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Last Papers in Linguistic Historiography

E.F.K. Koerner

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Volume 128

E.F.K. Koerner

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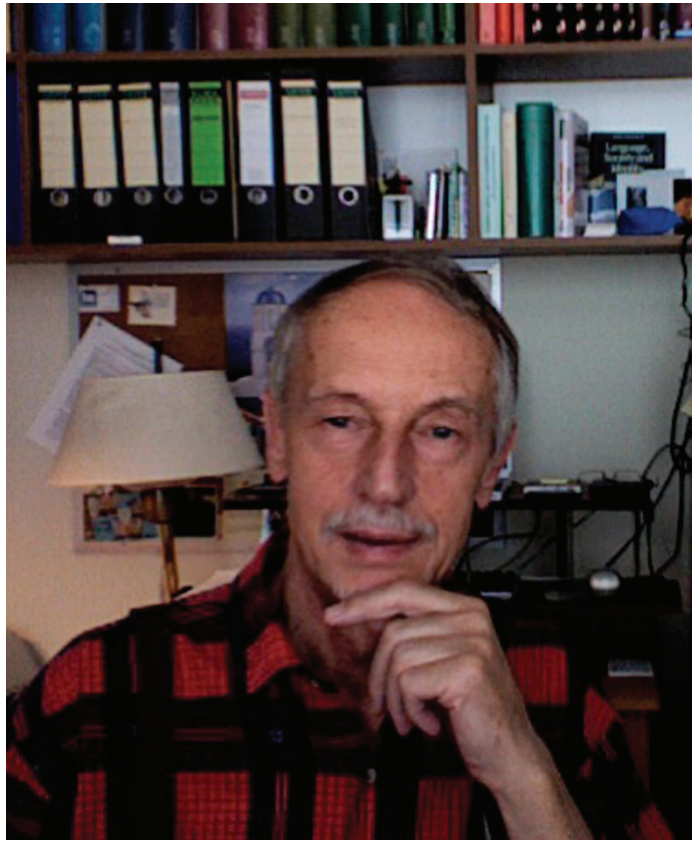

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Preface

The title of this volume – *Last Papers in Linguistic Historiography* – hardly requires an explanation, even though some readers may wonder why I should not have called it *Final Papers in Linguistic Historiography*. Be it as it may, having served as the editor of an internationally established journal for 46 years and being in the 82nd year of one's life, it is understandable that there must be an end to staying at the front for such a long time. (When the first 2021 issue of *Historiographia Linguistica* becomes available, the reader will see what provisions have been made to secure the continuation of the journal – under new leadership and an updated associated team.)

'Final' would be understood that the opinions expressed in these 8 chapters would not permit any further discussion. On the contrary, they are and remain open for discussion, extension, revision, and, of course, criticism.

Looking at the table of contents, there are two parts containing four items each. The first makes an effort to summarize what has been said before, where in my view we came from and now stand within linguistic historiography, and thirdly what further intellectual development could be considered.

The second chapter picks up where the third point in the first had left off. In fact, it is a bit longer than the first and certainly invites critical discussion.

The third chapter is, unusual at first sight, written in French; however, for the benefit of those who find the language difficult, an English summary has been provided at the beginning. The celebration of the centenary of the *Bulletin de la Société de Linguistique de Paris* offered the opportunity to see how other periodicals launched earlier or later have fared. Some journals did not survive for many years; others have done very well and are still available today and have exceeded 100 years of existence.

The fourth chapter might perhaps also be comfortably placed in Part II, but it deals with only one subject: analogy in historical linguistics and how authors could be ignored by the next generation who obviously like to claim originality for themselves. Many years before August Schleicher the concept of 'analogy' was not an unknown concept, beginning with Franz Bopp's (1791–1867) ground-breaking *Ueber das Konjugationssystem der Sanskritsprache in Vergleichung mit jenem der griechischen, lateinischen, persischen und germanischen Sprache* (Frankfurt/Main, 1816), which by 1845 was translated simply as *Comparative Grammar* (London:

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Madden & Malcolm). The editors of Saussure's *Cours de linguistique générale* chose exactly 1916 to mark the new approach to language in which historical linguistics was to play a secondary role.

Part II is mainly devoted to the relationship between individual linguists: Baudouin de Courtenay and Schleicher, Hermann Paul and Saussure, Mixail Baxtin and his contemporaries during the 1930s. Chapter 7 on Edward Sapir stands out because he has been placed within the center of attention. But his ideas are frequently compared with Saussure's *Cours* that soon had become available in English translation. By the 1920s, the two have been regarded as complimentary by many linguists.

All chapters in Part II deal with both dissension and agreement, influence or parallel development, though in the last chapter there is in addition the question of truth and falsehood at play.

In short, it is clear that the subjects treated in this study are by no means final. As long as linguistics in general – and the history of linguistics in particular – remain an academic field, there will be discussion and further development in one form or another, certainly a healthy scholarly situation..

Berlin-Prenzlauer Berg,
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PART I

Positions and problems in the history of linguistics

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CHAPTER 1

The historiography of linguistics past, present, future

1. Prefatory remarks

In this first chapter I am not going to say much that is new. Those who are familiar with my writings, which by now span close to forty years, may be somewhat disappointed. If my efforts in the establishment of the field have not been entirely fruitless, it may be because at least part of what I said and did since the early 1970s may have become common ground, i.e., *idées reçues*, so to speak.

Still, writing an introduction to what appears to be intended as a kind of encyclopedic undertaking is not an easy task, and this for a variety of reasons. One has to do with the fact that by the time the 20th century had come to a close, the History of Linguistics has established itself as a subject of serious scholarship within departments of language study throughout the world and, in many of places, a subject of regular instruction and/or research in which dissertations and major theses have been produced and more often than not made available with the imprint of internationally established publishers. As a result, it probably has become impossible for a single writer today to account for the development of the field during the past thirty or so years in a short article. Such a survey would call for monograph treatment, and it may require a team of authors, given the great variety – and sheer magnitude – of subjects that have been treated. The best illustration of the progress that has been made in the History of the Language Sciences is undoubtedly the three-volume work of almost 3,000 quarto-size pages edited by myself together with three other scholars, which finally has come to conclusion (see Auroux, Koerner, Niederehe & Versteegh, eds. 2000–2006). While Tome III covers in the main the achievements of the various modern subfields of linguistic science previously treated somewhat more selectively in Koerner & Asher (1995: 221–451), the first volume of the project, on which work had begun in the late 1980s, had been particularly innovative, since it gave special attention to the cultural areas and countries which had hitherto been accounted for in specialist literature only. I am referring to the linguistic traditions in the Near East, in China, Korea, Japan, India, and Tibet in particular (cf. Versteegh 2006, for an appraisal of the difficulties involved in their proper treatment).

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Another difficulty that must be stated right at the beginning of this introductory chapter is that any such survey of scholarly activity could hardly be attempted without a certain amount of personal bias. As a practitioner of the subject in several respects – as researcher, editor, and organizer – for all these many years this is bound to happen. By the time I had completed, late in 1971, my doctoral dissertation on Ferdinand de Saussure and the sources of his linguistic conceptions (Koerner 1973 [1971]), I had determined (probably at my own peril) that my future work was to be devoted to 19th and 20th century Western linguistics. By personal temperament, this was to be conducted essentially in an inductive manner punctuated from time to time with methodological statements and illustrations of both the pitfalls of historical research and the possibilities of getting things about right. This general approach of mine has not found unanimous acceptance, especially from scholars with a philosophical background, but in the final analysis, after all the discussions of 'epistemology', 'narrativity', and the like have been made, it will be the positive work, if done well, that endures. Even scholars seemingly preoccupied with more metahistoriogaphical issues like the late Peter Schmitter (1943–2006) prove that excellent historiographical work can be produced without reference to high-flying meta-theoretical precepts (e.g., Schmitter 1993, 2008 [2004]).

2. Motivations for writing the history of linguistics

Looking back on the past 150 and more years of history-writing in linguistics, it is possible to discern three distinct types, each associated with particular motives for engaging in such activity and each occurring at specific periods in the development of the discipline. A fourth type (argued for in, e.g., Koerner 1976 and in various places ever since) has begun to take shape since the later 1970s, namely, what has become known as 'Historiography of Linguistics', also termed 'Linguistic Historiography' (on which further below).

2.1 Summing-up histories of linguistics

There is a type of history writing which arises at a time when a particular generation, or an individual representing the ideas, beliefs and commitments of his generation to a significant extent, is persuaded that a desired goal – a kind of 'plateau' – has been reached and that sub-sequent work in the field will largely be concerned with 'mopping-up operations' (Kuhn). Such accounts assume that the theoretical framework had been sufficiently mapped out for the ordinary member of the scientific community to conduct his investigations and that there was no

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longer any need for any significant revision of the methodology or the approach to the subject matter under analysis. The result of these deliberations, which I term 'summing-up histories', tend to view the evolution of the field as growth of the field in an essentially unilinear fashion.

This idea of or motive for writing such a history seems to be best expressed in Theodor Benfey's (1809–1881) voluminous *Geschichte der Sprachwissenschaft und orientalischen Philologie* (1869), appearing one year after Schleicher's untimely death, but other works of the period, e.g., Rudolf von Raumer's (1815–1876) *Geschichte der germanischen Philologie* (1870), could be cited as reflecting much the same *Zeitgeist*. It is difficult nowadays to recreate the atmosphere of the late 1860s even if we limit ourselves to linguistic matters abstracting from external, e.g., socio-political currents: histories available today supply us with little, if any, information on this pre-neogrammarian period. Suffice it to recall for the present purpose that the works of Bopp, Rask, Grimm, and others had been sufficiently synthesized and methodologically developed by the generation of Georg Curtius (1820–1885) and, especially, August Schleicher (1821–1868), to the extent that one might speak of a 'paradigm' change having taken place at that time, of which the neogrammarian tenets of historical linguistic research, associated with the names their former students, notably Karl Brugmann (1849–1919) and August Leskien (1840–1916), respectively, were the logical, if somewhat overstated, outcome.

A similar observation about the motives of history-writing, it would seem, could be made about Holger Pedersen's (1867–1953) 1924 history of the achievements of 19th-century Indo-Europeanists, which was preceded by a similar and somewhat shorter account of his first published text in 1916, the year of the completion of the second edition of Brugmann's (and only in its first edition also Delbrück's) *Grundriss* as well as the appearance of Saussure's posthumous *Cours*. The sense of the need of such a summing-up history is, however, best illustrated in Wilhelm Streitberg's (1864–1925) voluminous undertaking, entitled *Geschichte der indogermanischen Sprachwissenschaft seit ihrer Begründung durch Franz Bopp*. However, this is in fact more of a résumé of the work accomplished in the various branches of Indo-European philology by that time than a regular history of linguistics (cf. Koerner 1978a: 16–17, for details). Begun in 1916, the enterprise was abandoned by 1936.

After World War II, it appears that the histories by Malmberg (1964 [1959]), Ivić (1965 [1963]), Leroy (1963), and others fulfilled a similar function of summing up previous attainments in linguistic science. However, this time the focus of attention was the post-1916 period in the history of linguistics, following the success story of Saussure's *Cours*, with its perceived emphasis on a non-historical 'synchronic' approach to language. The neogrammarian framework of linguistic

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research had been propounded in the histories of Pedersen, the organizational efforts of Streitberg from 1916 onwards, and other less influential books – and one may add that Pedersen, a second-generation Neogrammarian, reflects the 'data-orientation' of that school much more emphatically than the original group of scholars (note that neither Delbrück's *Einleitung* nor Paul's *Prinzipien* are even mentioned in his 300-page study of 1924). Only in more recent years (e.g., Einhauser 1990; cf. earlier Jankowsky 1972) have the Neogrammarians received a fairly adequate treatment. The histories by Malmberg, Ivić, Leroy, and other similarly slanted studies of the 1960s put forward particular post-Saussurean trends as the most significant achievements of the discipline, whether Copenhagen-type, Praguean, or Bloomfieldian. Their endeavour, like that of Benfey, Raumer, Pedersen, Streitberg, and others from earlier stages in the development of linguistics, was to a large extent the presentation of a framework of research in which they themselves had been brought up, and, concomitantly, an attempt to maintain the strength and impact of the particular mode of thought, i.e., 'structuralist', from the 1960s onwards.

2.2 Celebratory or propagandistic histories of linguistics

The second type of history-writing activity may be characterized by the intention on the part of an individual usually in his thirties or early forties – not close to retirement age, as is generally the case with the first type: Benfey, for instance, was sixty when his book appeared), again representing a particular group, to launch a campaign opposing previously cherished views and still prevailing doctrines. Thus in contrast to Benfey (1869), for example, Berthold Delbrück's (1842–1922) 1880 *Einleitung* served, together with Paul's *Principien* of the same year, as the mouth-piece of a new generation of scholars eager to demonstrate that their achievements significantly surpassed previous attainments in the field, and that their theories should rightfully replace those taught by the preceding generations of linguists. The claim in favour of discontinuity is what characterizes this type of activity, and Delbrück's book is a prime example of this endeavour. Typically, Schleicher was depicted by Delbrück (1882 [1880]: 55) as representing the conclusion of the phase of comparative-historical grammar inaugurated by Franz Bopp in 1816, and the *Junggrammatiker*, with whom Delbrück associated himself early in his career (in 1873, several years after he had received Schleicher's vacant post at the University of Jena), as marking a decisive new turn in the field.

No comparable history of linguistics was written in the 1930s or 1940s with regard to structuralism, but a look into Bloomfield's *Language* (1933: 3–20) or Louis Herbert Gray's (1875–1955) *Foundations of Language* (1939: 419–460) clearly suggests that the chapters devoted to the history of linguistics were an attempt to

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redress the development of the discipline and to suggest the superiority of the then modern approach over and above any other previous theory or method. This endeavour to prove earlier views to be insufficient or, worse, utterly inadequate has by no means been abandoned by adherents of the prevailing modes of linguistic thinking of today. On the contrary, it can easily be shown that their advocates have been eager not simply to foster an interest in the history of linguistics, but especially in order to rewrite it to an extent that the ideas of the generation immediately preceding the present one appear the least worthy of attention. As a matter of fact, what C. F. Voegelin in 1963 felicitously termed the 'eclipsing stance' that transformational-generative grammar had embarked on was best illustrated by Noam Chomsky himself, for instance in his plenary lecture at the Cambridge, Mass., Congress of Linguists of 1962 (Chomsky 1964). Soon thereafter, a number of his followers ardently engaged themselves in writing their particular view of history of inguistics; compare the articles by Dingwall (1963), Bach (1965), or the monograph by Bierwisch (1966).

More recently, in 1980, Frederick Newmeyer published a book which constitutes the best example to date of this *pro-domo*, Whiggish type of history-writing. It selects and reinterprets past linguistic research in an attempt to prove his view that linguistics was made a science only in 1955 (i.e., with the typescript compilation of *The Logical Structure of Linguistic Theory*, even though it appeared in print only in 1975!) or in 1957 (with the publication of *Syntactic Structures*), by no other than Noam Chomsky, and that previous work was totally inadequate, barring a few minor incidental insights foreshadowing the 'revolution' in the field (cf. Koerner 1983 and 2002, Chapter 8, for critical assessments of this kind of activity). Coincidentally, Newmeyer's *Linguistics in America* of 1980 appeared exactly 100 years after Delbrück's *Einleitung*, and the parallels between their authors are striking indeed: Both were less than forty years old when they wrote their books; both were primarily interested in syntax, not phonology, and neither had done his doctorate at the respective centers of activity whose success story they depicted. Delbrück did both his doctorate and habilitation in 1863 and 1867, respectively, under August Friedrich Pott (1802–1887) in Halle, and not under Georg Curtius in Leipzig; Newmeyer took his doctorate in 1969 under the direction of Robert B. Lees at the University of Illinois, not at MIT.

While Type I, i.e., the summing-up type of history-writing, may appear more benign as it seems to represent matter-of-fact accounts (though one should not be too sure about this), Type II History of Linguistics can best be described as propagandistic in nature, if not ideology-driven; the most successful example of this type is Chomsky's own *Cartesian Linguistics* of 1966, with Newmeyer's 1980 book a close second. Chomsky's book presents the author's views regarding the

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supposed ancestry of his own theories so brilliantly that many a young student of language was carried away by this new vision of history. Today, we can still discover a considerable amount of useful information in the histories written for the purpose of summing up previous work in the field, viz. the accounts by Benfey, Raumer, Pedersen, and others – although we may have become quite aware of their biases and shortcomings (for an assessment of these works, see Hoenigswald 1986 and Koerner 1990), whereas histories of the second category, though written much more recently, are already seriously dated. It appears that, once their propagandistic purpose in proselytizing for the new scientific belief system has been satisfied, these works rapidly lose their initial impact and informational worth. After the publication of the second edition of *Language and Mind* in 1972, we rarely see any historical references in Chomsky's work, barring those references to the ideas of Saussure and Jespersen (see Koerner 1994). A temporary return to the subject on Chomsky's part on the occasion of a public lecture given in São Paulo in November 1996 (Chomsky 1997) tells us more about Chomsky's mind-set rather than it offers interesting vistas on the way in which the history of linguistics could serve the practicing linguist (cf. Joseph 1999, for an assessment).

2.3 Detached histories of linguistics (Problemgeschichte)

There is a third type of History of Linguistics that is intended neither to advocate a particular framework or 'paradigm' nor to attempt to provide an argument in favour of a scientific revolution within the discipline. This type may occur at any time in the development of a particular field of research since its ultimate intent is less partisan than in the other two instances and often more holistic in attitude, though the motivation behind such work may be fairly personal. To my mind, the best example is Hans Arens' (1911–2003) 1955 *Sprachwissenschaft* (2nd enlarged ed., 1969), in which an attempt was made to delineate the development of Western linguistic thought from the early discussions of the Greeks about the nature of language to contemporary linguistic work, certainly with a view to indicating not only that our discipline has come a long way to gain those insights we now cherish and the methods we have developed, but also that we all have built, knowingly or not, on the findings of previous generations of linguists, and that we owe much more to these scholars than we might ever become fully aware of.

While this Type III manner of presenting the History of Linguistics might well have been the result of an individual choice, it appears that it still expressed the endeavour of a whole generation of scholars, namely, the rebuilding of a discipline after its almost total destruction through a world war. Taken in this way, Holger Pedersen's (1867–1953) 1931 book may well be included in the third category in

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that it sought to re-establish a linguistic tradition which in his view could continue to serve as a sound basis for subsequent work in comparative-historical linguistics after World War I. But Pedersen's view of linguistics (cf. Koerner 1985) is such that his accounting cannot be compared to Arens' detached view of the evolution of linguistics during the 19th and the early 20th century (see Dutz & Kaczmarek 1985, for an effort closer to Arens').

Undoubtedly, other, often non-linguistic, motives have played a role in presenting the history of the discipline in one way or another. Thus it should be recalled that particular socio-economic conditions, historical events, or political situations have often had a considerable influence on the motivation for writing the history of a particular discipline or in favour of the acceptance of a seemingly new theoretical framework of research or mode of thought – and in this respect histories of linguistics have largely failed to increase our awareness of the impact of matters or occurrences outside the field. The works of Benfey (1869) and von Raumer (1870), for instance, were highly motivated by the rise of German nationalism (as their prefaces suggest) and the aspiration of national unity, if not superiority, of Germany among the European powers. That such sentiments may well have played a role when after World War I German linguistic science did no longer hold sway in many parts of the world was pointed out by Malkiel (1969: 557), who observed that the success of Saussure's *Cours* "cannot be properly measured without some allowance for the feelings of that time: The acceptance of the leadership of a French-Swiss genius connoted for many Westerners then opposed to Germany a strongly desired, rationalized escape from the world of Brugmann, Leskien, Osthoff, and Paul."

2.4 Historiography of linguistics

Despite the respect scholars may have for works of the third type of history-writing, as exemplified by Arens' *Problemgeschichte*, some have felt a need for yet a fourth type of history-writing (e.g., Koerner 1976 [1972]; Simone 1975 [1973]), namely, the presentation of our linguistic past as an integral part of the discipline itself and, at the same time, as an activity founded on well-defined research principles which can rival, in terms of soundness of method and rigour of application, those of linguistics itself. This fourth type, now usually referred to as 'linguistic historiography' or, more properly, the Historiography of Linguistics demands that the history of linguistics should not merely be subservient to the discipline, but should assume a function comparable to that of the history of science for the natural scientist (cf. Koerner 2003a). In short, while recognizing the important distinction between chronicle and history which Benedetto Croce is credited with, modern contributors to the History of Linguistics have gone a step further by distinguishing between history

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and historiography. This is partly an attempt to make clear a departure from previous undertakings in the field, which only too often tended to be partisan-histories, if not what Henry Butterfield in 1931 termed 'Whig-histories', and partly because previous histories did not attempt to offer a usable guide for the adequate treatment of past developments in the history of the language sciences, and thus failed to provide for a better understanding of where current theories may lead us to.

3. The study of the history of linguistics: Early beginnings to the present

A discipline comes of age when it seriously contemplates its own past. The History of the Language Sciences as I have long preferred to call the field in an attempt to steer away from a narrow presentist view of 'linguistic science' which many non-historians tend to indulge in – and just History of Linguistics for short – as a *bona fide* subject of academic research (in which doctoral dissertations can be written for instance) began to develop, as already mentioned, only during the late 1960s, when the subject was drawing some attention from practicing linguists, notably Noam Chomsky (1964, 1966). Previously, such work had been done in departments of Germanic, Romance, or Slavic philology where such research surveys were at times undertaken to delineate the course of a particular field or the evolution of a specific idea or research project, often in the form of dissertations or monographs. (It had been customary at least since the 1880s to add such introductory chapters to textbooks in linguistics, but usually the intent has been to show the significance of recent advances in the field compared with previous endeavours, and the result was more often than not much less than history properly done.)

3.1 Early phases of history-writing in linguistics

It is true that we could perhaps by now speak of a 200-year tradition of linguistic history-writing, perhaps beginning with François Thurot's (1768–1832) 1796 *Tableau des progrès de la science grammaticale* (cf. Andresen 1978), though several earlier works have been cited, for instance Elias Caspar Reichard's (1714–1791) *Versuch einer Historie der deutschen Sprachkunst* of 1747 (cf. Koerner 1978a: vi, for references to other 18th-century works). However, as the record suggests (ibid., pp. 1–4), it is only from the late 1860s onwards that a more thorough type of treatment of History of Linguistics emerged of with Theodor Benfey's (1809–1881) *Geschichte der Sprachwissenschaft* of 1869 which may be regarded as the most outstanding example. It had been preceded by Heymann Steinthal's (1823–1899) work of 1863, which sought to supersede Laurenz Lersch's (1811–1849) three-volume *Die*

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Sprachphilosophie der Alten (1838–1841), but which dealt only with the contributions of Greece and Rome to linguistic thought, not to linguistics proper. Benfey's history of linguistics was followed by other influential works such as von Raumer (1870), Delbrück (1882 [1880]), Bursian (1883), which, however, were more limited in scope despite their at times impressive size. The same could be said of books such as Thomsen (1902; German transl., 1927), Delbrück (1904), Trabalza (1908), Jellinek (1913), Pedersen (1916; English transl, 1983), Pedersen (1924; English transl., 1931), Drăganu (1945; Italian transl., 1970), or Robins (1951) from the first half of the 20th century. (For detailed descriptions of all these books, see Koerner 1978a.)

3.2 Mid-20th-century efforts in history of linguistics

New endeavours and, at times, more insightful studies in History of Linguistics appeared in the 1960s, perhaps beginning with Paul Diderichsen's (1905–1964) work of 1960 on his compatriot Rasmus Rask (German transl., 1976). It was followed by works such as Ivić (1963; English transl., 1965), Leroy (1963; English transl., 1967), Tagliavini (1963), Malmberg (1964), Lepschy (1966; English transl., 1970), Mounin (1967), Robins (1967), Coseriu (1969, 1972), Helbig (1970), Szemerényi (1971), and others (see Koerner 1978a, for a full description of these textbooks down to 1976). Yet most of them relied uncritically on earlier accounts and rarely ventured into questions of historiographical method or touched upon matters concerning the philosophy of science, except perhaps for a then fashionable nod to Thomas Kuhn's (1922–1996) *The Structure of Scientific Revolutions* of 1962. Most of these books are largely forgotten now, and are at times cited for criticism. Many of them had been produced following Chomsky's 'big splash' with his 1962 plenary lecture at the Ninth International Congress of Linguists held in Cambridge, Mass. (Chomsky 1964), at which he first referred to his recently discovered 'ancestors'. More often than not these books were following the fad of the day to 'modernize' the ideas of earlier linguistic thinkers. Among the books of this period Robert H. Robins' (1921–2000) *Short History* has shown the greatest 'staying power'. I think that part of the popularity was largely due to the fact that it was the only one written by a native speaker of English, that it stood aloof from any criticism of Chomsky's work, not to mention the latter's forays into the history of linguistics, and that it was of a size that made it a handy textbook. Like all the other histories of the time, its elegant narrative cannot disguise its scholarly flaws: too many periods were covered without individual research leading to errors of fact either being committed or repeated and *fables convenues* perpetuated.

During the mid-1960s, following various claims made by Chomsky in his *Cartesian Linguistics* of 1966 that his own theories had little to do with the pursuits

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of his immediate predecessors and contemporaries, but instead follow quite different traditions such as those associated with the Port Royal grammarians and the work of Wilhelm von Humboldt, the bulk of the dissertations written in the history of linguistics were devoted to just those areas of interest that Chomsky had found worthy of consideration. More often than not these studies tended to be lopsided, at times seriously distorting the true intent and purpose of these earlier authors. For instance, Sanctius' *Minerva* was studied mainly because of a recent revival of interest on the part of generativists in the phenomenon of ellipsis, a subject to which Sanctius appeared to have made an important contribution (cf. Breva Claramonte 1983, which surely was one of the better ones of this sort).

Only from the 1970s onwards, following the creation of the first journal for this particular field of interest in 1973, *Historiographia Linguistica*, and the associated monograph series united under the umbrella title "Amsterdam Studies in the Theory and History of Linguistic Science", did serious work begin to emerge that challenged this largely *pro-domo* type of history-writing. These and other organized activities (on which further below) led to the much more recent field of study, now generally referred to as Historiography of Linguistics (on which see Koerner 1995b) or 'Linguistic Historiography' for short, an approach to History of Linguistics which is conscious of methodological and epistemological requirements in adequate history-writing in linguistics as in any science. (Note that most of the contributions to the two-volume *Historiography of Linguistics* published in 1975 under the chief editorship of Thomas A. Sebeok – though certainly not my own (Koerner 1975), which however was heavily edited by one of the associate editors where its methodological introduction and its critique of the available literature was concerned – were little else but surveys of previous scholarship; 'historiography' there being used in the old sense of the term.) The volume edited by Herman Parret in 1976 followed largely the Chomskyan manner of misappropriating the history of linguistics for 'ideological' purposes, namely, the attempt to provide 'evidence' for the correctness of Chomsky's claims made in the 1960s.

3.3 Late 20th-century work in the history of linguistics

I believe that we have learned enough by now about the complexity of the subject that no reasonable person will venture to undertake an attempt at covering 2,500 years of Western history of linguistics in barely 200 pages. The late Bertil Malmberg's (1913–1994) book of 1991 is most charitably described as an *Alterswerk*, the work of someone too old to read the signs of the times, not to mention the scholarship of the preceding twenty and more years. I think we all are agreed now that effective research even in areas such as the Classics (e.g., Taylor 1987), the early or the late medieval period (e.g., Law 1993 and Ebbesen 1995, respectively), or any other

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period in the history of linguistic thought is best pursued by a team of specialists. (No surprise then that the title of Swiggers' 1997 book, for instance, promises much more than it delivers.)

That this has been recognized by many scholars may be seen from the various multi-volume histories such as those edited or directed by Schmitter (1987–2005), Auroux (1989–1992, 2000), and Lepschy (1994 [1990]–1998). While Schmitter's project has been closed off, those two other editorial ventures are particularly weak when it comes to the history of linguistics in the 20th century. Indeed, while Lepschy (personal communication, May 2002) has abandoned the projected fifth volume (which even then would have taken us no farther than the 1930s), Auroux's Tome III (2000) takes us barely to the 1940s. In fact, it contains no chapter on subjects central to linguistics such as syntax, morphology, or phonology. (Instead we find in it chapters on semiotics, logic and even glossolalia.) This deplorable situation has finally been amended through the publication, in 2006, of Tome III of the trilingual *History of the Language Sciences / Geschichte der Sprachwissenschaften / Histoire des sciences du langage* edited by myself, together with Kees Versteegh, Hans-Josef Niederehe and Sylvain Auroux as associate editors,[1] which contains a broad coverage of most, if not all, major subfields of linguistics and adjacent disciplines such as philosophy, pragmatics, logic, mathematics, neurology, psychology, machine translation and other applications of computer science.

4. Approaches to linguistic historiography

On the methodological side, the 1980s saw a variety of studies offering alternative lines of historiographic conduct, entering the debate over the proper approach to the history of linguistics (e.g., Bahner 1981, Schmitter 1982, Grotsch 1982, Christmann 1987). However, no common ground has as yet been established as to how to proceed in linguistic historiography, and indications are that the debate will continue for some time (e.g., Schmitter 2003). In what follows, some of the positions and proposals that have been made concerning historiography by various authors outside linguistics and its history are analyzed, then a variety of considerations are offered within which research in the history of the language sciences could be undertaken and past developments in the field presented.

The search for the proper foundation of the historiography of linguistics has led to a number of differing proposals. These may have had their source in the particular area of study chosen by the researcher – no doubt it would make a difference

1. Somewhat misleadingly listed by the publisher simply in alphabetical order according to the editors' last names.

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whether one is studying the linguistic writings of the Middle Ages (e.g., Kelly 1996, 2002) or those of the 19th century (e.g., Morpurgo Davies 1998) – as well as in the researcher's *Erkenntnisinteresse* ("research focus"). Those who enter the history of linguistics from the study of literature will, one may expect, offer different perspectives than someone coming from philosophy, history, or linguistics proper, not to mention those who enter linguistics from mathematics or the 'hard' sciences. The position advocated here is that a historian of linguistics should have linguistic training in order to have an adequate understanding of what the issues in this particular field really are, though this again may apply more properly to the linguistics of the past two centuries and much less so to earlier periods. Of course, this is not enough. Too often present-day linguists tend to project their interests and understanding of the subject back on past theories and, as a consequence, are apt to distort the issues and theoretical commitments of previous periods. Therefore, the historiographer of linguistics has to familiarize himself with more than the delineation of transmission of linguistic theory and practice and their changes through time.

From a methodological point of view, one may well ask what other, already established fields of historical inquiry could offer the linguistic historiographer, while at the same time keeping in mind that the object of study, i.e., ideas about language and proposals of its description and explanation, will impose a particular approach on the investigator.

4.1 History of linguistics and intellectual history

It appears that a number of scholars consider the History of Linguistics to be part of an overall History of Ideas (e.g., Aarsleff 1982). This impression may also be drawn from the fact that the Henry Sweet Society of Oxford, launched in 1984, carries "for the history of linguistic ideas" as part of its name. It is a truism that the history of linguistics cannot be studied in a vacuum, simply as a succession of theories about language divorced from the general intellectual climate in which they were formulated. But the context must also include an awareness of what other, neighbouring as well as distant, disciplines were like at a given point in time. In short, too close an alignment of the History of Linguistics with the History of Ideas or a similar field of study does not appear to be a satisfactory solution to the problem of finding the proper methodology for historiographical research in linguistics. For instance, it appears revealing when in 1977, some forty years after the publication of Arthur O. Lovejoy's (1873–1962) influential book *The Great Chain of Being* (1936), a paper was published in the *Journal of the History of Ideas*, founded by Lovejoy in 1940, an intellectual historian asserts:

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> On the whole, the methodology of the history of ideas is in its infancy. The field is in this respect behind general history, of which it is a part. One may therefore suggest that the interest of historians of ideas should be more directed towards the methodological problems of their field than has hitherto been the case. The reason is that when the foundation of a house is shaky, it does not make much sense continuously to add new stories to it. (Kvastad 1977: 174)

Unfortunately, Kvastad's own proposals are far from satisfactory: the pseudo-formal apparatus and the 'logical' definitions which he offers do not seem to lead to any new insight or useful principles of research (nor did his 1979 'Method' paper). But a discussion regarding the points of contact – and the epistemological differences – between the History of Linguistics and the History of Ideas,[2] whether in the sense of Lovejoy (1936) or in the sense of 'intellectual history' (Mandelbaum 1965) need not be abandoned for good.[3] Indeed, some may argue that certain more recent works such as Dominick LaCapra's (b.1939) *Rethinking Intellectual History* (1983) and much of his other writings could lead the historian of linguistics to new insights. However, if the exchange between one of his reviewers (Pagden 1988) and LaCapra (1988) is any guide, it appears that this line of work has progressed little beyond the discussion stage, and where it appears to be exemplified – at least in the case of LaCapra (see p. 680 and n.3) – it produced analyses of literary or philosophical works, not intellectual history. As LaCapra himself concedes (1988: 678) his style is "often polemical", his objective being "to lay the groundwork for a more fruitful interchange among intellectual, cultural, and social history" and to defend

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2. Richter (1987) constitutes an interesting attempt to revive the German tradition of *Begriffsgeschichte*, largely going back as far as Wilhelm Dilthey's (1833–1911) work of the 1880s, in line with Lovejoy's program.

3. Scholars like Quentin Skinner, for one, may have useful ideas for historians of linguistics to offer, for instance when inveighing against facile invocations of 'influence' which I discussed some twenty years ago – against arguments such as brought forward by people like Hans Aarsleff (see Koerner 1987). On this issue, one could have read in Skinner (1969: 26) an entire set of necessary conditions that would have to be met before one could reasonably claim that one author influenced another: "Such a set of conditions would at least have to include (i) that there should be a genuine similarity between the doctrines of A and B; (ii) that B could not have found the relevant doctrine in any writer other than A; (iii) that the probability of the similarity being random should be very low (i.e., even if there is a similarity, and it is shown that it could have been by A that B was influenced, it must still be shown that B did not as a matter of fact articulate the relevant doctrine independently)." Of course, there are other possible factors, too. We could mention the investigation of an author's formative years, direct citations or acknowledgements, textual parallels (not taken out of context as Aarsleff and many others have tended to when they try to "prove" a preconceived idea), archival materials, and many more as avenues that could be pursued to establish 'influence'.

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"intellectual history against misguided attacks" (p. 679). In short, it appears that the focus is still on epistemological and 'attitudinal' problems rather than methodological ones, so that one may wonder how much a historian of linguistics can learn from these projects which are penetrated by post-structuralist French thought, notably the work of Jacques Derrida, and which pretend to enter into 'dialogical discourse' with 'past voices' without realizing "that to converse with the past one must first attempt to reconstruct it – text or author" (Pagden 1988: 526). Interestingly enough, while attacking the work of those who follow the French *histoire des mentalités* program – which, because of its emphasis on a particular 'mind set' which is said to determine a given culture, so that the analyst would have to resort to unspecified external pressures if he wants to account for change – the work of LaCapra and others seems to lead to presentist accounts rather than to historical analysis.

The nature of linguistics as a field with a well-defined object – human language in all its manifestations – requires perhaps more than an association with or an inspiration from the history of ideas – or the 'history of philosophy', for that matter. Passmore (1967) expresses himself against the idea, also championed by Kristeller (1964), that historians rather than philosophers write the history of philosophical ideas, arguing that the "pure historian with no philosophical enthusiasm is almost certain to compose a doxography" (1967: 229), i.e., an entirely detached chronological and biographical account of past philosophical schools of thought. In this Passmore is in full agreement with the view expressed by Malkiel, which I share, namely, that the historian of a given discipline must be equipped, in addition to "the specific knowledge of a scientific […] domain", "a good deal [of knowledge] about the intellectual history (embedded within the matrix of general history)" (Malkiel 1983 [1969]: 52). The historiographer of linguistics, however, probably needs more than this 'dual expertise', which must be regarded as the *conditio sine qua non* for anyone engaging in the research of past events in the development of linguistics

In my own work (e.g., Koerner 1978 and in later writings), I found Carl Lotus Becker's (1873–1945) concept of 'climate of opinion' particularly useful in mapping out the intellectual atmosphere of a given period in which certain ideas flourished, were received or rejected. Becker (1971 [1932]: 5) exemplified his concept in the following manner:

> Whether arguments command assent or not depends less upon the logic that conveys them than upon the climate of opinion in which they are sustained. What renders Dante's argument or St. Thomas's definition meaningless to us is not bad logic or want of intelligence, but the medieval climate of opinion – those instinctively held conceptions, in the broad sense, that *Weltanschauung* or world pattern – which imposed on Dante and St. Thomas a peculiar use of the intelligence and a special type of logic. To understand why we cannot easily follow Dante or St. Thomas it is necessary to understand (as well as may be) the nature of this climate of opinion.

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Those working in the history of linguistics will surely have learned to appreciate Becker's observation, though they will also have learned that not only the intellectual climate of a given period will have to be reconstructed but also a number of other factors that may have played a role in fostering certain views or theoretical positions. This means that if we are to obtain a better understanding of the general intellectual context in which particular theories have been developed, then a broadly defined history of ideas may prove of distinct use, but would not be a panacea.

4.2 History of linguistics and the philosophy of history

As suggested by Malkiel, general historical research and the discussions guiding it may have something to offer to historians of linguistics. In this field of study the work of Hayden White (b.1928) has been referred to frequently in recent years (albeit rarely by linguistic historiographers) as influential in the debate of proper historical method. In his book *Metahistory* White celebrates the work of four 19th-century master historians – Jules Michelet, Leopold von Ranke, Alexis de Tocqueville, Jacob Burckhardt – as representing "not only original achievements in the writing of history but also alternative models of what a 'realistic' historiography might be" (1973: 141). At the same time, White (p. 433) argued that "we are free to conceive 'history' as we please, just as we are free to make of it what we will", thus suggesting, it would seem, that there are no generally accepted criteria available to define the subject. In a collection of papers published five years later, we find him taking a 'linguistic turn' similar to LaCapra and others. There he claims that "the conventional distinctions between 'history' and 'historism' are virtually worthless" (White 1978: 101); instead, the author "seeks to show that in the very *language* that the historian uses to describe his object of study, […] he subjects that object of study to the kind of distinction that historians impose upon their materials in a more explicit and formal way" (p. 102; emphasis in the original).

In other words, White is not much interested in actually writing history but in analyzing and criticizing the 'discourses' of other historians or theorists of history, notably Michel Foucault (1926–1984) – hence his predilection for 'metahistory'. In his more recent collection of essays, subtitled 'Narrative discourse and historical representation', White characterizes Foucault's discourse as "'positively' wide (if seemingly capricious) erudition [presented as] solemn disclosures of the 'way things really were', aggressive redrawings of the map of cultural history, confident restructurings of the chronicle of 'knowledge'" (1987: 107). He does not venture into a discussion of how Foucault's *Les mots et les choses* of 1966, for instance, might have contributed to the discussion of early 19th-century historical-comparative philology as constituting a new 'épistémè' (cf. Foucault, p. 397) in the history of linguistics.

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Again, I have found observations by 'traditional' historians at times more enlightening than the theoretical 'discourses' of (post-)modern theorists with literary aspirations. In discussing 20th-century developments in linguistics and the manner they are presented, Herbert Butterfield's (1900–1979) description of the 'Whig interpretation of history' remains particularly apt:

> Through this system of immediate reference to the present-day, historical personages can easily be classed into the men who furthered progress and the men who tried to hinder it; so that a handy rule of thumb exists by which the historian can select and reject, and can make his points of emphasis. (Butterfield 1931: 11)

While some may think that 'Whig history' and 'presentism' are modern phenomena, we may in fact find them occurring as long as history has been written. Again from a methodological point of view there does not seem to be much guidance from regular historians or philosophers or theorists of historical analysis available to the linguistic historiographer, in part because the subject of inquiry, theories about language as well as linguistics itself, is epistemologically quite distinct from historical events, their description, interpretation, and explanation.

In 1984 the late philosopher Richard Rorty (1931–2007) offered four 'genres' for the historiography of philosophy. In his essay, he distinguishes between 'rational reconstruction', which is essentially presentist, 'historical reconstruction', which would be *grosso modo* what I have been concerned with, 'Geistesgeschichte', which is essentially a broadly conceived intellectual history (although the author treats it as "a richer and much more diffuse genre – one which falls outside this triad" [p. 68]), and 'doxography', which concerns itself with canon formation and maintenance of a particular position. While he believes that the first three have their uses, Rorty argues – and it would be hard to disagree with his suggestion – the fourth 'genre' should be abandoned as a scholarly activity. In his own assessment, his kind of 'intellectual history' "works to keep *Geistesgeschichte* honest, just as historical reconstruction operates to keep rational reconstruction honest" (p. 71). Earlier Rorty (p. 56) had contrasted 'historical reconstruction' as "contextualist accounts which block off later developments from sight" and 'rational reconstruction' as "'Whiggish' accounts which draw on our better knowledge". Small wonder that if the former is so narrowly conceived, the latter can be looked upon as benign, as Rorty suggests. At least in linguistic historiography it has become clear that a contextualist approach cannot succeed if the focus is too narrow, whereas the presentist activity of 'rational reconstruction', for all its intents and purposes, is not doing history at all. Still, there is of course nothing wrong in consulting works in the Philosophy of History for inspiration and reflection on our own activity as historians of linguistics (e.g., Ankersmit & Kellner, eds. 1988; Tucker 2004).

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4.3 Linguistic historiography and history & philosophy of science

In contrast to intellectual history and the various approaches to the treatment of general history – though linguistic historiography has to take into proper account intellectual currents of a given period which may have impacted on linguistic thinking, of course – the History and Philosophy of Science appears to have more to offer to the historian of linguistics, in part because of its advances in epistemology and methodology. Evidently, Kuhn's (1962) morphology of scientific revolutions played an important part in the discussion (cf. Lakatos & Musgrave 1970). However, it seems widely agreed that the nature and conduct of science and of the philosophy of science in particular, whether it be in terms of the more recent paradigmatism (cf. also Laudan 1977) or traditional inductivism and its opposing philosophy of science, refutationism (Popper 1959, 1962; Lakatos 1974), make their proposals of some relevance to the historian of linguistics. Of particular interest are proposals made by scholars such as Foucault (1966), Hesse (1963, 1980), Kuhn (1977), Lakatos (1974, 1978), Pandit (1983), Sneed (1971), and others (e.g., Krige 1980). But again, the linguistic historiographer should not expect a ready-made framework from any one philosopher of science.

As an example of how observations made by historians of science could offer historians of linguistics food for thought, I quote a statement, made years ago by the British historian of science Martin Rudwick who – referring to Hesse (1963) – noted the following about the desirability of investigating what he terms 'creative analogies' in the development of a field of research, especially in its formative stage:

> It is at least arguable that major cognitive innovation is most likely to emerge in the scientific work of individuals who choose to employ analogies that […] are strongly 'external': that is, analogies that are furthest removed from the 'normal practice' of the discipline concerned. This may happen when a scientific field scarcely yet deserves the name of 'discipline', because its practice is not yet strongly insulated and institutionalized. (Rudwick 1979: 67)

Rudwick was writing about Charles Lyell's (1799–1875) role in the development of geology as a science, but his observation could well have characterized the situation that the early comparative-historical linguists were faced with at the beginning of the 19th century. The 'displacement of concepts' is to be reckoned with each time a scholar or scientist is concerned with developing something of a new 'research program' (Lakatos 1978). How else could the innovator express himself in a new mode without resorting to analogy, metaphor, and borrowing (of concepts or terms) from other fields of knowledge?

There are of course many other instances where the historian of linguistics can profit from reading historians and philosophers of science. Indeed, especially for

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19th and 20th century linguistics many of their findings could offer useful concepts and tools for research and actual history-writing. As noted earlier, Kuhn's *The Structure of Scientific Revolutions* has been singularly influential, not only in linguistics but also in anthropology and sociology. In programmatic statements made during the 1970s I frequently referred to Kuhn's ideas, but nowhere did I suggest uncritical application of his proposals to the history of linguistics. Yet concepts such as 'paradigm' or, following Kuhn's own suggestion of 1970, 'disciplinary matrix', 'normal science', 'scientific revolution', and 'mopping-up operation' may still prove "useful to the historian of linguistics if he does not press the argument to a point where it no longer makes sense" (Koerner 1989 [1980]: 50). Needless to say, the same would apply, *mutatis mutandis*, to concepts and notions found in the works of other historians and philosophers of science, be it those in the line of Popper's refutationism or any other framework, e.g., Mario Bunge's epistemological approach (1984).

4.4 Linguistic historiography and sociology of science

Many years ago Roger Chartier complained about "the almost tyrannical preeminence of the social dimension" in historical studies (quoted by Pagden 1988: 520). It seems, therefore, inescapable that the historian of linguistics should take note of work in the sociology of science (e.g., Merton 1973, Mullins 1973, Amsterdamska 1987, Murray 1994), possibly even of the findings of *Wissenssoziologie* "sociology of knowledge" (Mannheim 1968). Likewise, Bourdieu's (1975) concept of the 'accumulation' and, one suspects, squandering of social as well as scientific 'capital', notions such as (scientific) 'domination', 'value', 'interest', 'visibility', 'legitimacy', and the like may deserve further exploration by the linguistic historiographer. His distinction between 'subversion strategy' and 'succession strategy', i.e., possible discourse strategies followed by younger scientists trying to establish themselves in a given field, but also his reference to 'foreclosing' and 'denial' strategies employed by those interested in keeping – and increasing – their 'scientific capital' might well prove useful in the analysis of 'revolutions' in linguistics: Chomsky's career – and, one may presume, Halle's strategic moves at various points in the development of Generative Grammar – come to mind (cf. Koerner 2002, chaps. 6–9 and Koerner 2003b, for illustration).

Again Kuhn's emphasis on the social nature of scientific revolutions played a role in the study of group formations in science – Kuhn spoke about the 'invisible college' – such as analyzed by Mullins (1973) for biology and, following him, Murray (1994) for anthropology and linguistics. However, Amsterdamska's (1987) account of one hundred years of linguistics from Bopp to Saussure from a point of

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view of Mertonian sociology of science, while not without merit, has brought out little which has not been known – and accounted for – by historians of 19th-century linguistics, such as the important role of the system of higher education in Prussia for the institutionalization of the field of linguistics (as well as many other disciplines of course, notably in the humanities).

Yet it remains true that the success of the *Junggrammatiker* – or of the followers of Chomsky for that matter – cannot simply be explained in terms of the victory of one particular, supposedly novel, linguistic approach over another, although neither can it be denied. Thus, the replacement of the 'Sanscrito-centric' view of the phonology of the Indo-European *Ursprache* by one giving ancient Greek and Latin more attention in phonological reconstruction played an important role in the change from the position upheld by Schleicher during the 1860s to the framework advocated by Brugmann and his associates from 1876 onwards (see Mayrhofer 1983). However, while certain proposals within a discipline have had their intrinsic merits, it also cannot be denied that their wider acceptance within the research community has been helped by external factors, such as the considerable expansion of post-secondary education after the Franco-Prussian war of 1870 (and also the emphasis on the teaching of Latin and Greek imposed by the educational reform initiated by Wilhelm von Humboldt). A parallel situation occurred one hundred years later through the drastic expansion of college education in the United States and also in Europe during the 1960s and 1970s, which had a significant effect on the widespread reception of the theories of Noam Chomsky during the period and the subsequent decades. (Chomsky's involvement in the anti-Vietnam War protests led a number of young and bright men and women to flock to the Massachusetts Institute of Technology in order to study under him was another factor.) Yet again it seems to me that no particular methodological framework can be drawn for linguistic historiography from sociological approaches to science, apart from retaining an awareness of – and where appropriate, an accounting for – extra-disciplinary factors which frequently have a considerable effect on the evolution of a given discipline, whether seemingly exact or less rigorously defined.

4.5 Toward a synthesis of differing approaches to linguistic history-writing

The above excursions into other historical fields, general history, intellectual history, history and philosophy of science, and sociological approaches to history suggest that the history of linguistics can learn something from all these disciplines or subdisciplines. However, none of these alone can serve the linguistic historiographer as a guide in his research. In fact, in the final analysis, historians of linguistic science will have to develop their own framework, both methodological and philosophical.

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In this effort, more than a nodding acquaintance with historical theory and practice in other fields may prove very useful indeed, even if the result is negative, in that the historian of linguistics discovers that this or that other field of historical investigation has in fact little to offer in matters of historiographical method.

As stated earlier, it appears that the History of Ideas provides little insight to linguistic historiographers that they would not come upon themselves; it provides a general recognition that linguistic theories are not developed in total isolation from the general intellectual climate of a period or the particular attitudes maintained by the society fostering scientific activity. In a similar vein, we may recognize that at least a smattering of the sociology of science and, perhaps more importantly, an understanding of the dynamics of social networks within any scientific organization (e.g., 'invisible colleges', citation cartels, control of publishing outlets by special interest groups, etc.) would do the historian of linguistics some good, as may be learned from Murray's detailed history of North American linguistics (1994), inspired by the sociology of science work of Nicholas C. Mullins (1939–1988), for instance as exemplified in Mullins (1980) for biology.

More promising results may come from the exposure to the discussions conducted among philosophers and historians of science. Kuhn's influential 1962 book has been invoked several times before, but the various reactions and counter-positions deserve comparable attention. The history of linguistics is not to be treated simply like a branch of the history of ideas, at least not since the advent of comparative-historical philology in the early 19th century. This is because linguistics, unlike philosophy for example, is a science which has to do with (usually) empirically verifiable facts as well as with (often rather complex) theories and at times rather rigorous research practices, not merely general ideas about the nature of language. This assertion might require the historian of linguistics to enter into the debate about the scientific status of linguistics, at least where 19th and 20th century linguistics is concerned, but it need not consume most of his energies. (For earlier periods of the study of language, preceding the scientific age ushered in by 19th-century natural science, other criteria may have to be developed; no doubt, the application of modern principles in the philosophy of science to these earlier periods is hazardous. Indeed, the understanding of what is 'scientific' and what constitutes 'science' may have to be redefined for different periods in the history of the discipline under investigation.)

The other, possibly complementary, avenues open to the historian of linguistics is the drawing up of principles derived from historical practice. I am thinking of the development of particular models which may guide his research (cf. Koerner 1989: 47–59), the critical analysis of the work of our predecessors, several of which have shaped much of our view of the past, and the discussion of particular problems facing the researcher, such as the (frequent misuse of the) argument of

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'influence' (cf. Koerner 1987), the continuity/discontinuity debate, or the question of 'metalanguage' in linguistic history-writing, to cite just a few examples (cf. Koerner 1995: 15–22, for discussion). These issues and the possible pitfalls will have to remain in the minds of historiographers of linguistics in order to produce respectable research results.

