

CURRENT TRENDS IN LINGUISTICS

Edited by

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VOLUME 13

Historiography of Linguistics

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1975

MOUTON
THE HAGUE • PARIS

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LANGUAGE CLASSIFICATION IN THE NINETEENTH CENTURY

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1. INTRODUCTION

1.1 Attempts to classify the known languages or dialects go back to a very early period: they aim both at establishing differences and similarities on what we would now call a synchronic or an 'achronic' basis, and at pointing out a genetic relationship from a diachronic angle. The terminology, or part of it, may be new, but neither the concept of language affinity nor that of genetic or genealogical relationship were in any way a discovery of the nineteenth century scholars or of their immediate predecessors. It is true, however, that it was in the nineteenth century that a new wealth of linguistic material became available and that new methodological tools were created which allowed the various types of linguistic affinity to be distinguished with some clarity and to be explored in greater depth than had previously been possible. Thus there is some reason to confine this chapter to the nineteenth century, ignoring not only previous achievements but also more recent research, which, in spite of different methods and different emphasis, still continues to plough some of the same fields.

Though it has often been done, it would be deceptive to make a sharp distinction between the different ways in which the languages were classified in our period. It would be equally deceptive to dissociate the concrete achievements of language classification from the general thinking about language and about the problems connected with it. Some of the earlier monographs and articles concerned with nineteenth century linguistics have been guilty of this particular sin. They reveal an excessive attention to the exploration of effective results (with all the related questions of priority and authorship) but a lack of interest in the method by which these were prompted and the background against which they were reached: a great deal of history of technology, but little history of ideas. The outcome is often a 'technical' history which registers the successes and ignores the mistakes: it may appear either as a triumphant and unimpeded progress towards greater knowledge or as a succession of dull squabbles about trivialities.

1.2 Paradoxically, some of the troubles which beset the historiography of the period depend on the richness of the material available. There is a large amount of what we

could call primary sources. Grundrisse, monographs, articles, reviews give us direct access to the thought of the scholars of the time. Letters, memoirs, biographies, autobiographies, documents etc. provide the necessary background knowledge.¹ A probably larger amount of material remains to be discovered in public and private archives and made available to a wider public, but it is still true that what is in fact available is hardly ever made use of. More easily accessible — and consequently more influential — is another type of contemporary evidence which is in the ambiguous position of having the status of primary and secondary source at the same time. More than in any other period, except our own, in the nineteenth century linguistics excelled in writing its own history, while it was making it. Not only were most monographs preceded — as is only right — by a chapter on the 'history of the question', but, more important, entire books were dedicated to the history of the subject for its own sake. F. Max Müller's *Lectures on the science of language*, which appeared in their first edition in 1861, contained what might count as a history of linguistics from classical times to the author's own days. In 1869 Theodor Benfey published his history of linguistics and oriental philology in Germany from the beginning of the nineteenth century; the *Geschichte der germanischen Philologie* by Rudolf von Raumer was published in 1870 and included a discussion of the work done by Raumer's contemporaries. Gustav Gröber's *Grundriss* with his *Geschichte der romanischen Philologie* started appearing in 1886. The highly influential *Einleitung in das Sprachstudium. Ein Beitrag zur Geschichte und Methodik der vergleichenden Sprachforschung* by Berthold Delbrück appeared in the first of its numerous editions in 1880 and was essentially concerned with the work of the three latest generations of scholars. This absence of a time gap between the historian and his subject gives to some earlier historiographical work a curious position in between real historiography and the innumerable obituaries, encomia, laudatory biographies and polemical reviews in which the period is rich. In the first half of the present century most works of historiography have directly descended from the earlier work. However, at the same time as twentieth century historiography has accepted many of the views and perhaps the preconceptions of its predecessors, it has also lost some of the contemporary awareness of the finer distinctions and the clarifying background. In some cases even the semantic value of some key terms has been forgotten (cf., e.g., the very valuable essay by Telegdi [1966] on terms like *historisch*). At present the 'accepted history of linguistics is partly myth and partly serious history' (Hoenigswald 1971: 423); this is certainly true for nineteenth century studies, but the signs are that here — as elsewhere — the situation may be changing.²

¹ Malkiel has compiled a tentative typological inventory of the varieties of primary material available to the historian of linguistics (cf. Malkiel and Langdon 1968-69: 541ff.).

² For some reflections on the recent interest in the history of linguistics cf. Malkiel (Malkiel and Langdon 1968-9: 530-32) and Koerner (1972a). It is probably true that after the experiences of structuralism and generative grammar we now look at nineteenth-century work through different eyes.

1.3 What follows aims at giving a brief critical survey of recent work about the theories and methods of language classification proposed in the last century, but also tries to point out some problems which are either ignored or unsolved. The starting point is perhaps a precursor, Sir William Jones, and the point of arrival F. N. Finck, but there will be occasional references to earlier and later authors. Incompetence has obliged me to concentrate on western scholars operating mostly on (and in) western Indo-European languages; even the history of Finno-Ugric studies is largely neglected. Other Oriental, African and American languages cannot be dealt with here. Some of the specialized historiography about particular sub-groups like Romance, Slavonic, Germanic, etc., is also neglected except when it is felt that it impinges directly on more general studies.³

The historiographical work considered belongs for the most part to the period 1945-70, but occasionally it will be necessary to discuss earlier work.⁴ A few articles and books published after 1970 have been quoted, but, in the absence of reliable bibliographical repertories (like the *Bibliographie linguistique*), chance more than anything else has dictated the choice.

The original aim, completeness, has long since been lost sight of, both in the bibliography and in the preceding survey. The latter was meant to be selective from the start, the former soon became so; lack of time, inadequate libraries, and an unclear delimitation of the subject have proved insuperable obstacles.⁵

1.4 The first question concerns periodization. Traditionally, the beginning of 'scientific linguistics' is dated at the turn of the nineteenth century, just after or just before, and is identified with the discovery of the historical and comparative method and with the demonstration of the kinship of the Indo-European languages, that is to say with two achievements which are both relevant to the problem of language classification. Most prewar and postwar histories of linguistics reflect this view

³ The problem is particularly difficult in the case of works in — and about — Slavonic languages. Scholars like, e.g., Potebnja and Fortunatov do not seem to have had much influence outside Russia, in spite of the originality of their theories. In a short survey perhaps the safest course was to ignore them, just as it has seemed best to ignore scholars such as Renan or Gaston Paris, or earlier authors like Bredsdorff.

⁴ The bibliography lists a few works which could count as primary sources, but the choice is entirely dictated by the references made in the text. I have not aimed at completeness.

⁵ I have ignored work specifically concerned with problems of the philosophy of language. Those introductions to general linguistics which dedicate a short chapter to the history of the subject have also been left aside. On the other hand I have tried to include in the bibliography all the recent histories of linguistics which I have been able to consult. Some omissions are due to library deficiencies: for a fuller list see the bibliography of Koerner (1972a). I particularly regret not to have been able to see Drăganu (1945), Džahoukjan (1960-62), Gagkaev (1957), Hamel (1945), Loja (1968), Verburg (1951): an asterisk premitted to the title of any work in the bibliography indicates that the quotation is second-hand and that I have not been able to see the actual book or periodical. A number of recent works on the history of linguistics have been translated into most European languages; I have made no attempt to list these translations nor have I listed the modern reprints of earlier works. However, in the cases in which a translation or a reprint was available, while the original text was not, the bibliography mentions them.

(together with the evaluative judgement implied in the adjective 'scientific'): sometimes they even give the impression of a science created *ex nihilo*.⁶ It is against this that a recent reaction has taken place. It is useful not to confuse the two different and often contrasting angles from which the attack has come. On the one side, there have been attempts to revalue the linguistic thought of earlier centuries, its general insights and its profound concern with the theory of language. Chomsky's *Cartesian linguistics* (1966) is neither the first nor the last work on the subject, but in spite of its obvious defects and the polemics that it has aroused (cf. Rosiello 1967; Hall 1969; Aarsleff 1970, etc.), it has helped to bring works and authors of this period into the limelight.⁷ An extreme result of its impact could be described in the paradoxical claim that, far from opening 'la voie rationelle' to linguistic thought (Leroy 1971:17), this period marks the end of a truly 'philosophical' approach to the theory of language and the beginning of a time of uninspired drudgery, from which some empirical results, but few new insights were gained. In a sense, then, the gap between nineteenth-century and pre-nineteenth century thought is not reduced, but increased. The only real difference in periodization would then concern the figure of Wilhelm von Humboldt, no more one of the founders of the 'new' linguistics, but the surviving hero of time past.

On the other side some less glittering but perhaps more solid work has tried to show that the gap between the two periods is not as wide as was assumed. Here too we may recognize two different directions. A number of scholars have concentrated on earlier periods and authors (Bacon, Leibniz, Locke, Condillac, Vico, Herder, etc.) and have recognized in them the beginning of an empirical and sometimes historical approach to language. The aim was to demonstrate that in linguistics empiricism and historicism do not arise in the nineteenth century as the result of the anti-Enlightenment bias of Romanticism, but are rather the outcome of a long and gradual process which had started and partly come to fruition one or two centuries before (cf., e.g. Apel 1963, de Mauro 1963, 1970a:52ff., Rosiello 1967 etc.).⁸ In this the history of linguistics probably follows a general historiographical trend towards reducing to more sensible proportions the exaggerated claims of novelty made by European Romanticism.⁹ An important factor is obviously the interest which modern linguistics takes in investigating theoretical and methodological problems in preference to concrete questions.

Other scholars — so far a minority — have preferred to explore in depth the

⁶ Obviously, the position of those histories of linguistic thought which concentrate on philosophy of language is different: cf. for instance the panorama traced in Cassirer's *Philosophie der symbolischen Formen* (1923). Some very thoughtful discussions about theory and methodology can also be found in Pagliaro 1930.

⁷ In this paragraph all references are purely indicative of particular trends and do not aim at completeness.

⁸ It may be instructive to see the somewhat exacerbated polemics on the subject between Bolelli (1966, 1967) and de Mauro (1967b).

⁹ This has often been pointed out: cf., e.g., Christmann 1966 (453ff.), Ginschel 1967 (5ff.), Hermann 1965 (447), Timpanaro 1972a (72).

intellectual background and presuppositions which determined the work of some of the 'new' linguists. The outcome of this work is two-fold: on the one side it has helped to differentiate the theoretical outlooks of the founders of the 'new' linguistics, so that the traditionally monolithic view is now shattered; on the other side it has recognized, in the thought of these authors, survivals — often important — of earlier doctrines. Verburg's (1950) brilliant analysis of Bopp's interests and assumptions (which has brought out the rationalistic element in his formation), Hjelmslev's (1950–51) challenging and somewhat perverse interpretation of Rask's work, M. Bjerrum's (1959) detailed study of his views, Diderichsen's (1960) important and solid reply with his study of Rask's background, L. Hermann's (1965) notes on Jacob Grimm, G. Ginschel's (1967) useful study of his progress towards linguistic history, and some of the more recent works on Wilhelm von Humboldt (e.g. Brown 1967, Christmann 1966, Heintz 1969, Viertel 1966) should be ranged in this category. In this way too the yawning gulf between rationalism and empiricism, Enlightenment and Romanticism, anti-historicism and historicism, in a word, between 'prescientific' and 'scientific' linguistics appears to be bridged.¹⁰

This new historiographical trend is so recent that there has been little time to judge of its validity.¹¹ Timpanaro's (1972a:72–5) balanced assessment of it, together with his reminder that the 'technicians' (e.g. Bopp and Rask) often had general views as interesting as those of the 'theoreticians' (e.g. Humboldt and Schlegel), is a first step in this direction. An equally balanced statement of the problem will be found in Koerner's (1972a) review of Arens (1969). He adopts a relatively traditional view (cf. also Koerner 1972c:216–7), but pleads for a suspension of judgement until more evidence is available about the epistemology of the earlier centuries.¹² One point which Koerner makes (1972a:430) but is oddly ignored in some histories of linguistics (though, see Mounin 1967:175) is that Bopp seems to have been the first scholar to 'emphasize the study of language for language's sake'. The claim to priority may need checking, but the point can be stressed by comparing, for instance, Adelung's attempts, in the introduction to his *Mithridates* (Adelung 1806–17:I iii), to justify the study of linguistics in terms of historical-ethnographical studies about migrations and the origins of peoples, with Bopp's stated purpose to treat languages 'als Gegenstand und nicht als Mittel der Erkenntniss' (Bopp 1833:I xiii-xiv).¹³

¹⁰ Cf. also Teeter 1966 for some strictures upon the standard accounts of the history of linguistics.

¹¹ Even so, it has already yielded fruit: Jankowsky (1972), for instance, starts his account of the linguistic background of the neogrammarians not with Jones or Schlegel but with Francis Bacon and Leibniz.

¹² Whether Thomas Kuhn's ideas about the development of science (cf. Kuhn 1970) could or should be applied to the history of linguistics is still dubious: for a few observations on this point see Bursill-Hall (1970a:114–15; 1970b:231–2, 239–40).

¹³ Even more impressive is Bopp's description, in an early curriculum, of his intellectual progress from a love of Oriental literature to an interest in 'Sprachen an und für sich' (cf. Lefmann 1891–97: II, 116*). Similarly, in a letter of 1815 to Windischmann he declared that he intended to dedicate himself to the study of languages and to endeavour 'das Sprachstudium zu einem philosophischen und historischen Studium zu machen' (*ibid.* 33*). In spite of the apparent clarity of these statements

1.5 This is not the place to discuss the ties between linguistics and Romanticism, a subject dealt with elsewhere in this volume. Nor can we consider the equally important question of the relationship between linguistics and other disciplines, though, e.g. law and classical philology come to mind.¹⁴ But even at the risk of repeating something said elsewhere in this volume, we cannot entirely pass over in silence two other problems which are closely related to each other: that of the influence of the natural sciences on linguistics and that of the organic concept of language.

It has often been noticed — and not only recently — that the language of the early nineteenth century scholars is rich in scientific jargon and scientific metaphors and comparisons: for some observations on the subject, cf. for instance Delbrück (1880: *passim*). More recently, the problem has been tackled again, both in general terms, and (more frequently) with reference to individual authors. However, we still lack a history of linguistics which traces not only the ideal parallelism, but also the effective connections between the study of language and that of the natural sciences.

Observations on the connections between sciences and linguistics (including phonetics) are found in most works by Zwirner (cf. especially Zwirner 1962; Zwirner and Zwirner 1966: 74ff.); see also Maher 1966 and Trim 1959. Attention has often been called to the influence of comparative anatomy on comparative grammar and to the frequent references to it which we find in the early nineteenth century: cf. Aarsleff 1967 (172, 210), Arndt 1965 (482f.), Hoenigswald 1963, Jankowsky 1972 (54ff.), Putschke 1969, Ruprecht 1963, Schwankweiler 1965 (458ff.), etc. Friedrich Schlegel's famous passage about 'vergleichende Grammatik' and 'vergleichende Anatomie' (F. Schlegel 1808:28) has been dismissed as a concession to the spirit of the time by Nüsse (1962:42), who argues that Schlegel was brought to the problem of language classification by his systematic temperament and not through the influence of his scientific studies. Timpanaro (1972a) is more willing to find in Schlegel a reference to Cuvier's work and a sort of programmatic statement aimed at upholding the functions of comparative anatomy as a guide for linguistics.¹⁵

At first sight it may appear that the frequent use of 'organic' and 'organism' reflects yet another of these scientific metaphors, but this time the problem is far more important, since the whole definition and understanding of language is involved. A useful chapter by R. L. Brown (1967:40–53) collects some evidence about the begin-

they pose a double problem of interpretation. First, are they simply meant to contrast the linguistic study of language with that directed towards 'l'explication des textes', or is there something more in them? Do they foreshadow, for instance, the mid-century discussions about the respective roles of *Philologie* and *Sprachwissenschaft*? Secondly, how should they be understood? Do they represent a new view of linguistic studies, or do they imply a return to a rationalistic 'philosophical' attitude? See below note 32 for Aarsleff's reflections on Jones' views.

¹⁴ For the influence of classical philology on linguistics Oertel 1901 is still useful. The more or less violent reactions of the classicists to the first appearance of the new theories about comparison have been discussed in detail by L. Rocher (1957–58); for the later period, cf. also Hoenigswald 1963 and Timpanaro 1963 (5ff., 39ff., 72ff.).

¹⁵ Timpanaro (1972a:96) corrects the popular misapprehension that Friedrich Schlegel was the first to speak of 'vergleichende Grammatik', and refers inter alia to earlier works by J. S. Vater.

nings of these organic conceptions. In an impressive but sweeping article Cassirer (1945) has compared the organic view of language with some forms of modern structuralism. Some qualifications, and an interesting study of the development of the concept of organism, can be found in Lepschy (1962), while the contrast and similarities between organism, system and structure are also the subject of an article by Rensch (1967), who does not seem to have made use of Lepschy 1962. Finally, a great deal of information is available in Haselback's (1966) book on Becker.

Numerous problems could be raised, but here it must be sufficient to point out the difficulties which arise when we try to give a closer definition of organic and organism in their application to language. As Cassirer (and Lepschy) have pointed out, the emphasis may be on 'organism' in the sense of structure (which sometimes incorporates the idea of the whole as distinguished from the sum of its parts), or on the equation of 'organism' and living being, i.e. on the ontological meaning of organism. Finally, 'organic' may be taken as quasi-synonymous to individual or individualized. Needless to say, the first view may lead to some form of structuralism, but the second, as Lepschy has shown, is far more likely to lead towards evolutionism or transformationism, or at any rate towards a conception of language as endowed with a life of its own and towards a conception of linguistics as a natural science. The difficulty consists in establishing whether and how any of these developments took place, and — at a more pedestrian level — in deciding which one of these meanings of organic and organism can be attributed to each author. There is an additional risk: it is all too easy to forget the distinction between a technical term used with its full value in any of the meanings indicated above and a stereotype adjective or noun introduced by some particular writer as a concession to the current fashion. Even so, at the very least it is possible to say that the introduction of the concept of organism in linguistics provides a verbal link with the biological sciences, i.e. with those sciences which had been most concerned with classification problems.¹⁶

1.51 It is likely that it is not the history of the various views of language which is the most profitable field for an enquiry into the influences of the natural sciences on linguistics, but the history of language classification, of its aims, and — up to a point — of its methods. Here historiography lets us down again, because such an enquiry, bearing on language classification, is just what we do not have.¹⁷ This function is not fulfilled by a difficult, controversial, and at times fascinating book by M. Foucault (1966), which among other things discusses the problems of classification in the history of Western thought. Even so, Foucault's book can perhaps provide a starting point which may be accepted because of its brilliant insights or rejected because of its lack of qualifications and distinctions (see Rosiello 1967:168ff.).¹⁸ At a different level

¹⁶ We shall discuss later (see 3.91) the evolutionary theories in linguistics. Here it is sufficient to point out that organic comparisons carry the immediate consequence of continuous reference to life, growth, maturity and decay of language. Yet, even within this context it was possible to conceive of language as undergoing a process of continuous growth or continuous decay (see below note 120).

¹⁷ For Sharadzenidze 1958 see below p. 654.

¹⁸ In spite of some very interesting points it seems to me that the paragraphs which Foucault (1966:

from that explored by Foucault, it becomes clear that the development of scientific classification from Linnaeus through Jussieu to Cuvier (and then to Darwin and Haeckel) had a definite reflection on linguistic thought, even if there may have been a time gap between the two developments. Some examples may prove the point. J. von Farkas (1952:2ff.) has pointed out that already in the eighteenth century August Ludwig Schlözer aimed at introducing Linnaeus's method into ethnology (thus following in the steps of Leibniz) and at using language as the classificatory criterion (cf. also Lauch 1968:278). Later on we find that Rask wanted to replace Linnaeus's division into *classis*, *ordo*, *genus*, *species* and *varietas* by a (genealogical?) classification of languages into *Race (Aet)*, *Klasse*, *Stamme*, *Gren*, *Sprog*, *Sprogart* (cf. Diderichsen 1960:139, but see also Pott 1856:192ff.);¹⁹ still later Pott, then in the full maturity of his thought, argued that for a linguist the highest task would consist in being a new Linnaeus capable of producing a classification of the languages of the world (Pott 1856:191). This was hardly a new idea, but from the context it becomes clear that Pott was also thinking of a 'physiological' i.e. a typological classification (*ibid.*: 197). A further qualification which Pott added is even more enlightening: the new classification — we are told — must not be 'artificial', like that of the Swedish naturalist, but 'natural' like that of Jussieu (*ibid.*: 196). We may contrast this statement with the words which an English scientist and philologist, T. Young, had written some forty years before in a review of Adelung's *Mithridates*: 'A perfect NATURAL order of arrangement, in treating of the peculiarities of different languages, ought to be regulated by their descent from each other and their historical relations: a perfect ARTIFICIAL order ought to bring together into the same classes all those genera which have any essential resemblance ...' [my emphasis] (Young 1813-14:252).²⁰

Well after Pott and Young the artificial/natural distinction reappears in the writings

292ff.) has dedicated to the beginning of comparative linguistics in the nineteenth century are among the least satisfactory of the book. I am not certain, for instance, that 'tout l'être du langage est maintenant sonore' as Foucault maintains (*ibid.* 298), nor am I certain that, in contrast to the earlier period, in the nineteenth century 'toutes les langues se valent' (*ibid.*). As we shall see, attempts to evaluate the various languages continue all through the century. The main point, however, is that Foucault too is mesmerized by the development of the comparative method and of genealogical classification, and, in spite of his reference to the theories of Bopp and Schlegel, he almost entirely ignores contemporary typological classifications. As I have tried to show above, typological and genealogical classifications appear in the course of the nineteenth century to follow a route which is similar to that reconstructed by Foucault for the earlier history of biological classification; this very fact makes it difficult to accept Foucault's point that similar developments had taken place at the same time in linguistics and in the sciences.

¹⁹ For the whole problem of Rask's attitude towards the classification of the natural sciences see Bjerrum (1959:63ff.). It is also worth remembering that Rask's compatriot J. H. Bredsdorff [1790-1841], who is best known to linguists for his very modern views on the causes of language change (cf. Thomsen 1927: 62-3, Jespersen 1922:70-1; I regret that I have not been able to see Bredsdorff 1970), was also the author of an interesting dissertation *De regulis in classificatione rerum naturalium observandis* (1817).

²⁰ According to the O.E.D. this review marks the first occurrence of the word Indo-European in written English. It should also be pointed out that in it Young argued that language had to be identified with a 'species', as Schleicher was to do much later (see below 3.7 and note 99).

of R. de la Grasserie (1889-90) who criticized as artificial all the previous typological classifications and proposed a natural classification based like those of botany and zoology on the principle of 'subordination des caractères'. In a period in which the controversy on evolution was just subsiding he then proceeded to wonder whether a typologically based natural classification, such as the one he had proposed, could also be proved to have a genealogical value.²¹

To sum up: in this field the natural sciences seem to set the task: first a comprehensive classification, secondly a 'natural' classification. The way in which nineteenth-century scholars tackled it is significant for the development of linguistics; even more so is the interplay, which we cannot help noticing, between genealogical and typological classifications as possible ways of solving the problem.

1.6 Neither the traditional nor the more modern historiography appear to have explicitly discussed the problems posed by the relationship between the various types of language classification in the nineteenth century.²² The 'comparative method' with its spectacular achievements has monopolized attention. However, at the same time in which this was being developed and experimented with, scholars (often the same scholars) were also interested in a so-called morphological classification, i.e. in a typological classification which aimed at grouping languages not according to their origin, but according to their structural similarities. Genetic and morphological classification had an important point of contact. By the turn of the century (and probably long before) it had become clear that a genetic (or genealogical) relationship between languages could not be established only on the basis of lexical agreement; structural similarity appeared to be a more important criterion. In the same period, however, we find that typological classification is also based on grammatical and structural considerations. Is this coincidence entirely accidental?

A posteriori we can see that another point also helped to bring the two classifications together: they both disclaimed any arbitrariness and artificiality and were confident that they rested on some essential characteristics of the languages which they classified. The pressing question — and one that impinges both on the history of comparative method and on that of typology — is whether all leading scholars in the first part of the century made a clear distinction between genetic and typological affinity. It is often not seen that this is in fact a problem, but once it is posed it appears that, in the early period at least, there is little justification for keeping apart the histories of the two types of classification. If we are obliged to do so in this survey, it is because there is so little overlap in the historiographical work concerned with the one field or the other. This conflict between history and history of historiography may perhaps explain some of the uneasiness which no doubt will emerge from the exposi-

²¹ For R. de la Grasserie see below 3.10 and note 127.

²² There have been a few relevant contributions, but they have concerned the interpretation of individual authors; cf., e.g., Hjelmlev 1950-51, Bjerrum 1959, and Diderichsen 1960 for Rask, Timpanaro 1972a for Schlegel.

tion, the frequent references from one section to the other, and the repetitions which we have tried to avoid but have kept creeping in. We shall first discuss the history of the comparative method and then that of typological classification; towards the end we shall try to gather the common threads.

2. GENEALOGICAL CLASSIFICATION

2.1 We have seen that most of the challenges to the traditional periodization have been based on the assumption that technical achievements count but little: what matters is the general outlook on which they are founded, and this is the result of a lengthy and continuous process. However, there is some agreement in pinpointing the comparative method as one of the concrete results which had a more lasting influence. If so, the challenge could also be posed from a different angle: how correct is the claim of absolute novelty made by — or on behalf of — the 'technicians' of the nineteenth century?

Work on this point appears to be either non-existent or somewhat disjointed and lacking in continuity. Even so, it is one of the great merits of some recent histories of linguistics that they have made available some information, however elementary and incomplete, about the first studies which impinged on language comparison and genetic classification.

Thus Arens (1969) devotes some space to early attempts at classification (Dante, J. J. Scaliger, Gesner, Leibniz, Ludolf, Ten Kate, etc.) up to Adelung's *Mithridates* (but more space should be dedicated to Hervás than has been done). Robins (1967: 164–9) briefly traces very much the same story and stresses the point that what characterizes early work is not so much lack of insight as lack of that continuity in scholarly production which characterizes the nineteenth century (*ibid.*: 164). Tagliavini (1968a: 32ff.) provides as usual rich factual information and is far better informed than most histories of linguistics about the comparison of non-Indo-European languages. Mounin (1967) makes an interesting effort to give the full picture of the concrete work attempted during the sixteenth, seventeenth and eighteenth centuries with its merits and its absurdities, but lack of space does not allow him to go much into detail.²³ Some of the earlier stages of pre-nineteenth-century comparative work have been discussed by Agrell (1955) in a useful monograph which concentrates on methodology, with particular reference to Germanic languages, and goes back as far as the Renaissance. In the fifties Bonfante (1953) reacted against the traditional assumption that the inter-relationship of the Indo-European languages had been first thought of at the end of the eighteenth century. In a twenty-page article he collected evidence about the various classifications of European languages from the Middle Ages to the

²³ For some information, which partly depends on Arens, see Apel 1963 (280–5). Neither here, nor in the bibliography, have we listed all work about individual authors, though some of them (e.g. Gesner) have come in for a large share of attention.

time of Sir William Jones. As a source of basic material this article still remains unsurpassed; it should be integrated with Bonfante's note on E. Brerewood (Bonfante 1955). More recently — and at a more popularizing level — Otto Zeller (1967) in his history of comparative Indo-European philology has broken with the tradition and started his exposition from Gesner, Megiser and J. J. Scaliger, arguing, somewhat perversely but interestingly, that comparative linguistics in our sense of the word was created by these three scholars. Zeller analyses their classifications and contrasts the alphabetic arrangement (with some genetic subgrouping) of the languages of the world, adopted by Gesner, both with the partly genetic, partly geographical classification of Megiser, and with the sound genetic classification of Scaliger linked with his firm denial of any religious presupposition. The next author discussed, before Friedrich Schlegel, is Leibniz, with his interest in language development, his distinction between language similarities due to common origin and those due to secondary contacts, and his dual subdivision of languages into an Aramaic and a Japhetic group. The information provided is not new, and consists largely of reproduction of primary sources. Even so, this short book has an unusual character of freshness and novelty which is obviously connected with its constant use of first-hand evidence.

