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Remarks on the origins of morphophonemics in American structuralist linguistics☆

E.F.K. Koerner

Institut für Sprachwissenschaft, Universität zu Köln, Albertus Magnus Platz, D-50923 Köln, Germany

Dedicated to the memory of Charles F. Hockett

Abstract

As recently as 1997, Noam Chomsky has reiterated what he had affirmed on several occasions, especially during the 1970s, namely, that when working out his ideas on rule ordering for his Master's thesis on *Morphophonemics of Modern Hebrew* in 1951, he did not have access to Bloomfield's "Menomini Morphophonemics" 1939 paper, suggesting that the generative model of linguistic analysis he developed at the time was more or less original with him. The present paper argues that even if he did not have direct access to a copy of *Travaux du Cercle Linguistique de Prague* vol. 8 prior to the completion of his M.A. thesis, he had very likely absorbed the essentials of Bloomfield's ideas about rule ordering from various sources, including reaading the proofs of his supervisor Zellig S. Harris' main theoretical work, *Methods in Structural Linguistics*, in early 1947, in which the salient points of Bloomfield's 1939 argument are discussed in a section entitled "Morphophonemics". Indeed, although Harris' book was

☆ It was the reading of Fought (1999) during the summer of 1999, when preparing its final copy for-*Historiographia Linguistica*, which first made me think that a paper on this topic might be worth writing, but it was not until I read Encrevé (1997), to whose existence Jean-Claude Chevalier (Paris) had drawn my attention in September 1999, that I eventually realize that there were enough interesting angles to the subject to actually pursue this idea. E-mail exchanges with Bruce E. Nevin, John Fought, Steve Murray, and others during December 1999 and March 2000 strengthened my resolve. To all of them many thanks for the stimulation and comments I received. Portions of this paper were presented at the annual meeting of the Henry Sweet Society for the History of Linguistic Ideas, Edinburgh, 20–23 September 2000, and at the joint annual meeting of the Linguistic Society of America (LSA) and the North American Association for the History of the Language Sciences (NAAHoLS), held in Washington, DC, 4–7 January 2001. Comments on earlier versions of this paper were received from John Fought, John A. Goldsmith, Winfred P. Lehmann, Stephen O. Murray, Bruce E. Nevin, W. Keith Percival, E. Wyn Roberts, Matsuji Tajima, and, more recently, John E. Joseph. To them, and to Phillip Herring for helping me smooth out some rough edges, my heartfelt thanks. Regular disclaimers apply. Much of the research for this paper was done while I was a visiting scholar in the Department of Germanic Studies at the University of Texas at Austin during the winter term 2001. Many thanks are due to the Chair of the Department, Dr Peter Jelavich, and his staff, particularly to Mr. Patrick Couture, for technical support.

E-mail address: efk.koerner@uni-koeln.de (E.F.K. Koerner).

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not published until 1951, it had been circulating in manuscript form since 1946, and Harris' preface, signed January 1947, thanks Chomsky for helping with the proofs. Furthermore, it should be pointed out that Harris' *Methods* contains the essentials of the generative approach to language which is by now almost exclusively associated with Chomsky's name. A further case can be made that Harris' 1941 and 1948 articles on Hebrew provided more than simply the data of which Chomsky's 1951 M.A. thesis constitutes largely a 'restatement' in a much more abstract, technical form of his own making. The present paper argues that there has been much more continuity and cumulative advance in American linguistics than we have been made to believe both by the active participants in the 'revolution' and its historians.

Keywords: Morphophemics; Structuralism; Chomsky; Morphology

1. Introduction: the treatment of the subject in historical accounts

When engaging in historiographical work, serious scholars ascertain all possible sources for the documentation of a given subject, notably written materials, published or unpublished. They will of course pay particular attention to the published work and any available written statement by the authors whose output is under scrutiny about matters of educational background, formative influences, and the like. If a given author is still alive, the historiographer might try to engage him and ask specific questions that are deemed pertinent to the investigation. In the case of Noam Chomsky the materials that the researcher can access are enormous, if not overwhelming (cf. Koerner and Tajima, 1986, which also lists a great number of published interviews and other biographical data). Additional information may also be gleaned from historical accounts that contain excerpts from interviews or epistolary exchanges with Chomsky (e.g., Newmeyer, 1980, 1996; Harris, 1993; Murray, 1994; Barsky, 1997). In short there is, unlike in so many other instances, no dearth of information available where Chomsky is concerned, the man and his work.

Still, despite this wealth of information, there may be puzzles confronting the historical investigator regarding specific points in Chomsky's intellectual biography. One puzzling subject concerns the exact role that Zellig Sabbettai Harris (1909–1992), Chomsky's acknowledged linguistics teacher at the University of Pennsylvania during the years 1946/1947–1951, played in the development of Chomsky's ideas concerning generative grammar, in part because Harris himself appears to have been rather reticent about what he regarded as personal or private matters.[1] As a result, the historiographer is largely left with statements about their student-mentor relationship made by Chomsky many years after the fact. Fortunately, there are a number of such acknowledgments available, notably Chomsky's introduction to the 1975 publication of an—indeed in parts heavily—edited (and reduced) version of his *The Logical Structure of*

[1] But see Harris (1990), in which he recounts the development of transformational grammar, which his close contemporaries regularly used to associate with his work of the 1940s.

Linguistic Theory manuscript of 1955–1956. There we can read the following important passage which bears quotation in the light of the subject of the present paper:

> 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. I found it very intriguing and, after some stimulating discussions with Harris, decided to major in linguistics as an undergraduate at the University of Pennsylvania. (Chomsky, 1975, p. 25)

Another, hardly less important part of the historiographer's narrative would have to be to map out the general atmosphere of the time and to provide an idea as to what would have been available to an undergraduate in linguistics in terms of publications, the prevailing mode of thought, and the discussion conducted in the regular scholarly outlets. This would lead us, among other things, to realize that, apart from several philological periodicals which carried studies in historical-comparative Indo-European linguistics, there were only three regular linguistics journals around at the time: *Language*, the official organ of the Linguistic Society of America, published since 1925; *International Journal of American Linguistics* (acronym:*IJAL*), first launched by Franz Boas (1858–1942) in 1917, petering out in 1939 and re-launched by Charles Frederick (''Carl'') Voegelin (1906–1986) in 1944, and *Word*, the journal created in 1945 by European émigré linguists residing in the New York City area, under the intellectual leadership of Roman Jakobson (1896–1982).[2] Unlike the late 1960s and 1970s, which saw the establishment of a large number of linguistics journals on both sides of the Atlantic, the American student of the 1940s and 1950s could manage to read everything in the field that had been published since the 1930s, including all the books, at least everything that had appeared in the United States.

In the historical accounts concerning American linguistics, notably Frederick Newmeyer's influential book of 1980, we find little on the subject of 'morphophonemics'. Neither the first nor the second, revised edition of 1986 has an index entry on it. Bloomfield's 1939 paper is listed in the references of both editions (Newmeyer, 1980, p. 253; 1986, p. 234) and is briefly mentioned in the section ''Early Generative Phonology'' (pp. 39–42), which is essentially a celebration of Morris Halle's 1962 paper ''Phonology in Generative Grammar''. Bloomfield's (1939) paper is credited with having been the first to observe that ''the synchronic order of rules characteristically

[2] Perhaps we should also add the existence of a working-paper kind of journal edited by George Leonard Trager (1906–1992), *Studies in Linguistics*, which appeared during 1942–1975, and indeed published a variety of interesting papers, including several by Charles F. Hockett, which Chomsky can be expected to have read at the time, given that, unlike today, there was at the time such a dearth of linguistics periodicals. Interestingly, one of the altogether five (including an unpublished paper by himself!) items listed by Chomsky in his M.A. thesis (p. 74) is Hockett's 5-page note on ''Which Approach in Linguistics is 'Scientific'?'' (Hockett, 1950a), to which an approving reference is made in Chomsky (1951, p. 3, note 4; p. 67), when the discussion concerns ''explicit considerations of simplicity imposed on the grammatical statement''. On the distribution of articles by Harris' generation in *Word*, *Studies in Linguistics*, and *Language*, see the table in Murray (1994, p. 218).

mirrors their relative chronology'' (p. 41 and 1986, p. 35). Earlier (p. 36 [not retained in the 1986 edition]), ''Bloomfield's treatment of Menomini morphophonemics (1939) and Jakobson's Russian conjugation (1948)'' are recognized as ''clearly'' exhibiting the spirit ''of a generative phonology, although their rules are not treated formally.''

In 1976, in his survey of *The Development of Morphophonemic Theory*,[3] James Kilbury—a former student of the late Charles Hockett, by the way—mentions Bloomfield's (1939) paper in Chapter 4, ''American Linguistics through Bloomfield'' (pp. 39–53) only very briefly (pp. 51–52), arguing (p. 51):

> It seems reasonable to conclude that Bloomfield's choice of the title ''Menomini Morphophonemics'' [...] was explicitly intended to honor Trubetzkoy and recognize his theoretical contributions. But neither this nor Bloomfield's earlier works give a picture of the position of morphophonemics within his own framework.

This is a fair enough assessment, but the rest of Kilbury's account does not contain a discussion of this paper beyond a mention of it in conjunction with Harris' (1942) ''Morpheme Alternants'' paper (p. 87). In the concluding chapter, ''American Linguistics since Generative Grammar'' (pp. 103–119), which begins with a presentation of the main points of Chomsky's (1951) M.A. thesis, Bloomfield's (1939) paper is simply mentioned as an example in support of the argument that ''earlier descriptions in terms of ordered morphophonemic rules [...] had been content merely to specify *sufficient* conditions of ordering'' (p.106; emphasis in the original). There is no mention of the possible influence that Bloomfield's paper might have had on Chomsky.

In Stephen Anderson's *Phonology in the Twentieth Century* of 1985 we find a section on ''Morphophonemics and the description of alternations'' (Anderson, 1985, pp. 270–276) in the chapter devoted to Bloomfield (pp. 250–276), but only a small portion of it is devoted to his 1939 paper. Anderson repeats Chomsky's (1962) statement that Bloomfield's ''classic 1939 paper on Menomini'' constitutes ''[t]he first systematic treatment of morphophonemics'' (p. 271), only to warn the reader against anachronism:

> While Bloomfield was certainly one of the most noteworthy early practitioners of the morphophonemic method of description, [...], we should not therefore make the anachronistic assumption that he understood such descriptions in the same way linguists do today.

Later in the book, Anderson describes Chomsky's ''first substantial results'' in the field of generative phonology in his 1951 M.A. thesis as having been ''achieved without substantial reference to structuralist assumptions'' (pp. 315–316). In the section ''The antecedents of generative phonological theory'' (pp. 322–327), Bloomfield's (1939) article is only mentioned as a reference to Halle and Chomsky's writings of the

[3] Kilbury (1976), curiously enough, is not listed in Newmeyer (1980, (2nd ed) 1986, 1996).

1950s. In essence, Chomsky is presented as an independent thinker who was in no way significantly influenced by his structuralist predecessors.[4]

Last but not least, the most specific treatment of the central subject of this paper, namely, whether or not (the main tenets of) Bloomfield's Menomini article had an impact on Chomsky's morphophonemics work during 1949–1951 is found in Newmeyer (1996, pp. 12–14). I shall assess Newmeyer's observations, claims, and deductions later in this paper (see also note 6).

