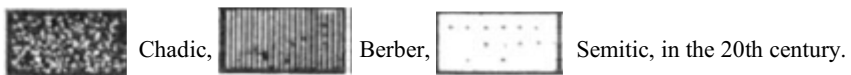
It is fortunate and helpful that his multifaceted previously published articles about his *Germania Semitica* hypothesis can now be found between two covers, thanks to Patrizia Noel Aziz Hanna (Patrizia Noel) of the University of Bamberg. The book is bilingual, and there being some redundancy between the different chapters, far from being a defect, improves the coverage even within either language's track of chapters. And as the chapters were not revised prior to being republished, we are able to follow the development of Vennemann's thinking on the subject of the book. Four studies appear in the volume under review for the first time (Chs. 3, 12, 22, and 25). Thirty numbered chapters, in English or in German (the latter comprising Chs. 1, 3, 12, 14, 16, 18, 20, 27, 28) are sandwiched between a preface by Vennemann and an introduction to the methodology of research by the volume editor, and at the end of the volume a list of abbreviations, a bibliography, and five indexes (prepared by the editor): "Index of Atlantic / Hamito-Semitic etymologies", "Index of Hamito-Semitic Words, word forms, and roots", "Index of Vasconic etymologies", "Index of Toponyms", and "Subject Index". The chapters are as follows:

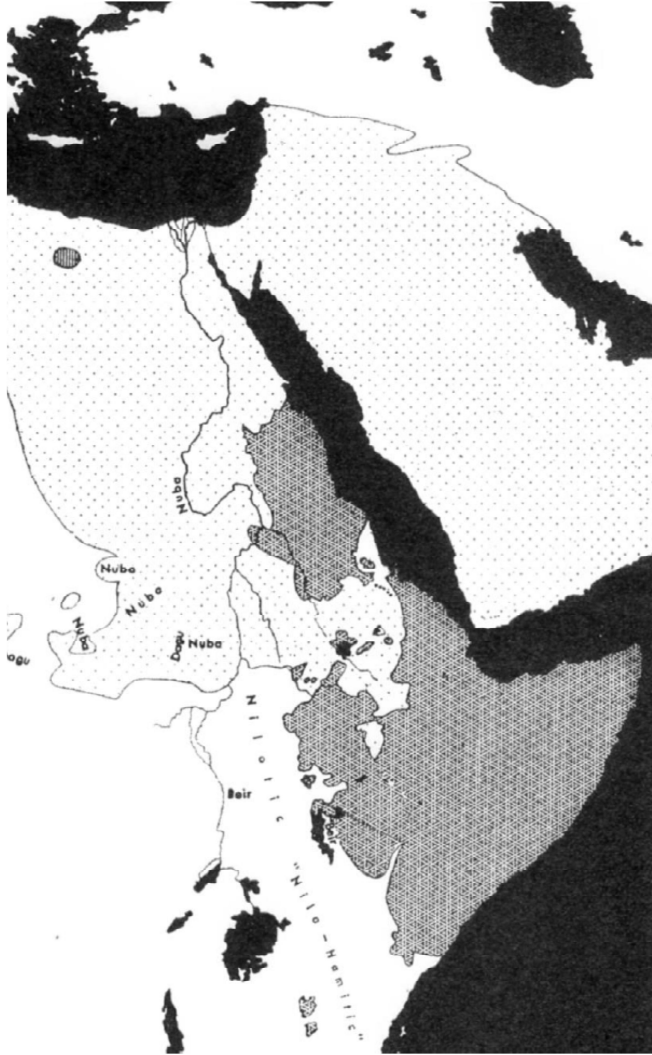
Introduction: Methodology of research in prehistoric language contact (xiii); 1. Zur Entstehung des Germanischen (1); 2. English as a "Celtic" language: Atlantic influences from above and from below (33); 3. Amsel und Merula (43); 4. *Germania Semitica*: ⁺*abr-* 'strong', with a reflection on *Abraham/Theodorik* (49); 5. *Atlantis Semitica*: Structural contact features in Celtic and English (57); 6. *Germania Semitica*: ⁺*aBal-* (OE *ædel-*, G *Adel*) 'nobility'. With an appendix on Gk. Ἀτλας (77); 7. *Germania Semitica*: Pre-Gmc. ⁺*-at-* in E *maiden*, G *Magd/Mädchen*, Goth *magaþs* (93); 8. Key issues in English etymology (107); 9. *Germania Semitica*: Gmc. ⁺*drag-*, ⁺*trek-* (Lat. *trah-*, Gr. τρέχ-) (135); 10. On the rise of 'Celtic' syntax in Middle English (147); 11. Semitic → Celtic → English: The transitivity of language contact (179); 12. Zur Etymologie von *Rauch* und *riecken* (219); 13. PGmc. ⁺*drepa-*, G *treffen* 'to hit' (225); 14. *Germania Semitica*: ⁺*sibjō* (233); 15. Languages in prehistoric Europe north of the Alps (257); 16. Syntax und Sprachkontakt: Mit besonderer Berücksichtigung der indogermanischen Sprachen des Nordwestens (269); 17. Note on the etymology of PGmc. ⁺*smītan* and ⁺*smīPaz* (E *smite*, *smith*, G *schmeißen*, *Schmied*, etc.) (299); 18. Sprachgeburth durch Sprachkontakt: Die Entstehung des Englischen (311); 19. *Phol*, *Balder*, and the birth of Germanic (345); 20. Glauben wir noch an die Lautgesetze? Zur Etymologie von *Phol* und *Balder* im Zweiten Merseburger Zauberspruch (361); 21. The name of the Isle of Thanet (391); 22. Was Proto-Germanic a creole language? (423); 23. *Münze*, *mint*, and *money*: An etymology for Latin *Moneta*. With appendices on Carthaginian *Tanit* and the Indo-European *month* word (447); 24. *Ne'er-a-face*: A note on the etymology of *penny*, with an appendix on the etymology of *pane* (467); 25. A note on the etymology of Germanic ⁺*skellingaz* 'shil-

ling': With an appendix on Latin *siliqua* 'a small coin' (485); 26. Grimm's Law and loan-words (497); 27. Germanische Runen und phönizisches Alphabet (529); 28. Zur Reihung der Runen im älteren Fuþark (591); 29. Semitic influence in Celtic? Yes and No (623); 30. The source of the *Ing* rune and of the futhark (635–646).



Current areas of Hamito-Semitic in Northwest and West Africa
(detail from Greenberg 1950a: 56):





Hamito-Semitic in Northeastern Africa and Southwestern Asia: Semitic and (in the Horn of Africa) Cushitic; Epyptian instead is extinct (detail from Greenberg 1950a: 56).

2. The British Isles

2.1 *Toponyms*

Vennemann's Ch. 21, "The Name of the Isle of Thanet", on pp. 391–422, was previously published in 2006 (Vennemann 2006a).

Thanet is an island off the coast of Kent, near the mouth of the Thames. In Vennemann's Ch. 21, "an etymology is defended suggesting that the island was named by the Phoenicians after the foremost city deity of their western capital, the goddess Tanit of Carthage" (391). See his discussion of the etymology of *Thames* on p. 412.

Concerning Vennemann's statement (on p. 416, in note 19), in the context of a discussion of words for 'edge, corner' in toponomastics: "The meaning would be the same as for the synonymous Phoenician *Ruspina* 'promontorium anguli'", and concerning the citation of Lipiński (1992, s.v. *Ruspina*: "vaste promontoire de Tunisie"), consider that Biblical Hebrew in Psalms 118:22, about the rejected stone that becomes the cornerstone, *rō(')š pinná* 'cornerstone' (literally 'head of the corner', possibly 'tip of the corner', the position where to place the corner stone) is a common name, and only became a place name (Rosh Pinnah) in the modern history of the Jewish early settlement in the Land of Israel. It is possible indeed that the Phoenicians would call a new settlement, or simply a place new to them, by their close cognate of *rō(')š pinná*, either as a propitious beginning for a newly built place, or then (which appears to be, reasonably, what Vennemann takes to be the case) the tip of a promontory because of the latter's angular shape. In Mandelkern's biblical concordance (1977 [1896]), the entry for *pinná* was glossed in Latin as "pinna, ramus muri" (*ramus muri*, because the corner is where the wall diramates in two directions); perhaps Mandelkern used Latin *pinna* because he was thinking of the narrowness of a feather, but this is deceptive, unrelated to Hebrew *pinná*. It would probably still be deceptive, if one was to think of Quintilian grammatical technical term (an adjective) *pinnus* interchanging with *acutus*.

An underlying assumption of Theo Vennemann is his acceptance of Colin Renfrew's Neolithic Diffusion Theory, which ascribes to incoming Indo-Europeans the spread of farming:⁷ Vennemann (260) assumes

⁷ But e.g. Adams and Otte asked, to say it with the title of their paper (1999): "Did Indo-European languages spread before farming?" Also see, e.g., Ammerman and Cavalli-Sforza's 1984 book *The Neolithic Transition and the Genetics of Populations in Europe*. "Classical analyses, which were the first that used genetic data to predict colonization from the Near East (Ammerman and Cavalli-Sforza 1984; [...]), have often been interpreted as implying a majority Neolithic input" (Richards et al. 2000), but later analyses showed that among extant human lineages in Europe, most

that the Indo-Europeans, when they spread “into the area north of the Alps” (as for which “I take a rather conservative view”),

I assume them to have moved, beginning in the sixth millennium, from the Pannonian Basin (the fertile region surrounded by the Carpathian mountains) into the area north of the Alps in all directions, reaching the basin of Paris in the middle of the fifth millennium and Scandinavia about the beginning of the fourth millennium. Their main economy I suppose to have been an advanced form of farming including both agriculture and cattle-breeding.

The theory that the Indo-Europeans brought farming to Europe north of the Alps has independently been developed, and elaborated much further, by Renfrew (1987). But I think Renfrew then caused more harm than good for it by assuming that those farmers directly spread into the areas where we find them at the dawn of history. This is untenable because the southern and eastern Indo-European areas were only Indo-Europeanized much later, essentially between the fourth and the first millennia B.C., and by military bands not by farmers. In my view these later great Indo-European migrations of *Völkerwanderungen* are a result of the militarization of Europe north of the Alps as a consequence of over-population in the fourth millennium, which was itself caused by three factors: a deterioration of the climate, loss of land around the North Sea, and advances of the Atlantic peoples in the West.

I find this misguided in more than one way. It is inferior to Renfrew’s view, it reintroduces from the window the military invasion modern myth that had been expelled from the door, it is quite unsatisfactory for explaining Italy (Mario Alinei’s argument for Indo-European presence there since the Palaeolithic is cogent), and it uses a circular argument: “advances of the Atlantic peoples in the West”.

Those Atlantic peoples entered the picture because Vennemann needed to provide an account of Northwestern Europe before the arrival of the Indo-Europeans⁸ and after the last Ice Age, something that

arrivals took place during the Palaeolithic, whereas only about one fifth arrived during the Neolithic.

Alinei (2000a: 999) showed that in a posthumously published article of 1978, Giacomo Devoto was prompted by the discovery of Mycenaean to propose that Indo-European waves penetrated Italy already in the Neolithic (Devoto 1978: 477–478), and that this makes Devoto into a precursor of Renfrew.

⁸ Vennemann claims (261): that “the first languages of the three families moving north” were the “Vasconic Old European languages”, and that these “eventually became adstrata and, as they were superseded by languages of the other two groups, increasingly substrata of these other languages”. In contrast, the “Semitic Atlantic languages” were, according to Vennemann (*ibid.*), “initially, in their areas of influence, superstrata and adstrata. In the West this affected the Vasconic Old European languages; in the Continental Northwest and in the North, where the Indo-Europeans

Alinei's Continuity Theory does not require, as it assumes that the Indo-Europeans were already in Europe during the Palaeolithic. In his section "Background Assumptions", the first in the paper "Remarks on Some British Place Names", Vennemann (1999a: 25) explained:

During the last few years I have developed a theory of the linguistic prehistory of Europe according to which Europe north of the Alps was, after the last iceage, i.e., beginning about ten thousand years ago, first taken possession of by people spreading north from the warmest region of the area, Southern France, and speaking "Old European" languages, which I assume to have been Vasconic languages, i.e., languages related to Basque. From about the beginning of the 5th millennium onward, the European Atlantic seaboard, from the Iberian Peninsula to Scandinavia, was colonized by seafarers speaking "Atlantic" languages, which I consider to have been Hamito-Semitic languages and indeed Semitic languages, namely languages most closely related to Semitic. Finally, beginning in the middle of the sixth millennium, most of Europe north of the Alps was gradually taken over by people practicing [sic] agriculture who spoke Indo-European languages.

Pre-languages, i.e., languages superseded by new languages brought into a territory, survive longest in typical retreat areas. This also holds for the two pre-Indo-European language families identified in this theory: The last Old European language,

arrived before the Atlantic peoples, especially in the area which was to become Germania, it affected the Indo-European languages as well. As for the latter, they "became everywhere in their areas of influence superstrata and adstrata, except for the Continental Northwest and the North where they became in part substrata of the Atlantic languages. In a much later wave of military expansion, in the last millennium B.C., Indo-European languages, viz. Celtic languages, became superstrata and adstrata of the Atlantic languages of the British Isles" (*ibid.*). Basque scholars have been unsupportive of Vennemann's claims, but at any rate, these include: "The original Old European toponymy is Vasconic" (262); "Certain Greek mythological names without accepted etymologies can be traced to toponymical or directly to Basque etyma" (262); "Certain West Indo-European words without accepted etymologies can be reconstructed as Vasconic loan-words" (262); and some "structural properties of the West Indo-Europeans languages" supposed being "carry-overs from the Vasconic substrata" (263), including the vigesimal way of counting, the first-syllable accent, SOV syntax, and postspecifying attributive adjective placement in Romance".

I would rather consider the occurrence of the vigesimal way of counting to be typological, rather than necessarily evidence in contact. That it is also found in Georgian in the Caucasus, as well as, e.g., in the Resian dialect of Slovenian, implies no more than co-occurrence, rather than Vasconic influence. After all, the adoption of the decimal or of the vigesimal ways of counting depends upon human anatomy: we have ten fingers in our hands, but if you count toes as well as fingers, the total is twenty. If toes were uncovered, some human groups would have found it convenient to also count on their toes, instead of just on their fingers, and to trade this down culturally.

Basque, is still spoken in the Pyrenees, and the last Atlantic language, Pictish,⁹ survived until the 10th century in northern Scotland.¹⁰

For example, following a hypothesis by Coates (1988a)¹¹ etymologising the name of the Solent from the Semitic root *s.l.ʕ*. ‘rock, cliff’ (does this imply that the last radical /ʕ/ was pronounced not as a pharyngeal, but in its nasal variant, occurring in Semitic eg. In the traditional pronunciation of Hebrew among Italy’s Jews?), Vennemann (1999a: 41) proposed an etymology for *Sylinancim*, an old name of the Scilly Islands off the western tip of Cornwall, by noting that “[s]ince the Scilly Islands are characterized by ‘many cliffs’ or ‘many reefs’, a plural marker would be welcome, so that perhaps the next two letters of the name form may be interpreted as a reflex of the Semitic plural marker (or earlier collective marker) *m* (-*im*) [...]?” Cf. Hebrew *s²lā’ ʕm* ‘rocks’. In Sec. 4.2 in Vennemann (1999a: 42), he etymologised the names of two British rivers with considerable estuaries, the Tay (Loch Tay indeed) in Pictland and the Taw in England, by turning to the hydronymy of the Iberian Peninsula; “these in turn have been identified with the appellative *tagus* ‘river’ in Hausa, a language of the Chadic branch of Afro-Asiatic in West Africa (Stumfohl 1989: 137)”. Hausa is a dominant language in northern Nigeria, but is peripheral to Hamito-Semitic.¹² Vennemann ventured into the suggestion that as also the Ta-

⁹ The Picts were a tribal confederation in eastern, central, and northern Scotland. Pictish is only attested in a few geographical or personal names from monuments or records. The affiliation of Pictish is controversial. Some consider it allied to Brittonic (like Welsh and Breton), and this is the common view (but in the 19th century, it was maintained that Pictish was a Goidelic language instead, like Gaelic, Irish, and Manx), whereas a few others (starting with John Rhys in 1892) have considered or still consider it fully or partly non-Indo-European. Kenneth Jackson in 1955 maintained that Pictic was allied to Brittonic, but had a non-Celtic substratum. Jackson’s view was popular among scholars in the second half of the 20th century, but was gradually undermined by advances in archaeology and in the interpretation of Ogham inscriptions.

See https://en.wikipedia.org/wiki/Pictish_language

¹⁰ And was replaced with Gaelic.

¹¹ Coates (1988b) interpreted the etymology of Uist as being similar to that of Ibiza in the Balearic Islands. Vennemann (1995: 59 ff.) accepted this.

¹² The scholarly career of Joseph Greenberg (1915–2001), “undoubtedly the most important African linguist in the second half of the 20th century” (Newman 2001: 169), began with a dissertation about Hausa non-Islamic religion, and papers on Hausa linguistics or prosody. Greenberg (1962) appeared in a journal in Jewish studies, and was entitled “On the African affiliation of Hebrew and the Semitic languages”.

jo/Tejo river of the Iberian Peninsula has an estuary, perhaps the semantic motivation was from river with an estuary'.¹³

Usually in scholarship, the Picts (a Roman-age and early medieval tribal confederation of northern and eastern Scotland) are considered to have been ethnolinguistically Celtic (and in particular, within the Brittonic linguistic branch rather than Goidelic), even though there are scholars who dissent (because of difficulties in translating some Ogham inscriptions, like those found on the Brandsbutt Stone).¹⁴ Such incerti-

¹³ This is within Vennemann's theory about "Atlantic" languages allied to Afrasiatic. Such European hydronyms that to Vennemann (2003a) are Vasconic (Basque-related), according to Kitson (1996) are Indo-European instead. By the way, Alinei's paradigm (1996, 2000) assumes that Indo-Europeans inhabited Europe if not from its earliest anthropisation, then at any rate since the Palaeolithic. That European hydronymy can be explained by Indo-European etymology is compatible with Alinei's paradigm. Kitson (1996) instead expressed a belief in Indo-European immigration. Sheynin (2004) criticises Vennemann (2003a) by stating, concerning Kitson (1996) and making the latter into an authority concerning the chronology of the Indo-Europeans' appearance: "All this is also a point against V.'s theories. We should mention also that Kitson's analysis moves the chronological frame of migration of the Indo-Europeans several millennia later than V. assumes. So the migration to Scandinavia should be postponed at least for two millennia, i.e. not earlier than second millennium BC". But Sheynin concluded, a few lines beyond: "This leaves us with 'Old European' being completely Indo-European or at least without a shade of Vasconic substratum". Of course, supporters of Continuity Theory (from personal communication, I understand from Sheynin he was not familiar with it) would not be surprised at there only being Indo-European evidence, as the theory claims that Indo-Europeans were in Europe already in the Palaeolithic.

¹⁴ https://en.wikipedia.org/wiki/Pictish_language explains: "John Rhys, in 1892, proposed that Pictish was a non-Indo-European language. This opinion was based on the apparently unintelligible ogham inscriptions found in historically Pictish areas. A similar position was taken by Heinrich Zimmer, who argued that the Picts' supposedly exotic cultural practices (tattooing and matriliney) were equally non-Indo-European, and a Pre-Indo-European model was maintained by some well into the 20th century. A modified version of this theory was advanced in an influential 1955 review of Pictish by Kenneth Jackson [(Jackson 1955)]. Jackson proposed a two-language model: while Pictish was undoubtedly P-Celtic, it may have had a non-Celtic substratum and a second language may have been used for inscriptions. Jackson's hypothesis was framed in the then-current model that a Brittonic elite, identified as the Broch-builders, had migrated from the south of Britain into Pictish territory, dominating a pre-Celtic majority. He used this to reconcile the perceived translational difficulties of Ogham with the overwhelming evidence for a P-Celtic Pictish language. Jackson was content to write off Ogham inscriptions as inherently unintelligible. Jackson's model became the orthodox position for the latter half of the 20th century. However, it has become progressively undermined by advances in understanding of late Iron Age archaeology, as

tude is because of the paucity of the extant linguistic data (with only place names and personal names surviving on monuments or in the records). In Sec. 4.3.2, Vennemann (1999a: 44) related the name of the Picts (through Celtic) to the supposed Proto-Semitic **pitt-* ‘area, region’ (on the evidence of Akkadian *pittu*) and of the reconstructed Proto-Hamito-Semitic **fit-* ‘land’, on the evidence of Central Chadic **fit-* ‘earth’. I must say that this is risky business, and especially in the etymology of the name of the Picts or of Pictland, hardly convincing. On p. 45, Vennemann (1999a) proposed: “A by-product of this analysis is the identification of Vulgar Lat. *petia (terrae)*, Fr. *pièce*, Engl. *piece* as ultimately an Atlantic word, transmitted through Celtic”.¹⁵ The Romance cognates include Italian *pezze* ‘piece of cloth’, and *pezzo* ‘piece’; for ‘(land) allotment’ Modern Italian has *appezzamento di terra*. How reasonable it is to etymologise these from quite peripheric occurrences within Afrasiatic, which after all are not so good a semantic match? Vennemann (1998c) proposes the Hamito-Semitic etymology for place-names from Pictland such as *Pitbladdo*, *Pitsligo*, and so forth, but denies that it is also valid for the names of Bitburg (a city in Rhineland-Palatia), Bedhampton (a town in Hampshire), and many other places (but for these, he considered Basque *bide* ‘road’).

One of the most visible supporters of Mario Alinei’s Palaeolithic Continuity Paradigm, Xaverio Ballester, discussed (2015, pp. 166–167) Vennemann’s (2003a) claims about the Basques, and proposed instead that the Basques arrived during the Neolithic into Celtic territory, and that moreover the Urnfield Culture is not Celtic, but likewise invaded Celtic territory. Ballester, who like Alinei considers the Kurgan archaeological culture to have been Altaic, claims (Ballester 2015: 171) that “Europe would have received at least two non-Indo-European components during the long Neolithic Age:” – the Kurgan Culture during the

well as by improved understanding of the enigmatic Ogham inscriptions, a number of which have since been interpreted as Celtic. Despite this, Eric P. Hamp in his 2012 Indo-European family tree [see Hamp (2013)], classified Pictish as a non-Indo-European language”.

¹⁵ Consider in contrast (from <https://en.wikipedia.org/wiki/Picts>) the following: “Place-names often allow us to deduce the existence of historic Pictish settlements in Scotland. Those prefixed with the Brittonic prefixes “Aber-“, “Lhan-“, or “Pit-“ (=“peth”, a thing) are claimed to indicate regions inhabited by Picts in the past (for example: Aberdeen, Lhanbryde, Pitmedden, etc.). Some of these, such as “Pit-“ (portion, share), may have been formed after Pictish times, and may refer to previous “shires” or “thanages”.”

fourth to third millennia B.C.E., and the Urnfield Culture during the second to first millennia B.C.E.

2.2 *Syntax*

Vennemann claims, in his chapter entitled “Atlantis Semitica: Structural contact features in Celtic and English”, on p. 59 in *Germania Semitica*:

The present paper is addressed to the former of the two predictions, that of structural Semitic influence in Celtic [as opposed to more lexical Semitic influence in Germanic]. Whereas the Insular Celtic lexicon and morphology have remained Indo-European, the syntactic transformation of Insular Celtic in the British Isles has been radical, to the point that Insular Celtic syntax, except for traces in the oldest poetic and “rhetorical” Irish, no longer shows the Indo-European head-final word order and in this and many other regards gives the impression of a non-Indo-European language. It is structurally similar to the Hamito-Semitic type represented by Berber, Egyptian, and Semitic (the latter in the narrower sense).

Indeed, the Insular Celtic languages are syntactically much more similar to Arabic and Biblical Hebrew than to Latin and German. That this is not a matter of accident, of internally motivated development, or of typological convergence but a result of prehistoric language contact is shown in the comparative work of John Morris Jones (1900)¹⁶ and Julius Pokorny (1927–30)¹⁷ as well as, most recently and most forcefully, in a global comparative linguistic study carried out by Orin David Gensler (1993).¹⁸

Can one really predict reliably, based on word order, that a substratum of Hamito-Semitic speakers affected Insular Celtic? All we definitely know, and that there is a typological feature of syntax that is shared by Hamito-Semitic and Insular Celtic. Or perhaps one would have to believe that Neolithic farmers in a diffusion process from the Near East, did preserve syntax in the British Isles, while not elsewhere in Europe? That would be strange indeed. I wonder about what the word order was in Iberian Celtic.

On p. 72, Vennemann states:

¹⁶ A study by Jones, “Pre-Aryan Syntax in Insular Celtic”, was published in 1900 in an edited book about the Welsh people.

¹⁷ Pokorny published in five instalments a study entitled “Das nicht-indogermanische Substrat im Irischen”.

¹⁸ Gensler’s Berkeley dissertation is entitled *A Typological Evaluation of Celtic/Hamito-Semitic Syntactic Parallels*.

It may be asked why all three instances of Semiticizing-Celticizing syntactic influence illustrated here, after centuries of no or only sporadic attestation, rose almost suddenly in Middle English. The answer is probably by the theory of language contact: substratal influence originates in the lower strata of a society and usually takes centuries to reach the written language, and regularly only after a period of social upheaval. That this applies to Irish was argued by Pokorny (1927–30), and that it applies to English is a fact well known to every Anglicist: Middle English is the period during which the language of the old ruling class dies out because the new ruling class speaks French; and when this French-speaking ruling class switches to English, that English is the Celticized English of the lower strata. [...]

“That the British Isles were Hamito-Semitic before they became Celtic was established more than a hundred years ago by John Morris Jones” (p. vii in Vennemann’s *Germania Semitica*),¹⁹ because of syntactic parallels, and scholars, especially Julius Pokorny (1927–1930) and Orin Gensler (1993), supported that theory and improved on it. Vennemann stated, in his preface: “Ten of the chapters of this book address this theory and interpret the changes referred to by those authors as typically substratal” (vii). The linguistic features concerned are syntactic. Because of what Vennemann calls “the transitivity of language contact” (vii), there is a continuation of such Celtic syntactic features which Vennemann considers to be of Semitic origin, “in English (Standard English or Irish English) through repeated language shifting, Semitic → Celtic → English” (vii).

Verb-initial word order is typical in Semitic. It is also found in Insular Celtic, as opposed to Continental Celtic, where the word order is verb-noninitial. The change from verb-noninitial to verb-initial word order in Insular Celtic has been argued by Eska (1994) to be internally motivated.

In contrast, Mario Alinei’s Continuity Theory²⁰ sees a Celtic Ireland, and a mixed Germanic and Celtic England already in the Mesolithic, when the North Sea was a plain, bordering on ice on its north and with marshes and lakes²¹ as well as people and abundant venison. Alinei sees the availability of territorial continuity through the North Sea, as the reason why not only Celts, but also Proto-Germanic people were already in England, just as Proto-Germanic people probably allied to the

¹⁹ See Morris Jones (1900). Cf. Morris Jones (1931).

²⁰ Continuity Theory was presented in English mainly in Alinei (2000b), and in Italian in Alinei (1996, 2000a).

²¹ Just as those areas of Central Europe that were free of ice were full of lakes.

identity surviving in Frisian existed in the territory which at present is the North Sea. (As I, too, think Celtic presence was early, I disagree with Vennemann's positing in "thesis G 3" on p. 261 that Celtic languages only reached the British Isles in a "much later wave of military expansion, in the last millennium B.C.")

Alinei does not believe in an Indo-European invasion by warriors in the Age of Metals. To that modern myth, Alinei prefers an arrival into Europe of speakers of what were to become Indo-European languemes, as early as the Palaeolithic. He presented his theory mainly in *Origini delle lingue d'Europa* (Alinei 1996, 2000).²² Alinei's hypothesis involves a lengthening of the timespans involved for example in Romance etymology,²³ but also in the development of Germanic.

Basically, Alinei's Continuity Theory stems as a remedy from the unacceptably stark contrast between what is now commonly admitted concerning the very long timescales of, say, Uralic, let alone of the peopling of Australia and the respective languages, and the insistence that Indo-European Europe, with already horse-riding warriors carrying metal weapons, sprang into being like, Alinei sarcastically remarked, Athena out of the head of Zeus. Genetic evidence appears to support the notion that there is a genetic continuity in Europe since the Palaeolithic, with a relatively minor genetic contribution from the Near East, probably during the spread of farming.

Sheynin (2013: 200) criticised Vennemann's interpretation of syntax typological similarities, and that objection carries weight indeed, the odds²⁴ for convergence²⁵ being too good:

²² Also see papers by Mario Alinei, Francesco Benozzo, and others, posted at <http://www.continuitas.org/texts/>

²³ It also involves the detection, in a multitude of Romance dialectal terms, of an internal dating in ancient stages of material or ideological and religious culture.

²⁴ The odds for similarities being too good is something expressed in the neologism *gamblemes* by Gyula Décsy (1999: 128): "I suppose that the largest part of the Nostratic similarities listed in Dolgopolsky's collection [...] as well as in Bomhard, [...] are mainly *gamblemes*: accidental similarities. [...] This does not mean that the work of Dolgopolsky and of other Nostraticists is futile. The opposite is true. [...] The point is that the similarity does not prove, *without further evidence*, a genetic relationship. It stands only for the experience that two forms in two (or more) languages are similar – and nothing else. The problem with interphyletic comparison is not that the word forms are not similar; they are similar but they are similar by accident".

Colin Renfrew (1998: xviii) remarked that it is often traditional Indo-Europeanists who are hostile to interphyletic comparisons such as in Nostratic. Renfrew exempli-

fied this by quoting from Sergent (1995: 398, in Renfrew translation). That quotation ended this way: “Among all these comparisons, only those between Indo-European and Semito-Hamitic appear to rely upon early and deep relationships”.

In fact, one comes across striking instances of similarity between Semitic and the lexicon of Indo-European or of given Indo-European languages. For example: the typically Tannaitic Hebrew singular masculine imperative *ʔol!* /*ʔol!*/ = Latin *tolle!* ‘take!’ Is this merely coincidental similarity? The Hebrew verb *nāʔal* ‘to take’ (a “weak” verb, as because of its initial radical /n/ missing from some inflected forms, it belongs to the *primae infirmae* class) is already found in Biblical Hebrew, but is not widespread in that historical stratum; e.g., the imperfective verbal form /*yitʔol*/ (<*/*yintol*/) ‘he takes’ in *Isaiah* 40:13. The verb spread in Tannaitic Hebrew because of the influence of Aramaic, where the lexical cognate is the verb for ‘to take’ (as opposed to the typical Biblical and Modern Hebrew verb for ‘to take’, *lāqáḥ*). Perhaps, in the perspective of Agmon (2010), a Mesolithic biliteral Proto-Semitic root *ʔ-l* became a Neolithic trilateral Semitic root *n-ʔ-l*.

The prominent linguist Graziadio Isaia Ascoli (Gorizia, 1829 – Milan, 1907) had published two works about “the Aryo-Semitic nexus” (a supposed relation between Indo-European and Semitic languages: “Del nesso ario-semitico”, published in the periodical *Il Politecnico*, but addressed as open letters, the first to Adalbert Kuhn in Berlin (Ascoli 1864a), and the other one to the linguist Franz Bopp (Ascoli 1864b). He then also published “Studj ario-semitici” (Ascoli 1867). Hermann Möller (1911) made a valiant effort at detecting lexicon shared by “Indo-Germanic” and Semitic, and he theorised an Indo-semitic Ur-language. Here and there, Vennemann resorts to Germanic/Semitic parallels found in Möller (1911), but Vennemann theorises them differently.

Moscatti et al. (1964: 17) claimed that “the ‘Aryo-Semitic’ (Ascoli) or ‘Nostratic’ (Pedersen, Cuny) hypothesis which is claimed as a common ancestor of Hamito-Semitic and Indo-European” is “very highly speculative,, especially on account of deep-seated morphological differences between those groups, although the inflexional structure appears to be common to both. A more reliable explanation is to be sought in the common Mediterranean environment [...] Such limited links as may exist between Indo-European and Hamito-Semitic should not, therefore, be regarded as a heritage from a ‘parent’ language, but rather as a haphazard collection of isoglosses not unconnected with the geographical proximity of the two groups and certain historical contacts between them” (quoted by Kaye 1999: 331).

In 1908, Raffaello Ottolenghi (Abram Raffaele Ottolenghi, 1860–1917), from Acqui in Piedmont, published in Florence a book entitled *Antichissime civiltà: studi sul nesso linguistico semitico-ariano* (*Very Ancient Civilisations: Studies about the Semitic–Aryan Linguistic Nexus*). Ottolenghi cannot be considered on a par with Ascoli and his scholarly rigour (or with Möller). That he was not always duly cautious can be seen from his book of 1908, *Il cristianesimo è un buddismo rinnovato?* (*Is Christianity a Renewed Buddhism?*), and a work of 1910, this one, too, published by Coenobium: *Il dogma cristiano in Eschilo* (*Christian Dogma in Aeschylus*).

²⁵ As an example of random lexical similarities between two languages, consider the following:

One of the reasons of V.'s failure as a historical linguist is that he mistook typological similarities of languages of different genetic families for those of genetic affinities. This thought occurred to V. We will not rewrite his long note 5 on p. 639 [in Vennemann (2003a)], but any interested reader can read himself how he treated the idea that Verb-Subject-Order of words in the sentences both in Old Irish as in Arabic and Classical Hebrew brought him to [the] conclusion that the West European megalithic culture was the result of language contact rather than typological convergence. It didn't occur to him that in very small number of possibilities of the word order similar structures may exist independently.

Vennemann however came to believe that convergence was an effect of contact (Sheynin concedes on p. 201 that sometimes it is contact that causes convergence). Sheynin (2013, pp. 201–202) set to refute²⁶ Ven-

Chinese:	SHE ²	SHE ⁴	SHE ²
Hebrew:	<i>Lāšōn</i>	<i>ʔēš</i> (stem <i>ʔišš-</i>)	<i>nāhāš</i>
Denotation:	'tongue'	'fire'	'snake'
Any correlation?	Random	Onomatopoeic sibilant? Random?	Onomatopoeic sibilant? Random?

Cf. Décsy (1999: 130, Sec. 12): “Ehret’s list (this volume) shows that it would be extremely easy to extend Nostratic to South (Subsaharan) Africa. As a persiflage, I put together within several days almost 100 lookalikes from Thai, Finnish and Hungarian. Hungarian *ház* and English *house* have almost the same sound form and meaning. Nevertheless, English goes back to Germanic *husum*, and Hungarian *ház* to Finno-Ugric *kota* (in Finnish today *kota* ‘shelter, Lapp hut’). Trained linguists know that Uralic and Finno-Ugric *k* became *h* before back vowels in Hungarians; and the ancient intervocalic *t* is rendered there as *z*. And *o* > *a* (*á*) is a regular sound change in Old Hungarian carried out in the first documents around AD 1350 (as the old documents show). What a coincident, convincing convergence! He who does not know these details might rush to the conclusion that English *house* and Hungarian *ház* prove a genetic relationship between these two languages. How many such cases may occur in Dolgopolsky’s and Bomhard’s collections?” (of Nostratic roots).

²⁶ Sheynin (2013: 201–202) wrote: “In Semitic, where the structure originated from proto-Semitic, the extraordinary systematization of ablaut refers to structure of contrasting verbal stems: basic, intensive, causative, passive, etc. [...] The systematization of rich ablaut in semitic is insured by the prevalence of tri-consonantal roots. In the Indo-European, especially in Germanic, languages the ablaut forms are used only for distinction of tenses. In Germanic, there are very little examples that can be interpreted as contrastive verbal stems (a kind of *sitzen* ‘sit, be seated’ and *setzen* ‘place, set, put’, Engl. *sit* and *set*), [...] which reminds {of the} relation of base stem and causative stem in Semitic [...] If one designates this resemblance as similarity at all,

nemann's idea that the regular Germanic verbal ablaut came into being, as opposed to the irregularly ablauting Palaeo-Germanic system (Vennemann 2003a, pp. 625–627), because of contact with people who possessed the Semitic ablauting of the broken plural. (This is still creatively productive in Arabic dialectology: whereas *fallāḥīn* is the usual plural, a suffixal, non-ablauting one, of *fallāḥ* 'tiller, farmer', dialects such as Baghdadi Judaeo-Arabic prefer *flālīḥ* < *falālīḥ*, a broken plural. Hebrew has the broken plural concomitantly with the plural suffix in the same words, in the formation of the plural of the segolate category of nouns; e.g. *kéleḥ* ['kelev] 'dog', plural *k^llābīm* /klabim/ [kla'vim].)

3. Rudiments of Alinei's Hypotheses Potentially Relevant for a Partial Earlier Periodisation of Contacts Postulated by Vennemann

3.1. *Alinei's Hypothesis about the Linguistic Identity of Palaeo- and Mesolithic Inhabitants of What Is Now the North Sea: An Updated Visual Representation*

When dealing with the British Isles, Alinei (2000a) was able – because of the very long timescales of Indo-European language groups in Europe according to his Continuity Theory – to make use of what has been known in recent decades about land bridges that are no longer above sea-level. This is the reverse of what during the Ice Age were obstacles to human movement during the last Ice Age: the lakes and marshes of what is now Germany, where freshwater was overly abundant because of melted ice in areas bordering on glaciers but not covered by them.

Alinei did not use the name *Doggerland* now in use among scholars²⁷ (as well as in popularisation)²⁸ for the land-bridge that is now the

this can be a limited typological similarity. Since V. thinks that Semitic ablaut was acquired by Proto-European or at least by Proto-Germanic, we should refer him to [several books about ablaut]. Jerzy Kuryłowicz was both an excellent expert in Indo-Europeanistics as well as a first-class expert in Semitic linguistics and had a keen interest in ablaut, but it never occurred to him to attribute Germanic ablaut to convergence with Semitic". Hardly a decisive argument.

²⁷ E.g. Gaffney et al. (2007, 2009), Ward et al. (2006). Fitch et al. (2005, 2007) and Ch'ng et al. (2007) are from the same team as Gaffney. The name *Doggerland* was introduced by Bryoni Coles of the University of Exeter, who researched the area

North Sea.²⁹ For hunter-gatherers,³⁰ it would have been attractive terrain, because there were food sources aplenty. In the 9th and 8th millennia BCE, the plain in what is now the North Sea “would for these two millennia have provided a concentration of food resources unparalleled elsewhere in northern Europe either at this time or subsequently” (Jacobi 1973: 245, quoted in Alinei 2000a: 375).

At about 8000 BC the north-facing coastal area of Doggerland had a coastline of lagoons, saltmarshes, mudflats and beaches as well as inland streams, rivers, marshes

in the 1990s, this resulting in e.g. Coles (1998). Coles (1999) discussed Doggerland’s loss in relation to the Neolithic. Cf. Coles (2000).

²⁸ E.g. Spinney (2012): “The story of that vanished land begins with the waning of the ice. Eighteen thousand years ago, the seas around northern Europe were some 400 feet lower than today. Britain was not an island but the uninhabited northwest corner of Europe, and between it and the rest of the continent stretched frozen tundra. As the world warmed and the ice receded, deer, aurochs, and wild boar headed northward and westward. The hunters followed. Coming off the uplands of what is now continental Europe, they found themselves in a vast, low-lying plain. Archaeologists call that vanished plain Doggerland, after the North Sea sandbank and occasional shipping hazard Dogger Bank. Once thought of as a largely uninhabited land bridge between modern-day continental Europe and Britain – a place on the way to somewhere else – Doggerland is now believed to have been settled by Mesolithic people, probably in large numbers, until they were forced out of it thousands of years later by the relentlessly rising sea. [...] Many have come to see Doggerland as the key to understanding the Mesolithic in northern Europe [...] Gaffney and his colleagues have digitally reconstructed nearly 18,000 square miles of the submerged landscape – an area larger than the Netherlands”.

A chain of theme parks in England is called *Diggerland* (www.diggerland.com). It may be that the name was coined in response to *Doggerland*, even though this is not strictly necessary. The four parks are near the town of Stroud in Kent, at Verbeer Manor in Cullompton in Devon, in Castleford in West Yorkshire, and at Langley park in County Durham. Diggerland is advertised as “The only place where children and adults can ride & drive real diggers and so much more!”, and visitors are enticed with “Children are taught to use everyday construction machinery including real diggers and dumpers in a safe environments”, “Try your excavating skills with a 6 tonne Digger”, “A unique opportunity to get behind the wheel of an 8.5 tonne JCB 3CX! Take this machine to its speedy limit!”, and “Get behind the controls of a 22 tonne excavator for our biggest experience yet!”

²⁹ Concerning the land-bridge in the North Sea, Alinei (2000a) relied on Jacobi (1973).

³⁰ Verhart (2005) is concerned with the Netherlands’ Mesolithic based on evidence from the North Sea floor, in what used to be the land-bridge.

and sometimes lakes. It may have been the richest hunting, fowling and fishing ground in Europe in the Mesolithic period.³¹

Before it flooded completely, Doggerland was a wide undulating plain containing complex meandering river systems, with associated channels and lakes. Key stages are now believed to include the gradual evolution of a large tidal embayment between eastern England and Dogger Bank by 7000 BC and rapid sea level rise thereafter, leading to the Dogger Bank becoming an island and Great Britain being finally physically disconnected from the continent.³²

As Alinei (1996, 2000a) argues that the ancestors of the Celts and the Germanic peoples were already in Europe during the Palaeolithic, an inference he makes is that in the land-bridge that is now the North Sea, one could find people whose language was a very ancient form of Celtic, and people whose language was a very ancient form of Germanic. There are attempts in course to obtain DNA out of Doggerland findings (Preston 2015), but by itself this would not provide solid evidence in support of linguistic identity. It would be helpful however if two co-territorial populations could be shown to have co-existed. According to Alinei (2000a), as the land-bridge was becoming submerged, part of the inhabitants would have withdrawn to Britain, to ice-free lands which are now above sea-level; early Germanic and Celtic people would have co-existed in England, the Germanic dominant in the Mesolithic and Neolithic³³ (if England's Broxbourne culture was Maglemosian, thus to Alinei probably Germanic), but the Celts being strongly dominant in the Iron Age.³⁴

Section 10.2 in Alinei (2000a) is “Presenza germanica nell'area oggi insulare?” (*ibid.*, pp. 375–376), and is concerned with whether there was an early Germanic, not only Celtic, presence in Great Britain during the Mesolithic.

³¹ <https://en.wikipedia.org/wiki/Doggerland>

³² <https://en.wikipedia.org/wiki/Doggerland>

³³ “The duration of the Neolithic varies from place to place, its end marked by the introduction of bronze implements: in southeast Europe it is approximately 4,000 years (i.e. 7000 BCE–3000 BCE) while in Northwest Europe it is just under 3,000 years (c. 4500 BCE–1700 BCE)”. https://en.wikipedia.org/wiki/Neolithic_Europe

³⁴ Germanic presence in England would thus have been much older (though far from dominant) than the Anglo-Saxon conquest. This fits within Alinei's preference to be sceptical of late conquests producing a sea-change in linguistic identity. Likewise, he posits an Italic character of Dacia (Romania) well before the Roman conquest under the Flavian dynasty; an Italic character of the Mediterranean coasts of what now are France and Spain, well before their conquest by the Roman Republic; Hungarian being spoken in Hungary well before the country's conquest by Arpad; and some Turcic presence in Anatolia well before the medieval incipient military occupation by the Turks starting with Berkyaruk.

Alinei reckons that the culture in the now submerged plain in what is now the North Sea would have mostly been of the western branch of the Maglemose culture, which Alinei assigns with confidence to already differentiated northern Germanic people (*ibid.*: 376). According to Alinei (*ibid.*: 64) Maglemosian groups that entered Scandinavia during the Mesolithic were already Proto-Scandinavian within Proto-Germanic.

Alinei (2000a: 376) reckons that the ethnolinguistically closest group that presumably most suffered from the loss of what we (but not Alinei 2000a) call *Doggerland* would have been Frisonian ancestors. He also reckons that the culture(s) of Doggerland either disappeared altogether, or lost distinctiveness by absorption into cultures such as Duvensee, Oldesloe, and De Leien-Wartena. The ethnolinguistic identity in Doggerland would have been “un insieme di geovariazioni, di cui il Frisone è oggi l’unica conosciuta” [“a set of geovariations, of which Frisian is now the only one known”] (Alinei 2000a: 376). Alinei continued arguing that if one accepts Janusz Kozłowski’s ascription of the Broxbourne culture from England (during a period coeval with Doggerland) to the Maglemose culture, then there would be no reason to exclude Germanic presence in England even before it became an island (Alinei 2000a, pp. 376, 411). What makes it difficult to confirm or disconfirm this, Alinei pointed out (*ibid.*: 376), is that one is at a loss trying to detect evident cultural boundaries in prehistoric England. Then however Alinei (2000a: 411) proposed that if there was such a Germanic presence in England, then it would have been dominant in the Neolithic, whereas there was Celtic strong dominance in the Age of Metals.

Refer to the following six maps, which I have edited³⁵ graphically. There is more detail here³⁶ than in the map which Alinei included in Alinei (1996–2000), of areas above sea-level in the British Isles, and that are at present underwater. There was a large lake in the south of Doggerland, and north of that lake, there was a territory, Doggerbank, nearly as large as present-day Wales, that was still above sea-level 8150 years BP. As sea levels arose by around two metres every century, the Doggerbank had gradually become an archipelago, and was submersed following a major tsunami 8150 years BP caused by

³⁵ Mainly so that shades of grey may adequately reflect information which in the original maps was conveyed by colour.

³⁶ At https://en.wikipedia.org/wiki/Doggerland#/media/File:Doggerland3er_en.png an English-language version appears, in the freely licensed Wikimedia Commons of the German original map, itself found at <https://upload.wikimedia.org/wikipedia/commons/d/d5/Doggerland3er.png> The German-language map was drawn by Juschki and uploaded in 2015. The English version was made by Francis Lima. The first three maps shown here are a reworking of two of the panels of the three-panel map accessible in colour at those addresses on the Web.

an undersea landslide (the Storegga landslide) off the coast of Norway.³⁷ Roughly 3000 cubic kilometres of sediment collapsed. The tsunami surged across what was left of Doggerland.³⁸ According to some, this is when the Doggerbank ceased to be inhabited, whereas others believe that people had left much earlier. Note the large lake (now referred to by scholars as the Outer Silver Pit Lake) south of the Doggerbank, when the latter was above sea-level. At present, the Doggerbank Shoal (or Dogger Bank) is a submersed sandbank. (Doggers, as they are known in English, are 17th-century Dutch fishing boats.) As for the former lake, it is at present a depression in the floor of the North Sea. There are studies into the palaeontology, palynology (the study of pollen), and palaeoanthropology and archaeology of lands now submerged by the North Sea.³⁹ Concerning the effects on humans of the tsunami, Weninger et al. (2008, p.16) stated the following:

³⁷ Cf. e.g. Bondevik (2003), Bondevik et al. (2003), Dawson et al. (1988, 1990).

³⁸ See the study “The Catastrophic Final Flooding of Doggerland by the Storegga Slide Tsunami” (Weninger et al. 2008). “Another version is that the Storegga Slide tsunami devastated Doggerland but ebbed back into the sea and that later the bursting of Lake Agassiz released so much fresh water to the world ocean that sea level over about two years rose enough to permanently flood much of Doggerland and make Britain into an island” (<https://en.wikipedia.org/wiki/Doggerland>). The hypothesis involving the glacial Lake Agassiz in North America is associated by studies by Teller et al. (2002, 2005); cf. Barber et al. (1997). “Lake Agassiz was a very large glacial lake located in the middle of the northern part of North America. Fed by glacial meltwater at the end of the last glacial period, its area was larger than all of the modern Great Lakes combined though its mean depth was not as great as that of many major lakes today. First postulated in 1823 by William H. Keating, it was named by Warren Upham in 1879 after Louis Agassiz, when Upham recognized that the lake was formed by glacial action” (https://en.wikipedia.org/wiki/Lake_Agassiz). “Around 13,000 years ago, the lake came to cover much of Manitoba, northwestern Ontario, northern Minnesota, eastern North Dakota, and Saskatchewan. At its greatest extent, it may have covered as much as 440,000 km² (170,000 sq[ua]re mi[les]), larger than any currently existing lake in the world (including the Caspian Sea) and approximately the size of the Black Sea. The lake drained at various times [...] The last major shift in drainage occurred around 8,200 years ago. The melting of remaining Hudson Bay ice caused Lake Agassiz to drain nearly completely. This final drainage of Lake Agassiz is associated with an estimated 0.8 to 2.8 m (2.6 to 9.2 ft) rise in global sea levels” (*ibid.*). “A recent study by Turney and Brown [(2007)] links the 8,500 years ago drainage to the expansion of agriculture from east to west across Europe; he suggests that this may also account for various flood myths of prehistoric cultures, including the Biblical flood narrative” (*ibid.*). “Lake Winnipeg, Lake Winnipegosis, Lake Manitoba, Red Lake, and Lake of the Woods, among others, are relicts of the ancient lake” (*ibid.*).

³⁹ See Glimmerveen et al. (2004), Erdbrink and Toacoma (1997), Fischer (2004); cf. Fischer (1997).

[...] Nor would the consequences be limited to the wave's immediate impact, as productive coastal areas could have been devastated, shellfish beds destroyed and covered by sands, together with any fixed fishing facilities, well-attested for the Late Mesolithic Ertebølle period (*Pedersen 1997*), but also known from the early Kongemose (c. 8300 calBP) in Denmark (*Fischer 2004*). Moreover, depending on the time of year that the wave hit, any stored foods meant to last over the winter may also have been lost (*cf. Spikins 2008*), with subsequent starvation among survivors. Indeed, macrofossil analysis of fish bone and twigs from deposits in Norway has shown that the tsunami probably occurred during late autumn (*Bondevik et al. 1997*). It is conceivable, particularly in the context of continuing rising sea-levels at this time, that the final abandonment of the remaining remnants of Doggerland as a place of permanent habitation by Mesolithic populations was brought about by the Storegga tsunami.

Thus, both the immediate and longer-term affects [*recte*: effects] of this event, in terms of population redistribution and social memory would have been considerable, although it remains difficult to provide more specific details at this stage (*cf. Coles 1998; Waddington 2007; Ward et al. 2006*). One clear effect of the final separation of Britain and the continent is a strong impression of insularity in the former, seen most clearly in the absence in Britain of the trapeze armatures that dominate later Mesolithic microlith industries on the adjacent continent from c. 8500 calBP (*Jacobi 1976*). Incidentally, this date is consistent with some of the more recent estimates given by palaeo-environmental researchers for the formation of the English Channel [...], and could even be interpreted as providing independent corroboration. While the process thus appears to have already been well underway, the Storegga tsunami may have finally severed any remaining (*e.g.* tidal) link between England and the continent.⁴⁰

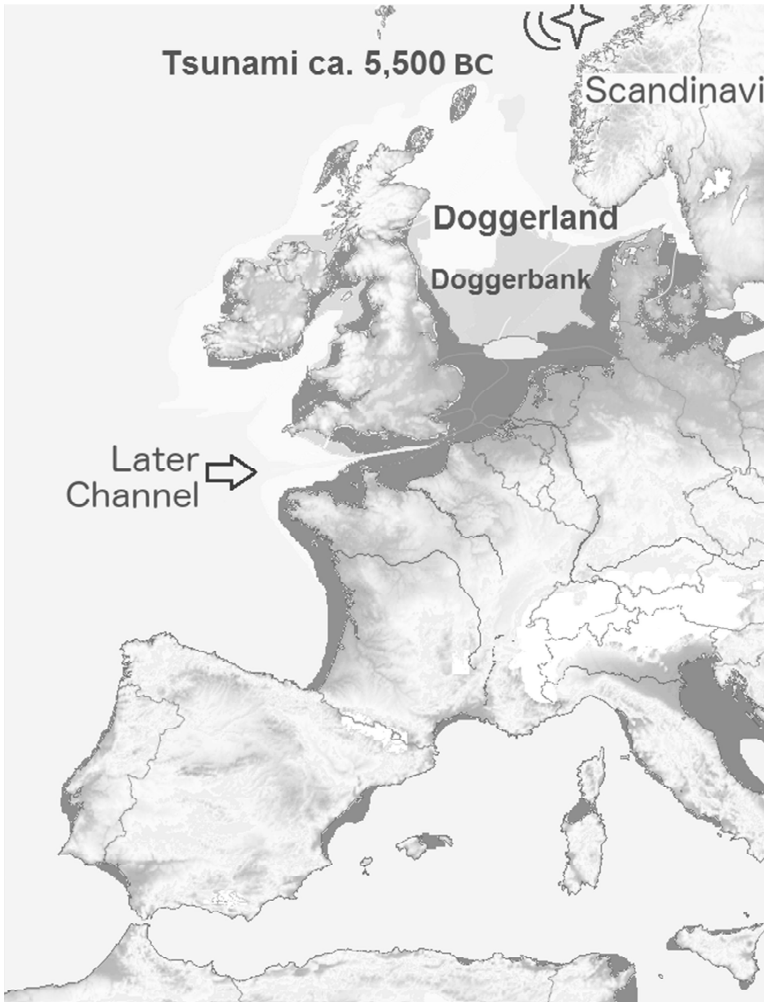
Of the migration out of Doggerland as that land was becoming submerged, Spinney (2012) writes:

“There would have been huge population shifts”, says Clive Waddington of Derbyshire-based Archaeological Research Services Ltd. “People who were living out in what is now the North Sea would have been displaced very quickly”. Some headed for Britain. At Howick in Northumberland, on the cliffs that run along Britain's northeast coast and would therefore have been the first hills they saw, his team has found the remains of a dwelling that had been rebuilt three times in a span of 150 years. Among the earliest evidence of a settled lifestyle in Britain, the hut dates from around 7900 B.C. Waddington interprets its repeated habitation as a sign of increasing territoriality: the resident people defending their patch against waves of displaced Doggerlanders.

As for the arrival of farmers into England and Ireland, Spinney (2012) states the following: which is the end of her paper: “Then, around 6,000 years

⁴⁰ “Following the Storegga Slide tsunami, it appears, Britain finally became separated from the continent and, in cultural terms, the Mesolithic there goes its own way” (*Weninger et al. (2008, p.17)*).

ago, a new people from the south arrived on the thickly forested shores of the British Isles”.



Light grey areas and dark grey areas outside the present contour of land above sea-level respectively identify now submersed land that was above sea-level 10,000 and 9,000 years BP (before present). In the middle of the Doggerland area shown here in light grey, Doggerbank is included, which was still above sea-level 8150 years BP, when a tsunami was provoked by the Storegga landslide off the coast of Norway, at a place indicated by a star contour. In the present map, Doggerbank is not set apart from the light grey area, but they can be told apart in the next map, where different shades of grey are used. Note the large lake (now referred to by scholars as the Outer Silver Pit Lake) south of the Doggerbank, when the latter was above sea-level. At present, the Doggerbank Shoal is a submersed sandbank.



Areas in black are now submersed, but were above sea-level 9,000 years BP. Note the confluence of the Thames and the Rhine in what is now the North Sea. Of the grades of shade, the darkest identifies areas above sea-level 10,000 years BP, and the next darkest, Doggerbank when it was above sea-level, 7,500 years BP. The lightest areas outside the contour of land currently above sea-level, was above sea-level 18,000 years BP, at which time the sea level was about 120 m (390 ft) lower than it is at present. Rivers are visible in this map.⁴¹

⁴¹ A river (Ch'ng et al. 2004) which drained the southeastern part of the Doggerbank hill area into the east end of the Outer Silver Pit lake has been named the Shotton River, after Frederick William Shotton, a Birmingham geologist.

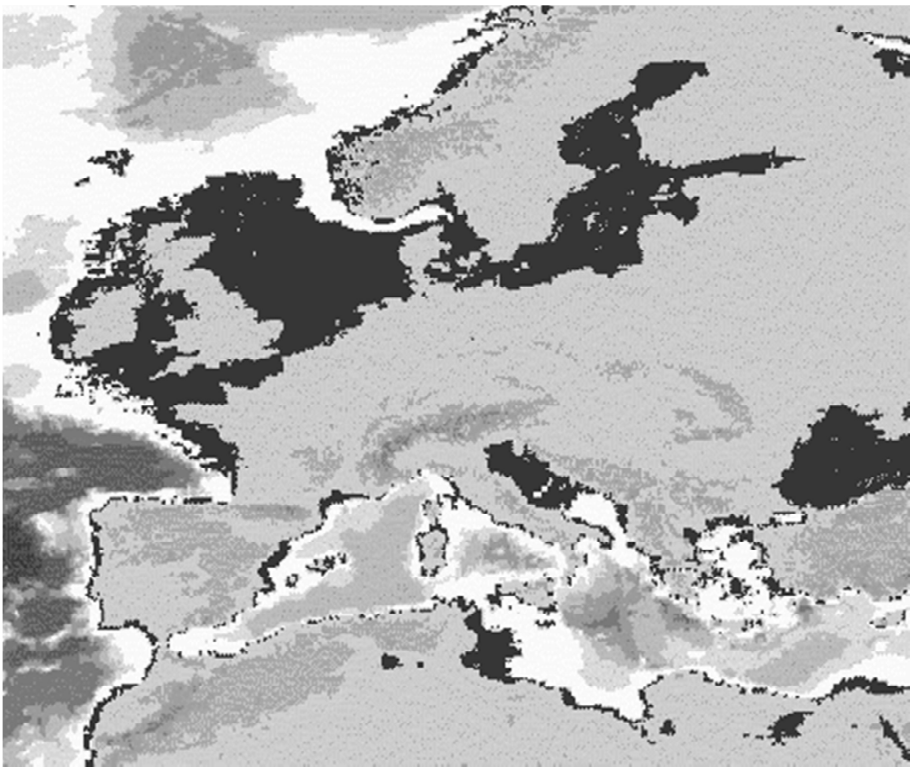


The Vistula-Würm glaciation was from nearly 115,000 to 10,000 years BCE. The greatest extent of the ice shield was nearly 20,000 BCE. Only part of Doggerland was not covered by ice, before the ices receded and sea-level rose.



Doggerland at a time when it was more extensive (the area in white beyond the present-day contours of the British Isles, France, Belgium, the Netherlands, Germany, Denmark, Norway, and the Faeroe Islands), but the ice shelf had already receded (to central Scotland and to Norway except the coastline). Deeper areas of the Atlantic Ocean are darker than its grey areas in this map. This is a reworking from a more complex, colour map kindly offered by the University of St Andrews in Scotland.⁴²

⁴² Cf. <https://www.st-andrews.ac.uk/news/archive/2012/Title,88471,en.html>



The now marine areas shown here in black were dry (including e.g. the Baltic Sea and the Black Sea: this does not excludes inner lakes or marshes) at a time corresponding to that of the previous map. Shades of grey show sea depth in the period concerned, and altitude above sea level in what is presently dry land.



Doggerbank when it was reduced to an archipelago, and land (now underwater) north of East Anglia. What had remained of Doggerland above sea-level is both those areas, and is show in a darker shade of grey.

Genetic studies must be considered cautiously, at the present state of the art,⁴³ and yet, there are interesting results worth considering. For

⁴³ Genetic evidence is not infrequently used to lend support to conflicting hypotheses. There also are those who claim that genetic evidence would support linking the arrival of the Indo-Europeans after 3,000 B.C.E. to the Yamna culture. Haak et al. (2015) was entitled “Massive migration from the steppe was a source for Indo-European languages in Europe” and claimed: “R1a and R1b are the most common haplogroups in many European populations today, and our results suggest that they spread into Europe from the East after 3,000 BCE” (Haak et al. 2015). To say it with https://en.wikipedia.org/wiki/Neolithic_Europe – “In 2015, a thorough study by Haak et al. about ancient DNA, concluded, however, that both R1a and R1b very likely spread into Europe from the Pontic-Caspian steppe after 3,000 BCE. There was a paucity of haplogroup R1b (or any other variant of R1) in European population samples predating the Bronze Age, with only one of the 70 individuals from Mesolithic and Neolithic Europe belonging to haplogroup R1. Among the analyzed male samples taken from Yamna culture sites, however, all possessed haplogroup R1b. Analysis of modern Europeans’ autosomal DNA also gives support to a large population displacement from the steppe into Europe”. This finding in conflict with Busby et al. (2012). The latter’s “conclusions were that it is likely that R1b-S127 was already present in native European populations and grew into several geographically distinct sub-lineages across Europe before Neolithic expansion occurred”

example, a study by Achilli et al. (2004) was entitled “The Molecular Dissection of mtDNA Haplogroup H Confirms That the Franco-Cantabrian Glacial Refuge Was a Major Source for the European Gene Pool”. It stated: “These findings have major implications for the origin of Europeans, since they attest that the Franco-Cantabrian refuge area was indeed the source of late-glacial expansions of hunter-gatherers that repopulated much of Central and Northern Europe from ~15,000 years ago. This has also some implications for disease studies”.

Achilli et al. (2004) also stated: “In Europe, with the exception of U5 and V, which most likely arose in situ, all mtDNA haplogroups (H, I, J, K, T, U2e, U3, U4, X, and W) are most likely of Middle Eastern origin and were introduced by either the protocolonization ~45–40 thousand years ago (kya), by later arrivals in the Middle/Late Upper Paleolithic, Neolithic dispersals, or by more recent contacts (Torroni et al. 1998; Richards et al. 2000)”.

In an article entitled “Tracing European founder lineages in the Near Eastern mtDNA pool”, Richards et al. (2000) favour MUP or LUP arrivals (i.e., in the Middle/Late Upper Paleolithic), with just nearly one fifth of extant human lineages in Europe having arrived during the Neolithic; their analyses

suggest that <10% of extant lineages date back to the first colonization of Europe by anatomically modern humans and that ~20% arrived during the Neolithic. Most of the other lineages seem most likely to have arrived during the MUP and to have reexpanded during the LUP. Given the uncertainties associated with the analyses, we should not rule out the possibility of a Mesolithic migration, but we have found virtually no evidence supporting this idea. The results of our study are consistent with the archaeological evidence but, nevertheless, are interesting for the low values obtained for the demic component of the Neolithic expansion. Classical analyses, which were the first that used genetic data to predict colonization from the Near East (Ammerman and Cavalli-Sforza 1984; [...]), have often been interpreted as implying a majority Neolithic input, but the identification of relatively few markers showing northwest-southeast clines ([...]) seems to be consistent with the mtDNA picture. [...]

A study by Achilli et al. (2005), entitled “Saami and Berbers – An Unexpected Mitochondrial DNA Link”, began as follows:

(https://en.wikipedia.org/wiki/Neolithic_Europe). In turn, Busby et al. (2012) was polemical *vis-à-vis* earlier interpretations of some genetic evidence.

The sequencing of entire human mitochondrial DNAs belonging to haplogroup U reveals that this clade arose shortly after the “out of Africa” exit and rapidly radiated into numerous regionally distinct subclades. Intriguingly, the Saami of Scandinavia and the Berbers of North Africa were found to share an extremely young branch, aged merely ~9,000 years. This unexpected finding not only confirms that the Franco-Cantabrian refuge area of southwestern Europe was the source of late-glacial expansions of hunter-gatherers that repopulated northern Europe after the Last Glacial Maximum but also reveals a direct maternal link between those European hunter-gatherer populations and the Berbers.

2.2. *Alinei and Nissan’s Identification of the Balkanic Areal Feature of the Suffixated Determinative Article*

Alinei and Nissan (2007) pointed out, among the other things, that within the Semitic linguistic family, Aramaic is typified by the suffixated determinative article *-ā*, which can be etymologised from the demonstrative *hā*. We suggested that this pinpoints the identity of an adstratal group that contributed to the Balkanic *Sprachbund* (Bulgarian and Macedonian, Romanian, and Albanian, which are phylogenetically unrelated) the feature of the determinative article being a suffix. Alinei had previously suggested that prehistorical incomers had brought this feature to the Balkans. We agreed that the context of the migration must have been the Neolithic spread of farming to the Balkans, apparently carried by farmers whose vernacular was Northwest Semitic, and akin to Aramaic. Actually, this backdates the adoption of the suffixated article in Aramaic. Alinei (2000a), in Sec. 8.1, “L’articolo posposto” (*ibid.*, pp. 215–216), which is about the suffixation of the determinative article in the Balkanic *Sprachbund*, ascribed that feature to the role of an unknown language of Middle Eastern farmers. When I signalled to Alinei that feature of Aramaic, that clinched it.

When in September 2016, I mentioned in an email to Hayim Sheynin that hypothesis about the origins of article suffixation as an areal feature in the Balkans, he remarked about a period in his career in the Soviet Union: “To tell you the truth, I am not familiar with Alinei’s work, but the idea of Aramaic influence on Balkan article suffixation occurred to me in the 60s, when I read on Balkanic languages in connection with some works by Mark A. Gabinsky. The only difficulty is to prove that there were language contacts between these languages”.

Concerning genetic evidence, consider the following:

Rather than a single, large-scale ‘wave of advance’ from the Near East, the apparent Hg J2 cline is produced by distinct populations movements emanating from different part of the Aegean and Near East, over a period stretching from the Neolithic to the Classical Period. Similarly, haplogroup E1b1b was also thought to have been introduced into the Balkans by Near Eastern agriculturalists. [(Semino et al. 2000)]. However, Cruciani et al. (2007)⁴⁴ recently discovered that the large majority of haplogroup E1b1b lineages in Europe are represented by the sub-clade E1b1b1a2- V13, which is rare outside Europe. Cruciani, Battaglia and King all predict that V13 expanded from the Balkans. However, there has been no consensus as to exact timing of this expansion (King and Battaglia favour a neolithic expansion, possibly coinciding with the adoption of farming by indigenous Balkaners, whilst Cruciani favours a Bronze Age expansion), nor as to where V13 actually arose (but point to somewhere in the southern Balkans or Anatolia). [(Battaglia et al. 2008))] Overall, Y-chromosome data seems to support the “Pioneer model”, whereby heterogeneous groups of Neolithic farmers colonized selected areas of southern Europe via a primarily maritime route. Subsequent expansion of agriculture was facilitated by the adoption of its methods by indigenous Europeans, a process especially prominent in the Balkans. [(Di Giacomo et al. 2004)]⁴⁵

2.3. *Alinei’s Approach to the Genesis of Germanic Linguistic Identities*

Alinei (2000a, Sec. 1.2.1: 416) proposed that differentiation within Germanic (which probably began in the Ice Age, as Germanic hunters’ groups on the periphery were in contact with Celts or Italids), in relation to archaeology, comprised three steps:

⁴⁴ Cruciani et al. (2007) state among the other things in their abstract: “The geographic and quantitative analyses of haplogroup and microsatellite diversity is strongly suggestive of a northeastern African origin of E-M78, with a corridor for bidirectional migrations between northeastern and eastern Africa (at least 2 episodes between 23.9–17.3 ky and 18.0–5.9 ky ago), trans-Mediterranean migrations directly from northern Africa to Europe (mainly in the last 13.0 ky), and flow from northeastern Africa to western Asia between 20.0 and 6.8 ky ago. A single clade within E-M78 (E-V13) highlights a range expansion in the Bronze Age of southeastern Europe, which is also detected by haplogroup J-M12. Phylogeography pattern of molecular radiation and coalescence estimates for both haplogroups are similar and reveal that the genetic landscape of this region is, to a large extent, the consequence of a recent population growth in situ rather than the result of a mere flow of western Asian migrants in the early Neolithic. Our results not only provide a refinement of previous evolutionary hypotheses but also well-defined time frames for past human movements both in northern/eastern Africa and western Eurasia”.

⁴⁵ https://en.wikipedia.org/wiki/Neolithic_Europe

- (1) In the Palaeolithic there was a continuous cultural area, from southern England and the Rhine, through the not yet submerged North Sea, and Denmark, to the glacial lakes of central Germany (blocking the way to Eastern Europe), and from the ice shelf to the Alps.
- (2) After deglaciation in the Mesolithic, there certainly were two migratory trajectories: (a) migration into areas freed from ice; (b) a south-bound migration retreating from the advancing sea.
- (3) The Neolithisation of southern Germany, the southern Netherlands and Belgium on the part of the early Neolithic Linienbandkeramik (LBK) culture – ca. 5415–4580 B.C.E. – of central Europe, which itself arrived from the Balkans. LBK culture is the subject of Alinei (2000a, Sec. 3.1, pp. 379–381). Alinei (2000a: 65) cites Whittle (1985: 307), as stating that LBK is “classically seen as the result of colonisation from the northern Balkans but possibly the result of indigenous transformation in connection with or in reaction to the emergence of the Vinča culture”. (Cf. Alinei 2000a: 380, and Zvelebil 1986, pp. 184–185.)⁴⁶

Alinei proposes (2000a: 418) that in the Palaeolithic, Germanic bifurcated into Western (more innovative) and Eastern (more conservative). A transient Gotho-Nordic union resulted during a time interval between the Palaeolithic and the Mesolithic. Alinei welcomed (2000a, pp. 413–414), as a useful simplification, Vennemann’s (1985) then novel model, purely phonological, of Germanic prehistory, with Proto-Germanic consonant phonology bifurcating into Low and High Germanic.

According to Alinei (2000a, pp. 61–64), early Neolithic LBK culture in central Europe reflects an area that had already become specifically Continental Germanic, whereas (*ibid.*: 64) during the Mesolithic, Maglemosian groups that entered Scandinavia were already Proto-Scandinavian within Proto-Germanic. Two Maglemosian groups, an

⁴⁶ Zvelebil (1986) provides, to say it with an abstract by J. Sheail, “an overview of Mesolithic innovations, alternative pathways of intensification, and transition to farming from a Near-Eastern and temperate Eurasian perspective. Several different factors were responsible for the transition to farming in the different parts of the temperate zone. They include an increase in population density, adverse environmental change, social competition, and forager-farmer interaction”.

Eastern, Gothic one, and a Western, North-Sea one, are conjectured to have jointly entered southern Scandinavia as the ice there was receding (Alinei 2000a: 419). [Alinei (2000a, pp. 139–141 and Ch. 8) rejects a claim made by Uralists, a claim that around 8,000 B.C.E., the Fosna culture of southern Norway was Uralic (the toponomastics is exclusively Germanic).]

Then, in the Mesolithic, according to Alinei (2000a: 420), within Germanic a bifurcation emerged or was reinforced, between Northern (maritime cultures) and Southern (continental and forestal cultures). Within this, Nordic and Gothic separated from each other, and so did Ingevonian from Pre-German. (Ingevonian now comprises English, Frisian, Netherlandic, and Low German.)

Next, in the Neolithic (Alinei 2000a, pp. 420–422), the Second Consonantal Shift took place, and High and Low German became definitively differentiated, as the LBK culture in the south was already Neolithic, at a time when areas of Germany now typified by Low German were still Mesolithic. LBK culture is recognised by archaeologists as corresponding to intrusive Neolithisation, and Alinei (2000a: 65) states that the Germanic spirantisation of the three unvoiced stops, in the Second Lautverschiebung (separating High German from the rest of Germanic) took place in such a context. [Alinei (2000a, pp. 65, 381) also claims that $k > h$, $p > f$, and $t > z$, which separated Hungarian within Uralic, took place in Hungary, corresponding to the area of the Lengyel culture, which in turn gave rise to the LBK.]

As previously mentioned, according to Alinei (2000a: 411), if there was such a Germanic presence in England at the time of Doggerland, then it would have been dominant in the Neolithic, whereas there was Celtic strong dominance in the Age of Metals. According to Alinei and Benozzo, megalithism in Europe originated with the Celts; see their study (2008a) “Megalithism as a Manifestation of an Atlantic Celtic Primacy in Meso-Neolithic Europe”. Cf. Alinei and Benozzo (2008b). In contrast,

According to Vennemann, Afroasiatic seafarers settled the European Atlantic coast and are to be associated with the European Megalithic Culture. They left a superstratum in the Germanic languages and a substratum in the development of Insular Celtic. He claims that “Atlantic” (Semitic or Semitic) speakers founded coastal colonies beginning in the fifth millennium BC. Thus “Atlantic” influenced the lexicon and structure of Germanic and the structure of Insular Celtic. According to Vennemann, migrating Indo-European speakers encountered non-IE speakers in northern

Europe who had already named rivers, mountains and settlements in a language he called “Vasconic”. He considered that there were toponyms on the Atlantic coast that were neither Vasconic nor Indo-European. These he considers derive from languages related to the Mediterranean Hamito-Semitic group.⁴⁷

2.4. *Alinei’s vs. Vennemann’s Discussion of How the Germanic Lexicon Evolved in Semantic Domains Associated with Prehistoric Cultural and Technical innovations*

Alinei (2000a, Sec. 11.2) traces how the Germanic lexicon (as well as some grammatical features) evolved through the semantic domains associated with prehistoric cultural and technical innovations. In principle, what is most relevant for Vennemann’s *Germania Semitica* is Alinei’s discussion of the Germanic lexicon during the Neolithic (Alinei 2000a, Sec. 2.3.3, pp. 453–460), and the Age of Metals (*ibid.*, Sec. 2.3.4, pp. 460–462). Nevertheless, I almost found no overlap in the sets of lexical items discussed there by Alinei, and Vennemann in *Germania Semitica*.

And yet, note in Alinei (2000a: 461) PIE **sek-* ‘to cut’ and its Germanic derivatives, along with Latin⁴⁸ *securis* ‘hatchet’ (cf. Italian *scure*), *secare* ‘to cut’, and Old Slavic *sekyra* ‘hatchet’. I argue that there appears to be metathesis, in the PIE root, *vis-à-vis* a bilateral Proto-Semitic root **q-š* ‘to cut’, whence the Hebrew and Arabic trilateral roots **q-š-š* ‘to cut’, and several related Northwest Semitic trilateral roots which I discussed in Nissan (in press, in Sec. 11: “A Survey of Semitic Alloroots for ‘to cut’”);⁴⁹ for example, Hebrew has the voiced roots *g-z-z* and *g-z-r* ‘to cut’, whereas the Hebrew and Arabic root *q-š-r* is associated with the senses ‘short’ and ‘to shorten’. The contact may have been quite hearly, of PIE with Proto-Semitic in the Near East and Eastern Mediterranean, or of Indo-European language families with Neolithic incomers speaking Proto-Semitic vernaculars. It is interesting

⁴⁷ [https://en.wikipedia.org/wiki/Atlantic_\(Semitic\)_languages](https://en.wikipedia.org/wiki/Atlantic_(Semitic)_languages)

⁴⁸ I would like to signal an etymological dictionary of Latin that has appeared in 2008, namely, Michel Arnaud Car de Vaan’s *Etymological Dictionary of Latin and Other Italic Languages*. I have not seen it.

⁴⁹ Cf. in Nissan (in press), Sections 12.6, “Dolgopolsky’s List of Cognates of the Hebrew Root *g.d.d.* ‘to cut’”, and 12.7, “Dolgopolsky’s List of Cognates of the Hebrew Roots *q.š.š.* ‘to cut’ and *q.s.s.*”

that Slavic and Latin also had forms akin to the Semitic alloroot with the third radical being /r/.

Besides, Alinei was concerned with German *Acker* (1996: 243) and *Ackermann* as a lexical compound: cf. Middle English *acremen* (< *acre* + *man*), Swedish *åkerman*, Dutch *akkerman* (2000a: 454). Elsewhere in my present study, we are going to consider the Semitic proto-word for ‘farmer’, and Biblical Hebrew and Modern Hebrew *ikkār* ‘farmer’ (cf. Sumerian ^{LÚ}ENGAR).

3. Prehistoric Ireland and Supposed Hamito-Semitic Contribution

Vennemann’s assumption that there may have been some Hamito-Semitic presence in the British Isles already before a Phoenician presence in the Near East’s historical period, is compatible not only with Vennemann’s theory, but arguably also with Alinei’s Continuity Theory, but then the context would be the spread of farming, and this in turn would possibly be limited to some vocabulary, without survival of the Near Eastern vernaculars through the process of diffusion throughout Europe to its insular periphery.

Claims about origin based on syntax have a penchant for leaving one in doubt, and yet, they cannot be easily explained away. Such hypotheses, carefully put forth by rigorous and meticulous linguists (which both Vennemann and Wexler are), have a strength about them which ought to entail that it would not be legitimate to ignore them, whereas it would be a welcome development that they be refined and improved upon by future scholarship.

It is quite possible that an adverse Pavlovian response, so to speak, upon being first faced in a scholarly context with proposals of Hamito-Semitic affecting Celtic results from awareness of there having culturally been fanciful lore about a supposed impact of Egypt on Ireland, namely, the Irish myth of Egyptian origins. See John J. Contreni’s article on a medieval Western tradition, “The Egyptian Origins of the Irish: Two Ninth-Century Notes” (1989). Cf. Erik Iversen’s *The Myth of Egypt and its Hieroglyphs in European Tradition* (1993).

The idea that there is a connection between Insular Celtic and Afroasiatic goes back to John Davies⁵⁰ (1632). It was expanded by John Morris-Jones⁵¹ in 1913 and developed further by Vennemann. This position is supported by Pokorny⁵² (1927–49) and Vennemann identifies Phoenicians as the likely people. A key factor is the dominant word order in Insular Celtic compared to other IE languages, together with lexical correspondences. Another important factor is the identification of the people later known as Picts. Vennemann holds the position that they spoke an Atlantic language. This belief was also held by Zimmer⁵³ (1898) but is not generally accepted.⁵⁴



The Welsh linguist and poet John Morris-Jones.

4. Vocabulary in Societal Organisation Domain, the Military, or Weapons

4.1. *Vennemann on Proto-Germanic* ⁺*fulka-* ‘division of an army’, and the folk *Word*

Vennemann (vii–viii, 118, 507, 526) derives Proto-Germanic ⁺*fulka-* ‘division of an army’ – as well; as Old Norse *fylki* ‘band of warriors, district’ (507) – from the Semitic root *p-l-g*, for ‘to divide’, whose Semitic lexical derivatives include the sense ‘division of an army’ indeed.

⁵⁰ [https://en.wikipedia.org/wiki/John_Davies_\(Mallwyd\)](https://en.wikipedia.org/wiki/John_Davies_(Mallwyd))

⁵¹ https://en.wikipedia.org/wiki/John_Morris-Jones

⁵² https://en.wikipedia.org/wiki/Julius_Pokorny

⁵³ [https://en.wikipedia.org/wiki/Heinrich_Zimmer_\(Celticist\)](https://en.wikipedia.org/wiki/Heinrich_Zimmer_(Celticist))

⁵⁴ [https://en.wikipedia.org/wiki/Atlantic_\(Semitic\)_languages](https://en.wikipedia.org/wiki/Atlantic_(Semitic)_languages)

The archisememe of that root is ‘division’. It is quite possible, I reckon, that the lexical borrowing occurred in the Bronze Age or in the Iron Age, from a Phoenician or Punic source. If the borrowing was in the Neolithic instead, during the spread of agriculture, then I suspect that the sense was not yet ‘division of an army’, but rather ‘clan’s offshoot looking for new land’. And in fact, Vennemann (507) marshals in support “Hebr. *p^olugāh* ‘division, district (as division of a tribe)’, *palag/pelag* masc. ‘part, half’ (NHebr. ‘faction’), Assy. *puluggu/pulungu* ‘district’. Phoenician has *plg* ‘district, region’” (507). Note that the final *h* in Hebrew *p^olugāh* reflects the Hebrew spelling, not phonetics.

In the *historia gentium* account⁵⁵ of Noah’s progeny in the table of Nations of *Genesis*, Pélæg (pausal form: *Pālēg*), Phaleg in the Vulgate, is the patriarch at the time of the Tower of Babel and the resulting Generation of the Splitting and Propagation of ethno-linguistic groups, and in fact, in early rabbinic Hebrew that generation is called *dōr hap-pallāgā* (spelled *dwr hplgh*). This is linguistic evidence of how the splitting of human groups was lexically described.

Add to this, that Proto-Germanic ⁺*fulka-* is compatible with an Aramaic word-form of the segolate category of nouns – see a nice formal model in Malone’s study (1971) “Wave Theory, Rule Ordering, and Hebrew Aramaic Segolation” – and moreover, that whereas Northwest Semitic has allophones for the phoneme /p/, these apparently were [φ]⁵⁶ and [f] before [φ] became [p] (at any rate, in Hebrew) sometime soon after late antiquity. Besides, note Hebrew segolate noun *pélek* [‘pelex’] ‘district’, whose base is /pilk-/. (Quite possibly, this was an intra-Semitic loanword in Hebrew.) These further data would all support Vennemann’s conjecture concerning ⁺*fulka-* and *fylki*, were it not that Bomhard⁵⁷ (1981: 409) felt able to compare to Indo-European lexical

⁵⁵ Christianisation brought about accounts of European peoples’ origins combined with the account from *Genesis*. Cf. Fromentin and Gotteland (2001).

⁵⁶ This does not exclude that somewhere, sometime in antiquity, [p] was pronounced by speakers of some given Semitic vernacular.

