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Book review

James Clackson, Indo-European Linguistics. An Introduction. Cambridge University Press (Cambridge Textbooks in Linguistics), 2007. xxii + 260 pp., £ 17.99 (pb)/£ 45.00 (hb) ISBN 9780521653671.

The present book is unique. It is neither a textbook teaching the student how to reconstruct Proto-Indo-European, nor is it a comprehensive survey of the way PIE is presently being reconstructed. Rather, it provides a clear and balanced discussion of some of the main theoretical questions in the reconstruction of PIE. In the words of the author (p. xii), *IEL* does not aim to be a “reliable guide” but “aspires more to the status of a toolkit, offering up sample problems and suggesting ways of solving them.” As a result, it seems to me that *IEL* will find a broad audience. On the one hand, students of Indo-European will profit from consulting *IEL* beside introductory textbooks such as Beekes (1995), Szemerényi (1996), Fortson (2004). On the other hand, professional linguists with an interest in historical linguistics will find in *IEL* a more accessible discussion of many famous Indo-Europeanist problems than is provided by the extant handbooks. Due to *IEL*’s focus on a selected number of case-studies, it should be possible to study separate chapters of the book without first mastering all the data from the previous chapters. The problem-oriented approach is borne out not so much by the titles of the seven chapters (1. The IE language family, 2. Phonology, 3. Morphophonology, 4. Nominal morphology, 5. Verbal morphology, 6. Syntax, 7. Lexicon and lexical semantics), but especially by the headings of the respective subchapters. For instance, Chapter 2 features: 2.1. Reconstruction and the comparative method, 2.2. The sounds of PIE, 2.3. The realization of PIE phonemes: the glottalic model, 2.4. Mergers and splits: PIE velars, 2.5. Reconstructing lost phonemes: laryngeals. Chapter 5 features: 5.2. The Greco-Aryan model of the PIE verb, 5.3. Reconciling Anatolian to the Greco-Aryan model, 5.4. The Hittite *-hi* conjugation, 5.5. The PIE middle, 5.6. Roots and stems. The chapter on syntax (p. 157–186) takes up a much larger part than is normally the case in handbooks of Indo-European.

Throughout the book occur a number of exercises which are generally intended for the beginner; the answers can be found on the website www.classics.cam.ac.uk/clacksonindoeuropean. Every chapter is concluded by a section ‘Further reading’, and by a number of ‘discussion points’. The latter may be interpreted as review questions by the student, while offering substantial food for thought for the advanced scholar. To give just one example (p. 156, at the end of the chapter on ‘Verbal morphology’): “4. ‘Indo-Europeanists are very good at finding ways of linking morphological forms in different languages. They are less good at finding convincing semantic pathways to explain the morphology.’ Is this fair criticism?”

It is inevitable that a colleague in Indo-European linguistics will disagree with some aspects of the reconstruction of Indo-European as offered in *IEL*. But instead of enumerating our differences here, I prefer to address a small number of more general linguistic issues. While the

points I will be making may be obvious to any Indo-Europeanist (or, indeed, to any historical linguist), the way they are formulated in *IEL* might confuse non-specialist readers. And since the book is such a good chance to explain what we do to a general linguistic audience, it seems recommendable to be clear on these points of methodology.

In section 1.4, Clackson discusses two different phylogenies of PIE which were recently composed: the 'New Zealand' tree by Gray and Atkinson (2003:437), and the 'Pennsylvania' tree by Ringe et al. (2002:90). These trees were arrived at by different methodology: the New Zealand tree uses a 'basic vocabulary list' and does not rely on IE scholarship, while the Pennsylvania tree takes into account the results of IE linguistic comparison. As was to be expected, the Pennsylvania tree is much closer to the usual form of the IE family tree that we have come to be accustomed to over the last 150 years. Clackson observes that the New Zealand tree measures the affinity of lexicons, while the Pennsylvania tree is based on the study of linguistic innovations and changes. Clackson concludes that "It is perfectly possible for both phylogenies to be correct." The value of this statement depends on the interpretation of the word 'correct'. This can only be true if one of the phylogenies is not a genetic tree of descent at all, and I would argue that this applies to the New Zealand model. Since linguists are used to regarding trees as representational models for diachronic or hierarchic relationships, the tree form of the New Zealand model inevitably invites a comparison with the traditional *Stammbaum*. But if the New Zealand tree merely maps the "relationship of vocabularies" (p. 13), it would be equally or more appropriate to use a network model, without the suggestion of a common origin for all branches. In section 1.5, Clackson addresses a version of the New Zealand tree which includes putative datings for PIE and the various IE branches. After criticizing the research method involved, he cautiously concludes that "the jury is still out" (p. 19) on the question whether the age of the IE language family can be computed by means of phylogenetic dating.