5. The consolidation of linguistic historiography

A major indication of the History of Linguistics having become a mature field of scholarly endeavour is of course what may be called its professionalization. In 1978, the first International Conference on the History of the Language Sciences (ICHoLS) was held in Ottawa, Canada, the same year in which the Société d'Histoire et d'Épistémologie des Sciences du Langage (S.H.E.S.L.) was founded in Paris. In 1984, the Henry Sweet Society for the History of Linguistic Ideas (HSS) was established at Oxford, and several similar international and regional societies have been established since (such as the Dutch society "Geschiedenis van de taalkunde" in Leiden, The Netherlands, and the German "Studienkreis 'Geschichte der Sprachwissenschaft'" in Münster, both launched during the mid-1980s). More recently, a work group on history of linguistics was established in São Paulo, Brazil, and a Society for the subject was launched in Mexico in 2001). Perhaps with a broader, international appeal have come about the North American Association for the History of the Language Sciences (NAAHoLS) started late in 1987, followed by the creation of the historically-oriented Società di Filosofia del Linguaggio (SFL) in 1994, and the Sociedad Española de Historiografía Lingüística (SEHL) in 1995. The latter has been held every two international conferences since 1997 (e.g., Fernández et al., eds. 1999; Corrales et al., eds. 2004). All these associations have held and are organizing scholarly meetings, both national and international, and several on a very regular basis, with researchers from many countries in attendance.

In the meantime further ICHoLS meetings have been held on a triennial basis: in 1981 (Lille, France), 1984 (Princeton, N. J.), 1987 (Trier, Germany), 1990 (Galway, Ireland), 1993 (Washington, D. C.), 1996 (Oxford), 1999 (Fontenay near Paris), 2002 (São Paulo), 2005 (Urbana-Champaign, Ill.) with the eleventh congress being scheduled to take place in Potsdam in August 2008. These ICHoLS meetings have regularly drawn some one hundred participants from around twenty and more different countries and have proved useful in providing a liaison between the various members of the national or regional societies. Since its inception, proceedings of ICHoLS have been published on a fairly regular basis, and reflect the progress that has been made in the professsionalization of the field.

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Apart from supplying fora for direct, *viva voce* exchanges on a wider scale, we should not forget the importance of specialist periodicals and monograph series which provide outlets for research in the discipline. In addition to *Historiographia Linguistica* established in 1973, a second journal with similar goals (albeit not with the same consequence), *Histoire – Épistémologie – Langage*, was started in Paris in 1979, and more recently in 1991, a third journal, *Beiträge zur Geschichte der Sprachwissenschaft*, began to appear in Münster, Germany. If the (until very recently) regular "Publications Received" rubric of *Historiographia Linguistica* is any guide,[4] we can witness a steadily increasing output of works in the History of Linguistics. (In the "Studies in the History of the Language Sciences" monograph series alone more than one hundred volumes have appeared since 1973.) In sum, much has been done for the professionalization of the subject.

In his 1978 Foreword to the first collection of my papers, R. H. Robins identified three "types of writing required in the history of linguistics", apart from the "most primary stage of research", namely, the editing and publication of previously unpublished texts. This latter kind of work has been done in various places, for instance in the "Grammatica Speculativa" series published by Frommann-Holzboog of Stuttgart (e.g., Kelly 1996), and there are several other series catering to particular areas of interest, such as the Classics or the Arab grammatical tradition. These three 'types' are according to Robins (in Koerner 1978b: xii–xiii):

1. General theoretical and methodological essays on the historiography of linguistics: what should the history of an academic discipline such as linguistics set out to achieve and how should it be undertaken for this purpose? Opinions, of course, differ on both these questions; a case in point is the applicability or non-applicability of the Kuhnian concept of a scientific paradigm to the history of linguistic science.
2. Studies more restricted in their time and place, devoted to particular trends and movements of thought on language and the development of particular linguistic concepts; […].
3. Biographical accounts of the work of individual scholars who have been influential in the course taken by linguistic science during some part of its history.

4. And more recently, the lists compiled in *Beiträge* (see, e.g., Kaczmarek 2007). Of course, the most obvious place to look for the increase of scholarship in the history of linguistics would be the *Bibliographie Linguistique de l'année 2003* compiled by Sijmen Tol & Hella Olbertz (Dordrecht: Springer, 2007). There one can count 319 entries (pp. 47–69), and together with "Biographical data" (69–83), one would come to a total of 636. In 1973, i.e., thirty years earlier, we had 131 "History of Linguistics" entries only.

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Those familiar with my efforts in the History of Linguistics over the years may agree that I have attempted to contribute in all these three areas. We can now safely say that, owing to the organization of the field in many places of the world, that all these three – if not four, if we include the editing of hitherto unpublished texts or republication of long forgotten books – types of historiographical work have been widely produced and, I think, historians of linguistics have outstripped any expectations one may have had during the pioneering 1970s.

This may be a rather quick *tour d'horizon* of our field, speaking in general terms and without making particular claims about research conducted in the various specialist areas of individual researchers, be it devoted to the Spanish Siglo de Oro, 17th century in Britain, *le siècle des Lumières*, the early Middle Ages, the classical or any other period, whether in the Western tradition or not. I myself can claim to only some specialist knowledge in 19th and 20th century European and North American linguistics, and usually do not venture much outside this terrain. Others have ventured well beyond a century or two (e.g., Law 2003) or have covered a particular research area much more thoroughly than anyone before (e.g., Graffi 2001). If we were to speak of the history of linguistics in Spain, for instance, we would have considerable evidence of the growth of scholarly activity over the past twenty years (e.g., Quilis & Niederehe, eds. 1986; Niederehe 1994–2005; Esparza & Niederehe 1999; Koerner & Niederehe, eds. 2001, among others and, most recently, Esparza et al., comps. 2008)).

This brief survey more or less states what has become obvious to the active practitioner of linguistic historiography, each of whom could add recent publications in his area of interest to the bibliography here appended. It may thus be appropriate that I take a leap from the present to the future for the remainder of this chapter.

6. Remaining challenges in linguistic historiography

Despite all the achievements referred to so far – and I know that I have barely touched the surface of what has actually been done by so many scholars around the world in the Historiography of Linguistics especially over the past fifteen and more years – I still do not think that there is that much reason to be smug. I feel, although I know that I shall have to leave it largely to the next generation to ensure continuity and quality of research in the field, that we must remain vigilant and not become self-indulgent, but remember that the History of Linguistics should remain an integral part of the language sciences and not become a separate department, either organizationally or conceptually (for instance by disappearing in a general intellectual history program). In fact, we should heed Raffaele Simone's

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(1995) warning that we ought not become mere historians of linguistic ideas, but should remain active within linguistics *tout court* as well, since "Purus historicus est asinus", as he warns us. This concern has been with me all along, and I returned to it at the 1999 ICHoLS meeting (Koerner 2003a), and not simply because of the lack of a suitable topic for an international meeting. That a historian of linguistics should also be a linguist however broadly defined does deserve to be reiterated. This may not apply with the same stringency to pre-19th century linguistics, but I cannot imagine anyone making sense of the work of Pāṇini for instance who is not thoroughly trained as a grammarian of Sanskrit (apart from knowing the intellectual context in which his system of analysis has been conceived).[5]

There are other issues – apart of the continuing battle for legitimization of linguistic historiography – which I remain concerned about. They pertain to questions of methodology and epistemology. I have addressed a number of these in various places and on many occasions ever since I decided, back in 1970, to concentrate my efforts on working in the history of linguistics. Specifically, I devoted papers to the concept of 'metalanguage' (see the relevant chapters in Koerner 1989 and 1996), to the argument of 'influence' (e.g., Koerner 1987, 2001), and other issues, attempting a summary of the potential problems of disagreement on questions of method at the 1993 ICHoLS Conference (Koerner 1995a). In recent years, I have added the issue of ideology in linguistic argumentation as an important subject in linguistic historiography (e.g., Koerner 2000, 2003c). It seems to me that a broader consensus on or at least a wider awareness of these to my mind important subjects would be desirable in the further development of the craft. It is my hope that the scholarly community will meet those challenges to the benefit of everyone of us.

In the meantime, much positive work in linguistic historiography has been produced, for instance in what has become to be known as 'missionary linguistics' (see Zwartjes & Hovdhaugen, eds. 2004, Koerner 2005, Zwartjes & Altman, eds. 2005; Zwartjes et al., eds. 2007, McGregor 2008, Tomalin 2008) and indications are that there will be more work done in this area as in many others in years to come.[6]

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5. I shall deal with Frederick Newmeyer's worries expressed in the introduction to his 1996 collection of papers on the history of generative linguistics in North America in Chapter 2, Section 2.

6. As a matter of fact there have in recent years been developments on the internet that should not only draw the community of historians of linguistics closer together but which have provided the means to make primary (including archival materials) as well as secondary sources available to any interested party. I must leave the proper appraisal of these developments to those more tuned into these developments than myself.

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CHAPTER 2

Pour une historiographie engagée; or where historians of linguistics could still do better

1. The issues

Several years ago, members of the Henry Sweet Society got to read a lengthy quotation from Frederick Newmeyer's introduction to his 1996 book *Generative Linguistics: A historical perspective* in which he reports that many of his colleagues "feared that [he] would become tarred with the brush of being an 'historian of linguistics', who, […], occupy a status level even lower than that of a 'semiotician'" (*HSS Bulletin* 26.25). Newmeyer explained "That this attitude results from the belief that most people who write on the history of linguistics have only the most minimal training in modern linguistics and devote their careers to attempting to demonstrate that their pet medieval grammarian or philosopher thought up some technical term before somebody's else's pet medieval grammarian or philosopher" (1996: 2).

This is no doubt a caricature of what most of us have been doing during the past twenty and more years, but the suspicion may be lurking that on some aspects Newmeyer's friends may not have been entirely off the mark. One does not have to share Rüdiger Schreyer's more recent assessment either, according to which "nobody takes much interest in, or notice of, linguistic historiography – nobody in the big world beyond the ivory towers [of academe] and nobody in the linguistic community that is the natural habitat of the linguistic historiographer" (2000: 206), and maybe this would be too much to expect: 'beyond the ivory towers' even Noam Chomsky would not have become known as he has, had he not become a critic of American foreign policy. One may be more inclined to share the late Peter Schmitter's disappointment that the findings of linguistic historiography have not successfully entered into textbooks, dictionaries of linguistic terminology, and other such places. He was no doubt right in saying that it is not enough to write "intelligent treatises on the necessity and usefulness of historiographic research", although he concedes (2003: 214) that he himself had no concrete proposal to make as to how to remedy the situation. It may well be that many practitioners of linguistic historiography have become too self-satisfied and inward looking over the years, given the availability of three journals, several bulletins, an ever increasing number of colloquia, conferences, and other international meetings around the world. It

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seems to me that there is enough blame to go around. In this chapter, I intend to voice a series of critical observations based on my thirty-five and more years in the field, while at the same time offering a number of suggestions as to how the history of linguistics may improve its scholarship, and thus its general image.

2. The challenge

Several years ago, Frederick Newmeyer appears to have shocked many historians of linguistics, when he noted that his American colleagues had suggested to him that he should return to real (read: generative) linguistics instead of writing about its history since, in his experience, most linguists regard those engaging in this field as occupying "a status level even lower than that of a 'semiotician'" (1996: 2) [in Section 1 above this is referenced as *HSS Bulletin* 26.25]. As I have stayed aloof of semiotics all my life (despite the fact I have written a great deal on Saussurean linguistics), I cannot tell how well this area of interest is being regarded outside of the specialist circles. But since I have devoted much of my life to working in the history of linguistics, Newmeyer's report should be of some concern to me, too.

Apparently, the editors of the *Henry Sweet Society Newsletter* at the time, Jonathan Hope and Laura Wright, were very concerned about their own status as scholars. They reprinted the complete passage from Newmeyer's introductory chapter in its May 1996 issue, p. 52. It appears that the two of them did take Newmeyer's warning to heart, since I haven't seen any work of theirs in this field since that time. (A year later they signed as editors of the *Bulletin* one last time.)[1]

I did not think that the worries expressed by Newmeyer in his 1996 collection of papers on the history of generativism in North America need of necessity apply to the great majority of linguistic historiographers, but the suspicion remains that some of these concerns might be justified. I would therefore like to quote two paragraphs from his Introduction (p. 2):

> The reaction of generative grammarians to my decision to chronicle and analyze the history of the field was more complex [than "from the community of historiographers of linguistics" (p. 1)]. On the one hand, they feared that I would become tarred with the brush of being an 'historian of linguistics', who, to many generativists, occupy a status level even below that of a 'semiotician'. This attitude

1. That they, in their comments, should have confused 'historical linguistics' with 'the history *of* linguistics', as I know is done by librarians quite regularly, suggests to me that they did not quite know which field they were in. That this confusion does also exist in the minds of some linguists, becomes evident when I receive for *Historiographia Linguistica* a submission on "Revising Finnish Consonant Gradation", for instance.

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> results from the belief that most people who write on the history of the field have only the most minimal training in modern linguistics and devote their careers to attempting to demonstrate that their pet medieval grammarian or philosopher thought up some technical term before somebody else's pet medieval grammarian or philosopher. Some well-meaning friends warned me that to join their ranks would be to commit professional suicide.
>
> On the other hand, there is also a certain snob appeal to being a front-line theoretician. Departing from their ranks to take a (more-or-less) detached view of their work appeared to many to be opting out of the only important task for a linguist: pushing back the frontiers of theory. While writing *LTA* [i.e., Newmeyer 1980], I was made to feel by some theoreticians like an art historian or critic who flunked out of art school and ended up living out his or her creative fantasies by passing judgment on the output of the real artist.

I'll gladly leave the full interpretation of these passages to others. At least on one point I am in agreement with Newmeyer, namely, that a historian of linguistics should also have a training in linguistics *tout court*. One could cite a number of examples where writers have gone astray because they did not understand the linguistic issues. This precondition may not apply with the same stringency to pre-19th century linguistics, but I cannot imagine anyone making sense of etymological efforts from Plato's *Cratylus* to modern times without proper philological knowledge – apart from the intellectual contexts in which these ideas were conceived.

3. Continuing methodological and philosophical disagreements

There are other issues – apart of [replace of with from] the persisting fight for legitimization of linguistic historiography vis-à-vis non-members of the 'discourse community' (Watts 1999: 43) – which I remain concerned about. They pertain to questions of methodology and epistemology. I have addressed a number of these in various places and on many occasions ever since I decided, back in 1970, to devote my efforts to the history of linguistics, a risky business at the time for anyone aspiring to an academic career. Specifically, I wrote papers to the concept of 'metalanguage' (see chapters in Koerner 1989 and 1995b), on the argument of 'influence' (e.g., Koerner 1987, 2004: 65–100), and other issues, attempting a summary of the potential problems of disagreement on questions of method at the 1993 ICHoLS Conference (Koerner 1995a).

It seems to me that a broader consensus on these would be desirable for the further development of the craft, but it requires the will of the scholarly community to meet those challenges head on. I only hope to have laid some of the issues on the table. (As some readers may have noticed, the last-mentioned paper has become in the eyes of certain scholars, both members of the editorial board of "Beiträge

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zur Geschichte der Sprachwissenschaft", as the epitome of 'positivism' [cf. *Beiträge* volume 13, No. 1 (2003), pp. 309 note, 316–317.[2] In Schmitter's [2003b: 53] terms, this position is to be characterized as 'naive realism'.)

I do not know how the generality of students of the history of linguistics took Newmeyer's statement at the time; since I had never regarded his work as serious historiographic scholarship but ideologically driven partisanship for a particular school of thought, I did not think I needed to be concerned about my own reputation. I had done linguistic and philological work well before I decided that I wanted to do history of linguistics, and I chose areas I could reasonably feel to be à cheval of.

In 2003, the late Peter Schmitter asked himself how far we got and whether indeed the findings of linguistic historiography have successfully entered into textbooks, dictionaries of linguistic terminology, and other publications that students of the language disciplines would get their information from. Sadly, his findings were disappointing, and historians of linguistics may ask themselves why this is so. Having lived most of my academic life in North America, I had been under the impression that the neglect of history and, in fact, the widespread absence of a historical consciousness of the part of the *homo americanus* was not to be found in 'Old Europe'. Schmitter was probably right in saying (2003a: 123) that it is not enough to write 'intelligent treatises on the necessity and usefulness of historiographic research',[3] but he also conceded (p. 214) that he himself had no concrete proposal to make as to how to remedy the situation.

It may well be that many practitioners of linguistic historiography have become too self-satisfied and inward looking over the years, given the availability of three journals, several bulletins, an ever increasing number of colloquia, conferences, and other international meetings, so that Rüdiger Schreyer's recent indictment, cited to [delete] by Schmitter (2003a: 116–117) may indeed not be too far off the mark – even though one may also detect some personal bitterness in this observation:

> In-group self-image rarely corresponds to out-group image. Historiographers of linguistics may see themselves as making an important contribution both to linguistics and history […]. However, to put it bluntly, nobody takes much interest in, or notice of, linguistic historiography – nobody in the big world beyond the ivory towers [of academe] and nobody in the linguistic community that is the natural habitat of the linguistic historiographer. (Schreyer 2000: 206)

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2. It is unusual to see two reviews of Schmitter (2003b) in the journal of which the author of the book is also the co-editor.

3. Since Schmitter (p. 115, 116n.3, and bibliography, p. 125) refers to two of my own such efforts (Koerner 1999a; b), I cannot but agree with his stricture.

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4. Observations on the development of history-writing in linguistics

During the early 1970s, in the earlier stages of the organization and institutionalization effort of linguistic historiography as a *bona fide* field of instruction within linguistics proper, it seemed natural to make a strong appeal to the methodological soundness of the subject in order to render it respectable in the eyes of 'real' linguists for whom linguistics meant 'theory' (cf. Koerner 1974, 1976 as examples of this approach). This original attitude toward matters historical might, at least initially, have had something to do with the success of Chomsky's *Cartesian Linguistics* (1966), given that Chomsky was in a way combining theory with an interest in finding antecedents to what he was doing. Even though this type of ancestor hunt, an essentially presentist approach, was soon discredited, Chomsky's incursions into the linguistic past made an engagement in this kind of activity appear legitimate for a number of North Americans during the late 1960s and early 1970s.

In Europe as well as among European-born linguists living in America, a historical approach to many subjects has had a long tradition, and this may explain why the scholarly reactions to Chomsky's *Cartesian Linguistics* were almost universally critical, at times rather harshly so (cf. Koerner & Tajima [1986: 24–26] which lists some 30 reviews of the English original alone). This long-standing European interest in the History of Linguistics would also explain the large number of textbooks in this area which had been written prior to 1966, from Delbrück in 1880 to Leroy (1963), Malmberg (1964), or Ivić (1965), at least the last two of which were first written in 1959 in Swedish and Serbo-Croatian, respectively, i.e., several years before the Ninth International Congress of Linguists held in Cambridge, Mass., in August 1962 at which Chomsky, for the first time, ventured into pre-20th-century linguistic thought (cf. Koerner 1978: 15–32, covering the 1916–1961 period).

In the meantime, from the late 1970s onwards, the History of Linguistics has become more of a recognized subject of serious scholarly endeavour, notably in Europe but also elsewhere, and it appears to many in the field that a discussion of the subject's *raison d'être* is no longer required. Perhaps given my long-standing North American exposure in matters historical, I may be permitted to differ, for my intention had never been to convince people in Germany, Italy, or Spain for instance that a historical perspective to their work in linguistics or language philosophy would be desirable. It would have meant carrying coal to Newcastle, so it has always seemed to me, since in these and many other countries there has been a long-standing tradition to approach subjects in a historical mode.

In North America, the situation does not look as rosy. With the advent of structuralism in its various articulations (Bloomfieldian descriptivism, Chomskyan generativism, etc.), even historical aspects in linguistics proper had generally fallen

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by the wayside.[4] Chomsky's *Cartesian Linguistics*, while it gave the History of Linguistics an initial boost and probably motivated the editor of *Current Trends in Linguistics* to have, as the concluding stone to his multi-volume edifice begun in the early 1960s, a two-volume *Historiography of Linguistics* (Sebeok, ed. 1975), it cannot be said that either undertaking produced much useful scholarly activity in the field, and probably neither of them really could: Chomsky's work was not truly history, but as he now would call it, an account of the way in which he believes things should have happened but didn't,[5] and Sebeok's volumes essentially contain contributions surveying past writings in the history of linguistics but including little original research – and certainly not a demonstration of how work in this area of interest ought to be conducted.[6] I could also add that, still today, the North American Association for the History of the Language Sciences (NAAHoLS), launched late in 1987, counts fewer than one hundred members, and that no more than a dozen papers are usually given at its annual meetings which are regularly held together with those of the Linguistic Society of America (which itself counts "about 4,000 personal members").[7]

For some time now, I have felt that changes in the kind of research that should be undertaken and in a variety of places has already been taken should also be reflected in historiographic research more generally. If I see my own work correctly, I have practiced for a long time what I'd like to call a historiography *engagé*: an effort, on the one hand, to defend persons whose work has been maligned by later generations and, on the other, to set the record straight and destroy (as much as I have been able to on the basis of the available evidence) a number of *fables convenues* or myths maintained in the literature. However, such efforts cannot be everything in linguistic historiography, whose tasks the late R. H. Robins saw in essentially three areas: (1) Discussion of the methodological foundations of the field; (2) studies "devoted to particular trends or movements of thought on language and the development of particular linguistic concepts"; (3) "[b]iographical accounts of the work of individual scholars who have been influential in the course taken by linguistic

4. This attitude has also affected Historical Linguistics greatly, notably between 1933 (the year of Bloomfield's *Language*) and the 1980s (I would like to believe that the launching of *Diachronica* in 1984 has helped in the subject's comeback, notably during the 1990s).

5. See his most recent – and rather curious – views on the history of linguistics (Chomsky 1997a; b) and Joseph (1999), for a critique.

6. In fact, most of the methodological discussion in my own contribution to the project (Koerner 1975) was deleted from the manuscript by one of the associate editors of the volume whose work in this area I had dared to criticize.

7. Information supplied by Margaret W. Reynolds, Executive Director of the LSA (e-mail to author, 8 April 1999).

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science", and probably a fourth area, which he defined as the "editing and publication of previously unedited texts" (Robins 1978: xii–xiii). No one will deny that they all are worthy causes that should be maintained, but it may be asked whether they are sufficient to make the history of linguistics interesting or even relevant to people outside of the growing number of participants in linguistic historiography. Peter Schmitter was right in saying, as quoted above, that it is not enough to write 'intelligent treatises on the necessity and usefulness of historiographic research', but he also conceded that he had no concrete proposal to remedy the situation. Perhaps the remedies of the deplorable situation that he identifies lie elsewhere?

5. Some possible remedies and changes in direction

My own analysis – and suggestions for the possible improvement of the situation identified by Schmitter and Schreyer – goes into a somewhat different direction. I feel that the current practice of the history of linguistics is too narrowly focused on strictly linguistic issues plus questions of philosophy and methodology pertaining to the treatment of this practice. It is not a new insight that language and, by extension, linguistics in some form or other permeates human life, including academic, but also political life, and the latter in ways we may not always be aware of. Recent works by Hutton (1999), Hausmann (e.g., 2000), and still more recently Knobloch (2004a) have shown that there are more issues and research areas that the history of linguistics must investigate.[8] The entanglement of linguistics with politics and ideology which works like these illustrate has in recent years made me change my own focus of attention, too (cf. Koerner 2000, 2001, 2002b, 2004a; b).

As a result, it has become my endeavour to widen the horizon of what may be included under the umbrella of 'History of the Language Sciences', if we recognize that questions such as religion, ethnic identity, nation building, and many other issues that we see occurring in the world have at the bottom more often than not linguistic and historical underpinnings. This reminds me of the *raison d'être* for the *Zeitschrift für Völkerpsychologie* und *Sprachwissenschft*. The first 2004 issue of *HL* constitutes an attempt to illustrate this understanding. Some readers may find that there is little 'linguistics' in the articles by Professors Bergunder and Trautmann or the review of a related volume by Dr Mees, but I'd argue that everyone familiar with Historical Linguistics, notably Indo-European, will recognize how much the debates of language origins over the past 150 years are involved here. Time will tell whether this broadening of the scope will be met with success as I hope it will,

8. See also the review and, respectively, the review article in *HL* (Tuite 2003; Knobloch 2004b).

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and make the pages of this journal interesting to more than the 'regular' consumer of history of linguistics matters. In the meantime, readers interested in the 'Out of India' and Indo-Aryan 'invasion' debates may get a fair idea from reading the articles by linguists, philologists, and archaeologists (including some pretending to be at least one of the three) published in *The Journal of Indo-European Studies*, volume 30/2002, notably pages 273–410 and 31/2003.107–240. (It needs to be seen whether the 2004 election victory of the India Congress Party will at least stem the tides, not only on the scholarly side but, much more importantly, on the political end where it really matters.)

Other lines of research I'd like to see pursued in linguistic historiography concern those that deal with the role of language in disciplines such as psychology for instance, similar to what one can typically find in *The Journal of the History of the Behavioral Sciences* (on whose editorial board I sat during 1997–2000, trying to have Linguistics play a larger role).

If I report on some experiences as Editor of *Historiographia Linguistica*, something I normally don't go public on, it would be in connection with our search for answers why the History of Linguistics as an academic pursuit still appears to have a low standing in some quarters outside of our field of interest. Over the past few years, I received among others the following submissions to *HL* – I shall leave particulars aside and mention the titles of the papers in rough outline (and in English even if the paper was written in a language other than English)[9] in order not to identify their authors:

1. 'The codification of American Indian languages'. The best thing one could say about this paper was that it had a good bibliography and included even the most recent articles published on missionary linguistics in *HL*. It did not include a single original idea, however. (Besides, there was no evidence that the author had studied any of these languages himself.)
2. 'The historical reinterpretations of the field in Antoine Meillet's work'. The author missed his subject because he tried to do too many things in his 60-page treatise, including criticizing Mounin's misinterpretations of forty years ago and demonstrating that others had copied them uncritically. Further, important primary sources had been missed.
3. 'The "History of Linguistics" written by a South American priest'. It was at best a book report that could have been adequate for an undergraduate seminar in which each student does one such report on a recent book on the subject.

9. These titles are given in single quotation marks here in contrast to submissions in English.

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4. 'The revival of Portuguese linguistics in the European context'. The paper did not contain anything that could not have been found in the writings of Eugenio Coseriu of the 1970s for instance.
5. 'The so-called 'middle voice' in the Greek grammatical tradition'. General comment: A total lack of focus; serious omissions in the bibliography; poor English style.
6. 'Pāńini and present-day historical linguistics'. It may perhaps have been of interest to some Indologists, but the paper lacked a real historical perspective. A was a mixture of Pāńini an ideas, those of subsequent Indian grammarians and certain features of Sanskrit. Pāńini's *Ashtadhyayi* was not cited even once.
7. 'Frege's views of language in the context of 19th-century German linguistics'. One referee wrote: "I have rarely come across a paper as muddled as this one" and "do not publish this piece, if you can help it".
8. A paper on a 17th-century lexicographer who the author felt had not been given his due caused one referee to comment: "This is a typical example of someone looking at a text/linguist in relative isolation and then deciding he must be revolutionary!" It was obvious that the author was not sufficiently aware of the intellectual context in which this particular lexicographer found himself.[10]

My list of complaints could be extended *ad nauseam*. What I find particularly disheartening is the reliance on translations and the ignorance of important materials not available in English. What I find particularly galling are treatises that not only rely on translations, but take passages out of context, linguistic and otherwise, to prove a preconceived idea such as suggesting that Humboldt was a racist, remarks found in Herder or Friedrich Schlegel show them as precursors of Nazism, and so on. There were submissions that received comments from three referees each and were returned for further, substantial revision; several of them were not heard from again since; apparently, the referees had asked for too much additional work from the author. It seems that there are a number of those who still believe that doing history of linguistics is easy. It seems that Newmeyer's remarks cited earlier apply in certain instances.

10. Another writer submitted a paper on his 'pet author', a certain late 19th-century grammarian from the Canarian Islands, who had written among others *Compendio de gramática castellana* (1895), without being able to show his significance for the study of Spanish.

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6. Concluding remarks

From the examples cited above, readers may draw their own conclusions. It occurs to me that the best advice to anyone wishing to distinguish him- or herself in the History of Linguistics, a subject I believe to be important for the intellectual health of Linguistics *tout court*, would be to undertake careful research, which also includes 'fare la bibliografia', which some seem to regard as a hindrance to their 'originality', before submitting anything for possible publication. Supervisors of theses should not encourage work [which they are not 'à cheval' with themselves]. I think that if we do our work well, we need not worry about the reputation of our subject. Good work speaks for itself. Extended metahistoriographical discourses (e.g., Schmitter 2003b) won't be of significant use in its furtherance.[11]

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11. Following my talk at the Oxford meeting, the late Werner Hüllen (1927–2008), longtime President of The Henry Sweet Society for the History of Linguistic Ideas, graciously sent me a copy of his collection of papers (Hüllen 2002), drawing my attention especially to his 1996 treatise of altogether 80 paragraphs of philosophical reflection, terminological definitions, and methodological recommendation, suggesting that my complaints about the sorry state of affairs would be quite unwarranted if his recommendations were to be followed. My own work in linguistic historiography, which antedates Hüllen's efforts by almost fifteen years, does not lead me to that kind of optimism.

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CHAPTER 3

La place du *Bulletin de la Société de Linguistique de Paris* parmi les principales revues de linguistique de son temps

SUMMARY / RÉSUMÉ

In 2005 the *Société de Linguistique de Paris* (S.L.P.) was to celebrate the appearance of the 100th volume of its *Bulletin*, which had long since become a major, internationally recognized journal. In preparation of this event, a conference was held at the École Normale Supérieure, Paris, 19–20 November 2004. My original task hd been to evaluate the place of the *Bulletin* within linguistic periodicals in general. This goal however could not be attained within the short time allotted. A comparison of the *Bulletin* of the *Société de Linguistique de Paris* (*BSLP*) with the journals outside of France over the past 100 years, not even for the first ten or so years of its long life, 1869–1879, could only scratch the surface. The first decade was arguably the most important period, during which the journal had to define itself and develop its status as the central voice of linguistics in France. Indeed, 1876, the year the *Bulletin* was given a more precise direction (and definition of its coverage) by ministerial decree, stands out as a singular point in time in the history of linguistics as a science as we know it (cf. Koerner 1976, for details) – from Verner's Law being published in April that year to Saussure's arrival in Leipzig in October, and much beyond (mention must be made of the books by Leskien, Sievers, and Winteler or the papers by Brugmann, Osthoff and others, and, perhaps also, Saussure's first submission to the *BSLP*). Instead of fulfilling the original task, the present chapter, after some general remarks on the importance of journals for the health of the discipline, offers yet another (cf. Meillet 1930, Vendryes 1955, Benveniste 1971, Bergounioux 1996, 1997) historical account – this time not from a presiding secretary of the S.L.P. – of the early years of the *Bulletin* and its relations with other journals inside and, in particular, outside of France. It concludes with a presentation of a select chronologil list of philological and linguistic periodicals published between 1841 and 1891.

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1. Remarques préliminaires

La création, la péremmité, le succès économique et la continuité d'une revue scientifique sont un indice important de la santé d'une discipline (cf. Koerner 1973c, 1984). Au XIXe siècle – et même au XXe – on pourrait identifier un certain nombre de revues qui ont réussi et d'autres non. Entre ces dernières, certaines ont cessé de paraître à la mort de leur créateur et rédacteur: je pourrais citer le cas de Friedrich Techmer (1843–1891) de l'Université de Leipzig, fondateur de l'*Internationale Zeitschrift für Allgemeine Sprachwissenschaft* (1884–1890, 5 vols.) et sans successeur (Koerner 1973b). La linguistique générale d'inspiration humboldtienne, représentée par la revue de Techmer, n'était pas dans l'air du temps; le positivisme scientifique régnait en maître au XIXe siècle. Cette atmosphère intellectuelle de l'époque expliquerait, par exemple, le manque de succès de la *Zeitschrift für die Wissenschaft der Sprache* d'Albert Hoefer (1812–1883) de l'Université de Greifswald, censée s'adresser au même public, et qui atteignit à peine 4 volumes (1846–1853).[1]

Par contre, la revue du *Gymnasialprofessor* de Berlin, Adalbert Kuhn (1812–1881) de la même génération que Hoefer, la *Zeitschrift für vergleichende Sprachforschung*, créée en 1850 – avec l'indologue Theodor Aufrecht (1821–1907) qui, dès le deuxième volume, abandonna son poste de co-rédacteur – a été un grand succès. La 'revue de Kuhn' ou, plutôt, 'Kuhn's Zeitschrift', comme on a continué à l'appeler jusqu'en 1987, quand certains dirigeants ont trouvé opportun de changer son nom pour celui de *Historische Zeitschrift* à compter du cent-unième volume – quel manque de sens historique! – a été, pendant les années 1858–1876 de cristallisation de la linguistique indo-européenne, accompagnée des *Beiträge für vergleichende Sprachforschung*, une revue à part co-rédigée par August Schleicher (1821–1868) jusqu'à sa mort prématurée. Les *Beiträge* avaient été institués afin d'accueillir le grand nombre d'articles et de comptes rendus concernant les langues indo-aryennes, celtiques et slaves qui n'étaient pas encore traités dans la *Zeitschrift*. De plus, la revue, qui publia des articles cruciaux pour la linguistique historique comme ceux de Graßmann (en 1863) et de Verner (en 1876), absorba les *Beiträge zur Kunde der indogermanischen Sprachen* (Göttingen, 1877–1906) en 1907.

Mais la 'Kuhn's Zeitschrift' n'était pas la seule revue allemande – ou étrangère – de l'époque: je parle des cinquante années, de 1841 à 1891, pour lesquelles j'ai dressé une liste fondée pour une grande part sur des compilations faites il y a plus de trente ans (cf. Koerner 1972, 1973a). Afin de donner une idée de la production

1. L'histoire s'est répéteee au XXe siècle avec la revue de Johannes Lohmann (1899–1982?) de l'Université de Freiburg, *Lexis: Studien zur Sprachphilosophie, Sprachgeschichte und Begriffsforschung* (Lahr in Baden, 1948–1955, 4 vols.).

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des revues d'une certaine envergure parues pendant cette période, j'ai présenté cette liste des revues de philologie et de linguistique européennes chronologiquement. Je la proposerai à la fin de ma présentation.

2. Petit survol historique

On m'avait demandé – et je ne sais pas comment je pourrais le faire – de préparer un travail concernant l'importance du *Bulletin* lancé quelques années après la fondation de la Société de Linguistique de Paris en 1863[2] – selon Antoine Meillet (1966 [1930]: 444) par « un groupe d'amateurs éclairés » – à l'instar des revues créées approximativement à la même époque à l'étranger, surtout en Allemagne qui, pendant le XIXe siècle et jusqu'à la fin de la Première Guerre Mondiale, dominait la plupart des sciences, y compris la linguistique. Meillet désiginait Antoine d'Abbadie (1810–1897), membre de l'Académie des Sciences depuis 1852, et le comte Charles Félix Hyacinthe de Charencey (1832–1916), auteur de nombreuses études sur les langues amérindiennes, dont plusieurs sont encore citées cent ans plus tard (e.g., Campbell 1997: 53, 439), comme les fondateurs de cette société savante. On devrait ajouter, peut-être, le nom d'A. Dufriche-Desgenettes (1804–1878), qui était membre dès l'origine et qui, comme les deux aristocrates et 'amateurs éclairés', ont disparu à peu près complètement des annales de la linguistique française.[3] Comme il arrive le plus souvent, ce sont leurs successeurs qui ont écrit l'histoire, y compris

2. La date n'est pas tout à fait exacte si l'on ne tient pas compte de la date de la première réunion informelle au domicile d'Antoine d'Abbadie; il faudrait plutôt lire: 1864, lorsque ce groupe d'ʻérudits éclairésʼ avait adopté un premier règlement (Vendryes 1955: 8), date retenue par Bergounioux, dans son article de fond (1996), qui retient 1864 (p. 8 et ailleurs) sans indiquer de source concernant ce choix. (On pourrait donner d'autres dates; par exemple, 1865 comme Vendryes le suggère dans le titre de son article ou l'année correspondant à l'approbation des ses premiers statuts en 1866 [cf. Vendryes 1955: 13].) Si l'on parle des « réunions informelles » (p. 10), il y aurait une indication dans le rapport présenté par Robert Mowat (1823–1912), officier de carrière, en 1878 (Bergounioux 1996: 11). Meillet (1966: 444) se trompait également lorsqu'il déclarait que Abel Hovelacque « fondait en 1869 une revue linguistique »; la date exacte était 1867, et le fondateur était Chavée (v. infra).

3. Bergounioux (1996), dans son historique sur les origines de la. S.L.P., mentionne d'autres 'érudits éclairés'. En particulier, il met en relief le rôle que jouait l'helléniste Émile Egger (1813–1885) comme 'protecteur' de la jeune génération, y compris Michel Bréal, à compter de sa présidence de la Société en 1866 et la 'victoire' des comparatistes contre les fondateurs (pp. 12–16 *passim*), qui créent une Société Philologique par la suite (1869). Les détails donnés par Vendryes (1955: 8–11) sont les plus complets à ce jour; Vendryes, Secretaire de la S.L.P. depuis la mort de Meillet en 1936, a certainement eu accès à la documentation la plus complète.

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l'histoire de la Société de Linguistique de Paris. (Pour des détails sur Dufriche, créateur, paraît-il, entre autres du terme 'phonème' en 1873, v. Koerner 1978 et également Benveniste 1971: 24–25.)

J'ai appris d'un article de Jean-Claude Chevalier (1988: 127) que, selon une étude qui avait été faite en 1887, il existait en France au XIXe siècle, avec des pérennités variables, environ six cents sociétés savantes avec « des statuts bien sûr inégaux selon qu'il s'agit de l'Institut de France ou d'une Société de petite ville ». Lancer une société de linguistique à l'époque n'avait rien de particulier et même le nom d'une telle sociéte n'a rien d'original. Dans son article sur les quatre-vingt-dix premières années de la S.L.P. paru en 1955, Joseph Vendryes (1875–1960), élève et ensuite collaborateur de Meillet, ignore le fait qu'il y avait une première société de linguistique, bien avant la première rencontre en mai 1863 entre Antoine d'Abbadie et les autres 'amateurs éclairés' (Vendryes 1955: 8; Benveniste [1971: 20] répète la fable). Une société portant ce nom,[4] dirigée par un certain Casimir Henricy (1819–1892),[5] existait entre la fin des années 1850 et le début des années. Parmi les membres correspondants de cette société, on trouvait le Belge Honoré-Joseph Chavée (1815–1877), qui fonda en 1867 – bien entendu! – la *Revue de Linguistique et de Philologie comparée*,[6] Émile Littré (1801–1881), l'auteur du fameux *Dictionnnaire de la langue française* (Paris: Hachette, 1863–1873), et notre Dufriche-Desgenettes, qui contribuait par une série d'articles sur l'alphabet universel à la revue de cette société, *La Tribune des Linguistes*, en 1859 et 1860 (v. Koerner 1978: 129n, 135). En 1874, le grammatologue belge Julien Tell (né en 1807) rapporte que ce « journal attaquant les abus, l'ignorance et la superstition, se fit naturellement beaucoup d'ennemis, et l'entreprise ne put pas se maintenir » (p. 364). Tell nous détaille également le programme de la société qui publiait la revue (ibid.):

> Il s'est formé à Paris en 1858, une société de gens d'élite, qui ont eu l'idée de fonder un journal linguistique, où l'on parlait d'études philosophiques, de réformes orthographiques, d'alphabet universel, de langue universelle, enfin de tout ce qui a rapport à la Philologie.

Ce programme diffère considérablement des objectifs de 'notre' Société de Linguistique (avant la réforme de 1876) qui avait « pour but l'étude des langues, celle des légendes, traditions, coutumes, documents pouvant éclairer la science

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4. Dans un article de 1859, Dufriche-Desgenettes rapporte (p. 2) qu'il avait présenté en 1856 « devant la *Société internationale* [*sic*] *de Linguistique* » une version abrégée de son système d'un alphabet universel.

5. Henricy est mentionné, entre autres, comme auteur d'une *Histoire de l'Océanie depuis son origine jusqu'en 1845* (Paris: Pagerre, 1845), 380 pp. en petit 8°.

6. Après la guerre franco-prussienne, Chavée fut nommé professeur d'allemand à l'École Supérieure Militaire, étant retourné à Paris en 1867 après avoir passé cinq années comme professeur de grammaire comparée à la Scuola Normale de Pise.

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ethnographique » (cité d'après Vendryes 1955: 13), mais qui n'avait pas encore une définition clairement orientée vers la linguistique proprement dite. Il est évident que les idées des fondateurs, d'Abbadie et de Charencey, étaient alors prépondérantes. Il fallait donc une 'révolution'[7] effectuée par la génération suivante après la Guerre de 1870. Je parle de la génération de Michel Bréal (1832–1915) et d'érudits encore plus jeunes comme les romanistes Gaston Paris (1839–1903) et Paul Meyer (1840–1917), ou autres encore.

Si, en effet, la Société de Linguistique de Paris avait été établie en 1863 (cf. Benveniste 1971: 22) – et non en 1865 (comme le rapporte Vendryes 1955), cela aurait correspondu à une date propice: 1863 marquait l'entrée en fonction, suite à sa nomination par Napoléon III, du nouveau Ministre de l'Instruction Publique (1863–1869), l'historien Victor Duruy (1811–1894), qui lançait une série de propositions concernant la réforme de l'enseignement supérieur.

Toujours selon le rapport de Meillet, la Société de Linguistique de Paris « après quelques mois d'essais, se constituait en 1865 et […] était officiellement organisée le 8 mars 1866 » (1966 [1930]: 444). On doit encore attendre 1869 pour que le premier fascicule du *Bulletin* de la Société paraisse. En attendant, plusieurs événements ont beaucoup contribué au succès de la Société. En 1864, Michel Bréal, après avoir suivi à Berlin les cours de Franz Bopp (1791–1867) pour la grammaire comparée et ceux d'Albrecht Weber (1825–1901) pour le sanskrit (et soumis sa thèse ès-lettres à Paris sur *Hercule et Cacus* en 1863), était nommé chargé de cours pour la grammaire comparée des langues indo-européennes au Collège de France et titularisé deux ans plus tard. En 1867 Bréal devenait membre de la Société et, l'année suivante, son secrétaire, poste qu'il a gardé jusqu'à la fin de sa vie. Le rôle de Bréal dans la politique académique – et même au-delà dans la vie intellectuelle en France du dernier tiers du XIXe siècle – ne peut être sous-estimé. Au contraire, il me semble qu'il était très important dans ce qu'on pourrait appeler 'la linguistique organisée' dans la France de l'époque. Lorsque le Ministre Duruy avait lancé, en 1866, des cours de soir, « dispensés par quelques maîtres spécialisés à un public restreint et tournés vers la recherche […] sur le modèles des séminaires allemands » (Bähler 2004: 124), il ne fait guère de doute que c'était à l'initiative de Bréal. Plus important encore me semble le rôle que celui-ci a joué dans la création de l'École Pratique des Hautes Études, fondée officiellement le 1er janvier 1869, et il devenait le directeur pour la IVe section, des « Sciences historiques et philologiques ».[8]

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7. Par une ironie du sort la veritable révolution avait été réalisée par le Conseil d'État en 1876, qui, en reconnaissant la S.L.P. d'utilité publique, réformait ses statuts (cf. Benveniste 1971: 25, pour les détails).

8. Selon Vendryes (1955: 11), c'était « sans doute la première fois que dans un programme d'enseignement le nom de science était attribué à ces disciplines. » Je suis certain que Bréal avait le terme allemand 'Wissenschaft' à l'esprit.

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La maison à Landau et la plaque commémorative/Palatinat où est né Bréal

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Ceux qui sont plus familiers avec la vie et l'œuvre de Michel Bréal pourraient sans doute ajouter à son palmarès d'autres hauts faits. (On raconte, par exemple, que c'est Bréal qui a donné au jeune baron Pierre de Coubertin (1863–1937) l'idée des Jeux Olympiques qui ont été rétablis à Athènes en 1896 – au moins l'idée du marathon [cf. Lovett 1997].)[9]

Deux autres érudits devraient être mentionnés ici: les romanistes Gaston Paris (1839–1903) et Paul Meyer (1840–1917) qui me semblent avoir joué un rôle important dans la professionalisation des études philologiques en France dans le dernier tiers du XIXe siècle, à partir de la fondation, d'abord, en 1865, de la *Revue critique d'Histoire et de Littérature* (Paris, 1866–1935),[10] et ensuit en 1871,[11] de *Romania: Recueil trimestriel consacré à l'étude des langues et des littératures romanes* (Paris, depuis 1872).

Gaston Paris avait fait d'abord des études de langues et littératures romanes à Bonn du temps de Friedrich Diez (1794–1876). Après avoir appris l'allemand (qui était la première langue de Bréal, né à Landau dans le Palatinat) pendant son séjour à l'Université de Bonn durant l'année académique 1856–1857, il avait suivi un cours de philologie classique avec Otto Jahn (1813–1869). Ensuite il se rendit à Göttingen en octobre 1857, où il suivit – sur la recommandation de deux professeurs parisiens[12] – les cours de philologie classique d'Ernst Curtius (1814–1896), frère aîné de Georg Curtius (1820–1885), mais également un cours sur la *Chanson de Roland* texte dont le professeur Theodor Müller (1816–1890), réalise la première

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9. Il se pourrait dire ce que l'historien-sociologue des sciences, Robert K. Merton (1910–2003), en 1968 pour la première fois avait appelé 'the Matthew Effect' (selon St. Mathieu xiii:12 et xxv:29) ait joué un rôle ici, i.e., trop d'événements attribués à une personne dans sa vie publique (cf. Merton 1988, pour une élaboration de cette observation).

10. La *Revue critique* évidemment suivit le modèle du *Literarisches Centralblatt für Deutschland* qui paraissait *à* Leipzig depuis 1850: elle était consacrée uniquement aux comptes rendus des publications de philologie et linguistique en France et à l'étranger. Le style des comptes rendus était également plus professionnel que dans les revues françaises d'alors.

11. Je donne les années des prospectus que Paul Meyer et Gaston Paris ont adressés à leur éditeur Friedrich Vieweg (cf. Bähler 2004: 697–698 et 669–702 respectivement. (Je ne résiste pas au plaisir de mentionner que Vieweg était l'éditeur de la 2e édition du *Mémoire* de Saussure en 1887 et partir la même que celui de Koerner 1973a.)

12. A savoir (cf. Bähler 2004: 51) Charles-Benoît (*alias* Carl Benedict) Hase (1780–1864), qui depuis 1852 occupait la chaire de grammaire comparée créée pour lui à la Faculté des lettres de Paris, et Joseph-Daniel Guigniaut (1794–1876), qui enseignait, entre autres, la géographie à la Sorbonne. C'est le même Guignaut qui écrira plus tard, en 1869, la « Notice sur la vie et les travaux de M. François Bopp », publiée dans les *Mémoires de l'Académie des Inscriptions et Belles-Lettres* 29:1.201–224 (1877), reproduit dans Franz Bopp, *Analytical Comparison* (de 1820) éd. par E. F. K. Koerner (Amsterdam: John Benjamins, 1974; 2e éd., 1989), pp. xv–xxxviii.

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edition critique de ce fameux texte littéraire.[13] Gaston Paris retourna à Paris en août 1858 où il devient le romaniste le plus éminent de France.

Il semble que, par contr Paul Meyer ne soit pas allé en Allemagne faire ses études. Diplômé de l'École des Chartes en 1861, il travailla ensuite au Département des manuscrits de la Bibliothèque Impériale (1863–1865), puis aux Archives Impériales[14] (1867–1872). A partir de 1876 il occupa le poste de Professeur de langues et littératures de l'Europe méridionale au Collège de France. Parallèlement (1876–1907), il fut Directeur de l'École des Chartes, où il avait auparavant donné des cours d'histoire de la littérature provençale. Il semble que Paul Meyer, dans ces différents postes, ait eu beaucoup de temps pour la lecture et la réflexion. (Je ne connais pas suffisamment sa biographie pour le presenter plus en détail, mais il y a nombre d'exemples dans l'histoire des sciences de langage où des bibliothécaires ont développé une production prodigieuse). Il est de fait que Meyer a très tôt dans sa carrière décidé de jouer un rôle important dans le développement des standards scientifiques de la philologie et de la linguistique en France. C'est dans ce but qu'il fonda, à l'âge de vingt-cinq ans, avec Gaston Paris (âgé de vingt-six ans), cette *Revue critique* dans laquelle ils voulurent, entre autres, offrir à la France un périodique comparable au *Litterarisches Centralblatt* de Leipzig et de l'*Athenaeum* de Londres (qui existaient depuis 1850), c'est-à-dire une publication hebdomadaire qui (selon leur prospectus) "ne se composera que d'articles critiques sur les ouvrages nouveaux" destinées "à la fois à rapprocher ceux qui s'y livrent et à *contrôler* leurs travaux [c'est moi qui souligne]".[15] En 1903, l'année de la mort de son co-rédacteur, revenant sur les premières années de la *Revue critique*, Meyer caractérisait la raison d'être du journal de la façon suivante: "… signaler les bons livres et, plus encore, stigmatiser les mauvais, […] renouveler notre haut enseignement, […] réformer l'université" (cité d'après Boës 1996: 633).

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13. Pour des détails sur la vie et les études de Gaston Paris en Allemagne, v. Bähler (2004: 38–88); elle montre également (pp. 81–82) que Paris a plutôt suivi le cours de philologie romane de Nicolaus Delius (1813–1888) que celui de Friedrich Diez (1794-1876), dont Paris traduit d'abord l'*Introduction à la Grammaire des langues romanes* (Paris: A. Franck, 1863) et ensuite, avec d'autres, dont Auguste Brachet (1845–1898), la *Grammaire des langues romanes* en 3 volumes (Paris: A. Franck, 1872–1874). Cependant, Paris aussi suivait "plus ou moins régulièrement […] le cours de lecture de *Gerusalemme liberata* chez Diez" (Bähler 2004: 44).

14. Qui, suite a désatre de Sedan le 2 septembre 1870 et à la proclamation de la république deviennent, respectivement, la 'Bibliothèque Nationale' et les 'Archives Nationales'.

15. V. le prospectus du 25 octobre 1865 développé par Gaston Paris et le Silésien Hermann Zotenberg (1834–*post* 1886), ami de Gaston Paris depuis ses études outre-Rhin et plus tard conservateur de la Bibliothèque Nationale. Ce prospectus fut contresigné par le philologue d'origine suisse Charles Morel (1837–1902) et par Paul Meyer (Bähler 2004: 697). Pour l'historique de la *Revue critique*, v. Bähler (2004: 121–135).

