The Old and the New:

Some remarks on phonology and its history

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"Linguistics has been preeminently a young man's pursuit ever since the 1920's"

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1. 1946-1996: a non-cumulative history of science

1.1. Leonard Bloomfield insisted on the necessity of developing linguistics as a science, that is, as a "cumulative" and "impersonal" discipline, refusing from the outset the idea of a discipline of linguistics organized into different schools each bearing the name of its founder: a Bloomfieldian school, for example. In 1946, in one of his last talks, he expressed his great satisfaction at seeing that the first twenty-one years of the Linguistics Society of America had managed to avoid precisely this danger, and had "saved us from the blight of the *odium theologicum* and the postulation of schools (...) denouncing all persons who disagree or who choose to talk about something else (...). The struggle with recalcitrant facts, unyielding in their complexity, trains everyone who works actively in science to be humble, and accustoms him to impersonal acknowledgement of error."[1] Barely ten years later, the battle between "post-Bloomfieldians" and "Chomskyans" was in full swing and soon, after the triumph of the Chomskyan school, orthodoxy, *odium theologicum* and contemptuous arrogance were to be the rule for a long time.

Already by 1950 Einar Haugen had expressed his concern that the task of "scientific accumulation," – the task of critically maintaining previous scholarship -- which had been practiced on both sides of the Atlantic until the war, no longer seemed to be an obvious scientific task for the young American linguists, who tended to neglect European research.[2] But in the 1960's, it was within American linguistics itself that the rupture took place. The cumulative task was correctly assured between linguists within the generative school itself[3] but, up till the 1980's, the work of the "post-Bloomfieldians" was practically unknown to the great majority of Chomsky and Halle's students.

1.2. *The Sound Pattern of English* (1968) appears today as the key book of 20th century phonology and only the *Grundzüge der Phonologie*, written thirty years earlier by Trubetzkoy, can rival its theoretical importance. Both of these works, moreover, were dedicated to Roman Jakobson, who is an obvious link between the authors of these works.[4] Today everyone recognizes distinctions between a pre-*SPE* phonology, an *SPE* -based phonology and a post-*SPE* phonology: *SPE* has thus been established as an imperative reference in the history of phonology. One of the intellectual strategies (among others) that permitted it to attain this position is revealed by the homage it paid to Jakobson and Trubetzkoy, which at the same time masks the deliberate "scorched

earth" policy practiced since the end of the 1950's, resulting in the destruction of the means for the intellectual subsistence of competing theories, all the structuralist phonologies being tied into the same bundle under the destructive label of "taxonomic phonologies."[5] If, during the preceding ten years, Chomsky, Halle (and some of their followers and students, including notably Paul Postal) had more and more aggressively attacked the positions of the masters in the field,[6] only sparing their respective teachers, Harris and Jakobson, in *SPE* there is no trace left of this polemic: the war was over, structuralism had breathed its last breath and no longer required the least theoretical debate. Generative phonology had extended its influence majestically and could permit itself to cite here and there – always sparingly – its great predecessors, but only in support of some point or other it was itself making. In the theoretical sections of *SPE* (I and IV, see above n. 4), Bloomfield is quoted once, Harris once (in a note), Hockett once, Jakobson four times (two of these in notes), while to Bloch, Fries, Joos, Pike, Trager, Twadell, Voegelin and other names of structuralism (including Householder and Martinet, who had, however, just been seriously trounced in debate) not the slightest allusion is made. Only Sapir, cited just once but in a passage which extends over seven pages, and Trubetzkoy, with eight quotations (two of which appear in notes), are given special attention. A good structuralist is a dead structuralist, perhaps. The new has done away with the old and cleared the path; from now on, the generativist phonologists can, if necessary, refer to such or such a pre-*SPE* work in a note, but they no longer submit works other than their own to discussion. A new theoretical principle has been established: either phonology is generative, or it is does not exist. Who could think that the young Chomsky had ever heard of Bloomfield (1939, *Menomini morphophonemics*) or the young Halle of Jakobson (1948)? And who could even imagine that this *sui generis* phonology was actually born in 1951, from the pen of a Chomsky who was inseparably generativist and post-Bloomfieldian?

The result is impressive. In 1968, phonology awoke radically expurgated. A rapid inventory of what was coldly abandoned, explicitly or implicitly, helter-skelter: the phoneme and the phonemic level, the relation between sound and meaning, the very notion of a phonological system, and in its wake, the concepts of opposition (primitive, equipollent...), of correlation, of neutralization (and the archiphoneme), but also, and more surprisingly, the syllable, the entire prosodic domain and the very idea of a suprasegmental unit, along with the concept of "a long component" which had seemed so promising a short time before (and to which Chomsky (1957) still referred positively). And finally, the very heart of the work of the *Grundzüge*, which everyone dreamt of dethroning: the – universal – study of the "phonological systems of distinctive oppositions": vocalic systems, consonantal systems, prosodic systems.

1.3. Since the end of the 70's, phonology has been undergoing a continual theoretical renewal. If we only consider the French publications, in 1984, Dell, Hirst and Vergnaud published *Forme sonore du langage,* a selection of articles devoted to "recent developments in phonology" which notably presented metrical and autosegmental phonology and a theory of the syllable; in 1988, I edited an issue of a review called *Nouvelles phonologies*, which introduced in particular dependence phonology and government phonology; nine years later, the editor of the March 1997 issue of *Langages* (n° 125) has legitimately taken up this same title to present entirely different theories, the most recent evolutions in the field of phonology, which nothing suggests is on the verge of being stabilized. The multiplication of theoretical frameworks has given rise to considerable dispersion, with the result that each linguist imprisons himself within a theoretical proposition, most often limiting his ambitions to just one empirical field. Debate between different theories is less and less frequent, and in most cases innovation is no longer being based on the falsification of a preceding theory. The new phonologies, however, have in common the rejection of the theoretical framework of *SPE*, and usually the quest for precursors in pre-*SPE* phonology. But the desire for a cumulative science is nowhere manifest. It is as if today's phonologists have more or less forgotten that the history of science is part of science. It is true that the historical

propositions recently put forth by the most qualified of today's linguists were not really made to convince them.

2. (Re)writing history

2.1. The first time Noam Chomsky's name appeared in linguistic literature, to my knowledge, was in 1947, at the end of Zellig Harris's Preface to *Methods in Structural Linguistics* (published in 1951): "The procedures of analysis discussed here are the product and outgrowth of the work of linguists throughout the world, to whose investigations the meager references cited here are an inadequate guide. This book owes most, however, to the work and friendship of Edward Sapir and of Leonard Bloomfield, and particularly to the latter's book *Language*. In preparing this book for publication, I had the benefit of many discussions with C. F. Voegelin and Rulon S. Wells III, and of important criticisms from Roman Jakobson, W.D. Preston, and Fred Lukoff. N. Chomsky has given much-needed assistance with the manuscript. (Z.S. Harris, Philadelphia, January, 1947)"

These lines are of extraordinary value for several reasons. First, they are a reminder that at that time, it was normal for the leader of a theoretical trend to refer to other important figures of linguistics, and also for the enumeration, on an equal footing with Jakobson, of names that have totally disappeared from current bibliographies (W. D. Preston, F. Lukoff)[7], which testifies that in the sciences, oblivion is a common fate. But above all this passage is of interest for its allusion to a nineteen-year-old Chomsky putting the finishing touches on the *Methods* manuscript, four years before its publication.

Some forty years later, Bromberger and Halle (1989) ended their plea in favor of a rule-ordered phonology by a "Note on Recent History," which — *a contrario* — appears to be an exemplary symptom of that formidable amnesia concerning genesis which struck generative phonology after the publication of *SPE,* prohibiting *de facto* the accumulation of the work of the preceding generation, and which continues to have an effect on one of the fathers of the discipline, at the very moment when he considers it indispensable to take a new look at history.

The authors remind us, as Halle has often done, that Bloomfield was one of the first to use extrinsically ordered rules in his "Menomini Morphophonemics" (henceforth *MM*), published in 1939. As in Halle (1962), the authors cite the well-known paragraph of *MM* (pp. 105-106), the same as that which Chomsky (1962) cited when asserting at the 9th International Congress of Linguists that *MM* is "the first modern example of a segment of a generative grammar with ordered rules."[8] Halle (1962) was already up in arms: "This study is unaccountably omitted in C.F. Hockett's work on the 'Implications of Bloomfield's Algonquian studies', *Language 24* (1949) 117-131" (n. 13 p. 67). After having alleged that, in *Language* (1933), Bloomfield considered the use of ordered rules in synchronic descriptions as inappropriate, Bromberger and Halle suggest: "As we have seen, some six years later, by the time he was composing 'Menomini Morphophonemics', Bloomfield had changed his position. The fact that he had done so, however, was totally ignored by the American linguistic community in the 1940's and 50's. The article was omitted — 'inadvertently' according to Hockett (1970, 494) — from his 'Implications of Bloomfield's Algonquian Studies', which was published in the issue of *Language* (24,1) dedicated to Bloomfield on the occasion of his sixtieth birthday in 1948. It is not referred to in Hockett's influential 'Two Models of Grammatical Descriptions' (1954) nor was it reprinted in Joos's (1957) *Readings in Linguistics.* The article was so unknown in America that Chomsky tells us he had not read 'Menonimi Morphophonemics' until his attention was drawn to it by Halle at the end of the 1950's."[9] Bromberger and Halle go on to recall the use of ordered rules by Chomsky (1979=1951)[10], and conclude with great confidence: "In 1951, Chomsky was thus independently

led to the same conclusions that Bloomfield had reached twelve years earlier. It is a matter of some puzzlement that none of Chomsky's teachers at the University of Pennsylvania drew his attention to Bloomfield's paper and suggested that he take account of it at least by including it in his bibliography. It is idle at this distance in time to speculate about the reasons for this oversight. In any event, as noted above, *Chomsky learned of the existence of Bloomfield's paper only in the late 1950's, many years after submitting his Master's thesis* "[11].

