Lehmann, Winfred P.: Theoretical Bases of Indo-European Linguistics. London/New York, Routledge, 1993 (geb.)/1996 (brosch.), gr.-8°, xii, 324 S. Geb. 50 £, brosch. 17,99 £.

The book consists of twelve chapters of some 25 pages. It follows the history of Indo-European linguistics and stresses the fundamental concepts. The first four chapters present the history up to recent times (although the titles do not indicate that: "Aims", "Methods" etc.). Such concepts as the comparative method, the rise of phonology etc. are reviewed. Ch. 5–10 discuss the recent developments, phonology and syntax each having a chapter for PIE and one for Pre-IE, while morphology has a chapter for the noun and one for the verb. Then follow ch. 11 on the lexicon and 12 on "The community of IE speakers". The book has a very good subject index, and an extensive bibliography which also gives many older publications, but it is not representative for more recent times (besides, most books are not discussed in the text). Regrettably, the table of contents gives only the names of the chapters, not a complete list of the sections: without such a list one cannot see what a book really consists of and find things again.

This organisation implies that every problem is discussed up to a certain point, and then taken up again in a following chapter. An organisation per problem would have been much easier for the reader. The book results from a series of lectures, which is probably another cause of large-scale overlapping, which is very unpleasant for the reader. Some sections recur almost verbatim in a later chapter. – The concentration on the

theoretical preconceptions implies that the book does not give an in any way complete history of IE studies. Thus there is not a word on the discovery of the nominal inflexion types. – We are reminded that there is no serious history of IE studies, at least not of later developments. L. could well have made one: an attractive aspect of the book is the author's wide knowledge of all phases of IE linguistics.

The book has been written by a scholar who knows all the material. The result is that only few examples are given, and that discussions are very short: if you do not know the point already, you cannot learn it from this book. Thus I wonder for whom this book is meant. You must know quite something about IE, but if you do, the book does not offer much that is new. The interesting thing is rather the view of the author, an eminent linguist who is extremely well-informed. The basic idea, a discussion of the theoretical conceptions, is a good idea, but in fact the book gives only occasionally an interesting insight, provided that you already know how the comparative method works, have an idea of the discovery of the laryngeals, etc. The organisation in detail also surprises me from time to time. E. g. 5.6 "Procedures implied in determining the segmental system of PIE" starts with a discussion of the Indo-Hittite problem; in 6.1.1 on the phonology of P r e -IE we are told that Sanskrit must have had **e* and **o*; in ch. 11, on the lexicon, the meaning of the injunctive is discussed (11.4); etc.

As to theory, L. often stresses the necessity of philological study of the details, of the factual material, and that theory is rapidly superseded. He has great admiration for our predecessors, like Brugmann and Delbrück (to which I can only subscribe). He calls the Neogrammarians "probably the most important group of linguists that has appeared in the study of language" (p. 5). Time and again he states that Indo-Europeanists can be proud of the high standards and achievements of their discipline (as opposed to much recent work in theoretical linguistics; he says e. g. that "the generative-transformational revolution is now being unmasked as increasingly vacuous", p. 262); this alone is already sufficient reason to read the book.

The author stresses the need for a theory notably when he asks for an e x p l a n a t o r y syntax. Of course, everybody would like to have such a theory (which is why so many are attracted to general theories), but the point is where to find it. L. thinks that Greenberg's typology is such a theory. We first note that this is a t y p o l o g i c a l insight, based on observing facts in a large number of languages. Secondly, that it is still difficult to establish what the relevance is for IE (see below). And thirdly, that this insight regards only one aspect of syntax, the importance of which must not be overestimated, if only for the fact that nearly 50% of all languages have an SOV structure, which leaves the enormous differences between these languages unexplained. The conclusion must be (see below)