2. A return to the sources of morphophonemics

To the scholar familiar with the literature of the period, specifically, the work of Trubetzkoy and Jakobson, on the European side, and the work of Bloomfield, Sapir and other linguists of the 1930s and 1940s, on the American side, some of the connections seem rather obvious, once one has put to rest the erroneous view that there was no contact between certain strands of European linguistics and American structuralism during the pre-war years. However, the memory of earlier linguistic work, especially that by Chomsky's immediate predecessors, tended to get obliterated through what Encrevé (1997) has called "the slash-and-burn strategy"[5] of 1960s MIT linguists, led by Chomsky (b.1928) and his close associate from the early 1950s onwards, Morris Halle (b.1923). Instead, we can see Chomsky building an alternative history of his own supposed linguistic inspiration during the same period (cf. Chomsky, 1964, 1966). I hope to show, in the course of the investigation below, that the idea of rule ordering played a central role in the 'revolution' in American linguistics. As a result, it is with regard to antecedents of this idea that some of the revolutionary claims of Chomsky and his associates may be said to stand or fall. If this assessment is correct, the emphasis on the part of the protagonists will be on discontinuity, possibly even *rupture* (Michel Foucault following Bachelard), with the immediate past. In other words, the supposedly first work which contained *in nuce* the concept of generative grammar—Chomsky's 1951 Master's thesis on *Morphophonemics of Modern Hebrew* done under Harris at the University of Pennsylvania—must be seen as entirely original, and not in any way indebted to his structuralist predecessors. To the 'revisionists', it will be incumbent upon them to

[4] Curiously enough, Matthews's *Grammatical Theory in the United States from Bloomfield to Chomsky*, merely mentions Chomsky's use of 'morphonological rules' and his argument in favour of the establishment of a 'morphonological level' in Chomsky (1975[1955/56]), but not Chomsky's M.A. thesis of 1951 (it is not even included in his list of Chomsky's writings [Matthews, 1993, pp. 255–256]), though Bloomfield's 1939 paper is at least mentioned (p. 93) for his reference to 'morpholexical variation' (Bloomfield, 1970, p. 352). This is all the more surprising as Matthews makes a long-running argument to demonstrate that "Chomsky and his colleague Halle [...] did not abandon Bloomfieldian ideas" and "[w]ith some qualifications, [...] retained the morpheme unit, much as the Post-Bloomfieldians had conceived it" (pp. 86–87).

[5] Encrevé (1997, p. 101) speaks of "une politique délibérée de la ≪terre brûlée≫", and his illustration of this kind of activity is summarized as follows: "Un bon stucturaliste est un structuraliste mort" (ibid).

demonstrate that Chomsky's ideas indeed had antecedents and that one can safely assume that he was familiar with them, when writing his M.A. thesis.[6]

2.1. The idea of ordered rules in Bloomfield

The linguists teaching at the Massachusetts Institute of Technology during the 'revolutionary' 1960s are reported as having actively discouraged students from reading the work of the so-called Bloomfieldians, except for what Postal later (1988, p. 134) called 'the right of salvage' (cf., e.g., Langendoen, 1968[1964]; Bever, 1967). The line that they were given was that their work was 'prescientific', barely attaining 'observational adequacy', and hence of little value. Needless to say, students are eager to follow their teachers' advice, especially if it makes their lives easier. As a result, few of them read Bloomfield's book *Language* attentively at the time, and even fewer with the intent of recognizing its significance for the development of American linguistics for more than one generation or for the linguistic discussions of the 1950s and 1960s.

Although it is hard to believe in view of the circumstantial evidence (see below) that Chomsky had not seen, or, rather, had no knowledge of the ideas put forward in Bloomfield's ten-page paper of 1939, "Menomini Morphophonemics", when drafting his Master's thesis on *Morphophonemics of Modern Hebrew* in 1951, let us assume for the moment that he had not. For someone who said that he 'decided to enroll in linguistics' in 1947, one would be safe to assume that he soon acquired—or borrowed from the main library at the University of Pennsylvania—a copy of Bloomfield's *Language*, the main reference in any serious linguistic discussion in America until well into the early 1960s. There, in the chapter on Morphology (Bloomfield, 1933, pp. 207–226), which, as Encrevé (1997, p. 112) notes, Harris was preoccupied with rewriting during the 1940s, we find Bloomfield discussing the formation of English plural forms, in particular "some instances of the constituent

[6] In his most recent historical accounts of the development of generative linguistics, Frederick Newmeyer has argued, among other things, that prior to writing his M.A. thesis, Chomsky had already worked out his ideas in a 1949 undergradute essay. Having compiled, with the endorsement of Noam Chomsky and the assistance of his secretary, a most detailed bibliography of his writings down to 1986 (Koerner and Tajima, 1986), I was not aware of the existence of this work which, according to Newmeyer [apparently following Chomsky's own (1979, p. 111) account], bore the same title. When I asked Newmeyer in December 1988, when the paper was originally presented at the first annual meeting of NAA-HoLS, held at Tulane University in New Orleans, whether he had a copy of the essay, he admitted that he had no such copy and in fact had never seen it but that Chomsky had told him all about it. It may well be the case that Chomsky's MA thesis, began as a term paper, but I know of no place in which Chomsky lists his 1949 essay, not even in his MA thesis, where it might have made sense, especially given the fact that he lists among the five (!) items of his bibliography (p. 74) an unpublished (and undated) paper of his which appears to be an elaboration on Nelson Goodman's (1943) article "On the Simplicity of Ideas". (Let us not forget that Chomsky at the time was about as much a student of Goodmann's as of Harris', and that it was the former who got him the four-year Harvard Junior Fellowship). Given that the paper's existence is only a rumor, it is surely presumptuous for Newmeyer (1996, p. 14) to use it in order to shore up Chomsky's claim by suggesting that in 1949 Chomsky might not yet have seen Jakobson (1948). Be that as it may, we will see in what follows that it hardly matters for the present argument whether or not Chomsky had laid down his ideas about generative grammars in 1949 or in 1951.

form'' in the latter as differing phonetically from ''the underlying singular form[s]'' as in *knife*/*knives*, *mouth*/*mouths*, *house*/*houses*. There he proposes (p. 213):

> We can describe the peculiarity of these plurals by saying that the final [f, θ, s] of the underlying singular is replaced by [v, ð, z] before the form [i.e., {-s}] is added. The word ''before'' in this statement means that the alternant of the bound form is the one appropriate to the substituted sound; thus, the plural of *knife* adds not [-s], but [-z]: ''first'' the [-f] is replaced by [-v], and ''then'' the appropriate alternant [-z] is added. The terms ''before, after, first, then,'' and so on, in such statements, tell the *descriptive order*. Their actual sequence of constituents, and their structural order (§ 13.3) are a part of the language, but the descriptive order of grammatical features is a fiction and results simply from our method of describing the forms; it goes without saying, for instance, that the speaker who says *knives*, does not ''first'' replace [f] by [v] and ''then'' add [-z], but merely utters a form (*knives*) which in certain features resembles and in certain features differs from a certain other form (namely, *knife*).

In the section to which Bloomfield refers back to, he notes, among other things (p. 210):

> The principle of immediate constituents leads us to observe the structural order of the constituents, which may differ from their actual sequence; thus, *ungentlemanly* consists of *un-* and *gentlemanly* with the bound form added at the beginning, but *gentlemanly* consists of *gentleman* and *-ly* with the bound form added at the end.

It is obvious from this discussion that Bloomfield was quite aware of the important distinction between 'descriptive order' (established by the linguist) and 'structural order' (which he observes or 'discovers'). Given the interest in, if not preoccupation with, the structure of English on the part of Bloomfield's students during the 1940s, it is unlikely that these passages in a twenty-page chapter could have escaped the attention of young linguists such as Noam Chomsky; indeed, in 1962, at the Ninth Congress of Linguists held in Cambridge, Massachusetts, Chomsky would take issue with Bloomfield's (1933, p. 213) passage quoted above in the following fashion:

> He regarded ordering of rules as an artifact—an invention of the linguist—as compared with order of constituents, which is ''part of language''. 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). (Chomsky, 1964[1962], p. 70, note 8)

Leaving Chomsky's claim about the lack of interest in theory on the part of Bloomfield and his followers aside—which could be disproven easily—it is significant

that Chomsky continues his remarks by noting that "Harris showed (1951a, p. 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 his method does not generalize to such examples as the one given below; ..." (Chomsky, 1964[1962], p. 70, note 8; italics in the original), and he goes on to criticize Harris essentially for not having clarified the "italicized condition on morphophonemic rules" as he, Chomsky, thinks he should have.

What may be more interesting is what Chomsky said in the running text of the same page (since remarks in footnotes are not usually read aloud during oral presentations). There, Chomsky makes a plea in favour of the importance of the 'ordering of rules' in phonology, notably the importance of "a fairly strict ordering [that] must be imposed on phonological processes" (p. 70). He then comes to talk about two important papers of the 1930s, namely, Edward Sapir's celebrated "La réalité psychologique des phonèmes" of 1933 (published in English only in 1949) and Bloomfield's "Menomini Morphophonemics" paper of 1939, stating that (ibid.):

> [...] most of the examples in Sapir (1933) involve ordering, though he does not mention the fact. Bloomfield was much more concerned with questions of ordering and his Menomini morphophonemics (1939) is the first modern example of a segment of a generative grammar with ordered rules.

Chomsky then goes on to note that "Bloomfield does not discuss the extent or depth of ordering[7] in this grammar" and that it probably "does not exceed five" whereas he, in his 1951 Master's thesis on Modern Hebrew, had arrived at "a depth of ordering that reaches the range of twenty to thirty",[8] pointing out that "[r]ecent work [the reference he gives leads to no such thing] gives strong support to the belief that ordering relations among phonological processes are quite strict" (p. 71).

There can be no doubt that important advances in phonological theory had been made between 1939 and 1962; it may just be another question whether these were simply due to Chomsky's work alone. Since he refers to a passage in Zellig Harris' *Methods in Structural Linguistics* published in 1951, where Harris discusses "some of Bloomfield's examples of ordering" in the same note we have already quoted from, it may be useful to inspect the page 237 in question, apart from other passages in *Methods*.

2.2. The idea of ordered rules in Harris

In a proper history of generative linguistics still to be written, the role of Zellig S. Harris (1909–1992) could hardly be underestimated. Together with Charles F.

[7] By 'depth of ordering' is meant the number of sequentially ordered rules in a grammatical description. Jensen (1999, p. 82) is therefore not quite correct when he suggests that Chomsky and Halle introduced the concept in *SPE*, though it is true that they regarded "[t]he hypothesis that rules are ordered [...] to be one of the best-supported assumptions of linguistic theory" (Chomsky and Halle, 1968, p. 342).

[8] Barely 5 years later, Thomas Gordon Bever (b.1939), a doctoral student of Halle, had augmented the depth count of Bloomfield's ordering to eleven and reduced the size of Chomsky's earlier claims considerably (Bever, 1967; cf. Chomsky and Halle, 1968, p. 18, note 4)—after having exaggerated the distance of their respective 'depths' earlier (see Bever, 1963).

Hockett (1916–2000), Harris was the main force in the theoretical discussions of the 1940s and 1950s in American linguistics. Small wonder that Chomsky includes, in his 'bibliography' to the 1951 MA thesis (p. 74), Harris' *Methods* and a small but rather insightful paper by Hockett (1950a).[9] Since the suggestion has been made that Harris' book or, perhaps more correctly, its contents had not been available to the young Chomsky when developing his ideas from, say, 1949 onwards, two claims may have to be countered first; namely, the unavailability of the work prior to its publication in 1951 and the suggestion (recently repeated by Newmeyer [1996, p. 14]) that the manuscript version of *Methods* did not contain the portions that are of relevance to the present argument.[10]

In their book *American Structuralism*, Hymes and Fought (1981) made the following interesting statement (p. 136):

> By 1946 a full-scale treatment of language structure as a whole in such [distinctively Bloomfieldian?] terms existed in the form of the manuscript of Harris' *Methods in descriptive* (later: *structural*) *linguistics*, recommended for publication by the LSA's committee on such matters, but languishing for lack of funds.