2.2 In questions of history, it is essential never to trust either one's own memory or the memory of those concerned, however reliable they may be. In reality, of the three theses postulated in Bromberger and Halle (1989) touching on the relation between the early Chomsky and the works of Bloomfield, none can withstand a careful, even though limited, inquiry.[12]

In the first place, it is difficult to defend the idea that Bloomfield (1939) was "unknown" to, or "totally ignored" by, the linguists in the United States during the 1940's and 50's, when the slightest perusal of the major texts on the subject immediately shows the opposite. In 1940, C.F. Voegelin wrote a review of *TCLP 8* (in which MM was published) in *Language*. Of the six pages of the review he devoted to the thirty-three different articles of this issue, two are reserved for *MM*. We can read in particular: "'Menomini Morphophonemics' is a construct applying on the whole to a level above phonetics and phonemics (...). Some attempts to show phonological alternation by special orthography have been far more complex than Bloomfield's. (...) Bloomfield's Menomini may be distinguished from the Nookta, Tûbatulabal and Potawatomi studies in that it alone is good to the reader: it gives him few theoretical forms and only simple rules to remember."[13]

In 1942, in the first important article on the question since 1939, "Morpheme Alternants in Linguistic Analysis," an article which is continually quoted within post-Bloomfieldian structuralism,[14] Zellig Harris refers readers six times to Bloomfield (1939) (that is, *MM*), from which he borrows for the first time the term "morphophonemic," introduced into the science by "Menomini Morphophonemics" with probable but implicit reference (at least according to Martinet's (1965b) interpretation), to Trubetzkoy's *morphonology* (*TCLP* n° 8 was dedicated to Trubetzkoy's memory). In 1947, in *Methods in Structural Linguistics*, in which he thanks Chomsky for his help, Harris cites, comments on, and contests Bloomfield (1939) several times in chapter 14 ("Morphophonemes"): in paragraph 14.32 and 14.33, where half a page is devoted to one of Bloomfield's (1939) examples (quoted in note 29), and above all in the appendix to 14.32, where Harris opposes a simultaneous application of rules to the successive order used in *MM*. "The effect of this descriptive order of the statements about alternation" he writes, "can be obtained alternatively by an exact statement of the representation of the morphophonemes" (p.237). In 1949, Rulon Wells, in "Automatic Alternation," also quotes Bloomfield (1939 both in the text (p.113) and in two notes (n. 1 and 9).

As for the 1950's, references to *MM* in articles on the question of morphophonology are just as frequent. In 1951, Zellig Harris published in *Language* a review of Edward Sapir's *Selected Writings* (1949). As might be expected, he cites and comments upon Bloomfield (1939), which he seems to consider to be a more advanced development of the treatment of processes than Sapir's.[15] F. Lounsbury (1952) cites and discusses Bloomfield (1939) four times, and includes it in his bibliography. In 1955, in what constitutes along with Harris's *Methods* the major reference on post-Bloomfieldian linguistics, *A Manual of Phonology*, Charles Hockett borrows from Bloomfield a number of examples from Menomini, and cites *MM* in his (very selective) bibliography. Finally and above all, *MM* is quoted three times by Noam Chomsky himself in *The Logical Structure of Linguistic Theory.*[16] Although the date of this text is always given as 1955, and the first manuscript versions as dating from the spring of that year, Chomsky points out in the introduction to the version published in 1975 (p. 3) that chapters I to VI were written in 1956. The notes in which

Bloomfield (1939) is quoted were written, then, either in 1955 or 1956. Whatever the case, these notes are sufficient to establish that Bromberger and Halle (1989) are mistaken when they suggest: "Chomsky learned of the existence of Bloomfield's paper only in the late 1950's, many years after submitting his Master's thesis" (p. 68), or on the preceding page: "this article was so unknown in America that Chomsky tells us that he had not read "Menonimi Morphophonemics" until his attention was drawn to it by Halle in the late 1950's." Perhaps they should have taken literally what they report Chomsky confided to them. He certainly must have known *MM* well before the end of the 50's since he quotes it in 1955 or 1956, and it obviously cannot be Halle who revealed its *existence* to him because he had already been acquainted with it in Harris's 1947 manuscript. However, he may have *read* it only at the end of the 50's on Halle's insistence.

If we look more closely at the evidence, however, even this *lectio facilior* cannot be maintained; it is difficult to understand how Halle, who was already very close to Chomsky in 1953, and had certainly read *LSLT* by 1955-56, could make such an error. The references to Bloomfield (1939) in *LSLT* are not simple referrals, but indicate close knowledge of the book, as in note 5 of chapter IV (p. 115): "Note that phonemes can often be 'embedded' in the morphophonemic level as primes of this level (…). In many languages when such morphophonemes have wide distribution and are complexly interconnected, such analysis can lead to very great economy. See Bloomfield 'Menomini Morphophonemics', my *Morphophonemics of Modern Hebrew*, and many linguistics studies." Who could imagine that the author of these lines would cite Bloomfield (1939) in this way without being familiar with the contents of the book? The evidence of Chomsky's exceptional qualities as a researcher is too great to believe this.

2.3. What is most troubling about Bromberger and Halle's assertion that Chomsky had been unaware of the very existence of *MM* until the 1950's is that it implies they have forgotten that Harris (1951=1947) included a long commentary on Bloomfield's article. This is all the more astonishing since Chomsky (1964, p. 70, n. 8) recalls explicitly that in *Methods*, Harris proposed to use simultaneous rules to account for what Bloomfield (1939) had accounted for with ordered rules: "Harris showed (1951, 237) that some of Bloomfield's examples of ordering can be handled by unordered rules that state the phonemic composition of a morphophoneme *in a strictly morphophonemic context*. But such methods do not generalize to such examples as the one given directly below." A reference follows to "a segment of the phonological component for modern Hebrew presented in Chomsky (1951)." The unique reference to the name (and the work) of Zellig Harris that appears in *SPE* (p. 19, n. 5) also alludes to this reinterpretation of Bloomfield's analysis: "The simultaneous application hypothesis was first made explicit by Z.S. Harris (1951, Appendix to §14.32), in a discussion of an example from Bloomfield (1939)[…]"

Even if they did not recall Harris's Preface (1951=1947), Bromberger and Halle cannot maintain that Chomsky had waited till the end of the 1950's to read *Methods*, when it is quoted five times in Chomsky (1953), his first linguistics publication, in which he asserts notably that his source is the chapters 15 and 16 of *Methods* (cf. n. 3). This article (received by the editor October 18, 1952) was written just after the "December 1951 revision" of *Morphophonemics of Modern Hebrew* (hereinafter, *MMH*), a work which clearly has as its source the preceding chapter of *Methods*, Chapter 14, in which we find the references to, and commentaries on, Bloomfield (1939). It is moreover a statement made by Chomsky himself, in the 1975 introduction to *LSLT,* which reveals what Anderson (1985 p. 314) and R. Harris (1993, p. 52) paraphrase without indicating the origin: "My formal introduction to the field of linguistics was in 1947, when Zellig Harris gave me the proofs of his *Methods in Structural Linguistics* to read" (p. 25). We conclude from this perusal of the texts that Halle and Bromberger have closely reread neither Harris (1951=1947), nor Chomsky (1975=1955), nor Chomsky (1964), nor even Chomsky and Halle (1968).

2.4. What is most difficult to accept in Bromberger and Halle's (1989) "Note" is the indirect attack on Zellig Harris: "It is a matter of some puzzlement that none of Chomsky's teachers at the University of Pennsylvania drew his attention to Bloomfield's paper and suggested that he take account of it at least by including it in his bibliography" (p. 68). Having read Harris (1942), Harris (1951=1947), Harris (1951b) we are left speechless. As far as the bibliography is concerned, it is particularly strange that the authors seem to be unaware that Chomsky (1979=1951) does not in fact give us a bibliography as such, but only a list of "references." Under this rubric we only find indeed the five titles which are quoted by the text, among which figures, of course, *Methods in Structural Linguistics.*

A reading of the 1975 introduction to *LSLT* confirms, moreover, the fact that Chomsky was very far from having cited in *MMH* all his references, since we discover there that Harris (1939), *Development of the Canaanite Dialects,* should have been included as a reference although it appears neither in the text nor in the bibliography, as well as his father's book, William Chomsky (1952), *David Kimhi's Hebrew Grammar*, the proofs of which he had read many years before its publication. The omission goes much further, and continues in 1975: after *Methods*, the most important and the most precise source for *MMH,* is Harris (1941) "Linguistic structure of Hebrew," as John McCarthy (1981)[17] has shown. This fundamental reference appears neither in Chomsky (1975=1955) nor in Chomsky (1979=1951), nor elsewhere in Chomsky's writings as far as I know —although, the young Chomsky must have found it already in 1947, on page 168 of *Methods,* note 29.