⁵⁷ A later book of his is Bomhard (2008), *Reconstructing Proto-Nostratic: Comparative Phonology, Morphology and Vocabulary*. A more recent study within the Nostratic approach, but Bomhard is not an author, is “Ultraconserved Words Point to Deep Language Ancestry Across Eurasia” (Pagel et al. 2013). Importantly in the context of my present study, the Nostraticist Bomhard is at odds with Alinei’s Palaeolithic Continuity paradigm (unless one casts Bomhard’s reconstructed proto-words in a much longer chronology than even he intended), because of his and other Nostrati-

cists, and Renfrew, clinging to old ideas about the original Indo-European homeland: “The Indo-European homeland was most likely to the north of and between the Black and Caspian Seas (this is the view of Marija Gimbutas and many others – it differs from the views of Renfrew, Dolgopolsky, and Gamrelidze and Ivanov, who posit an Anatolian homeland for Indo-European). However, Johanna Nichols (1997, 122–48) has convincingly argued that Pre-Indo-European originated in Central Asia and later spread westward to the North Pontic / Steppe zone that was the geographical location where Proto-Indo-European proper developed, where it began to split up into different dialect groups, and from which its descendants spread into Europe, the Iranian plateau, and northern India” (Bomhard 1999: 70). Bomhard’s chronology for Nostratic is as follows: “As the Ice Age began coming to an end, more permanent settlements started to appear, and there was a gradual transition from an economy based on hunting and gathering to one based on cultivation and breeding. This was the setting in which Nostratic arose. Nostratic was indeed at the right place and at the right time” (Bomhard, *ibid.*: 71). Bomhard’s timescales are too short not only considering the ones assumed by Alinei, but also what in my opinion Noam Agmon has shown for Proto-Semitic, with the transition from bilateral to trilateral lexical roots around the time of the rise of agriculture. See Agmon (2010), Agmon and Bloch (2013). Thus, Agmon’s timescales are longer than Militarev’s, who considered Hamito-Semitic to be some 12,000 years old, and supposedly beginning with the Natufians, who for Agmon instead would be Proto-Semitic. But an understanding that would make Hamito-Semitic derive from the Natufians would be like looking for a lost key under a lamppost because it is the spot where there is light; unless one is claiming that they spread linguistically because of the diffusion of farming.

Vitaly Shevoroshkin (1999) comprises Sec. 5, “Dating of Nostratic and its daughter-languages”, was subscribing to what in my opinion is an unrealistically short chronology for Indo-European. I think he was somehow conflating proto-farming with hunters-gatherers, who were able to gather many plants and also to store food for winter. But that would take us back to the Mesolithic, or even to the late Palaeolithic, not to as late dates as he was thinking of. In that section, he stated the following, with the beginning concerning Dolgopolsky (1998 = *NM*):

Dolgopolsky’s list of Nostratic words for grain, nuts, berries, fruit, edible roots (see *NM* entries 16–17 and 53–62), as well as reconstructed words for containers and food-processing, seem to indicate a relatively shallow dating of the Nostratic proto-language and its speakers (scarcely older than twelfth millennium BC), when these latter were able not only to gather edible parts of plants but also to store them for use in winter.

This thesis may be considered as a confirmation of a ‘sisterhood’ of both Proto-Nostratic and proto-Hamito-Semitic (= Afro-Asiatic), this latter being not much older than Proto-Nostratic. Both proto-languages existed at an age which immediately preceded the appearance of agriculture and cattle-breeding. According to Militarev *et al.*, the most ancient speakers of Hamito-Semitic (this latter being some

twelve thousand years old) were Natufians, the most cultured people at their time. The stage of hamito-Semitic people, which immediately preceded the natufian era, was that of hunters-gatherers with a developed system of food processing and conservation – which is close to the stage of the Nostratic people as described in *NM*.

If the Proto-Indo-Europeans inhabited a part of the predominantly Hurri-Caucasian territory not earlier than seven thousand years ago, then the former were not the people who had developed the high urban culture of Asia Minor at a much older age. It is more likely that (Pre-)Proto-Hurri-Caucasians developed this culture. In any case, the Proto-Hurri-Caucasians can be considered as being one millennium older than Proto-Indo-Europeans; the former may have inhabited a much broader territory than Proto-Indo-Europeans several hundred years later and, even if the earliest possible age of the Proto-Hurri-Caucasians was eight thousand years ago, their close relatives may have been dwelling in South-Central Asia Minor as early as 9000–8500 years ago. After all, Hurri-Caucasian languages seem to have been spread over extremely vast territories.

Hurri-Caucasian peoples and languages inhabited Europe and the Near East way before the Indo-Europeans started to expand. We may consider the historical Hurri-Caucasians (North-Caucasians, Hurrians, hattî people, probably also Sumerians and Basques) as ‘islands’ in the Indo-European ‘sea’. [...] When turning to the Nostratic people – the ancestors of the Indo-Europeans, and to the Sino-Caucasian people – the ancestors of the Hurri-Caucasians, we may be looking at a similar picture: the older, predominantly Sino-Caucasian territories (Northern Caucasus, Yeniseian settlements, Sino-Tibetan regions) are ‘islands’ in the Nostratic ‘sea’. Being older than the Nostratic people, the Sino-Caucasians have come not only to the Far East; they also have spread all over America some twelve thousand years ago; their offsprings [sic] speak Na-Dene-Athapaskan, Algic, Salishan, Wakashan, Siouan, and many other American Indian languages (whereas Nostratic is represented only by a relatively recent wave of Altaic, namely Eskimo-Aleutian).

For ones accepting Alinei’s paradigm, Shevoroshkin’s considerations quoted above (1999: 88–89) about the supposed lateness of the Indo-Europeans would surely appear to be quite misguided. By the way, neither Dolgopolsy (1998), nor Shevoroshkin (1999) included Militarev et al. in their respective bibliographies. Militarev was not a contributor to the volume edited by Renfrew and Nettle (1999) in which Shevoroshkin (1999) was published. Besides, note that Dolgopolsky (2008) was receptive of (though not overly committal to) theories about the Sino-Caucasians (see in his introduction). This other statement by Shevoroshkin deserves notice (1999: 90); “It is not excluded that Illyč-Svytič’s and Dolgopolsky’s conception of the Nostratic proto-language as having split into six daughter-languages will be changed, following Mili-

clusters not only from Germanic, the Proto-Semitic (actually Hebrew), roots **p.l.g.* “to split, cleave, divide” and **p.l.h.* “to split, cleave” (but cf. Hebrew *pērēq* ‘to dismantle’, Arabic *fāraqa* ‘to separate’, *fārraqa* ‘to do favours to some or to discriminate against some’ in modern discourse about partiality, favouritism, or poor parenting; cf. Arabic *fāllaqa* ‘to divaricate’, e.g. legs). Cf. Bomhard (1981: 408).⁵⁸ Thus the supposed contact is not specific to Germanic within Indo-European. Strictly speaking, this is not to exclude with certainty that the Semitic term somehow travelled to Germanic lands, regardless of possible parallels elsewhere in Indo-European.

Vennemann (507) also considers a separate, later event of borrowing of some derivative of *p-l-g* ‘to divide’ giving rise to the Germanic *plough* words. We are going to devote to this a separate subsection, in the section in which we are going to discuss agricultural or botanical terminology. “With regard to the Semitic root, a pair such as ⁺*fulka*- and ⁺*plōg*-, E[nglish] *folk* and *plough*, thus form a *Lautverschiebungsdublette*” (507).

Consider this Nostratic (N) entry from Dolgopolsky’s *Nostratic Dictionary* (2008, pp. 1631–1632); where only cognates from Semitic (S), Kartvelic (K), Uralic (U), and Dravidian (D) are listed; there is no In-

tarev’s and Starostin’s proposal to regard the old and complex Hamito-Semitic phylum as Nostratic’s sister (this won’t change much in the Nostratic reconstructions, probably resulting in a few simplifications). If so, we will have three sister proto-languages: Hamito-Semitic (or Afro-Asiatic), Nostratic, and Sino-Caucasian”.

⁵⁸ Even the leading Nostraticist, the late Aharon Dolgopolsky (who being a Nostraticist, is controversial in his own right), had reservations about Bomhard’s method. In the forward to his magnum opus, Dolgopolsky (2008) stated (his brackets): “Some earlier papers on Nostratic (among them those by A. Bomhard) have not been analysed although they are likely to contain useful comparisons (in spite of methodological drawbacks [cf. AD rTPN] and partially untenable hypotheses of sound correspondences). Analysing them is a lengthy and inefficient procedure that I could not undertake owing to time constraints”. The citation has this sense: “AD rTPN = A. Dolgopolsky, rev. of Bm. TPN. *BSL* LXXXI/2 (1986): 9 1–7”. *BSL* is the *Bulletin de la Société de linguistique de Paris*, published by Peeters. In turn: “Bm. TPN = A. R. Bomhard. *Toward Proto-Nostratic. A new approach*. Amst. / Phil., 1984”. Bomhard in turn has been critical of sound correspondences adopted by Dolgopolsky. (At any rate, such a large phonological system does not boost confidence.) Interestingly, in the very same year Dolgopolsky published his *Nostratic Dictionary* (2008), Bomhard published *Reconstructing Proto-Nostratic: Comparative Phonology, Morphology and Vocabulary* (2008).

do-European cognacy in this entry according to Dolgopolsky, but he conceded possible lexical borrowing from Dravidian into Old Indian (OI):

1717. *P₂ä'ŋ'ṽkṽ (or ***P₂Eŋ'ṽg'ṽ**) to split lengthwise, to divide' > **HS:** S: [1] CS *√płk v. 'split' > Ar √flq v. G 'split, cleave, divide lengthwise, cut in two halves' (→ Mz fəlləg 'chop\split [wood, etc.]), JEA √płk G {Sl.} 'split, smash', {Js.} 'split, create a gaping wound', JA [Mdr.] פּוֹרָא pal'kā {Js.} 'fissure, wound', {Lv.} 'Riss, Spalt', Jb {Jo.} √flk (3s sbjn. 'yṽ-fłzḵ) 'split, crack (like a skull)', intens.-conat. e'fōlk 'split into pieces'; [2] S *√płk > Ak {Sd.} √płk G '(Gebiet) abteilen', pīlku 'Abgrenzung, Gebiet' → MHb פּוֹרָא 'pələk, JA [Trg.] {Js.} פּוֹרָא pə'laḵ ~ פּוֹרָא 'pələk / em. פּוֹרָא pīl'kā 'district'; [3] S *√płg v. 'split, share' > Ar فَلَاح √flǧ G 'split asunder, share, allot sth. amongst', BHb √płg: N pf. נִפְּלַג nīp'lag 'was divided', D (pf. פִּלְּג pīl'leg, imv. פִּלְּג pa'l'leg) vt. 'split, furrow', פְּלַג 'pələg 'division' [Gn. 10.25, I Chr. 1.19], EpHb/IA {HJ} √płg 'half', MHb פּוֹרָא 'pələg 'part, share', Ug √płg G or N vi. 'be divided, disintegrate', IA, Plm √płg G or D, JA √płg vt. G, D 'divide, share', JA [Trg.] פּוֹרָא pə'lag / em. פּוֹרָא pal'gā, Sr פּוֹרָא pələ'gā 'part, half', Sr √płg vt. G 'divide, separate', Gz √flg (js. -fləg) G 'divide, split', Cn → Eg (EgSSc) pa-la₂-ga v. 'divide, share'; → S *'palag- 'ditch, canal, small stream' > Ak fOB {Sd.} palgu 'ditch, channel', BHb פּוֹרָא *pələg, Ug plg 'canal, small stream', IA {HJ} plg[?] s. e. 'canal', Ar فَلَاح falaǧ-, فَلَاح fa'ǧ- 'streamlet, brook', Gz falag 'river, brook, valley' (→ Gz √flg G 'flow'), Mh {Jo.} fəlēg 'watercourse', Jb E {Jo.} 'fe'lēg 'stream', Jb C {Jo.} 'fe'lēg 'oasis' ¶ The causes of voicing *k > *g are not yet clear ¶ OLS 349, Fr. III 70, GB 641-2, BK II 627-8, 631-3, Ln. 2436-8, 2441-3, Hv. 573-5, Sd. 813, 815-6, 863, KB 877-8, HJ 911-3, Js. 1175-6, 1182, 1185, Lv. IV 57, Lv. T II 266, 270, Sl. 914, Br. 569-71, Jo. M 93, Jo. J 57, L G 159, Hlk. #73, SivCR 79, ≈ Blz. LB #60 (equates S *'palag- 'canal, stream' with the reflexes of N *puḷu 'to spring forth' [→ 'to flow', q.v.) || K: G pelik-i 'Stück der Fleischseite des Schlachtviehs; Viertel eines geschlachteten Tiers', G R pelik-i 'Hälfte eines der Länge lang halbierten Schweins' ¶ Chx. 1460 || U: FU *pālṽkṽ > pOs *peṽk- ({Hl.} *pāṽk-) 'half, side' (× U *pāṽ

'half' < N *P₂ā]qA 'half, part' [q.v. ffd.] × U *pē]e 'side' < N *P₂e]rê ṛ *P₂e]rê 'side' || D *paL∇k- ({{ṢGS} *p-?) v. 'split, cut lengthwise' > Krx palk- id., 'crack (the earth, a wall)', Mlt palkē v. 'cut up (as fruit/vegetable)', ? Prj pālva 'split piece of wood'; D b> OI phalaka-m 'board, lath'? (unless ← OI 'phala- 'burst' ¶¶ D #3991, M K II 392-3
 ◇ S *√p]g v. 'split, share' suggests that the original N etymon was *P₂E]∇'g?']∇ with subsequent contactation *-g?-> *-k- ~ *-k- > S *k and *k (n S *√p]k and *√p]k), K *k, U and D *-k- ◇ An etl. connection with N *paTUKū 'axe, hammer' is possible, but not certain.

The following instead is quoted from item 3.19 in Table S3 (by Yigal Bloch) in the “Supplementary material” of Agmon and Bloch (2013): the Semitic proto-word is given as *palg “water-course”; Hebrew *peleg* (*péleg*) “artificial water channel, canal”; Aramaic *plg* (as spelled) for “canal” (this is indicated as occurring in Official Aramaic, being a loanword from Akkadian); Ugaritic *plg* “stream, canal”; Arabic *falaḡun*, *fuluḡun* “river, rivulet, running stream of water (for irrigation)”; Modern South Arabian (in Mehri) *fālēg* “water-course (not artificial)”; Ethiopic *falaga* “to flow, cause to flow in torrents, dig out, divide, split”, and *falag* “river, brook, valley”; Akkadian *palgu* “canal, irrigation ditch”. In fn. 45 to the Arabic entry, Bloch remarked: “With regard to an irrigation system whose name in Arabic is derived from the root *flg*, compare the *Aflaj* (*ʿaflaḡ*, ‘channels’) system in Oman, a World Heritage site that dates ca. 500 C.E. (<http://www.worldheritagesite.org/sites/aflaj.html>)”.

4.2. *Vennemann on Proto-Germanic +aPal- / +aPili- 'noble (noun/adjective)'*

Vennemann (vii–viii, 14, 77, 81, 83, 87, 88, 125), i.e., mainly in Ch. 6 – as well as in Vennemann (2012), which among the other things, surveys in detail unconvincing alternative etymologies found in the literature, and the chronological sequence of occurrence in English (e.g., still in 1844, “Ethelings, or princes of the blood”, and in 1861, “In the Atheling Alchfrid, Wilfrid had a friend”) – derives Proto-Germanic +aPal- / +aPili- ‘noble (noun/adjective)’ from the Semitic root ?-ṣ-l, whose Semitic lexical derivatives include the sense ‘noble’ indeed, especially in Hebrew (representing Northwest Semitic). Vennemann

acknowledges that he was preceded in detecting this parallel, by Hermann Möller (1911), who nevertheless did so in the context of his theory of Indo-Germanic and Semitic relation.

In Arabic instead the sense is ‘original’ (even though Vennemann (2012: 970) lists occurrences in Classical Arabic, including a phrase which means “someone of an excellent origin, or race, or stock”). Incidentally, even the present-day English terminology of linguistics comprises a derivative of the Arabic term through Malay. The Aslian languages are found throughout the Malay Peninsula; some of the Asli groups are hunter-gatherers. Because of the latter circumstance, Asli people from the Malay Peninsula suffer from prejudice in Malaysia. Clearly, what matters in their ethnic name, etymologically, is the sense ‘aboriginal’, from Arabic *ʾašlī* ‘original’, ‘the real McCoy’, and the Arabic noun /ʔašl/ ‘origin’.

Vennemann claims: “Ancient Germania shows a number of striking similarities to the old Semitic world in language and culture” (vi). This comprises vocabulary, religion, and writing. He hypothesises lexical borrowing. “Some of these words belong to lexical domains (war, the law, societal organization) where loanwords are regularly owed to superstratal contact influence” (viii). In the case of terms for ‘noble (noun/adjective)’, this would have been first applied to an immigrant elite, before mutual acculturation and absorption, and application to the native aristocracy. In my opinion it may be that the lexical borrowing occurred in the Bronze or Iron Age through contact with Phoenicians or Punic incomers, but even though this is perhaps more likely, I would not exclude the alternative possibility that the loanword was Neolithic, from the time of the spread of agriculture.

As Vennemann is inclined to consider the given Germanic lexical type for ‘noble’ to be a borrowing from Semitic, he may also have considered Akkadian *eṭellu*. This has a different root, not the one Vennemann considered. Moreover, as a loanword in Hurrian (a non-Semitic language), [d] is found in place of [t]. Fournet (2012: 93) remarked that Hurrian “*edeḥli* ‘prince’ is considered by Laroche to be a direct transcription of Akkadian *eṭellu*, but the ⟨h⟩ in *ede-h-li* requires some emendation or adjustment”. The reference is to Emmanuel Laroche’s *Glossaire de la langue hurrite* (1980: 73).

I would like to point out that in the historical period, one comes across a term for ‘knight’ in Semitic languages – Akkadian *mariani*,

Ugaritic *maryanu* – which is quite likely of Indo-Aryan origin (Sanskrit *marya* ‘hero’), and which was borrowed from Semitic into Egyptian as a descriptor of noble captives in the Annals of Thutmose III (but also in Amenhotpe II Karnak stele) in a form reconstructed as **maryana* by James Hoch according to several hieroglyphic spellings that yield that form (Hoch 1994, §175, pp. 135–137). This is interesting to mention, because the direction of the borrowing apparently was from Indo-European to Semitic, and because the occurrences in the Egyptian texts, while noting noble status, are also in a context in which the persons so described are in a situationally inferior status *vis-à-vis* Egyptian royalty.

Moreover, the following shows how some given cultural context affects meaning as perceived in a lexical occurrence within a text. As pointed out by Pinchas Wechter (1964: 16), the anonymous author of a Judaeo-Arabic commentary on Maimonides’ *Guide of the Perplexed*, cited, concerning the Hebrew word “*šīlēi* (*Exodus* 24:11), what the Spanish Jewish author from the end of the 11th century, Abū Ibrāhīm Iṣḥāq Ibn Barūn had written in an otherwise unpreserved entry of his extant comparative linguistic treatise in Hebrew lexicography in relation to Arabic, the *Book of Comparison*: the commentator on Maimonides claimed that Ibn Barūn stated in the *Book of Comparison* that Biblical Hebrew “*šīlēi* “denotes the root and origin of a thing and is related to Arabic ‘*āṣal*, since it is the people of learning who constitute our real foundation”. Whereas the biblical text was referring to the notables of the Israelites, the point that the commentator on Maimonides was conveying (in line with Maimonides’ own intellectual elitism), whether this was also Ibn Barūn’s view or otherwise, is that the notables were notables because of their exceeding learning. And indeed, the context in *Exodus* 24:11 is that the seventy “elders” of Israel accompanied Moses and had an ecstatic vision and remained unharmed by the experience.

4.3. Vennemann on Proto-Germanic ⁺*sibjō* ‘extended family’

Vennemann (vii–viii, 513 in Sec. 26.6.3.1.3, and Ch. 14)⁵⁹ derives Proto-Germanic ⁺*sibjō* ‘extended family’ (cf. German *Sippe*, English *sibling*) from the Semitic root *š-p-h*, whose Semitic lexical derivatives

⁵⁹ Ch. 14 in *Germania Semitica* originally appeared as Vennemann (2003b).

include Biblical Hebrew *mišpāḥā* in the sense ‘extended family’ indeed.⁶⁰ It must be said however that a possible difficulty is whether at

⁶⁰ In his harsh criticism for Vennemann’s book of 2003, Sheynin (2004) mentioned that his reliance on Orel and Stolbova’s (1995) reconstruction of the Hamito-Semitic lexicon (*Hamito-Semitic Etymological Dictionary*) is notwithstanding mistakes that were signalled by Diakonoff and Kogan (1996: 25–44) and Kogan (2002: 183–202, a version of the former). Sheynin wrote: “Since V. accepted the results of this publication uncritically, he inherited mistakes of this dictionary in his AA etymologies of the IE vocabulary”. Note however that Leonid Kogan is a co-author of Militarev and Kogan (2005), which as a matter of course used Orel and Stolbova’s dictionary, though by being cautious when appropriate (Sheynin’s point is that Vennemann did not possess the knowledge enabling one to use that resource critically). At any rate, Alan Kaye’s (1997a) review of Orel and Stolbova (1995), not cited by Sheynin (just as Greenberg 1996 was not), remarked: “let me express my profound disappointment in this book, particularly because many of these PAA roots involve what I consider to be coincidental look-alikes or *possible* cognates, while, at the same time, many genuine cognates have been unexplored or simply overlooked”, which frankly could be expected for the first attempt at such a task as Orel and Stolbova (1995) attempted. Kaye (1997a) exemplified both missed and mistaken instances of cognacy, and moreover cited approvingly a 1995 draft of Diakonoff and Kogan (1996), who stated an entire book would be required in response to the dictionary. Frankly, they should have written such a book: prompting a better book would have been a merit of Orel and Stolbova (1995). It must be said however that Diakonoff with four colleagues (one of them Stolbova) published in five instalments a *Historical Comparative Vocabulary of Afrasian* (Diakonoff et al. 1993–1997). It is a very difficult subject, and somebody had to start doing what Orel and Stolbova did. Ehret (1995) [cf. Ehret et al. (2004), “The Origins of Afroasiatic”] is an alternative, and stands in contrast to, Orel and Stolbova (1995), whose own updated version is the *DAE*, the online Database of Afroasiatic Etymology (Militarev and Stolbova n.d.). Actually, Kaye (1997a) foresaw that scholars in other disciplines may be misled by Orel and Stolbova (1995): “Non-specialists are certainly entitled to an accurate picture of the current state of Afroasiatic genetic classification, and will, unfortunately, parrot erroneous information, as has been pointed out, if they do not do some further checking of the matter themselves”.

Sheynin (2004) was damning to Vennemann when claiming in particular: “We can observe that the long list of Smc. etymologies in V. is united only by an [sic] incidental similarities and in no way better than popular etymology”. Whereas it is true that Vennemann is no Semitologist, so at times there were naïve spots, it must be said that the lay *étymologisant* does not usually think of resorting to formal rules. Moreover, throughout the history in recent generations of historical linguistics, scholars have often produced wrong etymologies, i.e., such etymologies that came to be more or less cogently shown to be wrong, or such that the *communis opinio* concerning them, if any obtains, is that they are wrong [Vennemann (1999b; 2006b, Sec. 3) was explicitly concerned with folk-etymologies, including learned ones.]. No human being, neither Vennemann, nor Sheynin (who admits it in our correspondence), nor of course

any time in antiquity the phonetic value [p] rather than [f] or [ɸ] was available for the Semitic phoneme /p/. The latter is transcribed as /f/ for Arabic, because its phonetic value in Arabic is always [f]. In Northwest Semitic instead there were two allophones. In Hebrew since the early

myself, could hope to know all potentially relevant vocabularies, and the rules of derivation of many languages. Scholarship of the prehistory of languages is pitted against what frankly is mission impossible, unless one commits to the rules of proto-languages. Sheynin has pointed out in his review (2013: 198), by referring to the scholar on Basque, Larry Trask, to whose memory the review was dedicated (and who had harshly criticised Vennemann's Basque etymologies): "In general the opinion of R.L. Trask about impossibility and silliness of linguistic reconstructions going back millennia from the first written records holds also for [the] Atlantic part of V.'s theory". [Trask was a joint author of *Time Depth in Historical Linguistics* (Renfrew et al. 2000). Vennemann exchanged emails with Trask, and allegedly Trask in one instance relented: it was about names with **aran-* relating to valleys, in relation to Basque *aran* 'valley' (Vennemann 2006b: 972).]

Even when aiming for partial success, there are decorous and professional ways to proceed. We may have gut feelings, or articulate an argument in favour of a comparison involving "incidental similarities", but to be punctilious, concerning Semitic data in Vennemann (2012) – but Sheynin was responding to Vennemann (2003a) – we should not confidently, indeed oracularly identify *en bloc* "incidental similarities". At most, we could properly speak of unlikelihood. In the volume of 2012, Vennemann's manner of proceeding is relatively sound (it is his premises that fail him, as it is easier to sday for supporters of Continuity Theory), even though one is not infrequently felt unconvinced. Sheynin is on firmer ground (indeed spot on) when he states, about the 2003 book: "In many cases V.'s Smc. etyma do not share meaning with his Gmc. or IE word or explained by a forced metaphoric extension of meaning". The lack of a good match, so that semantic constraints are relaxed, makes a hypothesis too costly, and the odds of "success" become too high.

Sheynin (2004) included a passage that is quite harsh (edulcorated in the later version of the review as published in 2013: but they both were published): "With all the criticism that completely dismisses V.'s theory of the Vascon character of the 'Old European', Kitson finds kind words for V., saying: 'Still Vennemann deserves thanks for supplying what had been a gap in the literature and showing us what a seriously worked up attempt to analyse the alteuropäisch linguistic material as non-Indo-European would [be] like'. We would like to reject even those kind words, because we understand that from intellectual aspect V.'s work was a non-serious manipulative attempt which was rejected by serious scholars and experts working in all the pertinent areas of linguistics". This apportioning of seriousness is unjustly wounding. I must say I side with Kitson finding kind words. Vennemann found the courage to jump into a *terra incognita*, with the big risk of incurring in errors; this opened a debate, and this is in my opinion a useful debate. What is more, his Semitic data can be reanalysed, even as one does not accept his premises and what these entail for his hypotheses.

Middle Ages, the two allophones are [p] and [f], but on the evidence of Greek and Latin transcriptions, still in the Imperial Roman period the two Hebrew (and Aramaic) allophones were [p̄] and [f] instead. I suspect that when the term at hand is concerned, the lexical borrowing into Proto-Germanic may have been as early as the spread of agriculture during the Neolithic.

Concerning Biblical Hebrew *mišpāḥā*, the sense is ‘extended family’, including servants as well, like Latin *familia*. Cf. Modern Hebrew *mišpāḥā* ‘family’ (including nuclear families), whereas Biblical Hebrew also has *šiphā* [šifḥā] ‘maidservant’, ‘female slave’,⁶¹ in the same semantic relation that Latin *famŭla* ‘maidservant’ has to Latin *familia*.

A male slave instead is called in Hebrew ‘*ébed* (Biblical Hebrew [‘εβεδ], Israeli Hebrew [‘eved]), semantically related to ‘labour’. From a publication which at first when writing this study I believed to be Maman (1999), but which I found out is not the one, I learned the following information, and thanks to the kindness of Aharon Maman (by email, in September 2016) I am now able to provide citations accurately.⁶² There is a brief relevant entry on p. 578 in Maman’s (2013)

⁶¹ Edward Bridge (2012) discussed the two Biblical Hebrew words for ‘female slave’, *šif’ḥā* vs. *’āmā*. He claimed he found “no general distinction in meaning between” the two “can be made”, and that the two terms “are synonyms, both when they designate women and when used by a speaker for deference. Patterns of use, or preference of one term over the other, however, occur”. He claimed that *’āmā* (when designating a woman) “is preferred in legal contexts”, whereas *šif’ḥā* “is preferred in Genesis. Outside Genesis, only” *’āmā* “is used in marital / conjugal contexts” (*ibid.*: 21). “Ultimately, the problem of determining distinctions in meaning between [the two terms] is due to the intertwining of context of use and genre of text. For each possible context of use, from which a distinctive meaning for each term could be proposed, the other term also appears, even if from a different genre of text” (*ibid.*).

⁶² Professor Maman’s citations are to an entry on ‘Black person’ in the chapter about Tunisia in Moshe Bar-Asher’s (5770 = 1999/2000) Hebrew-language book about traditions and languages of North African Jews (see pp. 211, 233); to an article in Hebrew by Yehudit Henscheke (1990/91) about Hebrew elements in the spoken Arabic of the Jews of Djerba, the island off the coasts of Tunisia, an island whose Jewish community generally speaking has specific traditions (Prof. Mamam also kindly sent me a scan of p. 98 from Henscheke’s 2007 book about the grammar and lexicon of the Hebrew component of Tunisian Judaeo-Arabic); to p. 174 in Joseph Tedghi’s (2003a) article “Évolution des recherches sur la composante hébraïque dans les parlers judéo-arabes maghrebins modernes”; as well as (in a different social and traditional context) to p. 667 in a book in Hebrew, by Aharon Ben David (2008), on the customs of the Jews of North Yemen. Note incidentally that Tedghi is a prolific

Synoptic Dictionary of the Hebrew Component in Jewish Languages, and it indicates in exemplification from Zarzis and Tatwin in Tunisia, that in the local Judeo-Arabic, one denotes ‘Black men’ by the Hebrew *Fremdwort* (m.pl.) *šfaḥim* (actually not found in Hebrew in that derivational and inflectional form, whereas *šfaḥót* for ‘female slaves’ is found in both biblical and later strata of Hebrew); for example, *hadúk lli yğanniw fəššáre kúllh̄m* [recte: *kúllhum*] *šfaḥim* means “those singing in the street are all Black”. An informant is quoted as stating that just an individual Black woman would have been referred to in that Judaeo-Arabic dialect as *šafhá* (which is the local pronunciation of Hebrew */šipḥa/*). But in the same sentence, the informant was using the presently polite Arabic word for ‘the Black people’, namely, *as-sūd* (literally, ‘the black’ m.pl.) which in his Maghrebine pronunciation was *əššúd* (that feature of Maghrebine Arabic pronunciation of the sibilants was also extended to Hebrew: Maghrebine traditional pronunciations of Hebrew also exchange the usual Hebrew /s/ [s] and /š/ [s] with /s/ [š] and /š/ [s], which is also the case of “Lithuanian”, i.e., northeast Polish or Bielorrussian or Baltic-state traditional pronunciation of Hebrew: “Lithuanian” refers to the former territory of the Grand Duchy of Lithuania). And yet, the very point of using Hebrew *Fremdwörter* for ‘slave’ while referring to a ‘Black person’ was in order to both maintain and disguise (by making non-interintelligible for non-Jewish Arabic speakers) the conflation which is the hallmark of the traditionally standard term for both ‘slave’ and ‘Black’. That Arabic term is [ʕabd].

In her grammatical study and lexicon of the Hebrew component of Tunisian Judaeo-Arabic, Yehudit Henscheke (2007: 98) lists among neologised Hebrew words, the following (my translation from Hebrew): “A clear instance of morphological neologisation emerges in the deliberate coinage [ʕše:fah] (= ‘Black man’). This secret-language word was formed from the feminine [šif’ḥa] ‘female slave’. The purpose of this formation intending concealment was disguise, and making the word unintelligible by non-Jews”. She cites for this from a publication by Moshe Bar-Asher she indicates as 5770 = 1999/2000: 211, whereas I find it instead in two spanned pages I was kindly sent by Aharon Maman, of Bar-Asher (5770 = 1999/2000, pp. 259–260). The context

author of scholarly papers, in French or Hebrew, about the Hebrew elements in Maghrebine and Levantine Judaeo-Arabic dialects (e.g., in French, Tedghi 1995, 2003b).

in Bar-Asher's book is a paragraph about secret language, which generally speaking is a part of a dialect which originates in use among merchants or artisans, even though it eventually becomes understood also by other members of the community speaking the dialect. Examples from the Hebrew component of Tunisian Judaeo-Arabic include: *ma'ase yadéinu*, literally 'our handiwork', for 'home-made brandy', 'bootleg brandy'; *tén lo perušó*, literally 'give him its explanation!', for 'carry out the deal!'; and [ʕše:fah] as a replacement for Arabic [ʕabd] in the sense 'Black man'; *Qedár* (the biblical name of a particular nomad ethnic group) referring to Muslims; and so forth.

Magrebine Jews had a social problem when, speaking among themselves, they needed to refer to a Black man (or woman, or group of persons), because of the social circumstance that Black people in the Maghreb were only allowed to live as Muslims,⁶³ and that made any unfree (or free) Black person into a social superior in terms of faith community membership to any free Jew in the Maghreb (because of the dhimmitude of the Peoples of the Book).⁶⁴ Had Jews referred to a Black man

⁶³ Quite possibly, this is why at one point in the 2000s, extremists sent both Barack Obama (the soon to be President of the United States) and Condoleezza Rice (the former Secretary of State) an ultimatum requiring them to convert. Arguably, they were singled out because of their skin colour, as though it was enough to exclude them from the option of submitting as unconverted dhimmīs.

⁶⁴ There was an episode in a Moroccan polity soon after the death of Sultan Muhammad III of Morocco (the fifth sultan of the Alaouite dynasty, which ruled the Tafilalet in 1631–1667, then ruled Morocco as sultans in 1667–1957, and since then have been ruling the country as kings). Muhammad III ibn Abdallah was succeeded by his once favourite son, but he had repeatedly rebelled against his father, and before his enthronement was a fugitive in the mountains. This was Moulay Yazid (r. 1790–1792). Yazīd was born in Fez in 1750, and was to die on 23 February 1792. Because of his actions, he was eventually referred to, punningly, by Maghrebine Jews in Hebrew as *hamMezid* (literally: 'the Deliberate Sinner', but in the sense 'the Malicious One'; Hebrew *yāzīd* is the third person singular masculine of the imperfective tense of the same verb of which *mēzīd* is the singular masculine active participle): that nickname is mentioned in Maman (1999: 178) and Hirschberg (1974: 293); it was given by a chronicler, Judah ben 'Obéd 'Attār. Yazid had his father's Jewish advisor executed, and ordered his Black slave troops (possibly locally born, rather than of sub-Saharan birth themselves: cf. Meyers 1977) to plunder Jewish neighbourhoods, something accompanied by rape. The order (apparently his very first order of business) was carried out, starting in Tétouan; then a delegation of Jewish women implored Yazid for the mayhem to stop, and the ruler grandly granted their request: see on pp. 308–309 in Norman Stillman's (1979) source book on Jews from Arab lands. Yazid's be-

in Moroccan Judaeo-Arabic as [‘ʕabd], which in Arabic vernaculars denotes both ‘slave’ and ‘black man’,⁶⁵ or had they the term replaced by relexifying it with the Hebrew [‘ʕebed] (in Moroccan Jewish pronunciation), this could have been overheard and understood by Muslims, thus making the infidel utterer vulnerable to an accusation of being illegally disrespectful towards the faithful, possibly with a dire outcome. What Moroccan Judaeo-Arabic did instead was to derive in Hebrew a masculine form (unrecorded in Hebrew usage) /šiph/, apparently pronounced in Tunisian Judaeo-Arabic as [‘šifah] (or

haviour towards the Jews is covered in great detail, quoting in English from a Hebrew nearly contemporary source, in Hirschberg (1974).

Arguably, Yazid’s *modus operandi* was that of an immature man (though aged forty at the time) who as soon as he was in power, played all societal parameters against each other: young against old, male against female, unfree but Muslim against free but Jewish, Black against white, and military against civilian. According to one interpretation, he was vindictive against the Jews of Tétouan because they had refused to provide him with funds to support him in his effort to overthrow his father. “He bore a deep grudge against Christians and Jews, especially the Sephardim, who had not helped him with loans when he was in distress. He began to take revenge on them as soon as he assumed control of Tetuan and Northern Morocco. Only the English were treated kindly because his mother or maternal grandmother had been an Englishwoman” (Hirschberg 1974: 293). Apparently, at first Yazid wanted to have all Jews killed, but was dissuaded by a qāḍī. The two years of Yazid’s reign “were steeped in blood; many acts of cruelty were committed against the Jews, especially the former friends of his father and their families” (*ibid.*). Yazid expelled the Jews from their neighbourhoods, and they lived in destitute conditions in temporary dwellings. Yazid also had the notables of the Jewish community of Meknes hanged by their feet during 15 days, to their death (Hirschfeld 1974: 298). At a later time, nearly one hundred Muslim notables in Marrakesh blinded with needles (Hirschfeld 1974: 300; cf. *Encyclopedia Judaica*, vol. 12: 338).

Cf. “Yazid of Morocco”, https://en.wikipedia.org/wiki/Yazid_of_Morocco and the section entitled “Les deux sombres années de Moulay Yazid (1790–1792)” in Abitbol (2009: 278–279), as well as the section entitled “Moulay el-Yazid (1790–1792)” in Julien (2001: 618–619). Cf. Mariano Arribas Palau’s several publications about Moulay Yazid, especially in relation to Morocco’s relations with European countries.

After Yazid’s death, he was succeeded (notwithstanding a siblings’ revolt) by his brother, Moulay Sliman (or Sulayman) ibn Mohammed, who reigned from 1792 to 1830 and (unlike the dissolute Yazid) was quite devout; he did not persecute Jews, and is remembered kindly not only by Moroccan Muslims, but also by Moroccan Jews (Hirschfeld 1974: 301 ff).

⁶⁵ In Arabic broadcasts, at present the political correct *áswad* (sing.m.), *sūdā’* (sing.f.), *Sūd* (pl.m.) are adopted for ‘Black’ as being an ethnic name or adjective. Etymologically, it is a colour term.

[‘šifəḥ]?)⁶⁶ – instead of (or along with) the regular expected outcome [‘še:fəḥ] – out of *šipḥā* [šifḥā], now [šif’ḥa] ‘female slave’. In this case, they evidently felt, there would be no cognacy between Hebrew and Arabic to assist non-Jewish Arabic speakers in making sense of the word. Somehow, cultural commensality entailed that ‘slave’ and ‘black man’ were perceived as semantically akin, like in the dominant culture (even though a slave could also be, for example, European, which was frequently the case in Algeria under the Dey’s rule). Unable (because of their acculturated mentality) to escape that double-sense, they shifted to a Hebrew neologism used as a *Frendwort*. It must be said however that sub-Saharan [West] Africa was referred to, in written Maghrebine Hebrew, as [‘ʔereš haššeḥo’ri:m], literally ‘land of the Black (pl.)’, as a semantic calque from Arabic *Dār as-Sūdān*.

In the first published version (Sheynin 2004) – posted at the LINGUIST website – of his very harsh review of Vennemann’s book of 2003, Sheynin stated that he would exemplify errors in Vennemann’s Semitic etymologies. There was an original etymology of Sheynin’s own, and it was omitted from the second published version of the review: Sheynin (2013), in the volume edited by Jürgen Udolph (2013) in order to refute Vennemann’s conjectured Basque and Semitic substrates in Europe.

I must say that before Sheynin kindly explained to me his line of reasoning, I was quite surprised by the etymology suggested (which he omitted from a later version of the same review) in his second example: “Eng. sib, Germ Sippe (PG ⁺sibjo: ‘family’) (V.’s p. XVII, 936) Smc. root /[^]s-p-h!/ (the meaning of the root according to V., is ‘family’). In fact, Ar. s-p-h! / Hb. [^]s-p-h! means ‘to shed, to spill’, and etymology of the ‘family’ in Smc. probably comes from either ejaculation of semen, or by popular understanding of conception as spilling blood of man into the vagina of his female sexual partner”. I was very surprised at this. In

⁶⁶ There is no entry for this in Maman’s (2013) *Synoptic Dictionary of the Hebrew Component in Jewish Languages*, which is attentive to Jewish vernaculars of North Africa (all Maghreb countries, but also Egypt), Yemen and Aden, Iraq (in Judaeo-Arabic) and Kurdistan (in Judaeo-Aramaic), Iran, Georgia (in Judaeo-Kartvelic), with entries also from Daghestan (in the Tat language), Turkey (from Bursa and Urfa), former Yugoslav Macedonia (in Judaeo-Spanish from Monastir, i.e., Bitola), as well as Italy (there are many references to a major study in progress by Maria Luisa Mayer Modena).

fact, the Hebrew verb for ‘to shed’, ‘to spill’, ‘to pour’ is *šāfak*. There is a nominal derivative in Biblical Hebrew, *šofkā*, which denotes the *membrum virile* with the semantic motivation that it spills (semen or urine). Sheynin was conflating the roots *š.f.k.* and *š.f.h.* The two phonemes /ḥ/ and /k/ are quite distinct and do not turn into each other,⁶⁷ even though in present-day Hebrew pronunciation, [x] is the sound of /ḥ/ and of one of the two allophones of /k/. Actually there are claims, only known to specialists (see Khan 1996) to the effect that in antiquity, /k/ was always [x] in Hebrew.

In email exchanges in September 2016 concerning Vennemann’s theories, Sheynin explained that the “question is about the period before anything [is] known of Semites”, so Vennemann was prompted to introduce the term *Semitides*, apparently intending “ancestors of Semities in Africa”. [In Vennemann (2012) this does not appear to be what Vennemann necessarily intended, but when referring to Hamito-Semitic, he probably meant that.] Concerning the etymology he himself proposed, Sheynin kindly explained to me: “If it is so, some African languages that have some common features with Semito-Hamitic ones should be considered (such as Hausa and Omotic languages). Hausa is closer to Semitic and has distinctinction of /h/, /ḥ/ and /ħ/, while some Omotic languages have only one voiceless glottal sound h. It appears that on the most ancient stage predecessors of Semito-Hamitic languages did not have distinctive phonemes /ḥ/ and /ħ/ (ḥet and ḥaf.) So on this stage shafaḥ = shafaḥ and mishpaḥa = mishpaḥa.” My response to this is that calling /ḥ/ by the name “ḥaf”, as Sheynin did in his informal explanation, is misleading, because it names that phoneme by the usual name, [xaf], of the similarly sounding allophone of the phoneme /k/, but /k/ and /ḥ/ do not mix.

If I was to venture in an attempt to etymologise Hebrew *mišpaḥā* ‘family’, I would quite tentatively begin by pointing out that members

⁶⁷ In Persian, through the conduit of the Manichaean texts (such as the Cologne Mani Codex), there is a mixup of the two Semitic phonemes, in the adaptation of the name of Enoch, *Aknūk* [ʔax’nu:x]. That is not the only form in which Enoch’s name appeared in Iranic texts however: “He was one of those prophets that the Holy Ghost (wāxš yōjdahr) used as instruments, in the Middle Persian M 299a (hwnwx, Hunōx; [...])” (s.v. *AKNŪK* in the *Encyclopaedia Iranica*, Asmussen (1984, revised 2011)). In an Uighur Manichaean text, “xunox burxan, ‘Buddha [God] Enoch’ is mentioned” (*ibid.*, brackets in the encyclopedia entry).

of what in Latin is called a *familia* are not only kin. Like in the familial group that Abraham headed according to the account in *Genesis*, also subordinated non-kin are included. In Latin, this is the case of domestic servants: the *famŭlus* ‘domestic servant’ and the *famŭla* ‘maidservant’, the female slave called *šifhā* in Hebrew. Therefore, I would rather seek a connection between the Semitic roots *š.p.h.* of *mišpaḥā* ‘family’, and *s.p.h.* of terms for ‘to attach’, ‘to annex’, ‘to become attached’, joining a social group, which is clearly the case in *Isaiah* 14:1 and *1 Samuel* 2:36, 26:19. Also note the agricultural sense of *sāfīaḥ*, defined as “frumentum anni antecedentis sponte proveniens”, in Mandelkern’s biblical concordance (1977 [1896]: 803, col. 2). And in fact, Mandelkern (*ibid.*: 1221, col. 4) declared himself in agreement with those etymologising *šifhā* “famula, ancilla, serva” from *s.p. ḥ.* because she became annexed to the household and the family. S.v. *mišpaḥā* “gens, universa familia, tribus; genus” (*ibid.*: 1222, col. 2), Mandelkern’s brief comment in Hebrew began with: “A group [*h^abūrā*] of persons whose are close/related and kin”. Note that the root *s.p.h.* and *h.b.r.* are semantically close.

4.4. Vennemann on Proto-Germanic ⁺maguz ‘boy’, ⁺magaḐ(i)z ‘girl’

Vennemann (vii–viii, 94–99, 100–104, 436, 443, 516) detects a Pre-Germanic ⁺-at- in Proto-Germanic ⁺maguz ‘boy’, ⁺magaḐ(i)z ‘girl’ This is instantiated in Gothic *magus*, *magaḐs*, in German *Magd*, *Mädchen*, and in English *maid*, *maiden*. This is the subject of Ch. 7, entitled “Germania Semitica: Pre-Gmc. ⁺-at- in E *maiden*, G *Magd/Mädchen*, Goth. *magaḐs**”. Vennemann claims that that Germanic word family “has clear cognates only in Celtic and is therefore suspicious of being a non-Indo-European loanword. Also the suffix -aḐ- < Pre-Gmc. ⁺-at- looks foreign: Inherited *t*-suffixes derive abstract feminine nouns, not concrete ones as in the case of ⁺mag-aḐ-iz” (94). Vennemann proposes that the source for this is the Semitic suffix -at- for deriving feminine nouns. He also makes a “similar proposal for two other Germanic words with a Pre-Germanic *t*-suffix: ⁺mit-ad-z ‘measure’, ⁺fah-ēd-z ‘joy’; an earlier proposal made for PGmc. ⁺furht- ‘fear’ is cited” (94).

Vennemann (96) mentions, admittedly without repeating his argument, his earlier suggestion in Vennemann (1997, pp. 454–456) “that these words and their Celtic relatives show the influence of the matrilinear culture of Prehistoric Atlantis colonizers of the Northwest” (96) and “that they are thus loanwords from languages closely related to Semitic” (96). We are on shaky grounds here, because the stem is unknown in Semitic. The space of possibilities is extended, through the speculative suggestion: “The Atlantic languages, Semitic languages or languages closely related to Semitic, with which pre-Germanic came into contact had a root ${}^+maC-$ ‘son, boy’ containing a final consonant ${}^+C$ which, according to the prosody of the borrowed words, the loanword adaptation strategies of pre-Germanic, and the prehistoric sound changes of Germanic, would yield PGmc. ${}^+g$. From this base we get the masculine singular stem ${}^+maC-u-$ ‘son, boy’ either in the Egyptian way with the masculine $-w$ suffix or in the Semitic way with the nominative singular $-u$ suffix” (98–99). As for “the matching singular feminine stem with the meaning ‘daughter, girl’” (99), it is obtained “either in the Egyptian way as ${}^+maC-at-$ (perhaps with some further vocalization) or in the Semitic way as ${}^+maC-at-u$ ” (99).⁶⁸

Vennemann claims that the tonic stress must have been on ${}^-at-$ in ${}^+maC-at(u)$, “or at least the word must have appeared as being so accented to the pre-Germanic borrowers, for otherwise the ${}^+t-$ would not have changed into ${}^-p-$ by Grimm’s Law but into ${}^-d-$ by Verner’s

⁶⁸ Agmon (2010: 26) remarks: “some Semitic affixes are widespread in AA, such as instrumental $m-$, feminine $-t$ or causative š- (see Lipiński 2001: §§29.20, 30.1–3, 41.7)”. Agmon then proceeded to suggest that during the passage from biconsonantal to triconsonantal lexical roots, such affixes may have been sometimes absorbed into the root. At some very early stage in Proto-Semitic, there may have been genders for given semantic categories (as opposed to masculine vs. feminine as in Semitic from the historical record and the present): “PS [i.e., Proto-Semitic] appears to “know” about the archaic use of animal fat for oil, because PS *šam means both ‘oil’ and ‘fat’. In fact, ‘animal fat’ may be considered a “body part”, in which case the final $-n$ could be a relic of the Semitic body-part gender (cf. Lipiński 2001: §30.11). To a certain extent, this supposition is also corroborated by AA data (*SED I* [i.e., Militarev and Kogan (2000)] #248; *DAE* #1571). [*DAE* is the online Database of Afroasiatic Etymology, replacing Orel and Stolbova (1995).] If so, one may reconstruct 2c [i.e., biconsonantal] *šam as originally meaning ‘animal fat oil’, such as was used for light in the UP [i.e., Upper Palaeolithic] (other Paleolithic uses may have been in ointments, but frying with animal fat already required metal pans)” (Agmon 2010: 33).

Law, as it did in the diminutive, OE *mægden*, OHG *magatīn* (< PGmc. ⁺*magad-īn-*)” (99). This is cogent enough. Vennemann proceeds to argue that expiratory stress and late accent placement is, though quite tentatively, not unreasonable to assume for the extinct, unknown, “Atlantic languages of northwestern Europe” (99) and in Phoenician, as well as for the Egyptian feminine *-at-* suffix (99).

I agree with Vennemann (102) that the word family of the proto-Germanic root ⁺*met-* ‘to measure’ is likely to be of Semitic origin, where the lexical root is *m-d-d* with the same sense). I suggest that the borrowing was Neolithic, at the time of the spread of agriculture. But note that the correct form is *middat*, not *midat*, when Vennemann writes: “In any event, the Hebrew word *midat* ‘measure’ looks disquietingly similar to Goth. *Mitad-* (with Grimm’s and Verner’s Laws < ⁺*medat-*) ‘measure’” (102). Are we to hypothesise that the conduit was speakers who degeminated? Moreover: “Unfortunately, I have no explanation for the apparent fact that the suffixes of PGmc. ⁺*fah-ēd-i-z* ‘joy’ and of PGmc. ⁺*mit-ad-(V)z* ‘measure’ were unaccented, whereas that of PGmc. ⁺*mag-aĪ-i-z* ‘girl’ was accented” (103). Nevertheless: “This problem does not exist if Thurneysen’s Law is assumed to operate in this case” (106, note 25).

Concerning *-t* for the feminine gender, it is interesting to consider a study, “An Aspect of Hittite Use of the Hurrian Suffix *-ta*”, by Paul Brosman, Jr. (1968). Neither Hurrian, nor Hittite were Semitic languages (Hittite was Indo-European), but they were from the ancient Near East. “The Hurrian directive ending *-ta* apparently occurs in the nominative-accusative plural of a number of Hittite neuter nouns. Johannes Friedrich has called attention to these forms, remarking that the suffix is used as if it were the case-ending”. Recently I cited evidence which seemed to confirm this suggestion” (*ibid.*: 526). On p. 527 he wrote:

If appearances concerning the Hurrian suffix are to be believed, its use seems to conflict with a rather fundamental view regarding Hittite grammar. Heinz Kronasser has stated that Hittite is clearly on the way to becoming genderless through restriction of the neuter. He suggests that this development may be due to the influence of Hattic and Hurrian, both genderless languages. The comment of Friedrich that the neuter is already in an advanced process of decline seems to contain a similar implication, particularly since it follows immediately upon a description of the prevailing belief in loss of a masculine/feminine distinction. The process by which an inflectional suffix is indirectly “borrowed” from one language by another is relatively rare and requires

intensive influence of one vocabulary upon the other. That it should have taken place in Hittite is, of course, quite plausible, for the Hittite vocabulary has been greatly expanded by loanwords. Moreover, Hurrian is a likely donor, for of known influences on Hittite aside from those of other members of the Anatolian family, its probably has been greatest. It seems unlikely, however, that such a process should be on the verge of accomplishment through borrowing into a dying category. This could be used as an argument against hasty assumptions concerning the significance of *-ta* in Hittite. However [...] On the basis of the usual assumption that inherited masculines and feminines are combined in the Hittite common gender, the ratio of barely more than two-to-one in either case would indicate that the neuter has held its own even if we began by arbitrarily assigning an equal number of words to each gender. Since, however, Indo-European evidence points to a relatively small number of neuter *i*-stems, it appears that far from declining, neuters were considerably more abundant among the *i*-stems in Hittite than in Proto-Indo-European..

On the next page, Brosman concluded:

That *-ta* is held to have had different meanings in Hittite and Hurrian does not in itself prevent the view that it was transferred through borrowing, for it is as true of suffixes transferred indirectly in borrowed words as of words actually borrowed that the meaning need not be the same in both languages involved. Friedrich has also proposed that *-ti* in the Hittite dative singular is the same *-ta* altered to correspond to the usual form of the dative in Hittite. If this suggestion be accepted, the English derivational suffix *-ette* provides an approximate parallel of the treatment of *-ta* to the extent that it shows a split in the meaning of a “borrowed” suffix involving a grammatical category not possessed by the recipient language, for *-ette*, as either a purely diminutive or purely feminine suffix differs in each case from the use of its source, Fr[ench] *-ette*, as a suffix forming feminine diminutives with or without corresponding masculines in *-et*.

4.5. *Vennemann on Proto-Germanic* ⁺*meta-* ‘to measure’

In the previous subsection, while discussing the suffix *-ab-* < Pre-Gmc. ⁺*-at-*, we mentioned that Vennemann proposes that the source for this is the Semitic suffix *-at-* for deriving feminine nouns, and that he also makes a “similar proposal for two other Germanic words with a Pre-Germanic *t*-suffix: ⁺*mit-ad-z* ‘measure’, ⁺*fah-ēd-z* ‘joy’; an earlier proposal made for PGmc. ⁺*furht-* ‘fear’ is cited” (94).

In Sec. 26.6.1.2.2 (506–508), Vennemann discusses Proto-Germanic ⁺*meta-* ‘to measure’ being borrowed from Semitic *mid-* ‘to measure’. This is fair enough. On p. 507, he states: “If Phoenic. *mdt* fem., which is likely to be the exact counterpart of Hebr. *mydt* [midat] fem. ‘measure’, found its way into pre-Germanic as a cultural loan-word,

this would nicely account – with Grimm’s law working on the root consonant and either Verner’s or Thurneysen’s Law on the suffix consonant – for Goth. *mitad-* fem. ‘measure’”.

I suspect however that the lexical borrowing may be (late) Neolithic, and at any rate, the Hebrew spelling is not *mydt* but *mdt* (like in Phoenician), and the word is *middat-*. Somebody was degeminating the double *d* into a single *d*: was it Semitic speakers, or Proto-Germanic recipients of the lexical borrowing, or some intermediate tradent (perhaps early farmers whose vernacular was not Semitic, though replete with Semitic vocabulary)? The Hebrew plene spellings *mydah* and (for the *status constructus*) *mydt-* are post-Biblical up to the present. This is not something to be encountered in the Hebrew Bible, because the plene spelling in the instance at hand represents as *y* an occurrence of /i/ even though it is a short *i*, not *ī*. In the Hebrew Bible, the headform *middā* (spelled *mdh*) is found in *Exodus* 26:2, 26:8, 36:9, 36:15, *1 Kings* 6:25, 7:37, *Isaiah* 45:14, *Ezekiel* 40:10, 46:22, 48:30, 48:35, *Zechariah* 2:5, *Nehemiah* 3:11, 3:19, 3:20, 3:21, 3:24, 3:27,⁶⁹ *1 Chronicles* 11:23, and 20:6. There are more occurrences, of the same word (in the *status absolutus* or in the *status constructus*) with a prefix (with the copula: *wmdh*, with the determinative article: *hmdh*, or with a preposition: *bmdh*, as well as *wmdt* and *kmdt* for both /k-middat-/ and /ka-middat-/ < /k-ha-middat-/), or of the plural (*mdwt*, /middot/) with or without a prefix (*kmdwt*), and its inflected forms with a possessive suffix (*mdwtyh*, *mdwtyw*).

The very first example of Semitic and Egyptian correspondence given by Wolf Leslau (1962: 45) is as follows: “Eg. *mdd* –measure for wine’ (late Egyptian); Hebr. *māḏad* ‘measure’, Akk. *madādu*, Ar. *madda* ‘stretch’ unless the Eg. *mdd* is a loanword”. Which it could well have been.

4.6. *Vennemann’s Hypothesis about Proto-Germanic Root* ⁺smit- of ⁺smitan ‘to strike, to throw’, the Semitic Root ṣ-m-d, and the Semitic Roots ṣ-p-d and ṣ-m-t

Vennemann (506, Sec. 26.6.1.2) exemplifies the shift ⁺*d* > ⁺*t* by means of Proto-Germanic ⁺*meta-* ‘to measure’, as well as by means of

⁶⁹ In *Nehemiah* 5:4, “*middat-* of the King” is a tribute, and it is sometimes claimed to be derived from a different root.

Proto-Germanic ⁺*smitan* ‘to strike, to throw’, “which has no Indo-European etymology” (506). Vennemann derives the root, ⁺*smit-*, of ⁺*smitan* from the Semitic “root *š-m-d* which means a certain kind of weapon for striking or throwing in Ugaritic and ‘to strike’ in Arabic” (506). This may be indeed.

Also note the Hebrew root *š-m-t* which is associated with a bifurcation but also, in the verb *hišmīt* ‘he killed’ (e.g., *ašmītēm* ‘I shall kill them’ in 2 *Samuel* 22:41) in a conjugation conveying (in the case at hand, as often) the ergative aspect, but also in the verb *šamaṭ* ‘to destroy’, ‘to cut’ (*Lamentations* 3:53: “they cut short / extinguished in the ditch my life”), which is derived in the basic conjugation.

Hebrew differs from Ugaritic and Arabic concerning the root *š-m-d*. In Hebrew the root *š-m-d* is associated with pairs and with adherence (‘to stick to’). The sense ‘adherence’ (‘to stick to’, but of the skin to the bones) is associated in Hebrew with another root, *š-p-d*. Phonemically, the two Hebrew roots *š-m-d* and *š-p-d* only differ because of their middle radical, but both phonemes are bilabial: [p] is the unvoiced bilabial stop, whereas [m] is the bilabial nasal consonant.

In his study “Noms de cordes en grec ancien: problèmes d’étymologie”, Michel Masson (1987) discusses the origin of ancient Greek names for ‘rope’, these being names which when a Greek origin cannot be found for them, are likely to be loanwords imported by travellers (sailors or merchants): these must have been *Wanderwörter*. For a Semitic etymological hypothesis to be accepted, Michel Masson requires that three conditions be satisfied: (a) the structure of the hypothesized etymon should match Semitic morphological patterns; (b) corresponding forms of the etymon are documented (other than as loanwords) in various Semitic languages, with at least one of these documented forms belonging to the most ancient lexicons: Biblical Hebrew, Ugaritic, Akkadian; (c) the adaptation of the Semitic etymon should be as economical as possible, and the pattern of each single hypothesized change should be supported by an identical treatment in other terms. These were his three criteria in his study about ancient Greek names for ‘rope’.

Let us consider, in particular, Michel Masson’s etymology of one of the names for ‘rope’: σφίδη. The Semitic root *š-p-d* yielded, in Arabic, names for ‘rope’. Biblical Hebrew has the verb *šāfād* ‘to stick’ (of the skin to the bones). In Tannatic (Roman-age) Hebrew, the same verb denotes ‘squeezing up’ (of the skin), while derivatives (of Aramaic

origin, but undocumented in Aramaic) include the name for ‘scurvy’, /ʃpidna/ or /ʃapdina/ (the latter variant, now pronounced as [tsaf’dina], is the standard name of this illness in Israeli Hebrew).

Michel Masson (1987) pointed out that the semantic shift from ‘tightening’ (and the like) to ‘rope’ is instantiated also for languages from other families: contrast German *Strang* (for ‘rope’) and English *string* against the Latin verb *stringere*, which denotes both ‘to press, squeeze’ and ‘to tighten’. Besides, Masson listed various instances of this semantic shift for Semitic roots other than the one, *ʃ-p-d*, we have been considering. Then, having found evidence for the semantic shift, Masson discussed morphological constraints. Let us indicate, by C, radical consonants in free-place formulae of Semitic derivational word-forms (the radicals are “plugged” into the “free places” in the formula which constitutes the derivational pattern in Semitic nonconcatenative morphology). If the consonants corresponding to the radicals of the root *ʃ-p-d* are removed from Greek σφίδη, then C₁C₂iC₃η remains. The final vowel and the accent are regular adaptations to Greek. There is, in Hebrew and Aramaic, the derivational pattern C₁^oC₂iC₃ā (phonemically /C₁C₂iC₃a/). Its lexical semantics is to form names for instruments. (The Arabic equivalent of this pattern is C₁aC₂iC₃a.) In particular, this morphological pattern is instantiated in several names for woven objects (‘basket’, ‘cradle’, ‘bandage’, ‘hosiery’ in Hebrew, and, in Arabic, ‘basket’, ‘knapsack’, ‘rope’, and so forth). Therefore, Masson considered his etymological conjecture to be plausible.

4.7. *Vennemann on Proto-Germanic Root ⁺drepa- ‘to hit’ and the Arabic Verb ǧáraba, with an Explanatory Analogy Using the Arabic Verb ǧarraṭa and the Recently Uncovered Hebrew Hapax of Root ʃ-r-ṭ*

In Sec. 26.6.1.1.2, Vennemann (505) exemplifies the shift ⁺b > ⁺p by means of Proto-Germanic ⁺drepa- ‘to hit’. Its representatives are Old English *drepan*, Old High German *treffan*, Old Saxon *-drepan*, and Old Norse *drepa*. There are “no correspondents in the other Indo-European languages” (505). Chapter 13 states (226):

Since the word contains the labial plosive ⁺p, it is a priori unlikely to be of Indo-European origin; this is so because PGmc. ⁺p derives, by Grimm’s Law, from PIE ⁺b, which is generally assumed to have been rare in Proto-Indo-European, or even not to

have existed at all. Therefore ⁺*drepa-* is likely to be a loan-word from a non-Indo-European language, and indeed a superstratal loan as its original use seems to have been that of a military term.

Vennemann mentions the etymology given in Mailhammer, Laker and Vennemann (2003)⁷⁰ – an article reprinted in *Germania Semitica* as Ch. 13 – where Proto-Germanic ⁺*drepa-* ‘to hit’ is compared to *ḍ-r-b* ‘to hit’ as in the verb *ḍáraba* ‘to hit’. This is fairly cogent indeed. Also note the Arabic verb *zárafa* ‘to perforate’.

Chapter 13 comprises Sec. 13.3, which ventures a hypothesis as for “why the word only occurs in Arabic and not in the other Semitic languages” (227). I would like to offer the remark that in Hebrew, the /ḍ/ in this verb would have resulted in /š/, similarly to the verbal form *mašrīt̄ im* (a masculine plural participle) ‘[they were] farting’, a hapax verb from a hapax root *ṣ-r-ṭ* (see below), which has been shown not to be a loan from Arabic but nevertheless corresponds to the Arabic verb (in a different conjugation *ḍarraṭa* and ‘to fart’ (in Modern Hebrew slang, the same Arabic verb for ‘to fart’ was adapted by replacing the [ḍ] with [d]). The Arabic verb *ḍarraṭa* and ‘to fart’ is in a conjugation which doubles the middle radical and expresses the intensive verbal aspect; contrast this to the Arabic verb from the same root in the basic conjugation: *ḍaraṭa* ‘to exert pressure’.

Now, let us consider a likely reason for Hebrew not to have a cognate of Arabic *ḍarraba* ‘to hit’: the Hebrew root *ṣ-r-b* was already taken by the transitive verb *šārab* ‘to scorch’ (‘he scorched’). Moreover, Hebrew used the verb *hikkā* in the senses ‘to hit’ and (in a military context in Biblical Hebrew) ‘to smitten’. These were too good reasons for Hebrew not to have a cognate of *ḍarraba* ‘to hit’. In contrast, the thrust of the argument in Ch. 13 in *Germania Semitica* is a discussion of alternative taxonomies of Semitic languages.

Let us turn to an explanation of the context in which the existence of the Hebrew root *ṣ-r-ṭ* was uncovered. The Cherubs made by Moses and placed on the Ark of the Covenant gave rise to a Christian tradition about an angelic order whose representation in art is as a winged baby (or his face, such as in the emblem of the University of Pisa), even

⁷⁰ “Arab. *ḍrb* ‘to hit’ was noticed in this connection by author Robert Mailhammer while writing his master’s thesis on Germanic verbal ablaut, and independently by author Stephen Laker in his studies of Germanic and Arabic philology” (231, note 10).

though the etymology seems to point to a sense ‘griffin’ (*griffin* apparently has the same etymology, and that as a visual motif, according to Jewish tradition, was associated with the curtain at the Tabernacle, the itinerant shrine built by Moses).⁷¹ In the Jewish tradition, too, as early as early rabbinic homiletics, the Cherubs are conceived of as looking like children, and a folk-etymology was invoked in support in the rabbinic tradition: “what is K’rub? – ... ‘Childlike’, for in Babylonia they call a child *rabia*” (*Babylonian Talmud*, tractate *Ḥagigah*, 13b, as rendered in the Jastrow dictionary (1903), s.v. *krwb* I); and likewise in *Midrash Tanḥuma*, as per the version edited by S. Buber (Vilna, 1885), *Genesis*, 25, by resorting to a similar folk-etymology, only referring to a Hebrew word instead of to its Babylonian Jewish Aramaic cognate. In the passage from *Ḥagigah*, 13b, the face of a cherub is described as being human, differing in size. Moreover, reference is made to *Ezekiel*, 1:10, stating that the face of the bull was changed into that of a cherub.

However, again with reference to that *locus* in *Ezekiel*, and with an inference made concerning Moses’ Cherubs from *Exodus*, a different interpretation of the Cherubs – identifying them with images of bulls – is proposed in the *Reuel Scrolls*, two tenth-century exegetic documents of Byzantine origin, in Hebrew with interspersed Greek glosses in Hebrew script, which were preserved in the Cairo Genizah and is now held at the Hebrew University (MS JNUL 4° 577.7/1). Referring to them as being the Reuel Scrolls is convenient: Reuel was the exegete or copyist who signed himself in the colophon. See Steiner’s Hebrew-language study (1995) “Linguistic Features of the Commentary on *Ezekiel* and the Minor Prophets in the Hebrew Scrolls from Byzantium”. The gloss to *Ezekiel*, 10:14, translated here from Hebrew, reads: “the face of the Cherub, i.e., the face of the bull [...]. Whence you learn that also the Cherubs made by Moses in the Tabernacle and that he placed in the Ark [] were bulls” (Steiner 1995: 42).

⁷¹ “The *kruvim*, the cherubs on the *kapporet*, or covering, of the Ark of the Covenant, had human faces and eagle’s wings [...] According to some sources, they had the bodies of lions [...] Lions and eagles appeared on the curtain of the Holy of Holies in the Tabernacle and later in the Temple. Some sources describe the veil as having been woven translucently, with the lions on the outside and the eagles on the inside, arranged in such a way that when one looked at the curtain one saw the illusion of lions with eagles’ wings, or griffins [...] The griffin appears often in later examples of art made by and for Jews, from the Middle Ages through the nineteenth century [...]” (Epstein 2015: 101).

An interesting contribution of the same document to Hebrew lexicography is, as Steiner points out, the plural participle *masrīṭīm* '[they were] farting', thus supplying a cognate for the Arabic verb *dárara* 'to fart' which has reflexes indeed in other Semitic languages, but prior to Steiner (1995) was not thought to have one in Hebrew. In a special section, Steiner (1995, pp. 54–56) shows that this is not a loan from Arabic, but rather must reflect an ancient Hebrew term from a Hebrew textual source that is no longer extant.

I would also like to remark that sometimes semantic motivation from 'hit' sometimes appears in different languages in idiomatic phrases with the same denotation, without there having been lexical borrowing or semantic calque to explain this. An example of this is the Arabic for "I have been ironing" "I hit (past) *ḥūṭī*". The idiomatic use of the lexical concept 'hit' in a phrase denoting 'ironing' (Arabic 'to hit *ḥūṭī*'), also occurs in the colloquial Italian *dare un colpo di ferro (a un indumento)*, equivalent to *dare una stirata*, which are a marked choice specifying a singulative action, as opposed to the unmarked term, the transitive verb *stirare*. The Arabic verb for 'to hit', *dáraba*, is involved in semantic calque in the following two instances: (a) A strike – in which workers stop working in order to put pressure on their employers or otherwise in protest – is called *idrāb* in Arabic, by semantic calque from English. (b) In Modern Hebrew, the transitive verb for 'to multiply', in the arithmetic sense, is /hikpil/ [hix'pil], by extending the sense of root *k-p-l* for 'double'. Historically however, one comes across the Hebrew verb *hikkā* 'to beat' in the sense 'to multiply', by semantic calque from that sense of Arabic *dáraba*.

5. Names from the Germanic Pantheon: The Theonyms

Phol and *Balder*

"The Phoenician theonym *Ba'al* has two reflexes in Germanic: ⁺*Pōl* (viz. OHG [= Old High German] *Phol*) and *Bal-* (in OHG *Balder*, ON [= Old Norse] *Baldr*)" (502). Cf. "the poetical Old English appellative *bealdor* 'lord' (506). The Northwest Semitic theonym *Ba'al* entered the Germanic pantheon twice, resulting in two different deities. Instances of double borrowing are called *Lehndubletten* or loan doublets; if one member is borrowed before, the other after a consonant shift, the pair is more specifically called a *Lautverschiebungsdublette*" (502). The latter is the case of the pair *Phol/Balder*, according to Vennemann. The two

theonyms co-occur in a given document, and have long been cogently taken to be co-referential: “They both occur in the same (Second) Merseburg Charm and have been understood as referring to the same deity by nearly all scholars since Jacob Grimm” (505–505).⁷² Vennemann interprets *Phol* as corresponding to the Phoenician and Punic *Ba'al* (literally ‘Lord’),⁷³ and *Balder* as an adaptation of Punic *Baldir*, which in turn results from an earlier form *Baliddir*, itself from *Ba'al 'Addir*, ‘Mighty Lord, Lod Almighty’, as Vennemann puts it (506): the theonym is accompanied by an epithet. The lexical items of which the compound consists are identical in Hebrew, but the contraction *Baldir* from *Ba'al 'Addir* occurs in Punic, not in Phoenician.⁷⁴

Incidentally, the bifurcation of divine identities, I reckon, is unsurprising:

- *Ba'al* was polytopic, a deity associated with different locales. And in fact, literally the name is associated with the semantic concepts of lording over, ownership, and being a woman’s husband. Whereas *Ba'al* on its own (or when it is accompanied by an adjectival epithet) is in the *status absolutus*, in contrast when it is followed by the name of a place it is in the *status constructus*, that is to say, its literal sense is ‘lord of’, intending the divine lord of that given place.⁷⁵

⁷² “The onset of *Phol* – *P^hol* in the manuscript – shows the effect of Grimm’s Law (⁺*b* > *p*) plus a superscribed *h* indicating the Old High German affricate of the Second Consonant Shift which is regularly spelled *ph* in Old High German manuscripts (*pf* in Modern German orthography)” (506).

⁷³ The context is Canaanite (including Phoenician and Punic) Baalism. Punningly, a new scholarly periodical, *Brill’s Annual of Afroasiatic Languages and Linguistics*, is acronymised by the publisher as *BAALL* (brill.nl/baall).

⁷⁴ “The later, only Punic form of the expanded name *Ba'al 'Addir*, *Baldir*, by contrast shows no sign of the shift (⁺*b*, ⁺*d* > idem) in the Old High German loan theonym *Balder* (nor in its Old Norse counterpart, *Baldr*)]” (506).

⁷⁵ The phenomenon of belief in polytopic deities, which on occasion causes conflict between two localised identities of that deity, is still extant. In his memoirs of his youth in Tunisia, Giuseppe Gabriele (2003) conveys poignantly the realities of the immigration of a large Pantesque community, i.e., emigrants from the island of Pantelleria off Sicily. There was competition and conflict between Italians and the French in Tunisia in the colonial period. Fascist Italy’s backstabbing of France when the latter was on her knees because of the German invasion, gave the French authorities in Tunisia a sense of legitimacy when right after the war, they punished the

- One also comes across a conflation of divine characters. In Ossetian mythology, following Christianisation (which in the mythology did not displace a pagan *forma mentis*), the angels

Italian community collectively (but afterwards, they allowed Italo-Tunisians to immigrate into mainland France with the same rights as the ethnic French, and this was considerably more than the Italian state did for Italian refugees from Tunisia).

Collective punishment of ethnic Italians in post-war colonial Tunisia by the French authorities extended to the religious domain. Discrimination against specifically Italian Catholic cults began much earlier in the colonial period. In Tunisia, the Capuchins were considered a hallmark of Italian identity (whereas in Damascus in the 19th century, they were a hallmark of French identity, even though some of them were of Italian extraction).

The Italian Capucins had been the Catholic monastic order active in Tunisia until 1887, when the French Cardinal Lavigerie brought about their expulsion from the country. He had them replaced with the French *pères blanches* of Carthage. Gabriele (2003: 71) states that when Lavigerie had the Capucins expelled, “the entire Italian community, freemasons and Jews included, rose in protest”. Gabriele is not innocent of *schadenfreude* when he relates (*ibid.*: 71–72) something that his own grandmother had learned from the *pères blanches*: namely, that the policy of having them mix with the Arab populace with conversionist intents, partly backfired, as allegedly 5% of the friars converted to Islam. Whether this is historically accurate or otherwise, it is significant that such lore was even voiced. (This phenomenon is also known from colonial history, in connection of the tsarist Russian policy of encouraging Buddhist monks from Mongolia to establish monasteries in areas of Russian-ruled Siberia, in the hope that once converted to Buddhism yet not steeped enough in it, the natives would have been more easily amenable to conversion to Russian Orthodox Christianity. What happened instead was that a substantial portion of the natives who had already converted to Christianity, became Buddhist. This motivated the Russian authorities to expel the Buddhist monks.)

Gabriele is more cogently, and definitely, gloatful when he relates how the French had been unsuccessful when, in 1946–1947 – having forbidden the procession of the Madonna of Tràpani (a town in Sicily) which was traditional in Tunis on August the 15th – they tried to have the French Notre-Dame des Victoires accepted instead (Gabriele 2003: 199–200). Some of the details in Gabriele’s book are potentially a feast for cultural studies, as the incidents concerning those processions appear to impinge directly on the nature of what a polytopic cult of the same sacred character amounts to, and moreover there are various degrees of syncretism: the author reads into empathetic Jews viewing the procession upon the reinstatement of the annual Sicilian cult, perhaps more sentimental participation than they provided, or than they would have admitted to themselves, and certainly to their rabbi and fellow congregants, but most likely it comes closest to the truth if we were to say that they distinguished the cultic aspect from just empathy for a joyful moment for community that was their cultural “commensal” (to use Norman Stillman’s term) in their shared Tunisian locale.

Michael and Gabriel were conflated into a god of plenty in Ossetian mythology, whereas Elijah was conflated, in the revised Ossetian pantheon, with Wasilla, a god of the previous pantheon. The following two quotations are from May (2016, pp. xxiv and xxiii respectively):

Mikalgabirta A popular cult among Ossetian Christians, which includes the names of two angel-saints Michael and Gabriel. In the nart epic the heavenly-dwellers Mikalgabirta, Rekom, and taranjelos arose where God's three tears fell, shed over the death of Batraz. Mikalgabirta is counted in Ossetian mythology as the god of plenty, and is much respected by those living on the upper reaches of the River Ardon. There in the Kahsar Ravine he has his shrine.

Elijah Wasilla in the pagan era, Ilya in the Christian era, god of thunder and bread-grain. Wasilla was also named god of lightning. Those slain by lightning stroke were not mourned, so as not to anger the god of thunder and lightning. They were buried where they had been struck down, with special ceremonies.

It could be said that the character of Elijah became bifurcated into \mathcal{E}_1 and \mathcal{E}_2 and that whereas \mathcal{E}_1 was conflated with Wasilla and Elijah's name replaced Wasilla's, in contrast \mathcal{E}_2 was conflated with Michael even onomastically. Actually however Colarusso (2016: 435) etymologises *Wasilla* /wacilla/ from /wac-illa/ "St. Elijah" (/c/, e.g., the affricate [tʃ], was spirantised to [s] in a sound shift that the Iron dialect of Ossetian underwent since the early Soviet period) just as he etymologises *Washtirji* /wastərji/ from */wac-žəržɨ/ "St. George".⁷⁶ (In Colarusso's notation, /j/ here is an affricate [ɟ] = [dʒ], as in English *job*, whereas /ž/ is the palato-alveolar fricative [ʒ] as in French *Jean*).

Vennemann's reasoning concerning the borrowing of the theonym *Ba'al* is as follows: "What happened was that the theonym was borrowed twice, once as ⁺*Bāl* before the operation of Grimm's Law, part ⁺*b* > ⁺*p* (and before the change of ⁺*ā* into ⁺*ō*), and again as *Bal-* in the late Punic form of the name, *Baldir*, after the operation of Grimm's Law" (502). In my opinion, considering that I assume the usual

⁷⁶ "**Washtirji** The heavenly dweller St. George, the patron saint of all travellers and warriors" (May (2016: xxvi). The proteiform shaping of lore about Elijah, St. George, al-Khidr, and St. Nicholas in monotheistic traditions is the subject of Nissan (2013 [2014]a).

timescales are telescoped and that Grimm's Law became operant considerably earlier than Vennemann assumes,⁷⁷ his remarkable insight marshals strong evidence that the earlier borrowing of the Northwest Semitic theonym was from a period compatible with the Neolithic spread of farming into what is now Germany. It is entirely plausible that the Northwest Semitic theonym persisted over a very long period, considering that the literal sense of the theonym *Ba'al* is as a descriptor of lordship.

Vennemann offers this caveat for his reconstruction (502):

Note that this explanation is only available in a reconstruction of Germanic which offers a pre-Germanic ⁺*b*, a change of ⁺*b* into ⁺*p*, and a Proto-Germanic ⁺*p* not liable to further shifting (except in the High Germanic shift of voiceless plosives into affricates, including ⁺*p* > *pf*; regularly spelled *ph* in Old High German). The change of pre-Germanic ⁺*b* into Germanic ⁺*p* is one of the crucial parts of the traditional reconstructions of the pre-Germanic plosive inventory and of the traditional formulation of Grimm's Law.

Vennemann's discussion of the Germanic theonyms *Phol* and *Balder* is rather cogent, in my opinion. Nevertheless, it is useful to bear in mind that coincidences of theonyms are not unheard of. Consider what Martin Schwartz of the University of California, Berkeley, wrote (2005 [2009]) in the *Skjærvø Festschrift* (on p. 148 in his article "Apollo and Khshathrapati, the Median Nergal, at Xanthos") about a wrong etymology of *Khshathrapati*, an Old Iranian divine epithet: "Thus the entire series of divine names similar to *Xšaθrapati* – Phoenician **šdrp**/Σατράπης, Man[daic] [and] Sogd[ian] (ə)Xšēš-pat, and Greco-Egyptian Σάραπις – is merely due to coincidence. One may compare (among many such banal random similarities⁷⁸ among names of gods as Akkad[ian] *Aššur*: Vedic *Ásura*-); the Phoenician *Melqart*:

⁷⁷ "That we find *Lautverschiebungsdubletten* also among the Semitic loan-words is important, because it proves that contact between the giving Semitic languages and the receiving Germanic language was not a brief affair but must have extended over a certain period of time, long enough to include the consonant shifts. This period of time may be compared to what is known about Phoenician history on one hand and to estimates of the time of the operation of Grimm's law based on Celtic loan-words on the other. It so happens that both strands of information converge nicely on the fifth to the third centuries BC" (502–503).

⁷⁸ Alan Kaye has pointed out (1999: 338); "The layman may well marvel that English *cook* and *cookie* are not related (see Trask 1996, 345–6), English *much* and Spanish *mucho* are not related, or that Persian *bad* 'bad' is not cognate with English *bad*. Imagine – identical meanings and identical phonemic structures!"

Corinthian Μελικέρτης, and West Semitic *Dagan, Dagon*: Southern Nuristani *Dagon, Dagan*". I must say however that I do not think that the similarity of *Melqart* and Μελικέρτης is merely coincidental, even though the Greek theonym may have been semantically remotivated.⁷⁹

In the history of modern ideas, there has been at least one 19th-century scholar who tried to etymologise *Balder* from a compound of *Baal*. That was the Scottish theologian Alexander Hislop (who in that book got it wrong on various things, but some other times was more or less correct) in an influential religious pamphlet⁸⁰ with the crankish title *The Two Babylons; or the Papal Worship: Proved to Be the Worship of Nimrod and His Wife*. The first edition appeared in 1853; the third edition, of nearly 500 pages, in 1862. The following is quoted from a popular edition of *The Two Babylons*, from a note entitled *The Identity of the Scandinavian Odin and Adon of Babylon*, at the end of Sec. 1 of Ch. 4 in that book (Hislop 1959, current online edition):

1. Nimrod, or Adon, or Adonis, of Babylon, was the great war-god. Odin, as is well known, was the same. 2 Nimrod, in the character of Bacchus, was regarded as the god of wine; Odin is represented as taking no food but wine. For thus we read in the *Edda*: "As to himself he [Odin] stands in no need of food; wine is to him instead of every other aliment, according to what is said in these verses: The illustrious father of armies, with his own hand, fattens his two wolves; but the victorious Odin takes no other nourishment to himself than what arises from the unintermitted quaffing of wine" (MALLETT, *20th Fable*). 3. The name of one of Odin's sons indicates the

⁷⁹ Controversial identities of deities occur in scholarship. Writing about Iran's early Sasanian king Ardashir I (Alexander Severus's foe in 222–235 CE), Touraj Daryaee wrote (2009: 5–6, his brackets): "Ardashir's coins also bear a standard formula which the succeeding kings in the third and the fourth centuries adopted: *mazdysn bgy ... MLK'n MLK' 'yl'n MNW ctry MN yzd'n* 'Mazdaean Majesty, [name of the king], King of Kings of *Ērān*, whose lineage (is) from the gods'. [...] Which 'gods' were his forefathers? The eponym of the dynasty, i.e., *Sasan* is clearly important to this question. It was thought that the epigraphic form *ssn*, which appeared on certain Parthian ostraca and other documents, designated *Sasan* as a Zoroastrian deity, although he was not mentioned in the *Avesta* or the Old Persian material [(Livshits 1977: 176)]. Recently, Martin Schwartz [(1996, 1998)] has suggested that the deity mentioned on the ostraca has nothing to do with *Sasan*, but represented *Sesen*, an old Semitic god which is found in Ugaritic [*recte*: Ugaritic] as early as the second millennium BCE". I find the latter hypothesis far-fetched in the Sasanian context, even though in the Byzantine context, the hypothesis of mutation into St. Sisinnios is an interesting possibility, end even though Schwartz is insightful and often worth heeding.