In an article based on an interview given to Anne Daladier in Paris toward the end of his life, we can read the author's own affirmation that *Methods* "était rédigé et circulait en 1946 mais n'est paru qu'en 1951" (Harris, 1990, p. 10). Indeed, Hockett, in his 1947 paper on "Problems of Morphemic Analysis", published in *Language*, refers (1947a, p. 331, note 25) to the work (without naming its title) as "In his unpublished material, Harris shows how this [theoretical requirement] can be handled. His example is English /tuw/ (*to*, *too*, *two*), which in the absence of semantic criteria first appears as a single morph." Martin Joos (1907–1978), when re-editing the paper in his *Reader* of 1957, identified the reference as follows: "See now his *Methods in Structural Linguistics*, p.202" (Joos, 1957, p. 235). That Harris submitted his manuscript to the Linguistic Society of America (LSA) for possible publication in their monograph series sometimes in 1947, may be gathered from the following excerpt from the *LSA Bulletin*[11] No.21:3 (1948), p.15—referred to in Hymes and Fought (1981[1975], pp. 9–10) and, in some more detail, in Murray (1994, p. 164), who also notes (with reference to *LSA Bulletin* No.22, pp. 13–14) that publication of Harris' book was recommended by Bernard Bloch (1907–1965), the Editor of *Language* since 1942,

[9] The other two were, next to an otherwise unknown, unpublished paper by Chomsky himself on "Some Comments on Simplicity and the Form of Grammars", Goodman (1943) and Greenberg (1950).

[10] When challenged by the present writer to reveal the basis of his assertion, Newmeyer conceded: "Unfortunately, I never did see the ms version of Harris. Everything that I know/think about it comes from word of mouth (mouths, actually, since I had input from several people)." (E-mail message to author of 8 October 2000).

[11] The complete source is: *Bulletin* of the Linguistic Society of America (Supplement to *Language: Journal of the Linguistic Society of America*), vol.24, No.3 (July–September 1948), p. 15 (from the Proceedings of the LSA meeting held at Yale University, New Haven, 29–30 December 1947).

Charles C. Fries (1887–1967), Robert A. Hall (1911–1997), Hockett, as well as Hans Kurath (1891–1992).[12]

> Mr. [J Milton] Cowan [(1907–1993), the LSA Secretary] read the following report for Mr. Hans Kurath [(1891–1992)], chairman of the Standing Committee on Research.
> Two manuscripts have been considered by the Committee; their publication under the auspices of the Linguistic Society of America has been recommended:
>
> 1. Robert A. Hall Jr., Descriptive Italian Grammar
> 2. Zellig S. Harris, Methods in Descriptive [*sic*] Linguistics
>
> Hall's manuscript is ready for the printer. Harris' manuscript should be carefully edited before it goes to press.
> Both books are important. Hall's is the first detailed grammar of a well-known language prepared in accordance with the new descriptive technique. Harris' book represents the first consistent exposition of the technique (or one technique) of analyzing and describing languages from a structural point of view.

It appears, however, that despite Kurath's suggestion, Harris' manuscript was not substantially revised, though it is fair to assume that at least some editing was done and certain changes and even additions were made before *Methods* was published by the University of Chicago Press in 1951.[13] Reasons for the delay in publishing

[12] This is based on information printed in *LSA Bulletin* No.22 (October–December 1949), where Kurath, as chair of the Standing Committee on Research, reports, after having reported that Hall's book was to be published shortly, the following (pp. 13–14):

> As far as the Committee knows, no arrangements have as yet been made for publishing Harris's book. It is to be hoped that this important contribution to the methodology of descriptive linguistics can be published soon. The manuscript was read not only by the members of the Committee [consisting of Kurath, Hoenigswald, and the Indo-Europeanist George S. Lane (1902–1981)], but also by Bernard Bloch, [. . .], who unanimously support the recommendation of the Committee.

[13] For instance, checking all of the many bibliographical footnotes in *Methods*, one notes that especially in the first chapter, "Methodological Preliminaries", Harris added references to studies by Martin Joos on acoustics of 1948 and 1950 (p. 4, note 1 and p. 16, note 17, respectively) as well as to the 1949 volume of Sapir's writings (p. 22, note 24; see also p. 59, note 1 and p. 226, note 17), on which he published a very insightful review article (Harris, 1951b). Finally, there are references to a 1949 paper by Einar Haugen (1906–1994) on "Phoneme or Prosodeme?" (p. 47, note 2) and, toward the end of his book (p. 359, note 19), to two minor pieces, a 3-page note and an even shorter addendum of 1950 and 1951, respectively, published by his colleague at the University of Pennsylvania, Henry M. Hoenigswald (b.1915), evidently added in the last minute to a much longer list of references. Also, one could imagine that the "Appendix to Chapters 7–9: The Phonemes of Swahili. A Sample Phonemic Analysis prepared with the collaboration of Nathan Glazer" (Harris, 1951[1947], pp. 97–124) as a post-1947 addition, though I doubt that the same holds true of the various other appendices which are found at the end of each of the 19 chapters: they are typically illustrations, with data from a variety of languages, though some of them are also addressing particular theoretical issues, e.g., "Appendix to 12.41: The Criterion of Meaning" (pp. 186–195), where one might assume—as long as one has not been able to inspect a copy of the 1947 manuscript—that it had

Harris' book are probably many, including, as those at the University of Pennsylvania who knew Harris personally could attest to, that he was not someone who would trouble himself to have a work of his published expeditiously;[14] once he felt he had done his part, others could be expected to see to it that it was printed. Still, the more important reason for the delay may simply have been economic. Unlike the fairly short and straightforward text by Robert A. Hall, who used to prepare carefully typed copy, often camera-ready text, Harris' book was much longer, more technical, and more complex. As Norman A. McQuown (b.1914) stated at the outset of his review of *Methods*, "This epoch-making book was much read in manuscript before publication, and the author's influence was patent in many articles and reviews long before this summation appeared" (McQuown, 1952, p. 495). Given the later—one is almost tempted to say *studied*—ignorance of the book, let us read a bit further to what else the Chicago anthropologist has to say about *Methods* in these introductory paragraphs (ibid.):

> [...]. Not since Bloomfield's Language has there been such an ambitious attempt to cover the whole field. Unlike Bloomfield's, however, this book is limited to the presentation of one principle and one method of linguistic analysis and description. [...]
>
> The book follows out these basic methodological assumptions [laid out by the reviewer in the preceding paragraph] to their logical conclusion, exploiting every extension, every parallelism, every implication. The field of linguistic analysis is covered. The methodological assumptions are followed even where they prove 'cumbersome'; indeed, we are warned (p. 371) that we must not stray from their rigorous application, no matter how we may be tempted: 'The utility

been added as a result of criticism Harris had received from colleagues. However, one cannot be certain, unless a copy of the 1946 manuscript has been located. In an e-mail message (dated 2 December 1999) to the present writer, Geoffrey J. Huck of the University of Chicago Press wrote: "I did search the University of Chicago Press files on Harris's Methods, which are in the archives in Regenstein Library on the University of Chicago campus. Alas, it was a sparse file and I could find nothing there which touched on the matter either way [i.e., whether certain passages in the book had been added later or not]."

[14] An example of Harris' 'carelessness' about the fate of his work may be seen from the fact that, as may be gathered from Bloch's footnote at the end of Harris (1965, p. 401, note 56): "[This article has not been proofread by the author.]". The paper had been submitted much earlier; Harris simply relied on Bloch to do the rest. (Note that this footnote was dropped in the 1970, 1972, and 1981 reprints). Interestingly, no further publication from Harris' pen appeared in *Language* after this 40-page "Transformational Theory" article; one may speculate as to the reasons for this, given that Harris remained scholarly productive right to the end of his life in 1992. No doubt, the change of the editorship after Bloch's untimely death of cancer in 1965 had something to do with it. In fact, Harris had not published anything in *Language* between 1957 and 1965. Matthews (1999, p. 114) comments: "I have not thought it my business to inquire into the circumstances", but historians might wish to do so. Reading Harris' last contribution to *Language*, in particular the footnote he appended to the his remark "To interrelate these analyses, it is necessary to understand that these are not competing theories, but rather complement each other in the description of sentences", it seems that Harris was giving his parting shot—in the strongest terms he ever seems to have used in print:

> The pitting of one linguistic tool against another has in it something of the absolutist post-war temper of social institutions, but it is not required by the character and range of those tools of analysis. (Harris, 1965, p. 365, note 6)

> of these operations is compromised, however, if any results are recognized other than those obtained by means of the stated operations.' Stated in the simplest terms, you must know what you are doing and why you are doing it, and be able to tell someone else how. There is a high premium on responsibility.
>
> The reviewer finds this emphasis wholly admirable, and looks forward to the day when similar descriptive systems will be applicable with equal rigor to all aspects of human behavior. He considers Harris' contribution epoch-marking in the double sense: first in that it marks the culmination of a development of linguistics AWAY from a stage of intuitionism, frequently culture-bound; and second in that it marks the beginning of a new period, in which the new methods will be applied ever more rigorously to ever widening areas in human culture.

Similar assessments were made by various others of Harris' peers (cf. Koerner, 1993, p. 513, for locations of reviews[15]). For instance, Stanley S. Newman (1905–1984), a former student of Sapir's, noted that "There will be little disagreement among linguists that this book is the most important contribution to descriptive linguistics since [...] Bloomfield's *Language*" (Newman, 1952, p. 404).[16] This is also the book of which Noam Chomsky stated in 1973—this testimony bears being repeated here: "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. I found it very intriguing and, after some stimulating discussions with Harris, decided to major in linguistics as an undergraduate at the University of Pennsylvania" (Chomsky, 1975, p. 25). Of course, one should stress the word 'formal' in Chomsky's statement since—as he acknowledged in a footnote (p. 50, note 44)—that he had "read proofs" of the edition of David Kimḥi's (1160–1235?) *Hebrew Grammar* by his father William Chomsky (1896–1977), a professor at Gratz College in Philadelphia (later also at Dropsie College), published in 1952 "many years earlier", i.e., *before* 1947, and had been been "studying Arabic with Giorgio Levi Della Vida [(1904–1971)]" (ibid.).

Chomsky's account on how he started out from Harris' suggestions and developed his own ideas, first in a 1949 'undergraduate thesis', then in his MA thesis of 1951 (Chomsky, 1975, pp. 25ff.) reads rather well, and Harris' *Methods* is given an important role in the narrative, but one must realize that Chomsky is reconstructing what had happened in his theoretical mind more than twenty years earlier and from the vantage point of the incredible success of his own linguistic thinking.

While it may be understandable that Chomsky should credit his former teacher Harris less than might have been his due, readers might be surprised to read that he

[15] Cf. also Householder's (1952) review with its proverbial 'God's truth' versus 'hocus-pocus' distinction in matters of the theorist's two possible attitudes (p. 462).

[16] Newman (1952, p. 405) also noted that "Harris' use of compact statements, particularly in the form of an algebraic type of descriptive notation, required concentrated attention." He lauds Harris for referring "more frequently to the contributions of European linguists than has been the custom in recent American books on linguistics" (ibid.). Indeed, one finds frequent references to the work of Trubetzkoy, Jakobson, Jespersen, Saussure, and others in Harris' many bibliographical footnotes.

had encountered an "almost total lack of interest" in his 1951 thesis (Chomsky, 1975, p. 30), "[w]ith the exception of Henry Hoenigswald, who read the work carefully and made helpful comments" (p. 51, note 51).[17] Given that certain accounts of the development of Generative Linguistics, notably those by Newmeyer beginning in 1980, tend to credit Chomsky with having developed the basic ideas almost singlehandedly, it may be useful to remind the reader that the notion of a generative grammar was neatly sketched in the concluding chapter of *Methods*, where Harris summarizes the results of his discussion (pp. 261–378)—even if the term 'generative' is not mentioned:[18]

> 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 initial elements and with theorems concerning the relations among them. The final theorems would indicate the structure of the utterances of the language in terms of the preceding parts of the system.
>
> 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 defining the elements at each successive level or stating the sequences which occur at that level [footnote 17 omitted]. Compactness, inspectability, and clarity of structure may be gained at various points by the use of symbols for class, variable member, and relation, or by the construction of geometric models (diagrams).
>
> 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 in the descriptive 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

[17] At least in the revised version of Chomsky's MA thesis of December 1951, where one might have expected it, no such acknowledgement can be found.