that we must use typological insights, in order to explore possible solutions, but that we must be very careful in basing conclusions on them. Only in one place (p. 40) L. reproaches Indo-Europeanists for rejecting typological considerations. He states that they are not prepared to accept conclusions based on exceptions, what L. often calls 'residues'. I do not think that this is the essential point. IE linguistics has always valued apparent exceptions as possible archaisms. I have often wondered why some solutions are accepted immediately while others take decennia to be accepted. This holds for the laryngeal theory, and now again for the glottalics. I think that the point is: the acceptance of unusual sounds (many Indo-Europeanists having not enough experience with non-IE, and spoken, languages); in general unusual, i.e. in IE further unknown phenomena; and the fact that new theories often presuppose other theories and contradict earlier ones: in the case of the glottalics e.g. the most important aspect is their influence on Balto-Slavic accentuation, where older interpretations of the history of the accentuation cannot be reconciled with the new theory.

The most interesting part of the book concerns syntax. Ch. 9 is devoted to the supposed OV structure of PIE (basic principle: modifier precedes head). This chapter is of course parallel to L.'s book, PIE Syntax, 1974. Here some statements are doubtful. Thus, when μῆνιν ἄειδε is cited as evidence for verb final sentence structure, it may be objected that μῆνιν was fronted and relates to pragmatics: it is the central theme of the Iliad. When it is stated that relative clauses precede the main clause, no mention is made of Sturtevant's view that there was a distribution between preceding and following relative sentences. This is now confirmed by Hettrich's study (Untersuchungen zur Hypotaxe im Vedischen, 1988). L. relies most of all on Hittite. It is said that OV languages do not have reflexive pronouns (p. 206f.), but **se*, adj. **suos*, can be safely reconstructed for PIE. That the negation, and the augment, precede the verb contradicts OV. When ἕνεκα is cited as an example of a postposition, this is only half the truth: it is used both before and after its head in classical Greek, and in Mycenaean, where it occurs five times, it always precedes the head. (Its etymology is unknown). This teaches us to be careful with isolated facts. Even more important is the question of the Hittite postpositions. Since Starke we know that Old Hittite had both prepositions and postpositions, and that these words can better be described as adverbs. This is, of course, the situation in Greek and Sanskrit. There is now the fine study by Silvia Luraghi (Old Hittite sentence structure, 1990), which shows that Hittite became much stronger OV in the course of time, and that Old Hittite is much closer to Greek and Sanskrit. L. relies heavily on Hittite as it was known twenty years ago. This warns us against relying too much on Hittite: there is always the possibility that Hittite has changed. So the postpositions cannot be used as evidence for OV. Still, PIE had, I think,

several clear OV-rules: the position of the adjective and the attributive genitive before their head (cf. **demspotis*, *sűre duhitá*, Διόσκουροι), the structure of compounds, the comparative following the standard (μέλιτος γλυκίων), 'titles' following (Ζεῦ πάτερ, *Iuppiter*), and probably the final position of the verb itself. And in several instances we can see that the OV construction was the older one, being replaced by a VO construction. But word order was highly pragmatic, which was facilitated by the autonomy of the inflected forms. – Returning to the subject of the book, it is clear that typological findings may be important, but the detailed study of the evidence always has priority. In this case, we were guided, but partly misled, by Hittite.

In ch. 10 L. defends the view that PIE was an active language, as proposed by Gamkrelidze-Ivanov in 1984 (= transl. 1995, Indo-European and the Indo-Europeans). L. himself published his view in 1995 in a short monograph (Residues of Pre-Indo-European Active Structure and their Implications for the Relationship among the Dialects, Innsbruck, IBS). This theory implies that both the verb and the noun distinguished between active and stative forms. Now the stative of PIE is a central issue in recent research. (It is a pity that L. does not discuss recent views on this point.) But the theory would have further implications, and the question is, whether these can be demonstrated for PIE. Thus, the fact that the nominative and the accusative neuter are identical and (partly) coincide with the accusative masculine, can be accounted for by most ergative languages. For the distinction between active and stative in the noun, only the two words for 'fire' and 'water' are given over and over again, but nothing more. And since this has been noted for a very long time, it is improbable that more words will be found. The presence of particles is stressed, but how much of this is of PIE date is uncertain. It is true that there was no verb for 'to have', but how important is this? L. argues that the plural is not well developed in PIE. This seems incorrect to me. The *bh-/m-* cases have a simple solution. The so-called external sandhi as in Skt. *mánobhis* does not prove late contraction with a postposition, but is due to restructuring of typical Indo-Iranian developments that gave difficulties. Greek -φι cannot be used to demonstrate that it had various functions: Mycenaean shows that it was an instrumental plural ending (not in the *o*-stems), just as in Sanskrit. There can be no doubt about the endings **-bhi* and **-su*. Nor is there reason to consider the plural endings of the verb as recent. E.g. the 2nd pl. has **-th₁e*, **-te*, **-dhue* and **-e*, which shows that there was a well developed system of endings. L. denies that *-r* is the 3rd pl. ending of the perfect, but there is no reason to do so; it is the ending of both the perfect and the middle systems. Thus not much remains from the evidence presented. However, it is possible that further research in this direction may yield new insights. – L. repeats his theory that the