2.12 The standard works of reference provide only very scanty information about the early stages of those comparative studies which do not concern Indo-European languages (though, as we have seen, Tagliavini 1968a is an exception).²⁴ This is understandable, but we must hope that some way (team work?) can be found to fill the gaps. Arens (1969:105–6) has a tantalizing quotation from the Orientalist Ludolf who at the end of the seventeenth century was convinced that in order to prove that two languages are related it is necessary to show that their 'grammaticae ratio' is similar. We should like to be told much more about Ludolf, but Arens himself quotes second-hand from Benfey (1869) (cf. also Jankowsky 1972:23). Elsewhere Arens (1969:148) mentions without any direct quotation the works of Sajnovics and Gyarmathi on Finno-Ugrian and points out the attention given to grammar in the latter's *Affinitas linguae hungaricae cum linguis fennicae originis grammaticae demonstrata* (1799). It would have been possible to make use of some recent and less recent articles and monographs about the development of Finno-Ugrian linguistics. Information about the early period of these studies is available, for instance, in a 1957 dissertation by A. Stehr, a pupil of J. von Farkas (but cf. also Farkas 1948 and 1952, Gáldi 1955 and 1956, Hormia 1964–65, Lakó 1970, Mezey 1965, Pražák 1967, etc.). Mention should be made of Stehr's (1957:7–23) study of the *Nachlass* of Martin Fogel [1634–1675], with his very clear distinction between loan words and inherited words and his demonstration, based on structural considerations, of the kinship of Hungarian and Finnish. Similarly the observations of J. E. Fischer [1697–1771] about the phonetic correspondences between Hungarian and the other Finno-Ugrian languages should

²⁴ I have not found mentioned in the recent histories of linguistics the *Dissertationes miscellaneae* (1706–08) of the Dutch scholar Relandus, who, according to Gabelentz (1891:26), was the first author who established 'Lautvertretungsgesetze' between two related languages (Malay and Malagasy).

be rescued from oblivion (Stehr 1957:51–100, especially 93–4; cf. also Vdovin 1954 and Gulja 1965 with interesting accounts of early work devoted to collecting and organizing linguistic data). Before the diffusion of specialization in the second half of the nineteenth century it was conceivable, and indeed normal, that a scholar working on Indo-European languages should have been influenced by a Semitist or an Ugrologist: Stehr has pointed out that Leibniz went to Hamburg to see Fogel's manuscripts, we know that Rask admired Gyarmathi and was a Finno-Ugrian scholar himself, and we have evidence of Grimm's interest in Finnish language and poetry (cf. Kunze 1957). However, until we have more detailed studies of the methodology involved in these earlier classifications, both when they were successful and when they were not, it seems necessary to suspend judgement about the degree of continuity that can be postulated between them and nineteenth century work.²⁵

2.2 We cannot discuss here the historical background against which the development of linguistics must be considered (for some general observations, cf. Pätsch 1960:211ff.). It suffices to repeat the banal observation that the end of the eighteenth century saw an increase in the linguistic material available (missionaries and colonizations contributed to it) and a deeper understanding of — as well as a greater interest in — the earlier phases of national languages, together with a stress placed on descriptive rather than normative grammar.²⁶ The great compilations by Hervás and Adelung (not to mention Pallas) continue an older tradition of similar collections of material, but the richness of the evidence confirms our point. They would deserve a far closer analysis than they have received in recent years; in particular Hervás's *Catálogo* (cf. Hervás 1784 and 1800–05) and his more theoretical works (e.g. Hervás 1787) are worth remembering both for their historical importance and for their intrinsic methodological interest. Not much has been written about them; at the moment we can only quote a very interesting discussion by Lázaro Carreter (1949:100–12), some factual articles by Batllori (1951 and 1959) and a few paragraphs in Mourelle-Lema (1968:162f.). For Adelung, whose *Deutsche Sprachlehre* has always attracted some attention, some valuable information is now available in Diderichsen 1960 where his views are often compared with those of Rask (cf. also Jankowsky 1972:33ff. and Spiridonova 1961).

Far more is known, though still not enough, about the first Western contacts with

²⁵ For Gyarmathi cf. also Zsirai 1951. I have not been able to make use of bibliography in Finnish and Hungarian and I have found it impossible to see some Finnish periodicals.

²⁶ In the history of linguistics more space should be devoted to earlier studies of individual branches of Indo-European. We have mentioned above Annelies Lauch's article (1968) about Schlözer's contributions to Slavonic studies, but figures like those of Josef Dobrovský [1753–1829], Bartholomäus (Jerney) Kopitar [1780–1844] and Vuk (i.e. Vuk Stefanović Karadžić) [1787–1864] should certainly not be forgotten, not only for the value of their contributions, but also because of the importance attributed to their work by Jacob Grimm. Some general information on Schlözer will be found in Winter 1916 and 1962 as well as in Lauch 1968; cf. also Eichler 1966 and Grau 1966. For information and bibliography on Dobrovský cf. Krbec-Michálková 1959 and Krbec-Laiske 1968 and 1970.

Sanskrit and with some of the Oriental languages. Benfey 1869 and Windisch 1917–20 remain indispensable, but Schwab 1950 is a mine of information (which sometimes needs checking) about anything oriental in Europe (and particularly in France) between the end of the eighteenth century and the beginning of the twentieth century. Willson 1964 is also useful as a German counterpart for the earlier period and makes good reading, but is less rich in information about linguistic matters. More recently a detailed and reliable monograph on Alexander Hamilton (R. Rocher 1968, cf. R. Rocher 1970) has provided us with first-hand evidence about one of the men who had a determining influence in introducing Sanskrit in Europe. Some more information can be obtained from the rich literature on Sir William Jones (see below 2.3). A useful piece of research (Oppenberg 1965) has tested at close quarters the knowledge of Sanskrit which appears in the translations of the first German who learnt this language, Friedrich Schlegel.

Traditionally Sanskrit is seen as the catalyst which accelerated or even determined the success of Indo-European comparative studies. No doubt, the causes are many and need not all be of a linguistic nature; however, it has frequently been suggested that the structural clarity or the transparency of the language is one of them.²⁷ Recently, Emeneau (1955 and 1971) has stressed the vagueness and possible irrelevance of this notion, and has pointed out that what mattered more was the contact with a different grammatical tradition and a type of grammatical analysis clearly distinguished from that of the Western tradition.²⁸

Whatever the cause, it seems likely that the contact with Sanskrit pushed further a preexisting tendency towards grammatical analysis and segmentation: this may possibly help to account for the persistent morphological interests of scholars like Bopp who had come to comparative grammar from Sanskrit. Another question, which has not been asked but is important, is whether Sanskrit had a determining influence only on the progress of the comparative method. Or did it have a part to play also in the beginning of 'morphological' classification?

²⁷ Georg von Gabelentz (1891:26) is more explicit; language comparison — he argues — requires etymological analysis and this is a difficult task in languages like the Western ones in which forms cannot be easily segmented; Sanskrit offers more promising material. But this is the moot point: Emeneau (1955:149) has maintained that he does not find Sanskrit more transparent than Greek. Perhaps another factor should be considered. Any teacher of Indo-European comparative grammar knows that it is difficult to induce a student steeped from youth in the classical languages to analyze those forms which he has learnt as indivisible wholes. The same student, however, when faced with a set of Sanskrit forms, and with an explanation of their meaning, will automatically proceed to segment them, if only to provide himself with a mnemonic aid. It is then likely that the similarity between some Sanskrit and some Greek or Latin morphs will lead him back to the more familiar forms and will induce him to look at them with different eyes. Conceivably this or a similar phenomenon contributed to the generally held assumption that Sanskrit was more transparent than the other European languages.

²⁸ The importance of the contact with Sanskrit grammarians had been mentioned before: cf., e.g., Gabelentz (1891:26), Pedersen (1962:21–2). In the 1950s the question has been the object of a short controversy between Emeneau (1955) and Master (1956). Emeneau argued that Sir William Jones's analyses were helped by Indian grammatical teaching in the Pāṇini tradition; Master has denied any influence of Pāṇini on Jones and indeed any originality in Jones's views on Indo-European, which he assumed were in part taken from Wilkins. A convincing answer has come from Emeneau (1971).

2.3 Even within the narrower limits imposed by dating the start of the comparative method at the end of the eighteenth century questions of evaluation and priority have arisen. Traditionally Bopp is hailed as the founder of comparative linguistics, but Arens (1969:165ff.) follows the line first developed by Benfey (1869:357ff.) and gives priority to Friedrich Schlegel. However, in his important book on the study of language in England from 1780 to 1860, Aarsleff (1967:124; cf. also 156 and note 112) points out that the credit normally given to Friedrich Schlegel as instigator of the new linguistic trend really belongs to Sir William Jones. Interestingly enough Aarsleff's challenge against the Schlegel 'orthodoxy' seems to proceed in parallel with the opposite challenge against the Bopp 'orthodoxy'. According to de Mauro (1970a: 84ff.) it is one of the great merits of Arens that he has modified decisively the traditional view which saw in the factual Bopp the founder of the new discipline. Wise words on the subject have been said by Timpanaro (1972a), who has put the question in its historical perspective. We might be somewhat amused by the parallelism of the two disputes but in fact there is a considerable difference between them. De Mauro's championing of Schlegel (and Humboldt) is in line with his wish to assert the continuity of linguistic thought from Leibniz to the nineteenth century; Aarsleff's interest in Sir William Jones vis-à-vis Schlegel is partly due to reaction against some of the 'mystical' attitudes of Schlegel, and partly to the observation that the fight against arbitrary etymologies, the emphasis on structural analysis of words, and the insistence on historical and comparative study of languages (Aarsleff 1967:156) had already found an expression in Jones's *Anniversary discourses*.²⁹

It is doubtful whether those scholars who, like Hockett (1965:185), date the first break-through in linguistics to 1786 (the year in which Sir William Jones gave the third anniversary speech to the Asiatic Society at Calcutta and pronounced the famous statement about the relationship of Sanskrit with the other European languages) are doing this for the same historical reasons as Aarsleff. However, the contrast is there. For people like Lehmann (1967:7ff.), Lounsbury (1968:174) and up to a point Waterman (1963:15ff., where Schlegel is ignored) Jones has a prominent position; for others (Bonfante 1953:696-7; Hoenigswald 1963:2-3, Ivić 1970:33, Kukenheim 1966:41, Tagliavini 1968:50; etc.; Zeller 1967 ignores Jones altogether) Jones is a remote precursor and Schlegel and Bopp (or Rask) the founding fathers. Aarsleff's intervention has put the discussion on a somewhat different level, but it may be useful to remember that in this type of argument it is all too easy to confuse a number of problems.

The first task is that of extracting from the sources enough evidence to allow an evaluation of the scholar or scholars under consideration. In this case Aarsleff rightly complains that, with the exception of the all too famous 'philologer' passage, Jones is more quoted than read. Recently, Jones's life and work have been the object of numerous articles and monographs (cf. Arberry 1943-46, 1944, 1946, 1960, Cannon

²⁹ Aarsleff's line is entirely adopted by Salus (1969:15-6), but not, for example, by Koerner (1972a: 434, 1972c: 216).

1952, 1953, 1958, 1963, 1964, 1968, 1970, Chatterji 1948, Edgerton 1946, Emeneau 1955, 1971, Fan 1946, Kirketerp-Møller 1962, Lohuizen-de Leeuw 1948, Master 1946, 1956, Mukherjee 1968, Waley 1943-46, 1952), but most of them have concentrated on the Orientalist rather than on the Comparativist.³⁰ Aarsleff's (1967) analysis is far more detailed.

Secondly, there is a real question of priority, both in the use of particular techniques (grammatical comparison etc.) and in the formulation of a general outlook. As far as the techniques used in comparison are concerned, it is dubious whether priority belongs to the students of Indo-European languages (see above para. 2.12). In particular, interest in grammatical similarity seemed to be in the air in the second half of the eighteenth century. Both Arens (1969:136-46) and Timpanaro (1972a:78) have called attention in this respect to Kraus's review of Adelung's *Mithridates* published in 1787. Timpanaro has also recalled the importance attributed by Hervás to the 'artificio delle lingue', i.e. to their grammar. It is worth stressing that in Hervás this is not an isolated dictum but is part of an interesting and well worked out theory about language plurality.³¹ Even so, priority does not certainly belong to Hervás: Lázaro Carreter (1949:105-9) has identified some of the Spanish antecedents of his ideas. Heinz Pohrt (1964:328) has underlined the importance given to grammatical structure in the somewhat fantastic monogenetic views of K. G. von Anton [1751-1818]; Diderichsen (1960:106) has referred in this connection to Gatterer's *Einleitung in die synchronistische Universalhistorie* (1771), and A. L. von Schläzer's [1735-1809] views have also been mentioned (e.g. by Lauch 1968:279). Some of the studies on Finno-Ugrian have already been quoted (cf. above para. 2.12); for the earlier period Lakó (1970) has referred to Mikael Wexionius-Gyldenstolpe and to his 1650 work on the grammatical structure of Finnish and Estonian. This list could continue, but the point is abundantly proved already. Obviously, general outlook is more important, but less easily definable. It would be useful, for instance, to know how to interpret the fact that linguistic considerations were never primary in Jones's work in the way in which they were, e.g. in Bopp.³²

Thirdly, a problem of influence and continuity arises. All too often, the real founder of a school is not the man who first formulated its tenets, but the man who managed to do so most effectively and who was then generally recognized as master. In the development of comparative linguistics, which according to Benfey (1869:15; cf.

³⁰ I have ignored those articles on Jones which do not give primary attention to his linguistic work. The biographies and the edition of his letters are quoted as sources of first-hand material and for further bibliography.

³¹ In his *Saggio pratico delle lingue* (1787:17) Hervás argued: 'Gli idiomi per la loro sintassi differiscono più che non per le parole' and, later on (*ibid.* 53): 'consistendo la vera diversità degl'idiomi nella loro differente sintassi, a metter questa in chiaro lume sono necessarj i loro elementi grammaticali'. The interesting point, however, is that Hervás's theory about language relationship is coupled with a theory about language change (see below 3.91).

³² Cf. above 1.4 and note 13. According to Aarsleff (1967:124) Jones's attitude to linguistics (which he considered a means rather than an end in itself) is a healthy sign of his rejection of the philosophical dreams of his predecessors and of a more empirical approach.

Robins 1967:170) is entirely German, what part is played by Jones the comparativist (as contrasted with Jones the Indologist, who was largely responsible for the wild enthusiasm with which Sanskrit literature was greeted in Europe)? We have evidence enough to prove unquestionably that the Anniversary Discourses were widely read, first of all by Schlegel (cf. Aarsleff 1967:156-7), but on the one side the extensive quotation of examples which Schlegel introduced into his 1808 book, and which Jones would have found impossible to introduce into the Discourses, was likely to be more impressive for potential followers, like the young Bopp; on the other side, in spite — or perhaps because — of some vagueness and nebulosity, Schlegel's philosophical background and what Aarsleff (1967:157) calls his 'nearly mystical idealism' helped to give the impression that the historical method and the comparison of languages could reveal endless new horizons (not only of an ethnological nature). This last point is perhaps more important than the rest, especially now that it is becoming more and more clear that not only the principle of grammatical comparison, but also the actual detailed comparison of forms in Sanskrit, Greek, Latin, German, etc. were not peculiar to Schlegel, let alone Bopp.³³ Some ten years ago Ludo Rocher (1961) published an interesting analysis of the comparative writings of Paulinus a Sancto Bartholomaeo, whose work appeared between 1790 and 1802; more recently Rosane Rocher (1968; cf. also R. Rocher 1970) has called attention to the man who taught Sanskrit to Schlegel, Alexander Hamilton. In an earlier article Chambers and Norman (1929) had reached the conclusion (to which R. Rocher subscribes) that the surprising coincidences between the observations made in Hamilton's review of Wilkins's Sanskrit Grammar (Hamilton 1809) and those of Schlegel's 1808 book (which Hamilton could not have read at the time) are due to the earlier influence of Hamilton on Schlegel. This is possible, and could perhaps serve as a further link between Jones and Schlegel. Even if it were mistaken, the coincidences would confirm that around the same time a number of scholars had reached — or was acquainted with — the same conclusions. What is not certain, however, is that the conclusions were in themselves shattering. They needed to be put in a more general framework, and this — with all its limitations — is what Schlegel could and did provide.³⁴

³³ A recurrent priority question concerns the supposed discovery of the common origin of Sanskrit, Greek and Latin by the French Jesuit G. L. Coeurdoux, who would have preceded Jones by almost twenty years, though the letter in which he discussed his theories was not published until 1808. Edgerton (1946:236, note 44) pointed out that Coeurdoux's words cannot be interpreted in this sense, but this did not stop J. Godfrey (1967), who does not seem to have known Edgerton's article, from pressing Coeurdoux's claim again (nor A. Arlotto [1969] from refuting him).

³⁴ In order to emphasize the importance of Fr. Schlegel's work in the history of comparative linguistics, I cannot refrain from quoting an inedited letter by Rask, which I hope to publish soon. On 27 April 1824, Rask wrote to Joseph Bosworth, the Anglo-Saxon specialist, discussing some problems in the classification of the Germanic languages. In the course of the argument he reiterated his belief that 'the European languages of the Gothic stock are related to those of India and Persia' and concluded advising Bosworth to read Schlegel's 1808 book. This is significant, if we consider that by 1824 the subject had advanced far beyond the stage of *Ueber die Sprache und Weisheit der Indier*.

2.4 The names of Humboldt, Rask, Bopp and Grimm are regularly mentioned in any history of linguistic concerned with the development of the comparative method. We shall refer to the literature on Humboldt further on (see below para. 3.5). Some works about Rask have already been quoted (cf. p. 611): one of the encouraging tendencies in modern historiography is the concentration on a correct interpretation of Rask and Grimm rather than on the older questions of priority; even when the latter are raised the emphasis is different (cf., e.g., Antonsen 1962, Ginschel 1967: 357ff.). There has been a number of shorter contributions discussing particular aspects of Rask's life and work (Bjerrum 1956, 1957; Djupedal 1956a, 1956b, 1958, Claveria 1946; Henning 1963; Meisner 1972; Nordstrand 1955, 1961; Skårup 1960, 1964; Sletsjøe 1957; de Tollenaere 1948, 1951, 1953, 1954),³⁵ but undoubtedly the most important question was raised by Hjelmslev (1950-51): was Rask a typologist or an historical linguist. Hjelmslev favours the first hypothesis and argues that historical-comparative linguistics, in our sense of the word, really begins with Jacob Grimm; M. Bjerrum's monograph (1959), which is also rich in detailed analyses, seems to follow in this line, but with a number of qualifications. Independently of Hjelmslev's article and before Diderichsen's monograph an interesting contribution by F. de Tollenaere (1951) had adopted a mid-way position and shown some of the oscillations in Rask's thought, while firmly upholding his genealogical interests. Diderichsen's work (1960) has provided a clear analysis of Rask's background and interests, of his compromise between Romanticism and Rationalism, of the contrast between his and Grimm's approach to language, and also of his striving towards a genealogical classification. Undoubtedly it is, and will remain for some time, the standard work on Rask. More recently, Percival 1970 has discussed Rask's views on language change, as attested in his review of Grimm's *Deutsche Grammatik*.

2.41 Understandably Bopp's work has called for less attention; there was no need to vindicate someone who since 1833 at least had been hailed as the founder of the new linguistics (cf. Pätsch 1960:211, Orlandi 1962:528ff.). On the contrary, as we saw before, there have been attempts to remove Bopp from this enviable position in order to allow some of the aura which surrounds him to pass to Schlegel or to some earlier scholar (cf. above para. 2.3). However, in 1969 the 150th anniversary of Bopp's birth was celebrated by the Humboldt University in Berlin with a collection of shorter contributions on Bopp, some of which dealt rather interestingly with Bopp's solution of specific comparative problems (cf. Barschel 1969, Desnickaja 1969, Fiedler 1969, Seidel 1969, Seidel-Sloty 1969, Sternemann 1969, Uhlich 1969; Valderrama 1969 does not belong to this series and does not really concern Bopp).

Other articles or sections of books have concentrated on Bopp's methodology and intellectual background (cf. Lohmann 1941, Neumann 1967, Orlandi 1962, Pätsch 1960, Rosiello 1967:173ff., Terracini 1949:61-72, Verburg 1949-50, as well as the

³⁵ Unfortunately I have not been able to see two recent works on Rask: one is the commentary (Bjerrum 1968) which has followed the earlier publication of his letters (cf. Hjelmslev 1941), the second is a 1971 monograph by G. A. Piebenga about Rask's Frisian grammar.

histories of linguistics quoted in the bibliography). In contrast to the traditional image of Bopp as an exponent of Romanticism, deeply influenced by Humboldt's ideas (cf. Terracini 1949), Verburg (1949–50) has effectively underlined the rationalistic slant of his interests, his connection with a tradition of linguistic thought started by Leibniz, and the static aspect of his view of language comparison if contrasted, e.g. with that of Grimm. Orlandi (1962) has brought the argument even further in his attempt to show that an understanding of Bopp can only come from a study of his concrete work as contrasted with his theoretical statements. According to him Bopp's only aim was that of using Sanskrit as a parameter or a lens to systematize the empirical data provided by the various languages (cf. also Lohmann 1941): his interests were not historical but methodological. This thesis, to which Rosiello (1967) seems to subscribe, may be too extreme; we should not forget that Bopp is really looking for a historical explanation of the forms in which he is interested. Terracini (1949) is probably right when he maintains that Bopp uses comparison and history to solve a problem of general linguistics. Neumann's (1967:16) point that Bopp applies to linguistics that principle of 'genetische Erklärung' to which Herder had given a theoretical expression is relevant. If Bopp believed (with a number of his contemporaries) that a correct explanation of any phenomenon could only be obtained by going back to its origin, then he must also have realized that his systematic interests were better served by a historical approach. A balanced view is perhaps that of Pättsch (1960), who recognizes in Bopp both a number of Romantic ideas and a strong rationalistic streak, but we still need a new synthesis to take into account Verburg's findings and to replace the well-informed but quasi-hagiographic biography by Lefmann (1891–97). Possibly this will refute or clarify the often quoted dictum by Meillet (1937:458) that 'Bopp a trouvé la grammaire comparée en cherchant à expliquer l'indo-européen, comme Christophe Colomb a découvert l'Amérique en cherchant la route des Indes' (cf. Mounin 1967:174, Leroy 1971:21). If Columbus really meant to reach the Indies, America could only be an obstacle in his way, but — in 1816 at least — for Bopp the 'discovery', or rather the exploitation, of the comparative method was a necessary factor in the demonstration of the point which he had set out to prove: the agglutinative origin of the inflectional forms.³⁶

2.42 It is hopeless to try to give in a few lines a survey of the recent work on Jacob Grimm. A number of contributions which are primarily concerned with the man, the collector of folk tales, the patriot, the German philologist, the comparative mythologist, etc., also provide some accidental information about the linguist. Denecke 1971 may serve as an introduction to this multifarious literature. Our bibliography only mentions articles and monographs of prevailing linguistic interest and

³⁶ It is well known that Bopp argued — against Fr. Schlegel — that a number of Indo-European inflections arose from the agglutination of monosyllabic roots with pronominal elements or with forms of the verb 'to be'. For the further history of this view it is still worthwhile to consult Pagliaro (1930:64ff.). We still lack a history of agglutinative theories, but Timpanaro (1972a:83 note 35) has announced that he will discuss the earlier origins of the doctrine and its diffusion in the eighteenth century in a forthcoming paper. Cf. also *infra* note 121.

ignores both the general biographies and the editions of primary sources, like documents, letters, etc. While in the case of Rask and Bopp it is possible to recognize some definite historiographical tendencies at work, the same is difficult to establish for Grimm. Some recent articles are concerned with background information and with specific Germanic problems (cf. Antonsen 1962, Bergsveinsson 1965, Bondzio 1965, Henning 1963, Hiehle 1949, Kunze 1957, Löther 1965, Mojasevic 1963, Peukert 1964, 1966, Pfeifer 1963, Schoof 1941, 1963a, 1963b, 1963–64, 1967, Trier 1964, Wanner 1963) or with questions of terminology (Ginschel 1955; cf. also Telegdi 1966, 1967a). A substantial contribution is G. Ginschel's attempt (1967) to trace Grimm's intellectual biography and to emphasize his originality both in the ideas which he shared with Savigny and the *Historische Schule* and in his approach to historical linguistics. A few other articles aim at a general assessment, and here too we sometimes recognize a reaction against the traditional view of Grimm: the stress is often put on the a-priori, non-historical and non-empirical elements of his thought (cf. Hermann 1965, Spreu 1965, and the very interesting discussion in Heinemann 1967 of the outlook of Grimm, Diez, and Meyer-Lübke). However, it is difficult to deny that, even when allowance has been made for these elements, there is in Grimm an overwhelming interest in historical development and in national language; this has often been reasserted (cf. Ginschel 1967, Hammerich 1963, Löther 1965, Ulmer 1951, and especially Arndt 1965). An interesting contrast is provided by two works both of which set out to compare Humboldt's and Grimm's 'Sprachauffassungen': Beneš (1958) and Schwankweiler (1965). Schwankweiler is eager to point out the common ground on which Humboldt and Grimm move (social character of language, historical conception, organicistic views etc.), while Beneš aims at stressing the differences both in their conceptions of language and in the largely historical approach of Grimm as against the descriptive and systematic interests of Humboldt. Finally, the 'modernity' of Jacob Grimm has been underlined by R. A. Fowkes (1964) in a rather schematic list of the seven chief services which he has contributed to linguistics (cf. also Arndt 1965, Waterman 1963:20–30, and the very interesting observations of Telegdi 1967b).

2.5 What were the arguments used to demonstrate genetic relationship? Historians of linguistics seem to agree that by the beginning of the nineteenth century the emphasis was on structural similarity rather than on lexical equivalences. Regularity of sound correspondences was sometimes mentioned, but certainly not by all linguists.

It is easy to give the wrong impression: even in the golden period of structural similarity, lexical correspondences were by no means neglected. In fact, the history of the way in which they were used both before and during the nineteenth century would deserve a separate study, similar to that which Mary Haas has dedicated to Amerindian linguistics in her paper on *Grammar or lexicon?* (Haas 1969). At present it is not easy — if indeed possible — to find out from the relevant histories of linguistics how well defined at any one time were notions which subsequently became banal, such as those of loan-word, of fundamental vocabulary (which is less subject to change),

of lexical similarities due to onomatopoeia, etc. Certainly we find them all in Hervás, but they did not start with him. In Adelung's *Mithridates* (1806-17:I xiii) we also find the observation (which need not be original) that the relationship of languages can only be shown through the comparison of root syllables, and the warning that comparison must be preceded by a correct identification of the root, in order to avoid equating e.g. Gr. *anainomai* with any word including just *an*, *na* or *in*.³⁷

Equally important — and perhaps more so — would be to trace the development of the criterion of structural similarity; at the moment we only have odd observations about the use made of it by individual authors (for the earlier period see above para. 2.3). Timpanaro (1972a:80ff.) points out that in Friedrich Schlegel it assumed an almost mystical, *a priori* value and became the absolute criterion for the classification of languages. This attitude is contrasted with that of e.g. Gyarmathi and Rask who reached the criterion inductively through their own experience of work on language. It would be possible to challenge this last point for Rask, whose methodology, though different from that of Schlegel, is not always determined by empirical considerations, as Diderichsen (1960) has shown (but cf. also Bjerrum 1959 *passim*). In the case of Gyarmathi we should like to know much more about the influences which he underwent in his Göttingen days.

