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CHAPTER 10

WILLIAM LABOV AND THE ORIGINS OF SOCIOLINGUISTICS IN AMERICA

1. *Introductory observations*

It appears to be a regular part of North American culture that when something is declared to be new, few people care to ask a question about what in effect distinguishes this allegedly novel idea, project or product from the old. The past is soon forgotten and people are happy to be part of a trendy present which holds out the promise of becoming the future. There are reasons for this phenomenon, historical, socio-political, and economic; however, an analysis of these reasons is not my concern here. I am simply trying to explain to myself why linguists on this continent often lack a historical consciousness regarding their own field of study and, as a result, can be easily led into believing claims of novelty, discontinuity, breakthrough, and revolution made by someone in favour of a new approach or, for that matter, a theoretical stance. I still recall my own astonishment about the enthusiasm of some of my teachers about 'sociolinguistics' during the late 1960s, which then was, as it is still today, largely associated with the name of William Labov (cf. Macaulay 1988: 154-157 *passim*),¹ at least in North America. Indeed, the *opinio communis* regarding the origins of sociolinguistics still today appears to be what the editor of the 1,000-page *Concise Encyclopedia of Sociolinguistics* has noted, namely, that "sociolinguistics as a specially demarcated area of language study only dates to the early 1960s" (Mesthrie 2001:1).²

In this chapter I refer mainly to this brand of sociolinguistics rather than the line of research usually pursued by scholars coming from sociology like Basil Bernstein (1924-2000)³ in Britain (e.g., Bernstein 1971) and Joshua A. Fishman (b.1926) in the United States (e.g., Fishman 1972), which is perhaps better defined by the phrase 'sociology of language', or the research programs

¹ For an interesting — and very detailed — criticism of Labov's theories, not attempted in the present chapter, see Figueroa (1994:69-110).

² Mesthrie's sole references regarding the history of the field are Shuy (1989) and Paulston & Tucker (1997). There is no chapter in this massive quarto-size volume on the subject.

³ For a sympathetic and thorough analysis of Bernstein's work — and its success outside the United States, see Hasan (2000).

laid out by others with anthropological backgrounds like Dell Hymes' (b.1927) 'ethnography of speaking' (e.g., Hymes 1974) and by scholars like John Gumperz (b.1922) who favour an interactionist and discourse analysis approach (e.g., Gumperz 1971).⁴

I admit to be non-plussed to hear students who took courses with a former doctoral student of Labov telling me that the concepts of 'drag chain' and 'push chain',⁵ for example, stem from William Labov instead of going back at least to André Martinet's (1908–1999) paper on "Function, Structure, and Sound Change" where these terms are used and explicated, e.g., with reference to palatalization phenomena in Romance (Martinet 1952a:11). It is also interesting to note that in a review of Sever Pop's (1910–1960) voluminous *Dialectologie*, published in the same journal the same year, Martinet made the following observation:

This is only a sampling of all the possible socio-linguistic patterns in connection with which the word 'dialect' is actually used [...]. Therefore we may expect to come across socio-linguistic situations which we may hesitate to class in one or another of our four categories [...]. (Martinet 1952b:261)

Thus for anyone familiar with Labov's career, it is clear that he got this idea 'drag chain' vs 'push chain' (and many other ideas), if not directly from reading Martinet's work,⁶ then mediated through Uriel Weinreich (1926–1967), Martinet's student during the late 1940s and early 1950s and subsequently Labov's teacher during the early through mid-1960s.⁷

⁴ On their various research programs, traditions, organizational strengths, etc., see the studies by Stephen O. Murray (1994, Chaps.10–14) and Murray (1998, a more focused reworking of those parts of the preceding work which deal with sociolinguistics in the broadest sense possible).

⁵ In Labov (2000:463), the terms used are 'pull chains' and 'push chains', respectively.

⁶ See also Martinet's influential 1955 book *Économie des changements phonétiques* where they are called 'chaîne de traction' and 'chaîne de propulsion', respectively. Since Martinet (1952a) appeared in *Word*, i.e., right in front of Labov's nose, so to speak, as it was edited by Martinet together with Weinreich at the time, one would be hard pressed to believe that Labov had no knowledge of this article, even if it is not listed in the "Bibliography" to his 1972 collection of his major 1963–1970 papers, which does include a reference to *Économie*, however (p.334). In earlier work, the 1952 article and the 1955 book together are properly acknowledged as "Martinet's theories of the internal economy of phonological structures" which "were more comprehensive and systematic than any published previously" (Labov 1966a[1964]:17). Of course, the same would apply to Martinet (1952b).

⁷ It should be pointed out that Labov himself would never have made any such claim and, as one may gather from his writings, indeed, he has been much more generous when it comes to acknowledging his indebtedness to others than, for instance, Noam Chomsky has done. His doctoral dissertation of 1964 (Labov 1966a) is a good source for tracing references to earlier work. There we find, *inter alia*, acknowledgments like this one: "Many of Martinet's ideas have found application in the present study" (p.26n.7; cf. also Labov 2001: 262, 498, etc.).

Given what I noted at the outset, I probably should not have been surprised to find next to nothing on the history of 'sociolinguistics' when I first ventured to investigate the history of the subject some fifteen years ago (cf. Koerner 1986).⁸ However, I had expected a scholar like Dell Hymes, who has written on other aspects of the history of linguistics during the past twenty-five and more years (cf. Hymes 1983, for a collection of his papers in this area), and who published, among other things, a book on the *Foundations of Sociolinguistics* (Hymes 1974), to have enlightened us on the origins, sources, and development of the field. However, one searches in vain for any such account in the bibliography of this prolific writer. An early contribution to the history of sociolinguistics was Yakov Malkiel's (1976) paper, which traces its development from Romance scholarship via dialectological work. There are of course a few brief textbook accounts of the 'history' of sociolinguistics to be found (e.g., Wolfram & Fasold 1972:26–32; Bell 1976:28–29; Milroy 1987:5–11), but these are rather short and totally inadequate, usually going little beyond acknowledging the existence of a link between work in dialectology and sociolinguistics. A number of sociolinguistics textbooks (e.g., Fasold 1984, Wardhaugh 1986, Romaine 1994, Holmes 2001[1990]) treat the subject without any historical perspective at all.

In other words, to a considerable extent (*pace* Murray 1998) the history of *Sociolinguistics* in America has still to be written.⁹ At least in Europe this unsatisfactory situation has been somewhat remedied by the publication of the first tome of the recent *Sociolinguistics* handbook (Ammon et al. 1988), which contains a fairly large section on "History of Sociolinguistics as a Disci-

⁸ At least not from the side of linguists; cf. the accounts by the sociologist Stephen O. Murray, such as his *Theory Groups and the Study of Language in North America*, which carry chapters 10, "Language contact and early sociolinguistics" (Murray 1994:249–287), which is devoted to the work of Haugen, U. Weinreich, Fishman, and others; 11, "The ethnography of speaking" (289–340), dealing with the work Ferguson, Gumperz, Dell Hymes, and others, whereas Labov is placed into Chap.12, "Related Perspectives" (341–389), together with sociologists and anthropologists, not linguists.