⁸⁰ See about it, https://en.wikipedia.org/wiki/The_Two_Babylons

meaning of Odin's own name. Balder, for whose death such lamentations were made, seems evidently just the Chaldee form of Baal zer, "The seed of Baal";⁸¹ for the Hebrew *z*, as is well known, frequently, in the later Chaldee, becomes *d*. Now, Baal and Adon both alike signify "Lord"; and, therefore, if Balder be admitted to be the seed or son of Baal, that is as much as to say that he is the son of Adon; and, consequently, Adon and Odin must be the same. This, of course, puts Odin a step back; makes his son to be the object of lamentation and not himself; but the same was the case also in Egypt; for there Horus the *child* was sometimes represented as torn in pieces, as Osiris had been. Clemens Alexandrinus says (*Cohortatio*), "they lament an infant torn in pieces by the Titans". The lamentations for Balder are very plainly the counterpart of the lamentations for Adonis; and, of course, if Balder was, as the lamentations prove him to have been, the favourite form of the Scandinavian Messiah, he was Adon, or "Lord", as well as his father.

6. Botany and Agriculture⁸²

6.1. *Proto-Germanic* plough *Words*

We have already devoted a subsection to Vennemann's treatment of ⁺*fulka-* 'division of an army'. Vennemann (507) also mentions original co-derivatives that resulted from separate events of lexical borrowing:

West Gmc. ⁺*pleha-*, ⁺*plega-* and ⁺*plōg-*, from Semitic words derived from the same root or related root forms (*p-l-g*, *p-l-ḥ*), must have been borrowed after the operation of Grimm's Law; for the plough word it is known that it is a very late borrowing. With regard to the Semitic root, a pair such as ⁺*fulka-* and ⁺*plōg-*, E[nglish] *folk* and *plough*, thus form a *Lautverschiebungsdublette*.

In my opinion, this is evidence that Grimm's Law became operant sometime when farming spread into *Germania*, and probably evolved ploughs were adopted. Also note that ⁺*pleha-* is compatible also with the Semitic (and more specifically, Aramaic) root *p-l-ḥ*, 'to work' (cf. the Hebrew semantically equivalent root *ʕ-b-d*), whence 'to work the land' and 'to adore, worship'. Cf. Hebrew and Arabic semantically

⁸¹ This etymology is spurious and untenable. The syntactic order of the compound is at odds with Semitic syntax (other than in the Bible in Ge'z, i.e. Classical Ethiopic, influenced by Greek syntax because of the prestige of the *Septuagint*). The Hebrew word for 'seed' is *zéra*'.

⁸² There exists a study by Militarev (2002), "The prehistory of a dispersal: The Proto-Afrasian (Afroasiatic) farming lexicon".

equivalent root *ʕ-b-d*. Whereas the latter is associated with both ‘to work’ and ‘to worship’ in Hebrew, and also with ‘to work the land’ with the appropriate direct object, in Arabic the root is associated with ‘to worship’. In Aramaic, it is the root *p-l-ḥ* that is associated with ‘to work’ and ‘to worship’, whence in Modern Hebrew the noun *pulḥān* ‘cultic procedure’. But *p-l-ḥ* in Hebrew is also associated with ‘to split’. As for Arabic, we have of course *fallāḥ* ‘tiller, farmer’. I suspect that the archisememe of *p-l-ḥ* within Semitic was ‘to till, to divide the soil into furrows’, and that ‘to work’ was a generalised sense, at first associated with farming. As for the sense ‘to worship’, consider that *ʕ-b-d* is a root associated with ‘to work’ as well as ‘to serve (somebody)’ and ‘slave’ (but also ‘servant, i.e., worshipper’, of some deity’).

I tentatively agree that it may be that for the sense ‘plough’, *p-l-g* ‘to divide’ (here, the direct object being the furrows, the earth) is behind ⁺*pleha-* just as it is behind ⁺*plega-* and ⁺*plōg-*, but I suspect that in the case of ⁺*pleha-* not only phonetic rules were at work (perhaps they would not have been expected in the time and place where the mutation occurred), but there was the facilitating factor of there presumably having been semantic attraction to the Semitic root *p-l-ḥ*.

Bomhard (1981: 452) relates Semitic terms for ‘to plough’ (such as Hebrew *ḥārās*) to Indo-European forms occurring in Hittite, Latin, Greek, and Gothic. Bomhard (1981: 409) relates Semitic terms for ‘to split, cleave, divide’ (such as Hebrew derivatives of the root *plg*, also for ‘canal’) to a few Indo-European forms with that lexical semantics which begin by *pl-* or *fl-* occurring in Old Icelandic and Lithuanian, but also Old English *flēan* ‘to flay’. The next cluster in Bomhard (1981: 452) relates Semitic terms for ‘to split, cleave’ (but also ‘to plow, till’, and ‘mill-stone’) derived from *plḥ* or *flq* to a reconstructed PIE form from which he derives Sanskrit *phálaxi* ‘to split, cleave’ and Old High German *spaltan* for that same sense. But apparently based on a Hebrew word for ‘mill-stone’, Bomhard (*ibid.*) also tries to relate (which I find rather unconvincing) to the same cluster some Indo-European terms for ‘stone’.

Alinei (2000a, pp. 871–874, Sec. 5.1.1.1: “Nomi dell’aratro”) discussed the development of the European terminology of ploughing.

The *plough*,⁸³ *Pflug* lexical type, found in Germanic, but also in Lithuanian *plūgas*, Russian *plug*, and Rumanian *plug*, is according to Alinei of Celtic origin (*ibid.*: 872; Alinei 1997).

In older English, as in other Germanic languages, the plough was traditionally known by other names, e.g. Old English *sulh*, Old High German *medela*, *geiza*, *huohilī(n)*, Old Norse *arðr* (Swedish *årder*), and Gothic *hōha*, all presumably referring to the ard (scratch plough).⁸⁴ The term plough or plow, as used today, was not common until 1700.

⁸³ British English spelling *plough*, American English spelling *plow*, but pronounced [ˈpləʊ].

⁸⁴ [https://en.wikipedia.org/wiki/Ard_\(plough\)](https://en.wikipedia.org/wiki/Ard_(plough)) The earliest form of the ard is the bow ard. Besides (*ibid.*):

Evidence appears in the Near East in the 6th millennium BC. Iron versions appeared c. 2300 BC both in Assyria and 3rd-dynasty Egypt. In Europe, the earliest known wooden ard (at Lavagone in Italy) dates from around 2300–2000 BC, but the earliest scratch marks date from 3500–3000 BC. All of these were bow ards, also depicted in the rock drawings of Bohuslän, Sweden, and Fontanalba, France.

One also finds in <https://en.wikipedia.org/wiki/Plough> (it, too, accessed on 8 September 2016) a section entitled “Ard”, which states:

Some ancient hoes, like the Egyptian *mr*, were pointed and strong enough to clear rocky soil and make seed drills, which is why they are called *hand-ards*. However, the domestication of oxen in Mesopotamia and the Indus valley civilization, perhaps as early as the 6th millennium BC, provided mankind with the draft power necessary to develop the larger, animal-drawn true **ard** (or scratch plough). The earliest was the *bow ard*, which consists of a *draft-pole* (or *beam*) pierced by a thinner vertical pointed stick called the *head* (or *body*), with one end being the *stilt* (handle) and the other a *share* (cutting blade) that was dragged through the topsoil to cut a shallow furrow ideal for most cereal crops. The ard does not clear new land well, so hoes or mattocks must be used to pull up grass and undergrowth, and a hand-held, coulter-like *ristle* could be used to cut deeper furrows ahead of the share. Because the ard leaves a strip of undisturbed earth between the furrows, the fields are often cross-ploughed lengthwise and widthwise, and this tends to form squarish fields (Celtic fields). The ard is best suited to loamy or sandy soils that are naturally fertilized by annual flooding, as in the Nile Delta and Fertile Crescent, and to a lesser extent any other cereal-growing region with light or thin

The modern word *plough* comes from Old Norse *plógr*, and therefore Germanic, but it appears relatively late (it is not attested in Gothic), and is thought to be a loanword from one of the north Italic languages. Words with the same root appeared with related meanings: in Raetic *plamorati* “wheeled heavy plough” (Pliny, *Nat. Hist.* 18, 172), and in Latin *plaustrum* “farm cart”, *plōstrum*, *plōstellum* “cart”, and *plōxenum*, *plōximum* “cart box”. The word must have originally referred to the wheeled heavy plough, which was common in Roman northwestern Europe by the A.D. 5th century.

Orel (2003) tentatively attaches *plough* to a PIE stem **blōkó-*, which gave Armenian *pelem* “to dig” and Welsh *bwlch* “crack”, though the word may not be of Indo-European origin.⁸⁵

The citation in the latter paragraph is of Vladimir Orel’s (2003) *A Handbook of Germanic Etymology*, s.v. **plōzuz*.

As for the lexical type of Italian *aratro* (< **arətrom*)⁸⁶ Alinei (2000a, pp. 872–873) considers it the outcome of the assimilation in the

soil. By the late Iron Age, ards in Europe were commonly fitted with coulter.

⁸⁵ The entire quotation is from <https://en.wikipedia.org/wiki/Plough> (accessed on 8 September 2016).

⁸⁶ Such as Spanish *arado* (whereas the noun for ‘plough’ in French is *charrue* instead). In Cicero, one finds the Latin form *ārātrum* ‘plough’, whereas *plamōrātum* ‘plough on wheels’ is found in Pliny the Elder (where the form that occurs is *plamoratatas*). For the latter term, see Alinei (1997), who proposed its etymology is Celtic, from a name for the helm of a ship; cf. Alinei (2000a: 568), Benozzo (2007, Sec. 2.2). The idea that behind a ship, one could see furrows creased in the sea occurs as well in the early rabbinic zoological imaginary: *Genesis Rabbah*, 31, states that the Re’em, the gigantic Wild Ox, being too large to fit inside Noah’s Ark, swam behind it, and while doing so it “was leaving furrows (*matlīm t’lāmiyyōt*) in the water as far as from Tiberias to Susitha” in the Galilee (cf. the idea of an outboard motor...).

More generally, Alinei (2001a, Sec. 5) stated: “The Middle-Eastern farmers who introduced Neolithic into Europe would be non-IE, and would have introduced non-IE influences in southern Europe, affecting but not replacing the indigenous IE languages of southern Europe. Eventually, the large cultural areas typical of Early Neolithic would undergo a process of cultural fragmentation (reflecting the previous cultural fragmentation of Mesolithic groups), accompanied by the parallel linguistic fragmentation of each language group”. Alinei’s boldface and underlining are there because of the contrast between “the ‘Neolithic Dispersal theory’ (henceforth NDT) [which he opposes], and the ‘Continuity theory’ (henceforth CT) [which is his own]. According to the former, the Proto-Indo-Europeans were the Middle-Eastern discoverers of farming, who would have introduced their language(s) into Europe along with their new economy. According to the latter, the Indo-Europeans would

Greek area of a Semitic term for ‘to plough’ (e.g., Hebrew *ḥāraš*, Aramaic *ḥ²rat*) to the Proto-Indo-European root **ar-* **(a)re-* **arə-* ‘to connect’, ‘to mortise’. The following is quoted from item 3.12 in Table S3 (by Yigal Bloch) in the “Supplementary material” of Agmon and Bloch (2013): the Semitic proto-word is given as **ḥrt* “to plow”; Hebrew *ḥrš* “to plow”; Aramaic (in Syriac in particular) *ḥrt* “to dig, cut in pieces, incise, plow”; Ugaritic *ḥrt* “to plow, till, farm the land”; Arabic *ḥaraṭa* “to plow”; Ethiopic *ḥarasa* “to plow”; Akkadian *erēšu* “to seed (using a plow), to cultivate a field”.

By the way, the diffusion of agricultural terms in ancient Mesopotamia is the subject of Blažek and Boisson (1992). Rubio (1999: 5), claimed, concerning *apin*, the Sumerian word for ‘plough’: “*apin* ‘plow’: it may be a *Wanderwort* (see Blažek and Boisson 1992: 21–23)”. Blažek and Boisson listed among likely *Wanderwörter* the Sumerian verb *uru₄* (spelled APIN) for ‘to plough’ (cf. Rubio 1999: 9). “The sign APIN is already in the archaic texts from Uruk, both lexical and administrative, the so-called List of Professions included [...], and also in Ebla” (*ibid.*, fn. 19).

For Hittite, cf. Weeks (1985, pp. 67–68):

8.21 – PLOW – The noun for ‘plough’ is always concealed by sumerographic ^{GIŠ}APIN (Akk. *epinnu*). The verb ‘to plow’, on the other hand, is well attested by two different terms, often occurring together in the asyndetic phrase **harszi terpi*. This expression reflects a situation similar to that denoted by *harra- malla-* ‘pound (and) grind’ (5.56), where the inherited word for an activity traditionally known to the early IE speakers is joined by a borrowed term for a more recently adopted Mesopotamian-based technique. Thus *teripp-* ‘plow’ matches Lat. *trepō* and Gk. *τρέπω* ‘turn’, with the same sense as in Lat. *terram vertere* ‘turn the sod’ and a possible meaning-parallel in Skt. *karṣ-*, Av. *karš-* ‘plough’ < **k^vel-s-* (**k^vel-* ‘turn’), whereas *har(a)s-* can be a technical loanword, from Akk. *harāšu* ‘plant’, *harāšu* ‘dig a furrow’, or WSem. *ḥaraš-* ‘plow’. See Puhvel, *AI* 1–3, 110–24; *Bi. Or.* 36 (1979): 57. Less likely competing explanations involve connections of *teripp-* with IE **treb-* (Mlr. *trebad* ‘plow, inhabit’, W. *tref* ‘homestead’, Goth. *þairp* ‘field’, Osc. *trībūm*, Lith. *trobà* ‘house’, etc. [IEW 1090]; e.g. G. Jucquois, *RHA* 22 (1964): 91–92), **drep-* (Gk. *δρέπω* ‘pluck’, *δρεπάνη* ‘sickle’ [Rosenkranz, *JEOL* 19 (1965–66): 502]), or **tri-* ‘rub’ (e.g. Gk. *τριβω* [Pisani, *Paideia* 9 (1954): 128]), but all present formal or

have always been – that is from at least Upper Paleolithic – in Europe and parts of Asia, and the Middle-Eastern farmers who introduced the new economy into Europe would thus be non-IE, and would have been absorbed by the Mesolithic populations”.

semantic difficulties. The derivation of *har(a)s-* from IE **ar(ā)-* ‘plow’ (from Goetze, *Tunnawi* 70), though very common, is doubtful (cf. T 182–83).⁸⁷

I would like to mention how Carlo Cattaneo began an essay of his (1861) – of course, obsolete for a 20th- or 21st-century reader – influenced by both Giovambattista Vico and early Indo-European philological comparisons, an essay entitled “Le origini italiane illustrate

⁸⁷ Where *EHS* stands for Heinz Kronasser, *Etymologie der hethitischen Sprache*, vol. 1 (1966; originally issued in 5 instalments, 1962–1966); where *IEW* stands for Julius Pokorny, *Indogermanisches etymologisches Wörterbuch* (1959); where *RHA* stands for the *Revue hittite et asianique*; where *JEOL* stands for *Jaarbericht van het Vooraziatisch-egyptisch Genootschap Ex Oriente Lux*; where Puhvel, *AI* stands for Jaan Puhvel’s *Analecta Indoeuropaea* (1981); and where “Goetze, *Tunnawi*” is a citation of Albrecht Goetze and E. H. Sturtevant, *The Hittite Ritual of Tunnawi* (1938). Cf. Weeks (1985: 7):

^{A.SĀ}*ku(e)ra-* ‘field’ is a ‘parcel’ (*EHS* 165) or ‘share’ of land, a thematic noun corresponding to the verb *kuer-*, *kur-* ‘cut’ (9.22); cf. Gk. τέμενος < τέμνω. Anat. cognates are Luw. *Mallitas-kuri-*, lit. ‘honey-field’, and possibly Lyd. *qira* ‘property, goods’ (T 611). Bomhard (*RHA* 31 [1973]: 112) preferred instead a comparison with Skt. *kr̥ṣāti*, *kārṣati* ‘plow’, *karṣūḥ* ‘furrow’, from an IE **kwer-* ‘plow’; for these see rather Puhvel, *AI* 118–24.

^{A.SĀ}*terippi-*, see 8.12.

where *EHS* stands for Heinz Kronasser, *Etymologie der hethitischen Sprache*, vol. 1 (1962); where *RHA* stands for the *Revue hittite et asianique*. And cf. Weeks (1985: 67):

8.11 – FARMER – ^{LÚ}ENGAR.

8.12 – FIELD – ^{A.SĀ}*terippi-* is from *teripp-* ‘plow’ (8.21); cf. late Church Slavic *ralija* (Pol. *rola*) from OCS *orati* ‘plow’, or Avest. *karšū-* (Skt. *karṣu-* ‘furrow’) from *karš-* (Skt. *kr̥ṣ-*) ‘plow’. An ‘irrigated field’ may be what is meant by ^{A.SĀ}*sissuras-* (*HWb.* 194, *HDW* 76; cf. Laroche, *Ugaritica* 5 [1969]: 778); denom. *sissuriya-* ‘irrigate’. It is a likely technical loanword, as are the terms for irrigation ditches or channels, *amiyara-* (P 48) and *alalima-* (P 28). The most probable connection of *arziya-* ‘cultivated land’ is with *arsī-* ‘cultivation, planting’ (> *arsai-*, *arsiya-* ‘cultivate’, 8.15); P 187.

where *HWb.* Stands for Johannes Friedrich, *Hethitisches Wörterbuch. Kurzgefasste kritische Sammlung der Deutungen hethitischer Wörter* (1952–54); where *HDW* stands for Johann Tischler, *Hethitisch-deutsches Wörterverzeichnis. Mit einem semasiologischen Index* (1982); and where *P* stands for Jaan Puhvel, *Hittite Etymological Dictionary* (1984–).

coi libri sacri dell'antica Persia" ["Italic Origins Illustrated with the Books of Ancient Persia"] (Cattaneo 2001: 143):

La prisca Italia, doviziosa di monumenti, non ha istoria se non di cinque secoli avanti l'era nostra. Più addietro, tranne il *serioso poema*, come parve a Vico l'istoria dei re di Roma, appena si raccolgono le date certe della fondazione d'alcune colonie greche. Greci e Latini non seppero chiarir bene le memorie dell'antica consanguinità, quantunque ne rilucesse loro evidente indizio ogni qual volta dicevano con voci simili in ambo le lingue: aratro, bue, toro, pecora, casa, pietra, nave, pelago, astro (*arotron*, *boûs*, *tauros*, *oîs*, *domos*, *petra*, *nays*, *pelagos*, *aster*), e perfino le membra del corpo: il piede, il ginocchio, il cuore, gli intestini, i denti. E penetrando nell'intimo organismo delle due lingue, potevano trovar d'ogni parte concordi le inflessioni e composizioni dei nomi e dei verbi; concordi, per un esempio scelto fra mille, le cadenze singolari *bos* e *boûs*, *aratum* e *arotron*, come le plurali *boves* e *boes*, *aratra* e *arotra*.

[Earliest Italy, rich of monuments, has no history earlier than five centuries before our era. Going further back in time, other than the "serious epic", which is how the history of the kings of Rome seemed to Vico [in *Scienza Nuova*, §1037], one can at most find out the certain dates of the founding of some Greek colonies. The Greeks and the Latins did not know how to adequately clarify the memories of their ancient kinship, even though an evident trace shone for them, each and every time they would say with similar lexical entries in both languages: plough (Italian *aratro*), ox (*bue*), bull (*toro*), sheep (*pecora*),⁸⁸ house (*casa*), stone (*pietra*), ship (*nave*), sea (*pelago*),⁸⁹ celestial body (*astro*) – the Greek for these respectively being *arotron*, *boûs*, *tauros*, *oîs*, *domos*, *petra*, *nays*, *pelagos*, *aster* – and even body parts: the foot, the knee, the heart, the intestines, and teeth. And penetrating into the intimate organism of the two languages (Latin and Greek), they could everywhere that the inflection and derivation of nouns and verbs were akin; this way, to pick an example out of a thousand, the pairs are akin of the singular forms *bos* and *boûs*, *aratum* and *arotron*, as well as the plural forms *boves* and *boes*, *aratra* and *arotra*.]

6.2. Latin *būris*, Roman-Age Hebrew *bóreḵ*, Arabic *burk* 'plough beam', a Semitic Loanword Motivated by the Shape of a Knee

A crucial part in the structure of a plough of the most rudimentary kind is composed of the plough beam – originally a tree branch – and a

⁸⁸ In Latin *ovis*, but Cattaneo listed the Italian, not the Latin words, and did not bother even as Italian *pecora* is not a corradical of Latin *ovis*. It was either because of distraction, or a reflection of Cattaneo's confidence that his readers would think right away of the Latin word.

⁸⁹ But that one is clearly a loanword from Greek.

ploughshare (Latin *vomer*, Italian *vòmere* or *vòmero*, French *soc*, Ger-German *Pflugschar*, Gaelic *risteal*, Old Norse *ristill*),⁹⁰ which originally was a secondary branch doing the ploughing, and still attached to the primary branch. The plough beam and ploughshare are still parts of the plough. The plough beam is attached to a longer beam (Latin *temo* > Italian *timone*, English *pole*) attached to the yoke, but could have been attached to the yoke itself, in more rudimentary ploughs. The ploughman pushing the plough holds the plough-handle, called *stiva* in Latin and Italian, and whose Arabic name, *wašla*, quite aptly also denotes ‘connection’, thus, ‘connecting piece’ (it is a cognate of the Roman-age Hebrew name <yšwl>).

The Latin name for ‘plough beam’ is *būris* (genitive *būris*), a feminine noun; it occurs in Virgil. A Latin variant is *būra* (genitive *būrae*). The Italian feminine noun *bure* ‘plough beam’ continues the lexical type of Latin *būris*. Such is not the case of French *age* ‘plough beam’, usually etymologised from Frankish **hagia*.

⁹⁰ Consider the following two entries from *The Concise Scots Dictionary* by Mairi Robinson (1985). Scots is the form of the English language that is characteristic of Scotland. The first entry we are reproducing here is for the noun *ristle* and its variants (Robinson 1985: 565, brackets in the original enclosing the etymology or cognates in Gaelic and Old Norse); it is documented beginning in the 18th century, it is a term chiefly from the Hebrides and the Highlands, and is now merely historical, no longer in use:

ristle &c [ˈristl] *n* a kind of small plough with a sickle-shaped coulter for cutting a narrow deep rut through strong roots 18-, *chf* Hebrides Highl, now *hist*. [Gaelic *risteal*; ON *ristill* a ploughshare; cf REEST³]

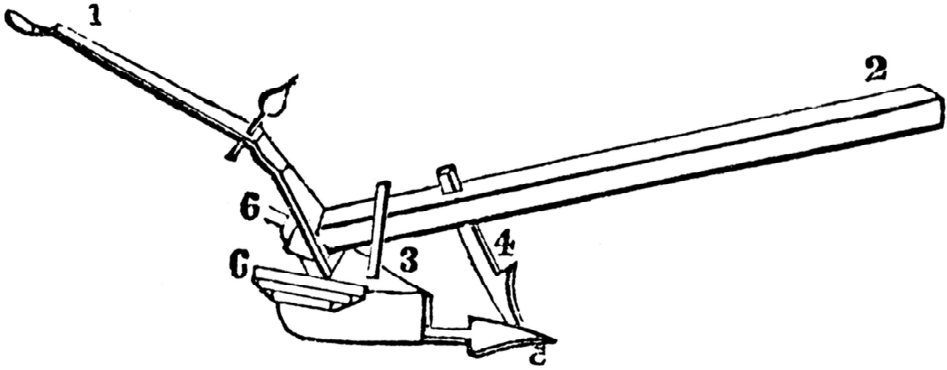
The second entry we are reproducing here (Robinson 1985: 551) is for the third lexeme of *reest* and its variants; as a noun, it is documented beginning in the 18th century, and is now in use in Aberdeenshire and in Central Scots, whereas as an intransitive verb, it is only documented in the 20th century, and is local in East Central Scots and West Central Scots. In brackets, Robinson pointed out cognates in Modern English dialects, as well as in early Modern English:

reest³ &c *n* the mould-board of a plough 18-, now *Abd C*.
vi tilt a plough to the right (*ie* to the mould-board side) 20-,
local EC–WC. [ModEng dial, eModEng; cf WREST²]

Agriculture in Eretz-Israel in the Period of the Bible and Talmud: Basic Farming Methods and Implements is a book (Feliks 1990) in Hebrew by Yehuda Feliks (1922–2006), a great expert in biblical and early rabbinic botany and agriculture. Whereas he does not seek an etymology for Latin *būris*, his discussion (*ibid.*, pp. 61–76) of the structure of ploughs in biblical and Roman-age Israel, as well as according to iconographical evidence from Mesopotamia and Egypt, the ancient Greek and Roman evidence, and kinds of ploughs as traditionally used by Palestinian Arab farmers per area of the country, is illuminating about ancient Hebrew vocabulary. The Roman-age Hebrew names for the various parts of the plough appear in the *Mishnah*, tractate *Kelim*, 21:2. A section in Feliks (1990) is concerned with the <bwrk> (see below) and the <yšwl> (*ibid.*, pp. 73–74).

In Roman-Age Hebrew, the name for ‘plough beam’ is <bwrk> (<kwrk> is a textual variant, because of the graphic similarity of the letters for and non-final <k>). This is apparently a segolate noun, *bóreḳ* [‘borekh] (the diachronic phonemic stem is /burk-/), and Arabic *burk* is either a cognate, or a loanword from Northwest Semitic. I propose (as Feliks already did: *ibid.*: 73) that the semantic motivation of Hebrew /burk-/ *bóreḳ* [‘borekh] is the sense ‘knee’ of the Hebrew noun *béreḳ* [‘berekh] (whose diachronic phonemic stem is /birk-/). In fact, traditionally the plough beam has been bent (Virgil stated that a tree was forced into the crooked form of the *buris*), and the shape metaphor is rather evident. I consider Latin *būris* to be a Neolithic loanword from Northwest Semitic. I find this plausible, because of my assumptions about the spread of farming from the Near East. Feliks instead stated (I translate): “In Latin, this part is called *buris*, unrelated to <bwrk>” (*ibid.*: 73, fn. 397). Latin *buris* denotes ‘draft-beam = plough-tail [of a plough]’ (the same as, from Greek, *histoboeus*). Anatomical metaphor is a semantic metaphor for this or that part of the plough: in Palestinian Arabic, the word for ‘ear’ names either posterior extension of the ploughshare in a type of plough from the north of the country (Feliks 1990: 68, Fig. 17), whereas in Roman-age Hebrew, the two surfaces that stand above those extensions are called by a name that literally means ‘cheeks’ (*ibid.*: 76, Fig. 21). But actually a Latin name for them was *ures*, ‘ears’ (in Italian, it is *orecchie*), like the

semantic motivation in Palestinian Arabic. The English name for the *aures* of the plough is *earth-boards*, or *mould-boards*.⁹¹



Plough parts and Latin names from Sir Thomas Browne's 1875 dictionary. Key list: the *buris* or *bura* (1) [Greek γύης, English *draft-beam*], the *temo* (2) [Greek ῥύμος, ἰστοβοεὺς, English *pole*], the *dentale* or *dens* (3) [Greek *elyma*, ἔλυμα, English *share-beam* or *ard-head*], the *culter* (4) [English *coulter*], the *vomer* (5) [Greek *hynis*, English *share*], and the two *aures* (6). The image is from Smith (1873: 32).

Italian *bure* 'draft-beam [of a plough]' was discussed with the other parts of the plough by Alinei (2000a), on p. 871 in Sec. 5.1.1, "Nomi dell'aratro e delle sue parti". Alinei was concerned with Germanic names for 'plough', such as German *Pflug*, in Alinei (1996: 614) and Alinei (2000a, pp. 229, 455, 568, 712, 872, 880). In northeastern Italian dialects, one finds *plovo* and the like for 'plough' (Alinei 2000a: 712).

6.3. A Digression about Early Sickles

The blades of early sickles apparently had indentations (they were "toothed").⁹² Alinei (2000a, pp. 847–848) discussed, among names for

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Cf.

http://penelope.uchicago.edu/Thayer/E/Roman/Texts/secondary/SMIGRA*/Aratrum.html

‘sickle’, the Italian lexical type *serra*, which he derives from Latin *series* (because of the series of indentures on the blades of early sickles), and this from the Latin verb *sero*, *serere*, ‘I order, I concatenate’, ‘to order, to concatenate’, with a transition *-ry-* > *-rr-* (cf. Alinei 2000a: 923) as typical of phonetic developments from southern Italy. The name *serra* is now widespread, Alinei remarked (2000a: 847), as denoting ‘saw’. Note however the Biblical and Modern Hebrew noun *maśśōr* ‘saw’ (now also *massōr*, but in Hebrew in Roman times *massār*), whose root is *n-ś-r* (which by Roman times was *n-s-r*) and the Roman-age noun *nēser* ‘board, plank’ and verbs *nāsār* ‘to saw’ and *nissēr* ‘to saw’ and ‘to make the sound of a saw’, and in Modern Hebrew *nsōret* ‘sawdust’. I suspect that the Hebrew trilateral root *n-ś-r* was in early Neolithic times a bilateral root **ś-r* (which became trilateralised by incorporating a preformative *n* of derivational patterns, perhaps of the passive, into the root). But Alinei’s reconstruction of the semantic motivation of *serra* from Latin *series* < *sero* is cogent. The semantic motivation of *n-ś-r* or *n-s-r* in Hebrew is unknown; it is a quite narrow word family. Was this an Italic borrowing into Northwest Semitic, already in pre-biblical times? Or is the similarity only by chance?⁹³ Moreover, what is the etymology of German *Messer* ‘knife’?

⁹² From early rabbinic Hebrew, Jastrow (1903: 14) one of the lexemes of early rabbinic Hebrew *āgār* include a second acceptation, whose verb he defined as “to halt”, even though that verb is not known to occur, other than its being instantiated in a participle; this acceptation was for a kind of knife denoted by the feminine participial noun *ōgēret*, which he defined thus: “a knife having indentations which catch the passing nail of the examiner”. That is a technical term from the domain of kosher slaughtering, as the smoothness of the blade of the slaughterer’s knife must be examined before use.

⁹³ Another example of *prima facie* striking similarity that perhaps is no more than by chance (or is this the outcome of Neolithic contacts of Semitic incomers in Italy?), is the following. Alinei (2000a: 850) mentions Latin *fessus* ‘cleft, divided in two’, from **fassus* and the verb *fatiscor* ‘to split, to cleave’ (cf. Italian *fendersi*). Alinei proposes a correlation with a Celtic lexical type, even though it is perhaps not very close. Note however that the lexical concept ‘fissure’ (Latin *fissūra*, Italian *fessura*) is shared with the Hebrew verb *pāšā* ‘to open (one’s mouth)’, the trilateral root being *p-š-(h)*, this root belonging to the *tertia infirmae* class. Also note the Hebrew noun *pissā* ‘slice’, and the roots *p-š-l* ‘to split’, *p-š-ḥ* ‘to crack open’, *p-š-ʿ* ‘to wound, to crack open’. The pre- and early Neolithic Proto-Semitic bilateral root would have been **p-š*. Importantly, the phonetic values [p] and [f] are allophones of just one phoneme, in Northwest Semitic, and apparently in Hellenistic- and Roman-time Hebrew (on the evidence of ancient transcriptions), the allophones were [p] and [f].

Is this similarity, too, just a coincidence? (But cf. Dutch *mes* ‘knife’, without a final *r*.)

Let us turn now to another topic, albeit one that still revolves around early sickles. As a preamble, however, I would like to signal a legend in Jewish homiletics ascribes to Noah the invention (or reinvention, Adam having been the original ploughman) of the plough (*Midrash Tanḥuma* to *Genesis* 5:29), and one textual *locus* (*Tanḥuma*, Buber’s version, *ad loc.*) states that Noah’s post-lapsarian precursors had only their fingers for ploughing. This is rather similar to the French lore behind the phrase *le peignoir d’Adam*, literally ‘Adam’s comb’: in order to comb his hair, Adam could only use his fingers. One using his or her fingers to comb their own hair is therefore using “Adam’s comb”.

Early kinds of sickle (or was it a plough that should have been considered?) were involved in a scholarly discussion (Barb 1972) of medieval traditions from the British Isles, concerning either an ass’s jawbone, or a camel’s jawbone,⁹⁴ being used by Cain when he killed

Perhaps also the bilateral root from the early days of agriculture was **f-ṣ*. This dovetails with Latin *fessus* ‘cleft, divided in two’, from **fassus*.

Also consider, in Arabic, the transitive verb *fāṣaqa* ‘to crack’, with the passive participle *maṣṣūq* ‘cracked, fissured’. Because of the phonological correspondence, in cognates, of the Arabic phoneme /s/ and the Hebrew phoneme /s/, the Hebrew cognate of the Arabic root *f-ṣ-q* ‘to crack’ is the root *p-s-q* ‘to interrupt’ (also in Aramaic; e.g., Hebrew now /psiq/ ‘comma’, from a Jewish Aramaic name of a diacritical mark from the first millennium C.E.).

What is more, the *t* inside the verb *fatiscor* reminds of the formative *t* that appears as a preformative or (in combination with some morphemes) infix inside word-forms of the reflexive conjugation in Northwest Semitic, such as in Hebrew. For example, if the root *p-ṣ-(h)* actually was *f-ṣ-(h)*, then along with the verb *fāṣā* one would have a reflexive **hitfaṣṣā*. But the position of the formative /t/ in Semitic, in some languages, could also be after the first radical consonant. This happens in Arabic: *iftāṣa!* (colloquial *fiāṣa!*) ‘he established contact’, with the action name *iftiṣā!* ‘establishing contact’ (as opposed to *ṣīla* ‘contact’). At any rate, it is not strictly necessary that the cue for the morphological constellation was provided along with the lexical borrowing; even assuming that the latter happened, yielding Latin **fassus*, it may be that the relation to *fatiscor* is explicable within Latin morphology.

⁹⁴ A jaw is prominently involved in another violent act from sacred narratives: according to the *Rāmāyaṇa*, the monkey Hanumān is called after something that happened to his jaw, and the noun *hanu* denotes ‘jaw’ indeed. “When Hanumān, in childish play, leapt into the sky and seized the sun, Indra, angry with him, hurled his thunderbolt and broke his left jaw, which gave him the name of ‘Jaw’ (Hanumān) (4.66.24) [...] But of course such darker concerns are not limited to children, and

Abel. The ass's jawbone must have been motivated by the biblical account Samson's using a jawbone to kill a multitude of Philistines to whom he had been consigned with no weapons, along with presumable reckoning about tools other than bones still being unavailable to Cain (especially if the murder he perpetrated was not premeditated).

Barb (1972: 387) tried the following argument, but while not being fully convinced, the line of reasoning is quite remarkable and ought not to elude the attention of scholars:

Now the oldest representation of a sickle (also used as an Egyptian hieroglyph) is frequently found on Pharaonic reliefs and paintings of harvesting scenes. Actually these sickles (see P1. 60c, e) closely resemble an animal's jawbone, often with clearly visible teeth. However, archaeological finds both in Egypt and Syria/Palestine show that from Neolithic times this type of sickle was carved from wood, and serrated flint blades were fastened with bitumen into a hollowed-out groove. The type lived on even into historical times in the Near East, where it was only gradually replaced by the metal sickle. The shape and construction of these wood-and-flint sickles make it clear that they derive, with technical improvements, from an earlier use of original jawbones. Although R. A. S. Macalister rejected the convincing hypothesis that there existed an intermediate form, where sharp flints were inserted into an animal's jawbone to replace the less effective original teeth, this theory was fully vindicated by the discovery of a number of specimens of just this kind; and in cases where the bone has been identified it has proved to be that of an ass.

Thus our fundamentalist Hiberno-Saxon artists appear to have been essentially correct in showing Cain killing Abel with the jawbone of an ass. But how could they have known this without being not just fundamentalists but also experts specializing in Near Eastern prehistoric archaeology? I suggest (despite the doubts of some scholars) that we see here another of the traces which emerge gradually of Near Eastern influences on the earliest Christian civilization of the British Isles. Syriac or Coptic monks, emigrating for one reason or another to the far West, might have been familiar with the animal jawbone as a primitive sickle, whether from wall paintings or reliefs in Old Egyptian ruins, or even from sickles still used in culturally retarded districts by the poorest class of peasant. Feeling that Cain had to be characterized as a 'tiller of the ground' by a sickle – just as occasionally Abel, the shepherd, by his lambskins (see P1. 60a) – they quite logically decided that his sickle was the jawbone of an ass. Perhaps there even existed in some Near Eastern idiom a colloquial expression for the sickle, meaning literally 'jawbone-of-an-ass'. I shall produce clues pointing in this direction later in this study (see below: 389).

when I said that the universe of the *Rāmāyaṇa* is a children's universe, I did not mean to restrict it, but only wished to account for its intensity. In the case of the descriptions of Hanuman, I believe there is a double voice: as an imaginary companion he is addressed, as it were, to children. But this voice, the one that is most certain of itself, sometimes lapses" (Moussaieff Masson 1981: 356).

On p. 389, Barb (1972) claimed indeed:

[...] On the other hand, as I said above, there might have existed an old popular name for the sickle which meant verbally just such a jawbone. There have always been in use in all languages similar picturesque animal names for tools, like ‘ram’, ‘crane’, ‘monkey-wrench’, ‘rat-tail’ (for a thin round file) in English, or the German ‘Geissfuss’ (goat’s leg) for a crowbar, ‘Fuchsschwanz’ (fox’s tail) for a tapering one-handed saw;⁹⁵ there was the Greek *onos* (ass) for a crane or millstone, and the Babylonian name *gamlu* for the sickle-weapon of Mardukh as been explained etymologically as ‘jawbone of a camel’.

But the story in the Book of Judges, and with it the traces of an ancient myth, does not end with these two verses; for after singing them Samson threw the jawbone away (Judges xv, 17) and the place where it fell was called Ramath-Lehi – ‘Elevation (or Hill) of the Jawbone’. Just so in Greek mythology did Kronos throw away his weapon after castrating Uranos, and the place where it fell was called Drepanon – ‘Sickle’ – the name of several localities in the Mediterranean region. The same story was also told about the Sicilian Zankle, this being a Sicilian dialect word for sickle or reaping knife. Admittedly these names could have originated from the sickle-shape of these localities, crescent-shaped islands, promontories or mountains, and the ancient myth could have been attached to them later. However, this kind of platitudinous explanation for an extant classical Greek place-name, Onougnathos – ‘Jawbone-of-an-ass’ – does not sound convincing: could such a sickle-shaped geographical formation really suggest to an observer the jawbone of just this particular animal? It seems far more likely that *onougnathos* was a word for ‘sickle’ and that the geographical names (Ramath)-Lehi, Drepanon, Zankle and Onougnathos were merely different expressions for the same sickle-weapon and the same ancient myth, told in various idioms about various mythological figures.

⁹⁵ As for German place-names, consider *Katzenelnbogen*, the name of a small town in Rhein-Lahn-Kreis in Rhineland-Palatinate. “Katzenelnbogen originated as a castle built on a promontory over the river Lahn around 1095” (<https://en.wikipedia.org/wiki/Katzenelnbogen>). The name of the place was motivated (or is perceived to have been motivated) by a river bend being likened to the elbow of a cat. A scholarly theory tried to prove a Roman origin (even though this is not supported by the records, as the name for the place first appears in medieval documents). It was claimed that the Roman-age name was **Cattimelibocus*, combining the Germanic tribal name of the Chatti, with “the name *Μηλίβοκον* (*Mēlibokon*) used by Ptolemy specifically for a mountain range farther to the east, either the Harz, the Thuringian Forest, or both” (*ibid.*). The town was the birth-place of Rabbi Meir ben Isaac Katzenellenbogen, also known as Maharam Padua (c. 1482 – 12 January 1565), who studied in Prague and then was chief rabbi of Padua and Venice, but was based in Padua. He is the ancestor of the still extant Katzenellenbogen family (https://en.wikipedia.org/wiki/Meir_Katzenellenbogen). Folk-etymology and German place-names (but not this one) are the subjects of Vennemann (1999b).

A short communication by Breeze (1992) in a journal in English studies pointed out that within a medieval Christian tradition on the narrative of Cain and Abel, the narrative variant that has it that Cain used a camel jawbone to kill Abel is distinctive of Irish sources, as opposed to the other variant (from texts more broadly co-territorial within the British Islands, i.e., also from Great Britain) that maintains that Cain's tool to that effect was the jawbone of a donkey. This detail was an evident product of narrative contamination from the story of Samson, who used the jawbone of a rather freshly deceased donkey (thus not fully skeletonised and dry) to kill several of his enemies, but see the discussion we have already quoted from Barb (1972).⁹⁶

Breeze (1992) was concerned in particular with an occurrence of the motif of the jawbone as Cain's murder-weapon, in the Old English *Dialogue of Solomon and Saturn* (Kemble 1848: 186). Oliver Emerson (1906: 853), quoted that Old English *locus* ("Chain his broðor ofsloh mid anes esoles cinbane":⁹⁷ I wonder whether there was intended wordplay on *Chain* and *cinbane*, with *Chain* having the velar stop [k] and *cinbane*, literally 'chin bone', also having [k], written as *c*).

⁹⁶ In this example, the kind of reasoning that matters is the difference between narrative variants as related to *stematology*, i.e., the dependence hierarchy between texts, or families of manuscripts (and early printed editions). In the given example, the geography of the manuscripts also matters. Stematology is due to the fact that manuscripts had to be copied, and even though copyists did not necessarily manage to be precise when copying, and for that matter did not even abide by a code of practice requiring that copies be exact, this is a far cry from creative intrusion into what they were copying. Unless a misreading is involved, it's no mere copying inexactitude when a tradition maintains that Cain used a camel jawbone instead of an ass jawbone. Howe et al. (2001) describes "using programs designed for biological analysis of sequence evolution to uncover the relationships between different manuscript versions of a text", i.e., for the purposes of stematology (van Reenen and van Mulken 1996): diagrams of phylogenetic analysis are used, in order to represent manuscript affiliation in a *stemma* (or stematological tree), which shows how manuscripts are clustered with respect to a supposed original. Stematological trees are not what is new about the method; it is application to stematology of software originally intended for biology, that was novel.

⁹⁷ "Tell me why stones do not bear fruit. I tell you because the blood of Abel, when Cain his brother killed him with an ass's jawbone, fell on a stone." Emerson's translation (1906: 854), with a changed syntactic sequence, of the Old English verse "Saga me forhwam stanas ne sint berende. / Ic ðe secge, forðamðe Abeles blod gefeol ofer stan, ða hine Chain his broðor ofsloh mid anes esoles cinbane".

Emerson (*ibid.*, fn. 5) remarked that the relevant verse from Kim-Kimble's edition "was quoted by Professor Skeat in *Notes and Queries*, 6th ser., II, 143 (1880), later reprinted in *A Student's Pastime*: 137, to explain 'Cain's jaw-bone' in *Hamlet*, v, i, 85. He also notes the lines from *Cursor Mundi*, quoted below, but mentions no further allusions in English and does not explain the origin of the tradition". In Shakespeare's *Hamlet*, we are told about "Cain's jaw-bone that did the first murder" (Emerson 1906: 853).⁹⁸

Emerson (1906: 854) proceeded to quote from various other sources as well. For example, in the Cornish mystery of *The Creation*, Cain says to Abel while hitting him: "Take that. / Thou foul knave, / On the jowl with bone of the jowl", thus, I would like to point out, with an exquisitely medieval *recirculatio*: Abel is hit on the jaw, and the instrument is an animal jaw. Emerson remarked (1906: 855) that Milton had Cain kill Abel by using a stone, whereas in Byron's *Cain, A Mystery*, Act III, Cain murders his brother "with a brand [...] which he snatches from the altar" on which he had offered his rejected sacrifice. Emerson then turns (1906, pp. 856–857) to a tradition according to which the scene of the murder was near or at the future city of Damascus. This occurs e.g. in Shakespeare's *I Henry VI*, I, iii, 39.

6.4. Harvest

Vennemann's main discussion of the Germanic *harvest* lexical type is in Sec. 26.6.3.1.5 on pp. 514–515. Like Möller (1911: 141), Vennemann cogently reconstructs "a west Indo-European root ⁺*karp-* 'to harvest fruit'" (514) – instantiated in Greek *karpós* 'fruit', Latin *carpere* 'to pluck', Lithuanian *kir̃pti* 'to cut', as well as Proto-Germanic

⁹⁸ In a section about the sources, Emerson (1906: 859) mentions his consulting Louis Ginzberg: "For the legend that made the instrument used by Cain the jawbone of an ass, I find nothing beyond the references in English itself. Dr. Ginzberg informs me that it is not Rabbinical in origin. I can suggest only that it may easily have come from some confusion with the story of Samson (*Judges*, 15, 16), but otherwise know of no explanation at present". Louis Ginzberg authored a now classic digest in seven volumes of Jewish traditions about biblical narratives, *Legends of the Jews* (Ginzberg 1909–1938). A discussion of Ginzberg's *magnum opus* can be found in an edited volume devoted to it (Hasan-Rokem and Gruenwald 2014).

⁺*harbista-/harbusta-* and some words for ‘sickle’ in Middle Irish, Rus-Russian, and Greek – in relation to “the Semitic root *ḥ-r-p* ‘to take fruit off the trees, to pluck, to harvest’ (Arab. *ḥarafa* ‘to pluck fruit from trees’, *ḥarīfun* ‘autumn, harvest, autumnal rain’), Hebr. *ḥoræp* ‘autumn, winter’, Akkad. *ḥarpū* ‘autumn’” (514).

In Proto-Germanic ⁺*harbista-/harbusta-* there is an added suffix ⁺*-st-*, “which combines with verbal and nominal bases to form (abstract) nouns” (514). Vennemann argues cogently that the (a derivative of) same Semitic root was borrowed into Germanic again: “The *harp* word (PGmc. ⁺*harpō* ‘harp’ [...]) derives from the same root, Semit. *ḥ-r-p* ‘to pluck’” (515). This word of an advanced culture was apparently borrowed later than the fructicultural term, viz. after Grimm’s Law, and only into Germanic” (515). In my own interpretation, the earlier borrowing was at the time of the spread of agriculture, whereas the second borrowing was in historical or (proto-historical) times.

6.5. Garden, Farm, Fenced Area

6.5.1. Proto-Germanic ⁺*gard-*

Vennemann in Sections 26.62.3.1 (not 26.2.3.1 as mis-referenced on p. 515) and 26.6.3.2.2 claims that Proto-Indo-European ⁺*g^hort-ó-s* – whence Proto-Germanic ⁺*gard-* ‘fence, fenced area, garden, farm, house’ – is a Semitic loanword, the original being *qart* as known in Phonician for ‘town, city’. “The root-final *-t-* of the Semitic etymon *qart* is reflected as ⁺*-t-* in the reconstructed Indo-European form and as ⁺*-d-* in Germanic owing to Verner’s Law” (515). “Words derived from this base are widespread in Indo-European. Their meanings extend from [Old Norse] *garðr* ‘fence, enclosed area, garden, farm’ all the way to Russ[ian] *górod* ‘town, city’” (511).

That may be. Note however that earliest farming with the associated increasingly large human settlements in the Neolithic included not only Semitic-speaking areas, but were also fairly prominent in parts of Anatolia. Clearly there are contacts and sequences of events that escape it. Having said that, the hypothesis makes sense.

I would like to signal what Greppin (1991: 724) wrote in support of the lexical semantics being either ‘town’ or ‘farm’, in etymologically related terms from different languages. He was discussing the

Armenian noun *art* for ‘field’, which first appears in a Bible from the fifth century, where the Greek text has ἀγρός. His etymology for Armenian *art* ‘field’ is from a Proto-Eastern Caucasian reconstructed form: “PEC **VrdwV-*, Chechen *urd* ‘peasant’s share of land’, Ingush *urd* ‘district’” (*ibid.*), with fn. 34 stating: “This *-rd-* might be a simplification of a larger cluster since root final *-rg* and *-rb* seem not to occur, according to Johanna Nichols”. What especially matters for our present purposes is Greppin’s fn. 35: “For a semantic parallel, see Slavic **gordь* ‘town’, ONorse *garð-r* ‘yard, farm’”.

We turn to another remark. Relevantly for when Vennemann states: “The emphatic voiceless plosive of Semitic *qart* (i.e. *ḳart*) had no exact phonetic copunterpart in Indo-European. While the velar plosive part of this sound posed no problem, a way had to be found to express the additional feature of emphaticness” (512), which Indo-European could replace with voicedness, aspiration, or voiced aspiration or breathy voice – note that even in Arabic, historically /q/ was not necessarily always a voiceless uvular stop, [q]. It has been suggested that it may also have had the phonetic value [G], a voiced uvular stop.⁹⁹ As a more recent analogue to the problem he stated, Vennemann points out that out of its three series of plosives, Ancient Greek rendered the Phoenician *q* with “κ, i.e. plain *k*, rather than χ /k^h/ (or γ /g/)” (512, cf. 525, note 35).

Moreover, there may have been lexical interference, when the sense is ‘fence’: I strongly suspect that a Semitic term like Hebrew *gādēr* ‘fence’ (which could be a boundary wall) was at work here, either on its own right, or by interference with the etymon proposed by Vennemann. For example, Balaam’s she-ass presses his leg against a vineyard wall (*gādēr*), when there were two such walls on both sides of the road (*Numbers* 22:24). In the plural, the word was used for pens in which to keep small cattle (sheep or goats), near a town (*Numbers* 32:16, 32:24, 32:36). Cognates occur in toponomastics: take the Hellenistic and Roman city of Gadara (near the Jordanian village of Umm-Qeis), with the ancient spa (Roman *thermae*) at Ḥammaṭ-Gader (Dvorjetsky 2007, Sec. 4.2, pp. 143–162), on the Israeli side of the Yarmuk Valley. “One of the most impressive monumental bathing complexes in the eastern Mediterranean Basin was revealed in the

⁹⁹ This was even mentioned in Geoffrey Khan’s (1996) “The Tiberian Pronunciation Tradition of Biblical Hebrew”. Even in that context, that bit of information may potentially make a difference for historical phonetics.

course of the excavations in Hammat-Gader” (*ibid.*: 143). Originally dedicated to the Three Graces, with pools associated with Eros, in Byzantine times the spa was associated with the prophet Elijah, but the Greek name is also evocative of Helios. “Already in the Hellenistic period Gadara seems to have been a city where medical therapies were practiced” (*ibid.*). Menippus of Gadara, the father of the Menippean satire, apparently moved to Greece in the mid-third century B.C.E. Philo of Gadara, in the third century C.E., improved on Archimedes’ approximation of mathematical π . “The Greek poet Meleager (first century BCE) described the city as ‘my first city, famous Gadara, Attica in the land of the Assyrians’. It appears to have been one of the most cultured of the Decapolis cities, known for its schools of philosophy and rhetoric, especially its Cynic school (Menippus, Philodemos, Theodoros, who taught the Emperor Tiberius rhetoric, and Oinomaos)” (Aviam and Richardson 2001: 187). Gadara “was built on a fertile spur of land high above the Jordan and Yarmuk Valleys (378 m. above sea level; the Kinneret [Sea of Galilee] is 210 m. below sea level). The site itself occupies a sharp saddle of land on the south side of the Yarmuk River, looking across to the Golan Heights (Gaulanitis)” (*ibid.*, their brackets).

A cognate of Hebrew *gādēr* ‘fence’ in Akkadian, *kudurru*, denoted ‘boundary-stone’, as well as an inscription on such a stone. A *kudurru*-inscription would include a warning with a curse (Fensham 1963). Boundary stones may also include a complex constellation visual motifs, relevant for the pantheon and imaginary, such as the scorpion-man archer from item 90858 of the British Museum,¹⁰⁰ a boundary-stone where the gods represented in relief on the limestone are witnesses of the decree, this being a charter dating from the reign of Nebuchadnezzar I (as opposed to the biblical Nebuchadnezzar, who was the second Babylonian king of that name). Cf. “And one who transgresses on a *gādēr*, a snake will bite him” (*Ecclesiastes* 10:8).

The following is the entry for Icelandic *garðr* on pp. 191–192 in the second edition of Cleary and Vigfusson’s (1957, repr. 1962) *An*

¹⁰⁰ Scorpion-men from ancient Mesopotamian art also appear in item Bab. 4375 from Berlin’s Vorderasiatisches Museum; as well as in item A 703 from the Antiquités Orientales collection at the Musée du Louvre in Paris. The motif of the Mesopotamian scorpion-man was discussed by Bord and Skubiszewski (2002).

*Icelandic-English Dictionary*¹⁰¹ (their brackets, my braces), and comprises such an array of acceptations that illustrates the semantic scope of the term in Scandinavian languages:

GARÐR, m. [Ulf. {= Ulfila} *gards* = οἶκος; A. S. {= Ancient Scandinavian} *geard*; Eng. *yard, garth, garden*; O. H. G. {= Old High German} *gart*; Germ. *garten*; Dan.-Swed. *gård*; Lat. *hortus*]: **I.** a yard (an enclosed space), esp. in compds., {...} 2. a court-yard, court and premises; {...} 3. esp. in Norway, Denmark, and Sweden, a house or building in a town or village, [Dan. *gaard* = Icel. *bær*]; {... p. 192: ...} 4. denoting a stronghold; {...} 5. in western Icel. a heavy snow-storm is called *garðr*. **II.** in Icel. sense a fence of any kind; {...} **III. Garðar**, m. pl. (í Görðum), **Garða-riki** or **Garða-veldi**, n. the empire of the Gardar, is the old Scand. name of the Scandinavian-Russian kingdom of the 10th and 11th centuries, parts of which were Hólm-garðar, Kænu-garðar, Nov-gorod, etc.; the name being derived from the castles or strongholds (*gardar*) {sic} which the Scandinavians erected among the Slavonic people, and the word tells the same tale as the Roman ‘castle’ in England; {...} the mod. Russ. *gorod* and *grad* are the remains of the old Scand. *garðr* = a castle; {...}

In Icelandic cosmogony,¹⁰² one comes across the following (Polomé and Rowe 2005: 3447):

¹⁰¹ Its first edition appeared in 1874. It is a very informative dictionary, attentive to the historical record of the entries throughout Icelandic literature.

¹⁰² The context is as follows (Polomé and Rowe 2005: 3446–3447):

In the *Prose Edda*, Snorri gives a complete description of creation that combines a number of older sources that are not always consistent with each other. [...] The first parts of the cosmos to emerge were Niflheimr (“dark world”), the sunless, misty world of death that lies in the north, and the blazing hot world of Muspell (the fire that would consume the earth) in the south. [...] The sparks and glowing embers flying out of Muspell met the hoarfrost and the ice, and from the slush and heat, life emerged in the shape of an anthropomorphic being named Ymir or Aurgelmir. From this primal giant sprang the dreadful brood of the frost giants, whom he engendered by sweating out a male and a female from under his left arm. In addition, one of his legs begat a son with his other leg. Here Snorri has merged two traditions [...] No direct source is available for the account of the origin of the gods that Snorri gives next: the melting rime has taken the shape of a cow, Auðumla, whose name contains the Old Norse word for “riches” and another term connected with the English dialect word *hummel* or *humble* (hornless cow), presumably designating a “rich hornless cow”. This cow feeds Ymir with the milk flowing from her udders, a tradition paralleling that of the primeval cow in Indo-Iranian mythology. Auðumla gets her own food by licking the salty ice blocks, which she shapes into another primal being, Búri, who begets a son, Borr. Borr marries Bestla, the daughter of the giant Boltorn (“evil thorn,” a term still used in the Jutland dialect [*bøltorn*] to designate a “scrappy, violent person”). Borr and his wife have three sons: Odinn, Vili, and Ve. The three divine brothers kill the giant Ymir, and the flow of blood gushing from his wounds drowns all the frost giants

The resulting world is circular, surrounded by a vast ocean. In the middle of the earth the gods establish *Miðgarðr* (“dwelling place in the middle”), a residence for mankind that is strengthened by a fence made from Ymir’s eyebrows, and they provide land on the shore for the giants to settle down. The next task of the gods is the creation of man. Finally, they build *Ásgarðr* (“dwelling place of the Asir”), their own residence.

except Bergelmir, who escapes mysteriously with his In the *Prose Edda*, Snorri gives a complete description of creation that combines a number of older sources that are not always consistent with each other.

It is interesting that the name of *Búri* or *Buri*, whom the cow *Auðumla* licks out of an ice-block, and who was the first god in Norse mythology (he is only known from Snorri Sturluson’s *Prose Edda*), has been etymologised from *burr* ‘son’. *Buri*’s son is *Borr*, who in turn fathered *Odin*, *Vili*, and *Ve*. Mention of an ice-block may be an adaptation to the cultural oikotype of the far north. The occurrence of a cow in cosmogony (also known from Iranian mythology) requires a civilisation stage where there were domestic animals (otherwise, a wild bull would have occurred). Polomé and Rowe remarked (2005: 3447): “A number of elements of the Eddic creation myth point to very old traditions. For example, the cow is a typical fertility symbol, and *Audumla* reminds us of the celestial cow in Middle Eastern and South Asian myths”. Could there have been any relation to the spread of agriculture and the lexicon of its original carriers from Northwest Semitic? In Aramaic, *bar* (which is in the *status constructus*) denotes ‘son of’, as opposed to the *status absolutus*, namely, *bərā* ‘son’ or ‘the son’; its inflected form *bərī* /bri/ denotes ‘my son’, and so forth. Naming two obscure generations of gods with names for ‘son’ would make sense. At any rate, <https://en.wikipedia.org/wiki/Búri> states:

The length of the u in the name is not explicitly marked in the manuscripts but it is traditionally assumed to be long because of its metrical position in Þorvaldr’s stanza. However, the metrical structure of *fornyrðislag* is hardly strict enough for definite conclusions to be reached from a single occurrence - especially when the imperfect oral and manuscript traditions are taken into account. It is thus entirely possible that the original form was *Buri*.

The meaning of either *Búri* or *Buri* is not known. The first could be related to *búr* meaning “storage room” and the second could be related to *burr* meaning “son”. “*Buri*” may mean “producer”.

It is quite possible that the similarity to the Aramaic noun is accidental. For that matter, one could compare, tongue in cheek, the names of those two Norse gods, the grandfather and the father of *Odin*, to the Samaritan Hebrew form (Talshir 1981: 334) of the Biblical Hebrew word spelled <hprh> (standard Hebrew *happārā*, ‘the cow’), which in the traditional Samaritan pronunciation is uttered as *abbarra* for ‘the cow’ (whereas without the determinative article, the word spelled <prh>, standing for standard Hebrew *pārā*, ‘cow’, the word is pronounced according to the Samaritan tradition as *farra*). Playfully, one may consider this apt, upon the evidence of the Icelandic tradition that this first Norse god was shaped into existence by the cow licking salty ice, until he emerged from that licked ice.

Moreover (Polomé and Rowe 2005: 3447):

Man lives in the center of the universe, and the major Germanic traditions concur in calling his dwelling place ‘the central abode’ (Goth., *Midjungards*; OHG, *Mittilgart*; OE, *Middangeard*; ON, *Miðgarðr*). But the center is also where the gods built their residence, *Ásgarðr*.

Dialectal English *garth* ‘yard’ comes from Old Norse, because of the Danish invasions. David Crystal writes (2011, pp. 57–58):

During the Middle English period we find Norse *egg* and *sister* ousting Old English *ey* and *sweoster*. And Old English *path* and *swell* ousted *reike* and *bolnen*. But there was a third solution: the Old English and Old Norse words both survived, because people gave them different meanings.¹⁰³ This is what happened to *skirt* and *shirt*. [... p. 58: ...] Cases like *shirt/skirt*, where both words survive, are known as *doublets*, and there are many of them in English. From the Danish period, we find Old Norse *dike* alongside Old English *ditch*, and similarly *hale* and *whole*, *scrub* and *shrub*, *sick* and *ill* and many more. There are even more in regional dialects, where the Old English word has become the standard form and the Old Norse word would remain local, as in *church* vs *kirk*, *yard* vs *garth*, *write* vs *scribe* and – of especial interest because of its widespread dialect use – *no* vs *nay*.

Alinei (1996, Sec. 4.2, pp. 617–618) discussed, by reframing it within his Continuity Theory, Pokorny’s (1959–1969 at 442, 444) clustering under the PIE roots **g̑her-* and **g̑herdh-* ‘to take’, ‘to connect’, ‘to fence in’, such terms as Hittite *gurtas-* ‘fortress’, Tocharian B *kerçīye* (plural) ‘palace’, Old Indian *gr̥hā* ‘dwelling’, Avestan *gərəða* ‘dwelling of magical beings’, Albanian *garth* ‘fenced area’, Greek *khórtos* ‘fenced place’, Welsh *garth* ‘fence’, Latin *hortus* ‘orchard’, Lithuanian *gārdas* ‘fence’, and Germanic and Slavic terms (also citing Buck 1949, Sec. 19.15). Alinei was especially interested in the Germanic lexical type; he pointed out that the transition from ‘fence’ to ‘garden’, to ‘town’ or ‘fortress’, is also displayed in the sequence *Zaun tuin town dunum* for “«recinto» «orto» «città» «fortezza»“, and noted how some scholars have found it problematic to conjecture in PIE the semantic shift from ‘taken (territory)’. Alinei

¹⁰³ This is what happened in Modern Italian (upon Italy’s unification) to the former synonyms (from different regions) *rubinetto* and *chiavetta* as denoting a water tap (both terms exist in quite similar forms in Romanian, where they also denote ‘water tap’). In 20th-century Italian, *chiavetta* became specialised for denoting the gas tap.

proposed (1996: 618) that as a pan-Indo-European word family assigned to the Neolithic, lexical borrowing was at work, from inside or outside Indo-European, considered borrowing from ancient southwest Asian involved in the spread of the Neolithic to be likely (this is quite relevant for our present context), and remarked that Allan Bomhard (1984: 231, no. 128) had considered Proto-Afroasiatic *g^var-, *g^var ‘to gird, enclose’ to be shared with Indo-European, whereas Linus Brunner (1969: 46, note 206) etymologised *Garten* and *hortus* from Ugaritic *hṛnq* ‘orchard’.

6.5.2. *The Context of Agmon’s Hypothesis of Correlation of Semitic Root Triconsonantalisation, and the Onset of Agriculture*

There is a possibility that a Proto-Semitic term for ‘arable land’, **hugār*, which occurs in Ethiopic as *garh*, thus without the first syllable, is etymologically related in a Neolithic spread of agriculture perspective. See at the end of the section for “Field” below. Moreover, consider that Arabic /q/ is pronounced as [g] in several Arabic dialects,¹⁰⁴ and perhaps in prehistory in some Semitic vernacular this is how the Proto-Semitic phoneme was pronounced. Now take Aramaic *qarta* ‘town’, with its Hebrew, Phoenician and Punic cognates. Agmon (2010) claimed, and tried to prove, that during the transition to agriculture in the Near East (and Mesopotamia), eventually there was a transition in early Semitic morphology from biconsonantal hunters to triconsonantal farmers (to say it with the title of Agmon and Bloch (2013)).

Agmon (2010) also claimed that formative (i.e., non-radical) affixes were possibly absorbed into triconsonantal roots. Now, I wonder whether there was a prehistorical Semitic bilateral allo-root **qar-* or **qār-*, associated with the sense ‘field’ (perhaps even in the Mesolithic, thus before farming, or at the time of proto-farming), and that there was

¹⁰⁴ But /q/ occurs as [ʔ] in Syrian Arabic.

a derivative **qar-t* where the *-t* suffix was formative, as often in Semitic. The transition to triconsonantalism would have absorbed the formative suffix *-t* into a triconsonantal root of a noun **qart* (whose stem of inflection was **qart-* as for example in Aramaic). It is not unthinkable that within early Semitic, there was a vernacular in which the allo-form **gart-* (or, by fuller voicing, **gard-*) appeared. If such was the situation of Neolithic migrants to the Balkans, whose introduction of farming eventually resulted in diffusion to Pannonia (Hungary) and what is now Germany, then quite possibly Vennemann did not get it wrong after all in this case, even though I do not share the general premises of his hypothesis. Besides, within early Semitic, was there a family of words, indeed a family of roots, which could accommodate the bilateral allo-root **qar-* or **qār-*, as well as **qar-t* whence **qart*, but also the lexical type of Hebrew *gādēr* ‘fence’, and Akkadian, *kudurru* ‘boundary-stone’? If so, did it even include the lexical type of the Arabic noun *qādar* ‘measure’ and of the Arabic verb *qāddara* ‘to measure’? If such was the case, the development could have been from the bilateral Mesolithic Proto-Semitic allo-root **qar-* or **qār-*, with incorporation of a third radical /r/ during the transition to triconsonantalism. Semantically, the shift would have been through the lexical concept ‘measure of land’, ‘land area’, in some relation to ‘land boundary’ and ‘contour of owned land’.

In fn. 37 to Table S3 in the “Supplementary material” of Agmon and Bloch (2013), Bloch wrote the following, and in the beginning referred to a reconstructed form in Ge‘ez, i.e., Classical Ethiopic:

Ge‘ez **garh*, *garāht* indicate the glottal voiceless *h* as a radical. Postulating an etymological connection between the Ge‘ez forms, Akkadian *ugāru* and Ugaritic *ugr* (as proposed by Militarev 2002: 144) requires one to assume metathesis of the root consonants and interchange between *ʔ* and *h* (glottal voiced and voiceless consonants, respectively). Both assumptions are acceptable. Since Ugaritic and Akkadian belong to two different main branches of the Semitic language family (West and East Semitic, respectively), it appears that the order of the radicals and the vowel pattern attested in these two languages is closer to the form that assumedly existed in PS, and the Ge‘ez forms are secondary. In any event, it is unlikely that Ge‘ez *garh*, *garāht* can be etymologically connected with Arabic *qarāḥ*, *qirwāḥ* “land without trees” (as suggested in *DRS*: 184): *h* and *ḥ* are not homorganic consonants, and there is no regular sound shift in either Ge‘ez or Arabic, leading from one of them to the other (reservation to this effect is expressed already in *CDG*: 202b). Rather, Arabic *qarāḥ* appears to derive from the PWS [i.e., Proto-West Semitic] root *qrḥ* “to be bald” (see *HALOT*: 1140a; *CDG*: 441a).

As for the abbreviations in this quotation, *CDG* = Wolf Leslau's (1987) *Comparative Dictionary of Ge^sez (Classical Ethiopic)*; *DRS* = David Cohen's (1970–2012) *Dictionnaire des racines sémitiques*; and *HALOT* = Koehler and Baumgartner's (1994–2000) *The Hebrew and Aramaic Lexicon of the Old Testament*.

6.5.3. *Proto-Northwest-Semitic *kb, Hebrew /šakab/ [ʃa'xav]*
 'to lie down', and *Latin cubo, cives, civitas*

The onset of contacts between Semitic and Italic, in my opinion (and I understand this is also Alinei's opinion) was in the context of the diffusion of agriculture and animal husbandry. Agmon's hypothesis about the onset of Semitic triconsonantalisation of lexical root, in relation to the onset of agriculture, is tantalising also because it is quite relevant for the following hypothesis.

I have long suspected that there may be early cognacy between the Hebrew verb /šakab/ [ʃa'xav] 'to lie down' (where the triconsonantal root *škb* may be the result of a biconsonantal root **k-b* with the conjugation-forming prefix *š*),¹⁰⁵ and Latin *cubo* for that same sense,

¹⁰⁵ Such a consonant has been since the Hebrew Bible a radical letter, i.e., a phoneme that is part of the lexical root. As I explained, quite possibly it has been a preformative (i.e., a non-radical consonant) in the verb, at a time when the root was biconsonantal ("bilateral") instead of triconsonantal ("trilateral"). The device by which a consonant which is non-radical in some extant term being used as a stem, becomes a radical in the new root of a coinage has been occurring historically in Semitic languages, and has become rather conspicuous in Modern Hebrew and Modern Arabic. Typically, a quadriconsonantal root derived from a consonantal root, with the two middle radical consonants in the new root forming a cluster; that cluster is equivalent to the middle radical, geminated, of trilateral roots: the cluster comes *en lieu* of the double consonant. Therefore, quadrilateral roots with a middle cluster can only occur in such derivatives whose word-form is such, that its pattern when applied to a trilateral root requires the latter's middle radical to be geminated (a double consonant).

Let us consider now an example in which language contact is involved, and a consonant that used to be part of a suffix in the source language (a non-Semitic one) has become a consonant in the Semitic-language lexical root of a noun. Italian *secchio* and *secchia* 'bucket' is derived from Latin *setūlus* or *setūla* 'bucket' (not to be mistaken for *saetūla* or *sētūla* 'bristle'), but the Classical Latin standard form is *sitūla* 'bucket'.

and perhaps also for Latin *cives* ‘citizen’, *civitas* ‘city’. The semantic shift from ‘to lie down’ to ‘human settlement’ is plausible, as people come together for daily activities, but also to seek social proximity for their dwellings. [Alinei (2000a: 432) was concerned with the semantic shift, in Germanic, from ‘place of lying down’ (e.g., for sleeping) to ‘encampment’, such as in Swedish *läger* (cf. German *Lager*).]

Such a semantic shift from the sense of Latin *cubo* to the senses of *civis* and *civitas* would have preceded the onset of agriculture, and it makes sense that before Semitic triconsonantalisation, the root was **k-b*, which dovetails with Latin *cubo*. This may suggest that speakers of a proto-Northwest Semitic vernacular, or their descendants still retaining some of their ancestors’ lexicon, at the time of contact with Italic had still retained the root **k-b*, not yet triconsonantalised into *š-k-b*.

6.6. *Apple*

The usual etymological claim for German *Apfel*, English *apple*, Russian *yabloko*, Lithuanian *óbuolas* ‘apple’ is the Proto-Indo-European root **AB-OL-* or **AB-EL-* for ‘sweetness’. In contrast, having mentioned his becoming aware of the entries in Orel and Stolbova (1995), Vennemann remarks (505):

Intermediate Vulgar Latin forms (which eventually resulted in the development of both the Italian and Arabic terms for the same lexical concept) dropped *ŷ*, thus yielding the cluster *-tl-*. Upstream of the development of the Italian terms, the consonantal cluster underwent change: *-tl-* > *-cl-*. A Vulgar Latin form with *-tl-* eventually yielded Arabic terms (but with all three radical consonants becoming velarised): for example, Baghdadi Judaeo-Arabic has the feminine noun *šətləyyi* ‘bucket’, as opposed to the Iraqi Arabic and Modern Arabic masculine noun */šətl/ šəṭəl* ‘bucket’. This is an example of how even in historical times, an affix (the Latin diminutive suffix *-ul-*) came to be incorporated in a Semitic triconsonantal root (Arabic *štl*).

Incidentally, and with no etymological relation, the Biblical and Modern Hebrew masculine noun */dli/* ‘bucket’ is a co-derivative of the Biblical and Modern Hebrew transitive verb */dala/ dālā* ‘to draw water’. There is an Arabic cognate, *dalla*, which in the 20th century and at present (in Baghdadi Judaeo-Arabic) has been denoting a small metal pot with no lid (the same denotatum as Italian *pentolino*).

Since I had predicted, on the evidence of the Semiticness of PGmc. ⁺*harbista-*/⁺*harbusta-* ‘fruit harvest’ (cf. 26.6.3.1.5 below), it was natural for me to bring the entry no. 8 [of Orel and Stolbova (1995)], **abol-* ‘genitals’ with “Sem. **’abal-* ‘genitals’” in the new dictionary to bear on this traditional etymological problem, especially in view of the fact that the phonological correspondence too could not be better.

Vennemann originally published his important Ch. 26, “Grimm’s Law and loan-words”, in 2006. I am not fully convinced by Vennemann’s (504–505) etymologising the “Germanic *apple* word”, also found, though with /b/ instead of the unvoiced /p/, in Old and Middle Irish, in Lithuanian, in Old Church Slavonic, and in Russian,¹⁰⁶ from Semitic. Vennemann (504) also detects the name for ‘apple’ in a place-name: “probably – the name of the town of Abella in Campania (now Avella in the district of Avellino), bynamed *mālifera* ‘apple-bearing’ by Vergil (*Aeneis* VII, 740)” (504). This is remarkable, because from *Avella* in turn, we have names for ‘hazelnut’ (*Corylus avellana*).¹⁰⁷ The standard Italian name is *nocciòle*, but a synonym is *avellane*.

¹⁰⁶ “The Germanic *apple* word (PGmc. ⁺*apla-*/⁺*aplu-*) is identical with, or closely related to, OIr. [= Old Irish] *ubull* (Mlr. [= Middle Irish] *aball* ‘apple-tree’), Lith. [= Lithuanian] *obuolys*, *obuolas*, *obalas* (*obelis* ‘apple-tree’), OCS [= Old Church Slavonic] *jablŭko*, Russ. *jabloko*” (504).

¹⁰⁷ In early rabbinic Hebrew, the names for ‘hazelnuts’ are forms of two lexical types: *īlasrīn* and *pundēqīn* (Feliks 1994: 248–250). The latter lexical type is, like Arabic *bunduq*, from ancient Greek ποντική = κάρυον ποντικόν ‘nut of Pontus’; cf. Modern Greek φουντούκι (*fundŭki*). The Arab world knew Venice by an Arabic name, *al-Bunduqīyya*, its Greek etymon being ποντική ‘maritime’, which was amply justified by both her urban reality in the lagoon, and her maritime power. Yet, Arabic *bunduqīyya* also means ‘rifle’. The shape of modern bullets is more aerodynamic than the round shape of bullets from the musketry of old, or then a blunderbuss. The round shape of ancient bullets could be metaphorised as *bunduq* or *bundaq*, ‘hazelnuts’.

Also consider, in Spanish, the noun *albóndiga* for ‘meatball’ and for ‘ball’, and this from Arabic *al-bunduqa* for ‘the ball’. Note moreover that the Arabic name for ‘hazelnuts’ is etymologically unrelated to some outwardly similar Indo-Iranic names for species of the narcotic plant *Ephedra*. Those names, listed in Table 3 of names for that plant genus on p. 70 in Flattery and Schwartz (1989), include *bandak-māu* as a Pashto name for the species *Ephedra ciliata* (citing Schapka and Volk 1979), and the name *bandukai* from west of the Indus, given to that same species (citing Stewart 1869), as well as the name *bandak-e-kōhī* (literally, ‘mountain *bandak*’) given in Afghanistan (according to Schapka and Volk 1979) to “*Ephedra* (species not specified)” (according to the rubric in Table 3 from p. 70 in Flattery and Schwartz (1989

I do accept (without accepting the etymology itself) that this etymology of the Germanic *apple* word from Semitic is an illustration of “the famous ‘labial gap’. Loan-words ‘filling’ the labial gap, so to speak, are most easily spotted: every word containing a free Germanic ⁺*p*, i.e. one not flanked by another voiceless obstruent, is *eo ipso* subject to *Lehnwortverdacht*. Several such words clearly betray their Semitic origin” (504). But why should it be Semitic of all things?¹⁰⁸ After all, for ‘apple’ Semitic has Hebrew *tappúah* /tappuh/, Aramaic *tappúha*, and Arabic *tuffāh* (the latter is a collective name used for both the singular and the plural). The etymon Vennemann proposes for the Germanic *apple* lexical type is “Semit. ⁺*abal-* ‘genitals’” (505), based on entry 8 in Orel and Stolbova (1995). This is not an obvious etymon for the *apple* word, and I concede that this impression of mine is because it is not found, either because it was never part of Northwest Semitic, or because the latter has lost this lexical item (because of tabooisation?). Already in his book of 2003, Vennemann had adopted that etymology (2003a, pp. 466, 564, 619–624).

In the book under review here, Vennemann claims (505):

Semantically this etymology could not be better: words with the meaning ‘apple’ or some similar meanings (‘pear’, ‘fig’, ‘cucumber’, ‘avocado’, ‘tail’, ‘egg’, ‘stone’ – i.e. a suitably shaped natural object that lends itself to metaphorical application) tend to develop an obscene secondary sense which may in time oust the original harmless meaning. This is a one-way street; i.e., assuming the ‘genitals’ meaning to be original is excluded by a universal of language change.

I am unconvinced by this. The Hebrew noun for ‘cluster (typically of grapes)’ is *eskol*, whose probable semantic motivation was, by

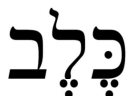
¹⁰⁸ Early farmers in the Middle East, and I mean once this was spread in that region, would have been people speaking Semitic or non-Semitic vernaculars. Farmers who hypothetically moved out from the Middle East, presumably from Anatolia or from the Syrian littoral to the Balkans, would have carried their respective vernacular, but generations afterwards, farmers in the places they reached (whatever their ancestry) may have shifted to another vernacular, whereas retaining a superstratal or adstratal component in their lexicon, as an effect of the cultural influence of the incoming farmers who had brought agriculture and animal husbandry to their new region. After all, the people who introduced farming into southern Mesopotamia in the Early Ubaid period (5300–4700 B.C.E.) appear to have been speaking one or more non-Semitic, non-Sumerian languages that was a substratum or an adstratum to both Sumerian and Akkadian. Cf. Rubio (1999), Whittaker (2008). As for the ancient languages of Anatolia, see e.g. a volume edited by Woodard (2008).

metaphor, from the segolate noun¹⁰⁹ *éšēk* (*éšēk-*) [‘ʔefex] ‘testicle’, plural *ášākīm* [‘ʔafaxi:m] ‘testicles’.¹¹⁰ It was probably not felt to be an



¹⁰⁹ Even the English linguistic term *segolate* (in use by Semitologists) is a cognate of Hebrew *éškōl*. Joseph Malone (1971) provided a formal model of segolation in Hebrew and Aramaic. Cf. Speiser (1926a). Hebrew *kéleḥ* and Aramaic *kalbā* ‘dog’ are segolate nouns, even though by the etymology of the adjective *segolate* it is only the Hebrew noun that deserves to be described by it. Segolate nouns in Hebrew take their name from the vowel diacritical mark,



under the letter for the consonant – the diacritical mark is the *sēgōl*, for the short /e/ – which typifies their word-form in the headform of nouns belonging to that category, such as Hebrew *kéleḥ* [‘kelev] being written as



In the main subcategory within segolate nouns, a segolates’ subcategory the word-form of whose headform is $R_1\acute{\epsilon}R_2\epsilon R_2$ where R_1, R_2, R_3 , are the three radicals, the second vowel of the headform replaces a historical phonetic zero, thus breaking a consonantal cluster that persisted instead in the corresponding word-forms of Arabic and Aramaic, and is still found in Hebrew was well in inflected (non-headform) segolate nouns. Even in spoken Arabic, where the consonantal cluster persists in nouns such as *kalb* ‘dog’, *qalb* ‘heart’, the consonantal cluster is broken in such dialectal forms as *gáḷab* ‘heart’ in the sense ‘darling’ (but cf. the same sense with a possessive suffix in *gáḷbī*, literally ‘my heart’). Another example is Arabic *baḥr* ‘sea’, colloquially *báḥar*.

Interestingly, the diacritic mark  got its name *sēgōl* because it resembles a cluster of grapes, and in Aramaic, the noun *sēgōlā* (a cognate of Hebrew *éškōl*) denotes ‘cluster’. The name *sēgōl* also denotes one of the liturgical cantillation marks (but this time, with the basis of the triangle formed by the three dots being at its bottom, this way: ) above some letter in the Hebrew or Aramaic text of scripture in printed Hebrew Bibles, and indicating prosody.

We therefore have a sequence of semantic shifts: from the sexual anatomy domain, to the botanical domain, and from botany to the technical lexicon of grammarians and in particular the Masoretes (who in the final few centuries of the first millennium of the Common Era, established the Tiberian system of vowel and cantillation diacritical marks for the Hebrew Bible).

¹¹⁰ Those anatomical terms that are documented in several Semitic languages are discussed in a special lexicon, the *Semitic Etymological Dictionary, Vol. 1: Anatomy of Man and Animals* by Militarev and Kogan (2000). Incidentally, the Hittite plural noun denoting ‘testicles’ is *arkiyēs* (in the nominative; the accusative is *arkius*). The

obscene semantic motivation; rather, shape as well as the associated concept of fruitfulness would have even made this a desirable semantic motivation.

Actually, Vennemann admits that he was only able to make his conjecture once he was able to peruse Orel and Stolbova's (1995) now classic Hamito-Semitic dictionary (actually, a dictionary whose shortcomings were pointed out by reviewers after it appeared, but which is important because it is around):¹¹¹ "The reconstructable semantic development from 'apple' to 'genitals' explains the elimination of the ⁺*abal-* word from the three 'school languages', Akkadian,¹¹² Hebrew, and Arabic, with the result that the etymology

Hittite term "matches Gk. ὄρχις, Arm. *orjik*', Alb. *herdhë*, Mr. *uirgge* (**orgh-*, and is further cognate with Avest. *arəzi-* 'testicle', [...]" (Weeks 1985: 38, §4.49).

¹¹¹ Cf. what Agmon (2010: 24–25) wrote about the problems of reconstructing Proto-Afro-Asiatic (i.e., Proto-Hamito-Semitic): "Proto-Afroasiatic (PAA), an older proto-language from which the Afroasiatic (AA) language families (Semitic, Egyptian, Berber, Cushitic, Omotic, and Chadic) have evolved, might also be relevant to our discussion (Blench 2006). However, unlike Semitic and Egyptian, the other African languages have only recently been documented. Consequently, there is as yet no consensus over the PAA lexicon and its temporal or geographic origins (indeed, there are many profound disagreements between recent studies that attempt to reconstruct a sizeable PAA lexicon: *HSED*, updated as *DAE*, on the one hand, and Ehret 1995, on the other). Therefore, we will deal with PAA in the present study only anecdotally". *DAE* stands for the Database of Afroasiatic Etymology, and is accessible at <http://starling.rinet.ru/cgi-bin/main.cgi?flags=eygtmnl> As for *HSED*, it stands for Orel and Stolbova (1995).