So far it does not seem that we have sufficient evidence to allow the emphasis on structural similarity to be traced back to its origins. It is tempting to recognize in it (with Lázaro-Carreter 1949:108-10) a much older influence: that of the Aristotelian-Tomistic distinction between *forma* and *materia* and that of the identification of *forma* with the *nexus partium grammaticalium*. The 'formal' criterion would first become a mark of distinction between different languages and then a criterion for genetic affinity. However, it is possible that even in this case the end of the eighteenth and the beginning of the nineteenth century signal the merging of two different traditions: a 'philosophical', theory-oriented, one and a data-oriented one which arises from the practical experience of comparative work.

Two main problems concern (a) the exact definition of the structural similarity criterion and (b) the purposes for which it is used.

The first question arises because of ambiguities in formulation. Given the morphological-syntactical slant which the criterion acquires, it could conceivably refer to similarity in grammatical categories independently of the form in which they are expressed (as in the case of e.g. Ancient Greek and Ostjak which show a similar contrast of singular/dual/plural in noun inflection). Alternatively it could refer to a functional and formal similarity of morphemes (as in the case of e.g. the Sanskrit verbal endings *-mi*, *-si*, *-ti* compared with the Greek *-mi*, *-(s)i*, *-ti*, three morphs which fulfill the same functions as the Sanskrit ones).

³⁷ For Adelung's definition of language relationship see also Jankowsky (1972:33ff.). For some reflections on the use of lexical comparison, cf. Gatterer quoted by Diderichsen (1960:106). Kylstra (1961:3ff.) offers a useful study of the work done from the seventeenth century onwards on Finnish vocabulary and its connections with that of Germanic.

We may want to ask how real these alternatives are and whether the criterion of structural similarity was in fact used in both these interpretations, but for the moment it may be convenient to leave these questions aside and to turn to the second of the problems mentioned above, *viz.* the purpose for which the criterion was used. It should be clear, by now, what this means. If structural similarity is taken in the first of the two meanings indicated, a comparison based on it may (though need not) lead to a typological and not to a genetic classification. If genetic (or genealogical) classification is what is aimed at, the second meaning should apply. Was this always clear? We are once again back to the problem mentioned in 1.6: did all nineteenth century scholars make a clear distinction between the two types of classification?

None of these questions is answered in the standard histories of linguistics, though we find occasional hints. Here we cannot deal with them at length, but it may be worthwhile to give some references to relevant authors, if only to show that the problems do in fact exist.

In the introduction to his *Mithridates* Adelung (1806-1817:I xii) argued that it was useless to give a list of grammatical 'Erscheinungen' in order to prove language kinship, since many languages may share them without being related. This (a) confirms that the structural criterion was already in operation before Adelung (as we knew) and (b) shows that Adelung understood it in the first of the two ways mentioned.³⁸ Shortly after, we find that the primary importance given by Friedrich Schlegel to the criterion leads indeed to some confusion: in his 1808 book we are told that all inflectional languages are organic and that all organic languages are related (cf. Timpanaro 1972a). The distinction between the two types of classification is there, but only in nuce; not for nothing has Schlegel been acclaimed as the founder of both comparative linguistics and typological classification (see below 3.4). At a much later stage, when the 'morphological' classification was well established, Max Müller could still claim on structural grounds that all Turanian (i.e. agglutinative) languages were related (F. Max Müller 1854). Pott's fierce rebuke (1855) is worth reading.³⁹

So much for the ambiguity; now for the clarification. Humboldt and Pott may well serve as protagonists.

In an Essay which was read to the Asiatic Society in 1828 (but published in 1830), and is an outstanding explanation of the aims and methods of comparative linguistics, Humboldt (1903-36:VI 76-84) argued that even the fundamental vocabulary cannot be guaranteed against the intrusion of foreign elements, warned against any comparison based exclusively on lexicon, and finally maintained that 'if two languages ... exhibit grammatical forms which are identical in arrangement, and have a close analogy in their sounds, we have an incontestable proof that these two languages

³⁸ Aarsleff (1967:132) refers to Jones's statement that Arabic could not be related to Sanskrit because 'Arabick ... and all its sister dialects, abhor the composition of words ...'. This is a typical example of an observation for which it is impossible to identify the underlying concept of grammatical similarity.

³⁹ Also interesting is Whitney's attempt (1867:291ff.) to argue that structural similarity (in Max Müller's sense) is not a sufficient, but an additional criterion for demonstrating language kinship.

belong to the same family'.⁴⁰ He then explained at length, with a number of examples and counterexamples, what was meant by 'close analogy in their sounds', and why this qualification was fundamental.⁴¹ The tone is that of someone fully aware that he was introducing a concept which needed clarification. Further on he proceeded to distinguish 'between the real affinity of languages, which presumes a filiation as it were among the nations who speak them, and that degree of relation which is purely historical, and only indicates temporary and accidental connexions among nations'.⁴²

It was Pott who made the distinction between 'physiological' and 'genealogical' classification into one of the leit-motivs of his life work.⁴³ Physiological classification — we are told — is determined according to 'den Besonderheiten der Structur und dem durch sie bedingten Sprachcharakter, welche sich auch bei genealogisch-verschiedenen Sprachen gleichartig zeigen können' (Pott 1833–36: I xxvi). The same points were often repeated and amplified, since Pott did not hesitate to describe in detail the task of the comparativist. That his arguments were not without influence is shown *inter alia* by the remarkable introduction which Böhlingk added to his *Ueber die Sprache der Jakuten* (1851) where terminology and concepts first found in Pott are recognizable, though here they are sharpened to an even higher level of critical awareness.

2.51 In addition to lexical and structural similarity, a third criterion, that of regular 'letter permutations', i.e. sound correspondences, is also mentioned. Rask

⁴⁰ By way of contrast we can refer to A. W. Schlegel's 1833 essay *De l'origine des Hindous* (cf. A. W. Schlegel 1846: III, 24–93, especially 64 and 82), where it is repeatedly stated that comparison must start with grammar, but the concept of grammatical comparison is never closely defined and the ambiguity persists. Ten years before the date of Schlegel's essay, Julius von Klaproth (1823: ix–x) had argued that *Stammverwandtschaft* (which he distinguished from *allgemeine Sprachverwandtschaft* which joined all languages) was mainly founded on word-similarity. The comparison of grammatical structures — he maintained — was not useless, since it helped to illuminate the development and progress of human *Geist*, but came into its own only when it served to establish the similarities or differences of dialects (as contrasted with languages). Once more the concept of *grammatischer Bau* is not closely defined.

⁴¹ Most of the same points were made by Humboldt — with more detailed explanations — in his 1827–29 work *Ueber die Verschiedenheiten des menschlichen Sprachbaues* (cf. Humboldt 1903–36: VI, 248f. and especially 250): 'Die Gleichheit der grammatischen Form in dem hier angedeuteten Sinne genommen, ist daher allein das die Einerleiheit der Sprache bedingende. Allein und für sich würde sie indess nicht hinreichen, dieselbe in zwei Sprachen zu beurkunden, wenn dabei das Lautsystem unbeachtet bliebe.' Further on he expressed much the same idea, spoke of the correspondences of 'concrete' grammatical forms, and then concluded (*ibid.* 254): 'Entkleidet man die Sprachform von ihren Lauten und lässt man bloss den Begriff, die Behandlungsart ihrer Wörter in der verbundenen Rede, in ihr zurück, so berechtigt sie durchaus zu keinem Schluss auf geschichtlichen Zusammenhang.'

⁴² It is perhaps worth mentioning that as late as 1885 K. Abel found it worthwhile to give extensive quotations of — and in practice to reprint — Humboldt's 1828 essay in his own article *Zur Frage nach den Kennzeichen der Sprachverwandtschaft* (1885).

⁴³ I am indebted to Mrs. Joan Leopold for parts of what is said here and elsewhere (cf. especially 2.61) about Pott. — As G. Pätsch (1967: 107ff.) has shown, Humboldt too had earlier made a distinction similar to that of Pott. Structural similarity — he argued — could prove membership of a linguistic class, but could not prove genetic relationship; cf. e.g. Humboldt (1903–36: VI, 254): 'Die Semitischen Sprachen stehen den Sanskritischen ... sehr viel näher, als beiden die Koptische und andre in die gleiche Kategorie gehörende, allein die Aehnlichkeit scheint doch nur eine Classenverwandtschaft, auf keine Weise eine zu Voraussetzung geschichtlichen Zusammenhanges berechtigende.'

and Grimm are normally quoted in this connection (cf., e.g., Leroy 1971:19, Mounin 1967:165, Waterman 1963:21, etc.) and in most histories of linguistics the history of the criterion appears to be inextricably linked with that of the discovery of the Germanic Lautverschiebung. In his brief sketch of earlier phonetic work, Timpanaro (1972a:78) has pointed out the arbitrariness of the method by which before the nineteenth century phonetic alternations were established for one language and then invested with general validity. Even so, he admits that some more serious work had been done, though not in the Indo-European field, and quotes with respect Turgot's statement (in his article on etymology for the *Encyclopédie*) that phonetic phenomena require historical study and that some changes are limited to individual languages. However, Sajnovics (quoted by Timpanaro) and Turgot are perhaps less isolated and less original than they seem. The contributions of J. E. Fischer have been mentioned in 2.12. In the Indo-European field we may choose to ignore the long list of scholars who anticipated parts at least of Grimm's law (cf. Arens 1969:192f., Jankowsky 1972:72, and especially Agrell 1955), but Sommerfelt (1952) has called attention to some impressive contributions by Edward Lhuys [1660–1709]. Not only do we find in his works a long list of sound correspondences between some Indo-European languages, but it is also said that it is necessary to distinguish between letters and their 'potestates' and between 'idiomatic' and 'accidental' alternations, i.e. between alternations which can be established for one particular language and alternations which recur in an accidental way. The regularity of sound correspondences is considered to be a proof of common origin. The list of precursors could be lengthened, but perhaps the most important contribution in recent years has been Diderichsen's analysis (1960:66ff.) of the direct link which joins Rask and one of them. As early as 1737 G. J. Wachter distinguished between sound alternations due to articulatory similarity and sound alternations peculiar to one given language. Rask took up and developed this principle, which is not new in him. His own thought, on the other hand, includes a number of layers, some of which are far from 'modern'. In the *Undersøgelse* published in 1818 (cf. Rask 1932–35: vol. I) three arguments which are considered equally important are advanced to prove the kinship between Greek and Icelandic: (a) the two languages (if correctly interpreted) have the same sounds; (b) they show the same rules of 'euphony'; (c) they show regular sound alternations (here the Lautverschiebung is explained). In other words, at this stage Rask does not yet distinguish between a typological statement (two languages have the same or a similar phonemic inventory and similar morphophonemic rules) and a historical statement intended to prove genealogical relationship. Yet Diderichsen (1960) has abundantly demonstrated in the course of his argument with Hjelmslev (1950–51) that genealogical relationship was what Rask was aiming at.

We may leave aside the rather tiresome question whether Grimm did or did not improve on Rask's formulation of the Germanic Lautverschiebung (cf. the opposite views of e.g. Jespersen 1922: 41ff. and Waterman 1963:20f.), but it is worth observing that Grimm, who certainly aimed at demonstrating the kinship of the Indo-European

languages, concluded his discussion of the Lautverschiebung with the statement that 'Aus dem verhältnis der consonanten geht also genügender beweis einer urverwandtschaft der verglichenen sprachen hervor' (Grimm 1822:592). The next question discussed is whether the same type of correspondence applies to the vowels.

In the history of linguistics Rask and Grimm, and after them R. von Raumer, Pott and Schleicher count as 'phonetically-minded', i.e. as interested in sound correspondences. What was the position of their contemporaries? This has not been closely discussed, or, at least, not recently. Pätsch (1960:218) rightly reproaches Delbrück for his unduly severe attitude towards Bopp's lack of rigour in his treatment of sound laws.⁴⁴ The point is that any history of neogrammarian inspiration is likely to notice the cavalier behaviour towards phonetic correspondences and not to make any further distinctions. What we should like to know for Bopp — and are not told — is how much importance he assigns to a certain type of sound correspondences as a convincing argument for genetic relationship. It is sufficient, however, to read the introduction to Bopp's 1833 *Vergleichende Grammatik* to see how all-important grammar is for him and how much sound correspondences were to take a second place, if they had a place at all. If Delbrück (1880:23f.) is unjust towards Bopp, Jespersen (1922:34) is no less unfair towards Schlegel, on whom he pours scorn for his self-imposed rule to compare only phonetically similar or identical words (cf. also Jankowsky 1972:53). Humboldt's position has been partly discussed by Pätsch (1967:107), who in spite of her close acquaintance with the sources, does not seem to make use of the 1828 paper quoted above (in 2.5). There Humboldt argued that in order to establish the common origin of two or more languages the systems of sounds and 'regular transformations of sounds' should be considered, but grammar is the decisive factor — in fact a position not very different from that of Rask. But in the almost contemporaneous *Ueber die Verschiedenheiten des menschlichen Sprachbaues* Humboldt (1903-36:VI 250) distinguished between 'dem Lautsystem in Allgemeinen, und concreten Lauten in Wörtern und grammatischen Formen' and argued that the comparison of the former did not lead very far; the real proof of kinship was given by the comparison of grammatical forms. In related languages these either appeared unaltered or almost unaltered, or could be traced back to each other 'nach aufzufinden Gesetzen' (*ibid.*: 267). In other words, as appears elsewhere, and as Pätsch has argued, Humboldt has formulated the concept of a sound law (though not, obviously, in the neogrammarians' sense), but still for him grammar remains the most important factor in proving kinship (cf. also Kovács 1971:223ff.). Why this is so, is again something which should be discussed, but the answer is probably to be found in Humboldt's concept of *Sprachform* as residing in grammatical structure (cf. Humboldt 1907-36:

⁴⁴ The reference is to Delbrück (1880:23f.) who pours scorn on Bopp's attempt at demonstrating the relationship of the Malay-Polynesian languages with Indo-European. In fact the monograph which Bopp (1841) dedicated to the subject is worth reading. Among other things it demonstrates that Bopp believed that the grammatical structure of a language could change to the point of becoming unrecognizable, and that other criteria had then to be used in order to prove genetic relationship.

VI 248), i.e. not in an empirical heuristic reason dictated by experience of comparison (though this probably played a part) but in a particular view of language and of language change.

2.6 What are the developments of the comparative method after Bopp? And how are they described by modern historiography? The second question is not easy to answer; if hitherto we could complain of a lack of attention to some specific problems, but also of an *embarras de richesse* from the bibliographical point of view, now the situation is reversed. If we exclude the general histories of linguistics, for the second part of the century we can list only some work on Ascoli and Schleicher (largely of a biographical nature), and some very recent work on the neogrammarians and the precursors of de Saussure. Steinthal, whom we shall discuss elsewhere, has recently received some attention (see below 3.71). The standard view, as represented in most histories of linguistics, has not varied greatly over the years: it sees the period between the later 1830s and the '70s as a period of increased philological knowledge of the various languages, of greater interest in phonetics, of more solid etymologizing and of slow progress towards the concept of a sound law in the stricter sense.⁴⁵ It also posits a contrast, greater or lesser, according to the historian's tendency, between the time at which the neogrammarians' influence made itself felt and the earlier part of the century. This is the period of Pott's *Etymologische Forschungen* (1st ed. 1833-36, 2nd ed. 1859-76), of von Raumer's etymological work, of Schleicher's *Compendium* (1861-62), and in more limited domains of Miklosich's *Vergleichende Grammatik der slavischen Sprachen* (1852-74), of Diez's *Grammatik der romanischen Sprachen* (1836-44), and of Zeuss's *Grammatica celtica* (1853). Mounin (1967:176) partly follows this tradition when he discusses the various historiographical attempts to distinguish a period in which comparative work was the main interest from a period geared towards historical research, and wonders where to draw the line: after the thirties, with Kukenheim, or between the sixties and the seventies, with Meillet.⁴⁶ But, as Mounin is the first to point out, definitions of comparative and historical grammar vary and in most cases it is difficult, if not impossible, to distinguish between work prompted by the one or the other interest. Two other factors complicate the question. First, this is the time of the merging of two traditions which in some cases had remained separated, that of philological work based on the examination of the texts (be they classical or Germanic or Romance) and that of comparative study. Secondly, it is also the time in which national contrasts become clearer. We may recall, for instance, Aarsleff's study (1967) and its conclusion that a new approach to philology was introduced to England

⁴⁵ The history of phonetics is dealt with elsewhere in this volume. It would be interesting to follow from the middle of the century onwards the beginning of a functional theory of phonetic distinctions: cf., e.g., Imart (1968) on Uslar, and Lepschy (1962:190, note 123) about some observations by Max Müller.

⁴⁶ In any case some work on language change as such seems to have been produced at all periods: for Bredsdorff's views see above, note 19.

by two scholars who had been in direct contact with Rask and Grimm. The German situation was very different, as we know, and the position of France, where comparative studies were introduced at a relatively late date, diverse again. In Italy too the problems discussed and the prevailing trends had their own distinctive character (cf. Timpanaro 1969).

2.61 Arens (1969:228ff.) groups Pott, von Raumer, Steinthal (or at least that part of his work which concerns classification), Curtius and Schleicher under the heading 'Der Weg zur Naturwissenschaft'. No doubt these scholars, with the possible exception of Steinthal, share a greater interest in phonology than e.g. Bopp, but here too it would probably be worthwhile to stress the differences rather than the similarities.⁴⁷

Pott may be mentioned as a test case. All too often he is remembered only because of Meillet's praise for his statement that the discovery of the Germanic Lautverschiebung was more important a result than many a philosophy of language (cf. Leroy 1971:20, de Mauro 1963:145, 1970a:89 [and 88]). Together with his etymological work (for which see Thieme 1952-53) this may have given the impression that he marks the beginning of that narrower and more specialized approach for which the linguists of the second half of the century have been so often reproached (or praised). The contrary is true, and what is so interesting in Pott is just his combination of a wide spectrum of anthropological, philosophical and linguistic concerns with a willingness to explore the methodology of his subject and its limitations. His distinction between genealogical and physiological enquiry (cf. above p. 628) and his polemics against any superficial exploitation of half-baked doctrines (cf., e.g., his attack against Max Müller) fit within this picture — as also does his determined fight against the confusion of language and race (cf., e.g., Pott 1852:508 and Pott 1856). At a time when language comparison was reaching its peak of success, he was no less aware of the limitations of genealogical classification: a comparison of syntax and style in different languages — he wrote in 1833 — may be less profitable for an enquiry into 'Völkergenealogieen, desto fruchtbarere aber für Geistes-, Menschen- und Völkercharakteristik' (Pott 1833-36:I xx).

Jankowsky (1972:83-8), who is among the few modern scholars who remember

⁴⁷ Arens (1969) ignores J. N. Madvig [1804-1886] in spite of the theoretical interest of his monographs and articles on linguistics, but this lack of interest for the Danish philologist is shared by most histories of linguistics. Earlier exceptions are Thomsen (1927:64ff.) and Jespersen (1922:84f.). More recently, Devoto (1958) has called attention to a number of motifs in Madvig's work which anticipate much later research: his interest in language change rather than in language comparison, the emphasis he put on the social character of language, the distinction he made between study of etymology and study of word-meanings, etc. De Mauro (1970a) has frequently quoted Bréal and Madvig as upholders of those syntactical and semantic interests which were shunned by most linguists 'who went the way of Pott' (but see below). Coseriu (1967b) has mentioned Madvig and Whitney in his history of 'l'arbitraire du signe', and Jensen (1958) has assessed his importance in the history of Danish philology. However, from the point of view of the history of linguistics, the most important contribution has been K.F. Johansen's lengthy introduction to his edition of Madvig's *Sprachtheoretische Abhandlungen* (cf. Madvig 1971:1-46; for Madvig see also below, note 57 and 3.10).

Pott at all, seems to agree with Arens in seeing in him an immediate precursor of Schleicher and an upholder of 'the inherent similarity between language study and the natural sciences'. But this, like Arens' heading, remains vague; if taken in a very precise sense, it is likely to be wrong. Pott, an Humboldtian, was fully conscious of the difference in methodologies required for his subject and for any scientific discipline — and he was at pains to make this explicit. In his discussion of the contrast between genealogical and physiological classification he emphasized that it was the latter which came nearer to scientific classification and at the same time he specified where the similarity lay (Pott 1856:197). Elsewhere he seemed to imply that there was no reason why languages should not move in opposite ways: e.g. not only from synthetic to analytic, but also from analytic to synthetic (1852:514, cf. also 1856:214f.). This would presumably clash with a theory of language as an organism which develops according to fixed rules: that Pott was no Schleicherian is no mystery (cf. Pott 1856:213f. about the non-independent existence of languages).

2.7 According to most histories of linguistics a rather curious combination of approaches and results marks Schleicher's contributions to the study of language. His system seems to embrace a Hegelian belief in a process of prehistoric growth followed by historical decay, a Darwinian theory of evolution, a greater rigour in the application of sound laws, the *Stammbaumtheorie*, and finally an interest in the reconstruction of *Ursprachen*. Together with this goes his claim that linguistics, or *Glottik*, is a natural science and not a form of historical knowledge (a task reserved for philology) and that all languages may be classified into three classes (isolating, agglutinative and inflectional).

The specific work about him emphasizes one or the other of these points. If we leave aside a few biographical and documentary articles (Dietze 1960, 1964, 1965a, 1965b, 1969, Fischer 1953-54, 1961a, 1961b, 1962, 1963) we are left with the 1958 dissertation by Beneš who sees Schleicher's comparative work as a continuation and systematization of Bopp's achievements and at the same time as the precursor of all subsequent further contributions in the field of Indo-European studies. In his general work she is eager to stress Humboldt's influence. In a more detailed monograph Dietze (1966) deals with Schleicher's life and work and then concentrates on his achievements in the Slavonic field. Trnka's appreciation (1952) is largely biographical and offers only a few general (but interesting) comments. Maher (1966) has rightly argued against the common belief in Darwin's influence on Schleicher and has shown that the chronological data, Schleicher's own statements about his intellectual development, and, above all, his non-Darwinian approach to the theory of evolution, conflict with this assumption. For his part Cassirer (1945:109ff.) has emphasized that Schleicher's system is built on an ontological interpretation of that organic theory of language which is at least as old as Herder (cf. above 1.5). Thus both Cassirer and Maher (cf. also Putschke 1969) appear to see in Schleicher a scholar tied in outlook and interests to the first part of the century. On the other side Percival (1969) has

stressed the continuity between some of Schleicher's general views (e.g. about language difference and general grammar) and those of scholars like Bloomfield.⁴⁸ This contrast may provide a means of assessing some of his achievements: do they point to a systematizer or to an innovator?

Pulgram (1953) has argued that the family tree model of language genealogy (i.e. the so-called *Stammbaumtheorie*) is only a schematization of the views generally held in Schleicher's time or before (cf. also Jankowsky 1972:106). Hoenigswald (1963:6f.) has assumed that both the family tree model and the techniques which Schleicher used in reconstruction show a strong influence of the techniques of textual criticism which Schleicher must have learnt during his classical training. Timpanaro (1963:73ff.), who has covered much the same ground, though in more general terms, is more doubtful, but points out that the comparison between textual criticism and linguistic reconstruction was explicitly made by Georg Curtius, Schleicher's friend and colleague. So far we have been looking backward, but other points may reverse the situation. Pisani (1949) does not hesitate to stress the inherent contradictions in Schleicher's theory, but also emphasizes his influence. Pulgram (1953), preceded by Leskien (cf. Jankowsky *loc. cit.*), points out that the *Stammbaumtheorie* still has modern validity. Timpanaro (*loc. cit.*) seems to argue that Schleicher marks a dividing line after which the development of textual criticism and that of the comparative method proceed in the parallel. More important, even the sound law question and the *Ursprache* reconstruction can appear under two different aspects. The polemics of the neogrammarians against Schleicher are well known, and equally well known is Johannes Schmidt's reply (1886) stressing how important the *Lautgesetze* were for Schleicher. These two viewpoints have their reflection in modern historiography. For Kovács (1971:235), who, admittedly, is an extreme case, Schleicher 'and his followers (Osthoff and especially Brugman) fought doggedly to make the principle of 'iron consistency' and 'exceptionless' accepted'. But for Robins (1967:183), Schleicher was not 'troubled by apparent exceptions to the general run of sound changes in the language'. Similarly, Schleicher's reconstructions have appeared as the culmination of the early nineteenth-century interest in primitive language (cf. Leroy 1971:23), but also as a preliminary to Saussure's Indo-European work (Pisani 1949).⁴⁹

⁴⁸ Unfortunately I have not been able to see Häusler's article on *Jan Baudouin de Courtenays Kritik an der Sprachtheorie August Schleichers*, quoted by Koerner (1972:65, note 8); the reference to GZH (*sic*, but GHZ?) 10 (1967) seems to be wrong. However, for the contrast between Schleicher and Baudouin see Häusler 1968 (27–33). Similarly, I have not been able to find A. Sabaliauskas 1969; Desnickaja 1971 came to my notice too late to be used here. Among the older contributions two articles by Streitberg (1897, 1909–10) are still very important.

⁴⁹ Jankowsky (1972:102ff.) has put up a spirited defence of Schleicher's reconstructions, but long before him Delbrück (1880:49), and especially Kretschmer (1896:8) had argued that it was far from certain that Schleicher did in fact attribute an historical value to his starred forms. The whole question of what is assumed to be the purpose and value of reconstruction at different moments of the history of linguistics would merit a discussion of its own. It is all too easy to give the impression that because Schleicher was the first to make constant use of starred forms he was also the first who thought of reconstruction at all, but we should not forget, *inter alia*, the intense work on Indo-European *Alter-*

The banal truth is probably that Schleicher succeeded better than anyone else in extracting the implications of a number of ideas which had been floating round for some time and to organize them into a system. The result is the impression of striking clarity which is left from the reading of any of his works, even when they are marred by contradictions and inconsistencies. Schleicher's courage and his determined attempts to carry any argument to its logical conclusion had the result that his successors could take sides for or against him in full awareness of what the points at issue were and what the consequences were of accepting or rejecting them.⁵⁰

2.71 It is regrettable that no attempt has been made to analyze the full import of Schleicher's ideas about language classification. As we have seen, his *Stammbaumtheorie* and his concepts of *Ursprache* and typological classification are often mentioned; yet it is rarely said — if at all — that Schleicher is one of the first scholars who, while being fully aware of the distinction between 'morphological' and genealogical classification, made an honest effort to propose an integrated theory which took account of them both. The starting point is Schleicher's assumption that genealogical relationship is not proved by structural similarity but by sound correspondences. This is not new: we have seen the beginning of the theory in Rask and Grimm and noted Humboldt's and Pott's contributions (cf. above 2.5 and 2.51). Practically the same statement was made by Böhlingk (1851:xxvf.). What is new is the clarity with which Schleicher proceeds to ask some other questions: if there were two languages which belonged to two different morphological classes but were genealogically related, should we be able to recognize such a case of genealogical relationship? And are there two languages in this position? According to Schleicher, the answer to the first question can be positive (since the proof or relationship is no more based on grammatical structure but on sound correspondences). The answer to the second question is negative; that is to say — Schleicher argued — since we have no examples of the phenomenon, there is no reason to suppose that it ever occurred. Schleicher's final conclusion is that genealogical relationship implies membership of the same morphological class (though the converse is not true) and that consequently the first subdivisions of each morphological class can be the *Sprachstämme*. The next step is then that of looking for further subdivisions of each *Sprachstamm* and these are to be found in the *Sprachfamilien*, which in their turn are subdivided into *Sprachen* or *Mundarten*.⁵¹ Given the framework in which it is inserted, it would then

tumskunde which started in the first part of the nineteenth century (the names of Adalbert Kuhn and Adolphe Pictet come to mind in this connection) and which by its own nature was based on some form of reconstruction (for some recent discussion of it see Pisani 1966, and for a more detailed account Schrader 1890:1–148).

