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Early and Late Indo-European from Bopp to Brugmann

1. If we want to speak meaningfully about Early and Late Indo-European, we must accept three assumptions:
 - a) it is possible to reconstruct a non-attested protolanguage (such as Indo-European),
 - b) like all languages protolanguages have a history,
 - c) we can reconstruct the history of a protolanguage — within limits.

In the history of our subject we cannot take any of these assumptions for granted. I start with a scholar who thought that they should all be upheld.

In the preface to the *Origines de la formation des noms en indo-européen* Benveniste (1935, 1-2) refers to a long period of sixty years during which comparative grammar had two main purposes: to establish correspondences between the Indo-European languages and to explain the development of the attested dialects starting from the state defined by those correspondences: "On met donc au compte de l'indo-européen tout ce qui semble hérité dans chacun des dialectes, avec la conviction, implicite ou avouée, qu'on ne saurait sans danger pousser la restitution au delà du prototype immédiat." He rejects this limited view and pleads for a different attitude: "La tâche la plus urgente est donc de restaurer la notion d'indo-européen, en l'arrachant à cette conception empirique et au fond négative: est indo-européen tout et cela seulement qui, postulé par la comparaison, ne résulte pas d'une innovation. ... l'indo-européen sera considéré, non comme un repertoire de symboles immuables, mais comme une langue en devenir, offrant dans ses formes la même diversité d'origine et de date qu'une langue historique, et permettant à son tour, quoique restituée, une analyse génétique". In France at least these words created some stir, as evidenced e. g. by the preface written in 1939 by Père Mariès (1946, 14 f.), an Armenologist and a pupil and friend of Meillet, to one of Cuny's books about Hamito-Semitic and Indo-European (Cuny 1946). As he points out (*ibid.*, 15): "Ce saut dans la pré-histoire faisait peur à Meillet". Mariès implicitly recognized the need to move beyond the flat reconstructions generally accepted and mentioned two possible avenues: on the one hand further comparison (with Tocharian-Hittite first, with Hamito-Semitic second), on the other hand the road hinted at by Meillet himself ([1934] 1937, 151) in the seventh edition of the *Introduction* when he visualized for Indo-European a pre-inflectional stage and wished for a theory of the root. Yet Mariès pointed

out that when confronted with e. g. Kuryłowicz's *Études indo-européennes* (1935), Meillet's judgement had been negative: "On conçoit bien qu'il y aurait intérêt à établir comment s'est formé le système indo-européen, mais la méthode par laquelle on établirait cette formation ne ressort pas de ces 'Études'." (Meillet 1935, 22). In other words Meillet was doubtful about the possibility of going beyond that form of reconstruction which was based on the comparative method. If so, one sees how Benveniste's first sentence may be seen as an act of defiance towards the very same "cher maître" to whom the *Origines* are dedicated.

2. But within this context what was Benveniste thinking of when he referred to the last "soixantaine d'années"? Counting back from 1935 we reach the 1870's, and the obvious implication is that Benveniste saw the neogrammarian dominated linguistics of the seventies and eighties as essentially hostile to his aims. The question then arises — a question which we shall have to tackle — whether he thought that the previous period was differently inclined. Meanwhile, however, let us remember that Benveniste himself had referred to one of the events happened some sixty years earlier, if this figure is liberally interpreted: the publication of Saussure's *Mémoire sur le système des voyelles dans les langues indo-européennes* (1879), after which, again in Benveniste's words, "le problème de la structure des formes indo-européennes elles-mêmes a été presque complètement négligé" (ibid., 1). Benveniste, in his revolutionary mode looked for support to the man who, as he had learned from his teacher, had been the major influence on the French school of linguistics (Meillet [1930] 1936, 178; [1930] 1936, 222).

What Benveniste does not tell us is whether in his view Saussure was a lonely pioneer or had had any predecessors. And yet before one embarks onto a dangerous road some reconnaissance is necessary. It would be foolish nowadays to discuss Indo-Hittite without realizing that one must take a firm position vis-à-vis the arguments used by Sturtevant in the 1930's or those adduced more recently by Warren Cowgill. If we want to side with Benveniste and see Indo-European not only as a developing language, which no one would deny, but also as a language whose development we can reconstruct, or if we want to go even further and think in terms of Early, Middle and Late Indo-European, we ought to understand whether this is an entirely new venture or we are following in the steps of our predecessors, albeit with different methodology and data. Put it more bluntly, we must understand what we are doing and why we are doing it; to compare (or confront) similar attempts in the past may help.

3. The first difficulty is a historiographical problem. At what period should we look in our search for a 'layered' view of Indo-European as a developing language?

Benveniste denied any input from the neogrammarians and their successors in the decades which divide Saussure's *Mémoire* and his own *Origines*. We shall have to consider if, as far as the neogrammarians are concerned, this is an oversimplification or not.