¹¹² While discussing possible Hurro-Urartian etymologies of some Old Armenian vocabulary, Diakonoff (1985: 600) included this entry: "17. *xnjor* 'apple(-tree)' < Hurr. (probably also Urartian, but not attested as such) *ħinzuri* /ħinj-orə/. The Aramaic *ħazzūrā* is certainly < Akkad. *ħinzūru* (*ħi-* > *ħa-* is typical). The Sumerian *ħašħur* 'apple-tree' is also from Eastern Caucasian but not Hurrian (possibly from Qutian which may have belonged to Western Lezghian languages)". Also on p. 600, Diakonoff claimed: "The IE kernel of Old Armenian contains all the necessary words denoting man, parts of the body, natural actions and states and also the most important terms for the domestic animals, except the camel. Of course, in the 2nd millennium B.C. the Proto-Armenians could not have been nomadic cattle-breeders. They had, no doubt, also a subsidiary agriculture, which is attested, e.g., by the IE words for 'barley' (*gari*) [whereas Hurrian has *kade* 'barley'] and the 'plough' (*arawr*) in Old Armenian. But they had to borrow from the Hurro-Urartians the most necessary terms of a settled agricultural early class civilization (such as 'slave', 'slave-girl', 'burned brick', 'tin', 'seal'), as well as words for local animals and plants ('camel', 'apple', 'plum' or 'medlar', 'quince' /?/). The only possible conclusion is, that the immigration of the Proto-Armenian speaking tribes postdated the settlement

did not become available until a general Hamito-Semitic dictionary was published (Orel/Stolbova 1995)” (505).¹¹³ At any rate, in this instance I am not persuaded.

In this particular case, my impression is that Vennemann has been assuming more closeness among the linguistic families of the Hamito-Semitic macrofamily that there actually is. It may be that Vennemann felt able to consider Hamito-Semitic data from both Asia and Africa because of the role that Phoenician (or, to him, also earlier Semitic) colonisation along the littoral (of both North Africa and Europe) plays in his theories about the Semitic lexical impact in Europe. To better explain my unease with how “the ⁺*’abal-* word” has been treated by Vennemann, I would like to briefly refer to relations within Hamito-Semitic.

In his short paper “Some Reflections on the Afrasian Linguistic Macrofamily”, Igor Diakonoff (1996)¹¹⁴ began by noting that he had

of the Hurro-Urartians in the Highland”. Besides: “In reconstructing these stages [of the development of Proto-Armenian], it is necessary to take into consideration that at the later stages there must have existed an Armenian-Urartian bilingualism, which must have influenced the process of the changes” (*ibid.*: 601). Diakonoff tried to date the period of bilingualism relative to those many stages.

Greppin (1991) partly disagreed with Diakonoff (1985), but Greppin’s article (1991) was published with endnotes by Diakonoff himself. On p. 724, entry 4, Greppin wrote with the Armenian term Arm. *xnjor* ‘apple’, which first appeared in a Bible from the fifth century. Greppin indicated the etymon as being Hurrian *ħinz-ora* ‘apple’, and moreover reconstructed a Proto-East-Caucasian form, then listing modern forms in related languages: “PEC *’Iāmco-*, Agul *ħač*, Archi *aInš*, Lezghian *ič*, Dargwa *’inc*, Khinalugh *myč*, Lak *hiwč*, Andi *inči*, Avar *’eč*, Ingush *x’ ežoga* ‘apple’”. In fn. 37 to the end of the text quoted here, Greppin remarked: “Note also Chechen and Ingush *ħamč* ‘plum’” (whereas Armenian denotes ‘plum’ by *salor* or *šlor*). In fn. 36, Greppin claimed: “It is difficult to imagine that this Ingush term would be a loan from Armenian, rather than a direct continuant of Proto-East-Caucasian. It is most likely a compound of *x’ez-* and *-orga*”.

¹¹³ A Semitologist, all the more so one also knows Berber and/or Egyptian, is better equipped to approach Orel and Stolbova’s dictionary with a grain of salt. It can be usefully used, but one must not be over-reliant, and for a Germanist it is very difficult to exercise that kind of caution. See corrections in Diakonoff and Kogan (1996), Kogan (2002), Kaye (1997a). Further criticism of Orel and Stolbova (1995) can be found in Diakonoff (1998) and Takács (1997). To better appreciate the perspective of the latter author, consider that Takács (1998) argued for an Afro-Asiatic substratum affecting a sublexicon of Proto-Indo-European; the article is entitled “Afro-Asiatic (Semitic-Hamitic) Substratum in the Proto-Indo-European Cultural Lexicon” indeed.

already remarked elsewhere about the shortcomings of Orel and Stolbova (1995), and explicitly though tentatively apportioning blame to Orel as based on his close acquaintance with his own former colleague Stolbova, she would not have been prone to make the kind of reconstructions he criticises. Diakonoff claimed that in his opinion, the abundant material about Chadic, due to Stolbova's expertise in that domain, is reliable as far as he can judge. Next, Diakonoff (1996: 293) recapitulated his own taxonomy the Afrasian macrofamily (i.e., Hamito-Semitic): he divides it into East-West Afrasian (EWA), comprising "Semitic, Cushitic (which can be regarded as one or several linguistic families, and Berbero-Libyan; and North-South African (NSA), which includes Egyptian and Chadic. I based this on grammatical grounds". Even though Diakonoff had qualms about "cases of spurious placement of individual glosses under wrongly reconstructed roots" in Orel and Stolbova (1995), he stated that the very availability of that dictionary makes it possible to check lexicologically "the difference between EWA and NSA verbal systems which I proposed". He found more coherence in the lexicon of NSA¹¹⁵ than in that of EWA. Concerning the latter, he remarked (Diakonoff 1996: 293) that

In spite of the grammatical isoglosses, the Semitic, Berber, and Cushitic languages (the Cushitic group probably comprising several families) do not, according to lexicological data (both ours and the *HSED*'s [i.e., Orel and Stolbova's (1995)]), constitute one subdivision: there are few, if any, entries which include Semitic, Berber, and/or Cushitic glosses but which lack Chadic and/or Egyptian correspondences.

¹¹⁴ Diakonoff published in Moscow *Semito-Hamitic Languages* in 1965, and *Afrasian Languages* in 1988. As Appleyard pointed out (1999: 304–305), Diakonoff (1965) "suggested an Afroasiatic 'homeland' or 'urheimat' in the southeastern Sahara", but Diakonoff (1988) seems to have accepted "the hypothesis proposed by his student Militarev [*recte*: Militarev] that the home of Afroasiatic was in Western Asia, and that whilst Semitic 'stayed put', speakers of the other families migrated into Africa. The latter thesis of course fits nicely with the Nostratic hypothesis [...] Needless to say, I find this all highly speculative, and I would question the very premise upon which this kind of linguistic palaeontology is constructed". "The rooting of Semito-hamitic in Africa [which Appleyard favours] has been posited already by Leo Reinisch in 1873 [...]" (Appleyard 1999: 316).

¹¹⁵ "Approximately 15 percent of the roots listed in the *HSED* [i.e., in Orel and Stolbova (1995)] are attested only in Egyptian and Chadic and thus are NSA, not Common Afrasian" (Diakonoff 1996: 293).

That the connections between the EWA languages are looser than those between the NSA languages can also be deduced from the fact that the great majority of the typical Semitic – but also Egyptian and others – triconsonantal (especially verbal) roots cannot be traced to a Common EWA proto-language.

Diakonoff (1996) proceeded to suggest an explanation “for the closer relation between Egyptian and Chadic” (*ibid.*: 293). The Proto-Afrasian vocabulary certainly existed, he claims, during the Mesolithic. But between 10,000 and 6,000 B.C.E., the lower (i.e., northern) part of the Nile Valley “was covered with lakes and swamps” (*ibid.*: 294). Therefore, “it must be assumed that the Proto-Egyptians lived higher up the Nile than in historical times” (*ibid.*). Diakonoff then claimed that the “point of contact between the Proto-Egyptians and Proto-Chadians must hence be sought south of Khartoum, where the Nile flows (and, probably, also did flow during the Mesolithic) through a Savannah zone stretching latitudinally across all of Africa” (*ibid.*), “a type of zone most favorable for population movements” (*ibid.*). Diakonoff suggested that the Proto-Chadians “could actually have moved through the savannah zone toward Lake Chad, while the speakers of Proto-Egyptian moved northwards down the Nile” (*ibid.*), and whereas “[t]here must have been an impetus” for that migration, one can only quite tentatively guess what it may have been: “It may have been the appearance of Nubian-Meroitic tribes in the valley, or it may have been something else” (*ibid.*). I think this provides some clarification for important reflections offered by Giovanni Garbini in his “L’egiziano e le lingue semitiche” (1978), concerning Egyptian being not as close to Semitic as contacts in Lower Egypt would have warranted; Garbini ascribed this to the political predominance of Upper Egypt.

As for the Maghreb, West Africa, and the Iberian Peninsula (in particular Basque: Vennemann claims a very wide area in Europe for the Vasconian family, in his view predecessors of the Indo-European arrival), there has been a hypothesis of Hans Mukarovsky relating (by reasoning about substrata and superstrata) Basque to Fula [also known as Fulani, Ful] (thus, south of the Berber zone); it was considered (with reason, I think) a poor and unlikely hypothesis (though not his considering Fula and Bantu having genetic affinities).¹¹⁶ See

¹¹⁶ Greenberg placed both Bantu and Fulani in the Niger-Congo family. He also pointed out (Greenberg 1950a: 63, fn. 19) that Edward Sapir had stated, in a book

Mukarovsky's riposte (1966), but it was meagre, considering his scant and unconvincing lexical data. At any rate, he concluded it by stating: "Perhaps I have demonstrated more clearly elsewhere that Basque is related to Hamito-Semitic (or Afro-Asiatic) as a whole; this will be valid also for 'Mauretanian'" (*ibid.*: 1252). I am not claiming that this reflects on Vennemann's own, different, independent hypotheses. Rather, in those years the boundaries of Hamito-Semitic were somewhat fuzzy in Africa, and some scholars mused about what to include or exclude.¹¹⁷ The point however is that Hamito-Semitic as a macrofamily is disparate.

6.7. Field

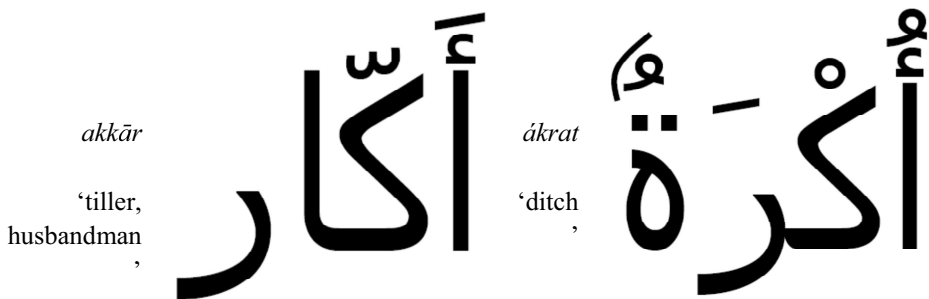
6.7.1. Hebrew *ikkār*, Arabic *akkār* 'tiller', and Ibn Barūn's *Off-the-Mark Etymology*

A Spanish Jewish author from the end of the 11th century, Abū Ibrāhīm Ishāq Ibn Barūn, is known because of his comparative linguistic treatise in Hebrew lexicography in relation to Arabic, the *Book of Comparison*, much of which is preserved, whereas further entries from that work are known because other medieval writers cited

review in 1913, that he "should not be surprised if further research demonstrated beyond cavil that Bantu and Fulani are genetically related".

¹¹⁷ Sometimes this was done serenely, when Marcel Cohen exercised critique introspectively on his seminal work. Some other times, there was acrimony, albeit restrained. Consider this passage by Joseph Greenberg, an important taxonomist of African languages (cf. his book Greenberg 1963): "As was mentioned earlier, I do not exclude the possibility of an ultimate relation between the Chari-Nile and Hamito-Semitic families as wholes. If this were so, then the 'Nilo-Hamitic' languages might be expected to show some significant resemblances to Hamito-Semitic languages but no more so than Nilotic or any other language group within the Chari-Nile family. But under any circumstances, if my view is correct, the 'Nilo-Hamitic' languages will show more resemblance to the Nilotic than to the Hamito-Semitic, while if Hohenberger is correct the reverse will be true. To my knowledge, Hohenberger is the first to maintain that there is greater vocabulary similarity between 'Nilo-Hamitic' and Hamito-Semitic than between 'Nilo-Hamitic' and Nilotic. In a recent discussion of the so-called 'Nilo-Hamitic' languages, G. W. B. Huntingford, who takes a position very similar to that of Hohenberger and attacks my point of view with great vigour, rests his case practically exclusively on grammatical rather than vocabulary data. [...]" (Greenberg 1957: 364). The reference is to early research by Johannes Honeberger, by whom, see e.g. the much later *Semitische und hamitische Wortstämme im Nilo-Hamitischen* (Hohenberger 1988).

them. Pinchas Wechter (1964: 68) included (in his English translation) Ibn Barūn's entry for the Hebrew noun from *Jeremiah* 51:23, *ikkār* (which also in Modern Hebrew denotes 'peasant') as being equivalent to Arabic *akkār* 'tiller, husbandman', "which is derived from *ákrat* 'ditch'" (this was Ibn Barūn's opinion, not the actual etymology, which we are going to discuss in the remaining subsections of the subsection "Field". Arguably Arabic *ákrat* 'ditch' is a cognate of the Hebrew verb *kārā* 'to dig').



Biblical Hebrew *ikkār* 'peasant' occurs in the singular in *Jeremiah* 51:23 and *Amos* 5:16, and in the plural in *Jeremiah* 14:4, 31:24, *Joel* 1:11, and *2 Chronicles* 26:10, as well as in the plural with a possessive suffix in *Isaiah* 61:5.

6.7.2. *German Acker, Indo-European Cognates, and Sumerian and Semitic Terms*

Vennemann did not discuss in his book under review the lexical type of German *Acker* '[farmer's] field', but he had done so in his book of 2003. Perhaps he did not in his book of 2012, because the term has cognates in Indo-European languages from the northern Mediterranean: Greek and Latin. And yet, once one adopts the hypothesis of a Neolithic spread of farming from the Near East, with a likely contribution of the Northwest Semitic vocabulary of farming, it makes sense to look at a possible relation closely.). German *Acker* denotes 'farmer's field': cf. Indo-European **aġ-ro-* (Sanskrit *árja-*, Greek *agrós*, Latin *ager*), and cf. Sumerian *a-gàr* 'farmer's field' (cf. Rubio 1999: 10). In this section, we are going to consider two alternative etymological hypotheses.

Also consider Armenian *agarak* ‘field’, first occurring in a Bible from the fifth century C.E. (Greppin 1991: 724). “This light *r*, rather than dark *ṛ*, is the standard Armenian reflex of Hurro-Urartian *r*” (*ibid.*, fn. 27). Greppin proposed the following etymology (where PEC stands for Proto-Eastern Caucasian): “PEC **ʷVrV-*, Rutul ‘*Iur* ‘lawn’, Lak *ar(e)* ‘flat land’, Avar ‘*uru* ‘virgin soil, turf’, Chechen, Ingush *ārie* ‘field, flat-lands’” (*ibid.*: 724). Greppin (*ibid.*, fn. 29) pointed out: “A long *a* can indeed be derived from loss of intervocalic *-w-*, but one would expect rounding from the **-w-*”.

Having mentioned Sumerian *a-gār* ‘field’, I would tentatively also signal as relevant for the Indo-European lexical type considered, the Aramaic noun *ḥaqlā* (was there an interchange of the liquid *r/l*?).¹¹⁸ [Cf. Akkadian *eqlu* ‘field’, Arabic *ḥaqlun* “field, land lacking trees, cereals at an early stage of growth”, Ethiopic *ḥaql* “field, plain, desert, countryside”(item 3.11 in Bloch’s Text_S3 file, in the supplements to Agmon and Bloch 2013).] Let us take it further. Such developments from [ḥāʔqal] and the like, perhaps something like like [‘ḥaql] or [‘ḥaql] or [‘ḥaql] or [‘ḥaql], are far from an insurmountable problem, especially as there may have been a transition through various speech communities, with a sequence of respective constraint sets. Sonorisation of Semitic /q/ ultimately leading to [g] may have happened within Semitic: think of the pronunciation [g] of Arabic /q/ for example in Egyptian Arabic, and besides, there are claims that at an early period, /q/ was not per force voiceless, and may have been [G], the voiced equivalent of the voiceless [q]. As for the initial radical of the given Semitic root ḥ.q.l., the omission of [ḥ] in general in loanwords by speech communities that do not possess that phonetic value is known for example from the medieval history of Hebrew as pronounced by some in Europe. Besides, there must have been at some point an exchange of liquid consonants, [r] for [l], which is the third radical of the Semitic triconsonantal root ḥ.q.l. Clearly Latin *agellus* ‘small field’ is a diminutive of Latin *ager* ‘field’, and if the etymology I suggest from Semitic [‘ḥaql] or [‘ḥaql] is correct, it is the *r* of *ager*, not the *l* of *agellus* that is a continuator of the Semitic third radical.

When Ephraim Avigdor Speiser (1926) set to apply the principle of relative sonority¹¹⁹ to Semitic phonology, he brought an example of

¹¹⁸ Aramaic *ḥaqlā* ‘field’ > Modern Hebrew *ḥaqlāy* ‘farmer’ and *ḥaqlā’ūt* ‘agriculture’. The latter turns out to be even etymologically akin to the Pan-European term of the lexical type *agriculture*.

¹¹⁹ “In the present connection it will suffice to state, that if highly sonorous sounds as, e.g., liquids and nasals come to stand between consonants with a relatively smaller

that principle from the reconstruction of the development of Latin *agel-agellus*.¹²⁰ Speiser even discussed, as an analogue from Germanic – the *Acker* word – of the appearance of a secondary vowel breaking a consonantal cluster (i.e., the appearance of anaptyxis), as an analogue of Semitic segolation¹²¹ (i.e., the nominal derivation pattern $R_1VR_2R_3 > R_1\check{V}R_2\varepsilon R_3$ of which Semitic /ħaql/ is an example).¹²²

There is an alternative. Alinei (1996: 243) listed among word families one could date as originating in the Neolithic, words for ‘field’ and for ‘plough’ or ‘to plough’. He respectively cited Pokorny (1959, at 6) and Buck (1949, Sec. 8.12), and Pokorny (1959 at 62) and Buck (1949, Sec. 8.21). Apart from Greek *agrós*, Latin *ager*, and Germanic terms such as *Acker*, he also noted Old Indic *ájra-h*. Thus, occurrence within Indo-European is not only in Europe. As for verbs for ‘to

amount of sonority, the former become automatically syllable-forming” (Speiser 1926a: 147–148).

¹²⁰ “Latin *agellus* ‘a small field’ goes back to the diminutive form **agro-los*. The intermediate changes may be set down as follows. First the *o* of the stem was lost through syncope as the result of strong stress-accent which rested in primitive Italic on the first syllable. In the form **agrlos* which thus resulted the *r* was between two consonants with a smaller amount of sonority. That sound had, therefore, to assume vocalic function (the process is called by the Sanskrit name *samprasāraṇa*). Next developed an anaptyctic vowel *e* yielding the form **agerlos*. From there it was only a question of time to reach the form *agellos > agellus* through assimilation (*rl > ll*) and vowel-weakening (*o > u*)”. (Speiser 1926a: 148).

¹²¹ “The conclusions concerning the origin and development of the segolates in Semitic may now be summed as follows: When two consonants were left in the Semitic languages at the end of a word, there arose the need to develop a secondary vowel in the final syllable if the last consonant was more sonorous than the preceding one. For practical purposes it may be said that this was the case between a stop sibilant and a following liquid or nasal. [...] That there was no phonetic need for the development of a segolate vowel if that order of consonants was reversed is proved by the fact that in modern Arabic there is no anaptyxis under such conditions [...] Later anaptyxis was extended to forms in which either of the final consonants was a sonorous one [...] And finally, in some languages the vowel was extended to all nouns capable of segolatization, notably in Hebrew [...]” (Speiser 1926a: 162–163).

¹²² “There are cases in Germanic which parallel, one might say, sound for sound some of the examples of segolatization in Semitic. A secondary vowel may develop in the languages of the former group before the final consonant provided that the latter is a liquid or a nasal following a stop or a sibilant. Cf., e.g., Goth[ic] nom[inative] *akrs* acc[usative] *akr*, (Gr[reek] *αγρο-ς, αγρο-ν*) which become *acchar* in OHG [Old High German], *æccer* in Ags. [Anglo-Saxon] ‘field’, Goth. nom. *fugls* acc. *fugl* over against OHG. *fogal*, Ags. *fuʒol* (German Vogel) ‘bird’, Goth. nom. *ibns*, acc. *ibn* as compared with OHG. *eban*, Ags. *efen* ‘even’. The parallels are sufficiently close require no further explanation” (Speiser 1926a: 162).

plough’, he listed e.g. Latin *arāre* (whence Italian *arare*), Greek *aróō*, Old Irish *airim*, Middle English *ere*, Russian *orat*’, Armenian *aracor*, and Tocharian A *āre*. If you also consider Sumerian a-gàr ‘field’, and the Semitic terms I listed, then there are two alternatives: (a) the lexical type belongs to both Indo-European and Afrasiatic; or then (b) the lexical type spread from the Middle East. Is the latter, if the lexical type originated in Sumerian or an earlier south Mesopotamian culture, then when it entered Semitic lambdacisms occurred ($r > l$), but if the lexical type originated instead in the Semitic root *ḥ.q.l.*, and the third radical *l*/ was original, then rhotacism intervened in its spread to Indo-European languages.

6.7.3. *The Setting for an Interpretation in Line with Agmon’s Framework*

Importantly, etymology from Semitic required that at the time of the Neolithic spread of farming, triconsonantal roots¹²³ had already prevailed within Semitic. In fact, I believe that Semitic already had triconsonantalism, as well as internal differentiation, at the time of the Neolithic spread of farming. But unlike with Colin Renfrew’s theory that Indo-Europeans came to occupy their territories because *they* were the carriers of early farming, so that this results in a shortish chronology for Proto-Indio-European and Indo-European, there is a novel theory that does not shorten, but (rather like Continuity Theory) much lengthens the chronology of Semitic, and ascribes the rise of triconsonantalism to the coming of the Neolithic and farming, with the proviso that the development of farming practices in turn has a long chronology in the Near East. This theory is associated with Noam Agmon’s article “Materials and Language” (2010), comprising an etymological appendix by Yigal Bloch (by whom, cf. Bloch 2008). Agmon (2010) study was published with an introductory note of twenty pages by Jean Lowenstamm (2010), who revisited the controversy on there having been bilateral roots in the backdrop to the rise of Semitic

¹²³ Ehret (1989) proposed a conjecture about the origins of the third radical in Semitic lexical roots.

trilateral roots. Agmon's paper, which is entitled "Materials¹²⁴ and Language: Pre-Semitic Structural Change Concomitant with Transition to Agriculture",¹²⁵ assumes that bilateral roots were real. Lowenstamm "concluded that the evidence weighs in favor of recognizing synchronically active biliteral roots subjected to templatic pressure. It is further suggested that a by-product of Agmon's study and findings is a time frame for the emergence of templatic morphology in the Middle East" (*ibid.*: 1).

In my opinion, if one is to understand that triconsonantalism only evolved in the early Neolithic, such a time frame is too short; it telescopes the timescales, a problem that has long afflicted Indo-European studies, and for which Alinei's Continuity Theory is a remedy. Interestingly, Agmon has hit upon the idea of auto-dating lexical evidence, as proposed by Alinei, even though he does not use Alinei's name for it.¹²⁶ What is more, Agmon himself actually

¹²⁴ Agmon [ʔag'mon] is a theoretical physical chemist at the Hebrew University in Jerusalem. His article has a long appendix on etymology, by Yigal Bloch, a doctoral student at the same academic institution.

¹²⁵ A more recent article, Agmon and Bloch (2013), appeared in a biology journal and is entitled "Statistics of Language Morphology Change: From Biconsonantal Hunters to Triconsonantal Farmers". The following is excerpted from its abstract: "Traditional comparative historical linguistics becomes inaccurate for time depths greater than, say, 10 kyr. Therefore it is difficult to determine whether decisive events in human prehistory have had an observable impact on human language. Here we supplement the traditional methodology with independent statistical measures showing that following the transition to agriculture, languages of W. Asia underwent a transition from biconsonantal (2c) to triconsonantal (3c) morphology. Two independent proofs for this are provided. Firstly the reconstructed Proto-Semitic fire and hunting lexicons are predominantly 2c, whereas the farming lexicon is almost exclusively 3c in structure. Secondly, while Biblical verbs show the usual Zipf exponent of about 1, their 2c subset exhibits a larger exponent. After the 2c > 3c transition, this could arise from a faster decay in the frequency of use of the less common 2c verbs. Using an established frequency-dependent word replacement rate, we calculate that the observed increase in the Zipf exponent has occurred over the 7,500 years predating Biblical Hebrew namely, starting with the transition to agriculture".

¹²⁶ The introduction to Lowenstamm's article begins as follows: "Proto-Semitic takes us approximately 6000 years back, that is some 4000 years after the completion of the agricultural revolution in Western Asia. Is it possible to go back further beyond those 6000 years without having to tap the much more speculative results of the reconstruction of proto-Afroasiatic? It is Agmon's claim that consideration of astutely selected external evidence makes it possible to discern distinct layers in the proto-

proposed a realistic timeframe, suggesting that such developments in morphology took place sometime during a time interval that is early enough. This ought to be most welcome, for supporters of [European] Continuity Theory, or the Palaeolithic Continuity Paradigm, which is how it is also known. The following is quoted from his abstract (Agmon 2010: 23):

Materials and language have evolved together. Thus the archaeological dating of materials possibly also dates the words which name them. Analysis of Proto-Semitic (PS) material terms reveals that materials discovered during the Neolithic are uniquely triconsonantal (3c) whereas biconsonantal (2c) names were utilized for materials of the Old Stone-Age. This establishes a major transition in pre-Semitic language structure, concomitant with the transition to agriculture. Associations of material names with other words in the PS lexicon reveal the original context of material utilization. In particular, *monosyllabic* 2c names are associated with a pre-Natufian cultural background, more than 16,500 years ago. Various augments introduced during the Natufian,¹²⁷ and perhaps even more intensively during the Early Neolithic,

Semitic lexicon. Specifically, archeological evidence interpreted in the light of what is known of early technology yields the striking generalizations in (1)", where:

- (1) i. the names of materials and technological processes which could not possibly have been available before the agricultural revolution all involve triconsonantal roots
- ii. biconsonantal roots exclusively underlie the names of materials and technological processes which had to be available before the agricultural revolution

After which, he adds: "Of course, Agmon's thesis presupposes that there is such a thing as a biconsonantal root. As such, it naturally links up with, and possibly sheds light on, a great classic of Semitic linguistics, viz. does the currently prevailing triconsonantal root format stem entirely or partially from an earlier biconsonantal structure?" (Loewenstamm 2010: 2). He stated the focus of her interest: "I am interested here in a portion of prehistory that lies much closer to us, namely the period during which the pressure towards triliteralism became irresistible, and reached a point of no return. This is clearly a Semitic development, for no other Afroasiatic subfamily has enforced triliteralism as determinedly. Why did this happen at all? The best conceptual tool available to us for construing this development is templatic pressure. Given a template with a fixed shape, roots will adjust, as in the case of a Chaha deaf verb *gāmāmä* 'chip' [...], while reduplicated biradicals will give up C₂ of their first conjunct as argued in Banksira (2000), yielding *q"äq"äsä* 'he became burnt' [...] from $\sqrt{q^w s + q^w s}$ " (*ibid.*: 20). The Chaha language is a member of the Gurage cluster of the Southern subgroup of Ethiopic.

¹²⁷ "Prehistoric chronology is geography-dependent. In the present work it is based on the prehistory of W[estern] Asia and, particularly, the archeology of the Levant

were absorbed into the roots, tilting the equilibrium from 2c toward 3c roots, and culminating in an agricultural society with strictly triconsonantal language morphology.

In his conclusions, Agmon stated, among the other things (2010: 46):

Indeed, some Semitic affixes are widespread in AA [i.e., Afro-Asiatic] (instrumental *m-*, feminine *-t*, causative *š-*, [...]), and thus they are plausibly innovations of the Natufian/PPNA period. Augments continued to pile up throughout the Neolithic. [...] Such augments were eventually absorbed into the root and created an ensemble of 3c roots supplementing the archaic 2c roots.

The agricultural “revolution”, which began with PPNA wheat domestication, continued with the replacement of hunting by domesticated mammals, the founding of large agricultural villages, the introduction of square brick houses and an everlasting quest for new materials. This was apparently accompanied by a “revolution” in language. Contrary to the implication of the term “revolution”, it did not occur overnight. The transition to agriculture likely followed many millennia of wild cereal harvesting, penning of wild animals, experimentation in propagating fig twigs, and the like. Eventually, the previous equilibrium was disturbed to the extent of inducing a

[...]. This does not necessarily mean that the ‘homeland’ of pre-PS [i.e., pre-Proto-Semitic] speakers was the Levant rather than, for example, NE [North East] Africa [...]. The Fertile Crescent (and notably Israel) is one of the more intensively investigated regions archeologically. By contrast, archeological studies of Africa, particularly for the pre-Neolithic era discussed herein, are very rudimentary [...]. Thus, archeological findings from the Levant may be interpreted as representative of their time rather than their precise location. More fundamentally, the Levant is unique in hosting the Natufian culture, ca. 15,000–11,700 BP [i.e., Before Present] (Bar-Yosef 2002). Up to the Natufian, the ‘classical’ nomadic lifestyle of

the hunter-gatherer prevailed, more or less as it existed during the Upper Paleolithic (UP). The Natufians constituted a more complex society, with innovations in sedentary and hunting techniques; among other things, they are accredited for introducing the sickle and the bow (Peterson 1988). It is possible that this was a trigger for the development of a more complex language (PAA [= Proto-Afrasiatic] Militarev 2002). The transition to agriculture in the Levant began earlier than in other regions of the world, in the Pre-Pottery Neolithic A (PPNA, 11,700–10,500 BP), and it was essentially complete by the Pre-Pottery Neolithic B (PPNB, 10,500–9,000 BP), when large agricultural villages were established. Pottery was introduced during (or slightly before) the Pottery Neolithic (PN, 8,500–7,000 BP), whereas the first metal processing installations are from the Chalcolithic period (7,000–5,500 BP). PS is attributed to this period, and therefore it is the most recent period relevant to our PS reconstructions. Nevertheless, we advance one step further in time, to discuss proto-West-Semitic (PWS) words as well, which already belong to the Early Bronze” (Agmon 2010: 27, 30). Cf. Bar-Yosef (1998) on the Natufian. Militarev (2002) is entitled “The Prehistory of a Dispersal: The Proto-Afrasian (Afroasiatic) Farming Lexicon”.

seemingly spontaneous transition. Analogously, the transition to triconsonantalism did not occur overnight. It likely followed many millennia of experimentation in more complex linguistic forms, such as various augments. As these became absorbed into the root, the equilibrium between 2c and 3c words was disturbed and the stage was set for a new language structure. The correlations revealed in this study suggest that the transition to agriculture, one of the most dramatic divergences in human lifestyle throughout prehistory, was the final catalyst that brought about a period of dramatic language development, creating, within just a few thousand years, a distinctly different language structure. The emerging Neolithic society turned out to be particularly conservative in adhering to the new 3c morphology, which was required for the development of the templatic grammar characterizing the Semitic languages. Formation of new 2c words became a taboo, [...]

6.7.4. *Agmon and Bloch's Semitic Proto-Word for 'Farmer'*

In Agmon and Bloch (2013), Table S3 in the “Supplementary material” (published as a zipped folder in an important e-journal in biology) is an etymological appendix for Table 3, and was authored by Yigal Bloch. I quote his definitions. Entry 3.1 includes the proto-word *²*ikkar* “farmer” (from Proto-Semitic, a triconsonantal word); Hebrew ²*ikkār* “agricultural worker in servitude, without land” (and Bloch suggests in parenthesis that it may be a loanword: “Akkadian lw.?”); Aramaic ²*kr* (headword of a verb as spelled) “to plow, cultivate a field, bear (progeny, fruit)” (this belongs to Syriac), ²*ikkārā* “farmer” (this belongs to Targumic Jewish Aramaic, Jewish Babylonian Aramaic of the *Babylonian Talmud*, and Syriac), and again Bloch suggests it may be an Akkadian loanword; no known occurrence in Ugaritic; Arabic verb ²*akara* “to till ground, dig a cavity for water in the ground”, and Arabic noun ²*akkarūn* “cultivator of land” (Bloch adds in parenthesis: “Aramaic lws.”); no known occurrence in Modern South Arabian; Ethiopic *akkārā* “to renew land by plowing and sowing” (Amharic), *t-akārā* “to build a house and cultivate the field around it for the first time (in the Chaha language); Akkadian *ikkaru* “farmer, plowman”. In a footnote (fn. 24) to the Ethiopian occurrences, Bloch stated: “The verbs in Ethiopian languages are unlikely to be loanwords from either Aramaic or Arabic (Militarev 2002: 146)”. Next, in a note (fn. 25) to the Akkadian occurrence, Bloch wrote:

Akkadian *ikkaru* is commonly considered a loan from Sumerian ENGAR “farmer”, and the corresponding words in Hebrew, Aramaic and Arabic are considered loans from Akkadian, with the Arabic words borrowed through the

mediation of Aramaic (*AHw*:¹²⁸ 369a; *CAD*¹²⁹ I–J: 49a, 54b; *CDA*:¹³⁰ 126a). This reconstruction is possible; however, as pointed out by Militarev, the evidence of the modern Semitic languages of Ethiopia suggests that the verbal root ²*kr* “to cultivate land” belonged to the PS lexicon (see the preceding note). Thus, whether or not the words derived from this root in Hebrew, Aramaic and Arabic are Akkadian loans, the noun *ikkaru* in Akkadian itself appears to be genuinely Semitic, and Sumerian *ENGAR* appears to be a loan from Akkadian.

Listing Sumerian names for profession which Landsberger (1974 [1944]) believed belonged to the Proto-Euphratic substratum in Mesopotamia,¹³¹ Rubio (1999: 4), who thinks that some of them originated in Semitic, has this entry without commenting on it: “engar (APIN), probably eġar (Krecher 37), ‘plowman’”. The Sumerian verb *uru*₄ is spelled APIN and denotes ‘to plough’. “The sign APIN is

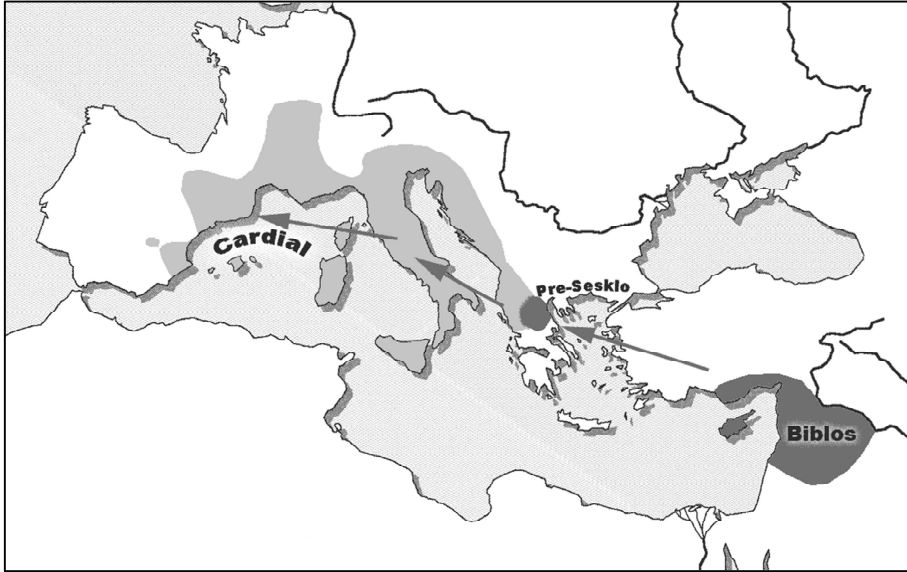
¹²⁸ *Ahw* stands for Wolfram von Soden’s (1959–1981) *Akkadisches Handwörterbuch*.

¹²⁹ In the scholarly literature of Assyriology, *CAD* is a standard acronym of *The Assyrian Dictionary of the University of Chicago* (Chicago 1956 ff.)

¹³⁰ The acronym *CDA* is standard for the *Concise Dictionary of Akkadian*, edited by Jeremy Black, Andrew George, and Nicholas Postgate.

¹³¹ https://en.wikipedia.org/wiki/Proto-Euphratean_language explains as follows: “**Proto-Euphratean** was considered by some Assyriologists (for example Samuel Noah Kramer), to be the substratum language of the people that introduced farming into Southern Iraq in the Early Ubaid period (5300–4700 BC). ¶ Benno Landsberger and other Assyriologists argued that by examining the structure of Sumerian names of occupations, as well as toponyms and hydronyms, one can suggest that there was once an earlier group of people in the region who spoke an entirely different language, often referred to as Proto-Euphratean. Terms for ‘farmer’, ‘smith’, ‘carpenter’, and ‘date’ (as in the fruit), also do not appear to have a Sumerian or Semitic origin. ¶ Linguists coined a different term, ‘banana languages’, proposed by Igor Dyakonov and Vladislav Ardzinba, based on a characteristic feature of multiple personal names attested in Sumerian texts, namely reduplication of syllables (like in the word *banana*): Inanna, Zababa, Chuwawa, Bunene etc. The same feature was attested in some other unclassified languages, including Minoan. The same feature is allegedly attested by several names of Hyksos rulers: although Hyksos tribes were Semitic, some of their names, like Bnon, Apophis, etc. were apparently non-Semitic by origin. ¶ Dyakonov and Ardzinba identified these hypothetical languages with the Samarran culture. ¶ Rubio challenged the substratum hypothesis, arguing that there is evidence of borrowing from more than one language. This theory is now predominant in the field (Piotr Michalowski, Gerd Steiner, etc.). ¶ A related proposal by Gordon Whittaker is that the language of the proto-literary texts from the Late Uruk period (c. 3350–3100 BC) is really an early Indo-European language which he terms ‘Euphratic’”.

already in the archaic texts from Uruk, both lexical and administrative, the so-called List of Professions included [...], and also in Ebla” (Rubio 1999: 9, fn. 19).



An interpretation of genetic evidence. “Ancient DNA of early Neolithic Cardial Pottery men in cave burials have been found to be mainly of Y-DNA haplogroup G2a”. In colour at https://en.wikipedia.org/wiki/File:Cardial_map.png

6.7.5. *A Nineteenth-Century Semantic Calque from German Ackermännchen*

Alinei was concerned with German *Acker* (1996: 243) and *Ackermann* as a lexical compound: cf. Middle English *acremān* (< *acre* + *man*), Swedish *åkerman*, Dutch *akkerman* (2000a: 454). The perceived sound-alike terms, German *Ackermann* (< *Acker*) and Biblical Hebrew and Modern Hebrew *ikkār* ‘farmer’ (cf. Sumerian ^{LU}ENGAR) were exploited for neologisation purposes, in his Hebrew-language natural-history lexicon, by Abramowitsch (1866: 132), who rendered the bird-name *Ackermännchen* – German *Ackermännchen* or *Ackermännchen* – with *ha’ikkār* (literally ‘the farmer’) in order to denote the species *Motacilla (alba)*, ‘(white) wagtail’ (cf. Grimm and Grimm 1854, Vol. 1: 174: *Ackermännchen* German *Ackermann* (from *Acker* denotes ‘farmer’s field’) denotes ‘man of the field’, ‘peasant’, whereas literally, *Ackermännchen* is a diminutive of the latter, thus, ‘little man of the field’, ‘little peasant’, because that bird is found in the

fields. Therefore, what we have here, with Abramowitsch's neologised Hebrew bird-name, is not merely phono-semantic matching, but a semantic calque.

The literal sense in Mendele's Hebrew neologism is similar to the literal sense of the German bird name. This was pointed out by Zuckermann (2000, 2003), in the context of his important research into the role phono-semantic matching was given in the service of the "nativisation" of neologism within the linguistic planning of some modern languages (of which Modern Hebrew is just one).

Fischler (1991: 24, no. 37, and p. 26, col. 1) noted that already Schönhak's Hebrew-language natural history (1841–1859), in his volume on zoology (1841), had introduced the Hebrew neologism *ha'ikkār*, as corresponding to the German term *Ackermännchen*, and actually Abramowitsch and Schönhak differed in that Abramowitsch transcribed that particular German compound in one word in the Hebrew script (according to the conventions of Yiddish spelling), whereas Schönhak did the same but wrote it as two words. Shalom Jacob Abramowitsch (ca. 1837–1917) was to become a famous Hebrew and Yiddish novelist, and as such, he is better known as Mendele Mokher Sfarim. The other author of a natural history in Hebrew was Joseph Schönhak of Suwalki (1812–1870). Abramowitsch was the most influential zoological terminologist in Modern Hebrew from the 19th century, but his neologism we discussed did not survive into Israeli Hebrew.

6.7.6. Ugaritic *ugr* 'field, soil', and *ugrt* 'Ugarit (toponym)'

Let us go back to Table S3 (by Yigal Bloch) in Agmon and Bloch (2013). I quote his definitions. Entry 3.10 includes the proto-word **hugār* "meadow, field, arable land" (from Proto-Semitic, a triconsonantal word); there are no known occurrences in Hebrew and Aramaic; in Ugaritic however one comes across both *ugr* "field, soil", and *ugrt* "Ugarit (toponym)". There is no known occurrence in Arabic and in Modern South Arabian, so the cases in the columns for Arabic and for Modern South Arabian are empty (just as the cases in the columns for Hebrew and Aramaic are empty). In the column for Ethiopic however, tantalisingly, one finds the term without the first syllable of the proto-word. Namely, in Ethiopic we have *garh*, *garāht* "field, arable land, farm", and *garha* "to plow". And finally, in Akkadian we have *ugāru* "grass-land, meadow, arable land".

May I add that perhaps, just perhaps, the Proto-Semitic term for ‘arable land’, **hugār*, which occurs in Ethiopic as *garh*, thus without the first syllable, is etymologically related in a Neolithic spread of agriculture perspective to the Germanic lexical type *garden* (and its Indo-European cognates) we discussed in an earlier section.

6.7.8. *Earth*

Under the assumption of some Semitic vocabulary being piggybacked on the Neolithic spread of farming and entering the Germanic lexicon of farming, it is tempting not to dismiss as a coincidence the similarity of the Germanic words *Erde* in German, Dutch *aarde*, and *earth* in English, to Semitic words that mean the same. Arabic *ard* for ‘earth’ and ‘land’, Hebrew *éres* (pausal form: *āres*, stem of inlected forms: *arš-*), Akkadian *eršetū*, Aramaic *arqā* (an earlier form, with a voiceless uvular stop), *ar‘ā* (a later form, with a voiced pharyngeal fricative), all of them denoting ‘earth’ and ‘land’. Cf. (though the relation, if any, is unclear) early rabbinic and Modern Hebrew *qarqa* ‘terrain’, but ‘floor (or the Tabernacle)’ in *Numbers* 5:17; ‘floor (of Solomon’s Temple)’ in *1 Kings*, 6:15–16, 6:30, 7:7; and inside a compound, ‘(sea) floor’ in *Amos* 9:3. An obvious loanword from Arabic is Turkish *arz*, whereas Turkish has the native terms *toprak* and *yeryüzü* as near-synonyms.

In an encyclopedia entry, Vennemann (2011) explained his etymology succinctly:

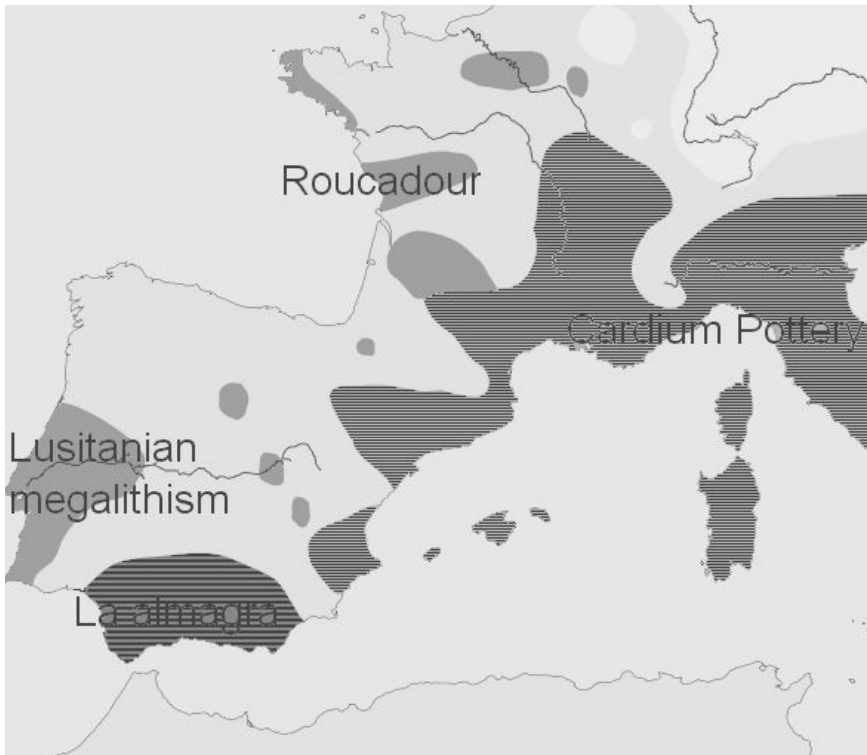
Erde/earth, Proto-Germanic ⁺*erþō*, Phoenic. *ʔrš*, Hebrew *ʔrš*, *ʔeres* (Arab. *ʔardun*, cf. Lipiński 2001: § 16.11 for Canaanite *š* ≈ Arab. *ḏ*), with Phoenic. *š* → Germanic *þ* [θ] (cf. Engl. *earth*) > German *d*, as also in *Adel*, see below); Greek *ἔραζε* / *ἔρασδε* ‘(down) to earth’, Hesychius *ἔρας* in *ἔρας* - *γῆς* may have the same Phoenician origin.

In *Germania Semitica*, Vennemann is concerned with this etymology from Semitic into Proto-Germanic ⁺*erþō* on p. 231, note 19; cf. on p. 437 and on p. 444, note 25.

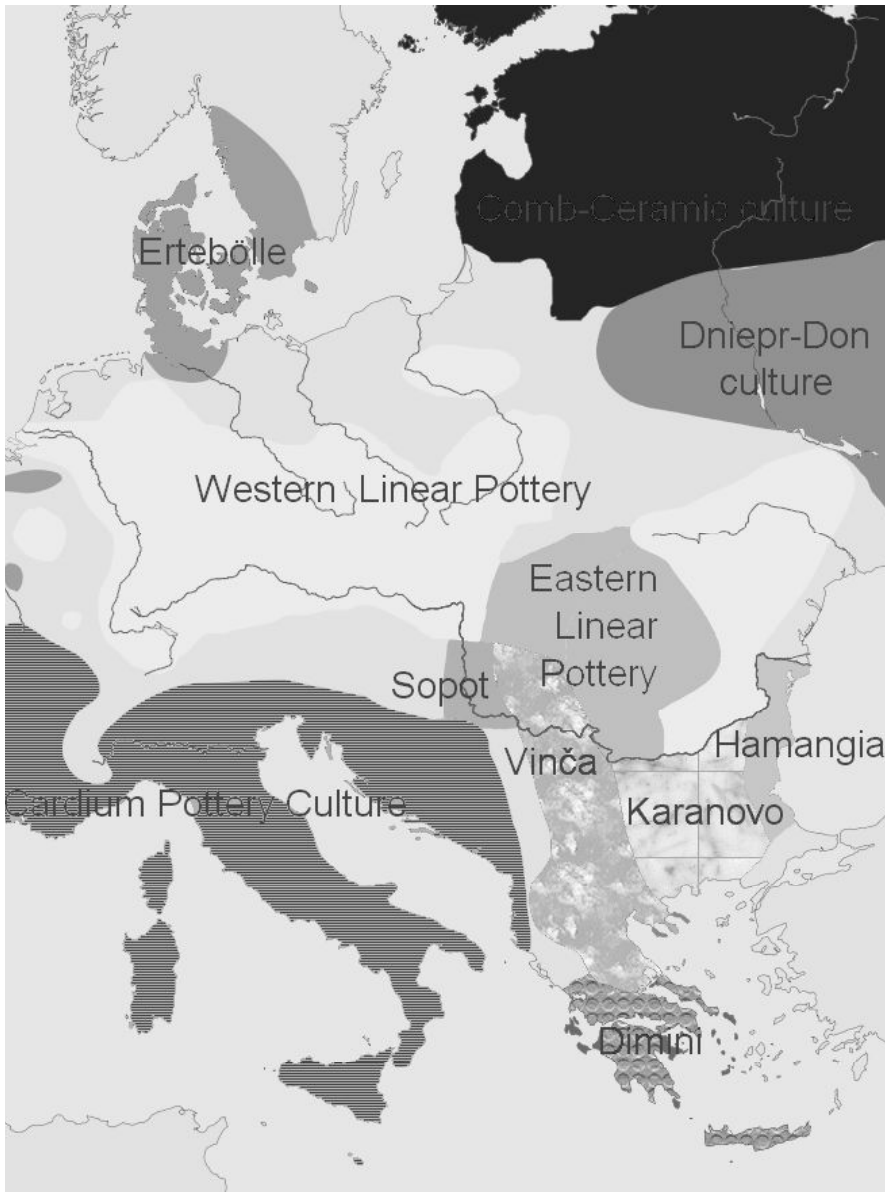
Alinei (1996: 520) briefly discussed German *Erde* and English *earth*, when mentioning a hypothesis found in the literature, to the effect that the adverbs whose PIE proto-forms are **upér* ‘over’, ‘above’ (Pokorny 1959–1969 at 1105), **iner* ‘inside’, and **anter* ‘anteriorly to, in front of’ are compounds of **up*, **in*, and **ant* with **er* ‘earth’, on the

evidence of Greek *érā*, German *Erde*, English *earth*, and the like. But then, I reckon, these forms would be correlated, rather than originating from, such Semitic terms as Hebrew *éres*.

Vennemann (2003a, pp. 254–255, 559, 614) argued for a Semitic etymology of the Germanic *Erde* lexical type. Sheynin's excoriation of Vennemann (2003a) claimed: "Since we cannot imagine ancient Arabs living in pre-historic Germanic lands, V.s reconstruction of etymology for Gmc. *earth* / *Erde* is impossible". When Sheynin published again a modified version of the same review, this became: "Since we cannot imagine ancient Assyrians or ancient Arabs leaving [sic] in Germanic lands in ancient time, V.s reconstruction of etymology for Gmc. *earth/Erde* is impossible". My response to this is that it is quite possible that during the spread of farming bringing about the Neolithic in European lands, such farming communities whose vernacular was no longer Semitic, nevertheless retained Semitic vocabulary relevant for farming or social organisation. Successive adaptations of that vocabulary (or of individual *Kulturwörter*) to newly acquired vernaculars would have involved in the process of transmission such mutation that elude our grasp, and of course (as Sheynin maintains anyway) one needs to consider proto-forms also on the Semitic side, not only on the Germanic side, which is what Vennemann does. It is very complex.



Western Europe in ca. 4500–4000 BCE. The extent of Cardium Pottery Culture is Illirico-Iberian, and encompasses Italy and Southern France as well. Andalusia is typified by the La Almagra culture.



Central and eastern Europe in ca. 4500–4000 BCE. Most of the area of the Cardium Pottery Culture is shown here. The dark area north of the Dniepr-Don culture is the Comb-Ceramic culture. Modified from <https://en.wikipedia.org/wiki/File:European-middle-neolithic-en.svg>

In order to be able to argue that the *Erde* lexical type originated with the spread of farming reaching Germanic lands,¹³² it is important to realise that it is not necessarily a Proto-Semitic reconstructed proto-form, but possibly one of a number of alloforms that belongs to the vocabulary of farming of the incoming farmers. Concerning Arabic *ard*, first of all note that transition from the last consonant to [d] is a straightforward development in lexical borrowing from Arabic (e.g., into Israeli Hebrew slang). Secondly, note that the phoneme transcribed as /d/ (and written in the Arabic script using the letter *dhād*, ض) is a velarised version of the voiced interdental fricative.¹³³ Medieval Arab grammarians used to boast of *dhād* ض as being a uniquely Arabic sound, which could also be claimed of ط (*dhā*). The difference between those two velarised consonants is in that in the latter, the tip of the tongue touches the upper lip, whereas in *dhād* the tip of the tongue touches the edge of the upper incisors. There is a habit among Orientalists of transcribing ط with *z*, but this is ambiguous, because for example in Baghdadi Judaeo-Arabic, in *jāzma*,¹³⁴ a loanword from Turkish denoting ‘boot(s)’, the phonetic value [z] is just a velarised [z], and moreover in *jāzḏān* ‘purse’, the cluster [zḏ] is a velarised version of [zd] (this is what that dialect does with loanwords from some languages). Likewise, there is a habit among Orientalists of transcribing

¹³² In contrast, Semitic words for ‘sand’ do not appear to have been transmitted to Germanic.

¹³³ An argument concerning Vennemann’s etymology of *Erde* made by Sheynin (2013), and which he had not made in the previous published version of that same review of Vennemann (2003a), is: “If we accept V.’s explanation, it will be impossible to explain other Indo-European forms without any trace of Semitic emphatic *ṣ* or *ḏ*” (*ibid.*: 194–195). I find this particular counter-argument unconvincing.

Note by the way, that as part of Vennemann’s research into the origins of the futhork, which he considers to have been “the adaptation of the Phoenician, specifically the Carthaginian or Punic, alphabet to the Proto-Germanic language”, Vennemann (2013a) discussed “how the Punic letters *B G D* were utilized in this process. Two hypotheses are discussed, one in which the *g* and *d* runes are derived from the Punic emphatic plosives, *Ḫ* and *Ṭ*, and another one, favored in this article, in which the late Punic development of *b g d* into fricatives and semivowels is considered”. Among the other things: “There is in fact ample evidence that in Neo-Punic all Punic plosives had changed into fricatives, except for the emphatics, *ḫ* and *ṭ*” (*ibid.*). “Punic *Ḫ* and *Ṭ* were suitable letters to represent Germanic *g* and *d* (i.e. unaspirated voiceless [k] and [t], or [g] and [d]) in the runic writing system” (*ibid.*).

¹³⁴ Where *j* is an affricate [j] = [dʒ] as in English *job*.

ض with *d*,¹³⁵ but this is ambiguous, because of what we have seen in the latter example, and moreover because Italian dialectologists resort to *d* in order to transcribe the retroflex *d* (also called a cacuminal *d*), which in the International Phonetic Alphabet is [d̠]. This leads us to a crucial point: Mario Alinei has argued repeatedly against Colin Renfrew's Neolithic Diffusion Theory (NDT), according to which the spread of farming and the Cardium (or Cardial) Pottery culture was carried by the incoming Indo-Europeans, whereas according to Alinei's Continuity Theory, the Indo-Europeans had already been in Europe since the Upper Palaeolithic. In the history of the ideas, Renfrew's theory has had the merit of pushing back the supposed date of the Indo-European arrival, thus resulting in a long chronology, and its justification is that Indo-European languages share agricultural or farming terminology, whereas according to Alinei's approach, the latter terminology was spread by lexical borrowing. According to Alinei's approach, the spread of farming and of Cardial pottery was carried by non-Indo-Europeans, in particular speakers of Semitic vernaculars. The following is quoted from Alinei (2002):

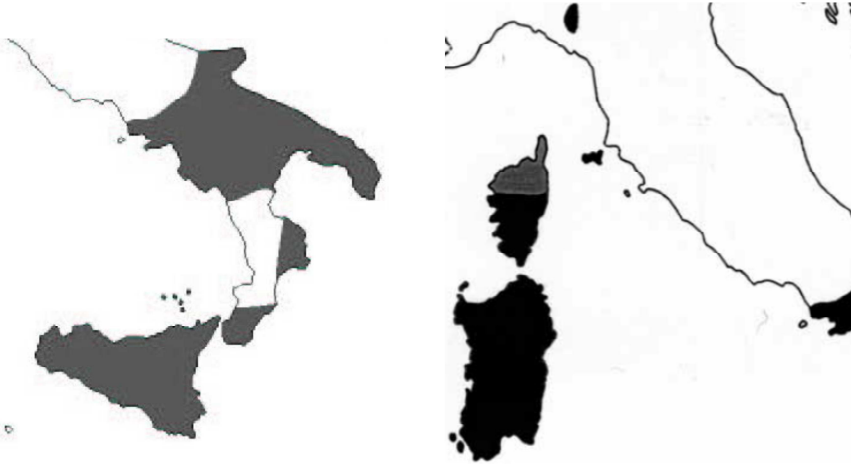
Unfortunately, even if this hypothesis were sufficient to solve the problem for Greece – which it is not [...] – the major problem is that also Southern Italy and the islands¹³⁶ are characterised by highly peculiar phonetic features – namely the retroflex or cacuminal rendering of /l/, /d/ and /tr/ – which are totally alien to Latin phonology, and which are usually considered a typical trace of the so called “Mediterranean”, non-IE substratum.¹³⁷ The following figure shows the distributional area of these retroflex sounds [...] Notice that this area comes very close to that of the earliest spread of the Impresso/Cardial Ware – that is Southern Italy and the islands –, and that

¹³⁵ Dolgopolsky (1998) chose to transcribe ض with an undercupped *d*, by graphic analogy with *ħ*. I have sometimes used an underdotted Greek δ (but quite rarely so, and then it would have been preferable to rather underdot the letter *eth*, namely, δ as found in the Perpetua font). Both options are rather problematic if one is to use available fonts. Perhaps the best option (which some scholars of Arabic or Judaeo-Arabic adopted) is to transcribe ض with an underlined *d* and to transcribe ط with an underlined *t* (but again, a problem with typesetting is that the underlining should not overstrike the dot under the letter). The reason a modification of the symbol *t* was chosen in order to represent Arabic ط is that that Arabic letter is a modification of the letter ط that represents *t*. That is to say, the reason is graphemic, not phonetic.

¹³⁶ Sicily, Sardinia, Corsica, Elba, and the areas in the Italian Peninsula shown in the map, but also Lunigiana (the northwesternmost area of Tuscany).

¹³⁷ Note however the occurrence of retroflex consonants in Scandinavian languages.

this is in total contradiction with what the NDT predicts, namely that the concentration of non-IE features ought to appear especially in the areas not or little touched by the Impresso/Cardial culture. But the opposite is true: not only do we not find any trace of non-IE influences in the areas not or little touched by the Impresso/Cardial Ware, but we find them only precisely where the impact of this culture was the earliest and the greatest! Only if we assume the Continuity Theory, and thus the reverse hypothesis that the autochthonous people were IE, and the intrusive farmers were non-IE, can we explain the coincidence between the area of retroflex sounds and that of the earliest spread of Impresso/Cardial.¹³⁸



Sicily and areas of peninsular southern Italy where retroflex stops occur. Detail of Fig. 5 in Alinei (2001b).

Sardinia, Corsica, as well as (in Tuscany) Lunigiana and Elba island, where retroflex stops occur. Detail of Fig. 5 in Alinei (2001b).

¹³⁸ Cf. in Alinei (2000a, Sec. 7.2: 924): “Fra i fenomeni fonetici che nella teoria tradizionale vengono attribuiti al cosiddetto sistrato pre-IE, e che nella TC [i.e., Teoria della Continuità] si lasciano interpretare come fenomeni di adstrato o superstrato peri-IE, vi è poi, certamente, il passaggio di *-ll-* a *-dd-* cacuminale, e la pronuncia cacuminale di /r/, /tr/, /str/, che caratterizzano i dialetti dell’Italia del Sud, della Sicilia, della Sardegna, della Corsica e della Lunigiana”. The following is quoted from https://it.wikipedia.org/wiki/Consonante_retroflexa “Le consonanti retroflesse, in italiano, non hanno rilevanza fonologica, vale a dire che non sono portatrici di un significato che si possa opporre a un’altra consonante: esse sono usate unicamente come varianti libere (allofoni) delle corrispondenti alveolari in diverse lingue del sud Italia, come nel siciliano, calabrese, sardo e salentino. Così vanno ad esempio pronunciati i nessi <tr>, <str>, <dd> o <ll> nel siciliano *trenu*, *strata*, *idda* o nel sardo *nudda* (rispettivamente [ˈtʁɛːnu], [ˈʂːaːta], [ˈidːa] o [ˈidːza] e [ˈnuɖːa]).”

This is very interesting and cogent indeed, but is important to explain how velarised or pharyngealised *d* and *t* from Semitic as currently known is correlated to retroflexed stops. Were the retroflexed stops a feature of part of the prehistorical Semitic-language or Afroasiatic-language speakers, along with retroflex rendering of the liquids (*l* and *r*)? Berber¹³⁹ does have retroflex consonants indeed.¹⁴⁰

Along with the plain voiced fricative *ð*, one finds in modern Berber its emphatic version, *ðʕ* (i.e., Berber **ḍ**). Along with the plain voiceless alveolar *t*, one finds its emphatic version, *tʕ*. Moreover, again in modern Berber, along with the approximant *l* one finds **ɭ** being its velar version, and along with the plain trill *r* one finds **ʀ** being its emphatic version. Along with the plain voiceless alveolar *s*, one finds the emphatic voiceless alveolar **sʕ**, and along with the plain voiced

¹³⁹ Maarten Kossmann (1999) discussed the phonology of Proto-Berber. Concerning the relative chronology of the emergence of language families and proto-languages, https://en.wikipedia.org/wiki/Berber_languages states: “Berber is a member of the Afroasiatic language family. Since modern Berber languages are relatively homogeneous, the date of the Proto-Berber language from which the modern group is derived was probably comparatively recent, comparable to the age of the Germanic or Romance subfamilies. In contrast, the split of the group from the other Afro-Asiatic sub-phyla is much earlier, and is sometimes associated with the Mesolithic Capsian culture”. The latter is the subject of https://en.wikipedia.org/wiki/Capsian_culture

¹⁴⁰ Was there ever the phenomenon of retroflex consonants in the northern Semitic vernaculars, perhaps in prehistory or prehistory? Was that a secondary development from original velarisation? Or perhaps both velarisation and retroflexion developed separately from another articulatory situation in Proto-Afrasian, or clusters within early Afrasian? Could retroflexion be reasonably ascribed to migrants from the Near East rather than early Berbers? If so, was the development of retroflexion facilitated by the migration itself? What was the interaction of Semitic emphatics and Berber retroflexion when early farmers moved from the Near East into North Africa? Or then perhaps the retroflex rendering gives as a hint about how velarisation or pharyngealisation developed in prehistoric Semitic? Were there competing renderings? How does this relate to non-Semitic speaking communities of now dead languages of the Near East (Hurrian, Hittite, even perhaps Euphratean)? So much in linguistic prehistory eludes our gaze, even when we do realise that some phenomena appear to have occurred.

Allan Bomhard (1981: 374) accepted Martinet’s suggestion that pharyngealised consonants evolved from ejectives. Bomhard has claimed (*ibid.*): “The Cushitic and Omotic languages provide the strongest evidence in favor of interpreting the emphatics of both Proto-Semitic and Proto-Afroasiatic as ejectives”. He then reasoned on Beja, Galla, and Somali retroflexes.

alveolar **z**, one finds the emphatic voiced alveolar **zʕ**. Along with the plain voiceless uvular stop **q**, one finds the labial voiceless uvular stop **qʷ**, but only the plain voiceless uvular fricative **χ** (i.e., [x]) is found, with no labial equivalent. Along with the plain voiced bilabial stop **b**, one finds in Berber the labial voiced bilabial stop **bʷ** (in Baghdadi Judaeo-Arabic [BJA], one considers **bʷ** and the labial nasal bilabial **mʷ** as emphatic versions of **b** and the plain nasal bilabial **m**, and linguistic transcription of BJA notates them indeed as underdotted **b** and **m**). In Modern Berber, one also finds the voiceless and voiced pharyngeal fricatives of Arabic, **ħ** (i.e., **ħ**) and **ʕ**.

Therefore, perhaps (as some of the relevant consonants were concerned), it would make sense to prefer a Berber origin of the retroflex consonants in Italy, in prehistoric times. In ancient Egyptian, there were no emphatics, and the Islamised Berbers appear to have acquired pharyngealised emphatics in imitation of Arabic (cf. Bomhard 1981: 374). One needs consider however the history of Afroasiatic (Hamito-Semitic), where the emphatics apparently developed from ejectives, and implosive according to Martinet developed from ejectives by anticipating the *coup de glotte*; Allan Bomhard (1981, pp. 374–375) has pointed out that the best evidence for the development of emphatics from ejectives is from Cushitic and Omotic languages, with retroflex **ɖ** occurring in both the Beja and Galla languages (Somali has it). An arrival from North Africa into southern Italy as a background for retroflex consonants now occurring in southern and insular Italian dialects does not need to have per force occurred in the context of the spread of farming, which brought about the Neolithic in lands it reached. This does not exclude that such was the context of migration from North Africa. In fact, during the 20th century a Berber origin of that phonetic phenomenon in Italy was proposed: “Anche se non vi sono prove conclusive per ipotizzarne un’origine camito-semitica (berbera), come supponeva Wagner (1931; cfr. Contini 1987, 172], è difficile ammettere che si tratti di uno sviluppo autoctono” (Alinei 2000a, Sec. 7.2: 924, his own brackets).¹⁴¹

¹⁴¹ This is a legitimate and even plausible idea, but it has a sinister antecedent in the history of ideas. In newly unified Italy, in final four decades of the 19th century, an inferiorising conceptualisation of Italy’s South developed, and it was combined with the European then prevalent concept of inferior Africans, in some theorisation that eventually came into being. The prejudice was internalised by a major proponent,

While criticising Vennemann's hypothesis deriving in his 2003 book the Germanic *Erde* or *earth* lexical type from Semitic (Vennemann 2003a, pp. 254–255, 559, 614), Sheynin (2013: 194) pointed out: “The Proto-Semitic form of this word is *’*araš*’, see Dolgopolsky (1999): 25, #44, not ‘*ard*’ like in Arabic”.¹⁴² I must say that first of all, whereas I am not surprised Sheynin cited Dolgopolsky (their academic affiliation was in Haifa, after they moved out of the Soviet Union, and already in the 1960s Sheynin was in a milieu where for example Igor Diakonoff was promoting the Nostratic hypothesis), it is hardly the case that a consensus could be claimed for *the* “Proto-Semitic form” as proposed by Dolgopolsky, even though he may be right in reconstructing this proto-form.¹⁴³ Secondly and more importantly, it is not the case that

Alfredo Niceforo, a polymath and himself a Southerner, as well as a famous criminologist. During the period 1910–1953 with no interruptions, Niceforo was in charge of the course in Criminology at the Scuola Giuridico-Criminale of the Facoltà di Giurisprudenza (School of Law) at the University of Rome. Niceforo himself was born in Castiglione di Sicilia, on 23 January 1876 (he was to die in Rome, on 10 March 1960). Nevertheless, he theorised the intrinsic inferiority of the Southern Italians (whose race he claimed to be “Euro-African”), with respect to the Northern Italians (whose race he claimed to be “Euro-Asiatic” or “Aryan”). This is an extreme example of how some southern intellectuals were partly accepting of the typological description of the populace of their regions, but in the case of Sicily it was convenient for the current elite to apportion the blame to a mythical past whose cultural bearers had been by then absent for centuries. (For example, Luigi Pirandello, himself a Sicilian, did so by blaming the perceived Sicilian indolence on the Arab past of the island.) Niceforo (1898a) was a volume about Italy's South, described in the main title as “Contemporary Barbarous Italy”. A volume published in that same year (Niceforo 1898b) was a book about (to say it with its title) “criminals and degenerates in Dante's *Inferno*”. Niceforo (1901) was a book about Italy's Northerners and Southerners.

¹⁴² It is in a sense refreshing that Dolgopolsky did not subscribe to the romantic notion that the set of Classical Arabic consonants is identical with the pristine set of Proto-Semitic consonants. Perhaps it was, but this should not be an unquestioned tenet.

¹⁴³ With no disrespect for Aharon Dolgopolsky intended, his reconstructions of proto-forms in general are controversial (as he himself has been all too aware) – Dolgopolsky's exposure was in front line, so to speak, defending the hypothesis of a Nostratic superphylum, whereas another very talented linguist, Igor Diakonoff, also a Nostraticist and an author of a Nostratic dictionary, was not as vulnerable, because part of his oeuvre which is cited is not predicated upon the validity of the Nostratic approach: it does not stand or fall on espousing the Nostratic hypothesis – even though Dolgopolsky was not at all adamant that co-occurrence in different phyla was

one must suppose that in reality there ever existed just one form that could be considered the proto-form, whereas prehistorical early alloforms are safer to assume, even when we cannot pinpoint them accurately. The already quoted statement by Sheynin (*ibid.*: 194) shows how for differences in understanding to arise, different sets of assumptions are crucial: “Since we cannot assume ancient Assyrians or ancient Arabs living in Germanic lands in ancient time, V.’s reconstruction of etymology for Germanic *earth/Erde* is impossible”. Not so, I must say, once one accepts the plausibility of Semitic vocabulary of farming being piggybacked to the Balkans, then Pannonia, then Germanic lands, during the Neolithic, by the spread of farming, regardless of how quickly farming communities switched to local vernaculars. Sheynin then averred (*ibid.*): “However Pokorny (1927–1930): 332 has a satisfactory etymology of this word sub v. *er-* (*er-t*, *er-w*) and there is no need to look for a Semitic etymon even if it seems very similar”. In my opinion this rather means that there are two competing hypotheses, and that Pokorny’s may be satisfactory. We can never be sure that Occam’s Razor (the *lex parsimoniae*)¹⁴⁴ would yield the factual reality: the simplest explanation¹⁴⁵ (more exactly: the one requiring the fewest assumptions)¹⁴⁶ is not necessarily what actually happened.¹⁴⁷ What Sheynin says next (*ibid.*, pp. 194–195) is a

because of phylogenetic cognacy, rather than because of lexical borrowing. He has been open to the latter being the aetiology for similarities.

¹⁴⁴ Occam’s razor is a problem-solving principle that states that among competing hypotheses, the one with the fewest assumptions should be selected. See https://en.wikipedia.org/wiki/Occam's_razor

¹⁴⁵ Ptolemy’s formulation is: “We consider it a good principle to explain the phenomena by the simplest hypothesis possible”. See https://en.wikipedia.org/wiki/Occam's_razor Cf. Ariew (1976), Maurer (1984).

¹⁴⁶ This is the standard formulation, first found in Aristotle’s *Posterior’s Analytics*: “We may assume the superiority *ceteris paribus* [other things being equal] of the demonstration which derives from fewer postulates or hypotheses”. The translation is Richard McKeon’s (published in 1963).

¹⁴⁷ https://en.wikipedia.org/wiki/Occam's_razor points out: “Francis Crick has commented on potential limitations of Occam’s razor in biology. He advances the argument that because biological systems are the products of (an ongoing) natural selection, the mechanisms are not necessarily optimal in an obvious sense. [...] Occam’s razor is not an embargo against the positing of any kind of entity, or a recommendation of the simplest theory come what may. Occam’s razor is used to adjudicate between theories that have already passed “theoretical scrutiny” tests and are equally well-supported by evidence. [...] Galileo Galilei lampooned the *misuse* of

statement to the cost for PIE reconstructions, rather than for the instance at hand: “If we accept V.’s explanation, it will be impossible to explain other Indo-European forms without any trace of Semitic emphatic *š* or *ḏ*”. Is this necessarily the case? Lexical borrowing does interfere with expected regularities. It does not need to be the case that Proto-Germanic borrowed the term directly from a Semitic vernacular; along the route of transmission through other vernaculars, the term may have already undergone adaptation.¹⁴⁸

7. Zoonyms

7.1. *Crab*

In order to denote the lexical crab, ‘crab’, Proto-Indo-European has **karkar-* (Pokorny 1959 at 531; cf. Alinei 1996: 507). Concerning Vennemann’s etymology (507–508, in Sec. 26.6.1.4) of the Germanic terms of the lexical type *crab* from Semitic names for ‘scorpion’ of the

Occam’s razor in his *Dialogue*. [...] Occam’s razor has met some opposition from people who have considered it too extreme or rash. [...] Karl Menger found mathematicians to be too parsimonious with regard to variables, so he formulated his Law Against Miserliness, which took one of two forms: ‘Entities must not be reduced to the point of inadequacy’ and ‘It is vain to do with fewer what requires more’”.

¹⁴⁸ It makes a lot of difference, when we actually know about the steps of a chain of lexical borrowing. Take Modern and Israeli Hebrew *būl* ‘postage stamp’. It is known for certain that it is a loanword from Arabic *būl* ‘postage stamp’, itself from Turkish *pūl* (whence in Republican Turkish spelling, *pul* or more specifically *posta pulu*). Had we not known that much, we may have rather vaguely conjectured that Modern Hebrew *būl* ‘postage stamp’ is in some relation to Italian *bollo* ‘stamp’ (> *francobollo* ‘postage stamp’ and *bollo postale* ‘stamping over a postage stamp’) < *bollare* ‘to stamp’ < Medieval Latin *bullare* ‘to stamp’ < Latin *bulla* ‘stamp’. That however is the ultimate etymon. Byzantine administrative practice possessed χρυσόβουλλον for the golden seal, and *chrysobulla* for the imperial document sealed in gold (in Modern Greek, a papal bulla is a βούλλα). Whereas in Turkish the first consonant was devoiced into [p] (almost certainly because Byzantine Greek no longer had β represent [p]: cf. in Modern Greek μπ = [b]), borrowing into Arabic revoiced it into [b] because of the foreignness of [p] to Arabic phonetics and phonology. In the spoken Iraqi varieties of Arabic instead, [p] used to be retained in loanwords, so in Baghdadi Judaeo-Arabic *pūl* ‘postage stamp’, and its plural is an Arabic broken plural (Ablaut) form *pwāl* (as the root is taken to be *p.w.l.*; cf. the spelling ⟨pwl⟩).

lexical type *'aqrab*,¹⁴⁹ that etymology is fairly cogent indeed, even though one wonders whether it is strictly necessary. After all, there is a Proto-Indo-European root, and crabs would have been familiar for hunter-gatherers as a source of food well before the arrival of agriculturalists.

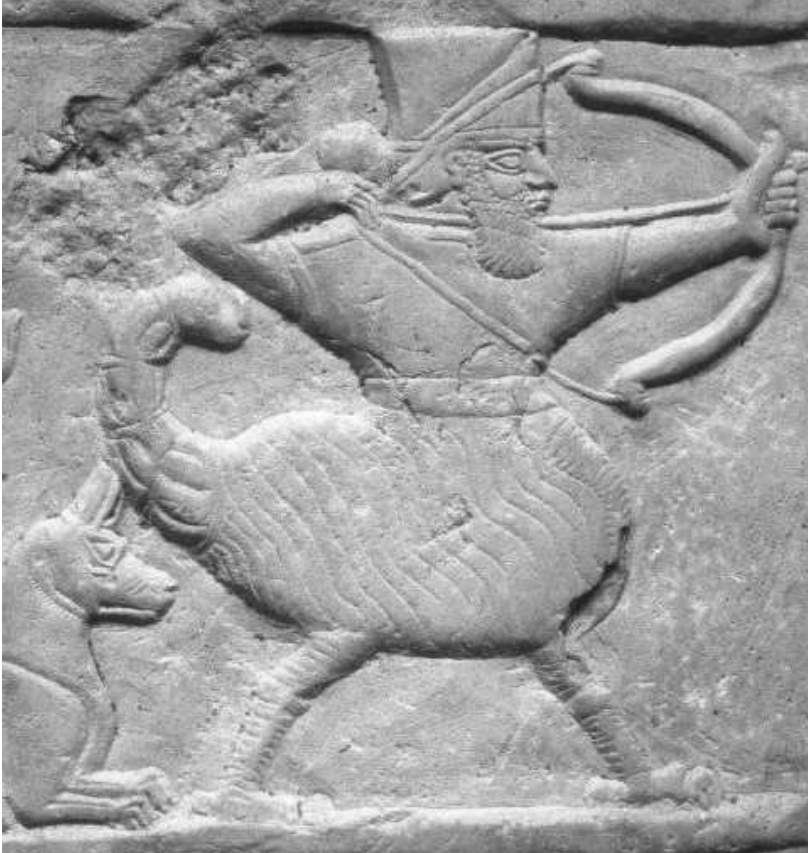
If Vennemann is right, then I reckon that there used to exist an older Germanic name than the lexical type *crab*, and that it was displaced. Perhaps that older name was replaced because the old name had become tabooised? “As also shown by the unshifted *q* [...] the word was borrowed after the operation of Grimm’s Law” (508). Besides, I wonder: was the older name one somewhat similar (as suggested by the Proto-Indo-European root), yet one which did not have the final /b/, and for that reason replacement was facilitated when there was language contact with immigrant farming communities retaining some Semitic vocabulary?

Hebrew *'aqrāb* ‘scorpion’ (now pronounced [ʕakʕav] or [ʔakʕav] in Israeli Hebrew, but e.g. in liturgical Iraqi Hebrew [ʕaqʕab]) is a masculine noun, but agrees with the feminine in the *Mishnah*, tractate *Shabbat*, 16:7. Militarev and Kogan (2005) have an entry “*ʕakrab- ‘scorpion’” (*ibid.*, no. 31, pp. 48–50). This lexical type is widespread in Semitic languages. They also list possible cognates from African languages. Vennemann’s etymology of the *crab* word would be cogent even if it hadn’t further support from this tantalising passage (Militarev and Kogan 2005: 49): “Forms derived from the present root participate in word combinations denoting various kinds of crustaceans”, such as Arabic “ʕakrabu l-māʔ- ‘écrevisse’” (literally ‘scorpion of the water’), and from Ethiopia, the compound “ʕarkäb bähär ‘crab’” (with metathesis) in the Tigre language (literally ‘sea scorpion’), and the compound “ʕankärbät bahri ‘crab’” in the Tigrinya language (literally ‘marine scorpion’. Tigrinya *ʕankärbät* ‘scorpion’ is a form that somewhat reminds me of colloquial Arabic *ʕank(a)būt* ‘spider’ (cf. Militarev and Kogan 2005, no. 33).¹⁵⁰ In fact, the entry “*ʕakrab-

¹⁴⁹ E.g., Hebrew *'aqrāb* ‘scorpion’, Israeli Hebrew [ʔakʕav], Ashkenazic Hebrew [ʔakyov], Iraqi Hebrew [ʕaqʕab]. The Samaritan Hebrew pronunciation of the Biblical Hebrew word spelled as <ʕrb> is given by Talshir (1981: 331) as *'āqrāb*.

¹⁵⁰ The entry “*ʕankab(ī)- ʕ *ʕankab(īʕ)- ‘spider’” in Militarev and Kogan (2005: 51–53) comprises forms from Hebrew, Jewish Middle Aramaic (which they call “Judaic Aramaic”), Arabic, and the South Arabian vernaculars Mehri, Harsusi, and

‘scorpion’” in Militarev and Kogan (2005) also comprises “stunningly similar forms in S[outh] Omot[ic] Ongota *ḵarrábatī* ‘spider’ [...] and metathetic E[ast] Cush[itic] Burji *ḵóobr-oo* ‘scorpion’” (*ibid.*: 50).



A scorpion-man archer, from a Mesopotamian boundary-stone.

Jibbali. “As for the well-known Arb. [= Arabic] form *ṣankabūt-* (masc., fem.) ‘araignée’ [...], it is usually regarded as an Aramaism [...]. This view is to be taken with caution since the Jud. [= Judaic Aramaic] form quoted above has a different vocalic shape and no *-n-* while no similar term is attested in other Arm. [= Aramaic] languages. Mnd. [= Mandaic] *ṣankabut* ‘spider’ [...] and Tgr. [Tigre] *ṣankäbot* (pl. *ṣanäkkəb*) ‘spider’ [...] are certainly Arabisms” (*ibid.*: 52). From Jewish Middle Aramaic, the same entry has *ṣakkōbūtā* or *ṣakkābūtā* ‘spider’ (*ibid.*: 51).

Because of geographical considerations, what especially matters is the occurrence in Arabic of the compound that literally means ‘aquatic scorpion’ to denote ‘crab’, even though the occurrence of ‘marine scorpion’ in Tigrinya (with an adjective derived from the term for ‘sea’)¹⁵¹ and of ‘sea scorpion’ in Tigre, moreover with lexical components so similar to Arabic lexical entries, provides important support.