⁵⁰ Koerner (1972c:216–17) has tried to make explicit the reasons which justify a distinction in the history of linguistics between a pre- and a post- 1865–70 period; among his points is the fact that Schleicher's *Compendium* (1861–2) represented 'the synthesis of the Indo-European linguistics of the first two thirds of the 19th century' (cf. also Putschke 1969).

⁵¹ Most of this paragraph is based on the first chapter of the introduction to *Die deutsche Sprache* (Schleicher 1869); some other points will be discussed later (cf. 3.7).

appear that the family tree model has both a genealogical and a typological value. In other words, the members of a language family are joined by a common historical origin and by a number of typological features which they share. This is worth pointing out, because it is probably at the origin of a number of methodological hesitations which accompanied the attempts at sub-grouping the Indo-European languages.

However, even when the typological implications were ignored, the family tree model served to give a graphic representation of a series of problems which were being discussed at the time. The question was no longer: how do we prove genetic or genealogical relationship? It had become: in a group of related languages, how do we conceive of the relationship of one language to the others? The same problem could be put in dynamic terms: how does it happen that any *Ursprache* or any language splits into more than one language or dialect?⁵² As became clear later, the *Stammbaumtheorie* could serve as a model for language classification and also as a model for the process of language change and differentiation. In some of Schleicher's formulations it also provided a concrete representation of the development and prehistory of the Indo-European languages. It could be tested (and attacked) from at least three different angles, and the following developments showed that the opportunity was not missed.⁵³

2.8 If we try to follow the history — and the historiography — of the comparative method and of genealogical classification after Schleicher, we soon realize that there are far too many independent (but related) strands. They can be pursued along a number of different lines.

First, the very existence from the early 1860s onwards of such works as Max Müller's *Lectures on the science of language* (1861 and 1864), Whitney's *Language and the study of language* (1867), *Life and growth of language* (1875), Paul's *Principien der Sprachgeschichte* (1880), and Gabelentz's *Sprachwissenschaft* (1891) — to quote only a

⁵² Pisani (1959:14) has pointed out that the theory involves a basic ambiguity. It was based on the concept of language as an organism which develops according to inherent laws, but this could hardly explain the differentiation of languages. To account for it Schleicher was obliged to appeal to geographical differences, but that meant that he had to reintroduce those historical considerations which he wanted to avoid. The contradiction is even stronger than it appears. Schleicher could not allow historical factors to play a part in language change because, if he had done so, he would have destroyed that absolute overlapping of typological and genealogical classification which he had proposed. As long as language change was gradual and 'organic' (as Schleicher wanted it to be), two dialects which arose as the result of a language split were bound to be typologically near to each other. However, if historical factors were allowed to have a determining influence on language development, then it became conceivable that in particular circumstances one of these dialects could alter up to a point at which it was typologically closer to another dialect with different origins.

⁵³ Maher (1966) has argued that the actual image of a family tree with its branches is too banal to require any particular explanation, and that there is no reason to suppose that Schleicher borrowed it from Darwin or Häckel. The last point is certainly correct. As for the first point, the question of possible influences from textual criticism has been mentioned before, but here it may be opportune to remember that, whatever the origin of the procedure of mapping languages on genealogical trees, it was already established by the first part of the century: cf. for instance the table of languages presented in Klaproth's *Asia polyglotta* (1823: near p. 217).

small number of works which came from different and often contrasting backgrounds — shows an interest in linguistic theory and in the systematization of current methodology.⁵⁴ The psychological view of language, which was explicitly advocated by Steinthal and then received far more general acceptance in the last quarter of the century, comes under this heading, as also does the continuing interest in Humboldt, exemplified by the works and editions of Steinthal and Pott.

At the same time a number of contributions of more specific theoretical interest start appearing: for the last part of the century the names of Bréal, Schuchardt, Kruszewski and Baudouin de Courtenay come to mind, but any choice is invidious. It would be useful to distinguish the work of those more theoretically minded scholars who proposed new ways of looking at language and pointed out new paths to explore from the work of those who were more interested in methodological clarification and in an exact definition of discovery procedures. The distinction is not easy to draw, and it is possible that this difficulty accounts for the different judgement passed on some linguistic trends. For some the end of the century is a time of retrenchment and disarray, for others it is a period of excessive confidence in the historical side of linguistic work. At any rate it is certainly true that in various ways and at various times we can recognize the first signs of a movement which will lead to the main currents of study of general linguistics: Saussure and the beginnings of structuralism on the one side, individualism and idealism on the other side.

It may well appear that these trends have little or nothing to do with the history of genealogical classification; that it is not so can be shown by referring to the work of some of the relevant scholars. First, a less well known writer: in 1881, the French scholar, Lucien Adam (1881a) started an article on *Les classifications de la linguistique* by saying that in rereading Whitney and Max Müller he had come to the conclusion 'qu'il est devenu nécessaire de soumettre dans notre Revue à l'épreuve de la discussion les points fondamentaux de la science du langage.' Secondly, Schuchardt, who is regarded as one of the founders of individualism in linguistics, is also the man who started the harshest attack against the comparative method and argued that no real classification of languages was possible, since by nature languages were non classifiable entities. Much later on Trubeckoj, with whom we are well within the domain of structuralism, tried to vindicate typology as a classificatory criterion and to introduce it into the traditional domain of genealogical classification, as the only adequate means of defining the Indo-European languages (Trubeckoj 1939). At this point — which unfortunately goes beyond the limits of this survey — it may well be said that the history of classification has come full circle since Friedrich Schlegel.⁵⁵

⁵⁴ Perhaps we should add to our list at least Sayce's *Principles of comparative philology* (1874) and Hovelacque's *La linguistique* (1876). The latter book had some importance in popularizing Schleicher's ideas about linguistic classification.

⁵⁵ Most of the points made by Trubeckoj were anticipated by Baudouin de Courtenay in a series of works which started in the last quarter of the nineteenth century and continued through the first decades of the twentieth century: cf., e.g., Baudouin de Courtenay 1972:312ff. [1884], 219ff. [1901], 296ff. [1930].

Closely related to these general studies — and at the same time to the problems of classification and dialectology which we shall mention below — is the scrutiny to which the very concept of language was subjected. Schleicher's work had brought the organic view of language (in the ontological sense) to the point where compromise was no longer possible. Above all it had shown what the consequences were of endowing language with a life of its own. A few scholars accepted his conclusions, but a number reacted against them. In various ways this reaction diverted attention from language as such towards the speaker. In this respect even the sociologically minded like Whitney, the 'mentalists' like Paul, or the individualists like Schuchardt could come to some measure of agreement. A formula which very few would have disclaimed is probably that of Paul (1880:232 [=1920:37]): 'wir müssen eigentlich so viele sprachen unterscheiden als es individuen gibt'.

A second line of investigation could follow the increasing interest in dialect studies through to the creation of an almost independent discipline, which reached its acme in the dialect geography of the twentieth century. Even at an earlier stage its impact cannot be underestimated. The interest in the study of some more modern linguistic phases is often quoted as one of its results, but there is more. Once again, a different definition, or perhaps a special definition of language, became necessary. How was it possible to distinguish one dialect from another? Or, for that matter, a dialect from a language? What indicated the boundaries? And what did it mean to say that a language split into dialects? Here too attention was drawn to the problems of classification and to the how and why of language change.

Thirdly, there was a new widely spread interest in experimental and descriptive phonetics. We may leave this point aside, since the history of phonetics is discussed elsewhere in this volume, but we must remember that most of the theoretical discussions which went on during the last quarter of the century would have been unthinkable without this particular background.

Fourthly, we should perhaps isolate (though it would be a somewhat artificial separation) that particular set of problems and discussions which went together with the so-called *Junggrammatische Richtung*. It was certainly not divorced from contemporary currents of theoretical thought (from which it absorbed a great deal and to which it contributed substantially), but it merits a place of its own because of the immediate and powerful impact that it had on the theory and practice of linguistic theory and comparison.

Fifthly and sixthly, there are special problems which concern genetic classification in particular. The need for a classification of the languages of the world was still there — and was more and more pressing. The material available increased daily, and anthropologists and ethnologists looked to linguists for a reliable and useful classification. This very fact encouraged a genealogical rather than a typological classification, but the emphasis was also determined by other reasons. On the one side the natural development of the subject was suggesting new attempts to demonstrate a genetic relationship between some of the linguistic families previously established.

The work of R. von Raumer, Ascoli, and, later on, Hermann Möller on Semitic and Indo-European is well known. For much of the last century it was still possible to hope that more sophisticated comparative work could drastically reduce the total number of families and eventually even create a gigantic family tree demonstrating the original kinship of all languages and affording a satisfactory and complete classification.⁵⁶ On the other side, some of the more theoretically minded scholars (Whitney and Pott could serve as examples) seemed to be moving more and more towards genetic rather than typological classification, although they certainly did not believe in linguistic monogenesis. The reasons for this would need a much deeper exploration than can be offered here (but *à propos* of Whitney see Terracini 1949:88f. and 94f.). An additional point which would be worth considering is the extent to which the views of linguists (even when they were not followers of Schleicher) were influenced by Darwin's criticism of the classifications used in the natural sciences and by his argument that it made little sense to speak of a natural classification which was not genealogical (Darwin 1859: chapter 14).

At a different level the attention of Indo-European scholars shifted towards a problem which had come to concern them directly. The kinship of the Indo-European languages did not call for further demonstration, but their internal classification needed more extensive discussion at both a theoretical and a concrete level. The question concerned, in equal measure, the classification of the Indo-European languages, of the Romance languages, of the Germanic languages, etc. and that of any smaller group of dialects. Yet, any study on the problems of sub-grouping was bound to have major repercussions on most problems about language relationship. We thus return to Schuchardt's criticism of the concept of *Sprachverwandtschaft*.

2.9 What is the position of recent historiography? In this survey we can only refer briefly to some of the lines of enquiry mentioned above. The first two in particular will receive only very summary treatment, and the third will be ignored.

Few of the general works have been studied for their own sake (for Becker see above p. 613). Likewise, the initial links between psychology and linguistics (Steinthal, Lazarus, Geiger) and the varying attitudes of scholars like Scherer, the neogrammarians' group (including Paul) and then Wundt (including the polemics between Delbrück and Wundt), have also been largely ignored by historians of linguistics (but for Steinthal, see below 3.71): two recent books by psychologists (Esper 1968 and Blumenthal 1970; cf. also Alkon 1959) are mostly concerned with the later period and with the links between this earlier tradition and the psychologism of the young Bloomfield, though Esper makes a number of very interesting excursions into the second half of the nineteenth century.

Even the recent anthology of Whitney's writings (Whitney 1971) does not offer a complete analysis of his thought, in spite of a brilliant essay by Roman Jakobson on

⁵⁶ The attempt was, of course, made; here it should be enough to refer to Trombetti's work (cf. Assirelli 1962).

other scholars' reactions to Whitney (*ibid.*: xxv-lxv) and of a very interesting introduction by Silverstein (*ibid.*: x-xxiii), whose main defect to my mind is that it does not sufficiently stress the influence of earlier scholars on Whitney and the effective points of contact which he had with his adversary Max Müller. Terracini's chapter on Whitney (1949:73-121) is still the most complete treatment and the only one which underlines both the new and the old features of Whitney's thought. For the connections with Saussure, cf. also de Mauro (1970b:299ff. and 327ff.).⁵⁷ Paul — to quote another great name — is mentioned in all works on the neogrammarians (cf., e.g., Jankowsky 1972 *passim*), but we have no complete analysis of the *Prinzipien*. Two very different (and very recent) contributions should be mentioned. In a paper which is mainly concerned with the theory of language change and not with the history of linguistics, Weinreich, Labov, and Herzog (1968) have offered a very closely documented discussion of Paul's theories about linguistic change; it is a brilliant piece of work in spite of some disputable points. Even more recently, Koerner (1972d) has tried to show once more that there is a direct link between Paul and Saussure (while denying the often postulated influence of Gabelentz on Saussure) and has offered an interesting examination of Paul's views with special reference to his attitude towards historical and descriptive linguistics (cf. also Kandler 1954:15 and Lieb 1967:22f.).

Georg von Gabelentz has both gained and suffered from the attempts to connect him with Saussure. They have rescued him from the almost complete obscurity into which he had fallen, but at the same time they have called attention to some particular parts of his work rather than to others which are equally deserving.⁵⁸ For the most detailed comparisons of the views of Saussure and Gabelentz we can refer to Rensch (1966) and Coseriu (1967c): for some reactions see de Mauro (1970b:xxii-xxiv) and Koerner (1972d). Christmann (1972) has tried to establish a link between Humboldt and Gabelentz and Gabelentz and Saussure, while in a paper which is more a programmatic declaration than a piece of historiography Lohmann (1967) has aimed at stressing the differences between Humboldt and Saussure.

A number of contributions, among which are some of those just quoted, arise from recent exploration of the sources of Saussure's thought and of his possible precursors: for a general account see de Mauro (1970b:347ff.) and for a complete bibliography Koerner (1972c). Principles such as that of the arbitrariness of the sign (cf. Coseriu 1967b, de Mauro 1970b:348ff., Koerner 1972c:128ff.), or dichotomies like those between synchrony and diachrony (cf. de Mauro 1970b:350, Koerner 1972c:97ff., Christmann 1971, 1972, Koerner 1972d, Lieb 1967, Scheller 1968 etc.),⁵⁹ between

⁵⁷ The similarities between Whitney's views and Madvig's would deserve a separate treatment; for some hints cf. Johansen's discussion (Madvig 1971:37, 468ff.).

⁵⁸ His thoughts on linguistic typology come to mind; cf. Gabelentz (1894) and see below, notes 124 and 129.

⁵⁹ As de Mauro and Koerner make clear, a number of scholars have been quoted as precursors of this distinction: Humboldt, Grimm, Comte, Ascoli, Kruszewski, Baudouin de Courtenay, G. von Gabelentz, Masaryk, Paul, Marty, Liebich, Schuchardt, etc. For the sake of completeness — and not because we have much faith in what has become a popular pastime — we could add here the names of

langue and parole (cf. de Mauro 1970b:350, Koerner 1972c:112ff.),⁶⁰ and between sound and phoneme (cf. de Mauro 1970b:352ff. and 401f.) have all been explored with regard to their origin. In this way we have indirectly gained a general view of the pre-Saussurean theories. It has also been shown that the all-important distinction between synchrony and diachrony had in fact been formulated in various ways (admittedly not always as impressive as that of Saussure) well before the publication of the *Cours*. This fact, which obviously does not detract from the importance of Saussure, is important for a correct understanding of the work done in the three last decades of the nineteenth century. It is all too easy to give the impression that the emphasis was on historical linguistics simply because no other type of work was conceived of; this is as wrong as the often repeated statement that all the work of the nineteenth century is atomistic in nature because of a lack of interest in synchronic studies and of the absence of the concept of system (against the opposite exaggeration see Buysens 1961).

Here it would be useless to refer to all the theoretical contributions of scholars like Marty, Noreen, etc. who have been quoted in connection with particular trends of twentieth century linguistics. More to the point, however, is a mention of recent historiographical interest in the so called Kazan school and in its exponents, Kruszewski and Baudouin de Courtenay. Most Western histories of linguistics barely pay lip service to them, but Graur-Wald (1961:54ff.), Ivić (1970:97ff.) and especially Zvegincev (1960:I 197ff.) give a more complete account. A far more detailed treatment is in Berezin (1968:100ff.); for a general introduction see Jakobson (1971b). The connections between Baudouin de Courtenay and Saussure have often been discussed (cf., e.g., Benveniste 1964:129-30, Leont'ev 1966, Slusareva 1963, and the general account of de Mauro 1970b:306-8). Yet, we cannot forget that both Kruszewski and Baudouin started as Indo-Europeanists and that part at least of Kruszewski's work had a definite influence on Paul's *Prinzipien* (to give a single example).

Indications about recent work on Kruszewski can be found in Berezin (1968) and in Koerner (1972d:294ff.); for an appreciation of his work see Jakobson (1965) and for an edition of his writings Kruszewski (1967) with Kurylowicz's introduction (*ibid.*: iii-ix), and the edition announced by Koerner (cf. Koerner 1972c:278 no. 1746). The Russian, Polish, etc. literature on Baudouin has by now reached unmanageable proportions; it would be useless to give the details here or in the bibliography, since most of it can be easily traced through the 1960 Russian collection of essays about him (Baudouin de Courtenay 1960), the Russian anthology of his works (Baudouin de Courtenay 1963), and the very recent anthology translated into English and edited

Thurot (see Coseriu 1967a) and Savigny (cf. Ginschel 1967:15). It is odd to see that Henry Sweet is not often mentioned in this connection (but cf. Trim 1959 and for Sweet see Jakobson 1966 and Wrenn 1946). An interesting essay is that of Christmann (1971) on the attitude taken by the *Grammaire des grammaires* to historical and descriptive linguistics.

⁶⁰ To the references given in de Mauro and Koerner, add Coseriu (1976a), who refers to Hegel for a similar distinction, and Bumann (1965:42f.), where the similarities between Steinthal and Gabelentz and Gabelentz and Saussure are again emphasized (see also Belke 1971:cx).

with a long and important introduction by E. Stankiewicz (Baudouin de Courtenay 1972).⁶¹ Further information can be obtained from Häusler's excellent monograph on phonetics and phonology in Baudouin (Häusler 1968, cf. also Koerner 1972b), and from Berezin's study of the history of linguistics in Russia (1968:100-50).⁶²

2.91 Linguistic geography may be left aside since it belongs to a later period than that in which we are interested, but dialect studies started well before the twentieth century and played an important part in the development of linguistic thought. General information is provided by most histories of linguistics: cf., e.g., Arens (1969:329-37 and 765f., 793f.). Particularly rich in detailed accounts are the histories of Romance philology (cf., e.g., Iordan-Orr-Posner 1970:144-278, 465-72). Other useful sources of evidence are provided by the general books on linguistic geography and dialectology (see especially Pop 1950-51). An up-to-date survey can also be found in the collective volume on *Sprachatlanten* which was published a few years ago (*Sprachatlanten* 1969).

A shorter account of the earlier collections of dialect material (especially in Germanic territory) and of the beginning and development of dialectology from the end of the eighteenth century onwards is given in Freudenberg (1965; cf. also Gerhardt 1947).⁶³ Some interesting factual articles have concentrated on the first *Idiotika* and the dialectological interests of the brothers Grimm: cf., e.g., Niemeyer (1962; on G. Landau), Schoof (1941, 1963/4 and 1967; on Grimm), Sonderegger (1967; on Heusler), Studer (1954; on F.J. Stalder), Wanner (1963; on Grimm). Malkiel (1967-68) has drawn attention to an earlier Romance dialectologist, August Fuchs; for the later work in the field cf. Kuhn (1947-48).

A detailed discussion of this and other work would be impossible, but some historiographical trends must be mentioned. In Italy there has been a great deal of research on Graziadio Isaia Ascoli and the many aspects of his activity; his general views on language, his substratum theory,⁶⁴ his work on dialects and his position in the neogrammarians controversy. A number of new documents offer an inside view of the problems of the time (cf. de Felice 1954 for Ascoli's terminology, Bolelli 1963, Conti Peca 1965, Cronia 1962, Faré 1964, Gadzaru 1967, Prosdociami 1969, Timpanaro 1959, etc. for letters and other evidence). In addition to some interesting

⁶¹ This is the most recent publication on Baudouin de Courtenay but the bibliography is not meant to be complete and some omissions (like that of Häusler 1968) are odd. It is also a pity that Stankiewicz has missed the chance of giving a complete list of the recent publications of documents, letters, etc. by and about Baudouin. For Häusler's article on Schleicher and Baudouin see above, note 48.

⁶² I have not mentioned here F. F. Fortunatov and the so-called 'Moscow school' because in the nineteenth century its influence outside Russia seems to have been very limited. For some basic information cf. the Russian anthology of Fortunatov's writings (Fortunatov 1956-57) with Peterson's introduction (*ibid.* I, 5-16), Zvegincev (1960: I, 197ff.) and Berezin (1968:28-99). There are a number of articles on Fortunatov, mostly in Russian publications (but see Vasenco 1960): cf., e.g., Kacnel'son 1954, Kert 1965, Tolkačev 1964, Ščerba 1963, Žirmunskij 1953.

⁶³ For a general bibliography on Swiss dialectology cf. Sonderegger 1962, which also includes some historiographical information.

⁶⁴ Most histories of linguistics give some information about the further development of the substratum theory; cf. also Jarčeva 1956, Nemirovskij 1956.

work by Terracini, which stretches almost over half a century (most recently see Terracini 1949:123-47, 1956, 1966 and 1967), and some chapters included in more general books (e.g. Vitale 1955:118-24 and Várvaro 1968:104-16) the most penetrating contributions have come from Timpanaro's studies (cf. especially Timpanaro 1969:229-357, 419-24, and 1972b).⁶⁵ The picture which emerges is that of a scholar who is much nearer to Schleicher and the neogrammarians than has been claimed by his later idealistic followers.⁶⁶ What we still do not know is how great Ascoli's influence was outside Italy (but see Gadzaru 1967), if we except the general favour with which some of his concrete results in the field of Indo-European reconstruction were received. However, the reasons why Ascoli is mentioned in this section rather than elsewhere are provided by his later studies of Romance dialects, and by his persistent faith in a linguistic (rather than ethnic or political) definition of dialects and in the possibility of their classification on linguistic grounds. This view, which offers an interesting contrast with the scepticism of Schuchardt, provoked the attacks of Paul Meyer and Gaston Paris (cf. Várvaro *loc. cit.*), thus starting the discussion about the definition and classification of dialects which was to continue for a long time after the 1870s and which can be regarded both as the origin and the result of a greater interest in descriptive techniques and in the study of language change.⁶⁷

A further enquiry into dialect studies would bring us to discuss, on the one hand, Schuchardt (for whom see below) and Gilliéron (for whom see the collective volume Pop and Pop 1959 and the sobering remarks of Malkiel 1967a and 1957:10 = 1968:213f.), and on the other hand Rousselot and the new data of experimental phonetics. We should also mention the beginning of field work and the first linguistic atlases which start appearing towards the end of the nineteenth century. All this must be left aside, if the limits of this survey are not to expand to impossible proportions. It may be useful, however, to conclude this paragraph with a note of warning. All too frequently general histories of linguistics give the impression that nineteenth century dialect study inevitably led to a drastic revision of the tenets of genealogical comparison

⁶⁵ I have ignored here the part played by Ascoli in the Italian 'questione della lingua'; for a general bibliography on the subject cf. Ascoli 1968, where Grassi reprints some of the relevant texts, and Timpanaro 1969 (xxivff.).

⁶⁶ This is essentially Timpanaro's view: some comments about the differences between Ascoli and the neolinguist Bartoli had been previously made by W. von Raffler (1953).

⁶⁷ In spite of the large number of monographs and articles about him, I have hesitated to devote even a page to Ascoli in a survey which is obliged to ignore works like those of Raynouard, Diez and Meyer-Lübke, and does not mention any of the earlier Italian scholars (Cattaneo, Biondelli, etc.). Some points may justify my decision. First, as Timpanaro has shown, so many linguistic trends merge in Ascoli that a study of his intellectual biography is almost paradigmatic for the period. *Inter alia* Ascoli shares with Johannes Schmidt the honour of being claimed as precursor by two very different schools; in the case of Ascoli the reference is to the Italian neogrammarians and the Italian 'neolinguisti'. Secondly, Ascoli's work is a good example of that phenomenon which we mentioned before: the different coloring taken by linguistics in various countries. Thanks to Aarsleff (1967) and to Timpanaro (1969) we are now quite well informed about Britain and Italy, i.e. two countries which were not in the forefront of linguistic developments (for France see Kukenheim 1966, for Russia Berezin 1968, Vinogradov 1958, for Spain Lázaro Carreter 1949, Mourelle-Lema 1968).

and therefore stood in strong contrast to it. As we have seen this can scarcely apply to scholars like Ascoli who, as Malkiel put it (1957:9 = 1968:213), elevated dialect studies to 'the rank of a respectable scholarly endeavour'. It applied even less to those who are considered the traditional enemies of dialect geography, the neogrammarians. Some of the first detailed enquiries into living dialects (e.g. Winteler 1876) were quoted by them in support of their thesis, and we must not forget that some of the most thoughtful phoneticians (Sweet, Sievers), who were directly involved in the study of living languages, were more for them than against them.

2.92 These observations bring us to our fourth line of enquiry: the neogrammarian school. The relevance to our main theme is clear; there was a time at which comparison and neogrammarians' work were almost synonymous. In spite of this — or perhaps because of it — few other nineteenth century movements have had a more curious historiographical reception. The neogrammarians have not been forgotten, but historians of linguistics either have treated them with undisguised and uncritical admiration (for a part of the American tradition cf., e.g., Hockett 1965:186ff.), or have underlined the concrete results (Verner's law, the nasal sonants, etc.) and forgotten the theory, or have branded the whole school with total disapproval (cf., e.g., Jordan in Jordan-Orr-Posner 1970:18). In spite of some attempts at conciliation, until very recently it has been difficult to speak about the neogrammarians without implying a quasi-moral judgement; in some academic circles the very word 'neogrammarian' has assumed particular connotations. The result has been in most cases a remarkably one-sided view. What historiography most needs now is a series of attempts to investigate both the neogrammarians' concrete achievements (about which much is known) and their theoretical presuppositions in their entirety (about which we are far less clear), to compare the two, and to set them in some sort of historical perspective.

Some very recent work has moved in this direction: cf. for instance the article by Putschke (1969) and the monograph by Jankowsky (1972).⁶⁸ In order to account for the basic ideas of the movement Putschke has felt the need to go back to the beginning of the century; Jankowsky has taken the enquiry back to Francis Bacon, but has perhaps lost in depth what he has acquired in the temporal dimension. Putschke firmly denies that the neogrammarians brought about a breakthrough in linguistic science (cf. for this view Hockett 1965, Robins 1967:186) and is ready to underline the motifs and principles which they inherited from their predecessors. According to him the school marks the confluence of the psychological and physiological approaches which linguistics had been developing from the beginning of the nineteenth century. The immediate influences were those of Scherer, with his concept of analogy, and of Schleicher, with his concept of sound law. Jankowsky is equally eager to stress the importance of Scherer and Schleicher, but also wishes to emphasize the difference between them and the neogrammarians. Both authors — to my mind — do not sufficiently explore the psychologism of the second half of the century, and in particu-

⁶⁸ About the neogrammarians cf. also Meriggi 1966, Pulgram 1955, and Raven 1954.

lar do not concentrate enough on Steinthal. Perhaps as a result of the anti-mentalistic attitudes of parts of twentieth century linguistics, they do not seem too eager to enquire how important psychology was in the neogrammarians' system.⁶⁹

Both works have obvious theses to defend. Putschke argues that the neogrammarians were slowly moving in the direction of a diachronic structuralism and that this attitude descended directly from their methodology, i.e. from their theory of analogy and their theory of sound change. The thesis may appear paradoxical, in view of the constant allegations of atomism, but has much to be said for it, as Putschke has shown. Jankowsky is far more eager to demolish some of the criticism of the neogrammarians and to give them credit for their achievements: the refinement of comparative and historical methodology, the interest in formal analysis, and the collection of an enormous amount of data.