For the earlier period Benveniste expressed no opinion, but if we accept the standard historiographical view which is now current, not only there was not at the time any chronologically layered view of Indo-European, but also the contemporary theories would not have permitted such a view. It is generally assumed that reconstruction is a technique or algorithm, or even a requirement, of the comparative method that begins to have an impact in the 1850's with August Schleicher. Before that it is assumed that linguists were concerned with demonstrating, a) that the Indo-European languages were related, b) that it was possible to distinguish in the individual languages between innovations and 'organic' features, i. e. features which were inherited and, to use the jargon of the time, belonged to the original organism. If so, it is difficult to see how the concept of an Indo-European layered into Early, Middle and Late could have arisen in the earlier period. Bopp's *Vergleichende Grammatik* (1833-1852) does not provide, as Schleicher's *Compendium* (1861-1862) will do, a list of Indo-European phonemes, morphemes etc. We must then look at the decades between 1850 and the late 1870's, the period referred to by Benveniste. Here too we come across a problem. For Schleicher the onset of change in Indo-European coincides with the beginning of the division. In other words languages are seen as undergoing a period of growth during which the normal features of change (sound change, "false analogy") do not operate; when the language is in full possess of its means of expression then decay sets in; sound change destroys the beautiful organism, 'unorganic' innovations occur and language split follows. The implications of this view are that, a) reconstruction naturally takes us back to the period which immediately precedes the split, that is to say to the period of maximum growth, b) our normal techniques of reconstruction, based as they are on our experience of change, i. e. on an uniformitarian view, cannot provide information about the period of growth since that is determined by different processes from those which we are acquainted with. Once again, if this set of assumptions is adhered to, it becomes impossible to think profitably in terms of different stages of Indo-European. Schleicher is absolutely clear about this point. In the very first pages of the *Compendium* ([1861-1862] 1871, 12 f.) he quotes "ai. *áçmā*, gr. *κοιμήν*, lit. *akmŭ*, lat. *homō*, got. *guma* d. i. **gumā*" to

suggest that we may want to reconstruct a final $*-\bar{V}n$, but he argues instead that the long vowel is more likely to have arisen independently in a number of languages after the separation and concludes that we ought to reconstruct e. g. *akmans* for Indo-European: "Wir können auch hier in der ursprache nur die echten grundformen voraus setzen". There is no suggestion that his reconstructed **akmans* may have changed to **akmān* within Indo-European. In other words if we reconstruct within the family tree model of the Indo-European languages we start from the higher branches and get down to where the first branching occurs but no further. Hoenigswald (1975) has drawn attention to the fact that Schleicher draws his tree with a trunk and does not think of a branching of the type $A \rightarrow B, C$ but rather of a splitting-off of the type $A \rightarrow A', B$. Schleicher, in Hoenigswald's words, "is also sure that Slavo-Germanic separated 'from' Ario-Greco-Italo-Celtic rather than either the reverse or than (as we would say) that the two separated from each other" (ibid., 158). If so, it should be possible to reconstruct different parts of the central trunk, but Schleicher here is, in my view, inconsistent with his own model.

We may summarize: Benveniste maintains that after Saussure Indo-European was conceived as static. Before Saussure we assume that Bopp was not interested in reconstruction and consequently could not be interested in different Indo-European phases; as for Schleicher himself his assumptions seem to debar him from reconstructing change within Indo-European itself. There remains Saussure, but what Benveniste attributes to him is not the idea of a developing Indo-European, but rather a study of the structure of Indo-European forms.

If what precedes is correct, then the idea of a development within Indo-European is new with Benveniste or his contemporaries. And yet, historiographically, this seems implausible: the great period of Indo-European reconstruction is from the 1860's to the 1920's and this is also the period in which the supporters of the most disparate and contradictory views, from Schleicher to Whitney, agree in the assumption that all linguistic explanation is historical explanation. Nevertheless Benveniste (1935,1) complains: "Il paraît communément reçu qu'on peut analyser l'évolution de l'indo-européen sans se soucier de ses origines, qu'on peut comprendre des résultats sans pousser jusqu'aux principes". Is it plausible that the parent language had never been envisaged as a developing language? Obviously some more probing is required.

4. We may begin with Bopp. Admittedly the *Vergleichende Grammatik* (1833-1852) does not start, as Schleicher's *Compendium*, with a reconstruction of Indo-European phonology and morphology, but in fact the work is a work of reconstruction; the whole book is an attempt at ex-

plaining what forms in the individual languages count as innovations and what forms are inherited. Ascoli (1877, 9), who was closer to it than we are, knew it very well: "La ricostruzione è naturalmente implicita in ogni comparazione che proceda con rigorosi criterj. Se il Bopp, nel ricombinare le forme e gli elementi delle varie favelle della famiglia, mostrava di continuo come ora all'una, ora all'altra spetti il vanto di meglio rappresentarci le condizioni genuine o originali, e per tal modo portava tutte codeste lingue come a reintegrarsi mutuamente fra di loro, egli in effetto istituiva una ripristinazione continua, sebbene non attendesse a darci concreta, in una nuova parola, la resultanza di cotali operazioni". In other words Bopp's *Grammatik* is dedicated to the reconstruction of Indo-European morphology. But Ascoli continued saying that from the first period of comparative grammar some form of phonological reconstruction was practised; given Skt. *ajant-*, Gr. *ἀγοντ-*, Lat. *agent-* it was normal to say that Sanskrit had the most 'genuine' form of vowel while the European languages showed a 'guttural' that was closer to the original consonant than the palatal of Sanskrit. In other words, in Ascoli's view, morphological reconstruction and some form of phonological reconstruction existed from an early stage. Indeed, even if Bopp never attempted a systematic reconstruction of Indo-European phonology, it would be misleading to assume that no form of phonological reconstruction is found before Schleicher. On the contrary, starting with Rask's *Undersøgelse* (1818, 167 f.) and, for that matter, earlier, we begin to find protoforms like Gr. *ο-δοντ-ς* for *ὀδούς* or Lat. *dent-s* for *dens* or even *ἄλλοδ* for *ἄλλο*. Similarly Bopp (1833-1852, i, 171) did not hesitate to reconstruct *dusmenes-os* in order to account for Greek *δυσμενοῦς*. In his first book (Bopp 1816, 17) he had argued that the third plural of the Skt. first person preterite (in fact imperfect) attested as *ādan* had replaced an original *ādant*. The reconstruction was based on the present third person plural *adanti* and was prompted by the desire to treat *-an* as an ending of third person plural, which required, in Bopp's view, a pronominal stem to mark the third person.