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Comme J.-C. Chevalier l'a montré dans deux articles, Paul Meyer, qui s'était déjà fait la réputation d'un critique sévère comme co-rédacteur de la *Revue critique*, condamna la *Revue historique de l'ancienne langue française et Revue des patois de la France* lancée par un libraire-éditeur de Niort (Deux-Sèvres), Léopold Favre (1817–1892), en 1877–1878 (Chevalier 1988, 1989). Cette *Revue historique*, qui se voulait à l'inverse « animée d'intentions bienveillantes pour tous les travailleurs, louant avec juste mesure […] » (« A nos lecteurs », cité dans Chevalier 1988: 134), ne survivra pas au delà de son second volume.[16] Comme cela se passait une douzaine d'années après la création de la *Revue critique*, la situation avait changé. La France avait perdu la guerre contre l'Allemagne, et un consensus eut tôt fait de s'établir à fin le renouvellement d'une telle catastrophe, en imitant le modèle allemand dans l'éducation et la recherche scientifique. En janvier 1872, Gaston Paris et Paul Meyer publièrent le premier fascicule de *Romania: Recueil trimestriel consacré à l'étude des langues et des littératures romanes*, et nous lisons, entre autres, dans leur prospectus (p. 1):

> […]: l'œuvre que nous voulons entreprendre, si elle est avant tout scientifique, est en même temps nationale, et nous avons la ferme conviction que la rupture trop brusque et trop radicale de la France avec son passé, l'ignorance de nos véritables traditions […], doivent être comptés parmi les causes qui ont amené nos désastres. […] nous nous maintiendrons avec un soin rigoureux dans la pure région de la science impartiale; mais c'est précisément cette habitude d'impartialité et d'étude méthodique qu'il faudrait substituer pour toujours à la légèreté superficielle, aux vaines prétentions[17] qui nous ont fait tant de tort.

Idéologie nationaliste à part (v. Bergounioux 1989), nos rédacteurs font référence à la revue allemande *Germania* fondée en 1855 par Franz Pfeiffer (1815–1868), professeur à l'Université de Vienne, pour justifier la choix de leur titre en disant (ibid.): « il nous a paru naturel de donner le nom de *Romania* au recueil où nous voulons faire pour les nations romanes ce que la *Germania* fait pour les nations germaniques ». Du point de vue du contenu, ils se réfèrent au *Jahrbuch für romanische und englische Literatur* (Leipzig, 1859–1871; n.s. 1874–1876), créé par le bibliothécaire de la Cour impériale d'Autriche, Ferdinand Wolf (1796–1866),[18]

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16. La critique féroce de Meyer visa également d'autres entreprises de Léopold Favre, mais avec un moindre succès (v., pour les détails, Chevalier 1989).

17. Bähler, qui reproduit le prospectus au complet (2004: 699–702), donne (p. 699) 'préventions' par erreur (cf., pour un bref extrait, Boë 1996: 633).

18. Il me paraît intéressant de noter que ni Pfeiffer – qui avait lancé sa revue contre la *Zeitschrift für deutsches Altertum (und deutsche Literatur)* qui existait depuis 1841 – ni Wolf n'étaient allemands, ils étaient autrichiens.

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dont Paulin Paris (1800–1881), son homologue dans la capitale française, et son fils Gaston avaient fait la connaissance pendant une visite à Vienne en 1856 à la recherche de manuscrits médiévaux.[19]

On notera que, même dans la création d'une revue pour les langues et la littérature romanes, Gaston Paris et Paul Meyer ont été devancés: depuis janvier 1870 paraissait à Montpellier la *Revue des langues romanes*, et les rédacteurs de la *Romania* étaient tenus de le reconnaître. Sans nommer les rédacteurs, ils les caractérisent, dans leur prospectus comme « d'excellents et zélés travailleurs » (*Romania* 1.3 [1872]).[20] J.-C. Chevalier (1988: 139) a noté que, contrairement au destin de la *Revue historique de l'ancienne langue française*, qui a été « soumise aux censeurs de la *Romania* » (1877: 305, 477–478, 630) et qui, selon lui (Chevalier 1989: 167), avait été un périodique « [m]almené, censuré, écrasé par les augures parisiens », la *Revue des langues romanes* avait été traitée par la *Romania* « avec plus de retenue », en partie, semble-t-il, parce qu'elle « exerce régionalement un pouvoir surveillé par les intendants du pouvoir scientifique (les maîtres étant allemands) » (Chevalier 1988: 134) et « parce qu'elle apporte des matériaux, parce qu'elle se limite à la langue d'oc […], parce qu'elle accepte l'ascèse des méthodes nouvelles » (p. 141). (Cette revue existe encore aujourd'hui; en 2001, elle était arrivée au 105e volume [v. Bergounioux 2001].)

La revue *Romania* a été un grand succès; elle a continué à jouer un rôle important dans son domaine jusqu'à présent (cf. Monfrin 2001: 87–103). Par contre, la *Revue critique d'Histoire et de Littérature*, suspendue entre 1870–1871, a été refondée en 1876 sous la direction de Gaston Paris, de Michel Bréal et d'autres, y compris l'élève de Paris, Gabriel Monod (1844–1912).[21] Elle a été arrêtée en 1935.

Les pages précédentes dressent, à peu près, le panorama en France de la fin des années soixante, jusqu'au milieu des années soixante-dix du XIXe siècle en ce qui

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19. Une grande partie au titre de rédaction du *Jahrbuch* a été assurée par Adolf Ebert (1820–1890), depuis 1862 professeur ordinaire à l'Université de Leipzig pour des langues et littératures de langues romanes et de l'anglais, après avoir passé les années 1849–1862 comme *privatdocent* et professeur extraordinaire à l'Université de Marburg, et un peu plus tard également par Ludwig Lemcke (1816–1884), qui en 1862 succéda au poste d'Ebert à Marburg, et qui ensuite, en 1867, fut nommé professeur ordinaire de philologie romane et anglaise à l'Université de Giessen. Selon Bähler (2004: 475–477) Gaston Paris s'inspire largement dans ses études sur la poésie épique, de l'œuvre de ce dernier.

20. Par ailleurs, une deuxième revue doit être mentionnée qui devançait la *Romania* d'un an; il s'agit des *Romanische Studien* d'Eduard Boehmer (1826–1906) parus d'abord à Strasbourg en 1871, ensuite à Bonn. Sur le rapport un peu difficile entre les rédacteurs respectifs de ces deux revues, v. Bähler (2004: 450, n.140).

21. Bähler (2004: 124) fait mention d'un certain C. de La Berge sur lequel je n'ai aucune information. Bähler (ibid., note 291) affirme que « [c]ette refondation était motivée par un changement d'éditeur: de Vieweg on passait à [Ernest] Leroux, qui, contrairement au premier, était en mesure de rémunérer les collaborateurs », mais je ne pense pas que cela ait été la seule raison.

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concerne les revues philologiques dans lequel s'insère la fondation de la Société de Linguistique de Paris et la publication des premiers fascicules de ses *Mémoires* en 1868 et de son *Bulletin* en 1869 (cf., pour les détails, Bergounioux 1996). En même temps, il nous faut avoir une idée de la situation de l'infrastructure et de la scène intellectuelle dans lesquelles se situait le système universitaire à Paris à l'époque. J.-C. Chevalier (1989: 166) nous a donné l'aperçu suivant:

> Les Sociétes [savantes comme la S.L.P.] ont […] besoin des maîtres parisiens, et particulièrement quand ils sont auréolés de leurs relations avec les Universités étrangères. […]
>
> La position des maîtres parisiens n'est pas moins ambiguë. Certes, appuyés sur des titres prestigieux, les Chartes ou Normale supérieure, jouissant du label allemand, ils tranchent avec autorité et usent volontiers du sarcasme. Mais ils sont très peu nombreux et ce sont surtout des généraux sans troupes: la Sorbonne a une seule salle de cours […] pour très peu d'étudiants – jusqu'à l'attribution de bourses de licence (1877) et d'agrégation (1880), jusqu'à la construction du « palais » de la nouvelle Sorbonne –; et quant à l'École Pratique des Hautes-Études (1868), ses rares séminaires, aux professeurs mal payés, rassemblent des poignées de disciples.

On ne devrait pas négliger que la publication du premier fascicule du *Bulletin* avait été devancée de deux ans par la publication de la *Revue de Linguistique et de Philologie comparée* rédigée d'abord par Honoré Chavée (1815–1877), un peu plus tard par Abel Hovelacque (1843–1896), nommé en 1876 à la chaire d'anthropologie linguistique de l'École d'Anthropologie fondée par Paul Broca (1824–1880), et ensuite par d'autres érudits qui avaient également une orientation privilégiant les langues 'exotiques' comme Lucien Adam (1833–1918) et surtout Julien Vinson (1843–1926), professeur d'hindustani et de tamoul à l'École des Langues Orientales. Il y aurait beaucoup à dire du groupe de linguistes réunis autour de cette revue (cf. l'étude fondamentale de Desmet 1996). Une chose doit être retenue ici, qu'ils se sont inscrits philosophiqement dans le matéralisme de Broca et l'évolutionnisme de Darwin de sorte qu'ils ont suivi linguistiquement – mais pas intégralement en ce qui concerne la rigueur de ses travaux scientifiques – le legs d'August Schleicher (1821–1868), et qu'ils se sont de fait isolés par la suite des idées des Néogrammairiens, en particulier de leur insistance sur la régularité des 'lois phonétiques'.

Je ne sais pas ce qui a incité Michel Bréal à faire traduire les deux fameux essais « darwiniens » de Schleicher (1868 [1863, 1865]), étant donné que Bréal a toujours été opposé à une philosophie qui faisait rentrer la linguistique parmi les sciences naturelles. Certes, son avant-propos est très bref et évasif.[22] Déjà Meillet,

22. La série des traductions qui a été lancée avec ces articles schleicheriens n'a pas eu de suite, que je le sache. Vendryes (1955: 18) rapporte que « [d]ans la séance du 20 décembre 1873, sur un rapport de Bréal, la Société fut saisie d'une proposition tendant à créer 'une collection qui

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en 1930, se posait la question de savoir pourquoi Bréal avait décidé de traduire la *Vergleichende Grammatik* de Bopp en quatre volumes, qui ne représentait plus l'état actuel de la science linguistique (Bopp 1866–1872),[23] au lieu du *Compendium der vergleichenden Grammatik der indogermanischen Sprachen* de Schleicher en deux volumes, publié pour la première fois en 1861–1862 et qui a encore connu trois éditions (1866, 1870 et 1876), à quoi s'ajoutent des traductions en italien (1869) et en anglais (1874–1877), et qui, selon Meillet (1966 [1930]: 445), « a exercé sur le développement de la grammaire comparée une action décisive ». Meillet (ibid.) explique l'attitude de Bréal de la façon suivante:

> Dans la leçon d'ouverture du cours de 1867–1868, professée le 9 décembre 1867 et publiée dans les *Mémoires de la Société de Linguistique*, I, page 72 et suivantes, Bréal annonce la mort récente de Fr. Bopp et, à ce propos, il indique où en était la grammaire comparée; il signale les erreurs commises, les progrès accomplis, les directions où il convenait de s'engager. Il cite les principaux maîtres qui, alors, à côté de Bopp, avaient développé la grammaire comparée. Un nom manque: celui de Schleicher.[24] Ce silence avait un sens: Bréal refusait de suivre les savants allemands dans la voie où ils étaient entrés à la suite de Schleicher. Des lors, Bréal était appelé à devenir de plus en plus un isolé.

Je pense que Meillet parlait surtout de Bréal comme scientifique, et non comme politicien académique. Dans le même article, Meillet (pp. 446–447) met en relief chez Bréal « une rare vertu: le libéralisme. Chez les jeunes qui venaient le trouver » – et cela devrait inclure Meillet lui-même à l'époque – « il ne cherchait que les promesses de talent, pour mettre à leur service son influence qui, durant longtemps, a été puissante. » Meillet fait référence à Louis Havet (1849–1925) et James Darmesteter (1849–1894) et ensuite au jeune Saussure au profit de qui Bréal abandonna, en 1882, son enseignement à l'École des Hautes Études.

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s'appelerait Bibliothèque de la Société de Lingustique et qui comprendrait des ouvrages de forme très diverse présentant seulement ce caractère commun de ne pas pouvoir facilement se découper en articles pour entrer dans les fascicules' (voir *Bull.* t. II, p. lxxxi). » Ce projet n'a pas abouti.

23. Pourtant, Bréal en 1866 reçut le Prix Volney pour sa traduction, tandis que Schleicher ne recevait que la moitié du Prix pour le *Compendium* l'année suivante. Selon Dietze (1960: 279), Schleicher présumait que Bréal avait joué un rôle dans cette décision. Pour une appréciation de l'œuvre de Schleicher, v. Koerner (1989: 325–375).

24. Pour cette raison, il paraît intéressant de noter que Bréal, en 1878, constata: « La linguistique proprement dite, la science de Bopp et Schleicher, n'a encore qu'une seule chaire dans toute la France » (*Bulletin de la Société de Linguistique de Paris* 4, p. lxxi).

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3. En guise de conclusion

J'avoue qu'il m'était difficile, impossible même, de répondre à la demande du Secrétaire de la Société de Linguistique d'une façon satisfaisante. D'abord, le sujet n'était pas suffisamment bien circonscrit. Estce qu'il s'agit d'un effort de comparaison de la diversité du contenu et/ou de la qualité des contributions à cette revue par rapport à d'autres périodiques de l'époque? Une telle évaluation ne serait pas facile à faire: je devrais couvrir nombre de domaines de la linguistique – historique et comparative, indo-européenne et non-indo-européenne, théorique et descriptive. Un tel travail est impossible à accomplir aujourd'hui (sans doute, Meillet aurait pu le faire). Autrement, cela serait plutôt le projet d'un groupe de chercheurs dont les collaborateurs interviendraient plus ou moins dans tous les domaines. Alternativement, un tel travail pourrait être le sujet d'une thèse de doctorat d'état d'au moins cinq cents pages.[25]

Une autre possibilité de traiter en quelque façon de la revue de la Société de Linguistique serait peut-être de faire un peu de statistique. Par exemple, on pourrait compter les nombres d'articles et/ou de comptes rendus qui ont parus pendant une période donnée par rapport à d'autres revues qui couvrent plus ou moins les mêmes sujets, et ensuite essayer une sorte d'évaluation – comme l'ont fait Campos Alberca et alii (1999) pour l'Espagne durant les années 1985–1994. Aussi pourrais-je imaginer de placer le *Bulletin de la Société de Linguistique* au centre de mon attention et essayer d'analyser son rayonnement, par exemple son impact sur l'érudition dans les périodiques et les monographies des autres pays. Bref, je vois que je me perds entre tant de possibilités, et je ne me sens pas en mesure de prendre le taureau par les cornes – il y aurait trop de taureaux dans cette *corrida*. En ce qui me concerne, face à ce défi je déclare forfait.[26]

A la place, j'offre ici une petite liste chronologique dans laquelle s'insère le *Bulletin de la Société de Linguistique de Paris*, bien au mileu des revues scientifiques entre 1841 et 1891.

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25. Un travail comparable de 633 pages a été déjà fait par Piet Desmet dans son ouvrage de 1996, *La linguistique naturaliste en France (1867–1922)*, qui traite surtout de la *Revue de linguistique et de philologie comparée*, qui de 1867 à 1916 a rassemblé surtout des linguistes en opposition à la Société de Linguistique.

26. Pour un survol des revues linguistiques et philologiques de langues modernes de l'époque, je pourrais faire référence au travail important de Storost (2001: 1259–1264). Je ne connais pas un travail comparable pour la linguistique générale ou la linguistique historique et comparée.

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Appendice

Liste sélective des revues de philologie et de liguistique
dans l'ordre chronologique, 1841–1891

Zeitschrift für deutsches Altertum (und deutsche Literatur). Wiesbaden **1841**–

Journal of the American Oriental Society. New Haven, Conn. **1843**–

Revue de Philologie, de littérature et d'histoire ancienne. 1re série, 2 vols. **1845**–1847; n. s., Paris 1877–1926 [= 50 vols. en tout]. (Repr., Amsterdam: John Benjamins, 1973–1974.)

Rheinisches Museum für Philologie. Frankfurt/Main, n.s. **1845**– [La revue a été nommée "*Rheinisches Museum für Jurisprudenz, Geschichte und griechische Philologie*" à l'origine, 1827.]

Archiv für das Studium der neueren Sprachen und Literaturen. Elberfeld, ensuite Braunschweig, finalement Berlin **1846**– [également nommé *Herrigs Archiv*].

Berichte der (Königlichen) Sächsischen Gesellschaft der Wissenschaften. Leipzig **1846**–1848; n.s. 1849–1918.

Zeitschrift für die Wissenschaft der Sprache. Greifswald **1846**–1853 [= 4 vols.]. [Également nommée *Hoefers Zeitschrift*.]

Zeitschrift der Deutschen Morgenländischen Gesellschaft. Wiesbaden, plus tard Stuttgart **1847**–

Literarisches Centralblatt für Deutschland. Leipzig **1850**–1944. [N.B.: La numérotation des volumes commençait avec vol. 51 (1900).]

Zeitschrift für vergleichende Sprachforschung auf dem Gebiete des Deutschen, Griechischen und Lateinischen [depuis 1876: … *auf dem Gebiete der indogermanischen Sprachen*]. Berlin **1852**– [Suspendue 1945–1947. Absorba en 1876 – > *Beiträge zur Kunde der indogermanischen Sprachen* et, in 1907, également – > *Beiträge zur Kunde der indogermanischen Sprachen*. Également nommée *Kuhns Zeitschrift* « La revue de Kuhn ».

Transactions of the Philological Society. Oxford **1854**– [Ils remplacent – > *Proceedings of the Philological Society*].

Mededе(e)lingen van de Koninklijke Nederlandse Akademie van Wetenschappen. Afdeling Letterkunde (Series A & B). Amsterdam **1855**–; n.s. 1938–

Beiträge zur vergleichenden Sprachforschung auf dem Gebiete der arischen, celtischen und slavischen Sprachen. Berlin **1858**– [fusionna en 1876 dans –> *Zeitschrift für vergleichende Sprachforschung*.]

Revue des études grecques. Paris **1858?**–

Jahrbuch für romanische und englische Literatur. Leipzig **1859**–1871; n.s. 1874–1876 [= 25 vols.]

Zeitschrift für Völkerpsychologie und Sprachwissenschaft. Berlin **1860**–1890 [= 30 vols.]

Nordisk Tidsskrift for Filologi. Kobenhavn **1861**–1922.

Nyelvtudományi Közlenények: A Magyar Tudományos Akadémia nyelvtudományi bizottságának folyóirata. Budapest **1862**– [suspendu 1944–1947].

Zeitschrift für ägyptische Sprache und Altertumskunde. Berlin & Leipzig **1863**–1943, 1954–.

Anzeiger der (Kaiserlichen) Wiener Academie der Wissenschaften. Philosophisch- historische Klasse. Wien **1864**–

Rendiconti dell'Istituto Lombardo di Scienze e Lettere. Classe di lettere e scienze morali e politiche. Milano **1864**–1867; n.s. 1868–

(Izvestija i) Učenye Zapiski imperatorskogo Kazanskogo Universiteta. Section "Učenye *Zapiski*". Kazan' 1834–1861 [1862–1864 divisée en sections]; **1865**–1883 [continuée comme – > *Izvestija Kazanskogo Universiteta*

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Revue critique d'Histoire et de Littérature. Paris **1866**–1935 [suspendue 1870–1871; nouvelle série, 1876–].

Revue de Linguistique et de Philologie comparée. Paris **1867**–1916 [= 48 vols.].

Mémoires de la Société de Linguistique de Paris. Paris **1868**–1935. [Vol. II (1872–1875); vol. III (1876).]

Studien zur griechischen und lateinischen Grammatik. Leipzig **1868**–1877/78 [= 10 vols.; repr., Hildesheim: Georg Olms, 1969–1970 (= 4572 pp. en tout)]. [Également nommé *Curtius' Studien*.]

Bulletin de la Société de Linguistique de Paris. Paris **1869**–

Zeitschrift für deutsche Philologie. Halle/Saale, plus tard Berlin **1869**– [Également nommé *Zachers Zeitschrift*.]

Revue Celtique. Paris **1870**–1934.

Revue des Langues Romanes. Montpellier (plus tard également) Paris **1870**– [vol. 105 (2001).]

Proceedings of the American Philological Association. Hartford, puis Middleton, Conn. plus tard Ithaca, N. Y. **1871**–; v. également –> *Transactions of the American […]*

Romanische Studien. Straßburg, ensuite Bonn, **1871**–1895 [= 6 vols.].

Transactions (and Proceedings) of the American Philological Association. Hartford, ensuite Middletown, Conn., enfin Ithaca, N. Y. **1871**–; v. également –> *Proceedings of the American Philological Association*

Journal of the Royal Anthropological Institute. London **1872**–1898 [= 27 vols.]; n.s. 1899–1965 [= 95 vols.]; n.s. 1995–

Magyar Nyelvor. [Le Puriste hongrois]. Budapest **1872**–

Rivista di Filologia Classica. Torino **1872**–

Romania: Recueil trimestriel consacré à l'étude des langues et des littératures romanes. Paris **1872**–

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CHAPTER 4

On the disappearance of August Schleicher in the writings of the Neogrammarians

The case of the analogy concept in historical linguistics

1. Introductory observations

Historians of linguistics have long since learned that a certain distrust of what authors say in their programmatic statements is a healthy attitude. This applies in particular to statements made by those who have an obvious agenda. Let us assume that not unlike Chomsky in the late 1950s and early 1960s, the Indo-Europeanists of the circle around August Leskien (1840–1916), notably Karl Brugmann (1849–1919), saw themselves as ushering in a revolution of their field of study. This may explain much of what they said and did in their earlier years, until their ideas had become 'mainstream' by the 1890s.

Much has been said about the 'Chomskyan Revolution' and how it was brought about. In his plenary address at the Eleventh Congress of Linguists held in Cambridge, Massachusetts, in August 1962 and his subsequent elaborations in his book *Cartesian Linguistics* of 1966, Chomsky made every attempt to dissociate himself from his immediate predecessors, notably those whose ideas he had inherited, and tried, not always successfully, to make the world believe that his sources of linguistic inspiration hark back to much earlier periods, from the authors of the *Grammaire générale et raisonnée* of 1660 to Hermann Paul's *Principien* of 1880. In Chomsky's narrative an important place was assigned to Wilhelm von Humboldt's linguistic work, in particular his famous phrase "Die Sprache muss von endlichen Mitteln unendlichen Gebrauch machen [Language must make infinite use of finite means]", since this was supposed to show Humboldt as a generativist *avant la lettre* (adumbrated in Chomsky 1965: 8–9, made more explicit in Chomsky 1966: 20–21). Once these stories stuck within the scientific community, the work of Chomsky's teachers could be assigned to the dustbin of history: it needed no longer to be read; it had become irrelevant. The new 'paradigm' replaced the preceding, now a seriously dated one. Those in 'modern linguistics', who had lived through the 1960s and 1970s, could observe how *Syntactic Structures*, first published in 1957, was soon

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taken by many as marking the beginning of linguistics as a science, and that the work of Bloomfield and his followers could be ignored since it was 'pre-scientific' (see Koerner 2002: 151–209, for historical details).

The Leipzig *Junggrammatiker* of the mid-1870s saw themselves in a comparable situation of revolutionizing their discipline. As a result, scant or no reference was made to the preceding generation of historical linguists, except for citation of passages that they would find fault with. On the other hand, authors who were not their teachers and whose work was less than central to their own pursuits could be referred to as leading to their own program. Here the work of Heymann Steinthal (1823–1899)[1] and, more importantly in the present context, Wilhelm Scherer (1841–1886), served in this role as carriers of ideas that supposedly inspired the working out of their linguistic theories.[2]

For the present chapter, I have chosen the concept of 'analogy' which, next to the neogrammarian insistence on the rigorous application of 'Lautgesetze' ("sound laws"), was one of the two main pillars of their argument in matters of explaining linguistic change. It will be shown in this paper that while Wilhelm Scherer's book *Zur Geschichte der deutschen Sprache* (Berlin, 1868) was selected for special praise in Osthoff and Brugmann's 'manifesto' of 1878,[3] in particular for his use of 'false analogy' in the explanation of hitherto seemingly irregular forms, The Neogrammarians were entirely silent on the contribution of August Schleicher (1821–1868) in whose *Die Deutsche Sprache* (Stuttgart, 1860) and subsequent editions of 1869 and 1874 they could have found much more explicit statements concerning the workings of the analogy principle in language development than anywhere in Scherer's book.[4]

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1. Brugmann (and Osthoff) cite in particular Steinthal (1860) as having provided the "first outlines of this science" according to which, in their view, "psychological factors […] are at work in countless sound changes and innovations as well in all so-called analogical formations" (1967 [1878]: 198).

2. Both were professors at the University of Berlin with no obvious Leipzig connection, Steinthal since 1862, Scherer since 1877. Johannes Schmidt (1843–1901), professor of Indo-European linguistics at the same university since 1876, remained on fair scholarly terms with Leskien only; both had been students of Schleicher in Jena and together they edited the 3rd and 4th ed. of his *Compendium* (published in 1871 and 1876, respectively). Yet Schmidt remained an opponent of the *Junggramatiker* and what they stood for, and a staunch defender of Schleicher's legacy.

3. As we know since Brugmann's own testimony of 1900 that in fact it was he who had drafted the 'Vorwort' with Osthoff doing some minor editing only. I therefore shall invert the names in the subsequent discussion. Crediting Brugmann exclusively would mean an injustice to Osthoff who bore the brunt of the ensuing attacks.

4. As in the case of Bloomfield for Chomsky, it certainly was useful for the strategy of the Young Turks of Leipzig that Schleicher was no longer around to draw public attention to his pioneering work; any defense from their students could be dismissed once they had discredited their heritage.

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2. The place of 'analogy' in the neogrammarian doctrine

A thesaurus would offer the following possible (semi-)equivalents to 'analogy', among others: similarity, resemblance, parallelism, agreement, correspondence. It takes time and particular circumstances that an expression of regular language becomes a technical term within a given discipline. The word 'analogy', which most speakers of English have used in one way or another, e.g., to express parallel action, involuntary movement, psychological behaviour, has taken on a specific meaning in the development of linguistics. As works like those of Skousen et al. (2002), Itkonen (2005), Wanner (2006), and others attest to (e.g., more recently, Hill 2007, Fischer 2008), 'analogy' for all its 200-or-so year history in the discussion of language structure and change,[5] is still a subject of attempts at more precise definition, specific categorization and classification (found as early as in Wheeler 1887; see Kuryłowicz 1995 [1949], for a classic attempt), broader application (e.g., in what is subsumed under 'cognitive linguistics'; cf. Anttila 2003), but also used in the investigation of underlying principles and psycho-sociological motivations of language use.[6]

For the present paper, the somewhat formal definition from the 1992 Oxford edition of the *International Encyclopedia of Linguistics* may suffice, even though its author gives only scant credit to those scholars in historical-comparative Indo-European linguistics who upheld the concept during the 1970s, when it was under attack from the Young Turks of the day:[7]

> Analogical change, or simply analogy, is a historical process which projects a generalization from one set of expression to another. The term 'analogy' has been used also in reference to the acquisition of grammatical regularities by child or adult learners, and to the use (production or comprehension) of novel utterances. The latter was traditionally attributed wholly to analogy; […] (Kiparsky 1992: 56)

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5. This presupposes that we credit, as Hock (2003: 443) has done, Friedrich Schlegel (1977 [1808]: 6–7) with the use of 'analogy' in his programmatic statement about the establishment of genetic relationship among certain European languages. It is obvious from Hock's quotation, however, that Schlegel did not use *Analogie* as a specific theoretical concept. See Koerner (1989 [1987]: 278) for the historical context in which Schlegel used the word.

6. Given this fact, it is very surprising indeed that neither in the first 10-volume edition of the *Encyclopedia of Language & Linguistics* (Oxford & New York: Pergamon, 1994), nor in its expanded 14-volume edition (Oxford & New York: Elsevier, 2006) do we find a separate entry on 'analogy', although both carry entries on 'sound laws'.

7. Typically, one of the strongest defenders of analogy during those years, Raimo Anttila, is represented by a single bibliographical entry (as against a dozen listed in Anttila & Brewer 1977: 2–3). There is not even a mention of Anttila's 1977 monograph *Analogy* (The Hague: Mouton) that could have been expected in an encyclopedia; instead Kiparsky (MIT Ph.D. 1965) lists seven papers of his own not all of them truly pertinent! Anttila & Brewer (1977: 21–22), on their part, list seven writings of Kiparsky's, including his unpublished dissertation on phonological change.

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In the writings of the *Junggrammatiker* (translated less precisely as 'Neogrammarians' ever since the 'Leipzig school' had become known outside German-speaking lands), one would look in vain for similarly formal definitions; the positivistic age felt more comfortable with the presentation of concrete examples and subsequent description or explanation of what most probably had occurred. The readers were expected to draw their own generalizations from the data presented.

Both the method of strict adherence to linguistic laws and the concept of analogy in the explanation of language change in the reconstruction of historical protoforms have rightly or wrongly – more wrongly than rightly, I would argue – been associated with the neogrammarian movement from the year 1876 onwards. Indeed, many important publications appeared during this 'annus mirabilis' (Hoenigswald), not just the famous article by Karl Verner (1846–1896) explaining the seeming irregularity of the third and last group of so-called exceptions to 'Grimm's Law'. For Brugmann and others around August Leskien (1840–1916), the first incumbent to the chair of Slavic philology in Leipzig, it was the latter's teachings and his methodological pronouncement of 1876 that marked the beginning of a new era in linguistics where the first point of their credo, the regularity principle of sound laws was concerned (for details, see the introduction to Wilbur, ed. 1977; also Koerner 1982).

In light of what Schleicher, Leskien's teacher during 1866–1867, said regarding linguistic method and the principles guiding the reconstruction of Indo-European (cf. Koerner 1981, 1982, for details), it is difficult to see anything revolutionary in Leskien's statement below made right at the end of a long, if not long-winded introduction to his monograph on Balto-Slavic and Germanic declension (Leskien 1876: xxviii)[8]:

> In my investigations I have started with the principle that the form of a certain case, as we meet with it, can never result from an exception to phonetic laws which are observed elsewhere. To prevent misunderstanding, I will add: if by 'exception' be understood those cases where the expected phonetic change has not taken place from definite ascertainable causes, such as the absence of *Lautverschiebung* in German phonetic groups like *st* etc., where one rule to a certain extent interferes with another, – then of course there is nothing to be said against the statement that phonetic laws are not infallible. For the law is not nullified in such circumstances, and works as we should expect it would do wherever these or other disturbances, i.e., the influence of other laws, are not present. But if we admit arbitrary, accidental deviations, such as are incapable of classification, we virtually confess that language, which forms the object of our research, is inaccessible to scientific investigation.

8. Quoted here in the English translation taken from Delbrück (1882: 60–61, note 1). The German original is cited in Wilbur (1977: xxv–xxvi).

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In Brugmann & Osthoff's (in)famous "Vorwort" to their newly founded journal *Morphologische Untersuchungen auf dem Gebiete der indogermanischen Sprachen* [Morphological investigations in the field of the Indo-European languages], to which only the two contributed during the entire life of this periodical,[9] Leskien is, next to Wilhelm Scherer (on whom see below), depicted as the shining light:

> Leskien above all seized upon the thought ["the seed sown by Scherer"], and since he reflected on the concept of "sound laws" and "exception to the law" more profoundly than had been done before, he arrived at a set of methodological principles which he at first made fruitful in his university lectures in Leipzig. (Brugmann & Osthoff 1967 [1878]: 205)

No mention of what Schleicher had said and practiced more than ten years earlier. However, our present paper is intended to focus on the analogy concept, not the sound laws and the debate surrounding what been thoroughly treated in Terence Wilbur's masterly historical account (1977: ix–xcv).[10] One cannot but agree with Wilbur, when he notes (p. xxv) that Leskien's 1876 book "was not a theoretical work at all", and we may add, not one laying out principles of research.

In their 'manifesto' Brugmann & Osthoff started with the following opening shot (1967 [1878]: 198):

> Since the appearance of Scherer's book *Zur Geschichte der deutschen Sprache* (Berlin, 1868), and principally through the impulses that went out from this book, the physiognomy of comparative linguistics has changed considerably. A method of research has been instituted since then and is winning more and more supporters; if differs in essential respects from the method by which comparative linguistics proceeded in the first half-century of is exsistence [probably since Bopp's *Conjugationssystem* of 1816].

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9. Creations of new journals by a particular group for the propagation of a supposedly new way of doing linguistics is not new; in the case of the adherence to the *Junggrammatiker* movement, we have the example of Hermann Paul (1846–1921) and Wilhelm Braune (1850–1925) launching the *Beiträge zur Geschichte der deutschen Sprache und Literatur* in 1874 in which the two almost exclusively published their studies. We may see parallels in the establishment of *Linguistic Inquiry* at MIT in 1970 followed by *Natural Language and Linguistic Theory* in 1983 and, in European contexts, *Linguistische Berichte* in Germany 1969 and *Journal of Linguistic Research* in The Netherlands in 1980, not to mention various 'working papers' of the 'revolutionary' period that tended to circulate among adherents of the school only.

10. At the same time, it is a bit surprising that in this account of more than 100 pages that the name of Schleicher does not figure at all. Only Georg Curtius (1820–1885), the teacher of Brugmann, Osthoff and many other students at Leipzig – including Ferdinand de Saussure – and his linguistic views are treated as representative of the earlier generation (pp. xxiii–xxiv, xxvi, and elsewhere).

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As is typical of such programmatic "revolutionary" pronouncements, the next paragraph is devoted to telling the reader that their predecessors had been saddling the wrong horse, were ignorant of 'real' language, ignored the speaker, etc. as if Schleicher for one had not been doing fieldwork in Lithuania, was not interested in dialectology, had not observed children learning their first language, etc., not to forget that he himself learned to speak several modern languages, something people like Brugmann had never shown much evidence that they had. Instead, Scherer, the Germanist, gets further credit (p. 199):

> Only in very recent times is one becoming more aware of that neglect [of "the psychological aspect of the speech process"]. Fortunately, the movement starting with Scherer's efforts, the "neo-grammarian movement", has already done away with some of the fundamental errors which dominated the entire older linguistics.

Scherer, who did not study with Steinthal (who was mentioned in positive terms for his psychological approach earlier in the 'Vorwort') during his Berlin years, now is being given such credit, probably because he was to appear as the crown witness for the concept of analogy several pages later (p. 203):

> As was already indicated above, it is Scherer's achievement to have effectively broached the question of how changes and innovations take place in a language. To the horror of not a few fellow investigators, but luckily for the discipline itself, Scherer in the book named above, made ample use of the principle of leveling in his explanations. Many forms of even the oldest historically accessible stages were suddenly according to him no other than formations by "false analogy"; […].

And if to add insult to injury to the preceding generation of scholars and outsiders to the *junggrammatische Richtung* (the "neogrammarian trend") Brugmann & Osthoff concluded the sentence averring that "until then investigators regarded these as purely phonetic developments from the original Indo-European forms" (ibid.).

To be sure, Scherer is not to blame for the use or abuse these young linguists made of his book. We will see later in Section 6 how original Scherer actually was in regard to the analogy concept over other scholars that Brugmann & Osthoff passed over in silence, notably Schleicher.

3. The treatment of Schleicher in linguistic historiography

More than thirty years ago, in my "Foreword" to Anttila & Brewer's 'basic bibliography' devoted entirely to the subject of analogy in linguistics, I boldly asserted:

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> That August Schleicher (1821–68), the teacher of such eminent linguists as August Leskien, J. Baudouin de Courtenay, and Johannes Schmidt, made frequent use of the analogy principle needs no substantiation here as it is a well-established fact in the annals of linguistic science. (Koerner 1977: vii)

Little did I know that in the great majority of historical accounts to the present day, Schleicher's contribution to linguistics has rarely been recognized, usually minimized, and not infrequently distorted, if not made the subject of ridicule. It seems that the Neogrammarians, from the late 1870s onwards, had succeeded in making the world believe that they had put the field of Indo-European comparative-historical philology on the proper scientific footing and that previous work with few exceptions could safely be ignored.

Myself, I had begun to study Schleicher's work during the first half of 1972, when writing what was to become Koerner (1975). It included an appraisal of Schleicher's contribution to the development of the scientific study of language evolution, his insistence on the importance of the adherence to 'sound laws', and his procedures of reconstruction of protoforms, and many more methodological advances discussed especially in the section "The Emergence of the Schleicherian Paradigm (1850–1870)" (745–759). This was intended to counter the rather ill-informed statements that I had encountered in the textbooks of the time.

Two quotations from the most (commercially) successful 'histories' of the late 1960s may suffice to illustrate the sorry state of affairs against which my complaints had been directed (I had received my first exposure to the History of Linguistics as a subject of instruction only in 1969 and acquired these and many other comparable books at the time):

> August Schleicher has to his credit a considerable output of work but few positive results. In spite of his admiration for the natural sciences and the amazing progress they were making by judicious use of the experimental method, Schleicher remained on the whole a man of the eighteenth century, intent of setting theory at the outset of research and making the facts fit into a predetermined logical scheme, […]. (Leroy 1967: 23)

> The conception of the sound law had been late in developing; Grimm had none of it, and mid-century scholars such as Schleicher were not troubled by apparent exceptions of the general run of sound changes in a language. But the years that followed the publication of Schleicher's *Compendium* [no date supplied] had seen the results of detailed research in the various branches of the Indo-european [*sic*] family, yielding more material and more evidence of order lying behind the sets of formal correspondences that had either puzzled or escaped the notice of earlier scholars; […] (Robins 1967: 183)

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One wonders whether these two writers had actually opened any book written by Schleicher,[11] especially the *Compendium*, first published in two large volumes in 1861–1862 (2nd ed., 1866), which must have been in considerable demand well after Schleicher's premature death in 1868, since no others than August Leskien (1840–1916) and Johannes Schmidt (1843–1901) saw to it that a third and a fourth edition were published as late as 1871 and 1876, respectively.

In the fourth edition of Robins' *Short History*, the above quotation was modified to read as follows:

> The conception of the sound law had been late in developing; Grimm and Bopp explicitly admitted exceptions, and Schleicher, despite an emphasis on regularity, allowed apparently irregular developments to pass as etymological evidence. The neogrammarians saw plainly the methodological requirements of comparative-historical linguistics as it had been practised in the past half century. (Robins 1997 [1967]: 207)

Having read, without acknowledgment, the various post-1967 analyses of Schleicher's work (e.g., Koerner 1981, 1982), Robins became much more circumspect in his choice of words, but his narrative displays diplomacy rather than careful scholarship: why use "apparently", if one is sure of what one is going to say, and who produced those "methodological requirements" that became so important for the Neogrammarians, if not their teachers Schleicher and Georg Curtius (1820–1885)?[12] Robins supplies no specifics.

In Bertil Malmberg's history of linguistics published in the same year as the third edition of Robins' book, we find Schleicher treated with regard to the genealogical tree ("Stammbaum"),[13] his alleged Darwinism (already discounted in Maher 1966), and similar popular subjects usually mentioned in conjunction with Schleicher, but Malmberg also pays tribute to his important contribution to the methodology of reconstruction in Indo-European linguistics (p. 301). However, one looks in vain for

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11. It had long since become fashionable to mention exclusively Schleicher's 1863 "Darwinian" essay of 31 pages (available in both French and English since 1868 and 1869, respectively) or, if they knew German, maybe also his subsequent 29-page essay (Schleicher 1865), as we still find it to be the case in Christy (1983: 120).

12. Thus even Berthold Delbrück (1842–1927), the self-appointed historian of the *Junggrammatiker* school, felt obliged to quote, among others, from Curtius' 1870 article on the range of application ("Tragweite") of sound laws the following passage that is of interest in the present context (and which I had quoted in Koerner 1977: vii) at greater length already: "Two fundamental notions are of the highest importance for linguistic research, that of analogy, and that of phonetic laws." (Delbrück 1882 [1880]: 105)

13. It is of course nice to see Schleicher still today being given credit for his genealogical tree in the literature (Blaček 2007), but this falls far short of an appraisal of his work in general.

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more than passing references to Schleicher in his long chapter on 'The definitive establishment of a comparative and historical science of Indo-European: The Leipzig school' (Malmberg 1991: 309–345; see pp. 311, 315, 327, 328, 335). There is no hint at the important indebtedness of the Neogrammarians to Schleicher's teachings. Indeed, it seems that since Berthold Delbrück's (1842–1922) *pro-domo* account of 1880 (see Delbrück 1882: 55–61) the view that the work of the *Junggrammatiker* had ushered in 'new beginnings' has become the standard view (cf., e.g., Hock 2003: 444).

It seems that we had to wait until 1992 for a more satisfying historiographical account of 19th-century linguistics, including the work of Schleicher, when Anna Morpurgo Davies published her extremely detailed and judicious account (see especially Morpurgo Davies 1998 [1992]: 167–172, 174, and elsewhere)[14] Still, if we were to rely on histories of the kind Pieter Seuren has produced, the distorted image of Schleicher, so familiar from the textbooks of the 1960s and 1970s, will continue to be around (see Seuren 1998: 84–86): Schleicher who could not make too much sense of the 'sound laws' (it required the next generation to put things right); Schleicher, the author of the genealogical tree that his former student Johannes Schmidt knocked down several years later; Schleicher, the follower of Darwin, and as the author of the fable written in Indo-European as a source for derision. Such ignorance of the available literature is painful indeed to those involved in trying to do justice to Schleicher's legacy.

4. 'Analogy' in 19th-century linguistic thought

It is not the place here to write the history of the analogy idea (which would have to be of monograph length). The word 'analogy' has a long history – together with its counterpart 'anomaly' – going back to the Classical period, as may be gathered from Best (1973: 13–23; see also Anttila 2003).[15] In the present context, suffice it to note that as a technical term in the analysis of language change 'Analogie' began to take shape most likely in the mid-19th-century linguistics only.[16] In Adolf Holtzmann's

14. See the many references to Schleicher in her "Index" (Morpurgo Davies 1998 [1992]: 430), many more than, for example, to Brugmann (cf. ibid., p. 413).

15. Typically, Best moves from this prehistory of analogy right to the Neogrammarians; Schleicher's name does not appear even once. Scherer (1868) is at least listed in the bibliography (p. 117).

16. Itkonen (2005: [v]) quotes a statement from Wilhelm von Humboldt with an 1812 date as a motto to his own book, but he does not supply its source. Hassler (2007: 164) offers an English translation of Humboldt's statement and supplies a bibliographical location. On Humboldt's

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(1810–1870) books on 'umlaut' and 'ablaut' of the early 1840s the idea comes up several times – though only once as 'falsche Analogie' (cf. Benware 1977: xv), well before Wilhelm Scherer (1841–1886) referred quite frequently to 'false analogy' in his *Zur Geschichte der deutschen Sprache* (Berlin: Duncker, 1868), which is usually cited in the history of linguistics as having made the concept of analogy *tout court* current. This happened, I suppose, because the frequent reference to Scherer's work as pioneering in Osthoff & Brugmann's influential 'Foreword' to the first volume of *Morphologische Untersuchungen* (1878) imprinted on the minds of the subsequent generations of comparative linguists, and those who wrote the historical accounts simply copied previous ones.

However, there are other 19th-century linguists referred to in the annals of the discipline who laid out the analogy principle before or contemporaneously with Scherer. The American W. D. Whitney (cf. Wheeler 1887: 44; Christy 1983: 87; Alter 2005: 222–223) and the Pole J. Baudouin de Courtenay (Anttila & Brewer 1977: xii; Mugdan 1984: 111; Adamska-Sałaciak 1996: 67) appear to be the most prominent contenders.

4.1 Analogy in Whitney (1867, 1875)

William Dwight Whitney (1827–1894) is not infrequently referred to as one of the 19th-century linguists who popularized the analogy principle, and indeed in his 1867 *Language and the Study of Language* the index (p. 491) promises under the first entry "Analogies"[17] that he means by this the "extension of prevailing [analogies], its influence in producing the changes of language". When looking up the page references (pp. 27–28, 82, and 85), one finds Whitney referring to such forms as *bringed* (instead of *brought*) produced by language learners – and even adult speakers – by analogy to other verbs with a dental preterit, and that these "lower

understanding of 'Analogie', see Di Cesare (1989). Noordegraaf (2005), for his part, takes the phrase "Man kann als einen festen Grundsatz annehmen, dass alles in einer Sprache auf Analogie beruht … [One can assume as a definite principle that everything in language is based on analogy …]" and supplies all sorts of circumstantial, but no textual, evidence for his claim that Humboldt owes this insight to the Dutch school of classical philology headed by Tiberius Hemsterhuis (1685–1766). Whatever its source, it is safe to say that Humboldt used the term in the traditional sense still found in Scherer (1868) in several places (see below). It deals with the acquisition, workings, or use of a given language, i.e., a 'synchronic' concern, but not with the concept of analogy as an explanatory tool in the explanation of linguistic change, the subject that concerns us here.

17. The second entry of "Analogies" refers to those "between linguistic and certain physical sciences, 46–7, 52", i.e, what we would probably call, more appropriately, 'parallels' or 'similarities' between different fields of study. For Whitney, at least metaphorically, geology played such analogical function (see Koerner 1992, for details).

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strata of language" are "always threatening to rise to the surface, and now and then succeeding in forcing their way up, and compelling recognition and acceptance from even the best authorities" (p. 28). On page 82, Whitney adds, after having added *goed* (for *went*) and *seed* (for *saw*) to the list of those "irregular" forms:

> Prevailing usage has in our language [= English] already ratified a host of such blunders; a large portion of the ancient Germanic verbs, formerly inflected after the analogy of *sing, come, bind, give* and their like, we now conjugate "regularly." […] *bake, creep, fold, leap, laugh, smoke, starve, wade, wield*.

Finally, on page 85 Whitney provides many more such examples from English, adding: "The alterations find support in one of the analogies of the language", giving no hint that he regards it as a principle of historical investigation; in other words, Whitney does not leave the safe ground of observation.[18] In his *Life and Growth of Language* of 1875 subtitled "An outline of linguistic science", the reader would not find much further instruction concerning the idea of 'analogy'. In fact, Whitney supplies a few more examples, more from language acquisition than language history actually, noting that "the principle is often appealed to in explaining the processes of earlier language-making". He concludes his observations with the seemingly powerful statement: "The force of analogy is, in fact, one of the most potent in all language-history", which he weakens, in my view, by continuing "as it makes whole classes of forms, so it has power to change their limits" (p. 75). No example is provided for this statement, and one wonders how original this observation is in the light of what Schleicher noted in 1860 (see Section 6 below).

In his important biography of Whitney, Stephen Alter (2005: 221) cites the first part of this quotation, arguing that "[t]his understanding of analogy as a historically normal kind of language change was an outworking of Whitney's uniformitarian principle", which may well be an admissible interpretation in the context of what Whitney said generally in his 1875 book. His claim, however, that August Leskien took up "the analogy principle" from Whitney when he was translating *Life and Growth of Language* (Whitney 1876) and that this constitutes a "fact" which "reinforces the case for Whitneyan influence" (p. 222) may again be true in general terms. However, where the principle of analogy in particular is concerned, Leskien as a former student of Schleicher's would not have had to wait fifteen additional years for this insight. Besides, Brugmann & Osthoff (1967 [1878]: 204), contrary

18. Before leaving this book, it is worth mentioning in the present discussion that Whitney refers to Schleicher as one of the authors whose work he "had constantly upon [his] table" (Preface, p. vii) referring in the footnote expressly to his *Compendium*, but stating that "other writings […] are referred to by name in the marginal notes". Thus, we find Whitney making mention of Schleicher's *Die Deutsche Sprache* of 1860 (1867: 364*).

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to Alter's suggestion (p. 222), did not credit Leskien (nor Whitney, for that matter) for the analogy idea, but only the latter for the stricter development of the sound law principle (see also Alter, p. 226), something which the quotation made earlier from Leskien's introduction to his own 1876 book would justify.

What Brugmann, Delbrück, and others said in response to the 1894 invitation by the American Philological Association to pronounce themselves on Whitney as a 'comparative philologist' following his recent death, as Alter, Christy and others have tended to do, should however be taken with more than a grain of salt. By that time, the Neogramarians were at the height of their power and could be gracious in giving credit to their deceased predecessors, though Schleicher (and Curtius for that matter) were not among the beneficiaries of this *largesse d'esprit*.

4.2 Analogy in Baudouin de Courtenay (1868)

If we are to believe Adamska-Sałaciak (1996: 67), for instance, Jan Baudouin de Courtenay (1845–1929), who had published a monograph-length article "Einige Fälle der Wirkung der Analogie in der polnischen Deklination [A number of cases of the effect of analogy in the declension system of Polish]" in Germany in 1868, which "is now commonly referred to as one of the pioneering studies of analogy", although it had been "hardly noticed" at the time, all the credit being given to Scherer's book of the same year. One may wonder why credit was not given to Baudouin's work by Brugmann & Osthoff (1878). Could it have been the fact that he had been a former student of Schleicher's? On the other hand, no other than Leskien saw to it that Baudouin was granted a doctorate from the University of Leipzig in 1870 on the basis of essentially this 70-page study.

Baudouin's article is listed in Anttila & Brewer (1977: 4) and mentioned in their "Preface" (p. xii), but it does not seem to be discussed in books dedicated to Baudouin de Courtenay's life and work (Baudouin 1972, Mugdan 1984), nor are excerpts being included in the anthologies of his writings (Baudouin 1972, 1984), although Baudouin himself appears to have regarded it as a major work of his, publishing it in Polish translation in a 464-page collection of his papers (1904: 176–248). The entry on 'analogy' in the "Topical Index" of Baudouin (1972: 401) refers to much later writings of his. Be this as it may, Baudouin's analogy article, done probably at the behest of Schleicher (although Baudouin in the preface to the Polish translation reports [1904: 176] that it was he who had taken the initiative)[19] and was published in a journal edited by Schleicher and Adalbert Kuhn (1812–1881).

19. In an 1897 autobiography Baudouin goes even so far as to claim that he had undertaken his study 'in protest' against Schleicher's 'morbid formalism […] and complete absence of understanding of new linguistic phenomena' (English translation quoted from Williams 1993: 29). Anyone familiar with Schleicher and his work would take such assertion as preposterous.