A historiographical problem, which is not often discussed, concerns the subsequent fortunes of the neogrammarians: why did they pass into history almost exclusively as supporters of the principle of regularity of sound change, while no mention is normally made of their other theories? Yet some other points at least should be mentioned: their fight against Schleicher's concept of organism with their determined attempt to bring language back into the domain of history rather than in that of the natural sciences; their self-avowed mentalism; their uniformitarian principle, according to which language develops at all times according to similar rules, and there is no question of a period of growth followed by a period of decay; and their interest in modern phases of languages as the best source of information about what we could call diachronic universals.⁷⁰ That none of these tenets is entirely original hardly

⁶⁹ Esper (1968) gives a detailed analysis of the controversy which took place between Delbrück and Wundt at the beginning of this century, but also offers important information about the psychological views of Paul. For the earlier period, rather than refer to the famous preface to the *Morphologische Untersuchungen*, written by Brugmann when he was not yet thirty, it is better to emphasize the importance of Brugmann's inaugural lecture in Freiburg (cf. Brugmann 1885:33ff.) with its clear analysis of the tenets which the neogrammarians accepted from their predecessors (above all Steinthal). Gadzaru (1967:58) has published a letter which Brugmann wrote to Ascoli in 1887 and in which it is stated that the great controversy which followed the neogrammarians' declarations did not centre so much 'um solche Fragen ... welche die Verbindung der Sprachforschung mit der Sprachphysiologie betreffen, als um solche, welche sich auf das Verhältniss der Einzelforschung zur Psychologie beziehen'.

⁷⁰ A problem which calls for some attention concerns the position of the young Saussure; how close was he to the neogrammarians' views? His first book has been analyzed in detail by Vallini (1969) who has tried to look at it from the point of view of the *Cours*. De Mauro (1970b:292ff.) appears inclined to stress the differences between Saussure and the neogrammarians, both from the point of view of the concrete results (e.g. in the theory of Ablaut) and from that of Saussure's interests and theories. To my mind, some of these points have been carried too far; an interest in syntax is not altogether unknown in the school to which Delbrück belonged, so that Saussure's dissertation on the genitive absolute in Sanskrit is not altogether isolated, the influence of Whitney (which according to de Mauro is very recognizable in Saussure) is officially acknowledged by Brugmann in more than one occasion, and as for the journey to Lithuania, which according to de Mauro is a proof of Saussure's interest in living languages and in descriptive linguistics, this must have been almost de rigueur for a budding comparativist at some stage (cf. the parallel enterprises first of Schleicher and then of Brugmann and Leskien). In any case the neogrammarians had vociferously claimed that the study of

matters: perhaps the greatest single achievement of the neogrammarians is to have shown that even a data-oriented movement needs a coherent methodology and that such a methodology is only possible within an explicit theoretical framework. It is not worth while to discuss here the ambiguities which existed in the theory (Putschke 1969 has mentioned some of them); they do not detract from the importance of the attempt.

We shall return later to the neogrammarians and to their views about sub-grouping (cf. 2.941). Here we must still examine their attitude towards the problems of language classification. Once again, this has not been formally explored, but some facts at least are obvious. Schleicher's elaborate construction could not be accepted by the supporters of the uniformitarian principle. The neogrammarians were bound to reject in toto any theory which combined morphological and genealogical classification at the cost of postulating a prehistoric period of linguistic growth followed by a period of historical decay. More important, their whole attitude to the problems of classification was different. On the one side, these were bound to appear less urgent, since linguistics was no longer to be treated as one of the natural sciences, nor language as an independent organism. On the other side, the primary task for the neogrammarians had become the exploration of language change. This called for an intensive analysis of the history of individual languages or language families, but the aim was that of tracing a historical process, not of arriving at a classification.⁷¹ From this angle, too, typological classification receded into second place; the interest in comparison and in the comparative method remained — all the more so since it seemed that for the first time it was put on firm methodological basis — but of course the bias was different. The result, however, was that the new rigour employed in the study of language change was reflected in the practical work of language comparison and reconstruction. The data speak for themselves: in the course of a few years the neogrammarians and their contemporaries almost completely altered the view of Indo-European that they had inherited. At the same time a clearer understanding of the methodology used meant also a clearer understanding of its limitations: it is noticeable that Brugmann's

living languages was as important as that of the ancient Indo-European languages, if not more so. It is possible to wonder whether at the time in which Saussure was in Leipzig the disagreements — or rather the lack of sympathy — between him and his German contemporaries really depended on profound theoretical divergences rather than on differences in temperament and inclinations. Another problem which should be discussed in the context of any work on the neogrammarians concerns the numerous coincidences between their views and those of the young Baudouin de Courtenay (and perhaps of Kruszewski).

⁷¹ In the paper which they wrote to celebrate the 100th birthday of Bopp, and which was published in the first number of the *Indogermanische Forschungen*, Brugmann and Streitberg (1891: vii) stated: 'Die wahre Aufgabe der indogermanischen Sprachwissenschaft — daran lässt sich nicht zweifeln — besteht viel mehr darin den gesamten Entwicklungsgang der indogermanischen Sprachen von den dunkelsten Zeiten ferner Vergangenheit bis zum hellen Tage lebendiger Gegenwart zu durchforschen und die Gesetze aufzudecken die seine Richtung bestimmt haben.'

reconstructions of Indo-European forms are far less daring than those of Bopp or Schleicher.⁷²

2.93 Concrete work about genealogical classification has attracted little attention. The motives which determined very different scholars (e.g. Rudolf von Raumer, Ascoli, Möller, Trombetti, etc.) to explore the possibilities of linking the Indo-European languages with other language families still need exploring; at present we know little about them, and even less about the methodology used in each case. Similarly, there has not been much discussion of the attempts to provide a genealogical classification of all languages of the world. The books of Fr. Müller and F.N. Finck (published in 1876–88 and 1909 respectively) where all language families are listed and then grouped according to racial criteria, are sometimes mentioned (cf., e.g., Tagliavini 1963:415 ff.), but almost exclusively in order to deplore this connection of language and race. Here too a number of distinctions should be drawn. The spirit in which the anthropologist and ethnologist Fr. Müller undertook his classification was certainly different from that of the linguist Finck, who was more interested in typology (see below 3.81) and for whom a genealogical classification (presented with all possible hesitations and qualifications) was very much a preliminary step to some more important work. Other works which are on the borderline between linguistics and anthropology (cf., e.g., Maury 1857 and perhaps Pott 1884–90 with Pott 1887) have not even been mentioned in the relevant literature (but for a list of these works cf., e.g., Meillet-Cohen 1952 and Veith 1969).

2.94 The problems of sub-grouping have aroused more interest. In the 'Geschichte der Forschung' section of his book on *Die Gliederung des indogermanischen Sprachgebiets*, W. Porzig (1954:17–52) has given a detailed history of the controversies over the internal classification of the Indo-European languages and the related questions of Indo-European reconstruction and *Altertumskunde*.⁷³ The facts are all there, but because of the purpose of the book, no attempt is made at any general historiographical statement. Even so, Porzig's analysis is a healthy reminder of the weight that concrete problems may have in the evaluation of theoretical positions. Any scholar who wanted to give an account of the differentiation of the Indo-European languages from the *Ursprache* to modern times and to use for this the family tree model had to answer questions such as: to what branch do the Slavonic languages belong? Are they closer to Indo-Iranian or to Germanic? If these and similar problems could not be solved in spite of what appeared to be a wealth of material, attention was redirected towards the model, and its very validity could be challenged. It is probably within this framework that we should consider Johannes Schmidt's criticism of the *Stammbaumtheorie* and his own alternative proposal for a model (the so called *Wellentheorie*) which laid the stress on the horizontal rather than on the vertical

⁷² It is just this paradox — of apparent retrenchment in some fields, balanced by advance in comparative and historical method — which may account for the ambivalence of attitude (still discernible) towards the achievements of the neogrammarians.

⁷³ A useful analysis of the early discussions about classification of the Indo-European languages from Schleicher onwards is to be found in Schrader (1890:68–110).

spread of linguistic features and on geographical continuity rather than on genealogical transmission (cf. also Terracini 1949:218). Schmidt's pamphlet on *Die Verwandtschaftsverhältnisse der indogermanischen Sprachen* (1872) is constantly mentioned, but especially among Romance philologists, priority for the *Wellentheorie* is often given to Schuchardt rather than to Schmidt (cf. Jordan in Jordan-Orr-Posner 1970:52 note for an account of the question, and see Höfler 1955:30, Gadzaru 1967:144-64, Malkiel 1955-56, 1968:18). However, we miss in general historiography a discussion of some different, though interlocking, problems.

First, it is often not realized that in their simplest form the questions of sub-grouping are as old as the beginning of genealogical comparison. The decision taken by some of the earliest comparativists that Sanskrit was one of the Indo-European languages and not the mother tongue of all of them comes under this heading. Similarly A.W. Schlegel's 1818 attack on Raynouard's thesis that all Romance languages derived from Old Provençal revolves round a problem of sub-grouping. As early as 1833 Pott felt the need to stress the difficulties inherent in this field (cf., e.g., Pott 1833-36:I xxviii ff.). Later on he argued that within a group of genealogically related languages further distinctions could only be based on the different grades of relationship, but gave a warning against confusing human and linguistic kinship. The worst complications — he maintained — were likely to arise in the attempt to distinguish between ascendants and collaterals and to give a correct definition of daughter languages. Somewhat earlier, similar problems had been discussed by Steinthal (1849), to whom Pott referred (cf. Pott 1856:213 ff.).⁷⁴

Secondly, even the earlier controversies about the family tree model or similar models are hardly ever mentioned. Yet, as Porzig has pointed out, Schmidt was not the first to imply that those Indo-European languages which were geographically contiguous were also linguistically nearest, even when the traditional classification attached them to different branches of the family tree. In the late fifties and early sixties he had been preceded by Pictet and Ebel, who had compared the connections among the Indo-European languages to those between the links of a chain. In more general terms, Pott had pointed out that language and dialect boundaries keep crossing, and that languages influenced each other in a way which cannot even be compared to grafting because reciprocal influences were not limited to languages of the same type (cf. Pott 1856: *passim* and especially 203 and 213-20).

Arens (1969:304) argues that an important feature of Schmidt's theory is that he shattered the belief in the unity of the Indo-European *Ursprache* and put in its place a multiplicity of dialects. However, this point too should be considered within the framework of a discussion which had been going on for some time. Brugmann in 1884

⁷⁴ Steinthal's review (1849) is very much worth reading. The beginning shows a profound dissatisfaction with the vagueness of the current terminology and current theories: 'Noch ist die Sprachclassification nicht so weit gediehen, dass man auch nur über die ersten Grundbestimmungen derselben im Klaren wäre. Man versteht sich wohl so ungefähr, wenn von Sprachstämmen, -familien, -geschlechtern die Rede ist; aber noch ist man in Verlegenheit, wenn man genau und scharf anzugeben hat, was, d.h. welcher bestimmte Grad von Verwandtschaft oder Aehnlichkeit damit ausgesagt ist.

(239 f.) could still refer to Max Müller as the author of similar observations (cf., e.g., Max Müller 1861:177 ff., but see also the earlier references given in Schrader 1890:72 ff.). This is worth remembering since in more than one respect Max Müller can count as a Schleicherian. More important, however, is the polemical attack of Whitney (1867:177) on those writers who 'have committed the very serious error of inverting the mutual relations of dialectic variety and uniformity of speech, thus turning topsyturvy the whole history of linguistic development'. They 'affirm that the natural tendency of language is from diversity to uniformity; that dialects are, in the regular order of things, antecedent to language; that human speech began its existence in a state of infinite dialectic division, which has been, from the first, undergoing coalescence and reduction'. (*ibid.*; cf. also Whitney 1875:177). The reference is to Max Müller, but especially to Renan and to his highly influential *Histoire générale et système comparé des langues sémitiques* (1858b:98 ff.) and his *De l'origine du langage* (1858a:183 ff.).⁷⁵ It is unlikely that if Schmidt had been obliged to take part in this controversy he would have sided with Renan and Max Müller: his point seems rather to be that we are incapable of reconstructing a unitary *Ursprache* and that we should suspend judgement when we are confronted with what appear to be dialect variations (Schmidt 1872:29). One of his concluding sentences could hardly have been acceptable to Renan: 'Dass eine einheitliche indogermanische ursprache einmal vorhanden gewesen sei, ist höchst wahrscheinlich, ja ganz sicher, wenn sich erweisen lässt, dass das menschengeschlecht von einigen wenigen individuen seinen anfang genommen hat.' (*ibid.*: 29-30).⁷⁶

2.941 This last reference brings us back to our original problem and to a question which deserves more attention than it has received: what were the reactions to the *Wellentheorie*? But there is a preliminary point which cannot be ignored. We have seen that the paternity of the *Wellentheorie* has often been attributed both to Schmidt and to Schuchardt. Yet no attempt has been made to contrast the positions of these two scholars. In recent times there has been practically no historiographical work on Johannes Schmidt per se, and even the recent work which concentrates on Schuchardt, either in order to extol his contributions (cf., e.g., Vitale 1955:125-33, Vårvaro 1968:91 ff.) or to criticize him (cf. Hall 1963:10-17), fails to consider this very problem (though Terracini 1949:205-33 and Vitale *loc. cit.* come near to it). It is perhaps worth noticing that, while no compromise was suggested between the views of Schuchardt (in their later version) and those of the more traditional comparativists, a

⁷⁵ Yet another attack against Renan was made by Whitney in that passage of his 1867 book where he challenged the definition of roots as pure abstractions (1867:284). Whitney's whole theory of original monosyllabic roots contrasts strikingly with Renan's (and not only Renan's) fight against 'ce besoin de l'esprit qui nous porte à expliquer la complexité actuelle par la simplicité primitive' (Renan 1858b:98; cf. 1858a: 107 ff.).

⁷⁶ For some views of the Italian Cattaneo which anticipate the criticisms of Schmidt and Schuchardt cf. Timpanaro 1969: 273 ff. It is also worth noticing that in his 1877-78 programme of lectures Baudouin de Courtenay (1972:118) proposed to discuss precisely the problems raised by Max Müller and Renan.

compromise between the *Stammbaumtheorie* and Schmidt's *Wellentheorie* was often proposed. Arens (1969:311ff.) quotes Leskien's 1876 discussion, where it is argued that the two models are complementary, and the same point is often repeated (cf. Jankowsky 1972:106ff. with his reference to Pulgram 1953 and see also Graur 1967). The neogrammarians, as often, took their clue from Leskien, at least if we are to judge from Brugmann's discussion in his paper about the classification of the Indo-European languages (Brugmann 1884).

The contrast between the paper by Brugmann just mentioned and other reactions to Schmidt's pamphlet is also enlightening. In his 1873 book Fick objected to Schmidt's suggestion that Balto-Slavonic was particularly close to Indo-Iranian, since this went against his dichotomy between Asian and European languages (made in a strict 'family tree' spirit); in order to do so he plunged straight into a discussion of the detailed connections between the two groups. Brugmann's attitude was different: he realized that the whole question really concerned a particular model of sub-grouping and revolved round the criteria which could prove or disprove the connection of two languages or dialects within a certain group. After having stressed the point that any argument adduced was equally relevant to all language families and all genealogical classifications, he proceeded to take the discussion a step further. Schmidt had shown the importance of innovations as a criterion for sub-grouping, but had ignored the possibility of parallel innovations which could occur independently in similar circumstances. The introduction of this new factor showed that even Schmidt's model was unrealistic and that the problem was even more complicated than he had imagined. Brugmann's conclusion was that no single criterion could ever be sufficient to demonstrate the closer relationship of two languages or dialects: a very large number of agreements in phonetic, inflectional, syntactical and lexical innovations was necessary.⁷⁷

The next point concerned the possibility of reconstruction. Brugmann agreed with Schmidt that, while a family tree model allowed an almost mechanical process of reconstruction, things had become more difficult now that the model had been challenged. Reconstruction had to be based on a clear understanding of the how and why of language change. This involved (if we can put some more modern terminology in Brugmann's mouth) a series of generalizations about diachronic events (i.e. an interest in diachronic typology and diachronic universals), conjoined with a study of the particular linguistic systems concerned, in order to understand whether some phenomena could have arisen in them as innovations or were bound to be archaisms.

There is a sense, then, in which Brugmann is in a direct line of descent from Schmidt. To argue, as it is sometimes done, that the neogrammarians were strict upholders of the family tree theory seems to be impossible. On the contrary, it would be

⁷⁷ Jakobson (1938:49-50) gives Meillet the credit for having established that 'les concordances entre deux ou plusieurs langues ... proviennent, beaucoup plus qu'on ne l'imagine au premier abord, de développements parallèles'. It is proper to recognize that Brugmann too had emphasized the same principle. Cf. also note 78.

possible to stress some points of agreement with the contemporary views of Baudouin de Courtenay, who was also ready to criticize both models. But Baudouin's arguments were different: even the *Wellentheorie*, he maintained, was too abstract and treated language as if it were something separate from the speakers (cf. Baudouin de Courtenay 1910 and for references to earlier work Baudouin de Courtenay 1972:113, 311f.; see below 3.8).

However, if Brugmann was not too far from Schmidt, he was certainly not near to Schuchardt. The Romance philologist was far more drastic than Schmidt in his attitude. Admittedly, like Schmidt, he had spoken of linguistic waves, but he had then proceeded to argue, as we have seen, that by their nature language and dialects were not classifiable (cf. Schuchardt 1928:187 [published 1900]). He had also quoted Darwin's dictum, that every real classification is genealogical, but merely in order to point out that any genealogical classification could only be an external one (*ibid.*: 166). On the one side he felt obliged to extend his misgivings about the possibility of the genealogical sub-grouping of dialects to the whole concept of genealogical classification, on the other side he found that the distinction between similarities due to common origin, similarities due to early influences, and similarities due to what he called *Elementarverwandschaft*, was far from clear (1928:248ff. [first published 1912]) and he came to argue that any attempt to establish a genealogical classification broke down when it was not supported by ethnological, historical, and anthropological evidence.⁷⁸

2.10 Practically anything that we can say — or have said — about Schuchardt is an oversimplification, but we have now reached a stage where — in the context of an enquiry into the history of language classification — we are entitled to ask whether he should be listed among the comparativists or among the typologists (or among neither of them). The dilemma is typical of this period: the end of the century seems to be marked by a polarization of interests and opinions. The neogrammarians had brought into the open their criticism of earlier methods of comparison and had pleaded for a more historically minded approach and for a greater methodological rigour. In their case, the result was a new fervour of historical and comparative work. Classification,

⁷⁸ At this stage virtually every word needs qualification. What is the theoretical difference between Brugmann's point that identical innovations may occur independently and the explanation in terms of *Elementarverwandschaft* which Schuchardt gives of some linguistic changes (e.g. 1928:252)? Sometimes we have the impression that the theories differ less than at first appears, but that the practical conclusions diverge: Schuchardt would never have written Brugmann's *Grundriss*. The opposite may of course be true. On the other hand when we speak of Schuchardt as a typologist further qualifications are necessary: he is interested in historical events in a way in which the earlier typologists were not, and consequently he is far less interested in generalizing statements. Since it is normal to speak of the neogrammarians as data oriented, we may wonder whether the same adjective might not apply to Schuchardt too. In a somewhat paradoxical vein we may also ask whether the real difference between Schuchardt and the neogrammarians does not consist in the fact that the latter are structuralists before their time (see above for Putschke's views), while Schuchardt is much nearer to individualism and idealism.

as we have seen, was not their main concern, but when in the early 1920s Meillet affirmed that the only way in which languages could be usefully classified was genealogically, he was in fact well within the tradition which they had started.⁷⁹ In other circles the criticism had gone further, and the very bases of the comparative method were called in doubt: we have mentioned the position of Schuchardt, but it would be equally and, perhaps, more interesting to discuss the views of Baudouin de Courtenay. The obvious consequence, this time, was a shift of interests: hence the numerous studies of word histories, *Wörter und Sachen*, history of culture and history of language, etc. In the rare cases in which attention did concentrate on language classification, this rejection or criticism of the comparative method was bound to stimulate interest in areal classification and in a typological characterization of languages. Along this line it would be possible to follow one of the trends of thought which, in the twentieth century, led for instance to Trubeckoj's definition of the notion of *Sprachbund*. However, this lies outside our chronological limits, and the moment has now come to abandon the history of the comparative method and pass to the second part of our survey.

3. TYPOLOGICAL CLASSIFICATION

3.1 The typological views of the nineteenth century have not found a generous reception in standard histories of linguistics: all mentions of them are accidental and almost apologetic in nature. Historically this is understandable. As we shall see, the traditional form of 'morphological' classification was discredited by the end of the nineteenth century, and modern historiography has reflected this attitude. Moreover, the tendency has been to concentrate on what, for most scholars, marked the real dividing line between the nineteenth century approach and the 'philosophical' outlook of the century which preceded it (see above 1.4): the genealogical method of comparison, with its historical aims, seemed to fulfil this function, while linguistic typology, with its inevitable element of arbitrariness and its synchronic or perhaps achronic methodology, did not. This may account for instance for the views of Pedersen (1962: 99) who dismisses in less than a page the fact that 'in the course of the nineteenth century certain attempts were made to devise a short-cut by establishing a number of types under which all languages could be classified'. A similar attitude had been taken long before by Berthold Delbrück (1901:47): 'Wie bekannt, hatten sich die Sprachforscher einmal dahin geeinigt, die Sprachen nach ihrem Bau in isolierende, einverleibende, agglutinierende und flektierende einzuteilen. Aber diese Klassifikation ist uns allmählich unter den Händen zerronnen'. Delbrück was not the first with his criticism: similar doubts had been expressed in an equally influential work, Fr.

⁷⁹ See below 3.1.

Müller's *Grundriss der Sprachwissenschaft* (1876-88:I,1, 81ff.).⁸⁰ As we have seen before (2.10) a classic sentence by Meillet (1924:1) gave the accolade to a similar position: 'La seule classification linguistique qui ait une valeur et une utilité est la classification généalogique, fondée sur l'histoire des langues.'

3.11 In a recent article Greenberg (1970) has underlined the dramatic features of the history of typology. At the beginning of the nineteenth century it shared the stage with the new comparative linguistics; towards the end of the century it fell into neglect and disrepute. Sapir appears to have saved it from complete extinction. During the last two decades it has revived, thus showing the intrinsic vitality of the subject, its tenacity and its adaptability.

Parts of this picture could be challenged. We may choose to ignore the link between the work of Finck and that of his pupil Ernst Lewy (cf. Lewy 1951 and 1952), but we should not forget Schuchardt's interest in an *Elementarverwandschaft* with no historical connotations (see above p. 651), or, later on, Trubeckoj's paper on the typology of Indo-European (1939, see p. 637) or Jakobson's contributions to the subject. Particularly important is the prominence given to typology in Hjelmslev's linguistic theory (cf. Bès 1967, and especially Lepschy 1970:xii), not least because with its undeniable influence on, e.g., the work of Uspenskij and Birnbaum, it seems to guarantee some continuity between the recent revival and earlier doctrines (cf. Birnbaum 1970: *passim* and especially 80). Even Marr's typological-genealogical theories, which are outside the chronological limits of this survey, should not be entirely forgotten. However, if by typology we refer to the 'morphological' classification in its standard form, Greenberg's statements are essentially correct for the nineteenth century.

3.2 For the reasons given in 3.1 an account of the basic facts concerning the typological classifications of the nineteenth century is not easy to find. As a list of primary material to turn to in the first instance, the long bibliographical introduction to the second edition of *Les langues du monde* (Meillet and Cohen 1952:I) remains unrivalled. It offers an almost complete list of works dedicated to language comparison and typology from the beginning until recently; nineteenth-century titles form the greatest part of the list. A short survey of the most important books is also offered by Veith (1969) who gives a brief summary of their content and offers some comments on their importance (cf. especially Veith 1969:11-18). Yet another brief account of various types of classification (geographical, genealogical, typological) from Dante until the present day will be found in Leroy (1960); parts of the text are repeated in Leroy (1971: *passim*).

In his *Introduzione alla glottologia* Tagliavini (1963:381ff.) offers a clear account (which relies in part on earlier work of his) of the various classifications proposed from Schlegel onwards till the present day. The starting point is the usual distinction

⁸⁰ Baudouin de Courtenay could also be quoted: cf., e.g., Stankiewicz's discussion (Baudouin de Courtenay 1972: 39) with his reference to Baudouin's programme of lectures for 1877-78 (*ibid.* 119).

between typological and genealogical classification, but Tagliavini postulates a further subdivision between morphological and psychological classifications. According to him Schlegel, Bopp, Pott and Schleicher based their theories on a structural comparison of languages, while Humboldt, Steinthal, Misteli and Finck arranged their evidence according to psychological criteria. The distinction goes back to Fr. Müller's *Grundriss* (1876-88: I, 1, 63-4).

Linguistically inaccessible to me is T. S. Sharadzenidze's book in Georgian on the classification of languages and their principles, which I know only through the brief Russian summary at the end (Sharadzenidze 1958: 503 ff.). Sharadzenidze distinguishes seven types of classification: genealogical, morphological (Schlegel, Humboldt, Schleicher), psychological (Steinthal, Misteli), conceptual (Sapir), 'in Sprachkreise' (W. Schmidt), in stages (Marr, Meščaninov), in *Sprachbünde* (Trubeckoj). The distinction between morphological and psychological classifications is similar to that of Müller and Tagliavini, but Humboldt is assigned to the former and not to the latter group.⁸¹

In the last two decades the renewed interest in the problems of language classification has produced two monographs which aim at providing the reader with a simple history (chronologically arranged) of the studies of typological classification. Kuznecov (1960) offers a brief resumé of the theories of the most obvious authors (from Schlegel to Sapir and then to Marr). Horne (1966) starts with a theoretical chapter which relies heavily on Greenberg's work and discusses the various ways of approaching language classification. He then follows very much the same pattern as Kuznecov, though the emphasis varies slightly, especially for the more modern authors. Kuznecov emphasizes the importance of Marr in the USSR, while Horne dedicates more space to Sapir and introduces two more modern theorists: Greenberg and Martinet. For the earlier period Horne and Kuznecov do not differ much: both authors consider the contributions of the two Schlegels, Humboldt, Bopp, Schleicher, Steinthal, Misteli, and Finck, and are practically alone in mentioning Fortunatov.