3. Genealogy

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3.1 Thus, as we have seen, Chomsky asserts on at least four occasions that he did not know Bloomfield (1939) when he wrote *MMH*. First, in 1975: "Bloomfield's 'Menomini Morphophonemics' *TCLP* 1939, might be regarded as a segment of a generative grammar in approximately this sense. My *MMH* was written, I regret to say, in ignorance of Bloomfield's study" (n. 6, p. 47); second, in 1977, in his *Language and Responsibility*: "In fact [the notion of an explanatory theory] is a preoccupation which appeared very early, as early as my work on the morphophonology of modern Hebrew. I was then an undergraduate student. This work, which was never published, was the first modern 'generative grammar' in a domain that today we would call generative phonology (although there had been classical precedents, for example Panini's grammar of Sanscrit or more recently Bloomfield's *Menonimi Morphophonemics,* which I did not know at that time)" (p.122); third in 1988, according to Bromberger and Halle (p. 67 cf. below 2.2)[18]; and finally in 1995 in an interview with Robert Barsky, who was preparing his biography: "Hoenigswald and Harris were very close to Bloomfield and certainly knew his work [=*MM*]. But neither of them mentioned to their only undergraduate student that he was rediscovering, more or less, what Bloomfield had just done eight years before. It's not surprising in Harris's case, because he didn't know what I was doing. But Hoenigswald read it, and must have recognized the similarities, back to classical India. I learned nothing of this until the 1960's, when Morris Halle found out about Bloomfield's work" (31 March 1995)[19] We can note that six years after the publication of Bromberger and Halle (1989), the date of Chomsky's becoming aware of *MM* was once again delayed: it is no longer "only in the late 1950's" as Bromberger and Halle maintain, citing Chomsky directly, but "the 1960's."

If we attempt to reconcile this insistence of Chomsky's with what we otherwise know, we must imagine a complex and precise scenario. After having read the manuscript of *Methods*, in 1947, and having had "some stimulating discussions with Harris," Chomsky, who had some knowledge of historical linguistics and the grammar of medieval Hebrew from his father's work, and who was studying Arabic with Giorgio Levi Della Vita, decides to study linguistics at the

University of Pennsylvania. Harris suggests that he undertake the construction of a systematic structural grammar of a language and he chooses Hebrew, which he knows very well: "I constructed a detailed grammar concentrating on the rules for deriving phonetic forms from abstract morphophonemic representations. A version of this was submitted as an undergraduate thesis in 1949, and a more extensive version as a master's thesis in 1951" (Chomsky, 1975, p. 26). We can allow, if we take literally what he says to Mitsou Ronat in 1977, which is indirectly reported by Bromberger and Halle (1989), that the 1949 version was written without his having read *MM,* although he must have known of its existence from Harris's (1942) and (1951=1947) references at a time when he was preparing a paper in which he used Bloomfield's (1939) neologism *morphophonemics*, which Harris himself had borrowed in chapter 14 of *Methods*. This chapter made reference not only to *MM* but also to Bloomfield's *Language* (1933). In fact, it is only to this last book that Harris relates the use of *ordered rules*: "More regular statements can sometimes be obtained by employing the technique of descriptive order used in Leonard Bloomfield, *Language,* 213 " (p. 236, n. 39). It should be noted that the allusion to a *descriptive order* is here totally positive. In *Methods*, on the following page we find the "Appendix to 14.32," "Morphophonemic Equivalent for Descriptive Order of Alternation" that Chomsky (1964) and *SPE* cite as opposing to Bloomfield (1939) a simultaneous application of the rules.

The theoretical evolution in Harris's *Methods* between note 39 (p. 236) and the "Appendix to 14.32" (p. 237) leads one to believe that the appendix in question might have been added after the note. Between 1947 and 1951, the text of *Methods,* which had been widely circulated in its manuscript form, might have been modified.[20] We can suppose that in 1949 Chomsky directly used Bloomfield's (1933) "descriptive order" without having first read the 1939 article (which, in fact, the paragraph 14.32 of *Methods* did not indicate as using "the descriptive order"). It can also be imagined that it was after having read the first version of Chomsky's work, in 1949, that Harris developed an "equivalent" to avoid recourse to the descriptive order, which would explain the "Appendix to 14.32." It would be only then that Chomsky became acquainted with "Menomini Morphophonemics"— this is just a hypothesis, but difficult to exclude —, a probable reading that would have reinforced him in his bias in favor of ordered rules, because, in spite of Harris, his 1951 text is founded on them. Or perhaps again, he read *MM* between *MMH* and *LSLT.* According to one or the other of the hypotheses, it is quite natural that, in 1955-56, he places *MMH* in the line of *MM*.

3.2 If we follow this scenario (which Chomsky alone can confirm or invalidate) Chomsky's memory is good: it is not in Bloomfield (1939), whose existence he had known of since 1947 but that he had not yet read in 1949, that he first encountered the idea of a generative grammar. But what he does not say explicitly, and which yet could not be clearer, is that — on the suggestion of *Methods* — he certainly did borrow it from Bloomfield: from *Language* (chapter 13: "Morphology") in which he found the proposition for employing *ordered rules* in synchrony. And that, moreover, it is quite the same model as in Bloomfield (1939) that Chomsky himself used in December 1951 in writing *MMH*, pushing it however much further since, he suggests, he had reached "a depth of order of at least twenty five" (while "according to Bever's (1976) analysis, Bloomfield had only reached a depth of eleven")[21]

This kind of morphophonology, then, was certainly well known in 1951. But the use of ordered rules had convinced no one, especially since Harris, as Chomsky (1964) emphasizes, had demonstrated that the problems concerned could be accounted for just as well without having recourse to a sequential ordering in the application of the rules. The idea was not ignored so much as abandoned. The genius of the young Chomsky, between 1947 and 1951, was to take it up again, but in his own way, submitting it to the service of a criterion of *simplicity*. ("The rules are ordered in order to give the simplest grammar" (Chomsky, 1979=1951, p. 4), and this despite the

propositions of his thesis director. Recognizing the place of Bloomfield (1939), the first attempt at a generative grammar — without any immediate continuation, in the birth of Chomskyan phonology narrows in no way the breakthrough that Chomsky (1951) constitutes, which was to find, with Halle's collaboration, its most brilliant echo in *SPE*. On the other hand, the manner in which the old and the new are linked is then somewhat clarified.

3.3. There is something else that is surprising in Bromberger and Halle's (1989) "Note": the insistence on separating the Bloomfield of *Language* (1933), which Chomsky, like any linguistics student at the end of the 40's must have undoubtedly read, from the Bloomfield of *MM*, which Chomsky says he had not read and whose relation with *MMH* Bromberger and Halle take some pleasure in emphasizing.

Harris (1951=1947), as we have seen, pointed out in a positive way Bloomfield's (1933, p. 213) proposal to use a *descriptive order* which allows more regular rules, and Chomsky (1964 p. 70) confirms, as clearly as possible, Bloomfield's contribution to this point, comparing him advantageously to Sapir: "Most of the examples in Sapir (1933) involve ordering, though he does not explicitly mention this fact. Bloomfield was much concerned with rule ordering and his "Menomini Morphophonemics" (1939) is the first modern example of a segment of generative grammar with ordered rules." Notice above all that Chomsky attached to the expression *rule ordering* a referral to note 8: "Cf. Bloomfield (1933, 213). He regarded ordering rules as an artifact — an invention of the linguist (…) But this depreciation of the role of order of synchronic processes is just one aspect of the general antipathy to theory (the so-called 'anti-mentalism') that Bloomfield developed and bequeathed to modern linguistics." There follows the reference already mentioned to Harris (1951, p. 237) and his treatment of the examples of *MM* by unordered rules.

