

ANDRZEJ ZABORSKI

Kraków

**R.L.Trask**

***The Dictionary of Historical and Comparative Linguistics.*  
Cambridge 2000. Edinburgh University Press. XII + 403 pp.**

Although dictionaries and encyclopedias of general linguistics have been rather numerous in the last period, this “Dictionary” limited to historical and comparative linguistics offers a lot that cannot be easily found elsewhere. It has a wide selection of modern books and articles as its sources. It is almost complete as far as newer terminology and newer ideas are concerned and it offers also a substantial part of older terminology used in English and to a limited extent also in French, German and Latin. Fortunately enough modern comparative-historical linguistics has been relatively free of terminological extravagancies and ‘revolutions’ sometimes limited largely if not exclusively to terminological misuse. In this dictionary not only current and universally accepted terminology is explained but, rightly, even some unnecessary new terms can be found, e.g. “symplesiomorphy” for “shared archaism” (p. 332), “elaboration” (p. 101) for “language split” or terms which are rather facetious like “diachronic conspiracy” (p. 71 and p. 106), “panda principle” (p. 245), “saltation” (p. 296) and “superjump” (p. 330), “Hydra’s razor” (p. 311) etc. The author has added also lemmata on major language families of the world as well as on unclassified and otherwise important languages. In the case of the classification of languages specialists on particular families and areas will, inevitably, find proposals which are a matter of dispute but in general the overall picture seems to be well-balanced. Definitions are clear and precise, usually there is a sufficient number of good examples.

Here are some remarks which might be useful for a new edition:

p. 13: Akkadian was not “recorded in writing from 2350” – Old Akkadian texts are usually dated about 2500–1950 B.C. although no absolute dating is available.

p. 19: Table 1: second person sing. in Latin is *amas*, not *amo*!

p. 20: the main lemma on “Analogy” is too general although other lemmata on particular problems of analogy are good.

p. 25: the Koran was written down in the mid seventh and not in the eighth century A.D.!

p. 25: it is not precise to say that Aramaic “displaced Hebrew as the mother tongue in Palestine in the third century BC”. There was a triglossia i.e. Aramaic, Biblical Hebrew and Spoken Late Hebrew were used (plus Greek among educated people) and it is possible that there were some “naitive speakers” i.e. speakers having Hebrew as mother tongue even in the first two centuries A.D. By the way, there is no lemma “mother tongue” in the “Dictionary”!

p. 26: R. Hetzron has greatly contributed to the promulgation of the principle that “in the absence of any conditioning factors, the most heterogeneous system of several related systems in related languages is the closest to that of the ancestral language, and the more homogeneous systems result from simplification” but this was known long before Hetzron (see e.g. Th. Noldeke in case of Semitic linguistics) and I doubt whether it should be called “Hetzrons principle”.

p. 37–8: Bantu languages are agglutinating, not simply prefixing!

p. 38: “Barth’s Law” deals mainly with the prefix-vowel of the ‘Imperfect’ in Semitic languages.

p. 39: “becanism” is just another name for “folk etymology” to which there is no cross-reference here!

p. 41: Trask makes a dangerous shortcut when he says that Berber is spoken “principally in Morocco”. Although the percentage of Berber, first of all Kabyle speakers in Algeria is smaller, nevertheless Berber language and culture is equally important in Algeria. Berber is one of the official “national” languages of Mali and Niger.

p. 54: it is questionable whether Hausa “has more speakers than any other sub-Saharan African language” since Amharic and Oromo have probably more.

p. 65: systematic sound correspondences between Afroasiatic languages have not been discovered in mass so far simply because little research has been done as yet and not because the languages are, allegedly, “moderately closely related”. The degree of relationship of the more archaic Afroasiatic languages does not seem to be more remote than that of most Indo-European languages. It is rather the lack of older records of the majority of Afroasiatic languages which makes the reconstruction of systematic sound correspondences difficult but certainly not hopeless.

p. 75: The classification of Omotic as allegedly constituting a primary branch of Afroasiatic is based on very inconclusive negative evidence. The traditional classification of Omotic as West Cushitic has not been really refuted yet.

p. 76: Arabic and not Coptic is a liturgical language of Coptic Christians since a long time! Coptic is used, if at all, only as a very marginal embellishment of the Coptic liturgy. It is interesting that since several decades there have been attempts by some educated Copts to start learning how to use Coptic in everyday speech and some Copts even pretend that they speak Coptic which deserves the greatest respect in spite of the fact that this is rather a case of beautiful and harmless wishful thinking.

p. 77: I do not think that every Creole “derives from an earlier pidgin”. Imperfect knowledge of the source language does not automatically mean that there is a real pidgin behind a Creole.