Lounsbury's brief survey of the history of classification (1968) is less propaedeutic and more argumentative; it adopts a line which is very similar, at least in its first part, to that of Jespersen's article on language classification (Jespersen 1962: 693-704 [first published 1920]). It starts with the observation that the concern with differences among languages was linked in the nineteenth century with the attempt to discern their mental and cultural significance. Linguistic typology was comparative, in a non-historical sense of the word, but it also came to have a time dimension thanks to its evolutionary tendencies. Humboldt was the first to postulate a development from one linguistic type to another, and this assumption was accepted by some of his successors

⁸¹ I am grateful to Mrs. Joan Leopold for bringing this book and Lounsbury's article to my attention. The main purpose of Sharadzenidze's book does not seem to be historical, and it is no oddity that it appears to have been written outside the main Western tradition of the history of linguistics. Even so, the book seems to be solidly based on a good acquaintance with the primary sources, and this increases the regret that it cannot be accessible to a wider public.

until Max Müller connected the evolution of language with the evolution of society and assumed that each of the three major language types (isolating, agglutinative, inflectional) corresponded to one stage of the development of community life (family stage, nomadic stage, political or state stage). The attention then shifts to the typological views of the American school: Whitney and then Boas, and Sapir.

3.3 From most of the literature just quoted the development of typological research appears as relatively unproblematic. The aim was to find some criterion which could be meaningfully used to classify all languages of the world. After Fr. Schlegel's distinction between affixing and inflectional (or organic) languages, which in practice contrasts Indo-European with all other language types, we have a series of further refinements, due to A. W. Schlegel, Humboldt, Schleicher, etc. A common feature is the attention given to word structure which allows the famous tripartite division into isolating (or monosyllabic), agglutinative (or affixing), and inflectional languages. Other distinctions may be introduced, such as a fourth class of incorporating languages, intended to accommodate, inter alia, most of the Amerindian types (which Schleicher prefers to consider agglutinative). A further subdivision which gained popularity was that proposed by A. W. Schlegel, between synthetic and analytic languages, which was meant to take care of the contrast between two inflectional languages such as Latin and English. Steinthal's analysis followed different lines, but it is this 'morphological' classification which gained prevalence until its partial rejection towards the end of the century and its reformulation and reintegration in Sapir's theory.

3.31 Yet, for all its apparent simplicity, even this view of the history of typological classification may be challenged. We are given a list of scholars, which, needless to say, is not exhaustive, but we are left in doubt about the criteria which determined the selection. Other names come to mind. Steinthal (1860: 13) quotes with some respect the theories of the psychologist-anthropologist Th. Waitz, who now appears to be forgotten.⁸² Should we yield to the urgent plea of Ernst Lewy (1961b [first published 1944]) and mention James Byrne with his psychological classification?⁸³ What is the position of Georg von Gabelentz, whose theoretical contributions have recently been revalued (cf. above 2.9), and to whom we owe, inter alia, a very interesting article on typology (Gabelentz 1894)? Most authors ignore R. de La Grasserie: is this a wise

⁸² The first volume of the *Anthropologie der Naturvölker* appeared in Leipzig in 1859 and was translated into English 'with numerous additions by the Author' by J. F. Collingwood in 1863. The book appeared as the first of a series of translations published by the Anthropological Society of London (founded in that year). The chapter on language classification which is part of section 5 [part 1] ('On the classification of Mankind'), though not new in its approach, is an interesting reminder for the linguist of the importance that typological classification had for the anthropologists of the first three quarters of the nineteenth century. It also shows some vagueness in the distinction between genealogical and typological classification.

⁸³ Byrne was more influential than is normally supposed. Lewy has repeatedly stressed the connection between him and Finck, but we may also remember the high praise bestowed upon him by G. von Gabelentz (1894: 1).

decision? And what about the views of Baudouin de Courtenay? Other problems arise from the divergent interpretations of some authors. Should we count Rask among the typologists, as suggested by Hjelmslev (cf. above 2.4)? The starting point of the list requires some comment too. Why should we start the canon with Friedrich Schlegel rather than with J. C. Adelung or even D. Jenisch (for the latter see Jespersen 1922:29–31 and Timpanaro 1972a:97–8)?⁸⁴ To Adelung we owe a clear-cut distinction between monosyllabic and polysyllabic languages, which, for all its evolutionary overtones, is not too remote from that between isolating and agglutinative languages. The name of Adam Smith is not normally associated with linguistic typology, but Coseriu (1968) has suggested that he is the source of A. W. Schlegel's distinction between analytic and synthetic languages. In an independent essay Scott Elledge (1967) has tried to show that Adam Smith's views were influenced by Bacon. But even if we do not go so far back as Bacon, we should not forget the typological distinction between 'langues transpositives' (like Latin), 'analogues' (like French) and 'mixtes' (like German) made by the abbé Girard as early as 1747 (cf. Diderichsen 1960:127, Rosiello 1967:146). If so the history of typology seems to stretch far back through the centuries.

All these are open problems and in view of their existence the agreement about the name of F. Schlegel as the first author of a typological classification may be puzzling. Here too, the usual explanation is forthcoming. Modern historiography has accepted the views of the past century. This is probably a right decision; there is little doubt that there is a strong sense of continuity between Schlegel and the later typologists, while Adelung, Jenisch, Adam Smith and l'abbé Girard, not to mention Bacon, remain at a distance. Even so, the choice is derivative and few attempts have been made to give it an explicit justification (for an exception see Timpanaro 1972a:97–101, who offers an interesting sketch of the early non-genealogical comparisons, and see also Diderichsen 1960:125ff., for the contrast between the nineteenth century and the earlier period).

Other questions arise; they concern first and foremost the correct interpretation of the authors mentioned, but also some more general problems. We have spoken already of the difficulties which we find in distinguishing between the early typological and genealogical classifications (cf. 1.6) and between the criteria used in each type of enquiry. This is not the only trouble. What justifies the fervour of typological research which we find in the first three quarters of the century? Should it be taken as a con-

⁸⁴ Jenisch is scarcely ever mentioned, but Jespersen's own interests induced him to give some attention to Jenisch's attempt to characterize each language in order to find out which one came nearest to the 'Ideal einer vollkommenen Sprache'. Timpanaro (1972a:97) points out that in spite of Jenisch's rare impartiality the book reveals the basic subjectivity and infertility of this type of comparison. In fact what is interesting in Jenisch (1796) is not so much his evaluation of the various languages as the attention which he gives to language as a whole, including grammar, which is also considered as a means of establishing language kinship (cf., e.g., his statement about English, *ibid.* 57, note). Jenisch was well aware of the possibilities of historical research and knew the correct methodology for it (cf., e.g., *ibid.* 41); at the same time as he stated that similarities due to common origin go deeper than any other form of similarity, he seemed to exclude any form of evolution which would drastically alter a language type.

tinuation of eighteenth-century classificatory interests? Or is it due to the influence of the natural sciences (cf. above 1.51)? Or does it simply correspond to the practical need to arrange in some way the new wealth of linguistic material available? And finally, what can explain the disenchantment with morphological typology which prevailed in some circles by the end of the century? None of these questions can be answered here, but they may be kept in mind and used as a framework for our survey of recent work on the subject.

3.4 The first problematic figure is F. Schlegel. The enormous influence which his equation of organic and inflectional exerted on the history of nineteenth-century linguistics in general, is well known.⁸⁵ An analysis of F. Schlegel's thinking about language differences can be found in Nüsse (1962:37–49), who argues that his typology arises from the merging of his systematic interests with his concept of organism. However, Timpanaro (1972a) is certainly right when he emphasizes that in Schlegel's statements there is still something of the old classifications of the eighteenth century which were concerned with the genius of languages. While his 'vergleichende Grammatik', in contrast with that of his predecessors,⁸⁶ appears to aim at a genealogical classification, his typological classification is essentially static and has evaluative overtones: the contrast between the two language types becomes in his mind a contrast of divine and ferine languages (Timpanaro 1972a:101). Yet, to my mind, not even Timpanaro stresses sufficiently that nowhere in *Ueber die Sprache und Weisheit der Indier* (Schlegel 1808) is there a clear statement of the distinction between linguistic comparison which aims at genealogical results and linguistic comparison which aims at typological statements or classification. The purpose of genealogical comparison is stated once at the beginning: it elucidates the historical origins and progress of nations and their early migrations and wanderings. Later on, the chapter on typological classification states clearly that all organic languages share the same structure and are genetically related. It then appears that the organic/non-organic distinction is meant to provide further evidence about the kinship of the organic languages, while at the same time stressing their superiority. If so, typological classification may seem to play a subordinate role vis-à-vis genealogical classification. And indeed Sharadzenidze (1958: Russian resumé 513–4) argues just this: Schlegel is only interested in genealogical relationship; he knows that Semitic is not related to Indo-European and consequently he is compelled to make the somewhat surprising statement that Semitic, differently from Indo-European, is a non-inflectional, non-organic language. However, this view too is probably too drastic. Apropos of the non-organic languages Schlegel denies the possibility of tracing them back to a common source, so that it

⁸⁵ Cf. above 2.3. It is certainly wrong to argue that by inflection Schlegel means only the modification of the root vowels (as in Latin *foedus* vs. *fides*), yet this has been frequently maintained (most recently by Horne 1966:11f.). For a correct interpretation (scil. inflection refers both to ablaut, umlaut and the like, and to a particular type of suffixes and prefixes) cf. Timpanaro 1972a (82f.). For La Grasserie's point that the concept of inflection had never been properly defined see below note 127.

⁸⁶ For the earlier history of this phrase see above, note 15.

would seem that the organic/non-organic classification had a wider scope than the genealogical one.

The problem which has not been tackled a propos of F. Schlegel is why his somewhat rough classification, with all its ambiguity, was regarded with respect, even when attacked, for most of the nineteenth century. The beginnings of a solution could perhaps be found in the observation that all through the work of Humboldt, Schleicher, Steintal, etc. — in spite of the difference of presuppositions and backgrounds — we can recognize a continual search for a non-arbitrary principle on which linguistic classification could be based. Non-arbitrary in this context can only mean essential to language, i.e. based on the study of the nature of language. From this point of view the aesthetic classifications of the previous centuries were bound to appear unacceptable, but F. Schlegel's classification, even when bitterly opposed — as it often was —, seemed to be on a different level.⁸⁷ In the first part of the century there could be debate about the way in which the idea of 'organic' should be applied to language, but no one would have denied its essential and non-arbitrary nature.⁸⁸

3.41 Timpanaro (1972a) is eager to underline, against Nüsse (1962), the development in F. Schlegel's ideas. In his earlier work the distinction between inflectional or organic and non-inflectional or non-organic languages impelled him to deny any genetic connection between the two types; later on, religious scruples induced him — a new convert — to accept the traditional monogenetic view and to postulate a sort of degenerative process which was supposed to bring the organic languages to a non-organic or mechanical stage. As Timpanaro (1972a) points out, this phase of Schlegel's thought, however interesting biographically, remains outside the main stream of the history of linguistics. This was influenced, and in a sense directed, by Friedrich Schlegel only through his 1808 book and through the modified version of his theory proposed by his brother A. W. Schlegel.

It is to A. W. Schlegel in fact that we owe the traditional distinction between languages without grammatical structure, languages with affixes and languages with inflection, and it was he who canonized the division of inflectional languages into analytic and synthetic and the theory that synthetic languages had to become analytic in the process of time.⁸⁹ Nüsse (1962:48) points out that in fact the three-fold dis-

⁸⁷ Steintal (1860:4f.) sharply criticized Schlegel's classification but gave him credit for having introduced the concept of organism into linguistics (though, see *ibid.* 21, where he gives priority to Humboldt). Further on, he argued that Schlegel's classification should be grouped with other classifications of languages into masculine and feminine, or crystal-like, plant-like and animal-like (an obvious dig at Schleicher); they all contained part of the truth, but only touched the surface of the language phenomenon (*ibid.* 7).

⁸⁸ Needless to say, other reasons, which were more concerned with the history of linguistics in general than with that of typological classification in particular, helped to give the views of Fr. Schlegel's an authority which those of his predecessors lacked.

⁸⁹ The history of this distinction, which still enjoys some popularity, deserves a separate study. I have given above some hints about its earlier history (cf. 3.31). From the point of view of the terminology employed it could be added here that, well before A. W. Schlegel, S. de Sacy (1808:255f.) spoke of a 'système de synthèse' for the grammar of Coptic (cf. also Humboldt 1903-36:IV, 312),

inction destroyed the whole point of F. Schlegel's organic/inorganic contrast, while at the same time showing some progress in the application of the comparative method. This is worth stressing; another point, however, which is often ignored is that A. W. Schlegel, in contrast to his brother, refused to prejudge the possibility of an evolution from the first of his language classes to the second or the third. His statements on the subject (A. W. Schlegel 1846:214 note 7 [first published 1818]) implied that the problem of the evolutionary interpretation of his scheme could only be solved by empirical evidence and that this was not available at the time. The very existence of this sceptical position at the beginning of the century is important. We shall have to return to the question later, when talking about evolution (see 3.91); for the moment it is sufficient to point out how much more aware A. W. Schlegel appears than his brother of the importance of the historical method. It is to be hoped that the promised work of S. Timpanaro on his thought (cf. Timpanaro 1972a:93 note 63) will shed some more light on this and other points.

3.5 We mentioned above (in 2.5) Wilhelm von Humboldt's definition of the criteria which prove genealogical relationship. Here we must concentrate on other parts of his work. In the last twenty years or so, he has attracted great attention from numerous angles, not all of which need concern us here. However, even if we limit ourselves to strictly linguistic work, we have to register a number of contributions of very unequal value, often written in ignorance one of the other. A book has attempted a summary of Humboldt's linguistic thought accompanied by a resumé of his posthumous book (Valverde 1955). A lengthy monograph (Liebrucks 1964-70:II, cf. also I) has examined Humboldtian problems more from a philosophical than from a historiographical point of view. No less than three monographs and a number of articles have concentrated on Humboldt's role as founder or upholder or precursor of the so-called Sapir-Whorf hypothesis (Brown 1967, Miller 1968, Penn 1972, Apel 1959, Christmann 1965, 1966, Mattoso Camara 1970, Politzer 1963a, 1963b; cf. also Hansen-Løve 1972). His thought has been discussed in general (Evans 1967, Gipper 1965, H. J. Mueller 1966, Seidel 1968, Teeter 1966, Wald 1962, etc.) or with reference to particular concepts (Beneš 1958, Pleines 1967, Rensch 1967, Stolte 1948, Schwankweiler 1965,

though he was only concerned with the way in which languages pass from a monosyllabic isolating stage to a polysyllabic agglutinative form. The popularity of A. W. Schlegel's distinction must have been partly due to the fact that it fulfilled the (more or less explicit) need to believe that all languages belonging to the same family also belonged to the same linguistic class (cf. 3.93). Since some Indo-European languages had been taken as a model for the inflectional type, it did not seem possible to give a different label to the younger offshoots of the family, even if by rights they appeared to be nearer to the agglutinative or the isolating type. However, this sub-classification was not universally accepted. Soon after its formulation, Humboldt rejected it because it was too vague and because linguistic history showed that the so-called analytic languages often created synthetic forms as secondary developments not inherited from the parent language (cf. Humboldt 1903-36: VI, 260f.). At a much later stage (see below 3.93) Baudouin de Courtenay argued that English was really an isolating language on a par with Chinese, which implied a de facto rejection of the analytic/synthetic distinction as a sub-classification of the inflectional class.

Weisgerber 1953–54).⁹⁰ The connections between Humboldt and modern linguistics have often been explored, though sometimes in a desultory and uncoordinated manner (but cf. Christmann 1972, Lohmann 1967, Pätsch 1968), and attempts have been made to put Humboldt in his setting and to trace the influences which helped to form his setting and theoretical outlook (Apel 1963:372ff., Christmann 1965, Funke 1957, Heintz 1969, Viertel 1966, Weimann 1965, cf. also above 1.4). Modern anthologies and translations⁹¹ document a lively interest in this author even from outside the traditional territory of the German Neohumboldtians.

Few works, however, have been concerned directly with Humboldt's classification. This may perhaps explain a puzzling phenomenon: the general books and monographs mentioned above in 3.2 present different accounts of his classificatory schemas. Kuznecov (1960:10) and Horne (1966:13–15) attribute to him a four-tier classification of languages into isolating, agglutinative, inflectional and incorporating classes, while recognizing that this is basically A.W. Schlegel's classification schema with a fourth class added. Lounsbury (1968:191ff.) follows very much the same line, with some qualifications. Other scholars go further and mistakenly seem to attribute to Humboldt rather than to A.W. Schlegel the paternity of the whole schema (cf., e.g., Robins 1967:176). Tagliavini (1963:I, 393) attributes these views to an error of Schleicher,⁹² while he accepts another schema (obviously taken from Steinthal 1850 and 1860) according to which all languages are to be categorized as being more or less 'complete'. The former may be either isolating or inflectional, the latter may be either particle languages (in which the verb has no special expression) or pronominal languages (in which the verb is characterized by the juxtaposition of pronominal elements).

In a thoughtful article, in which she is practically unique in considering both the concrete and the theoretical work of Humboldt, G. Pätsch (1967) has pointed out that, although Humboldt is always quoted for one or the other version of linguistic classification, in fact he never reached any firm conclusion on the subject. He simply distinguished the two extremes (the inflectional languages versus the languages like e.g. classical Chinese) and assumed that the affixing languages represented an intermediate point. Warnings about the misunderstandings which have arisen in the interpretation of Humboldt's theories are also to be found in Gipper (1965:9ff.). Hansen-Løve (1972:37f.) goes a step further and denies that Humboldt ever admitted the possibility of any language classification.

In fact, most of the various schemas attributed to Humboldt are in some way

⁹⁰ Some books or articles which I have not been able to see are not quoted here or in the bibliography. I particularly regret that I have not found the series of essays *Die Brüder Humboldt heute* (Abhandlungen der Humboldt Gesellschaft, 2), Mannheim 1968, mentioned in BL 1969: 20 no. 46.

⁹¹ Anthologies and translations are not quoted here or in the bibliography with the exception of Cowan 1963, which is frequently referred to in American publications, but which I have not been able to see.

⁹² The reference is to Schleicher 1848:6, note; the error had already been pointed out by Steinthal (1970:132 [first published, 1850]).

grounded in his own work. A distinction between isolating, agglutinative, and inflectional types occurs frequently (with this terminology) in the posthumous (1836) *Ueber die Verschiedenheit des menschlichen Sprachbaues und ihren Einfluss auf die geistige Entwicklung des Menschengeschlechts* (Humboldt 1903–36:VII, 1ff.) and is also implied in the earlier work (1822) *Ueber das Entstehen der grammatischen Formen, und ihren Einfluss auf die Ideenentwicklung* (*ibid.*:IV, 285ff.). However, a contrast between agglutinative and inflectional types is explicitly denied in VII,117, where only two types seem to be envisaged; elsewhere (VII,117, cf. also VI,275ff.) it is stated that these must be interpreted as abstract forms and all languages are mixtures of two or more types.⁹³ The last chapters of the 1836 book (on which Steinthal's schema is based) seem to express strong doubts about the possibility of an exhaustive classification of all languages, but it is reiterated that this is feasible with a particular aim in mind and using a particular phenomenon as a criterion (cf. VII,254ff., and especially VII,278–9). Then a sort of classification is suggested which corresponds, though not exactly, to that given by Steinthal. The main defect of the histories of typology mentioned before is not so much that they give a wrong interpretation of Humboldt's theories as that they do not add the necessary qualification to their statements. An easy reminder of the difficulties encountered in understanding Humboldt's views on the subject could have come from a first-hand acquaintance with the polemical (but friendly) exchange about this problem which took place in the 1850s and later between Steinthal and Pott (cf., e.g., Pott 1852a, 1852b, Steinthal 1970: 139–64 [first published 1852], Pott 1859–76:II,4,ii).

3.51 Telegdi (1970) has competently discussed the part which typology plays in the linguistic work of Humboldt: it is interesting to see that some of his conclusions come near to those of Steinthal, but it is a pity that he does not discuss the earlier work on the subject (e.g. Pätsch 1967, Gipper 1965, parts of Liebrucks 1964–70:II). According to Telegdi, Humboldt's typological studies are a direct consequence of his linguistic theory; they represent an essential part of his search for universals and they indicate the extent of his reactions against the universal or philosophical grammar of his predecessors. Moreover, Humboldt's conception of language as an organism with a unity and a reality of its own accounts for the attention which the problem of language plurality receives in his system. It is at this stage that the problem of Humboldt's views about linguistic relativity should be considered (cf. the literature quoted above in 3.5). One of Brown's key points is that it was his organicistic view of language which allowed Humboldt to set up language as the independent variable required for a strong version of a linguistic relativity theory (Brown 1967: *passim* and especially 53).

It may be added here that the primary importance given to typology, as the main source of information about the language faculty and its expression, may well explain Humboldt's hesitations and the reluctance with which he proposed — or perhaps did

⁹³ Steinthal (1970:145 [first published, 1852]) pointed out that this implied that according to Humboldt the division into isolating, agglutinative, and inflectional types could not be sufficient by itself to provide a classification of languages.

not propose — a classification schema: too much depended on it. However, we should not forget Steinthal's detailed analysis of this very question (1860). According to him, Humboldt was clear in his mind that any classification had to depend on a correct identification of the *Sprachform* of each language, but he did not succeed in defining this key concept clearly and oscillated between two opposite views; on the one side he identified the *innere Sprachform* with the general form of thought and on the other side he treated it as the organic principle which introduced an essential — i.e. non-superficial — difference between the various languages.⁹⁴

We have to refrain here from any discussion of the Humboldtian concept of *innere Sprachform*, which would take us too far, and which is indeed examined in practically all works quoted above (for a short introduction see Funke 1957), but we may add by way of postilla to what precedes that it is regrettable that, in spite of a few exceptions (e.g. Liebrucks 1964-70:II), most linguists have not given attention to the anthropological essays which Humboldt wrote in the last decade of the eighteenth century and which have been recently discussed again by Leroux (1958; cf. also Brown 1967:106). His conclusion: 'c'est donc en anthropologue et afin de promouvoir la connaissance des mentalités et des caractères des peuples que Humboldt aborda l'étude des langues' (*ibid.*:69, note 2) is important, and some of the endless discussion about Humboldt's key concepts (like that just mentioned of *innere Sprachform*) could receive some light from a comparison with his early psychological-anthropological work.

Another important point to consider concerns the relationship between Humboldt's views and those of Friedrich Schlegel. In his view of 'organic' inflection Humboldt is obviously on the side of Bopp, and assumes that inflection may arise from agglutination, something which Schlegel hotly denied. Nüsse (1962:46) has pointed out that Humboldt managed to overcome Schlegel's dualism. All languages (including the isolating ones) are characterized by an organic principle (the *innere Sprachform* again!) so that the organic/non-organic contrast loses its value as a classificatory criterion. Even so, Humboldt, like Schlegel, has no hesitation in arranging the different languages on a priority scale. Gertrud Pätsch (1967:114ff.) has tried to establish what it was that impelled Humboldt to take this evaluative position, and attributes it to three causes. First, for him language is simply a *Vehikel* used in order to penetrate the secrets of the 'geistige Welt', i.e. Humboldt looks at language from an extra-linguistic point of view. Secondly, his philosophical presuppositions induce him (like Bernhardt) to look for an ideal language as an 'unbedingte Form'. Thirdly, like all his contemporaries he is conditioned by his interest in Indo-European languages. There is little to add to this analysis, but the first point needs some clarification. This comes — in my opinion — from Humboldt's relativity theory. Since language can influence

⁹⁴ It is possible to give weight to this interpretation without necessarily following Steinthal all the way or seeing the real cause of Humboldt's hesitations in a conflict between *Theorie* and *Empirie* which would have blighted the whole life of this 'wahrhaft tragischer Held' (Steinthal 1970:139 [first published, 1852]).

thought, it can also hinder it. It follows that the nearer a language comes to a formal expression of logical relationship the more it fulfils this aim of favouring rational thought. As Humboldt argued in his 1822 paper (1903-36:IV,285ff.) this does not exclude, of course, that anything can be expressed in any language, provided that some external cause (such as contact with other cultures) has created the need for it in the mind of the speaker; it simply means that some languages make a particular type of thought easier than some others.

3.6 Two other scholars are referred to in the histories of typology: Franz Bopp (cf. above 2.41) and August Friedrich Pott (cf. 2.61). Bopp's attempt to classify languages according to the form of their roots and their capacity for composition and internal alteration is often mentioned (cf. Tagliavini 1963:I,384f., Horne 1966:15), but this classification is meaningful only if considered in connection with Bopp's theory of the root and of Ablaut (cf. 2.61 and see Verburg 1950) and only if one understands Bopp's early fight against Friedrich Schlegel's concept of the organic development of the root (for some hints about this see Lounsbury 1968). In fact the whole question is more interesting for an understanding of Bopp than for the history of typology, to which Bopp contributed but little. Yet, Bopp's views about the superiority of the Indo-European languages over the others (cf. Bopp 1833:113) would deserve some mention, if only because, based as they were on a somewhat vague notion of richness and harmony, they emphasize by contrast the greater impact of Humboldt's and F. Schlegel's evaluative criteria.⁹⁵

Tagliavini (1963:I,385), Horne (1966:15f.), who follows Steinthal (1860) and Jespersen (1962:693-704 [first published 1920]), Lounsbury (1968), who follows Steinthal (1850) and Jespersen (*loc. cit.*), all agree in attributing a particular classification schema to Pott. The same statement is found in Steinthal, as we have seen, and in Fr. Müller (1876-88:I,67). Directly or indirectly reference is made to an article by Pott in an obscure periodical, the *Jahrbücher der freien deutschen Akademie* (Frankfurt a.M.) for 1849. Pott himself has tried to correct this mistaken attribution (cf., e.g., Pott 1852a, and 1859-76:II,4, iff.); he repeatedly argued that he had always felt incompetent to produce any classification and that what passed for his own effort was in fact the way in which, after due warning, he had set out in a short and popularizing article his interpretation of Humboldt's views on the subject.⁹⁶ The misunderstanding has lasted too long. Pott's reflections about the whole problem of classification have

⁹⁵ Obviously I do not mean to select here for praise, still less acceptance, the criteria and conclusions of Humboldt and Schlegel, but it is important to point out that at least in the case of Humboldt these were far more thoughtful and theoretically well grounded than the traditional aesthetic judgments about the relative value of languages. Bopp's attitude meant a regression to earlier views (while at the same time his version of the morphological classification was perhaps naive but far better defined).

⁹⁶ As far as one can see, the periodical was almost always quoted at second hand. Already in 1851 Böhlingk (1851:i) mentioned that he had found it impossible to get hold of Pott's article, and a similar declaration was made in the late 1850s by Madvig (cf. Madvig 1971:168 note 1). Steinthal withdrew part of his statements in 1852 and again in 1860 (cf. Steinthal 1970:139ff., 1860:10).

been quoted above (cf. 1.51 and 2.61) and certainly earn him a place in the history of typology and in that of genealogical studies, but he should not be attributed views which he himself maintained that he had never held.

3.7 In the main stream of typological work are two contrasting and often antagonistic figures, A. Schleicher and H. Steinthal. We shall discuss them in turn.⁹⁷

All the works mentioned above point out that Schleicher's version of typological classification (with its evolutionary or transformationistic statements and its division of languages into isolating, or monosyllabic, agglutinative, and inflectional) became widely known and still holds the field at a lower level of vulgarization. They also illustrate Schleicher's formulae for indentifying languages according to the different ways in which they express lexical meaning and relations (*Beziehungen*). Most accounts give the impression that we are dealing with a somewhat modernized version of Humboldt's and A. W. Schlegel's classifications. Humboldt's influence on Schleicher must indeed have been great, and is often recognizable even in minor details, but it is all too easy to forget that the general approach and the thought behind the schema differ considerably. We have seen that for Humboldt the plurality of languages was strictly tied to the spiritual differences of nations (as it was in Herder): his 'vergleichendes Sprachstudium' was the outcome, and, in a sense, the precondition of his 'vergleichende Anthropologie'. For Schleicher any attempt at analyzing the spiritual life of peoples came under the purview of philology and not of linguistics or *Glottick*. Since language was an organism which obeyed natural laws, the normal methodology of the natural sciences was relevant, and called in all cases for a comparative, classificatory study. Dietze (1966:68-9) has mentioned some of these points (cf. also the observations by Esper 1968:95ff.), but Schleicher's classification theory needs further exploration. How does he, for instance, justify the choice of his linguistic parameter, the relationship of meaning and *Beziehungen*? He himself tells us (1850:6-7) that this contrast is basic to human thought. But if language is envisaged as being closely connected with thought, how can it be an independent, 'natural' organism?⁹⁸ We may well ask whether there is here one of those inconsistencies in Schleicher's theoretical position which have been underlined by Trnka (1952:140).