What is more, all three occurrences make it all the more likely that also in Northwest Semitic speakers, sometime throughout unrecorded ancient history, protohistory, or prehistory would have here and there conceptualised crabs as being creepy-crawlies resembling scorpions, but aquatic, thus making those animals suitable for being denoted by a descriptor that included ‘scorpion’ as the *genus* and ‘aquatic’ or ‘marine’ as the *differentia specifica* (without their explicitly adopting such an Aristotelian framework). In a seaside environment, indicating the *differentia specifica* explicitly could have been superfluous, especially if, which was the case of the *mare Germanicum*, the colder

¹⁵¹ Note that the Arabic noun *baḥr* ‘sea’ and the adjective derived from it, *baḥrī* ‘marine’ in Egypt may also refer to the Nile. “The Bahri Mamluks were not so called because they came from across the Black Sea ([Izady] p. 244, n. 1) but because their barracks were by the Nile (*Baḥr al-Nil*)” (Peacock 2006: 602). In parts of northeastern Africa, such as South Sudan, southeastern Chad, and the Centrafrican Republic, the Arabic noun occurs in Arabic names for particular rivers. *Baḥr al-Ghazāl* (literally ‘river of the gazelles’) is the name of a river and its marshy basin in South Sudan, an area disputed by England and France in 1894–1899. The White Nile and the Blue Nile in Sudan are respectively called *Baḥr al-Abyaḍ* and *Baḥr al-Azraq* in Arabic (with the equivalent literal sense). In the northwest of Centrafrican Republic and southernmost Chad, *Baḥr Sara* flows north as a tributary of *Baḥr Salamat*, which gives the marshy region in Chad’s south its name. But in the Salamat, there is a *Baḥr al-Azraq* which is not the Blue Nile. There is more to it. Alan Kaye reviewed (1997b) Samir Abu-Absi’s (1995) 44-page introduction to Chadian Arabic, a structural sketch of that sub-Saharan, macro-Sudanic Arabic variety. Kaye himself carried out fieldwork in Chad (Kaye 1976). Kaye (1997: 203) pointed out: “Arabic dialectologists who specialize in the Middle East are often amazed to learn of some ‘unusual’ Chadian Arabic common, everyday vocabulary”, and one such example is “*azrag*, ‘black’, ‘blue’ (cf. Nigerian Arabic ‘blue’, ‘green’)”. Chadian Arabic comes in different varieties; e.g., Map 75 in Irvine (1994), shows that on the shores of Lake Chad, the Baggāra variety (no. 34) of the Guhayna subgroup of Eastern Arabic is in use, except a large enclave north of N’Djamena, with a short shore on the lake, which belongs to the Chari variety (no. 37) of the Chadic subgroup of Eastern Arabic. Cf. Owens (1993).

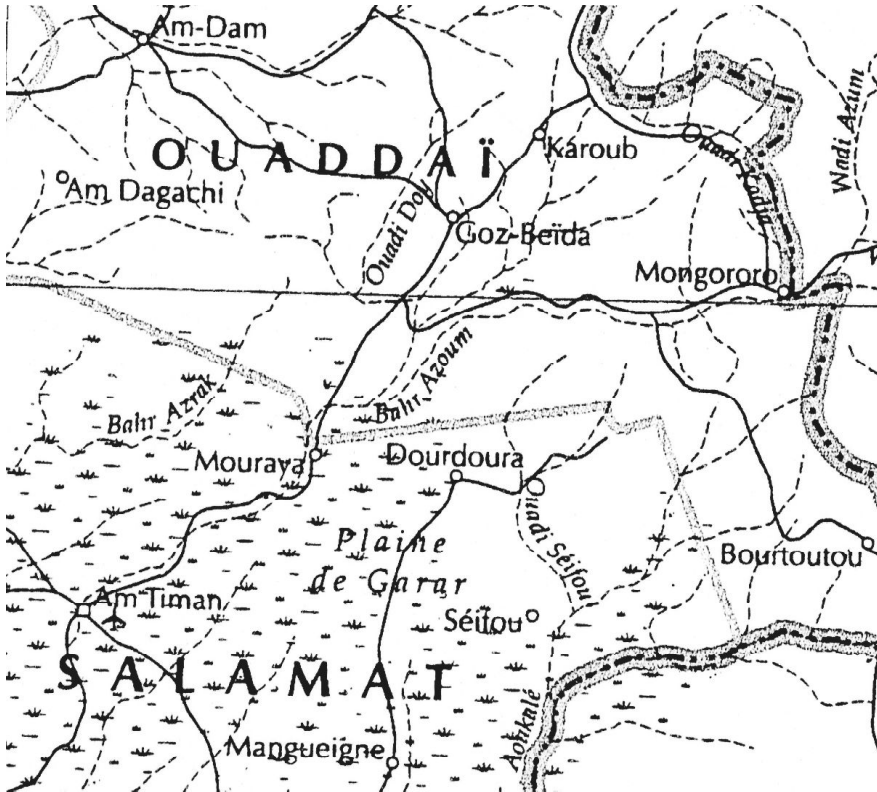
climate prevented scorpions to be as available as they are in the Middle East.

According to Vennemann's etymology of the Germanic *crab* word, it is an "example for an unshifted *b*" (507), and "the word was borrowed after the operation of Grimm's Law" (508). It may be that Phoenician, Punic, or Gaditan traders were the trigger for the lexical borrowing (and this is his general framework in *Germania Semitica*, even though he is not saying that on pp. 507–508), even though compatibly with my hypothesis that his timescales are telescoped, it may be that the phonetic developments unfolded over a much longer time period, and that the loanword was even late Neolithic, from the time of the spread of agriculture up to the North Sea coasts. Vennemann remarks about the conflict between the *Oxford English Dictionary* declaring that *crab* is "In no way related to L. *carabus*, Gr. *κάραβος*" – "which is inevitable if *crab*, *carabus*, and *κάραβος* are understood to be inherited Indo-European words"¹⁵² (Vennemann, 507) – and "Kluge/Seebold (2002: s.v. *Krabbe*) writ[ing] about the German word: 'Ursprünglich niederdeutsch; (bezeugt seit dem 15. Jh.); mndl. [= Middle Dutch] *crabbe*, ae. [= Old English] *crabba m.*, an. [= Old Norse] *krabbi m.* beruhen kaum auf einem Erbwort, sondern hängen wohl mit gr. *kárabos*, lt. *Cārabus* 'Meerkrebs' zusammen (die aus einer unbekanntem Sprache stammen'" (Vennemann, 507–508).

In the Sicilian dialect of Italian – where apparently because of tabooisation, the lexical type of Italian *scorpione* (Latin accusative *scorpiōnem*, ultimately from Greek *skórpios*) is never used for the sense 'scorpion' (Lanaia 2012, pp. 248, 252) but *scurpiuni* occurs for the sense 'gecko' (*ibid.*, pp. 248, 250) – the lexical type *surfizziu* (a variant of *scurfizziu*) more often denotes 'scorpion', but it may also denote 'crab' (*ibid.*: 251). This semantic shift lends support to Vennemann's hypothesis. Alfio Lanaia considers *scurfizziu* somewhat close, phonetically, to French *écrevisse* 'crab' (< Frankish *kreditja*, of the Germanic *crab* word family). He claims that the other Sicilian forms he had first listed *ibid.* on pp. 248–249 as denoting 'scorpion' but

¹⁵² What if Proto-Indo-European's early differentiation took place (in an "Out of Africa" scenario) while in the Near East – which is compatible with Alinei (2000) – or at any rate while still co-territorial and in contact with Hamito-Semitic, thus including not just Western Asia, but also North Africa? This however be problematic, because of the later phonetic phenomena within which Vennemann's contextualises his discussion of the *crab* word.

in the town of Sciacca, ‘gecko’ (*surfizziu*, *sirfizziu*, *salifizziu*, even – by attraction to the term for ‘sacrifice’ – *sacrifizziu* and *sacrificiu*) are either deliberate distortions (out of tabooisation), or attempts at semantic remotivation (*ibid.*: 252).



Bahr in names for rivers in southeastern Chad.

7.2. *Hind*

Vennemann (106, note 26) credits Peter Schrijver (*in litteram*) for signalling to him “several other West Indo-European words with a difficult *-t-* suffix”, such as Latin *amita* ‘father’s sister’ (for this *āmīta* I would like to signal Arabic *ġāmmat-* ‘father’s sister’; the stress is on the next syllable in inflected forms); as well as Old Irish *elit* ‘hind’. This is quite simply Semitic, I would like to point out: it displays the

feminine suffix *-it*. Cf. Hebrew *ayyāl* ‘deer’, Akkadic *ayyalu* ‘deer’, and Hebrew *áyil* /ʔayl/ ‘ram’, plural *eylīm*.

Also consider (with [r] replacing [l]) *i₂=-r=ya* ‘ram’, a Semitic loanword in Egyptian (Hoch 1994: 29, no. 18) recorded in Papyrus Anastasi IV, 14, 5 (Gardiner 1937). Hoch (1994: 29, no. 18) tentatively reconstructed the pronunciation as being either **'êlya* or **'ayla*. Hoch (*ibid.*) preferred to associate the entry with the sense ‘ram’ of Biblical Hebrew *‘áyil*, rather than with the sense ‘stag’ of Biblical Hebrew *‘ayyāl*, because “[b]oth instances¹⁵³ of the word occur in lists of domesticated and semi-domesticated animals”. He listed Coptic cognates: **ⲟⲉⲓⲗⲉ**, **ⲁⲓⲗⲉ**, **ⲟⲓⲗⲓ** ‘ram’. Hoch (1994: 29, no. 18) stated: “The Coptic derives from **'ayla*, since it indicates the presence of the semivowel *i/y* in medial position, but it may be a later loan word or later came under the influence of another dialect” (something which, Hoch suggests in fn. 72, happened for Egyptian *dīʒ=tu* ‘olive’, a Semitic loanword, vs. Coptic **ⲭⲟⲉⲓⲦ** ‘olive’).

While listing 11 to 12 Semitic loanwords in Egyptian of the New Kingdom and Third Intermediate Period in either agriculture or animal husbandry, Hoch (1994: 466) indicated that 2.9% to 3.2% of all nearly 500 Semitic loanwords he discussed in his book belong to those semantic domains. Hoch (1994: 467) listed 7 fauna terms (1,8% of the total) – “Wild animals (including insects), their parts, and things associated with them” – as well as 7 to 8 flora terms (1.8%–2.1%), “[p]rimarily wild plants and their parts”.

The word may have been confused with *'a=yu-r* ‘stag’ (also a Semitic loanword), Hoch suggested while discussing *i₂=-r=ya* ‘ram’ (1994: 29, no. 18). In fact, the word *'a=yu-r* from *The Misfortunes of Wenamun* 2, 68 (Gardiner 1932, pp. 61–76), whose pronunciation Hoch reconstructed as **'ayyōla* (1994: 17, no. 1), was defined by Hoch as ANIMAL USED FOR FOOD, PROBABLY “STAG,” POSSIBLY “RAM.” Hoch also listed demotic **ⲩⲓⲡⲓⲣ** and Coptic **ⲉⲓⲟⲮⲗ** ‘hart’. Hoch stated (*ibid.*): “The *u* of the Egyptian writing probably represents **[ō]*. If so, the word perhaps derives from a Phoenician original, since only in this language did original short accented **[ǎ]* (after being stress-lengthened to *[ā]*) undergo the **[ǎ]–*[ō]* shift. Hebrew nouns, according to the Masoretic

¹⁵³ The other Egyptian instance is *iʒ=ra=ya* from an inscription from Hermopolis, dealt with in the *Annales du Service des Antiquités de l'Égypte*, 52 (1954), pl. III, 1. 5, in Roeder (1952) after p. 442.

tradition, exhibit stress-lengthening, but not a change in vowel quality”. Concerning the *[ǎ]–*[ǒ] shift, Hoch (*ibid.*, fn. 4) cited Garr (1985, pp. 33–34), and remarked: “In Hebrew rthe shift basically involves only *[ǎ].

Dolgopolsky (1998, pp. 41–42),¹⁵⁴ having listed Semitic names for ‘deer’, and then Late Egyptian and Coptic names for the same (noting that this is by borrowing), turns to Indo-European, and lists Greek ἔλλός and ἐλλός ‘young deer’, ἔλαφος ‘deer’, Armenian *eln* (gen. *elin*) ‘female deer’, Old Irish *elit* (< **eln-t-* but he places a circle diacritic mark under the *n*) ‘chamois’, Welsh *elain* ‘female deer’, Old Lithuanian *ellenis* ‘deer’, Lithuanian *ėlnis*, *ėlnias* ‘deer’, Prussian *alne* ‘female deer’, and examples also from Old Church Slavonic and from Russian. Also note Old High German *eltho* ‘elk’, Anglo-Saxon *eolh* ‘elk’, and so forth. Within Uralic, one finds *ile* ‘deer’ in Tundra Yukagir. Cf. *elik* ‘roe-buck’ Old Turkic and several Turkic languages (Dolgopolsky, *ibid.*).¹⁵⁵ One also comes across items from Kartvelian – *ilw* ‘chamois’ in Lashkhi Svan, and *il* or *hil* ‘roe’ in Lower Bal Svan – and from Dravidian: Malto *ilaru* ‘mouse deer’, and Telugu *irri* ‘antelope’ (*ibid.*). Citing these data from Dolgopolsky’s book does not amount to an endorsement on my part of Nostratic. Rather, it can be explained out as a case of extensive borrowing (Dolgopolsky himself is fairly pragmatic, and does concede that borrowing rather than phylogenesis is a possible aetiology).

See a critical analysis of Dolgopolsky (1998) in the nearly twenty contributions in Renfrew and Nettle (1999). Concerning the relevant entry in Dolgopolsky (1998), the rival Nostraticist Allan Bomhard claimed (1999: 57, item 37): “*?E1/ǐ ‘deer’: the Afrasian forms should be removed. The rest of the etymology can stand as it is. I would reconstruct Proto-Nostratic *?il- ‘hoofed, cud-chewing animal’”. Alexander Vovin (1999: 369) remarked that “Nostratic ?E1/ǐ ‘deer’ (no. 37) is well supported by both Turkic *elik ‘female roebuck’ and Mongolic *ili ‘young deer’”.

¹⁵⁴ Dolgopolsky (2008) is a fuller Nostratic dictionary.

¹⁵⁵ An author who is very sceptical of Nostratic, Lyle Campbell (1999: 193, item 37), commented about that entry from Dolgopolsky (1998) by stating: “Uralic is represented only by Yukaghir (not a Uralic language, but thought related by some) *ile* ‘deer’, which may well be a borrowing from Turkic **e|älik* ‘roe-buck’”.

Concerning that same entry from Dolgopolsky (1998), Starostin (1999: 145, item 37 in a table), noting in the second column that the distribution of the Nostratic root *ʔEl/ʔi ‘deer’ includes Indo-European, Altaic, Kartvelian, and Dravidian, made this comment in the third column: “HS [i.e., Hamito-Semitic]: only Semitic. Uralic: only Yukagir”. In the next column, Starostin offered as “Sino-Caucasian evidence” (the title of the column) Sino-Tibetan *lǎ ‘musk-deer’.

The next item, in Starostin’s table (1999: 145, item 8) was concerning Dolgopolsky’s (1998, item 38, pp. 42–43) Nostratic root *boča ‘young deer’.¹⁵⁶ Noting occurrence in only Kartvelian and Uralic, Starostin made this cautionary comment: “A [i.e., Altaic]: The Tungus forms cited (Neg. bočan, Ulch, bočan etc.) go back actually to Manchu *bugu-čan (bučín), with *bugu (also attested in Manchu as buyu, buxu) being most probably borrowed from Mong[olian] buyu ‘deer, aurochs’. HS [i.e., Hamito-Semitic]: only Arabic with a very unsecure East Chad[ic] (Lele) parallel”. Being a specialist in the supposed Sino-Caucasian phylum (Starostin 1989), in the next column in the table Starostin (1999: 140, item 7) offered this evidence from Sino-Tibetan within Sino-Caucasian: *ś(r)iǎŋ (= *r-śiǎŋ) ‘cold, frost’. “A [i.e., Altaic]: The Tungus forms cited (Neg. bočan, Ulch, bočan etc.) go back actually to Manchu *bugu-čan (bučín), with *bugu (also attested in Manchu as buyu, buxu) being most probably borrowed from Mong[olian] buyu ‘deer, aurochs’. HS [i.e., Hamito-Semitic]: only Arabic with a very unsecure East Chad[ic] (Lele) parallel”. Dolgopolsky himself was uncertain concerning the relevance of Lele *bisi* ‘duiker’. His Arabic term was baḍaḡ-, buḍḡ- ‘lamb’. Which is questionable evidence.

Generally speaking, consider that such Indo-European or Uralic names for artiodactyls whose semantic motivation is from the animal being horned have been considered to be noa replacements¹⁵⁷ of some

¹⁵⁶ Lyle Campbell (1999: 193, item 37), commented about that entry from Dolgopolsky (1998) by stating that “[t]he Uralic form is reconstructible only back to Finno-Permic [...] and almost certainly involves borrowing in northern Eurasia as part of reindeer culture”. Campbell further insists about forms being loanwords rather than evidence for genetic relationship between languages.

¹⁵⁷ Incidentally, consider that noa replacements apparently affected the name of the fox in languages such as Icelandic. The following is the entry for *fóa* on p. 167 in the second edition of Cleary and Vigfusson’s (1957, repr. 1962) *An Icelandic-English Dictionary* (its first edition appeared in 1874): “FÓA, u, f. a fox; this curious

earlier name that came to be tabooised – apparently a name of the lexical type **el-* – by Rédei for Uralic (Redei 1988–1991: 487), by Gamkrelidze and Ivanov (1995: 438) for Indo-European, and by Alinei (1996: 570) for both Indo-European and Uralic.

In Semitic, the lexical type *el-* and derivatives of the root ʔ-*y-l* are semantically associated not only with artiodactyls (in particular with cervids, or with the male ovine: ‘ram’), but also with physical strength (because of the horns?), as well as with the divine. Arguably the archisememe of the Semitic root is ‘strength’, and the sense ‘divine’ is derived (because of the praeternatural power wielded by deities or by the Deity, thus, because of the primary feature of the divine), just as the sense ‘artiodactyl’ is derived from the archisememe – arguably because of the physical power of the impact from a charging horned animal.

Also note, as an analogue, that the name for a male human adult, *vir* in Latin (cf. Latin *vis* ‘force’) is called *geber* [‘gever] in Hebrew, and this is a co-derivative of names for ‘force’ and for ‘valiant man’ (‘hero’), but Hebrew *geber* [‘ge:ver] is also a name for ‘rooster’ (especially rooster when crowing), because of its perceived male strength, presumably because of how conspicuously the rooster displays self-possessing male behaviour among chicken.

word, which answers to Goth. *fauhō*, O. H. G. {= Old High German} *foha*, only occurs in Edda (Gl.), unless the present Icel. *tóa* (the common name for a fox) be a corruption of *fóa*; if not, the etym. of *tóa* is quite uncertain. It is a common superstition not to call the fox by his right name, whence the variety of names in different languages, and number of synonymes {*sic*} in the same language” (my braces). That entry is preceded in the same column by this other entry (their brackets, my additions in braces): “FOX, n. [A. S. {= Ancient Scandinavian} and Engl. *fox*; Dutch *vos*; Germ. *fuchs*; this word occurs in the old northern tongues only in a metaph. sense, and even then rare and obsolete]:–*a fraud* in selling, *adulteration*; fox er illt í exi, Eg. {= Egils Saga} 184 (in a verse); otherwise only in the phrase, selja e-m fox né flærð, Gpl. {= Gulaþings-lög} 492; kaup-fox, veð-fox (q.v.), *fraud in sale* or *bailing*, Gpl.” It would appear then that *fox* was replaced with *fóa*, which was replaced in turn with *tóa*, as denoting ‘fox’ (the animal), but the older terms were preserved in metaphorical usage, perhaps in such *milieux* that did not have to do with foxes in real life. Incidentally, consider from Cleary and Vigfusson’s (1957, repr. 1962), in the Supplement, on p. 819, the entries “**rebbi**, m. *a fox*; lyddan líktist rebbi, Geðr. 9. 54”, and “**refligr**, adj. *foxy, cunning*; [...]”. Taboo being the reason for the Indo-European name for ‘fox’ being replaced in the Germanic languages with a noa name semantically motivated by ‘tail’ is something that occurs in a discussion by Alinei (2000a: 433).

Hittite is rather unhelpful when it comes to checking its name for 'ram' for comparison to Semitic and Indo-European. There are two reasons for this: Hittite's use of sumerograms, and semantic motivation being from the animal's maleness. Weeks (1985) has the following Hittite entries (items 3.12 and 3.13 on p. 23, and entries 3.25 to 3.27 on p. 25):

3.12 – MALE – In most cases the sumerogram refers automatically to the male of animal species, unless additionally marked 'female' (SAL[.AL.LAL]) or a distinct female form (see 3.13). The terms used to refer to human beings (2.23) could probably also be applied to animals for particular emphasis, especially NITÁ as the masculine counterpart to SAL(.AL.LAL).

3.13 – FEMALE – Female animals are in part designated by distinct sumerograms, e.g. ^{UDU}U₁₀ 'ewe' vs. ^{UDU}ŠIR 'ram', ^{GUD}ÁB 'cow' vs. GUD.MAH 'bull', ÛZ 'nanny goat' vs. MÁŠ 'he-goat', EMÈ 'jenny' vs. ANŠU 'ass'. In other cases the qualifier SAL(.AL.LAL) 'female' could be affixed (cf. *EHGl.* 18–19 and n. 2).

where *EHGl.* stands for Harry Hoffner's "English-Hittite Glossary" (1967);

3.25 – SHEEP – Luw. *hawi-*, Hier. *ha-wali-i-ša*, Lyc. *χawã*, and H. **hawi-* (UDU-*is*) reflect the initial laryngeal phoneme in PIE **A₁^wOWi-*, attested only indirectly or lost without trace in Gk. οἶς, Lat. *ovis*, OIr. *óí*, OHG *ouwi*, Lith. *avis*, OCS *ovica*, Skt. *avi-*, etc., and seen as well in Arm. *hovi-w* (T 230).

^(UDU)*iyant-* is originally the participle of *i-* 'go', and the notion of 'walking wealth' is comparable with Gk. πρόβατα and ON *ganganda fé* (Pedersen, *Hitt.* 148) as well as Oscan *éitiuvam* 'pecuniam' < **ey-tu-* (P 348 with refs.).

3.26 – RAM – UDU.NITÁ-*an*, acc. sg., also UDU.ŠIR (ŠIR 'testicle').

3.27 – WETHER – Possibly UDU.ŠE 'fatted sheep', cf. Ved. *pétva-* 'wether' < *pī-* 'grow fat'.

7.3. Ram

7.3.1. The Semitic Lexical Type of Hebrew *rě'ēm* ⟨r'm⟩

The Biblical Hebrew zoonym *rě'ēm* denotes a particular kind of wild ruminant, whose strength is in relation to its horns or the use it makes of them, and the term perhaps denotes the aurochs (*Bos*

primigenius). Quite possibly however, the identity of the denotatum did not remain the same throughout the biblical corpus: it may be that sometimes it denotes (A) a large wild ox (was this the water buffalo,¹⁵⁸ i.e., *Bubalus bubalis*, or the aurochs, i.e., *Bos primigenius*?), and some other (apparently later) times, (B) either the Arabian oryx (*Oryx leucoryx*) or, much less likely,¹⁵⁹ the addax (*Addax nasomaculatus*),¹⁶⁰ or then, otherwise, (A) and (B) are alternative to each other, rather than dividing among them the set of occurrences.

Dor (1965, s.v. *rē'em*, pp 321–322) stated that older occurrences (but also *Job* 39:9)¹⁶¹ denoted the aurochs (and in fact, Akkadic *rīmu* labels a visual representation of the aurochs), whereas once the aurochs had become extinct, later on (*Psalms* 92:11), “And Thou raised like a

¹⁵⁸ However, it is relevant to consider the question discussed by Zohar Amar and Yaron Serri in their article (2005) “When did the water buffalo make its appearance in Eretz Israel?”. Eretz Israel is the Land of Israel.

¹⁵⁹ That was an identification proposed by the zoologist Israel Aharoni in the early 20th century.

¹⁶⁰ “The addax (*Addax nasomaculatus*), also known as the white antelope and the screwhorn antelope” (<https://en.wikipedia.org/wiki/Addax>). “Addax were formerly widespread in the Sahelo-Saharan region of Africa, west of the Nile Valley” (IUCN Red List, <http://www.iucnredlist.org/details/512/0>). “The addax was once abundant in North Africa, native to Chad, Mauritania and Niger. It is extinct in Algeria, Egypt, Libya, Sudan and Western Sahara. It has been reintroduced in Morocco and Tunisia” (<https://en.wikipedia.org/wiki/Addax>). “In ancient times, the addax occurred from Northern Africa through Arabia and the Levant. Pictures in a tomb, dating back to the 2500 BCE show at least the partial domestication of the addax by the ancient Egyptians. These pictures show addax and some other antelopes tied with ropes to stakes. The number of addax captured by a person were considered an indicator of his high social and economic position in the society. But today excess poaching has resulted in the extinction of this species in Egypt since the 1960s. Addax fossils have been found in four sites of Egypt – a 7000 BCE fossil from the Great Sand Sea, a 5000–6000 BCE fossil from Djara, a 4000–7000 BCE fossil from Abu Ballas Stufenmland and a 5000 BCE fossil from Gilf Kebir. Apart from these, fossils have also been excavated from Mittleres Wadi Howar (6300 BCE fossil), and Pleistocene fossils from Grotte Neandertaliens, Jebel Irhoud and Parc d’Hydra” (*ibid.*). “Today there are over 600 addax in Europe, Yotvata Hai-Bar Nature Reserve (Israel), Sabratha (Libya), Giza Zoo (Egypt), North America, Japan and Australia under captive breeding programmes” (*ibid.*).

¹⁶¹ Cf. Militarev and Kogan (2005: 249): “The specific association of *r.* with bovids (as against antelopes and other hoofed animals) is obvious in Jb [sic] 39.9–12 where *r.* appears in expressions otherwise typical of *šōr* ‘ox’ [...] Note the variant form *rēm* in *Job* 39.9, 10 (consonantal *rym*) and *Ps* 22.22 (consonantal *rmym*)”.

rě'ēm my horn" (i.e., strength) denoted *Oryx*.¹⁶² Dor (1965: 322) re-remarked that ancient Egyptian depictions of the oryx were in profile, and that this may have given rise to the legend of the unicorn (that hypothesis of identification is already found in Lewysohn 1858; cf. Dor 1997: 38, s.v. *qéresh*), and, one may add, to the identification (especially by Christians) of the *rě'ēm* with the monoceros or unicorn. In the Babylonian Talmud, tractate *Hullin*, 59b, it is stated that the tiger is stated to be the equivalent (arguably in the sense of ecological vicariance) of the lion in Be 'Illay ("the High House/Place", i.e., the mountains of central Asia), just as the *qéresh* ('unicorn')¹⁶³ is stated to be Be 'Illay's equivalent of the gazelle.¹⁶⁴

Chapter 5 in Toaff (1996, pp. 79–100) is entitled "Unicorni e monoceronti", but it is only on p. 95 that mention of an animal with only one horn appears. In the context, Toaff is discussing *Tsémaḥ David* by David De Pomis (Venice, 1587). On p. 95, Toaff traces the mentions of the unicorn in the Babylonian Talmud (tractate *Avodah Zarah*, 8a), in the midrashic literature, and in illustrations in Jewish medieval manuscripts from Ashkenazic and Italian communities (where the animal was imagined as being horse-like, in Italy the horn always being twisted and very long), of from Spain (where the animal was drawn to resemble a giraffe). Toaff states that to Moses of Rieti (1388 – after 1460), the unicorn resembled an ass (I reckon that in this he was following Aristotle's description of the Indian rhinoceros, probably because of its long ears). On pp. 96–98, Toaff (1996) is concerned with the discussion of the medical use and of the authentication tests of the horn of the unicorn, in *Tsémaḥ David*.

¹⁶² In Dor (1997) – a book that was edited by others than Dor, based on his notes – there is an entry on pp. 38–40 of the aurochs and possible biblical names for it (*tě'ō* and *rě'ēm*); there is an entry for the oryx in Dor (1997: 37–38).

¹⁶³ The concept of 'unicorn' has been widely treated across cultures, and in particular, in given European Christian cultures – see, e.g., Kökeritz (1963), Pietersma (2005) – but mentions and visual representations occur in Jewish texts as well. Sometimes the concept was mixed up with that of the imagined rhinoceros, and moreover, a horn of the narwal – thus, of a cetaceous mammal from the northern seas – would on occasion be misascribed to the unicorn. Also see Joachim Schaper's (1994) "The Unicorn in the Messianic Imagery of the Greek Bible". Ezio Albrile (2003) is concerned with the *asellus unicornis* in relation to Gnostic influences.

¹⁶⁴ It has been suggested that the etymology of *qéresh* is from the Greek name for 'horn'.

In an article applying archaeozoology to the identification of biblical ruminants, Zohar Amar, Ram Bouchnick and Guy Bar-Oz (2009a, 2009b) identified the *re'ēm* with the oryx; note however than in a table, they listed one instance of archaeozoological remains of aurochs occurring in the Land of Israel in the biblical period, and those remains are from Transjordan, from the Iron Age (whereas no remains are known from the Late Bronze period and the Achaemenid period). In that same table, they also listed one instance of oryx remains from the Achaemenid period, but none from the previous two periods (which of course is unlikely to imply that no oryx were previously roaming the local deserts). No remains of *Addax* and no remains of the water buffalo (or, for that matter, of the rhinoceros)¹⁶⁵ occur from any of those periods.

In early rabbinic and then medieval Hebrew, the *re'ēm* is the gigantic wild ox. The rabbinic *re'ēm* is wondrous, in that it is huge (Slifkin 2007, pp. 52–55).¹⁶⁶ In zoologists' Israeli Hebrew, the plural

¹⁶⁵ The habitat of the rhinoceros in historical times included neither the Land of Israel, nor Egypt. Yet, ancient Egyptian had a name for the rhinoceros, just as it had a name for the giraffe. Quite possibly, rhino horns and giraffe hides were traded from Egypt. The Egyptian name of the rhino was *šqb*. See about it on p. 82 in Raphael Giveon's (1978) *The Impact of Egypt on Canaan*, as well as on p. 285 in Störk (1977), *Die Nashörner*, a book which deals with the rhino both in ancient Egypt, and other African cultures. Dmitri Meeks' *Année lexicographique*, Vol. 1, §77.4304 made me aware of the treatment of *šqb* in Störk: 5, whereas from Meeks, Vol. 2: 382, §78.4202, I learned about Giveon: 82.

¹⁶⁶ Add to the texts which Slifkin quotes (2007: 52–55), a passage added to the Hebrew early medieval mock-sapiential *Life of Ben Sira*, about the child prodigy Ben Sira answering Nebuchadnezzar's questions (like the boor Markolf, Solomon's in the German and Latin comic epics, and in the serious medieval encyclopedia *Livre de la fontaine de toutes les sciences*, the Christian wiseman Sidrac, the questions of King Boctus of Bactriana). Cf. Yassif (1982). Nebuchadnezzar has to pay Ben Sira his fee, this being a *re'em*'s weight in gold, so he asks him how to weigh it. Ben Sira replies that the King is certainly able to obtain a *re'em*. Let him order a ship built, as long as the Red Sea, *Yam Suf*. (This suggests knowledge of the long and narrow contour of that sea.) Then let the *re'em* go inside it, and let the King take a knife and make a sign on the hull how deep it was submerged (i.e., let the full load waterline be measured). Then let the King have the *re'em* exit the ship, and let him fill the latter with gold, until the waterline as measured before is reached. Ben Sira also asks the King to repeat the process with pearls. The King is nonplussed: is he to pay the weight of two *re'emim*, one in gold and one in pearls?! Ben Sira points out that wisdom is quite valuable, and worth it. The King obtains a discount, and Ben Sira only obtains the weight of one *re'em* in gold. This passage appears In Yassif (1984: 284; but the entry

rě'ēmīm denotes the subfamily *Hippotraginae* within the family *Bo-Bovidae*. Among the other things, Slifkin discusses (2007, pp. 50–51) whether the biblical *re'em* was the aurochs (*Bos primigenius*). Importantly, the *re'em* is not enumerated in Scripture in the list of the seven clean ruminants, and archaeozoological finds are important: probably the ruminants in that list were enumerated based on their relative availability in the Land of Israel, and the aurochs and the oryx were not relevant in that sense (also because the aurochs would have been subsumed under 'ox' anyway). See Amar et al. (2009, 2010).

7.3.2. *The Samaritan Evidence for Hebrew <r'm>*

Hebrew *rě'ēm* (spelled <r'm>) of *Numbers* 23:22, 24:9, appears in the Samaritan manuscripts as <r'm> indeed and is pronounced by the Samaritans as *rām*, whereas the occurrence of <r'm> (as per the Masoretic text) in *Deuteronomy* 33:17 appears in the Samaritan manuscripts as <r'my> or <rmy>, pronounced *rāmi* (Talshir 1981: 340). Samaritan Aramaic manuscripts render the term as <rymh> and variants (Talshir, *ibid.*).

7.3.3. *Semitic cognates of Hebrew <r'm>*

The *Semitic Etymological Dictionary, 2: Animal Names* by Militarev and Kogan (2005) have an entry (No. 186, on pp. 248–250) for **riṁ* 'aurochs'. They do not really problematise the sense of the Biblical Hebrew <r'm>, and it may be that in the *denotatum* they indicate for the cognates they opt too quickly for a bovine sense. And yet, they signal the sense 'white gazelle' ('gazelle, surtout celle don't le pelage est blanc') of Arabic *riṁ-*. (They also signal Arabic *raṁ-* 'petit de chameau'.) They also remark: "The prominence of the poetic and metaphoric usage of reflexes of **riṁ* in [the] Syro-Mesopotamian area likely suggests that the respective terms denoted a semi-mythical rather than a real animal".

for *rě'ēm* in Yassif's index missed p. 284). Yassif (1984: 284, fn. 5) detects here medieval lore about Archimedes, and remarks that the tale from *Pseudo-Sirach* about weighing the *re'em* was quoted in *Shibbolei HaLeqet*, ed. Buber, 124a, §254, and in *Sefer Tanya Rabbati*, Warsaw 1879, 61b, §58.

For the purposes of what follows in the next subsection, I would have liked to be the case that there was direct evidence from within Semitic that the lexical type ⟨r'm⟩ could sometimes denote some non-bovid strong, horned ruminant used to butt, which is the case of rams. The hypothesis that there would have been a semantic shift to 'ram' is costly, as we would need to rather circularly use as support precisely the Germanic lexical type *ram*, but that is *quod est demonstrandum* in the first place. It is a circular argument.

Note however, as an example of how semantic shifts affect Semitic zoonymy, that for the Semitic lexical type or Hebrew *par* 'young bull',¹⁶⁷ which in Militarev and Kogan (2005) is discussed at No. 181, on pp. 239–242, there are languages such as Syriac, Mandaic, and the Neo-Aramaic dialect of Hertevin, and perhaps also Hatran Aramaic, in which the sense of the respective cognate is 'lamb'. Moreover, the cognates in Arabic denote the young of sheep, goats, or water buffalos (*ibid.*: 241). “Note Arb. [Arabic] *fūr-* (pl.) ‘chamois’” (*ibid.*: 241). Militarev and Kogan (2005: 242) pointed out: “All the forms adduced above denote domestic animals (with the exception of buffalo which may be wild as well). At the same time, cf. [in the W[est] Chad[ic vernacular] Angas *fīr* ‘roan antelope’ [...], C[entral] Chad[ic vernacular] Hona *ʔuf^wara* ‘duiker’ [...].”

7.3.4. Any Relation to the Germanic Lexical Type of English *ram*?

Whereas early occurrence es in Eastern Semitic are in the form *rīmu* (whichever ruminant it denotes), in the context of Vennemann's method it is interesting he did not compare Semitic ⟨r'm⟩ to Germanic as in English *ram* [ræm] (Old English *ram*, *ramm*). Presently in standard German however, a ram (the animal) is called either a *Widder* or a *Schafbock*, whereas *Rammler* denotes 'buck'. English *ram* in its technical senses however is variously denoted in German by *Ramme* (f.), *Rammbär* (m.), or *Rammbock* (m.).

Did an early form of (wild) sheep exist in Europe before the Neolithic arrival of domestic sheep? Upon the evidence of the Hebrew

¹⁶⁷ But Militarev and Kogan (2005: 240, item [4]) have pointed out that according to Péter (1975: 487–492), “the widespread interpretation of *p.* as ‘young bull’ (contrasting with *šōr* ‘adult bull’) is erroneous: *pār* and *pārā* are used whenever it is necessary to stress the sex distinction rather than the age of the large cattle” (Militarev and Kogan's formulation).

Bible, the salient features of the animal denoted by ⟨r'm⟩ are its horns and its strength. Now, these are also salient features of rams, because of their habit of ramming into some target (English *to ram* 'to butt or strike like a ram', 'to drive or force by heavy blows',¹⁶⁸ Middle English *rammen*, German, from Middle High German, *rammen* 'to ram, to batter, to drive in'). Cf. the sense 'battering ram'¹⁶⁹ of English *ram*. Note however that the *OED* (1989, Vol. 13: 151) has a second lexeme for the verb *ram*, one that is indicated as both obsolete and rare; it is a transitive verb, defined as "To leap (the ewe)".

There is a tentative hypothesis (by Klein) that connects forms of the zoonyms such as English *ram*, Old English *ramm*, and Middle Low German, Middle Dutch, Dutch, and Old High German *ram* to Old Norse *rammr* 'strong', and Old Church Slavonic *ramenu* 'impetuous, violent'.¹⁷⁰

From Middle English *ram*, *rom*, *ramme*, from Old English *ramm* ("ram"), from Proto-Germanic **rammaz* ("ram"), possibly from Proto-Germanic **rammaz* ("strong"). Cognate with Saterland Frisian *Rom* ("ram"), Dutch *ram* ("a male sheep"), German *Ramm*, *Ramme* ("ram"). Possibly akin also to Danish *ram* ("sharp; acrid; rank"), Swedish *ram* ("strong; perfect"), Faroese *ramur* ("strong; competent"), Icelandic *rammur* ("strong; sturdy").¹⁷¹

In the second edition of Cleary and Vigfusson's (1957, repr. 1962) *An Icelandic-English Dictionary* (its first edition appeared in 1874), on p. 482 there is this entry (their brackets, but our bracketed ellipsis):

RAMR, adj., röm, ramt; **rammr** is a less correct form, [...] mod. Usage distinguishes between ramr, *strong*, and rammr, *bitter*, whence remma, *bitterness*: [North E. *ram*]:-*strong, stark, mighty*, of bodily strength, [...]

¹⁶⁸ But also metaphorically, in the sense 'to push firmly', as in *to ram a bill through the Senate*, or then in the sense 'to force (a charge) into a firearm, as with a ramrod' (www.dictionary.com/browse/ram).

¹⁶⁹ Other metaphorical senses of the English noun *ram* include 'warship intended to sink other ships by ramming them' (this is a *naval ram*), 'piston powered by hydraulic pressure', and 'weight which strikes a blow, in a ramming device such as a pile driver, a steam hammer, a stamp mill' (<https://en.wiktionary.org/wiki/ram>).

¹⁷⁰ <http://www.etymonline.com/index.php?term=ram>

¹⁷¹ <https://en.wiktionary.org/wiki/ram>

(But in Old and Modern Icelandic, “**RÁMR**, adj., hoarse”). Cf. *ibid.* on p. 482, “**ram-görr**, adj. *strong-built*”; “**ram-efidr**, part. *powerfully strong*”, and on p. 819, “**ramm-gjörðr**, adj. = ***ram-görr**”.

7.4. Dog

In that same endnote of his, Vennemann (106, note 26), having mentioned Old Irish *elit* ‘hind’, turns English and Old English *hind*, which is related to Old High German *hintā*. This leads Vennemann to remark that (in order to be able to relate them to a *-t-* suffix) English *hound*, German *Hund* (from Proto-Germanic ⁺*hundaz*) “would require the assumption of a masculine back-formation¹⁷² but is otherwise especially interesting because the root itself is likely to be of Afro-Asiatic origin” (106, note 26) on the evidence of **kan-* ‘dog’ in East Chadic and Omotic. **kun-* ‘dog’ in Berber and Omotic, and **kūHen-* ‘dog’ in West Chadic, Mogogodo, and Omotic (citing Orel and Stolbova 1995, nos. 1425, 1498, and 1511 in that order). Vennemann (*ibid.*) points out that as these are claimed by Orel and Stolbova to be alloroots forms of one and the same root), this “may throw new light on the unexplained Indo-European allomorphy in Lat. *canis*, Gk. Κύων/κυνός, OI [i.e., Old Icelandic] *śuvá/śúnas* etc.”

Arguably this is a throwback to early anthropisation, considering the exceeding antiquity, as understood at present, the domestication of the dog, and the earlier association of dogs with human settlements. There is no need for accepting Nostratic; Aharon Dolgopolsky, s.v. **ḵun|ḥV(ṛV)* (1998: 50, §52) – whose data are from Altaic, Balto-Slavic, South Cushitic, Chadic, as well as Arabic *qndr* for ‘beaver’ – has a definition that is a conflation: “small carnivore (marten, polecat, wild cat, or sim.)” but tacitly excluding ‘dog’, which does not appear in Dolgopolsky (1998).

The widespread Semitic lexical type /*kalb*/ ‘dog’ has been related to “Afras. **k^wVL-* ‘wolf, dog’ (pointing to the suffixal origin of *-b* in Semitic)” (Militarev and Kogan 2005: 158). Perhaps, I reckon, an alternation of the voiced dental lateral *l* and the voiced dental nasal *n* is

¹⁷² We have already considered a masculine back-formation in a Hebrew *Fremdwort* neologised in Moroccan Judaeo-Arabic, [‘šifah] from [šif’ha] ‘female slave’.

what separates the African instances of *kul-* ‘wolf’ from **kun-* ‘dog’ in Berber and Omotic. In Ayr Berber, *ǎ-kûlen* denotes ‘lycaon’; it is etymologised (*ibid.*) from **ku(H)l-Vn-*. In Central Chadic, one comes across *kûlem* ‘hyena’ in Gudu, *kila* ‘dog’ in Bura-Pela, *kàlè* ‘dog’ in Logone, and *keli* ‘dog’ in Buduma. In Cushitic, **ta-k^wVI-* denotes ‘wolf’ (where *ta-* is a preformative), e.g. Kemant *takwila* ‘wolf’, Baja *ták^wla* ‘Wolf; *Lycaon pictus*’ (Militarev and Kogan 2005: 158). Dogopolsky (2004, pp. 426–427) related what to him is Semitic **kalab-* to forms from Berber and Bura, but some further forms he listed are criticised by Militarev and Kogan (2005: 158). Item 1521 in Orel and Stolbova (1995) is Hamito-Semitic **kVwVI-/kVyVI-* ‘dog, wolf’; they claimed: “Sem. **kalb-* may also belong here [...] with the suffix of harmful animals **-b*”. It is dubious that there has ever been such a suffix.

In *Germania Semitica*, Vennemann (518) lists some as yet unsolved problems, and among the other things, there is this item in the list (when brackets are not my ellipsis, which is when they enclose dots, [...], I make comments in brackets):

Why do we sometimes find labialization on shifted Germanic velars that are plain velars in Semitic? Cf. **k^w* in *Quelle* (if from Semit. *g-l-l* as in Hebrew *gal* ‘source, spring’ [*recte*: ‘wave’, but other senses as ‘heap’ and the *hapax* lexeme ‘turtle’],¹⁷³

¹⁷³ Probably the archisememe of the Hebrew root *g-l-l* is ‘to roll’. Waves appear to roll. As to a heap of stones, somebody rolled them into to make that heap. In Mishnaic Hebrew, the stone rolled up to the opening of a burial cave is called *gōlāl*. As for the zoonym ‘turtles’, that sense of the plural *gallīm* is likely to elude by far most present-day Hebrew speakers. The plural masculine noun *gallīm* (the singular being *gal*) denotes in Hebrew ‘waves’ or ‘heaps’ (usually of stones), but there also is a third lexeme, as a name for ‘tortoise’, which only occurs at *Hosea* 12:12 in the Hebrew Bible. The prophet Hosea is referring to ineffective sacrifices. The second hemistich states: “Also their altars [are] like *gallīm* on the furrows of a field.” Both the usual Jewish exegesis, and Christian exegesis as being based on the Vulgate, understood *gallīm* as though in this context, too, it meant ‘heaps’ (as ‘waves’ would make no sense in the landlocked locale: the Gilead region of northern Transjordan, and Gilgal, a place name that puns with *gal*. Onomastic puns are not infrequent in the Hebrew Bible). The Septuagint alone understood *gallīm* as denoting ‘tortoises’, and rendered the word into Greek accordingly. In fact, both Arabic and Syriac have lexical cognates that mean ‘tortoise’. Moreover, there exist contexts in late antique as well as medieval Hebrew texts, such that *gallīm* denoting ‘turtles’: in his last book, the Israeli zoologist Menahem Dor (1997: 168) claimed cogently that *gallīm* in the midrashic text *Sifra* at *Shemini*, 4:3, “The *gallīm* and the frogs that grow in the sea

Assyr. *gille* ‘wave, flood’, *gillu* ‘wave’, etc., cf. [...] Vennemann 1995: § 7.11); ⁺*h*^w in PGmc. ⁺*h*^w*elp-* ‘young dog’, MHG *welf(e)* m./n., OHG *welf* n., OS OE *hwelp*, ON *hvelpr*, also in Irish, Lithuanian, and Greek (if from Semitic *klb* ‘dog’ as in Akkad. *kalbu*, South Arabian *kalb*, Arab. *kalbun*, Aram. *kalbā*, Hebr. *kéleb*, Phoenic. *klb*, cf. references in Vennemann 1995: § 7.6, [...])

Perhaps it was a linguistic feature of some Neolithic farming group that had reached ancient *Germania*, and in whose Northwest Semitic vernacular there was such a phonetic feature; or then, if they had taken up some other vernacular on their way from the Balkans and through what is now Hungary, perhaps some phonetic feature intervened on their sublexicon of Semitic stock. In fact, I am familiar with a dialect that velarises consonants (but with bilabials, it labialises them: *p* > *p*^w and *m* > *m*^w), in borrowed lexicon, but only if it is from a particular set of source languages.¹⁷⁴

and grow on land” denotes turtle species that live in water. In a gloss by Rashi (b. Troyes, Champagne, 1040 – d. Worms, 1105), “which are found dry”, Dor saw tortoises.

¹⁷⁴ Consider that a feature of the Baghdadi Judaeo-Arabic (BJA) dialect (cf. Blanc 1964 on the communal dialects of Baghdad), is that velarisation affects the syllables in loanwords (other than from Hebrew, Aramaic, other varieties of Arabic, Kurdish, Persian, Turkish, and Indian vocabulary), especially in European loanwords (and sometimes in Indian vocabulary, e.g. *čarpāya* or *čarpāyī*, ‘chassis of a bed’). On occasion, BJA treats that way a loanword from Hebrew, but there is an explanation for that: the *m* in Arabic *māy* ‘water’ influenced the Iraqi liturgical pronunciation of Hebrew *máyim* ‘water’ as *māyim*, and of Hebrew *šāmáyim* ‘sky’ as *šāmāyim*, but the latter yielded as a *Fremdwort* in BJA the dialect’s standard word for ‘sky’, which is *šāmāyām* (in both senses ‘sky’ and, as a religious concept, ‘Heaven’ not in the sense of Paradise, but as a reference to the Divine). Consider in contrast standard Arabic and formal Iraqi Arabic *samāʔ* ‘sky’, as well as in BJA *sāma* ‘sky’ but only inside the compound *bāʔbāʔ mān-əs-sāmæ*, i.e., literally, “cookies of ‘manna of heaven (the sky)’”, i.e., nougat with pistachio and almond filling, using many egg whites (Another name for ‘nougat’ is *bāʔbāʔ qādræsī*). A relative born in the 1850s and who died in 1943, while on visit in the 1930s or early 1940s to the flat into which one of her daughters had moved (from a detached house on the River Tigris), indicated she would not stay for the night by stating: “I want to see the *šaMāyām!*” (*ʔəʔəyānū ššəməyām!*). This is because in a sense there was no way the open air could be part of the domestic space, such as staying in an internal courtyard (in the usual traditional courtyard houses) or, in the case at hand, from the long balcony just above the river’s water, which is where that daughter had been living until then. (In comparison to Semitic, consider that in Hausa *sama* denotes ‘above’, and that in Logone *sama* denotes ‘rain’: see Greenberg (1950a), on p. 61. The Logone River is a tributary from southeast of Lake Chad.)

In his harsh review¹⁷⁵ of Orel and Stolbova (1995), Alan Kaye (1997a) – a scholar worth paying heed to – warned as well as conceded (*ibid.*, pp. 366–367):

As an example of European loanwords undergoing velarisation (and this is what especially matters here), it is interesting that such terms that appear to be loanwords into Baghdadi Judaeo-Arabic from some variety of Italian or from the *lingua franca* of the Mediterranean (thus, earlier than the onslaught of French and English), sometimes appear to come specifically from Venetian, or at any rate, they appear to be close to a Venetian form of the given term. The difference is usually the occurrence in the Baghdadi Judaeo-Arabic (BJA) term of the velarised version of a consonant, where in the European original term that consonant is not velarised. This is because, as already said, Baghdadi Judaeo-Arabic has a penchant for velarising some consonant in European loanwords (and also in Turkish and Indian loanwords). Arguably the clearest example of Venetian lexical influence can be found in the Baghdadi Judaeo-Arabic name for ‘vest’ (the top piece of underwear), which is *fānēla* (thus, with a velarised *f* and a velarised *n*). The BJA plural is *fānīlāt*. See Appendix C. In the Italian dialect of Venice, *fanèla* denotes ‘rough woollen cloth’. Giuseppe Boerio’s 802-page *Dizionario del dialetto veneziano*, in its second edition, which is of 1829 (I didn’t see the third edition, of 1867), has this entry: “FANÈLA, s. f. *Albagio*, Panno lano grossolano.” The Venetian term, like the Baghdadi Judaeo-Arabic term, is a feminine noun. By contrast, the standard Italian name for ‘vest’ (the top piece of underwear) is the feminine noun *canottiera* (literally, a vest one wears inside a *canotto*, i.e., a boat). Also the BJA feminine noun *lāmpa* for ‘lamp’ contains a velarised consonant (it is the *p*), and whereas the standard Italian name for ‘lamp’ is *lāmpada*, in the Veneto (i.e., Venetia) region of Italy it is *lāmpa*.

As for the lexicographer Giuseppe Boerio (b. Lendinara in the estuary of the River Po, 1754, d. Venice, 1832), by profession he was an official of the Republic of Venice, a magistrate, and a jurist (under Venetian, then Austrian, then Napoleonic, and then again Austrian rule). In a juridical handbook, he approved of the Austrian abolition of the defence lawyer, and such reactionary views attracted criticism. For his dictionary of Venetian, Boerio has several collaborators, among the others three natural scientists (N. Contarini for fish names, G. D. Nardo for bird names, S. A. Renier for the rest of natural science). Another collaborator was Daniele Manin, who was the publisher as well, and was to become famous in Europe as the leader of the 1848 insurrection in Venice, from where he went into exile in Paris. The first edition of the dictionary was published in instalments in 1827–1829. In 1821, Boerio had tried to have it published by the Ateneo Veneto, but it was only in 1826 that he was able to sign a contract with Manin. A posthumous updated edition appeared in 1856, and it comprised in addition the Italian-Venetian part, which Boerio had prepared but not published (De Michelis 1969).

¹⁷⁵ Kaye (1997) concluded by stating: “Although it has been said many times before, the following still needs reiterating: in comparative linguistics it seems that anything goes, because there are no sanctions on poor or unreasonable comparisons”.

Of course, what is reasonable for one linguist might be unreasonable for another. I have little problem, e.g., in accepting the idea of PAA [i.e., Proto-Afro-Asiatic] **kan-* ‘dog’ (#1425: 311) being related to PAA **kun-* ‘dog’ (#1498: 327), since even proto-type [sic] languages can be assumed to have had dialects, just like present-day languages (this is but one reason why I prefer the term ‘proto-type’ language over the more traditional but inadequate designation ‘protolanguage’). This may be deemed the principle of reasonable cognation. However, no matter how tempting it would be to relate, say, Latin *canis* ‘dog’ (= **kan-*) as a cognate in a larger phylum (e.g., PAA-Indo-European), this would, in my opinion, be pseudo-science in terms of today’s comparative method and its limitations (the possibility of borrowing is yet another matter entirely). The lexeme *canis* is happily, in fact, not utilized by Saul Levin in his *Semitic and Indo-European: the principal etymologies with observations on Afro-Asiatic* (Amsterdam: John benjamins, 1995), since – like Malay *mata* ‘eye’ and Modern Greek *mati* ‘eye’ – they are not cognates. After all, it is well-known that Persian *bad* ‘bad’ is not cognate with English *bad*, even though Persian and English are both Indo-European languages. [...] ¹⁷⁶

In fact, being able to point out *kana* ‘dog’ in Wollamo, a West Cushitic language, is nothing to enthuse about in relation to Latin *canis*. Consider indeed the context of the term within closer languages, in Greenberg’s (1950a) discussion of Hamito-Semitic: “dog: Chad–Hausa (1) *kare*; Klesem (2) *kere*; Logone (2) *kəle*; Sukur (3) *kirra*; Muturuwa (4) *kirri*; Gidder (5) *kra*; Mandara (6) *kare* Cushite–Wollamo (W) *kana*; Kule (E) *karo-*; Saho (E) *kare*” (*ibid.*: 60). ¹⁷⁷

¹⁷⁶ For that matter, take the Yiddish male first name *Vélvele*. I may legitimately wonder whether it is correlated (through Germanic) to the Tuscan family name, *Gualguaglini* (I found it in Pisa). I would not be reasonable instead to suggest an etymological connection to *walwalla-* being the Hittite name for ‘lion’. “3.72 – LION – *walwalla-* is inferred from the collocation of ^{LÚ.MEŠ}*walwalla-* and ^{LÚ.MEŠ}UR.MAH ‘lion-men’. It may be from the same ultimate source as Gk. λέ(φ)ων, thus *(*wa*)-*lwa-lla-*; Puhvel ([1984–] s.v.) also suggests verbal origin in PIE **wel-w-* ‘steal’, comparing semantically ON *vargr* ‘wolf’ < ‘robber’ (< ‘strangler’)” (Weeks 1985: 28).

¹⁷⁷ Nile Nubian *go:s* ‘throat’ is related to other East Sudanic names for ‘throat’ (Greenberg 1950b: 160), but is related to neither French *gosier* ‘throat’, nor Italian *gozzo* ‘goitre’. Shilluk *ober* ‘wing’ and Kenuz and Dongola *abir* ‘wing’ (*ibid.*) is quite probably unrelated to Biblical Hebrew /ebra/ for ‘wing’ and ‘feather’. It is even worse when the lexical semantics is not identical, and semantic shift is assumed. Take Masai *tua* ‘to die’, which outwardly resembles the unrelated French *tua* ‘[he/she] killed’. A comparison of Italian *dieci* ‘ten’ to Dinka *dyeč* ‘five’ (from Eastern Sudanic) appears even more preposterous. Also consider this tongue-in-cheek *demonstratio ad absurdum*: Hebrew *gulgólet*, Aramaic *gulgulta*, denote ‘skull’. This is commonly perceived to be motivated by the roundedness (root *g.l.l.*) of the skull. Now, “compare” it to *gol* ‘head’ in Modgel, a Chadic language from the banks of the River

7.5. *Goat*

Again in his list of unsolved problems on p. 518 in *Germania Semitica*, Vennemann asks this other question, concerning Germanic names for ‘goat’ (this time, the brackets are Vennemann’s own, except the ellipsis [...]):

The *goat* word only occurs in Italic and Germanic, where it allows a pre-Italic and pre-Germanic reconstruction ⁺*g^haid-*: Lat. *haedus* ‘young goat, kind’, *haedīnus* ‘of the kid’; Goth. *gait-s*, ON *geit*, OS *gēt*, OE *gāt*, OHG *geiz* [fem. *i*-stem] ‘goat’, Goth. *gaitein*, OHG *geizzīn*, OE *gāten* ‘kid’. The restriction to two neighboring branches of Indo-European puts it under *Lehnwortverdacht*. A word of similar form and meaning occurs in Semitic:¹⁷⁸ Akkad. *gadū*, Arab. *ḡady*, Hebr. *g³dī*,¹⁷⁹ Phoenic. *gd* ‘young goat, kid’ (cf. [...] Vennemann 1995: § 7.4, [...]). I therefore consider the *goat* word a Semitic loan-word. But this poses a problem: Why was Semitic *gdy* ‘young goat, kid’ represented as ⁺*g^haid-* rather than **gaid-* in pre-Italic and pre-Germanic? Was

Logone, the major tributary from southwest of Lake Chad. If we knew that football is a global cultural phenomenon, but were unable to state even approximately when it came to be so, perhaps somebody would propose that as a human head is round, and a ball is round, and what really matters about football is when people shout [‘go::!!], both Modgel *gol* ‘head’ and Hebrew *gulḡólet* instantiate metaphorical motivation of ‘head’ from ‘football’. It is nonsense, and we realise this only because we know better about cultural history: football, and more importantly, the shout *Goal!*, are not from the early Palaeolithic, when names for ‘head’ presumably began to be given. Now consider that Greenberg (1950a: 61) compared the Hebrerw and Aramaic terms to *kirkirta* ‘head’ in Bilin (a Central Cushitic language), to *kalkale* ‘head’ in Saho (an East Cushitic language), and to *a-kelkel* ‘brains’ in Tuareg (a Berber language); and that he compared the Modgel term to *ko* ‘head’ in Bolewa and Affade (also Chadic languages). For sure there is no relation to Italian dialectal *co* ‘head’.

¹⁷⁸ This seems quite likely indeed, and I am keen to accept that etymology. And yet, I sometimes balk at rapprochements, because of the high odds of coincidences. One can easily come up with wordplay which is not true etymology. Very close to the central western coast of the Dead Sea, there is the oasis and town of *Ein-Gedi* (‘water source of the kid’, known in Church Latin, following Flavius Josephus’ Greek, as *Engaddi*: there still is a wild ibex population on the hills near the town). For years I have been quipping that it would be splendid if this was the “etymon” (or toponomastic reapplication by nostalgic emigrants of old) of the name of Engadina, a region in Switzerland’s Canton Ticino... I have discussed such issues in Nissan (2012). And yet, are we not on the brink of such pitfalls, when setting forth two of more candidates for real etymology? Cf. Nissan (2014a, 2015). Nissan (2008) was much expanded into Nissan (2014a). Vennemann (1999) is concerned with folk-etymology.

¹⁷⁹ But the pausal form of Hebrew *g³dī* ‘goat’s kid’ is *gédī*. It is the pausal form we find in the toponym *Ein-Gedi*.

borrowing the *goat* word as **gaid-* avoided because of a late application of the root constraint barring two plain mediae from roots, *DVD?

That may have been the reason indeed. But consider that /g/ is one of the phonemes that in Northwest Semitic have two allophones. Whereas in Israeli Hebrew, the name for the third letter of the alphabet is *gimel*, as per Ashkenazi pronunciation, Iraqi Jews speaking in Baghdadi Judaeo-Arabic have still in living memory being referring to the two allophones of /g/ in Biblical Hebrew (and the Jewish Middle Aramaic of the *Babylonian Talmud*) as *ġīmāl wa-ġīmāl*.

It is moreover tantalising that in Arabic, the cognate for ‘young goat, kid’ is with the letter and phoneme *ghayn* (the voiced velar fricative), not *jīm* (an affricate [j] = [dʒ] as in English *job*, but in some dialects, a voiced velar stop as [g] in English *game*, or a voiced palato-alveolar fricative [ʒ] as in French *Jean*).

In his note 46 on p. 528 to his question “Why was Semitic *gdy* ‘young goat, kid’ represented as ⁺*g^haid-* rather than **gaid-* in pre-Italic and pre-Germanic?” (518), Vennemann states: “The same root consonants are found in the pre-Germanic root ⁺*digh-* or OHG *ziga*, G *Ziege* ‘goat’ and its relatives, possibly from a metathesized varian of ⁺*ghaid-* (cf. Walde/Hofmann 1982: s.v. *haedus*).

7.6. *Bull*

In Sec. 26.6.3.2.1 on p. 515, Vennemann deals with a “well-known example”, “the widespread *steer* word, which occurs without the movable *s-* in the other West Indo-European languages” – including Greek ταῦρος, Latin *taurus*, Middle Irish *tarb*, and so forth – “and also as a variant in Germanic”, this being represented by Old Icelandic Old Icelandic *Þjórr* ‘bull’ reconstructed from ⁺*Þeuraz*, as an example of the development ⁺*t^h* > *Þ/d*: “Semit. ⁺*t_haur-* (⁺*t* for the voiceless interdental fricative) was borrowed as ⁺*teur-* or ⁺*t^heur-* into pre-Germanic, as ⁺*taur-* into the other western Indo-European languages [...]; as late pre-Germanic ⁺*t^heur-* it was shifted to ⁺*Þeur-*, whence OIcel. *Þjórr*”.

Vennemann (527, note 41) discusses other etymological hypotheses for the *steer* word or the other names for ‘bull’ in relation to Semitic. I would like to signal Mario Alinei’s discussion in “Etimologie latine e neolatine” (1977), in whose first section he discussed the etymology of

the Latin verb *obtūrāre* ‘to plug’, ‘to seal’, and proposed that the etymon is the lexical type that persists in southern Italy and Corsica in the verbs *t(a)urare*, *t(a)urire*, *inturare* ‘to cause the bull to mount the cow’. Alinei proposed that this semantic motivation fits in the pattern discussed by Rohlf’s in his chapter “Sexuelle Tiermetaphern”.

Without endorsing the Nostratic hypothesis, I would like to signal a convenient list of parallels in Dolgopolsky (1998, entry 41, pp. 43–44). It includes terms from Semitic, Mycenaean, Greek, Albanian, Latin and Italic, Celtic, Slavic, Baltic, as well as Finnish (*tarvas* ‘reindeer’), and Germanic. For the latter, Dolgopolsky lists Old Norse *Þjórr*, Swedish *tjur*, and dialectal Dutch *deur* ‘bull’. Next, in the same entry, Dolgopolsky included a list of Indo-European parallels of the *steer* word with the initial *s-* from Avestan, Middle Persian, and Persian, as well as from Germanic languages: Gothic *stiur* ‘male calf, bull’, Old Norse *stiórr*, Old High German *stior*, German *Stier*, Anglo-Saxon *stēor* ‘bullock, steer’, and English *steer*. This is followed with a list of parallels from Altaic for kinds of cervids (different by species and age), but also for ‘ox’ and ‘castrated ox’.

These Germanic forms beginning by *st-* are perhaps unrelated to Chechen *stu* and Ingush *ust* ‘bull’, but it is worthwhile to signal their existence. Greppin (1991: 726) dealt with those two terms when discussing (*ibid.*, entry 13) Armenian *ult* ‘camel’ (first recorded in the Bible from the fifth century). The entry signalled Urartian *ultu* ‘camel’, and indicated the absence of a term for ‘camel’ in Proto-East-Caucasian (“PEC vacat”). In fn. 60, Greppin remarked: “The camel is not found in the mountainous area of Daghestan, and an early native Proto-East-Caucasian form would be surprising. It is almost sure, then, that the Urartian word is borrowed from elsewhere. It is probably related, somehow, to Akkadian *udru/uṭru/utru* (Diakonoff 1985: 600), Avestan *uštrō-* ‘camel’, Skt. [Sanskrit] *uṣtra-* ‘buffalo, camel’. Chechen *stu* and Ingush *ust* ‘bull’, would be possible Iranian loans”.

Concerning the relevant entry in Dolgopolsky (1998), the rival Nostraticist Allan Bomhard claimed (1999, pp. 57–58, item 41): “*č[a]w[V]rV (or * čurV) ‘bull, calf’: this etymology is plausible, though not without problems. First, Dolgopolsky is surely correct in seeing Proto-Indo-European *tauro-s as a borrowing”. As for the *steer* word, Bomhard claimed (*ibid.*): “It is difficult to reconstruct the Proto-Indo-European antecedent of the other Indo-European forms cited by

Dolgopolsky, though *steuros ‘steer’ or something very similar is probably the best that we can do. If this word is ancient in Indo-European and not a derivative of the root *tēu- ‘to swell’, as some have maintained, then Dolgopolsky’s etymology can be accepted. In my opinion, *steuros is not a derivative of *tēu-. I would reconstruct Proto-Nostratic *t^{yh}aw-r- ‘bull, steer’. The Altaic forms should be removed”.

Moreover, I would like to show how historically in a given cultural (and legal) context, confusion arose between the lexical type of Latin *taurus*, and Hebrew *tōrā* ‘Torah’, ‘Pentateuch’ (which is derived from the Hebrew root for ‘to teach’, with the *t-* being a preformative, not a radical, in the derivation). Within a discussion of how in the early modern period New Christians (who may have had very little knowledge associated with their ancestors’ Judaism) coped with being investigated by the Portuguese Inquisition, Rowland (2001, pp. 142–143) related the following:

In other cases we can almost observe the process of dilution and contamination of the original tradition. In several sixteenth-century trials, for instance, we find references to the Torah. In a trial of 1562, it is described as a roll of parchment that was shown to those who used to gather in the accused’s home and was then put away. In 1557, we are told of a room ‘where they worship the *Toura*.’ The use of the vernacular form *Toura*, which could also mean ‘cow’, paved the way for a semantic contamination. In 1634, in Évora, a twelve-year-old girl confessed that her mother [p. 143:] had taught her a prayer that was to be recited ‘to the golden calf.’ In 1609, in Covilhã, we find a silver statuette of a female calf (*bezerrinha de prata*) carefully preserved and transmitted from generation to generation by a New Christian family. In northeastern Brazil at the end of the sixteenth century, a clay statuette in the form of a cow is described to the incredulous Inquisitorial Visitor in an accusation as being ‘the *Toura* of the Jews.’ Some time later, still in Brazil, we find references to a ‘Confraternity of the *Toura*,’ organized and financed by New Christians.

In Nissan (2012, Sec. 34: 323) I quoted from Rowland’s study, and remarked as follows:

The Jewish sense of a Torah scroll had been replaced with a sense that out of ignorance some people inferred by derivation from a quasi-homophone in Portuguese. Semantic contamination, in this case, was semantic supersession of the original signified by a different sense sharing the signifier.

In Latin, *taurā* means ‘(sterile) cow’. But a feminine derivative of the Latin (and Romance) name for ‘bull’ is somewhat awkward, just like deriving the feminine in Semitic from the name *shor* (Hebrew), *tauro* (West Syriac) and their cognates, for ‘bull’. And yet, in Semitic, too, we find in Biblical Aramaic *twrh* (*tora*), and in early

Aramaic *šwrh*, for ‘cow’, and from Phoenician *Θουρώ* is known as a name for a god-goddess or divine cow. See a discussion of these Semitic and Indo-European terms in Levin (1995, pp. 15–17).

Bomhard (1981: 407) claimed a relation between such Semitic nouns as Hebrew *pārā* ‘cow’, *par* (and Akkadian *pūru*) ‘young bull’, and such Indo-European nouns as Greek *πόρις* and the like for ‘calf, heifer, young cow’, and Old English *ferr* ‘young bull’, which he derived from Indo-European **per-* ‘heifer, calf’.

7.7. Hoof

In Sec. 26.6.3.1.2., entitled “PGmc. ⁺*hōf-* ‘hoof’”, Vennemann (513) tentatively considers the Germanic names for ‘hoof’ – namely, *hōf* in Old English, Old Saxon, and Old Frisian; *huof* in Old High German; *hófr* in Old Norse – along “with Russ[ian] *kopyto* ‘hoof’ and OInd. [= Old Indic] *śaphá-* m. ‘hoof, claw’, Avest[an] *safa-* ‘horse’s hoof’” as being the result of lexical borrowing from Semitic ⁺*hupp-* such as in Arabic *huff-*. I am less than enthusiastic (just as a gut feeling; rationally, I am more open-minded). If this hypothesis is valid, then it would fit nicely with the spread of farming and domestic farm animals in the Neolithic.

Vennemann acknowledges that this “etymology involves several problems of detail” (513). The Russian word is derived by some scholars “from a proto-Slavic root **kopati* ‘dig, hit’” (526, note 37) “The problem of the centum treatment of a ⁺*k-* word in Slavic is thus bypassed, though at the cost of semantic fit. The OED mentions neither the Slavic nor the Indo-Iranian words” (ibid.). There is a silver lining: “If correct, this position would be most in harmony with my proposal: isolating a simplex word in one branch of Indo-European increases the probability of its being a non-Indo-European loan-word” (ibid.). Indeed.

Vennemann proposes a reasonable development to Proto-Germanic ⁺*hōf-* ‘hoof’ (apart from my considering the vernaculars of the early farmers to be adstratal, so I would not speak of “pre-Germanic” at the time of the lexical borrowing): “assuming that the foreign word was integrated with a long ⁺*ō* and with initial accent into pre-Germanic, ⁺*kōp-* (or ⁺*kōp-*), after becoming ⁺*k^hōp^h-*, would regularly shift to PGmc. ⁺*hōf-*” (513). If this etymology “is correct, this is a case of the

representation of Semit. *h* as Indo-European ${}^+k̂$, which in Germanic would regularly become ${}^+k^h$ and shift to ${}^+x$ ($> h$)” (517).

How widespread are co-derivatives within Semitic, even allowing for alloroots, and for an alternation between alloroots of the phonemes (or proto-phonemes) */h/* and */h'/?* (See on this below.) The Biblical Hebrew root *h.p.p.* is associated with the senses ‘to cover’ and ‘to protect’ (“tegere, protegere”, according to the Latin definition in Mandelkern’s concordance (1977 [1896]: 415, cols. 2–3). This suits the salient function of a hoof. In fact, there is post-biblical evidence (through the lexicon of Mishnaic Hebrew) that – in such situations when a hoof is no longer protecting, and being broken requires protection itself – farmers would sometimes provide a cow with a protective shoe, or that attempts would be made to remedy to a donkey’s hoof being damaged. A Biblical Hebrew alloroot of *h.p.p.* is the *tertia infirmae* root *h.p.(h)*. It, too, is associated with the sense ‘to cover’.

And yet, Hebrew only has the noun *parsā* for ‘hoof’. In Umbrian, *petur-pursus* is equivalent to Latin *quadrupedibus* (where Umbrian¹⁸⁰ *petur* ‘four’ = Latin *quatuor*). It is quite tempting to consider the Italic occurrence of *pursus* to be a Northwest Semitic loanword, *sed quaere*. It cannot be legitimately taken for granted, and caution is required.

The Hebrew root *p.r.s.* has several lexemes. A lexeme of verb *pārās* whose direct object is the *Shēma* prayer (“Hearken ye Israel”, the affirmation of monotheism) belongs to Mishnaic (Roman-age) Hebrew, and apparently denotes ‘to utter in public’, the etymon being Greek *παρρησία* (cf. the Mishnaic Hebrew verb *pīrsēm* ‘to publicise’, ‘to make public’). Clearly, this is a lexeme that came into being rather late. Also the Biblical Hebrew ethnonym and toponym *Pārās* ‘Persia’ is not relevant for the name for ‘hoof’.

In early rabbinic Hebrew, *parsā* as being a measure of length denotes a Persian parasang. A parasang was equal to nearly 6,300 metres, according to what is understood to have been meant by Herodotus, or to 5,940 metres according to Xenophon. In Modern Hebrew, *parsā* used to stand for a verst (Russian *versta*), a Russian measure of length equal to 3,500 feet (1,067 metres). Also note that in

¹⁸⁰ Umbrian here is an ancient Italic language, not the Umbrian dialect of Italian. Ancient Umbrian is the subject of “Umbro e Proto-Osco-Umbro” by Helmut Rix (1983).

the Ottoman Empire, a *fersah* was a measure of length equal to 5,685 metres. The native Hebrew noun *parsā* is a different lexeme from *parsā* as being a measure of length; the older lexeme denotes ‘hoof’: this is apt, as transport depended upon hooved animals, and when the Persian term was adapted into Hebrew, quite possibly there was lexical interference from *parsā* ‘hoof’.

The lexeme of Hebrew and Aramaic *p.r.s.* that in my opinion is relevant for the development of the sense ‘hoof’ of Hebrew *parsā* is the lexeme associated with ‘to split’, of the Mishnaic Hebrew verb *pārās* and the Aramaic verb *p³ras* ‘to split’.¹⁸¹ Hebrew *pārās* also denotes ‘to break’, the direct object being bread (already in *Isaiah* 58:7, and without naming the direct object explicitly, in *Jeremiah* 16:7, where the context is mourning, when sharing food with visitors was expected), hence the Modern Hebrew name for ‘slice’ [pru’sa].

In Mandelkern’s biblical concordance (1977 [1896]: 971, cols. 2–4), the Hebrew verb *pāram*, which takes cloth or garments as a direct object, is defined “lacerare, discindere”, but is now restricted to the sense ‘to unstitch’ (cf. Arabic *θārama* ‘to shred’), whereas the Hebrew verb *pāras* is defined “frangere (panem); habere divisam (ungulam); *ungulam habere*”. The latter, italicised sense is that of the verb in another conjugation, *hifrīs*, which takes *parsā* as its direct object. It can be said that according to the context, that verb denotes either ‘to have a hoof’, or ‘to have a cloven hoof’. In *Leviticus* 11:26, we are told about such a beast that (with the feminine participle) *mafréset parsā* (“has a hoof”) *w³-šésa’ e^vnénna šōsá’ at* (“and a cleavage she does not split”, i.e., “her hoof is not cleft”).¹⁸²

¹⁸¹ As a noun, Aramaic *p³ras* denotes ‘one half’. In early rabbinic Hebrew, *p³ras* became specialised for denoting a coin of half a mina, as well as for denoting half a loaf of bread, and in Modern Hebrew it denotes ‘prize’, probably by phono-semantic matching of the old term to the European term. This manner of nativising foreign terminology occurs in a number of modern languages, and was researched by Ghil’ad Zuckermann (2000, 2003).

¹⁸² In zoologists’ Israeli Hebrew, *Artiodactyla* (i.e., the even-toed ungulates) are called *makhpiléi-parsá* (literally, ‘doublers of hoof’) or *shesu’ei-parsá* (literally, ‘cleaved/cloven [in respect] of hoof’), whereas Biblical Hebrew *mafriséi-parsá* (literally, ‘ones “hooving” a hoof’) oscillates according to the context between the sense ‘ungulates’ in general and ‘artiodactyls’ in particular. In all three compounds, the compound is composed of a masculine plural participle in the construct state, and a feminine singular noun for ‘hoof’ in the absolute state. However, whereas *mafriséi-* and *makhpiléi-* each are a construct-state masculine plural active participle

I tentatively suggest that at a stage earlier than the records of Hebrew we possess, perhaps, just perhaps Proto-Hebrew did possess for ‘hoof’ a name derived from *h.p.p.*, even though I hesitate. At any rate, even though Biblical Hebrew expresses the sense ‘cloven hoof’ differently, it may be that originally *parsā* used to denote precisely that, and that eventually its sense became generalised to ‘hoof’, supplanting any other term that may have denoted the latter, more general sense. Hebrew also possessed the dual name *ʔlāfáyim* (the singular *télef* is only modern) for ‘cloven hoofs’.

When claiming a connection of the Hebrew root *h.p.p.* to Arabic *huff*, it is very important to bear in mind that the Hebrew grapheme ⟨ḥ⟩ used to convey two phonemes, /ḥ/ and /ħ/ (which are Semitic proto-phonemes which are both preserved in Arabic). In Hebrew, both /ḥ/ and /ħ/ still existed in Hellenistic and Roman times (even though the process was beginning, in some speech communities, perhaps especially ones that had shifted to Aramaic, of the gutturals becoming undermined, with [ḥ] > [h], and [ḥ], [h], [ʔ], and [ʕ] being sometimes exchanged).¹⁸³ The retention of the phoneme /ħ/ is inferred from some

in the inchoative aspect, by contrast *shesu'ei-* is a constructed masculine plural past participle. In contrast to *Artiodactyla*, the zoologists’ Israeli Hebrew name for *Perissodactyla* (odd-toed ungulates: equines and tapirs) is *mafritéi-parsá* after the single, or *odd* (Hebrew *péret*, Greek *περισσόος*), number of toes in the hoof.

¹⁸³ The effects of this are quite extreme in Samaritan Hebrew and Samaritan Aramaic. In his Hebrew travelogue, the medieval Jewish traveller Benjamin of Tudela (Tudela was in the Kingdom of Navarre, but he set on his voyage from Saragossa; he returned to Spain in 1173) referred to that pronunciation of the Samaritans (turning the gutturals into an *aleph*, a glottal stop) in an unflattering manner: for three consonants they do not pronounce, he respectively selected a Hebrew noun denoting a good quality and beginning by that consonant – the three consonants being *hod* ‘glory, splendour’ for [h], *hésed* ‘piety, charitable disposition, kindness’ for [ḥ], and *ʔnāvā* ‘humility’ for [ʕ] – and this enabled him to write that the Samaritans have no *hod* and so forth (the denotatum being taken to be a missing feature of their personality). Concerning Nablus, Benjamin of Tudela wrote: “It is the abode of about one hundred Cutheans, who observe the Mosaic law only, and are called Samaritans. They have priests, descendants of Aaron the priest, of blessed memory, whom they call Aaronim. [...] The Samaritans do not possess the three letters *He*, *Cheth*, and *Ain*; the *He* of the name of our father Abraham, and they have no glory; the *Cheth* of the name of our father Isaac [*Yiḥšāq*], in consequence of which they are devoid of piety; the *Ain* of the name of Jacob [*Yaʔqóv*], for they want humility. Instead of these letters, they always put an *Aleph* [the glottal stop], by which you may know that they are not of Jewish origin, because, in their knowledge of the law of Moses, they are

ancient transcriptions (see Sperber 1937/8 on these).¹⁸⁴ See especially Speiser's studies about the Second Column (the *Secunda*), where the Hebrew text is transcribed into the Greek script,¹⁸⁵ of synoptic Hebrew Bible of Origen (185–254 C.E.), the *Hexapla*: “The Pronunciation of Hebrew Based Chiefly on the Transliterations in the Hexapla” (Speiser 1932 [about the laryngals, continuing 1926b]). Speiser (1932, pp. 235–236) claimed that the weakening of the distinction between the two phonemes conveyed by the Hebrew letter ⟨ḥ⟩ was early:

That Hebrew did not succeed in preserving the laryngals in their original strength is an obvious and recognized fact. There is, however, a considerable difference of opinion regards the date and extent of the reduction. Our evidence for a pronounced weakening process is twofold. First there is the fact of considerable changes in vocalization made necessary in the majority of syllables containing laryngals. No less

deficient in three letters”. According to the biblical *Book of Kings*, the Cutheans were people brought into the region by the Assyrians after they deported the tribes of the Kingdom of Israel, and the Cutheans converted to the local religion while retaining at the time also their cults of origin. It is not unlikely that both this, and the Samaritans' own claims of descent, are true, as the residual population left behind after the deportation of the tribes from the fallen Kingdom of Israel would have merged with the incomers.

¹⁸⁴ It was the transcription of proper names into the Greek script in the *Septuagint* (in the Ptolemaic period), and their transcription in Jerome's *Vulgate* as well (late 4th century C.E.). The entire Hebrew text was transcribed into Greek script in the *Secunda* (the second column) of Origen's *Hexapla* (third century C.E.).

¹⁸⁵ “An examination of the available material of Origen's second column shows that while approximately half the words are written more or less in full, and may be read easily by a Greek, the other half are written defectively and would be perfectly useless to a Greek as an aid to reading the Hebrew text” (Staples 1939: 71), thus being not as accurate as the transcription of proper names in the *Septuagint*. Staples claimed his arguments ought to make one “thoroughly convinced that this second column was never created by a Greek, and that it was never intended originally for the use of a Greek-speaking student without a strong Jewish tradition behind him, for it contains a large percentage of words entirely lacking in secondary vowels and a few even without primary ones. The numbers of these are entirely too great to be due to copyist errors” (*ibid.*: 72). He suggested that the *Secunda* was originally a reading aid for Jews: “Many Jews found great difficulty in reading their Holy Writ in Hebrew characters, although they could still utter the sacred words. However, when supplied with a text in Greek letters, although only partly supplied with vowels, they had no difficulty in reading them. There would be a decided difference between a transliteration of Hebrew into Greek characters made by a Jew and one made by a Greek. [...] a transliteration prepared by a Jew would be largely mechanical” (*ibid.*).

conclusive is the eventual fusion of the two pairs of sounds represented by Arabic ¹⁸⁶ ح خ and ע ג into single ן and ץ sounds in Hebrew.¹⁸⁷

This coalescence may be looked upon as the first definite indication of actual reduction of the laryngals, (the term is here retained even though stricter phonetic terminology will assign ע and ח to the group of velars). Our terminus post quem is furnished by Egyptian transliterations of Canaanite names dating from the time of the 18th dynasty. In these records of the 14th century the four sounds are still clearly differentiated. It is likely that by the 10th century fusion had set in, to judge from the evidence of the [Pharaoh] Shishak list. To be sure, the Septuagint (and 'ΑΣΘ) occasionally transliterates ץ with γ and ן with χ; but this usage is sufficiently inconsistent and without real regard of etymological correspondences to warrant the assumption that the actual distinction had been lost by that time.

That is neither the prevalent view, nor one current. Joshua Blau in *On Polyphony*¹⁸⁸ in *Biblical Hebrew* (1982) and others before and after

¹⁸⁶ The Arabic letters ح خ represent [h] and [x] in that order. The Arabic letters ע ג represent the voiced pharyngeal voiced fricative [ʕ] and the velar fricative [χ] (the voiced equivalent of the voiceless [x]).

¹⁸⁷ Traditionally, in the pronunciation of Arabic-speaking Jews, the Hebrew letters single ן and ץ are respectively pronounced as the the voiceless [h] and the voiced [ʕ]pharyngeal voiced fricative. Concerning the latter, note that the letter ץ 'áyin is named after 'áyin 'eye', the initial of that noun being that very letter, i.e., according to a principle of acrophony (likewise, in Arabic 'ayn denotes both 'eye' and the letter of the alphabet that is the initial of that noun, but the Arabic letter ghayn instead, for [χ], is named simply by modifying the name 'ayn). The pictographic, acrophonic nature of the earliest Semitic alphabets is prominent in the Sinaitic alphabet, apparently an archetype of the Semitic alphabets discovered in an ancient Egyptian mine in the Sinai Peninsula: "16. 'Ayin. The word means 'eye', and it is under that form that we find it in the inscriptions; sometimes the eye is given with the pupil, at other times without it: once it appears with the eyelids. The eye is generally horizontal, but if space or some other reason requires, it is given vertically. On this point also there is agreement" (Butin 1928: 17). "Was the mine under operation when the tablets were written? Had it been closed, or had it not yet been opened? If we knew the exact position of this workshop we might venture an opinion, but the front of the mine has evidently crumbled down [...] Our tablets lie among fallen rocks, and they themselves may also have fallen from the cliff above the mine; if so, they may have been there long before the mine was in operation" (*ibid.*: 67, fn. 49).