[18] In order to counter the suggestion that these statements were added much later and not part of the manuscript that Harris submitted to the LSA, I may quote from an e-mail message from Professor Chomsky to Dr Bruce E. Nevin of 28 June 2001 (quoted here with permission granted by Chomsky on 23 July 2001), where he wrote:

> You asked about the status of the Methods ms. My information is limited. I read proofs—I think page proofs, but can't be sure after all this time—in 1947; my understanding was that that was the final proof-reading. I assumed that the ms had been circulated in 1946 or even before, and though I don't recall, I presume the Preface was signed before the ms went to the publisher, so January 1947 is not at all surprising. I doubt that substantive changes would have been introduced after proof-reading of the proofs. I never noticed any, and wouldn't have been able to check anyway, since I did not have the proofs available after I finished with them in 1947 and gave them back to Zellig.

element as it is subjected to various processes and extensions. (Harris, 1951, pp. 372–273)[19]

Reading these passages, one is inclined to underline various observations or add other means of emphasis, but it should suffice to consign accounts of 'Chomsky's revolution' as found in Newmeyer (1980, 1986, 1996) and emulators to something other than history. Chomsky himself acknowledged that "[t]he concept of 'grammatical transformation' developed out of Harris's work on discourse analysis" (Chomsky, 1975, p. 41), but when he dates this as *after* the completion of *Methods* and connects it with Harris' turning his investigation of 'discourse analysis' as if this was an unrelated subject matter from what he himself was to pursue, the above quote at least weakens his acknowledgment, since no doubt Harris had held these ideas well before 1951.[20]

However, since the present paper is concerned essentially with the genealogy of the idea of 'morphophonemics' and the concept of 'ordered rules' in American linguistics, we must turn to it in what follows.

2.3. Suggested lines of transmission: Bloomfield→Harris→Chomsky

Even from Chomsky's own early work, if read carefully, the impression prevails that there was more evolution than revolution occurring in American linguistics during the 1940s and 1950s.[21] In order to put all the pieces together, a monograph would probably be required, but perhaps even a sketch will suggest that the transmission of ideas in linguistics during that period was much facilitated by the small size of the

[19] Part of footnote 18 that Harris added to the above quotation reads: "In such presentations, a relation between two elements a and b is essentially the difference between two historical or otherwise derivational paths: that from A to a and that from A to b. A is set up as a base from which both a and b have, by different paths, been derived."

[20] This holds also true of Charles Hockett's ideas of 'transformation', which go back at least to 1949 (cf. Koerner, 1989, pp. 126–128, for details). Interestingly, Katz and Bever (1976, p. 17) affirmed that "contrary to popular belief, transformations come into modern linguistics, not with Chomsky, but with Harris's rules relating sentence forms. These are genuine transformations, since they are structure-dependent mappings of phrase markers onto phrase markers. That this is so can be seen from the examples of transformations Harris gives". Coming from people with close ties to Chomsky, this statement should count for something. Indeed, a list of 'grammatical transformations' of English was included in the report presented to the Linguistic Society of America in 1950, and published in Harris' "Discourse Analysis", *Language* 28 (1952), pp. 1–30, at section 2.33.

[21] In what follows, as well as in other parts of this paper, I gratefully acknowledge the spade work and a variety of suggestions made by Pierre Encrevé (1997), who, unlike myself, is a phonologist and much more familiar with the internal history of 'generative' phonology, having translated the theoretial portions of Chomsky and Halle (1968) as *Principes de phonologie générative* (Paris: Éditions du Seuil, 1973). It should be pointed out at least at this stage of the discussion that Encrevé's article is an impressive attempt at writing the pre-*SPE*, *SPE*, and post-*SPE* history of generative phonology (see especially pp. 100–102, 107–113, and 114–120 passim), whereas my paper focusses on the subject of the transmission of ideas surrounding the concept (and term) of 'morphophonology'/'morphophonemics'—note that the phonology/phonemics distinction would soon be used to distinguish 'generative' phonology from 'Bloomfieldian' phonology (see Chomsky, 1964[1962])—and the idea of 'ordered rules', while at the same time trying to offer at least hints at what might be called 'external history', but which I found largely missing in Encrevé.

community of scholars and the relatively small number of publications: most American linguists of the time knew each other personally. They met regularly at the annual meetings of the Linguistic Society of America, always held in the last week of December in cities like Philadelphia, New York, or Chicago, and during the annual Summer Institutes, mostly held at the University of Michigan at Ann Arbor during that period. There was collaboration (and co-publication) among these linguists and they circulated manuscipts, sometimes for several years before they were published.

While personal information on Harris is not readily available in print,[22] we may glean some information on his intellectual biography by looking at his scholarly output. From this we may gather that his training was in Semitics in a fairly broad sense, both philological and linguistic, and from his master's thesis of 1932 until the publication of his monograph *Development of the Canaanite Dialects* in 1939, we do not see him as a theorist and generalist (cf. Koerner, 1993, pp. 510–511, for details). The focus of attention appears to have changed around 1939, as we may gather from his list of publications, beginning with an edition of *Hidatsa Texts* collected by Robert H. Lowie (1883–1957), a student of Boas' and a friend of Sapir's, with grammatical notes and phonograph transcriptions by him and C. F. ("Carl") Voegelin, a one-time student of Sapir's. With this undertaking, Harris opens his horizon toward the study of Amerindian languages and non-historical linguistic data, and given the complexity of many of these indigenous languages (as well as their structural differences from Semitic and Indo-European languages), special techniques were needed to analyze and describe them adequately. For instance, the American Philosophical Society Library in Philadelphia holds "Cherokee Materials" of some 575 slips and 10 discs dating from 1941–1946, giving evidence of Harris' work with informants during the period. In his short Preface to *Methods*, signed Philadelphia, January 1947, Harris acknowledged, among others, the friendship he had held with Sapir and Bloomfield, both of whom had studied Amerindian languages extensively.

It is around 1939/1940 that Harris appears to have begun to take a special interest in general linguistic questions and matters of theory and methodology.[23] It is not by

[22] Apart from obituaries of Harris which are short on biographical particulars (apart from the suggestion that Harris' name might have been made up by himself—see Nevin [1992, p. 60]), I know only of Barsky (1997), which contains a chapter (pp. 47–93), which however deals only in passing with linguistic matters (pp. 49–72), and mainly with Harris' social and political beliefs and activities and their influence on Chomsky during the late 1940s and early 1950s (cf. Harris, 1997 on his political views; Chomsky's are sufficiently well known to require a particular reference here; cf. Koerner and Tajima [1986, pp. 91–162], for detailed, partially annotated list of Chomsky's publications on political issues). It appears that Harris was a rather private man. Curiously, no American was found to write the official obituary that every other deceased former president of the LSA (Harris served in that capacity in 1955) has received in *Language*. The one that P. H. Matthews of Cambridge was eventually commissioned to undertake possibly constitutes a fairer appraisal of Harris' work than any American linguist of today might have written (Matthews, 1999).

[23] One indication is that, apart from a one-page review of a volume in his original area of interest in 1935 (*Language* 11, pp. 262–263), Harris starts his series of contribtions to *Language* in 1940 with a review of a book on general linguistics, followed by Harris (1941a,b, 1942, 1944, etc). and a variety of other articles until 1957, after which only Harris (1965) appeared in that journal. Matthews (1999, p. 112), too, refers to 1940 as the year by which Harris "had already turned to general linguistics".

chance that in his seminars, as Leigh Lisker (b.1918) recalls, transformations were mentioned between 1940 (perhaps as early as late 1939, when Lisker took his first linguistics course) and the term ending June 1941, when he got his B.A. (Lisker in an e-mail message to Bruce Nevin, 1 March 2000). Influential papers of the 1940s, such as "Morpheme Alternants in Linguistic Analysis" (Harris, 1942), "Simultaneous Components in Phonology" (Harris, 1944), and "Discontinuous Morphemes" (Harris, 1945), had already been published by the time the 17-year-old Chomsky enrolled at the University of Pennsylvania, and well before he met Harris, possibly in late 1946, though the year regularly given by Chomsky as the time he decided to seriously study linguistics with the former is 1947 (e.g., Chomsky, 1975, p. 25; cf. Barsky, 1997, p. 51). Indeed, there is overwhelming evidence that matters of phonology and morphology and their interrelationships were very much discussed at the time by the main representatives of the so-called Post-Bloomfieldians, in addition to Harris, such as Bloch, Hockett, Nida, Swadesh, Trager, Voegelin, and Wells (cf. Joos, 1957, for the reprinting of many of their papers), when young Chomsky entered the scene. There was little evidence of a lack of interest in 'theory' during the 1940s and 1950s, as Chomsky in 1962 and thereafter frequently tried to depict these decades. Chomsky's own writings of the period (e.g., Chomsky, 1955b, 1955/1956) were quite typical of what was then said and done, and Harris' ideas loom large.

Before we attempt a discussion of the issues that many of the American descriptivists of the 1940s and 1950s were concerned with, let me quote a passage in Barsky's biography of Chomsky, in which he draws an interesting picture of Harris' social and intellectual habits (which corroborates what former colleagues and students of Harris have told me over the years):

> Harris encouraged the kind of unstructured, lively, and creative debate that had been the mainstay of Chomsky's early education and upon which he had thrived [...]. Course requirements, formal relationships, and scholarly hierarchies were rejected in favor of informal gatherings, broad-based discussions, and intellectual exchange. The University of Pennsylvania's linguistics department comprised, at that time, a very small group of graduate students who shared an enthusiasm not only for linguistics, but also for politics. They shunned the classroom, and met either at the nearby Horn and Hardart Restaurant or at Harris's apartment in Princeton or New York. The discussions could last for days, and Chomsky remembers them as being "intellectually exciting as well as personally very meaningful experiences". (Barsky, 1997, p. 51)[24]

Harris' manner of conducting himself suggests a general generosity, which certainly included his readiness to share his ideas with interested parties, and not claim ownership, as many teachers do, of his particular discoveries, theoretical findings, or

[24] This last quotation has been taken from *The Chomsky Reader* ed. by James Peck (New York: Pantheon, 1987), p. 8.

terminological choices.[25] Still, it appears from what I have been given to understand by those who knew him, that Harris was a person who often went his own way and left those behind who did not follow the general path of his thinking. In an attempt to round out the picture of Harris' enigmatic personality, I would like to add a statement of his. Characterizing both Bloomfield and Sapir in 1973, Harris noted the following, which may well apply to his own professional credo, and possibly meant as a swipe at what he observed in American linguistics from the 1960s onwards:

> Neither competed, or saw his scientific achievement as a matter of personal aggrandissement. And this was not for lack of a sense of history about their work. Both men knew that they were creating—or rather participating centrally in the creation of—a science. There was an excitement around them, in their ideas among their students and colleagues. Each of them pushed for his ideas—Bloomfield by incisive argument, Sapir by brilliant exposition—though without seeking to pre-empt the field. (Harris, 1973, p. 255)

Much has been made by MIT linguists (see Section 3, below) and their 'historians' (see Section 1, above) of the issue of whether or not the young Noam Chomsky, when working on his ideas from 1949 onwards that led to his MA thesis *Morphophonemics of Modern Hebrew* (Chomsky, 1951), had had access to a copy of *Travaux du Cercle Linguistique de Prague*, volume 8 (Prague, 1939), which contained Bloomfield's subsequently celebrated $10^1/2$-page article "Menomini Morphophonemics" (which he had submitted as a tribute to Nikolaj Sergeevič Trubetzkoy, who had died in Vienna in August 1938). True, the fact that the University of Pennsylvania Library had acquired a copy of this *Gedenkschrift* early on,[26] or that Harris reviewed Trubetzkoy's *Grundzüge* (which had appeared in the same series in the same year) two years later (Harris, 1941b) could at best serve as circumstantial evidence that Bloomfield's article was in all likelihood available to any inquisitive student there in the 1940s.