Yet another problem arises when we consider the details of Schleicher's classification theory. We have mentioned above (in 2.71) the way in which he tried to combine genealogical and morphological classifications. This is not all: most text books also refer to his evolutionary theories to the effect that in prehistoric times every inflectional language passed through a monosyllabic and an agglutinative stage. It would be easy to make use of this belief in evolution in order to argue that Schleicher extended the genealogical principle to all classification, as Darwin wanted to do, but this conflicts with all that we know about his thought. For Schleicher, who did not believe in

⁹⁷ For the bibliography on Schleicher, cf. above 2.7.

⁹⁸ Cf. Schleicher 1869:5: 'Die Sprache ist also lautlicher Ausdruck des Denkens, lautes Denken ... (cf. also Dietze 1966:65).

monogenesis, two *Ursprachen*, which belonged to the same morphological class, did not need to be genealogically related — and in fact could not, if the distinction between evolution period and decay period had to be taken seriously. Let us combine this conclusion with the point mentioned by Schleicher (e.g. in 1850:23) that the concepts of species and genus are relative: the *Sprachstämme* are to the morphological classes as species to genus, but to the *Sprachfamilien* as genus to species. In this way we find another ambiguity in the schema (in addition to those pointed out by Lepschy 1962:189).⁹⁹ The criterion used for identifying e.g. the species Germanic versus the genus Indo-European (or European) is a genealogical one and is very different from the typological criterion used for identifying the species Indo-European versus the genus 'inflectional'.

That for Schleicher typological classification was not simply a side interest is shown, inter alia, by some work which has often been neglected. His first large scale book, *Zur vergleichenden Sprachgeschichte* (1848), as well as one of his last essays (1865b), are attempts at typological analyses, in the first case of a phonetic phenomenon which is studied diachronically, in the second case of the distinction between noun and verb (cf. for this Percival 1969). It is in this latter paper that Schleicher argued more clearly than most of his contemporaries had done that a study of language plurality had to rest on a description of the linguistic categories of the individual languages, and that this task was only possible if we had a criterion which allowed us to define the categories. According to him the real problem was: are there linguistic functions which have no formal definition? Or alternatively: do we start in our description of languages from the function or from the form? Is there an 'inner form' which is independent of the external form? More concretely: can we say that a language has a noun/verb distinction when there is no formal sign of this distinction? Schleicher's answer was that we should not attribute to any language a category which is not formally identifiable in it. From our point of view what matters here is not so much that — admittedly with a more sophisticated meaning of 'form' — these views have been claimed as a conquest of the first half of the twentieth century (cf. Percival 1969), but that here the view is implied that typology depends on synchronic description and that a correct description depends on the solution of some theoretical problems.¹⁰⁰ Obviously some of the same thoughts were in Humboldt's mind too (cf., e.g., Humboldt 1903-36:IV,289), but it is more interesting to find them in a man who passed to fame as a comparativist rather than as a theoretical thinker.

⁹⁹ Lepschy (1962:187ff.) offers a very interesting discussion of some points concerning the interpretation of Schleicher's theories; among other things he mentions Schleicher's identification of language with species and the difficulties which this creates within the context of his organicistic views (for language as species see above note 20 and cf. 3.91). Interestingly Lepschy also stresses the survival of Schleicher's theories in both the contentants of the later generation, Whitney and Max Müller (*ibid.* 190).

¹⁰⁰ Even so, Schleicher does not tackle the basic problem and does not ask how can it be that while he is willing to identify his categories on a formal basis, he still feels able to use the same labels for categories of different languages.

3.71 The impact of H. Steinthal on the history of linguistics is difficult to assess. Part of the twentieth century seems to have surrounded him with a curtain of silence: Jespersen (1922 and 1962), Pedersen (1962), Mounin (1967), Robins (1967), Waterman (1971), Leroy (1971) either ignore him or barely mention him. Graur-Wald (1961: 42 ff.) and Ivić (1970: 52 ff.) are almost exceptional in devoting some space to him as a prelude to their discussions of Potebnja and (in the case of Ivić) of Marty, Gabelentz, and Wundt. Arens (1969: 242–5, 277–85) and Zvegincev (1960: I, 105–50) are far more interested, but they are isolated. The scarcity of twentieth century work about Steinthal emerges clearly from Bumann's bibliography (1965: 149–53). The short dissertation by Beneš (1958) discusses some of Steinthal's views, but with a lack of sympathy which at times comes near contempt and prevents understanding. The constant reproach, that of obscurity, woolliness, and metaphysical inclinations, levelled at him by some of his contemporaries (e.g. Max Müller and Whitney) has stuck. Even works such as de Mauro (1970a) which aim at tracing within the nineteenth century the theoretical interests of the previous century only quote Steinthal in order to reproach him for his too simple dismissal of Aristotle's theories or to put him together with Pott, Rudolf von Raumer and Brücke (de Mauro 1970a: 82, 89). In the last ten or fifteen years, Lohmann's attempt (1960) to put Steinthal in his setting, the teaching of *allgemeine Sprachwissenschaft* in Berlin, Jost's study of the concept of language as *energeia* with his chapter on Steinthal (Jost 1960: 83–92), Haselback's references to him in his monograph on Becker (1966), and Bumann's excellent monograph (1965) together with his edition of some of Steinthal's minor essays (Steinthal 1970) have slightly altered the balance (cf. also Esper 1968 *passim* and Jankowsky 1972 *passim*). Even more recently, in the lengthy introduction to her publication of the Lazarus-Steinthal correspondence, Ingrid Belke (1971) has dedicated some space to Steinthal's linguistic thought (including his theories about classification and about the origin of language) and to the differences between him, Paul, and Wundt (Belke 1971: cxiii–cxii).¹⁰¹ In spite of this recent work, it is no wonder that Horne, Kuznecov and Tagliavini simply summarize Steinthal's classification of languages without any close attempt at setting it against its theoretical background. Tagliavini (1963: 393f.) sees in Steinthal a continuator of Humboldt's psychological classification, while Horne (1966: 17f.) is eager to stress that his classification is based on a syntactical concept of 'form'.¹⁰²

Bumann's explanation of Steinthal's views on language classification follows closely that of Steinthal's own books: the earlier *Die Classification der Sprachen, dargestellt als die Entwicklung der Sprachidee* (1850), and the later *Charakteristik der hauptsächlichsten Typen des Sprachbaues* (1860). Here, as elsewhere, Steinthal finds

¹⁰¹ *Inter alia*, Belke (1971: cxiii) points out that some of Steinthal's theories changed considerably after he had read Darwin's *Descent of man* (1871).

¹⁰² More surprising is Jespersen's statement (1962: 703): 'Steinthal ... in various works tried to classify languages partly from geographical, partly from structural points of view, without however arriving at any definite and consistent system.'

his starting point in an appraisal and criticism of Humboldt's views. We have mentioned before (in 3.51) some of the points he makes. Humboldt's definition of *innere Sprachform* — we are told — is inadequate; he tended to identify it with the general form of thought without realizing that this effectively prevented him from giving any serious consideration to the problems of language plurality. Moreover he seemed to share the common assumption of philosophical and historical [i.e. empirical?] grammar that there is a set of categories common to all languages. This is just the point that Steinthal is eager to deny: it is absurd, he argued, to want to find logical categories in grammar, since language is not an expression of logical thought. There was no such thing as an *allgemeine Grammatik* and indeed there were no language categories outside the language to which they belonged. According to Steinthal the sort of typological work which looks for the different expressions of some linguistic category in different languages is misguided and is incompatible with any theory which takes seriously its definition of language as an organism. Categories must be extracted from each language, not imposed onto it a priori.¹⁰³ Thus, starting from his own psychological or socio-psychological presuppositions, Steinthal seems to have reached some conclusions which could well belong to the thirties or the forties of this century.¹⁰⁴

Equally important, from our point of view, is that Steinthal explicitly mentions the function which he assigns to language classification: it was to replace the old *allgemeine Grammatik* as the supreme task of linguistics. Its aim was to show how each individual language in its organic unity was an implementation of the concept of language (*Sprachidee*) and how all languages in their turn could be considered as organic elements of a system in which they participated in various ways according to their relationship and the value of their inner organization (cf. Bumann 1965: 110f.).

Bumann's otherwise excellent exposition does not sufficiently emphasize, to my mind, how it was just this explicit need to compare and classify linguistic organisms (i.e. organized wholes) rather than single linguistic elements and categories which induced Steinthal to reject any classification based on individual *Merkmale* and to break new ground, using simultaneously a multiplicity of syntactical, morphological, etc. criteria in order to characterize the various languages and to assign them to classes.¹⁰⁵ Yet, the point is important in the history of language classification, and perhaps of classification in general. 'La classification, comme problème fondamental et constitutif de l'histoire naturelle, s'est logée historiquement, et d'une façon nécessaire, entre une théorie de la *marque* et une théorie de l'*organisme*.' (Foucault 1966:

¹⁰³ This emerges very clearly from the short explanation given in Steinthal's book on *Grammatik, Logik, und Psychologie* (1855: 386–7). Steinthal 1850, which is closely followed by Bumann, is more long winded and less perspicuous.

¹⁰⁴ For some very structuralistic views about the meaning of words and the function of grammatical categories see Steinthal 1855: 387. Some of Steinthal's assumptions in this field may have been inherited from his teacher Heyse, whose work he edited. For Heyse's theories about structural semantics, cf. Coseriu 1967d.

¹⁰⁵ Cf. especially Steinthal 1970: 156f. [first published 1852].

158). What Steintal had to offer was obviously a 'théorie de l'organisme'. But there is more. While breaking away from the idea of a single *Merkmal* as principle of classification, Steintal foreshadowed more modern developments, which, though starting from different presuppositions, came to similar conclusions: Sapir's classification comes to mind, but we could also refer to some tendencies of statistical typology.

However, the development of Steintal's thought is not altogether clear (and has hardly been explored). Between the classificatory schema proposed in *Classification* (1850) and that of *Charakteristik* (1860) there is a sharper distinction than is often realized. Bumann (1965:113) points out, rightly, that in the latter work a greater simplicity was obtained by limiting the classification to some linguistic types — a concept and method which, as Lewy (1951) and Lohmann (1960) have shown, was first introduced by Steintal.¹⁰⁶ In an interesting article on the notion of linguistic type, Petr Sgall (1971:77) argues that in his earlier work Steintal looked for substantial distinctions (while Humboldt aimed at distinctions of degrees) but later on he preferred to concentrate on the detailed analysis of a variety of linguistic features and their combinations, without aiming at a rigid classification. Yet, the 1860 schema also introduced a straightforward distinction between *Form-Sprachen* and *formlose Sprachen*, which almost implied a return to the dualism of the younger Schlegel and to a more metaphysical interpretation of Humboldt's concept of form.¹⁰⁷ On the other hand there were, in the more recent book, some other new features in addition to the notion of linguistic type which we mentioned earlier: first, the explicit affirmation that form may be expressed in a number of ways, and secondly the practical consequence drawn from it, that Classical Chinese was listed among the *Form-Sprachen* because of its fixed word order. The necessary background for this last conclusion was a less naive view of grammar than that which stated without qualms that Classical Chinese had no grammar at all. Here, as elsewhere, Steintal's thought is marked by a mixture of new insights, some of which had a considerable part to play in the further development of linguistics, and of unprovable assumptions. Whitney's criticism was justified, but one-sided.

3.8 Three other scholars are regularly mentioned in the works quoted above (3.2): Max Müller, Misteli and Finck. The latter is perhaps outside our chronological

¹⁰⁶ This is probably correct, but we should not forget the admirable preface which Böhlingk wrote for his *Sprache der Jakuten* (1851). There he argued that any grammar should include a characterization of the language which it describes, so that this could be used for typological purposes, and advocated a study of both 'external' and 'internal' *Merkmale* for the purpose of 'physiological' (i.e. structural) classification. In his characterization of the language he was describing he listed phonological and morphophonemic phenomena (number of vowels, consonants, etc., existence of vocalic harmony) as well as a number of morphological and syntactical features.

¹⁰⁷ But Gabelentz (1891:405) pointed out that the terminology represented an advance on Humboldt's distinction between *vollkommene* and *minder vollkommene Sprachen*, in which it was still possible to recognize the survival of earlier contrasts between Hellenes and Barbarians, 'Culturmenschen und Wilde'.

limits, but the ties which join him to his predecessors, Steintal and Byrne on the one side, and Fr. Müller on the other, are so close that he cannot be ignored here. None of these authors has been very fortunate with modern scholarship: Misteli has been almost entirely ignored in spite of Koerner's recent interest in his review of Paul's *Prinzipien* (cf. Koerner 1972d). Max Müller has received some attention, mostly due to his political activity (Voigt 1967, Rüstau 1965), but his linguistic work — in spite of its earlier popularity — had partly fallen into discredit by the time of his death (1900), and has never been the object of thorough analysis.¹⁰⁸ Some basic information can be found in the pages dedicated by Terracini to Whitney (1949:73–121), in a short chapter by Jankowsky (1972:177–181), and in Graur-Wald (1961:40f.), Ivić (1970:46). As far as his typological classification is concerned, Lounsbury (1968:194f.) follows Jespersen in mentioning Max Müller's tripartite classification of the languages of the world, his evolutionary views, and his attempt to connect the language classes with different stages of the culture of society (family stage ~ isolating languages; nomadic stage ~ agglutinative or Turanian languages; 'political' stage ~ inflectional languages).¹⁰⁹ Horne (1966) does not mention this attempt at historical interpretation. It is probably worth stating here that, though Max Müller made much of it in his superficial and often contradictory essay published together with Bunsen's magnum opus (cf. Max Müller 1854), the whole suggestion was toned down in the successive editions of his *Lectures on the science of language* and did not appear at all in his 1868 Rede lecture *On the stratification of language*.

3.81 Horne (1966:25–7) starts his chapter on twentieth century views of classification with a paragraph on F.N. Finck, but the sub-heading 'Continuation of older views' is significant for his assessment. Tagliavini (1963:394–95) mentions Finck as a follower of Steintal and Misteli and as an exponent of psychological classification, but points out that in his last work he implicitly rejected his earlier classification based on different grades of 'excitability' of the speakers. Elsewhere Tagliavini (*ibid.*:418ff.) criticizes the anthropological and racial presuppositions of the genealogical classifications proposed by Finck (see above 2.93, but cf. also Koerner 1970a:93).

If we ignore the close ties which joined Finck to Ernst Lewy (cf. Lewy 1952), and the intellectual bonds which still exist between him and the group of German scholars which in the forties and the fifties of this century congregated round the periodical *Lexis* (cf., e.g., Lohmann 1960:454–6), his figure is almost forgotten. There are some exceptions: Leroy (1971:166–7) is critical but interested; Arens (1969:405–15) stresses the importance of Finck's views in a period which foreshadowed a renewal of linguistic thought. Some valuable information can also be obtained from the criticism which Jost levels at Finck's interpretation of a Humboldtian principle (Jost

¹⁰⁸ But it is certainly exaggerated to say with Dussaud (1951:10) that together with Renan and Mommsen 'Max Müller est un des trois philologues qui ont dominé leur époque'.

¹⁰⁹ For Max Müller's theories about the Turanian languages cf. *supra* 2.5; for his evolutionary views see 3.92 and note 121.

1960:100–02), and from the more violent attack of G. Pätsch (1955:8–13); cf. also Gipper (1963 *passim*) and Lehmann 1965. More recently a short article by Koerner (1970a) gives a summary of Finck's views and of his biography.¹¹⁰

In a history of language classification Finck should certainly not be neglected, but it is permissible to doubt whether a list of the schema or schemata for classification which he proposed is particularly significant. Once again, what matters more is the theoretical framework which led him to the problems of classification, and which Tagliavini and Horne ignore. Basic is his denial of the existence of language as a supra-individual phenomenon: 'gibt es doch in Wirklichkeit NUR ein ganz INDIVIDUELLES Sprechen' (Finck 1910:3). It follows that any study of language which is not limited to a single speech-act implies a classification, i.e. the establishing of particular groups or types and that classification is indeed at the centre of linguistics (*ibid.*). There is more: not only did Finck challenge the concept of language with which most of the previous century's work had operated (as we saw, he was not unique in this: cf. 2.8 etc.), but he also drew from this the consequence that the primacy given by his predecessors to historical linguistics was to be rejected. Historical grammar was in no position to explain linguistic facts: all that it did was to contrast two different *Sprachwesen*, one of which had served as a model for the other, though it had in no way originated it (cf. Finck 1905:16 and see also 1909:5 and 45 ff.).¹¹¹ Any causal explanation that it claimed to provide was — according to Finck — out of place since language transmission was simply a process through which an individual in all freedom created language using a model (or more than one model) in different ways. Hence genealogical classification could be used as a *Vorarbeit*, but it could not be the aim of linguistics and had no theoretical validity by itself. The real explanation of the linguistic facts arose from the individual characters of the *Geist*. There is a wealth here of old and new motifs, often inextricably connected, which would deserve a thorough exploration. It is perhaps too easy to label Finck as an individualist; even if we were to do so we would be obliged to explain how his position differed from, for example, that of Schuchardt.

The historian of linguistics will no doubt recognize in his thought one of the numerous attempts at a new start in linguistic theory which characterize this part of the century. The historian of classification will at least want to bring out two points: one is the importance which Finck gave to his notion of linguistic type (cf. Lohmann 1960 and see above 3.8). Sgall (1971) has pointed out that Finck's later writings belong 'to the foundations of a more modern approach to typology' and that he was clear

¹¹⁰ Short resumé of two of his works can also be found in Gipper-Schwarz (1961 ff.: I, nos. 4304, 4305). Also useful is Lewy (1961c).

¹¹¹ In Finck (1909:1 ff.) the attack is obviously turned against Schleicher's *Stammbaum* model, and the criticism of genealogical classification is toned down but not abandoned. Here too the aim of linguistics consists in explaining the more or less unitary *Redearten* of each people from their 'geistigen' peculiarities. This calls for a work of description of languages (*Sprachkunde*) and of combination of the various idioms into *Sprachgruppen*. Some of the similarities may then be explained as derived from a common model: hence the preliminary usefulness of genealogical and historical work.

about the distinction between linguistic type and linguistic class and about the fact that a given language can show a combination of the properties of different types. In other words, Finck's methodology was not too far from that of Sapir.¹¹² The second point concerns Finck's earlier psychological classification, on which it is all too easy to pour scorn. In fact this is a logical (though naive) corollary of Finck's general theory. Those scholars who had claimed that the explanation of a linguistic fact could only be given in terms of its history obviously favoured a genealogical classification, or at least a historical classification of some type; but if the principle of *genetische* (i.e. historical) *Erklärung* was to be given up it was all too natural to turn to other explanations: psychology offered one. The historical antecedents of this line of thought are clear: Humboldt's concept of language as *energeia* on the one side, Steintal's psychological interpretation of Humboldt's theories on the other.

We may support both our points with a quotation from a very different scholar: Baudouin de Courtenay. In his 1910 paper on language classification he claimed that all previous attempts at morphological classification had been failures; the correct aim was not a classification of languages but a *vergleichende Charakteristik*. He then proceeded to argue that the 'reelle Grösse' was not language, abstracted from the speaker, 'sondern DER MENSCH, ALS TRÄGER DES SPRACHLICHEN DENKENS'. And he continued 'Wir sollen nicht Sprachen klassifizieren, respektive vergleichende charakterisieren, sondern eine VERGLEICHENDE CHARAKTERISTIK DER MENSCHEN nach dem ihnen eigenen sprachlichen Denken geben' (Baudouin de Courtenay 1910:55).¹¹³ There is considerable difference between Baudouin's suggestions and the 'excitability' of which Finck spoke in his early works, but we should recognize that the two scholars shared a number of presuppositions and that these did indeed diverge from those of, for example, Schleicher.

3.9 Before we turn to a general overview of the typological classifications of the nineteenth century we must consider some other features which we have occasionally mentioned without an explicit discussion. Two problems call for attention. First, most classifications are accompanied by an evaluation of the language types or language classes in terms of some particular criterion. In other words, each type or class is assigned a certain rank according to a definite scale of values. Secondly, a classifica-

¹¹² Cf. below 3.10 for Sgall's appreciation of Finck. So far there has been no attempt to investigate how much Finck's thought was influenced by Gabelentz, but cf. Coseriu 1967c:75 and for Gabelentz's definition of language type *ibid.* 95.

¹¹³ Baudouin's formulation in an earlier programme of lectures (1877–78; cf. Baudouin de Courtenay 1972:119) is nearer to Finck's early views: 'Internal form simultaneous with phonetic form. However, one should define first the psychological character of language and then its external form as a reflection of internal (psychological) activity'. Baudouin's views on the subject changed in the course of time, but he never abandoned his psychological interests. In a 1930 article (cf. Baudouin de Courtenay 1972:296 ff.) he distinguished between historical affinity (mostly based on genetic transmission), affinity based on geographical continuity, and similarities common to all mankind, and concluded by arguing that in all three types of affinity the explanation of facts required 'simultaneous reference to history and geography, physiology and psychology, and even to physics and mechanics'.

tion may be static or dynamic, that is to say it is assumed either that the linguistic types or classes are unchangeable or that they are derived from each other through a continuous process of alteration.

It has often been pointed out that, whatever the criterion, the Indo-European languages always seemed to appear at the top of the scale. The chauvinism which prompted this conclusion has been repeatedly attacked and rejected (though occasionally keeps cropping up). Yet, in the enthusiasm for the truth, the duties of the historian have been forgotten. The problem is no so much that of destroying an obsolete belief as that of understanding it. If so, a double question arises. Why did contemporary scholars feel this almost compulsive need to arrange all linguistic types on a given scale? And what justified one particular arrangement in preference to another? To my knowledge little or nothing has been done to provide an answer to the first part of the question — and indeed it is likely that more than one answer is necessary. From a linguistic point of view the influence of the aesthetic classifications of the previous centuries and the belief in an absolute idea of language, variously realized in different languages, both come to mind as possible causes. Others could be added, but would still not be sufficient. It is possible that here too a substantial part of the explanation would have to depend on a comparison with the classifications of the natural sciences. A sentence of Pott (1856:197) may clarify this point: 'Auch kann zwar in der ersteren Rücksicht, in physiologischer, aber nicht eigentlich in genealogischer nach einer stufenmässigen Rangordnung und Werth-Abschätzung der Sprachen vom Niederen zum Höheren und edler Gebildeten hinauf, wie etwa im Thierreich — höchstens nach mehr oder minder edler Abkunft — gefragt werden.' At the same time we find that the comparisons of the isolating languages with the zoophytes of the animal kingdom keep appearing again and again.

As we have seen (3.51), Gertrud Pätsch has tried to give a solution to the second part of our question in the case of Wilhelm von Humboldt; it is likely that *mutatis mutandis* most of her conclusions can be applied to the views of other scholars (but for Bopp, see above 3.6).¹¹⁴ It may be worthwhile to remember here that, according to G. Pätsch, Humboldt was impelled towards his evaluation by extralinguistic reasons. We have discussed this point before (3.51); here it may be useful to show how at the beginning of the twentieth century scholars had reached a very different position. At the end of his booklet on the tasks and division of linguistics F. N. Finck (1905:54–5) mentioned the question of the respective values of human languages and concluded that the problem was legitimate, but did not belong to linguistics any more than the question of the relative usefulness of domestic animals belonged to zoology; if anything, it had to be discussed under the heading of *Völkerkunde*, since this discipline was concerned inter alia with the influence of language on the nature of peoples. There is a general lesson to be extracted from this: when linguistics moves towards a

¹¹⁴ For a discussion of Steintal's evaluative criteria see Bumann 1965:114f. and Belke 1971:cxiiif.

clearer conception of its own autonomy the problem of language evaluation assumes secondary importance.¹¹⁵

3.91 A satisfactory history of the evolutionary or anti-evolutionary views which accompanied typological classification has yet to be written. In the history of scholarship the question is closely related to that of monogenesis versus polygenesis and in the last resort to all religious implications which accompany the biblical history of the Tower of Babel.¹¹⁶ Yet, in the course of the nineteenth century it became more and more possible, both in theory and in practice, to preserve an agnostic attitude towards monogenesis and its religious implications, while taking a definite stand on the problems of evolution.

In two interesting essays on *Language and evolutionary theory* and *Language and evolution* Greenberg (1971:78–92, 106–25) has pointed out that in the history of linguistics, even more than in that of the natural sciences, it is necessary to distinguish between 'transformationism', which is contrasted with 'creationism' and assumes that 'species develop from other species' (*ibid.*:106), and the natural selection theory (or theories) which asserts that 'natural selection has played a major role in producing this result'. Associated with the latter view, according to Greenberg, is often a concept of advance or progress which need not accompany simple transformationism. No doubt Greenberg is right in stressing that linguistic transformationism arose well before Darwinism, but I find it difficult to accept his statements that in linguistics 'the triumph of transformationism over creationism' was most clearly marked by the discovery of the interrelationship of the Indo-European languages, and previously by that of the Semitic and Finno-Ugric languages' (*ibid.*:111). In view of Greenberg's own definition of transformationism, the statement is only valid if individual languages are equated with species — but this is just one of the problems which linguistic classification had to solve.¹¹⁷ Languages may also be compared to individuals; if so, the differences between, e.g., Latin and French, or Indo-European and Latin, could be compared to the normal differences which obtain between father and son (or mother and daughter) and the problem of the development of a species into another species does not arise. In the history of linguistics this particular position could be represented by Hervás, who recognized the existence of a number of language families, but was a creationist and denied that the members of the same family could show any structural difference.¹¹⁸

¹¹⁵ Obviously this conclusion does not apply to that type of evaluation which is based on particular views of historical development, but for it see below, note 120.

¹¹⁶ A complete account of the theories about the subject from the beginning until modern times can be found in Borst 1957–63 (especially III,2 for our period), which is a mine of information on a number of problems. However, Borst is not exclusively concerned with the development of linguistic thought as such, but extends his enquiries to literature, poetry, etc.

¹¹⁷ For the identification of language and species see above, notes 20 and 99.

¹¹⁸ Cf., e.g., Hervás (1787:15f.): 'Bisogna dunque dire, che la perfezione, o rozzezza dell'artificio degli idiomi non ha nessuna relazione alla civiltà, o rusticità delle nazioni: hanno questa relazione le scienze, e le manifatture; ma non gli idiomi, che troviamo sostanzialmente invariabili. Osserviamo in nazioni barbare, e civili, i dialetti provenienti d'una stessa lingua madre, e sempre vi ritroveremo l'ossatura del corpo onde furono formati.'