In view of our reference to Schleicher's treatment of it, it is worthwhile to turn to Bopp's statements about the nominative singular of the athematic *n*-stems. Differently from Schleicher Bopp (1833-52, i, 166) was not interested in the length of the vowel, which he took as Indo-European, but in the final consonant. The problem is that forms like Skt. *rājā*, Lat. *homō*, etc., have no final *-n* while Gr. *εὐδαίμων* does. Bopp takes for granted that there was an original *-n* (since this appears in all other case forms of the words in question) and admits that it may have been independently lost in a number of languages. Yet his preferred solution is that the agreement of so many languages "deutet auf Ver-

wandtschaft oder auf das hohe Alter eines solchen Verlusts, und versetzt in vorliegendem Falle die Ablegung eines stammhaften *n*, im Nominativ in die Zeit vor der Sprachwanderung, und in den Raum des Ursitzes der später getrennten Volksstämme". As for Greek, one could ask whether the final *-n* was a survival of the original form or whether the *n*-stems on the analogy of other consonantal stems or their own oblique cases had returned to the original form after having lost the consonant; Bopp favoured this second hypothesis. In other words Bopp here proposes an Indo-European reconstruction with final *-n* in the nominative, which is then lost, and is later restored in Greek. The technique of reconstruction is no different from that of Schleicher, except for the fact that Bopp is more flexible and less obsessed by the desire to reconstruct perfect forms for the *Ursprache*.

5. Enough has been said to show that Bopp did not fight shy of reconstruction, but this does not mean that he saw Indo-European as subject to development or had a concept of early and late Indo-European. Or did he? Let us reconsider the example which we have just discussed. The various Indo-European languages have a nom. sing. of the *n*-stems which end in a long vowel or more rarely in a nasal. Bopp concluded from comparison that the Indo-European forms ended in a long vowel and that the final nasal of Greek was an innovation. Nevertheless he also assumed that Indo-European originally had a form with a final nasal and that this had been lost in Indo-European itself. In more modern notation we could state that Proto-Indo-European **-ōn* > IE **-ō* → Greek *-ōn*. In other words Bopp operated in terms of two different stages of Indo-European. If we ask how he did it, this example provides a clear answer. First, comparison leads to uncertainty between two possible reconstructions **-ōn* and **-ō*. Secondly, a choice is made: both the supposedly oldest language (Sanskrit) and the majority of other Indo-European languages favour the ending without a nasal. Thirdly, it is recognized that an ending **-ō* in the parent language is structurally out of place (as it is in Greek); we expect the nominative to keep the final consonant of the stem. Fourthly, the difficulty is solved postulating that the earliest stages of the parent language had a final nasal in the nominative but this was lost. At a different level a similar reconstruction procedure is followed for the personal endings, though here a question of interpretation arises. Comparison of the middle endings Skt. *-e*, *-se*, *-te* with Greek *-μαι*, *-σαι*, *-ται* leads to the conclusion that Greek is closer to the original forms. For Bopp (1833-1852, i, 672 ff., 681 f.) Skt. *-e* cannot be original because it breaks the structural parallelism with the active endings: *-e* vs. *-mi* contrasts with *-se* vs. *-si*, *-te* vs. *-ti*. Assuming then that the Indo-European

middle endings can be reconstructed on the basis of Greek, the next task is to explain their forms. According to Bopp most personal endings are derived from pronouns and here too *-ma* (in *-mai*) is recognizable as the first person pronoun (which also accounts for the active *-mi* ending). To explain the meaning of the middle endings Bopp postulates original forms such as *-mami*, *-sasi*, *-tati* with a double pronominal form which accounts for the middle, reflexive value of the ending; as he puts it, his reconstructed "*dat-sa(s)i*, *δίδο-σα(σ)ι*" meant "give to thee thou" or "give thou to thee". The actual forms *-mai*, *-sai*, *-tai* are due to the loss of the internal consonant (apparently an Indo-European loss).¹

We may take a third and final example again from the personal endings. Bopp (*ibid.*, 633 ff.) contrasts the reconstructed primary ending of first person sing. *-mi*, which he sees as due to a weakening of the *ma* first person pronoun, with the secondary ending *-m*, which in his view is due to a further weakening of *-mi*. Of *-m* we are told that it is "uralt", as emerges from the agreement of Sanskrit with the European sister languages, but nevertheless cannot belong to that time "when the linguistic organism blossomed in full health in all its parts", since it is difficult to believe that the language in the period of its youth would tolerate two sets of endings with the same function. The point is that clearly Bopp assumes that both sets of endings are not creations of the individual languages but developed (he explains the conditions) in the parent language.² If so, we must assume that he thought in terms of three different stages:

1. Indo-European has only one ending of first person singular, *-ma*;
2. In Indo-European *-ma* is weakened to *-mi*;
3. In Indo-European in specific environments *-mi* changes into *-m* but both endings survive side by side.