Yet, before entering this discussion, let me quote a few passages from Bloomfield's paper, which cannot but strike even the reader of today as carefully crafted and

[25] Those who knew Harris well—like Bruce E. Nevin (b.1945), who was his student from 1966 through 1970—may feel that in the following characterization which Harris gave of Bloomfield and Sapir he reveals much of himself:

> Each [Bloomfield and Sapir] were, to the good fortune of those who knew him and I hope of themselves, an extremely decent person of high integrity; each had utter and explicit contempt for the posturings and status in this society as well as for its vast injustice and inequality. They were people not with ambition, least of all with ambition in the terms of this society, but with satisfaction in what he was producing. Those who remember Bloomfield and Sapir know this about them. (Harris, 1973, p. 255; cf. Nevin, 1992, p. 63)

[26] According to C. Thomas Mason III of Tucson, Ariz. (e-mail to author of 6 March 2000) "UPenn's Franklin Library owns a copy of TCLP 8; it's shelved in High Density Storage and carries a Dewey call number, suggesting that the volume has probably been there for the past 60 years."

succinctly stated. It deals with "the *internal sandhi* or *morphophonemics* of the language [i.e., Menomini, an Algonquian language spoken by a small number (Bloomfields mentions some 1700 of mostly bilingual speakers residing in Wisconsin), which he had studied very carefully for many years (see Bloomfield, 1962)]" (Bloomfield, 1939, p. 105; italics in the original), thus providing a nod in the direction of both Trubetzkoy (1929, 1931, 1934) as well as traditional Pāninian type grammar.[27] Bloomfield early on in his paper describes his approach to the subject in the following terms:

> 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 supplies our statements (§§ 10 and following) in the order in which we give them, one will arrive finally at the forms of words as they are actually spoken. Our basic forms are not ancient forms, say of the Proto-Algonquian parent language, and our statements of internal sandhi are not historical but descriptive, and appear in a purely descriptive order. (Bloomfield, 1939, pp. 105–106; italics in the original)

However, as if to anticipate criticism by later commentators, Bloomfield (p. 106) points to the fact that most of these 'theoretical basic forms' and the processes applied to them "approximate the historical development from Proto-Algonquian to present-day Menomini". The result of his 'morpholexical' treatment of the language is that "[t]he forms now arrived at are *phonemic* forms of the actual Menomini language. Menomini phonetics, however, allows a great deal of latitude to some of its phonemes and of some overlapping between phonemes" (1939, p. 115; italics in the original). Charles Voegelin, who reviewed the Trubetzkoy memorial volume in *Language* in 1940, spending about 30%[28] of the six odd pages on discussing Bloomfield's "unrestricted description of a sound system [as compared to the other contributions, notably those by fellow-Americans Morris Swadesh and George L. Trager]", answers his own rhetorical question about the advantages of Bloomfield's morphophonemic approach as follows:

> Bloomfield's Menomini may be distinguished from Nootka [described in detail by Sapir and Swadesh], Tübatulabal [Swadesh and Voegelin, 1939], and Potawatomi [done in Hockett's 1939 doctoral dissertation under Bloomfield] studies in that it alone is good to the reader: it gives him few theoretical forms and only single rules to remember. (Voegelin, 1940, p. 257)

Two years after the appearance of these comments on Bloomfield's article, we have Harris' short, programmatic paper in the same journal, which, as mentioned

[27] The fact that Bloomfield regarded Pāṇini as a major influence has been shown by Rogers (1987); see also Cardona (1965).

[28] Out of altogether 31 contributions (not counting a couple of posthumous papers by Trubetzkoy himself), which are all enumerated and a number of which, especially in the area of phonology, are discussed.

earlier, led to a lively debate. In "Morpheme Alternants in Linguistic Analysis", Harris characterizes the purpose of his paper as doing nothing else but

> [...] to suggest a technique for determining the morphemes of a language, as rigorous as the model used now for finding its phonemes. The proposed technique differs only in details of arrangement from the methods used by linguists today. However, the small differences suffice to *simplify the arrangement of grammars*. (1942, p. 169; emphasis added: EFKK)

Not surprisingly, perhaps—we ought to remind ourselves that the article appeared four years before Bloomfield's debilitating stroke in 1946—the first reference (p. 169, note 1) is to Bloomfield's 1933 book,[29] but in the elaboration of his technique on the subsequent pages Harris explicitly refers six times in all to Bloomfield's 1939 article, beginning with a mention that "[s]ome linguists have called such pairs [of alternating forms] morpholexical alternants of one morpheme" (p. 170). His statement "The difference between those two cases [of alternants in Hebrew] is seen again in the Menomini *e*, which is an alternant of the morpheme juncture /-/" (p. 175) deserves mention especially because of the data cited. Further, when talking about 'external sandhi' and similar phenomena (p. 176), Harris refers to examples from Bloomfield's analysis three times (notes 18–20), and, finally, when supplying examples for morpheme alternants in unique enviroments (p. 178), Harris again quotes from Bloomfield's Menomini paper (in note 22).

Given Harris' manner of conducting himself as a mentor and Chomsky's enthusianism for doing linguistics with him during the late 1940s and early 1950s—not to mention Chomsky's voracious reading habits, then as still today, one would be hard pressed to believe that when he was writing his BA honours paper in 1949, Chomsky was unaware of Bloomfield's proposals concerning rule ordering and morphophonemic analysis, whether or not he had read Bloomfield's *Language* and his 1939 Menomini article at the time or not. As hinted earlier, Harris' 1942 paper got a discussion going that included most of the main players in the descriptivist debate about phonology, morphology, and their interaction.[30] Bernard Bloch (1947, p. 398) sets out the framework "[t]o describe the structure of a language as a whole", specifying that "the linguist must be able to describe also the structure of any single sentence or part of a sentence that occurs in the language. He does this in terms of constructions—essentially in terms of MORPHEMES and their ORDER" (emphasis in the original). Section 7, entitled "Morphophonemes" (pp. 414–418), discusses the treatment of different forms that occur in the same environment, but are not entirely in free variation with each other. Hockett's paper of the same year as Bloch's (and published *primo loco* in

[29] Followed by another on page 171 (note 7), not to mention an unreferenced discussion of Bloomfield's *knife/knives*, etc. examples (p. 173) quoted in Section 2.1 of the present paper.

[30] That the (Post-)Bloomfieldians regarded this area of interest as most representative of their work may be gathered from the reprints in *Readings in Linguistics* "[e]dited for the Committee on the Language Program by Martin Joos" (Joos, 1957, title-page). Apart from the fact that 28 articles out of a total of 43 were originally published (with few exceptions in *Language*) between 1941 and 1951, the great majority were devoted to these areas of linguistic structure, their analysis and description.

the same issue of *Language*) announces that it "develops further the morphemic analysis presented by Zellig S. Harris in 1942" (p. 321). Hockett does not refer to Bloomfield's 1939 article, but when illustrating his procedures, he notes: "I choose Fox [another Algonquian language treated by Bloomfield] rather than Menomini because the examples are a bit easier to cite" (p. 332, note 22). Nida, referring to both Harris (1942a) and Hockett (1947a), likewise, does not refer to Bloomfield's Menomini paper either,[31] but to pages in Bloomfield's Morphology chapter in *Language*. Interestingly, in a footnote extending the bibliographical reference to Bloomfield (1933, p. 217), Nida remarks: "William L. Wonderly has proposed in discussion that these French forms [i.e., 'subtractive' morphemes, taking the feminine form as 'basic'] might be most *economically* handled *morphophonologically*" (Nida, 1948, p. 441, note 51; emphasis added: EFKK), and adds a reference to Trager (1940).

That the alleged failure to make explicit references to Bloomfield's 1939 article is not truly an indication of neglect may be gathered from the fact that even in his article in *Language* entitled "Peiping Morphophonemics" Hockett does not make a mention of it, but refers instead to Harris (1942) and his later revision of some of Harris' assumptions (Hockett, 1947a) as the basis of his treatment of Chinese (Hockett, 1950b, p. 63, note 1). It seems understandable that once the essential points of Bloomfield's proposals had become part and parcel of the ongoing discussion, such an explicit mention was no longer regarded as necessary. These linguists certainly did not see a change of position from Bloomfield (1933) to Bloomfield (1939), but rightly saw his later analysis as fully in line with his previous work.

Other papers published during the 1940s, notably in *Language*, albeit not exclusively, could be referred to in order to document that when Noam Chomsky entered the field, linguistics was anything but a theoretical wasteland (cf., e.g., Bloch, 1941, 1948). Harris' *Methods* of 1947, in which there is an entire chapter devoted to "Morphophonemes" (pp. 219–242), has two explicit references to Bloomfield's Menomini paper (p. 231, note 29, p. 237, note 42,[32] and a third reference in a later chapter on "Constructions" (pp. 325–348), where he discusses 'zero morphemes' (Harris, 1951a[1947], p. 336, note 22, referring to Bloomfield, 1939, p. 108). It is difficult to believe that these passages (and explicit references) were not in the 1946 manuscript version whose proofs Chomsky himself confirms he had read (Chomsky, 1975, p. 25; 1979, p. 196, note 5). On page 231, Harris discusses a "slightly different type of regularity [. . .] in Menomini, where every morpheme ending in a non-syllabic

[31] However, as Encrevé (1997, p. 105) has documented, there are other places where Bloomfield's 1939 article is referred to and/or quoted from; for instance, in Harris' review article of Sapir's *Selected Writings* of 1949 (Harris, 1951a, p. 291, note 7, p. 292, note 8, p. 293), in Floyd G. Lounsbury's (1914–1998) methodological chapter to his important *Oneida Verb Morphology* book (Lounsbury, 1953), and in Hockett's *Manual of Phonology* of 1955, which Chomsky reviewed in 1957. However, my main focus in the present paper has been on pre-1949 publications in which Bloomfield's morphophonological ideas were discussed.

[32] For extensive quotations from Harris (1951a, p. 231, p. 237), see Fought (1999, pp. 315–316), who concludes, having cited relevant passages from Bloomfield (1933, 1939): "Together, then, these excerpts from Harris (1951a) cover the essential elements of Bloomfield's morphophonemics: dual levels of representation, the need to use knowledge of morphological boundaries, and the principles of conversion from basic to phonemic forms using rules ordered for that purpose."

[structure?] has a member with added /e/ when it occurs before a consonant'', evidence for which he takes directly from Bloomfield (1939, p. 109). On page 237, he addresses the issue of 'Morphophonemic Equivalent for Descriptive Order of Alternation', where he adds in a footnote supplying the full bibliographical reference: ''Bloomfield calls the necessary order of the statements [. . .] 'descriptive order.' See also in his Language 213'' (note 42; see also n.39 on page 236). After having discussed examples taken from Bloomfield's article, Harris concludes: ''The effect of this descriptive order of the statements about alternation can be obtained alternatively by an exact statement of the representation of the morphophonemes'' (p. 237).

The concept and importance of 'descriptive order' is then treated at length in Harris' chapter on ''Morpheme Classes'' (pp. 243–261 on pp. 246ff.). It is probably not by chance that he presents examples of 'descriptive order' from Modern Hebrew (pp. 246–248), given that he was a Semiticist, a speaker of the language, and had previously published a succinct paper on ''Componential Analysis of a [Modern] Hebrew Paradigm'' in *Language* in 1948, which starts out with the statement: ''The linguistic structure of an utterance is presumed to be fully stated by a list of the morphemes which constitute it, and by their order'' (Harris, 1948, p. 87). Isn't this what Chomsky was working out in his B.A. essay of 1949 and his M.A. thesis of 1951? Earlier, in 1941, Harris had already published a very detailed paper on ''Linguistic Structure of [Classical] Hebrew'', which he says, ''is an attempt to state the structure of Hebrew (of 600 B.C.) in terms of a formal method, which asks only what forms exist and in what combinations'' (Harris, 1941a, p. 143). There (pp. 153–154) he notes: ''The phoneme, or phoneme combination, or absence of a phoneme, which is replaced by other phonemes in the variants of a morpheme-unit may be called a morphophoneme.'' In short, both in terms of data from Hebrew and ideas about morphonological analysis there was a plethora of material and suggestions available from Harris' pen alone to set the stage for Chomsky's early linguistic efforts.[33]

2.4. Methodological statements in Chomsky (1951)

Lest we leave the *tertium comparationis* out of the present discussion, at least a few passages from the introductory statements in Chomsky's 1951 thesis should be supplied in order to give an idea of his argument. If we assume that Chomsky turned an undergraduate paper into an MA thesis, something which happens quite regularly, it is understandable that he has not made a reference to it in the bibliography of *The Morphophonemics of Modern Hebrew*, though one might have expected a mention of it in the ''0. Introduction'' (pp. 1–6), as is customary. Instead the revised text of December 1951, dispensing with any such traditional talk about the origin of or motive for the thesis, addresses the subject right from the start in a surprisingly self-assured manner:

> A grammar of a language must meet two distinct kinds of criteria of adequacy. On the one hand it must correctly describe the 'structure' of the language (i.e., it

[33] Or should we assume that already the term 'morphophonemics' itself was an independent creation of Noam Chomsky's in 1949?