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If we ignore the wordy preface to the original 1868 text, which Schleicher had replaced by a much shorter one (on which see Koerner 2006, for details), Baudouin, in typical 19th-century fashion, goes *medias in res* in his investigation of the operation of analogy in the declension system of Polish, and offers a rather discursive discussion of 'analogy' only right at the end of his article (p. 88):[20]

> Schließlich möge noch eine allgemeine bemerkung über die analogie platz finden. Aus meiner ganzen darstellung erhellt:
>
> 1. daß jeder casus von jedem substantivum in potentia alle endungen hat, die in der sprache leben, um die diesem casus entsprechenden beziehungen auszudrücken. Überwiegt nun eine gewisse analogie, gleich tritt an die stelle der einen endung eine andere, früher diesem casus gar nicht zukommende. – Größere aussicht sich zu erhalten haben hierbei die an anzahl überwiegenden formen, formen die sich häufiger in der sprache wiederholen, die stets gebraucht wurden, deren analogie überwiegend ist. Denn die wiederholung der eindrücke macht diese stärker und fester haftend. Es kann so geschehen, daß eine gewisse analogie die erhaltung seltnerer formen begünstigt und selbst neue kategorien für sie schafft. – Bei alledem strebt das volk nach vereinfachung der sprachlichen formen, deren nothwendigkeit es nicht mehr fühlt.
> 2. Nur dann ist die wirkung der analogie ermöglicht, wenn es gewisse berührungspuncte und übergänge von einer wörterkategorie zur anderen giebt.
>
> Zuletzt fragt es sich, wie sollen wir uns die wirkung dieser sprachlichen kraft, der analogie denken? Es versteht sich, nur mechanisch, nur nach den einzelnen entwickelungsmomenten. Man soll also eine ganze reihe der allmählich wirkenden einflüsse annehmen, die das sprachgefühl der einzelnen die gegebene sprache redenden individuen stufenweise verändern (nicht aber aufheben) und es in dieser oder anderer richtung sich entwickeln und sich neue anschauungen bilden lassen. – Dies aber streng, genau und erschöpfend zu bestimmen, wird niemals der wissenschaft gelingen.
>
> [Finally, I'd like to add a general remark about analogy.
> From my entire presentation it becomes clear
>
> 1. that potentially each case of every noun has all the endings that live in the language in order to express its corresponding relationships. If a certain analogy carries the day, another ending immediately takes its place which originally

20. It is true, however, that early on in his 70-page article Baudouin offers some general discussion of especially the psychological reasons for analogical formations (see Adamska-Sałaciak [1996: 68] for an English translation of a paragraph from the 1904 Polish translation of the 1868 text). But I doubt it provides much more than another example of the Baudouin's long-winded manner of argumentation, which a reviewer of a much later work by him characterized as carrying on the argument "with a profitless vagueness", not as "an observer but […] a *doctrinaire* who formulates his conclusions first and then looks round for facts to support them", and a person who "ends […] like many another revolutionist, by setting up things which are hard to distinguish from those which it was his mission to destroy" (Lloyd 1896: 617).

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did not have this function. – Here the forms which are much more frequent in occurrence, i.e., those that are repeated more often in the language, those that are regularly used, [and] whose analogy prevails, have the better chance to survive. Since the repetition of the impressions renders them stronger and more memorable. In this manner, it can happen that a certain analogy favours the retention of more rare forms and even creates new categories for them. – At the same time people strive toward simplification of linguistic forms whose necessity they do no longer feel.

2. The operation of analogy is rendered possible only if there exist certain points of contact and transition from a one-word category to another.

To conclude, one may ask how should we imagine the effect of this linguistic power, analogy? It goes without saying that one proceeds mechanically only, exclusively according to individual moments in the development. So one should assume an entire range of gradually operating influences which changes (but does not cancel) the feeling for the language on the part of individual speakers and which develops in this or that direction and permits the formation of new insights. – However, science will never succeed in defining it rigorously, exactly, and exhaustively.]

To distill from these deliberations a few generalizations about the workings of analogy and its psychological underpinnings (as I believe Baudouin intended) must have caused well-meaning interpreters of Baudouin de Courtenay's article some difficulties. Mugdan (1984: 111), who briefly refers to a few examples of analogy from this 1868 article, notes with surprise Baudouin's assertion that analogy is more important than sound laws in language change, and otherwise cites (p. 112) parts of the introductory remarks that Schleicher had struck from the original submission,[21] as noted by Mugdan early on in his biography (p. 12; see also Williams 1993: 98).

5. The treatment of 'analogy' in Scherer (1868)

In the present context, it pays to revisit Scherer's 492-page book, beginning with the rather dense index ("Register" [476–492]). There, one looks in vain for an entry on 'Analogie'. Instead, one discovers (p. 480) the expression 'falsche Analogie' tucked away in the entry beginning with '*Formübertragung*' ("form transfer"), which continues "Uniformirung, falsche Analogie, Umdeutung, Missverständniss, falsche Folgerung" ("uniformization, false analogy, reinterpretation, wrong conclusion").

21. Reading the original introduction without any prejudice, it is difficult to agree with Williams' (1993: 29) assessment that it represented "Baudouin's theoretical preface". At least, I couldn't detect much evidence for this when studying an English translation of it that Professor Arleta Adamska-Sałaciak (Poznań) kindly supplied me with.

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To be sure, Scherer is not to blame for the apparent disregard of Schleicher's work in Brugmann (and Osthoff)'s famous "Vorwort" of 1878.[22] In Scherer's book of 1868, which they had selected for special praise, Schleicher is referred to well over 60 times,[23] More than half of these references are to Schleicher's *Compendium*, apparently (cf. Scherer, pp. 96, 451) in its second edition of 1866. The bulk of the other references are to Schleicher's 1852 Morphology of Church Slavic (see, e.g., pp. 202, 246, 277, 370, 373, 408) and to his 1856 Lithuanian Grammar (see pp. 101, l48, 208, 241, 257, 270, 273–274, etc.). Indeed, like most of the other sources used in his book, Schleicher's writings are referred to for data and in support of his argument; rarely is Schleicher being disagreed with or criticized (e.g., 177, 451). What may be interesting in the present context (see below) is that, as far as I have been able to determine, Scherer mentions Schleicher's 1860 book *Die Deutsche Sprache* only once, and this in a footnote (p. 314). Given that Scherer's book was supposed to be devoted to the historical development of German, even though it is true that Scherer, much more than Schleicher who wanted to write a book for the educated classes (Schleicher 1860: v), did his detailed investigations with reference to many examples from Indo-European, and so it could be expected that he was familiar with its contents. That Scherer should not use the term 'analogy' as specifically as did Schleicher six years before him may be surprising. Still, the word 'analogy' comes up in Scherer's book, and so its uses deserve to be looked at more closely.

In fact, Scherer uses 'Analogie' not infrequently in a general every-day sense meaning parallel or similar development, much less often in a technical sense. This may explain why we find 'Analogie' in the text (e.g., pp. 205, 206, 208, 312) but not referred to in the otherwise detailed index. Instead, we come across observations such as "interessante Analogien" between Slavic and Germanic in the treatment of the 'Verbum substantivum' (p. 208), "koptische und andere Analogien" of a typological nature (p. 312), or "eine germ[anische] Analogie" to the treatment of the

22. Christy (1983) does not serve our purposes well when he quotes almost exclusively from the second, much revised edition of Scherer's 1868 book (see pp. 71, 72, 74), by which time Osthoff & Brugmann's "Vorwort" dated June 1878 in all likelihood had already gone to press. Besides, unlike the 1868 book, the 1878 edition had been severely criticized by Hermann Paul among others as having fallen behind recent developments in the field (Paul 1879). In his encomium of Scherer, Jankowsky (1995: xxiv) prefers to quote Paul's positive remark on the effect of the first edition (Paul 1879: 307–308) only, acknowledging however that the review of the 1878 edition was in fact "devastating".

23. In locating these references, I must be grateful to Kurt Jankowsky who provided the 1995 reprint of the work, among others, with a carefully elaborated list of works cited by Scherer (xxxiii–xlix) as well as an "Index of biographical names" (li–lv).

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numeral '10' in Latvian (p. 451).[24] In most instances where Scherer refers to the idea of analogy for explanatory purposes of particular developments, he uses the term 'Formübertragung' ("transfer of a form"), which describes the phenomenon quite well. Thus, he speaks of the 'Macht der Formübertragung' ["power of form transfer"] expanding his view as follows (p. 463):

> Dass sich der Abl. Sing. der *u*-Stämme im Osk. und Umbr. nach Analogie der *i*-Stämme richtet mit völliger Einbusse des thematischen *u*, erinnert schon Schleicher.
>
> [Already Schleicher reminds [us] that the ablative singular of the *u*-stems in Oscan and Umbrian follows those of the *i*-stems by analogy at the cost of the entire loss of the thematic *u*.]

Scherer does not explicitly refer to Schleicher's definition of analogy as a technical term in *Die Deutsche Sprache* or any other of Schleicher's writings; on the same page he refers twice to the latter's *Compendium* for data (this time from Lithuanian) or a linguistic observation (the formation of double forms for the distinction of grammatical functions). Franz Bopp (1791-1867) refers in his *Vergleichenden Grammatik* of 1833 frequently of 'Analogie' (s. Volume I, pp. 29, 98, and more frequently), for instance (p. 182):

> Wahrscheinlicher ist es, daß die *a*-Stämme nur fester an der einmal angenommenen Endung hafteten, weil sie bei weitem die zahlreichsten sind, und somit der *Zerstörung der Zeit* durch eine *größere Macht der Analogie* stärkeren Widerstand leisten konnten [italics mine: Koerner].

So, when Schleicher (1860: 60) speaks of 'Macht der Analogie', he is not original, and Scherer's 'Macht der Formübertragung' does not appear to be entirely co-incidental. Interestingly, as far as I can see, Scherer defined his term only toward the very end of his book, in his "postscripts" (Nachträge), in the present instance to a previous statement made on page 177:

> Es wäre sehr verdienstlich, wenn Jemand solches Aufdrängen, solche Form-übertragung oder Wirkung der "falschen Analogie" einmal im allgemeinsten Zusammenhange erörterte
>
> [It would be meritorious if someone was to discuss such persistence, such form transfer or the operation of "false analogy" in the most general interconnection]

---

24. It is true, however, that the term analogy in the linguistic sense pops up here and there in Scherer's book, e.g., when speaking of case syncretism in Germanic, he offers an explanatory guess in parentheses is by saying "vielleicht nach Analogie [perhaps by analogy]" to another inflected form (p. 448).

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Scherer then offers the following generalization (p. 473):

> Formübertragung. Als eine Regel die für viele Fälle ausreicht, lässt sich vorläufig hinstellen: Wenn eine Form *a* es über eine Form *b* davonträgt und sie verdrängt, so haben *a* und *b* ein Element *a* gemeinsam, das sie von ähnlichen und zunächst verwandten Formen unterscheidet; die thatsächliche Uebermacht von *a* aber beruht auf der Häufigkeit des Gebrauches,
>
> [Form transfer. As a rule which suffices for many cases we can propose provisionally: If a form *a* wins over a form *b* and drives it out, *a* and *b* share an element *a* that distinguishes it from similar and previously related forms; the factual superiority rests upon the frequency of its use.]

It is hard to see whether this "definition" is in any way superior to what Schleicher had noted eight years earlier on the operation of analogy in the history of Indo-European languages.

6. The place of 'analogy' in Schleicher (1860)

August Schleicher's *Die Deutsche Sprache* of 1860 was an attempt to write a popular book,[25] not one addressing the experts in historical linguistics as Scherer did in his *Zur Geschichte der deutschen Sprache* of 1868. Although it went through five editions until 1888, it is more likely that it was instead used as a textbook for students of the German language. Since it had a second and third edition in 1869 and 1874, respectively, the first being one in which his former student Johannes Schmidt (1843–1901) introduced corrections on the basis of handwritten notes that Schleicher had made in his *Handexemplar*, all others being reprints of the latter, there can be no doubt that all Leipzigers had easy access to a copy of *Die Deutsche Sprache*,[26] whether they mentioned it in their writings or not. This would include Osthoff and Brugmann, although the latter's main subjects of teaching were the classical languages, in particular Greek, not the Germanic languages as in the case of Osthoff – apart from Indo-European linguistics more generally, of course.

It is in the first edition of Schleicher's *Die Deutsche Sprache* already that we find, when checking the general index, the term "Analogie" and two pages referring to

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25. He expressed his desire ("Vorwort", p. v) to write "ein für jeden Gebildeten unserer Nation zugänglich und brauchbares Werk ['an accessible and useful work for every educated person in Germany']".

26. Schleicher himself used 'Deutsche' with a capital 'D' in referring to his book, and so it should stand. I'd not be surprised if the creators of the journal *Die Deutsche Sprache* in 1973 had Schleicher's book in mind. Blaček (2007: 108) follows Schleicher's choice correctly.

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its discussion.[27] (There is no mention of 'false analogy' or the like.) Since one rarely finds a single reference to these passages in the linguistic literature to the present day,[28] it seems important to quote the first treatment of the term fully. When discussing morphological changes in a variety of languages, Sanskrit, Greek, Latin, and Old High German among them, Schleicher (1860: 60) notes:

> Allein schon in älteren Sprachperioden, […], beginnt sich eine Macht geltend zu machen und feindlich auf die Mannigfaltigkeit der Formen zu wirken und sie mehr und mehr nur auf das allernothwendigste zu beschränken. Dies ist die oben schon erwähnte Anähnlichung namentlich der weniger häufig in der Sprache gebrauchten, in ihrer Besonderheit aber wohl gerechtfertigten Formen, an andere, vor allem an vielfach gebrauchte und so stark ins sprachliche Gefühl sich einprägende, die Analogie.[29] Das Streben nach bequemer Uniformirung, nach Behandlung möglichst vieler Worte auf einerlei Art und das immer mehr ersterbende Gefühl für die Bedeutung und den Ursprung des Besonderen hat zur Folge, daß spätere Sprachen weniger grammatische Formen besitzen als ursprünglichere, daß der Bau der Sprache mit der Zeit sich immer mehr vereinfacht.
>
> [Even in the older periods of language, […], a force begins to make itself felt which affects the diversity of forms adversely and which reduces it more and more to the bare minimum. This is the already mentioned process, namely, analogy of rendering forms, in particular those less used in the language, which would otherwise be

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27. I believe that I can safely dispense with going through Schleicher's *Compendium* or any other of his lifetime writings as Schleicher himself, in the first footnote to the "Einleitung" of his *Compendium* explicitly refers his readers to his 1860 book for more detailed exposition of his general views on 'sprachwissenschaft' or 'glottik' (Schleicher 1861: 1)

28. It is interesting – and indeed typical for linguistic historiography – that Schleicher's name does not even show up once in Anttila & Brewer's *Analogy: A basic bibliography* (1977), which lists some 350 items from 1816 to 1977. Nor does he appear in Best (1973), where Scherer (1868), however, is referred to (p. 113). In Itkonen (2005: 188) Schleicher is referred only for his acceptance of "the analogy between evolutionary biology and diachronic linguistics", a view which according to Itkonen "has again become fashionable" in works like Haspelmath (1999). In Wanner's (2006) *The Power of Analogy* Schleicher's name does not appear even once. It was therefore a pleasant surprise to discover that Hock (2003), in his discussion of the use of analogy since the Neogrammarians, actually provided an English translation of exactly the same passage, albeit tucked away in endnote 8 on page 458, after having made a passing reference to a 1983 article by myself in which "a very similar view [to Brugmann & Osthoff (1878)] can be found in the work of Schleicher" (p. 444). No further comment on Schleicher's is found in this otherwise very informative article. The item Hock referred to quotes the original passage (Koerner 1983 [1982]: lii*-liii*).

29. 'Spread print' was regularly used for emphasis in 19th-century texts; this alone makes it clear that Schleicher used *Analogie* as a technical term. Nothing of this sort is found in Scherer (1868) for instance – nor in the writings of Baudouin de Courtenay and Whitney referred to earlier in this paper.

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> justified in their special character, more similar to those frequently used ones which etch themselves so sharply on our feeling for the language. This drive toward more convenient uniformization, toward treatment of as many words as possible in one and the same fashion and the continuing weakening feeling for the importance of the meaning and the origin of the particular has the consequence that later languages have fewer grammatical forms than the more original ones, [and] that the structure of the language becomes more simple with time.]

Schleicher illustrates this observation in the pages following this long quotation, with examples especially from Germanic (but also from Sanskrit and Latin), concluding (p. 61):[30]

> Wir werden im Deutschen so viele Fälle von späterer Analogie finden, dass ich füglich unterlassen kann, hier weitere Beispiele vorzuführen.
>
> [We will encounter in German so many instances of later analogical formations that I can forgo supplying further examples at this point.]

Later on, when discussing changes in the verb paradigms from Middle High German to Modern German, taking as an example MHG *ich greif, du griffe, er greif, wir griffen* etc. changing to *ich griff, du griffst, er griff, wir griffen*, Schleicher returns to the idea of "das immer stärkere Walten der Analogie [the ever increasing power of analogy]" (p. 168). In short, it should be obvious that for Schleicher analogy was a principal regularizing force in language history.

In her encyclopedic entry on Schleicher Theodora Bynon (1992: 381) summarized what I have long maintained to be correct:

> Although he was never in fact mentioned by name, Schleicher was the main target of Neogrammarian criticism in their manifesto (Osthoff & Brugmann 1878). But the writers of the manifesto failed to mention the fact that the methodological foundation of their discipline had in fact been laid by Schleicher.

Although she did not mention the analogy principle expressly, I believe that I have shown sufficiently well that Brugmann & Osthoff (1878) had no need to refer to Scherer (and anyone else) on this point, had it not been for 'strategic' rather than truly scholarly reasons.

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30. On this page alone, the term 'Analogie' comes up four times. Another occurrence can be found on page 62 in conjunction with Schleicher's observation that we should assume that analogy, the tendency of economy – when he speaks (p. 63) of "das Bedürfniß, die Thätigkeit der Organe auf ein geringeres Maß zu bringen [the need to reduce the activity of the organs [of speech] to a lower degree]" – a remark in which (as in many other places of Schleicher's work) Christy (1983) could have found an instance of a 'uniformitarianist' argument, well before Whitney or Scherer hit upon it. However, one looks in vain for a reference to Schleicher's writings other than the popular essays of 1863 (cited after the 1873 edition, which is just a reprint of the first edition) and 1865.

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7. Concluding remarks

Many years ago, John E. Joseph of Edinburgh admonished me in personal correspondence to finally admit that there was a 'Chomskyan Revolution' in linguistics (cf. also Joseph 1995). Indeed, it does not depend on my personal opinion whether this wasn't so or whether it really occurred in the manner in which it has often been depicted. The fact remains that possibly a great number, if not the majority of linguists from the late 1960s or early 1970s onwards *felt* that there was a change of significant proportions occurring in their field. How this 'revolution' was brought about may be another story, and I doubt that many practitioners of the craft really care. Likewise, we'd have to concede that there was a 'Neogrammarian Revolution' in comparative-historical Indo-European linguistics taking place. Certainly not overnight, but by the end of the 19th century, soon after the 'Battle of Monographs' (Jankowsky) of the mid-1880s had been fought out and the last member of the 'Old Guard' had disappeared, almost everyone in the field had become busy with 'mopping-up operations' (Kuhn), i.e., applying the same methodological principles to their particular area of specialization. In the 20th century, great linguists like Meillet and Bloomfield still felt that way and acted accordingly, and this to the considerable benefit of the discipline. No other scholar than William Labov recognized the importance of the neogrammarian 'regularity principle' (Labov 1981, 1982), and in historical linguistics every serious researcher has become a follower of the 'junggrammatische Richtung' that Brugmann and his colleagues advocated, and this with little consciousness of how this had come about. The role of Schleicher in all this has become irrelevant to them; still, it must remain the task of the historian of the field to try to give an account of what really has happened. For the late Indo-Europeanist Henry M. Hoenigswald (1915–2003) the matter was fairly obvious more than thirty years ago:

> Until more is known we shall say that it is in the [eighteen] sixties, and with August Schleicher, that the great change occurred. (Hoenigswald 1974: 351)

I for one at least have not seen much evidence for requiring a revision of this view.

Acknowledgements

I'd like to dedicate this paper to the memory of Winfred P. Lehmann (1916–2007) of the University of Texas at Austin, who had always shown an active interest in and support of work in the history of linguistics. Thanks are due to the late J. Peter Maher (Chicago) for several stylistic improvements; the remaining shortcomings are my own responsibility. An earlier version of this chapter appeared in *Lingua Posnaniensis* 50.7–25 (2008).

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PART II

Studies concerning the work of individual scholars

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CHAPTER 5

Baudouin de Courtenay's relationship with Schleicher

1. Introductory remarks

In the annals of linguistic science the Balto-Slavist and Indo-Europeanist August Schleicher (1821–1868) has not been given his full due (see however Dietze 1966). He died much too early to defend himself, when attacks on his legacy were mounted by the *Junggrammatiker* and their associates. Even among his most distinguished students – August Leskien (1840–1916), Hugo Schuchardt (1842–1927), Johannes Schmidt (1843–1901), and Jan Baudouin de Courtenay (1845–1929)[1] – there came little support during the heyday of the so-called 'Neogrammarian Revolution', when it appeared that everything that Schleicher had said and done was little else than working out Bopp's model which by then was regarded as at best an earlier stage of linguistic science.

Looking back at more recent history, one feels reminded of what happened to the great work of Leonard Bloomfield (1887–1949) in the 1960s and thereafter, when Chomsky and his associates and followers consigned it to the dustbin of 'pre-scientific linguistics'.

In the historiography of linguistics the argument concerning 'influence' has been made quite frequently. Apart from the fact that the term itself is rarely defined or made more precise, it has been invoked on many occasions and for a variety of reasons. Possibly the most frequent one has been to establish chronology, historical succession, or some sort of connection between one or the other author or school of thought. Needless to say that it is the historian's task to establish that these

1. There were of course many other scholars, a considerable number of them in the field of Slavic philology, notably from the time of his professorship at the University of Prague (1851–1857), such as Alois Vaníček (1825–1883), his first Czech informant from the time of his work as a journalist in the city following the 1848 Revolution, Eduard Novotny (1833–1876), Jan Kvícala (1834–1908), the Slovak Martin Hattala (1821–1903), and many others were his students (see Syllaba 1905: 32–37, for details). During his professorship in Jena, Schleicher also counted, among foreign students, not only Poles but also Russians and Bulgarians among his students. Mention should be made of Vasil Dimitrov Stojanov (1839–1910), later on a co-founder of the Bulgarian Academy for Science, who also served him as an informant on Bulgarian (Syllaba 1995: 62).

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hypothesized or assumed sequences of transmission are real, i.e., can be shown in various ways to be true or at least very likely.

There have been other motives for invoking 'influences', and some may be less insidious than others. I'd call those 'insidious' that are not motivated by an honest effort to establish what in Leopold von Ranke's (1795–1886) terms was characterized as "wie es eigentlich gewesen", i.e., how certain events can be shown to have occurred in all likelihood, but by the intent on the part of someone to demonstrate that this or that scholar was much less original – if original at all! – than has been assumed by many, and that in fact a host of others had anticipated his ideas, not only the terms and concepts, but even important parts of the argument. In this connection the supposedly 'structuralist' work of Ferdinand de Saussure has been the subject of many attempts to "prove" that he was little else than the mopper-upper of the ideas that had already been expressed, at times even better, by a host of (alleged) precursors, not only in linguistics (e.g., Humboldt, Georg von der Gabelentz), but

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also in philosophy (e.g., Hegel), psychology (e.g., Hippolyte Taine), sociology (e.g., Durkheim), possibly by scientists in the field of mathematics (e.g., Hilbert) and political economy (e.g., Pareto).

While some of these alleged precursors have become accepted by various textbook writers as evident – there is no doubt that certain (usually superficial) similarities between ideas can be found; others have not become matters of scholarly – or, not so scholarly – controversy. I cannot deny the fact that I have myself been involved in several of these hot debates, and, as a result, I have been branded as having engaged in "cum ira et studio" disputes (see Albrecht 2000: 25). There is probably no denying that I have lacked in diplomacy on occasion, but such accusations, I believe, typically have come from those who, if they were honest to themselves, had lost the scholarly argument. There is a difference between writing in the indicative and in no uncertain terms and assuming the moral high road when their cherished preconceived ideas are being challenged and making an honest effort to

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show, through external and especially internal, textual evidence, that a particular author had benefited from the writings of another scholar.

When in 1969 I first delved into questions of the sources of Ferdinand the Saussure's linguistic inspiration, I was motivated by an effort to understand his kind of theories which, for someone like myself with a previous formal training in historical philology and literature (1962–1968), Saussure's theories were not something easy to grasp. My general approach to the *Cours* was therefore a genetic one; my philosophy was, as it is today, that, as Schleicher had stated in his 1863 "Darwinian" tract (p. 10), if we do not understand how something has become what it is now, we don't really understand it.

Unlike others, I had not come to Saussure with a baggage of certain traditions of linguistic thinking (apart from those that concerned historical-comparative Indo-European), but essentially with a certain curiosity and a desire to understand his special views on how to analyze language. Although I had begun my formal exposure to linguistics in the fall of 1968, at the height of Chomskyism and had to enroll in courses of transformational grammar and generative phonology among others, I was lucky enough to hear about Bloomfield and Sapir, the Prague School, Hjelmslev, J. R. Firth, and of course Saussure, who was frequently mentioned as the 'father of modern linguistics'. And so it was natural for me to start with Saussure, but not what I could read in the textbooks of Ivić, Leroy, Malmberg or Robins. I asked myself which subsequent school of linguistic thought had been influenced by Saussure's ideas, but in the opposite direction: I wanted to understand how Saussure had come to his particular positions.

In doing these empirical investigations, I came to the conclusion that Saussure was indeed influenced by the work of other linguists, most of which he did not fail to mention in his Geneva lectures of 1907–1911, on which the bulk of the posthumous *Cours de linguistique générale* was based. I refer to Whitney, Hermann Paul, Gaston Paris, for instance. Others, notably Baudouin de Courtenay and Kruszewski,[2] have been known to Saussure as well, as Godel (1957: 51) was the first to point out. By contrast, various other names that had been declared as precursors in certain quarters, like Durkheim, Gabelentz, Humboldt and others, were, interestingly, not referred to in Saussure's lectures, not even in the various autographs, including those discovered in 1996 only (see Saussure 2002: 15–88, 91–97, 125–135, 277, 285–294).

2. These scholars were all mentioned in positive terms in Saussure's first 1891 lecture as incumbent of the chair of Comparative Linguistics at the University of Geneva (see Saussure 2002: 147)

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2. The Schleicher–Baudouin connection

In this short chapter, I won't return to old battles fought about Saussure and his sources, though it bears to reiterate that I am interested first and foremost in questions of influence within linguistics rather than speculate about extra-linguistic influences which, more often than not, are conceivably matters of the general intellectual climate of a given period and are less easy to specify.

Before I enter into the subject matter proper, however, I should stress that I do not think that Jan Baudouin de Courtenay (1845–1929) was something like a pupil of August Schleicher (1821–1868) in the manner we have experienced with many famous students of Noam Chomsky who subsequently acted almost like clones and felt that it was their duty not only to defend their master's views but, if they happened to know a language other than English, to prove his theories right. There can be no doubt that Baudouin, like other major scholars, was an independent mind in many ways, and that it would be quite wrong to see him as a close follower of Schleicher of whom he was a student for less than a full academic year. Formally, Baudouin had enrolled at the University of Jena for the winter semester 1867–1868 only, and he was back in Warsaw by the late fall of 1868 after a few months' stint at the University of Berlin to study Sanskrit with Albrecht Weber (1825–1901) and quite likely listening to lectures by Heymann Steinthal (1823–1899) as his many references to his ideas would suggest. Compare Baudouin's well-known contribution on linguistics to a major Polish encyclopedia:

> The originally metaphysical character of this branch of science has receded more and more behind the psychological treatment of language (Steinthal, Lazarus, and others), which today receives more and more adherents and which will gradually, in agreement with the psychic basis of the human language, become the sole trend in linguistics. (BdC 1903: 284; English transl. in Koerner 1973: 145n.8)

As we know, the psychological approach did not become the 'sole trend' in 20th-century linguistics, and Baudouin's psychologism (for instance in BdC 1895b) was criticized toward the end of his life by members of the Prague School and others. However, his 'mentalism' in matters linguistic was evident in his early work already – hence part of his opposition to Schleicher's views in certain regards – and remained with him throughout his life.

Still, I would like to show that Baudouin – never mind his reiterated claim that in matters linguistic he was essentially self-taught (1897a: 22; Mugdan 1984: 11) – that he owed more to Schleicher than either he or his historiographers ever acknowledged. Given limitations of time and space, I shall in what follows only sketch ways in which more substantial investigations could demonstrate beyond the shadow of

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doubt Schleicher's importance for the development of the young Baudouin, as in fact has been pointed out much earlier by Häusler (1968: 33), who stated that "Die Kritik an grundlegenden Thesen Schleichers half den jungen polnischen Gelehrten, seine eigenen sprachphilosophischen Standpunkt zu bestimmen [The critique of Schleicher's basic assumptions assisted the young Polish scholar to determine his own philosophical position in linguistics]".

2.1 Baudouin de Courtenay's early publications

In 1851, the Berlin *Gymnasialprofessor* Adalbert Kuhn (1812–1881), with initial participation of the Classical philologist and Sanskrit scholar Theodor Aufrecht (1821–1907), had launched the *Zeitschrift für vergleichende Sprachforschung auf dem Gebiete des Deutschen, Griechischen und Lateinischen* (Berlin, 1852–present). In 1856 preparations were made, with Schleicher taking the leading role, to add *Beiträge zur vergleichenden Sprachforschung auf dem Gebiete der arischen, celtischen und slavischen Sprachen* to complement the *Zeitschrift*. The *Beiträge* appeared in altogether eight volumes between 1857 and 1876, at times one volume stretching over more than two years. They were subsequently merged with the main journal, which since then bore the characterization "… *auf dem Gebiete der indogermanischen Sprachen*" (until in 1988, beginning with volume 101, the journal's name was changed to the rather bland name *Historische Sprachwissenschaft*). It was obvious that Schleicher covered the areas of Indo-Iranian, Celtic and Slavic and thus was essentially directing this journal. I am so explicit on this because it was the *Beiträge* to which the barely 25-year-old Baudouin de Courtenay contributed altogether twelve (!) items between less than a page in length (BdC 1869h, i) and up to seventy pages (BdC 1868a). Indeed, the lastmentioned item, "Einige Fälle der Wirkung der Analogie in der polnischen Deklination", constituted the first regular scholarly article in Baudouin's 60-year scholarly career with some 600 publications to his credit, several of them still today being regarded as trailblazing (e.g., BdC 1870). I am pointing out this fact too because Schleicher's name is mentioned by Baudouin only once as the co-editor of *Beiträge*,[3] and then no more, thus, I feel, giving the – misleading – impression that his students, August Leskien (1840–1916) and Johannes Schmidt (1843–1901), carried the burden of the editing in these areas not covered by Kuhn. It is true that Schleicher died rather suddenly (and prematurely at age 47)

3. See BdC (1974 [1904]: 328 [176]), where the volume VI of *Beiträge* are said to be edited by Adalbert Kuhn "unter mitwirkung von A. Leskien und J. Schmidt", a reference repeated in several other 1868 items republished in Polish translation (see ibid., pp. 401 [249], 409 [256]). The volume as a whole in question eventually appeared with an 1870 imprint.

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on 6 December 1868, but he surely remained in charge until his last days. There is at least indirect evidence for this from Baudouin himself. For instance, in his 1904 collection of papers, he refers to and quotes from several letters he had received from Schleicher during 1868 (BdC 1904: 255, n.*; 261, notes * and ***).[4]

This study on the effects of analogy in the declension system of Polish was written – there is no doubt about this in my mind – at the instigation of Schleicher, when Baudouin was student of his at Jena (cf. also Dietze 1966: 71). Other pieces followed in the same year, most of them under Schleicher's tutelage as may be gathered from the comments Baudouin made when he republished several of them in Polish translation in 1904 (BdC 1868a, 1869b, e, f, g, i), others during the summer month of 1868, when he resided in Berlin. Much has been made of the fact that Schleicher had struck the introductory remarks that Baudouin prefaced his study on analogical formation in Polish with (see, e.g., Mugdan 1984: 112; Adamska-Sałaciak 1996: 67). Baudouin himself appears to have been still angry about Schleicher's 'Eingriff' when translating the article into Polish in 1902 (see BdC 1904: 176; cf. also BdC in 2005 [1888–1889]: 19); he cites a letter from Johannes Schmidt in which he was dissuaded at the time from responding to this move in the *Beiträge*, in part because it 'would offend the memory of recently deceased Schleicher'.[5] Still, while reinstating the introductory remarks in the Polish translation, Baudouin was fair enough to print the three short paragraphs by which Schleicher had replaced them (BdC 1904: 178n.1). Since they are not cited in either Mugdan (1984: 112) or Adamska-Sałaciak (1996: 67), who both take issue with Schleicher's conduct, it may be worth quoting them in full here:

> Wenn man die in der sprache wirklich vorkommenden worte nimmt wie sie sind und wie sie vom sprechenden empfunden werden, so kann man keine vocalischen stämme in der polnischen declination annehmen. Vocalische stämme werden bei den polnischen nomina nicht gefühlt.
>
> Es gibt gegenwärtig in der polnischen declination nur consonantische stämme, wenigstens werden nur solche im sprachgefühle empfunden.
>
> Uebrigens sieht man leicht, dass sich im polnischen die theilung der declination nach den stämmen nicht durchführen lässt. (BdC 1868a: 19)

4. In note *** on p. 260, Baudouin also quotes from a letter by Johannes Schmidt of 1869. In all instances, these contain comments on linguistic matters regarding Polish or Slavic.

5. Baudouin reinstated the deleted text in the Polish translation (BdC 1904: 177–178); it suggests that Schleicher did not endorse Baudouin's speculations that in Schleichers view went beyond the subject at hand. A full discussion of this matter would exceed the limits of the present chapter (see, however, chaper 4 above for details).

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> [If one takes words in language in the exact manner in which they are actually found and as the speaker feels them, one cannot assume the existence of vocalic stems in the Polish declension. Vocalic stems are just not felt in the nouns of Polish.
>
> In the present state only consonantal stems are felt in the Polish declension, at least only those are sensed by the Sprachgefühl [of the native speaker].
>
> By the way, it is easy to see that in Polish the division of the declension according to stems cannot be carried out.]

These observations may appear somewhat redundant in parts, but they were hardly in contradiction with the tenor of Baudouin's study, although the original text went beyond what Schleicher could accept, namely, extrapolation for Indo-European beyond the available evidence (see Mugdan 1984: 112; Adamska-Sałaciak 1996: 67–68, for the relevant excerpts).

What may be worth mentioning is that Baudouin received, through the intervention of Leskien, who had recently become the first incumbent to the Chair of Slavic Philology at the University of Leipzig, in the summer of 1870 a doctorate for this work on analogy, with Georg Curtius (1820–1885), the classical scholar and teacher of Brugmann, Osthoff, and many others, and Hermann Brockhaus (1806–1877), the Sanskritist, serving as examiners (Mugdan 1984: 13).

2.2 Baudouin's assessment of Schleicher's legacy

There are two early documents that give a fair idea of Baudouin's evaluation of Schleicher's legacy. There is first of all his obituary written in Polish for a Warsaw popular magazine, which appeared one year after Schleicher's death. It was largely held within the scholarly conventions of necrologies. Indeed, it begins with a summary appraisal of Schleicher's achievement: 'This splendid state of linguistics is due to a few fine men. One of the highest places among those very few belongs no doubt to August Schleicher.' Then a regular account of Schleicher's career follows, taking up the bulk of the entire obituary. However, before concluding the personal acknowledgment which reads:

> The memory of Schleicher as a scientist will never die, as long as people occupied with science are alive, memory of him as a person will live in the hearts of his friends and students. The last disciples of Schleicher were two graduates of Warsaw Main School [i.e., Warsaw University], Lucyan Malinowski [(1839–1898)] and the author of this article.

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– Baudouin signals at least one slightly critical qualification (p. 319):[6]

> Schleicher did not create any new science, nor expressed any new great thought which would cause a revolution in a given matter, like Bopp and Wilhelm [von] Humboldt, but he had a strictly scientific mind, energy and stamina, a sober view of facts, ability of clear and well organized thinking to present his knowledge to others. Without doubt he was the one to move forward the knowledge of the structure of Indo-European languages and in this respect he presents the current state of knowledge. Only those blinded by their self-esteem, putting their greed of hatred and revenge over truth and justice, can abuse Schleicher in such a mean fashion, as did Hattala[7] in his last three treatises, even lowering himself to denounce publicly the deceased and his disciples.

The view that 'Schleicher did not create any new science' is essentially correct, even though Schleicher had added significantly to the edifice of Indo-European linguistics that paved the way for the new generation of scholars, not only the *Junggrammatiker*, but also Baudouin himself and many others (cf. Koerner 1981, 1982, for details; see also Syllaba [1995: 31–38], on the many Czech and other foreign students Schleicher had during his professorship in Prague, 1850–1857).

When Baudouin returned to Warsaw in the fall of 1868, he had to discover to his dismay that Warsaw University ("Szkoła Główna") had been Russified, excluding him from the chance to teach there. However, at least in part in order to allow him to improve his active knowledge of Russian; he was sent to continue his studies at St. Petersburg on a scholarship. His teacher in Indo-European linguistics there, the Academician Izmail Ivanovič Sreznevskij (1812–1880), suggested to him to study the earliest documents of Polish for his Master's thesis, a research that led to an important monograph (BdC 1870). But Sreznevskij, about whom Baudouin had little positive to say many years later (BdC 1897: 23), also asked him to prepare a report on Schleicher's work, a task that offered Baudouin the opportunity to discuss at length Schleicher's linguistic views in a much more critical manner. This analysis, presented at the April 1870 meeting of the Philological Society of St. Petersburg (Häusler 1968: 27), remained manuscript until 1963, when it was published (with few omissions) in the first tome of a two-volume selection of his

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6. The first two sentences of this quotation reappear almost verbatim in Baudouin's appraisal of the following year (see BdC 1963 [1870]: 44), which I will quote in full toward the end of the present section.

7. The reference is to the Slovak Martin Hattala (1821–1903), who owed much of his career at the University of Prague to Schleicher, and who from 1866 onwards attacked both Schleicher as a person and as a scholar in the meanest manner imaginable (cf. Syllaba 1995: 63–65, for an account of this sordid affair).

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writings in Russian (BdC 1963 [1870]).[8] Adamska-Sałaciak, like Häusler (1968: 27–33) before her, recognized the importance of this report submitting it to a detailed analysis (Adamska-Sałaciak 1996: 69–75). She may well be justified in saying (p. 69) that Baudouin's "discussion of Schleicher's position turned out to be a mere starting point for the formulation of the author's own views" – although one may want to question the exclusive 'mere' in her statement. For his analysis, Baudouin referred to many of Schleicher's works, from his first book *Zur vergleichenden Sprachgeschichte* of 1848 to his popular essay *Die Bedeutung der Sprache für die Naturgeschichte des Menschen* (Schleicher 1865).

Having begun by noting the need to distinguish between the general principles adopted, the theory, and the application of the principles, or, the processing of the linguistic material, Baudouin acknowledged that '[i]f one may only rarely agree with Schleicher's general pronouncements, it is all the more astounding to recognize his deep knowledge of facts, the enormous capacity of reasoning, and systematizing, and, lastly, the clarity and orderly way of thinking' (1963 [1870]: 35). He subsequently discusses his various points of disagreement with the views of his one-time teacher, or what he identifies as Schleicher's 'dogmas', a loaded term indeed. Having asserted that '[s]cience, be it glottics or philology, engages only in the pursuit of understanding, not in tilling, the perfection of an object and the appreciation of its beauty' (pp. 35–36), he criticizes Schleicher for having been – unduly in Baudouin's estimation – fascinated by the historical development of science and for taking the 'unproductive state of classical philology with its outdated routines as a model of philology in general' (p. 36). Schleicher, in his striving for the perfection of the science of language, i.e., a perfection that would emulate the deductive stringency of mathematics, took language as a phenomenon of nature and, as a result, wanted linguistics to be equal to natural science. For Baudouin this was untenable; for him '[i]n regard to the nature of the subject matter it [i.e., linguistics] belongs to the psychological-historical sciences – even sound relations cannot be adequately explained without reference to the *Sprachgefühl* of a people' (p. 37). In Baudouin's view, Schleicher had fallen victim to his dogma of language as a natural organism which led him to contradictions, and which, according to Baudouin (p. 38), would

> disappear if one puts in the place of the personifying word 'organism' (creature, occupying its own space, feeding itself, multiplying itself, being tangible) the expression 'functions of an organism' (i.e., the consequences of the activity of organs). Then the development of language becomes very understandable without strain,

8. Curiously, Mugdan (1984: 47 and 147n.144) refers to this "Referat über Schleicher" and quotes from it (p. 100), but does not include it in his bibliography of Baudouin; Adamska-Sałaciak (1996: 69, 75) dates it to 1869 instead of 1870 as the Russian editors do.

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> and the application of the categories of "struggle for survival" (*Kampf ums Dasein*), "natural selection" (*natürliche Auslese*), can be limited to the history of forms that are internal to language proper. In a figurative sense, and only in a figurative sense [perenosnom smysle] can one speak of "Kampf ums Dasein", or "natürliche Auslese" etc. of languages, since it is people that are the carriers of languages.[9]

For Baudouin, the development of language and the history of a people are in a mutual relationship, and that language is not completely independent of the human soul, as Schleicher would have it. Baudouin also noted inconsistencies in Schleicher's own treatment of language as a natural organism: 'As if contradicting himself, Schleicher stood up very often against several, what he called, non-organismic phenomena in the German language, introduced by ignorant know-betters. Schleicher called upon his fellow countrymen to abstain from such wrongs' (p. 40).

Baudouin also objected to what he saw as Schleicher's 'dualisms' that he thought permeating his theory, such as soul vs. nature, language vs. history, linguistics vs. philology, stating (p. 41):

> Although Schleicher was a declared enemy of guess work, unfounded by inductions, and therefore did not support at all the so-called philosophy of the human word [whatever BdC meant by this], he quite often did pronounce general propositions in a completely dogmatic fashion. As it seems, even the morphological classification of languages were formulated a priori, not sufficiently considering the nature of the languages [in question].

Baudouin was in particular criticizing Schleicher's tripartite division of languages into 'monosyllabic', 'agglutinating', and 'inflectional' types and the occasional value judgments that accompanied his classifications, which covered the bulk of his popular book *Die Sprachen Europas* (cf. Schleicher 1850), in which the Indo-European languages took pride of place (113–240).[10]

Baudouin identifies further 'narrow views of the facts of language' on the part of Schleicher, such as undue attention to ancient language phenomena, neglecting

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9. More like an afterthought, Baudouin recognized (p. 38) that Schleicher '[o]nly at the end of his life, […] became convinced that, without that category [of the psychological explanation of language phenomena], much remained unexplained. But he did not have the time for making this insight bear on the practical work.'

10. Interestingly, Baudouin does not refer to this book, but to Schleicher's earlier book of 1848 and his monograph *Zur Morphologie der Sprachen* (Schleicher 1859b), in which he identifies, following August Friedrich Pott (1802–1887) it would seem, four classes. (It should be added that it was Schleicher who introduced the term 'morphology' into linguistics in 1859, and that Baudouin many years later – in 1881, to be exact – coined the term 'morpheme' on the model of 'phoneme'.)

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more recent ones; viewing the task of linguistics only in the reconstruction of a protolanguage concentrating on what he regarded as the main kinship relations between languages, leaving out differences.[11] While it is true that Schleicher preferred to investigate ancient language states (because of their much fuller morphology) and that indeed he regretted what he saw as *Verfall* (desintegration) in their younger descendants, Baudouin, as on other occasions, tends to exaggerate. While Baudouin refers in passing (p. 43) to Schleicher's fieldwork, notably on Lithuanian (e.g., Schleicher 1856–1857), and mentions Schleicher's 1858 book on the dialect of his native town, both activities that must have inspired Baudouin's own work on Slavic dialects (e.g., Baudouin 1884, 1895a),[12] he does not recognize that this might at least have weakened his criticism. Still, Baudouin is ready to offer a list of Schleicher's positive achievements, after '[h]aving established the negative aspects of Schleicher's work', such as expressing with near-to-mathematic precision the sound laws of the Indo-European languages, emphasizing his 'strong deductions concerning sound laws [which] gave Schleicher the possibility to reconstruct the main features of the Indo-European protolanguage', adding that '[a]lthough he did not describe his method of reconstruction, it is obvious from the whole of the *Compendium* and other works' (p. 42). Moreover (p. 43):

> Schleicher presented his findings clearly, in a condensed fashion, well-founded, thoroughly, and in a definite style in a commonly known shared language, without any unnecessary adornment, simple, easy to understand, well-thought-out, even though he repeated the very same idea, which – by the way – happens to everybody.

What Baudouin valued above all was Schleicher's strict methodology. In his estimation, he was not revolutionary, but did establish his own science (*nauka*). Summarizing his appraisal, Baudouin (p. 44) had this to say about his one-time teacher:

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11. Baudouin does not specify what he means by 'differences'; in matters of reconstruction, it stands to reason that the focus must be on parallels and similarities of development. In later references to Schleicher's work Baudouin's assessment becomes more of a caricature, for instance when he asserts that "Schleicher, […] with maniacal obstinacy went on repeating "die Sprachwissenschaft ist eine Naturwissenschaft [linguistics is a natural science]", [claiming] at the same time […] that the recovery of various 'Ursprachen [protolanguages]' and 'Grundsprachen [actually another word of Schleicher's for 'Ursprachen'] is the main task, even the exclusive task of linguistics, and that our science cannot exist at all without such reconstructional conundrums" (2005 [1888–1889]: 30).

12. It certainly inspired his countryman and Schleicher student Lucjan Malinowski (1839–1898), whose work is often regarded as marking the beginning of Polish dialectology (Malinowski 1873).

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> Schleicher did not create a new science, nor caused a revolution in the existing ones, as did for instance Bopp, Grimm and Wilhelm [von] Humboldt. But he had a strong scientific mind, loved hard work, had a measured temperament, had a sober judgement of facts and the ability to present his insights clearly and in an organized manner. After Bopp, without doubt, he moved forward above all the knowledge of the strata of the Indo-European languages, and in that regard he has spoken the last word on science.[13]

In the penultimate paragraph of his 1870 appraisal Baudouin also refers to Schleicher's great ability to learn foreign languages, albeit in a somewhat jocular vein. Speaking of Schleicher's work on a Slavic language whose last speakers had disappeared by the early 18th century, if not earlier, Baudouin noted that '[h]e was said to master the language to such a degree that, should a speaker of Polabian have risen from the dead, he would have been able to conduct a conversation with him.'

3. A brief summing-up

As we may gather from the preceding analysis of Baudouin's early assessment of Schleicher, the criticism he brought forward against some of Schleicher's views were not as damaging as some historians of linguistics have made out. Indeed, while taking a broader view of philology than Schleicher did, Baudouin did subscribe to linguistics as an independent field of study with a scientific method of its own. Indeed, Baudouin used Schleicher's term 'Glottik' or 'Glottologie' for it in several of his publications devoted to general linguistics (cf., e.g., Mugdan 1984: 136; BdC 1972: 57).[14] More importantly, especially in his early career but by no means exclusively, we meet Baudouin on the paths that Schleicher had cleared the ground for. Thus, he investigated the phenomenon of assimilation ('Zetazismus' [BdC 1869e]) to which Schleicher (1848) had devoted an entire monograph. He also investigated child language acquisition early in his life (BdC 1869c, indeed using the same title as Schleicher 1861b) and throughout much of his career, at least until 1904 (see the selection in BdC 1974). I mentioned his dialectological studies before in this chapter,

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13. Such an assessment agrees very much with what I said myself in Koerner (1981), in ignorance of Baudouin's evaluation of more than one hundred years earlier.

14. Perhaps the fact that Graziadio Isaia Ascoli (1829–1907), whose lectures in Milan he attended in spring 1873 (Mugdan 1948: 15), could have reinforced in Baudouin the use of the term, while the opposite is possible, namely, that Baudouin motivated Ascoli to call his new journal *Archivio glottologico italiano* (Florence 1873–present), thus introducing Schleicher's term into the Italian lexicon.

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and, as I noted as early as in my review of the 1972 Baudouin anthology already (Koerner 1973: 47–48), Baudouin's love of field work and his concentration on phonology and morphology (rather than syntax) are reminiscent of Schleicher, too.

Finally, to round out the picture at least somewhat, a word should be said about Schleicher's and Baudouin's extra-linguistic interests. I'm not thinking of Schleicher's love of gardening and the breading of flowers or his physical prowess, where I don't think Baudouin would have been a match, but his thoroughly democratic leanings and sense of justice: here are indeed touching parallels between the two scholars and, as their respective biographies suggest,[15] both paid a high price for expressing their political convictions.

Acknowledgements

A previous version of this chapter appeared as "A Matter of 'Influence': Jan Baudouin de Courtenay's relationship toward August Schleicher and his work" in *Tova čudo – ezika˙t! Izsledvanija v čest na Prof. dr Jivko Boyadjiev / Language: this miracle! Papers in honour of Prof. dr Zhivko Boyadzhiev* ed. by Petya Assenova, Biliana Mihailova, Fotiny Christakoudy, Snezhana Filcheva & Maya Alexandrova (Sofia: University Publishing House "Saint Kliment Ohridski" / Universitetsko izdatelstvo "Sv. Kliment Ohridski", 2007), 213–227. The manuscript had been submitted on 28 February 2006 in anticipation of the honoree's 70th birthday in April that year. The Festschrift eventually appeared in print in July 2007, a few days before my longtime friend's sudden death. – My return to the work of Schleicher and Baudouin – after a hiatus of twenty-five years or more – had been prompted by my participation in the internaional conference "Jan Baudouin de Courtenay (1845–1929): Linguist, publicist, man", held at the Jagellonian University, Kraków, Poland, 19–20 September 2005, at which I revisited the relationship between the 'Kazan School', represented by Kruszewski and Baudouin, and Saussure. However, I ended up in the discussions to claim that Schleicher's importance for Baudouin has regularly been downplayed in the annals of linguistic science. One of the conference participants challenged me to follow my own principles and make the argument stick.

15. See Vaníček (1996 [1866]: 92–99), Dietze (1966: 63–64), and Sylaba (1996: 48–54) regarding Schleicher, and Mugdan (1984: 23–45 *passim*) and Glück (2004) concerning Baudouin.

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1869b [1868]. "Wortformen und selbst sätze, welche in der polnischen sprache zu stämmen herabgesunken sind". *Ibid*. 6:2.204–210. (Polish translation in Baudouin 1904: 249–256.)

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CHAPTER 6

Hermann Paul, Saussure, and general linguistic theory

1. Introduction

During the years 1969–1971, when I was working on my dissertation on Saussure and the possible sources of his intellectual inspiration, Hermann Paul (1846–1921) had been seen essentially as a philologist of German (which, among other things, he definitely was), as a spokesperson of the Neogrammarians (which to a considerable extent he was too), and as a typical example of 19th-century positivism in linguistics (which, in a positive, though not positivist, sense, he was too; cf. his influential *Deutsches Wörterbuch*, first published in 1897).[1] In addition, he was characterized in matters of general linguistic ideas, more often than not, as representing everything to which Ferdinand de Saussure (1857–1913) stood in opposition to.[2]

By this time, it is noteworthy to point out that Paul had been completely written out of the record of many historical accounts of the discipline (e.g., Dinneen 1967, Putschke 1969, Lepschy 1970, Leroy 1971, Mounin 1972). Malmberg, who spoke of Paul's *Prinzipien der Sprachgeschichte* as "the bible of the junggrammatiker school" (1964: 12), does no more than quote the (now notorious) passage from the *Prinzipien* in which Paul emphasizes his view that only the historical approach to language can be regarded as adequate and truly scientific (14–15).[3]

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1. For a monograph-length appraisal of Paul as philologist and linguist, see Santulli (1995).