Before we abandon Bopp we must reconsider his *modus operandi*. There is little doubt that his work includes instances of reconstructed forms and that his reconstructions lead him to assumptions about early

¹ Later on (*ibid.*, 685) Bopp proposes an alternative explanation for the middle endings, according to which they would be formed from a personal pronoun + the reflexive pronoun (Skt. *sva-*, etc.); if so *-mai* and *-tai* would come from *-masi*, *-tasi* or *-masvi*, *-tasvi* and *-sai* would come from *-sasi* where, however, *-si* would be the reflexive rather than the second person pronoun. The explanation of the middle endings as originating from a combination of two pronouns goes back to Adalbert Kuhn: cf. the bibliographical note in Schleicher (1871, 670 f.).

² He tentatively suggests (*loc. cit.*) that the shorter endings could have arisen in those verbal forms where there was some addition to the stem, i. e. augmented forms or suffixed forms like those of the optative.

and late developments in the parent language. But how does he reach these conclusions? Our first example gives us an admirable instance of the interplay of the comparative method (which speaks for a nominative singular which ends in a vowel) and internal reconstruction (which speaks for a nominative singular which ends in a vowel + a nasal). Internal reconstruction does of course account for some of Rask's reconstructed forms (see above sect. 4.) and for Bopp's reconstruction of *ādant* in 1816. It is more difficult to assess Bopp's conclusions about the personal endings but there too after the (correct) application of the comparative method we find a mixture of internal reconstruction and etymological or glottogonic speculation. The latter is discussed but no attempt is ever made to justify the principle of internal reconstruction. We are given a salutary reminder that at the beginning of the nineteenth century internal reconstruction is trivial and the comparative method is the novelty.

6. The highways and byways of comparative linguistics in the early nineteenth century cover a much wider ground than is normally assumed. A serious study of reconstruction techniques at the very least ought to look carefully on the one hand at the reconstruction of vocabulary (meant to identify Indo-European realia), on the other hand to the etymological work. We find the former in e. g. Eichhoff's 1836 book which preludes to the later paleontology by Pictet (1859-1863),³ for the latter we must at least mention Pott's magnum opus (1833-1836), where the roots are given in their Sanskrit form but it is clearly implied that this is a conventional decision and what Pott has in mind are reconstructed Indo-European roots. In his important review of Pott's book Theodor Benfey ([1837] 1890, ii, 5 ff.) pleaded for real phonological reconstructions.

³ In this connection one should remember the work by Adalbert Kuhn about Indo-European religion, formulae etc. which starts in the 1840's. Isolated from the mainstream is a curious work by H.-J. Chavée entitled *Lexiologie indo-européenne ou Essai sur la science des mots* (1849) which, without much knowledge of the current development of these studies, aims at reconstructing the basic verbal roots of Indo-European in order to identify the conceptual characteristics of the race. Chavée (1815-1877), a Belgian who lived in Paris, may count as the founder of that French school of linguistics which from 1867 concentrated round the *Revue de linguistique et de philologie comparée* and adopted a strong positivistic and Schleicher-like attitude, in contrast with the followers of Michel Bréal and Gaston Paris who joined forces in the *Société de Linguistique de Paris*. Given the school allegiance it is not surprising, though disturbing, that Abel Hovelacque, one of the school leaders, attributed jointly to Chavée and Schleicher "la première mise en réalisation de cette conception féconde d'une forme commune primitive des langues indo-européennes" (Hovelacque [1876] 1922, 259).

We do not need to go further; for our purposes it is enough to have shown, against the communis opinio, that even in Bopp we find some references to different stages of Indo-European. We may now move forward to the 1860's and the early 1870's. According to conventional wisdom this is the period of reconstruction as evidenced in Schleicher's *Compendium* (1861-1862), in Curtius' *Grundzüge der Griechischen Etymologie* (1858-1862) etc., but is also the period of 'growth and decay', i. e. the period in which linguists believe that to study language change means to study the decay of language and that decay occurs in the individual languages and not in the parent language. We have already seen the contrast between Bopp's and Schleicher's reconstructions of the Nom. sg. of *n*-stems. Bopp operates in terms of an early *-ān* followed by *-ā* in the *Ursprache*. Schleicher ([1861-62] 1871, 12 ff., 510 f.) reconstructs *-ans* and is then obliged to attribute to each language a series of rules which delete the *-s* or the *-ns* or the *-n*. For Greek he notes that e. g. τέκτων must derive from *τεκτων-ς but honestly points out, without further comment, that *μελων-ς gives μέλας (ibid., 511). So far, however, the policy is a consistent one. The question becomes more difficult with the personal endings. For the first person sing. Schleicher (ibid., 647) accepts in full Bopp's conclusion. The original form is *-ma*, then weakened to *-mi* and eventually to *-m*, all this in the parent language; in the perfect moreover *-ma* becomes *-a* 'villeicht schon vor der sprachtrennung'. For the middle endings the *Compendium* (ibid., 670 ff.) follows Kuhn and Bopp and reconstructs **-mami*, **-sasi*, **-tati*, which already in the *Ursprache* change to *-mai*, *-sai*, *-tai* with loss of the second consonant and/or weaken to *-ma-m*, *-sa(s)*, *-tat*, which eventually yield the attested secondary endings. Here then the original principle which required for the *Ursprache* only "die echten grundformen" (ibid., 13) seems to be abandoned. In other words Schleicher too, like Bopp, does not hesitate too much before attributing to the *Ursprache* both the original formations (which are structurally understandable) and the immediate antecedents of the forms of the attested languages. Theory and practice do not seem to move in unison.