The genealogy is clear: Bloomfield (1933) Bloomfield (1939), Harris (1951=1947), Chomsky (1951). This genealogy, by direct transmission, seems to be historically uncontestable. But Bromberger and Halle do their best to dislocate it: "In fact, in his book *Language* (1933) Bloomfield fully shared the views about the irrelevance of rule order in synchronic descriptions" (p. 66). This is followed by the quotation from Bloomfield (1933) that is referred to in Chomsky (1964): "The descriptive order of grammatical features is a fiction and results simply from our method of describing the forms." Bromberger and Halle, as we have seen, assert then, without comment: "As we have seen, some six years later, by the time of composing 'Menomini Morphophonemics' Bloomfield had changed positions"(id.). Twenty years after Chomsky (1964), Bromberger and Halle take exactly the opposite position on this point of history: where Chomsky shows the continuity from Bloomfield (1933) to Bloomfield (1939), they see a rupture. They take up the word *fiction* to argue that Bloomfield (1933) denied what he was the first to propose explicitly, and which, as Chomsky pointed out, Sapir failed to do. In 1939, moreover, it is difficult to see how he could have changed: he develops a generative grammar of Menomini in the form of ordered rules, but specifies: "Our statements of internal sandhi are not historical but descriptive, and appear in a purely descriptive order"(p. 106). The reference to *Language* (1933, p. 213) is direct, and it follows that, in *MM* too, for Bloomfield, the sequence of ordered rules is only a "fiction," an artifact of the linguist. But it is obviously Chomsky who is right to place the use of this term in its historical context. At the same period, Twadell defined the phoneme "as an abstractional fiction," recalling that Bloomfield had asserted: "The phoneme is an abstraction obtained from series of utterances."[22] The descriptive order is then for Bloomfield on the same plane as the phoneme —on the very plane of the activity of the phonologist, and any attempt to separate radically *MM* from *Language* on this point is untenable.[23]

This amounts to saying that one of the essential founding blocks of generative phonology is encountered in *Language* (1933), not as a previously posed concept, but as a concept effectively

transmitted: "accumulated" by the young Chomsky, by way of Harris, who, I repeat, stated that "more regular statements can sometimes be obtained by employing the technique of descriptive order used in Leonard Bloomfield, *Language*, p. 213."[24]

3.4. Reconstituting the genealogy of generative phonology requires that we go back once more to Harris (1951=1947). The last chapter, "Survey," includes at least two essential passages with regard to this. In the first (20.21: "To State Regularities or to Synthetize Utterances"), Harris holds that the researcher in linguistics must choose between two projects: "He may seek all the regularities which can be found in any stretch of speech, so as to show their independences [...]; or *he may seek just enough information to enable anyone to construct utterances in the language such as those constructed by native speakers* [...]"[25] It is Chomsky himself who leads us to this paragraph as a source, in 1975, in the introduction to the edition of *LSLT*: after asserting that for Harris, Bloch, Trubetzkoy, Pike and others "the notions of generative grammar, in the sense of the foregoing discussion, are never clearly developed" (p. 11), he specifies in a note: "See, however, the remarks in Z.S. Harris's *Methods in Structural Linguistics* (p. 365-366), on a grammar as a device for "synthesizing" utterances."[26]

In 1955-56, in *LSLT*, Chomsky makes reference to a second passage in Harris (1951=1947): "Several methods of promoting grammar of the first type [i.e.: "With its law-like rules for the combination of elements, a grammar can thus be said to "generate" a certain set of utterances on the basis of a given observed sample."] are discussed by Harris, *Methods* paragraph 20.3; cf. Bloomfield, "Menomini Morphophonemics," Jakobson, "Russian Conjugation," as examples of grammars of this general form" (p. 78, n. 2).[27] Note, first of all, the reference to *Methods* 20.3 where Harris directly opens the way to generative phonology. I think it is necessary to quote several sentences (unfortunately missing in Bromberger and Halle (1989)) in which those who do not know them will be surprised to find a vocabulary that they thought had only come later: "The work of analysis leads right up to the statements which enable anyone to synthesize or predict utterances in the language. These statements form a deductive system with axiomatically defined elements and with theorems concerning the relations among them (…) There may be various ways of presenting this system, which constitutes the description of the language structure. The system can be presented most baldly in an *ordered set of statements* (…) Other types of presentation which have frequently been used have depended ultimately on moving-parts models such as machines or historical sciences. In using such models, the linguistic presentation would speak, for example, of base forms (e.g. in morphophonemics, where the observed forms are obtained from the base form by applying a phonemic substitution), of derived forms (e.g. stems plus those affixes which are added first in the description order might be called derived stems), or processes which yield one form out of another. In all these types of presentation, the elements are seen as having histories, so that the relation of an element to sequences which contain it becomes the history of the element as it is subjected to various processes and extensions" (*Methods*, p. 372-373. Our italics).

As Chomsky (1975) suggests, this little-known text is evidently the text which engendered his own undertaking, which he inaugurated with generative phonology. As he indicates, there is a "generative grammar" of this type which precedes this text, which proceeds from it then, in the sense that Harris extracts from it a theorized generative model: this grammar is in Bloomfield (1939), "Menomini Morphophonemics." Another generative grammar comes immediately after it, Jakobson (1948), "Russian Conjugation."[28] Still another generative grammar achieves the process, (re)founding phonology for a long time to come, Chomsky (1951), *Morphophonemics of Modern Hebrew* — but who knows this history?

3.5. Jakobson (1948), which Chomsky cites in 1955-56 as one of the two generative type grammars that preceded *MMH*, is not even mentioned in Bromberger and Halle (1989). This is all

the more surprising since, according to Anderson (1985, p. 318), the young Morris Halle was very much impressed and interested by this work, "which was virtually ignored by American linguists other than Slavicists at the time." Curiously, if it is quoted by Chomsky (1975=1955) and noted in his bibliography, Jakobson (1948) is never mentioned anywhere else in the classical texts of generative phonology, unlike Bloomfield (1939). It is neither found in Halle (1962), nor in Chomsky (1964), nor in Chomsky and Halle (1965), nor in *SPE*. Even more surprisingly, it is absent from Halle's (1959) bibliography, which is devoted to Russian. It is true that, if we look carefully, we can find mention of it in the notes, but exclusively as a source for examples or local treatments and never as a theoretical source, as a text which was a forerunner of generative grammar.[29] It is absent once again, then, from "A Note on Recent History," despite its "reintegration" within the world of generative phonology by Chomsky (1975=1955) and Anderson (1985). And yet Morris Halle indicates directly Jakobson (1948) as the article which gave him the idea of a grammar as a system of *rules*, specifying that what was missing in Jakobson (1948), which he himself owed to Chomsky, was the idea that the *rules* should be *ordered*.[30]

This is exactly what we are struck by if we compare *MM* and "Russian Conjugation." Both articles are short: ten pages for the first, twelve for the second. Roman Jakobson adopts an American style of presentation, relieved of what was left of verbosity in the European articles appearing in *TCLP*. After a half-page introduction defining his project, he rapidly presents the "fundamental notions" his description makes use of (stem, desinence, full stem, basic forms, etc.), and then develops a set of "general rules." The concluding paragraph states: "the rules formulated above and printed in boldface enable the student glancing over a bare inventory of full-stems to deduce their whole conjugational pattern with all the pertinent alternations in stem, desinence, and accent. If these few introductory rules are added, then a dictionary listing verbs only as full-stems would suffice to supply the reader with a complete knowledge of their inflection" (p. 162-163). A list of exceptions follows. The analysis is thus presented in fact as a generative type grammar, permitting us, if not to account for the competence of the speaker, at least to infuse a complete knowledge of the language in someone learning to speak Russian, and, if the order of the rules is never asserted, nor even mentioned, it seems to be clearly present, more or less explicitly, in certain places.[31]

3.6. The plan of "Russian Conjugation" is very close to that of *MM,* which was organized into thirty-seven brief paragraphs, in which the rules are neither more formalized, nor their ordering more apparent, but in which this order is asserted. Indeed, *MM* contains the following paragraph, exceptional in the entire literature of linguistics to that date, and which will remain an exception up until *Methods* and *MMH*: "The process of description leads us to set up each morphological element in a theoretical basic form and then to state the deviations from this basic form which appear when the element is combined with other elements. If one starts with the basic forms and applies our statements (§ 10 and following) in the order in which we give them, one arrives finally at the forms of words as they are actually spoken. Our basic forms are not historical but descriptive, and appear in a purely *descriptive order*. However our basic forms do bear some resemblance to those which would be set up for a description of Proto-Algonquian, some of our statements of alternation (namely those in § 10 to 18) resemble those which would appear in a description of Proto-Algonquian, and the rest (§19 and following), as to content and order, approximates the historical development from Proto-Algonquian to present day Menomini."[32]

Bromberger and Halle are right to insist, as Chomsky (1964) does too, on the theoretical importance of this text. But the quotations given above from *Methods* show that, as early as 1947, Harris had learned and transmitted the lesson perfectly —including the relations between synchronic and diachronic order. We should emphasize that all the terms used are in perfect continuity with *Language*: Bloomfield proposes neither there nor here a grammar of the speaker's

competence, but a generative grammar whose goal is descriptive. In Chomsky's (1964) vocabulary, he does not aim for "explanatory adequacy," but only "observational adequacy" and "descriptive adequacy." Just as for the chapter "Morphology" of *Language*, this text had been constantly reworked by Harris during the 1940's, as in all his articles touching on morphology. This is not the place to set up a history of this domain, from Trubetzkoy's (1929) introduction of the term to its translation into generative grammar, formalized by Chomsky (1951). It should, however, be emphasized that the position of Bloomfield, who proposed in 1933 and realized in 1939 a grammar with ordered rules leading from "theoretical basic forms" to "actually spoken" forms, is considerably more inventive than that of Trubetzkoy, and we must observe that Jakobson (1948) is much closer to Bloomfield (1939) than to the propositions of his Viennese friend**.**

Jakobson obviously knew Bloomfield (1939), which was published in *TCLP*, whose preparation he must have contributed to before he was obliged to leave Prague. He may have discussed it directly with Bloomfield, who was one of those who came to his assistance in the United States.[33] If there must have been questions for Halle to ask about a certain silence with regard to *MM*, this silence cannot be attributed to the American linguists, who quote it whenever they approach morphophonology, and even less so to Harris, who continually refers to it, but rather to Halle's own teacher, i.e., Jakobson. For *MM* is never mentioned nor even given as a reference in Jakobson (1948). And yet this silence can hardly be interpreted as a dissimulation, because "Russian Conjugation" refers five times to Bloomfield, notably to establish a terminological correspondence. Thus, introducing the term "basic," Jakobson notes (p. 156 n. 2): "cf. Bloomfield's stimulating remarks about the 'theoretical basic form' (or 'artificial underlying form'), *Language* 13.9." In a "Note" in the annex, he specifies this (pp. 166-67): "In the stimulating chapter 'Morphology' of Bloomfield's *Language*, the way has been indicated (…) Following Bloomfield's suggestions, we would say that 'the simple and natural description is to take as a starting point the non-truncated stem from which we can easily infer the truncated alternant as well as the use of each"[34] If Jakobson quotes Bloomfield (1933) and not Bloomfield (1939), which his work is nevertheless very close to, this is presumably because, like Harris and Chomsky and unlike Bromberger and Halle, he sees in *MM* an application of the propositions of *Language*, and because within this continuity, it seems normal to him to refer to the theoretical founding text.