¹⁸⁸ In his 400-page review of a 1000-page grammar of Ugaritic, Dennis Pardee of the University of Chicago (2003/2004) signalled in fn. 9: "For a detailed presentation of the data on non-standard phonetic correspondences across the Semitic languages, see the University of Chicago dissertation by Douglas L. Penney, *Towards a Prehistory of Biblical Hebrew Roots: Phoneme Constraint and Polymorphism* (1993)". This is potentially very useful for better understanding the set of constraints, when evaluating possible ancient loanwords from Semitic.

him have argued for a later retention of /ħ/ and [ɣ] in Hebrew, at the time of the Greek translations. Wevers (1970) defended that theory, but focuses on /ħ/, rather than [ɣ]. Ružička (1908) instead attacked the hypothesis, and focused on refuting it for [ɣ]. Blau (1982: 70) claimed that Hebrew lost [ɣ] before it lost /ħ/. Steiner (2005) agrees. But Steiner also proposes (*ibid.*, pp. 266–267):

The earliest evidence for **ħ > ħ* in spoken Hebrew and Western Aramaic comes from the Masada inscriptions (66–73 C.E.), the Kidron Valley dipinto (first half of the first century C.E.), and a lead weight from Gaza (26 C.E.). However, the merger may have taken place well before the first century C.E. Evidence for the retention of **ħ* in the spoken languages seems to peter out in ca. 100 B.C. The latter date suggests the possibility that the loss of **ħ* had something to do with the Hasmonean conquest of the Upper Galilee at the end of the second century B.C.E. Phoenician influence was very strong in that region; there were probably many speakers of Hebrew and Aramaic there who had merged **ħ* with *ħ* in imitation of Phoenician. Once these speakers came under Has-monean rule, the way was open for the innovation to spread gradually to Judea over the course of the following century. [...] The biblical reading tradition(s) was/were more conservative than the spoken languages. The transcriptions of Josephus and Aquila show that **ħ* did not disappear from that/those tradition(s) until the second century C.E., although signs of its decline are already apparent in the first century C.E. The preservation of **ħ*, without support from spoken Hebrew and Aramaic, is an impressive achievement of the proto-Masorettes. The successful transmission of the double realization of ך from one generation of readers to the next must have required long periods of training. Readers had to learn the correct values of ך by rote, verse by verse. Such training was clearly impossible during the war with Rome. It appears that when the last readers trained before the war died, the tradition died with them.

At any rate, as late as the Middle Ages, the Hebrew grapheme <ך> came to represent just one phoneme, not two. There was conflation, as though the grapheme stood for just one phoneme. There was however geovariation in how it was pronounced: [ħ] among Arabic-speaking Jews; [h] for a while in western Germany; [x] in eastern and southern German lands and Slavic lands; and so forth. (See what I have written about *Bnei hes* and *Bnei khes* in the section about the futhark.) Wevers (1970) discussed <ך> in Classical Hebrew. Currently Hebrew historical linguistics routinely reconstructs whether in antiquity, Hebrew <ך> stood, in individual terms or rather in families of co-derivatives, for /ħ/ or /h/, and this is done by analogy from cognates in other Semitic languages. In principle however, one needs to consider the possibility

that a root with /ḥ/ in some Semitic language(s), such as Arabic, may have corresponded to a semantically close alloroot with /ḥ/ in another Semitic language, such as Hebrew. These considerations are important for evaluating my hypothesis about Arabic *ḥuff*- having a possible relation to the Hebrew root whose spelling is <ḥ.p.p.>.

By the way, the Biblical Hebrew root *p.r.s.* also has a zoonymic lexeme: Biblical and Modern Hebrew *péres*¹⁸⁹ (spelled <prs>)¹⁹⁰ is the bearded vulture, the lammergeyer¹⁹¹ (*Gypaetus barbatus*).¹⁹² *Deuteronomy* 14:12 lists three day-raptor birds among unclean birds, and these are the *néšer* (i.e., the vulture, *Gyps fulvus*), the *péres*, and the *‘ozniyyá* (i.e., the lappet-faced vulture, *Torgos tracheliotus*). These are the three largest scavenging raptors found in the Land of Israel. Dor (1997: 95) pointed out that having confidently identified the other two birds,¹⁹³ by exclusion the *péres* must be the bearded vulture. Dor (*ibid.*)

¹⁸⁹ The Israeli Hebrew pronunciation is [‘peres]; such are traditional Hebrew pronunciations of this word, except in Ashkenazic Hebrew, where it is [‘peyres]. The Samaritan Hebrew *Hexateuch* (i.e., the *Pentateuch* with *Joshua*) has the word spelled <prs> (*péres* of the Masoretic Bible) appear with the determinative article, as <hprs>, and this word is pronounced in Samaritan liturgical Hebrew as *affērás* (Talshir 1981: 335). This Biblical zoonym is discussed by Talshir (1981) on pp. 242–245.

¹⁹⁰ But as a 20th-century family name, *Peres* is an adaptation of *Persky* apparently by attraction to the zoonym.

¹⁹¹ Also the Italian name, *avvoltoio degli agnelli*, for this raptor species explicitly associates the bird with lambs.

¹⁹² The *Vulgate* translates Hebrew *péres* with *gryps* ‘griffin’, thus adopting in Latin the Greek word which translates *péres* in the *Septuagint*.

¹⁹³ Dor (1997: 93) found evidence for *néšer* being the vulture, in *Micah* 1:16, where the female personification of the nation is announced future mourning (at the time involving shaving one’s head): “broaden thy baldness like the *néšer*”. The bearded vulture instead is not bald (Dor 1997: 95). As for the *‘ozniyyá*, Dor identified it with the lappet-faced vulture, by etymologising it (and its early rabbinic Hebrew synonym ‘oz) from Hebrew ‘oz ‘strength’ (the root is Ṣzz): the lappet-faced vulture is larger and stronger than the vulture, indeed than the other two species. Being the strongest scavenging raptor, both in Israel’s deserts, and in Africa this species eats first, when carrion is available, and other vultures wait and eat once it is sated. Cf. Militarev and Kogan (2005, no. 51: 71–72), about *‘ozniyyá* and its Semitic cognates. Their entry is weak on zoological identifications. Menachem Dor (1901–1998) was a zoologist; Yair Achituv (1999) discussed his contribution to the identification of biblical fauna. Dor (1997: 95) quoted a passage from the *Babylonian Talmud*, tractate *Hullin* 63b, in which Rabbi Yoḥanan (who, Dor remarks, was one of the few Sages who had a good knowledge of animals) extolled a hunter’s knowledge of animals in relation to zoonyms, over the bookish knowledge of a talmudic scholar, as the latter

etymologised *péres* from its breaking (thus, splitting) bones by letting them fall from the sky – this being a habit of this species – hence one of its Latin names, *ossifragus*, its Spanish name *quebrantahuesos*, and one of its Arabic names, *kāsir al-‘azām* (literally, ‘bones breaker’).

7.8. Boar or Male Pig, and Semitic Names for Young Ungulates

may know the zoonyms but not the proper identifications. Notwithstanding the value of Militarev and Kogan (2005), their entry 51 dealing with the ‘*ozniyyā*’ is one where one gets the feeling that they and their predecessors have at least sometimes not been overly reliable when glossing a term with a modern European zoonym as identification. Militarev and Kogan were able to exercise their excellent critical sense as linguists, but a zoologist’s considerations were not their cup of tea. Of course, a zoologist may get it wrong on linguists’ ground.

Both Dor (1997) and Militarev and Kogan (2005) criticised Aharoni’s etymology of ‘*ozniyyā*’ from Hebrew ‘*ēz*’ ‘goat’; accordingly, Aharoni identified the ‘*ozniyyā*’ with the bearded vulture. Israel Aharoni (1880–1946) was the zoologist who in the second quarter of the 20th century, yielded great influence on the evolution of which denotation was ascribed in Israeli Hebrew to Biblical Hebrew zoonyms, and unfortunately not infrequently got it wrong in terms of identifying the ancient denotation. The *nēšer* he identified as ‘eagle’ (*Aquila*). David Talshir (2012, Ch. B.5: 47–64) discusses how in the first half of the 20th century, because of a lexicographical misconception on the part of the zoologist Israel Aharoni, the traditional denotations of the Biblical Hebrew daily raptor names *nēšer* and ‘*āyīṭ*’ – respectively: ‘eagle (*Aquila*)’, and a generic collective name (for hovering raptors that then come down to feed) – were confusingly replaced in Israeli Hebrew with the word senses ‘vulture’ and ‘eagle’ in that order. Talshir remarks (*ibid.*: 61) that Aharoni’s only (and fallacious) reason for ascribing to ‘*āyīṭ*’ the sense ‘eagle (*Aquila*)’ was that he perceived it as sounding similarly to Greek *ἀετός* (*Aētos*). There is no historical etymological relation between the Hebrew and Greek terms, Talshir remarked, as the Greek term has an accepted Proto-Indo-European etymology (Talshir, *ibid.*: 60). The *Vulgate* rendered ‘*āyīṭ*’ with the general *aves* (thus considering it as a collective name for birds) or *volucris* (thus signalling their flying over the carcass). The general sense occurs in all Aramaic translations of the Hebrew Bible: Jewish, Christian, and Samaritan (but for the latter, see Talshir 1981: 330, 121–122; cf. *ibid.* on p. 320 the terms for ‘wing’).

When proposing identifications for biblical fauna, Aharoni was discretising the spectrum of faunal biodiversity in terms of Linnaean species, not in terms of folk-taxa. Generally speaking, this is Menachem Dor’s main critique of Israel Aharoni’s zoological identifications. Unlike his predecessor Aharoni, for generational reasons Dor (who lived to be one almost hundred, something significant because of the several generations he spanned in the development of the study of historical Jewish zoonymy) was quite aware that discrete taxa from biblical zoonymy cannot legitimately be mapped unquestioningly onto Linnaean taxa, because Linnaean systematics is not how traditional cultures understand the fauna.

In Sec. 26.6.3.1.4, Vennemann is concerned with Proto-Germanic ⁺*ebura-* m. ‘male pig’ (whence e.g. Old High German *ebur* and Old English *eofur*). Admittedly, he is not the first one to relate Indo-European cognates such as Latin *aper* ‘boar’ “to Semit. ‘-p-r ‘boar’ (Arab. *ifr* ‘boar, piglet’, Akkad. *appâru* ‘wild pig’)” (514). He is not fully committed to a Semitic etymology in this case, but at any rate he remarks: “If the old comparison [...] is correct [...], this would be another example of Verner’s Law, case ⁺*p* > ⁺*b*, applying to a Semitic loan-word” (514). Cf. Dolgopolsy (1998, entry 46 on pp. 46–47). For Dolgopolsky, the entry was part of the Nostratic lexicon. (Dolgopolsky was not averse to concede the possibility of lexical borrowing, rather than phylogenetic cognacy.)

I would just like to suggest that the outlier among the Semitic cognates, Hebrew *’ófēr*, which denotes ‘fawn’, is the outcome of semantic shift due to the reason that the culture did not consume pork, and therefore had little use for more than one term (and its compounds) in the semantic field of ‘pig’. Or then, this may have been a different specialisation of a more general term for ‘young of ungulate’. This is more likely to have been the case, based on the cognates listed in Militarev and Kogan’s (2005) *Semitic Etymological Dictionary, II: Animal Names*, which comprises two entries under the rubric “Pig” (*ibid.*, in the index on p. 412): nos. 110 and 111, namely, **hVnmVš* ‘piglet’ (absent from Hebrew), and **hV(n)zīr* ‘pig’. Moreover, there are six entries under the rubric “Young of Ungulates” (*ibid.*: 413), one of these being no. 88, **γ/’Vpr-* ‘young of ungulate’, which appears on pp. 128–129, where parallels from South Cushitic (e.g., *’eferet* ‘goat’ in the Asa language) and, less convincingly, West Chadic are also listed. Apart from the Hebrew term, which is defined ‘young fallow deer’, there are cognates from non-Jewish languages: Official Aramaic *pr* ‘young stag/gazelle’, and Arabic *γafr-*, *γufr-* ‘petit de chamois ou de chèvre, chevreau’ (‘kid of chamois or kid of goats’), *γifr-* ‘veau’ (‘calf’), as well as Arabic *ya’fūr-*, *yu’fūr-* ‘gazelle; petit de gazelle ou de biche’, (‘gazelle; young of gazelle or of deer’). Thus, there are semantic parallel for the Hebrew derivative.

Sheynin (2013, pp. 196–197) criticised Vennemann’s etymology (2003a, pp. 664–665) as follows: “V. notes that raising pigs was brought to the Northern Europe by megalithic Semitides”; “Curiously

enough, without any evidence, V. attributes also the IE word for wild pig *Ever/Eber* to Semitic etymon”; “If he wanted to exhibit sound *f* in this word, he had to chose [*sic*] rather examples from South Arabic or from African Semitic languages [Hamito-Semitic?] if this word is attested in them”; “It is known that boar hunting and pigs’ domestication in Northern Europe is known from the Neolithic period” (obviously hunting is pre-Neolithic!); “Atlantic people who arrived to Europe, according to V. in the post-Neolithic period, definitely didn’t introduce this breed to Europe. If they didn’t bring these animals to Europe, there was no need to name pigs and boars by Atlantic settlers. Also it is known that in historic times Semitic people didn’t like to include swine in their diet”. My response to this is that whereas the idea of “Semitidic” people in what were to become Germanic lands is misguided as per Vennemann’s postulates (themselves conditioned by his assumption that Indo-Europeans arrived late and brought farming), the occurrence of the likes of Latin *aper* in both Semitic languages and Indo-European languages is known in the scholarly literature. We cannot project back attitudes to pork from biblical and later times back to, say, the Natufians, co-territorial yet millennia apart. There is no need to assume that once Neolithic farmers who apparently came from the Levant to the Balkans and acculturated locally, the wave of farming that then spread to Pannonia and next to Germany held this or that attitude to swine or pork, and in particular that it equated Near Easterners’ attitudes. All it takes, is that they transmitted a technical vocabulary ultimately of Near Eastern origin. Did they transmit the *aper* word to Europe? Dolgopolsky simply took care of reconstructing a proto-form that would accommodate the lexical data from Semitic and Indo-European. Who is to say that a proto-form was not shared by ancestors of both language families by the time they had come out of Africa into the Levant? Actually, given Dolgopolsky’s ideas about early contacts in the Levant and Anatolia, this probably comes close to what he would have thought of the matter.

Concerning that same entry 46 from Dolgopolsky (1998, pp. 46–47), Starostin (1999: 146, item 46 in a table), noting in the second column that the distribution of the Nostratic root *ǂVpVrV ‘wild boar’ only includes Indo-European, made this comment in the third column: “HS [i.e., Hamito-Semitic]: only Arabic”. In the next column, Starostin (a specialist on Sino-Caucasian) offered as “Sino-Caucasian evidence”

(the title of the column) an Old Chinese reconstructed word for comparison, **prā* 'pig', but only tentatively so ("? Cf. OCh **prā* 'pig'").

In the following, I reproduce Dolgopolsky's (2008) entries for Nostratic roots denoting swine. Let us begin with the summary of entry 142 as summarised on p. 2565 in Dolgopolsky (2008):

142. (2?) * $\zeta \nabla \rho \nabla R \nabla$ 'wild boar' > IE **hepero-* > NaIE **ap(e)ro-s* 'wild boar'

The full entry 142 from p. 222 in Dolgopolsky (2008) is reproduced hereby:

142. (2?) * $\zeta \nabla \rho \nabla R \nabla$ 'wild boar' > HS: S * $\zeta \nabla \rho \nabla$ > Ar *ḡifr-* ~ *ḡufr* 'wild boar, swine, young pig' ({Fr.} *ḡifr-* 'porcus, aper', *ḡufr-* 'porcus') ¶ Fr. III 183 || ? ϕ Eg *ḡXX ḡpḡ* 'swine' ¶ EG I 69 || IE **hepero-* ({EI} **h₁eperos* 'boar [adult male of *Sus scrofa*]') > NaIE **ap(e)ro-s* 'wild boar' (with **a* on the analogy of **kaprō-s* 'he-goat') > L *aper*, -ī 'wild boar', Um *apruŋ*, APROF id. (accus. pl.), *abrunu* id. (accus. sg.) || pGmc **ebura-* 'wild boar' > AS *eofor*, MLG *ever*, OHG *ebur*, NHG *Eber* || BSl **weprya-* (with **w-* on the analogy of a different word) > Ltv *vepris* 'castrated boar' || Sl **veprь* ~ **veprь* 'wild boar' > OR **вспрь** *veprь*, R *вепрь*, gen. 'вепря, Bg 'вепър 'wild boar', Uk 'вепер 'wild boar, hog', SCr *věpra* (gen. *věpra*), P *wieprz* (gen. *wieprza*), Cz *vepř* 'hog' || ? σ Thr *έβρος* 'ram' ¶ P 323, EI 425, WH I 56, Ho. 92, KM 150-1, EWA II 941-3, Be. G 327, Kar. II 507, Glh. 666-7, Vs. I 292, Pln. II 724 || A possible highly questionable cognate: **п** **ep'ḡrḡ* 'horn' (if from 'fung' < 'wild boar's fung' < 'wild boar?') > M **eber-* 'horn' (> MM [HI, S, MA, IM] *eber*, WrM *eber*, HIM *эвэр*, Mnr *uyer*), pKo {S} **s-pír* id. (> MKo *spír*, NKo *pul*), Tg **oporḡ* 'nose' (> Nn *oporō* & *oforo*, WrM *oforo*) ¶¶ DQA #646 (A **ḡop'érḡ* → **ḡaplorḡ*), S AJ 46, 291, MED 286, SM 480, Ms. H 52, H 40, Pp. MA 150, 436, S QJ #43, Nam 274, MLC 832, STM II 22 ◇ AD NM #46; S CNM 13 and SN 146 (÷÷ OChn?).

The summary of entry 740 is on p.2592 in Dolgopolsky (2008):

740. 2 * $\Gamma \nabla R \nabla$ 'wild boar'

This entry includes data from Kartvelian and Cushitic, and appears in full in Dolgopolsky (2008: 715):

740. 2 *Γ∇R∇ 'wild boar' > K: GZ *ʔor- 'swine' > G ʔor-, Mg, Lz ʔeš- id. ¶ K 205, K² 232, FS K 351-2, FS E 394-5 || **HS:** EC: Af {PH} h́eray 'pigs' (snglt. h́eráytu 'pig'), {R} h́erāy, Sa {R} h́erā coll. 'wild boars' (snglt. -yṯā) Or {Brl.} eria 'wild boar': C b> Gz h́arāwya 'swine' ¶ R S II 19, PH 128, L G 244, Brl. 138.

And then again, this other entry, summarised on p. 3563 in Dolgopolsky (2008):

89. * 1 ʔ∇ 1 š 1 ú 1 H 2 ∇ 'wild boar' > IE: NaIE *sū-s, *su 1 w-0S '(wild or domesticated) pig'

appears in full in Dolgopolsky (2008: 170):

89. * 1 ʔ∇ 1 š 1 ú 1 H 2 ∇ 'wild boar' > K: pGZ *ešw- 'wild boar, swine' > OG ešw- 'wild boar', 'fang', G ešv- 'fang', Mg a-sk-u (< *0-askw-u) 'pigsty' (< *askw- 'swine'), ʔσ Lz čkva 'fang' ¶ K 81, K² 48, FS K 11, Abul. 152 || **IE:** NaIE *sū-s, *su 1 w-0S ({EI} *sū-s / gen. *s(u) 1 w-0S) '(wild or domesticated) pig' > Av hū 'swine' gen. sg. (< *huwō) || Gk ūc (gen. úóc) 'wild swine; pig' || Gk (< Pls?) ōŭc id. || pAl {O} *sū(s) > Al G/T thī {AlBED} 'boar' || L sū-s id., Um sim accus. 'swine', sif accus. pl. 'swines' || ON sý-r, OHG sū, NHG Sau, AS sū 'sow', NE sow || Ltv si vėĩns, su vėĩns 'small pig, sucking pig' || Tc B suwo 'pig, hog' (< *suw-on-) | d. IE *suw-īno- 'belonging to pigs' > L suīnus id. || pSl *svinъ > OCS, OR СВИНЪ svinъ id., dadj.: R свиной, Uk свиний id. | Pru seweynis 'pigsty' || Tc B swārie (in swāriana misa 'pork') | sunstantivized adj.: Gt swein, ON svín, OHG, AS swīn, NHG Schwein 'swine', NE swine, d. from adj.: Sl *svinbja 'swine' > OCS СВИННІА svinija, SCr svīnja, Sln svīnja, Cz sviňe, Slk sviňa, P świnia, R свињья, Uk свинья ¶ P 1038-9, EI 425, F II 824, 973-4, WH II 635-6, Bc. G 346, O 477, Ç II 219-220, 478, Kf. 366, AlbED 891, Ho. 329, 337-8, Vr. 570, 574, KM 628, 691, Kb. 982, Fs. 465, Wn. 446, Kar. II 189-90, En. 246, Vs. III 578-9, Glh. 599-600, Ad. 698 || **A:** Tg: Ud siu 'two-year-old boar' ¶ STM II 100 || **HS:** DEg iš 'swine, sow', Eg fMK {EG} ššy, šš 'swine', Eg L ššy.t ({Vc.} *[ešʔdyet]) 'sow', Cpt: Sd шє же 'swine', Sd/B ешш ешō, B ешш ешaw 'sow' ¶ EG IV 401, 405, Fk. 260, Er. 44, Vc. 49, 254 ¶ š (for the expected s) is puzzling (something like **-s 1 - or **-s 2 - > Eg š?) || The Ak word saḫû- 'swine' is considered a loan from Sumerian (Sd. 1133): is it a return loan of a (Hamito-)Semitic word? ◇ The initial i in DEg iš and the initial zero cns. in pGZ *ešw- suggests a N initial *ʔ-; in most descendant lgs. the tentatively reconstructed N *ʔ∇- was lost.

The Nostratic root of Latin *verrēs*, *verris* 'boar' appears in Dolgopolsky (2008) on pp. 2358–2359:

2530. *w^ri^rʒ^v 'young herbivorous mammal (calf, lamb, etc.)' > K: GZ *werʒ- 'ram' > OG, G verʒ-, Mg erʒ- id. ¶ K 84, FS 123 || IE: NalE *wersē-/ī- '(young) herbivorous mammal' > Lt ver̃šis 'calf', Ltv versis 'ox, cattle', Pru dim. werstian 'calf' || L verrēs /-is 'a boar' || Ir (× IE *wers- 'male'): OI v_oṛṣah_h 'bull', Av varəšni 'male, ram' || ? pTc {Ad.} cd. *keḡ-wārsān 'bull' (lit. 'bull-calf' or 'bull-male') (× IE *wers- 'male') > Tc: A kaḡurṣ, B kaḡurṣe 'bull' ¶ WP I 269, WH II 761, M K III 251-2, M E II 575-6, Frn. 1228-9. En. 273, Ad. 212, ≈ E 363 (*wersēn 'male [as sire]') || R *bīra(gu) 'young herbivorous mammal (calf, lamb, etc.)' (× N *b^rA^lʒ^vr^v 'ungulate') > T *bīra(gu) (or *bu^raga?) ({SDM} *bu^raga) 'calf' > OT, OOsM buzaḡu, MQP buzaḡu ~ buza^wu, [CC] buza^w 'calf', Chg buzaḡu ~ buzag ~ buza^w 'young of a cow\buffalo', Az buzo^v 'calf of the first 6 months of its life', Δ bizō 'two years old calf', Tk buzaḡi, Ggz buzā, Qmq buzaw, VTt bozay b^ozaḡ, Bsh b^oḡay, Qzq b^ozay b^uzaw, Qq, Nog bizaw ~ buza^w, Qrg muzō, Alt biza & bozu, Uz buzoq, ET mozay, Xk pizo, Shor piza, Tv bⁱzā, Chv L pāp^y p^rru (obl. pāpā^v- p^rr^v-), Chv Δ p^rr^v, Chv H p^rru (Chv {Md.} p^rru) 'calf' ¶ Rs. W 74-5, Cl. 391, Shch. Zh 100-1, ET B 239-42, TL 438-9, Jeg. 149, Fed. I 402-3, Md. 57, 175 (T *bu^raga). In my opinion, the original vw. of the first syll. is likely to have been *i, which in many T lgs. was labialized to *u by as. to the adjacent *b- || M *bīrayun 'calf' > MM [S] bura^u 'two-year-old calf', [MA] burū 'three-

year-old calf, WrM *biragu(n)*, HIM *бярү* *barū*, Kl *бүрү* *bürü*, {Rm.} *bürü*, Ord {Ms.} *bürü* 'calf of a second year', Brt *бүрү* *burū* 'calf of the first year', 'young bear\lynx\elk\deer (of the first year)', MMgl {Z} {Iw.} *buraū* 'two-year-old cow', *burayul* 'three- or 'four-year-old cow', Mnr M {Pot.} *piru* 'calf'; M *б* ? Yk *hōrōsku* ~ *hōrōsko* 'calf (after its first 4-5 months)' ¶ MED 106, H 22, Pp. MA 126, Chr. 114, KW 60, KRS 130, Iw. 93, SM 36, T 320; in some M lgs. the original vw. *i was labialized to u due to the infl. of b- || Tg **bjaru* (or ← M?) > Ewk: Np *bēru*, Tkm *bēranki* 'sheep', Ewk Olkm/Tng *borē* 'ram' ¶ STM 1 78 || pKo {S} **puruk* 'young bull' > MKo *puruk-so* (so 'cow'), NKO *purugi* ¶ S QK #1153, MLC 813 || pJ {S} **pítú-nsí* 'sheep' > OJ *píítúúí*, HJOJ [RJ] *pítúúí*, J: T *hìcuúí*, K *hícúúí*, Kg *hicúúí* ¶ S QJ #330, Mr. 411 ¶¶ SDM95 s.v. **bīṽíṽ* 'calf, lamb', SDM97 (A **bāf'í* id.), DQA #136 (A **bīṽíṽ* id.), Pp. VG 21, 60, 81, 131, 146-7, KW 60 || HS: S **√wrz* > Gz *warēzā* 'young' (of humans and animals), 'young man', Tgr *wāreza* 'young man', OAk *urāṣ-um*, Ak OB/MB/MA *urīṣ-um* 'billy-goat' ¶ LG 619, Sd. 1430-1 ¶ The Ak glottalization of the sibilant (ṣ for the expected z) is puzzling || EC: Rn {PG} *ōr* 'bull, male camel', HEC {Hd.} **warʔe* 'young female calf' > Sd *wā'r-iččū* (pl. *wā'dda*) 'female calf', Hd *wēʔ-iččū* 'young female calf', pl. *wā'la* 'young calves'; Sd *war-āmō* 'older male calf; ox, bull' ¶ PG 241, Hd. 35, 302-3, 400 ¶ The disappearance of the reflex of *z is still to be explained ◊ AD GD #40 (IE, K, A [T, M]). Here A **í-* presumably goes back to pre-Altaiic ***-rj-* < N **-r3-* (unless the N cluster was **-í3-*).

Also relevant for the vocabulary denoting swine is p. 2256 in an entry from Dolgopolsky (2008, pp. 2255–2257):

2423. **torʔ* ∇ 'fresh, new, young, young animal, child' > HS: WS **√trʔ* > Ar *ṭariʔ-* 'recent, fresh', *√trʔ* (pf. *ṭaruʔa*, ip. *ya-truʔ-u*) v. 'be fresh, juicy', Mh *ṭe'rayʔ* 'wet, damp, fresh', Hrs *təriʔ* 'fresh', Jb C/E *'te'riʔ* 'fresh (food)'; with the loss of root-final *ʔ: Ar *√trw/y* (pf. *ṭaruwa* ~ *ṭariya*) v. 'be quite fresh, freshly plucked', *ṭarīy-* 'fresh, recent', BHb **ṭā'rī* (attested: f. *הַרְיָ תְּאֵרִי* *ṭarī'yā*), MHb *רְיָ תְּאֵרִי* 'fresh', Ug *ṭry* 'fresh food', Sr *ṭarrūn-ā* 'recens', Gz *ṭarāy* 'raw, crude'; ? BHb *רְיָ תְּאֵרִי* *'təreṃ* 'not yet', 'noch nicht' (← *'earlier', cp. EpHb *bṭrm* 'before') ¶ HJ 430, KB 363, KBR 379, BDB #2961, A #1125, OLS 481, Fr. III 45, 54, BK II 65, 80, Ln. 1852, Hv. 428, 432, Br. 289, LG 598, Jo. M 411, Jo. J 279, Jo. H 130 || ? B **√trr* 'new' > Si {La.} *a-trār* (pl. *trār-ən*, f. *ta-trār-t*), Skn {Sarn.} *trīr* (pl. *trīr-ət*), Nf {CM} *a-trar* (pl. *ta-trar-ət*), Awj {Par.} *atrār* (pl. *trār ən*) 'new' ¶ La. S 163, 266, Sarn.

22, Prd. 170 || **K**: OG *ṭarig-i* 'lamb' (Joh. 1.36), G *ṭarig-i* 'yearling lamb, sacrifice lamb' ¶ Ser. 153, Chx. 1329 || **IE**: NaIE **torno-* 'young man, young animal', **t^her^hu-no-* 'young', **torm-/ *t^hrm-* id. (× NaIE **ter-*, *teru-* 'delicate, weak' < N **ṭar₁ṽ₁H^hū* 'delicate, thin', q.v.) > OI **taruṇa-* 'young, delicate, fresh', Av *tauruna-* 'young, boy', Oss I *tārən* ~ *tārən*, Oss D *tārna* 'boy'; OI *tarnā-*, *tarnāka-* 'calf, young animal' || Lt *tañnas* 'servant' (← *'young man') || Arm *թոռն* *t^hoṛn* (gen. *թոռնին* *t^hoṛin*) 'grandson', *թաքս* *t^haṣm* 'young, fresh, green' || pAl {O} **trima* > Al *trim* 'grown man; brave/valorous man, hero', † 'warrior' || Gt *παριης* (= ἄγρῶπος) 'ungewalkt, neu (vom Tuch)' (P: ← 'fresh') ¶ M E I 632, M K I 483-6, P 1070-1, Ab. III 280, Frn. 1060-1, O 464, Slr. 304-5, Fs. 490, ≠ EI 490 || **U**: FU (att. in BF) **tōre* 'fresh, raw' > F *tuore*? 'fresh', Es *toores* 'raw, crude; unripe, green', Lv *tuḡr^hz* 'green, raw' ¶ SK 1409-10, Kt. 441 || **A** {DQA} **t^hōr^hṽ* 'young animal, child' > T [1] NaT **t^hō₁ru₁* 'young', *t^hō₁or* 'calf' > Tk Δ {SDD} *toru* 'young' (of a man, tree), Slr *tōri* 'foal', Chg {RI} *tor* 'calf' | [2] NaT **t^hōrum* 'young camel' > OT *torum* id., MT [IM] *torum* 'suckling young camel', Tk *tōrun* '2-year-old camel', Tk Δ {SDD} *torum* 'young camel', Tkm *tōrum*, Tv *d₁orum* 'camel in its 2nd year'; T → M: WrM *torum*, HIM *топом* 'young camel in its 2nd year', WrO {Krg.} *torōm*, *torom*, Kl {KRS} *torām* 'a two-year-old camel' | [3] NaT **t^hō₁ru₁n* 'grandchild' > Osm {Rh.}, Tk *tōrun*, Kr *torun* ~ *tōrin* 'grandchild', Vt *turun* 'great-great-grandchild' | [4] NaT **t^hō₁ru₁rpak* 'calf in its 2nd year' > Chg [MA] *torpaq* {Pp.} 'three-year-old calf', {Shch.} *t^hō₁ru₁* 'calf in its 2nd year', Qzq *torpaq* 'yearling calf', Qrg *torpoq*, StAlt *torboq*, Xk *torbaχ* 'calf in its 2nd year', ET *to(r)paq* 'heifer in its 2nd year'; ds. (?): Brb *torboγiṣ* 'big calf', Yk *torbos* 'calf', ? Qrg *toropoy* 'young pig' | [5] NaT **t^hō₁ru₁y* 'child, young pig' > Qmq *toray* 'child', Tf *t^hōray* 'yearling bear', and possibly Qrg *toray* 'young wild pig', Qzq, Qq, Nog *toray* 'young pig' (Qzq/Qq/Nog/Qrg *toray* are likely to be influenced by or borrowed from M) ¶ SDD III 1345-6, Cl. 549, DTS 578, IS AD 42 [#29], Rs. W 491, Shch. Zh 102, 106-7, 125-6, Rl. III 1179-80, 1183, 1189-90, Rh. 607, TvR 174, Pp. MA 126, BT 154, BIG 233, KRPS 539, MM 350, KrkR 649, NogR 358, Tn. SJ 517, Pek. 2736, Ra. 235, MED 827, Krg. 509, KRS 508 || M **toruyi* 'young pig' > WrM *tōru₁i* 'suckling pig', HIM *топой* id., 'young yak', Oyr, Brt *tōrou* 'young pig', Kl {Rm.} *tōrā* 'young wild pig' ¶ KW 401, MED 827, Ra. 235 || ?σ NrTg **toro-kīū* 'boar' > Ewk

Urm/Ucr/Z *torokī*, Neg *torokī*; Tg *b* > Yk {Pek.} *toroku* ~ *toroχu* 'boar'. An alt. possibility is that the source lge. is Yk *b* > Ewk, Neg, but this is less plausible for two reasons: [1] Neg and Ewk Urm are spoken in regions outside any contact with Yk (namely, on the Middle and Low Amur and on the Amgun), while Yk has a strong Tg substratum, [2] Ewk, Neg *torokī* ~ *kī* are explainable within Tg as ds. with the sx. of animal names *-kī* (Ewk *tuksakī*, Neg *töksakī* 'hare') ¶ Vas. 761, Pek. 2741 ¶¶ Shch. Zh 125-6, Pek. 2741 ¶¶ DQA #2446 (A **t'ōr∇* 'young animal') ¶¶ The pA vw. length with an acute (> vw. length in T and shortness of the vw. in Tg) goes back to a compensatory lengthening caused by an additional element after the vw. or the following cns. within the N word || *D *ōtar* 'child' > Kn *taruvali* 'boy, girl', *tarale* 'girl'; the D word may have been influenced by OI *taruṇa* 'young, fresh, tender' ¶ cp. D #2817; *F* also M KI 483 ◇ D **a* still needs explaining ◇ The length of the vw. in T and FU is explained by complementary lengthening connected with the loss of N **ʔ*. It is possible that the pN reconstructions **tar₁∇₁H¹ū¹* 'delicate, thin' and **torʔ∇* 'fresh, new, young, young animal, child' represent the same pN etymon (if the difference between vowels of the first syll. can be explained away) ◇ Blz. SNE I 243 [#10] (equates S and FU with IE **ter-*).

7.9. Honey Bee

Pokorny (1959 at 116) and Devoto (1962: 868) have the Proto-Indo-European root **bhei-* for 'bee'. Basically, one would suppose, it is unnecessary to suppose that the lexical type arose in Atlantic Europe, especially as we find the lexical type *apis* 'bee' in Latin.¹⁹⁴ Vennemann

¹⁹⁴ Romance names for 'bee' include Occitan and Portuguese *abelha* (plural *abelhas*), Spanish *abeja* (plural *abejas*), Aragonese and Gallego *abella* (plural *abellas*), Asturian *abeya* (plural *abeyes*), Romanian *albină* (plural: *Albinele*). Also note Esperanto *abelo* (plural *abeloj*).

In Italian one finds *ape* (plural *api*), but also (from a diminutive *apicula*) *pecchia* (plural *pecchie*), whence the name *falco pecchiaiolo* for the insectivorous hawk-like raptor species *Pernis apivorus*, whose English name is [*European* or *Eurasian*] *Honey-buzzard*, about which Cocker and Mabey in *Birds Britannica* state (2005: 113): "Lesley Brown, the great scholar of the world's raptors, once noted that this scarce migrant is badly named, given that it is neither a buzzard (not even closely related to the buzzard genus *Buteo*), nor does it eat honey. The old name, 'bee hawk', was closer to the facts, since the bird is predominantly insectivorous and the shortness of its stay in Britain – it arrives as late as June and leaves just 12–16 weeks later – flows from a dependence upon the grubs and adults of wasps or bees". The current Welsh name for the same species is a semantic calque from the English flawed term: *bod y mêl*, which literally means 'honey buzzard' (*ibid.*: 478). In Portuguese, that bird

is concerned with Germanic names for ‘honey bee’ (518–519).¹⁹⁵ “The ⁺*bi-* word for the honey bee [...] occurs with an initial *b-* in all in all three Indo-European families that have the word, Germanic, Balto-Slavic,¹⁹⁶ and Celtic” (518).¹⁹⁷ Vennemann (1998b), “Germania Semitica: *Biene* und *Imme*:¹⁹⁸ mit einem Anhang zu lat. *apis*”, proposed

is denoted by synonyms that are compounds made up with names for respectively bees or wasps: *tartaranhão-apívoro* and *bútio-vespeiro*. The word formation of the name for the bird involves a name for wasps in languages such as German (*Wespenbussard*), Dutch (*Wespendief*), Catalan (*aligot vesper*), Asturian (*viésporu*), Israeli Hebrew (*ayyát tsra'im*), Lithuanian (*Vapsvaėdis*, from *vapsva*, ‘wasp’), and Esperanto (*Vespobuteo*, from *Vespo* ‘wasp’), but a name for bees in languages such as French (*bondrée apivore*), Spanish (*abejero*), Gallego (*miñato abelleiro*), Swedish (*bivråk*), and Faroese (*bývákur*).

¹⁹⁵ Such as German *Biene* (plural *Bienen*), Old High German *bīa*, Alemannisch *Biine*, in German dialects also *beie*, Middle Low German *bīe*, Low German *bigge*, Middle Dutch *bie*, standard Modern Dutch *bij* [‘bey], English and Scots *bee* (plural *bees*), Old English *bēo* (plural: *bēon*), Old Norse *bý*, Danish and Norwegian (in both the bokmål and nynorsk versions of the latter language) *bie* (plural *bier*), Icelandic *Býflugur*, Faroese *bý*. Cf. English *honey bee*, German *Honigbiene*.

¹⁹⁶ Latvian *bite*, plural *bites*; Lithuanian *bitis*, plural *bitės*; Old Prussian *bitte*. Also note Albanian *bleta*, which just satisfies the criterion of an initial *b-*.

¹⁹⁷ Outside Indo-European but inside Europe, Basque has the name *erle* for ‘bee’. Vennemann in his publications of the 1990s, up to his 2003 book, has been much concerned with Basque, and of course he can be assumed to have noticed that Basque does not contribute to clarifying the origins of the lexical type *bee*. Incidentally, in the perspective of Basque itself, it is notable that *erle* contains a consonantal cluster. In his posthumously published *Etymological Dictionary of Basque*, Larry Trask (d. 2004) wrote: “The existence in the historical language of such words as *ernai* ‘awake, alert’ and *erle* ‘bee’ suggests that a very few other clusters may have been possible in Pre-Basque, but these other clusters are so rare that I hesitate to assign them to Pre-Basque. These clusters may have arisen by phonological developments such as syncope, as is commonly thought to be the case in one or two other odd cases, such as modern *esne* ‘milk’” (Trask 2008: 18). In his entry for *erle*, he remarked, among the other things: “In med[ieval] Navarra, the sobriquet *Erlea* is well recorded, and *erle* is frequent as a first element in toponyms” (*ibid.*: 175). Of course, Basque *erle* is etymologically unrelated to Basque terms borrowed from Romance, including *erlijio* ‘religion’, *erlikia* ‘relic’, and *erloju* or *erloj* ‘clock, watch’, the latter “From Rom[ance] reflexes of Lat[in] *hōrologium* ‘clock’, such as Cast[ilian] *Reloj*” (*ibid.*). It is also unrelated to Basque *izerleka* ‘sticky sweat’; cf. *izerdi* or *izardi* ‘sweat’ (*ibid.*: 236). In the entry for *mustela* or *musterle*, for ‘weasel’ (*Mustela nivalis*), one reads: “The strange second form perhaps by contamination from *erle* ‘bee’” (*ibid.*: 293).

¹⁹⁸ *Imme* ‘bee’ is a feminine noun from the poetic register in German (plural *Immen*). Vennemann (21998b) also discussed Old High German *imbi* and Old English *ymbe*, which denote ‘Bienenfolk’, ‘[swarm of] bees’. Vennemann (21998b: 480)

that the Egyptian *bj-t* fem. ‘honey bee’ was “carried by Phoenicians to northwest Europe” (518 in the book under review).¹⁹⁹ This is far-fetched.²⁰⁰ Why wouldn’t they rather use a cognate of Hebrew *d̥bōrā* ‘honey bee’?²⁰¹ (There are close Semitic cognates in the varieties of

compared the first syllable to Proto-Semitic **amm-* ‘people, crowd’ and to South Cushitic **im-* in the same sense, and on p. 482, fn. 48, to East Chadic **im-* or **um-* ‘bee, honey’, and even to English *yum-yum!* and the adjective *yummy* for ‘delicious, delectable’.

¹⁹⁹ Vennemann was preceded in that hypothesis by Linus Brunner’s (1969) *Die gemensamen Wurzeln des semitischen und indogermanischen Wortschatzes: Versuch einer Etymologie*. By the way, Bomhard (1981) often compares Egyptian to Proto-Indo-European or early recorded Indo-European languages, and for example, he relates (*ibid.*: 437) the Egyptian zoonym *wt* “sheep and goats, animals, flocks” to Latin *ovis* ‘sheep’, Luwian *ḫa-a-ú-i-iš* ‘sheep’, Old English *ēow* ‘sheep’, and their several Indo-European cognates. On p. 425, Bomhard (1981) compares Indo-European occurrences from only Germanic (the *hair*-names) to the Semitic occurrences of *š.r./s.r.*, but that only makes sense if one considers (like Agmon 2010) early biconsonantal roots in Proto-Semitic, where a *š-* prefix could be eventually absorbed into a triconsonantal root. Sometimes Bomhard’s comparisons are quite far-fetched, but some other times, his examples are tantalising.

²⁰⁰ Vennemann (1998b) reasoned that Egypt was the first place where bees were domesticated.

²⁰¹ The Hebrew pronunciations of *d̥bōrā* for ‘bee’ and ‘Deborah’ vary: [dvo’ra] or [dvo’ya] is the Israeli Hebrew for ‘bee’ and ‘Deborah (the prophetess)’, but because of affective stress (frequent in Israeli Hebrew for first names), it is [‘dvora] or [‘dvoya] in order to refer to any other woman named after her. (The plural with the determinative article is *hadd̥bōrīm*.) Liturgical pronunciations of Hebrew *d̥bōrā* include for example Iraqi [Debo’ra] (among speakers of Judaeo-Arabic), [devo’ra] among Italian Jews (buit [devo’ya] is more likely among the Piedmontese), [‘dveyyu] among southeastern (Rumanian) Ashkenazic Jews (as Hebrew *ā* > Ashkenazic Hebrew *o* is for them *u*, whereas Hebrew *o* > Ashkenazic Hebrew *oy* is for them *ey*), and so forth.

The Ashkenazic pronunciations of Hebrew are not the same as the pronunciation by Yiddish speakers of the Hebrew component of Yiddish. Beider (2015, p.326) gives the “female name Deborah” from the Hebrew component of Yiddish as *Dvore* in Lithuanian Yiddish and Polish Yiddish (“both regionally”) as well as Czech Yiddish. He gives the form *Dvoyre* (this being the Standard Yiddish form) on pp. 300 and 329. On p. 300, Beider states: “The diphthongization of [o:] can be illustrated by such spellings of biblical names as *Двоўра* דְּבֹרָה ‘Deborah’, St[andard] Y[iddish] *Dvoyre*) in 1591 [...] in Volhynia”.

In sources from the 14th or 15th century, one finds the form *Twora*, as well as the Yiddish hypocoristic forms *Twerl* and *Tworl*, for ‘Deborah’ (Beider 2015: 329). In fn. 369 on p. 326, Beider states: “In the Białystok area and eastern Galicia, Dwora/Dwore (LitY *Dvore*, PolY *Dvori*) was three to four times more common in the

Aramaic and South Arabian vernaculars, and with some semantic change also in Arabic and Amharic. Moreover, along with the family of derivatives with an initial *d-* there is a family of derivatives with an initial *z-*, such as Syriac *zebbūrā* or *zenbūrā* ‘bee’, and Arabic *zanbūr*, plural *zanābīr*.)

Besides, it seems to me too simple to assume that navigators brought the term to North Europe’s littoral. Perhaps, unless it is a mere coincidence, we should also consider Latin *apis* ‘bee’, along with the Egyptian term, as a sister grouping of Semitic names for ‘bee’. We are not necessarily talking about domestic honey bees. If one is to concede any correlation, then those two correlated groupings were denoting wild honey bees, at the time the relevant vocabulary came into being.

In his critique of Vennemann (2003), Sheynin (2013: 198) stated:

The Lat. *apis* “bee” he explains after Brunner (1969) from Ancient Egyptian reduced form *af* from *ʃf* ([Vennemann 2003a,] pp. 713–714, 723, 727) and Indo-European root ⁺*bi-* or ^{*}*bhi-* by a different form *bj-t* ‘honey bee’. On the bases of combination of Sem. ⁺*HVm-* ‘Volk, people’ ⁺*bi-* he reconstructs [the] compound word *Imme/imbi* and its relatives in the West Germanic languages. If this reconstruction is

nineteenth century than the variants that correspond to local dialectal rules, namely, Dwejra (LitY *Dveyre*), and Swojra/Dwojre (PolY *Dvoyri*), respectively. In other regions of the whole area that between the two World Wars belonged to Poland, local forms of Deborah perfectly fit local Yiddish rules, for example, Dwejra in the Łomża area and Dwojra in Warsaw, Kalisz, Piotrków, and Lublin provinces. The CzY ^{*}*Dvore* can be deduced from its hypocorism *Dvor(e)l*”, and Beider indicates two forms of spelling in the Hebrew script of that hypocoristic form of the personal name, from tombstone inscriptions of Prague in the 17th and 18th centuries.

The Samaritan pronunciation of the Hebrew word <dbwrah> ‘bee’ is *dūbērā* in the singular. In the plural with the determinative article (Talshir 1981: 307) it is *addūbērām*. As explained by David Talshir (1981: 109–110), in the Samaritan version of the *Pentateuch* bees are mentioned twice: in *Numbers* 14:45, and in *Deuteronomy* 1:44. The MSS of the Samaritan Aramaic translation render the occurrence (in the plural) in *Deuteronomy* with <mlya> or <dbrya>, whereas in *Numbers*, some MSS of the Samaritan Aramaic translation render the occurrence (again in the plural) with <znbwrya> or <znbryn> (which resemble the Arabic term). A marginal note to *Numbers* 14:45 in MS M states <‘qrbwty[h]> (i.e., ‘scorpions’), side by side with <mlyh>, apparently as a co-hyponym of the lexical concept ‘stinging animals’ (Talshir 1981: 110).

Talshir remarked (1981: 202–203) that other scholars understood the Aramaic Samaritan translation according to its spelling <mlya> as Aramaic *millāyā* ‘words’, as though the Biblical Hebrew word (plural and with the definite article), <hdbrym>, was taken by the translator to be *hadd^hbārīm* ‘the utterances, the things said’, but Talshir instead interpreted <mlyh> as a loanword into Samaritan Aramaic from Greek *méli* and *mélissa* ‘bee’ (Talshir 1981: 203).

true, the question arises why only West Germanic languages retain this word. If the Afrasians migrated from Africa along [the] Atlantic coast northwards why these Imme-words didn't live [*recte* leave] traces in any of the languages spoken south and west from the areas of West Germanic tribes?

Actually Vennemann also listed Celtic (but for example in Welsh, the term is *gwenynen*). As for the form *af* claimed for ancient Egyptian, note (co)incidentally two forms from Italo-Romance: Friulan (Furlan) *âf*, whose plural is *afs*,²⁰² and Emilian and Romagnol *ev* for both the singular and the plural; *ali ev* 'the bees').²⁰³

Militarev and Kogan (2005) have a Proto-Semitic entry (no. 66, very long, on pp. 96–99), **di/ab(b)ūr-* 'bee, wasp' – note in particular, in the Arabic dialect of Oman, *dabiyy* 'bees', which "may suggest the suffixal origin for *-r* in the present root" (*ibid.*: 97) – and they also have another entry (no. 62), **bur-* 'kind of insect', with parallels in Berber (names for 'cricket' or 'locust'), Chadic, Cushitic, and Omotic (*ibid.*, pp. 88–89). "Afrasian insect names with the biconsonantal element **bVr-* exhibit a wide semantic variety ('locust', 'kind of flying insect', 'ant, termite'), probably implying a reconstruction of more than one proto-form" (*ibid.*: 89). In the entry for **di/ab(b)ūr-* 'bee, wasp',²⁰⁴ earlier authors are cited on p. 97, especially Diakonoff, suggesting that *-b* and *-r* may be treated as fossilized markers of harmful and useful animals" (*ibid.*) – too good to be true, I would say – and accordingly, Militarev and Kogan propose: "one could tentatively suppose that a biconsonantal nominal root **dVb-* 'fly' has been extended with *-b* and *-r* to produce independent terms for "harmful fly" and a "useful fly" (i.e. 'fly' and 'bee') respectively" (*ibid.*).²⁰⁵

²⁰² The drone (the male bee) is called *avon* in Friulan (<https://fur.wikipedia.org/wiki/Âf>): "Il mascli de âf si clame avon".

²⁰³ At <https://eml.wikipedia.org/wiki/Ev> there is a Wikipedia page in a stardadised form of the those dialects.

²⁰⁴ Arabic has *dabbūr* 'hornet'. Israeli Hebrew *dabbūr* 'hornet' is a loanword, but also a co-derivative of Hebrew *d'ḥōrā* /*dbora*/ [dvo'ra] 'bee'. Syriac *debbōrā* denotes 'wasp', vs. *debbortā* 'bee', which is also the sense of Jewish Middle Aramaic /*dabbarta*/ or /*dborta*/ (plural /*dabbare*/). In Soqotran, *idbeher* 'bee'.

²⁰⁵ Dolgopolsky (1998: 64) has a Nostratic entry **madu* 'honey' (cf. **medhu-* – see Pokorny 1959, at 707) with several Indo-European occurrences (cf. English *mead*; both 'honey' and 'mead' are denoted by Old Indian *madhu-*), as well as several Dravidian occurrences. Within Hamito-Semitic, he lists no Semitic occurrences, but he had one occurrence in East Chadic (Mokilko *máddé* 'bee, honey'), and several in

Omotic: the lexical semantics is sometimes ‘honey’, sometimes ‘bee’, and sometimes both senses being denoted by the same word in the given language. In a word list Greenberg gave for the East Sudanic family, he listed “*to be sweet*: (1a) Dinka *mit*, Shilluk *met*; (3) Didinga *med*” (Greenberg 1950b: 160). But then there is the Hebrew root *m.t.q.* of *mātôq* ‘sweet’, *mâtáq* ‘to be sweet’: the risk of fortuitous resemblance is ever present. As for the likes of Latin *mel* ‘honey’ (**meli-t* – see Pokorny 1959, at 723), not considered by Dolgopolsky notwithstanding his entry **madu* ‘honey’, note that a transition between [d] – [ḏ] – [ʃ] – [l] is conceivable, and is indeed known from the phonetics of Italian dialectology (with no relation to ‘honey’). “5.84 – HONEY – Hittite *milit* joins Goth. *milib*, OIr. *mil*, Alb. *mjal*, etc. in reflecting faithfully IE **mélit(om)*–“ (Weeks 1985: 55). [Cf. Vennemann (1998b: 477).] Actually, Greenberg (1950b: 149) criticised Carl Meinhof’s comparative Hamitic vocabulary at the end of the latter’s book *Die Sprachen der Hamiten* (Meinhof 1912), as follows: “In other instances an ingenious Hamitic etymology when an obvious Nilotic one is at hand. Thus Masai *mat* ‘to drink’ is with Hausa *mo:da*: ‘drinking vessel’ and Somali *mu:d* ‘brandy’ when we have Nilotic, Lango *mato*, Shilluk *ma:do*, preterit *mat*, etc., meaning ‘to drink’. This leaves us with a remainder of ten etymologies which have any degree of plausibility. Altogether this is hardly more than a chance number of similarities. If one were to compare English to all the Hamito-Semitic languages, I should that ten possible etymologies might be found”.

But consider this statement in the introduction to Dolgopolsky’s Nostratic Dictionary (2008: 33): “N[ostratic] **ʔitê* ‘eat’ survives in HS [i.e., Hamito-Semitic] (namely in Cushitic and Chadic), but because of the devocalization of verbal roots it was lost in Semitic (otherwise it would have been undistinguishable from other verbs with the same historical consonants, [...]).”

Joseph H. Greenberg’s (1915–2001) “initial reputation was established through his monumental work in the area of African linguistic classification. This was published first as a series of articles in the late 1940s and then ultimately in reworked and revised form as *The Languages of Africa* (1963), a work that thirty-five or more years later still stands as the cornerstone of African language classification. In his work, Greenberg dismantled the then standard classification of [Carl] Meinhof [(1857–1944)] and assigned all of the languages of Africa into one of four phyla: Afroasiatic [...], Niger-Congo [...], Nilo-Saharan [...], and Khoisan [...].” (Newman 2001: 169).

A scholar very sceptical of Nostratic, Larry Trask, commented (1999: 167) about Dolgopolsky’s (1998) entry 21 for “‘tasty beverage’, involving Finno-Ugric, Turkic, Laz, East Cushitic and Indo-European. The semantics is troubling: ‘tree sap’ or ‘milk’ in Finno-Ugric, ‘honey’ in Indo-European, ‘(butter)milk’ in Laz. Given that Greek /meli/ ‘honey’ is cited here, I cannot resist mischievously noting Hawaiian /meli/ ‘honey’”.

This is akin to this other, arguably random coincidence: Hawaiian *kahuna* ‘priest’ is outwardly similar to Hebrew *k’hunah* ‘priesthood’. “The Hebrew word *k’hunná* is pronounced *k-huná* by some, and *kehunná* or *kəhunná* by others, and historically there may have been the Ashkenazic *k-hino*, and towards the end of the first millennium: *k’hunná*. Therefore, just imagine what a myth of ancient contacts could be made out

Vennemann asks: if the hypothesis of his concerning the *bee* word is correct, “the question arises why the words entered the language with ${}^{+}b^h$ - rather than ${}^{+}b$ -, at least in Germanic where a representation with ${}^{+}b$ - would have yielded words with a root ${}^{*}pi$ -“ (518). Vennemann states that “[t]he only explanation [he] can think of is that the words were borrowed so early that speakers of northwest Indo-European were still uncomfortable with initial ${}^{+}b$ - as a consequence of the ‘labial gap’ and therefore preferred a representation with a phonetically similar phoneme, ${}^{+}b^h$ -“ (518–519). Vennemann recognises that this “is a difficult explanation, however, because Proto-Germanic itself has several words with initial p -, all of them lacking Indo-European etymologies and thus probably loan-words” (519). In note 48 on p. 528, Vennemann averred there is an alternative, namely, “the assumption of borrowing after the operation of Grimm’s Law. The difficulty in this case would be the fact that the *bee* word is not limited to Germanic”. In note 47 on p. 528, Vennemann credits Gamkrelidze and Ivanov (1995: 524) for finding “the similarity of PIE ${}^{*}b^he$ i- ‘bee’ to Egyptian *bj.t* (with the feminine gender marker *-t*) is striking” (their words), even though they did not explain out this similarity.

Note that whereas Hittite is an Indo-European language, we do not possess the Hittite word for ‘bee’, because what we have is the Sumeri-

of that coincidence of the Hebrew and Hawaiian names for ‘priesthood’ being “the same”.

Trask (1999) listed 15 resemblances to Basque, “which look surprisingly good to me” (*ibid.*: 173), while considering the 124 Nostratic etyma proposed by Dolgopolsky’s (1998). “Now, I certainly do not want to be known as the person who introduced Basque into the Nostratic hypothesis: I am doing no such thing. But this amusing little exercise does suggest to me that chance resemblances between arbitrary languages are by no means so difficult to find as is sometimes suggested.; And note that I have only cited the cases that leapt off the page at me: I have not scoured a large Basque dictionary looking for possible matches, and I am confident that, especially with a little more semantic generosity, a dogged search would turn up a number of further plausible-looking matches, at least as good as many of those cited by Dolgopolsky for modern languages” (Trask 1999: 173). “Nevertheless, I do not consider the Nostratic hypothesis to be *a priori* out of court. This hypothesis deserves the most careful scrutiny from the rest of us. Unlike so many self-styled ‘long-rangers’, Professor Dolgopolsky and his colleagues have rejected the mere assembly of miscellaneous look-alikes – an activity which is little better than a waste of time, in my view – in favour of the more rigorous methods which are the only ones that have ever brought us success in these enterprises” (*ibid.*).

an Cuneiform logogram for ‘bee’ as used in Hittite texts: “3.82 – BEE – NIM.LÀL (Akk. *nubtu*); LÚ NIM.LÀL ‘beekeeper’” (Weeks 1985: 29). A general Hittite term for ‘insect’ is unknown, but a few names for insects are known instead, including *lalakuesa-* ‘ant’, *mutgalla-* ‘caterpillar’, *masa-* ‘locust’, and *gagastiya-*, which perhaps denotes ‘grasshopper’ (Weeks 1985: 29, §3.81). We do possess a Hittite term that among the other things, denotes ‘swarm of bees’: “1.74 – MIST (FOG, HAZE) – The likely basis of *kammara-* ‘fog; haze, smoke; cloud; swarm of bees’ is IE **kem-* ‘cover’ (T 472–73), seen e.g. in [Old Norse] *hamr*, [Old English] *-hama* ‘covering, skin’, perhaps from the appearance of fog or mist covering the ground; cf. [Sanskrit] *dhvānati* ‘cover’, *dhvānta-* ‘darkness’, Av[estan] *dvaṇman* ‘cloud, mist’” (Weeks 1985, pp. 12–13).

In his book-length review of a grammar of Ugaritic, an ancient Semitic language, Pardee (2003/2004: 72) remarks:

Though I have myself vocalized *nbt*, ‘honey’ as /nubtu/, following Hebrew *nōpet*,²⁰⁶ the presence of the {b} may indicate that in the Ugaritic form there was a vowel between that consonant and the /t/. T[ropper] observes that the Hebrew form with /p/ may be owing to devoicing in proximity with the /t/ and that the base form may have been /nubt-/. He does not, however, remark that forms such as *ḥp̄t* and *lp̄š* [...] indicate that proto-Ugaritic /nubt-/ may be expected to have become /nupt-/ in Ugaritic. One should perhaps postulate, therefore, that the Ugaritic form was /nūbatu/, i.e., the ‘long’ form of the feminine morpheme would have been retained in Ugaritic because of the long vowel in the stem.

Pardee (2003/2004, pp. 72–73, fn. 263) further states:

Arabic shows a form *nūb^{um}*, ‘bees’, which is considered by the traditional lexicographers as the plural of *nāyib^{um}* but of uncertain etymology (see Lane,²⁰⁷ p. 2863). For Akkadian, *CAD* indicates²⁰⁸ the late form *nūbtu*, meaning “‘honeybee’.” By comparison with this hypothesis for Ugaritic, Hebrew *nōpet* would plausibly have come from proto-Hebrew /nūbatu/ which became /nuptu/ through loss of the feminine /a/, shortening of /ū/ because the syllable was now closed, and devoicing of the /b/ in proximity

²⁰⁶ This is the Biblical Hebrew segolate noun *nōfēṭ*.

²⁰⁷ I.e., Lane’s *An Arabic-English Lexicon* (1863–1893, repr. 1984).

²⁰⁸ In the scholarly literature of Assyriology, *CAD* is a standard acronym of *The Assyrian Dictionary of the University of Chicago* (Chicago 1956 ff.)

to the /t/; this proto-Hebrew /qult/ form²⁰⁹ would by the normal processes have become *nōpet* in Biblical Hebrew as we know it from the Massoretic tradition.²¹⁰

7.10. A Digression about English monkey and ape

I just noncommittally signal here that Dolgopolsky (1998: 21) glossed his Nostratic entry 6 ‘monkey’. From Hamito-Semitic, he was only able to list a word from Mubi, and East Cushitic language: *móngó* ‘small black monkey’. He has several parallels within Dravidian,²¹¹ such as Kannada *maṅga* and Koraga *maṅgi* ‘monkey’. He also has parallels within Altaic, such as Manchu *moño* ‘(a kind of) yellowish monkey with a short tail’. Importantly, Dolgopolsky is cautious when it comes to European terms, in the same entry: “The origin of English *monkey* and of the Romance word **monna* (> Spanish, Portuguese *mona*, -o ‘monkey’, Italian *monna*, French *mone* ‘female monkey’) remains rather obscure. They may be loanwords of unknown origin. Nothing is known of their possible connection with Nost. *man^ʀg^ʀ▽ ‘monkey’” (*ibid.*). The symbols^ʀ and ^ʀare uncertainty brackets. ▽ stands for an unspecified vowel in Dolgopolsky (1998). Clearly, for Europeans the *denotatum*, ‘monkey’, was an exotic animal, somewhat like with parrots.²¹² Concerning that entry in Dolgopolsky (1998), the

²⁰⁹ /qult/ is simply the name for a Semitic derivational pattern in which, according to the convention that a trilateral root is symbolised by *q.t.l.* (the root associated with ‘to kill’), the middle radical symbolised by *t* is missing (so the root in the given derivational pattern is of the so-called *mediae infirmae* category), and the *t* is present in the pattern as a formative consonant, not as a radical consonant.

²¹⁰ The Massorettes were the Jewish scholars who in the Galilee in the early Muslim period developed a diacritical notation conveying a complex system of vowels for Biblical Hebrew and Biblical Aramaic. The Tiberian diacritic marks of vowels and prosody still appear in the printed Hebrew Bible.

²¹¹ Generally speaking, see Kamil Zvelebil’s discussion of how Dolgopolsky (1998) handled data from Dravidian.

²¹² “Talking” birds were exotic. In Nissan (2011), I identified (as the parrot and the myna) and etymologised *andrafta*, which in the *Babylonian Talmud*, tractate *Hullin*, 62b, is stated to come in two kinds: one of them called after Shabur (Shāhpūr, a major Sassanian monarch), *Shabūr andraftā*, and the other called *Pērōz andraftā*, apparently after another monarch. The former was claimed to be kosher, the second, not kosher. “If the kosher *andrafta* was indeed a myna and thus an Indian member of the family *Sturnidae*, we must now consider the status of the starlings as far as Jewish dietary norms are concerned. One cannot conclusively determine the kosher or unkosher status of the starling in talmudic times, but it is certainly not to be excluded that the myna was considered to be kosher by the Jewish sages of Babylonia” (*ibid.*: 450).

rival Nostraticist Allan Bomhard claimed (1999: 53, item 6): “*maŋ[g]V or *maN[i][g]V ‘monkey’: this etymology is plausible. I would reconstruct Proto-Nostratic *maŋ-g- ‘monkey’”. A scholar very sceptical of Nostratic, Larry Trask, remarked (1999: 167) that Dolgopolsky’s (1998) entry for “‘monkey’, really consists of a Dravidian word for ‘monkey’, plus some names for particular species of monkey in Tungusic and one Chadfic language. I do not consider this substantial”.

Concerning that same entry from Dolgopolsky (1998), Starostin (1999: 140, item 6 in a table), noting in the second column that the distribution of *maŋ[g]V ‘monkey’ only includes Dravidian, and even that questionably, made this comment: “In HS [i.e., Hamito-Semitic]: only East Cushitic. Otherwise: only Drav[idian] *maŋk- ‘monkey’; Manchu moño is actually a variant form, the basic one being boño (also reflected in Church. Bonen < PTM *boña ‘monkey’. The phonetic match between Drav. *maŋk- and Manchu boña is impossible. On the other hand, Drav. *maŋk- could go back to *malVk- and be compared (as a loanword?) with Sino-Tib[etan] *mlūk ‘monkey’. On the whole, a very dubious case”. Being a specialist in the supposed Sino-Caucasian phylum, in the next column in the table Starostin offered just this evidence from Sino-Tibetan (ST) within Sino-Caucasian: *mlūk ‘monkey’, and in the last column in the table he indicated “ST?” as Sino-Caucasian distribution of Dolgopolsky’s Nostratic root considered.

Note that Dolgopolsky (2008: 1372)) has entry 1452 for “m∇ŋK∇ ‘beaver, mole’ “(a substratum word?)”, from which he derives Uralic and Altaic terms. Dolgopolsky (2008) comprises entry 1451 on pp. 1371–1372, for the Nostratic root of the *monkey* word; and it is summarised on p. 2626:

1451. *m Aŋ^g∇ – *m AN₁i^g∇ ‘monkey’

1452. UA₂ *m∇ŋK∇ ‘beaver, mole’ (a substratum word?)

Dolgopolsky’s (2008) entry in full on pp. 1371–1372 is as follows, the Hamito-Semitic instance being from Mubi, an East Cushitic language, whereas from Altaic, his data are from a Tungusic (Tg) language, Written Manchu (WrMc), as well as Sibe Manchu; from Manchu, the term would have been borrowed by the Solon, Orochi, Udihe, Ulcha, and Nanay languages, with a South Tungusian variant occurring in Written Manchu; other data coming from Dravidian:

1451. *mAn^g∇ – *mAN_Li^g∇ 'monkey' > HS: ECh: Mu {J} mōngò 'small black monkey' † J Mu, ChC || A: Tg *omońo > WrMc mońo (spelled moniyo) {Z} 'ε yellowish monkey with a short tail' ('обезьяна, желтоватая съ короткимъ хвостомъ'), {Hr.} 'kurzschwänziger Affe', but on p. 511 Zaharov refers to mońo as 'common monkey' ('обыкновенная обезьяна'), Mc Sb {Y} /moni/ [móni] 'monkey', {Mrm.} mońu, Mc N {Rdn.} меңу мэну 'monkey'; Mc → Sln mońō, Orc, Ud, Ul, Nn mońo 'monkey'; there is also a variant STg word *bońo 'monkey' > WrMc bońo {Z} 'large monkey', {Hr.} 'monkey', Jrc dobi bonon 'monkey' † STM I 94, 545, Krm. 260, Z 510-1, 890, Hr. 111, 665, Y#2212, Klz. MS 226, Rdn. 7, Kiy. 105 || D (in SD) *maŋk- {ḡS} *maŋg- 'monkey' > Ml moŋŋa, Kn maŋga, Krg maŋgi 'monkey', Tu mange id., 'ape' † D #4666 ◇ The origin of NE monkey and of the Romance word *monna (> Sp, Port mona, -o 'monkey', Olt, It monna, Fr mone 'female monkey') remains unknown. They may be loans from an unknown source (but hardly from Prs [sc. CINPrs maymūn, NPrs māymūn] 'monkey', as believed by ML #5242). For other unconvincing etymologies (e.g., < It madonna) see Pian. 872-3. Nothing is known about their possible connection with N *mAn^g∇ 'monkey' ◇ Tg *o goes back probably to *a, that was labialized due to the ass. infl. of *m- ◇ AD NM #6, ≠ S CNM 12, ≠ Vv. AEN 7 (rejects the Tg cognate because of the irreg. Mc -ń-, which, however, may be explained if we assume pN *mAN_Li^g∇).

Besides, Dolgopolsky (2008) comprises this entry summarised on p. 2561 for the Nostratic root of the *ape* word:

61. ?₂ WW? ≈ *ʔ∇p∇ 'monkey' > IE: a word reconstructible as NaIE {P} *abō(n)- 'monkey', but most probably spread by borrowing

The entry is given in full on p. 147 in Dolgopolsky (2008), with data from Germanic, Celtic, West Cushitic, Central Cushitic, and tentatively Berber as well:

61. ?₂ WW? ≈ *ʔ∇ρ∇ 'monkey' > IE: a word reconstructible as NaIE {P} *abō(n)- 'monkey', but most probably spread by borrowing: Gmc: ON *apī* 'monkey, fool', OSx *apō*, OHG *affo* m., *affa* f., NHG *Affe*, Mdt *ape*, Dt *aap*, AS *apa* 'ape (Pongidæ)', NE *ape*; Gmc ⇨ OR **ΟΠΗЦΑ** *opica*, OCz *opice* 'monkey, ape' ||| Clt: Gl {P} *ἄββάνας 'long-tailed ape' (from Hs.'s gloss ἄβράνας [emendated by Schrader as *ἄββάνας] · Κελλτοὺς τοὺς κερκοπιθήκους) ¶ P 2-3 [hyp.: *abō(n) is a loanword], EI 384, Vr. 11, Ho. S 3, Ho. 6, KM 8, EWA I 58-60 ||| **HS:** Ch {Stl.} *ʔiʔpuki 'monkey' > WCh {Stl.} *ʔiʔpuki 'baboon' > Krkr {ChL} *yifki*, Cg *yipək-ən* id. ||| CCh {Stl.} *ʔ∇,puki 'monkey' > {ChL:} McTr: Bk *fuši*, G'nd *fíçà*, Gbn *fíçè* | Mrg *pçu* | Kps *pši* | FIM *viži* | Nkc *vìkény*, Gv *vič-χadzya* 'monkey' ¶ Stl. IF 34 || ??? B: CM, Izd *abaγus*, Zng {IC} *äbugär* 'monkey' ¶ Mrc. 236, NZ 85, TC D 4.

7.11. *Codfish*

A dogger used to be a kind of Dutch fishing boat,²¹³ which in the early modern period was commonly operated in the North Sea. It had two masts and broad bows. It originally was single masted (as first described in the 14th century), but in the 17th century it had two masts. The term occurs in Middle English in the form *doggere*. The Dutch pronunciation of *dogger* is [ˈdɔxər]. In English the noun *dogger* is pronounced as [ˈdɔgə(r)] instead.

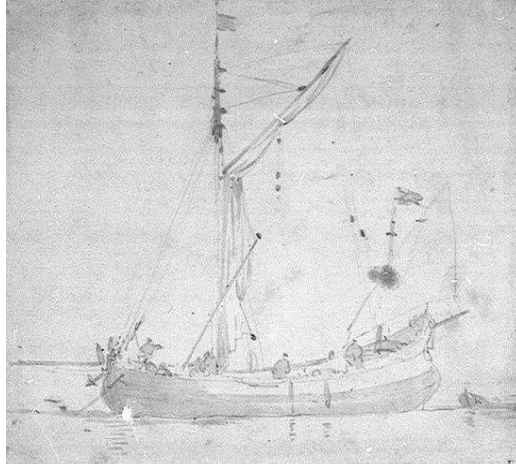
They were largely used for fishing for cod by rod and line. Dutch boats were common in the North Sea, and the word *dogger* was given to the rich fishing grounds where they often fished, which became known as the Dogger Bank.²¹⁴ The sea area in turn gave its name to the later design of boat that commonly fished that area, and so became associated with this specific design rather than the generic Dutch trawlers. [...] The dogger was a development of the ketch.²¹⁵ [...] Doggers were considerably

²¹³ It was “originally, an early fishing trawler, later applied (in English but no longer in Dutch) only to Dutch vessels” (Mayne 2000: 92).

²¹⁴ The Dogger Bank, the large sandbank (about 170 miles long, 65 miles wide) in the North Sea between England and Denmark, took its name from dogger boats – it is a major fishing ground indeed, with great banks of shoals – and gave its name in recent decades to the prehistorical Doggerland. The Dogger Bank is first recorded using that name, in the 1660s.

²¹⁵ But sailing crafts known by the name *ketch* have two masts. “The distinguishing characteristic of a ketch is that the forward of the two masts (the ‘mainmast’) is larger than the after mast (the ‘mizzen’). Historically the ketch was a square-rigged vessel, most commonly used as a freighter or fishing boat in northern Europe, particularly in the Baltic and North seas. In modern usage, the ketch is a fore-and-aft rigged

smaller vessels in comparison, usually displacing around 13 tonnes, and carrying around a tonne of bait, three tonnes of salt, and half a tonne each of food and firewood for the crew. Around six tonnes of fish could therefore be carried. They would generally have been around 15 metres long, with a maximum beam of 4.5 metres, and a draught of about 1.5 metres. They had a rudder rather than a steering oar and high sides.²¹⁶



A dogger drawn by Willem van de Velde the Younger around 1675.²¹⁷

vessel used as a yacht or pleasure craft. [...] The square-rigged ketch was largely supplanted by the brig, which differs from the ketch by having a forward mast smaller (or occasionally similar in size) than the after mast, and by the hoy, which was fore-and-aft rigged. Other similar craft include the snow and the pink. [...] Both the ketch and the yawl have two masts, with the main mast foremost; the distinction usually being that a ketch has the mizzen mast forward of the rudder post, whereas on a yawl, it is aft of the rudder post. But the balance of sail area can be an overriding characteristic. If 20% or more of the sail area is in the mizzen sail the rig would be termed a ketch. This is particularly true on center cockpit yachts. Compared to a ketch, a similar size yawl's mizzen sail is much smaller than the main, because of the limitations of the mizzen sheet. So on a ketch, the dual purpose of the mizzen sail is to both propel and balance the vessel, while on a yawl, the smaller mizzen mainly serves the purpose of trim or balance. Yawls tend to have mainsails almost as large as those of comparable sloops". As for the etymology of *ketch*: "Ketch was a 'catch' or fishing boat (*ketch* from Middle English *cache*, from *cacchen*, 'to catch')"

(<https://en.wikipedia.org/wiki/Ketch>).

²¹⁶ [https://en.wikipedia.org/wiki/Dogger_\(boat\)](https://en.wikipedia.org/wiki/Dogger_(boat))

²¹⁷ In colour at [https://en.wikipedia.org/wiki/File:Dogger_\(boat\).jpg](https://en.wikipedia.org/wiki/File:Dogger_(boat).jpg)

The etymology of the boat name *dogger* is often given as obscure by dictionaries, even though the *Longmans English Larousse* proposed (Watson 1968, s.v. *dogger*): “perh. fr. DOG”. What I find cogent is another etymology:

Dutch, from *dogger* (“codfish”).²¹⁸

Likewise, *Webster’s Revised Unabridged Dictionary* states:²¹⁹

D., fr. *dogger*, codfish, orig. used in the catching of codfish

*The Collaborative International Dictionary*²²⁰ gives this identical etymology:

[D., fr. *dogger* codfish, orig. used in the catching of codfish.]

And indeed, doggers were used “particularly in the cod- and herring-fisheries”.²²¹ While being silent about the ultimate etymology, Merriam-Webster states: “Middle English *doggere*, perhaps from Middle Dutch *dogge* fishing boat”, the first known use of the English term being in the 14th century. Cf. in Mayne (2000: 93): “From Anglo-French and ME ‘doggere’, and not in Dutch until a century later. First attested in 1356 in a Statute of Edward III (*Act 31 Edw. III, III, c 1*).

I would like to tentatively suggest an etymology for Dutch *dogger* ‘codfish’ (fish of the genus *Gadus*). As important staple as codfish is likely to have already had a name in early societies engaged in fishing in the North Sea; they may have used a specific name for ‘codfish’ already in the Mesolithic, well before the arrival of early farmers with a Northwest Semitic lexicon (whether their vernacular still was Northwest Semitic, or otherwise); these would not have had experience of codfish (they had arrived to the North Sea littoral from the interior). Or

²¹⁸ <https://en.wiktionary.org/wiki/dogger>

²¹⁹ <http://www.finedictionary.com/Dogger.html>

²²⁰ Accessed at <http://findwords.info/term/dogger>

²²¹ *Century Dictionary and Cyclopaedia*, entry at <http://www.finedictionary.com/Dogger.html> The etymology of the *herring* lexical type was discussed by Dirk Boutkan in his study “Pregermanic Fishnames, III: A New Etymology of ‘herring’” (2000).

then, perhaps in protohistory or the historical period, contact with Phoenicians, Carthaginians, or Gaditans brought to the mainland's North Sea coast such traders²²² who used for any fish, including codfish, the Northwest Semitic name /dag/ *dâg* 'fish' ([dɔg], because long /a/ was [ɔ]). There would have been semantic specialisation for 'codfish', because of the prominence of codfish in the North Sea. Of course, whether this comparison is reasonable or unreasonable revolves upon whether it is reasonable to consider a Semitic or Proto-Semitic presence on the North Sea littoral. Perhaps, just perhaps. The way I am open to this possibility is not identical with Vennemann's theory of Semitic presence there.

I do not dare suggest that English *cod* and Latin *gadus* are the outcome of metathesis from the Northwest Semitic name /dag/ *dâg* [dɔg] 'fish'. But should this be the case, then we would have a doublet of loanwords: *dogger* and *cod*. There is more to it. Gordon Whittaker (2008) discussed what he considered Indo-European loanwords in Sumerian (or as phonetic values of Sumerians logograms). On p. 163, he has an entry for the place-name *Kuara*. It used to be written by using the logograms for FISH+WATER+PLACE, that is to say, **HA.A.KI**. This stood for 'Kuara'. Whittaker proposed to etymologise that place-name from Proto-Indo-European **d^hǵ^u-o-s* 'fishy' (cf. IEW 416–417;²²³ Greek *ikhthuērós* 'fishy'). In the Sumerian King List, the god Dumuzid is described as a fisherman coming from Kuara" (*ibid.*). Now note that Arabic, within Semitic, calls 'fish' *sámak*, as opposed to Northwest Semitic name /dag/ *dâg* 'fish'. Did the latter emerge from the contact of Proto-Semites and Proto-Indo-Europeans,²²⁴ not just because of Indo-Europeans who remained as linguistic groups in the ancient Near East, but perhaps even as far back as the "Out of Africa" scenario, sometime in the early Palaeolithic?

Of course, one could also claim that as the root for Dutch *dogger* 'cod' is found in Proto-Indo-European and is associated with the general lexical concept 'fish', it is unnecessary to suppose there was

²²² In English, the terminological pairs *pork* / *swine*, *beef* / *ox*, came into being because Anglo-Saxon suppliers had their own vocabulary, whereas they were catering to Anglo-Normans who used terms from French.

²²³ IEW stands for Julius Pokorny's (1959) *Indogermanisches Etymologisches Wörterbuch*, Vol. 1.

²²⁴ In Ch. 15 of Alinei (1996), Sec. 3.5 (p. 554) is entitled "Il ruolo dell'Asia sud-occidentale nelle fasi iniziali della differenziazione" within Indo-European.

transmission from Northwest Semitic into the Germanic of the North Sea littoral. We do not really know which is what, and as I already pointed out, Occam's Razor, selecting the simplest explanation, does not necessarily capture the factually true explanation. Importantly, note however that Proto-Indo-European had a few very different lexical types for 'fish'. It had the lexical root **pejsk-* [the way Whittaker notates it] (which I surmise is a native root, at any rate for our present purposes),²²⁵ and the other lexical root, instantiated in Greek (but metathetically²²⁶ and through voicing: *dh>th* and *gh>kh*) and which resembles Northwest Semitic <dg> 'fish' (Hebrew /dag/ 'fish', but one finds the consonant cluster /dg/ inside the verbal form <wydgw> *w³-yidgú* 'and they shall multiply like fish' in *Genesis* 48:16).

²²⁵ Consider however how Vitaly Shevoroshkin (1999) responded to Dolgopolsky (1998). On p. 88, item 40, Shevoroshkin claimed that Dogolpolsky's entry 76, p/p'ayV (denoting a kind of fish) may represent a genuine set of roots from Nostratic (occurring in Uralic *payV and Dravidian *payy-), buyt "which has nothing to do with IE *peisk-". Shevoroshkin went on to suggest that this Indo-European root may well be borrowed from North Caucasian *bVswA 'fish', and to suggest that "*-k- may be IE diminutive suffix". Likewise, On p. 88, item 39, Shevoroshkin claimed the following about Dogolpolsky's entry 43, in relation to North East Caucasian, Kartvelian, and, within Hamito-Semitic, Omotic from the Horn of Africa: "Entry 43 *diq'a 'goat' seems to include a IE borrowing *dik-/ *digh- from NEC *dV(r)q'V 'goat'; this may also be the source of kartv. Borrowing (*dqa- 'goat'). Starostin (this volume) also mentions irregular IE phonology (note, by the way, a precise match to IE *k/*gh [better: *kh/*g] in HS: Omotic *dVk'-/ *dVg- 'capricorn, lamb'" (Shevoroshkin's brackets).

North Caucasian languages are not included in the Nostratic hypothesis. Cf. Trask (1999: 164–165): "I conclude that the posited Proto-Nostratic system of 50 consonants, including 20 coronal affricates and fricatives, is at best unusual. It appears that only North Caucasian languages and Khoisan languages even approach such figures – and neither grouping is included in the Nostratic hypothesis. [...] Now, I do not consider that a reconstructed phoneme system which is much larger than the system found in any daughter language is a fatal objection: such a state of affairs is perfectly possible. But it is not appealing, and it opens the door to an obvious question: can we, simply by multiplying proto-segments as required, obtain spurious 'systematic correspondences' wherever we require them? After all, if you will allow me to posit additional proto-segments without limit, I can probably establish spurious correspondences between any languages at all – though naturally each one of these spurious correspondences is going to be instantiated only in very few cases".

²²⁶ Then perhaps English *cod* and Latin *gadus* continue the metathesised Proto-Indo-European root?