7. We may at this stage turn to Schleicher's contemporary, Georg Curtius (1820-1885), the man who convinced classicists of the value of comparative work and at the same time initiated that Leipzig school of Indo-European studies which dominated the last part of last century and the first two decades of this century. We shall keep in mind his two large monographs *Grundzüge der griechischen Etymologie* (1858-1862; 5th ed. 1879) and *Das Verbum der griechischen Sprache* (1873-1876; 2nd ed. 1877-80), but our main text must be his 1867 essay *Zur Chronologie der*

indogermanischen Sprachforschung. This is a sort of hymn to the results of comparative grammar, but above all to the difference that it makes to introduce a chronology of development in the study of language. Classicists noticed long since the coexistence of two forms $\delta\varsigma$ and $\sigma\delta\varsigma$ for pig but could not go any further; we now know that, Curtius observes, $\sigma\delta\varsigma$ is the original form. They also noticed three types of aorist in Greek; we now know that the sigmatic aorist is more recent. Curtius' point is that the greater awareness of chronological layering that we have acquired also helps us to understand the processes of language formation. The fixed point is the distinction between the language (Indo-European) spoken before the separation of the tribes and the various languages which emerged after the split. However, to make sense of the language before the division we must allow for a long period of 'organisation', in the course of which we are obliged to recognize different layers. Bopp had demonstrated that the pronominal stems provide most of the inflectional material, i. e. of the nominal and verbal endings. But if we find the same pronoun used with different values we cannot assume that it was assigned these disparate functions at the same time. Rather we are obliged to attribute the two functions at different stages. Consider, for instance, the identity or similarity of the markers of nom. sing. and gen. sing. (in Greek $\acute{\omicron}\delta\acute{\omicron}\varsigma$ vs. $\pi\omicron\delta\acute{\omicron}\varsigma$), both of which Curtius (1867, 193), in agreement with Bopp, derives from the pronoun *sa*. If we can hypothesize that the nom. sg. was created at one stage and the gen. sing. at another all becomes clear. Take also two forms like the reconstructed pre-reduplication *dā-ta* 'gives' (Skt. *da-dā-ti*, Gr. $\delta\acute{\iota}\text{-}\delta\omega\text{-}\tau\iota$, etc.) and *dā-ta-* 'given' (Skt. *dā-ta-s*, Gr. $\delta\omicron\text{-}\tau\acute{\omicron}\text{-}\varsigma$); in both forms Curtius (1867, 211 f.) recognizes original compounds of *ta* 'this, he', but he argues that in the first instance the pronoun fulfils a predicative function: {give he} → {he (is) the giver} → {he gives}, while in the second it is attributive: {give there} → {the given there} → {given}. The two formations reflect different approaches which make sense only if they are chronologically differentiated.

This type of argument allows Curtius to identify not three but seven different phases in the history of Indo-European "organization": the period of root formation, largely shrouded in mystery, is followed by a longer stretch of time in which at different stages determinatives, primary verbal forms, stems, compound verbs, cases and adverbs were formed. The hypothetic nature of these conclusions is obvious to their author too but he puts up a spirited defense:

Solche Hypothesen sind aber für unsere Wissenschaft schlechterdings unentbehrlich. Nachdem mehr als fünfzig Jahre lang die Ana-

lyse der einzelnen Formen eifrig betrieben ist, wird es unerlässlich den Versuch zu machen diese nicht bloss für die historisch bezeugten Zeiten des Sprachlebens zusammenzufassen und zusammenzudenken, sondern auch ein gleiches in Bezug auf jene Urzeit zu wagen. (Curtius 1867, 200 f.)

To understand Curtius' motivation we must remember that the 1850's and the following decades are the golden period of the so-called morphological classification of languages. In its standard form this divided all languages of the world into three types: isolating (like Chinese), agglutinative (like Turkish), and inflectional (like Indo-European and perhaps Semitic). The classification goes back to August Wilhelm Schlegel, even if sometimes it is wrongly attributed to Humboldt or to Pott. The novelty in our period is the transformationism adopted by Schleicher and Max Müller, very much in contrast with the original assumptions of e. g. Friedrich Schlegel. In their view all languages had started as isolating; some then developed into agglutinative and some went further and became inflectional (Morphurgo Davies 1975). For Indo-European the communis opinio followed Bopp: inflections had started through agglutination. If so, there was no reason not to move one step further and think in terms of an even earlier isolating stage, the period of roots. Curtius' seven phases are simply an attempt at defining further the general process of language development with special reference to Indo-European, the best known and studied of the world languages. As he points out, there are other reasons for envisaging a long period of Indo-European unity: we need a long lapse of time to account for the creation of myths and, if we believe in Westphal's theories, that of metres.

Beyond the technicalities there are at least two new (and general) points in Curtius' approach. First the insistence on chronology which introduces a new element of rigour even in the most glottogonic reconstructions. Secondly, the realization that the "normal" types of change, such as sound change, belong to the period before the division of Indo-European and may even belong to the period of organization which precedes the later evolution (*Ausbildung*). For Curtius there are forms which must have belonged to the parent language and yet show signs of change. Thus, if we believe with Bopp that the first pers. plural active ending *-masi* (that Curtius attributes to Indo-European) derives from *-ma-tva* ('I' = *-ma* + 'you' = *-tva* → 'we'), then we must allow for two changes: 1. *-matva* > *-matvi*; 2. *-matvi* > *-masi* (Curtius 1867, 199). Similarly "it will have taken some time to develop from the organic *Grund-*

form *varka-sa* ['wolf?'] the Indo-European *Grundform varka-s*. Perhaps there were intermediate stages, i. e. something like *varka-si*."

From our point of view what matters is that we have here a remarkable breakthrough. Not only do we find in Curtius a developmental view of the growth of Indo-European, but through a curious mixture of glottogonic and inductive reasoning he deliberately breaks the anti-uniformitarian shibboleth of his time: for him sound change can happen both in the period of growth and in that of decay. There is, of course, no substantial difference from the practice of Schleicher's *Compendium*, but the important point is that Curtius gives us a justification of his *modus operandi*.