The Chomskyan mainstream, after *SPE*, looked for a legitimate antecedent in Sapir's "mentalism," while Bloomfieldianism was relegated to the purgatory of a caricatural behaviorism. This overly historical revisionism, which, without an argument, cuts Bloomfield (1933) off from Bloomfield (1939), is not convincing. Sapir's and Bloomfield's phonologies are, first of all, linguistic propositions, and their divergences as to the psychological reality of the objects they construct are secondary with regard to the convergence of their theories and their specifically linguistic practices. From this point of view, it is fortunate that Postal, a pure supporter at that time of generativist orthodoxy, had the lucidity, before the Ice Age of generative phonology, to establish that, for those points held as essential by Chomskyan phonology, there is a continuous progression from Boas (1909, 1911) to Sapir (1925, 1933) and Bloomfield (1933, 1939): "One of the clearest features of the development of thought from Boas to Bloomfield and Sapir is the fact that, while Boas as well as the latter had a relatively explicit concept of linguistic rule on the level of phonology (euphonic laws), the rules in Sapir's and Bloomfield's works are *ordered.*" In a footnote, he adds: "Bloomfield explicitly notes that his rules are ordered in his 'Menomini Morphophonemics' (*TCLP* 1939) and in his *Language*, chapter 13 (1933). The conclusion that Sapir utilized ordered rules is thus far based on the fact that … it is impossible to obtain the phonetic forms from the phonological representation by means of non-ordered rules."[35]

In 1933, the same year in which both "The Psychological Reality of the Phoneme" and *Language* are published, contrary to the clichés which are taken for history where morphological

alternance is concerned, Bloomfield was only slightly more precise than Sapir, by making the concept of ordered rules explicit. In 1939, Sapir dies (at 54) and Bloomfield gives us in *MM* a remarkable and explicit application of the theoretical concept of a system of ordered rules, thus taking one more step forward. In the following years, Harris reworked the Bloomfieldian conception of morphophonology and ended up by proposing, at the conclusion of *Methods*, grammars which would "synthesize" utterances, i.e., generative grammars. Bloomfield (1939) and Jakobson (1948) were prefigurations of these grammars and Chomsky (1979=1955) was to give us a systematic and formalized version of them—after having taken up and perfected for his own purpose the notational apparatus already elaborated by Harris, thus proving himself to be his very brilliant pupil and successor.

4. A.N. Chomsky, a post-Bloomfieldian generativist phonologist

4.1. The proto-history of generative phonology is now clearly in place. What does it matter if Chomsky read Bloomfield (1939) in 1948, in 1950, or only in 1956, when he rewrote *LSLT*? The essence of what he borrows from Bloomfield is the concept of a morphophonemic grammar as a system of ordered rules, and for this he did not need to read *MM;* it was enough to have read Harris (1951=1947) and Bloomfield (1933). But the real source of Chomsky's work in *MMH* is not Bloomfield, not even indirectly, but obviously Harris and very precisely the final chapter of *Methods,* particularly the paragraphs 20.21 and 20.3, quoted at length above.

"A Note on Recent History" is thus clarified, and above all the biased attack on Harris, who is not even named but absorbed within the troop of Chomsky's supposedly negligent teachers. Does the repeated assertion concerning Chomsky's long ignorance as to the very existence of Bloomfield (1939)—which, after all, is a problem of no consequence— not come to cover up the incontestable dependence of Chomsky (1951), and so of generative phonology itself, on *Methods*, and more generally on Harris, and still more generally on American structuralism on a whole, pretendedly confined within its behaviorist taxonomics?

MMH is an extraordinary text, one such as can only be compared to one other in the history of linguistics, clearly revealing the exceptional capacity of a young man.[36] This is not the place for a study of it. We will only remark that, beginning notably from the analyses and propositions in *Methods* already mentioned, which opened the program for a generative grammar, Chomsky went far beyond Bloomfield (1939), Jakobson (1948) and Harris (1951=1947). In reality, aside from the analysis of segments into distinctive features, which is the particular contribution of Morris Halle to the enterprise, everything which distinguishes *SPE* from structural phonologies is already found in *MMH*, which is incontestably the founding text of generative phonology, and of generative grammar as such, since Chomsky also took his inspiration from it for his theory of syntax.[37]

In note 46, p.51, Chomsky (1975) confides something which deserves more attention than it has thus far received: "Then the historical analogy discussed briefly in paragraph 56.2 was actually the source of my own work in generative grammar." Concerning historical analogy, paragraph 56.2 of *LSLT* only says: "Our general conception of grammar is formally somewhat analogous to a description of historical change. Each statement "α→β" states that α becomes β. The analogy can be extended if we rewrite "α→β" (…) as b→b' in env.: "α—c" where α= a ⌐ b⌐ c and β =a ⌐ b' ⌐ c. Thus a ⌐ c can be understood as the conditioning context for the change of b to b' [….] In its full generality, our notion of grammar has the full power of a descriptive statement of historical change." (Chomsky 1975=1955, p. 203-204). This reference to diachronic analogy in the heart of synchronic derivation leads us back to Chomsky's father, his first teacher of linguistics, before Harris, but also of course to the analogies made by Harris (1951=1947, § 20.3), and beyond that to

the passage from *MM* cited earlier, in which Bloomfield underlined that, although purely synchronic, his grammar included rules that could also find their place in the history of the language.[38] There again, at the same point at which, according to Chomsky, the desire which was to lead him to elaborate generative grammar was born, the link Bloomfield-Harris-Chomsky is particularly strong, and not in an additive, but in a cumulative sense.

McCarthy (1981), just after the publication of *MMH*, noted that "Chomsky (1951) contains all the notational apparatus later adopted by Chomsky and Halle (1968)." He emphasized in particular —which Chomsky (1975) did not do— that *MMH* adopted a resolutely transformational formalism (in the sense of *SPE*, p. 360): "The fundamental characteristic of Chomsky's proposal is a rule moving (or intercalating) long component vowel patterns into triconsonantal roots relying crucially on transformational rules, notations and integral subscripts on segments in the structural description" (p.375) We offer two remarks on *MMH*'s rule 3 that this refers to. On the one hand, it relies crucially on the analysis of Hebrew into "long components" such as Harris (1941) proposed: the consonantal roots on the one side, the vocalic patterns on the other, that is the "melodies" that McCarthy was to isolate. McCarthy presented them as separate autosegmental tiers that are integrated linearly by means of a skeleton. Here the treatment remains unilinear, the vowels placed between the consonants by means of a transformational rule – which is not absurd in itself, the "long components" discontinuous certainly, but composed of segments. Nevertheless, this limitation did not escape Chomsky, who, in *LSLT*, after having recalled the linearity of the treatment of discontinuous elements in *MMH*, pointed out: "In this study, suprasegmental features (pitch, stress, juncture) have not been seriously considered. Ultimately, of course, these phenomena must be incorporated into any full syntactic theory, and *it may be that this extension still requires a more elaborate system of representation* (cf. Hockett, "Two models" for some discussion of this possibility). See Chomsky, Halle, and Lukoff, "On accent and juncture in English," for discussion of a phonemic transcription for English that includes stress and junctures but preserves linearity in a natural way."[39] It can be noted that, as early as 1955, Chomsky was aware that taking into account all the varieties of suprasegmentals would probably lead to non-linear representations, and he immediately thought, first, of Hockett's (1954) propositions. Note also that it was the propositions of Chomsky, Halle, Lukoff (1956), the first draft of stress rules such as we find them in *SPE,* which illustrated the very model of the linear treatment of stress, but Chomsky doubts these will be able to account for everything as far as the suprasegmentals are concerned. In short, the consideration of "long components" normally leads to the supposition of more elaborate representations than linear representations and Chomsky unfortunately sticks to these in *SPE*, from which any reference to the theory of the "long component" was banished.