⁹ This chapter leaves out an entire strand of sociolinguistic work which should be part of an overall history of the subject, but which I believe North American scholars, including Labov, were not aware of at the time (and probably still are not today): I am referring in particular to the work of John Rupert Firth (1890–1960), but also Bronislaw Malinowski (1884–1942), (Sir) Alan Henderson Gardiner (1879–1963), and possibly others in Britain during the 1930s through the 1950s (e.g., Abercrombie 1948; cf. Aarts 1976:240–244), a tradition which informs the work of M.A.K. Halliday (e.g., Halliday 1975) and, especially, Ruqaiya Hasan (e.g., Hasan 1973).

pline",¹⁰ though as far as I can see, only Hagen's contribution comes in any way close to what I am trying to do here (Hagen 1988).¹¹ The late 1980s saw also at least two modest contributions to the subject (Koerner 1986; Shuy 1989, written in ignorance of the former). The 1990s started on a more hopeful note with a series of more in-depth papers (Shuy 1990, Koerner 1991, Joseph 1992) and were followed by several book-size publications. The first was in the form of five chapters included a 594-page "social history" of North American Linguistics (Murray 1994). In it, chapters 10–14 [pp.249–429] deal with various research programs, traditions, organizational strengths of areas such as language contact and bilingualism (with Haugen 1953 and Weinreich 1953 as prime 'exemplars'), 'ethnography of speaking' (represented especially by the work of John Gumperz, Dell Hymes and their associates [e.g., Gumperz & Hymes 1964, 1972], and "The Sociology of Language" (419–429), in which one looks in vain for a treatment of Joshua A. Fishman's contribution, given that Fishman himself has described his research area as such.¹² Curiously, although Fishman has had few students and hardly a follower of note,¹³ he gets Murray's tabular sociological analysis treatment in terms of leadership, 'paradigm statement', 'exemplars', and the like (p.268), something he denies William Labov, who is tucked away in Chapter 12, "Related Perspectives" (341–389), together with, *inter alios*, British-born anthropologist Gregory Bateson (1904–1980) and Canadian-born sociologist Erving Goffman (1922–1982), neither of the two a linguist (374–389). This is all the more surprising as Murray (p.389) concludes his fairly detailed account of Labov's work by saying: "In Kuhnian terms, Labov produced exemplary research that others could emulate." Hardly anyone in the field of 20th-century sociolinguistics would doubt that Labov's 1966 *The Social Stratification of English in New York City* constitutes such an exemplar, and several other seminal works were to follow (e.g., Labov 1969b, 1972c).

The next publication, *The Early Days of Sociolinguistics: Memories and reflections* edited by Paulston & Tucker (1997), does not compare at all in thor-

¹⁰ This section includes also a paper by Michael Clyne on the history of language contact and another by Ian Hancock on research on Pidgins and Creoles, thus effectively enlarging the scope of 'sociolinguistics' beyond what is meant by the term in the present chapter.

¹¹ As in the case of Malkiel, it is revealing to note that those who account for the history are, in most instances, Europeans, not Americans.

¹² However, Fishman's work and influence is discussed at considerable length in Chap.10 ("Language Contact and Early Sociolinguistics"), pp.259–268, the reason being that Fishman, too, had, like Haugen and U. Weinreich, an "interest in bilingualism and language maintenance" (p.259). But unlike the former, Fishman is essentially a sociologist, not a linguist, and, as a result, had a rather different approach and research practice.

¹³ According to Murray (1994:265n.12), "Robert Cooper is a very notable exception."

oughness with Murray's work. It contains many individual pieces of reminiscences, including a chapter on "Early Institutional Supporters for the New Field" which consists of an interview with Rudolph C. Troike (b.1933), formerly of the Center for Applied Linguistics, by the first editor, who also recalls Einar Haugen in the "Remembrances" section. The most substantive contribution to this volume is a reprint of Roger W. Shuy's (b.1931) 1990 article "A Brief History of American Sociolinguistics, 1949–1989" (11–32), which in the main focusses on the Black English work of the 1960s and 1970s.¹⁴

The most comprehensive effort in the direction of a history of sociolinguistics to date is Stephen O. Murray's *American Sociolinguistics* of 1998, which however is, by the author's own description, a "study of postwar anthropological linguistics" (p.1). In fact, this book is essentially a selection and slight reworking of five chapters of Murray (1994);¹⁵ as a result, the work of Labov, surely the prime mover and shaker in this field, is not given more space here than in the earlier account. The fact that Murray is a sociologist, not a linguist may help explain his choices, interpretation, and emphasis. So the challenge is out for a sociolinguist to at least to complement Murray's work.

William Labov, the leader of this field of research, should not be expected to have engaged himself in writing the history of sociolinguistics, of course. In his early work he reveals many of the sources of his own linguistic thinking and approach, notably in his (1964) dissertation (cf. Labov 1966a:8–41) as we shall see in what follows.¹⁶ To be sure, the pioneering organizer of the modern field of sociolinguistics does not need to involve himself in history-writing.

¹⁴ The volume's contributions are divided into six main thematic fields: "Sociolinguistics: A Personal View" by Basil Bernstein is one of the contributions to the chapter "Pioneers". Other articles in this chapter are by William Labov, Joshua Fishman (who also contributes to the chapter titled "Journal Editors"), the Montreal psychologist Wallace Earl Lambert (b. 1922) and Carol Myers-Scotton, among others.

¹⁵ The section on Labov (Murray 1998:167–179) is almost identical to the one in Murray (1994), with only a few footnotes reduced or deleted; this section certainly worth reading, in particular the one on "Prestige dialects" (Murray 1998:170–177 = 1994:378–382).

¹⁶ Still much less about his biography is publicly known than about several other important figures in sociolinguistics broadly defined (cf. Haugen 1980, Hymes 1980, McDavid 1980a, Fishman 1991, Ferguson 1998). In my experience, Labov has been rather reticent when it comes to writing an autobiographical account, and, as a result, one has to obtain any such information, as much as this is possible, from remarks in his scholarly publications. The "Published Interviews" listed in Guy et al. (1996), vol.II, p.352, apart from Rosen (1967), which contains nothing about Labov's biography, are hard to come by as they appeared in rather obscure places. Typically, there is no entry on him in the *Directory of American Scholars*, vol.III: *Foreign Languages, Linguistics & Philology*, 9th ed. (Detroit, London, etc.: Gage, 1999). It is only very recently that we have a more personal statement by Labov concerning the reasons why he decided to return to university life in 1961 and what this decision has led to (Labov 2001[1997]).

(That he may have done so nevertheless through the manner in which he represented and criticized previous work and ignored or dismissed other important writings is another matter).¹⁷ Indeed, we may be grateful that Labov, unlike his close contemporary Noam Chomsky, has not taken a personal interest in writing the history of his field of study. While it is true that Chomsky's *Cartesian Linguistics* of 1966 has given the History of Linguistics an initial boost at the time, it did not lead to serious historiographical work by those who followed his example but encouraged others to produce Whiggish accounts of the development of generative linguistics (cf. Newmeyer 1980, 1996, for the most accomplished¹⁸ examples of this kind of writing).

It remains true, however, that a scientific field reaches its maturity only by becoming aware of its own history and by taking a serious interest in having it documented. The present account is hardly more than an attempt to come to grips with the task of presenting the sources and early development of sociolinguistics, an area of research generally and erroneously thought to have arisen in the mid-1960s, perhaps as a result of the publication of the papers of the November 1963 San Francisco Conference on the Ethnography of Communication (Gumperz & Hymes 1964) and, more likely, of the proceedings of the 1964 UCLA — Lake Arrowhead — conference devoted to the Sociolinguistics *expressis verbis* (Bright 1966),¹⁹ which no doubt served as a rallying point for this line of research. It is interesting to note that William Labov had, early on in his career, ample opportunity to present his findings at both these meetings and have his views discussed (Labov 1964, 1966b), quite a feat if we remember that Labov had entered Linguistics only in 1961 and completed his Master's thesis in 1963, and that his doctoral dissertation was in the course of being written at the time (published, with minor changes, as Labov 1966a). His mentor, Uriel Weinreich, to whom the invitations had originally been extended,

¹⁷ As one such example, one wonders why Labov had only three lines of comment on a programmatic article by Stanley A. Sapon (b.1924), who, as Joseph (1992:121) reports, had done his Ph.D. in Romance linguistics at Columbia in 1949, with Martinet as one his advisors and "who was working on a larger study of socioeconomic variables in Mexico City that was never published". Sapon's (1953) paper is simply described as "A methodology for studying socio-economic differentials in language" of which "no results seem to have appeared" (Labov 1966a:21), but one wonders whether Labov could not have had access to Sapon's unpublished work during his years at the same university (1961–1969).