8. Schleicher's transformationist views of morphological classification and Curtius' seven phases of Indo-European did not survive the onslaught of two new principles, both associated with the *Junggrammatische Richtung* (though not unique to it): the new fully fledged uniformitarianism and the stricter observance of sound laws. The hallmark of the neogrammarians' "revolution" in my view is not, as is sometimes said, technical advancement joined to a narrow mechanical approach, but rather a new deeply felt and earnest concern for an explicit methodology joined to a commitment to the implementation of whatever method had been adopted. That explains why the neogrammarians were associated from the start with the "blind operation of sound-laws" — not because they believed that sound laws played a more important role in language change than other factors (for them analogy was as important and probably more interesting) but because sound laws had — heuristically — to be used as a starting point in the analysis of change.⁴ In the context of this stricter methodology what suffered most were the glottogonic speculations of their predecessors.

When confronted with the problem which later exercised Benveniste (should we see Indo-European as a developing language so that it is comparable to the attested languages?) all too often the neogrammarians adopted a negative or sceptical or agnostic reaction, which involved a rejection of Curtius' conclusions. This is a coherent attitude for two reasons at least. First, if uniformitarianism was to be taken at all seriously, Curtius' half-way position was inadequate. True, if Indo-European was a language like all other languages, it had to undergo sound change — so much Curtius had accepted. But if it did, sound change ought to be com-

⁴ It is impossible to defend this view here or even to refer to the immense amount of literature on the subject; for a good survey cf. Einhauser (1989), for the thesis supported here Morpurgo Davies (1978; 1986) and Graffi (1988).

parable to sound change in real languages, i. e., in the neogrammarians' view, had to obey sound laws. Yet all Curtius' reconstructions of the early changes broke the principle. How is it possible to derive both the primary ending *-mi* and the secondary ending *-m* from an original *-ma*? Or what evidence is there that a supposed first person plural ending like *-matva* would turn into *-matvi* and then into *-masi*? There are instances of final *-a* preserved, which speak against these assumptions.

The second and perhaps even more powerful reason for caution is that in their newly found methodological awareness the neogrammarians started questioning the very status of Indo-European reconstruction. The 1907 article by Eduard Hermann (not a neogrammarian) *Über das Rekonstruieren* collects a list of statements — both by 'official' neogrammarians and by other contemporary scholars — which call in doubt the validity of Indo-European reconstruction as such and often refer to it as a short-hand device to indicate what is actually attested in the individual languages, a sort of mathematical formula, as Friedrich Müller saw it (Hermann 1907, 6). Delbrück put it most strongly in his *Einleitung* (1880, 52 f.):

"... es ergibt sich somit die Folgerung, dass die Ursprache nichts ist als ein formelhafter Ausdruck für die wechselnden Ansichten der Gelehrten über den Umfang und die Beschaffenheit des sprachlichen Materials."

In this view he was not alone: others preceded and followed him. More than twenty years later Meillet ([1903] 1937, viii) reiterated the same point; his *Introduction* was not meant to be a grammar of Indo-European since:

"l'indo-européen est inconnu, et les concordances sont la seule réalité qu'ait à étudier le comparatiste".

Clearly there was a great deal of uncertainty and oscillations about the historical reality of reconstructed forms and indeed about the position taken by individual scholars: Kretschmer (1896, 8 ff.) stated that Schleicher did not believe in the historical reality of his reconstructions while Brugmann did; most people would have read the two authors in the opposite way, but neither reading was entirely unfounded. Hermann rightly recognized that most scholars were prone to internal contradictions and also found that their practice was often at odds with their theory. A serious analysis of both beliefs and practice in this field would take us too far, but two points should at least be mentioned. In a period

in which the studies of dialectology were becoming more and more successful and provided the impetus for a reconsideration of the existing views about language variation and language development it could reasonably be objected to the standard reconstructions of Indo-European that they showed no trace of dialect variation, while no known language had that degree of unity. The second point concerns relative chronology, since paradoxically (given Curtius' views) it is just the greater interest in, and experience of, relative chronology questions which seeded doubts about the possibility of reconstructing various stages of development in Indo-European. Johannes Schmidt made this clear as early as 1872 (p. 31) in his pamphlet *Die Verwandtschaftsverhältnisse der indogermanischen Sprachen*, which formed one of the first attacks against the family tree model of language kinship. As he pointed out, if we try to reconstruct a sentence of the protolanguage we are more than likely to reconstruct forms which are chronologically incompatible, as if someone translated a verse from the Gospels taking a word from Wulfila, one from Tatian and one from Luther. Otto Bremer (1894, 8), in an article dedicated just to relative chronology, went further and argued that the same objection applied to the reconstruction of a word, since even a word was likely to contain chronologically incompatible material. The only thing which it was possible to reconstruct with some confidence were individual sounds. It may seem paradoxical that on the one hand scholars paid increased attention to chronology but on the other they rejected Curtius' attempt to establish a chronological development of Indo-European. Yet there is a rationale behind this move. As we have seen the attempts at identifying old and more recent features in Indo-European were largely based on internal reconstruction, i. e. on the identification of morphemic alternations in the reconstructed language; this is how Bopp came to reconstruct an early IE $\bar{V}n$ nominative which lost the final $-n$ in the parent language. But if doubt is thrown onto the possibility of reconstructing a linguistic system, since the various elements that we reconstruct may never have coexisted, then even the possibility of using morphophonemic alternations for further reconstruction disintegrates.