4.2. We can now measure the importance of the obstacles put in the way of young phonologists looking for access to the "recent history" of their science. And yet, as always, the repressed returns. The ancestors that were deleted from the genealogical trees in the family romance of the discipline will haunt the discourse of their descendants without their being aware of it, until the mismarriages, the spurious offspring, the illegitimate links that were intentionally occulted have once again found their place. A natural child of *MM* and of *Methods,* the "forgotten" father of generative phonology, *MMH* is a resolutely generative, transformational and post-Bloomfieldian text that *SPE* owes almost everything to.

If *LSLT* had been accepted, as Chomsky wished, by MIT Press in 1955-1956, the face of linguistics would have been changed. The rejection of this manuscript had particularly grave consequences for the development of phonology. *LSLT* included, as we know, all of *MMH* (in the revised version of December 1951) in the Appendix to chapter VI. Had it been published in 1956 or 1957, *MMH* would have immediately been placed in its true historical place as the inaugural text of generative and transformational linguistics, several years before *Syntactic Structures*, which is

derived from it, just as all the works already in progress in 1953 with Halle, which were to lead to *SPE*. [40] This link, which Chomsky (1975) makes very clear,[41] was to remain hidden, for too long, alas. Had it been revealed, it would have permitted the Bloomfieldians, who generally received *Syntactic Structures* favorably, to recognize the continuity between their work and generative phonology, instead of soon seeing in the latter only an unacceptable attack against the phonemic perspective. It is significant, from this point of view, that Bloch, "perhaps the most Bloomfieldian of them all,"[42] who had shown particular interest in *MMH* and *LSLT* (he had deposited a copy of in the library of Yale University) was the one who had most encouraged Chomsky. The continuation of a dialogue with structuralist phonologists would have prevented, not only their considerable work from being ignored by the new generation of phonologists, who only had eyes for an *SPE* deprived of its roots, but also —probably — the theory of the "long component" and the necessity for treating the suprasegmental domain with non-linear representations (which, we saw, Chomsky already had the intuition of, after Hockett, in *LSLT*) from being lost from sight for a long time. Perhaps we could even dream of the exceptional theoretical and empirical achievements that might have been achieved in the 1960's by a phonology that had accumulated the contributions of Harris, Hockett and Bloch along with those of Chomsky and Halle, above all if they had all directly combined their efforts.

4.3. To finish with the approximate history that "A Note on Recent History" offers us a sophisticated example of, I shall just mention, without comment, one example of this curtailed history. I recommend that the reader relate it to all of what he has just read; or to what Chomsky himself (1975) explicitly establishes; or even just to Bromberger and Halle's "Note," which, in support of the thesis of their entire article pleading for the independence of phonology from syntax, recalls the anteriority of generative phonology to generative syntax.[43] This example is a response of Morris Halle's during an interview with Jean Paris recorded at MIT on May 11, 1970, broadcast on French radio (ORTF) on May 9, 1971 and published in Paris in 1972: "J.P.: What was the point of departure for your own research in transformational linguistics? M.H.: If I can take a second for an autobiographical point, it was first of all my personal friendship with Noam Chomsky. I had been particularly struck by the fact that his first works, *Syntactic Structures* as well as his dissertation, *The Logical Structure of Linguistic Theory*, gave practically no place to phonology. But I was really more interested in phonology. So I decided to begin a study which would complete what Chomsky was doing at the same time in the domain of syntax. *Syntactic Structures*, as we know, was published in 1957; my work, *The Sound Pattern of Russian*, published in 1959, is an attempt, which I would label today as rather rudimentary, to show how the concepts, the ideas, the basic principles of transformational grammar might be applied profitably to the study of phonological problems."[44]

5. New phonologies

The *SPE* model was abandoned, as we know, because of the question of linearity. In the middle of the 1970's, phonologists began to realize that tones, stress and syllables could not be given a satisfying solution with a linear treatment. A little later the treatment of vocalic harmony and, above all, non-concatenative morphology proved to pose the same sorts of problems for the theory. The standard framework of generative grammar, which, it was thought, had been elaborated first for Russian and then for English, could not meet the challenge of tone languages nor of Semitic languages. This showed, in return, the cruel deficiencies of a phonological model that had eliminated the syllable, invalidating with the same stroke the treatments of stress that had won for it its first success. The explicit warnings that had been given within the school of MIT itself concerning this crucial point, even before the final publication of *SPE*, had been carefully dismissed.[45] If it had been known that the model had its roots in the morphology of modern

Hebrew, it would have been possible, from the beginning, to draw the necessary consequences from the fact that it could only function in *MMH* because of the recourse to "long components" — and that taking this concept seriously required taking into account not only discontinuous phenomena but the entire suprasegmental domain, and the development of a theory of the syllable. As early as 1947 (*Methods* ch. 10), Harris evoked propositions that had been made, parallel to his own: "For a new field of possibilities in componential notation, along the lines of chords in musical notation, see Charles F. Hockett, 'Componential Analysis of Sierra Popoluca', *IJAL* 13, 258-267 (1947)" (p. 126, n.4). In short, in 1941, 1944 and 1947 with Harris, in 1947, with Hockett, then again in 1951 and 1955 with Chomsky himself, everything was in place for the development of this new non-linear or multilinear phonology, of which autosegmental phonology, inaugurated by Goldsmith (1976=1979), extended by McCarthy (1979) and consecrated by Halle and Vergnaud (1980), remains the most accomplished form.

The young phonologists who, abandoning *SPE*, achieved the rupture, were generally aware that American structuralism, as disparaged as it was, must yet have addressed the questions they were confronting. A series of references to pre-*SPE* works appeared, generally the same from one article to another, but, for a long time, no cumulative reconstruction of the history was really engaged, and even recently the authors were rare who went further than a footnote reference to the titles of the works which did not seem to have been much read. There were notable exceptions, of which I will only mention a few: Goldsmith (1976) first, who immediately brings up (in "Prelude") the question of the suprasegmentals, and discusses Harris (1944) and Bloch (1948) at length. Then McCarthy (1981), which, as we have seen, points out straight off the link between autosegmental theory and the theory of the "long component," happily referred here to its most elaborated development in Harris (1951=1947), which was strangely ignored by Goldsmith (1976). He also devotes an appendix to Harris (1941) — in which the long components appear without yet being named — and to *MMH*. [46] And finally Goldsmith (1990), which is the most striking. His referral to Harris, the master entirely re-legitimated by Chomsky (1975), to Bloch (who had shown interest in Chomsky as early as 1951 and had published him in *Language,* in 1967), or to *MMH*, finally published in 1979, did not represent a real transgression of the communitary order. But since his radical rupture in 1965,[47] it would be an understatement to say that Charles Hockett, who in the 50's and 60's was considered as the executor of Bloomfield's will, has not been in the good graces of MIT.[48] And yet, it is he that Goldsmith (1990) installs as the true, but unrecognized, inventor of autosegmental phonology.

Goldsmith did, in fact, publish in 1990, one year after the publication of Bromberger and Halle, a work of synthesis, *Autosegmental and Metrical Phonology*, which he places under the invocation of Hockett. The introduction includes a paragraph, just as unusual, devoted to the "Relation to other theories" — while, since *SPE*, it was normal not to pose this question, at least as far as American structuralism was concerned.[49] There is particular insistence in this paragraph on Hockett (1955), with a long quotation and this commentary: "With hindsight, these analyses jump off the page and claim, with justice, historical precedence in the multilinear approach currently called autosegmental phonology […] Hockett's (1947) analysis of Sierra Popoluca, which in turn engendered considerable discussion in the literature (e.g. Longacre (1952), Hamp (1954), Longacre (1959)), is even strikingly autosegmental, and grapples with the same problem that I deal with in the present book, the interaction of internal syllable structure and elements on separate autosegmental tiers (though, of course, Hockett used neither the term 'autosegmental' nor 'tier': he referred to Harrisian "components" (Harris 1944, 1951)). The epigraph in chapter 1 below gives the reader a sense of the identity of Hockett's concerns and my own" (p. 4). Chapter 1 presents, indeed, as an epigraph, a long quotation from Hockett (1947), astonishingly "modern", explicitly putting in doubt the "linearity assumption" (p. 8). Post-Bloomfieldism is at last reunited with post-Chomskyism, and it turns out that pre-*SPE* phonology was already post-*SPE*: the eclipse (during

28 years!) of *MMH* and its recourse to the long component had led the SPE-type of generative phonology, which should have been — and was considered as — a step forward, to close itself off in an impasse. What is really strange is that we had to wait until 1990 for Goldsmith to rediscover Hockett (1947), which Harris (1951=1947) had so clearly pointed to. Could it be that while he was writing his dissertation at MIT (in which Hockett's name is not even mentioned), from 1974 to 1976, his teachers had forgotten to point out to him a text which prefigured to such an extent his own work?[50]

Clements "Nonlinear Phonology and its Antecedents" (this volume) underlined subsequently the importance of the introduction by Harris (1941) of the term "nonlinear" and of its adoption by Hockett (1947). He recognized more generally the totality of the work accomplished in this field both by American structuralism, and the Firthian school, as Goldsmith also did (1993). It is probable that, in the end, this resolve to "accumulate," even if it was delayed, has touched other phonologists of the new generation. In any case, the imposing *Handbook of Phonological Theory* (1995), edited by Goldsmith, a reference work for today's multiple frameworks and theoretical domains, is reassuring from this point of view. Most authors emphasize the link between their research and pre-*SPE* research. It is no longer the moment for justifying the break with *SPE* but rather for reconstructing the ties with what *SPE* had symbolically barred access to. And yet, if the neglect of the attainments of American structuralism by the founders of generative phonology is supported today by more and more documentary evidence, McCarthy (1981) remains the only article, to my knowledge, which has approached the equally, if not more, important question of the debt of generative phonology to Bloomfield and the post-Bloomfieldians, notably Harris and Hockett. The "new phonologies" cannot be constituted theoretically as such without having theoretically constructed "the anteriority" of the precedent theories — that is, without providing the history of phonology, to make it once again the cumulative science it was up to the 1960's. Digging up a text or an author from here and there will not be enough, nor will rapid allusions to previous treatments of the questions concerned. It is not a matter of writing the dead history of concepts, but rather the living history of their transmission or their non-transmission. In this direction, Bromberger and Halle (1989) pose the right question when they wonder what Chomsky's teachers pointed out to him, or hid from him, of previous works. It is not superfluous today to return the question to them, or, more usefully, to accomplish this internal historical work.