A Nostratic proto-form *doTgiHU ‘fish’ has an entry (no. 74) in Dolgopolsky (1998, pp. 61–62). T stands for an unspecified dental stop. H stands for an unspecified laryngeal. U stands for an unspecified round vowel (*ibid.*: 15). From *doTgiHU, Dolgopolsky derived the Narrow Indo-European *d^hġ^hu- ‘fish’. From this, he derived Greek ἰχθύς ‘fish’ (where the initial ἰ may be prosthetic, according to many scholars) through *ġ^hd^hu-, itself being the outcome of metathesis from *d^hġ^hu-. The claimed cognates in Baltic and Uralic are rather unconvincing, and the Nostratic proto-form with the unspecified dental stop inserted was apparently devised in order to accommodate the Uralic lexical form (**totke* > e.g. Estonian *tõtkes*), which all denote a particular cyprinid species, usually ‘tech’ (*Cyprinus tinca*). Dolgopolsky also proposed that Altaic fish names derive from the Nostratic proto-form, through an Altaic reconstructed proto-form *dóǵ|ki ‘fish’, whence a Tungusic proto-form with an initial affricate, *ǰzogi or *ǰzoyi, and Proto-Japanese *(d)íwuá < Old Japanese *iwo* ‘fish’. Dolgopolski (1998) has another entry (no. 76, on p. 63) for a Nostratic proto-form *p|ǰay▽ (that is, *pay▽ or *ǰay▽), where ▽ stands for an unspecified vowel (*ibid.*: 15), and ǰ is emphatic. From that Nostratic proto-form, Dolgopolsky derives the Indo-European proto-form – which he gives as *pisk(o)- / *pisk- ‘fish’ – of Latin *piscis*, Gothic *fisks*, Old Norse *fiskr*, Old High German and Anglo-Saxon *fisc*, German *Fisch*,²²⁷ English *fish*, as well as Slavic names for particular fish kinds, such as Russian лещкаръ ‘gudgeon’, Slovene *piškur* ‘lampern (*Lampetra*)’, Czech *pis-koř* ‘loach (*Misgurnus*)’ (*ibid.*: 63). Dolgopolsky also listed Old Irish *īasc* (< *peyskos), genitive *ēisc* ‘fish’. He was in doubt whether to include Uralic *pay▽ based on Votyak, Tavda Vogul, and some Samoyedic names for particular kinds of fish. He was more confident about including a few Dravidian names for fishes.


In his biblical concordance, Mandelkern (1977 [1896]: 290, col. 2) proposed an etymology of Hebrew /dag/ in relation to the Arabic verb *dájja* [‘dædʒ:æ] = [‘dæjʒæ] (but in some pronunciations: [‘dæggæ]) for ‘to wriggle’:

²²⁷ Alinei (2000a: 564) mentions the *Fisch* lexical type of Germanic, in the context of a discussion of voicing of the initial *f* in the Neerlandic area (Dutch *vis* ‘fish’).




– the semantic motivation for the name for ‘fish’ would have been from a verb for a kind of movement one can observe in fish. Did the verb originate from the zoonym? Or did the zoonym originate from the verb for movement, as suggested by Mandelkern? If the latter, then, I suggest, the lexical type of the zoonym is native to Proto-Semitic, and Proto-Indo-European acquired (as an alternative to its native root **peisk-*) the lexical type that was adopted in Greek for ‘fish’, and which moreover Whittaker (2008) detects as a Euphratic substratal relic in a Sumerian logogram. If Whittaker (2008) is right, at any rate, I reckon (and I think he would agree), the prominence monosyllabism of the pre-Sumerian conjectured Euphrateans of southern Mesopotamia (thus, speakers of an Indo-European vernacular with different features from Iranian, at any rate from Iranian as recorded only much later) would have been a secondary development.²²⁸ But in Whittaker (1998), most terms he reconstructed for that substratum are bisyllabic.

Earlier in the same paper, Whittaker wrote (2008: 158): “Not only has Akkadian borrowed a large number of phonetic, semantic, and logographic values (loanwords) from Sumerian, but also Sumerian itself would seem to have borrowed in its turn from a linguistically unrelated community, that of the Indo-European-speaking Euphrateans. Among the earliest signs are a number of faunal logograms with values surprisingly similar to their Indo-European equivalents, beyond what might be expected from coincidence”.

The first two logograms Whittaker listed were the following:  for “**ku**₆ ‘fish’: **(dh)ḡ^huh-* ‘fish’ (IEW 416–417; Mallory and Adams

²²⁸ This is a phenomenon one finds in other languages as well. When discussing the Eastern Sudanic language family, Greenberg (1950b: 150) wrote: “It is clear that the predominant monosyllabism of languages like Shilluk and Nuer is historically secondary, just as in the case of Chinese or English. The intricate internal variations of languages like Nuer must be the result of changes induced by former affixes which have been dropped after influencing the root. This is the manner in which internal changes are normally known to develop where historical evidence is available. Thus the English umlaut alternatives *foot/feet* is known to have arisen from a former *-i* in the plural which modified *o:* to *e:* before it dropped”.

2006: 147)” (Whittaker 2008: 158); and  for “peš ‘be wide’ ≠ *pejšk- (or *pejšk'-) ‘fish’ (IEW 796; cf. EIEC 604;²²⁹ Mallory and Adams 2006: 146). The Sumerian value has no connection with the item depicted. Of interest is the fact that this, like many other parallels to Indo-European, betrays a strong lexical affinity to that area of the IE dialect continuum from which the Western (or Northwestern) languages emerged” (Whittaker 2008: 159).

For comparison to the specialisation for ‘cod’ of a general name for ‘fish’, as I have suggested above, consider the neologised Israeli Hebrew name for ‘trout’ (the species *Salmo trutta* in particular, i.e., the brown trout):²³⁰ it is *shémekh*. (It is mentioned as a synonym in Dor’s 1965 Hebrew-language *Zoological Lexicon*, but he preferred to use as headword on p. 145 the term *truta* ⟨ṭrwṭh⟩, a name in use among zoologists. On p. 336, there is a headword *shémekh* ⟨šmk⟩, with a cross-reference to ⟨ṭrwṭh⟩.) In practice *shémekh* is used (if used at all) just that way: uninflected, and in particular not in the plural (*[ʃma’xim]). The substandard term Hebrew-speaking people in Israel use at the market is the loanword *forél* (Dor 1965 did not mention it),²³¹ and which is

²²⁹ EIEC stands for the *Encyclopedia of Indo-European Culture* (Mallory and Adams 1997).

²³⁰ https://en.wikipedia.org/wiki/Brown_trout states: “The **brown trout** (*Salmo trutta*) is a European species of salmonid fish that has been widely introduced into suitable environments globally. It includes both purely freshwater populations, referred to *Salmo trutta morpha fario* and *S. trutta morpha lacustris*, and anadromous forms known as the **sea trout**, *S. trutta morpha trutta*. The latter migrates to the oceans for much of its life and returns to fresh water only to spawn. Sea trout in the UK and Ireland have many regional names, including sewin (Wales), finnock (Scotland), peal (West Country), mort (North West England), and white trout (Ireland)”.

²³¹ A webpage of the Fish Breeders Association in Israel (visited in 2008) does not mention the name *shémekh* at all. In a table of fish names of fish bred in ponds in Israel, under the column “Hebrew name” one finds ⟨ṭrwṭt ṣyn hqšt⟩ *trutat ‘ein hakkéshet* (a semantic calque from the English name *rainbow trout*), and evidently this is the formal name in use among zoologists, whereas the next column, “Usual name”, only has the name ⟨pwrl⟩ for *forél*. The Linnean name is given as *Onchorhynchus*, as a genus in its own right, as this is no longer considered just a species of the genus *Salmo*. At that same website (<http://dagim.org.il/>), the webpage for ‘trout’ lists two Hebrew names: ⟨pwrl⟩ and ⟨šmk⟩ but not the formal name ⟨ṭrwṭt ṣyn hqšt⟩, whereas English *trout* does appear, followed by the statement in Hebrew that this is “a relative of the salmon” (the latter’s prestige makes stating this good for marketing). Because of the massive immigration from the former Soviet Union in the early 1990s, there also is a webpage in Russian, and the trout image is labelled Судак (by the way, *Su-*

one of Yiddish words for ‘trout’; the other one is *strónge*, whereas the main Yiddish word for ‘salmon’ is *laks*. [Cf. German *Lachs*, Lithuanian *lašišà*, *lašiš*, Latvian *lasis*, Old Prussian *lasasso*, Russian *losos*’, and Tocharian B for ‘fish’, *laks*. Names for ‘salmon’ were discussed by Alinei (1996, Sec. 7.3.1., pp. 578 – 580; cf. on p. 597). Within Continuity Theory, Alinei proposed that the *Lachs* lexical type was originally a Baltic innovation of the post-glacial Mesolithic, with semantic motivation (from ‘dotted’) only found within Baltic (*ibid.*: 579). Alinei (1996, Sec. 7.3, pp. 576–581) considers the Ice Shelf a good reason for the absence of a pan-Indo-European name for ‘fish’, a classic problem for Indo-European studies (*ibid.*, pp. 576–577): invasionist theories have it that the Indo-Europeans were still united during the Neolithic, so how can it be that they did not share a name for ‘fish’? (*ibid.*: 577). There are four lexical types: Celtic-Germanic-Italic, Greek-Armenian-Baltic, Slavic (*ryba*), and Indo-Iranic (Avestan *masya*-).]

The Modern Hebrew coinage *šémek* [‘šemex] was clearly made by linguists, by patterning it after Arabic *sámak* [‘sæmæk] ‘fish’ (this is a collective noun, whereas a singulative singular noun is *samka* ‘fish’). The consideration must have been that in Semitic comparative phonology, in cognates Hebrew /š/ corresponds to Arabic /s/. As for the Hebrew phoneme /k/ in final position, the allophone is [x]. Also note that in this case, the neologiser looked at Arabic, rather than at Jewish Middle Aramaic (which is the language of choice, when Hebrew neologisers tap a foreign lexicon, and this because Jewish Middle Aramaic is the language of the two *Talmudim*). The Aramaic of the *Babylonian Talmud* has *šamkā* ‘onion’, but apparently the neologiser was not concerned lest there would be confusion, as the intended semantic domain for the neologism was that of fish and of fish ponds intended for the consumers’ market. These circumstances also made it possible to impose a specialised sense ‘trout (*Salmo truta*)’ on the artificial cognate devised for the general Arabic term *sámak* ‘fish’. Besides, note that the neologiser apparently aimed at retaining the paroxytone stress of Arabic *sámak*, and therefore did not select the equivalent derivation pattern, which would have yielded the coinage being *šāmák * [ja’max] instead, just as Arabic *jámal* ‘camel’ ([‘dʒæmæl], Egyptian pronuncia-

dak exists as a Jewish family name). At supermarkets instead, one finds a poster, with the formal and informal Hebrew names, with Russian added.

tion [‘gæmæl]) and Hebrew *gāmāl* ‘camel’ (modern pronunciation [ga’mal]) correspond to each other.

More importantly than the previous example (because it was deliberate neologisation informed by linguists’ knowledge), the following also illustrate the specialisation of a general zoonym. Fir of all, consider the onomasiological difference of German *Tier* ‘beast’, vs. English *deer* but in Shakespeare’s *King Lear*, Act III, Scene 4, one comes across “small deer” for small animals syncategorematic with mice. Edgar says:

Poor Tom, that eats the swimming frog, the toad, the todpole, the wall-newt and the water; that in the fury of his heart, when the foul fiend rages, eats cow-dung for sallets, swallows the	1925
old rat and the ditch-dog, drinks the green mantle of the standing pool; who is whipp’d from tithing to tithing, and stock-punish’d and imprison’d; who hath had three suits to his back, six shirts to his body, horse to ride, and weapons to wear;	1930
But mice and rats, and such small deer, Have been Tom’s food for seven long year. Beware my follower. Peace, Smulkin! peace, thou fiend!	

In that example from *King Lear*, there was specialisation. Likewise, an obsolete Italian name for ‘European elk’ (akin to the North American moose) is *granbestia*,²³² because it is the largest cervid, as well as the largest land animal available in the given environment.²³³ In the

²³² Italian *granbestia* (literally, *gran bestia*, ‘big beast’), vs. standard Italian *alce* for ‘European elk’ and ‘moose’. The same term split in two words, *gran bestia*, means ‘large beast’ in general. It became the lexicalised signifier for the particular animal kind.

²³³ The extinct, subfossil bird of Madagascar, *Aepyornis*, a relative of the ostrich, is known in English by the name *elephant bird*. The adult was about 2.4 m to 2.7 m tall. While not as tall as the tallest *Moa* bird species from New Zealand, *Aepyornis* was the heaviest bird known, weighing perhaps as much as 450 kg, by far the heaviest of any known bird. Some intact eggs were found, and even assuming that the earliest human communities on the island never saw such a bird alive, they certainly inferred its size from the size of its eggs. This bird was known to their cultures. A Czech scholar, Pavel Hosek, among the other things maintains a database (accessible at <http://vesmir.msu.cas.cz/Madagaskar/slovniky/ptaci.html>) of Malagasy names for Madagascar birds (along with the equivalent Latin, English and French names). I inquired with him about possibly extant names from Madagascar cultures for *Aepyornis* or its relics, as *Aepyornis* did not appear in the database. He kindly replied as follows (in an email of 3 August 2000): “Unfortunately in literature I find only the word vo-

Jewish Neo-Aramaic of the town of Zakho, in northernmost Iraqi Kurdistan (ZJNA), *mehe* used to denote a particular kind of fish, and I suspect²³⁴ that the etymon is the general Persian name for ‘fish’ (Persian *māhi* ‘fish’, but *māhi* ‘month’),²³⁵ even though ZJNA *mehe* is pronounced with a short *e* rather than a long *a* as in the Persian noun²³⁶ (Persian *māhī*, vs. Kurdish *māsī* ‘fish’).

ronpatra (great bird). In this case I have no experience with native people and native language”. My own reckoning is that in principle, ‘great bird’ could have been the lexicalised name for *Aepyornis* (as known from relics: eggs and the like).

²³⁴ I discussed that hypothesis of mine concerning the etymology of *mehe* with Hezy Mutzafī (an expert on Neo-Aramaic) in emails on 19 and 20 December 2011.

²³⁵ Charles Ferguson of Harvard University began an article (1957) by stating: “The position of stress is generally recognized as contrastive in modern Persian, and examples of minimal contrast are often cited, such as *māhi* ‘a month’ *māhi* ‘fish’. But grammars of Persian fail to give a complete account of the distribution of stress and most dictionaries give either sporadic and unreliable indication of stress or none at all. The primary purpose of this paper is to examine in some detail the position of word stress and its role in Persian morphology; but it will be necessary in addition to describe the phonological status of stress and to offer some preliminary observations on patterns of sentence stress and their role in the syntax of spoken Persian. Finally, some comparative and historical notes are appended”.

²³⁶ From Iran, names for given species listed at <http://www.fishbase.org/search.php> and which include the general term *māhi* include: *Shah Mahi* (for *Alburnus chalcoides*, the Danube bleak, a native cyprinid), *Mahi-ye Sim* (for *Abramis brama*, the freshwater bream, a native cyprinid), *Mahi Sim Kondpuzh* (for *Ballerus sapa*, the white-eye bream, a native cyprinid), *Mahi-e Kaviar* (for *Acipenser nudiventris*, the fringebarbel sturgeon, from family *Acipenseridae*, order *Acipenseriformes*), *Mahi Prchmeh* (for *Aphanius vladykovi*, an endemic fish from family *Cyprinodontidae*, order *Cyprinodontiformes*; the Israeli Hebrew name for the genus *Aphanius* is *navit*, transparently motivated by cuteness), *Gav Mahi Nokhardkeh* (for *Anatirostrum profundorum*, i.e., the duckbill pugolovka, a native species from family *Gobiidae*), and *Mar Mahi Ma'muli* (for *Anguilla anguilla*, the European eel, an introduced fish, from order *Anguilliformes*). In English, too, *fish* as being an element in compounds being common names for this or that fish taxon is commonplace. Incidentally, the list freshwater fishes of Iraq retrieved from the same database is not identical with that of Oman (1984), and also lists a few introduced species, but in the last column in the table, far less local names from Iraqi Arabic are listed than in Oman (1984), namely: *Shilik* for the cyprinid *Aspius vorax*; *Shabbout* for the cyprinid *Barbus grypus*; *Himri* (i.e., *h□imri*) for the cyprinid *Carasobarbus luteus*; *Hishni* (*quod corrige* into *khishni*) for the mugilid *Liza abu* (an English name is listed: *Abu mullet*); *Biss* (*quod corrige* into *Bizz*) for the cyprinid *Luciobarbus esocinus* (an English common name is listed: *Mangar*); *Gattan* for the cyprinid *Luciobarbus xanthopterus*; *Bunni* for the cyprinid *Mesopotamichtis sharpeyi* (or *Barbus sharpeyi*); and *Shour* (which is an error) for the clupeid *Tenualosa ilisha* (the English name listed is

Moreover, consider vulgar Latin *bīstia*, which corresponds to both Italian *bestia* ‘beast’ and, in a specialised sense, Italian *biscia* ‘non-venomous, non-dangerous snake’ – excluding also snakes like a boa, which smother and crush their prey, but snakes like the boa are not found in nature in Italy. Note however that the name *boa* for a snake is found in a late antique, medieval, and Renaissance tradition stemming from Pliny,²³⁷ *Naturalis historia*, 8:37, about a snake that sucks oxen, or then a snake that sucks cows’ milk. In late antiquity, see Isidore of Seville²³⁸ (ca. 560–636) in his *Etymologiae* 12, 4:28, and Solinus, *Polyhistor* or *Collectanea rerum memorabilium* 2:33. In the Middle Ages, see Thomas de Cantimpré, *Liber de natura rerum* 8:5, and Albertus Magnus, *De animalibus* 25:14.²³⁹

Let us turn to the terminology of fish across languages. The modern, mostly newly coined Hebrew terminology for genera of fishes, in use among Israeli zoologists, is often at variance with colloquial names for fishes as usual in the trade: Tnuva, a leading firm in the food trade in Israel, had a laminated colour poster displayed at the fish stand at supermarkets. Every image of a fish species is accompanied by Hebrew

Hilsa shad). The latter fish in Oman (1984, §18) is called *šbūr* (according to al-Daham 1977), the name being also given in other transcriptions; it is listed under the synonymised scientific names: *Hilsa hilsa* or *Clupea hilsa*, *Clupea palash*, *Hilsa macrura*, or *Clupandon* (sic) *ilisha*. The scientific names the database lists for Iraq’s freshwater fishes are not identical with those in Oman (1984), they often are synonyms. Interestingly, no Turkish name of those listed for freshwater fishes from Turkey in the same database appears to be etymologically relevant for with either Iraqi Arabic, or ZJNA fish-names I examined.

²³⁷ The passage from Pliny the Elder is as follows: “faciunt his fidem in Italia appellatae boae in tantam amplitudinem exeuntes ut divo Claudio principe occisae in Vaticano solidus in alvo spectatus sit infans. aluntur primo bubuli lactis suco, unde nomen traxere”. That is to say: “Credibility attaches to these stories on account of the serpents in Italy called boas, which reach such dimensions that during the principate of Claudius of blessed memory a whole child was found in the belly of one that was killed on the Vatican Hill. Their primary food is milk sucked from a cow; from this they derive their name” (Rackham 1940: 28–31).

²³⁸ The passage from Isidore of Seville is as follows: “Boas, anguis Italiae immensa mole, persequitur greges armentorum et bubalos, et plurimo lacte riguis se uberibus innectit et sugens interimit, atque inde a boum depopulatione boas nomen accepit”, which Barney et al. (2006: 257) translate as follows: “The boa (boas), a snake in Italy of immense size, attacks herds of cattle and buffaloes, and attaches itself to the udders of the ones flowing with plenty of milk, and kills them by suckling on them, and from this takes the name ‘boa’, from the destruction of cows (bos)”.

²³⁹ I am grateful to Davide Ermacora for these Latin sources concerning *boa*.

and other names (including Russian: this was in the 1990s, after the wave immigration from the former Soviet Union). The Hebrew names come in pairs: the term in the trade, and the term in use among Israeli zoologists.

Biblical Hebrew does not preserve fish names (except sea monsters). By contrast, one finds Aramaic fish names in the *Babylonian Talmud*. In Barbier (1910), Sec. 89 is about Latin *asellus*, *asinus* (which are primarily names for ‘donkey’) – Saul Levin (1995) argued that the etymology of Latin²⁴⁰ *asinus* is Northwest Semitic²⁴¹ – but are also

²⁴⁰ In the same year, Semitic loanwords in Latin were discussed by Paolo Martino (1995).

²⁴¹ Cf. German *Esel* ‘donkey’. Latin *asellus* is a diminutive of *asinus* ‘donkey’. Saul Levin (1995) etymologised *asinus* rather cogently by proposing that the accusative plural *asinos* was yielded by dissimilation from a form **asonos*, itself a loanword from Northwest Semitic as documented in the Biblical Hebrew *ʿāṭōnōṭ* [ʔ(a)θoʾno:θ] ‘she-asses’ (this being the plural of *ʿāṭōn* [ʔāʾθon]). It wasn’t unusual to refer to she-asses rather than to male asses. What is more, in the biblical episode about the lost she-asses of Saul’s father (see below), the spelling is *ʿmwt* instead of *ʿtwnwt* (which displays a *scriptio plena* in full). There is more to it: each and every instance of the word in the Hebrew Bible is spelled *ʿmwt* with only three exceptions: the two occurrences with fully plene spelling (*ʿtwnwt*) in the Book of Job (1:3, 42:12, about Job owning 500 she-asses before calamity stroke, and about his owning one thousand she-asses once he was restored to his florid status); and the occurrence (with the determinative article) in *1 Samuel* 9:3 (Saul’s father orders him to search for the lost she-asses), where the spelling is fully defective (*hʿmt*).

This does not necessarily mean that the first /o/ was shortened in the plural (thus yielding *ʿāṭōnōṭ*), which would be one way of explaining why in Latin *asinus*, the tonic stress is on the antepenult. It would be simpler to explain that whereas in the Semitic language from which the lexical borrowing took place, the tonic stress was on the last syllable of *ʿāṭōnōṭ*, in Latin it receded, and to recede it had to move to the antepenult. In Hebrew however, historically a long instance of the phoneme /a/ whose position was earlier than just before the tonic stress became an *a*-coloured schwa, and thus being very short, is not compatible with being tonic.

The episode of Saul looking for his father’s she-asses is ironic. This is consistent with Samuel, early on, who when the people requests to be given a king, warns them about the evils of kingship. Samuel nevertheless complies, but once a good-looking lad is selected, he stumbles through a sequence of unheroic situations. Saul has good looks and is young when first introduced, but he is especially very tall (*1 Samuel* 9:2). His father’s she-asses are lost, so he orders his son to go looking for them (9:3). Samuel is informed prophetically that the man to be made king is about to arrive, and how Samuel is to behave. Saul has not found the missing beasts, and now worries his father would be worried about Saul’s own absence, but he is advised to consult the prophet before going home. Upon Saul’s arrival, Samuel tells him that the she-asses

names for a fish kind.²⁴² By semantic calque from the Greek equivalent (the fish name *onos*), one also finds the “sea donkey” (the genus *Gadus*? If so, the sense is ‘cod’) in the Aramaic of the *Babylonian Talmud*: *ḥamra de-yamma* (spelled *ḥmr’ dym’*) stated to be kosher (*Babylonian Talmud*, at *Avoda Zara* 39a; cf. Dor 1997: 174), whereas contiguously the *tora de-yamma* (spelled *twr’ dym’*; it is Aramaic for “sea bull”) is stated to be non kosher (*Avoda Zara* 39a; Dor 1997: 180). As a mnemonic device, the talmudic text pointed out a paradox: “Abbaye said: the ‘sea donkey’ is kosher, the ‘sea bull’ is non kosher. The signs for you [to remember] are: the one unclean [i.e., the kind of beast living on earth] is clean [i.e., the fish so named in the sea], and the one clean [the kind of beast on earth] in unclean [i.e., the fish].”

Dor (1997) remarked that Lewysohn’s identification of the rabbis’ “sea donkey” (spelled *ḥmr’ dym’*) with the cod was based on names in

were found and that he should not be worried for them (9:20). Samuel has Saul fed, then sends away a servant, so now they are alone (9:27), and Samuel anoints Saul king (10:1) and tells him that Saul is going to meet people who would inform him that the she-asses had been found, and that by then, Saul’s father was concerned because of Saul’s absence, not about the she-asses being absent (10:2). As Saul walks by, a group of persons is “prophesying” (in trance? Having epileptic seizures?), and Saul does the same, leaving onlookers who knew his family bemused, so that “Is also Saul among the prophets?” becomes proverbial. Saul comes down from the platform, and meets his uncle and tells him that they went to consult Samuel concerning the she-asses. Saul’s uncle asks him what he was told by the prophet, and Saul says that this was about the she-asses having been; he does not tell his uncle about having been anointed king (*1 Samuel* 10:14–16). Ironically, in the next verse we read that Samuel convokes the people, to announce the oracular choice of a king: the oracle selects the tribe of Benjamin, and within that tribe, Saul, the son of Kish. Saul however is nowhere to be seen, and the oracle (literally, the verse states that the Deity Himself) informs them: “Here he is, hiding amid the pottery” (10:23). When standing among people, the young Saul towers above them; it is because of the wording expressing this in Scripture, that the English idiom *he stands head and shoulders above them* originated. Concerning Saul’s moods, see Nissan and Shemesh (2010 [2012]).

²⁴² The standard Italian name of the fish species *Merluccius merluccius* is *nasello* (literally ‘little nose’, known in English by the name *whiting*), from a conflation of Latin *asellus* ‘little donkey’ and Italian *naso* ‘nose’ (Devoto and Oli 1968, s.v. *nasello*). Actually *asinello* ‘little donkey’ (*ibid.*, s.v.) has also (in the form *asinello* itself, or equivalent dialectal forms), regionally in Italian, the denotation of both *Merluccius merluccius* and *Maena maena*. It must be said (*sed quaere*) that remotivational attraction to *naso* ‘nose’ may have eventuated by folk-etymology, after false segmentation of the noun with the article: *un *asello* and the like. Of course, to state that much with more confidence, an in-depth dialectological analysis ought to be carried out.

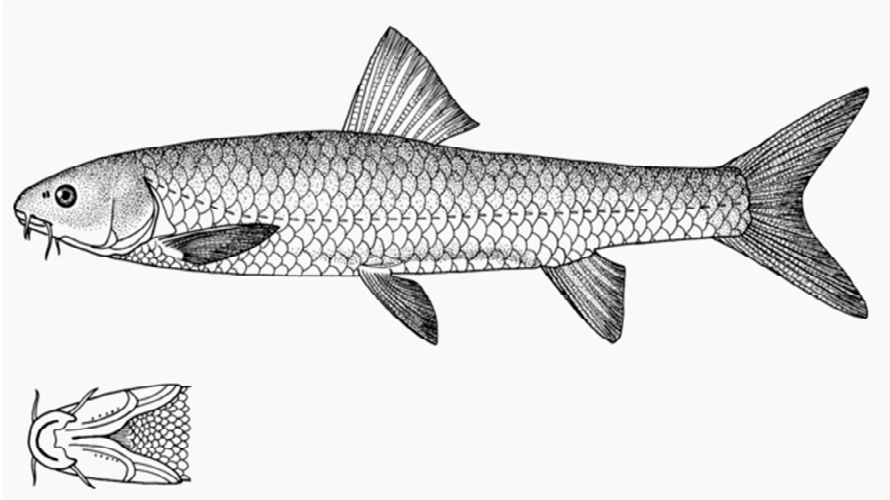
Aristotle and Pliny, and proposed that the analogy is correct: “One is right to assume that a fish called *Onos* in Greek and *asellus* in Latin – the ass of the sea – was called likewise in Aramaic. In the Talmud, usually the Greek names for fish appear in loan-translation.” Such semantic calques are also found in Syriac: *ʿrnb’ dyma* [as spelled] (literally “sea hare” is found in Syriac, with a cognate in Arabic, and is a calque after the Greek *λαγώς θαλάσσιος* (Löw 1969: 21, §84).

Lewysohn (1858), followed by Dor (1997: 180), identifies the *tora de-yamma* with the ray, a cartilaginous fish. Dor also has an entry for the *ḥamra de-yamma*, which he identifies with the genus *Gadus*, i.e., the cod, a sea-fish called *šibbūt* in Israeli Hebrew, in contradiction with the talmudic tradition and the Iraqi Jewish tradition up to the present, for which that name denotes a particular, much appreciated kind of riverine fish. Dor (1997: 174), s.v. *šybwṭ* (as spelled), proposed that perhaps the talmudic fish called *šybwṭ* (this is the spelling) was *Gadus*, referring to the statement (in the *Babylonian Talmud*, tractate *Ḥullin* 109b) that “the pig [tastes like] the brain of *šibbūtā*”. In fact, *Ḥullin*, 109b, curiously juxtaposes “pig”/“pork” and “*šibbūtā*-brain”. As rendered by Jastrow’s dictionary (1903, s.v. *šibbūtā*), the wording is: “the swine is forbidden, and as a compensation for it the brain of the *sh[ibbūtā]* serves”. It doesn’t actually claim that they taste the same, but such is the usual understanding.

As for cognates of the talmudic fish-name *šybwṭ*, namely, Baghdadi Judaeo-Arabic *šabbūt* and Zakho Jewish Neo-Aramaic *šabūtī*, and what Iraqi Arabic and Arabic in general intend by *šabbūt* (or rather *šabbūt*): this is the species *Barbus grypus*.²⁴³ The family *Cyprinidae* (a typical riverine fish family) is called in Modern Standard Arabic *šabbūtīyyāt*, this being a term in use among zoologists. Amin Malouf’s (1932) Arabic zoological dictionary is a modern classic. Diyab (1995) is more recent, with twice as many pages. Within Arabic lexicography, when it comes to fishes one must refer to Giovanni Oman’s synchronic

²⁴³ Cf. Zivotofsky and Amar (2006), concerning the identification of the talmudic fish name with the close cognate from Iraqi Arabic. *Barbus grypus* is one of the fish preferred by Baghdadis in general, and Baghdadi Jews in particular. It is also found in the north of the country: *Barbus grypus* in Iraqi Kurdistan is the subject of Abbas and Hamady (2009) and Al-Shamma’a et al. (2002).

perspective (Oman 1974a, 1974b, 1983, 1984, 1992a, 1992b) on ge-
 ovariation in the names for sea and freshwater fish in Arab countries.²⁴⁴



The *šabḥūt* fish, i.e., *Barbus (Tor) grypus*. By kind permission of Brian Coad. Drawing by S. Laurie-Bourque. ©Canadian Museum of Nature, Ottawa, Ontario.

Because of the importance of fishing in human foraging, a practice older than the entrenchment of agriculture in what we can gather of the

²⁴⁴ Oman 1966 was concerned with Arabic names for fishes from the coasts of the Mediterranean; Oman (1973a, 1973b) was concerned with Egyptian freshwater fishes; after one more paper on Arabic names for fish (Oman 1983). Oman (1984) turned to Iraqi Arabic names for freshwater fishes. Oman (1992a) is a book in which the Arabic names are collected of marine fishes and other marine animals from the northern Indian Ocean, the Egyptian, Sudanese, and Eritrean coasts of the Red Sea, as well as from the coasts of Arabic countries on the Persian Gulf. This series of publications by Prof. Oman is a synthesis of local terminological lists that he organised by Linnaean taxa, not an obvious choice for dialectologists – as after all, folk taxonomies are anything but Linnaean: see, e.g., Brent Berlin's studies (1992, 1979) – but nevertheless a sensible criterion if one is to try to pinpoint first of all the denotations from material culture, and impose some order on the material, owing to sheer size and to the form in which the Naples-based scholar was able to access it.

(pre)history of human cultures,²⁴⁵ the study of the lexicon of fishery²⁴⁶ provides important cues to scholarship's epistemic conduits to cultural reconstruction in remote times; this has been applying, in particular, to the cultures of Northern Europe. Two volumes by Mario Alinei (1996, 2000a) on the prehistory of European languages have relevant sections and passages on this.²⁴⁷ Such is the case of the names for 'salmon', "ben noto tema di ricerca IE!", i.e., "a well-known topic in Indo-European studies!" (Alinei 1996: 579; cf. Alinei 2000a, pp. 546–547). A specific study of names for salmonid fish is Diebold (1985).

The journal *Language in Society* published Jernudd and Thuan's study (1984) "Naming fish: A problem exploration". Sometimes, in research into fish names, concern in language contacts is foregrounded: this is admittedly the case of Celestina Milani's study (1983) into names for fishes within Middle English kitchen or cuisine terminology (cf. Cochran 1984). Some other times, the perspective is that of proto-languages: Mallory (1983) for Proto-Indo-European (cf. Seebold 1985); Boutkan (1999a, 1999b, 2000) for fish-names found in Germanic languages or historically occurring in texts (such as in Old Saxon glosses), and whose etymology is reconstructed as being pre-Germanic; or then, it is cross-linguistic in a phylogenetical perspective (Bammesberger 1996).²⁴⁸ Franceschini's (1998) is a comparatist Romance perspective

²⁴⁵ Even a popularistic presentation could tell that whereas as early as the Neolithic, there was a gradual shift in "economic life from universal dependence on hunter-gathering to nearly universal dependence on farming and herding. Only fishing remained, as it still does today, a vestige of hunter-gatherer culture" (Buzan and Segal 1998: 67).

²⁴⁶ In a paper concerned with Akkadian agricultural terminology from ancient Mesopotamia, A. Livingstone complained as the terms and techniques of Babylonian lexicographers "have been neglected even by the lexica, as remarked in this *Journal [of Semitic Studies]* by W. G. Lambert: 'It is indeed curious that workers that would willingly take *any piece of fishmongers' slang* disdain the technical vocabulary of their own predecessors'" (Livingstone 1997: 5, quoting from Lambert 1967). The emphasis is added here. Livingstone was expressing desolation at the situation in one domain, lexicographic history, while indulging in subserviently conveying an overly rosy depiction of the state of affairs in a different domain, which belongs in a subdiscipline of zoonymy, namely, in the study of the terminology of fish and fishing.

²⁴⁷ See in particular, in Alinei (1996), Ch. XVI, Sec. 7.3 (pp. 576–581); and in Alinei (2000): 303, 428–431 (Ch. XI, Sec. 2.2.1.4), 448–450 (Ch. XI, Sec. 2.3.2.3), 546–548 (Ch. XII, Sec. 3.2.3), 837–838 (Ch. XX, Sec. 3.2.3), 802, 866–869.

²⁴⁸ Brent Berlin (2005) discussed size-symbolic properties of fish names. Fluck (1974) was concerned with German fishing terms; Ribí (1939, 1942), with German

on the terminology of fishing. Benozzo (2010) is concerned with fishermen in Galicia, Spain. Tuailon (1984) considered Romance terms for fish. Vinja (1968, 1976) is concerned with Romance names for given kinds of fish.

In a historical perspective, when it comes to Aramaic fish-names, one must consider the by now one-century old, unrivalled, dense study, by Immanuel Löw (1854–1944), of Aramaic names for fish in Aramaic sources, be they from Jewish literature, or Syriac, in relation to parallels from the Arabic lexicon, as well as (when appropriate) to Greek lexical items that can be shown to have been borrowed. Löw (1906) – an early publication of the essay “Aramäische Fischnamen” – appeared in the same book as an essay by Arthur Hjelt (1906) on Syriac names for plants²⁴⁹ “Aramäische Fischnamen” is now accessible in Löw (1969, pp. 3–24). In the essay on the chameleon in Löw (1969): 88, fn. 1 states: “Meine Fischnamen erschienen in der NÖLDEKE-Festschrift 549–570”, which was published in Giesen in 1906. The book Löw (1969) was edited by his pupil, the late Alexander Scheiber. Much better known is Löw’s four-volume work on names for plants in Jewish sources (Löw 1924–1934).

In his book *Freshwater Fishes of Iraq*, Coad (2010: 33) points out: “Fishing in Assyrian and Sumerian-Akkadian times used nets, spears, traps, weirs, and copper hooks and line, sometimes fished from boats. Contracts concerned with fish ponds date from the reign of Darius II, in 422 B.C., and with fishing in 419 B.C.” Moreover, ancient Mesopotamia had a peculiar place in the world history of fishing, because of the use of ponds, as pointed out in the following enumeration of early methods of fishing (Higman 2011, Ch. 4: ‘Hunting, Herding, Fishing’, Section ‘Fishing’: 95):

Tidal fisheries channeled large shoals into attenuated traps as the water became rapidly shallow. Either from near shore or out at sea, seines or encircling nets were

fish names; Goltz (1981), with Low German fishing terms. Old English fish-names were investigated by Kohler (1906), cf. Whitman (1907); some late Middle English fish-names, by Mills (1964). German and Polish terms for hunting and fishing are covered by Cierpiel (1978). Eichler (1963) was concerned with German and Slavic names for fishing instruments. Baltic fish-names and bird-names are covered in Urbūtis (1981). Latvian fish-names are the subject of Hinze (1984). The terminology of in-shore fishing in England and Wales (in English and Welsh), of Elmer (1973).

²⁴⁹ A search of the British COPAC database (www.copac.ac.uk) would only return for this an offprint on hold at the University of Manchester library.

spread out like a curtain and then drawn and closed. This technique was practiced by the ancient Egyptians of the lower Nile marshlands. By 4500 BP [= before present] the towns of Mesopotamia had fish ponds, while sea fish, including shark, flying fish, sole, turbot, and swordfish, were kept alive in tanks. Well-off ancient Romans had aquaria, much preferring the taste of sea fish. Asian fishers employed all of these techniques, adding to their arsenal screens of split bamboo placed across streams, dip or lift nets, plunge baskets and blow guns. Close to shore, conch was dived for in the Indian Ocean and in the Caribbean.

8. The Case of Copper

In the section about the name for codfish, we mentioned the use of copper hooks in fishing in ancient Mesopotamia. Vennemann began Ch. 5 (58) by mentioning the prophet Ezekiel reference (*Ezekiel* 27:12, 12:25) to Tyre's trade with Tarshish (Tartessos in the Iberian littoral). Vennemann states: "The prophet could have added copper. The copper trade from Ireland, where industrial copper mining for export has been demonstrated by archaeologists for the second millennium BC, was in the hand of Phoenicians".²⁵⁰

For Latin *cuprum*, French *cuivre*, English *copper*, German *Kupfer*, perhaps the earliest cognate is from Hurrian (Hurrian, a non-Semitic language associated with the Hurrians, whose kingdom's capital city was Mitanni). In a volume edited by Volkert Haas (1988), *Hurriter und Hurritisch*,²⁵¹ the contribution by Erich Neu provided a preliminary discussion of a bilingual, Hurrian/Hittite epic (it includes for example the description of a banquet of the chthonic goddess Allani). Neu signalled the first occurrence ever of the Hurrian noun *kabali* 'copper'. Through an exchange of bilabials (voiced for unvoiced: *b/p*)²⁵² and liq-

²⁵⁰ Graham Webster (1993) remarks that iron was abundant in pre-Roman and Roman Britain. Copper could be mined in Britain, but was outside areas under Roman control. Romans were able to mine silver in Britain: it is found in very low-grade ores (0.01%) in Britain, but could be extracted in open-pit mines. In contrast, in Spain the grade of silver ore is variable, up to 6%, but mines were deep, and with a penchant for flooding. For tribes to supply Romans with silver in Britain, much more ore had to be extracted than in Spain in order to obtain the same amount of silver, but the Romans after the conquest under Claudius (whose army invaded in the summer of 43 C.E.) had many prisoners of war to employ in the silver mines.

²⁵¹ It was reviewed by Emilia Masson (1990).

²⁵² In his study "Hurrian Consonantal Pattern", Purves (1941: 379) remarked: "The pattern concerned is generally understood as one in which stops were voiceless when initial and when doubled but voiced medially when occurring undoubled after

uids (*l/r*), Neu felt able to propose a connection to the Greek name for Cyprus, Κύπρος.



Ancient Tartessos and the surrounding area.²⁵³ Nearly one hundred Tartessian inscriptions survive, from the southwest of the Iberian Peninsula.²⁵⁴

vowels or adjacent to the sonants *l*, *m*, *n*, or *r*. These rules are not without their exceptions. Since this stop pattern apparently resulted from Semitic hearing of Hurrian, it was concluded that, phonemically, Hurrian had only one set of stops, the actual phonetic difference in voicing being unrecognized by speakers of Hurrian but readily perceived by Semitic listeners to whom difference in voicing was phonemic". In fn. 10, Purves claimed: "At present the writer's opinions diverge from the view expounded by Speiser in *Language*, XVI (1940), 319–40, who maintains that difference in voice existed phonemically in Hurrian".

²⁵³ Detail of <https://en.wikipedia.org/wiki/File:Tartessos.png> (itself in colour).

²⁵⁴ https://en.wikipedia.org/wiki/Tartessian_language states: "The Tartessian language is the extinct Paleohispanic language of inscriptions in the Southwestern script found in the southwest of the Iberian Peninsula: mainly in the south of Portugal (Algarve and southern Alentejo), and the southwest of Spain (south of Extremadura and

The likelihood is low of a connection of that non-Semitic lexical type of the name for ‘copper’ to Hebrew *kófer* ‘ransom’ or ‘price for expiation’. The likelihood is high that that Hebrew noun belongs to the family of derivatives of the Hebrew root *k.p.r.* whose archisememe is ‘to cover’. Such co-derivatives include the post-Biblical Hebrew verb *kāfar* for ‘to deny a statement’ or ‘to renege’ or ‘to reject a belief’ (cf. Arabic *káfara* ‘to blaspheme’), the Biblical Hebrew verb *kippēr* ‘to expiate’ (and related nouns), the Biblical Hebrew noun *kappōret*, ‘cover’ (the golden cover of the Ark of the Covenant), probably the Biblical Hebrew noun *kófer* ‘pitch’²⁵⁵ (as the function of that stuff is for covering and making impermeable), probably the Biblical Hebrew noun *kāfōr* ‘frost’ (as it covers the earth, trees, and objects in the open). By one opinion, also *kāfir* ‘young lion’ belongs to this family of co-derivatives, by reckoning that it is at an age when the male’s mane begins to grow.

And yet, the Biblical Hebrew²⁵⁶ plant-name *kófer* ‘Lawsonia’, ‘henna’ (or more generally, plants of family *Lythraceae*)²⁵⁷ is related to Latin *cyprus* and Greek κύπρος in the botanical and *materia medica* sense. Akkadian had *kupura*, in Ebla one comes across *kā-pá-lu* or *kaparum*, whereas Middle Aramaic possessed *kufrā*, in the same sense. Cf. Coptic *kuper* or *kufēr*, whereas Demotic Egyptian has *ḳupre*. (In contrast, in

western Andalusia). There are 95 of these inscriptions, the longest having 82 readable signs. Around one-third of them were found in Early Iron Age necropolises or other Iron Age burial sites associated with rich complex burials. It is usual to date them to the 7th century BC and consider the southwestern script to be the most ancient Paleo-hispanic script, with characters most closely resembling specific Phoenician letter forms found in inscriptions dated to *c.* 825 BC. Five of the inscriptions occur on *stelae* with what has been interpreted as Late Bronze Age carved warrior gear from the Urnfield culture. [...] The most confident dating is for the Tartessian inscription (J.57.1) in the necropolis at Medellín, Badajoz, Spain to 650/625 BC. Further confirmatory dates for the Medellín necropolis include painted ceramics of the 7th–6th centuries BC. In addition a graffito on a Phoenician sherd dated to the early to mid 7th century BC and found at the Phoenician settlement of Doña Blanca near Cadiz has been identified as Tartessian by the shape of the signs”. Cf. e.g. Correa (1989), Prósper (2014)

²⁵⁵ *Genesis* 6:14., where Noah is ordered to cover (/w-*kaparta*/) the Ark with pitch (/kopr/ *kófer*).

²⁵⁶ *Song of Songs* 1:14, and in the plural, 4:13.

²⁵⁷ See the chapter about *Lythraceae* (sic) in Löw (1924–1934), Vol. 2: 218–225.

Mishnaic Hebrew, *qafrišīn* denotes ‘capers’,²⁵⁸ and in Modern Hebrew, *Qafrišīn* ‘Cyprus’.)

The Hebrew name for ‘copper’ is *nʰóšēṭ*. Consider the Brazen Serpent (*nʰaš hannʰóšēṭ*) of *Numbers* 21:9 and *2 Kings* 18:4. Is there any etymological and semantic relation to *nāhāš* ‘snake’? Was the motivation for the name of the metal because of the shape of the ore inside the rock? Is it serpentiform in the region? Apparently this is (at least sometimes) the case of chrysocolla, a copper ore, in the Timna Valley in southern Israel, as shown in the photograph. The ore is mixed with Cambrian sandstone. This was the site of ancient, Chalcolithic mines.



Chrysocolla (a copper ore) at the site of a Chalcolithic mine in the Timna Valley, Israel, is shown here in the darkest shade of grey, and is somewhat serpentiform in that it tends to appear roughly in a long line, albeit with smaller patches here and there. Turning the colour photograph the way it appears in Wikipedia²⁵⁹ into greyscale required manipulating how dark green would appear, because in the original colour photograph, the finger casts a black shadow and touches a whitish brown area, whereas the area in the forefront is light brown, yet darker, sand. The copper ore appears there in two stripes of light green. Just turning the colour photograph to greyscale makes the light green and light brown indistinguishable.

²⁵⁸ See the chapter about *Capparidaceae* in Löw (1924–1934), Vol. 1: 322–331.

²⁵⁹ https://en.wikipedia.org/wiki/File:Chrysocolla_Timna_070613.jpg

A factor that militates against this hypothesis is that the lexical type of this name for ‘snake’ only occurs in Hebrew and, in metathetical form, in Arabic and Egyptian. By contrast, the lexical type of *n^ohóšēṭ* is more widespread, being found in Hebrew, Phoenician, Aramaic (and Syriac), Arabic, and Ethio-Semitic.

A counter-counter-argument is that the name for the metal spread through trade. The Biblical Hebrew participial adjective *nāhūš* ‘brazen’ is also used metaphorically for ‘hard’ as well as for ‘stubborn’ (in the feminine, applied to the forefront in *Isaiah* 48:4), whereas in Israeli Hebrew it is used (apart from the latter collocation) in the sense ‘determined (to achieve a goal)’.

Copper occurs naturally as native metallic copper and was known to some of the oldest civilizations on record. The history of copper use is at least 11,000 years old, estimated to have begun in 9000 BC in the Middle East; a copper pendant was found in northern Iraq that dates to 8700 BC. Evidence suggests that gold and meteoric iron (but not iron smelting) were the only metals used by humans before copper. The history of copper metallurgy is thought to follow this sequence: 1) cold working of native copper, 2) annealing, 3) smelting, and 4) the lost wax casting. In southeastern Anatolia, all four of these techniques appear more or less simultaneously at the beginning of the Neolithic c. 7500 BC.

Just as agriculture was independently invented in several parts of the world, copper smelting was independently invented in different places. It was probably discovered in China before 2800 BC, in Central America perhaps around 600 AD, and in West Africa about the 9th or 10th century AD. Investment casting was invented in 4500–4000 BC in Southeast Asia and carbon dating has established mining at Alderley Edge in Cheshire, UK at 2280 to 1890 BC. Ötzi the Iceman, a male dated from 3300–3200 BC, was found with an axe with a copper head 99.7% pure; high levels of arsenic in his hair suggest his involvement in copper smelting.²⁶⁰

In an article about the name, *Eire*, of Ireland (*Ériu* in Old Irish, from an older ⁺*twerijū*), Vennemann (1998c) proposed a Semitic etymology ⁺*y-wr'(m)*, whose vocalisation he tentatively suggested was ⁺*iy-weri'um* with the sense ‘Isle of copper’. He based this upon an Akkadian name for ‘copper’, *weri'um*. Vennemann reckoned that it would have been a fitting name, because copper mining in Ireland for export dates back as far as the second millennium before the Common Era. It is an interesting idea, and I would like it to be true, even though I do not feel fully confident that this is what actually came about to pass.

²⁶⁰ <https://en.wikipedia.org/wiki/Copper>

Vennemann made the important argument that his interpretation is in harmony with a traditional explanation of the name *Britain* as ‘Tin Islands’, in Greek κασσιτερίδες νῆσοι, from κασσίτερος ‘tin’.²⁶¹

Strabo 3.5.11 claimed that the Cassiterides, the “tin islands”, were exploited but kept secret by the Phoenicians, who wanted to protect the sources of their trade. In 1676, the lawyer Aylett Sammes (c. 1636–1679) published the book *Britannia antique illustrate, or, The Antiquities of Ancient Britain, Derived from the Phoenicians*. Sammes claimed that the Phoenicians settled in the Isles of Scilly (of the western tip of Cornwall), as well as in Cornwall and in Devon, “mining tin and trading it to the Mediterranean” (Quinn 2018, pp. 182–183). Sammes (1676, pp. 39–41) claimed that local place-names in those territories were of Phoenician origin. He identified the Isles of Scilly with Strabo’s Cassiterides (Quinn 2018: 183). Based on an article by Timothy Champion (2001), Josephine Quinn remarks in her book about evolving ideas about the Phoenicians (2018, n. 45, pp. 265–266): “Champion (2001, 454) points out that this interpretation of Strabo ignores Diodorus’ report that the tin trade of Cornwall was locally controlled and that tin from Cornwall came overland through France (Diod. Sic. 5.22). The Cassiterides of the ancient sources are probably islands off the Atlantic coast of Spain”. Is that warranted by geology?

²⁶¹ By the way, Old Armenian *anag* ‘tin’ has been etymologised by Igor Diakonoff (1985) from Hurrian **anagi*, itself a loanword from Akkadian *a(n)naku* ‘tin’ or ‘lead’, which in turn is etymologised from Sumerian AN.NA, i.e., *an-(n)a*. Biblical Hebrew has the noun *ānāk* ‘plumb line, plummet’ (whence the Modern Hebrew adjective *ānākī* [ʔana’xi:] ‘vertical’). Aramaic used to have *an(n)kā* ‘tin’. Concerning the etymology of Old Armenian *anag* ‘tin’, Diakonoff stated (pp. 598–599): “A Hurrian mediation is here nearly a certainty, because only in Hurrian but not in Urartian and Aramaic is *-k- in medial position reflected as -g-“.

Also consider Sanskr. *nāga-* ‘tin’, a Mesopotamian loanword in Sanskrit, or rather both one of the Sumerian forms, and the Sanskrit term originating in another culture: “The Sumerian AN.NA has the readings *an-na* and *nag-(g)a*, *nig-(g)i*, both of which have been borrowed” into other languages (Diakonoff 1985: 598, fn. 14). Diakonoff pointed out that “it is not impossible that both Sum. *nag-(g)a* and Sanskr. *nāga-* might be borrowings from the same common source (the language of the Harappa-culture?), and that only *an-(n)a* is genuine Sumerian. Akkad. *a(n)naku* means ‘tin or lead’ [...], and *an-(n)a* is even ‘iron’ (cf. A. A. Vaiman, “Eisen in Sumer”, *AfO [Archiv für Orientalforschung]* 19 (1982), S. 33–38). But Arm. *anag* cannot be etymologized from either Sanskr. *nāga-* or Sum. *nag-(g)a* but only from Akkadian through Hurrian” (Diakonoff 1985: 599, fn. 14).

Concerning Greek *κασσίτερος*, it is the etymon of Latin *cassiterum* ‘tin’, which in turn has been shown by Alinei (2001b, Sec. 4.2.5, pp. 36–40) to be the etymon of some Italian dialectal names (in the Trentino, Veneto, and Romagna regions) for particular kinds of metal vessels (copper vessels indeed). There are words whose etymon is *κασσίτερος* in the Balkans, and Alinei proposes that the lexical type spread from the Balkans into northeastern Italy, towards the territory of the Villanova culture.

Concerning the Akkadian name for ‘copper’, I quote the following from Rubio (1999), who in a note to his mention of the Sumerian metal name *urudu* ‘copper’ (cf. the Israeli Hebrew neologism *ārād* ‘bronze’),²⁶² wrote (*ibid.*: 9, fn. 20):

Piotr Michalowski has pointed out to me that *urudu* has a good Semitic origin, as a feminine form of the same root of Akkadian *werûm* ‘copper’. This feminine (**warûtum*) was borrowed from a Semitic language other than Akkadian and it is reflected in the ED form a-ru₁₂-da (see *PSD* A/1: 161–62).²⁶³ Interestingly enough, the most common Semitic word for copper is feminine in Northwest Semitic languages: Heb. *nəḥûšāh/nəḥošet*; Aram. *nḥāšā*; Phoe. *nḥšt*; Mand. *nhaša*. The root of *werûm* seems to be attested in other Afroasiatic branches, especially Chadic, in which it means ‘iron’ (see Orel Stolbova 1995: 16 no. 55).

²⁶² As for Biblical Hebrew, the name for ‘copper’ also denoted ‘bronze’, because for such objects (such as weapons) for which what mattered was their strength, there had been a transition to using bronze instead of copper. In general, when we meet with variation in the names for a given metal within Semitic, there may have been any of various reasons conducive to difference or to differentiation. For example, consider Yona Sabar’s study (1984) of the Arabic component in mostly homiletic texts (in the Hebrew script) in a dialect (or dialects) of Jewish Neo-Aramaic, from two towns of Iraqi Kurdistan. “The Neo-Aramaic manuscripts from which these Arabic elements were gleaned were mostly copied in the second half of the seventeenth century. The texts, however, indicate an earlier tradition of literary style, whose traces are well observed in the religious literature of the Kurdish Jews to the present day. The multilingual vicinity of the towns of Nerwa and ‘Amādīya, with its cross-section of various ancient and new cultures, Hebrew-Aramaic, Kurdish, Persian, Turkish and, particularly, Arabic, had made a strong impact on this literary style” (*ibid.*: 201). Sabar remarked about a metal name: “Some loanwords have developed a new meaning, such as *nuqra* ‘silver’ (<‘silver ingot’)” (*ibid.*: 208). That is to say, a word that in Arabic denotes ‘silver ingot’ was borrowed into the given dialects of Neo-Aramaic, where it denotes ‘silver’ instead.

²⁶³ *PSD* stands for *The Sumerian Dictionary of the University Museum of the University of Pennsylvania*, published from 1984 to 1998, and edited by Åke W. Sjöberg.

The overall thrust of the argument in Rubio (1999) is to refute some claims about a pre-Sumerian substratum in Sumerian. Among the other things, Rubio discussed Indo-European etymologies: “Within the framework of lexical borrowing, Gamkrelidze and Ivanov (1995) have proposed several connections between Sumerian and Indo-European words” (Rubio 1999: 9). Rubio considered their relevant entries. One of them is the following, quoted here the way it appears and is refuted in Rubio (1999: 10):

urudu: IE **h₁reud^h-*, **h₁rud^h-ro-*, **h₁roud^ho-* (Beekes 1995: 143) or just **reud^h-*, without initial laryngeal (Pokorny 1959: 872–73), “red; red metal, copper” (Gamkrelidze and Ivanov 1995: 616–17, 773, 862) – admitted by Diakonoff (1984: 48), who points to a possible “pre-Sumerian” substratum. As seen above, urudu is probably a Semitic loanword from a feminine form (**warûtum*) of the root of Akkadian *werûm* “copper”.

Rubio (1999, pp. 10–11) then remarked:

In most cases (gud/gu₄, gígir, urudu, etc.), even if one accepts the relation between these Sumerian words and some Indo-European ones, the direction of the borrowing would be unclear. Are these words Indo-European loanwords in Sumerian, or Sumerian loanwords in early Indo-European? It is also possible that both Indo-European and Sumerian had borrowed these words from a third language – or even that these words are mere look-alikes. The criteria for the direction of borrowing are frequently quite difficult to establish (see Anttila 1989: 158–60), and the very small number of (rather uncertain if not unlikely) words in this list is not enough to prove an early contact²⁶⁴ between Sumerians and Indo-Europeans.

Rubio concluded his influential study as follows (1999: 11):

The picture of the linguistic situation of Mesopotamia in early periods should be that of fluidity, of words traveling together with the objects and techniques they designate (*Wanderwörter*, *Kulturwörter*), of different languages and their dialects (most of which have left no traces or just a few, from toponyms to loanwords, in surviving languages), all of them sharing the same space and perhaps even sometimes the same speakers. Thus, there is no monolithic substratum that would have left, in a sort of primeval age, its vestiges in the Sumerian lexicon. All one can detect is a complex and fuzzy web of borrowings whose directions are frequently difficult to determine. Furthermore, and from a theoretical point of view, one should not overlook that the search for origins (*Ursprache*, *Urheimat*, etc.) is an intellectual construct of the past – fre-

²⁶⁴ As opposed to an absence of contact, with individual words wandering from one linguistic area to another which was not bordering on it. Yet another kind of cultural indirect connection is correlation of events in regions far apart, such as a domino effect throughout Asia. The classic work on such correlation is Teggart's *Rome and China: A Study in Correlations in Historical Events* (1939).

quently a misconstruction of it – and belongs to the realm of our concerns as scholars rather than to the world of events.

Gordon Whittaker (2008)²⁶⁵ discussed what he considered Indo-European loanwords either in Sumerian, or “orphan” logograms whose

²⁶⁵ Gordon Whittaker and Gonzalo Rubio are at odds with each other professionally, as Rubio dismisses the hypothesis of a pre-Sumerian substratum, thus taking issue with several earlier scholars. Whittaker (2008: 157) claimed: “the major flaw in the standard view [...] is the assumption that at the dawn of history Southern Mesopotamia was home to a pristine and pure population of Sumerians and that, if any evidence at all for the presence of the Sumerian language can be discerned in the archaic tablets of Uruk, all arguments for the presence of other languages and ethnic groups are demolished. This flies in the face of all that we know about the ethnic history of Mesopotamia down to the present day. The land has always been a crossroads of civilization and throughout the entire span of recorded history it has been home to a variety of ethnic groups living side by side. Why should it have been different in the 4th millennium?” Rubio (1999: 6–8) criticised Whittaker (1998) as follows: “In a recent article, Whittaker (1998) has attempted to identify the pre-Sumerian substratum (Landsberger’s ‘proto-Euphratic’) with an until now unknown Indo-European language, which would be the earliest attested language of this family. One has to admit that this IE language would fit in the most commonly accepted evolution of IE phonology and morphology, as a sort of ‘pre-Anatolian’ Indo-European. Whittaker’s earliest IE language would still retain this feminine suffix **-a* (< **-eh₂*), which is not attested in the earliest IE branch, Anatolian. It is commonly accepted that Anatolian lost this ending, since traces of it are present in almost all the other languages [...] Moreover, Whittaker’s earliest IE would have conserved laryngeal consonants, which remained only in the Anatolian branch [...] Although Whittaker’s earliest IE does not confront our expectations of a possible pre-Anatolian IE language, there are substantial problems in his reconstructions. Among his proposed two hundred IE loanwords, only a few belong to what has been called the pre-Sumerian substratum, but many are words whose phonotactic structure looks perfectly Sumerian [...] Whittaker bases all his comparisons on a very complex reconstruction of ‘proto-Sumerian’, which would have thirty-eight consonants and six (or nine) vowels. [...] he reconstructs palatalized counterparts for almost every single consonant. [...] A far more important problem, which Whittaker does not mention, is merely typological: as a basic phonological principle, marked phonemes are less frequent than unmarked ones. Thus, one would expect voiceless stops (unmarked phonemes) to be more frequent than voiced stops (unmarked phonemes). [...] Whittaker’s hypothesis presents a more important problem: its faulty methodology. His ‘proto-Sumerian’ is based mostly on the phonological shape of the alleged Indo-European loanwords (although also sometimes in Emesal correspondences), but the borrowings are established on the basis of this hypothetical reconstruction of ‘proto-Sumerian’ phonology”, this being a “form of circular reasoning”. Rubio (1999: 8) claimed it would have been too early (why that? Spell out the underlying assumption) for there being direct contact with Indo-Europeans,

visual motif associated a sound with what appears to be an Indo-European word (typically, an animal name), but which has no semantic motivation in Sumerian itself. Whittaker considered such occurrences as evidence for the earliest Indo-European language of which there is any record, and he called it *Euphratic*. Among the other things, Whittaker wrote: “Among the Indo-European loans in Sumerian are several sets with well-known derivational suffixes, such as *-ti-* above” (2008: 162). “Of greater interest are the cases in which the suffix *-ah₂*, attested but only indirectly in Anatolian, occurs. A selection of these terms follows” (*ibid.*). “Another colour term was **huc** ~ *ruc*, Akk. *huššû* ~ *ruššû*, ‘reddish, ruddy; furious, angry’ from **h₁rusto-* (< **h₁rudhto-*) ‘red, ruddy’” (*ibid.*). Of **h₁rusto-*, Whittaker claims: “The latter term in Indo-European probably relates to the ruddy colour of copper ore (cf. Early Dynastic **hašum**, glossed ‘ore?’ in the ePSD,²⁶⁶ < **h₂aġ-s-om* ‘copper’ with *ġ* becoming Sum. **š**; the IE *s*-stem neuter has been rebuilt in Euphratic on the analogy of **h₂aŷs-om* ‘gold’ and **h₂arġ-nt-om* ‘silver’” (*ibid.*). Next, Whittaker turned to Sumerian *urud*, by stating a problem, and answering it affirmatively, though tentatively: “It remains to be seen whether Sum. **urud**, Early Dynastic **a-ru₁₂-da**, ‘copper’ derives from IE **h₁rud^h*- ‘ruddy’ (rather than the reverse as occasionally suggested). Given the Akkadian equivalent, *werium* (with Akk. *-um*) ‘copper’, it seems possible that both derive independently (with vowel harmony in the Sumerian) from an IE **ueġh₁-r-ġo-s* ‘wire’ (cf. IER 96),²⁶⁷ related to Celtic and Germanic terms for the same” (*ibid.*).

Let us turn to what Noam Agmon was stating about the etymologies provided in the Etymological Appendix (EA) to Agmon (2010), by Yigal Bloch: “We believe that most of the reconstructions in the EA would be considered commonly acceptable, with a few exceptions

but he did not “rule out the possibility of some isolated IE loanwords in Sumerian, [...] as scattered *Wanderwörter* or *Kulturwörter*”.

²⁶⁶ ePSD stands for the *Electronic Pennsylvania Sumerian Dictionary*, at <http://psd.museum.upenn.edu/epsd/>

²⁶⁷ IER stands for *The American Heritage Dictionary of Indo-European Roots* by Calvert Watkins (2000).

(such as **šupr* ‘yellow; copper’)²⁶⁸ for which we think there is nevertheless reasonable evidence.

Agmon’s (2010) discussion of the early Semitic terminology of metals. The following is quoted from p. 43:

Commensurate with lead being the first metal to be smelted from ores, which were likely first crushed to powder, its PS [i.e., Proto-Semitic] name, **abār* or **āpart*, may derive from PS **apar* ‘soil, dust’ (but also ‘ore’, see EA). In contrast to lead, ‘copper’ (PS **šupr*, PWS [i.e., Proto-West Semitic] **nuḥāš*) requires higher temperatures (ca. 1,100 °C). Copper smelting in the Tauros-Zagros belt (Anatolia-Iran) started over 7,000 years ago. But (unlike the previous erroneous dating of copper smelting in Timna, Israel, to the same period) smelting in the Southern Levant (with ores from Timna, Israel, and Feinan, Jordan) began only in the Early Bronze, ca. 5,500 BP [i.e., Before Present] (Hauptmann 2003: 91–92). It is tempting to ascribe PS **šupr*, which is not attested in Semitic languages of the Levant (except as a loanword), to the early phase of copper extraction in Anatolia. It is likewise tempting to ascribe PWS [i.e., Proto-West Semitic] (dated to 5,400 BP by Kitchen et al. 2009) **nuḥāš* to the copper endeavors of the S[outhern] Levant. The high temperatures required for copper smelting are only achievable using charcoal furnaces. Charcoal (nearly pure carbon) has an additional role in this process, in reducing CuO to metallic Cu. Thus, although there is no field evidence allowing us to date the onset of charcoal production, it can tentatively be attributed to the PN [i.e., Pottery Neolithic] or Chalcolithic periods. Indeed, ‘charcoal’ has a 3c [i.e., triconsonantal] reconstructed PS name, **paḥm*.²⁶⁹

In his Etymological Appendix to Agmon (2010), Yigal Bloch stated in fn. 56 on p. 70 that “[t]he etymological connection between *ʿašfar* “yellow” and *šufr* “copper, brass” in Arabic is beyond doubt, and it appears that the rare meaning “gold” for *šufr* in Arabic derives from the yellow color of copper and bronze objects, which resembles that of gold (*AEL*: p. 1697b).²⁷⁰ Bloch also claimed in fn. 57 on pp. 70–71 the following, concerning terms for ‘copper’:

According to *CAD* [i.e., Gelb et al. (1956), *The Assyrian Dictionary*...] E: 323a, Mesopotamian scribes differentiated between the terms for copper (Akkadian (*w*)*erū*, Sumerian URUDU) and bronze (Akkadian *siparru*, Sumerian ZABAR, (UD.)KA.BAR), although in later texts the sign URUDU was sometimes used as a determinative for

²⁶⁸ It is based upon Akkadian *siparru* ‘copper’, along with the Arabic name for ‘copper’. Ugaritic *spr* for ‘bronze’ is a loanword from Akkadian. Incidentally, also note the Akkadian name Akkadian *qū* ‘bronze’.

²⁶⁹ Bomhard (1981: 407–408) tried a comparison of Semitic terms for ‘charcoal’, to semantically different Indo-European terms (for ‘fire’), but is rather far-fetched.

²⁷⁰ *AEL* stands for the 1984 edition of Lane’s 1984 *An Arabic-English Lexicon* (originally of 1863–1893).

bronze objects. However, as argued by Brinkman 1988: 136–138, by the first millennium BCE both (*w*)*erû* and *siparru* could be used with reference to either bronze or copper. It does appear that (*w*)*erû* (URUDU) was generally used for copper, and *siparru* (ZABAR) for bronze (i.e., an alloy of copper and tin), but it is not clear whether such a semantic distinction already existed in pre-historic times; the situation is further complicated by the existence of arsenic bronze, produced from copper ores which had a naturally high admixture of arsenic (Reiter 1997: 288, suggested that arsenic copper was called URUDU/(*w*)*erû*, but that is unclear). Akkadian *siparru* has often been considered a loan from Sumerian ZABAR, or both these forms have been considered as reflexes of a Kulturwort—i.e., a word denoting a specific commodity spread over a number of languages in an area where that commodity was traded, without a possibility to find out the language in which such word originated. In keeping with this hypothesis, Salonen 1952: 7–8 listed Arabic *şufr* as another manifestation of the same Kulturwort. However, given the difference in the vowel patterns of *şufr* and *siparru*, and the likely etymological connection between *.sufr* and Aramaic *şaprā*⁷ “morning” (which is not a Kulturwort), it appears that *şufr* and *siparru* are genuine Semitic cognates. This implies that *siparru* is the origin of Sumerian ZABAR rather than the other way around.

9. Names for Coins

Names for coins are the subject of three chapters in *Germania Semitica*, namely: Ch. 23 (with Ch. 25, possibly the best chapter in the volume), “*Münze, mint, and money: An etymology for Latin Moneta. With appendices on Carthaginian Tanit and the Indo-European month word*” (447–465); Ch. 24, “*Ne’er-a-face: A note on the etymology of penny, with an appendix on the etymology of pane*” (467–484); and Ch. 25, “*A note on the etymology of Germanic ⁺skillingaz ‘shilling’: With an appendix on Latin siliqua ‘a small coin’*” (485–495).

Concerning the Germanic word ⁺*skillingaz* or ⁺*skillingaz* ‘shilling’, Vennemann surveys four etymological hypotheses from the scholarly literature, and proposes his own interesting etymology, “which assumes the word to be a Semitic loan-word, viz. an adaptation of the Phoenician form of the *shekel* word, whose spelling *şql* he reads +[sə’kel] (485, 487), “by means of the affixation of the same suffix *-ing-* that also occurs in the names of other coins” (485). The heaviness of the root syllable is preserved, and *-az* is added, “preserving the masculine gender of the Semitic model” (485). “The quality of the proposed etymology is underlined by the fact that the two meanings given in the literature for the Semitic and the Germanic word are the same for both: (1) ‘a seg-

ment of fixed weight (of precious metal)', and (2) 'a certain coin'" (485).

Vennemann's reasoning that led him to the reading +[sə'kel] of the Phoenician form of the *shekel* word appears on pp. 487–488. I would recommend to consider, firstly, that the *shekel* word is a segolate word; secondly, the word-forms that in Aramaic occur for segolate words; and thirdly and most importantly, the formal model in Malone's study (1971) "Wave Theory, Rule Ordering, and Hebrew Aramaic Segolation". I suspect that a more solid argument for Vennemann's reading could emerge from Malone (1971).

Vennemann's etymology of the *shilling* word – from the *shekel* word – might be the right one, but it is costly because of the lack of Phoenician or Punic inscriptions in what were Germanic lands. At any rate, note that for example Roman coins have been found in areas where not only any Roman army, but also any Roman persons perhaps never arrived.

"Hellenistic and Roman coins have been found throughout southern Africa, but, as is the nature of sporadic coin finds, these are probably casual removals", Roller (2006) remarks on p. 115, fn. 3 (citing Mauny 1954; cf. Mauny 1956). Finding just one coin "must be regarded as random loss", this being, e.g., the case of a coin from Khurasan found in Amantea in Calabria, Italy, at a time when it was an emirate (Heidermann 1997: 219). Concerning Roman coins, Ramin states in his *Le Périple d'Hannon* (Ramin 1976: 74): "The discovery in the Congo of a coin of the time of Trajan [p. 106, note 90: 'Rivista Italiana di Numismatica, VI, 1893: 505'] doesn't prove that the Romans went to tropical Africa. Such a find merely indicates that there were trade links with the region [...]". It doesn't even mean that the Roman coin arrived in Roman times into the place where it was found. By contrast, quantity tells a different story: "From the beginning of the tenth century a large number of Arabic coins (the latest estimate for Sweden is 80,000) appeared in Scandinavia; at an earlier period Arabic coins had appeared in the Baltic lands, but rarely in Sweden", on p. 172 in Wilson's "The Viking Adventure" (2003). How far the reach of trade can get is also shown by a report about a Roman phalera from the Urals region of Russia, near Perm (Kolobov et al. 2000).

In his appendix to Ch. 25, Vennemann derives Old High German *silihha* (a coin's name) from Latin *siliqua* as being a name for a type of coin (494, note 15), but he also claims that this particular sense of Latin

siliqua did not simply derive from the botanical sense, but “that in Latin we are dealing with more than one word of the shape *siliqua*” (489). He proposes that like the Greek forms of the borrowed Semitic *shekel* word, *siliqua* as a name for a small unit of measure or coin was derived from the semitic *shekel* word. Vennemann claims that it “was borrowed into Latin as ⁺*siquila*, *-ae* and metathesized to become Lat. *siliqua*, perhaps to improve the phonotactic structure of the word, perhaps under the influence of the botanical *siliqua*” (489–490).

Let us turn to commenting about Vennemann’s Ch. 23. Latin *Monēta* was an epithet of the goddess Juno. Traditionally, that epithet has been interpreted within Latin, in relation to the function of providing admonition (*monēre*, ‘to admonish’). According to Vennemann, the goddess *Monēta* is to be identified with “the wide-spread name of a Mediterranean Fortuna goddess that appears as *Mēnī* in Hebrew²⁷¹ [cf. Nissan (in press), which is about the fortune theonym *Gad*, but also deals with both *Mēnī* and Fortuna],²⁷² as *Manūtu* (*mnwtw*) in Nabataean, as *Maniyyāt* and *Manāt* in Arabic, and as a by-name *Menūtum* of the Mesopotamian goddess Ishtar, who is herself identified with Juno as a protectress of cities (Ištar *Menūtum* = *Iūnō Monēta*)” (447). Vennemann relates this to both such terms as Hebrew *mānē* = Akkadian *manū* = Latin *mina* = Greek *μνα* (a unit of weight, which was also used in order to weigh precious metals, or frankincense), and the Semitic root *m-n-w* or *m-n-y* or *m-n-h* ‘to count, to apportion’. Cf. an 11th-century Hebrew gloss by Rashi, for *la-Mēnī* at *Isaiah* 65:11, is as follows: “to the *minyān* (number), to the computation (*hešbōn*) of the pagan priests, you fill vessels of poured wine”.

I would like to signal, in early rabbinic Hebrew, the idiom “*monīṭin* went out for him”, i.e., ‘he became famous’, ‘coins were struck with his

²⁷¹ The *New King James Version* renders that verse as follows: “But you are those who forsake the Lord, Who forget My holy mountain, Who prepare a table for Gad, And who furnish a drink offering for Meni” (*Isaiah* 65:11).

²⁷² Hypotheses tend to be risky, and there is a 19th-century tradition of relating things Norse or Germanic to things Semitic. In Nissan (in press, Sec. 2.1), I remarked: “Concerning Meni (*Mēnī*) in *Isaiah* 65:11, Alexander Hislop in the third chapter of *The Two Babylons* (as per later editions) went as far as comparing him to the Old Norse moon deity from the *Edda*, *Mané* (presumably, because of the phono-semantic match). *Caveat emptor*. Hislop’s book, whose first edition appeared in 1853, is erudite but with a clearly crankish streak: it is subtitled *or the Papal Worship: Proved to Be the Worship of Nimrod and His Wife*”.

name and face’, where *monīṭin* < Latin *monēta* ‘coin’²⁷³ In Israeli Hebrew, *monīṭin* [moni’tin] denotes ‘prestige’. The idiom “*monīṭin* went out for him” is still in use, in the sense ‘he became famous’ (positively connotated).

In Appendix 1 to Ch. 23, Vennemann reconstructs the name of Carthage’s city goddess Tanit as deriving from **Ta-Mnīt*, but I am not fully convinced; I am open to that possibility, but am cautious. Vennemann needs Punic **Mnīt* in order to derive *Monēta* from it. Tanit’s face was “shown on just about every Cathaginian coin” (457). Istar/Astarte had been superseded by Tanit in Carthage (457).

In Appendix 2 to Vennemann’ Ch. 23, “the Germanic *month* word, based on the name of the moon as the counter of time, is compared to the Semitic *m-n-w* root and its derivatives, among them Akkadian *minūtu(m)* ‘counting of time, especially calculation of time by means of the stars’” (447). “Unfortunately the exact meaning ‘month, moon’ does not seem to be recorded for derivations of the root ⁺*m-n-w* in Semitic but only the original meaning of counting” (460).

Interestingly, in Aharon Dolgopolsky’s *Nostratic Dictionary* (2008), there are many, semantically disparate, Nostratic roots displaying the sequence of consonants *m...n...* We are only going to consider a few of them, which are the ones most relevant here. One of them, on p. 1346, is relevant for the Semitic root *m-n-y*, and is reproduced here. The archisememe of the Nostratic root **meṇ∇* (where ∇ stands for an unspecified vowel) is ‘tear, tear into pieces, divide’, and it occurs in Uralic (but only within Ugric), as well as in Semitic. In the latter however, Dolgopolsky claims, the derivatives (such as Hebrew *mānā* ‘portion’) resulted from contamination of the given Nostratic root **meṇ∇* in its reconstruction Hamito-Semitic form **manay-* or **manat-* with the root *m-n-y* ‘count’.

²⁷³ Concerning the Latin term, see Hubert Zehnacker’s book *Moneta* (1973). Also consider Latin *familia monetalis* or *familia monetaria* (Bernareggi 1974, Lafaurie 1972). As for the nummularii and monetary policy in the Roman empire in the fourth century, see Vera (1974).

1424. ₂^* **meñ**∇ 'tear, tear into pieces, divide' > **U:** FU (in Ugr only) ° **meñ**∇ 'tear, tear into pieces' > ObU ° **mēñ**- (Ht.) ° **mēñ**- v. 'tear' > pVg ° **māñ**- > Vg: ML **māñit**-, UL **maniṽt**- id., T **māñam**, Ss **manam**- id., T/MK **māñamt**-, LK **māñamt**- id.; pOs {Ht.} ° **meñ**-, {Hl.} ° **māñ**- > Os: D **mēñemə**-, K **māñimə**-, Nz/O **menəm**-, Kz **meñəm**- 'abreißen', D **mēñč**-, K **māñč**-, Nz **māñšə**-, Kz **māñšī**-, O {Hl.} **māñsi**- 'zerreißen' † Hg Δ **ki-mēñyūl**- 'verrenkt werden, sich verrenken', **ki-mēñyūt**- 'ver-/aus-renken' ¶ UEW 870, Ht. #391, Hl. rHt 71, 75 || **HS:** S ° **manay**-, ° **manat**- 'share' (× S ° **mnny** 'count') > BHb **ḥḥḥ mā'hā**, Ug **mnt**, IA/Plm **mrh** 'share, part, portion', JEA em. **ḥḥḥ māḥā**-ā (cs. **ḥḥḥ māḥā**-ā) 'share', Ar **مَنْى** **manā-n** 'fate' (→ 'death'), du. **manawāni** ~ **manayāni** 'two pounds (unit of weight)', Ak fOAK **marū** ~ **manawū** 'mina (unit of weight)', Ar ° **mnw** TL (pf. **تامانā** **tamānā**, ip. **يَتَمَانِي** **yatamānā**) 'se partager qch. en séparant avec les doigts', Ak ° **mnwly** G (inf. **marū**), D 'assign', BHb ° **mnwly** D (pf. **ḥḥḥ min'hā**) 'assign (a share), zuteilen', Mn {MA} ° **mnw** 'attribuer', ? Sb **mr** 'bénifice' ¶ KB 567, A #1600, OLS 264, HJ 657, Lv. T III 49, Js. 802-3, Sl. 688-9, BK II 1158-9, Hv. 735, CAD X/1 219-27, MA 62 ◇ The comparison is valid unless the S words are derived from S ° **mnny** v. 'test, count' (< N ° **moñ**∇ 'to test, to think' [q.v.]).

Dolgopolsky (2008) has a different entry, 1427 on pp. 1347–1348, for the Nostratic root ° **moñ**∇ associated with the semantic lexical concept 'many, much'. It occurs within Hamito-Semitic in Cushitic and East Cushitic, and within Uralic and Altaic, and perhaps some other language family. But it is in entry 1428, on pp. 1348–1349, for the Nostratic root ° **moñ**∇ 'to test, to think', that Dolgopolsky (2008) places the Biblical Hebrew and Biblical Aramaic root *m-n-y* 'count' from Semitic ° *m-n-y* ~ ° *m-n-w* 'test, reckon, count'. The Nostratic root of entry 1428 also occurs in Berber and Ead Cushitic, as well as in Narrow Indo-European (i.e., IE excluding the Hittite-Luwian family) as ° *men*- 'think'. Examples of the latter include Greek **μνήμη** 'remembrance, memory'; Latin **meminī** 'I remember, recollect'; Russian **мнить** 'to imagine'; Old, Middle, and New High German **meinen** 'to think, to have an opinion, to mean', and English *to mean*. The same entry also mentions Latin **mēns**, **mēntis** 'mind', Avestan **manah-** 'mind', and the Latin theonym **Minerva** < **Menerva**. Entry 1428 also includes Uralic, and tentatively (and at any rate, by contamination with another Nostratic root) Altaic.

Also see (on p.1355) entry 1433, for the Nostratic root ***mAyñ∇** ‘desire, ask’, occurring in both Indo-European and Hamito-Semitic, as well as in Dravidian and Altaic. Cf. entry 1442 (pp. 1364–1365) for the Nostratic entry ***mAñd∇** ‘excitement, wish, desire’, which includes terms from Indo-European (only Germanic), Dravidian, and perhaps Kartvelian; the Germanic terms are Old High German *mendī* ‘joy, pleasure’, *menden* ‘to rejoice’, *mendōn* ‘to be glad’; Old Norse *munda* ‘to aim (with a weapon)’; and Gothic *mundrei* ‘goal’. But entry 1442 also includes Celtic (Welsh *mynnu* ‘to want, to wish’, Cornish *mennaf* ‘I want’), with the proviso that in that case, the Nostratic root ***mAñd∇** is contaminated with the Nostratic root ***mAyñ∇** ‘desire, ask’.

As for Latin *moneō*, *monēre* ‘to admonish, warn, remind’, along with New High German *mahnen* ‘remind’ and Anglo-Saxon *manian* ‘claim, advise’, Dolgopolsky (2008) included them on p. 1361 in entry 1439 for the Nostratic root ***mañ_∇y_∇** (where _∇x_∇ stands for x or nothing) for the lexical concepts ‘speak, call, invoke magic forces’. That root also occurs, according to entry 1439, in Uralic, Altaic, Dravidian, and perhaps East Cushitic and Central Cushitic (branches of Cushitic within Hamito-Semitic).

In Ch. 24, “*Ne’er-a-face: A note on the etymology of penny, with an appendix on the etymology of pane*” (467–484), Vennemann etymologises “the West Germanic *penny* word (⁺*paning*, ⁺*panning*, ⁺*panding*)”, and in contrast to earlier literature in which “derivations from the ⁺*pand* ‘pawn’ and *pan* words” are “the major candidates” (467), he rather prefers to etymologise from Punic, by assuming that Carthaginians brought about the word. A difficulty with Vennemann’s hypotheses concerning the Carthaginians is that neither on the North Sea coast of the European continent, nor in the British Isles, any Phoenician or Punic inscription was found. This does not exclude that traders ever landed, though, but it makes such hypotheses more costly. The hypothesis concerning penny is enticing, yet even more costly because how additionally speculative it is:

Almost every Carthaginian coin showed the face of the city goddess Tanit (by-named *pn B’l*, reconstructible as ⁺*pane Ba’al* ‘face of Baal’). This suggests that ⁺*pane* ‘face’ was, in the jargon of those traders, an expression for a coin, or for a specific coin, just as *face* was a slang word for a coin in English, according to the OED.