¹⁸ If we keep in mind what Henry Butterfield, in *The Whig Interpretation of History* (New York: Scribners, 1931), p.v, had to say about "the tendency of historians [...] to produce a story which is the ratification if not the glorification of the present".

¹⁹ Calvet (1999) celebrates the latter as a kind of breakthrough for sociolinguistics and devoted a detailed "analyse interne" (34–41) to it, which makes for interesting reading. Labov (1972b:296), however, regards Ferguson & Gumperz (1960) as the defining publication.

had Labov participate in his place, surely not merely because of his declining health.²⁰

2. *The sources of modern sociolinguistics*

My own research suggests that we could envisage the broad Labovian type of sociolinguistics to be the confluent, if not the synthesis, of various lines of research that go back to at least several generations of linguistic workers, though it remains true that the field was crystalizing during the 1950s and essentially on American soil. The link between dialect geography and sociolinguistics, which is very obvious from Labov's writings right from the beginning, has been made by various scholars (e.g., Grassi 1980, Trudgill 1983). It is probably too obvious to be overlooked and may also explain why it has not been as frequently mentioned as one might expect.

Another line of linguistic thought goes back to the later 19th century when scholars such as William Dwight Whitney (1827–1894) in America, Michel Bréal (1832–1915) in France, Hermann Paul (1846–1921) in Germany, Jan Baudouin de Courtenay (1845–1929) in Russia,²¹ and others reacted against the view, usually associated with the views propounded by August Schleicher (1821–1868), Max Müller (1823–1900), and others, according to which linguistics should be thought of as a science and that language ought to be treated like a living organism, and that, consequently, linguistics was to be ranked among the natural, not the social, sciences.²² This change in philosophical outlook among linguists became fairly general in the wake of the publication of such works as Wilhelm Dilthey's (1833–1911) *Einleitung in die Geisteswis-*

²⁰ Weinreich died of leukemia early in 1967, at the age of 41, "not suddenly, of cancer, at the age of 39", as Labov had it in the 1997 version of Labov (2001). – Labov's cometic rise to prominence by the time he finished his doctorate is a story not easily explained. By a curious coincidence, Zellig S. Harris (1909–1992), Chomsky's (more than merely nominal) supervisor for both the M.A. (1951) and Ph.D. (1955) theses at the University of Pennsylvania, ceded to his student a perhaps still more important opportunity to present his views in the same year, namely, at the Ninth International Congress of Linguists held in Cambridge, Massachusetts, in 1962. Calvet (1999:47, following Murray 1994:377) believes that Weinreich appears to have been the only person at the time who had a strategy to advance sociolinguistics and, in particular, Labov's career.

²¹ While Bréal and Baudouin are not mentioned in Labov (1972b), Paul comes in for criticism on his views on linguistic change in Labov (1972b:261–263; 1972a). However, Paul's views had been much more thoroughly discussed earlier by Weinreich in Weinreich et al. (1968:104–129).

²² It therefore comes as a surprise when the author of a recent sociolinguistics paper makes the following remark: "Languages are, in fact, remarkably stable organisms, transmitting their essential characteristics from one generation to the next" (Macaulay 1988:156–157); elsewhere in his paper he complains about a lack of methodological rigor in the field.

senschaften of 1883 and the ensuing debate over the essential differences between *Naturwissenschaft* and *Geisteswissenschaft* in Germany and elsewhere (cf. Koerner 1982:187-188). This reference to the change in the intellectual climate is important as it provides the background for a better understanding of the establishment of a specific line of research. So in addition to dialectology, we may also have to recognize a particular kind of approach to language in general and to questions of language change that is sociological in orientation. Finally, we may become aware of the influx somewhat later of work on bi- and multilingualism — and, much more recently, issues of language contact, language planning, and linguistic conflicts²³ — into sociolinguistic research.

The influence of Whitney, Paul, Baudouin de Courtenay, and others on Saussure is well established (see Koerner 1973, for details); it suggests at the same time that Saussure did not need Durkheim in order to be able to characterize language as a 'fait social' (pace Bierbach 1978). To illustrate this point, let me cite just one passage from Whitney's *Language and the Study of Language*, to which Saussure frequently referred in his lectures on general linguistics at the beginning of the 20th century:

Speech is not a personal possession, but a social; it belongs, not to the individual, but to the member of society. No item of existing language is the work of an individual; for what we may severally choose to say is not language until it be accepted and employed by our fellows. The whole development of speech, though initiated by the acts of individuals, is wrought out by the community. (Whitney 1867:404)²⁴

I shall return to the importance of Whitney in section 2.2 (below). The role he played in European linguistics during the last quarter of the 19th century has been discussed elsewhere already (Koerner 1980).

2.1 *From dialect geography to sociolinguistics*

As mentioned earlier, Malkiel (1976) established a regular line of connection between dialectological work in Romance languages and sociolinguistic efforts. In other words, we do well to go back to the beginnings of fieldwork in dialect geography during the last decades of the 19th century to see the sociological component slowly infiltrating linguistic geography. Unlike Malkiel, I am thinking in particular of the *Marburger Schule* established by Georg Wenker (1852-1911), which is still active today (cf. Knoop et al. 1982)

²³ If these areas of investigation are not more correctly ascribed to 'sociology of language' programs, not 'sociolinguistics' as here defined.

²⁴ Labov (1972b:261) quotes the first sentence only, though he also quotes (ibid.) another passage from Whitney (1901[1867]:401) to illustrate Whitney's emphasis on the social-communicative function of language: "Man speaks, then, primarily not in order to think, but in order to impart his thought. His social needs, his social instincts, force him to expression."

and the school created somewhat later by the Swiss Jules Gilliéron (1854-1926) in Paris (cf. Jaberg 1908), whose students Jacob Jud (1882-1952) and Karl Jaberg (1877-1958), together with the assistance of Paul Scheuermeier (1888-1973), Gerhard Rohlf (1892-1984) and Max Leopold Wagner (1880-1962), compiled the voluminous *Atlas linguistique et ethnographique de l'Italie et de la Suisse méridionale* (Jaberg & Jud 1928-1940). Both the German and the Swiss enterprises are of particular interest in the present context as I shall indicate in what follows.²⁵

To begin with, Max Weinreich (1894-1969), the father of the much better known Uriel Weinreich (1926-1967), did his doctoral dissertation on Yiddish, both its language and its literature, under the direction of Ferdinand Wrede (1863-1934), Wenker's successor at the University of Marburg (Weinreich 1923). (From 1926 onwards, Wrede brought out in successive volumes the massive *Deutscher Sprachatlas*, initiated many years earlier by Wenker.) More interestingly perhaps, Wrede — whom Meillet cites in his famous 1905 paper (see Meillet 1921:255) — much earlier drew parallels between ethnography and dialectology, distinguishing between 'individuallinguistische' and 'sozial-linguistische' instances of borrowing among languages (Wrede 1902).²⁶

Perhaps more important in the present context is the fact that, in 1931, the Swiss dialectologists Jud and Scheuermeier were brought over to the United States for the summer in order to train American students to undertake dialectological field work. The Austrian-born Hans Kurath (1891-1992) had secured a grant from the American Council of Learned Societies for this purpose. We know that Raven I. McDavid (1911-1984) for instance (McDavid 1980b:8) was one of those young trainees who later participated in the research that led to the *Linguistic Atlas of New England* edited by Kurath and others (1939-1943). It is therefore interesting in the present context that McDavid published an article entitled "Dialect Geography and Social Science Problems" as early as 1946. More importantly, his 1948 'social analysis' on "Post-Vocalic /-r/ in South Carolina", has been hailed as a pioneering instance of 'variation study' (Shuy 1989:297).²⁷ By the time McDavid and his student

²⁵ The fact that Labov does not seem to refer to these works in his writings (e.g., Labov 1972b) is no reason to leave out mention of this research. In fact, dialectological traditions and practices loom large in Labov's *oeuvre*.