> must isolate the linguistic units, and, in particular, must distinguish and characterize just those utterances which are considered 'grammatical' or 'possible' by the informant), including as a special subclass those of the analyzed corpus. On the other hand it must meet requirements of adequacy imposed by its special purposes (e.g., pedagogical; as a basis for comparative study, etc.), or, in the case of a linguistic grammar having no such special purposes, requirements of simplicity, economy, compactness, etc.[1] Thus the linguistic analysis of a language L can be described as the process of determining the set of 'grammatical' or 'significant' sentences of L (i.e., of determining the extension of the predicate 'grammatical in L'), or, in other words, it is the process of converting an open set of sentences—the linguist's incomplete and in general expandable corpus—into a closed[2] set— the set of gramatical sentences—and of characterizing this latter set in some interesting way. Accordingly we might distinguish and consider separately two aspects of the linguistic analysis of a language, a process of 'discovery' consisting of the application of the mixture of formal and experimental procedures constituting linguistic method, and a process of 'description' consisting of the construction of a grammar describing the sentences [p.2] which we know from step one to be grammatical, and framed in accordance with the criteria related to its special purposes. (Chomsky, 1951, pp. 1–2)[34]

In other words, Chomsky moves *medias in res* so to speak very quickly and his argument becomes more and more technical. Much is made of what he calls 'certain criteria of simplicity' (p. 4), and "any relatively precise notion of simplicity" (p. 5),[35] in order to be kept "from reducing to an absurdity, the notations must be fixed in advance" (ibid.). Referring to his unpublished paper (p. 9), Chomsky defines "the criteria of simplicity governing the ordering of statements [...] as follows: that the shorter grammar is the simpler, and that among equally short grammars, the simplest is that which the average length of derivations of sentences is least" (ibid.)

Having read at least part of what Harris, Hockett, and others had written during the 1940s and early 1950s, Chomsky's statements do not appear all that novel, and are certainly in line with the discourse of the time and place. Still, one may detect an edge to the argumentation indicative of someone striving to develop a voice of his own. The argument in favour of 'simplicity' (which appears to be a forerunner of the term 'idealization'; cf. Chomsky, 1979, pp. 55–58 *passim*) which is made use of early on seems to be brought to bear in order to differentiate himself from his predecessors—but isn't this concept much the same as speaking of "the saving in work" when disregarding

[34] Footnote 1 refers to Goodman (1943); footnote 2 (both on p. 67) specifies: "Though not necessarily finite. Thus the resulting grammar will in general contain a recursive specification of a denumerable set of sentences."

[35] Cf. Harris, when discussing 'morphemic long components': "The criteria for selecting a basic alternant are not meaning or tradition, but *descriptive order*, i.e. resultant *simplicity of description* in deriving the other forms from the base." (1951a[1947], p. 308, note 14; emphasis added: EFKK)

"the very small classes which are included in some general class" in morphological analysis (Harris, 1951a[1947], p. 251) to cite just a brief remark from *Methods*?[36]

The rest of Chomsky's *Morphophonemics of Modern Hebrew* is a veritable tour de force, replete with a most involved technical apparatus, to set up the various phonological, morphological, and in part syntactic rules necessary to produce the relatively small body of material from Modern Hebrew. Commenting on what he was trying to do in his M.A. thesis twenty-five years later, Chomsky, 1979[1977], p. 112) conceded:

> Reading back into this work the explicit concerns of a later period, one might say, then, that the goal was to show exactly how this grammar with its empirical consequences would be constructed by someone initially equipped with the framework for rules and the definition of simplicity ([. . .]), and given a sufficient sample of the data. Actually, this was done in far greater detail and scale than anything I've attempted since, and was far too ambitious, I suppose.

One can imagine that Harris' general philosophy of science—and what Hockett in 1968 called, perhaps not quite fairly so but still in the direction of a tendency on Harris' part, 'theoretical nihilism'—could not satisfy a young student who more likely is looking for certainty, not at a variety of possible solutions. Still, it is at least obvious to me that Chomsky received much more than food for thought from his mentor than Harris has been credited for. However, as we shall see from what follows, this influence has routinely been minimized by Noan Chomsky and his close associate Morris Halle.

3. A counter-history emanating from MIT

What may have appeared as a fairly gradual development, in scientific terms, of linguistic theories in American linguistics in the preceding portion of the present paper was not in the interest of the strategists at MIT's Modern Languages Department, which soon become the Linguistics Department in time for the 9th International Congress of Linguists held in Cambridge, Mass., in August 1962, at which Chomsky had his first international exposure and, one may add, major breakthrough. Germans have an expression what would characterize their attitude at the time: *Es kann nicht sein, was nicht sein darf* ("It cannot be what cannot be allowed to be"). What I mean by this is that by the time the 9th Congress was on the horizon, which was ably prepared and effectively run by Halle,[37] the strategy had become to sell Chomsky's ideas as

[36] Of course, terms such as 'simplicity', 'economy', and the like show up rather regularly in the writings of American descriptivists well before Chomsky made use of them (see some of the quotations earlier in this paper, notably Harris' (1942) article in which he comments on Bloomfield (1939), but also McQuown's (1952) review of Harris (1951b).

[37] For details regarding events before and after the Congress, and what happened to Joshua Whatmough (1897–1964) of Harvard, who "was the chief figure in securing the invitation for the 9th International Congress to meet in the United States, and who was instrumental in obtaining two substantial grants for support of that congress" (as Eric P. Hamp reported in *Language* 52, 1966, p. 622), cf. Koerner (1989, pp. 116–117).

having little to do with the linguistics of his American teachers and predecessors, but as being the result of a radically different approach which supposedly harkens back to ideas found in the 'Cartesian' linguistics of the 17th-century Port-Royal grammarians and to the early 19th-century treatises by Wilhelm von Humboldt (1767–1835). In order to make this argument stick, not only did connections have to be drawn to the work of these pre-20th century thinkers, but also connections with the work of Chomsky's immediate predecessors had to be minimized, if not erased. As we know, this strategy has been rather successful and the story of the non-cumulative, indeed 'revolutionary', nature of 'generative' linguistics has been reproduced in text books and historical accounts, to the extent that this concoction has become accepted as historical fact by many followers. The present paper deals only with the first aspect of the strategy, which, as Sydney M. Lamb (b.1929) described, led to the following situation:

> Older-generation linguists, upon encountering some of these pages [in Chomsky, 1964, 1965], will stare with incredulity and no little irritation at the distortions and misunderstandings of their ideas and practices and those of their colleagues; while students who never knew what neo-Bloomfiedian linguistics was really like, [...], are led to the false impression that all linguists before Chomsky (except, of course, Humboldt, Sapir, and a few other candidates for canonization) were hopelessly misguided bumblers, from whose inept clutches Chomsky heroically rescued the field of linguistics. (Lamb, 1967, p. 414)

We all know from history that revolutionaries have always targeted the younger generations as they will soon define the agenda once the older ones have been pushed aside. As in the American electoral system, it is winner take all.

3.1. Morris Halle's claims concerning Bloomfield's 'morphophonology'

Halle's important role as an academic politician and strategist cannot be underestimated. Indeed, I find it doubtful that Chomsky's success during the 1960s could have been as great without the help of his clearly committed running mate from about 1953 onwards or at least since 1955, the year in which both he and Chomsky completed their doctorates and Chomsky received his first appointment at MIT's Laboratory of Electronics. However, the present paper deals only with one particular ingredient of the over-all strategy, namely, what Barsky (1997, p. 55), following Harold Bloom, called 'the anxiety of influence', which involves at least the downplaying of an impact that their immediate teachers might have had, Roman Jakobson (1896–1982) in Halle's case, and Harris in Chomsky's case, at times in the face of impressive evidence to the contrary. Encrevé (1997, pp. 111–114 *passim*) devotes much space to the manner in which Jakobson's 1948 paper "Russian Conjugation" was treated, by Halle and also by Chomsky, namely, ignored, although it could be shown that it had had an influence on both linguists. But Jakobson represents in the main the European structural tradition, not the American 'descriptivist' tradition, which is the focus of the present paper.

At least for outsiders, by the time Halle's *Sound Pattern of Russian* appeared in 1959, readers of the author's preface could not fail to realize that he had effectively hitched his phonology carriage to the Chomskyan syntactic bandwagon (see Halle, 1962), and it was obvious that Halle had become a determined promoter of Chomsky's cause by that time. That he should unhitch his cart thirty years later was not done to harm his relationship with Chomsky, who had not worked in phonology since their joint work *The Sound Pattern of English* in 1968, and by 1988 was going into the direction of abandoning all thoughts of rule ordering so dear to Halle. That this is so is evident from Halle's joint paper with Sylvain Bromberger (b.1924), another senior member of Chomky's Department of Linguistics and Philosophy, which was first presented at a symposium, "The Chomskyan Turn", held in Jerusalem in April 1988, and to which the authors appended a "Note on Recent History" (Bromberger and Halle, 1989, pp. 65–69), which confirms Halle's continuing commitment to Chomsky. In it, the authors give the reader the benefit of a long quotation from Bloomfield's "Menomini Morphophonemics" paper (1939, pp. 105–106; see Section 2.1 above for the relevant text), but only to conclude that Bloomfield erroneously held to a belief that was typically shared by linguists of the 1930s, namely, "that principles operative in languages conceived as synchronic systems functioning autonomously were totally different from the principles operative in the historical evolution of languages" (Bromberger and Halle, 1989, p. 66). Apart from the fact that Bloomfield—even in the quotation provided—does not use the word 'totally', but admits that the synchronic rules he had set up for present-day Menomini "approximate the historical development from Proto-Algonquian", language historians have agreed for a long time that linguistic change involves rule change; they have however not argued that these rules are recoverable in synchronic analysis (cf. Hoenigswald, 1946, for an early statement concerning diachronic change).

Next, Bromberger and Halle (1989, p. 66) provide the quotation from Bloomfield's *Language* in which he affirmed that "the descriptive order of grammatical features is a fiction [set up by the analyst]" (Bloomfield, 1933, p. 213), with the intent of proving that when Bloomfield wrote his Menomini paper, he "had changed positions", adding: "The fact that he had done so, however, was totally ignored by the American linguistic community in the 1940s and 1950s". They cite the fact that Hockett had not referred to it either in a 1948 paper in an issue of *Language* in honour of Bloomfield or in his oft-quoted "Two Models of Grammatical Description" paper, which contains a passage (Hockett, 1954, p. 211) "which echoes the passage from Bloomfield (1933[not 1939!]) almost verbatim" (Bromberger and Halle, 1989, p. 66) as well as the fact that Bloomfield's (1939) paper was not included in Joos (1957) as evidence for this neglect, adding in a footnote (p. 68, note 16) that "Bloomfield's paper was treated as a curious experiment—not to say, indiscretion—that did not merit extensive discussion." Indeed, Bromberger and Halle go so far as to claim that "the article was so unknown in America that Chomsky tells us[38]

[38] Note that the claim emanates from Chomsky himself, and does not appear to be based on the Bromberger and Halle's independent research.

that he had not read 'Menomini Morphophonemics' until his attention was drawn to it by Halle in the late 1950s'' (p. 67).