noun originally had only four markers, *-s*, *-h* (i.e. h_2), *-m* or zero. But there is no adstruction in this book.

In the earlier chapters, too, there are several points with which I disagree. I shall mention some of them. – It is denied that PIE had comparative forms (7.6.1; 10.7.3). However, the comparative with *-ies-* can be reconstructed with certainty. The forms Skt. *-tara-*, *-tama-* do not invalidate that: the rise of these forms is well explained and their meaning is known. – The discussion of the thematic inflexion is not very clear (8.4.1), but the essential fact, viz. that they have their own endings, is not mentioned. – As to laryngeals, it is incorrect that *VH* became a long vowel already in PIE (as shown e.g. by the Balto-Slavic acute, p. 62, 110). – There is no indication at all that the lengthened grade arose upon loss of a following vowel (6.5.2.). That it is due to assimilation of a final *-s* (or even a laryngeal) is also incorrect in my opinion. – I agree with the view that long *i*, *u* derive from the short vowel + laryngeal (106, 109, 111, 139). Many scholars disagree with this view. It seems that these long vowels date from the time when one assumed, *a*, *e*, *i*, *o*, *u* and their long equivalents for PIE. We now know that *i*, *u* had an essentially different position in the phonemic system from *e*, *o*. They were vocalic allophones of consonantal phonemes. There was no ablaut series of *i*, *u*, as for *e*, *o*, so no lengthened grade is to be expected. – I do not think that the laryngeal theory is of any help for the theory that the neuter plural was originally a collective (74). – We must now stop saying that there is no agreement on the laryngeals (110); there is agreement on the main points, only some details are not yet solved, as is so often the case. – It is said that the arguments for the glottalics are purely typological (98). This is not true; most important is the acute accentuation they cause in Balto-Slavic; for several other possible direct reflexes see Kortlandt, Folia linguistica historica 6/2 (1985). – L. relies heavily on Hittite and its assumed closer relationship with Germanic (19, 68 ff., 88). It is now generally recognized, I think, that this is a phantom.

Ch. 12 discusses the society ("community") of the PIE speakers. L. stresses that it must have been a very simple society, and e.g. objects to the notion 'king'. The only remarkable point is that L. suggests that PIE did not have numerals beyond 'five', at most 'ten'. This is certainly false. 'Twenty' and the tens can be safely reconstructed. That 'hundred' indeed had this meaning is neither invalidated by its meaning '120' in Germanic nor by the fact that a Greek hekatombe did not always count a hundred animals. This is forcing the facts according to a preconceived idea. ('Four' was **k^{w}etuores*, not a form cognate with Hitt. *meywes*. Nor can 'nine' be cognate with 'new', as it had an initial laryngeal.)

Thus, there is much that is doubtful in the book, and it should have

been better organised. Still, it is an interesting overview by a reputed scholar who has a profound knowledge of the history of the field.

Rijksuniversiteit Leiden
Faculteit der Letteren
Vakgroep vergelijkende taalwetenschappen
Postbus 9515
NL – 2300 RA Leiden

Robert S. P. Beekes