²⁶ It is safe to assume that Paul's *Prinzipien*, which had its third edition by 1894, had an influence Wrede's thinking concerning the social nature of language.

²⁷ Curiously enough, in the bibliography to his 1972 *Sociolinguistic Patterns* one looks in vain for an entry on McDavid. One wonders whether McDavid's review of Labov (1966a) in *American Anthropologist* 70.425-426 (1968) has something to do with it, in which McDavid wondered why "[c]ertain groups are not represented — notably the old stock white Protestants, who still make up a very large portion of the New York upperclass [...] and have other

Raymond O'Cain published their 1973 paper on "Sociolinguistics and Dialect Geography", the connection between dialectology and sociolinguistics had become more widely recognized, though perhaps more implicitly than overtly acknowledged.

Even outside established schools important dialectological work was done in the later 19th century; mention can be made of Schleicher's little known study of his native dialect (Schleicher 1858) and of Jost Winteler's (1846–1929) celebrated *Kerenzer Mundart* of 1875 as just two such examples. That the social component in language variation was recognized before the turn of the 20th century may be gathered from Richard Löwe's (1863–c.1942) paper of 1882, which Hagen (1988b:408) has referred to as the "only [...] known early study on social dialect variation in cities", and the dialect work of yet another scholar of the time, Philipp Wegener (1848–1916), who, dealing with the same geographic area as Löwe, noted the following in his 1891 contribution to Hermann Paul's *Grundriss*:

In the Magdeburg region the rural workers go into the cities in large numbers to work there as masons, handy-men or in the factories. The joint work brings them into regular contact with the urban workers; the Low German rural worker usually does not mind being influenced by the common speech of the city dwellers, and this the more so, the larger the distance from his rural dialect and the higher his esteem for the advantages of urban life. (Wegener 1891:937; my translation: EFKK)

[In der Magdeburger Gegend gehen die ländlichen Arbeiter in grosser Zahl in die Städte, um hier als Maurer, Handlanger oder in den Fabriken zu arbeiten. Die gemeinsame Arbeit bringt diese in steten Verkehr mit den städtischen Arbeitern; der niederdeutsche ländliche Arbeiter lässt sich durchweg von der städtischen Vulgärsprache beeinflussen, und zwar um so mehr, je grösser der Abstand derselben von der ländlichen Mundart ist und je höher die Schätzung der städtischen Vorzüge.]

I shall resist the temptation to give this statement a modern interpretation. I believe, however, that in observations like these we may discern an awareness of the 'sociology of language' *avant la lettre*, and I am sure that many other such statements could be found in the early work of dialectologists. (Cf. also Olmsted & Timm [1983] on Baudouin de Courtenay, who, a former student of Schleicher's, conducted considerable field work from the 1870s onwards.) No doubt the actual contact in these linguistic investigations with speakers of different varieties of language in differing socio-economic settings fostered such

prestige models than Labov's" (p.379). (In fact, Labov's 1966 'social stratification' had left out the upper classes entirely.) McDavid's name appears in Labov's (1972b[1963]:11) reference to Kurath & McDavid (1951). McDavid's (1948) paper is included in the bibliography of his 1964 dissertation (Labov 1966a:586), but given only two lines in the body of the thesis (p.22). McDavid's (1946) paper is not included. Is this just an oversight?

awareness, to the extent that it becomes at times difficult to distinguish sharply between dialectology and sociolinguistics in the work of these scholars, especially in areas of research that are now being called 'urban dialectology'.

In order to establish a more obvious connection between the different lines of development in the history of sociolinguistics, let me draw something like a genealogy. Before we are able to do so, however, a few additional links will have to be established. I mentioned Saussure's high esteem for Whitney which probably goes back to his studies at the Universities of Leipzig and Berlin (1876–1880).²⁸ During his years in Paris, Saussure's most distinguished student was Antoine Meillet (1866–1936),²⁹ who in turn had André Martinet (1908–1999) as his student. I mention this fact because Martinet wrote a monograph-length study of his native dialect during his time in a German prisoners of war camp, which was published shortly after World War II (Martinet 1946), and also because Labov, like Meillet and Martinet, has always been particularly interested in questions of language change.³⁰ More important still, while a professor at Columbia University in New York City during 1948–1955, Martinet had Uriel Weinreich as his student, both for the M.A. and the Ph.D. degrees.³¹ It was Weinreich's 568-page doctoral thesis of 1951 on

²⁸ Thanks to the diligent search among the Whitney papers at Yale's Sterling Library by John E. Joseph, we know that Saussure wrote a letter to Whitney on 7 April 1879 in which he referred to their meeting "il y a quelques jours"; see his paper, "Saussure's Meeting with Whitney, Berlin, 1879", *Cahiers Ferdinand de Saussure* 42.205–213 (1988). Whitney had returned to Germany in order to see his *Sanskrit Grammar* and its parallel German translation by one of Saussure's Berlin professors, Heinrich Zimmer (1851–1910) through the press. (Both were published in Leipzig by Breitkopf & Härtel in 1879.)

²⁹ Labov (1966:10) quotes from a paper given by Meillet in 1906 – not 1905 – in which Meillet had argued that because language was a social science "le seul élément variable auquel on puisse recourir pour rendre compte du changement linguistique est le changement social dont les variations du langage ne sont que les conséquences parfois immédiates et directes, et le plus souvent médiates et indirectes" (Meillet 1921:17). Labov offers a – not always felicitous – English rendering of the French original. One can sympathize with Labov's difficulties, however, considering that even native speakers of French find Meillet somewhat impenetrable at times.

³⁰ Indeed, I regard his contribution to this subject as the centre piece of his work, as Labov himself would admit if asked (cf. his reply to Rosen 1967:19).

³¹ In his autobiography *Mémoires d'un linguiste: Vivre les langues* entretiens avec Georges Kassai et avec la collaboration de Jeanne Martinet (Paris: Quai Voltaire, 1993), Martinet later claimed to have been instrumental in the establishment of a chair for Yiddish for his former student. Interestingly, Murray (1994:256) credits Roman Jakobson (1896–1982) with having been Weinreich's thesis supervisor, an attribution apparently relying on Malkiel (1969), where Martinet is at least recognized for his "major influence" on Weinreich during the latter's "training period" (Malkiel 1969:128, cited in Murray 1994:257). Maybe the fact that Jakobson wrote a preface to Weinreich's *College Yiddish* (1949) has led to this confusion?