In the following paragraphs the authors expand on their claim that this ''alternative approach to phonological description [...] tested successfully by Bloomfield was hardly known at the time'' (ibid.), and that as a consequence Chomsky had to develop his challenge to 'the prevailing wisdom' in ignorance of this earlier work. Although Chomsky makes no reference to the fact that ''some rules of Modern Hebrew are identical with well-known sound changes'' in his 1951 thesis, Bromberger and Halle (1989) boldly affirm that he ''was of course fully aware of these parallels between synchronic and diachronic rules'', adding: ''Unlike most linguists of that period he was not concerned about confusing synchronic and diachronic descriptions and viewed parallels between the two types of rules as evidence in support of his proposed analysis [Chomsky (personal communication)]'' (pp. 67–68). Well, if that was Chomsky's thinking at the time, it could be explained by his lack of training in historical linguistics; it is certainly not a strong argument at all. The authors go on to summarize their story by saying (p. 68):

> In 1951 Chomsky thus was independently led to the same conclusions that Bloomfield had reached twelve years earlier.[39] It is a matter of 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 1950s, many years after submitting his Master's thesis.

Encrevé (1997, p. 105), referring to these affirmations, is quite surprised, to say the least, since, he argues, Chomsky and Halle had become close friends by 1953, and that Halle could be expected to have read Chomsky's *The Logical Structure of Linguistic Theory* of 1955/56 at the time in which explicit references to Bloomfield (1939) can be found. Myself, I am inclined to believe that Halle may not have read the rather bulky manuscript and, as we have seen from the above quotes, simply relied on Chomsky's word on the matter. Be that as it may, it is safe to assume that Chomsky had seen the paper in question *at least* by the early 1950s, if not earlier.[40]

[39] Note that no reference is made to the 1949 B.A./undergraduate thesis/paper/essay; references to its very existence seem to go all back to Chomsky's own affirmations alone. Several efforts to obtain information from the University of Pennsylvania administration on whether at that time any formal requirement existed or whether the submission of such an essay or thesis had been registered or a copy of the piece kept have led to nothing.

[40] In an exchange with the present writer, Thomas G. Bever, who finished his doctoral thesis in 1967, but who had worked with Bloomfield's (1939) paper several years earlier (see Bever, 1963) wrote (orthography normalized): ''My [...] cher maître would be Morris not Noam, but Noam claims that he was directed to B[loomfield]'s Menomini morphophonemics paper by Harris, could have been very early fifties at the latest. For a long time I had a xerox of Noam's own copy, but it was not dated'' (E-mail of 1 December 1999).

3.2. Chomsky's own affirmations concerning Bloomfield (1939)

While some may find Halle's arguments at different points in the evolution of generative linguistics, notably generative phonology, sufficiently transparent and 'politically' motivated, Chomsky's remarks regarding the development of his linguistic ideas appear to be more complex, if not diffuse, and at times contradictory, if we follow the actual chronology of his work.

For instance, in a letter to Robert Barsky of 31 March 1995, Chomsky writes:

> Hoenigswald and Harris were very close to Bloomfield, and certainly knew his work. 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.[41] But Hoenigswald read it [i.e., what was to become Chomsky, 1951], and must have recognized the similarities, back to classical India. I learned nothing of this [i.e. Bloomfield, 1939] until the 1960s, when Morris Halle found out about Bloomfield's work. (Barsky, 1997, p. 55)

As Barsky (ibid.) puts it, Bloomfield's Menomini paper "is an extraordinary text, completely inconsistent with Bloomfield's other writings about language and how research should be done. This, Chomsky believes, was one of the reasons Bloomfield decided to publish it in Europe."[42] People familiar with the published work by American linguists of the 1940s and 1950s (see Sections 2.2 and 3 above for illustration) will find the above claims hard to swallow. That Chomsky 'found out about' Bloomfield's (1939) paper only in the 1960s is clearly contradicted by Chomsky himself. In the printed version of *The Logical Structure of Linguistic Theory* (henceforth: *LSLT*)—and I might add that all references to the work of Hjelmslev for example in the 1955/56 version were expunged from the 1975 publication (cf. Koerner, 1995, pp. 98–99, for details)—there are still three explicit references to this paper (including its listing in the bibliography, p. 571). The most interesting reference is found in Chapter IV, "Simplicity and the Form of Grammars" (pp. 113–128), where we can read in a lengthy footnote:

> Note that phonemes can often be "embedded" in the morphophonemic level as primes of this level. English, in fact, is a poor source for interesting morphological examples, and the simplification effected by morphophonemic analysis in

[41] Given Chomsky's frequent references to Harris' lack of interest in what he was doing and the possible implication that he might not have supported his advancement, it may not be inappropriate to mention that the photocopy of his dissertation housed in the Joseph Mark Lauinger Library, Georgetown University (call no. P 291 C47 1955a), was signed by Zellig S. Harris in two capacities, namely, as "Supervisor of Dissertation" and "Chairman of Group Committee", with the lines provided for Committee members empty. As well, from what I have been able to ascertain from persons close to Harris, it appears that he suggested that Chomsky take his place as Plenary Session speaker at the 1962 Congress of Linguists which provided Chomsky the launching pad for his first and all-important international exposure.

[42] There are many assertions in Barsky's book which are based on what Chomsky had told him, not on the author's own research.

> this case is rather small. But in many languages where such morphophonemes have wide distribution and are complexly interrelated, such analysis can lead to very great economy. See Bloomfield, "Menomini morphophonemics," my *Morphophonemics of Modern Hebrew*, and many other linguistic studies. (Chomsky, 1975[1955/1956], p. 115 note 5)

Both the order of the references and the acknowledgement that 'many other linguistic studies' treated this subject by at least 1954 are important here. Indeed, we should recall that Chomsky had been working on these matters eventually combined in *LSLT* at least since his master's thesis,[43] so we could expect that (other) pre-1951 writings (than Bloomfield's) are meant to be included here.

The other reference—hardly less significantly, since it suggests, among others, that when writing *Methods*, Harris made use of them—is to "[s]everal methods of presenting grammars of the first type [i.e., the 'operational' form] are discussed by Harris, *Methods in Structural Linguistics*, Section 20.3;[44] cf. Bloomfield, "Menomini morphophonemics," Jakobson, "Russian conjugation," as examples of this general form" (Chomsky, 1975 [1955/56], p. 78, note 2). Although there is no explicit reference to Jakobson's (1948) paper to be found in *Methods*, Harris refers to earlier papers by him (cf. Harris, 1951a[1947], p. 334 note 19);[45] besides, in his Preface (p.v), Harris acknowledges "important criticisms" from Jakobson and two American linguists,[46] and he may have seen Jakobson's paper, which was inspired by Bloomfield's work, in manuscript.

As we have seen from Chomsky's (1962) paper presented at the Ninth International Congress of Linguists, of which a pre-print was available many months in advance (cf. Koerner and Tajima, 1986, pp. 14, 18–19 for details), it sounded as if he had been well aware of Bloomfield's Menomini paper for quite some time (cf. Section 2.3 above). However, by the early 1970s, for reasons difficult to fathom, Chomsky changed his mode of discussing the paper. In 1973, when he penned his Introduction to *LSLT*, Chomsky affirmed that his 1951 MA thesis had been written "in ignorance

[43] In the original manuscript of *LSLT* Chomsky included about everything he had written by that time with the exclusion of his published reviews and articles of the early 1950s; for instance, his M.A. thesis was added as an appendix to chapter VI, "Lower Levels of Grammatical Structure" (cf. Chomsky, 1975, p. 169, where a brief summary is offered in its stead). His PhD dissertation, submitted in 1955 to the University of Pennsylvania, formed chapter VIII (and chapter IX in the 1975 publication). No doubt, in its 1955/56 form *LSLT* was unpublishable (for details on the fate of the altogether three 750 + page versions of *LSLT*, see Murray, 1999). In 1973, Chomsky himself admits that the "manuscript was never actually prepared for publication" (1975, p. 1). See also Koerner and Tajima (1986, pp. 3–5, p. 56) for some details on the different versions and, for a closer analysis, Ryckman (1986, chapter 3), notably note 1, which extends over 4 pages (pp. 143–147).

[44] That is on pp. 372–373 ("Description of the Language Structure"), from which I have cited passages in Section 2.2 (above).

[45] In a letter of 17 November 1988 to Newmeyer, Chomsky went so far as to assert that "in an American linguistics programme [at that time] such as that at Penn, no one ever read a word of Jakobson's, on any topic" (1996, p. 14).

[46] A rather obscure William D. Preston (d.1954), who published a few reviews and short papers during 1946 and 1949 (e.g., "Problems in Text Attestation in Ethnography and Linguistics", *IJAL* 12 [1946], pp. 173–177) and Fred Lukoff (1920–2000), who was known for his work on Korean.

of Bloomfield's (1939) study'' (Chomsky, 1975, p. 47 note 16), and in his January 1976 conversations with a young French follower, Mitsou Ronat (1946–1984), he reiterated the claim in the following fashion, in response to her question ''When did you think for the first time of proposing an explanatory theory in linguistics?'':

> That was what interested me about linguistics in the first place. As an undergraduate at the University of Pennsylvania in the late 1940s I did an undergraduate thesis called ''Morphophonemics of Modern Hebrew,'' later expanded to a master's thesis with the same title in 1951. That work, [...], was a ''generative grammar'' in the contemporary sense; its primary focus was what is now called ''generative phonology,'' but there was also a rudimentary syntax.[47] I suppose one might say that it was the first ''generative grammar'' in the contemporary sense of the term. Of course there were classical precedents: Panini's grammar of Sanskrit is the most famous and important case, and at the level of morphology and phonology, there is Bloomfield's *Menomini Morphophonology*, published a few years earlier, though I did not know about it at the time. (Chomsky, 1979[1977], pp. 111–112)[48]

There are probably other places where Chomsky has made the same or rather similar claims. Two more recent ones, published in 1996 and 1997, respectively, may suffice. In a letter of 17 November 1988 to Frederick Newmeyer, Chomsky tells nearly the same story and elaborates on 'the mood of the times' in this way:

> It is rather astonishing that no one at Penn suggested to me that I look at the Bloomfield article. It is not surprising that Harris didn't, given his theoretical outlook. But more surprising that Henry Hoenigswald[49] never mentioned it. He must have known about Bloomfield's article as well as the Paninian tradition on which it was based. The fact that none of this was ever brought to my attention in a department consisting of Bloomfield's students and close friends is quite remarkable, [...]. (Newmeyer, 1996, p. 14)

By the 1990s, this view of his own past with regard to Bloomfield's work, and the failure of his teachers to draw attention to it, seems to have become Chomsky's fixed position. We have already quoted from his 1995 letter to his biographer Barsky above, and I shall quote just one other passage where the essentials are repeated—within the context of a public lecture on the History of Linguistics,

[47] A few years earlier, Chomsky (1975[1973], p. 26) had volunteered that ''the syntactic component was rudimentary. It consisted of phrase structure rules modeled on Harris's morpheme-to-utterance formulas (cf. *Methods*, chapter 16)'', which seems to suggest that he had access to the manuscript version at that time, though we could also refer to Harris (1946) where this approach had been worked out in detail. Curiously, in the *Bibliography* to Chomsky (1975), there are no references to pre-1951 publications of Harris (cf. p. 572).

[48] Interestingly, the reference to Bloomfield's (1939) paper was added to the English version.