As Jakobson subtly remarked, "isn't it Musset who asserted that only schoolmasters from the most remote provinces still believe that there might be really new ideas somewhere in the world?"[51] The new phonologies of today and those to come soon tomorrow, which will consider the most advanced point of today's research as out-dated and out-moded, have no chance to reap fruitful innovations unless they thoroughly nourish themselves with the work effected up till now, and *understand* the reason for the obstacles placed in the way. I suggest that young phonologists pay particular attention to the way the question of the cognitive reality of linguistic models has been treated and the increasing confusion at MIT, during the 70's, between "the model of reality and the reality of the model." On this point, a return to the position of *SPE* would be, in my opinion, an unquestionable progress.[52]

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Notes

[1]Bloomfield (1946), p. 23.

[2] "American linguists are finding it increasingly difficult to read European writings; in our field younger linguists are neglecting the older writers, so that we are in some degree losing contact both with the tradition of linguistic science and with its present-day representatives in the rest of the world" (Haugen (1951), Presidential Address at the meeting of the Linguistic Society in Chicago, December 29, 1950, p. 211).

[3]Cf. for example, for the history of an exemplary internal cumulative debate, Encrevé (1988b).

[4]When I proposed, in 1971, to translate into French the theoretical parts of the *SPE* (the first and third parts), Chomsky and Halle agreed without hesitation, pointing out that they wished the book thus made up to be entitled *Principes de phonologie générative* in explicit reference to Trubetzkoy's great book, which can be interpreted both as an affirmation of filiation and as a attempt to take its place. The ambition was legitimate.

[5]Cf. Chomsky (1964), "4.3: Taxonomic Phonemics," p. 75-95. In 1966, as a preface to the reedition of *Readings in Linguistics*, Martin Joos remarked pertinently: "Hockett called descriptive linguistics a classification science; an excellent synonym for this is *taxonomic* linguistics, which certain proponents of the new trend use as a derogatory term. Now when anyone holds that the existence of two kinds of linguistics, taxonomic and, for example, generative-transformational, strictly implies the inferiority or even nullity of one of them, that opinion can only be pernicious no matter which of the two is denigrated. Both extreme views will fade out in due course, especially as each side reads more of what has been published by the other." Alas, the young generativists hardly read even Joos' edited collection, and above all not its preface.

[6] Cf. notably Chomsky (1957b), review of Hockett (1955); Chomsky (1964); Chomsky and Halle (1965), response to F. Householder; Chomsky (1966), responses to Lamb and Marckwardt; Postal (1967), review of Martinet (1964); Schane (1967), review of Martinet (1965a); Postal (1968)

[7] Who even knows today, among the linguists under forty, that he cosigned with Chomsky and Halle the first embryonic precursor of *SPE* in 1956? Cf. Chomsky, Halle and Lukoff (1956).

[8]Chomsky (1964), p. 70.

[9]Bromberger and Halle (1989), p. 66-67.

[10]Chomsky (1964) specifies that "in the light of more recent work, the grammar presented there would have to be modified in many respects, but the conclusion concerning ordering, so it appears, would, if anything, be strengthened" (p. 71, note 9).

[11]Bromberger and Halle (1989), p. 68 (P.E.'s italics).

[12] Science must be impersonal. Everyone is aware of everything Morris Halle has done for phonology. It is obvious that a critical study of the "Note on recent history" in Bromberger and Halle (1989) in no way questions the good faith of the authors, but only a carelessness, current in linguistics, with regard to historical methodology. Linguists have nothing to lose from carefully rereading the texts.

[13] Voegelin (1940), p. 257. The studies mentioned are, respectively, those of Sapir and Swadesh (1939), Swadesh and Voegelin (1939) and Hockett (1939). Voegelin's elegance in recognizing the merits of Bloomfield's solution over notably the one he proposed himself, at the same date, for Tûbatulabal can be compared to Bromberger and Halle's assertions (1989) which, after having recalled that Bloomfield (1939) is one of the first to use extrinsically ordered rules, adds in note 14: "Very similar approach is Swadesh and Voegelin's (1939) paper on Tübatulabal. It is difficult at this distance in time to establish whether Bloomfield influenced Swadesh and Voegelin, whether the latter influenced Bloomfield, or whether the ideas were developped independently." And yet, as early as *Language* (1933), Bloomfield, uncontestedly the first to do it, *explicitly* proposed the use of *ordered rules* —which in fact Swadesh and Voegelin (1939) do not do any more than Sapir and

Swadesh (1939), or Hockett (1939). Even at this distance in time, we can establish, as Postal (1964) did, that Bloomfield (1933) has Boas as a source, as does Sapir (1933), but obviously not Swadesh and Voegelin (1939). I will return to these points below.

[14] Cf. for example, Bloch (1947), Hockett (1947, Nida (1948).

[15] Cf. Harris (1951b), n. 7, p. 291, n. 8, p. f292 and 293.

[16] Cf. Chomsky (1975=1955), p. 78; note 2 (Chapter II) and p. 115 note 5 (chapter IV) and (original) bibliography, p. 571.

[17] Cf. McCarthy (1981), "Appendix ." The list of "References" indicated by error *JAOS* 62.

[18] Bromberger and Halle (1989) is a modified version of a communication presented April 14, 1988 in Jerusalem at the symposium "The Chomskyan turn."

[19] Barsky (1997), p. 55.

[20] Hockett (1947b) cites an example from *Methods* without indicating its title: "In his unpublished material, Harris(…)" (n.25). Joos (1957), republishing Hockett (1947) pointed out: "See now his *Methods in Structural Linguistics*, p. 202" (p. 235). Unfortunately, I was not able to have access to the 1947 manuscript.

[21] Cf. *SPE*, p. 18, n. 4. Chomsky (1964) makes this point: "Bloomfield does not discuss the extent of depth of ordering in this grammar (…). It apparently does not exceed five (cf. Bever, 1963). (…) In Chomsky (1951), a depth of ordering that reaches the range of twenty to thirty is demonstrated and this is surely an underestimate." (p. 71). Five years later, Bever having redone his count, the depth of *MM* had largely increased and that of *MMH* had slightly decreased.

[22] Twadell, 1935 (in Joos (1957), p.61- 68). Trubetzkoy (1949) commented: "Thus, for example, B. Collinder and P. Meriggi eagerly took Twaddell's claim that the phoneme is no physical and mental reality but "an abstractional fictitious unit" as a flat rejection of the phoneme concept. In reality, of course, Twaddell only meant the same as what Ferdinand de Saussure considered the essence of every linguistic value ("entités oppositives, relatives et négatives," *Cours de linguistique générale,* p. 164 (…). The value of a currency unit, for example, the dollar, is also neither a physical nor a psychic reality, but an abstract and "fictitious" value. But without this "fiction" a government cannot exist." *Principles of Phonology* p. 43)

[23] Notice moreover that Halle (1962) takes the same position as Chomsky (1964). After having quoted *MM* and deplored Hockett's (1949) silence on this text, he adds: "cf. also Bloomfield's comments on 'descriptive order' in his *Language* (1933) p. 213 and 222" suggesting from Bloomfield (1933) to Bloomfield (1939) no rupture of continuity.

[24] Harris (1951=1947, p. 236; n. 39.

[25] Harris (1951=1947), p. 365 (P.E.'s italics)

[26] Note 16, p. 47. In this same note Chomsky writes: "Bloomfield's 'Menomini Morphophonemics' might be regarded as a segment of a generative grammar in approximatively this sense. (My *MMH*) was written, I regret to say, in ignorance of Bloomfield's study.) Some of Sapir's ideas on phonology tended in a similar direction," with reference to Harris (1951b).

[27] I quoted a part of this note above to establish that Chomsky knew *MM* as early as 1955-56. But the whole text contains, as we can see, two other important points of information, which find their place in the discussion here.

[28] We may recall that in January 1947, Harris thanked Jakobson for the criticism he afforded him during the preparation of his book.