p. 78: Cushitic: the relegation of Beja from Cushitic was simply a mistake. Beja is the only representative of North Cushitic. Alleged division into ‘South Cushitic’ and ‘North Cushitic’ and the single language Beja is simply wrong.

p. 80: A dead language is simply a language which is not a mother tongue of any living person! Latin is still used either as a secret language (e.g. by medicine doctors at least in some countries) or as a traditional official written language of some documents (in the Catholic Church) or even as liturgical language (to some limited extent in the same Church and in some of its offshoots). How languages die out is another question and should be dealt rather under “language death” (p. 183). It is true that in the current usage usually no differentiation is made between dead and extinct languages and between language death and language extinction but perhaps we should try to use “dead language” for languages which have ceased to be spoken either because its speakers shifted to another language or because they died out or have been exterminated and the term “extinct languages” should be used rather for languages like Latin which have simply evolved so far that its later stages have to be treated as separate languages. There should be also something on languages which are “half-dead”/“half alive” like Modern Literary Arabic which is used primarily in writing, only to a limited extent in speech (it is learnt in schools!) but which is really nobody’s mother tongue, viz. it has no real native speakers although most Arabs consider it to be their “mother tongue” in another sense of the term for ideological reasons.

p. 83: there should be an example of degrammaticalization in the sense 1. and 2. A better example for degrammaticalization in the sense of ‘deaffixation’ (p. 80–81) is bus < omni-bus ‘for all/everybody’.

p. 85: There should be cross-references from ‘density of attestation’ (p.85–86) to ‘multilateral comparison’ (p. 221) and vice versa! It was already e.g. A. Meillet who insisted that the best cognates are those found in at least three or more languages rather than in two only.

p. 87: German “Kuchen” (not “kuchen”) is “cake”, not “cook”. Cf. also /x/ > /c/ e.g. in Küche “kitchen” and its derivatives like Küchengeschirr etc.

p. 89: there should be a lemma on “dialect cluster”!

p. 91: It is wrong to say that “the Afroasiatic languages show extensive evidence of dialect mixing”. Perhaps only few of over 250 Afroasiatic languages may show it.

p. 91: nothing on “dialectometry” (German “Dialektometrik”) as developed e.g. by W. Möhlig.

p. 92: it is wrong to say that “Diglossia was first identified by Charles Ferguson (1959)” since for obvious reasons it was studied and defined for the first time by Greek scholars, e.g. Psichari at the end of the 19th century while in case of Arabic studies it was introduced by William Marçais (most probably following the studies on Greek diglossia) later in the twentieth. It would be better to limit the use of ‘diglossia’ only for cases when two languages are close relatives or simply different stages of the same language like Latin and Italian in Italy still in the 11th century and today’s Arabic, German as used in Switzerland, Luxemburg and some other regions or Greek in which the diglossia has been officially abandoned but still survives marginally. When ‘High’ and ‘Low’ languages belong to different families (like Spanish and

Guarani in Paraguay) or, in other cases, are only distantly related we should speak rather of a special kind of bilingualism for which (i.e. for this special case!) a new term, e.g. “bilingualism with complementary distribution” should be used. There are also cases of triglossia (e.g. Spoken Arabic plus Modern Literary Arabic plus spoken and written French in Maghreb and in Lebanon) and even quattroglossia when Berber is added in Maghreb. Note also that many Arabists deny the existence of diglossia in contemporary Arab countries and speak only about “dialect spectrum” or “spectroglossia” which is controversial.

p. 96: I wonder whether Dolgopolskiy still subscribes to his 1964 list of “the fifteen words which are supposedly most resistant to lexical replacement”. It looks like “I/me” have been replaced at least in the great majority of the Omotic (West Cushitic ?) branch of Afroasiatic where the first person sing. is synchronically “ta” and the second person sing. is “ne” while elsewhere in Afroasiatic it is “na” and “ta” respectively. This development might have taken place in Omotic as a combination of internal change and borrowing from Nilo-Saharan.

p. 98: we should not forget that very learned and rare “dysphemism” has “vulgarism” as a good current synonym!

p. 99: Eblaite, as much as we know it so far, is closely related to Akkadian and even classified as a dialect of Akkadian by some specialists which is disputable. It was deciphered not “in the mid-twentieth century” but shortly after its discovery in 1974. It is not clear at all whether it was “displaced by Amorite”.

p. 101: a critical edition is not simply “based upon two or more variant manuscript versions of a single text”!

p. 101: Egyptian is recorded also in “hieratic” script!

p. 104: there should be an example for “endogenous pidgin”!

p. 121: There is nothing on “fashion” – its importance for “sound laws” was emphasized already e.g. by H. Schuchardt.