It is not surprising then that the young Karl Brugman in 1878 found it in himself to rebuke an unhappy scholar called Karl Penka, the author of *Die Nominalflexion der indogermanischen Sprachen*, for having joined the sect of the "Glottogonologen" and advised him to learn some historical facts before proceeding any further. By contrast Brugman (1878, 1040) emphasized that among Indo-Europeanists one party at least was fully aware of the uncertainty of our reconstructions and the impossibility to build anything on them: "Diese zweite Partei sieht nämlich ein, dass heutzutage fast noch alle Vorbedingungen zu einer Analyse der indo-

germanischen 'Grundformen' und einer Darstellung ihrer Entwicklungsgeschichte mangeln; sie weiss, dass verhältnissmässig erst wenige 'Grundformen' mit einiger Sicherheit rekonstruiert sind ...". What was then left? Brugman in that same review indicated that the glottogonic party was happy to go on writing "viel schönes, oft recht Geistvolles" about early developments and managed to fill whole books with that stuff. But what about the others? The others could of course have turned, and some did, to further comparison. In the earlier period von Raumer and then Ascoli, later on Hermann Möller and then Cuny tried to prove that Indo-European was related to Semitic; le père Mariès, from whom I started, saw this as the only way to salvation (in more than one sense). Pedersen went further accepting a link with Ugro-Finnic and the Altaic languages.⁵ Failing that, however, abstinence from "Vor-Indogermanisch" seemed to be called for, unless the newly found languages like Tocharian and Hittite could provide useful evidence, but in fact they were not sufficiently understood until relatively recently.

Meanwhile — and not for the first time — even the methodologically aware neogrammarians made some compromises, in their practice if not in their theory. In an early article Osthoff (1879, 113 ff.) gave a list of Indo-European contractions, i. e. of contractions that had taken place in Indo-European before the separation: thus in Indo-European $\bar{a}e > \bar{a}$, as shown by the nom. plur. of the a -declension for which Osthoff reconstructs an original $-\bar{a}es > -\bar{a}s$. In the first volume of the second edition of the *Grundriss* Brugmann (1897, 27) defined two meanings of "urindogermanisch"; the adjective referred to the period before the separation of the various languages, but could also refer to the fact that the reconstructed forms could sometimes be brought back to something even more original: $*smés$ 'we are' to $*esméś$; the compound $duō-dekəm$ 'duodecim' to a phrase made up of two words, etc. These are not very daring conclusions but the principle of internal reconstruction is called in from the cold. Somewhat earlier (1889; 1891) Brugmann had also proposed a new theory about the origin of the feminine gender, which once again implied going back well beyond the time of our basic reconstructions. It would be interesting to know what Saussure thought that he was

⁵ Pedersen's views about reconstruction (which cannot be identified with those of the neogrammarians) would deserve a special study. Hermann (1907, 5) includes him in the list of those scholars who, in his view, argued inconsistently in favour both of elaborate theories of Indo-European development and of a conventionalist view of the results of reconstruction: "Wenn für Pedersen ebenfalls, die idg. Grundsprache weiter nichts als 'eine Formel ist, der wir zur Erklärung der Einzelsprachen bedürfen', so verstehe ich absolut nicht, wie er selbst dann Ablauttheorien und Hypothesen über die Entstehung der Palatalreihen aufstellen kann ...".

doing in the *Mémoire* (1879). Meillet ([1913-14] 1936, 176), who knew both the author and the book well, described the latter as a book on Indo-European vocalism. Louis Havet (1879) stressed the importance of the discovery of disyllabic roots and used this point to make a dig at those who on the model of Bopp tend "à raisonner sur les 'racines', comme si l'on avait quelque idée de ce qu'elles sont". Möller immediately used the conclusion to support his views about the original kinship of Semitic and Indo-European (Möller 1906, vi). Benveniste, as we have seen, spoke of a book on the structure of Indo-European forms. No doubt the *Mémoire* contained the most daring — and most impressive — use of internal reconstruction, but Saussure too was frightened of going back in time within Indo-European; he wondered whether his Ablaut discoveries could have led him back into "l'époque paléontologique antérieure à la flexion, telle que M. Curtius la reconstruit par la pensée ..." (Saussure 1879, 236), but concluded that he did not know and did not want to know.

9. We may now return to the assumptions mentioned in our first paragraph. Clearly the idea of reconstruction goes back to the first part of the nineteenth century. Schleicher did not discover it; his novelty consisted in insisting on total rather than partial reconstruction — at all levels: phonology, morphology and even syntax (hence the Indo-European fable). Yet the way in which scholars thought of reconstruction and of the reconstructed parent language kept changing through the century. For Schleicher reconstructed Indo-European belongs to the last phase before the separation and is the result of a process of growth, which differs from the normal processes of language change or decay and which we cannot recapture. For Georg Curtius, on the other hand, it is possible to use the standard techniques to go back to, or speculate about, the period of growth. We may wish to compare Benveniste's attempt with that of Curtius but we ought to resist the temptation. For Benveniste, as for every (reputable) scholar who came after the neogrammarians, the development of Indo-European is a continuous process, from the earliest period which we can reach, to the last period in which the protolanguage is treated as unitary, to the history and development of the individual languages. The question is at what stage the earliest reconstructable period must be situated.