At the end of section 1.4, Clackson favourably discusses a recent article by Garrett (2006), who disputes the widespread view of Mycenaean (attested on clay tablets dating roughly from 1400 to 1200 BC) as the oldest attested Greek dialect. Garrett claims that none of the morphological innovations that distinguish the later alphabetic Greek dialects from PIE can also be shown to exist in Mycenaean. This would imply that no such entity as 'Proto-Greek' could be reconstructed; rather, the Greek sub-group was (in Clackson's words) "only truly formed in the period after Mycenaean, when convergence between the different dialects of Greek took place, in part related to social changes coupled with a strong sense of Greek ethnic identity" (p. 15). The point which Garrett is making seems to me true enough: the IE sub-groups that we know are but the scanty remains of a much larger number of prehistoric, intermediate dialects. The "true historical filiation" of the IE family (if 'true' means 'complete') is therefore unknowable. But it also appears to me that Garrett is overstating his case, partly because he gives an incomplete analysis of the Mycenaean facts. The language of the Linear B tablets shows Greek innovations such as the *-t-* in the oblique cases of *mn*-stem neuters (*pema* 'seed', *kemata* 'pieces'), the spread of the genitive plural ending **-sōm* to the *ā*-stem nouns (*-a-o*), the use of the suffix *-ēu-* to form m. agent nouns and names (*ijereu* 'priest', *amoteu* 'cartwright', etc.), the acc.sg. pronoun *min* (*-mi* 'him, her, it'), the spread of the *s*-aorist to denominative verbs (*ereuterose* 'set free', with restoration of *s* after the sound-law **s > *h*), the rise of alpha-thematic *s*-aorists (*dekasato* / *déksatol* 'accepted', *akera₂te lagerrantes* 'gathering'), and the rise of the perfect middle. Of course, one may hypothesize that, if we had more evidence for other IE languages/dialects from the southern Balkans in the second millennium BC (e.g., from pre-Illyrian, pre-Macedonian, or pre-Thracian), "we might not be able to separate out what was 'Greek' about Mycenaean from its neighbours" (p. 15). The point is: we don't have such evidence. It is exactly *because* the

intermediate dialects which existed earlier have been lost, and because the known dialects of Greek share a unique set of linguistic innovations, that we can work with the family tree model. Moreover, it would be easy to turn the argument around: if we possessed more and longer Mycenaean texts, which were written in a full alphabet instead of a defective (C)(C)V-syllabary, we might see the Greekness of Mycenaean much clearer.

In section 1.6, the Nostratic theory is discussed, which holds that PIE is but one offshoot of a prehistoric macro-family which also encompassed other language families in Eurasia such as Uralic, Altaic, Kartvelian, Dravidian, and maybe others. Clackson concludes in the negative (p. 23), giving both a general reason (Nostratic data are ambiguous and doubtful) and a specific one (the reconstruction of the PIE word for ‘husband’s sister’ does not become any clearer by comparing the putative Nostratic cognates) why Nostratic linguistics can be neglected by Indo-Europeanists, at least for now. His rejection is understandable in view of the unconvincing results yielded by large-scale comparisons between all possible Nostratic languages at the same time. Yet one could very well attempt an alternative method, viz. to first compare PIE with one other language family, in order to take one step at a time. After all, as Clackson notes throughout *IEL*, it is becoming more and more of a communis opinio that the Anatolian languages were the first to branch off PIE, and that, strictly speaking, we should therefore reconstruct a Proto-Indo-Hittite stage of language and an Anatolian-less later PIE—the terminology to refer to these stages has not been agreed on yet. In the same vein, it seems perfectly legitimate to go looking for the language family mostly resembling PIE, and see if we can reconstruct a common ancestor. In practice, indications of geographic proximity to the IE homeland to the north of the Black Sea, and what few serious comparisons have been made so far, suggest that the Uralic family would be the most likely candidate for this role. For an attempt to put the Indo-Uralic theory into practice, see Kortlandt (2002, 2004).

On p. 32, Clackson remarks that “the regularity of sound change is not an essential factor to ensure the success of the C[omparative] M[ethod].” This claim is incomprehensible to me: in my view, the regularity of sound change is the *only* reliable instrument we have in establishing linguistic descent. While Clackson rightly credits William Labov and other sociolinguists for showing that sound change does not happen overnight, I do not share his conclusion that modern studies have shown that sound changes are not exceptionless. On the contrary, the most striking aspect of detailed dialectological and sociolinguistic studies is the very fact that the ‘exceptionlessness’ of sound changes is confirmed time and again. Of course, ‘exceptionlessness’ is to be understood in the original Neogrammarian sense, with due attention for the different phonetic surroundings in which a sound occurs in different words. The phenomenon of lexical diffusion, and the restriction of some sound changes to certain groups in a speech community (p. 33), are due to sociolinguistic factors which in-depth studies like Labov’s are often able to distinguish. In reconstructed stages of language, the process was no different; but we are usually ignorant about the sociolinguistic variables that played a role. Hence, we are often unable to say why a certain sound change observed in an extinct corpus language is not found in all the words it was supposed to affect. But this ‘exceptionality’ is due to our lack of background knowledge, not to the inherent characteristics of the change itself (compare Campbell, 2004:211–224 for a similar summary of this issue).