[29] Cf. Halle, 1959, op. 38 n. 27, p. 49 n. 39, p. 67 n. 22. In n. 1 on p. 55, Halle quotes Jakobson (1948) in a list of treatments proposed for the morphological rules of Russian, along with N. Trubetzkoy, H. Rubenstein, H. Klagstad and E. Stankievicz, without attributing to him the slightest special role in the genealogy of generative phonology unlike Chomsky at the same time, since *LSLT* and the first version of *The Sound Pattern of Russian* (revised version of Halle's Ph.D) are

contemporary articles. It can be noted that Roman Jakobson did not retain this article for his *Selected Writings*.

[30] Personal communication, Paris, May 31, 1987.

[31] For example, en 2.42, a quasi-explicit disjunctive order, with use of the term Morris Halle later has recourse to for this purpose: "otherwise" (p.16).

[32] Bloomfield (1939), p. 105-106 (Bloomfield's italics)

[33] Cf. Halle (1988). Jakobson arrived in the United States in 1942. Bloomfield, who was to die in 1949, had an attack in 1946 from which he never recovered.

[34] Jakobson continues, still following Bloomfield (1933): "If on the contrary we took the truncated stem as our basic form, we would be unable to predict the corresponding full-stem, and we would have to show by an elaborate list what phonemes are added" (p. 166-167). The phonologists of French will have no difficulty recognizing here Schane's (1968) argumentation for truncation in French. But they will not find Jakobson (1948) in the bibliography of *French Phonology and Morphology*. Cf. also Encrevé (1988), p. 131 n. 10)

[35] Postal (1964), p. 274 n. 28. Harris (1951b) asserted as well that, for the treatment of linguistic facts such as "processes" which characterize Sapir's work, Bloomfield represents "a later stage."

[36] The other is, of course, the *Mémoire sur le système primitif des voyelles dans les langues indo-européennes*, defended by Ferdinand de Saussure, in 1878, at the age of 21.

[37] "I tried to construct a system of rules for generating the phonetic form of sentences, that is what is now called a generative grammar. I thought it might be possible to devise a system of recursive rules to describe the form and structure of sentences, recasting the devices in Harris's *Methods* for this purpose, and thus perhaps to achieve the kind of explanatory force that I recalled from historical grammar." Chomsky (1975), Introduction, p. 25. Cf. also in note 45 (p. 50-51). "Harris did not elaborate on the suggestion that a grammar can be regarded as a device for "synthetizing utterances," an idea that does not, strictly speaking, seem compatible with the general approach of *Methods*. This tension, or perhaps incompatibility, troubled me for quite a few years"; and also Chomsky (1961), note 5, p. 220.

[38] "Thus there was a certain loose resemblance between the synchronic grammar selected by application of a formal evaluation measure, and a historical grammar such as Harris's *Development of the Canaanite Dialects*. In 1951, Bar-Hillel suggested to me that I put aside my hesitations on the matter and postulate something very much like the reconstructed historical forms on the abstract morphophonemic level. It was with this in mind that I carried out the revision to the final 1951 version,." Chomsky (1975), Introduction, p. 29.

[39] Chomsky (1975), n. 11 p. 111 (=1955, n. 11 p. 22), (P.E.'s italics)

[40] Halle (personal communication May 31, 1987) points out that it was in Paris, in the summer of 1953, that he began to work with Chomsky, within the logic of the notational system of *MMH*, on a generative grammar of English stress, in the form of an algorithm formalized in ordered rewriting rules, transformational rules and cycles, in discussions with the French mathematician Maurice-Paul Schutzenberger, who he had at that time introduced to Chomsky. Chomsky, Halle and Lukoff (1956) was the first result of this work. Thus Chomsky brought to Halle the ordered rules and their formalization. Halle brought to Chomsky the theory of distinctive features, leading him to the idea of a very simple evaluation measure which plays an important role in the *SPE* model.

[41] "By the spring of 1959, I found myself deeply involved in work on generative phonology, applying to English an approach that had been presented in my *MMH* in 1951, which I had planned to include as an appendix to chapter VI of *LSLT* ." (Chomsky, 1975, n. 4). In a footnote (n. 8, p. 46), Chomsky specifies that a part of this work had been presented to the 1959 Fourth Texas Conference and that he had been working on it with Halle for several years. As he indicates, *LSLT* (chap. VI, n. 11) already refers to "Halle and Chomsky, 'Evaluation procedures in linguistics', in preparation," an unpublished work of 1956. "This work was finally published as *SPE*" (id). Halle's

Ph.D dissertation, *The Russian Consonants: A Phonemic and Acoustical Study* (Harvard, 1955), had been largely elaborated within the logic of his research with Jakobson. He added chapter II on the rules of Russian, rules that were ordered but not formalized, while preparing the manuscript for publication.

[42] Cf. Stark (1972), p. 387, quoted by R. Harris (1993), p. 265, n. 25.

[43] For example: "Chomsky (1988a) notes that his work on the phonology of Modern Hebrew naturally led him to explore whether some of the devices he had used there might also have a use in syntax. Such a project was especially attractive at that time as phonology was then widely viewed …as a model for other linguistic domains to follow. It took two decades of intensive research for Chomsky to conclude that the syntax of a language does in all likelihood not include a system of extrinsically ordered rules (ordered transformations)" (p. 68-69)

[44] M. Halle (1972) "Entretien avec Jean Paris" p. 52 (the only published text of this interview is the text established in French by Mitsou Ronat and Jean Paris. I do not know in what language the interview was actually given). I quite understand today that these hasty declarations were undoubtedly for their author only a shortcut without consequences, but I must not have been the only one who took them, at that time, as true to the letter, and taught this version of history, where generative syntax, rather than structural phonology, gave birth to generative phonology, the usual version of the facts until Chomsky (1975) (cf. for example, Hall, 1973).

[45] Cf. in Encrevé (1988), p. 93-96, both the magisterial manner with which *SPE* uses Bailey and Milner (1967) to install the "syllabic" feature, even though these authors were arguing that this feature, inevitably *prosodic*, could not be introduced into the segmental matrices, and Milner's curious consent to this inversion of his conclusions.

[46] Contrary to Goldsmith (1976), McCarthy considers himself in the direct line of Harris:"What is offered here is a new theory of non-concatenative morphology, one which owes a great deal to Harris's (1941, 1951) notion of long component" (p. 373); "In view of apparent similarities between many of the notions of autosegmental phonology and Harris's long components, we could reasonably expect the theory developed here to have been prefigured somewhat by earlier work. In fact there exists a fairly detailed account of Biblical and Modern Hebrew in terms of the theory of long components. I summarize these analyses in this appendix" (p. 414, Appendix). There follows a critical presentation of Harris (1941) and of Chomsky (1951).

[47] In 1957, Chomsky published a very detailed review of Hockett (1955), which shows the esteem he had for Bloomfield's "heir ." Hockett himself had shown a certain interest in Harris's pupil for a long time. Hockett (1966) gives us a striking picture of the conclusion of the rupture.. In note 3, Hockett emphasizes that on the question of linguistics and science, there is, in his opinion, continuity from Bloomfield to himself and to J. Katz ("Mentalism in Linguistics," *Language*, 40, 1964). This is followed by: "NOTE ADDED IN PROOFS: "Further study of the work of Chomsky and his associates, especially of the first chapter of Noam Chomsky *Aspects of the Theory of Syntax* (1965) … shows that the last sentence of the preceding paragraph of this footnote is an error. Conciliatory remarks elsewhere in this essay, and in my article "Sound Change" (*Language* 41, 1965), reflect the same misunderstanding on my part. Chomsky's outlook […] is so radically different from Bloomfield's and from my own that there is, at present, no available frame of reference external to both within which they can be compared. Chomsky's own attempts to evaluate Bloomfieldian and post-Bloomfieldian (but pre- and non-Chomskyan) linguistics […] are about as meaningful as would be an attempt to measure, say an electrical voltage with a ruler […] Let the record show that I reject that [Chomsky's] frame of reference in almost every detail" (p. 156, n. 2).

[48] Goldsmith (1979), partially quotes note 11 of *LSLT* (cf. above in 4.1), but he omits the reference to Hockett (1954) made by Chomsky (1955).

[49] Dell (1973) took the trouble to compare generative phonology to that of Prague (cf. "Conclusion," p. 261-268).

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[50] "I I don't recall hearing anything about structuralist work on this subject while I was doing the research or writing my dissertation. It was only after the dissertation was completed – literally, all written -- that Halle told me (warned me,more accurately) that people would attack it for being no different from Firth, or from contemporaneous American linguists; he urged me strongly to add material showing how my perspective was different. I did as he suggested, and it was only because the dissertaton was already written that I entitled that early chapter "Prelude" rather than Chapter 1 (which it really should have been)." John Goldsmith (personal communication, 1999).

[51][51] Jakobson (1972), p.44.

[52] Cf. *SPE*, p. 3: "We use the word 'grammar' with a systematic ambiguity. On the one hand, this term refers to the explicit theory constructed by the linguist and proposed as a description of the speaker's competence. On the other hand, we use the term to refer to this competence itself." Cf. also, on the evolution of Chomskyan orthodoxy, J-C Milner's pertinent epistemological article (1982), "Linguistique, biologie, psychologie," p. 302-317.