“Research Problems in Bilingualism with Special Reference to Switzerland” which formed the basis for his succinct book he became famous for, namely, *Languages in Contact* (Weinreich 1954), a socio-geographical study of bilingualism whose title he had taken from a series of lectures given by Martinet (as Weinreich indicates in his Acknowledgements, p.x). It should be added that Weinreich also studied under Jakob Jud — the same scholar who had trained American linguists in dialectological fieldwork in 1931 — in Zürich during the academic year 1948–1949, doing fieldwork for his dissertation, thus establishing another Swiss connection in the early history of sociolinguistics. Finally, we need only recall the fact that William Labov took both his Master’s and doctoral degrees with Weinreich (see Labov 1963, 1966a) in order to establish a kind of genealogical line leading from Whitney to Labov and to contemporary sociolinguistics — if one may be permitted to draw such a unilinear picture, which is, admittedly, an oversimplification of intellectual indebtedness of any kind:³²

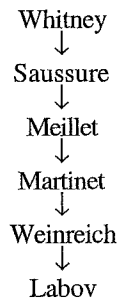


Fig.1: *From Whitney to Labov*

No doubt this is an overly simplistic ‘lineage’ and much more evidence, textual as well as biographical, would have to be supplied in order to offer a more adequate picture. The next section will add to this composite account. But those familiar with Labov’s work know of his frequent acknowledgement of debt to his teacher Uriel Weinreich³³ and of his references to the works of

³² Needless to add that all these scholars a referred to, at times critically, by Labov (cf. Labov 1972: xiii, 185–186, 266–267 [on Saussure]; 185n2, 263, 266, 318, 319 [on Meillet]; xiv, 2, 181, 185, 262, 265, 266, 271 [on Martinet], etc.). We have mentioned Labov’s approval of Whitney already (see note 22, above); *Sociolinguistic Patterns* is expressly dedicated to Uriel Weinreich.

³³ Cf. Labov’s (1982[1966]:iii) acknowledgment: “When I entered the field, I had my own ideas about contributing an empirical foundation to linguistics. But I find that Weinreich had anticipated me, and many of the ideas I thought were my own were undoubtedly a reorgan-

Meillet, Saussure, Hermann Paul, and others³⁴ to make this genealogy at least somewhat more realistic.³⁵

That the transition from dialectological work to sociological research is something of a natural development can be shown also by reference to work done for example on Dutch (and Low German) during the first decades of the 20th century. Noting that ‘the sociological approach had scarcely found its way into linguistics’, Jacobus van Ginneken (1877–1945) attempted to bring about just this sort of approach in his 2-volume *Handboek der Nederlandse taal* (Van Ginneken 1913–1914), as he indicated in the subtitle to his work: “De sociologische structuur van het Nederlands” (cf. Hagen 1988a:271–272, for details). The next decade saw the publication of a work which Hagen (p.273) rightly characterizes as “a very advanced socio-linguistic study”, namely, Gesinus Gerhardus Kloeke’s (1877–1963) *De Hollandsche expansie* of 1927, a work which Bloomfield, in his book *Language*, treated as paradigmatic for the discussion of isoglosses in his chapter on dialectology (Bloomfield 1933:328–331).³⁶ As the full title of his book indicates (see the References), the author is combining in his research geography, dialectology, and history. Sound change — one of his famous examples is the divergent development of Common

ization of my thinking under the influence of one of the most profound and powerful linguists of our time.” Labov (2001) is appropriately dedicated to Uriel Weinreich.

³⁴ Not to mention the by now ‘classic’ studies by Gauchat (1905) and Hermann (1929), which in a way god-fathered his M.A. thesis on Martha’s Vineyard (cf. Labov 1972b[1963]: 15n.25 and 23), since Labov dealt with a similar situation and lapse of time between Guy Lowman’s analysis of the early 1930s and his own of the early 1960s.

³⁵ Calvet (1999), who reproduces an earlier (Koerner 1991) version of this diagram (p.27), takes issue with it and almost builds his entire 30-page article around it without providing a more convincing scenario, in part because his bibliographical base (55–57) is much too limited.

³⁶ However, as Gerritsen (2001) has shown, the idea of ‘expansion’ (German: Ausbreitung), with which Kloeke is often credited, derives from research conducted by Theodor Frings (1886–1968) and published in 1926, a year before Kloeke’s magisterial work was published. As Gerritsen (p.1545) illustrates, Kloeke was well acquainted with Frings’ work:

In the 1920s Frings worked at the university of Bonn. This job gave him plenty of time to do research but due to strong inflation not enough money to support his family. In the same period, Kloeke worked as teacher of German at the Leiden gymnasium, spending long hours, but making lots of money. Being a bachelor at that time Kloeke could afford to wish more for research time than money. In light of their respective circumstances, the two arranged to switch places in ’21–’22. In his new situation in Bonn period Kloeke became well acquainted with expansions as an explanatory device (W.U.S. Kloeke personal communication).

See also Frings & van Ginneken (1919) and Frings’ work with Jozef van den Heuvel (1889–1966) of 1921, which, according to Gerritsen (p.1541) constitutes “[t]he most scientific publication of Dutch dialect texts of this period [...], in which we find among other things the 40 sentences that Wenker used for the first dialect survey in the world [Wenker 1877]. They are translated and phonetically described in 56 southern dialects.”

Germanic pair *hūs/mūs* ("house"/"mouse") in Dutch — in his work is demonstrated as taking place by a process of social adaptation or borrowing from the speech of the upper classes by speakers from the socio-economically lower classes.³⁷

2.2 Historical linguistics, language change, and sociolinguistics

While the dialectology–sociolinguistics connection seems rather obvious (cf. Grootaers 1982), the link that exists between certain traditions in historical linguistic work and sociolinguistics perhaps not. Interestingly enough, very early in his career, Labov had made it clear that the focus of his research "has always been on the understanding of linguistic change" (1966b:102). It is therefore not surprising that, realizing where he had come from, we can find the connection between sociolinguistics and early work on language change acknowledged in the work of Labov. It began early on in his linguistic studies, when working on the materials for his Master's thesis (Labov 1963), which later formed the first chapter of his *Sociolinguistic Patterns* of 1972, the year he wrote what appears to be his first thorough critique of traditional historical linguistics (Labov 1972a), inspired no doubt by his close collaboration with Uriel Weinreich on the by now classic article "Empirical Foundations for a Theory of Language Change" (Weinreich et al. 1968). Already in his M.A. thesis, we note Labov's opposition to the positivistic approach to the subject he still found in Lehmann (1962), whereas Martinet's *Économie des changements phonétiques* of 1955 is seen as having received empirical confirmation in William G. Moulton's (1914–2000) dialectological work of 1962 (Labov 1972b[1963]:2). Edgar H. Sturtevant (1875–1952), too, receives praise both in 1963 and in 1972 for his recognition of social factors such as prestige (Sturtevant 1947:80–81,³⁸ cited in Labov 1972b: 3 and 263). Sturtevant would find himself in a group of praiseworthy scholars together with Whitney and Meillet, though barely, representing "a late survival of Meillet's fading notion that we might search for an explanation of the fluctuations of linguistic change in the fluctuation course of social events" (263–264). In his important article orig-

³⁷ I dwell on the German and Dutch work in dialectology because it is underrepresented in Labov's scholarship, if recognized at all, although it was this work, together with the Swiss research led by Jaberg and Jud (which itself was an outgrowth of the 'Marburg school'), and which was transplanted into North America, which was so important, not the work of Swiss-born Jules Gilliéron (1854–1926) and his single, energetic, bicycling field worker Edmond Édmond (1849–1926) in France on the *Atlas linguistique de la France* (Paris, 1902–1910).