2. For a refreshingly detached assessment of Paul's life and work, with no reference to Saussure, see Neumann (1996). My earlier attempts to put Paul's *Prinzipien* forward as an important source of Saussure's inspiration (Koerner 1971, 1972) were criticized by László Antal (1985), who instead argues in favour of Durkheim (pp. 122, 127–129) whose direct influence on Saussure still awaits definite proof today. More recent work on Saussure is tending to take Paul's influence on him more seriously. Bouquet (1997), for instance, based on intense study of Saussure's lectures and unpublished papers (a large portion of which were discovered only in 1996), makes frequent references to Paul's *Prinzipien* (see Index, p. 390), without however offering an analysis of Paul's general linguistic thinking.

3. Still in 2001, Einhauser (p. 1347) expresses her regrets that Paul's linguistic views had been reduced to this passage, first stated in the 2nd ed. of Paul's *Prinzipien* (1886: 20) in reaction to Franz Misteli's critique of the first edition, notably Misteli (1882: 380ff.)

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This perspective on Paul was reiterated by others (e.g., Mounin 1967: 210, Robins 1995 [1967]: 209–210]), who essentially had nothing more to say about a scholar who had exerted such strong influence on linguistic thinking in Europe and America during the period extending from 1880 to 1920, during which five editions of his *Prinzipien* plus two translations and one adaptation into English had appeared (e.g., Paul 1880, 1889).

It is true that there are some noteworthy exceptions (Ivić 1965: 61–63; Arens 1969: 346–359; Helbig 1970: 15–19). Yet no one of them demonstrated convincingly the (perhaps somewhat latent) modernity of Paul's principles of language study. Arens, who reproduced passages from Paul's *Prinzipien* in his 'Problemgeschichte', seems unaware of the inherent structuralism of Paul's work (cf. Arens 1969: 346–347) and Ivić (1965: 61–62) does not advance her analysis any further than to suggest that a number of Paul's ideas had in fact been taken up by Saussure, who is now generally accepted as the 'founder of modern structuralism'. Only Helbig (1970: 18–19) offers a few suggestive hints, noting that Paul's work contained "bereits den Keim zur Selbstüberwindung mancher junggrammatischer Axiome

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[carried already the germ for the self-correction of many neogrammarian axioms]" (p. 18), without however, carrying out a more detailed study of Paul. In contrast to the arguments of the chief promoters of the 'Gabelentz myth' (as I would like to call it), namely, Coseriu and Christmann (cf. Koerner 1974, for an early critique), who both were Romance scholars, but there was also Eberhard Zwirner (cf., e.g., Zwirner 1966 and later repeatedly), a phonetician with an interest in the predecessors of his own craft, who stood in oppositiom.[4] Finally, as an exception to the rule must be mentioned Kurt Jankowsky, a Germanist, quite independently and with a dedicated

4. As a result, Hermann Paul's *Prinzipien* is referred to exclusively with regard to his 'variationist' ideas (see Zwirner & Zwirner 1982: 107, 116n., 122–123, 140n., 200n.)

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effort to place Paul in his historical context, gaving Paul his dues, showing at the same time why Paul's ideas are still relevant today (see Jankowsky [1972: 262], for his many references).[5]

2. Problems in recognizing Paul as a general linguist

What appears to have complicated matters at the time was that Noam Chomsky's *Cartesian Linguistics* of 1966 had provided an unfortunate model for dealing with the linguistic past that found uncritical emulators and led in some quarters to an 'ancestor hunt' that precluded any serious historiography. Indeed, it is in my view correct to say that before the mid-1970s and early 1980s the history of linguistics was in matters of methodologically and epistemologically seriously jejune. The question of what were the appropriate bases on which the subject was to be pursued had not even entered the realm of public debate among linguists who had shown an interest in the history of the sciences of language.

It is true that my own interest in Hermann Paul as a general linguist had been heightened at the time by the attempts on the part of two major scholars, Eberhard Zwirner (1899–1984) of Münster and Eugenio Coseriu (1921–2002) of Tübingen, prop up the work of the Sinologist and Humboldtian generalist of language Georg von der Gabelentz (1840–1893) as Saussure's precursor *par excellence* in matters of linguistic theory (see Koerner 1972). Gabelentz' book *Sprachwissenschaft*, first published in 1891, was not unknown during the 1920s, when Saussure's *Cours* became the major point of reference in many intellectual centers outside Germany, notably Prague, St. Petersburg, Paris, and Copenhagen. For instance, the great Danish scholar Otto Jespersen (1860–1943) stated in his book *Language* that he owed more to Gabelentz than to any other linguist (cf. Jespersen 1922: 98), and his countryman Louis Hjelmslev (1899–1965), in his *Principes de linguistique générale*, has numerous references to Gabelentz' work (see Hjelmslev 1928: 11, 39, 43, 76, 84, 91, etc., especially pp. 112–113), to cite just two examples. However, neither of the two made any overt claims that Gabelentz had anticipated Saussure. Yet long before Zwirner and Coseriu came out with their contentions, various other scholars had pointed to Georg von der Gabelentz' *Sprachwissenschaft* as the precursor of structural linguistics in phonology, like the Dutch scholar Cornelius Leonardus Michels (1887–1984) in 1952, and much earlier hints at Gabelentz' supposed precursorship can be found in the literature. To refer to just two examples, as early

5. I took note of Jankowsky's book well after completion of my own early research (Koerner 1971b, 1972), in fact only on 9 August 1972, when I visited the offices of Mouton & Co. in The Hague and the book had just arrived from the press.

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as 1918 Leo Spitzer (1887–1960) noted what he believed to be obvious parallels between Gabelentz's ideas of *Sprache* and *Rede* and Saussure's all-important *langue/parole* distinction (Spitzer 1918: 345), and Friedrich Kainz (1897–1977) felt that it was an "Akt geschichtlicher Gerechtigkeit" ["an act of historical justice"] no less, to draw attention to the fact that the threefold Saussurean distinction between *langage, langue*, and *parole* had been made earlier by Gabelentz (Kainz 1941: 20). These and other references may suffice to suggest that Gabelentz' *Sprachwissenschaft* had not been forgotten, that Zwirner's and Coseriu's findings were no true novel discoveries, though for the most part misleading interpretations of Gabelentz's goals and exaggerated claims of his precursorship of Saussure's linguistic theories (see Koerner 1974, for details). It appears that the very title of Gabelentz's book *Sprachwissenschaft* "science of language" made it possible for hurried readers and writers to dismiss Paul's book *Prinzipien der Sprachgeschichte* "principle of language history" as not dealing with general linguistic theory at all, while making it easy to embrace claims that the former was the obvious source for Saussure. Only independent minds like Kurt R. Jankowsky (b.1928), who studied Paul's *Prinzipien* carefully (see Jankowsky 1972: 145–160), came to a fair appreciation of Paul's contribution to general linguistic principles, summarizing his view on the matter by stating: "Saussure grew not on Saussure, but on Paul and others who shortened, by their mistakes and by their achievements, [facilitated] the way for Saussure to reach where his predecessors could not" (Jankowsky 1972: 149). The extent to which one may speak of Paul's influence on the form and nature of Saussure's linguistic theory will be clearly argued in the remainder of this chapter.

3. Paul vs. Gabelentz as forerunners of descriptive linguists

The focus of this chapter is Paul's *Prinzipien*[6] and its possible impact on Saussure's arguments in his lectures on general linguistics (1907–1911), and not Gabelentz' *Sprachwissenschaft*, the view that the latter was a major source of Saussure's linguistic thinking which was so forcefully promoted during the 1960s and 1970s that the late Romance scholar Hans Helmut Christmann (1929–1996) concluded at the time that "Auf Grund von Coserius [1967/1969] Demonstration kann man Gabelentz' Buch mit Fug und Recht als die wichtigste [*sic*] Quelle von Saussures *Cours* ansehen"

6. Jörn Albrecht (1994) quotes from various other works of Hermann Paul to discuss in which way Paul might have been a structuralist *avant la lettre*, and indeed one should regard Paul's linguistic work as a unity; cf. also the recent collection of a number of his papers (Paul 1998). However, it is in his *Prinzipien* where Paul argues most forcefully as a theorist.

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(Christmann 1972: 245). Similar claims can also be found repeated in more recent work (e.g., Figge 1994: 656–657; Dove 1994: 1341; Albrecht 2007: 23).

Early references to Gabelentz and certain ideas found in the *Cours* apart, it appears to have been Eberhard Zwirner (1899–1984) who, on the occasion of the Fifth International Congress of Phonetic Sciences held in Münster in 1964, made a number of largely unsupported claims regarding Gabelentz's importance for Saussure's linguistic theories,[7] statements which Zwirner reiterated frequently thereafter, culminating in the misleading contention that Gabelentz had been Saussure's teacher at Leipzig.[8] One year after K. H. Rensch (b.1937) published a paper attempting to qualify Zwirner's poorly founded assertions (Rensch 1966), Eugenio Coseriu's (1921–2003) bold and, in my opinion, rather provocative article appeared in which the author tried to persuade the reader that Gabelentz's *Sprachwissenschaft* was in fact one of the principal sources of Saussure's linguistic inspiration alongside the work of Whitney and Durkheim [*sic*].[9] In order to cement his interpretation, Coseriu's study of 1967 was included in a slightly modified version in the 1969 reprint of the second revised edition of Gabelentz's book. In this chapter, whose appeal will depend on the extent to which the reader adopts Coseriu's contentions, the author calls for the desirability of considering Saussure and his linguistic theory "dans ses rapports avec la linguistique antérieure" (1967: 100). However, in his attempt to salvage Gabelentz's work in general linguistics by 'demonstrating' Saussure's dependence on Gabelentz's insights (instead of showing that Gabelentz's *Sprachwissenschaft* merits recognition and a reappraisal in its own right – as Els Elffers [2008] has very recently done), Coseriu seems to have ignored a number of important considerations and, thereby, defeats his own intentions.

To begin with, Coseriu failed to provide any historical perspective; Gabelentz's work is presented as if Paul's *Prinzipien* had not appeared in its second much

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7. See the *Proceedings* of this Congress ed. by Zwirner and Wolfgang Bethke (Basel & New York: S. Karger, 1965), pp. 7–9.

8. See Zwirner in Zwirner & Zwirner (1966: 81, 101–103, 109, 166); p. xiv in Zwirner & Ezawa (1968); *Cahiers de linguistique théorique et appliquée* 3: 189–190 (1966); *To Honor Roman Jakobson* (The Hague: Mouton, 1967), vol. III, pp. 2445–2446; *Frankfurter Allgemeine Zeitung* (Saturday, 11 Oct. 1969), Zwirner (1969: 31, 35–36), and again in Zwirner & Zwirner (1982: 109–111, 117, 168, 281, and elsewhere). Georg Stötzel appears to have adopted Zwirner's claim uncritically when he speaks of "von der Gabelentz, der Lehrer Saussures" (*Poetica* 3, p. 17 [1970]) mentioning Paul in the same sentence without a similar qualification. Indeed, in Zwirner's work Paul is discussed only when he sees interesting points in Paul's work for his own phonemetric and dialectological theories (cf., e.g., Zwirner & Zwirner 1982: 40, 50, 107, 116, 122–123, 140).

9. Strangely enough Coseriu does not mention any of Zwirner's papers to this effect and ignores the paper of Rensch. His assertion that Durkheim influenced Saussure considerably (1967: 100) follows the *fable convenue* which nobody has been able to prove unto the present day.

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revised and enlarged edition in 1886 – five years before the first publication of the *Sprachwissenschaft* (*nota bene*). Although Coseriu must have taken notice of the fact that the majority of the important theoretical statements in Gabelentz were added or at least made explicit in the second edition of 1901, long after Gabelentz' demise, he does not ask himself which developments could have led Gabelentz's nephew Albrecht Graf von der Schulenburg (1865–1902) to state in his preface to this edition (p. vii) that he had expanded and emended the original text, "wo der Fortschritt der Wissenschaft es dringend verlangte" ("where the progress of science had demanded it urgently").[10] This is all the more surprising when we note that Coseriu (1967: 87, 99) suggests his acquaintance with Robert Godel's (1902–1984) *Sources manuscrites* of the *Cours* (Godel 1957) from which he could have gathered that Saussure developed most of his ideas concerning general linguistics in the early 1890s and certainly much earlier than 1901. Another questionable procedure underscores this lack of a historical treatment: Coseriu does not elucidate Gabelentz's theory of language in the light of its own terminology but instead introduces what he suggests are the Saussurean equivalents of Gabelentz's terms, thus causing a subtle change of semantics which seems to support his argument but in fact tends to invalidate it.[11]

In this chapter I won't argue at any length against a number of Coseriu's and others' claims concerning the allegedly strong influence of Gabelentz's theories on Saussure's linguistic thought.[12] My central aim is to demonstrate how Paul's *Prinzipien* anticipated important components of Saussure's 'structural' theory of language, also in order to suggest that Paul (and neither Durkheim nor Gabelentz) was, next to William Dwight Whitney (1827–1894), on whom see Koerner (1980) and Joseph (2001), the most influential source of Saussure's general linguistic ideas. Neither Durkheim nor Gabelentz is mentioned in any of Saussure's lectures,

10. For an accounting of the altogether 80 pages worth of additions Schulenburg made, see Koerner (1971b: 192, n.11).

11. In 1972, Edward Stankiewicz of Yale used the same technique of using Saussurean terms in his translations from the Russian and German in order to "prove" that Saussure was unoriginal, and that Jan Baudouin de Courtenay (1845–1929) had anticipated many of his essential theoretical observations; cf. my critique of his Baudouin anthology in *Language Sciences* No.27.45–50 (Bloomington, Indiana, Oct. 1973).

12. While one may welcome Els Effers' not very original intent to present Gabelentz's *Sprachwissenschaft* as a work that is certainly deserving historiographical attention (Effers 2008), one regrets that she relies on Albrecht's (2007) 'discussion' which still likes to see Gabelentz's work as a source of Saussure's inspiration (p. 192). None of my own objections since 1970 to this view is mentioned. Besides, treating Gabelentz's *Sprachwissenschaft* of 1891 as "one of the first introductions to General Linguistics" (p. 191) fails to recognizes Paul's *Principien* of 1880 as a much earlier attempt.

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unpublished writings, or personal correspondence, there is however ample evidence for Saussure's appreciation of the general linguistic writings of Whitney and his critical attitude towards Paul, as lecture notes taken by students and notes from Saussure's own pen reveal.[13] It is therefore curious to see that Rolf Hiersche (1924–1996), while (correctly) rejecting the idea that Saussure's all-important *langue*/*parole* distinction had anything to do with Gabelentz's distinction between 'Sprache' und 'Rede' – neither term was given any strict definition in Gabelentz's work (Hiersche 1972), followed those who, ever since Witold Doroszewski (1899–1976) first enunciated this hypothesis in 1931, credited the French sociologist Émile Durkheim (1858–1917) with having led Saussure to this insight.[14]

4. Non-historical aspects of Paul's theories of language study

Paul's *Prinzipien der Sprachgeschichte* did not find unanimous approval when it was first published in 1880. The Zurich professor of general linguistics and German philology Ludwig Tobler (1827–1895), for instance, found a number of theoretical inconsistencies in Paul's *Principles* but concluded that the merit of this work lies in the fact that Paul had brought observations made by himself and others into 'a systematic context' ("einen systematischen Zusammenhang") and thus made them much more easily accessible to the linguistic researcher (Tobler 1881: 126). Friedrich Techmer (1843–1891) expressed himself even more positively still, stating that Paul's work belongs to the most important publications to date in the area of general linguistics (Techmer 1887: 357). Much more thorough and critical, however, was the review article of Franz Misteli (1841–1903), a later collaborator of Heymann Steinthal (1823–1899), whose *Völkerpsychologie* Paul had criticized in his book. Misteli took issue with the title of Paul's book and the viewpoint it implied. Misteli argued that it should have had 'Sprach*wissenschaft*' instead of 'Sprach*geschichte*' in its title, since he believed that linguistics cannot justifiably be restricted to language history only (Misteli 1882, esp. pp. 380ff.). In fact, Misteli uncovered a number of misconceptions and contradictions in Paul's linguistic argument in particular

13. Cf. Godel (1957: 51); the critical edition of the *Cours*, prepared by R. Engler, vol. I (Saussure 1968: 16). See also Saussure's affirmation of 1891 regarding the importance of Paul and a few other linguists for the development of a general linguistics published in Saussure (1954: 66 = Saussure 2002: 147).

14. For a detailed rebuttal of this myth – still maintained in Robins (1995 [1967]: 220) for instance – see already Koerner (1971: 45–60), which, among others, shows Saussure had come to his ideas well before Durkheim first defined 'fait social' in the long preface to the second edition of his *Règles de la méthode sociologique* (Paris: Félix Alcan, 1901).

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concerning the relation between 'descriptive Grammatik' and 'Sprachgeschichte'. For Paul only the latter is tenable; i.e., only the historical treatment of language is adequate and scientifically sound since language is constantly changing.

Misteli demonstrated that Paul's empiricism and his rejection of abstraction as an acceptable procedure for generalization is thwarted by Paul's concession that a 'certain average' of language phenomena at a given period had to be established (which is just what descriptive grammar does) in order to discover its generally due system (Misteli 1882: 384–386). Indeed, Paul had stated already in the first edition of his *Prinzipien* (1880: 78 = 1909: 189):

> Wie wir überhaupt nach einem gewissen durchschnitt das in einer bestimmten periode allgemein übliche darstellen, so sind wir auch im stande, für jede entwicklungsperiode einer sprache ein im wesentlichen allgemein gültiges system der gruppirung aufzustellen.
>
> [As we generally present through a certain average the on the whole customary of a given period, we are able, for each period of development of a language, to establish an essentially valid system of arrangement.][15]

This suggests to me that only if certain horizontal averages are established, can observations of changes be made that have occurred through time. As later editions show, Paul never changed his views on this point.[16]

It is important to realize that, as early as the 1880s, there was among a number of linguists an awareness of the existence of two distinct kinds of approaches, a descriptive one concerned with a language at a given period and a historical one dealing with the changes of languages through time. It is therefore not surprising that Saussure could write in 1894 that he had been convinced for a long time ("depuis bien des années") that linguistics was a double science (cf. *SM* 45).[17] It appears therefore quite probable that Meillet was referring to Saussure's teachings in Paris at the École Pratique des Hautes Études (1881–1891), when he noted in his obituary of his former 'maître' in 1913:

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15. Here and elsewhere I have made an effort to offer an English translation of Paul's not always easy prose, as the 19th-century translators (see Paul 1888, 1891) had to concede.

16. Cf. the 8th edition (actually a photographic reproduction of the 5th ed. of 1920) of the *Prinzipien* (Tübingen: Max Niemeyer, 1968), p. 189, which contains the same statement with only changes made in the orthography.

17. Here and in the following *SM* stands for *Sources Manuscrites* (Godel 1957) and *CLG/E* for the 'édition critique' of the *Cours* provided by the late Rudolf Engler (1930–2003) as *CLG* stands for the 'vulgata' text of the *Cours*, with the pagination referring to the 2nd (1922) and subsequent editions.

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> F. de Saussure voulait surtout bien marquer le contraste entre deux manières de considérer les faits linguistiques: l'étude de la langue à un moment donné, et l'étude du développement linguistique à travers le temps.[18]
>
> [F. de Saussure wanted above all to clearly underscore the contrast between two manners of treating linguistic facts: the study of language at a given point in time and the study of the linguistic development through time.]

This testimony of his most important French student at least casts doubt on those arguments which suggest that Saussure, in making the distinction between synchronic and diachronic linguistics in his Geneva lectures, followed Gabelentz who distinguished in 1891 between 'the study of an individual language' ("einzelsprachliche Forschung") and 'genealogisch-historische Sprachforschung' (see Gabelentz 1901 [1891]: 8–9, 54ff., 136ff.). Gabelentz did not state anywhere – as Saussure did – that we have to do with the same object, but looked upon from two distinct perspectives.

If, later in this chapter, I often quote – essentially for convenience sake – from the fourth edition of Paul's *Prinzipien* of 1909, I do so mainly because the fifth edition of 1920, which constitutes nothing more than a corrected reprint of the former, has been re-published frequently since 1968 and is the most readily accessible one. This procedure will be followed after having checked previous editions of the *Prinzipien* and compared them with the fourth edition in order to avoid any anachronistic argument. In fact, I shall supply page references to earlier editions, notably to the second, much enlarged edition of 1886, to which Paul had added a number of new chapters, and if the passages in question are already available, also to the first edition of 1880, the year Saussure submitted his doctoral thesis at the University of Leipzig. As a matter of fact, Paul maintained his original position; reinforcing his views in response to criticism, and the main principles remain almost completely unaltered throughout the subsequent editions.[19] Indeed, comparison

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18. Cf. the reprint in *Linguistique historique et linguistique générale*, vol. II (Paris: C. Klincksieck, 1936), p. 183; it should be borne in mind that Meillet did not know as late as 1915 that students' notes from Saussure's lectures on general linguistics of 1907–1911 had been preserved and that an edition was in preparation. We should also state that Meillet became Saussure's student in 1886 only, by which time Paul's *Prinzipien* had its second edition. Chronology remains an important methodological feature of linguistic historiography.

19. Even a comparison between the first and the fourth edition shows the following picture of correspondences: 1880: 1–26 = 1909: 1–22; 1880: 27–33 = 1909: 23–29; 1880: 231–44 = 1909: 37–48; 1880: 40–60 = 1909: 49–73 (but with many additions on pp. 62–67); 1880: 61–77 = 1909: 106–15; 1880: 183–99 = 1909: 174–188; 1880: 78–99 = 1909: 189–216; 1880: 145–153 = 1909: 242–250; 1880: 131–144 = 1909: 154–182 = 1909: 325–351; 1880: 200–230 = 1909: 352–372; 1880: 245–265 = 1909: 373–389; 1880: 266–288 = 1909: 404–422. While the third edition of 1898 was expanded by about 30 pages, the fourth has about 35 pages more owing to an additional preface

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between the 1886 text and the modern reprints of the 1920 edition shows that not a single new chapter had been added since the second edition.[20]

4.1 Descriptive vs. historical linguistics and the concept of language state

For the present discussion it is essential to demonstrate that Paul's affirmation that only historical linguistics could be scientific does not represent a mere casual remark (1909: 20 = 1886: 19). Paul's apparent opposition to any linguistic method other than the historical one led, in Milka Ivić's words, to him being "mainly remembered as a tireless apostle of historicism". She also noted (1965: 61): "It has been forgotten that Paul's *Principien der Sprachgeschichte* contains some excellent observations which directly hint at many innovations which were to come after his time." However, this notoriety of Hermann Paul led to the 'structural' aspects of his theorizing being conveniently overlooked, probably because one writer of textbooks copied from another without actually consulting the book. In fact, Paul showed himself very much aware of the fundamental differences between what we now call the synchronic and the diachronic approach to language analysis when he stated:

> Die *historische Grammatik* ist aus der älteren bloss *deskriptiven* Grammatik hervorgegangen, und sie hat noch sehr vieles von ihr beibehalten. Wenigstens in der zusammenfassenden Darstellung hat sie durchaus die alte Form bewahrt. Sie hat nur eine Reihe von deskriptiven Grammatiken parallel aneinandergefügt. Das Vergleichen, nicht die Darlegung der Entwickelung ist zunächst als das eigentliche Charakteristikum der neuen Wissenschaft aufgefasst. Man hat die *vergleichende Grammatik*, die sich mit dem gegenseitigen Verhältnis verwandter Sprachfamilien beschäftigt, deren gemeinsame Quelle für uns verloren gegangen ist, sogar in Gegensatz zu der historischen gesetzt, die von einem durch die Überlieferung gegebenen Ausgangspunkte die Weiterentwicklung verfolgt. […]. Aber auch auf

(v–vii) in which Paul replied to Wundt's criticisms of his book, a few textual additions, and a general index (423–428), which had been absent from previous editions. It is probably for that reason that Arens (1969: 744) quotes Paul after the third edition of 1898. – I have abstained from citing the fifth edition of 1920 in order to avoid the misleading impression which Tagliavini (1963: 304) appears to have fallen victim to by suggesting that Paul's insistence on historical linguistics as the sole scientific method of treating language phenomena was prompted by Saussure's affirmation to the contrary in the *Cours* of 1916. Truth is that, apart from the fact that the first edition of the *Cours* was hard to come by in the middle of World War I, Paul was growing blind by 1914 and was concerned with completing his 5-volume *Deutsche Grammatik* (Halle/S.: Max Niemeyer, 1916–1920) to the best of his abilities.

20. In the third edition of 1898, the title of the book was changed from *Principien* to *Prinzipien* as in fact the entire text was changed to modern standard orthography with, *inter alia*, nouns beginning with a capital letter.

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> dem Gebiete der historischen Grammatik im engeren Sinne hat man die selbe Art des Vergleichens angewandt: man hat deskriptive Grammatiken verschiedener Perioden aneinandergereiht. (1909: 23 = 1880: 26–27; italics for spread printing in the original)
>
> [*Historical grammar* has its origin in the older, merely *descriptive* grammar, and still has kept much of it. At least in the summarizing presentation it [i.e., historical grammar] has maintained the old form. It only has placed a series of descriptive grammars side by side. Comparison, not the demonstration of development was originally conceived of as the proper trait of the new science. *Comparative grammar*, which concerns itself with reciprocal relationship of related languages whose common source had been lost to us, has been contrasted with historical grammar which pursues the further development from a point that is given by transmission. […]. Even in historical grammar in the narrow sense of the term the same kind of comparison has been employed: descriptive grammars of different periods were placed in sequence.]

We may conclude from this passage: (1) historical grammar is epistemologically identical with comparative grammar since it compares various stages of a given language in order to account for its changes through time; (2) historical grammar stands in opposition to 'merely descriptive grammar' which indeed provides the necessary prerequisite for the historical treatment of language in as far as the language historian has to first establish at least two different periods of a language before he can account for the changes it underwent through time.

Paul recognizes the methodological difference between comparative and historical grammar in that comparative studies arrange grammars of different languages in parallel ('parallel aneinander gefügt'), whereas historical studies arrange them in series ('aneinander gereiht'). The difference would have been better described had Paul spoken of a chronological or vertical order in the latter case since the time factor (frequently disregarded by the early comparativists, notably by Bopp) plays an important role in historical linguistics. But inaccuracies of expression are frequently to be met with in Paul's theoretical argument.[21] One would not be surprised if Saussure's 'Methodenklage' (thus Karl Bühler in 1934) in his letter to Meillet of 4 November 1894 had not something to do with this, when he writes:

> Sans cesse l'ineptie absolue de la terminologie courante, la nécessité de la reforme, et de montrer pour cela quelle espèce d'objet est la langue en général, vient de gâter mon plaisir historique, quoique je n'aie pas de plus cher vœu que de n'avoir pas à m'occuper de la langue en général.

21. Indeed, as Clemens Knobloch (1997) – and others before him – has shown, Paul was indeed a poor theoretician-philosopher but that his empiricist outlook and his linguistic practice assured his legacy.

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> Cela finira malgré moi par un livre où, sans enthousiame ni passion, j'expliquerai pourquoi il n'y a pas un seul terme employé en linguistique auquel j'accorde un sens quelconque. Et ce n'est qu'après cela, je l'avoue, que je pourrai reprendre mon travail au point où je l'avais laissé. (Saussure 1964: 95)[22]
>
> [All the time this absolute inaptitude of current terminology, the necessity to reform it, and in order to do so to show what kind of object language in general is, is spoiling my historical pleasure, even though I do wish not to have to occupy myself with language in general.
>
> It will, despite myself, end up in a book wherein, without enthusiasm or passion, I shall explain why there is no single term in linguistics to which I attach a particular meaning. Only after [having done] this, I admit, could I return to my work at the point where I had left it.]

Since he was convinced that the historical treatment of language was the sole scientifically valid approach, Paul felt the necessity of arguing in favour of a 'science of principles' ("Prinzipienwissenschaft") which is concerned 'with the general conditions of life of the historically evolving object' and which 'investigates the nature and effects of factors constantly prevailing during all changes' (1909 [= 1880]: 1). But he posits this general science as an ancillary to historical linguistics, at the same time denying that the historical and the empirical viewpoint oppose each other: "der eine ist gerade so empirisch wie der andere" (ibid.).[23] In fact, Paul attempts to bridge the opposing standpoints of the historical (i.e., also empirical) and the pure sciences which aim at formulating laws by introducing a 'doctrine of principles'

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22. We know that Saussure did not write that book, but we know also that when in December 1906 he was asked by the administration of the University of Geneva to lecture on Indo-European philology and general linguistics, he went back to his reflections and, I presume, some of his notes of the early 1890s (cf. *SM* 30–33). In fact, in 1996 manuscripts of Saussure's devoted to general linguistics were found by repair men behind the heating system of the family mansion's orangery, a portion of which was transcribed by Rudolf Engler and published in *Cahiers Ferdinand de Saussure* 50.202–205 (1997) and in full in Saussure (2002: 17–97, 127–135, 285–336). This volume contains, apart from the texts of Saussure's first three (apparently public) lectures at the University of Geneva in November 1891 (Saussure 2002 = *ELG* 143–173), also notes for a book that Saussure intended to write during 1893–1894 (*ELG* 197–202, 222–235).

23. This perhaps somewhat surprising affirmation reveals Paul's conception of history which in 19th-century linguistics had quite a different meaning from what it now has. Zsigmond Telegdi, in his paper "Struktur und Geschichte: Zur Auffassung ihres Verhältnisses in der Sprachwissenschaft", *Acta Linguistica Hungarica* 17.223–243 (1967), noted that 'Historie' and 'Empirie' were regarded as identical since antiquity (pp. 226–227). Siegfried Heinimann ("Zur Auffassung des Geschichtlichen in der historischen Grammatik des 19. Jahrhunderts", *Festgabe Hans von Greyerz* [Bern: H. Lang, 1967], 783–807) traces this particular interpretation of 'historical' by the Neogrammarians back to the Darwinian evolutionalist concept of language development as put forward by Schleicher (pp. 799–800). Cf. Paul's reference to Darwin (1909: 32 = 1886: 30).

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("Prinzipienlehre") which is capable of demonstrating the interaction of the various forces operating within language (1909: 2 = 1880: 3). He hopes to deduce these general principles from the nature of the historical development itself as far as they are not of purely logical origin (1909: 5 = 1880: 6). There seems to be little doubt that Paul's ideas must have given rise to a number of definite theoretical conclusions to anyone with a philosophical mind and a special interest in general theory of language. For the post-Saussurean linguist the concept of a general science which formulates principles but which is subordinate to a particular science does not appear acceptable, and a reversal of their relationship would seem logical. It is with this caveat in mind that we may parallel Paul's notions[24] of

allgemeine Prinzipienwissenschaft { *deskriptive Grammatik* / *historische Grammatik*

with Saussure's concepts of

linguistique générale { *linguistique statique (synchronie)* / *linguistique évolutive (diachronie)*

Both linguists acknowledge the primacy of the descriptive approach over the historical one, although Paul contradicts himself on this point on several occasions and does not take his affirmations to their logical conclusion.[25]

As everyone in linguistics knows, Saussure's concept of synchrony is tied up with two other important assertions, i.e., the dichotomy of *langue* versus *parole* (cf. *CLG* 31–32, 36–39) and the notion of *état de langue* (*CLG* 142–143). In fact the distinction between synchrony and diachrony presupposes that between *langue* and *parole*, and since *langue*, the underlying system of speech, is, according to Saussure, the proper object of linguistics, its operation can only be discovered (more or less arbitrarily) by setting up a period of a given language in which the time factor can be disregarded or eliminated. We thus would have to do with a language state.

The first quotation above from Paul's *Prinzipien* of 1880 reveals very clearly that the author was aware of the necessary prerequisite of establishing a 'certain average

24. This is, admittedly, an oversimplification. As Einhauser (2001) has shown, Paul attempted an overall edifice of the philosophy of science in which to integrate his principles of language history (Paul 1880: 7–9 = 1909: 5–8); cf. her diagram (Einhauser 2001: 1347).

25. Kandler (1914–1984) appears to have been the first to point to Paul's non-atomistic concept of language, admonishing the reader to study Paul's work very closely in order to recognize that he did not reject the idea of describing language states. Kandler (1954: 15) argued that the real contrast between Saussure and Paul lies in the fact that 'Paul isn't inclined to believe in the 'state' of a language in the sense of a unified, well-articlated system' ("Paul ist nicht geneigt, an den 'Zustand' einer Sprache im Sinne eines einheitlichen, wohlgegliederten Systems zu glauben").

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of a given period' in order to set up 'for each period of development of a language an essentially and generally valid system of [linguistic] arrangement' (1909: 189 = 1880: 78). Significantly, Paul had observed in 1880 that it is much more fruitful to investigate the most recent periods of a given language since the data to work on would be larger and more easily accessible, and this particularly for the following reason:

> Der *gegenwärtige sprachzustand*, welcher unmittelbar und vollständig zu beobachten ist, wirft natürlich das meiste licht zurück auf die zunächst vorhergehenden stadien der entwickelung. (1880: 25; my italics: EFKK)[26]
>
> [The present state of language, which can be observed directly and fully, naturally casts the strongest light on the immediately preceding stages of development.]

Therefore, Paul proposes to illustrate his theoretical views with the help of examples taken from Modern German and (without actually acknowledging this procedure) treats general aspects of linguistic method as well as 'synchronic' matters of language first before discussing questions of linguistic change (which take into account earlier periods of language).

Paul's often-cited insistance in the second edition of the *Prinzipien* (1886: 19 = 1909: 20) that there is no other scientific treatment of language apart from the historical approach had been prompted by Franz Misteli's (1841–1903) criticism.[27] In fact the most essential changes of emphasis in Paul's argument are to be found in this edition. Expatiating on his views concerning the exclusive validity of the historical treatment of linguistic phenomena Paul concedes, after having asserted his position as language historian:

> *Der Beschreibung von Zuständen* wird er nicht entraten können, da er es mit grossen Komplexen von gleichzeitig neben einander liegenden Elementen zu tun hat. Soll aber diese Beschreibung eine wirklich brauchbare Unterlage für die historische Betrachtung werden, so muss sie sich an die realen Objekte halten, d.h. an die eben geschilderten psychischen Organismen. Sie muss […] uns zeigen, wie sich das Sprachgefühl verhält. (1909: 29 = 1886: 26; emphasis in the original)
>
> [He cannot do without *the description of states* since he has to do with a large number of complexes of elements that lie simultaneously next to one another. In order for this description to serve as a truly useful basis for historical analysis, it must conform to the real objects, i.e. the psychical organisms just illustrated. It must […] show us how the language instinct behaves.]

---

26. Note that this passage was omitted in the later editions, although the concept and term 'Sprachzustand' was not (see further below).

27. There is, however, a passage in the first edition already in which Paul speaks of "Veränderungen des ursprünglichen Sprachzustandes" (1880: 238 = 1909: 43).

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And when Paul points out that such a description of a language state ('Sprachzustand'), 'which could serve as a useful basis for historical research', is by no means an easy task (1909: 31 = 1886: 28), we may recall Saussure's affirmation that descriptive linguistics is in fact quite difficult, and that by comparison with it historical linguistics is fun ("la linguistique evolutive est amusante").[28] In the same paragraph Paul notes that previous knowledge of an earlier or later language state or knowledge of another related language tends to dull the picture; careful consideration should therefore be given to the elimination of all these interferences and hasty conclusions must be avoided.

Saussure appears to have made the following affirmation in his lectures on general linguistics:

> La première chose qui frappe quand on étudie les faits de langue, c'est que pour le sujet parlant leur succession dans le temps est inexistante: il est devant un état. Aussi le linguiste qui veut comprendre cet état doit-il faire table rase de tout ce qui l'a produit et ignorer la diachronie. (*CLG* 117; cf. *CLG/E* 181)
>
> [The first thing that meets the eye when one studies the facts of language is that for the speaker their temporal succession is non-existant: he finds himself in front of a state. Likewise, the linguist who wishes to understand this state, must remove completely everything that has produced it and ignore diachrony.]

Saussure added that the intervention of historical considerations could only warp the linguist's judgment. In the second edition of the *Prinzipien* Paul, continuing his argument in the paragraph following the last quotation, makes a clear statement regarding the logical and methodological precedence of descriptive linguistics over historical linguistics:

> Ist die Beschreibung verschiedener Epochen einer Sprache nach unseren Forderungen eingerichtet, so ist damit eine Bedingung erfüllt, wodurch es möglich wird sich aus der Vergleichung der verschiedenen Beschreibungen eine Vorstellung von den stattgehabten Vorgängen zu bilden. (1909: 31 = 1886: 29)
>
> [Once the description of different epochs of a language has been established in accordance with our demands, the condition will have been fulfilled whereby it becomes possible, through the comparison of the different descriptions, to obtain an idea of the processes that have taken place.]

A clearer statement, I believe, can hardly be found in the linguistic literature of 1886 (when Paul inserted it in the second edition), and a comparison with the affirmations in Gabelentz's work of 1891, which Coseriu (1967: 79–86) tried to interpret in

28. Cf. *SM* 88 (Ms. Georges Dégallier, p. 259); see also *SM* 30 (A. Riedlinger's report of an interview with Saussure in January 1909), and (*CLG* 141; *CLG/E* 228).

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support of his claim that Gabelentz was a prime source of Saussure's inspiration and does not turn up anything which could even come close to that of Paul. In fact, Paul links his argument clearly with the second concept which, besides that of 'language state', renders the distinction between synchrony and diachrony theoretically valid: the dichotomy of language versus speech (cf. 4.2 below).

In addition, Paul observed in the same edition of the *Prinzipien* what the linguist of today would ascribe to Saussure without hesitation:

> Sehr leicht wird das Bild eines bestimmten Sprachzustandes getrübt, wenn dem Betrachter eine nahe verwandte Sprache oder eine altere oder jüngere Entwicklungsstufe bekannt ist. Da ist die grösste Sorgfalt erforderlich, dass sich nichts Fremdartiges einmische. Nach dieser Seite hin hat gerade die historische Sprachforschung viel gesündigt, indem sie das, was sie aus der Erforschung des älteren Sprachzustandes abstrahiert hat, einfach auf den jüngeren übertragen hat. (1909: 31 = 1886: 29)
>
> [Very easily could the picture of a specific language state be blurred, if the observer is familiar with a related language or an older or more recent stage of development. In such a case, great care must be taken that nothing extraneous interferes. Here historical linguistics especially has committed a sin in that it transferred into younger stages of language what it had drawn from research of older stages.]

To conclude, we could not think of a clearer pronouncement concerning the necessity of ignoring other stages of a language when describing a given language state. Moreover, Paul's 1886 criticism of linguists who did not discern neatly between various language stages would seem to make Saussure's later critique look somewhat anachronistic, had Paul's contemporaries read his work more attentively.

It should be kept in mind however that Paul, advocating the 'historical' treatment of language over and above the 'merely descriptive' approach (cf. 1909: 23 = 1880: 27), did not draw the same conclusions as post-Saussurean linguists would be tempted to do. The apparent underestimation of the descriptive aspect in Paul's argument was laid bare at the turn of the century by Ottmar Dittrich (1865–1951), a psychologist and philosopher of language and pupil of Wundt (to whose criticism Paul replied in the fourth edition of the *Prinzipien*). Dittrich's comments appeared first in a review of the third edition of Paul's work (1898), then more explicitly in his own *Grundzüge der Psychologie* of 1903.[29] It would go beyond the scope of the present discussion to outline Dittrich's criticism of Paul (see Koerner 1971: 111–112, for details); suffice it to note that Dittrich termed Paul's analytic procedures and the

29. Cf. *Zeitschrift für romanische Philologie* 23.538–553 (1899) and *Grundzüge* (Halle/S.: Max Niemeyer, 1903); see especially the introductory chapter, "Sprachpsychologie und Sprachwissenschaft" (5–63 *passim*), which is almost exclusively devoted to a critical discussion of Paul's contentions about the scientific investigation of language.

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argument underlying them throughout his book as 'blazing proof against the narrow limitation of linguistics' ("ein flammender Beweis gegen die enge Umgrenzung der Sprachwissenschaft"), i.e., Paul's identification of linguistic science exclusively with historical linguistics.[30]

Paul's theoretical argument concerning the relation between descriptive and historical grammar can be summarized as follows:

1. There is a methodological and epistemological distinction between these two approaches to languages (1909: 23–24 = 1886: 21–22).
2. There is an acknowledgement that descriptive grammar has to do with a given language state (p. 31 = 1880: 28), which however remains difficult to establish.
3. Yet once such "Beschreibungen verschiedener Epochen einer Sprache", i.e., the description of different periods of a language, in accordance with his strict requirements have been executed, a comparison between those different states would give the linguist an idea of past events (ibid.).
4. The practical value of descriptive work is acknowledged since the previous establishment and comparison of two or more stages constitute the prerequisite in order to make valid statements about language change (29–32 = 1880: 28–30).

Despite all the talk of "Beschreibung von Sprachzuständen", i.e., the description of language states, Paul remains adamant that this activity is of heuristic value only, and that the true task of the linguist remains the investigation of language change, which means the observation of the speech of the individual, since all change emanates from the interaction between individual speakers (1909: 32–34 = 1880: 29–31).[31]

It may already be pointed out at this stage of the inquiry that these affirmations of Paul, already contained in the first edition of the *Prinzipien* but argued more forcefully in the second of 1886 (cf. 1909: 20–32), could not have failed to arouse

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30. *Grundzüge*, p. 6. – When Dittrich distinguishes between 'Syn-' or 'Metachronismus' and 'synchronistische Grammatik', on the one hand, and 'Auto-' or 'Heteronomie' of linguistic phenomena, on the other, he does not, as Zwirner (1969: 38) claimed, anticipate Saussure's terms of synchrony and diachrony since Saussure defined as early as 1894 or thereabouts 'diachronique' as "opposé à synchronique ou idiosynchronique" (see *SM* 49).

31. This Paul illustrates amply in his chapter "Analogie" (1909: 106–120 = 1886: 85–98), which would be interesting to compare with Saussure's treatment of the concept (*CLG* 221–237; *CLG/E* 365–399, based almost exclusively on Riedlinger's notes from the 1907 lecture series), but would go beyond the present chapter. Given the many examples taken from German, such a comparison could probably prove what Rudolf Engler communicated to me in a personal letter sometime in 2000 (if memory serves me well, since large portions of this correspondence got lost when I relocated from Ottawa to Germany in the summer of 2002), namely, that he had found a handwritten note from Saussure in which he stated that a copy of Paul's book was 'always nearby'. Unfortunately, to the best of my knowledge, this note has not yet made its reappearance.

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opposition among language theoreticians whose philosophical background differed from those of the positivistic tradition that inspired Paul. His obvious contradictions concerning the validity of descriptive grammar could not have been overlooked by Saussure as they had been subjected to scrutiny by Misteli, Tobler, and others.[32] Saussure's conclusions, as is well known, have been quite contrary to Paul's contentions. Saussure emphasized the importance of a descriptive linguistics and its validity over and above the historical treatment of language to the extent that 20th-century readers of the *Cours* believed that the two could be treated as independent branches of linguistic research. It may well be asked whether Paul's statements, which were so influential in the linguistics of his time,[33] did not provoke Saussure, with the result that the manner in which his theory of synchronic linguistics was presented in at least the "vulgata" text of the *Cours* tended to be regarded as the sole truly scientific approach. The apparent denial of any dynamic aspect to descriptive linguistics was to be criticized by members of the Prague school from the late 1920s onwards.[34]

When questioned by his most assiduous student Albert Riedlinger (1883–1978) in a January 1909 interview after his lectures on 'synchronic linguistics', Saussure responded revealingly:

> Il n'y a pas de sujet plus ardu que celuilà: il faudrait reprendre, pour le réfuter, tout ce que Paul et les modernes ont écrit là-dessus. […] La meilleure manière de procéder serait de prendre les expressions dont se servent les bons linguistes quand ils parlent de phénomènes statiques, et de voir les *erreurs* et les *illusions* qu'elles contiennent. (Godel 1957: 29; emphasis in the original)
>
> [There is nothing more tough than that: one would have to retake, in order to refute, everything that Paul and the moderns have written on this. […] The best manner of conduct would be to take the expressions used by the good linguist when they speak of static phenomena, and to see the *errors* and the *illusions* they contain.]

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32. Cf., for example, Mikołaj Kruszewski's (1851–1887) critique *Über die Lautabwechslung* (Kazan: Universitätsverlag, 1881), pp. 3–4, of which he had sent Saussure a copy with the dedication 'A Mr le professeur Ferdinand De Saussure' (see Kruszewski [1995: 3] for a reproduction of the front cover).

33. Even after the publication of the 2nd ed. of the *Cours* in 1922, a number of linguists reaffirmed Paul's position; e.g., Friedrich Schürr in *Deutsche Vierteljahrsschrift* 1.482 and 485–486 (1923); Hermann Güntert, *Grundfragen der Sprachwissenschaft* (Leipzig, Quelle & Meyer, 1925), pp. 129–130, and Eugen Lerch. *Historische französische Syntax*, vol. I (Leipzig, O. Reisland, 1925), p. 11. Cf. also Jespersen's affirmation made right at the beginning of the preface to his book *Language*: "The distinctve feature of the science of language as conceived nowaydays is its historical character […]. This manner of viewing languages constitutes a decisive improvement on the way in which languages were dealt with in previous centuries" (1922: 7).

34. This suggests that the reception of the *Cours* by the scholarly community did take a number of years, in a number of places even decades.

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In other words, Saussure conceded that the dealing with 'la linguistique synchronique' or 'statique', as he termed it until his third lecture series of 1910–1911, was difficult, and that the best approach would be to study the works of Hermann Paul and other "good" linguists and show how they contradict themselves in order to refute their arguments. Here is, I believe, direct evidence that Hermann Paul's *Prinzipien* was Saussure's prime target.

4.2 Language custom vs. individual speech act

In accordance with the purpose of his *Prinzipien* Paul wishes to discover the laws governing linguistic change or what he calls the change of language custom ('Sprachusus'). Its cause has, according to Paul, to be found in the speech activity of the individual:

> *Die eigentliche Ursache für die Veränderung des Usus ist nichts anderes als die gewöhnliche Sprechtätigkeit.* (1909: 32 = 1886: 229; italics for spaced printing in the original)
>
> [*The real cause of change in custom is nothing else than common speech activity.*]

Paul's emphasis on direct observation and his apparent aversion to abstractions lead him to be primarily concerned with the activity of the individual speaker and his interaction with other speakers, in contrast to the study of the language of a given speech community. This seems to be in contradiction to his affirmation that linguistics is concerned with language custom, "mit den allgemein usuell feststehenden Verhältnissen der Sprache", whereas philology directs its attention to the use that the individual makes of language (1909: 33, note = 1886: 30, note), since he points out – with a reference to Brugmann (1885) – that the transformation of language custom *could* not be rightly understood without "ein Studium der individuellen Sprechtätigkeit" (ibid.). In fact, Paul strongly opposed Wilhelm Wundt's (1832–1920) conception of national psychology ('Völkerpsychologie') in the fourth edition of the *Prinzipien* (cf. 1909: v–vii), having already claimed in the first edition that Johann Friedrich Herbart's (1776–1841) 'individual psychology' is the only appropriate kind of psychology.[35] As we have already seen, Paul accused descriptive grammar of having contented itself with analyzing grammatical

35. Paul stated in 1880: 'If we conceive of Herbart's psychology as the science of the behavior of conceptions to one another, there can only be a psychology of the individual, which should not be placed in opposition to a social psychology' ("Fassen wir, […] die psychologie im Herbartschen sinne als die wissenschaft von dem verhalten der vorstellungen zu einander, so kann es nur eine individuelle psychologie geben, der man keine völkerpsychologie oder wie man es sonst nennen mag gegenüber stellen darf." [pp. 14–15 = 1909: 12–13])

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forms and their relations of a certain period and of having dealt with abstractions rather than with linguistic facts. The true object of the linguist, he maintained, "*sind vielmehr sämtliche Äusserungen der Sprechtätigkeit an sämtlichen Individuen in ihrer Wechselwirkung aufeinander*" (1909: 24 = 1880: 22; emphasis in the original). Paul is quite aware of the fact that such a goal is not quite attainable, but he believes that such an ideal should be kept in mind while analyzing language (ibid.).

Concerning the relation between individual speech and the language of a community Paul states that there are in reality as many dialects (we would nowadays speak of 'idiolects') as there are speakers (cf. 1909: 38 = 1880: 232). The procedures advocated by Paul consist of introspection, analysis of one's own *Sprachgefühl*, and, wherever possible, observation of living individuals (1909: 30 = 1880: 28). Paul asserts that every linguistic creation is the work of an individual (1909: 18 = 1880: 17), and maintains that (as quoted in the preceding paragraph) the true object of linguistic investigation is '*the totality of manifestations of speech activity in all individuals in their reciprocal interaction*' (p. 24 = 1880: 22; emphasis in the original). Therefore, Paul argues, the description of language, in order to serve as a useful foundation for historical treatment, must not only list all its individual constituents but depict 'the relation of the elements to each other, their relative strengths, the connections into which they enter, the degree of closeness and strength of these connections' (p. 29= 1880: 26).

Everyone reading Paul's *exposé* will suspect that such a program is quite impracticable, and that Paul's empirical approach, which starts from the investigation of the individual linguistic expression and the observation of the interaction of the speech acts of various individuals before these are related to the abstract concept of *Sprachusus*, would seem to add to this difficulty. When he affirms (in line with his empiricist bias) that even the 'idiolect' is subject to change and cannot be regarded as a constant quantity (pp. 39–40 = 1880: 37), Paul takes away any possible basis for a general theory of language. However, he attempts to bridge the opposition between the individual and the speech community by positing a kind of constant interaction between these two entities on the basis of the intrinsic social nature of language. In Paul's view linguistics, as Whitney had pointed out before him,[36] is a moral science ('Kulturwissenschaft') and for Paul this meant further that it is always a social science (1909: 7 = 1880: 9), it being solely through social intercourse ("Verkehr") that speech is produced (p. 39 = 1880: 37). Indeed it is one of the innermost characteristics of language to be a means of communication, a 'Verkehrsmittel' (p. 58 [= 1880: 51). There is a constant interaction between the individual, who

36. Cf. *CLG*, 26; *SM*, 43, 143, 158, and elsewhere. See also Karl Brugmann's revealing outline of the main principles of Whitney's linguistic theory in his commemorative paper, "Zum Gedächtniss W. D. Whitney's", *Journal of the American Oriental Society* 19.74–81 (1897 [1894]).