Greenberg is obviously right in calling for some clarification, but he does not go far enough. Even if we retain his distinction between natural selection theories and transformationism, there are some other distinctions to make. First, we must acknowledge that there is an element of transformationism in any theory which at all recognizes the existence of language change.¹¹⁹ From this point of view, if interest in — and indeed emphasis on — linguistic change is synonymous with transformationism, the whole of the nineteenth century is transformationist.¹²⁰

Secondly, there is a different version of transformationism which assumes, in a somewhat ill-defined way, that some types or classes of languages (without necessarily establishing a distinction between the two) can arise from each other. Diderichsen (1960:125) mentions the 'genetic' theories of the eighteenth century, which postulated that languages developed from a simpler to a more complex stage, but which made no attempt to establish a genealogical link between any exponent of the various types.

Thirdly, some transformationist theories are far more explicit (in spite of some remaining ambiguities). These could only arise (and survive) in a period in which linguists were convinced, a) that it was possible to give a non-arbitrary definition of the notions of language type and typological class, and to base a typological classifica-

¹¹⁹ We may notice here that this recognition is not as immediate as it may appear. The layman is aware only of very minor and unimportant variations, and does not realize the magnitude of the changes except when he is shown documentary evidence provided by written tradition. Interestingly enough as late as 1863 Sir Charles Lyell found it useful to dedicate some pages of his book on 'The geological evidences of the antiquity of man' (1863:457ff.) to wondering how difficult it would have been for a philologist to convince a lay audience that languages did indeed change. However, at a certain level of learning, evidence proving the basic facts of change had been known in the West for a very long time. Even if we forget the classical theory that Latin derived from Aeolic, we may remember that the derivation of the Romance languages from Latin (with the possible, but doubtful, exception of Rumanian) was never doubted.

¹²⁰ Of course some distinctions are necessary here too. Even within this mild form of transformationism, which is perfectly compatible either with an interest in genealogical classification and an agnostic or negative position towards typological classification, or even with a study of change within the same typological class, it is permissible to ask what direction the change is likely to take. In the first part of the nineteenth century it was customary to speak of language growth, maturity and decay, in terms which were obviously inspired by the identification of language with an organism or a living being, but which often remained ill defined. Alternatively, change could be conceived of as a linear process, often with an associated idea of progress. In both cases, then, an evaluative conception seemed to be associated with linguistic development. However, if we try to understand what was in effect meant by 'growth' or progress in both types of analysis, we often find that the concept is tied to a historical view of the root-development and of its composition processes. For Bopp, for instance, (who distinguished typologically between languages whose roots were monosyllabic and did not admit of composition and languages which also had monosyllabic roots but did admit of composition and agglutination), it made perfect good sense to speak of growth with reference to his second class. His thesis was that linguistic change caused a shift from an earlier stage in which a small amount of agglutination occurred to a later stage in which inflection prevailed. Whitney's position was in some respects similar: cf. Silverstein (Whitney 1971:xvii f.), who rightly observes that 'in such a unilinear evolutionary schema [as that of Whitney] it is fitting and proper to speak of Indo-European languages as the "highest" or most developed, since no other languages investigated by his day were inflective in the same manner, and also because the farther back we trace the history of forms in, say, Indo-European, the closer we get to a completely syntactic — formless — tongue, the root or radical stage of language'.

tion of languages on it, and b) that it was possible to demonstrate that two languages were genetically related. It is only at this stage that the contrast between transformationists and non-transformationists can be clearly stated. The former argued that there was nothing to prevent a language from going through different typological stages while remaining in a meaningful sense of the word the 'same' language. They also assumed that the different linguistic types had in fact arisen through a slow process of development from the lower to the higher type and that the representatives of the higher types still preserved traces of the earlier stages. Yet, at this stage an element of ambiguity crept into the theory. In spite of their general views a number of transformationists were unwilling to admit the possibility that two existing languages could be genealogically related, while belonging to two different language classes.

A number of points in these theories were of direct concern not only to the typologists but also to the historical linguists. The extent of variation caused by linguistic change, and the effect that this could have on linguistic structure, was their problem too. Similarly, it was their evidence that was required in order to elicit the traces of earlier types in the attested phases of languages. The attempt to substantiate the claim that even the 'organic' Indo-European languages showed an agglutinative stratum had first come from Bopp. Once more we return to the observation that the histories of typological and genealogical classifications cannot be kept entirely apart.

3.92 Transformationist and evolutionary theories have often been discussed in recent literature (cf. also Jakobson 1958:18f.), but there is not always agreement about the plain statement of the facts. Lounsbury (1968:191), who follows Jespersen (1962), states that Humboldt was the first scholar to introduce 'an explicit theory of linguistic development to go along with the concepts on which Schlegel [*sic*] typology was based'. Kuznecov (1960:39) appears to share this view, but Sharadzenidze (1958:514 [Russian summary]) argues that the whole idea of development was foreign to Humboldt's conception of the morphological classification of languages. In a pamphlet dedicated to the concept of evolution in linguistics, Leroy (1950:17) states that the tripartite classification, which was originally static, first became dynamic with Schleicher. To sum up, it seems that Schleicher and Max Müller are the two authors whose transformationist views have never been doubted. Modern scholars are eager to stress Schleicher's belief in a prehistoric period during which all languages progressed towards their more developed expression and which was followed by a historical period of decay. As we have seen (2.7), Maher (1966) has pointed out that this form of evolutionism or transformationism preceded that of Darwin and that Schleicher's views are nearer to those prevailing at the beginning of the century than to those inspired by Darwin himself.¹²¹

¹²¹ Max Müller repeatedly argued in his Rede Lecture (1868) that there was no clear dividing line between the three basic linguistic types (isolating, agglutinative, and inflectional), and that this fact could be used as a proof of his transformationist views. However, agreement among the various followers of Schleicher was far from complete. In the second part which Max Müller added to the reprint of his lecture (1875), he took issue with Georg Curtius's somewhat too rigid conception of seven different periods of growth in the prehistory of Indo-European (cf. G. Curtius 1867). Max

It seems then that the real historiographical problems concern the earlier period. We have mentioned Hervás's views above (3.91), but here we should not forget the transformationism of Adelung, who argued that all languages were on a developmental scale (cf. *supra*, the second type of transformationism); in this he was preceded by Rüdiger, as Steinthal (1860:4) was at pains to point out. We have also seen (3.4) how F. Schlegel made a sharp distinction between organic and non-organic languages and at first denied the possibility of a development from one type to the other. In view of this background the agnosticism of A. W. Schlegel appears all the more remarkable (cf. above 3.41). But we are now led back to Humboldt and to the contrasting interpretations of his views. Gertrud Pätsch (1967:113f.) has argued that part of the difficulties arise from oscillations in Humboldt's own thought which are reflected in his writings. But there is another source of confusion. As early as 1855, Pott (1855:412) argued that we must distinguish between two different concepts, that of typological 'Stufengang' or 'stufenweise' progress, and that of genetic-historical development. Neither of the two concepts, according to Pott, is logically dependent on the other, and Humboldt, who recognized different degrees of 'completeness' in the different language types, remained agnostic about the possibility of a historical development from one class to the next (see e.g. Humboldt 1903-36:VII, 344, but cf. also the more ambiguous observations of VI, 275). It seems to follow that, though the terminology is at times confusing, we should probably accept Humboldt's statement at their face value and refuse to commit him to any evolutionary or transformationist theory (cf. also Ruprecht 1963:231f.).¹²²

The position of Steinthal was not very different. Here too, however, it is difficult to reach a clear interpretation. At times he seemed to imply that a shift from e.g. agglutinative to inflectional languages was possible (cf. Bumann 1965:112), but we must remember that for Steinthal agglutination and inflection were not in themselves marks of different linguistic types; on other occasions he was obviously concerned with an ideal ordering of the linguistic types. A sentence by Finck may perhaps show that even at a much later stage the problem of distinguishing between these two forms of development (ideal and historical) was still very much alive. At the beginning of his

Müller's point was that 'the growth of language does not lend itself to a chronological division in the strict sense of the word ... Inflection does not put a sudden end to combination, nor combination to juxtaposition' (1875:144). He even went so far as to state that 'combination', the main power which operated in the prehistory of languages, 'is not extinct even in our own time' (*ibid.*). The inconsistency is obvious; if Max Müller had seen the implications of this point, he should have concluded (like Gabelentz: cf. note 124) that language development moved in a sort of never ending spiral and should have refused to acknowledge any distinction between a prehistoric period of growth and a historical period of decay. Curtius was more rigid and his picture was less plausible but more consistent.

¹²² If this is so, there is an interesting side result. Humboldt, as we know, accepted Bopp's agglutinative theory. Now, if he had also accepted a division into typological classes based on the contrast between agglutination and inflection, this would have implied that he was as much of a transformationist as Schleicher, which is just what we have been arguing against. It should follow that Steinthal's criticism of the traditional interpretation of Humboldt is right (cf. above 3.5) and that Humboldt did not classify languages into isolating, agglutinative and inflectional.

booklet on *Die Haupttypen des Sprachbaus* he felt the need to point out that the ordering of the linguistic types 'entspricht nicht etwa einer historischen Entwicklung, auch nicht einer ideellen in dem Sinne, dass ein Aufsteigen von Einfachsten zum Kunstvollsten oder vom Urzustande zur vollendeten Geistesbildung veranschaulicht werden sollte' (Finck 1910:6).

The conclusion seems to be that, while there is ample evidence in the history of linguistics for our first and second versions of transformationism, far fewer scholars were committed to the third — and more powerful — version. Schleicher was among these few, but even he denied the possibility that two genealogically related languages belonged to two different typological classes and relegated that form of development which involved typological change to the prehistoric period. In the historical period only 'decay' could occur, and apparently 'decay' did not allow for a shift from one typological class to the other (cf. *supra* 2.7).

3.93 We are then left with yet another problem. What justifies this reluctance to assume that within the limits of our experience linguistic change may involve a change of typological class?¹²³ Clearly, there is no single answer to this question, and here we can only suggest some possible causes. As we have seen, the typological classification of the nineteenth century was based on grammar; it is likely that in the early period at least this reluctance was justified by the importance given to grammatical similarity (in the ambiguous sense which we discussed in 2.5) as the main, and perhaps the only, criterion used to prove genetic relationship. Another factor which may have been relevant was the lack of an explicit distinction between synchrony and diachrony. Any distinction between linguistic types or classes was bound to be descriptively based, but at a period in which the emphasis was on history, and in which history seemed to offer the only acceptable explanation of any synchronic phenomenon, a distinction which had no diachronic validity was likely to be challenged as arbitrary. Hence for any typologist the temptation must have been either to reject the third version of transformationism, thus guaranteeing the 'non-arbitrary' character of the whole classification, or to project it back into mythical times, as Schleicher did. This last solution was soon exploded; the neogrammarians' vociferous propaganda for the so-called uniformitarian hypothesis, i.e. for the theory that the development of

¹²³ Meillet (1938:169f.) was well aware of the beliefs generally held round the middle of the century. A propos of Renan he wrote: '... au temps, où Renan a fixé ses idées, on croyait couramment à la pérennité des espèces. Et l'on admettait volontiers pour chaque groupe de langues un type idéal — situé assez loin dans le passé — dont les langues attestées même à date ancienne seraient des déformations plus ou moins prononcées. On était loin de la conception moderne suivant laquelle tout état linguistique est simplement un moment de transition entre un état antérieur et un état suivant, et où l'on constate des changements radicaux du type linguistique d'une période à l'autre d'une même langue. Comme tous les savants de son temps, Renan croyait donc à la fixité des types linguistiques. A cet égard, il va très loin, allant jusqu'à écrire que 'les caractères de famille sont immuables'. If Renan was certain of his ground, Pott, who is perhaps more representative, was considerably worried by the problem and discussed it repeatedly, but he too ended taking refuge in the old saying 'Art lässt nicht von Art' and came down against the possibility that a language could change type (cf., e.g., Pott 1856:204ff.).

language had always occurred in similar ways and similar conditions, was bound to kill it at the roots.¹²⁴

Here we cannot go into details, but that our suggestions are not entirely wrong can perhaps be shown by a reference to two linguists, Saussure and Baudouin de Courtenay, both of whom have received credit for the clarity with which they distinguished between synchronic and diachronic study. Well within the limits of the twentieth century they still felt that the subject called for some strong pronouncements. In the notes on which Saussure's *Cours* is based we read: 'poser une question de caractères dans une famille de langues, c'est s'attendre à une réponse non absolue, parce que le temps peut amener changement total' (Saussure 1967-68:I, 510) or 'On ne peut pas attacher une famille non plus à un type, à une classe préétablie, comme par exemple le type flexionnel et le type agglutinant; aucune garantie qu'une langue, qu'une famille garde ces caractères' (*ibid.* 511); and elsewhere 'En reconnaissant que la prétention de Schleicher de faire de la langue une chose organique (indépendente de l'esprit humain) était une absurdité, nous continuons, sans nous en douter, à vouloir faire d'elle une chose organique dans un autre sens en supposant que le génie indo-européen ou le génie sémitique <veille> sans cesse <à> ramener la langue dans les mêmes voies fatales' (*ibid.*:514). In 1910 Baudouin was even more explicit: '... Die landläufige sprachwissenschaftliche Theorie von der Stufenmässigkeit in der Entwicklung der sprachlichen Formen erblickt darin aber einzig und allein eine Vervollkommenung d.h. einen Übergang von einer noch ganz unvollendeten 'Isolation' durch eine mehr vollendete 'Agglutination', zu der vollendetsten 'Flexion'. ... Die Erfinder ähnlicher 'morphologischer Klassifikationen' schliessen ihre Augen vor evidenten geschichtlichen Tatsachen. Es ist ja, so recht eigentlich die englische Sprache eine 'einsylbige', nebensetzende, 'isolierende' Sprache, wie die chinesische. Und doch gehört die englische Sprache zu den 'indogermanischen' (arioeuropäischen) Sprachen. ... Aus dem Vorhergesagte erhellt, dass unsere vermeintlichen Evolutionisten die Evolution gerade dann vergessen, wenn dieser Begriff sich sozusagen selbst zur Anwendung anbietet. ... Wir sehen also dass die LANDLÄUFIGEN SYSTEME MORPHOLOGISCHER KLASSIFIKATIONEN der Sprachen GEGEN DEN richtigen BEGRIFF DER EVOLUTION VERSTOSSEN' (Baudouin de Courtenay 1910:53). We have seen above the conclusion of this paper: 'morphological' classification was antiquated and had to go. The real aim was that of a comparative characterization of languages (*ibid.*:54).

3.10 As we have seen, few attempts have been made to give an overall view of the development of typological classification. It is frequently said that there was an early

¹²⁴ The position of Georg von der Gabelentz was different. He was ready to point out (1894:5) that there were both isolating and agglutinative languages which belonged to the same family, and he assumed that all languages moved repeatedly from agglutination through inflection to isolation and back again, this movement being a spiral-like development determined by two main forces which direct language change: *Bequemlichkeitstrieb* and *Deutlichkeitstrieb* (cf. Gabelentz 1891:250ff., and see also Sgall 1971:77).

period in which typological and genealogical classifications flourished side by side and that at a later stage the former fell into discredit. We have tried to show that any such statement is only a partial account of the evidence, if it does not emphasize the hesitations which accompanied the beginning of a distinction between the two types of classification. Moreover, all too often it is implied that the only typological classification envisaged was that which proposed a partition of languages into isolating, agglutinative and inflectional, i.e. the traditional morphological classification. Yet it is clear from what precedes that there were also suggestions for other schemas, intended to classify languages from a non-genealogical point of view.¹²⁵ In fact, if we ignore its great vogue at a lower level of popularization, it is difficult to pinpoint any one time at which the traditional form of morphological classification was generally accepted. A.W. Schlegel proposed it, but did not dwell on it; Humboldt was too cautious to commit himself (even if some of his contemporaries believed otherwise), Bopp's views were somewhat different, and Pott tended to be sceptical or at least agnostic. This is not all: criticism was not late to come. We have already mentioned some of the reasons which justified the rejection of morphological classification in the second part of the century, but it should be added here that the tripartition could also be found wanting in two basic senses. First, it was possible to prove that real languages could show both agglutination and inflection, or isolation and agglutination, or any other combination of these features. In other words, if what was required was a mathematical partition of the set of all languages, properties like agglutination, inflection, etc. were not sufficient to establish it. Moreover, as it was pointed out before, since it was not clear whether a synchronic or a panchronic classification was being aimed at, those scholars who were not willing to commit themselves to a strong version of transformationism found that the current belief that inflection derived from agglutination was a stumbling block which prevented them from accepting the whole schema. For some this was no impediment, and indeed it was the transformationists like Max Müller and Schleicher who gave the tripartition a new lease of life.¹²⁶ However, as soon as the classification appeared to be necessarily tied to an idea of historical development, it was possible to attack it on empirical grounds. Could the development be proved by means of the normal techniques of reconstruction?

The second criticism came at different times and in different ways from a number of scholars. Nineteenth-century linguists prided themselves on having moved a step further than their predecessors. They were interested in a non-arbitrary non-artificial classification, which touched the real nature of language; but were agglutination, inflection etc. properties which could justify this claim? The contrast of *Beziehungen* and meaning (to use Schleicher's formula) was often identified with that of form and content: Schleicher had argued that since this contrast was essential to human thought

¹²⁵ I feel reluctant to accept Fr. Müller's distinction between psychological and morphological classification (see above 3.2), since the classifications of Steinthal and Misteli were as much based on linguistic data as was the standard tripartition into isolating, agglutinative and inflectional languages.

¹²⁶ Cf. above, note 121.

it could provide a non-arbitrary non-artificial criterion for classification. But in the 1850s Madvig pointed out that it was a gross mistake to argue that the relation of ending and stem in inflectional languages was one of form and content; inflectional endings could be as relevant to the content as the stems: 'Das, was durch die Biegung bezeichnet wird, ist entweder eine besondere Modifikation des Inhalts der Vorstellung, wie dieser durch das Wort selbst gegeben ist (z.B. die Mehrzahl, die Passivauffassung), oder eine Relation ausserhalb des Inhalts' (Madvig 1971:171 [first published 1856]). Steinthal had shown similar signs of disquiet.¹²⁷ In other words we do not need to wait until the last quarter of the century to see the first attacks on morphological classification: the discussion continued all through the century.

The conclusion seems to be that the standard historiographical account is somewhat too simple. First, it is mistaken to have a monolithic view of morphological classification itself, for there were sufficient differences among its upholders to justify individual treatments of their theories and of the motives which determined them; secondly it is equally mistaken to identify all non-genealogical classification of the century with morphological classification.

In a recent article Petr Sgall (1971) has given a more articulated and altogether more faithful picture of the development of typological classification. His view is that with Humboldt, and before him with Fr. Schlegel, linguistic thought became interested in a notion of language type which was not yet distinguished from that of language class. While Humboldt saw only a distinction of degrees between the various linguistic types, Fr. Schlegel aimed at an effective partition in terms of types, and so did Steinthal in his early work. At a later stage, however, Steinthal, like Humboldt, renounced his rigid classification and preferred to concentrate on a more detailed analysis 'of the variety of typological properties and of their combinations in individual languages' (*ibid.*:77).¹²⁸ Much later Finck established a clear distinction between language type and language class, and acknowledged that the individual languages could combine properties belonging to different types. Sgall's conclusion is that at this stage typological classification has ceased to aim at a partition of the set of all languages into subsets corresponding to their types, but aimed rather at a more complex analysis. However, according to Sgall, it was only Sapir who 'came to the conclusion that it was necessary to classify languages in various manners, corresponding to various groups of typological properties' (*ibid.*). The result was a final classification which could be stated

¹²⁷ Yet other criticisms were put forward by R. de la Grasserie (1889-90: II, 296-7): 'D'ailleurs elle [*scil.* 'morphological classification'] ne péchait pas seulement parce qu'elle ne prenait pour terme de comparaison que le mot vide auxiliaire, en excluant tout le reste, mais encore en ce que l'école qui l'avait adoptée n'a jamais pu définir ce que c'est que la flexion et dire si celle-ci consistait dans la modification vocalique de la racine, ou dans celle de la syllabe thématique, ou dans celle de la désinence, ou dans la soudure de ces trois parties ou des désinences entre elles; enfin l'écart qui existe entre l'isolement et l'agglutination est immensément plus grand que celui qui sépare l'agglutination sans flexion de l'agglutination avec flexion, de telle sorte que la division tripartite aurait dû céder en tout cas la place à une division binaire suivie dans un de ces termes d'une subdivision'

¹²⁸ This would require further study, but see above, 3.71.

'in terms of a Cartesian product of those partitions of the set of languages that correspond to the groups of typological properties' (*ibid.*).

Here too there are some observations to make. In a sense Sgall is on the same wavelength as Baudouin, who argued that the efforts towards language classification had to make way for attempts at language characterization. Presumably Sgall would be willing to accept that this is the direction in which typological studies (or rather that part of them which was interested in classification) moved during the nineteenth century. But once more it should be pointed out that the development was not linear. As we said before, the problem was complicated at first by an insufficient distinction between typological and genealogical work, and later on by the wish to believe that a family of interrelated languages could only count as a subset of the set of those languages which showed the same typological property or properties. At this point development could occur in different directions, not necessarily correlated. The possibility of defining or redefining the typological properties involved could be considered; Steinthal's attempt to get away from the standard morphological classification and to introduce a number of additional criteria is a step in this direction. Transformationist views could be introduced in an attempt to solve the ambiguity between genealogical and typological classification. Alternatively, genealogical problems could be left aside and research could turn towards a language characterization which would either be sufficient in itself or would count as a first step in the direction of classification (well before Baudouin, Böhlingk seemed to have reached this stage). Finally, extra-linguistic criteria could be appealed to within particular theoretical frameworks.

As a corollary to the first of these possibilities, we should perhaps mention R. de la Grasserie's attempt to provide a number of provisional and subjective classifications in terms of individual features (phonological, morphological, syntactical, etc.) and to argue that this preliminary work could be used as the foundation for a 'classification totale, naturelle et objective' of all languages (la Grasserie 1889-90: II, 335). The principle to be used for it was that of the 'subordination des caractères' (cf. Cuvier's principle of 'corrélacion des formes') and the model that of the biological sciences. In other words, la Grasserie suggested that typological features occur in clusters: e.g. some languages which show vocalic harmony turn out to be also agglutinative and 'enveloppant', as if there were a sort of necessary correlation between these properties. Why this was so — la Grasserie continued — was doubtful; it was possible that, as the evolutionists argued for biology, so too in the case of language the answer was to be found in genealogical relationship. However that might be, nonetheless, if the principle was valid, it could provide an obvious criterion on which to base a classification. It is apparent how much — in 1890 — the discussion was still being conducted within the tradition of nineteenth century typological speculation, but another aspect is also noticeable. There is a sense in which R. de la Grasserie's findings anticipate the current interest in implicational universals (cf., e.g., Jakobson 1958:20ff.; Greenberg, Osgood, Jenkins in Greenberg 1966:xix-xx, xxiv-xxvi).

In this respect too the work of la Grasserie, though not prominent perhaps for depth or novelty of enquiry, was symptomatic of the typological classification of the whole period.¹²⁹ In spite of its numerous hesitations, contradictions and false starts, this type of work fulfilled an important function in the history of linguistics. One of its achievements was that it obliged scholars not to forget the existence of theoretical and descriptive problems. If we ignore its existence we do so at our own risk; it means that we effectively prevent ourselves from understanding much of the background of this century's views on general linguistics.

4. CONCLUSION

4.1 We may be brief. Most of the conclusions that we want to draw at the end of this survey are, or should be, implicit in what precedes. Since we still lack a monograph which traces the history of language classification from the Renaissance or even from the beginning of the nineteenth century until modern times, we have been obliged to concentrate on work bearing on the general history of linguistics in the nineteenth century. We have inherited from the last century and from the first half of this century a number of works concerned with the concrete results of linguistics. In practice this means that we are relatively well acquainted with the technical progress of one particular branch of comparative studies, that which concerned Indo-European languages. Yet, even in this well trodden field we remain ignorant about methodology and theory. The last few years, with their renewed emphasis on history of linguistics, have produced a steady flow of books and articles which have gone some

¹²⁹ Some of the points made by la Grasserie were criticized, and some others explored in greater depth, by Georg von der Gabelentz (1894) in a remarkable article which appeared posthumously — and in which la Grasserie was not mentioned. Gabelentz was sceptical about the possibility of applying Cuvier's method to linguistics: 'Es leuchtet auch ein, dass gewisse Züge in der Physiognomie der Sprachen, zumal lexikalische, stilistische und syntaktische, besonders charakteristisch sind. Geht man aber weiter, will man es der Zigeunerin nachmachen, die aus den Zügen der Handfläche den ganzen Menschen deutet, oder einem Cuvier, dessen Geist aus dem einzelnen Knochen das ganze Tier aufbaute, — misst man die Theorie an der Thatsachen: so scheint es bald, als hätte man nur die traurige Wahl, sich sofort für insolvent zu erklären oder mit Kunstmitteln Wechselreiterei zu treiben bis der Bankerott von selbst ausbricht' (*ibid.* 4). However, in the same paper Gabelentz (*ibid.* 5) observed that the frequent cases in which two or more non related and geographically remote languages showed identical or similar clusterings of linguistic features could hardly be due to chance. He explained that we should aim at a series of statements like: 'die Erscheinung A trifft mit so und so grosser Wahrscheinlichkeit mit B, C, D, usw. zusammen, selten mit E, nie mit F' (*ibid.* 5-6). And he continued (*ibid.* 6): 'Und dies ist die Statistik, die ich zunächst verlange. Es fragt sich: Ist sie schon jetzt erreichbar? und was würde mit ihr erreicht?'. The immediate aim for Gabelentz was to establish a programme of future work; its first task was to be a purely descriptive one, that of formulating a sort of questionnaire 'der kategorienweise alle grammatischen Möglichkeiten erschöpft'. The gist of the whole article is clear; the study of these linguistic features with their universal or statistical implications cannot lead us to a classification, but is a basic prerequisite for a greater knowledge of language differences and similarities. Like Baudouin, Gabelentz is obviously more concerned with language characterization than with language classification in the strict sense of the word.

way towards closing the gap. A great deal still remains to be done. In particular it is easy to predict that in this field too it will soon become necessary to return from theory to practice and to see how the two overlap or disagree. At the moment this remains unknown territory. Humboldt's views, to give just an example, have been repeatedly discussed and analyzed, but little has been written about the way in which he tackled his descriptive and comparative problems. Similarly, though we know something about the theoretical views of the neogrammarians, and though we know a great deal about their concrete results, we are still in ignorance, for instance, of how, if at all, they applied their psychological theories to the reconstruction of linguistic history.¹³⁰

Yet, at the moment the worst gap in our historiography concerns non-genealogical work. Morphological classification is often mentioned in 'Introductions to linguistics', but only to be immediately rejected. The result is that we still lack the basic preconditions of any historiographical work: a knowledge of the data and an understanding of the background. The strictures bestowed by Léon Wagner (1950-51:123) upon the historians of Romance philology are applicable here too — not to any particular individual but to the situation in general: 'Chez tous les critiques perçue ou du dédain ou une moquerie à l'égard des 'irréguliers', des 'rêveurs', des 'fous'. J'ai tenté de dire que l'on ferait mieux d'essayer de comprendre ces bizarreries.' In this survey we have tried to show the number and the magnitude of the problems which still remain open; we have also made an attempt to recognize a line of development or to endorse views put forward by others. Perhaps all this is premature: what we need at the moment is a series of detailed analyses and a correct assessment of the data available. One point, however, is worth making; if we do not make a serious attempt to explore the problems and the discussions which centered round typological classification we shall lose any chance we might otherwise have of fully understanding the development of genealogical classification and later on that of areal linguistics and modern typology.

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¹³⁰ From this point of view Brugmann's later syntactical work would repay a full study.

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