³⁸ Labov does not seem to have referred to Sturtevant's earlier book (Sturtevant 1917; repr., 1961), which dealt expressly with language change.

inally commissioned by Thomas A. Sebeok for *Current Trends*, "The Social Setting of Linguistic Change" (1972a), Labov states:

In 1905, Meillet predicted that this century would be devoted to isolating the causes of language change within a social matrix in which language is embedded. But that did not happen. In fact, there were almost no empirical studies of language change in its social context in the 50 years following Meillet's pronouncement. (1972b:267)

Labov appears to be referring to the celebrated paper by Antoine Meillet,³⁹ "Comment les mots changent de sens", which Meillet published in volume 9 of Émile Durkheim's (1858–1917) periodical *Année sociologique*, and which still today is regarded as exemplary in the semantic change debate (cf. Arlotto 1972: 163–183 *passim*; Lehmann 1962:198–199; 21973:212–213). One might assume that Labov (cf. 1972[1963]:23; also Labov 1966:11, 2001:279) would have taken the by now 'classic' studies by Louis Gauchat (1866–1942) of 1905 and by Eduard Hermann (1869–1950) of 1929 on language change in a particular speech community as exceptions to his pronouncement (cf. Lehmann 21973: 163–164)⁴⁰; however, one misses a reference to the work of Kloeke for instance in Labov's chapters on historical change in *Sociolinguistic Patterns*.⁴¹ Earlier in his work Labov (1966a:263) quoted, with approval, the following passage from Joseph Vendryes' *Le Langage: Introduction linguistique à l'histoire* (completed, according to the author, in 1914, but published only in 1921; and translated into English by a former student of Franz Boas in 1925):

Language is [...] the social fact par excellence, the result of social contact. It has become one of the strongest bonds uniting societies, and it owes its development to the existence of the social group. (Vendryes 1925[1921]:11)

Vendryes (1875–1960), first a pupil and later a long-time collaborator of Meillet's, fully shared his teacher's views on the social nature of language and on the desirability of establishing a sociological linguistics. His goal, like Meillet's and his school (cf. Bolelli 1979), was to pinpoint the causes of linguistic change and not simply describe the mechanism of linguistic evolution as was

³⁹ I say "appears to be" since Labov does not supply a reference; in fact, from other references to Meillet in the same volume (Labov 1972b: 185n.2, 263, 318), it would seem that he is in fact referring to Meillet's paper of 1906 inaugural address (as in Labov 1966:10).

⁴⁰ Interestingly, neither Gauchat (1905) nor Hermann (1929) are mentioned in Lehmann (1962); their findings are, however, discussed in Weinreich et al. (1968) and probably in other historical linguistics texts. In his 1973 edition of *Historical Linguistics* (p.163), Lehmann refers to Sommerfelt's 1930 article (listed in Labov 1972b:335, but only criticized for lack of terminological clarity on p.277) as a study which found similar results (as those of Gauchat) concerning the difference among speakers of varying ages in Welsh and Irish speaking communities.

⁴¹ Kloeke's name does not appear in Weinreich's huge bibliography [1953:123–146] either.

common practice among the more traditional, Indo-Europeanist historical linguists, of the 19th and early 20th century. While Meillet ventured little beyond the area of lexical change (which in many instances offers itself best to a sociological interpretation as regards the reasons for meaning change, loss of words and the like), Vendryes, as the title of his book suggests — although he too devotes two chapters to meaning change alone (Vendryes 1925:192-211 and 212-230) — tried to argue that linguistic evolution is but a reflection of social evolution (pp.352ff.).

Yet while other students of Meillet's, such as Alf Sommerfelt (1892–1965) from the 1930s onwards (e.g., Sommerfelt 1932) and Marcel Cohen (1884–1974) in his later years (e.g., Cohen 1956a), belaboured the subject of a sociology of language, it is fair to say that little concrete advance was made in the explanation of language change on the basis of social factors (cf. Labov's [1972:267] remarks). However, Meillet's student André Martinet did instill in his student Uriel Weinreich a strong interest in historical linguistics and the explanation of the causes of linguistic change (cf. Martinet 1955), an interest Weinreich in turn passed on to his student, William Labov (cf. Weinreich et al. 1968), as may be gathered from much of his research from the mid-1960s onwards (e.g., Labov 1982, for a monograph-length account; Labov 1992, 1994). Labov's work may thus be said to constitute a synthesis of earlier attempts at a sociological approach to questions of language change, beginning with Meillet's paper of 1905 (if not much earlier) and dialectological research done in the United States since the 1930s which, as we have seen, goes back to European traditions established during the last quarter of the 19th century. Even Whitney, who made such seemingly modern-sounding observations as the following, was largely European-trained:

We regard every language, [...], as an institution, one of those which, in each community, make up its culture. Like all the constituent elements of culture, it is various in every community, even in the different individuals composing each. (Whitney 1875:280)

2.3 Bilingualism, multilingualism, and languages in contact

There is yet a third line traceable in more recent work (ignoring the late 19th-century debate on 'Mischsprachen' and the like) that filtered, I believe, into much modern-day work in sociolinguistics. I am much less concerned here with the kind of bilingualism research that Werner F. Leopold (1896–1984) conducted in the 1930s and 1940s (Leopold 1939–1950),⁴² which is

⁴² Leopold's work served however as a valuable source for Roman Jakobson's (1896–1982) work on child language, for example.

more directly associated with 'psycholinguistics', especially first and second language acquisition, but with bilingualism research that is conscious of the socio-political environment in which it occurs. I am thinking in particular of the work of Einar Haugen (1906–1994) from the early 1950s onwards, especially his ground-breaking study of the Norwegian language in the United States (Haugen 1953). In this context, it is interesting to note that Weinreich *père*, like his son a fluent speaker of Yiddish, published papers on bilingualism as early as in 1931 (M. Weinreich 1931a,b). It is easy to imagine that following the arrival of the Weinreichs on the North American continent during World War II, and given the multilingual situations that they must have encountered in New York City, their interest in plurilingualism and language contact would have increased. (In this last regard Martinet's influence on Uriel Weinreich must have been of singular importance.)

In other words, the sources of modern-day sociolinguistics are diverse and complex, and they all had a bearing on the development of the various research programs from the 1960s onwards. In the North American context, where over 10% of the population is of African ancestry, we should not forget the importance that was attached by the Kennedy and Johnson administrations to the study of what was soon called 'Black English' (cf. Drake 1977:78-106, for details), research in which Labov was very prominent (cf. Labov 1966a, 1972a; see also Wolfram & Fasold 1972), though by no means the first (e.g., Stewart 1957).⁴³

However, there is a line of work in the United States — conveniently dismissed by Labov early on in his career so that it was completely ignored by his followers — which rightly should have been added to an adequate history of 'sociolinguistics'. I am referring to the pioneering work by the sociologist at the Catholic University of America in Washington, D.C., Paul Hanly Furfey (1896–1991), from the 1930s onwards, and the field work carried out in the inner city of Washington, D.C., by two of his doctoral students, George Nelson Putnam (1909–1991) and Edna M. O'Hern (b.1919), whose results were published in a combined work in the monograph series of the Linguistic Society of America (Putnam & O'Hern 1955),⁴⁴ years before it became fashionable to study 'Black English' and to combine urban dialectology with sociological theory.

⁴³ I have not addressed the question of the tremendous funding that Labov and his associates received during this period, a subject at least Americans know how crucial this aspect is when it comes to developing a following. Until Congress established the National Defence Education Act (NDEA) in 1958, there was no such largesse available to linguistic research.

⁴⁴ Indeed, Pickford (1956:223) found their study "remarkable for its sociological awareness" and "a significant attempt to establish the importance of speech as a mark of social status".