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tends to be creative and introduces variations, and the 'association of traffic/intercourse' ("Verkehrsgenossenschaft"), which permits changes of the language custom only to a certain degree. Linguistic changes are therefore not only due to the spontaneity of the individual but also regulated by the social constraint exercised on the individual who wants to be understood, a regulatory principle which Paul calls (p. 61 = 1880: 53) the 'constraint exercised by the society of intercourse' ("Zwang zur Verkehrsgemeinschaft").[37]

Despite these affirmations Paul insists that the language of the speech community is nothing but an artifact of the linguist: The language custom ('Sprachusus') arrived at by the analyst through comparison of a number of 'idiolects' is an abstraction and has no existence of its own (1909: 37–38 = 1880: 232); to call it a linguistic average would not make it less hypothetical (cf. p. 29 [= 1886: 26–27]). We may conclude that language custom ("Sprachusus") – a term which Paul nowhere defines, apart from stating that it would be "das eigentlich Normale", the normal properly speaking (p. 29 = 1886: 27) – is derivative, vague, and not directly accessible to the investigator, in contrast to the language of the individual, for which Paul claims so much theoretical significance.[38] Paul emphasizes his views by denying (1909: 404 = 1880: 266) a real existence to 'what descriptive grammar calls a language by summarizing what is [taken to represent] the common' ("was die deskriptive Grammatik eine Sprache nennt, mit der Zusammenfassung des Usuellen").

Saussure, on the other hand, affirmed that *langue* is no less than *parole* "un objet de nature concrète" (*CLG* 32; see also *CLG/E* 44; *SM* 157), and contended that these two could be studied separately (p. 31), although he also stated that 'a kind of average' ("une sorte de moyenne") would establish itself among the individual speakers of a given language (p. 29), an observation strikingly similar to Paul's suggestion that 'a certain average' ("ein gewisser Durchschnitt") could be deduced from the observation of individuals in order to define the language custom (1909: 29 = 1886: 27). Further, Saussure appears to echo Paul when he states:

> Entre tous les individus ainsi relies par le [the social nature of] langage, il s'établira une sorte de moyenne: tous reproduiront, – non pas exactement sans doute, mais approximativement – les mêmes signes unis aux mêmes concepts" (*CLG* 29; *CLG/E* 39)

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37. Much more likely in Paul's observations of 1880 and 1886, and not in Durkheim's expositions on 'contrainte sociale' of 1901, would we possibly find *the* source of Saussure's insistence on 'langue' as a 'fait social'. Indeed, where the "vulgate" text speaks of "la contrainte de l'usage collectif" (p. 131), all student notes have "un caractère impératif" (*CLG/E* 206).

38. This view appears contradicted by Paul's useful terminological distinction between 'usuell' and 'okkasionell' (1909: 74ff. = 1886: 66ff.) when dealing with semantic change.

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> [Between the individuals tied to one another this way by [the social nature of] language a kind of average will be established: everyone reproduces – not exactly, no doubt, but approximately – the same signs united with the same concepts.]

Saussure regards, it would seem, heterogeneity within the 'Sprachusus' as no serious barrier to studying language, much in conformity with Paul's view (1909: 19 = 1880: 22; emphasis in the original) that the 'great unifornity of all linguistic processes among different individuals contitutes the essential basis for exact scientific information' ("[d]ie *grosse Gleichmässigkeit aller sprachlichen Vorgänge in den verschiedenen Individuen ist die wesentliche Basis für eine exakt wissenschaftliche Erkenntnis derselben*").

Otherwise Saussure, who emphasized the importance of descriptive linguistics in contrast to the historical treatment of language prevailing in his time, took the opposite view when he introduced his influential *langue/ parole* dichotomy in the second course on general linguistics in 1908 (cf. *SM* 147ff.). According to Saussure language is not complete in the individual but exists perfectly in the multitude only (*CLG* 30). He concedes that it is *parole* which initiates language evolution (p. 37), but he is confident that *langue* and *parole* could be separate objects of linguistic research. Godel (1966: 481) has pointed out that Saussure's attitude towards language was a philosophical rather than, say, an empirical one (as was typical of Paul). Saussure, characteristically, while admitting that *langue* was not the "phénomène initial" (*CLG/E* 57) in language, did argue that, for the theorist, *langue* precedes *parole* and not vice versa (cf. *SM* 149). In his words the *langue/parole* distinction has to be regarded as the 'first truth' about the nature of language (cf. *SM* 30) which makes a general theory possible; according to Saussure – at least in Bally and Sechehaye's interpretation – linguistics proper is therefore concerned with *langue* (*CLG* 39; cf. *CLG/E* 58).

Saussure must have seen the difficulties which Paul was faced with when he tried to make the investigation of the individuals' linguistic expression the object of linguistics in the hope of attaining from there an 'average' to define the *Sprachusus* (1909: 29 = 1886: 27). It is therefore not surprising that Saussure never provided a linguistics of *parole* as the editors of the *Cours* noted with regret (*CLG* 197, note).

Despite the fact that Saussure, in a 'rationalist' fashion, took the opposite stand regarding the importance of the language system ('code de langue') agreed upon by the linguistic community (cf. *CLG* 31, 47, 107; *CLG/E* 41, 74), in contrast to the linguistic expression of the individual, I believe that, once again, Paul was a valuable source of Saussure's inspiration, though essentially *ex negativo* (*pace* Antal 1985: 128). When Saussure affirms that *langue* is independent of individuals (*CLG* 37; *CLG/E* 56) and also external to them (*CLG* 30, but not supported by *CLG/E* 42), he seems to be reacting against Paul's contention that language can only be observed in the individual speaker.

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Yet aspects of Paul's theoretical argument could have supported Saussure's attempt to place his *langue/parole* distinction on a firm epistemological basis. When Saussure tried to clarify this dichotomy by affirming that *parole* pertains to the individual aspect of language in general (*langage*) whereas *langue* was to be regarded as the social fact (*SM* 149ff.; *CLG* 112), there is no evidence that he was influenced by Durkheimian sociology as has been suggested frequently in the histories of linguistics (e.g., Dinneen 1967: 191–195; Robins 1995 [1967]: 225; Antal 1985: 127–128). Knowing Paul's work as he did, however, he was familiar with the following related distinctions made by Paul:

1. the distinction between 'individal speech actity' ("individuelle Sprechtätigkeit") and 'language custom' ("Sprachusus") (1909: 32–33 = 1886: 29–30) which Paul paralleled with
2. the distinction between 'okkasionell' and 'usuell' (when dealing with questions of semantic change [1909: 75 = 1886: 67]);
3. the distinction between the 'laguage of the individual' ("Individualsprache") and 'common language' ("Gemeinsprache") (1909: 404ff. = 1880: 266ff.), and
4. from an extra-linguistic point of view, the opposition between the 'individual' ("der Einzelne") and the 'community of intercourse" ("Verkehrsgemeinschaft") (see 1909: 39ff. = 1880: 234ff.) or 'language fellowship' ("Sprachgenossenschaft" (p. 24 = 1880: 27).

It was argued by Coseriu (1967: 76–80) that Saussure was particularly influenced by Gabelentz's tripartition of 'Rede', 'Einzelsprache', and 'Sprachvermögen', which Coseriu conveniently parallels with Saussure's concepts of *parole, langue*, and *(faculté du) langage* (cf. *SM* 147ff.). A closer inspection of Gabelentz's terms and a comparison of them with what Saussure had in mind reveal important semantic differences which contradict Coseriu's contention. First, the intellectual background of the two linguists' theories were quite dissimilar; Gabelentz followed the Humboldtian tradition in which language was regarded as a creative force (*energeia*) and the static aspect (*ergon*) of language was rejected.[39] That neither *langue* nor *parole* corresponds to the *energeia* concept had, I believe, convincingly been demonstrated by Mueller (1966: 99–102). Secondly, because of the emphasis on the reproductive aspect of language Gabelentz proposed to investigate 'parole' rather than 'langue' (Coseriu 1967: 79), a position that brings him closer to Paul's position. Thirdly, according to Gabelentz, the object of general linguistics ("allgemeine Sprachwissenschaft") is the analysis of the 'Sprachvermögen', i.e., the

39. It is true that Paul (1909: 109–110 = 1880: 68–69) emphasized, with reference to Humboldt, the creative aspect of speech when discussing the phenomenon of analogy. However, in general terms, Humboldt's influence on Paul is fairly marginal.

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language faculty (Gabelentz 1901: 302ff. = 1891: 292ff.), in contrast to Saussure's affirmation that *langue* is the proper object of general linguistics; *langage* or *faculté du langage*, however, cannot, in Saussure's terms, be appropriately studied by the linguist (*CLG* 25–27; cf. *CLG/E* 36).[40] Fourthly, Saussure's concept of *langue* is defined in terms of a system of (arbitrary) signs (*CLG* 32, 33, 106), a code on the basis of which mutual understanding is made possible (*CLG* 31, 47, 107; *CLG/E* 42, 74, 164); nothing in Gabelentz's *Sprachwissenschaft* corresponds to this semiotic outlook. Fifthly, and I believe that a number of further points in support of my refutation of the principal implications of Coseriu's suggestions can be found, Coseriu's analysis of Gabelentz's conception of 'Rede' (1967: 78ff.) can much more easily be paralleled with Paul's affirmation concerning the nature of the 'individuelle Sprechtätigkeit' than with anything in Saussure; indeed, it would be quite revealing to investigate in some detail how many ideas in Gabelentz's work were anticipated in Paul's *Prinzipien*.[41]

4.3 Formal and material connections between words

It has to be recalled that during the last quarter of the 19th-century psychology became a very powerful branch of research and speculation within the humanities; linguistics, which generally tends to reflect the intellectual climate of its time, was strongly influenced by the psychologism of the period (cf. also Knobloch 1997).[42] Saussure himself could not escape its pervasiveness and discussing the relation

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40. In a 1966 paper R. Engler pointed out that certain ternary terminological groupings in Saussure, such as for example the tripartition of *langage, langue* and *parole*, "sont volontiers reduits à deux termes" (p. 37). – The reader of the *Cours* will notice that (*faculté du*) *langage* is mentioned only *en passant* and does not play any significant role in Saussure's linguistic argument. Extensive students' notes from Saussure's third course (1910/11) illustrate the Genevan linguist's position: "Chez chaque individu, [il y a une] faculté du langage articulé, mais cette faculté ne pourrait être mise en jeu, si le corps social ne donnait à l'individu le moyen de l'exercer: la langue" (*CLG/E* 31). And: "Langage: phénomène non seulement très complexe, mais multiforme et hétéroclite dans ses différents aspects. On n'arrive pas à classer le langage dans les faits humains" (p. 32).

41. In fact, if we read affirmations like "Die menschliche Sprache ist ihrem Wesen nach Verkehrsmittel [human speech is by its nature a means of communication]" (Gabelentz 1901: 319 = 1891: 310), we notice a much greater affinity between Gabelentz and Paul than between Gabelentz and Saussure.

42. On this subject generally, see Knobloch (1988); it is strange, however, that while Steinthal, Wundt, and others are given broad treatment in the 560-page study, Paul's *Prinzipien* is not even included in the bibliography; in the section devoted to the treatment of the importance of "Verkehr und Verständigung" ("[social] commerce and communication") in the development of language, the work of Herbart, Madvig, Whitney, and Wegener (with which Paul was familiar) is discussed (161–181), but not Paul's.

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between linguistics and other areas of human curiosity such as ethnography, anthropology, and sociology. Thus, he admitted to the difficulty in separating linguistics from psychology, affirming that after all 'everything in linguistics is psychological, including what is mechanical and material' ("tout est psychologique dans la linguistique, y compris ce qui est mécanique et matériel" [*CLG/E* 21; *CLG* 21]).

An adherent of Herbartian psychology applied to matters of language by Steinthal, Lazarus, Misteli, and others, Paul (1909: 25–28 = 1880: 29–33) believed that individual words attract each other in the human mind forming a number of smaller or larger groups. Paul asserted that a reciprocal attraction is 'always based on partial agreement between sound[s] or meaning[s] or sound and meaning conjointly' (p. 106 = 1886: 85).[43] Paul distinguished between 'material' ("stoffliche") and 'formale Gruppen' of associations which, however, do not necessarily remain separate entities but may combine to form further groups or amalgamate. He exemplifies his ideas in the following manner:

1. Material groups are for instance (a) the various cases of a noun, (b) groups of words showing a correspondence in meaning which may at times be paralleled by formal similarities since an etymological relation is often its basis, (c) those which are connected by semantic opposition only, e.g., *man* vs. *woman, girl* vs. *boy, sister* vs. *brother, small* vs. *big*, and finally (d) words which are related by their contents, e.g., *to die* and *mortal, good* and *better, am, is*, and *was/were*.
2. Formal groups are, according to Paul, 'the sum of all nomina actionis, all comparatives, nominatives, all first persons of verbs, etc.' ("die Summe aller Nomina actionis, aller Komparative, aller Nominative, aller ersten Personen des Verbums etc." [1909: 107 = 1886: 86]). Paul does not however expatiate on his views; he appears to see more interrelations and crossings of groups than distinct formal arrangements when he notes that the material groups 'are as a rule crossed by the formal ones' (ibid.). Formal groups, Paul holds, can be held together by phonetic agreement ("lautliche Übereinstimmung"), e.g., *libro* and *anno, mensae* and *rosae*, but also *gab* and *nahm, bot* and *log*, etc. Although Paul is concerned with different cases of nouns and tenses expressed by the verb, he does not speak of paradigms or paradigmatic relations as the modern linguist might expect.

In the subsequent paragraphs of the *Prinzipien* Paul introduces further distinctions. Thus he speaks of material-formal groups of proportions ("Proportionsgruppen") offering the following examples: *Tag: Tages: Tage = Arm: Armes: Arme = Fisch: Fisches: Fische; führen: Führer: Führung = erziehen: Erzieher: Erziehung*, etc. (1909: 107 = 1880;

43. Saussure spoke of the receptive and coordinative aspects of the human mind (cf. *CLG* 30; *CLG/E* 40); the faculty of *association*, however, which Saussure certainly did not deny, was added by the editors of the *Cours* (see *CLG/E* 39).

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62).[44] Paul later on adds what he terms material-phonetic or etymological-phonetic groups as well as certain syntactic associations (1909: 108–109 = 1886: 87–88). Paul discusses all these various kinds of relations in order to demonstrate his point that there must be some kind of a combinatory activity embedded in the human mind which furnishes these formal, material or formal-material groupings, since it appears doubtful that man is capable of reproducing isolated words and smaller groups of lexical or morphological organization. Paul calls this underlying principle, which permits these various combinations and associations, analogic formation or analogy (1909: 110 = 1886: 89), a principle to which he devotes ample space (pp. 110–120 = 1886: 89–97), also because Paul and the Neogrammarians held this phenomenon to be one of the main causes of linguistic change.

Saussure appears to have made similar observations when he attempted a formal classification of words from an internal point of view (*CLG* 173–175; *CLG/E* 286–289). Mental association, Saussure notes, does not limit itself to connecting only those terms with something in common which can easily be specified but tends to establish almost innumerable kinds of series of relations (*CLG* 173). Consequently, Saussure does not attempt a classification of associative relations but contents himself with presenting a number of examples. Thus, he states that the two words *chapeau* and *hôtel* have no connection whereas a relation between these vocables can be established when we derive from them *chapelier* and *hôtelier*, respectively (*CLG/E* 286). In this case formal properties provide a possibility for association, although Saussure concedes that 'in any form association meaning plays its part ("dans toute association de formes, le sens y joue son rôle" [ibid.]).

Similarly, the word *enseignement* can be associated in the human mind with *enseigner, enseignons*, etc. since they share the same root (*CLG* 173); but *enseignement* could also, on a more formal basis, be connected with *armement, changement*, etc. because of the suffix they have in common (p. 174). In addition, mental associations may spring from certain connections between the 'signifiés' – we would nowadays speak of 'semantic field', e.g., *enseignement, instruction, apprentissage, éducation* (ibid.; cf. *CLG/E* 287).

These few examples illustrate Saussure's views sufficiently for comparison with those put forward by Paul. By contrast with syntagmatic relations which, in Saussure's words, immediately suggest the idea of an order of succession, associative

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44. In the first edition of his *Prinzipien* (not retained in the subsequent editions) Paul spoke of 'parallelreihen' and noted: "Es besteht also eine analogie zwischen den reihen, die sich durch die mathematische formel einer proportion ausdrücken lässt: a : b = α : β, und dem gemäβs auch a : α = b : β" (1880: 63; spaced print in the original). However, Paul continued to speak of 'Proportionengruppen', 'Proportionengleichungen' and the like (cf., e.g., 1909: 107–109 = 1886: 86–88). Bouquet (1997: 116–117) notes that Saussure referred to Paul's idea of a 'quatrième proportionnelle' as early as in the second of his three 1891 public lectures.

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relations appear quite undetermined and the order in which they occur is much less predictable (*CLG* 174; *CLG/E* 288). As the basis of their argument is essentially psychological, it is not surprising that neither Saussure nor Paul is able to present, from a strictly linguistic point of view, the categories involved in these relations.

From what Saussure has said, it appears to be possible to discern three kinds of associative relations. There are words which share the same morphological or etymological characteristic; e.g., *hôtelier* and *chapelier* or *hôtel* and *hôtelier* and words which are semantically related, e.g., *enseignement, instruction*, Saussure notes that there can also be simultaneously a similarity of form and meaning (*CLG/E* 287). This tripartition corresponds clearly to Paul's distinction between *stoffliche, formale* and *stofflich-formale Gruppen*.

As Paul had done before him (1909: 114ff. = 1886: 93ff.), Saussure appears to have referred to the aspect of associative (as well as syntagmatic) relations when discussing the principle of analogy as the predominant creative force in language (*CLG* 227; cf. *CLG/E* 374).[45] If this could be established, it would lend support to my claim that Paul's *Prinzipien* was a prime source for Saussure's notion of *rapports associatifs*. What then remains to be traced is the origin of Saussure's concept of syntagmatic relations for which Paul's work does not seem to provide a model.

Arens (1969: 347) invited a comparison between the views of Paul and Saussure concerning the influential distinction between associative and syntagmatic relations outlined in the *Cours*. But Arens also points to the pronouncements of the Polish linguist Mikołaj Kruszewski (1851–1887) who distinguished between two fundamental kinds of associations, 'Angrenzungs-assoziationen' or associations of contiguity and 'Ähnlichkeitsassoziationen' or associations of similarity (Arens 1969: 359–361).[46] While Kruszewski's associations of similarity do not add any clarity to Paul's formulations,[47] it is likely that his concept of associations of

45. It must be pointed out, however, that the critical edition would not clearly confirm this observation (cf. *CLG/E* 376–377).

46. There is evidence of reciprocal influence between Paul and Kruszewski on a number of points which it would be out of place to detail here.

47. In fact, it should be pointed out that Paul most likely owes his distinction between 'Lautwandel' and 'Lautwechsel', one referring to historically attested linguistic change, the other to synchronically alternating sounds (or phonemes, to be more exact) to Kruszewski's monograph *Über die Lautabwechslung* of 1881. Paul never acknowledged his debt but all instances in which the term 'Lautwechsel' occurs (cf. 1909: 21, 68, 108, 117, 118, 191) cannot be found in the 1880 edition of the *Prinzipien* but were added to the second edition of 1886 (pp. 20, 61, 87, 95, 96, 154). Strangely enough, Paul listed the German translation of Kruszewski's major work (cf. next footnote) in the 4th (1909) edition of *Prinzipien* for the first time (p. xv) and, specifically, p. 49, note, at the beginning of his chapter on *Lautwandel* "sound change" (Kruszewski 1885: 260–268, 1887: 145–170). This is all the more surprising since he supplies bibliographical references quite generously otherwise.

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contiguity represents the immediate source of Saussure's notion of *suite linéaire* and syntagmatic relations of words.[48]

5. Terminological and conceptual correspondences

I have tried to show certain affinities between Paul's linguistic ideas and the concepts put forward in the *Cours*. It should be recalled that Saussure spent his formative years in Leipzig (1876–1878 and 1879/80) and Berlin (1878/79) and associated himself, at least in matters of historical-comparative methodology, with the *junggrammatische Richtung*; the parts of the *Cours* (*CLG* 193–260) dealing with historical linguistics do not essentially stray from the neogrammarian doctrine, though Saussure placed his argument on a clearer theoretical basis. There are indications, however, that Saussure soon became disillusioned with the teachings of the *Junggrammatiker*, in particular with their strong data orientation and lack of theoretical adequacy.

He exaggerated, however, when he noted in 1908 that Paul (among other linguists) had only provided the material for a general discussion of linguistic phenomena (see *SM* 51). Saussure knew Paul's work well and certainly was disappointed with the contradictions, the failure to draw (in his view) the correct, theoretical conclusions from the observed facts, and the shortcomings of the purely empirical approach. This explains why he told Albert Riedlinger in January 1909 with reference to the difficulties involved in dealing with synchronic linguistics that it would be necessary to go back to what Paul and the modern linguists had said on this matter in order to refute them (*SM* 29). I believe that this attitude is embodied in Saussure's theoretical position: Paul rejected descriptive grammar as not truly scientific but made use of its practical implications; Saussure placed synchronic linguistics on a firm theoretical basis and stressed its validity in opposition to diachronic linguistics. Paul took the empirical view, which maintains that the *Sprachusus* can only be arrived at through the investigation and comparison of individual linguistic expression. Saussure, in a rationalistic manner, postulated the existence of *langue*, within a given speech community and as a system in the head of each of its members, and showed little regard for *parole*, the aspect of language comprising the individual speech act.

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48. Besides *Über die Lautabwechslung* (see note 32 above) Saussure owned a copy of the German translation of Kruszewski's principal study, "Prinzipien der Sprachentwickelung", which appeared during 1884–1890 in instalments in Techmer's *Internationale Zeitschrift für Allgemeine Sprachwissenschaft* (Gambarara 1972: 346, 368). Paul, as an editorial board member of Techmer's Journal, owned the entire set, too.

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On the other hand, Saussure could find a number of essential ingredients for his own theories in the work of Paul as I hope to have demonstrated. This does not only apply to the three important components of Saussure's linguistic theory, namely, the dichotomies of *langue/parole* and synchrony/diachrony, and the notion of *rapports associatifs*, but also to a number of other ideas which are relevant to Saussure's linguistic thought. Coseriu (1967: 97) presented "un petit lexique terminologique parallèle de Gabelentz et de F. de Saussure" in order to suggest the important influence of the former on the latter. Some of the terms which Coseriu contrasted with those found in the *Cours* appear strikingly similar to Saussure's terminology and certainly very suggestive, but a closer analysis would reveal that they are quite dissimilar in substance and implication. I do not consider it sufficient to concede, as Coseriu (1967: 99) did, that Gabelentz's concepts were not adopted by Saussure without modification. Rather, I agree with Iordan/Orr (1970[1937]: 283n.) who pointed out that similarities are to be met with in the writings of a number of Saussure's contemporaries, including Paul and Gabelentz, and concluding further on (p. 294n.) that

> it is more appropriate to consider him [i.e., Saussure] as having focused a number of ideas which were taking shape in the linguistic world, and which were, in a sense, common property. His originality, which is indisputable, would thus consist in having evolved a complete and coherent system, all his own, irrespective of the source of any particular ingredient.

I therefore take the view that Paul as just one of the sources, albeit an important one, of Saussure's linguistic inspiration, his *Prinzipien* reflecting in a number of respects the ideas current in the last decades of the 19th century when Saussure's theory began to take shape. In setting out the list of terminological and conceptual parallels between Paul and Saussure which follows, my aim is not only to support my claim that Paul's *Prinzipien* had a profound influence on the development of Saussure's linguistic thought but also to indicate the 'climate of opinion' of the period which was not as unaware of structural ideas as is generally held by historians of linguistic science.[49] The parallels have to be taken with a grain of salt, for reasons of historical adequacy and potential epistemological differences which the terms themselves may not immediately reveal; on the other hand, the presence of terminological similarities and conceptual affinities, I submit, cannot be denied:

49. It is in this respect that Leonard Bloomfield could state in his review of the second edition of Saussure's posthumous work: "The value of the *Cours* lies in its clear and rigorous demonstration of fundamental principles. Most of what the author says has long been 'in the air' and has been here and there fragmentarily expressed; the systematization is his own." (Bloomfield 1924: 318).

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1. For Paul linguistics is a *Gesellschaftswissenschaft* (1909 [= 1886]: 7). The social nature of language is frequently stressed as the repeated use of expressions with social connotations and implications demonstrates, e.g., *Verkehr* "social intercourse" (pp. 39, 45, 59, 415, 419–420, and elsewhere),[50] its equalizing effect ("ausgleichende Wirkung des Verkehrs" [p. 44 = 1886: 42 = 1880: 240]), its particular situations ("Verkehrsverhältnisse") as well as intensity ("Verkehrsintensität") influencing language development (40–41 = 1880: 234–235), *Verkehrskreis* (115, 406 = 1880: 76, 269), and, in particular, two expressions which could be paralleled with Saussurean notions:

Sprachgenossenschaft	*communauté linguistique*
(1909: 46, 71, 418 = 1880: 241, 58, 273)	(*CLG* 104, 281, 304–306; but not in *CLG/E*)[51]
Zwang der Verkehrsgemeinschaft	*contrainte de l'usage collectif*
(1909: 61 = 1880: 53 = 1886: 57)	(*CLG* 131; not in *CLG/E*)[52]

The term 'Verkehr' itself may also be compared with Saussure's special use of the English word 'intercourse'[53] (*CLG* 281–285 *passim*), although here, as well as in a number of respects which concern the social aspect of language, I think that Saussure derived the term not from Émile Littré's (1801–1881) *Dictionnaire de la langue française* (1873), as Godel (*SM* 78, n. 83) suggested, but from William Dwight Whitney (1827–1894), as a number of references to the American linguist in Saussure's lectures would make much more likely (cf. *CLG/E* 8, 14, 16, 33–34, etc.).[54] Saussure's affirmations about the social

50. See Paul (1880: 234, 240, 252, 280, 284; 1886: 37, 43, 56, and *passim*, 364–365). Henceforth I leave out references to the 2nd ed., if the term is already found in the first. Note that between the first and subsequent editions, entire chapters were at times moved to other places within the book.

51. In all instances where the editors of the *CLG* wrote 'communauté linguistique' or 'communauté de langue' (pp. 304–306), we actually find, if anything, the term 'communauté' standing alone or, what appears to be Saussure's favorite expression, 'masse parlante' (e.g., *CLG* 112. 113, 128 = *CLG/E* 171, 174, 198) or 'masse sociale' (e.g., *CLG* 104 = *CLG/E* 159) and even 'société humaine' (e.g., *CLG/E* 158). However, Bally & Sechehaye's emendations do not lead to a misinterpretation of Saussure's intentions, I believe.

52. Again, this exact term or 'collectivité' (*CLG* 104) has no basis in the student notes; however, Léopold Gautier (1884–1973), Saussure's confidant in scholarly matters, had taken down in Saussure second course (1908–1909): "[…] la langue est surtout quelque chose à subir, non quelque chose dont on est maître" (*CLG/E* 159).

53. In the students' notes (*CLG/E* 466ff.) as well as in Saussure's own (see ibid., p. 470) the term always appears in italics.

54. Littré (1873: 219) gives only a brief etymology of the term whereas Whitney employed it in connection with his linguistic argument concerning language change. Thus Whitney (1867: 156) spoke of the "ordinary intercourse of life" (see also ibid., pp. 18, 123, 159, 405).

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character of language are well known and need not be cited here. However, they have some bearing upon the second complex of notions which I shall now discuss.

2. I have outlined above (3.2) Paul's opposition between 'individuelle Sprechtätigkeit' and 'Sprachusus' (1909 [1880]: 32, 33, etc.) and shown how this concept is interwoven in Paul's argument together with the social nature of language. Paul states this explicitly when he observes: "Es gehört eben zum Wesen der Sprache als eines Verkehrsmittels, dass der Einzelne sich in steter Übereinstimmung mit seinen Verkehrsgenossen fühlt" (1909: 58 = 1880: 51). Saussure attempted to substantiate his distinction between *langue* and *parole* by affirming that the former is 'un fait social' (*CLG* 21; cf. *CLG/E* 1974: 16)[55] and language "une institution sociale" (pp. 26, 33; *CLG/E* 33, 45) whereas *parole* is characterized as the 'acte individuel' and the 'côté exécutif' of language (p. 30; cf. *CLG/E* 40, 41). Paul contrasted (particularly with respect to pronunciation) 'common language' ("Gemeinsprache"), which he characterized as the 'Norm' that holds sway over the individual (1909: 413 = 1880: 226–227), with the 'individuelle Sprache' or the 'Individualsprachen' which tend to form 'Gruppen' (1909: 41 = 1886: 39); Saussure spoke of the 'esprit collectif des groupes linguistiques', an affirmation which he attributes to the Neogrammarians (*CLG* 19), and referred frequently to the collective nature of linguistic phenomena (cf. *CLG* 32, 38, 108, 138, 139, 157).
3. Although, as a scientfic approach to language, Paul gave full credit only to historical linguistics, he conceded the existence of a 'deskriptive Grammatik' whose heuristic value he acknowledged (1909: 23ff. = 1880: 28ff.). Saussure tended, perhaps as the result of an overreaction to Paul's claim, to reverse the picture emphasizing the predominance of the 'fait synchronique' over the evolutionary aspect of language (*CLG* 114ff., 140ff.). A number of correspondences, however, can be detected in connection with the opposition of these two methods of linguistic investigation which would suggest the importance of Paul's (though at times confused) reasoning for Saussure's linguistic thought. As Paul did before him, Saussure employed the following terms in his attempt to clarify the concept of 'static' or 'synchronic' linguistics, in particular *état de langue* which he preferred to the term *époque* (for its cultural connotations; see also *CLG* 246) as well as to *periode* since it denotes more a space than a point of time (*CLG* 142; see especially *CLG/E* 229–230). In Paul we find a number of occurrences of *Epoche*

55. In an unpublished paper probably dating back to 1894, if not earlier, Saussure noted "la langue est un fait social" (*SM* 40); Durkheim's *Règles*, often cited as Saussure's supposed source for the phrase, first appeared in 1895, giving it a full definition in the 2nd, 1901 edition only.

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(1909: 31, 36 = 1886: 29, 33) as well as *Periode(n)* (1909: 23, 31 = 1880: 21, 29), *Entwicklungsperiode* "period of development" (p. 19 = 1880: 22), and *Entwicklungsstufe* "stage of development" (p. 20 = 1886: 19), but in particular an explicit use of the concept of *Sprachzustand* "language state"(p. 31 = 1880: 21 [three times], and 43 = 1880: 29), *Zustände* (p. 32 = 1886: 29), and *Zustand einer Sprache* (p. 29 = 1886: 26 [twice]).

As Paul hopes that, after observation and comparison of individual speech utterances, 'a certain average' ("ein gewisser Durchschnitt") could be established in the description of the 'Sprachusus' (1909: 29 = 1886: 27, cf. also p. 62 = 1880: 55), Saussure stated: 'Among all individuals connected in this manner by language, a certain average will be established' ("Entre tous les individus ainsi reliés par le langage, il s'établira une sorte de moyenne" [*CLG* 29; cf. *CLG/E* 39]). Similarly, Paul had noted that, owing to the fact that no intermediate stages of linguistic change can be detected, the 'change of common language use' ("Veränderung des Sprachusus") appears to consist of some kind of 'substitution' ("Unterschiebung") of one form by another (1909: 34 = 1886: 32); Saussure went a step further when he affirmed that in diachronic linguistics a "déplacement d'un système" takes place (*CLG* 134; see also p. 140, and cf. *CLG/E* 227 for a contrary view). Where the concept of system in language is concerned, however – and it is as central to Saussure's model of *langue* as much as synchrony is –, Paul's *Prinzipien* would not have offered much support to Saussure's ideas. Paul's affirmation that there is harmony within any given sound system (1909: 57 = 1880: 49) remains a remark made *en passant*.[56] The only passage I am aware of (1880: 78 = 1909: 189), however, is quite significant and has already been cited earlier. Saussure also appears to follow Paul when he distinguished between 'changements phonétiques' and 'alternance' (*CLG* 197; see also *CLG/E* 327–328), the first phenomenon pertaining to diachronic linguistics (cf. *CLG* 198ff.), the second dealing with regular correspondences of coexisting forms (*CLG* 215–220, and especially *CLG/E* 356) and therefore with synchronic facts, which are diachronically motivated (*CLG* 218–219).

56. This observation was made earlier – and much more explicitly – by Eduard Sievers (1850–1936) in his *Grundzüge der Lautphysiologie* (Leipzig: Breitkopf & Härtel, 1876) which Paul adopted in the first edition of the *Prinzipien* (1880: 49). – Moreover, Sievers used the term 'Lautsystem' or 'System' (including 'Schriftsystem') about a dozen times in the introductory chapter of his *Grundzüge* (1876: 2–5) and even spoke of 'complete shift of entire sound systems' ("vollständige Verschiebungen ganzer Lautsysteme" [p. 127]) in his chapter on phonetic change (1876: 125ff.), observations which Paul did not take up but which could not have escaped Saussure's notice. Already in his famous *Mémoire* (Saussure 1879), he referred to Sievers' book several times (pp. 6n.1; 26n.2, 27n.1).

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These two terms correspond clearly to Paul's distinction (likely taken over from Kruszewski 1881) between *Lautwandel* "sound change" and *Lautwechsel* "sound alternation" (1909: 21, 68, 108, 117–118, 191 = 1886: 20, 61, 87, 95–96, 154).

4. Finally, a further complex of parallels in strictly general linguistic matters (for traditional historical matters are largely excluded from the present discussion)[57] between Paul's ideas and those outlined in the *Cours* can be established. It concerns the psychologism of both linguists which, as pointed out above already (3.3), was a prevailing trait in the linguistics of that period, although it is obvious that Saussure, unlike Paul, attempted to steer away from psychologizing language phenomena. I shall dispense with a broad discussion of this aspect of Paul's as well as Saussure's theory and merely point to terminological correspondences. There is in particular the term *Vorstellung* "conception" (cf. Paul 1909: 24, 26 = 1880; 28, 30, etc.) which played a role in Paul's theory together with related terms such as 'conception content' ("Vorstellungsinhalt" [pp. 14, 15 = 1880: 16, 17]) and 'groups of conceptions' ("Vorstellungsgruppen" [p. 27 = 1880: 31–32]), Paul spoke of the 'conception of the sound one is expected to speak' ("Vorstellung des […] zu sprechenden Lautes" [p. 56 = 1880: 49]) which has an influence on morphophonological changes such as assimilation. He also frequently used the term *Lautbild* "sound picture" (pp. 52 = 1880: 51–52 [twice] and 58–59 = 1880: 43–44 [8 times!]), a most likely model for Saussure's 'image acoustique' (*CLG* 28, 32, 98–99). This concept was given the following definition in the *Cours*: it is not

> […] le son matériel, chose purement physique, mais *l'empreinte psychique* de ce son, la *représentation* que nous en donne le témoignage de nos sens". (*CLG* 98; emphasis added; see also *CLG/E* 149)
>
> […the material sound, something entirely physical, but the *psychic imprint* of this sound, the *representation* which gives us the tstimony of our senses.]

This emphasizes the psychological nature of linguistic phenomena, which is not typical of Saussure and Paul alone but, as noted previously, charateristic of much of late 19th and early 20th century linguistic thought.

57. Leaving aside Paul's 'synchronic' conceptions, Uriel Weinreich (1926–1967) succinctly analyzed Paul's theories of linguistic change (1968: 104–119) and demonstrated their modernity.

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6. Concluding remarks

Hincha (1971) points to chapters in Paul's work (several of which had been added to the 1886 edition) which treat questions of syntactic relations (1909: 121–173), linguistic categories (263–312), the parts of speech, and with the problem of orthography in relation to the phonetic realities of language (352–389) – chapters which I have ignored in the present discussion – stating that these sections contain 'a great number of initial steps toward description and generalization'.[58] Albrecht (1994) remains uncertain as to what extent Paul was a "Strukturalist *ante litteram*", but notes a number of points in Paul's work, including his *Wörterbuch* of 1897, which are indeed quite modern. As recently as 2004, the late Winfred P. Lehmann (1916–2007) pointed (p. 67) to Paul's *Prinzipien* to show that Noam Chomsky could have learned from Paul's psychological conceptions of language.

But perhaps it was Milka Ivić who summarized the situation best when she noted that statements made by Paul, "although in a roundabout and longwinded way", reappear in a much more coherent fashion in Saussure (Ivić 1965: 62). Indeed, from my earliest research into the sources of Saussure's linguistic inspiration undertaken more than thirty-five years ago by now, I have maintained that there are little grounds for the still on-going debate on extra-linguistic 'influences' beyond what could be attributed to the climate of opinion prevailing during the last decades of the 19th and the early 20th century. I am still waiting for convincing evidence for a contrary view.

7. Coda

Months after completion of this chapter which constitutes essentially the basis of the present chapter, John E. Joseph of the University of Edinburgh, who has been working for some time on what promises to become a most substantial biography of Saussure (see now Joseph's 780-page book of 2016) and who, for this purpose, has consulted a considerable amount of archival material, much of which still awaits careful analysis, has drawn my attention to the existence of two[59] notes in Saussure's handwriting that point to his discussion of Paul's *Prinzipien* during the

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58. "[E]ine Fülle von synchronischen Deskriptions- und Generalisationsansätzen". Cf. also Cherubim (1973).

59. I am sure that there exist more than two critical references to Hermann Paul in Saussure's papers, in addition to those cited in, e.g., Godel (1957: 29, 51), which however belong to the period when Saussure gave his 1907–1911 lectures, which formed the basis of the posthumous *Cours*.

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early to mid-1880s,[60] while teaching at the École Pratique des Hautes Études in Paris (1881–1891). This would suggest to me that it is very likely that Saussure had access to a copy of the 1880 edition Paul's book when he was in Germany for much of that year, not just for his thesis defence at the University of Leipzig in February 1880 (although Gambarara 1972 does not list any of the four editions of the *Prinzipien* that appeared during Saussure's life-time). These notes indicate that Saussure was concerned with general linguistic principles well before his appointment to a professorship of comparative history of the Indo-European languages in Geneva in 1891. These preoccupations also suggest that Saussure was forming certain of his linguistic ideas in reaction against Paul, something that I had argued for as early as in Koerner (1972), when I characterized Paul's influence on Saussure as more often than not as a matter of "ex negativo".

One of these passages Joseph communicated to me runs as follows (words enclosed in <…> are additions made by Saussure to a running text, usually added between the lines or extended out into the margins; words crossed out in the original have been omitted in order to make the text more readable):

> <Je ne sais pourquoi> M Paul, avec qui je <ne me> sens du reste aucun désaccord semble opposer grammaire descriptive et comparative à la gramm. historique, comme si cette dernière ne reposait pas également sur la description ds la comparaison <p. 28>.[61] Toute la différence entre l'ancien et la nouvelle école, c'est que soit qu'on fit de la gramm. descript. soit qu'on fit de la gramm. comparative ou historiq., on n'avait pas le sentiment de l'essence <la nature> du langage, on n'observait pas sur le vif les procédés. Ecole historique me semble <pour cette raison> un titre mal choisi, <en 2d lieu> un titre téméraire, car avant d'avoir réédifié la grammaire descriptive sur sa nouvelle base, je crois bien difficile d'avoir <plus qu'> une vue superficielle du développement historique.[62]

60. Indeed, we find that the sheet from which the second quote is taken has a reference at top of page to "Mr Manan. Communication à la Soc. de Biologie de Paris du 28 avril 1883. Cas de cécité des mots."

61. Saussure refers to the passages in the *Prinzipien* where Paul argues, among others, that 'as long as one is satisfied with dealing with descriptive grammar in abstract terms, one is far away from a scientific conception of the life of language'. Paul never changed his position as we may gather from the fact that this argument was retained in all subsequent editions (see Paul 1909: 24).

62. This and the subsequent quotation from Saussure's papers could be consulted under the code of Archives de Saussure 374, Cahier 1, f. 126 and Cahier 2, between ff. 71–72, f. 1 verso, respectively, in the Bibliothèque de Genève (formerly: Bibliothèque universitaire et publique de Genève), Département des Manuscrits. Published with permission.

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A translation of this excerpt would read something like this:

> <I do not know why> Mr Paul, with whom I feel essentially no disagreement, appears to set descriptive and comparative grammar off against historical grammar, as if the latter did not as well rest upon description within comparison. The entire difference between the old school and the new is, whether one dealt with descriptive grammar or comparative-historical grammar, that one had no feeling for the essence <the nature of language>, that one did not observe processes in actuality. <For this reason> the term 'historical school' seems to me badly chosen, <secondly> at least an audacious title, since before having re-erected descriptive grammar on a new foundation, I believe that it is difficult to have more than a superficial view of historical development.

So, here we have early textual evidence – something which is the essential basis for any argument of 'influence' in linguistic historiography – for Saussure's insistence that not only must there be a descriptive (many years later called 'synchronic') and a historical ('diachronic') linguistics, and that the former must take precedence over the latter. By criticizing Paul's – evidently – clumsy and contradictory views, it is obvious that Saussure was sharpening his own argument.

In the second quotation, Saussure becomes more severe in his criticism of Paul whose *Prinzipien* was to become the 'bible of the Junggrammatiker', with whose early days in Leipzig (1876–1880) he had been associated. In hindsight, Antal (1985: 128) was led to the remark that "Paul's influence on Saussure is obvious; it is unlikely that we can find a single linguist after Paul who has not been affected by the *Prinzipien der Sprachgeschichte* in some form." It is to the historian of linguistics to substantiate any such sleight-of-hand statement.

> Paul, p. 119: il n'y a pas de la langue de différenciation <phonétique> intentionnelle destinée à marquer une différence de fonction. Pas de sens. En effet, de 2 choses l'une, où cette différenciation <a lieu>, dans la transmission fysiq. des images, et alors il va sans dire qu'elle n'est pas intentionnelle ≠ <≠qu'elle va même contre l'intention>.\. Ou bien c'est l'image qu'on modifie, et alors il ne s'agit plus d'un fait fonétiq. que si c'est précisément cette différenciation des images que M. Paul appelle diff. fonétiq. Sa proposition est fausse: toute „formation d'analogie" rentre dse ce cas, et beaucoup de ces formations correspondent à une diff. de fonction.
>
> [Paul on p. 119: there is in language no intentional <phonetic> differentiation designed to mark a functional difference. Nonsense. In fact, there are two distinct possibilities where this differentiation <takes place> in the physical transmission of images, and then it goes without saying that it is not intentional, <which is not equivalent to saying that it goes against intention>. Or else it is the image that gets modified, and then it is no longer a question of a phonetic fact than it is if it is precisely this differentiation of images that Mr Paul calls phonetic difference. His proposition is false: this applies to every 'analogical formation', and many of these correspond to a difference in function.]

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What Paul had stated in the passage in question in a chapter entitled "Reaction gegen die zerstörung und verwirrung der gruppen [Reactions against the destruction and confusion of (morphological) groups]" (1880: 100–130) was the following (p. 119 = 1909: 215–216) lengthy discussion of analogical formations in, e.g., the Old High German declension paradigm of *kalp*, pl. *kalbir* ("calf, calves"):

> Diese beispiele werden genügen um anschaulich zu machen, wie eine ohne rücksicht auf einen zweck entstandene lautliche differenzierung, durch zufälliges zusammentreffen verschiedener umstände begünstigt, ungewollt und unvermerkt in den dienst eines zweckes gezogen wird, wodurch dann der schein entsteht, als sei die differenz absichtlich zu diesem zwecke gemacht. Dieser schein wird um so stärker, je mehr die gleichzeitig entstandenen zweckwidrigen differenzen getilgt werden. Wir dürfen unsere aus der verfolgbaren historischen entwickelung zu schöpfende erfahrung zu dem satze verallgemeinern, dass es in der sprache überhaupt keine absichtliche zur bezeichnung eines functionsunterschiedes gemachte lautdifferenzierung gibt, dass der erstere immer erst durch secundäre entwickelung zur letzteren hinzutritt, und zwar durch eine unbeabsichtigte, den sprechenden individuen unbewusste entwickelung vermittelst natürlich sich ergebender ideenassociation.

In Herbert Augustus Strong's (1841–1918) 1888 English translation of the second, 1886 edition of Paul's *Prinzipien* (revised in 1890, pp. 227–228) this reads – without any italics – as the concluding paragraph of the chapter:[63]

> These examples will suffice to make it plain how a variation which sprang up without any idea of purpose, if favoured by the casual coincidence of different circumstances, may unperceived, and unintentionally, be made to subserve [*sic*; read: subvert] a purpose, causing it to appear as though the variation were designedly made to suit this very purpose. This appearance grows actually stronger the more perfectly the differences which arose at the same time unintentionally are abolished. We may generalize our experience drawn from historical development as far as we can trace it, in the proposition that there is no such thing as variation of sound created of set purpose with a view to denote a difference of function. The difference of function only attaches itself to the variation of sound by secondary development which the individual speaker neither designs nor perceives by means of a natural association of ideas.

63. In the second and subsequent editions the final paragraph of the first (1880: 120), in which Paul speculates about folk etymology (volksetymologie) as an area where sound and meaning are being approximated, has not been retained (cf. Paul 1886: 178).

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Already in Chapter IV of the first edition Paul treats the subject of "Analogie" at considerable length (1880: 61–77), a subject which he expanded slightly in the second (1886: 85–98) and subsequent editions (1909: 106–120) without changing the original wording. The above-mentioned Chapter VI supplies further examples and discussion of analogical change. It appears again that while Paul brings to bear many examples for discussion, when it comes to generalizations from the mass of the historical material that he provides, he tends to express himself in a round-about manner that obviously annoyed a mind like Saussure's which strives toward 'clarté'.[64]

Still, that Saussure was well acquainted with Paul's *Prinzipien* can no longer be denied. In autumn 1880, when Saussure was barely twenty-three, he received a copy of the book, together with a personal letter, from the author himself. It is strange that, while the first 1891 edition of Gabelentz's *Sprachwissenschaft* is listed in Gambarara's "La Bibliothèque de Ferdinand de Saussure" (1972: 339), there is no trace of Paul's book. It therefore was a pleasant suprise to discover in September 2008, in a booklet with obviously limited distribution, an appraisal of Paul as a theorist and historian of language (Kilian 1997) which almost incidentally provides a photographic reproduction of the following letter from Saussure to Hermann Paul (p. 44):[65]

64. That Paul's *Prinzipien* are still today regarded as an important source of inspiration well beyond the circle of linguists in Germany may be gathered from a very recent appraisal of his contribution to syntax (Vanneufville 2008), for instance.

65. Please note that I used a font that imitates Sausssure's handwriting; no change has been made in the original orthography and punctuation. The original letter can be found among the Hermann Paul papers ("Nachlass") of the Universitätsbibliothek München.

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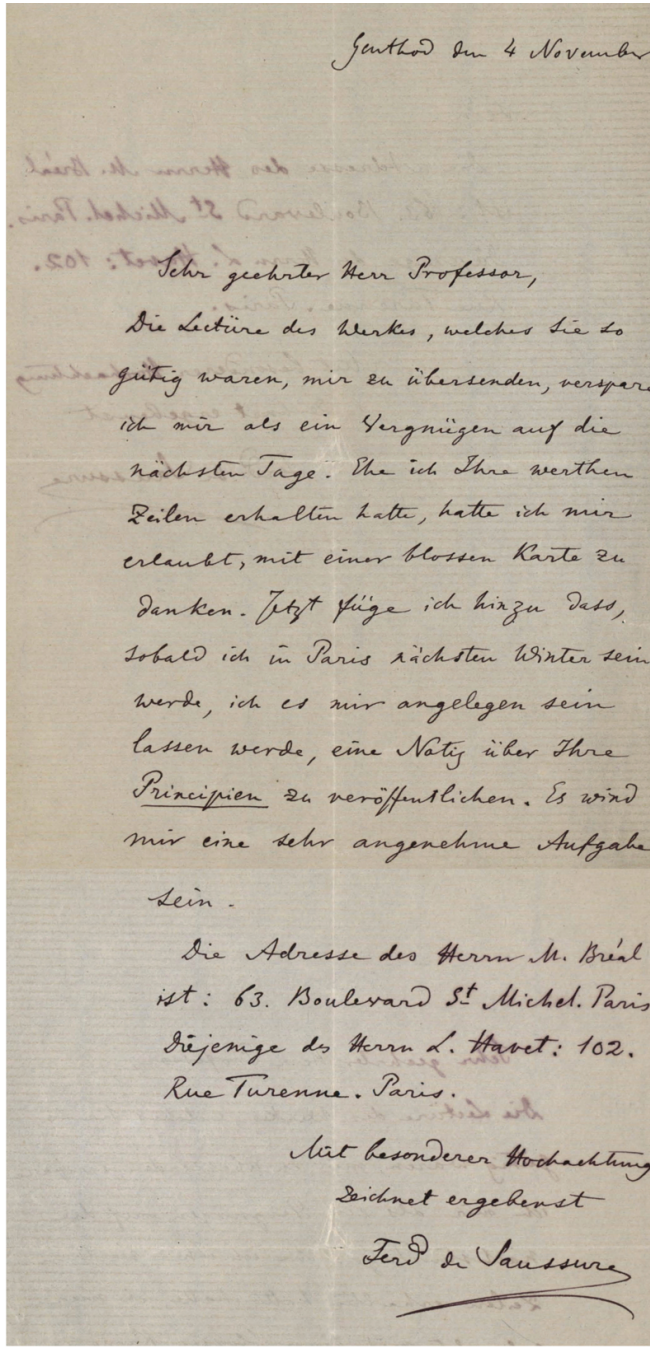

Genthod den 4 November

Sehr geehrter Herr Professor,

Die Lectüre des Werkes, welches Sie so gütig waren, mir zu übersenden, verspare ich mir als ein Vergnügen auf die nächsten Tage. Ehe ich Ihre werthen Zeilen erhalten hatte, hatte ich mir erlaubt, mit einer blossen Karte zu danken. Jetzt füge ich hinzu dass, sobald ich in Paris nächsten Winter sein werde, ich es mir angelegen sein lassen werde, eine Notiz über Ihre Principien zu veröffentlichen. Es wird mir eine sehr angenehme Aufgabe sein.

Die Adresse des Herrn M. Bréal ist: 63. Boulevard St. Michel. Paris.
Diejenige des Herrn L. Havet: 102. Rue Turenne. Paris.

Mit besonderer Hochachtung
zeichnet ergebenst
Ferd de Saussure

Letter of Saussure in response to Paul's present of *Principien der Sprachgeschichte* (1880)

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Translation

Genthod den 4 November [1880]

Sehr geehrter Herr Professor,
Die Lektüre des Werkes, welche Sie so gütig waren, mir zu übersenden, verspare ich mir als ein Vergnügen auf die nächsten Tage. Ehe ich Ihre werthen Zeilen erhalten hatte, hatte ich mir erlaubt, mit einer blossen Karte zu danken. Jetzt füge ich hinzu dass, sobald ich in Paris nächsten Winter sein werde, ich es mir angelegen sein lassen werde, eine Notiz über Ihre Principien zu veröffentlichen. Es wird mir eine sehr angenehme Aufgabe sein.