[...] By simply adding the coin-name deriving suffix *-ing* to this base, the form ⁺*paning* [⁺pa.ning] would result. However, if the bi-moric character of the base was to be preserved under accent in the Germanic adaptation, this could be achieved by geminating the *n* or adding *d* to it, yielding ⁺*panning* [⁺pan.ning] and ⁺*panding* [⁺pan.ding]. (467)

Note however that this requires the assumption that it was the status constructus of the Phoenician word for ‘face’ that was involved. The same remark also applies to the following. Moreover, in an appendix to Ch. 24, Vennemann etymologises “the English word *pane* with the meaning ‘a side, section, or portion’, in particular the now obsolete meaning ‘a flat side, face, or surface of an object having several sides’, as distinct from the meaning ‘a piece of cloth’ (< Lat. *pannus*)” (467), from “Phoenician ⁺*pane* ‘face’, transmitted to Middle English through Catalan and French” (467).

The Northwest Semitic terms for ‘face’ is somewhat tricky, because in Hebrew the term belongs to the pluralia *tantum class* (in the *status absolutus* it is *pānīm* ‘face’, and in the *status constructus*, it is *p^enēi-* ‘face of’), and whereas Israeli Hebrew has formed a singular *pān* in the sense ‘facet’ (Vennemann mentions that neologism on p. 474), anthroponomastics suggests that in biblical times, there was a singular *pōn* ‘face’. Moreover, The Biblical Hebrew word for ‘lest’ is *pen*, but in Aramaic it is *pōn*. My main evidence is from a Hebrew singular form of the term for ‘face’ (*pōnī* ‘my face’), incorporated in a compound being a biblical woman’s name, which in early rabbinic homiletics was claimed to be the name of Samson’s mother.²⁷⁴ Upon that evidence, I understand that the fuller inflection paradigm of *pānīm* including its singular form was the same as that of the *yōm* ‘day’, *yāmīm* ‘days’. The following is quoted from Nissan (2014b):

The women in Noah’s household not having their names mentioned is not unique. In the account of how Samson came to be a Naziree from birth, his mother comes across as smarter and more important in the economy of the tale, than his father. And yet, we know that his father was Manoah, but we are not told what his mother’s name was. The biblical text names Samson’s father, but is silent about the mother’s name,

²⁷⁴ I discussed the name *Samson* itself in Nissan (2013 [2014]b). In that article, I stated in fn. 3: “The midrashic literature also attempts to nevertheless give Samson a Judahite ancestry, in that his father’s mother is claimed to have been from the tribe of Judah, and, more conspicuously so, in that Samson’s mother (whose name is not stated, in the biblical account of her) is identified with *Hatslelponi*, a woman from the tribe of Judah”.

notwithstanding her role in the narrative being at least equal, or, as the early rabbinic Sages come to recognize, even superior. In the Midrashic (rabbinic homiletic) literature, it was proposed that her name was *Haṣṣēlelpōnī*, spelled *hšllpwny* (now pronounced *Hatslelponī*), a biblical name of another woman (a Judahite woman in *1 Chronicles* 4:3; in English Bibles, the form is *Hazzelelponī*) – such is the alleged name of Samson’s mother in the homiletic work *Numbers Rabbah*, sec. 10 – or a variant they made up with a feminine suffix *-it*, and by dropping what appears to be the initial definitive article prefix *ha-*, namely: *Šēlelpōnūt*, spelled *Šllpwnyt* (now pronounced *Tslelponūt*), which is the form in the usual editions of the *Babylonian Talmud*, tractate *Bava Batra*, 91a.²⁷⁵ Jastrow (1903, Vol. 1: 363, s.v.), also lists from MS Rome forms spelled *hšllpwnyt* and *Šlwlpwnyt*.

Basically, the identification of Samson’s mother with the biblical woman bearing the name *Hatslelponī* fits in a context intended to posit and emphasise Judahite tribal connection of Samson, even though Scripture explicitly affiliates him with the tribe of Dan. Tradition claimed that both his mother, and his father’s mother were from the tribe of Judah.

My own understanding of the etymological sense of the female name was *Haṣṣēlelpōnī* is “my face’s reflected image”. It is a compound which comprises what appears to be the singular form (*pon*), inflected with a possessive (*ponī*) of the noun for ‘face’ (it usually is a *pluralia tantum* noun in Hebrew: *panīm*; cf. *yom* ‘day’, *yamīm* ‘days’), and a coderivative of the noun (*šel*, /*šell*/) for ‘shadow’, preceded by the definite article.

Concerning *Hatslelponī*, Tal Ilan (2002) does not mention that name, as it wasn’t in use during the period she covered in that volume. Nevertheless, Ilan (2002): 9, §1.1.2.2, remarks: “Many women mentioned in the Hebrew Bible are themselves not named. As a result a complex literature developed,²⁷⁶ beginning with the Second Temple period, in which various names were invented for these women. Obviously these names do not feature in this corpus, because the characters they purport to name date from an earlier period. However, a similar phenomenon is also visible with relation to the New Testament. Several nameless women mentioned therein receive names in Christian apocryphal compositions.”

²⁷⁵ The so-called Soncino English translation – edited by I. Epstein (1935–1948), but this tractate in particular was translated by Maurice Simon and Israel W. Slotki – has this passage (their brackets, my omission in braces): “R. Hanan b. Raba further stated in the name of Rab: [The name of] the mother of Abraham [was] Amathlai the daughter of Karnebo; [the name of] the mother of Haman was Amathlai, the daughter of ‘Orabti; {...} The mother of David was named Nizbeth the daughter of Adael. The mother of Samson [was named] Zlelponith, and his sister, Nashyan. In what [respect] do [these names] matter? – In respect of a reply to the heretics.” That is to say, narrative gaps in Scripture are undesirable, when these could be exploited by heretics. Perhaps it was especially the extravagant use of Scripture as made by Gnostics that was disliked. On the face of it, the text is saying one should have a counter-argument ready, in case somebody would say that the Sages and their tradition were ignorant of details of Scripture.

²⁷⁶ Ilan (2002: 9, fn. 12), cites a paper of her own about that phenomenon in Jewish literature, namely, Ilan (1993).

Vennemann (474), citing various authors for bits of information while trying to explain (all in all sensibly) the vowel variation in the two Greek spellings $\varphi\alpha\nu\epsilon$ Βαλ and $\varphi\epsilon\nu\eta$ Βαλ of the Phoenician phrase for ‘face of Baal’, and whereas it is true that vowel reduction is reflected in the second spelling (also note the contrast of ϵ and η), I would like to call for some caution concerning Vennemann’s use of “colloquial” when he states: “Thus the Punic pronunciation of the word ⁺*panaeh* probably varied between basic [p^ha.’ne] and colloquial [p^hə.’ne]” (474). Perhaps this is correct, but can we be sure that the avoidance of vowel reduction was not instead the “lazy”, thus colloquial option? Take this example. In Israeli Hebrew, [‘mkom/me’kom ha’avo’da] /mqom-ha-ʿboda/ denotes ‘the workplace’. There is a poem by Yehuda Amichai (1924–2000) in which the narrator relates how he goes to the bank to get his salary, and is taken to the basement before being given cash. In the poem, one comes across the deliberately substandard form [ma’kom ha’avo’da], with the first vowel lazily not reduced (so that instead of the *status constructus* with vowel reduction, one gets the same form as in the *status absolutus*). One more consideration is that perhaps it is the transcription into the Greek alphabet that is in the first spelling, “lazy”. We get a hint of that not only from the very rationale of omitting the vowels in Semitic-language scripts (this probably made for better interintelligibility of texts across vernaculars), but especially from the Syriac scriptorial convention of writing a term in its singular form, with two dots above its end to signal that the plural form should be read instead.

I must say that I have published a hypothesis myself interpreting a Phoenician inscription from Tharros in Sardinia, according to what the sense may be in Hebrew (Nissan 2010 [2011]a); in the English abstract of that Italian paper, I stated: “*L^e miqnē-ḩānīm* is an undocumented phrase from Northwest Semitic, let alone a known collocation. Still, could it be plausibly read in the fifth line of an inscription on a silver plate from Tharros? The literal sense would be ‘in order to buy face’, i.e., ‘to curry favour’, ‘to acquire benevolence’, ‘to become deserving of a positive attitude’. Whereas in the case of Vennemann’s hypothesis about names for coins the main weakness is the lack of Phoenician inscriptions in the British Isles, the weakness of my own interpretation of the wording from the Tharros silver plate is that whereas the Hebrew phrase I suggested is possible and makes sense, that phrase is otherwise undocumented, even though its individual words are well-known

from Biblical Hebrew, and then there is the leap of faith (common in Phoenician studies) that something very similar can be assumed to have been acceptable wording with the same sense in Phoenician.

10. The Futhark According to Vennemann

A direction of research pursued by Vennemann with the least potential for controversy (relatively speaking), is his analysis of futhark. The variants of Germanic runic scripts have been used, based on extant inscriptions, in the first millennium of the Common Era (inscriptions on artefacts in the Old Futhark are from the second to eighth centuries), with some usage even in the early modern period.²⁷⁷ The time frame Vennemann adopts for the origins of the futhark is close to the beginning of the Common Era, and ascription to Punic influences is not as conducive to controversy as hypotheses about prehistorical Europe from the end of the last Ice Age to the arrival of Roman conquerors. What still can be controversial is when Vennemann, based on his assumptions, considers Proto-Germanic when in fact what is involved is Germanic coastal vernaculars coeval with the Carthaginian Empire in its final century and half or so. Vennemann's hypotheses about the futhark are more cogent (provided one admits Gaditan or Punic trade on the Atlantic coasts), than current ideas seeking the origins of the Germanic runes in Old Italic scripts, or in the Rhaetic "alphabet of Bolzano" in particular (as suggested by various scholars, and also at a website for teaching purposes, by Jost Gippert²⁷⁸ of the University of Frankfurt) – cf. Prosdocimi (2003–2004) – or then, as proposed by Giuliano and Larissa Bonfante (Bonfante and Bonfante 1983: 119), in the Venetic alphabet (no later than 200 B.C.E.).

The book under review here was published in 2012, and since then two important papers have appeared: "The Mediae (*b g d*) in Punic and in the Futhark" (Vennemann 2013a), and "Vowels in Punic and in Ru-

²⁷⁷ Medieval runes were in used until the 15th century, but in the Dalecarlia region of Sweden, Dalecarlian runes were in use from the 16th century (<https://en.wikipedia.org/wiki/Runes>), and there is a controversy about whether later use in the 19th and early 20th centuries was a revival (based on learning about runes from books, which is likely), or ascribable to an unbroken tradition.

²⁷⁸ <http://titus.uni-frankfurt.de/didact/idg/germ/runealph.htm> Cf. https://en.wikipedia.org/wiki/Elder_Futhark

nic” (Vennemann 2013b). The latter “offers explanations for all four still difficult vowel runes, **ī**, **i**, **e**, and **o**: It is shown that all four derive from Neo-Punic consonant letters, **ī** and **i** from *Y* (*Yodh*), **e** from *H* (*He*), and **o** from *ʿ* (*ʿAyin*), as these were regularly employed for indicating the vowels [i], [e], and [a], respectively, especially in Latin loanwords and names” (*ibid.*: 265); “evidence is accumulating that the proto-futhark was nothing but the Punic alphabet of the 3rd and 2nd centuries BCE applied to Proto-Germanic” (*ibid.*: 266). The following is quoted from Vennemann (2013b, pp. 266–267):

Whereas the traditional Punic writing system reflects a rather conservative development of the oldest Phoenician alphabet, the Neo-Punic alphabet used in many parts of the collapsed Carthaginian empire after the fall of Carthage at the end of the Third Punic War (149–146 BCE) shows a more radical departure, inspired by cursive writing but used for all kind of inscriptions [...] Most of the runes clearly reflect traditional Punic letters. But some of them, among them the vowel runes to be discussed here, remained without a clear source in that system. As will be seen, additional explanations become possible under the assumption that knowledge of the Neo-Punic alphabet in Germania led to certain modifications of the proto-futhark. There existed indeed an urgent reason for such modifications: As already mentioned, the Punic alphabet was a pure consonant script; there were absolutely no letters reserved for vowels. There was, however, a tradition of occasionally using certain consonant letters to express the presence of a vowel, even to some extent the nature of the individual vowel. This practice became rather common in Neo-Punic times – understandably, because with increasing Roman influences on Punic language and culture it became an equally frequent necessity to write Latin words, especially names, with Punic letters, and Latin names would have been nearly unrecognizable without an indication at least of some of the vowels in them.

That is to say, a few letters could represent either a consonant, or act as a *mater lectionis* indicating the presence of some vowel, something familiar from the Hebrew and Aramaic *scriptio plena*,²⁷⁹ and from the standard usage of the Arabic script. In the Punic alphabet, the shape of the letter *ʿayin* is a circle (○). In the Neo-Punic script, this was used for indicating the presence of an *a* (which could also be indicated by an *aleph*, or, rarely, by a *h*).²⁸⁰ The *aleph* was used in order to denote any vowel, but especially an *o*. For comparison, consider that in Hebrew *scriptio plena*, an *aleph* is used, when not in its role as the glottal stop, [ʔ], then typically in order to represent *ā*, but the latter had the phonetic

²⁷⁹ See Weinberg’s study in five instalments (1975–1979), “The History of Hebrew Plene Spelling”.

²⁸⁰ This is briefly explained in an itemised list in Vennemann (2013b: 267).

value [ɔ].²⁸¹ The Neo-Punic letter for *w* could stand for the vowel *u*. In the Hebrew script, the letter for *w* could stand for either /u/ or /o/. The

²⁸¹ Hebrew for ‘I called’ or ‘I read’ is *qārāṭī* (now pronounced [ka’rati]). In Plautus’ comedy *Poenulus*, 930, one comes across the Punic word *carothi* for ‘I called’. That is to say, it is the long *a* bearing the tonic stress that became a vowel that Plautus transcribed with the *o* vowel.

The semantic shift from ‘to gather, to collect’ (through ‘to count’) to ‘to say’ or ‘to read’ has been discussed by Alinei in a section about Latin *legō* (1996, Sec. 7.2.4: 651–652), involving a semantic calque from a semantic shift found in Greek; with a parallel in the Uralic languages, one comes across the semantic shift ‘to count’ > ‘to recount’ > ‘to read’. I would like to signal the senses in Biblical Hebrew of the lexical root *ʔ-g-r*, whose derivatives comprise the transitive verb *āgār* ‘to gather, to collect’, as well as (unless there are two lexemes of the root in Biblical Hebrew) *iggēret* ‘epistle, letter’. A letter (an epistle) is something one reads, either reading it out, or silently. As for the Biblical Hebrew root *ʔ-g-r*, I suspect it is akin to the Proto-Indo-European root **ger-* ‘to gather, to collect, to group’ (for which, see Pokorny 1959 at 382, and Alinei 1996: 658). There also is a Biblical Hebrew noun *āgōrā*, denoting a certain small coin; in the 1960s, the value of one [ʔago’ra] was one hundredth of an Israeli lira. (Was the original semantic motivation because one used to hoard small coins, or carry a collection of them? Or because of the value of the coin, or of the coins being counted? Cf. Estonian *luge-* ‘to read’, ‘to recount’, ‘to count’, but also ‘to evaluate’).

In Tannaitic Hebrew (from the times of the early Roman Empire), a new lexeme of the root *ʔ-g-r* emerged: ‘to attack (somebody with arguments)’, which according to Saul Lieberman had antecedents in Aramaic legal terminology; Moreshet (1980: 100) accepted Lieberman’s view, and derived the lexeme from the Aramaic root *g-r-y* ‘to provoke’ as in an ergative conjugation (in which the /ʔ/ was a preformative phoneme rather than a radical phoneme, which is what it became in the new Hebrew lexeme). In contrast, Jastrow (1903, s.v. *āgār* on pp. 13–14) considered the sense as in “that means him who heaps arguments up against him (his opponent in litigation)” as just being a metaphorical acceptance of the Biblical Hebrew lexeme. Jastrow (*ibid.*: 14) also have one more lexeme of early rabbinic Hebrew *āgār* ‘to gird, arm (loins)’, related to *hāgār* having the same sense. Under that lexeme of *āgār*, Jastrow included a second acceptance, whose verb he defined as “to halt”, and only instantiated in a participle; this was for a kind of knife denoted by the feminine deparicipial noun *ōgēret*, which he defined thus: “a knife having indentations which catch the passing nail of the examiner”. That is a technical term from the domain of kosher slaughtering, as the smoothness of the blade of the slaughterer’s knife must be examined before use. (Elsewhere in this study, we have referred to early sickles whose blade had indentations.)

Also consider the following, concerning Latin *legō*. If one accepts Agmon’s hypothesis of the transition from bilateral to trilateral roots in Semitic around the time of the emergence of agriculture, then one may hypothesise that the Hebrew and Arabic root *l-q-ʔ* ‘to collect’ developed from a bilateral root **l-q* ‘to collect’. And indeed, the

Neo-Punic letter for *y* could stand for the vowel *y* (like in the Hebrew script).


The *i* rule is \downarrow and Vennemann explains (2013b: 268): “In Neo-Punic writing, the shape of the letter *Y* is identical with the *ī* rune, given three constraints of runic style: (1) Draw no curved lines, (2) Draw no horizontal lines, (3) Attach twigs to a vertical staff. And that the *ī* rune is by origin indeed nothing but a *Y* is underlined by the fact that it is placed right after the *y* rune (= *j* rune) in most recorded futharks”. In Neo-Punic inscriptions, as Vennemann shows, one comes across *y* being written as a letter specular and rotated with respect to \downarrow or then resembling the digit 2.

Vennemann’s (2013b) section “The *e* rune, Π , \mathbb{M} ” (which derives it from the Neo-Punic letter for /h/, shaped as \curvearrowright or \curvearrowleft) of which, the former resembles the current shape of the Hebrew letter η for /h/) the former resembles begins with the explanation: “The Punic letter *H* with the name *He* was adopted into the proto-futhark with the sound value of [ē] which, pronounced as an open [ē], or [ǣ], later developed into North and West Germanic long [ā], turning the original *H* letter by form, position, and then sound value into the **a** rune” (Vennemann 2013b: 271). In fact, the Greek letter E and the Roman letter E both derived from the Phoenician (Northwest Semitic) letter for /h/. Later on, in Hellenistic and Roman-age transcriptions from Hebrew, one observes a penchant of the Hebrew phonemes /h/, /ħ/, and /ʕ/ to come across as /e/. A clear example is the European onomastic type *Eva* where the Hebrew Bible has *Hawwā*.

Vennemann explains: “In Neo-Punic times the *H* letter was taken over once again with the sound value of [e], the short *e*, probably because the long *e* and the short *e* differed significantly in quality, the long *e* being lower, nearer in quality to phonetic [a] than to phonetic [e]. This Neo-Punic *H* letter remained in the futhark with its original sound value, [e], because short *e* did not change the way long *e* did” (Vennemann 2013b: 271). “Adopting a letter a second time with a different sound value is not unusual” (*ibid.*, fn. 12).

The *o* rune is \times and it had not part of the proto-futhark, and in fact, in the order of runes, it occupies the last of next-to-last place. Vennemann (2013b) devotes to this rune Sec. 4. As I pointed out above, in the

Proto-Afroasiatic root *lāk’-/*lak’ ‘to collect’ has already been proposed, in the literature.

Punic alphabet (as well as in the very similar Phoenician and Palaeo-Hebrew²⁸² alphabets), the shape of the letter ‘ayin is a circle (O). Vennemann (2013b: 273) explains that because of the constraint that futhark runes have no curves, when the circle entered the proto-futhark it became a square or a diamond shape, “with the sound value of the velar nasal. In Neo-Punic, however, the circle letter ‘is the usual grapheme to represent /a/” (Jongeling and Kerr 2005: 8)”. Vennemann (*ibid.*, pp. 273–274) explains the situation of Germanic vowels and the process by which there was room for  to take on the role of representing the *o* vowel. The following is quoted from p. 274:

When the vowel system of the futhark was completed by adopting consonant letters used for vowels in Late Punic, the natural choice for Proto-Germanic ^{+/ǣ/} was the Late Punic circle letter ‘Ayin, O. Its shape in the futhark became of necessity that of a diamond, and for the same reasons as in the case of the **ŋ** rune. So when the circle letter was added to the futhark once again, this time with the sound value of a low back vowel *ɔ*, little tails were added to the diamond to distinguish it from the pre-existing plain diamond, the **ŋ** rune. This ^{+/ɔ/} **ɔ** rune, the **o** rune of the historical futhark, then changed its sound value together with the change of ^{+/ǣ/} into /ō/ in North and West Germanic.

Let us go back to “with the sound value of the velar nasal”. In a footnote, Vennemann (2013b: 273) cited Vennemann (2010) – it was reprinted as Ch. 30, the last chapter, in his 2012 book under review – for his discussion “of the sound value of the Semitic circle letter”. In fact, Ch. 30 discusses the **ŋ** rune, also called the *Ing* rune, of the futhark. In Ch. 30 in the book under review, on p. 639 Vennemann claims:

In Vennemann [(2009) = Ch. 28 in the book under review]: 846f., I used the phonetic affinity between the circle letter in the Phoenician alphabet, where its sound value was [ʕ], and in the fuþark, where its sound value was (or continued) [ŋ] – both being back, voiced, continuant consonants – as one argument in favor of the Phoenician alphabet as the immediate source of the fuþark [...]. A second consideration was that Germanic had no *ʕ* phoneme and therefore could not adopt O with its original value. A third consideration was that the order of the runes in the proto-fuþark, where the circle rune probably followed directly after the nasals **m** and **n**, suggested to the Germanic alphabet adaptors that it be used as the third Germanic nasal, [ŋ].

²⁸² See Naveh (1982), or more simply https://en.wikipedia.org/wiki/Paleo-Hebrew_alphabet

Next, in Ch. 30 goes on to propose a new argument, based upon his realisation (based on an email he received) that beginners in Arabic are advised to substitute *ng* if they cannot pronounce the pharyngeal [ʕ]. “Clearly Professor Wright (1830–1889) was in his days confronted with exactly these two mispronunciations of Arabic ‘Ain and Hebrew ‘Ayin by his students, as a vowel and as a nasal. We may safely conclude that the Ancient Greeks and the early Germans were simply the first offenders, the former substituting a vowel, the latter the Velar nasal” (641). Perceptively, Vennemann also found support in the Turkic “runic” script expressing [ŋ] by means of ☉ – a circle with a dot inside, which is the original (indeed pictographic) shape of the ‘ayin letter, the motivator being ‘eye’ (the name for the latter is the noun denoting ‘eye’, by the acrophonic principle of the letters of the Phoenician and Old Hebrew alphabet, and of the names for those letters).

Without detracting from Vennemann’s argument, indeed somewhat strengthening it, I would like to signal that among Italian Jews, until the early 20th century, the phonetic value of the Hebrew letter ‘ayin was [ŋ] or rather [ŋp], sometimes described, by simplifying, a [p], a nasal consonant which is orthographically written in Italian as *gn*. (Italians speaking Italian or its dialects pronounce *gn* as [p̄p], but as [p] in the generally degeminating North, whereas Spanish-speakers pronounce *ñ* as [p̄].)

By the time Vennemann wrote the Preface of the 2012 book under review here, he apparently realized that within Semitic, a nasal pronunciation rather than a pharyngeal pronunciation of the ‘ayin phoneme does occur indeed: “Only in Germanic and in Semityic do the letters have acrophonic names that are nouns with a referent outside the world of writing, e.g. Germanic ⁺*fehu* ‘cattle’ for the **f** rune exactly like *ʔalep* ‘head of cattle’,²⁸³ for the Semitic letter [for] ʔ. [...] Only in the Ger-

²⁸³ I.e., *ālef*, a pausal form. Consider moreover this hypothesis from Vennemann (2013a, Sec. 3): “The name of Phoenician *G* (in Hebrew *Gimel* ‘camel’) had no exact Germanic equivalent, because camels were not known in the north; with the explanation that it was a very big wild or semi-domesticated animal with a hump, they may have associated the aurochs, ⁺*ūruz* in their own language, which converged nicely with the sound value [ḡ] of the letter *G*”. In fn. 22, Vennemann argues: “This substitution of a locally known animal for an unknown foreign one may appear surprising at first glance. But it finds a perfect parallel in the substitution of the cat-drawn chariot with which Freyja, the Germanic equivalent of Ishtar, rides to Baldr’s funeral in Scandinavian mythology [...] The Germanic people did not know lions, so they substituted the animal that appeared to them similar enough on the evidence of a Phoenician’s description of a lion”.

manic and Carthaginian alphabets does the circle letter, \circ (in the runic systems also stylized \square and \diamond) stand for phonetically similar and confusable back voiced continuant consonants, [ʕ] in Semitic, [ŋ] in Germanic. [...] Clearly the source of runic \circ / \square / \diamond with the sound value [ŋ] cannot therefore be the Greek, Etruscan, or Latin letter \circ with its sound value [o] but only the Phoenician letter \circ with the sound value [ʕ], which occurs nasalized in Arabic and as [ŋ] in several varieties of Hebrew” (ix, my underlining).

Besides, I would like to mention that the Hebrew ‘*ayin*’ letter came to represent /e/ in the adapted Hebrew script as used for writing Yiddish. For maximal precision, I cite from a recent bulky history of Yiddish. Beider (2015: 178) refers to the origins of that scriptorial practice: “the presence of one basic graphic convention elaborated by BNEY HES: the use of *ayin* for /e/-colored vowels”. Indeed, “[s]ince the end of the fifteenth century *ayin* has been systematically used for /e/-colored vowels” (*ibid.*: 302). Cf. Beider (*ibid.*, pp. 300–304, Sec. 3.7.3: “E Effect”). “The most detailed coverage of the history of the use of *ayin* by Ashkenazic Jews can be found in [Timm 1987, pp.] 124–35” (Beider 2015: 302, fn. 262). Beider himself (2015) discussed the pronunciation of Hebrew ‘*ayin*’ on pp. 267–268.

Bney hes (punning with the sense ‘Hittites’, ‘the Children of Heth’) refers to ‘those Jews who pronounce[d] the letter <ḥ> as the aspirate [h]’. The name is in opposition to *Bnei khes* (for ‘those Jews who pronounce the letter <ḥ> as the velar [x]’). In the late Middle Ages, *Bney hes* corresponded to the Ashkenazic Jews of western Germany, as opposed to those of other German lands (who were *Bney khes*).²⁸⁴ For example, <ḥ> was [x] in Austria, but so it was in Slavic lands (eventually, such was the case of all Yiddish speakers in the modern period).²⁸⁵

“The Hebrew pronunciation of medieval BNEY HES was characterized by a distinctive feature that will be conventionally called

²⁸⁴ Beider (2015) dealt with the *Bnei hes* on pp. 5, 173, 232–233, 247–255, with a map on p. 528.

²⁸⁵ The Hebrew letter <ḥ> was pronounced as [x] also by Judaeo-Spanish speaking Jews from Turkey and the Balkans (Bunis 1993). It is the more prestigious pronunciation among Israeli Hebrew speakers. Concerning these two pronunciations of <ḥ> in relation to the origins of Yiddish, also see Beider (2015, 247–255), and the discussion by Dovid Katz (1993: 21–22), and the treatment of phonology in Ilan Eldar’s [= Adler’s] *The Hebrew Language Tradition in Medieval Ashkenaz* (1978–1979, Vol. 1). The pronunciation of the Hebrew letter <ḥ> was even zero in southern France in the 14th and 15th centuries (Beider 2001: 339). For <ḥ> pronounced as [h], see in Irene Garbell’s (1954) “The Pronunciation of Hebrew in Medieval Spain”.

E-EFFECT. The stressed vowels *pataḥ*, *ḥatef-pataḥ*, and *qameṣ*, [which in the Tiberian²⁸⁶ grammatical vowel system of Hebrew respectively stand for *a*, *ā*, and /*ā*/ *ā*,] when adjacent to *heth* [i.e., the letter <ḥ>] or *ayin*, were pronounced as some sort of front mid-vowel: [e(:)] or [ɛ(:)]” (Binder 2015, pp. 300–301). In fact, there are antecedents in how in the Hellenistic and Roman periods, Biblical Hebrew was transcribed into the Greek or Roman scripts. Cf. (Masoretic) Biblical Hebrew *ḥ^amōr* ‘donkey’, plural *ḥ^amōrīm* (pronounced in Israeli Hebrew as [xa’mor], [xamo’rim]), as opposed to the Samaritan Hebrew pronunciation *ēmor* (plural *ēmūrām*); I owe the Samaritan data to Talshir (1981: 313).

Considering the traditional nasal pronunciation of Hebrew ‘*ayin* in Italy, I tend to agree with Werner Weinberg on a point on which Alexander Beider disagrees with him: “Weinberg (1969:22) considers the originally consonantal *ayin* to be responsible for the initial consonant in WphY [i.e., Westphalian Yiddish] *neveyre/nevēre* ‘sin’ [...] as well as WphY *neshīres* ‘wealth’ [...] This theory is doubtful. Alternatively, the original /n/ could be derived from the Yiddish indefinite article *an* [...] If we follow Weinberg, we need to consider these forms [where initial *aleph* rather than *ayin* yields initial *n* in a few Lithuanian Yiddish or Alsatian Yiddish forms] as hypercorrect” (Beider 2015: 268). This is in a sense the reverse of the phenomenon of discretion of the article, as in the etymology of English *adder*, from *a nadder* being separated as *an adder*, or then as remarked about in Vennemann (2011), an encyclopedia entry entitled “Arabic Loanwords in German(ic)”: the initial *n-* of Arabic *nāranj* was lost, once borrowed into Italian *arancia*, “by confusion with the final *-n* of the indefinite article, e.g. Italian ⁺*un narancio* > *un arancio*”. That same encyclopedia entry also includes (under the section headline “Other Semitic influences in German”) loanwords from Hebrew, as well as a summary of Vennemann’s several etymologies as discussed in this review article, even though these are introduced as being from Punic:

The third language that has influenced German – more generally: Germanic, viz. already at the Proto-German level – is Phoenician, more precisely: Punic, the language of the Carthaginian empire. This is a fairly recent discovery ([six self-citations, plus two citations of a student of his:] Mailhammer 2004, 2006 [*recte*: Mailhammer 2006, Mailhammer et al. 2003]), and it may still be a matter of controversy. The criteria for the identification of these influences are (1) that they are restricted to Germanic or, if

²⁸⁶ The Tiberian reading tradition of Hebrew is the subject of two papers by Geoffrey Khan (1996, 2013).

they also occur in neighboring Indo-European languages, occur in a shape or a distribution that only allows an explanation as direct imports from Semitic, and (2) that linguistic properties of some of these imports point to the language of Carthage rather than to Semitic in general. As to Phoenician as the source language, there is the problem that the language is poorly attested. Therefore in some cases one has to rely on closely related Hebrew for comparison, trusting that most Hebrew words occurred in the same or a similar form in Phoenician.²⁸⁷ [...]

11. Creolisation?

Chapter 22 in Vennemann's *Germania Semitica* is "Was Proto-Germanic a creole language?" remarking in the abstract that Raimo Anttila, to whom the chapter in its original forum was dedicated (it was originally intended for a jubilee volume), had once asked this as an examination question he asked Vennemann in the 1960s at the University of California, Los Angeles; Vennemann answered "No". Also in Chapter 22, Vennemann concludes his abstract by claiming: "A comparison of English with its strong superstratal French influence shows that Proto-Germanic was not a creole but merely a language heavily affected by language contact" (423).

In Chapter 22, Vennemann devoted considerable attention to what he considers the Phoenician superstrate. I argue that Alinei's longer chronology ought to be accepted, and that an influx of Northwest Semitic vocabulary can already be ascribed to the spread of farming. I would like to signal the following statement from Rubio (1999, pp. 8–9): "Blažek and Boisson (1992)²⁸⁸ have presented noteworthy evidence that would prove that some agricultural terms were *Wanderwörter* that travelled with the object they named. [...] Some of the connections suggested by Blažek and Boisson are more possible than others, but several do deserve a careful study".

There is similarity between the debate about the Indo-Europeans' arrival (or, in particular, the arrival of the Proto-Germanic speakers into *Germania*), and the debate about whether the Sumerians were autochthonous; to say it with Gonzalo Rubio (1999: 1): "One of the most dis-

²⁸⁷ That is a big if. But I myself had to rely on such a tentative premise when discussing some Punic wording in an article entitled "*L^emiqnē-φānīm* – 'Per comprare la faccia' – 'per conciliarsi il favore'. Una proposta di rilettura di un'iscrizione punica su una lamina d'argento di Tharros" (Nissan 2010 [2011]a).

²⁸⁸ That article is entitled "The Diffusion of Agricultural Terms in Mesopotamia".

cussed Assyriological topics is the ‘Sumerian problem’: Were the Sumerians an autochthonous Mesopotamian population or did they come from somewhere?” Rubio then added (*ibid.*): “The archeology [sic] and ecology of early Mesopotamia, as far as we can understand it, does not really support any strong claim concerning Sumerian origins (Potts 1997: 43–55)”.

There is also an analogue for Vennemann’s asking whether Proto-Germanic was a creole language. Rubio states (1999, pp. 1–2, fn. 1):

Following Nissen’s [(1988: 66–69)] arguments on an alleged archeological discontinuity, Høystrup (1992: 27) has assumed that between Early and Late Uruk, a large body of immigrants came to constitute the majority of the working population in southern Mesopotamia, while the “ruling class” would have been autochthonous. This scenario (similar to that of a plantation economy), and a misleading use of linguistic typology, have led Høystrup (1992) to propose that Sumerian was a creole. Although he suggests that the substrate language would correspond to Landsberger’s [(1974 [1944])] “pre-Sumerian substratum”, the impossibility of identifying both the superstrate and the substrate languages, together with the pitfalls of his typological approach to creoles would rule out his theory.

12. An Ingrate Task by Its Very Nature: An Illustration Through Examples

12.1. Icelandic *Serkir* ‘*Saracens*’ or ‘*shirt*’

Etymologising can be quite an ingrate task, even when done professionally by linguists. Competing hypotheses may result into unsolvable aporia, for scholars surely much smarter than Buridan’s donkey from proverbial usage. That sometimes a big row among scholars erupts, which has been the case of Theo Vennemann’s theory about Vasconic and Hamito-Semitic-related presence in Europe (and Germany in particular) in prehistorical times – and this in relation to Vennemann’s and his opponents’ notions concerning the arrival of the Indo-Europeans being akin to Colin Renfrew’s Neolithic Diffusion Theory²⁸⁹ – ought not to be accompanied by bitter aspersions being cast. The debate has been stimulating.

In a lighter vein, consider how “diabolically” confusing the data may be. Let us open the second edition of Cleary and Vigfusson’s (1957,

²⁸⁹ It is a cruel quirk of fate that in the case at hand, heeding the Neolithic Diffusion Theory arguably results in a situation resembling jumping from a trampoline into a swimming pool you assumed was full, only it is empty.

repr. 1962) *An Icelandic-English Dictionary* (its first edition appeared in 1874). On p. 523, there is the following entry about the Saracens and their perceived ancient ilk (brackets in the original, except ellipsis brackets; our additions in braces):

Serkir, m. pl. [said to be derived from Arabic *sharkeyn*²⁹⁰ = *Easterlings*], *the Saracens, the people of Serk-land*; used of northern Africa, northern Spain, Fms. {= Formanna Sögur} vi, vii, ix, Orkn. {= Orkneyinga Saga}, also in translations of ancient Lat. writers, of the Assyrians, Babylonians, Stj. {= Stjórn}, Al. {= Alexanders saga} passim: Serkja-konungr, Serkja-ríki, *the king, kingdom of the S.*, Al., Stj. **Serk-neskr**, adj. to render the Lat. *Punicus*, Róm. {= Rómverja Saga} 324.

The entry following that one makes it clear that there is homonymy with an Icelandic name for ‘shirt’.²⁹¹ For the sake of the discussion, let us seek a parallel in which an ethnic or geographical name is nearly homophonous or even related to a name for ‘shirt’ in the respective same language. The Italian²⁹² for ‘shirt’ is *camicia*. Consider the fol-

²⁹⁰ That is a spurious transcription. In colloquial Arabic *šarqiyyīn* ‘Easterners’. Is there any plausible possibility that the etymon of the Icelandic ethnic name for the Saracens was the lexical type *Saracen* with the Greek kappa (or at any rate, [k]) retained? As for the geographical fuzziness, consider that the Abbasid Caliph (based in Baghdad) Hārūn al-Rashīd sent as a gift to Charlemagne an elephant, with the Jewish merchant Isaac. The *Annales regni francorum*, 802:117, state that “venit Isaac cum elefanto et ceteris muniberus, quae a rege Persarum missa sunt, et Aquisgrani omnia imperatori detulit; nomen elefanti erat Abul Abaz”. If it was *Abul-’Abbās* (‘father of Abbās’), then there is an irony to be potentially detected, because the sender was from the Abbasid caliphal dynasty. The Latin chronicles however do not appear to have been as subtle as that. *Abul-bazz* is as likely to have been the elephant’s name, as *Abul-’Abbās*. Isaac and the elephant embarked in Tunisia, disembarked in Genoa in October 801, spent the winter in Vercelli, started to cross the Alps in the spring, and reached Charlemagne in Aachen on 1 July 802. The elephant died suddenly in 810. The *Annales regni francorum* 810:113 claim it was the elephant sent by “Aaron, King of the Saracens”: “ubi dum aliquot dies moraretur, elefant ille, quem ei Aaron rex Saracenorum miserat, subita morte periit”. Allegedly, this elephant’s bones were conserved at Lippenham until the 18th century.

²⁹¹ By the way, note that the Icelandic author Snorri Sturluson was no stranger to folk-etymologising. Polomé and Rowe remarked (2005: 3447): “The Germanic gods are divided into two groups, the Æsir and the Vanir. Following the medieval practice of etymologizing, Snorri says that the Æsir’s name shows that they came from Asia, but this interpretation does not appear to reflect any authentic tradition”.

²⁹² Tongue in cheek: let this being in Italian not be an obstacle. Navigation skills have famously abounded in Italy, historically (too bad Italian explorers explored the Atlantic, not the northern Pacific). At any rate, more provably so than the navigational

lowing tongue-in-cheek, Varronian aetiology²⁹³ I have made up²⁹⁴ for the name of the Kamchatka Peninsula of eastern Siberia: “Si chiamerà anche «Camiciàtica»,²⁹⁵ ma in maniche di camicia non ci si può stare” [“*Shirtland* it may be called, but you can’t stay there in your shirt’s sleeves”].²⁹⁶ So why call such a region after a shirt? Icelandic vulgate history provides an answer: as a promotional lie, it may presumably work, just as when Eric the Red, the Norse chieftain, called Greenland²⁹⁷ that way to make the place attractive to potential settlers he wanted to lead there. Another “possible” route is to claim that the inhabitants are poor, only owning the shirt they wear (and perhaps the other one in the wash); historically, ascribing semantic motivation for a

skills of Vennemann’s hypothetical “Semitidic” predecessors of the Phoenicians. (I am not denying that coastal navigation in the Mediterranean quite probably began earlier than recorded history.)

²⁹³ Such as Verro’s “*canis a non canendo*” (a dog is called *canis* because by no means can it be canorous, it cannot sing), “*lucus a non lucendo*” (a forest is called *lucus* because there is no light there).

²⁹⁴ Playful etymologies are the subject of Nissan (2012, 2016); Nissan and Ha-Cohen-Kerner (2014).

²⁹⁵ Cf. Spanish *cepadgo* (< *cepo* ‘shackle’ + the type of the Latin suffix *-aticum*) for the fee inmates were required to pay to staff clinching their shackles.

²⁹⁶ You can tell the same (punning on *shirt*) about Shetland (but it was formerly *Zetland*), with the latitude “afforded” by variation in English *r* pronunciations. In cold climates, in summer mosquitoes would eat you.

²⁹⁷ In the context of the Norse discovery and settlement of southern Greenland. Socio-cultural competence involved in making sense of this is the rule of thumb that often discoverers bestow the name on the land (or thing) they have discovered. See a formal representation of that in Nissan (2002). Kamchatka is, for Europeans, a far away country that was “discovered”. The southern tip of Kamchatka was reached by Jean-François de Galaup, count of La Pérouse during his exploration of the Pacific in 1785–1788. The pun in Italian is partly enable by the cultural expectation, in Italian culture, that Italians were prominent among discoverers of far away lands (though in the service of other countries; Cesare Pascarella made much of that in his comic, yet deeply compassionate epic of 1893, *La scoperta de l’America*, in the dialect of Rome: see Nissan 2016 [2017]). Columbus is invariably considered to have been a Genoan, in Italy. Amerigo Vespucci explored the Atlantic coasts of South America in the service of Portugal. Antonio Pigafetta took part in Ferdinand Magellan expedition (1519–1522), and wrote the account of its circumnavigating the world. Giovanni da Verrazzano, in the service of France, discovered in 1523–1524 the bay of New York, the Hudson river, and Terranova. Giovanni and Sebastiano Caboto, in the service of England, explored the Atlantic coasts of North America. Alvise Cadamosto and Antinotto Usodimare, in the service of Portugal, both discovered the Cabo Verde archipelago in 1456.

country name to the supposed poverty of the locals is documented.²⁹⁸ Of course, in this paragraph I have been joking, but we all call the bluff

²⁹⁸ The Biblical (and modern) Hebrew name of **Ḥaḏramawt** (traditionally, the part of South Yemen east of Aden, even though at present the Hadhramaut Governorate has a smaller area; until 1990 South Yemen was the People’s Democratic Republic of Yemen) is **Ḥāṣarmāwet** (now pronounced [xatsar’mavet]), which lends itself to be folk-etymologised as ‘courtyard of death’. In early rabbinic homiletics, in *Genesis Rabbah* 37:11, commenting about mention of **Ḥāṣarmāwet** (Hazarmawet) as being one of Joktan’s sons in *Genesis* 10:26, it is stated: “Rav Huna said: ‘There is a place whose name is **Ḥāṣar Māwet** (Courtyard of Death), as they [i.e., the inhabitants] eat leeks, and wear garments of papyrus, and wait for death every day’. Rav Samuel said: ‘Even garments of papyrus they do not have’”. Being so poor, they are supposedly not confident of surviving. The two sages mentioned were from Persian Mesopotamia, even though *Genesis Rabbah* is a collection from the Land of Israel: in the critical edition by Julius Theodor (1849–1923) and Hanoch Albeck (1890–1972), Albeck dated it to 425–500 C.E. (Lerner 2006: 149; Albeck 1965, “Einleitung”: 94–96). In the Roman imperial period, **Ḥaḏramawt** used to export frankincense for great profit (so the notion that it was quite a poor region was based entirely on folk-etymology, we may presume), and it was still exported from there to Bombay in the early 20th century. Moreover, the community of Hadhrami Jews had already existed in pre-Islamic times, and only came to an end with emigration to the newborn State of Israel, or the conversion to Islam of some other families.

Having considered the rabbinic homiletic explanation, note that there is a similar Arabic, Islamic folk-etymology (the quotation is from <https://en.wikipedia.org/wiki/Hadhramaut>):

The most common folk etymology is that the region’s name means “death has come”, from /ḥaḏara/ (Arabic for “has come”) and /mawt/ (“death”), though there are multiple explanations for how it came to be known as such. One explanation is that this is a nickname of ‘Amar ibn_Qaḥṭān, a legendary invader of the region, whose battles always left many dead. Another theory is that after the destruction of Thamūd, the Islamic prophet Ṣāliḥ relocated himself and about 4,000 of his followers to the region and it was there that he died, thus lending the region its morbid name “death has come”. [...]

Scholarly theories of the name’s origin are somewhat more varied, but none have gained general acceptance. Juris Zarins, rediscoverer of the city claimed to be the ancient Incense Route trade capital Ubar in Oman, suggested that the name may come from the Greek word ὑδρευματα, or enclosed (and often fortified) watering stations at wadis. In a *Nova* interview, he described Ubar as

a kind of fortress/administration center set up to protect the water supply from raiding Bedouin tribes. Surrounding the site, as far as six miles away, were smaller

because we know better. But there are so many situations in historical linguistics when we are in the dark, and it is difficult to agree on some of the very criteria for telling which is what.

12.2. *Nocturnus' Epithet* ὠμελανφοκ 'He Who Wears a Black Mantle'

Alfredo Buonopane (2016) discusses a minor Roman god, Nocturnus, whom Plautus in his comedy *Amphitruo* mentions (concerning a

villages, which served as small-scale encampments for the caravans. An interesting parallel to this are the fortified water holes in the Eastern Desert of Egypt from Roman times. There, they were called *hydreumata*.

Though it accurately describes the configuration of settlements in the Pre-Islamic Wadi Ḥaḍramawt, this explanation for the name is anachronistic and has gained no wider scholarly acceptance.

Already in the Pre-Islamic period, variations of the name are attested as early as the middle of the First Millennium BC. The names *ḥḍrmt* and *ḥḍrmwt* are found in texts of the Old South Arabian languages (Ḥaḍramitic, Minaic, Qatabanic, and Sabaic), though the second form is not found in any known Ḥaḍramitic inscriptions. In either form, the word itself can be a toponym, a tribal name, or the name of the kingdom of Ḥaḍramawt. In the late Fourth or early Third Century BC, Theophrastus gives the name Ἀδραμότα, a direct transcription of the Semitic name into Greek.

As Southern Arabia is the homeland of the South Semitic language subfamily, a Semitic origin for the name is highly likely. Kamal Salibi proposed an alternative etymology for the name which argues that the diphthong "aw" in the name is an incorrect vocalization. He notes that "-ūt" is a frequent ending for place names in the Ḥaḍramawt, and given that Ḥaḍramūt is the colloquial pronunciation of the name, and apparently also its ancient pronunciation, the correct reading of the name should be "place of ḥḍrm". He proposes, then, that the name means "the green place", which is apt for its well-watered wadis whose lushness contrasts with the surrounding high desert plateau.

Which, we may add, may remind of the name *Greenland* given to a barren place, and yet, things are complex: even in a barren environment, some spots may happen to be green.

night which will apparently be very long) in a comic posture (*Credo ego hac noctu Nocturnum obdormisse ebrium*, “I believe that this night Nocturnus fell asleep intoxicated”), but who in magic practice turns out to be a demon causing panic attacks by night (what psychologists refer to as *pavor nocturnus*), and, on the evidence of an epithet, wearing a black cape: as indeed, in some *defixiones* he is called *ωμελανφοροκ* (*sic* in Buonopane’s article), a word which Attilio Mastrocinque (with whom Buonopane 2016: 57, agrees) proposed to read as *ὁ μελάμφορος* in an article entitled “A Magical Name: OMELANPHAROK [*sic*]: He Who Wears a Black Mantle” (Mastrocinque 2012). But doesn’t the *lectio* (in Buonopane!) end in *φοροκ*? It may be tempting, but nobody would succumb to the temptation, of claiming that as we are in the semantic field of garments, here we have an early record of the word *frock*. Nobody would succumb, because it violates some known criteria about geography chronology. English *frock* ‘monk’s habit’ (or by semantic shift, ‘a kind of woman’s light dress’ or ‘a kind of child’s dress’, or as frock coat, ‘a man’s long-skirted, double-breasted coat’), from French *froc* ‘monk’s mantle’ or by generalisation, ‘monk’s habit’, in turn from Frankish.) The problem is that sometimes the criteria are not as clear cut.

12.3. *A Crux Interpretum in Psalm 68: the Phrase hārīm gabnūnīm – ‘Hump-backed Mountains’, vs. a ‘Mountains of Cheese’ Tradition*

Historically, misinterpretation has on occasion resulted in surprising effects. As a preamble to an example of this, consider that /gabb/ [gav] in Biblical Hebrew denotes – to say it with Dolgopolsky’s *Nostratic Dictionary* (2008: 565, entry 586: Nostratic *gab∇(-)|í∇) where ∇ stands for an unspecified vowel) – “‘back of the body’ (so called because it is the highest part of pack animals)” (of course, disregarding their head, thus, in their postcranial morphology, as zoologists would say), as for Dolgopolsky, the archisememe of Semitic *gabb- is ‘top of something’. In Syriac, one finds *gabībā* for “hump-backed (gibbosus)” (Dolgopolsky, *ibid.*, citing Brockelmann’s 1928 *Lexicon Syriacum*). In Modern Hebrew, *gibbén* ‘hunchback’, but historically, in the *Mishnah* (ca. 200 C.E.), tractate *Bekhorot* 7:2 (in a passage included in the *Babylonian Talmud*, tractate *Bekhorot* 43b), that noun in *Leviticus* 21:20 (which is derived by applying a word-form associated with the bearer of a deformity) has been interpreted alternatively as “hump-backed, or

one having defective eye-brows” (Jastrow 1903: 207, s.v.), e.g. because “his eyebrows (ḡbnyw) ḡbīnāw are lying”. In Syriac, one finds ḡbīnā as denoting “vertex, cacumen” (Dolgopolsky, citing Brockelmann). Dolgopolsky’s entry also includes p[arallels from Indo-European, including Old High German ḡbal ‘skull, forehead’, ḡbil ‘skull’, ḡbilla ‘skull, head’, Middle High German gebel ‘skull’.²⁹⁹ Dolgopolsky (2008: 566) has a different entry, entry 588, for the Nostatic tentative root he notates as “? *ḡū|ub|pE”, associated with the sense “heap, hump, hunchback”. Instances of derivatives include Jewish Palestinian Aramaic ḡbynt’, which I read as ḡbīntā, and which he

²⁹⁹ Dolgopolsky (2008: 565, entry 586) *AdS* [i.e., this root is an additional source] of Gt [Gothic] *gibla*, OHG [Old High German] *gibil* ‘summit’, NHG [New High German] *Giebel* ‘gable(-end)’, π [in poetry] ‘summit’, AS [Anglo-Saxon] *zafol*, *zēafel* ‘gable’, ON [Old Norse] *gafl* ‘Giebelseite’, Spitze einer Insel’ (< N[ostatic] ? *ḡ^rU^rb^r∇ (L∇) (LV) ‘hill, mountain’); NrGmc [Northern Germanic] -d→ [i.e., whence a derivative in] Fr[ench] *gable* ‘gable’ -b→ [i.e., whence a borrowing in] NE [New English] *gable*”.

Dolgopolsky (2008: 2793) defines *AdS* as follows: “additional source (if a root/word in a descending lge. [language] goes back to coalescence/contamination of two etymological sources: a main one and an additional one)”. In the introduction to Dolgopolsky (2008), Sec. 8.4., “Merger of homonyms” (p. 37), provides these examples of merger (pp. 37–38):

In Russian there is a word сало ‘lard, tallow, animal fat’ and a corresponding adjective сальный ‘made of tallow, of animal fat’. In the 19th century Russian borrowed from French the adjective sale ‘dirty’, that according to the laws of Russian morphology turned into сальный (*souris sale* [recte: *sourire sale*] ‘dirty smile’ -b→ сальная улыбка). But for any speakers of Russian (including those knowing French, like myself) сальный in both meanings is the same word. If in Russian we hear сальная улыбка (as of a man looking at a woman with indecent thoughts), we imagine a face stained with dirty fat.

In Georgian there is a word **Quli** ‘slave’ (an old loan from Turkic *qul*; -i is a suffix of nominative). In the 19th century Russian borrowed the word кули from English *coolie* (of Dravidian origin). The word won popularity in Russia (probably due to the translation of the English novel “Coolie” by the Indian writer Mulk Raj Anand, preceded by occasional mentioning of this word in “Fregat Pallada” by Goncharov and in short stories by other Russian authors), and in the famous song [...] “From border to border”, by the poet Lebedev-Kumach [...] there are words: [...] “This song (about Stalin) is sung by rikshas and coolies, this song is sung by a Chinese soldier”. From Russian the word penetrated Georgian. But in Georgian it coalesced with **Quli** ‘slave’. For speakers of Georgian this is obviously the same word, because the meanings ‘slave’ and ‘coolie’ are very near. A formal proof of this coalescence is the uvular consonant **Q**- in **Quli** ‘coolie’ (rather than the velar *k*’- that usually renders Russian к-).

defines as ‘hill’. These other instance is from Oromo (in the Horn of Africa): *gōba* ‘hump of cattle’; *gobo* “gobo, gibboso” (as defined by Mario Borello, in the posthumously published Borello 1995).³⁰⁰ Even though the Horn of Africa was under Italian rule for a while in the modern period, those words are definitely not loanwords from Italian. In that same entry, Dolgopolsky also lists Indo-European parallels, from Latin and Romance, but suggests contamination with another root from NaIE (i.e., Narrow Indo-European, i.e., all IE languages except Hittite-Luwian), itself derived from a different Nostratic root:

(×NaIE *gejb^h- ‘bend’ < N *kib∇ ‘hump, bent; to bend’, NaIE *gh^hejb- ‘bend’ < N *gu^rb¹∇ ‘to bend’ [trans., intr.]): L *gībbus*, VL {ML} *gūbbus ‘hunchback’ > Port *gebo*, Ctl *gep*, It *gobbo*, It Pv *güb* id., d.: It *gobbino* ‘humpbacked’ → Fr *gobin* id. ¶ ML #3755, WH I 597 ◇

where “It Pv” stands for Old Pavian (i.e., the “dialetto antico pavese”). Italian³⁰¹ *gobbo* ‘hunchback’, but *gobba* denotes ‘hunch, hump’, ‘hunch-backed (f.)’, ‘woman hunchback’, whereas *gibbosità* denotes ‘hump’ or ‘hunchedness’. From Latin, I would also list *gibber*, *gibbēris* ‘hunch’ from Lucilius and Pliny; *gibber*, *gibbēra*, *gibbērum* (the adjective for ‘hunch-backed’, from Terentius Varro and Plautus, as well as *gibbērosus*, which apart from the literal sense, also appears in Fronto in a metaphoric sense, ‘contorted’, said of verbal expression).

As for the noun Latin *gibbus*, it denotes ‘hunch, convexity’ in Juvenal, but denotes ‘hunchback’ in Lampridius. As for Old Italian, we find in Dante, *Paradiso* 21.109, Peter Damian³⁰² (Pier Damiani,³⁰³ Pietro Damiano)³⁰⁴ in the heaven of Saturn telling Dante: “e fanno un gibbo

³⁰⁰ The *Dizionario Oromo-Italiano* by Padre Mario Borello (1893–1981) was edited by Hans-Jürgen Sasse and Paolo Tablino (Borello 1995).

³⁰¹ Incidentally, it is interesting that in the bibliography of Dolgopolsky (2008), the only etymological dictionary of the Italian language that appears there is the old one by Ottorino Pianigiani (1943). It must be said that Dolgopolsky’s (2008) use of Italian publications is overwhelmingly for lexical data from the Horn of Africa.

³⁰² St. Peter Damian (Holopainen 2012; https://en.wikipedia.org/wiki/Peter_Damian) was born ca. 1007 in Ravenna, and died in 1072 or 1073 in Faenza. He was a Benedictine Camaldolese monk, a reformer of that order, and a cardinal in the circle of Pope Leo IX sent on various missions.

³⁰³ This is his usual Italian name (https://it.wikipedia.org/wiki/Pier_Damiani).

³⁰⁴ Another Pietro Damiano (https://it.wikipedia.org/wiki/Pietro_Damiano) was bishop of Asti in Piedmont from August 1475 to his death in November 1496.

che si chiama Catria”, i.e., “and [those stones] form a hunch [i.e., a hill] whose name is *Catria*”.³⁰⁵ Mount Catria³⁰⁶ (1,701 m) is the place of the monastery of Fonte Avellana (which at 700 m of altitude over sea level), where Peter Damian resided (he was its prior from 1043),³⁰⁷ and which is near the city of Gubbio³⁰⁸ (the Roman-age Iguvium).



The position of Mount Catria in peninsular Italy.

³⁰⁵ The context is as follows: “Tra ‘ due liti d’Italia surgon sassi, / e non molto distanti a la tua patria, / tanto che ‘ troni assai suonan più bassi, / e fanno un gibbo che si chiama Catria, / di sotto al quale è consecrato un ermo, / che suole esser disposto a sola latria”. In the *Divine Comedy*’s English translation in blank verse, which first appeared in 1814, by Henry Francis Cary (1772–1844), those lines are rendered as follows (my brackets): “[...] ‘Twixt either shore / Of Italy, nor distant from thy land [i.e., Florence], / A stony ridge ariseth; in such sort, / The thunder doth not lift his voice so high. / They call it Catria: at whose foot, a cell / Is sacred to the lonely Eremitic; / For worship set apart and holy rites’”. A footnote to “A stony ridge” explains: “A part of the Apennine. Gibbo is literally a ‘hunch’. Thus Archilocus calls the island of Thasus, οἶνον ραχίς. See Gaisford’s *Poetæ Minores Græci*, t. i: 298”.

³⁰⁶ https://it.wikipedia.org/wiki/Monte_Catria

³⁰⁷ Peter Damian introduced extreme forms of penitence for the monks at that monastery and at subject hermitages. He made a linguistic claim when stating that the first grammarian was the Devil, who taught Adam to decline *deus* ‘god’ in the plural (thus introducing polytheism).

³⁰⁸ The *Annales Camaldulenses* claim that in 1318, Dante was a guest of Bosone of Gubbio (a jurist and poet), and that on that year Dante also visited Fonte Avellana.

Fonte Avellana³⁰⁹ (literally, ‘fountain of the hazelnut trees’) was established as a monastery around 980. 76 saints of blessed lived there, and 54 bishops had resided there before attaining bishop rank. Mount Catria was a holy mountain to both the Umbrians and the Romans, and there was a temple for Jupiter Apennine,³¹⁰ near where the Flaminia road crossed the Apennine mountain range. That temple was at approximately one Roman mile from the ancient town of Mutatio ad Ensem, now Scheggia. There exists a webpage for the monastery of Fonte Avellana (disbanded under Napoleon and in 1866, but reopened), at a website entitled “Il Gibbo”.³¹¹

In *Psalms* 68:17, there is this rhetorical question: “Why do you *r-ṣ-d* (lurch? Or: leap as though dancing?), *har’im gabnūnīm* (hunched mountains)?” The more difficult *crux interpretum* of the two in that verse is the verbal form, which the Vulgate translated with the Latin verb *susplicere*, whereas in the Pseudo-Jonathan Jewish Aramaic translation, the verb was rendered with an Aramaic verb (from root *ṭ-f-z*) for ‘to leap’ (and indeed, the Onqelos translation of the pentateuchal list of unclean quadrupeds, renders the name for the hyrax with *ṭafzā*). The traditional interpretation is that the other mountains envy the Temple Mount. (A more common trope, in Judaism, is mountains envying Mount Sinai having been chosen for Revelation.) In a fairly recent Jewish translation into Italian, the verb used is *impennarsi*, for ‘to rear’, ‘to prance’ (like a horse), and metaphorically ‘to bristle’, ‘to fire up’ (in anger).

In Aramaic, one comes across *gibbābā* ‘hill’ (see Dolgopolsky, *ibid.*). To speakers of Modern Hebrew, the mountains in *Psalms* 68:16–17 being described as [gavnu’nim] is (perhaps deceptively) unproblematic: the mountains have peaks, more typically rounded ones in the Land of Israel and especially around Jerusalem, so the mountains are taken to be described as being hump-backed. And yet, in the Septuagint, the apposition to “mountains”, in that verse from *Psalms*, was surprisingly rendered with *τετυρωμένον* ‘turned into cheese’, as though the mountains were made of cheese (cf. Hebrew /*gbina*/ ‘cheese’). Jerome used the adjective *coagulatus*. Danuta Shanzer (2016: 178), who claims that “[T]he meaning of the Hebrew word [...] is unknown”, explains (*ibid.*, pp. 178–179):

³⁰⁹ https://it.wikipedia.org/wiki/Monastero_di_Fonte_Avellana

³¹⁰ https://it.wikipedia.org/wiki/Tempio_di_Giove_Appennino

³¹¹ http://www.ilgibbo.it/index_file/page955.htm

[...] The lines were rarely discussed and cause an exegetic bifurcation. The first Westerner to mention them before Augustine was Hilary of Poitiers, one of the witnesses for the Old Latin Bible text that read *montes coagulatos* (accusative). He had a pejorative interpretation: the mountains were diabolical powers because cheese was corrupted milk.

For Augustine, however, the *mons* was a happy place with paradisiacal connotations. He would later elucidate it as Christ, for he fed the young on milk and appeared on a mountain. His positive paradisiacal exegesis here might seem in the first instance to be motivated by a clear pun between ‘Cassiciacum’ and ‘incaseatus’. To achieve this Augustine used an Old Latin Bible text of limited attestation that had the reading ‘caseatus’ for Hilary’s ‘coagulatus’. *Incuseatus* seems to be Augustine’s *unicum*. Verecundus lent Augustine his estate at Cassiciacum for a period of philosophical *otium*, and the Lord is asked to grant Verecundus rest in return in (or through) Christ the Cheese-Mountain.

Augustine’s exegesis looks much less odd, however, in an Eastern context, where the Septuagint was the definitive text. Here, when exegetes had to make sense of the ‘coagulated’ or ‘cheesy’ mountain in v. 16, the Christological solution was current. Gregory of Nyssa provides the first identification of Christ with the coagulated mountain. For Athanasius, however, the coagulated mountain was the Church, full of milk, which is identified as simple speech. According to him the Psalmist challenged those who suspected that churches of the heretics were full of milk. Asterius of Emesa read the question as addressed by Christ to those who identified him with Moses or Elijah.

It is Dydimus the Blind’s exegesis, however, that is most relevant. He saw Jesus as the mountain of God, for he was the ‘fat’, ‘cheesy’, or ‘coagulated’ mountain that gave milk (in thye Pauline sense, viz. doctrine) to some to drink and solid food (viz. cheese) to others: [...]

I suspect that the reason the Septuagint translated *gabnūnīm* by opting for association with ‘cheese’ was because in v. 16, the Bashan is mentioned. The Bashan (Biblical Hebrew <bšn>³¹² *Bāšān*, which was called in Hellenistic and Roman times *Batanea*, though with a reduced area)³¹³ was, and is, a mountainous region in present-day southern Syria

³¹² Members of the Italian Jewish family Bassan (so named after the Venetian name of the town of Bassano del Grappa), when writing in the Hebrew alphabet for example in the first half of the 18th century, used to spell it <bšn>, thus by identification with the biblical toponym. This was facilitated by a regional pronunciation of Hebrew /š/ as [s], perhaps (at least in part) under the influence of pronunciation in the Venetian dialects of Italian (i.e., the dialects from the Venetia, of which, Venice’s dialect is just one).

³¹³ Steve Mason explains (2001: 54, fn. 310) that Batanea was “[t]he remnant of the biblical Bashan, but more confined in scope: essentially the region E and NE [east and northeast] of Gaulanitis [the Golan], which was immediately E [east] of Lake Gennesar”, i.e., of the Lake of Tiberiad. By that understanding, the Trachonitis region was not part of Batanea. Concerning Trachonitis, Mason states (2001: 54, fn. 309):

(including or excluding the Ḥauran/Jebel Druze),³¹⁴ and its being cattle-raising country, must have inspired the metaphor in *Amos* 4:1, “cows of Bashan who are on the mountain [i.e., hill country] of Samaria”, which is how the prophet Amos was castigating elite women in the capital (and its surrounding) in the northern Kingdom of Israel in his own days. Bear in mind that Amos was a cowherd: when a hostile official urged him to move to the Kingdom of Judah, where he could supposedly expect to earn his living as a prophet (whereas in the North his services were neither required, nor appreciated), Amos retorted that he was neither a (professional) prophet, nor the son of a prophet, but rather a cowherd and a sycamore fruit picker (*Amos* 7:14). Therefore, Amos’ reference to cows of the Bashan, yet ones found in or around Samaria, came from a person who had professional knowledge in cattle-raising.

I suspect that cheese was produced in the Bashan region and exported from there in Hellenistic times, so interpreting Hebrew *gabnūnīm* by relation to Hebrew *gēbīnā* ‘cheese’ may have seemed to the translators

“Literally ‘rough area’. Trachonitis was the uneven field of broken lava some 40 km S [south] of Damascus, in the area now known as the Leja, NW of Auranitis and E/NE of Batanea, in spite of this statement of Josephus that it was in Batanea”. In his autobiography indeed, Flavius Josephus (*Life of Josephus*, §54) gives Batanea a wider area, as he writes: “He [Varus] further planned to take up weapons, with those of Trachonitis in Batanea, and attack the ‘Babylonian Judeans’ – for this is how they are called – of Ecbatana” (Mason 2001: 54). Mason remarks (*ibid.*, fn. 311): “Varus’ plan is not gratuitous: these are precisely the people of Philip son of Iacimus, Varus’ rival. They are descendants of the Babylonian fighters settled in Batanea by Herod the Great to protect Judea from Tachonitis (*Ant[iquities of the Jews]* 17.23)”.

³¹⁴ Following 1869 when it became the centre of the Druze faith community, Mt. Hauran (where approximately one hundred years earlier a Druze settlement had been established), became known as Jebel (el-)Druze, whereas the latter had formerly been how Mt. Lebanon had been known. There had been civil strife between Lebanese Druze and their neighbours, especially the Maronite Christians from the 18th century, leading to a conflagration in 1860 that provoked the intervention of the great powers, brining about administrative autonomy favouring the Maronites. The situation had worsened because a leading family among the Druze that had become Maronite, requested continued feudal on the part of their Druze subordinate population. In the 18th century, Druze dominance in Lebanon was demographically replaced with Maronite dominance. Napoleon was unaware of that new situation, when, advancing from Egypt up to the walls of Acre, he sent the Druze a call to side with him. Druze leaders took this grimly, as they realise that Napoleon would change sides as soon as he would learn that the Maronites were dominant. They determined that were Napoleon to conquer their territory in what is now the Republic of Lebanon, they would emigrate to a hill region in northwestern Syria near Aleppo (Saleh 1989).

who produced the Septuagint to be appropriate. But I do not think this is what the Psalmist intended.

13. Concluding Remarks

I find it useful that Theo Vennemann has developed his theories about the linguistic prehistory of Europe and Germanic in particular, because of the debate they have provoked, and this even though my interpretation of the data discussed by Vennemann is different from his. Our differences are because we make different assumptions, mainly my acceptance of the longer chronology of Alinei's Palaeolithic continuity paradigm of Indo-European presence in Europe, and my acceptance of both Alinei's approach to the diffusion of farming in Europe, and Agmon's distinction of pre-Neolithic Proto-Semitic bilateral lexical roots, and Neolithic trilateral Semitic lexical roots, associated with concepts from material culture, whereas Vennemann makes hypotheses about either a "Semitic" or a Phoenician conduit of vocabulary he detects in Germanic as having Hamito-Semitic etyma.

In this study, I have tried to do several things at once, the trigger being the aim of evaluating and responding to Vennemann's book *Germania Semitica*, a book whose strongest chapters are the ones about the futhark and about names for coins (i.e., at the more recent end³¹⁵ of an-

³¹⁵ The most ancient end of interaction between Proto- or rather Pre-Semitic speakers and early Indo-Europeans (not yet in Europe) would have been in the Near East (in the perspective of out-of-Africa dispersal waves), according to Alinei's framework (1996, 2000); such joint presence in the Near East is also believed by part of the Nostraticists, but their timescale is shorter, because they subscribe to Colin Renfrew's or earlier theories about the time of the arrival of the Indo-Europeans; of the earlier theories claiming a late arrival of mature, militarised Indo-Europeans, Alinei (1996) rightly considers this as a modern myth, and he had likened this to the Indo-Europeans never allowed to be seen at too primitive a stage, and therefore rather springing out like Minerva out of Jupiter's head. Mallory (1989), according to Alinei (1996), is to blame for lending dubious support from archaeologists' quarters, pandering to the modern myth of Indo-European invasion as entertained by the philologists' tradition from the 19th century. Cf. Nissan (2010). Alinei (1996) has also also claimed that in parts of Europe, the Palaeolithic arrival of Indo-Europeans was either the earliest anthropisation, or joint presence with an adstratum.

For Alinei, whereas other disciplines had done away with catastrophism, in linguistics the catastrophe was maintained, being made into "the blitz invasion of the Indo-Europeans, and the obliteration of the pre-IE, whereas the antediluvian of the 'ac-

cient Semitic impact in Europe, through Punic traders). Whereas my own interpretation of the linguistic data in most other chapters is at significant variance with Vennemann's own, I find most of his formal reconstructions still valid and quite useful, if reframed in the longer time-scales (including the ones for the Shifts in the developments within Germanic) as assumed in Alinei's paradigm of Continuity from the Palaeolithic of Indo-European presence in Europe. This does away with the need, which Vennemann has tried to fill, to figure out the linguistic situation of western Europe before the arrival of the Indo-Europeans.

Alinei's theory of Europe's linguistic prehistory, and Noam Agmon's theory about the transition from bilateral to trilateral roots in Proto-Semitic around the time of the long transition from hunters-gatherers to farmers, much strengthen each other, I argue, and the two theories have a strong affinity in how they date lexical data.

Furthermore, without committing myself to the Nostratic hypothesis, I find that the detection of lexical parallels (however interpreted) in Dolgopolsky's, Bomhard's, and Ehret's collections are useful reper-

cursed race' was transformed into the unknowable pre-IE" (Alinei 1996: 344). The reference is to Johann Jacob Scheuchzer who in 1726, considered the remains of a fossil salamander to be those of the *Homo Diluvii Testis*, the man who witnessed the Deluge and was destroyed by it along with the rest of his accursed race (Alinei 1996: 348–349). Alinei also claimed (1996: 357–358, my translation):

[A]lso for the traditional theory, especially in its most canonical versions, the IE never knew the Palaeolithic or the Mesolithic. The IE the way we know them never were barbarians, let alone savages. They were born civilised, with vehicles with wheels, kings, horse-mounted warriors, priests and tripartite religions, buying, ransom, credit, rent, prices, and salaries! The IE cannot have had nothing to do with the 'accursed race' that became intermingled with the antediluvian fossil salamanders. A remark by Mallory is enlightening in this respect: 'Only by assuming the preposterous notion that the PIE language originated simultaneously with human speech itself can we imagine it to have been anything other than a segment of the overall continuum of human speech in Eurasia' (Mallory 1989, 145). The very idea that modern languages have anything to do with the birth of *Homo loquens* – an idea currently accepted by many interdisciplinary scholars – he finds 'preposterous'. Why? [...] The hypothesis is 'preposterous' simply because it denies a dogma, this being about the very recent and privileged character of IE, and about it being impossible that it participated in the prehistory of humankind.

toires. Alinei's and Agmon's insights interilluminate with, on the one hand, some of the Nostraticists' (whom they force into longer time-scales), and part of the formal reconstructions offered by Vennemann (ditto). The present study, in a sense, is a supporting reassessment of the potential impact of Alinei's paradigm on important insights of other theories, components of which can be complementary with Alinei's paradigms.

I am not in a position to assess Vennemann's *Europa Vasconica* hypothesis from his earlier book (2003a), but I can, and do, assess his "Semitidic" hypothesis, which I find more plausible to recast in terms of the diffusion of farming in Europe (in line with Alinei's understanding of it, not Colin Renmfrew's). As for Vennemann's *Europa Vasconica* hypothesis, scholars of Basque linguistics pointed out major flaws – see, e.g., in the volume edited by Udolph (2013) – and yet, in one of his papers (Vennemann 2006b, p. 972), not included in the book under review here, Vennemann was able to recall an email exchange with one of his most prestigious opponents on the Basque studies front, Larry Trask, who allegedly in one instance relented, being willing to concede that Vennemann's Vasconic etymology for names with **aran*-relating to valleys, in relation to Basque *aran* 'valley', in what were Celtic and Germanic areas, perhaps withstands scrutiny. Trask's (1999, pp. 160–161), criticising the Nostraticists (he was responding to Dolgopolsky 1998), remarked:

In fact, our standard handbooks undoubtedly underestimate the degree of diversity. [...] Nowhere is this diversity more evident than in the Americas, where specialists have so far been unable to reduce the number of established families below about 150 – and even this figure necessarily excludes an unknown number of indigenous languages which were obliterated by European settlement before they could be recorded. [...] What we see in the Americas, in New Guinea, in Siberia and elsewhere satisfies that most rigorous of all academic criteria: it confirms my prejudices. I believe that, before the comparatively recent economic and technological events stressed by Renfrew, most or all of the planet must have been a crazy-quilt of thousands of isolates and tiny families, with even a middle-sized family being a rarity at best. That appears to me the almost inevitable outcome of many millennia of foraging existence. In the last few thousand years, non-linguistic advantages have increasingly enabled some languages to spread and to grow into huge families, in the process obliterating the earlier diversity. Where this has not happened, we can still see that early diversity much as it formerly was. It is not Indo-European, but the immense diversity of the Pacific coast of North America, which best represents our linguistic past. [...] In fact, it is quite possible that our current picture of the world's languages significantly underes-

timates the degree of historical diversity. The reason for this is the existence of convergence phenomena, and above all of diffusion.³¹⁶

This is wise. But arguably, there are reconstructions that can be made of past contacts of proto-languages of such families to which we still have access through their daughter languages, and this even though we have no access to language families or isolates that disappeared (including in Europe), and therefore reconstructions must per force be incomplete, with etyma that are neither known, nor amenable to be conjectured.

I would venture to suggest that proto-language future research could hopefully see better into the matter of possible prehistorical influences, if any, of Vasconic, once the state of scholarly knowledge will have developed a better understanding of the Franco-Cantabrian refuge during the Ice Age. Cf. the study by Achilli et al. (2004), “The Molecular Dissection of mtDNA Haplogroup H Confirms that the Franco-Cantabrian Glacial Refuge Was a Major Source for the European Gene Pool”.

It stands to reason, I reckon, that linguistically different prehistoric groups that had already been present in Europe would have converged into that area, as the climate became more inclement. Some would have been Vasconic speakers, I suggest, whereas others would have been Indo-European by language, especially Proto-Celts, according to Alinei’s theory. But as the ice receded,³¹⁷ those people would have been enabled to spread out: even some Proto-vasconic speakers may have spread out in the process, as a minority within the Celtic (and Germanic) diffusion.

³¹⁶ Moreover (Trask 1999: 162–163): “Convergence, of course, need not be an insuperable obstacle. In southeast Asia, for example, specialists have enjoyed some success in extracting a genetic signal from the large amount of diffusional noise: the assignment of Vietnamese to Austro-Asiatic is a good example. [...] Even so, we cannot hope to succeed in this without limit, and southeast Asia must be very close to the limits of what we can hope to achieve in distinguishing common ancestry from diffusion. Unless we want to claim that cases more difficult than Vietnamese cannot exist, we are forced to confront the possibility that some of our accepted groupings are chimaeras resulting from massive diffusion. [...] If our understanding of convergence phenomena is severely limited, we may be in danger of mistaking areal groupings for genetic families, and of reconstructing proto-languages which never existed. That is precisely the concern which worries me and some of my colleagues”. For good reason indeed.

³¹⁷ And also, the Ice Age lakes that in practice were an obstacle to human groups moving between western and eastern Europe, also receded. This is an aspect of the matter that Alinei has incorporated into his theoretical framework.

Alinei's claim about Palaeolithic, Mesolithic, and Neolithic presence of Indo-Europeans in Europe readily concedes the possible presence of adstrata (as opposed to earlier theories resorting to pre-IE substrata). Such spreading out from the Franco-Cantabrian refuge possibly being ethnolinguistically diverse, as ice had been receding, is somewhat similar to the later process by which both Celtic and Germanic people from the plain being submerged by what became the North Sea ended up in Great Britain, as Alinei would have it.

Vennemann's theonyms evidence, I think – indeed the separate borrowing of the same theonyms in different periods – is very important for telling which is what, and propending for acculturation, as opposed to mere *Wanderwörter* which the other lexical evidence does not exclude. The earlier borrowing would have been during the Neolithic spread of farming. By the way, if one is to adopt Agmon's (2010) and Agmon and Bloch's (2013) theory of the evolution of biconsonantal hunters into triconsonantal farmers in the prehistory of Semitic languages, it stands to reason *Ba'al* developed as a Neolithic epithet (the semantic motivation being from 'lord', 'owner'), the veteran, Mesolithic theonym and general term for deity rather being *El*, whose semantic motivation is from 'strength'.

Apart from his hypotheses about names for coins, Vennemann's research in the futhark is more cogent than anything else in the book under review. Earlier scholars had pointed out that extant runic inscriptions as found in Germany and Germanic lands are more abundant in areas close to the Atlantic or North Sea or Baltic littoral, and less abundant in German lands far from the northern littoral. This used to be a problem for the theory of derivation of the Germanic runes from Old Italic scripts. But, it is fair to add, that very distribution may be an argument in favour of the plausibility of Vennemann's approach to the futhark. True, one does not actually find on that northern littoral Punic archaeological and epigraphic findings, in contrast to their conspicuousness in the western Mediterranean, and *non vidimus* is a weighty argument, but perhaps it was the policy of secretiveness of the Gaditans of Cadix and of Punic traders in general in their navigation along the European Atlantic littoral³¹⁸ (such was the secretiveness, that upon no-

³¹⁸ They were apparently not as secretive along the African Atlantic littoral. Cf. Ramin (1976), Roller (2006), Nissan (2010 [2011]b), Demerliac and Meirat (1983), Blomqvist (1979).

ting he was being followed by a Roman ship, a captain caused his own ship to sink), that made monumental or epigraphic displays of presence superfluous.

Typos are quite rare, in Vennemann's *Germania Semitica*. On p. 434, one comes across both "Mother Tongue" and "Mother Tonge".³¹⁹ On p. 484, in note 26 read "Roussillon" instead of "Roussillin". On p. 526, note 38, "segolite" should be "segolate" (a class of nouns in Hebrew grammar). On p. 58, line 2, replace *an* with *a*, in "an Hamito-Semitic substratum". On p. 515, in Sec. 26.6.3.2.2, the cross-reference to Sec. 26.2.3.1 is wrong (Sec. 26.2 has no subsections); it should rather be to Sec. 26.6.2.3.1 on pp. 511–512. On p. 267, note 1, line 3, one finds "the the"; and on p. 418, note 40, line 7 from the bottom, "culture" for "culture". The bibliographical entry for Mufwene, on p. 688, is not in its proper alphabetical place. The two bibliographical entries for Mogk are misplaced after Moltke.

On p. 71, in an example from Hebrew, for the biblical personal name *Sheba*, the transcription *Šæbar* is given; of course, the last letter should be ^{◌̣} or ^{◌̣̣} – the voiced pharyngeal, not *r*. On p. 415, note 15, the Hebrew word for 'fragrance', 'balsam', which is spelled *bšm* (but the middle consonant standing here for the phoneme /š/ rather than /ṣ̌/: the Hebrew letter <š> stands for either phoneme) and is transcribed by Vennemann as [bæšæm], should be *béšem*, but then note that this is an unusual variant (occurring in *Exodus* 30:23), whereas the usual form of the noun is *bóšem* (*Exodus* 30:23 [the second occurrence of the term inside this verse, the first one being *béšem*], *Exodus* 35:28, *1 Kings* 10:10 [with a parallel in *2 Chronicles* 9:9], *Isaiah* 3:24, *Ezekiel* 27:22, *Song of Songs* 5:13, 6:2).

My present readers would hopefully agree that even as one may often disagree with Vennemann, nevertheless engaging with the book under review is something fruitful. Once one also considers a few other theories that have been around since the 1990s to the present, it offers an opportunity for bringing into focus problems and ideas, and the outcome of this conversation does not necessarily have to be according to a zero-sum game. There are components, or elements, or interpretations, or identified data, in these theories that when looked at and en-

³¹⁹ Surely not referring to the fiery Baroness (Jenny) Tonge at the House of Lords in London, most noted for her extreme views against Israel, because of which she had to leave the Social Democrat party.

gaged with critically, enable an enhanced picture to emerge. Renfrew's theory was an improvement upon what preceded it, but I and some others find Alinei's paradigm more cogent, and Agmon's recent theory appears to lend it support (even though Agmon's theory does not contradict Renfrew's theory, for which it is mostly irrelevant). Just as nobody in his or her good senses would dream of belittling Renfrew's efforts (and let us remember how generous he in Cambridge, enabled in turn by a grant from the Sloan foundation, has been to the Haifa-based Aharon Dolgopolsky, even after the latter's demise, by making this Nostraticist's later works, culminating with his 3,000-page e-book, available for scrutiny without fully endorsing them),³²⁰ likewise Theo Vennemann deserves appreciation for his efforts and for inviting us into the arena of a lively debate. Alinei, Renfrew, Dolgopolsky, and Vennemann are courageous people who have been willing to take risks. I hope to have shown that it is a debate conducive to progress.

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³²⁰ Renfrew is an archaeologist. In a book he edited in response to Dolgopolsky (1998), Lyle Campbell (1999: 218) alluded to Renfrew not being a linguist, when stating: "This leads me to caution non-linguists from embracing Nostratic prematurely, which, as I try to show in this paper, is not generally accepted by historical linguists for good reasons". After noting that a statement made in Renfrew's preface to Dolgopolsky (1998) is immaterial, Campbell warned sternly: "Rather, the methods of linguistic palaeontology are rather exacting, permitting inferences where warranted by the evidence, but not encouraging raw 'speculation'. Until such time as the Nostratic hypothesis may, if ever, pass muster with the standard, legitimate, and accepted methods of historical linguistics, there can be no trustworthy non-linguistic inferences based on it". Basically, the main thrust of Campbell's argument against Dolgopolsky was that many of his data appear to be loanwords rather than cognates. Note however that Dolgopolsky was quite open to this possibility, and thus to diffusion rather than genetic relation.

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³²¹ But Carl Darlin Buck, born in 1866, had already died in 1955.

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