p. 124: “Folk etymology” is not tantamount to “analogical reformation”.

p. 125: Fortunatov’s “law” on Slavic accent was independently proposed by de Saussure!

p. 127: there is nothing on “frequency”!

p. 130: Gə‘əz is still used as a liturgical language by the Ethiopian Church.

p. 132: It would be interesting to know which particular languages might be seriously (?) considered as genetically related on the basis of only two “genetic markers” as alleged by Johanna Nichols.

p. 134: Can German be defined simply as “a major language of central Europe”? No!

p. 138: I doubt whether the term “glottogenesis” should be limited to “the creation of a non-genetic language”. E.g. in case of artificial languages I should speak only of “creation”.

p. 146: In case of a “hapax legomenon” it is first of all the difficulty of discovering its precise meaning that matters while its reality is not always in doubt.

p. 148: There is a dispute when Hebrew actually died out and there can be no doubt that it was still partially used as a “high” language in speech after the third century B.C. in a situation of triglossia (Aramatic – late Spoken Hebrew/late Mishnaic

Hebrew – Biblical Hebrew). Modern Hebrew is not “the mother (spacing – A.Z.) tongue of most Jewish inhabitants of Israel” yet.

p. 156: In Egyptology hyphen is used to separate parts of a compound while it is a dot (full stop) which separates endings from the stem, see e.g. W. Schenkel, *Einführung in die klassisch-ägyptische Sprache und Schrift*, Tübingen 1989, p. 31.

p. 157: no differentia specifica between ‘ideophone’ and ‘onomatopoeia’ (p. 239) is given and there is no cross-reference.

p. 161: there should be a warning that in every hypothesis of an alleged ‘independent parallel innovation/parallel development’ in closely related languages there is a serious risk of a vicious circle error.

p. 196: the notion of “linguistic area” (German “Sprachbund”) has been introduced by Trubetzkoy.

p. 215: Mbugu or Ma’a has proven not to be a “mixed language”. Its variant containing many lexical Cushiticisms is actually used as a secret language by the people who use a clearly Bantu language as their “main” language. I also have doubts whether Mednyj Aleut is a “real” mixed language i.e. a language with allegedly two ancestors – its verbal morphology has been borrowed from Russian but obviously this language is so much closer to Aleut (and totally incomprehensible for Russians!) that there is no doubt that it is Aleut diachronically.

p. 215: there is no proof that Moabite was spoken as early as the fourteenth century, the Qldest indisputably Moabite inscription dates back to the 9th century B.C.

p. 226–227: in the lemmata on Neogrammarian school there should be a mention that finally Neogrammarians considered sound laws (cf. p. 316) to be exceptionless only on a given territory and in a given period!

p. 256: it is astonishing that usually the existing definitions of a pidgin say nothing about the crucial prerequisites for the creation of a pidgin language: rudimentary knowledge (often limited to some lexicon) of the source language and lack of possibility and/or will to learn it properly plus the lack of practical need to develop the skill beyond simple practical, e.g. commercial communication. Pidgins do not develop when for any reason people can and want to learn foreign languages.

p. 259: “Portmeanteau language” which has allegedly “emerged from a mixture of elements contributed by .... closely related languages” is another “ghost” concept haunting comparative linguistics – no case of this is known apart from little known Tlingit. As a rule, alleged “mixed” languages are unknown or little known languages.

p. 280: there is nothing on ‘relationship’!

p. 290: nothing on ‘root determinatives’ although there is “root extension”.

p. 295: Ukrainian and Belarussian are not “southern and western varieties” of Russian even diachronically! They are different languages!

p. 320: under “split” there should be a mention of language split i.e. language genesis in cases like Macedonian, Moldavian, Tadjik, Bosniac (?) etc. in which extra-linguistic factors contributing to the split greatly dominate linguistic factors.

p. 327: there is nothing on “style” and style (e.g. archaizing) variation.

p. 327–328: although the importance of shared innovations cannot be overestimated, nevertheless it is not really true that “shared archaisms are of little use in

subgrouping” since shared innovations without shared archaisms should be attributed rather to contact.

p. 344: Trask rightly emphasizes that glottochronology is wrong but nevertheless he insists e.g. that “the time depth of the Indo-European family is commonly estimated at around 6000 years” and that “a few seemingly secure families, such as Afroasiatic, appear to be older than this (viz. 10 000 years –A.Z.). See also p. 26 on ‘archaeolinguistics’. What is the basis of these estimates then?

p. 366: ‘Wanderwort’ is sometimes also used in the French version as “mot voyageur”.

In short: this is a very useful tool not only for beginners and advanced students but a good synthesis and reference book for professionals.

Kalwaria, July 2001