Die Adresse des Herrn M. Bréal ist: 63. Boulevard St Michel. Paris. Diejenige des Herrn L. Havet: 102. Rue Turenne. Paris.

Mit besonderer Hochachtung
zeichnet ergebenst

Ferd de Saussure

In this letter Sausure not only acknowledges with gratitude receipt of Paul's *Prinzipien*, but in fact promises to write with pleasure a review notice once he has taken on the post of Maître de conférences at the École Pratique des Hautes Études in Paris in autumn 1881.[66] No such review ever appeared, but the excerpts from his unpublished comments (see above),[67] which John E. Joseph (p.c.) dates to the early 1880s, suggest that he studied Paul's ideas critically, as the above excerpts clearly testify.

Acknowledgements

Parts of this chapter go back to my work on the dissertation during 1969–1971 (see Koerner 1973 [1971]: 107–124; 1972). In Spring 2005, this research was broadened eventually leading, in the Fall of 2006, to a submission to *Language Sciences*, where it eventually appeared in January 2008 (volume 30:1.102–132). The present chapter retakes most of this article, but adds, apart from revisions of the earlier text, a very important coda, which could only be hinted at in the previous version. – I am very gratelful to Wyn E. Roberts (Vancouver, B. C.) for his substantive comments and precisions of expressiom. I also wish to thank Craig Christy (Florida) for his corrections.

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66. The expression 'nächsten Winter' ("next winter") is ambiguous when written early in November, when winter has certainly set in already. Professor Joseph believes that 1880 was meant as Saussure had made plans to undertake further studies in Paris.

67. It is no surprise therefore that Saussure's name does not appear in Paul's letter to his publisher Max Niemeyer in Halle/Saale of 25 June 1886 in which he requests that complimerntary copies of the second edition of *Prinzipien* be sent to altogether twenty colleagues, including Adolf Noreen in Uppsala and Henry Sweet in London. (The letter in question has been reproduced on the back cover of Burkhardt & Henne (1997). The original is in the possession of Helmut Henne of Braunschweig.)

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Postscript

Months after completion of this chapter, I discovered by some accident – and much to my delight – that the University of Freiburg (where he was a professor, 1874–1893) now has a Hermann Paul Centre for Linguistics. Checking the web site <http://www.hpcl.uni-freiburg.de>, I noted, among other things, that the 'Hermann Paul School for the Language Sciences' – the presentation is in both German and English – currently offers 5 doctoral fellowships.

We are informed that "[o]n March 22nd 2006 the University of Freiburg Senate granted the Hermann Paul Centre official recognition as a legitimate institute", with Peter Auer, a scholar widely known as a sociolinguist (with appropriately broad interests, including phonology and cognitive linguistics), as its first director. And further that the Hermann Paul Centre "has since then celebrated its recent induction with an opening ceremony in the elegant university auditorium (Kollegiengebäude I) on 18 May 2007." In addition, according to the same source, the "Hermann Paul School for the Language Sciences" currently offers 5 doctoral fellowships. In the meantime, a volume of selected papers was edited by Peter Auer and Robert W. Murray entitled *Hermann Paul's 'Principles of Language History' Revisited* (2015) under the auspices of that 'School'.

In other words, unlike the Institut Ferdinand de Saussure, founded in October 1999 in Geneva, which is a virtual establishment only whose purpose it is to encourage research on Saussurian thought and texts from the viewpoint of a "science of signs within social life" (as its web site states),[68] the Hermann Paul Centre for Linguistics has a physical existence and not only invites scholars from abroad – such as Peter Mühlhäuser (University of Adelaide, Australia), who gave a lecture on Pidgin and Creole studies – but supports research by promising graduate students, too.

As far as I know, this is the first time in linguistics that an institute has been named after a distinguished scholar of the past, and not a person who donated lots of money to have his name put on a campus building. (Anyone who is familiar with the University of Texas at Austin for example will know what I mean. But many other North American universities could be referred to for illustration, with Harvard most likely being the biggest *profiteur* of the largesse from certain of their alumni.)

68. Still, it should be acknowledged that the Institut Ferdinand de Saussure is genuinely active, organizing significant publications, with Simon Bouquet as prime mover. The future belongs to virtual institutes like this one, uniting people who aren't in the same place physically. For November 2008, there is an announcement of a Colloque international "Le Monde du Symbolique – en hommage à Claude Lévi-Strauss" in order to celebrate his 100th birthday.

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CHAPTER 7

Edward Sapir

Assessments of his life and work

1. Introductory observations

Until the publication the 600-page *Selected Writings of Edward Sapir in Language, Culture and Personality* (Sapir 1949), which according to its editor, David G. Mandelbaum (1911–1887) "carry the gist of his thought" (p. xi), the generality of scholars in linguistics were familiar almost exclusively with Sapir's *Language*, first published in 1921, and several of his early articles typically published in the (then recently created) organ of the Linguistic Society of America *Language*, notably his 1925 "Sound Patterns in Language" and the 1929 "The Status of Linguistics as a Science", and a couple of items in the *International Journal of American Linguistics*, which periodical his former teacher Franz Boas (1858–1942) had launched in 1917, a number of them actually book reviews which are cited still today. This situation has changed considerably during the 1980s and 1990s, when more and more information about Edward Sapir (1884–1939) and his work became available.

For the hurried reader interested in Sapir's career there are nowadays, apart from what is available on the internet, quite a number of biographical dictionaries have since appeared, of which I may mention just a few, such as Victor Golla's entry in *International Dictionary of Anthropologists* ed. by Christopher Winters (New York: Garland, 1991), pp. 603–606,[1] W. Keith Percival's entry in the *Lexicon Grammaticorum* (Tübingen: Niemeyer, 1996), pp. 822–823, or Regna Darnell and Judith T. Irvine's 21-page account in *Biographical Memoirs* No. 71 (Washington, D.C.: National Academy of Sciences, 1997).[2]

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1. In it Sapir's place of birth is misspelt: instead of "Lauenberg (Germany)" read "Lauenburg (Pomerania)", then a part of Germany, since World War II: Lębork in Poland, about 100 km southwest of Danzig. (I mention this also because this Lauenburg is often confused with a place of the same name not far from Hamburg.) Also, Sapir died on February 4th, not 6th, 1939.

2. The last-mentioned item has been included at the end of Part III in Koerner (ed. 2007) since it summarizes, I believe, Sapir's *vita* best. Yet, unlike most biographers, I happen to own a photograph of the one-page entry Nr. 22 in the Lauenburg city registry dated 28 January 1884, according to which Jacob David Sapir announces the birth of his son Eduard (*sic*) and gives the maiden name of his wife as 'Dwosche', not Segal or Seagal, as is maintained by the Sapir family and as is found in the literature (e.g., Darnell 1990a; Darnell & Irvine 1997).

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The 1980s witnessed a revival of interest in Sapir's ideas, although his name had frequently been mentioned in connection with the so-called 'Sapir-Whorf Hypothesis' since the 1950s (see Koerner 1992, 2000, 2002). The centenary of Edward Sapir's birth in 1984, however, appears to have served as a rallying point, though a few papers here and there had been published before. For this special occasion, at least three events should be cited, namely, the special issue of *Historiographia Linguistica* which carried, apart from the editor's preface (11:3.349–354), a reproduction of Sapir 1905 Columbia Master's thesis (355–388), Yakov Malkiel's article on "The Prospects of a Sapir Renaissance in Linguistics" (389–396) and Hélène Bernier's account of "Edward Sapir et la recherche anthropologique au Musée National du Canada, 1910 à 1925" (397–412), both of which were chosen for inclusion in the 2007 anthology (Koerner, ed. 2007), and Tetsuro Hayashi's report "Edward Sapir in Japan: A survey of translations, 1940–1983" (461–466), which demonstrates the longstanding interest in Sapir's work in that country, an interest that has by no means abated, as may be gathered from the contributions to the annual *Bulletin of the Edward Sapir Society of Japan*, whose 32th number appeared in 2018.

The next item to be mentioned would probably be a book that I put together in anticipation of the Edward Sapir Centenary Conference held in Ottawa, 1–3 October 1984, *Edward Sapir: Appraisals of his life and work* (Koerner, introd. & ed., 1984), in which I brought together obituaries and subsequent evaluations of Sapir's legacy that had been available in print in most instances, but scattered in many places, at times difficult to trace. The Centenary Conference itself and the projected program and rosters of speakers had already been announced in print in *Historiographia Linguistica* 10:3.367–369 (1983). The 600-page proceedings of the meeting in the Victoria Memorial Museum in downtown Ottawa, in which Edward Sapir had once had his office as Chief of Anthropology of the Geological Survey of Canada, was published two years later (Cowan, Foster & Koerner, eds., 1986). In 1984, other publications appeared in time to celebrate the 100th anniversary of Sapir's birth (notably Dallaire, ed. 1984 and Golla, ed. 1984). However, the 3-volume set of 'critial assessments' of Edward Sapir's life and work (Koerner, ed. 2007) is clearly indebted – I dare say – to the efforts spearheaded by the present writer (cf. also the acknowledgment in Cowan et al., eds. 1986: 604). This chapter follows the organization of this 2007 publication and in a number of instances references are made to its table of contents (see Annex 1, for details) and, in a number of instances, to the numbered items included therein in order to keep the bibliography within a more acceptable size.

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2. Obituaries and biographical sketches, 1939–1952

The first volume of this three-volume set (Koerner, ed. 2007) includes the majority of obituaries of Edward Sapir appearing during 1939–1941; they are presented here in a chronological order, both with regard of the age of their respective authors and the date of their first publication. While the latter aspect is more accidental in nature, the former is not: it appears natural that the obituary by Sapir's former teacher at Columbia University in New York, Franz Boas, should take pride of place. Professionally, Sapir owned him a great deal, not only for having "roused him from dogmatic slumbers" in matters of language, as Robert Lowie (Item 20 in the set of 'critial assessmets') put it some forty years ago. This is followed by the necrology of his colleague at Yale, the Indo-Europeanist Franklin Edgerton (1885–1963), and the notice by his successor to the post of Chief of the Anthropological Division at the Canadian National Museum in Ottawa, Diamond Jenness (1886–1969). Given his close contacts during the last years of his life with Harry Stack Sullivan (1892–1949) of the William Alanson White Psychiatric Foundation in New York, I have also reproduced Sullivan's brief memorial of Sapir (Item 5). Sapir's relationship with Ruth Benedict (1887–1948), another former Boas student, was still another one, and, as her obituary reveals, she had a cordial relationship with the Sapir family as a whole and a special friendship with Edward Sapir based on a number of common interests, including the writing of poetry. Thus, while there is a certain amount of repetition in these obituaries, especially with regard to the external dates of Sapir's career, each of them reveals a different perspective on him, as a scholar and as a person.

Leslie Spier (1893–1961), a one-time collaborator (e.g., Sapir & Spier 1930), was almost ten years younger than Sapir; from one of his two death notices of the same year I have excerpted observations not found in the other obituaries (Item 6). Because of World War II, it appears that few scholars in Europe took notice of Sapir's premature death. The only brief obituary from the other side of the Atlantic appears to have been Louis Hjelmslev's (1899–1965), which I included in the anthology (Item 7). The next two stem from former students of Sapir's, Morris Swadesh (1909–1967), who had followed Sapir from Chicago to Yale and with whom Sapir collaborated on a variety of projects for the remainder of his life (e.g., Sapir & Swadesh 1932, 1939, 1946), and from David Mandelbaum (1911–1987), who, unlike most other Sapir students, did not take up a linguistics career but continued to work in anthropology.

As has been noted already, there is a certain amount of overlap in these accounts, but I felt I should not condense any of them to a mere list of excerpts,

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as they represent individual testimonies about Sapir.[3] Comparing Boas' obituary (which is characterized by its assessment of Sapir as an anthropologist rather than a linguist), for instance, with various other statements included, a composite picture of Sapir emerges, casting light on him as a human being, a teacher, a field worker, a colleague, and a writer with artistic leanings and wide cultural and scientific interests. It may be interesting to compare the appraisals included in Part I with an assessment written by C. F. ("Carl") Voegelin (1906–1986) more than a dozen years after Sapir's untimely death (Item 40). Voegelin, unlike Benedict, Spier, Swadesh and Mandelbaum, was neither a student of Sapir's, nor a student of Boas. He thus was able to write with a certain detachment. However, as one of the few students that Kroeber trained (in addition to James Alden Mason [1885–1967], Jaime de Angulo y Mayo [1888–1950] and his wife L. S. ("Nancy") Freeland [1890–1972]), he was much in tune with the 'Boasian tradition' (on which see Darnell 1998) – note that Kroeber was Boas' first Columbia Ph.D. – Voegelin, who was in personal contact and held many years of epistolary exchanges with Sapir, was well acquainted with Sapir's work.

Mention should perhaps also be made of a necrology coming from the physical anthropologist Earnest Albert Hooton (1887–1954). Hooton's obituary may be of interest, as it makes mention of Sapir's involvement in the reorganization of Harvard's Anthropology Department, following the death of Roland Burrage Dixon (1875–1934) there. Hooton also mentions Sapir as "the cornerstone upon which that masterly organizer and teacher, Fay-Cooper Cole [(1881–1961)], rebuilt the department of anthropology in Chicago". (Regarding this reorganization Cole's own account of 1952 [p. 167] may also be consulted.) However, Hooton's (1940: 158) admission that he "is incompetent to give any original and authoritative appraisal of Sapir's scientific contributions to anthropology [let alone linguistics]", and that he "was not privileged to know [him] intimately" beyond 'occasional contacts' (p. 159), motivated me to exclude his obituary from the anthology. For a summary of Edward Sapir's career, the reader can still be referred to Mandelbaum's well-known account of 1949 and, of course, the biographical entries mentioned earlier in the present chapter. Since 1990, however, we have Regna Darnell's masterly biography, never mind certain detractors (e.g., Silverstein 1991, and Darnell's response in the same journal).

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3. Note that I have not reprinted the various bibliographies of Sapir's scholarly output appended to the obituaries of Boas (1939: 59 63), Benedict (1939: 469 477), and the addenda to a 1938 list in Edgerton (1940: 463 464), since there is a much more complete and updated listing of his writings in linguistics and related fields at the end of Tome III. Only the bibliographical footnote in Swadesh (1939: 134n) has been retained for illustrative purposes.

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3. Intellectual influences and exchanges

Before presenting discussions of Sapir's work beyond what is found in the obituaries, which often had been written within a very short time following Sapir's rather sudden death, I thought fit to offer a selection from the scholarship devoted to Sapir's intellectual background, real and imagined.[4] I prefaced this with a critique from a seemingly unexpected side, namely, the co-author of this influential book *The Meaning of Meaning* (Ogden & Richards 1923), the Oxford philosopher and psychologist Charles Kay Ogden (1889–1957). As Joseph (1996) has shown, despite the fact that Ogden & Richards had criticized Sapir's 1921 book for not living up to their expectations, Sapir reviewed their book favorably (in *The Freeman* 7, 1923, 572–573) engaging in an epistolary exchange with Ogden in 1923 (see Dallaire 1984: 152), and was influenced by their work when formulating his ideas about language and world view (Item 70 in Koerner, ed. 2007). The various other items in this Part are of a much more recent date (1968 through 1988), dealing with such diverse issues such as the role Wilhelm von Humboldt (1767–1835) might have played in Sapir's intellectual make-up (Items 14 and 18) and the much more concrete questions related to his formative years as a graduate student at Columbia (Items 16 and 17). Item 19 investigates the question as to what extent Sapir's phonological theories impacted on members of the Prague School, a subject hitherto neglected in the annals of linguistic science. Its author, Gregory Eramian, shows that in fact the most explicit references appear in Nikolaj Sergeevič Trubetzkoy's (1890–1938) published scholarly work but also in his correspondence from 1929 onwards.[5]

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4. For instance, I think that Benedetto Croce's (1866–1953) impact is probably overrated; not surprisingly, his work on Aesthetics is referred to by Sapir exclusively in the concluding chapter on "Language and Literature" in his 1921 book *Language* (pp. 237, 239) and does not appear to have had any important bearing on his linguistics.

5. See now Patrick Sériot's edition of the French translation of Trubetzkoy's letters to Roman Jakobson (1896–1982), *Correspondence avec Roman Jakobson* (Lausanne: Payot, 2006), beginning with letter No. 38 of early 1928, in which Trubetzkoy writes that he read Sapir's "Sound Patterns" article of 1925 'with pleasure'. See Name Index (p. 558) for more than a dozen further references to Sapir.

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4. Comments on Sapir's work in the first half of the 20th century

Most of the items reprinted in the present section of Koerner (ed. 2007) devoted to comments on Sapir's were written a decade or more after Sapir's death, all by scholars who knew Sapir well, with the exception of Joseph Greenberg (1915–2001) who, contrary what his anthologist asserts (Greenberg 1971: xi), did not take a course from Sapir,[6] though he studied for a year at Yale under Leonard Bloomfield (1887–1949) and Bernard Bloch (1907–1965).

Lowie, a student of Boas like Sapir, published his review of *Time Perspective* fairly late, perhaps because he saw a conflict between his personal ties with Sapir, on the one hand, and the fact that he was assistant editor of the *American Anthropologist* at the time, on the other. (cf. Sapir's inquiry of 10 July 1917 about the review in his letter to Lowie [1965: 25].) I do not know of other contemporary reviews of Sapir's 1916 monograph, but the study is still frequently cited (cf. Kroeber's comments in 1984 [1959]). By contrast, Sapir's book *Language*, published in autumn 1921 in the United States and in spring 1922 in Britain, has had numerous reviews.[7] No doubt, many review copies were sent out to editors and publishers of periodicals, including those for 'educated laymen', and indeed newspapers with regional or national readerships. Both Harcourt and Sapir were interested in publicity and sales. From what we know of Sapir's biography, it is fair to assume that he was looking

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6. Greenberg told the present writer in a chance encounter in the main library of Stanford University in February 1984 that he missed studying with Sapir by about a year but that he always felt close to his linguistic thinking.

7. For the record I am listing a number of brief reviews of Sapir's *Language* not included in the References; I owe knowledge of these to Philip Sapir, Edward's second son, born in Ottawa in 1916, who visited me at home in Hull, Quebec in the late 1980s. Thus there appeared anonymous notices in the following newspapers and magazines: *New York Evening Post* (14 Nov. 1921), *Boston Herald* (27 Nov. 1921), *Boston Evening Transcript* (8 Dec. 1921) – which suggest that Sapir's book appeared in print in November 1921 – and in *Times Literary Supplement* (18 May 1922), *Notes & Queries* (20 May 1922), *The Smart Set* (22 May 1922), *The Spectator* (27 May 1922) – which seems to indicate that it appeared in Britain early in May 1922. Further notices were published in *Journal of Education* (Nov. 1922), *The Freeman* (22 Feb. 1922), and also signed by initials only in *Detroit News* of 8 Jan. 1922 (T.L.M.) and *New Statesman* of 1 July 1922 (signed L.A.K, which obviously refers to Alfred Louis Kroeber). That journalist friends of Sapir's jumped into the act may be gathered from E. W. Harrold's (1922) review, which was the basis of an editorial in the *Toronto Daily Star* (27 Feb. 1922); another Canadian journalist, unlike Harrold (1889–1945), who lived in Ottawa, apparently stationed in Toronto was John Daniel Robins (1889–1952) – cf. his review of Sapir's book in *The Canadian Forum* (Sept. 1922); interestingly enough, Robins wrote a 296 page thesis at the University of Chicago in 1927, i.e., during Sapir's professorship there, on *Color Words in English*.

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for international recognition and a regular academic position,[8] and that a number of colleagues and personal friends were eager to help him in attaining this goal.[9]

Out of the great number of reviews of *Language* (cf. also notes 8–9 and the references below), I have chosen what I believe to be the most significant ones, namely, those by Lowie and Leonard Bloomfield, together with an excerpt of Kroeber's more popular account. A few others should at least be mentioned here.

Some of those reviews not included in Volume I are by such distinguished (frequently European) scholars as James Wilson Bright (1852–1926), the English philologist and editor-in-chief of *Modern Language Notes*, who is perhaps best known for his *Anglo-Saxon Reader*, first published in 1891, still in press in the 1950s, and revised and updated much more recently (cf. Bright-Cassidy-Ringler 1971). The most distinguished French comparative linguist Antoine Meillet (1866–1936); the Austrian-born Indo-Europeanist then teaching at Bryn Mawr College before his move to Yale, Eduard Prokosch (1876–1938) – who, by the way, impressed no other than Bloomfield so much in 1906 that he decided to enter upon a career in linguistics, and Henry Bradley (1845–1923), the English philologist and joint editor of the *OED*, to mention just a few. Interestingly enough, most of these reviews compared Sapir's *Language* with Otto Jespersen's (1860–1943) much larger book by the same title as well as with Joseph Vendryes' (1875–1960) 439-page *Le Langage* of 1921 (Bradley 1923; Meillet 1922; cf. also Warnotte 1922), or with Joseph Schrijnen's (1969–1938) Introduction to Indo-European Philology of the same year (Prokosch 1922). These reviews certainly merit mention, as does the one by the British Germanist at the University of Liverpool, William Edward Collinson (1889–1969), who, like Bloomfield, compared Sapir's book with Saussure's *Cours*, which had just appeared in a second edition (Collinson 1924).

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8. It appears that there have been many reviews of Sapir's *Language* in Continental Europe; apart the one by Meillet (1922), I know only one by the Dutch Anglicist Etsko Kruisinga (1879–1944) and the Belgian sociologist historian Daniel Warnotte (1871–1949), who included it in a "Science du langage" rubric in a sociological journal (cf. Kruisinga 1925 and Warnotte 1922). However, owing to the kind offices of Philip Sapir I received copies of letters dated 30 Dec. 1921 and 16 Feb. 1922, respectively, which the distinguished Danish Anglicist Otto Jespersen (1860 1943) and the great Swedish Sinologist Bernhard Karlgren (1889–1978) sent to Sapir thanking him for the presentation of copies of *Language*. But it does not appear that either of the two scholars commented on the book in print.

9. It is interesting to note that three (out of four) of the persons Sapir's mentions in the preface to *Language*, namely, Kroeber, Lowie, and the literary scholar Jacob Zeitlin (1883–1937), wrote reviews; the only person who didn't was Wilson Dallam Wallis (1886 1970). Another non-linguistic scholar writing a review of *Language* was Arthur F. J. Remy (1871–1954) of Columbia University (Remy 1922).

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As is well known, Bloomfield in 1922 regarded both Saussure's *Cours* and Sapir's *Language* as belonging to 'the newer trend of linguistic study'. Indeed, no reviewer of distinction failed to note the exquisiteness of Sapir's style and the intellectual freshness of his approach. It is no wonder that Sapir's book was still recommended reading for linguistics students several generations after its first publication, a recommendation based on far more than the 'mentalism' that some have claimed Sapir shares with Chomsky (cf. McCawley's remarks [1967: 106; see Item 44], with regard to phonological theory). Kroeber had predicted in 1922 that *Sapir's Language* "is unique in its field, and is likely to become and long remain standard" (see Item 33).

I have given more space in Part V to reviews of Sapir's *Selected Writings*, for several reasons. To begin with, this volume is much broader in scope than Sapir's study of 1916 and the 1921 book. Until those volumes of *The Collected Works* become available that would retake his writings in general linguistics,[10] the Mandelbaum volume will continue to be regarded as Sapir's 'summa', at least in this respect. The reviews reprinted here are, with the exception of the brief one by George Leonard Trager (1906–1992), a pupil of Bloomfield, all those published in North American linguistics journals. Typically, while Stanley Newman (1905–1984) was concerned that the (anti) mentalism debate launched by Bloomfield might "have obscured Sapir's position on the relation of linguistics to the other sciences of human behavior" (1951: 185; Item 38), Trager was relieved to note that despite "the possible 'mentalistic' implications of some of the phrasing, […] on closer inspection [… there] is very little that is not highly objective scientific statement" (1951: 18; cf. also Harris 1951 = Item 39).[11]

While both Harry Hoijer (who also wrote a short, but insightful obituary of Sapir in *American Journal of Sociology* 44, p. 721) and Stanley S. Newman did their doctorates under Sapir (at Chicago and Yale, respectively), Zellig S. Harris (1909–1992), trained as a Semiticist at the University of Pennsylvania, followed in the main the descriptive and 'mechanist' approach associated with Bloomfield. (He knew both Sapir and Bloomfield personally.) This difference in background

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10. Although the first meeting of the editorial committee took place during the Sapir Centenary Meeting held in the National Museum of Man in Ottawa in October 1984 (see the photograph in Cowan et al. 1986: 40), the first volume appeared only in 1990, and after 2008 no further volume has come out to the present day. Those that have been published – except for Tome I – deal with Sapir's work in the areas of ethnology, culture, psychology and, especially, Amerindian materials. (Tomes II, IX, XI–XIII still do not seem to have become available yet.)

11. This view is echoed by Hjelmslev (1939: 77), who points out that "it should not be forgotten that even if Sapir is almost constantly speaking in psychological terms, there is in his conception no trace of real psychologism", and that "[t]he psychological terminology is a garment that can easily be stripped off without in the least affecting the results."

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and outlook alone does not make Harris' review article an important statement (though it is interesting to note that Harris is reported never to have completed one single field report on an Amerindian language, and that he, like his student Noam Chomsky, has been more interested in working out theoretical constructs, steps removed from the living language).[12] By early 1947, Harris had completed his work on his influential *Methods in Structural Linguistics* (Chicago, 1951), and he evidently took out much time to read and reread Sapir's *Selected Writings*. Although his preferences were for Sapir's linguistic work – the analytic technique and the theoretical flights – Harris devoted considerable space to Sapir's cultural, social and psychological interests, much of which went beyond language description. Indeed Harris' 45-page review constitutes a valuable document of the linguistic paradigm of the period and its surrounding *Zeitgeist* (cf. his references to Marx and Freud), while at the same time revealing, more than any other publication, the author's scientific credo. Thus we find explicit references to Saussure's synchronic theory of language (e.g., 1951: 289), an argument in favour of a 'process model' (p. 291), which was further developed by Hockett (1954) and subsequently exploited by the generativists, and a clear awareness of the importance of Bloomfield's use of 'base forms' in morphophonemics (ibid., note 7), which no doubt is the source of Chomsky's 'generative' approach in his 1951 M.A. thesis, to mention just a few points of interest to the historian of modern linguistic science.[13] To this we could add Harris' frequent references to mathematical procedure in linguistics; note his affirmation (p. 301): "The formal analysis of language is an empirical discovery of the same kinds of relations and combinations which are devised in logic and mathematics", an affirmation which foreshadows much of what is commonly (and misleadingly) associated with the 'Chomskyan revolution'.

An evaluation of Sapir's work, however, would be incomplete if no mention was made of his work as a teacher and field researcher. As regards the first point, references to Sapir's success as a lecturer and teacher can be found in various obituaries reprinted in Part I of Koerner (ed. 2007), to which may be added Kenneth Lee Pike's

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12. That this traditional view of Harris is much too narrow may be gathered from his full bibliography, 1932–2002, in Koerner, *Essays in the History of Linguistics* (Amsterdam & Philadelphia: John Benjamins, 2004), pp. 239–254. For instance, contrary to a myth probably diffused by the Generativists, Harris in fact did fieldwork on American Indian languages, more particularly on Iroquoian languages. His materials are conserved by the American Philosophical Society in Philadelphia, where we find Cherokee material of 1941–1946, actually a typed document and autograph of 620 [*sic*] leaves, Seneca and other notes of 1947 of 122 and 37 leaves and, finally, Seneca and additional items of 20, 62, and 5 leaves

13. This subject is treated in much detail in Koerner, *Toward a History of American Linguistics* (London & New York: Routledge, 2002; paper back edition, 2012), Chapter 9.

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(1912–2000) acknowledgment of 1984 (Item 25 in Koerner, ed. 2007).[14] Mary R. Haas' (1910–1996) 1953 comment on "Sapir and the Training of Anthropological Linguists" (reprinted as Item 41, also in Koerner, ed. 2007) at least offers us an inkling of Sapir's general approach. Sapir's celebrated article of 1933, "La réalité psychologique des phonèmes", whose English original was published only ten years after his death (an article which during my years at the University of Ottawa 1976–2001, I still required my third-year linguistics students to study carefully), offers a good idea of both his method and analytical talent.

5. The reception of Sapir's ideas in the second half of the 20th century

Those scholars in North America who never regarded linguistics as an autonomous discipline, divorced from social context and cultural patterns, have continued to work in a broadly Sapirian framework. However, already during the 1950s these anthropologically oriented scholars represented more an undercurrent (albeit an important one) in linguistics, with the post-Bloomfieldians and their successors, the Chomskyites, representing the positivistic tradition (which in a way has its origin in the Neogrammarians and Saussure) and forming much of 'mainstream' linguistics. Part of this development is reflected in the paucity of papers *on* Sapir during the later 1950s and 1960s. The fact that Lowie's edition of Sapir's letters to him, though ready to go to press by 1956, did not see publication (and then only in typescript form) before 1965, may just be an indication of the lack of interest in Sapir at the time. Similarly, papers given by Alfred Kroeber and Yakov Malkiel in May 1959 on the same campus where Lowie had been anthropology professor for many years, remained unpublished until 1984, the centenary of Sapir's birth, though tape recordings had been available to interested parties. While Kroeber's retrospective on Sapir's career appears here in only slightly amended form – his talk having not been intended for publication (see Item 21), Malkiel's paper benefited from its author's revisions (see Malkiel 1984 = Item 22 in Koerner, ed. 2007). Papers of a comparable nature too did not see the light of day before 1976, until Regna Darnell published her paper, included there in somewhat revised form (Item 53 in Koerner, ed. 2007), unless we are to refer to a string of articles by Dell Hymes, who

14. It should not be forgotten that Pike dedicated his *opus magnum* (Pike 1971) to Sapir, with a photograph he had taken of him during the 1937 Linguistic Summer Institute held in Ann Arbor, Mich., and the following inscription: "Trail blazer in the study of sounds with reference to 'The inner configuration of the sound system of a language, the intuitive 'placing' of the sounds with reference to one another', and pioneer in the stating of the relation of language to other cultural patterns of man."

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has written extensively on the Sapirian tradition (as he sees it) from about 1960 onwards, and who has no doubt instilled in Darnell the interest in the history of anthropology and in Sapir so clearly manifested in her work since the mid-1960s. (For references to Sapir in Hymes' writings, see his 1983 collection of papers.)[15]

Actually, following Sapir's death in 1939, only a small number of scholars in North America and elsewhere wrote about Sapir, if we leave out the writings of Sapir's students and associates, in particular the obituaries of 1939–1941, a few incidental notes (e.g., Voegelin 1942), and the memorial volume (Spier et al. 1941; cf. the discussion in Hymes 1983: 161–163). The 1950s saw several reviews of *Selected Writings*. To those mentioned in the previous section, we may add the accounts of Sommerfelt (1952) and Gregores (1953), Mikuš's (1953) discussion of Sapir's syntagmatics, and Guxman's (1954) presentation of his ethnolinguistic views. Otherwise the harvest was rather meager. The 1960s saw a few papers, notably by Hymes and by anthropologists (e.g., Preston 1966; Mandelbaum 1968) and linguists (see Swadesh 1961 and McCawley 1967, both reprinted in the 2007 project), but also, and more importantly, several translations of Sapir's *Language* into German, Italian, and French. The 1970s witnessed a few more studies, but all written by anthropologists, not linguists (e.g., Ferry 1970, Woolfson 1970, Allen 1970, 1974, Darnell 1976), apparently with few exceptions (e.g., Haas 1976). This trend continued into the 1980s (see Preston 1980, Cain 1980, and Murray 1981a, b). Part VI ("Evaluation of particular Aspects of Sapir's Work and Legacy") in volume II reflects this distribution: in addition to the evaluations of Sapir's *œuvre* and personality by anthropological linguists (Haas, Swadesh), anthropologists (Preston), it contains two papers by linguists (Malkiel, McCawley and, more recently, Shapiro, all included in the 2007 project) and one by a sociologist with anthropological leanings (Murray).[16]

Interest in aspects of Sapir's work has continued unabated. This is not only manifested in the publication of his *Collected Works* (Sapir 1990–) – which most likely had been motivated to no small degree by the commercial success of Roman Jakobson's *Selected Writings*, but perhaps more so from the entries that can be found with regularity in the annual *Bibliographie linguistique*, only a few of them will be referred to in the present chapter. Special mention, however, deserves to be

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15. However, Hymes has not, to my knowledge, written a paper of any length especially devoted to Sapir which could have been included in the present selection, though he has presented Sapir's ideas on a number of occasions, including in his tributes to Kroeber in 1960 and Swadesh in 1971 (reprinted in Hymes 1983, on pp. 245 272 and 273 330, respectively).

16. I'd be the first to admit that the selection – and grouping – of papers could have been done differently, but anyone attempting a similar task will soon realize the difficulty, simply because few papers cover only one specific facet of Sapir's life and work.

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made of the efforts of María Xosé Fernández Casas of the University of Santiago de Compostela who wrote papers (e.g., Fernández Casas 2000), including one on Sapir's 'mentalism' (Fernández Casas 2005), and in her PhD thesis on the continuity of his linguistic thinking (Fernández Casas 2004). Other recent publications pertaining to Sapir will be referred to in the next section.

6. The discussion of particular aspects of Sapir's theories

Since the emphasis in the present project is on linguistic, rather than anthropological or various other aspects of Sapir's legacy, a few remarks on the reception of particular ideas of his in the study of language are called for, before reference is made to other areas to which Sapir contributed in an interesting way. Indeed, there are several areas in which Sapir's work has received, at times considerable, attention. To cover them all adequately would require a considerable amount of additional research and monograph treatment, something that cannot be contemplated here. However, a number of subjects must be mentioned in order to offer at least in inkling of Sapir's productive mind.

6.1 Language typology and language classification

Language classification, both genetic and typological, is a subject in which Sapir follows in general terms the Humboldtian tradition (see Koerner 1977, 1995: 159–161), and which has frequently been discussed in the literature, among many others by his former student and successor at Chicago, Harry Hoijer (1904–1976), in a variety of papers (e.g., Hoijer 1941, 1946). In this Morris Swadesh (1909–1967), who followed Sapir from Chicago to Yale and who published more studies in collaboration with him than anyone else (cf. also the posthumous publications of 1953, 1955, and 1960 listed in the Sapir Bibliography that can be found in Koerner, 1984), played an important role. Swadesh had received many of Sapir's unpublished manuscripts either directly or indirectly through the offices of Franz Boas. His 1961 paper, here reprinted as item 41, is just one example of the debate that Sapir aroused following the publication of *Language* in 1921 with its chapter six, "Types of Linguistic Structure", the note on the grouping of American Indian languages north of Mexico published in the same year (cf. the reproduction in Koerner, ed. 1984: 140), and other suggestions Sapir made during his career. That he was particularly interested in family relationships among Amerindian languages aroused special interest among anthropological linguists (e.g., Darnell & Hymes 1986; Foster 1988, both reprinted in Part IX of the 2007 project).

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Joseph Greenberg's *Language in the Americas* (1987), dedicated explicitly "To the memory of Edward Sapir (1884–1939)", has become the most debated attempt to reduce the number of Amerindian languages to a very small number, much beyond what Sapir might have approved of. From the many critiques of this work on the part of linguists working in the field of native American languages, I shall refer to the perhaps strongest attempt to refute Greenberg's hypotheses, namely, Lyle Campbell's twenty-five-page review article of the following year (Campbell 1988). Still, that Sapir's ideas have fallen on fertile ground cannot be doubted and may be gathered from the many references to Sapir in the recent 1,800-page encyclopedia devoted to language typology and universals (see the name index in Haspelmath et al., eds. 2001: 1817), which also incudes a separate article on his ideas (Haase 2001).

6.2 Sapir's contribution to phonological theory

I have already referred earlier to Trubetzkoy's approval of Sapir's ideas in phonology (see now Trubetzkoy 2001: 298, for references in various of his articles). McCawley's (1967) paper included here (Item 44 in Koerner, ed. 2007), though written by a pupil of Noam Chomsky and a linguist with a background in mathematics as well as modern (but not Amerindian) languages, addresses another subject in which Sapir inspired much fruitful debate: phonology. As Hayashi's (1984) listing of Japanese translations of Sapir's writings informs us, there have been a number of translations into that language of "Sound Patterns of Language" (Sapir 1925) in 1940, 1957, 1958, and of "The Psychological Reality of Phonemes" (Sapir 1949 [1933]) in 1958 in 1983. In North America itself, these two papers have been reprinted not only in Mandelbaum's 1949 volume of Sapir's major papers, but also in an anthology on *Descriptive Linguistics in America* (Joos, ed. 1957, ^{4}1966)[17] and in another one on the history and current practice of phonology (1972, ^{2,1}1977). The French version of the 1933 paper, "La réalité psychologique des phonème", which preceded the publication of the original English version by sixteen years, was retranslated on the basis of the English text in 1968, followed by a reprinting of the 1933 version, and excerpts of it appeared in another anthology (Léon et al. 1977: 179–182), largely based on a mistaken interpretation of Sapir as a precursor of generative phonology, a view already criticized by McCawley in 1967.

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17. Actually only the 1925 paper was included in the Joos volume, most probably because of the 'mentalism' evident especially in Sapir's (1933) paper; typically, a German anthology, *Beschreibungsmethoden des amerikanischen Strukturalismus* ed. by Elisabeth Bense, Peter Eisenberg & Hartmut Haberland (Munich: Max Hueber, 1976), also includes only a translation of Sapir's 'Sound Patterns' paper (pp. 49–63).

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6.3 Sapir's contribution to historical-comparative linguistics

Another subject which received widespread attention was Sapir's concept of 'drift', elaborated on in chapter seven of *Language*, "Language as an Historical Product", though first adumbrated in his 1916 monograph on *Time Perspective in Aboriginal American Culture* (cf. the observations made by Kroeber in 1959 = Item 21). This fascination with Sapir's concept of drift is also evidenced in references made early by many other scholars, e.g., Lowie (1923 = Item 34), Spier (1939a = Item 6), Harris (1951 = Item 39) and much later, from an ethnologist's perspective, Preston (1980 = Item 70). Some linguists of the 'generative' school offered their interpretations of Sapir's suggestions, but with little success (e.g., Lakoff 1972, Vennemann 1975). Concerning the complex question of language change, Malkiel (1981) presented a more sophisticated account, but only much later do we see more satisfactory elaborations of Sapir's initial idea (e.g., Andersen 1990, but also Shapiro 1987).

At the Sapir Centenary Conference held in Ottawa, Ives Goddard examined Sapir's (not always very explicit) use of the comparative method in the field of genetic relationships adduced on the basis of at times limited evidence (see Goddard 1996 = Item 57). That his hunch could at times be borne out by much more detailed fieldwork has been shown by M.-L. Tarpent with regard to several native language on the West Coast of Canada (Tarpent 1997 = Item 62; cf. also Dinwoodie 1999, for an analysis of Sapir's 1929 Navajo field work).

However, it appears that Sapir's suggestions regarding distant linguistic relationships has received much more attention (cf. Bright 1991, Kaye 1992 = Items 60–61; cf. also Smith-Stark 1992, Darnell 1999), a subject which first had culminated in Greenberg's (1987) reduction of all native American stock to essentially three macro-families which he called 'Eskimo-Aleut', 'Na-Dene', and 'Amerind', the last encompassing all languages from Northern Canada to Tierra del Fuego, a somewhat extravagant claim which Sapir in his most flamboyant moments might have enjoyed, though most probably not have approved of on sounder linguistic grounds.

6.4 Sapir's 'psychology of culture' and other non-linguistic ideas

Given Sapir's wide range of intellectual pursuits, he contributed in an interesting way to a number of areas outside of linguistics. Part X of the present volumes assessing his legacy is an attempt to account for this. Psychology in general terms had an attraction for Sapir, but as scholarship of the 1980s and 1990s has shown, there were particular subjects which he elaborated on, though perhaps more often in his lectures than in his published work. For the Ottawa Centenary several scholars had prepared papers on his 'psychology of culture' concept (Preston 1986 = Item 66) or

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the development of a 'psychology of human behavior' (Newman 1986 = Item 64). On the former, an attempt was made to reconstruct an entire monograph based on what Sapir had written about the subject and various student notes (Irving, ed. 1994; cf. Murray's 1995 review = Item 69). For a much broader effort to bring Sapir's ideas on society, culture, and personality together, the interested reader may want to turn to volume II of *The Collected Work*s (Sapir 1999 edited by Darnell et al.; see also Rodseth 1998).

More recently, Sapir's 1938 article "Why Cultural Anthropology Needs the Psychiatrist" (first reprinted in Sapir 1949: 569–577) has been given renewed attention, after it had been reprinted in the above volume (Darnell et al., eds. 1999: 353–362). The paper shows the strong influence of the psychiatrist Harry Stack Sullivan (1892–1949) who, according to Darnell (1990: 289) "was the closest friend of Sapir's mature years." It was from him that Sapir adopted the concept of 'interpersonal relations', and it was in Syllivan's newly founded journal *Psychiatry* that the above-mentioned article first appeared. Interest in this article must have been important enough to have it reprinted in the same journal more than sixty years later (in *Psychiatry* 64:1.2–10 [2001]) together with assessments of Sapir's contribution to the subject of 'culture and personality' (Frederickson 2001) and on his "thought experiment in the interdisciplines of cultural anthropology and psychiatry" (Darnell 2001). Various aspects of Sapir's anthropological interests are addressed in the articles included in Part XI ("Sapir as an Ethnologist"), though they do not exhaust his contribution to this field by any means as may be gathered from the 963-page volume IV of his *Collected Works* ("Ethnology"), which brings together all articles and reviews that the editors felt could be assembled under this heading, from his 1916 "Time Perspective" monograph (Sapir 1994 ed. by Darnell & Irvine, 31–120) to his posthumous (not quite finished) paper "Songs for a Comox[18] Dance Mask" (edited by Leslie Spier and published in *Ethnos* in late 1939).

7. Edward Sapir and the so-called 'Sapir-Whorf hypothesis'

Although Sapir is seen as having made significant contributions to the fields mentioned in the preceding section as well as to such seemingly esoteric subjects as Semiotics (Shapiro 1987 = Item 47; Berthoff 1991), it seems impossible to speak of Edward Sapir without mentioning the so-called 'Sapir-Whorf hypothesis', with which his name is frequently associated, though perhaps not justly, as Kroeber pointed out in 1959 (Item 21 in the present selection), and for which Penny Lee (1996)

18. Comox is the name of a Salish language of Vancouver Island on Canada's Pacific coast.

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favoured the term 'linguistic relativity principle'. Interestingly, the Dutch Anglicist Etsko Kruisinga (1875–1944), in his 1925 review of *Language*, noted with disappointment, that Sapir did not support the *Weltanschauungstheorie* he subscribed to himself, citing two statements from Sapir to the contrary: "It is impossible to show that the form of a language has the slightest connection with national temperament" (Sapir 1921: 232), and, on the next page, "Nor can I believe that culture and language are in any true sense causally related" (Kruisinga 1925: 179). Indeed, as early as 1912, Sapir had affirmed that "apart from the reflection of environment in the vocabulary of a language, there is *nothing* in the language itself that can be shown to be directly associated with [the physical] environment" (Sapir 1949: 100; emphasis added: EFKK).

It is true however that we find in Sapir's 1929 paper, "The Status of Linguistics as a Science", remarks that sound 'Whorfian', especially when read out of context:

> Human beings do not live in the objective world alone, nor alone in the world of social activity as ordinarily understood, but are very much at the mercy of the particular language which has become the medium of expression of their society. It is quite an illusion to imagine that one adjusts to reality essentially without the use of language and that language is merely an incidental means of solving specific problems of communication or reflection. The fact of the matter is that the 'real world' is to a large extent unconsciously built up on the language habits of the group. […] We see and hear and otherwise experience very largely as we do because of the language habits of our community predispose certain choices. (Sapir 1929: 209–210 = 1949: 162)

In this quotation we may indeed see one – albeit only one – source for Benjamin Lee Whorf's (1897–1941) much more radical views on the subject of the interrelationship between language and world view. But as early as 1951, Zellig Harris, quoting another statement further down in the same paragraph from which this quotation was taken, noted:

> There is no contradiction here [and Sapir's statement of 1912, from which Harris quotes in support of his claim], since the 'environing world' is the physical world, whereas the 'real world', in quotes, is also called 'social reality' ([Sapir 1949:] 162) and constitutes the physical world as socially perceived: "Even the simplest environmental influence is either supported or transformed by social forces" ([Sapir 1949:] 89); "The physical environment is reflected in language insofar as it has been influenced by social forces" (90). (Harris 1951 = Item 39, note 22)

Despite Harris' observation, a close follower of Sapir's such as the linguistic anthropologist Harry Hoijer (1904–1976) of the University of Chicago, who was much interested in linguistic categories (e.g., Hoijer 1951) organized – and probably coined the term – a conference devoted 'Sapir-Whorf Hypothesis' (Hoijer 1954), and it

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seems that from then on the subject developed a life of its own. A first systematization of the subject was attempted by another linguistic anthropologist of the generation George L. Trager (1906–1992) in 1959. He had collaborated with Whorf at Yale University during the academic year 1937–1938, when Whorf had been offered to teach a course on Amerindian linguistics while Sapir was on leave (see Darnell 1990a: 380–381). Many scholars did continue to speak of the 'Sapir-Whorf Hypothesis', although by the 1970s there were those (e.g., Davis 1976, Jessel 1978) who argue that a more careful analysis of Sapir's – and Whorf's – writings would have to connect this much debated 'theory' with Whorf's name alone.[19] The subject was taken seriously enough to lead the late Helmut Gipper to engage in fieldwork on a Hopi reservation in order to put the hypothesis to a test (Gipper 1972) and in a controversy about his findings (Dürbeck 1975, Gipper 1976). Years later, one of his students conducted much more detailed studies in order to disprove Whorf's claims and what had been made of it (Malotki 1979, 1983). However, these findings did not lead to an abandonment of the hypothesis, and the subject has continued to attract considerable attention, especially psychologists (see the bibliography in Koerner 2002 [= Item 78]: 56–62; see also Lucy 1996, for an assessment of empirical findings).

8. Sapir's other engagements, including his literary pursuits

It probably was the Centenary Conference of Edward Sapir's birth held in the National Museum of Man in Ottawa early in October 1984 (see Cowan et al., eds. 1986) that drew attention the fact that Sapir had spent more years in Canada than at the University of Chicago and at Yale taken together. That he was not as isolated as he himself had tried to impress on many of his correspondents (cf. Daillaire 1984, for the astounding list of letters he wrote during 1910–1925), was perhaps first pointed out in Murray (1981a). As a result, no apology is offered for my apparent bias in this collection in favour of Sapir's residence in Canada, given the scant information usually found in the American literature on this most productive period of Sapir's scholarly career, while he held this "exceptionally favourable position in Ottawa" (Lowie 1984 [1956]: 124; cf. also Darnell 1984 [1976]: 174–176). Indeed, the anthropological museum in Canada's capital, now called National Museum of Civilizations once an entirely new building had been constructed opposite the Parliament buildings on the other side of the Ottawa river, and the Canadian National Archives bear witness to Sapir's anthropological and linguistic

19. For a very close analysis of Whorf's 'relativity principle', see Penny Lee, *The Whorf Theory Complex: A critical reconstruction* (Amsterdam & Philadelphia: John Benjamins, 1996), Chapter 3.

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productivity: many of the cultural products of the various native tribes of this huge country were collected and identified by him and there are manuscripts pertaining to his fieldwork among Amerindian languages that still await publication.[20] Sapir's work as research director and museologist during his years at the National Museum have therefore been given an entire section in this selection (see Part VIII: *Sapir and His Work at the National Museum of Man, 1910–1925*), which includes contributions by two authors (Bernier and Foster), who had for many years been employed as anthropologists and/or linguists in the Museum, and by William N. Fenton (1908–19??) of the State University of New York in Albany, who had held various research contracts there (see Items 54–56).

As Darnell (1990a: 164–165) in her section on "Ottawa Intellectual and Social Life", briefly circumscribes Sapir was definitely involved in it. The writing of poetry and the discussion of literary subjects played an important role in this. As we can see from the Sapir bibliography appended to Volume III of the 2007 selection and Mandelbaum's list of Sapir's "Poems" (Sapir 1949: 614–617), he wrote poetry quite frequently during the years 1917–1925, and continued thereafter publishing in Canadian literary periodicals such as "The Dial" and "The Canadian Forum" until 1928, but apparently no more after his move from Chicago to New Haven in 1931. He had stopped writing literary reviews in the same year. In 1917, he brought out a collection of his poms in Boston, but no further collection was ever published in regular form.[21] Thanks to Margaret Mead (1901–1978), one of Franz Boas' youngest

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20. In this connection it would not be inappropriate to refer to the tremendous work of John Peabody Harrington (1884–1961), a very close contemporary of Sapir (who exchanged letters with him during 1910–1915). As Ives Goddard (1996) reports, Harringon did not pursue a regular academic career (after receiving a B.A. from Stanford and following a one-year stint at the universities of Berlin and Leipzig 1905–1906), he appears to have devoted all his energies to the study of the native cultures and languages of California. For 40 years (1915–1954) he was employed as ethnologist by the Bureau of American Ethnology of the Smithsonian Institution in Washington, D.C., but many of his field notes and sound recordings of well over a dozen Indian languages, some of them now extinct, still await publication (cf. the announcement in *Bulletin of The Society for the Study of the Indigenous Languages of the Americas* No. 239 of 11 July 2006, for details).

21. I recall that the late William Cowan (1929–2001) of Ottawa had put together a typescript selection and indeed had argued in favour of publishing them, but the Sapir family, in particular Philip Sapir, vetoed any such move. In 1984, also on the occasion of the Centenary Conference, the late Tetsuro Hayashi (1921–2002) of Kyushu University, Fukuoka, Japan had prepared a small selection from Sapir's poems published in various outlets, including the two already mentioned as well as *Poetry, The Queen's Quarterly*, and *The Nation*. He circulated a revised and extended mimeographed 45-page booklet in 1985 among friends and colleagues, notably in Japan where, according to his Preface (p. v), Sapir's poems too had "received a favourable reception".

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students, we know of Sapir's intense correspondence with Ruth Fulton Benedict (1887–1948) especially during 1923–1928 concerning the writing of poetry (Sapir 1959: 158–190). The last Part of Koerner (ed. 2007), viz. Part XIII: *Sapir as a Student of Literature*, offers several articles dating from 1983–1990 which account for this side of Sapir's interests, but a thorough evaluation as favoured by Alfred Kroeber in 1959 (see volume I, Item 21) is still extant. It probably will have to await the publication of the bulk of Sapir's poems, many of which were circulated among friends only. It should be done by someone thoroughly familiar with Sapir's life (since many of the poems are autobiographical in nature) and the period in which they were written. This would require an effort to recapture the atmosphere of the literary circles of the time, including those in Ottawa, where Sapir was an active member of the local literary scene, exchanging letters with the Ottawa novelist Madge Macbeth (1878–1965) and the better known Ottawa-born poet Duncan Campbell Scott (1862–1947), who was, like Sapir, a civil servant in Ottawa eventually rising to the position of deputy superintendent-general for Indian Affairs.