That this is no moot point becomes clear on p. 60–61, where Clackson adduces the example of the way in which many of the original guttural consonants of Arabic were lost in Maltese, probably due to language contact. Quoting from an article by Comrie (1993:92–95) who observes several “idiosyncratic developments” of the Maltese gutturals, Clackson draws a parallel with the incompletely understood development of PIE laryngeals in Greek. Possibly, he argues, the

Greek developments were due to language contact between the Greeks-to-be and the earlier inhabitants of Hellas. Clackson then repeats that “the comparative method does not rely on absolute regularity”, and that the PIE laryngeals may be reconstructed “without the assumption of rigid sound-laws.” To my mind, there is no distinction between ‘rigid’ and ‘non-rigid’ sound-laws. Our method requires us to assume that a sound-law is always valid under the conditions for which it was formulated; but, also, to keep looking for the best formulation of the sound-law. Many exceptions to a sound-law are found to obey a more detailed, previously overlooked sound-law; other exceptions are due to system-internal analogical pressure. Where there are sociolinguistic factors at work (i.e. in the famous examples of Latin *bōs* ‘cow’ and *lupus* ‘wolf’, which appear to be loanwords from a non-Roman, Sabellic dialect into Latin), we are by definition dealing with forms coming from competing systems, which are therefore not exceptions to the regularity of a single system. In the case of the Maltese gutturals, it should be noted that we are lacking some essential data on the precise sociolinguistic mechanisms that were at work when Malta was repopulated with Arabophone settlers in 1048 AD (possibly coming from Sicily) or during the reintegration into the Christian Sicilian kingdom after 1220 (see [Prevaes, 1993:17–25](#)). Also, as Clackson indicates himself, the vowel quality after the loss of gutturals is influenced by morphological factors, which means that analogical changes have played a role in the outcome.

I will end this review with one suggestion and a few small corrections. On p. 84–85, Clackson discusses the theories of Hock (1993) and Halle (1997) that there could have been a difference between accent and stress in PIE: all morphemes of a word would have been inherently accented or non-accented, but only one morpheme per word could receive stress. Combining the accounts of Hock and Halle, Clackson arrives at three accentual rules which could explain the paradigmatic stress patterns of PIE nouns: “(1) Words with only one accented morpheme stress that morpheme, (2) In words with two accented morphemes, the leftmost is stressed, (3) Words with no accented morpheme receive stress on the leftmost element.” I observe that these rules introduce the new notion of ‘(un)accented morpheme’ without giving criteria for why a certain morpheme would be accented or non-accented. Therefore, it is a reformulation of the distributional rules that we already know, but no explanation: one would like to know *why* certain morphemes were inherently accented or non-accented. In particular, one would expect either a phonetic or a semantic/pragmatic motivation for such a class distinction. A phonetic background has been proposed by Lubotsky (discussed by Clackson on p. 78), at least for part of the evidence: the different accentual behaviour of PIE morphemes would be due to an earlier stage of PIE as a tonal language, in which the tone of a morpheme depended on the phonation type of the consonants.

Small corrections:

p. 8: In table 1.1, the names Lusitanian and Albanian should move one column to the right; the name Nuristani should move three columns to the right.

p. 76: The insight that the tonal contrasts of Baltic and Serbo-Croatian do not directly continue PIE differences in intonation, but are an innovation, is older than 1987. It has developed roughly between 1957 and 1975 by the investigations of Stang, Dybo, Illič-Svityč, Kortlandt and others (see [Derksen, 1991](#), for a survey).

p. 92: The ‘Overview of nominal declensions’ distinguishes “three separate classes” of nouns with “different system of case-marking”: nouns in **e/o*, nouns in **-h₂*, and the remaining class of root nouns, *i*- and *u*-stems and consonant stems. Yet it has been sufficiently shown and seems to be generally accepted that the suffix **-h₂* originally behaved as any other consonantal

suffix, and that the main separation accordingly lies between **e/o*-stems on the one hand, and the stems with a consonantal suffix (including root nouns, **h₂*-stems, **i*- and **u*-stems) on the other.

p. 94: For “PIE **dyéw-s* or **dyéw-s*”, read “**dyéw-s* or **dyéw-s*”.

p. 95: PIE **-ēn* and **-ōn* did not develop into Avestan *-ā*, but into Old Avestan *-ā* and Young Avestan *-a* (*-ā* in monosyllables). Hence, the word *ziiā* ‘winter’ cannot directly reflect PIE **g^hyēm* or **g^hyōm*, but goes back to a preform in **-ā-s*, with *-s* being added analogically to the expected form **zyā*.

p. 193: For Greek *ócris*, read *ókris*.

p. 204: Albanian *nuse* ‘bride; daughter-in-law’ cannot be a regular reflex of PIE **snusos* ‘son’s wife’. The probable Proto-Albanian form was **nutja*, the origin of which is disputed by Albanologists. Possibly, it arose by cluster reduction from a Latin loanword *nupta* ‘wife, bride’ or *nuptiae* ‘wedding’.

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Michiel de Vaan
Comparative Indo-European Linguistics, Leiden University,
P.O. Box 9515, 2300 RA Leiden, The Netherlands
E-mail address: M.A.C.de.Vaan@let.leidenuniv.nl

18 December 2007
 Available online 2 April 2008