A reviewer of this work by Putnam and O'Hern in *Language* was very critical of what he regarded as their poor methodology (Evans 1956),⁴⁵ and it appears that Labov echoes this judgment, when he notes:

The work suffered from a number of limitations: the selection of informants was totally unsystematic, and from the occasional background information which was collected, it appears that only a minority of informants had any connection with the neighborhood or Washington during their formative years. The speech of the informants was judged as a whole, and it is not clear what the judges were reacting to, or how representative their judgments were. (Labov 1966a[1964]:19)

Indeed, one wonders whether Labov based his severe criticism on his own study of this rather compact 32-page work or whether he was simply guided by Evans' critical review. As Joseph (1992) has shown in his careful analysis of Monsignor Furfey's contribution to sociolinguistics, the work by his students was anything but a careless effort to study the speech of a socially disadvantaged group of people, but an all-out effort at what Labov likes to call a "socially realistic description" (Labov 1966a: 9, 25n.5). As Joseph (120-121) shows, Putnam & O'Hern (1955) undertook a large number of interviews presenting their informants with various tasks, transcribed the responses on the spot as well as taped them for subsequent spectrographic analysis. They offered in addition a description of the non-standard morphological and syntactic features that they had noticed, before undertaking a social analysis of the speech of a dozen selected informants with regard to the social status that others would associate with it. Tapes were presented to altogether seventy other persons in order to obtain their reactions and rankings in terms of social class. These rankings in turn were compared with the actual status of these speakers in terms of the Index of Status Characteristics (I.S.C.), which had been developed by W. Lloyd Warner (1898-1970) during the 1940s (see Warner et al. 1949), and to which Labov refers in his own work, e.g., in his chapter on "Class Differentiation of the Variables" (Labov 1966a:207-268, at p.236 and note 10 on p.268; see also Labov 2001:60).

Putnam & O'Hern reported that the ratings of only two out of the twelve speakers had been rated by these judges "above other speakers whose I.S.C. scores were higher" (p.26). They pointed out that in their view (p.27): "These results bear out the hypothesis of this study, that the dialect of Columbus Court

⁴⁵ Evans (1956:825) concludes his review in the following manner:

Interesting as it is, the Putnam-O'Hern study is almost completely vitiated, [...] by the absence of an adequate methodology, both in obtaining and analyzing the data. Any subsequent investigations in sociolinguistics ought rather to profit by its mistakes than to repeat them.

Cohen's (1956b) review of the same 32-page study is much more generous, if less thorough.

residents does reflect low socio-economic status". They also found it "most remarkable" that their study had demonstrated "that untrained judges could rate the social status of speakers so accurately after listening to a very short speech selection in the absence of all irrelevant cues" (p.29).

In Joseph's (1992:120-121) assessment, in Putnam & O'Hern's study, we see the coming together of "phonetics, the distributional method, dialect geography, highly organized sociological investigation, formal scientific method", in addition to the social activism that their mentor was known for, when they conclude their work with the following observation:

The importance of speech as a mark of social status (at least in the case of this particular group) is a matter of great social significance [...]. Persons who grow to adulthood as members of an underprivileged social group may carry a mark of their origin through life and suffer from the various forms of discrimination which society imposes on members of the lower socio-economic classes. (Putnam & O'Hern 1955: 29)

It appears that the result of Labov's dismissal, as the first item on the agenda in his section on "Studies of subjective evaluation of language" (1966a: 19-23), was that Putnam & O'Hern's pioneering efforts were effectively written out of the historical record.⁴⁶ Joseph (1992:123) found only one other reference to their work by the social psychologist Susan Ervin-Tripp (b.1927; on her, cf. Murray 1998:98-99). She gave credit to the authors by describing their work in the following manner:

Another kind of participation-form study is illustrated by Putnam and O'Hern (1955) of the relation between social status, judged by sociological indices, and linguistic features of speech in a Negro community in Washington, D.C. This study has many similarities in method to dialectology, but adds a procedure of judges' blind ratings of status from tapes, to make a three-way comparison possible between objective status, perceived status, and specific features. (Ervin-Tripp 1964:92⁴⁷)

But then she adds: "Labov (1964) gives a sophisticated analysis of a status-form relation" (ibid.), thereby suggesting that from now on, beginning with Labov's Columbia dissertation, Putnam & O'Hern's work could safely be ignored, which indeed it was. As Joseph (1992:122) suggests, Labov's work was soon taken by the younger generation of linguists, for instance Ralph W. Fasold (b.1940) and Walter A. Wolfram (b.1941), as the beginning of a new field of research with "tremendous potential for generating grant support".

⁴⁶ There is no mention of their work in Labov's 572-page *Principles ...*, vol.II: *Social factors* (Labov 2001).

⁴⁷ The correct location of this quote has been taken from Murray (1998:56n.11), who also notes that Labov had also ignored Furfey's work entirely, notably his (1926) book on gangs and his (1944) paper on 'substandard' English (Murray, p.56).

3. Concluding observations

Writing at the beginning of the century under the influence of Durkheimian sociology, Meillet (1905, 1906) did not have a name for the new approach to language and language change in particular. But just a few years later, in 1909, his compatriot Raoul de la Grasserie (1839–1914) spoke of 'sociologie linguistique' in a programmatic article.⁴⁸ The term 'sociolinguistics' however — at least in North America — did not make its appearance in regular print before around 1950, but apparently was still too fresh to be employed in (the titles of) either Haugen's or Weinreich's studies of 1953. Supposedly coined by Haver C. Currie (1908–1993) and used in a programmatic paper dealing with what we would refer to as '(social) register' of speech, the term 'sociolinguistics' was picked up by Ethel Wallis⁴⁹ in 1956, the same year that Glenna Pickford⁵⁰ offered a 'sociological appraisal' — and indeed a very critical view of the dialectological work of Kurath and his associates.⁵¹ It appears, however, that it

⁴⁸ Leo Jordan's "Essai de sociologie linguistique", *Romanic Review* 20.305–325 (1929), has little to do with the subject. It's essentially a philological paper devoted to treating the history of select lexical items of French by reference to dialect mixture and social stratification.

⁴⁹ Ethel Emily Wallis (b. c.1920), still today associated with the Summer Institute of Linguistics, Arizona branch in Tucson, published a 146-page book entitled *God Speaks Navajo* (New York: Harper & Row, 1968). Her previous work (1951–1963) is listed in *Bibliography of the Summer Institute of Linguistics 1935–1972* compiled by Alan C. Wares (Huntington Beach, Cal.: Summer Institute of Linguistics, 1974), pp. 93–94, 149–150, and 152.

⁵⁰ As the author (e-mail of 30 March 2001) kindly informs me, this article "was written as [her] thesis for a Master's degree in English at Fresno State College, now California State University, Fresno" under Earl D. Lyon and in consultation with William Beatty, anthropologist, and Wendell Bell, sociologist. Mrs Pickford (b.1921) is the wife of the well-known American aquarellist Rollin Pickford, and has not pursued an academic career. Her article is a severe critique of the methodology, training, provincialism, unreliability of the data collected by Kurath and his associates for the American Linguistic Atlas project (see Kurath 1949; Kurath et al. 1941). She concludes her (1956) article with the following observation:

The relative insignificance of a geographical study causes the Atlas project to be ignored by the very social scientists whose cooperation would be most salutary. It is hoped that future research in American speech will be used to determine the more significant questions and will bring the professedly sociological branch of linguistics up to date on social theory and scientific method.

A reader of this passage marked "Labov" in the margin of the page in the copy of *Word* held by the Perry-Castañeda Library of the University of Texas at Austin. Pickford's article is not mentioned in Labov (1972).