As noted earlier, the bulk of Sapir's poetry was written during his Ottawa period and his first couple of years in Chicago, where his poetic pen began to run dry following his marriage, in 1926, to Ottawa-born Jean Victoria McClenagan (1899–1979). For an appreciation of Sapir's poetry, I may refer in particular to the 1983 paper by Richard Handler, "The Dainty and the Hungry Man: Literature and anthropology in the work of Edward Sapir" (see Item 79) and his contribution to the Ottawa Centenary Conference (Item 81) as well as the selections he made of Sapir's reviews and articles with literary content in Sections 4 and 5 of Sapir's *Collected Works* (Sapir 1990–), volume Tome III.

9. Concluding remarks

If one were to speak of Sapir's – enduring – legacy, it most likely would not be in relation to areas such as sociology, ethnology, psychology, and other not strictly linguistic areas, but rather to his work devoted to the study of the Indian languages of North America and the at times rather astounding hypotheses he put forward about their genetic relationships. This I trust becomes clear from the present selections too, which not only is evident from Part VII ("Comments on and Evaluations of Sapir's Work on Amerindian Languages") but also articles and reviews found elsewhere in these pages (cf., e.g., Items 41, 42, 45, 47, 57 through 62). That this legacy is assured is not only evident from the republication of a number of Sapir's studies on the native languages of Canada's West Coast and of the Southwest in the United States (see Sapir 1990– volumes V–XII, X, and XIV, published thus far), but

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in the annual meetings, bulletins, and newsletters of the Society of the Study of the Indigenous Languages of the Americas (SSILA) and the truly impressive activities surrounding it since 1981.[22]

Apart from the pioneering work in the analysis of native languages he performed and the linguists he trained during his Chicago and New Haven years (1925–1938), among them Stanley Newman, Mary Haas, Carl Voegelin, and others and, in turn, their students (see the "Reminiscences" section in Cowan et al., eds. 1986: 371–404, for interesting details), there are other things – besides the general 'scripta manent' factor – that will assure that Edward Sapir will not be forgotten. Given my Ottawa biases, I'd first like to mention that when preparing the Centenary Conference, Michael Foster (who then still was on staff at the National Museum), William Cowan and myself had also conceived of the idea of a memorial plaque to be mounted on the wall in the entrance hall to the Museum, and indeed the governmental commission to which we submitted the request (and the bilingual English/French text) agreed with our project. The plaque was unveiled half a year following the Conference (see the photographs of the plaque, the ceremony, and the text of Michael Foster's historical sketch, all appended to the proceedings (Cowan et al., eds. 1986: 606–616). Also, soon after the Ottawa meeting the Linguistic Society of America established the Edward Sapir Chair to be conferred on a major scholar in the field teaching a course at their biannual Summer Linguistic Institutes, and mention could also be made of the establishment by the Society for Linguistic Anthropology, in 2001, of an Edward Sapir Book Prize "to be awarded alternate years to a book that makes the most significant contribution to our understanding of language in society, or the ways in which language mediates historical or contemporary sociocultural processes" (source: *SSILA Bulletin* No. 149 of 1 November 2001).[23]

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22. In this connection, mention must be made of Victor Golla of Humboldt State University in Arcata, California, incidentally born only a few days after Sapir's death, who like no other has been the mind and soul of this Society which by now counts members from all over the globe. Apart from his own important scholarship in the field of American Indian linguistics and his vivid interest in the history of the subject, he has displayed the organizational talent that turned an informal gathering of anthropological linguists started by the Sapirian Charles Frederick ("Carl") Voegelin (1906–1986) at Indiana University, Bloomington, Ind., many years earlier into an extremely well structured organization.

23. Earlier, in 1988, the University of Ottawa had established an Edward Sapir scholarship to be awarded to a promising graduate student in linguistics, and I would not be surprised if similar grants or honours had not been created elsewhere in Sapir's name.

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Acknowledgements

This chapter in essence retakes the text of the Introduction I wrote for the 3-volume anthology *Edward Sapir: Critical Assessments of Leading Linguists* (London & New York: Routledge, 2007), vol. I, 1–28. This anthology is referred to hereafter as Koerner (ed., 2007). It should be clear from the start that the 'assessments' (plural) were made by the scholars therein anthologized, not by myself. For a much more competent overall assesssment of Sapir's contribution to linguistics and its enduring legacy there is more than one would expect in Golla (2001)'s 15 pages. – I have to thank Pierre Swiggers (Leuven/Louvain) for his many critical comments which defintitly have improved this chapter.

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24. This bibliography does not contain a few items which are however listed in Koerner (1984). On the other hand, it includes several entries which I do not refer to in this chapter (and did not include in the 3-volume 'Critical Assessments'), but which I thought readers might expect to find here.

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CHAPTER 8

The Baxtin myth and its historiography

1. Prelude: Some remarks on Russian linguistic historiography

It is perhaps presumptuous of me to say anything about a subject in this short chapter which should properly fall within the bailiwick of a Slavic language specialist. The fact is that I am not conversant with Russian (beyond possessing the rudimentary ability to use a dictionary), and this alone disqualifies me from pronouncing on this subject with any authority. However, if I do so, I must give clear reasons for overstepping my bounds of competence within an area that I have not previously been intellectually and scholastically involved. At least I can claim that, despite my linguistic limitations, I have made extensive and serious attempts to keep abreast of published work on Russian (as well as Slavic) linguistics, beginning with Sergej Konstantinovič Bulič (1859–1921) multivolume opus *Očerk istorii jazykoznanija v Rossii* (St. Petersburg: M. Merkušev, 1904), which interested me in particular during the 1970s since it began with a Russian translation (pp. 1–148) of the third edition of Berthold Delbrück's (1842–1922) influential *Einleitung* (1893 [1880]) and included at least some remarks on Mikołaj Kruszewski (1851–1887) and on Jan Baudouin de Courtenay (1845–1929), of whom Bulič was a former pupil. As the record shows (cf. Koerner 1972, 1973 [1971], 1986), I had an early interest in these two Polish scholars working in Czarist Russia (and publishing the bulk of their work in Russian) because of my research into the sources of Ferdinand de Saussure's (1857–1913) inspiration for his general linguistic and, apparently, 'structuralist' thinking (see Koerner 1973 [1971]: 133–165).[1] Also, despite its subtitle, my 1978 bibliographical survey of works in 'Western' history of linguistics, the book also includes brief descriptions of a number a books by Russian scholars, such as the following – Rozalija Osipovna Šor (1894–1939)'s 1938 work, Solomon Davidovič Kacnel'son's (1907–1985) essay (1941), Viktorija Nikitična Jarceva's (1906–1999) article (1954), or the very important anthologies compiled by Vladimir Andreevič Zvegincev (1910–1984) that acquainted the Soviet public with structuralist trends from the Prague tradition, from the writings of Hjelmslev as well as from Chomsky and others (cf. Koerner 1978: 21, 22, 27, 29, 28, 42). In a different manner, the

1. But I also have given the work of Baudouin de Courtenay (cf. Koerner 1972) and Kruszewski (cf. Koerner 1986, 1995) my attention in their own right.

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last-mentioned service had also been rendered by Mirra Moiseevna Guxman's (1904–1989) 360-page monograph *Osnovnye napravlenija strukturalizma* (Moscow: Izd. "Nauka", 1964), which was followed in 1966 by Jurij Derenikovič Apresjan's (b.1930) 300-page 'brief survey' *Idei i metody sovremennoj strukturnoj lingvistiki* (Moscow: Izd. "Prosveščennie"), which was soon translated into several Western languages (see again Koerner 1978: 44, for details).[2]

Before turning to the particular theme of this chapter, I would like to mention the various efforts of the late Fedor Mixajlovič Berezin (1930–2003), who burst onto the scene in 1968 with his first book devoted to the history of linguistics (Berezin 1968; cf. Koerner 1978: 52–53) and who from early on in the life of *Historiographia Linguistica* (see entry on Berezin 1973 in volume I, fasc. 3, p. 291 [1974]) kept sending me review copies of his books, which followed in short order in 1974, 1975, and 1976 (cf. the descriptions in Koerner 1978: 71, 73–74, and 77).[3] What was characteristic of Berezin's work is that he dealt with the history of Russian (and European) linguistics up to World War I; the author appears to have been most comfortable with the 19th century (e.g., Berezin 1976, 1979).[4] It seems that he was not the exception but rather the rule at the time. A typical example of this kind of largely derivative history of linguistics – from the Ancient Greeks to the Neogrammarians and their opponents – was the one by T. A. Amirova (b.1928) et al. (1975), which was eventually published in German translation in East Germany (Amirova et al. 1980).[5] Only a few Soviet scholars during the 1960s and later decades were bold

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2. I shall leave aside books by the Latvian-born Jan Viljumovič Loja (1896–1969) of 1968, Amirova et alii's 559-page *Očerki po istorii lingvistiki* of 1975 (cf. Koerner 1978: 54, 73 for brief descriptions) – cf. the German translation of 1980, and other pieces that I did manage to lay my hands on then and thereafter but which I listed in Koerner (1978: 86, 98, 99, 101), such as those by a certain Viktor Andreevič Polovcov of 1874 [*sic*], by A. M. Dokusov of 1955, Viktor Vladimirovič Vinogradov (1895–1969) of 1958, and Aleksandr Aleksandrovič Reformatskij (1900–1978) of 1970 (Koerner 1978: 86, 98, 99, 101, respectively).

3. Review notices of other books by the same author can also be found in *Historiographia Linguistica* 2.274–275 (1975), 3.418–419 and 267–268 (1976), and later issues.

4. But see also Berezin, ed. (2002–2003), which documents that he was on his way to producing work in 20th-century linguistics beyond the early period toward the end of his life.

5. Only in the short concluding chapter, "Neue Gedanken und Methoden in der Sprachwissenschaft des 20. Jahrhunderts" (Amirova et al. 1980 [1975]: 471–477), do we read the names of Saussure and the main concepts laid out in the *Cours*, of the so-called 'Strukturalismus' of Prague, Copenhagen, London (!) and the one in the United States (read: Bloomfield, Harris, Chomsky) without any details of what 'structuralism' in these various articulations entailed. A number of further names are mentioned in passing, including Baudouin de Courtenay and Kruszewski as precursors of the phonology of Trubetzkoy and Jakobson. There seems to be something offered for everyone.

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enough to deal with various kinds of Western structuralism. (Even the Czechs had to refer to Prague structuralism as 'the Prague tradition' or some other circumlocution in order to avoid problems with the authorities of the day.) The external circumstances had much to do with the choices that historians of linguistics in Eastern Europe made – or felt they had to make – at the time. It was easier for scholars at the period of *glasnost'* and, in particular, following the fall of the Berlin Wall to deal with linguistics in Russia from the time of the October Revolution to the present. A typical example of the new circumstances was the monograph by Vladimir Mixajlovič Alpatov (b.1945), a specialist in Japanese and a representative of the younger generation, on the alleged myth of Marrism (Alpatov 1991), a subject which the Berkeley Slavist Lawrence L. Thomas (1924–2009) had already treated much more thoroughly some thirty-five years earlier (Thomas 1957), though without the benefit of access to local knowledge and archival material.[6]

It may be due to my ignorance of Russian scholarship in the history of linguistics however, that it appears to me that little attention to the subject had been given in post-Communist Russia. It may well be that other subjects – not only in linguistics! – have come to be seen as more important (a possible exception: Alpatov 1998).[7] As a result, it has been left largely to scholars outside of the country to treat the history of linguistics in Russia, as Patrick Sériot (b.1949) and his students and associates at the University of Lausanne have been doing for some time (see, e.g., Sériot 1999; Sériot, ed. 2005). There is of course nothing wrong with *slavisants* from outside Russia taking such an active interest in the area (cf., much earlier, Bruche-Schulz 1984), but it is my personal conviction that any country should account for its own history as a matter of course, and this, one would hope, critically. This applies in particular to the investigation of the influence of ideologies of various sorts on linguistic argumentation and their impact on the development of the discipline. I am not thinking necessarily and by no means exclusively of ideological stances maintained by political regimes, but also of the perhaps more subtle, but by no means less insidious, views held by a group of scientists, a section of society, or

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6. It is true that the goals of these two books very entirely different: Thomas's had been to analyze the underpinnings of Marrism, Alpatov's to treat its rise and fall. As we shall see later, Alpatov has done much more work on the history of linguistics over the years.

7. Both works follow well-trodden paths and are more designed to inform Russian readers about Western linguistics from the 16th century onwards, with a chapter on "Soviet linguistics of the 1920s–1950s" (228–266) sandwiched between one on the 'Whorf hypothesis' and 'Criticism of linguistic structuralism', at least to some extent continuing where Berezin's writings left off. Even Amirova et al. (1975) has made its reappearance under a new title (Amirova et al. 2003). Kondrašov (1996) actually goes back to 1979; its coverage stops in the early 1970s. The author lived 1919–1995.

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nationalist élites which tinges the outcome of research findings. Take for example the long-standing debate concerning the original home of the Indo-Europeans over the past 150 or more years where its geographic location often depended on frequently not openly stated preconceived, nationalist, at times even racist ideas (cf., e.g., Koerner 2000, 2004; Kuzmina 2004). On the other hand, I am wondering whether there were not enough *linguistic* aspects involved in the so-called 'Aryan myth' that historians of this field should do the proper investigations of rather than leaving it to political historians to undertake who obviously have little knowledge of linguistics and its history (e.g., Laruelle 2005).[8]

2. *The authorship of* Marxizm and the philosophy of language

For years I have been puzzled by the frequently reiterated claim that the literary historian Mixail Mixajlovič Baxtin (1895–1975), who since his death has become a celebrated theoretician of literary discourse and 'narrativity' (especially in North America and among those who, in my opinion, had nothing better to do than to ape what Americans had turned into an industry) and who, during the late 1920s and early 1930s, had published important work under a pseudonym, in the present case under the names of persons with whom they were closely associated. Already in Czarist times and again during the Stalinist era it had not been unusual to use a pseudonym, when politically or otherwise socially sensitive subjects were treated. At times books appeared anonymously (also in the West, for instance following the 1848 Revolution). However, if in fact Baxtin had been using the names of living persons with whom he was associated, namely Pavel Nikolaevič Medvedev (1891–1938) and, in particular, Valentin Nikolaevič Vološinov (1895–1936) while they had still been alive,[9] this technique does raise moral concerns. While I shall leave

8. To illustrate the author's ignorance in the history of linguistics, let me refer to just a couple of examples that even the most modest textbooks in the field would not exhibit. On p. 105 of her book, the author states that the first edition of Bopp's *Comparative Grammar* appeared in 1816 (not 1833) and makes it appear that Bopp was opposing Schleicher's views, which were expressed in 1861. A few pages later, Laruelle affirms – without supplying any evidence – that "Les linguistes slavophiles [which Schleicher himself evidently was] condamnent […] les travaux [which?] de Schleicher, qui mettent le slave dans une position de dépendance face à l'allemand" (2005: 109). No textual proof for his claim is supplied.

9. Other such supposed cover names used by Baxtin were Lev Vasil'evič Pumpjanskij (*alias* Leib Meerovič Pumpjan, 1891–1940), who published a work on Dostoevskij about the same time as Baxtin, and, with probably more justification, in the case of a two-part article on vitalism, which appeared in 1926 under the name of the of Baxtin's biologist friend Ivan Ivanovič Kanaev (1893–1984).

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it to others to discuss the ethics of such a conduct, one has to remember that many Russian intellectuals of the period, among them the arguably most distinguished student of Baudouin de Courtenay from his St. Petersburg years, Evgenij Dmitrievič Polivanov (1891–1938),[10] Medvedev, another distinguished student of Baudouin de Courtenay from his St. Peterburg years and – as is frequently (albeit mistakenly) maintained in the literature – Valentin Nikola'evič Vološinov (1895–1936) died of tuberculosis; he never had any political problems. However, it must be pointed out that in the 1930s arrests, persecutions, banishment and, in many instances, liquidations of persons suspected of not adhering to whatever the political leadership thought proper were not uncommon events. Here, my focus is on the 1925–1930 period which was, comparatively speaking, liberal, and during which the writings of 'disputed texts' by the members of the so-called 'Baxtin Circle' appeared. In other words, one may wonder whether already during this period particular caution on Baxtin's side was required that made him use the identity of close friends and associates to publish his views which, at the time they appeared, can hardly be regarded as particularly politically sensitive or counter-revolutionary.

If the ascription to Baxtin of works published under the names of others is maintained in the literature, as they frequently appear to be, I am particularly interested in finding out the truth concerning the authorship of at least one important work of the period, *Marksizm i filosofija jazyka* [Marxism and the philosophy of language], which first appeared in Moscow in 1929 under Vološinov's name and with a second edition the following year.

While Jakobson and Trubetzkoy had left the Soviet Union early enough to be saved for linguistics, "Bakhtin, the author of *Marxism and the Philosophy of Language*, probably owed his own survival entirely to his cover agents Voloshinov and Medvedev, who likewise disappeared later in the purges" (if we are to believe Marina Yaguello (1991 [1984]: 79).[11] This claim is all the more surprising as Yaguello (p. 209) is aware of the 1973 English translation, in which Vološinov is identified as the author, an ascription which has been followed by the editor of the German translation (Vološinov 1975). In her own translation of the text

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10. He had dared to publically criticize Marr's 'Japhetic' theory and its scientific value for a Marxist linguistics (Polivanov 1929). However, this public criticism may not have been the sole reason why Polivanov was shot. After all he was a Japanologist, and he may well have been suspected to be a spy for the Japanese, being consequently shot by the Russians (Alpatov 1991, p. 113).

11. The ascription is maintained in two notes (p. 202 n.1 and p. 203, n.22) and in the bibliography (p. 209). Like Medvedev, who is best known as the author of a book on literary theory (Medvedev 1976 [1928]), Vološinov is written out of the record; their names do not appear even once in the index (217–223).

Indeed, as I mentioned earlier Vološinov died of tuberculosis in 1936.

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into French, she assigns the book to Baxtin (Vološinov 1977). Indeed, since she refers not infrequently to Roman Jakobson (1896–1982) as an authority on a variety of topics (see Yaguello 1991: 42, 68, 100–101, 103, 115–116), it is strange that she should have ignored Jakobson's famous 1957 "Shifters" article, in which Vološinov is identified, without comment, as the author of *Marksizm i filosofija jazyka* (Jakobson 1971 [1957]: 130, 147).

The ascription of the last-named book to Baxtin could be disregarded if it was a single occurrence and made by a scholar who tended to be somewhhat careless with the facts.[12] However, I have come across this claim quite frequently over the years; it appears that what the late sociologist of science Robert K. Merton (1910–2003) has termed 'The Matthew Effect' is at work here: "For unto everyone that hath shall be given, and he shall have in abundance; but from him that has not shall be taken away even that he hath" (St. Matthew XII, 12 in the wording of the King James version of the Bible). For instance, although reference is made to the German translation of *Marksizm i filosofija jazyka* (1975 [1929]), which gives Vološinov as the author, Tat'jana Naumova (2004: 214, 220–221) identifies Baxtin as the author, claiming that Baxtin was distinguishing between two trends of linguistics at the time, 'individualist subjectivism' and 'abstract objectivism' (which especially Saussure is supposedly guilty of), and that in his 1929 book on Dostoevskij Baxtin had used the term 'metalinguistics' (p. 220), a term which is usually associated with the name of the Polish logician Alfred Tarski (1902–1983).[13] As if to add insult to injury, Michael Hohlqvist and others in the same collective volume in which Naumova's article appeared, also claim Vološinov's critical essay on 'Freudianism' of 1927 for Baxtin (Hohlqvist et al. 2004: 264), in fact they added other titles to the list, including Medvedev's well-known 1928 book on a formal method in literary criticism (ibid., 264–265), as he and another colleague had forcefully argued twenty years

12. Thus, she writes (p. 68) that "Marr's 'research' began in 1866", when, as a matter of fact, Marr (1865–1934) was barely one year old. On the preceding page, Marr's birthyear is given as 1863. Saussure is described (p. 82) as "[a] distinguished Sanscrit scholar, [who] just [Yaguello has the period 1890–1900 in mind] refused Michel Bréal's chair at the Collège de France", where in fact Saussure had left Paris for a professorship in Geneva in 1891 and Bréal (1832–1915) held on to his chair at the Collège de France until 1905, when Meillet, a former student of Saussure's, took over the position. In fact, such an offer was never made and Saussure, a Swiss citizen, could never have been an incumbant, given the rules of the Collège. From all we know of Saussure, there was no intention on his part to surrender his Swiss citizenship as his compatriot Jules Gilliéron (1854–1926) had done.

13. Although it appears to have been introduced by his teacher, Stanislaw Leśniewski (1886–1939), it was Tarski who used it extensively from 1931 onwards (see Koerner 1995 [1993]: 28–29, for details). Indeed, as Matejka (1996: 266) has pointed out, this term can nowhere be found in Baxtin's 1929 book but only in the expanded 1965 edition of his work on Dostoevskij.

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earlier already (Clark & Holquist 1984: 148–149, 151).[14] One cannot but express astonishment that Baxtin should have written three thematically quite different books within three years, in addition to other works were his authorship is not in doubt as they appeared under his own name (e.g., Baxtin 1929).[15]

In this short chapter, no attempt will be made to untie all the knotty issues that appear to be involved in these reiterated attributions. Interestingly enough, in the same 500-page collective volume in which the articles by Naumova and Hohlqvist and others were published, there are at least two authors who cast doubt on Baxtin's authorship of *Marxizm and the Philosophy of Language*. Jeanette Friedrich, in the intense debate ("heftige Diskussionen") concerning matters of authorship of the works of the 'Baxtin Circle', argues that on the basis of stylistic similarities between three 1930 articles by Vološinov and the 1929 book, all of which appeared under his name, argues in favour of Vološinov (Friedrich 2004: 118–119 n.10, 123). A much more explicit identification of Vološinov as the author of this work and several others has been made by Katharina Meng, co-editor of this collective volume. She separates quite clearly between Baxtin's life and work (Meng 2004: 154–165, 179–185) and Vološinov's (165–179). In fact, Meng appears to have updated her paper somewhat in comparison to the other contributors.[16] She refers for her biographical sketch of Vološinov to an account by Vasil'ev (1995) and a book-length publication of conversations conducted with Baxtin in 1973 (Duvakin 1996). She thereby clarifies several misapprehensions. For instance, Vološinov was not a victim of Stalin's purges, as has generally been assumed in the literature, but in fact died in 1936 of tuberculosis from which he had suffered since 1914 (as Vasil'ev had been able to ascertain through interviews with Vološinov's widow). More importantly for the present discussion, Meng (2004: 163) quotes Duvakin, according to which Baxtin told him in 1973: '[…] I had a close friend – Vološinov. He is the author of the book "Marxism and the Philosophy of Language", a book which has been, in a manner of speaking, attributed to me' (Duvakin 1996: 77–78). Such a statement is not *per se* a really solid attribution of authorship, though it is an indicator of what kind of opinion had already been in circulation at the time.

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14. The American translator of the book in question, Albert J. Wehrle, "solved" the problem by placing the names side-by-side as M. M. Bakhtin / P. N. Medvedev, while claiming Baxtin's sole authorship in his "Introduction".

15. This productivity is all the more astounding when we read that Baxtin was much of his life of ill health and in 1938 had a leg amputated (Meng 2004: 156n.6). Maybe the fact that all three books had appeared with the same Leningrad publisher lent some support to Baxtin's authorship?

16. The bulk of Ehlich & Meng (2004) goes back to a conference held March 1991 (cf. The editors' "Vergegenwärtigungen", p. 11); there is little evidence that much updating occurred thereafter.

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What apparently had been rumor among literati in Russia from the 1960s onwards – Aleksej Alekseevič Leont'ev (b.1936) has been cited as having been the first to speak of the existence of a 'Baxtin Circle' (Leont'ev 1967: 86–88; cf. Clark & Holquist 1984: 375 n.2) – appears to have been expanded by Vjač. Vs. Ivanov from 1970 onwards to suggestions that *Marksism i filosofija jazyka* in particular should be regarded as in fact authored by Baxtin, and not by Vološinov (Ivanov 1973 [1970]). The occasion was a November 1970 meeting of the Association for Structural Linguistics at Moscow State University to mark Baxtin's 75th birthday (see Ivanov 1976: 343; cf. Revzin 1971). These were the early days of the development of Semiotics as a cultural industry. In the printed version of this 1970 paper, published in Tartu, Estonia, at whose University Jurij Mixajlovič Lotman (1922–1993) had recently established a school of semiotic studies, Ivanov advanced the claim that Baxtin must be seen as a brilliant forerunner of modern-day sign theory. In the 1976 "revised and expanded" English translation, Westerners could first see five works that had appeared under Vološinov's name between 1926 and 1939 [read: Vološinov 1930b] and Medvedev's (1928) book listed under the heading of "Cited Articles and Books by M. M. Bakhtin" (p. 342) and in the concluding endnote 101 (p. 366) the affirmation:

> The basic texts of works 1–5 and 7 [i.e., those that had not appeared under Baxtin's name] are by M. M. Bakhtin. His students [!] V. N. Voloshinov and P. N. Medvedev, […], made only small insertions and changes in particular parts (and in some cases, such as [p. 5],[17] in the titles) of these articles and books. That all the works belong to the same author [i.e., Baxtin], which is confirmed by the testimony of witnesses [no one is identified], is evident from their very texts, as one may easily convince oneself by the quotations presented.

As Ken Hirschkop (1999: 126–127) surmises, Ivanov, for "local reasons",[18] when "presenting Bakhtin as semiotician, needed to attribute Voloshinov's theory of the sign to Bakhtin in order to make credible the description of Bakhtin as a figure in the development of a regional 'science'." Ivanov's views on Baxtin's pioneership were soon carried to the West (e.g., Ivanov 1975, 1976, 1985) and, it appears, that they soon became a widely accepted opinion. Holquist (1986: 77) conceded that "Baxtin himself was less concerned with the nature of the sign as such", but the ascription of the works of Medvedev (1928) and Vološinov (1926, 1927, 1929) as "primarily the work of Baxtin" (p. 76) remained intact. Interestingly, two years earlier, he and his colleague had already complained about their difficulty in obtaining reliable

17. Ivanov is referring to *Marksizm i filosofija jazyka* by this number.

18. I presume that Hirschkop had the Tartu–Moscow axis in mind which Ivanov and Lotman had established.

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data, noting that "not all the official documents can be taken as reliable. Some of them, particular his [i.e., Baxtin's] work records, were drawn long after the period covered, by which time Bakhtin himself was vague about the facts […]. There are also inconsistencies and contradictions among the extant documents" (Clark & Holqvist 1984: ix–x), observations that did not prevent them from mounting the strongest claims possible regarding Baxtin's authorship of so many works that had appeared under the names of others, apparently helped by his "eloquent wife" who insisted that books like Medvedev (1928) and Vološinov (1929) were Baxtin's (cf. Matejka 1996: 264).

3. Coda

If I accept the view of those who believe that Vološinov was not only the author of *Marksizm i filosofija jazyka* of 1929 but also of various other articles and essays, notably the one on 'Freudianism' (1927)[19] and the critique of Western linguistics (1928), which illustrates not only the author's acquaintance with the *Cours de linguistique générale* but also with the recent work of such greats as Karl Bühler (1879–1963), Ernst Cassirer (1874–1945), Antoine Meillet (1866–1936), and others, the question may be asked why Baxtin should have been credited with any major writings by Vološinov in the first place. After all, especially *Marksizm i filosofija jazyka*, but also the critique of Western scholarship of the year before demonstrates the author's familiarity with the linguistic literature of the period not found in the writings where Baxtin's authorship is not in question. Baxtin had a background in classical philology – although there is no indication that he has acquired any degree in this field when he attended St. Petersburg University during 1914–1917; there is no record of his ever having formally registered there (see Hirschkop 1999: 141) – and certainly not in linguistics, a subject studied quite assiduously by Vološinov during the 1920s (cf. Meng 2004: l64–165).

I do not presume to firmly resolve all the historical issues, but I can, at least, offer a few suggestions as to how these misascriptions could have occurred in the first place. Many years ago, when the question of authorship was not much of an issue for me, Vjačeslav Vsevolodovič Ivanov (b.1929) told me that he personally had it from Baxtin that the book on *Marxism and the Philosophy of Language* had been his, though published under the name of V. N. Vološinov. I don't recall whether he referred me to his writings at the time where he had stated this in print (see Ivanov

19. Interestingly, the publishers of the Italian translation of *Marxismo e filosofia del linguaggio* and *Freudismo* (Baria: Dedalo, 1976–1977) had no qualms to attribute these two books to Vološinov (see Eschbach & Eschbach-Szabó 1986: 732).

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1973, 1975), but it appears that on his authority this affirmation was accepted by the generality of scholars, certainly in the West and notably by those who did not read Russian. For instance, Grübel (1979) took this as a matter of fact and Ivanov himself (1985) also maintained it, without even mentioning his earlier claims. Still, the translation into 1973 English of this work, evidently undertaken at the instigation of Roman (Osipovič) Jakobson (1896–1982),[20] by Ladislav Matejka (b.1919) and the late Irwin Titunik (1929–1998) undertook the translation.[21] Here, the authorship was attributed to Vološinov in no uncertain terms. In my opinion, this should have led at least Slavists to take a closer look at the Russian text and compared it to works that were undoubtedly written by Baxtin.

Instead, it appears two American scholars, Katerina Clark (b.1941) and Michael Holquist (b.1935), both professors of comparative literature at Yale University, produced a monograph on Bakhtin (Clark & Holquist 1984), in which they broadened what Hirschkop (1999: 127) had termed Ivanov's "bibliographical assault". In *Mikhail Bakhtin*, they made such a strong case for Baxtin's authorship of various works which had appeared under the names of Medvedev and Vološinov during the late 1920s, that these attributions became widely accepted, at least until the late 1990s.[22] While conceding that "no account of how and by whom these texts were written can ever be indisputable", they maintained (p. 147) that

> […] there is good reason to conclude that the disputed works were written by Bakhtin to the extent that he should be listed as the sole author,[23] Medvedev and Voloshinov having played a largely editorial role in each instance. For one thing, nothing has established that Bakhtin could *not* have written the disputed texts and published them under friends' names. More important, many eyewitnesses have said that he was the author, as did Bakhtin and his wife on private occasions.

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20. In fact, a year earlier another former Jakobson student like Matejka, Cornelis Hendrik van Schooneveld (1921–2003), had seen to it that the 2nd (1930) edition of this book was reprinted in his "Janua linguarum; Series anastatica" series as its number five (actually only three volumes appeared in that series; cf. *The C. H. van Schooneveld Collection in Leiden University* ed. by Jan Paul Hinrichs, p. 176. Leiden: Leiden University Library, 2001).

21. Titunik also translated the other major supposedly 'disputed text' of Vološinov (1927) in the same year.

22. These attributions are still maintained in Holquist et al. (2004) as I noted earlier in this paper (but see footnote 11, above).

23. In fact, throughout their book, whenever titles had appeared under the names of Medvedev or Vološinov, their names are always placed within double quotation marks (cf. Clark & Holquist 1984: 377, notes 25–32, 35–36, even in the case where Matejka & Titunik had identified Vološinov as the author (p. 377n.33). The same technique is also maintained with regard to the *Freudianism* book (see ibid., p. 379, notes 9–25).

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However, nobody to my knowledge has been able to "produce the promised smoking pen" (Hirschkop 1999: 127); in the face of the fact that Baxtin refused to formally claim authorship, Clark & Holquist (1984: 148) admitted that whenever the various Russian scholars "asked Bakhtin directly about whether he had authored the disputed texts, he either avoided the question or was silent." Still they brought all sorts of arguments to bear in order to maintain their story, including the one that "Medvedev and Voloshinov were sufficiently cynical to see no harm in such a thing" (p. 151), i.e., having their names being used by Baxtin as a cover.

This is all the more astonishing as the authors interviewed Vološinov's widow in 1983 who told them that around 1927 Vološinov had become a believer in Marxism (Clark & Holquist 1984: 117, 370). Why they did not ask her whether or not Vološinov had written *Marxism and the Philosophy of Language* must remain a mystery. It seems that once they had convinced themselves of Baxtin's authorship of this book, they saw no reason to question it. Not only Vološinov but Medvedev too had embraced Marxism to the extent that, as Matejka (1996: 263) notes, their work displayed "striking similarities" in contrast to Baxtin's approach of the same period, which was non-Marxist and followed German neo-Kantian ideas, to the extent that in his work he effectively plagiarized Broder Christiansen's *Philosophie der Kunst* of 1909, of which a Russian translation had become available in 1911. However, Baxtin never overtly acknowledged this source (cf. Matejka 1996: 258–261 *passim*).[24]

In 1973, Baxtin, who died in 1975 at age 80, was an old and frail man, and it may have been that he had forgotten things that had occurred some forty-five years earlier, and that indeed he might have suggested to Ivanov and others that certain books that had appeared under the name of his friends Medvedev and Vološinov were in fact his. Those who have listened to Noam Chomsky over the past thirty or more years and read his accounts of where he had taken his inspiration from may, if they are not hagiographers, have noted the many revisions of his intellectual past he has offered between 1973 and the present, and so lapses in memory could account for Baxtin's human failings. Still, it is interesting that Baxtin is reported to have refused to claim the copyright to these books when it had been offered (Hirschkop 1999: 127; cf. also Matejka 1996: 264). So, there must at least have been a hesitation on Baxtin's part to lay claim to work that wasn't his. The question that I shall not pursue here is why have Baxtin's heirs have apparently been silent on this issue or preferred to leave the issue of authorship in the air; see Hirschkop (1999: 127–139) for an analysis of the different strategies employed by the various players in the game to muddle the waters.

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24. In fact, as Matejka (1996: 262–265) has shown, unlike Vološinov for instance, Baxtin was rather 'shy' when it came to indicate his sources, among which Leo Spitzer's (1887–1860) writings between 1910 and 1919 were particularly prominent.

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In Russia itself, research into the issue of authorship has been spearheaded by Alpatov since the early 1990s. Indeed, since Nikolaj Alekseevič Pan'kov had discovered, in the archives of St. Petersburg, a 1928 draft of *Marxism and the Philosophy of Language* that Vološinov had prepared as a report on his doctoral studies during the academic year 1927/28 at the University of Leningrad, it appears to have been evident at least to the two scholars (Alpatov & Pan'kov 1995, Alpatov 1995) that Vološinov was indeed the author. It simply required Slavists in the West to make their findings more widely known (cf. Meng 2004: 164–165, for a report). However, this is not quite the case. In Alpatov (2004 [2000]: 71), the author is still uncertain what side to take:

> I do not propose to examine here the tangled question about the authorship of *Marxism* and the other publications that appeared under Voloshinov's name, as I have outlined my view of this ultimately insoluble problem in other articles [footnote and reference omitted, but see Alpatov 1995]. I will simply say two things. First, there are to date no grounds for considering that these publications are the work of Bakhtin alone and for ruling out Voloshinov's authorship. Second, Voloshinov's oeuvre, […], should be viewed as a whole, including not only *Marxism* and the article 'The word in life and the word in poetry' [Vološinov 1926], but also his three articles published in the journal *Literaturnaia učeba* [e.g., Vološinov 1930b].

While I have no strong opinion on these three 1930 articles, which appeared in English translation with Vološinov's name attached (Shukman, ed. 1983: 93–152), I note with some surprise that Alpatov does not mention either *Frejdizm* (Vološinov 1927) or another 1930 article which has a dedicatedly linguistic turn to it (Vološinov 1930a),[25] also because Alpatov expatiates on Vološinov's 'training in linguistics' between 1922 and 1924 (p. 77). Even in his most recent monograph devoted to the relationship between Baxtin and Vološinov Alpatov cannot bring himself to a clear decision regarding Vološinov's authorship of Marxism, although he's leaning toward it, following a lengthy history of the argument (Alpatov 2005: 94–110):

> I suggest that the text of *Marxism and the Philosophy of Language* and other works of Vološinov were probably written by Vološinov with a review of the ideas, formulations, and sometimes perhaps phrases and fragments thought up by Baxtin and on the basis of the common conception of the Circle. Baxtin took responsibility for the common conception after Vološinov's death, but not for the details of Vološinov's work. (Alpatov 2005: 118; translation supplied by Craig Brandist)

25. Clark & Holquist (1984: 163) appear to have had an easy game. Having assigned the authorship of Vološinov (1926, 1927) to Baxtin, they felt free to claim that Baxtin "by 1926 […] had already turned his attention to linguistics."

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Since I am not tied into matters of literary theory and related issues in which Mixail Baxtin's has been very popular, notably in the Anglo-Saxon world, I have no way of knowing whether Ken Hirschkop' book, *Mikhail Bakhtin: An aesthetic for democracy*, in which he thoroughly treated the entire affair (Hirschkop 1999: 126–140) had any immediate effect. If not, the collective volume edited by Craig Brandist and others, which contains the translation with detailed commentary of archival work related to this matter (see Brandist et al. 2004: 223–250), should eventually lead anyone interested in the proper assignment of authorship, namely, that Vološinov did in fact write, among other so-called 'disputed' works, *Marksizm i filosofija jazyka* in 1929. As far as I can read the literature, few if any have asked themselves how Baxtin, a literary scholar without any linguistic training could reasonably have been familiar with all the Western European scholarship that Vološinov displays in his book. This alone should have raised more than an eyebrow. For those who still want to believe in this Baxtin myth, I would turn the tables and invite them to prove, on the basis of a computerized statistical investigation, that they had been right all along. The technology has been around for well over thirty years, and especially since the availability of fairly accurate scanners in recent years, the job should not be too difficult for people in computational linguistics to undertake.

V. N. Vološinov

M. M. Baxtin

In 2009, Vladimir Mixajlovič Alpatov in his Vološinov entry is still wavering whether or not *Marksizm i filosofija jazyka* should fully ascribed to him, stating that "Baxtin's [*sic*] authorship […] is not proved and it is possible that the ideas expressed in V[ološinov]'s works were elaborated by V. and Baxtin together" (Alpatov 2009: 1593), it seems that, at least in the West, the authorship question is settled once and for all, if the recent bilingual French-Russian edition (Vološinov 2010) has been read with attention to detail. The reader of this chapter may well have made his own decision and agree that Vološinov carries off the prize.

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List of biographical names in the history of linguistics

A
Aarsleff (orig. Årsleff), Hans (Christian, b. 1925)
Abbadie, Antoine d' (1810–1897)
Adamska-Sałaciak, Arleta (b. 1957)
Alpatov, Vladimir Mixajlovič (b. 1945)
Angulo y Mayo Jaime de (1888–1950)
Antal, Lázló (1930–1993)
Alois Vaníček (1825–1883)
Anttila, Raimo (b. 1935)
Apresjan, Jurij Derenikovič (b. 1930)
Arens, Hans (1911–2003)

B
Baxtin/Bakhtin, Mixail Mixajlovič (1895–1975)
Baudouin de Courtenay, Jan (1845–1929)
Benedict, Ruth (1887–1948)
Bergunder, Michael.(b. 1966)
Benveniste, Émile (1902–1976)
Benware, Wilbur A(lan, b. 1937)
Berezin, Fedor Mixajlovič (1930–2003)
Bergounioux. Gabriel (b. 1954)
Best, Karl-Heinz (b. 1943)
Bloch, Bernard (1907–1965)
Bloomfield, Leonard (1887–1949)
Boas, Franz (1858–1942)
Bopp, Franz (1791–1867)
Bradley, Henry (1845–1923)
Braune, Wilhelm (1850–1925)
Bréal, Michel (1832–1915)
Bright, James Wilson (1852–1926)
Brockhaus, Hermann (1806–1877)
Brugmann, Karl (1849–1919)
Bühler, Karl (1879–1963)
Bulič, Sergej Konstantinovič (1859–1921)
Bynon, Theodora ("Thea", b. 1936)

C
Campbell, Lyle (R., b. 1942)
Cassidy, Frederick Gomes (1897–2000)
Cassirer Ernst (1874–1945)
Charencey, le comte Charles Félix Hyacinthe de (1832–1916)
Chavée, Honoré (Joseph, 1815–1877)
Chomsky, Noam (b. 1929)
Chevalier, Jean-Claude (1925–2018)
Christiansen, Broder (1869–1958)
Christmann, Hans-Helmut (1929–1995)
Christy, Thomas Crag (b. 1952)
Clark, Katerina (b. 1941)
Collinson, William Edward (1889–1969)
Coseriu, Eugenio (1921–2002)
Cowan, William George (1929–2001)
Croce, Benedetto (1866–1953)
Christy, Craig (b. 1952)
Curtius, Ernst (1814–1896
Curtius, Georg (1820–1885)

D
Darnell, Regna Diebold (b. 1943)
Darwin, Charles (1809–1882)
Delbrück, Berthold (1842–1922)
Di Cesare, Donatella (b. 1956)
Diez, Friedrich (1794–1876)
Dinneen, SJ Frank (Francis Patrick, 1923–1994)

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Dürbeck, Helmut (1930–2019)
Dufriche-Desgenettes, Antoni (1804–1878)
Durkheim, Émile (1858–1917)
Duruy, Victor (1811–1894)

E
Edgerton, Franklin (1885–1963)
Egger, Émile (1813–1885)
Einhauser, Elwine (b. 1960)
Engler, Rudolf (1930–2003)

F
Fenton, William N(ash, 1908–2005)
Firth, John Rupert (1890–1960)
Fischer, Olga (b. 1951)
Foster, Michael K(irk, b. 1938)
Freeland, L. S. ("Nancy", 1890–1972)
Frege, Gottlob (1848–1925)
Freud, Sigmund (1856–¬1939)

G
Gabelentz, (Hans) Georg von der (Conon, 1840–1893)
Gambarara, Daniele (b. 1948)
Gilliéron, Jules (1854–1926)
Gipper, Helmut (1919–2005)
Golla, Victor Karl (b. 1939)
Graßmann, Hermann (Günter, 1809–1877)
Greenberg, Joseph (Harold, 1915–2001)
Gregores. Emma (1927–2007)
Guigniaut, Joseph-Daniel (1794–1876)
Guxman, Mirra Moiseevna (1904–1989)

H
Haas, Mary R(osamond, 1910–1996)
Haspelmath, Martin (b. 1936)
Harris, Zellig S(abbettai, 1909–1992)
Harrold, Ernest William (1889–1945)
Hattala, Martin (1821–1903)
Hausmann, Franz Josef (b. 1943)
Hayashi, Tetsuro (1921–2002)
Helbig, Gerhard (1929–2008)
Hemsterhuis, Tiberius (1685–1766)
Henricy, Casimir (1819–1892
Hill, Eugen (*post* 2016)
Hirschkop, Ken(neth, b. 1957)
Hjelmslev, Louis (Trolle. 1899–1965)
Hock, Hans-Henrich (b. 1938)
Hockett, Charles F(rancis, 1916–2000)
Hoefer, Albert (1812–1883)
Hoenigswald, Henry M(ax, 1915–2003)
Holquist, Michael (b. 1935)
Holtzmann, Adolf (1810–1870)
Hooton, Earnest Albert (1887–1954)
Hoijer, Harry (1904–1976)
Hovelacque, Abel (1843–1896)
Humboldt, (Friedrich) Wilhelm von (*Freiherr*, 1767–1835)
Hutton, Christopher M(ark, b. 1958)
Hymes, Dell (Hathaway, 1927–2009)

I
Iordan. Iorgu (1888–1986)
Itkonen, Esa (b. 1944)
Ivanov, Vjačeslav Vsevolodovič (b. 1929)
Ivić, Milka (b. 1921)

J
Jarceva, Viktorija Nikitična (1906–1999)
Jahn, Otto (1813–1869)
Jakobson, Roman (Osipovič) (1896–1982)
Jankowsky, Kurt R(obert, b. 1928)
Jenness, Diamond (1886–1969)
Jespersen, Otto (1860–1943)
Joos, Martin (George, 1907–1978)
Joseph, John E(arl, b. 1956)

K
Kacnel'son, Solomon Davidovič (1907–1985)
Kainz, Friedrich (1897–1977)
Kanaev, Ivan Ivanovič (1893–1984)

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Kiparsky, (René) Paul (Victor, b. 1941)
Knobloch, Clemens (b. 1951)
Koerner, (Ernst Frideryk) Konrad (b. 1939)
Kroeber, Alfred Louis (1876–1960)
Kruisinga, Etsko (1879–1944)
Kruszewski, Mikołaj (1851–1887)
Kuhn, Adalbert (1812–1881)
Kuhn. Thomas S(amuel, 1922–1996)
Kvícala, Jan (1834–1908)

L
Labov, William (b. 1927)
Lazarus, Moritz (1824–1903)
Lehmann, Winfred P(hilipp, 1916–2007)
Léon, Pierre (1914–1976)
Leont'ev, Aleksej Alekseevič (b. 1936)
Lepschy, Giulio C(iro, b. 1935)
Lerchenmüller, Otto (1943–2011)
Leroy, Maurice (1909–1990)
Leśniewski, Stanislaw (1886–1939)
Leskien, August (1840–1916)
Littré, Émile (1801–1881)
Lloyd, Richard J(ohn, 1846–1906)
Lowie, Robert Harry (orig. Löwe, 1883–1957)
Harry Stack Sullivan (1892–1949)

M
Macbeth, Madge (1878–1965)
Maher, J(ohn) Peter (1933–2018)
Malkiel, Yakov (1914–1998)
Malmberg, Bertil (1913–1994)
Mandelbaum, David G. (1911–1887)
Marr, Nikolaj Jakovlevič (in Georgian: Marr, Nik'o, 1865–1934)
Marx, Karl (Heinrich, 1818–1883)
Mason, James Alden (1885–1967)
Matejka, Ladislav (b. 1919)
McCawley, James D(avid, 1938–1999)
Mclenagan, Jean Victoria (1899–1979)
Medvedev, Pavel Nikolaevič (1891–1938)
Meillet, Antoine (1866–1936)
Merton, Robert K. (1910–2003)
Meyer, Paul (1840–1917)
Michel, Cornelius Leonardus (1887–1984)
Mikuš, Radivoj Franciscus (1906–1983)
Morpurgo Davies, Anna (1937–2014)
Mounin. Georges (*alias* Louis Julien Leboucher, 1910–1993)
Mowat, Robert (1823–1912)
Müller, Theodor (1816–1881)
Mugdan, Joachim (b. 1951)

N
Newman, Stanley S(tewart, 1905–1984)
Newmeyer, Frederick J(aret, b. 1944)
Noordegraaf. Jan (b. 1948)
Novotny, Eduard (1833–1876)

O
Ogden, Charles K(ay, 1889–1957)
Orr. John (1985–1966)
Osthoff, Hermann (1847–1909)

P
Paris, Gaston (1839–1903)
Paul. Hermann (1846–1921)
Percival, Walter Keith (b. 1930)
Pike, Kenneth Lee (1912 –2000)
Plato (427–347 B.C.)
Polivanov, Evgenij Dmitri'evič (1891–1938)
Pott, August Friedrich (1802–1887)
Preston, Richard J(oseph, b. 1931)
Prokosch, Eduard (1876–1938)
Pumpjanskij, Lev Vasil'evič (*alias* Leib Meerovič Pumpjan, 1891–1940)
Putschke, Wolfgang (b. 1937)

R
Ranke, Leopold von (1795–1886)
Richards, Ivor Armstrong (1877–1979)
Ringler, Richard N. (b. 1934)
Roberts, E(llis, b. 1939)

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Robins, Robert Henry ("Bobby", 1921–2000)
Robins, John Daniel (1889–1952)

S
Sapir, Edward (1884–1939)
Sapir, Jacob David (b. 1935)
Saussure, Ferdinand (1857–1913)
Scherer, Wilhelm (1841–1886)
Schleicher, August (1821–1868)
Schmidt, Johannes (1843–1901)
Schmitter, Peter (1943–2006)
Schreyer, Rüdiger (b. 1941)
Schogt Henry (1927–2020)
Scott, Duncan Campbell (1862–1947)
Sebeok, Thomas A(lbert, c.1915–2001)
Sériot, Patrick (b. 1949)
Seuren, Pieter A. M(arie, b. 1934)
Sievers, Eduard (1850–1936)
Skousen, Royal (b. 1945)
Sommerfelt, Alf (Axelssøn, 1892–1965)
Šor, Rozalija Osipovna (1894–1939)
Spier, Leslie (1893–1961)
Spitzer, Leo (1887–1960)
Steinthal, H(eymann = Chajim, 1823–1899)
Stojanov, Vasil Dimitrov (1839–1910)
Strong, Herbert Augustus (1841–1918)
Sullivan. Harry Stack (1892–1949)
Swadesh, Morris (Maurizio, 1909–1967)
Swiggers, Pierre (b. 1955)

T
Taine, Hippolyte (1828–1893)
Tajjma. Matsuji (b. 1942)
Tarski, Alfred (1902–1983)
Techmer, Friedrich (1843–1891)
Tell, Julien (1807–*post* 1874)
Thomas, Lawrence L. (1924–2009)
Titunik, Irwin (1929–1998)
Trager, George Leonard (1906–1992)
Trautmann, Thomas R(oger, b. 1940)
Trubetzkoy/Trubeckoj, Nikolaj Sergeevič (*Prince*, 1890–1938)

V
Vaníček, Alois (1825–18830)
Velmezova, Ekaterina (b. 1973)
Vendryes, Joseph (1875–1960)
Verner, Karl (1846–1896)
Voegelin, Charles Frederick ("Carl" (1906–1986)
Vološinov, Valentin Nikolaevič (1895–1936)

W
Wallis, Wilson Dallam (1886–1970)
Wanner, Dieter (1934–2019)
Warnotte, Daniel (1871–1949)
Watts, Richard James (b. 1943)
Weber, Albrecht (1825–1901)
Wehrle, Albert J. (1891–19??)
Weinreich, Uriel (1926–1967)
Wheeler, Benjamin Ide (1854–1927)
Whitney, William Dwight (1827–1894)
Whorf, Benjamin Lee (1897–1941)
Wilbur, Terrece H(arrison, 1924–2000)
Williams, Joanna Radwańska (b. 1961)
Winteler, Jost (1846–1929)

Z
Zeitlin, Jacob (1883–1937)
Zvegincev, Vladimir Andreevič (1910–1984)
Zwirner, Eberhard (1899–1984)

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This volume brings together – in 8 chapters – what has occupied the author during his many years as editor of *Historiographia Linguistica*. Namely, how the history of linguistics has developed into a major field of scholarly research, and that the discussion of questions of method and epistemology needs to be continued to avoid stereotypical practice. The author takes up a number of subjects that often had been regarded as settled, but which require a revisit. This is shown in several chapters, whether it appears subjects like 'analogy' or the relationships between well-known linguists like Saussure, Hermann Paul, and others.

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