For the neogrammarians and most of those who followed them the dividing line between growth and decay disappears, thus opening the way, in theory at least, to a layered view of Indo-European such as that which is discussed nowadays. Yet at the same time a greater methodological sophistication calls in doubt the validity of reconstruction. At the

beginning of the new century Hanns Oertel, the German professor who replaced Whitney at Yale, congratulated himself on having independently reached the same conclusions as Kretschmer (1896) and wrote that the reconstructed forms "are nothing but ideal types, for which no perceptual reality can be claimed. The whole operation of reconstruction of parent forms is a logical, not a historical one; it is in fact, no reconstruction at all, but a construction." (Oertel 1901, 128).

It now becomes clear that, even if the concept of a reconstructed Indo-European goes back to the time of Bopp, each successive generation had different theoretical reasons which made it difficult or impossible to attempt a reconstruction of the development of Indo-European before the separation. If at first the contrast between growth and decay was the real obstacle, later on the denial that the reconstructed structures had historical validity effectively prevented further historical inferences.

Yet this is not the end of the story. It is striking that the very same linguists who were debarred by their general views from making a distinction between early and late Indo-European, in their practical work moved beyond the conception of a static Indo-European and felt free to label one or the other reconstructed form as early or late, i. e. as early Indo-European or late Indo-European. If we ask what criteria they used, it emerges that in each case they relied either on etymological speculation (the verbal endings must arise from pronominal roots, etc.) or on internal reconstruction — though this is not the name they would have used.

It is internal reconstruction that allows both Bopp and Schleicher to reconstruct a nominative singular in $-\bar{V}n(s)$ rather than in $-\bar{V}$ and allows Osthoff to list a set of Indo-European rules of contraction, thus attributing to Indo-European some far-reaching changes. Internal reconstruction is also the dominating factor in Saussure's *Mémoire* and is the basis on which Benveniste builds his theory of the root. From this point of view the methodology seems to be almost the same in Bopp and in the later generations.

Two questions then arise: first, what justifies the contrast that we have noticed between the theory and practice of reconstruction? And, secondly, how do we account for the fact that in the nineteenth century the comparative method is constantly referred to and discussed, while internal reconstruction is not explicitly considered?⁶

⁶ Serious attempts at formalizing internal reconstruction start in the 1940's with Hoenigswald (1944, 1946; cf. 1960a, 1960b), Marchand (1956), Chafe (1959), etc. But much earlier Hermann (1907) had discussed "Einzelrekonstruktion" at length. In 1945, but independently of Hoenigswald, Bonfante drew attention to the problem, considered Meillet's attitude in detail and pointed out that at an earlier stage Pisani had already discussed "la ricostruzione interna" (Bonfante 1945, 146 f.).

Any answer will have to start from an observation which we have already hinted at. Internal reconstruction — once again not the name but the procedure — is much older than the nineteenth century. When the Greek grammarians argued that, given the genitive δελφίνος the nominative singular of the word for 'dolphin' had to be δελφίν and the attested δελφίς was due to a "p a t h o s" of the expected form, they were in fact practicing internal reconstruction in a synchronic and not diachronic mode.⁷ This is indeed the normal way in which grammar was discussed before the historical volte-face of the nineteenth century; most of the time the aim was some sort of internal (synchronic) explanation of the existing forms, much more rarely was diachronic explanation. There was no need for the like of Bopp or Curtius to explain the procedure because it was as well known as the setting up of paradigms and grammatical rules. By contrast the comparative method was a novelty, indeed their novelty. Yet just because it was so well known and it was part and parcel of the normal criteria used in grammatical description, internal reconstruction had a powerful force and was impossible to resist even when, in tune with the interests of the time, turned into a means of diachronic elucidation. The result was the contradiction that we observed, at its most blatant, in the neogrammarians: the explicit rejection of any attempt at going beyond the standard reconstructions of the comparative method and at the same time the acceptance of specific reconstructed forms or rules which were based on internal reconstruction within Indo-European.

In a paper concerned with history and historiography there is no need to proceed any further but the reconstruction of almost 180 years of reconstruction may also induce us to clarify our methods and procedures. I just mention one example. It may have seemed that the implicit belief underlying this account is that internal reconstruction must be abandoned and a 'return' to the 'purity' of the comparative method is called for: Meillet would have been more cautious and more coherent than Benveniste. Yet such an attitude would be misguided simply because there is no 'pure' comparative method. For us, as for our forefathers, internal reconstruction is, and must be, part of the comparative method. When we look for a Greek equivalent to Skt. *ric-*, Lt. *linquō*, etc.

⁷ For the twentieth century the similarities between internal reconstruction (which aims at recovering history) and e. g. Bloomfieldian morphophonemics or generative phonology (which has synchronic aims) have often been stressed; cf. e. g. Lass (1975, 15) who argues that "if we had no external records, I[nternal] R[econstruction] and G[enerative] P[honology] would fully coincide".

we come out with [lejp-], even if we know that the classical Greek form is [lēp-]; admittedly it is (perhaps) possible to find epigraphical evidence which supports this view, but most scholars would simply assume that an ablauting stem calls for an original contrast [lejp-/lip-] and not [lēp-/lip-] and they would be right. Comparison does, of course, confirm this view. It is then reasonable to ask whether applying internal reconstruction to the reconstructed forms of Indo-European in the restrained manner practised by Brugmann or even in the more extensive way of Saussure and perhaps Benveniste does not have a higher form of legitimacy than is sometimes supposed.⁸

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⁸ This is not meant to argue that internal reconstruction is wholly and always reliable (cf. Lass 1975). Rather internal reconstruction must be seen as a useful technique that is used for different purposes at different times and is most effective when is calibrated by historical data and by the results of comparative reconstruction.

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