⁵¹ Thus Shuy (1989:298) is probably quite wrong in asserting that Currie's use of the term 'sociolinguistics' in 1952 was "the only extant use preceding the Lake Arrowhead and the 1964 UCLA Institute". For instance, Paul Friedrich, reviewing Ferguson & Gumperz (1960), spoke quite liberally of 'sociolinguistics' as if it had already been a widely established term (see Friedrich 1961:163). Indeed, Joseph (1992:125n.1) cites a number of other *loci* where the term 'sociolinguistics' was used such as by Eugene A. Nida (b.1914) in 1949 in the second edition of his influential *Morphology* (Ann Arbor: University of Michigan), Einar Haugen

took almost ten more years before 'sociolinguistics' became the generally accepted name for an important subfield of linguistic research (e.g., Bright 1966).

Considering the different sources of modern sociolinguistics traced in this paper, we might depict the evolution of the field with the help of the following (obviously incomplete) diagram — admittedly leaving out several indeed pioneering studies which did not become part of the Labovian 'canon':

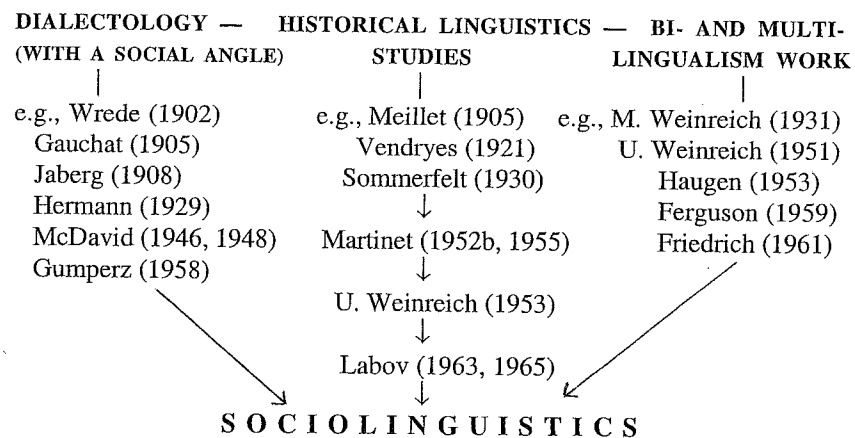


Fig.2: *The sources of sociolinguistics*

In particular, the diagram excludes extralinguistic, in notably sociological and psychological,⁵² work that has exercised an influence on sociolinguistic theory

(1906–1994) in 1951 (the printed text of his December 1950 LSA Presidential Address), to cite at least two predating Currie (1952). The most intriguing reference, however, appears to be Thomas Callan Hodson's (1871–1953) use of the hyphenated term as early as 1939. Hodson had retired from the British Civil Service in India before he was made the first occupant of the Chair of Anthropology at Cambridge in 1932. (I wonder whether there wasn't any connection between Hodson and J. R. Firth during the 1930s, given Firth's many years in India during the 1920s and his own sociolinguistic outlook.)

⁵² Characteristically, Labov had given the work in both areas short shrift in his dissertation, asserting: "In general, it may be said that psychologists and sociologists have lacked the linguistic training required to isolate particular elements of structure, and have worked primarily with vocabulary in content analysis" (Labov 1966a:23). No particular works are cited, but this assertion soon entered textbook history (e.g., Wolfram 1969:7).

and practice, and so at least one such example should be mentioned.⁵³ In view of the fact that the work of the French sociologist Jean Gabriel de Tarde (1848–1904) has almost been forgotten (cf. Lubek's 1981 account of "psychologies sociales perdues") and largely pushed aside by Durkheim and the work of his nephew and successor Marcel Mauss (1872–1950), it is refreshing to see some of his ideas being rehabilitated in Labov's work (in which Durkheim's work receives short shrift). In several places of his writings Labov refers to what he calls 'Tarde's Law',⁵⁴ though without explaining it. In a nutshell, Tarde has argued that imitation ("l'amant imite l'aimé") is at the bottom of social change, and language may fall under a similar spell that Labov and others have called 'prestige'.

While it is true that the appearance of a cover term for a particular field of research does not necessarily signal the beginning of a discipline, it may be seen to mark the point at which professional identification of a particular enterprise is regarded as desirable by at least some of its practitioners. Labov himself (1972b: xiii, 183–184), with initial hesitation, settled on 'sociolinguistics', which has become the broad term which also has come to include 'sociology of language' in most instances. He celebrates Ferguson & Gumperz's 1960 volume *Linguistic Diversity in South Asia* as "the beginning of the current interest in sociolinguistic studies" (p.296). But, as we have shown, Labov had absorbed many of the ideas of much earlier work in linguistics which, when working out his own program of a "socially realistic linguistics" (Labov 1972b:xiii).

Labov's kind of sociolinguistics has been seen, with some justification, as an antidote to the kind of socially unredeeming and linguistically non-empirical work that has come out of Chomskyan-type of linguistics over the past forty and more years. His dedicated return to both internal and social issues in the investigation of language change (Labov 1994, 2001) promises to become his legacy.

⁵³ Notably in his 1964 dissertation Labov, a number of works by sociologists are listed in the relatively short bibliography (1966a:583–588), but discussed only very briefly (e.g., p. 23).

⁵⁴ On page 286 of *Sociolinguistic Patterns*, speaking of "Tarde's law", he refers (in n.11) to "his *Lois de l'imitation* in 1890"; p.308, he refers to "Tarde's law (1913)", and in the bibliography (p.335), the entry reads simply: "Tarde, Gabriel. 1873[sic]. *Les lois d'imitation*. [no place, no publisher supplied]. See the present References under Tarde (1890), for correct and complete entry. Tarde's 'law' is nowhere explained in the book.

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⁵⁵ Labov's own website <http://www.ling.unpenn.edu/~labov/WLBIB.html>, consulted on 6 April 2001, does not include a single publication beyond 1995. The one in Guy et al. (1997 II, 339-352) is much more complete. See now also the one in Labov (2001[1997]) for more recent works.

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IN LIEU OF A CONCLUSION

ON THE IMPORTANCE OF THE HISTORY OF LINGUISTICS

As can be shown from history, those who have no past, usually have no future either.

Manfred Fuhrmann*

1. *Introductory remarks*

For some thirty years now, I have been arguing for the importance of the history of linguistics, and while not everyone has been convinced by the arguments, the climate of opinion has indeed changed. During the early 1970s, in the earlier stages of the institutionalization effort of the history of linguistics as a *bona fide* subject of instruction within linguistics proper, it seemed natural to make a strong appeal to the methodological soundness of linguistic historiography in order to render the subject respectable in the eyes of 'real' linguists for whom linguistics meant 'theory' (see Koerner 1972, 1976 as examples of this approach). This original attitude to 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 for what he was doing. Even though this type of ancestor hunt, an essentially presentist and unhistorical 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.¹

In Europe as well as among European-born linguists living in America, a historical approach to many subjects had a long tradition, and this may explain the fact that the scholarly reactions to Chomsky's *Cartesian Linguistics* were almost universally critical, at times rather harshly so (cf. Koerner & Tajima

* "Wie sich durch Beispiele aus der Geschichte belegen läßt, pflegt derjenige, der keine Vergangenheit hat, auch keine Zukunft zu haben." (Manfred Fuhrmann, *Bildung: Europas kulturelle Identität*, Stuttgart: Philipp Reclam, 2001, p.111).

¹ For a critique of this naïve approach to the subject, see my reviews of Peter H. Salus' *On Language: Plato to von Humboldt* (New York: Holt, Rinehart & Winston, 1969) in *Lingua* 25:4.419-431 (1970), and of the same author's *Pāṇini to Postal: A bibliography in the history of linguistics* (Edmonton, Alta. & Champaign, Ill.: Linguistic Research, Inc., 1971) in *Foundations of Language* 10:4.589-594 (1973).