[49] Elsewhere in the book, Hoenigswald was singled out as the only linguist, apart from Bernard Bloch, ''the well-known Yale phonologist'', as having shown an interest in Chomsky's 1951 thesis (1979[1977], p. 130).

thirty years after *Cartesian Linguistics*, interestingly enough. On this occasion the story line went thus:

> [I]n 1939, Bloomfield wrote a generative grammar of Menomini, an American Indian language, very much in the style of Panini. That work, even though he was the leading American linguist, was not known in the United States, except to his immediate students. In fact, when he died a few years later and there was a comprehensive study of his work on Algonkian languages, done by one of his main students, this was omitted.[50] It wasn't even listed. He actually published that article in the *Travaux du Cercle Linguistique de Prague*, in Czechoslovakia (Bloomfield, 1939).
>
> I didn't know Bloomfield personally. But in retrospect what I would have liked to ask him is whether he published it in Prague because it was not the kind of work hard-headed linguists did in the United States. If you look at the ideas, you will see that his schizophrenia is rather deep. In his [...] major text of modern American linguistics [i.e., Bloomfield, 1933], he is very critical of the concept of hidden structures, ordered rules, and that sort of thing: "this is old fashionable mentalism, we want to get rid of this crazy ideological baggage".[51] On the other hand, if you look at his grammar of Menomini, his generative grammar in the Paninian tradition, it is full of hidden structures and ordered rules. [...]
>
> Even more striking was that no one pointed out to me, a young undergraduate doing some work, that just a few years earlier, the leading figure of American Linguistics had done something very similar, on another language. I found out about it almost twenty years later, when I had become interested in History of Linguistics. (Chomsky, 1997, p. 108)

Reading this account, one is not sure what to make of it, especially if one has read at least the few bits of what Bloomfield actually stood for, such as those passages quoted earlier in the present paper (Section 2.1). Paul Postal (b.1936), who in the 1960s worked fairly closely with Chomsky, in later years took a rather critical view of this 'principle' of how to make strong arguments without providing the evidence. It suggests to "simply assert" and as boldly as possible so that few will dare to question the assertion. By way of illustration, Postal (1988, p. 133), quotes Chomsky as saying: "Suppose that counter evidence is discovered—as we should expect and as we should in fact hope, since precisely this eventually will offer the possibility of a deeper understanding of the principles involved."[52] Chomsky is of course referring

[50] The footnote refers to Hockett's 1948 paper and his later admission, when he reprinted it, that he had (inadvertently, he said) overlooked it at the time (see Bloomfield, 1970, p. 495).

[51] Note that this phrase is not a quotation from an existing source, but a typical example of the caricature that Chomsky and his followers disseminated regularly during the 1960s until the next generation of linguists took it as truth. The entire campaign against the Bloomfieldians at the time was full of these half-truths if not falsehoods about their 'taxonomic', mindless, atheoretical, inductive, positivistic, etc. approach to language and linguistic analysis, and still in the 1990s textbooks in 'modern' linguistics contain much of these distortions. For how this is done in pro-generativist textbooks, see Lawson (2001, pp. 8–14).

[52] N. Chomsky, *Some Concepts and Some Consequences of the Theory of Government and Binding* (Cambridge, Mass.: MIT Press, 1982), p. 76.

to matters of theoretical argument in this quotation, but one may ask whether the same approach does not also apply to the manner in which sees his own intellectual development.

3.3. Some tentative conclusions

Reading Chomsky's statements concerning the development of generative linguistics—and also Halle's propagations of some of these views—one may come to the conclusion that they served much the same purpose, at least on the surface of things. Why some of these—in the face of the available evidence—at best contradictory claims have been made when they were made, may not be part of the historiographer's task. Ideally, he wants to be seen as merely letting the facts (in as much as they can be established) speak for themselves. The evidence seems to suggest that in Halle's case these assertions have been made in support of his friend Noam Chomsky and, of course, and as part of the long-standing 'eclipsing stance' (Charles Voegelin, 1963) of 'The Chomskyan Enterprise' that Chomsky had embarked on by the mid-1950s. That many of these claims have been made many years after the 'Revolution' had succeeded—in most instances from the mid-1970s onwards—must be a puzzlement for the historian, as they do not seem to serve even any political purpose.

In Chomsky's case a number of historians of American linguistics seem to hold the view that when it comes to the depiction of his linguistic past, Chomsky cannot be relied on. Most of the Generative Semanticists interviewed by R. A. Harris (1993) expressed the sentiment that Chomsky's self-history was dishonest and manipulative, and in 1998 he has come out to say so himself. Huck and Goldsmith (1995), in their searching analysis of the Generative Semanticists' clash with the Interpretive Semanticists led by Chomsky during the late 1960s and the early 1970s (which was covered by Harris in 1993, but perhaps not with comparable thoroughness), were careful not to pass a similar judgment even though some of their findings might have suggested it.[53] Taking the high road,[54] they only went so far as saying that the claim of Chomsky and his associates that Generative Semantics was falsified was "essentially ideological in character and scientifically unjustifiable" (p. 93). Stephen Murray, whose well-researched 1980 paper "Gatekeepers and the 'Chomskian Revolution'" so upset historians of the victory march of Generative Linguistics like Newmeyer, and who has done more serious research in this area of interest than anyone else (e.g., Murray, 1994, especially chapters 8 and 9; Murray, 1999), characterized Chomsky as being 'delusional' (Murray, 1994, p. 246). However, John E. Joseph, familar with the views held by the above scholars, states "I am not convinced. My own dealings with Chomsky have strongly suggested that his belief in

[53] Including their observation that many ingredients of Generative Semantics argument that had been attacked and dismissed by Chomsky subsequently found their way into his own argument. Compare, as just one example of how the losing side felt, Paul Postal's reply to their question whether there had been 'an intellectual battle or a social battle [with Chomsky] or both?': "Mostly a propaganda battle" (Huck and Goldsmith, 1995, p. 137).

[54] After all, their book was dedicated to Chomsky, Halle, and several other scholars.

this view of his history is genuine and absolute", adding "To affirm the sincerity of Chomsky's interpretation of his own past is by no means to say that I agree with it" (Joseph, 1999, pp. 421–422). I think we might safely agree with the following conclusion drawn by John Fought, who has given the matter discussed in the present paper a careful analysis (Fought, 1999, p. 316):

> [...] I believe that the similarities between the strategy and techniques of Bloomfield's *Menomini Morphophonemics* and the architecture of early Chomskyan generative phonology are most plausibly explained by Chomsky's prior acquaintance with the Bloomfield paper, either directly or through summaries in Harris (1951a). I regard Chomsky's denial of any such influence by Bloomfield as another example of his solipsism, though perhaps a less glaring one than his failure even to mention Harris in this connection.[55]

Whatever it is, it appears to me that Chomsky is at least doing what most of us do, and more often than not unconsciously, namely, to reinterpret our own past as we grow older, while at the same time our memory of this past has become much less reliable than we may believe it to be. Reading Chomsky's accounts of his own intellectual development—which is always given as a statement of fact—one is left with the impression that he actually believes what he's saying to be true at the time he is saying it. (He also appears to be too busy to check what he said about the same subject on previous occasions.)

For the, admittedly, tremendous success story of the 'Chomskyan Paradigm', when the history of 20th-century is being written, Chomsky's own accounts of its development will probably not count for much. The historiographer will rely on what has actually been written by the various participants in the undertaking and not how the discussions are remembered and often misremembered by the central figure in the story. There can be no doubt that whatever training Chomsky got and how much attention his teachers gave or did not give to his early work, one will have to admit that on the basis of what he learned (at home, at college, and from others, elders or peers), Chomsky—largely single-handedly—developed a research program that many young men and women found attractive, notably during the 1960s and 1970s. That this program has turned out to be truly structuralist in conception—more like the epitome of what his mentors did not manage to carry out[56]—and very

[55] For the more recent period of Chomskyan linguistics, a catalogue of instances where Chomsky's as well as Halle's [and his co-author's] claims clash with well-documented facts has been provided in Pullum (1996, pp. 139–144 *passim*). Indeed, Chomsky is accused as being "disingenuous" (p. 142) and commenting on Halle's and Alec Marantz' (PhD, MIT, 1981) joint contribution, Pullum (p. 144) notes: "Halle and Marantz argue not like scientists interested in theory improvement but like crusaders defending a faith against a minor heresy".

[56] Encrevé (1997, p. 108) has argued "Le 'génie' du jeune Chomsky, entre 1947 et 1951, a été de la [i.e., the idea of ordered rules] reprendre, à sa manière, la mettant au service de *simplicité* (... [Chomsky, 1951, p. 4]), et ce à l'encontre de son directeur de thèse [i.e., Harris]", who had not ignored, but abandoned this idea. That this reading of Harris cannot be sustained may be gathered from Nevin (1993) where the author not only gives some references regarding simplicity in Harris, but also shows that the issue of sequence derivational steps is manifest in all the transformational work and in Operator Grammar.

Bloomfieldian in outlook, may be regarded as the irony of his career, and more often than not a regular occurrence in history as in human life: after all these efforts of our adolescence and early adulthood to set us off from our fathers, we end up being very much like them.[57]

4. Some observations on the history of 'generative linguistics'

In his 1997 article, which has provided me with much of the impetus to revisit aspects of the history of Chomskyan linguistics, Pierre Encrevé documented that while for a certain time the work that culminated in Chomsky and Halle's *The Sound Pattern of English* of 1968 (but which had been circulating in drafts since 1964 or 1965) succeeded in stamping out knowledge of all the previous advances in phonology and redefined the *points d'appui* of research in this area, dissatisfaction with many of its proposals has led younger generations of 'generative' phonologists to revisit earlier concepts (such as 'phoneme', 'syllable', 'long component', suprasegmental features generally etc.) and research, including at least part of what so-called (Post-)Bloomfieldians had produced (Encrevé, 1997, pp. 114–117, 118–120). I am certain that I was not the only one who was upset when Sanford A. Schane (b.1937), a former student of Halle's, 'reinvented' the phoneme—so dear to American structuralists—in 1971, after all the ridicule that had been heaped on anyone who still worked with the concept during the 1960s (cf. Chomsky and Halle's savage attack on Fred W. Householder in 1965 as just one such example).[58]

Although he was not entirely alone in this, Encrevé rightly, I think, singles out the work of another former student of Halle's, John A. Goldsmith (b.1951), who has done much to go back to pre-*SPE* work in phonology, and to recover work that Chomsky and Halle had all but buried for good. We may begin with Goldsmith's (1976) MIT dissertation *Autosegmental Phonology*, published, ironically enough, in the same year and in the same series that reproduced Chomsky's MA thesis. In it, Goldsmith recognized the significance of Harris' (1941a) "Linguistic Structure of Hebrew" article with regard to the idea of 'long components'. However, a much more significant event occurred, when Goldsmith published his book *Autosegmental and Metric Phonology* in 1990. In it, he referred to several early works by Charles F. Hockett, who is rightly seen as Bloomfield's executor (see Bloomfield, 1957, 1962, 1970), notably Hockett's 1955 *Manual of Phonology*, whose importance Chomsky must have recognized when he tried

[57] John Lyons (b.1932), who in 1970 published a commercially successful textbook on Chomsky and his linguistics, noted ten years later that "there is far less difference between Bloomfield's and Chomsky's views of the nature and scope of linguistics than one might expect" (Lyons, 1981, p. 23).

[58] There are many such 'rediscoveries' of the 1970s, including Halle's outrageous claim that very little research had been done in morphology until he had 'opened up the field'. For a contemporary reaction to Halle's claims, see Lipka (1975). An earlier, shorter version of this discussion piece, submitted in 1973 to the journal *Linguistic Inquiry*, was rejected essentially with the following argument provided by one of the referees: "This appears to be inappropriate as a squib since it is indirectly an attack on the signifiance of Halle's work." It seems that this has been one of the ways in which certain people have been protected from public criticism.

so hard in his review (Chomsky, 1957b) to minimize its accomplishments. Referring to it and other writings of Hockett from the 1940s, Goldsmith (1990, p. 4) notes:

> 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 [...], 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, 1951a[1947]). The epitath in chapter 1 [on "Autosegmental Representation"] below gives the reader the sense of the identity of Hockett's concerns and my own.[59]

There are other important observations about Hockett's pioneering phonological thinking which Goldsmith recognizes in his book (cf. Encrevé, 1997, pp. 119–120).[60] Indeed, the 1990s have seen a considerable diversity of theoretical positions in phonology (cf. Goldsmith, 1995), something of a liberation from the *SPE* straight jacket, and, as a result, pre-*SPE* work is being revisited. Given his rather broad scholarly background, it may not be surprising that Clements (2000[1993]) not only provides a careful re-evaluatation Hockett's phonological ideas (pp. 37–42) but also of the work of John Rupert Firth (1890–1960) on prosodic analysis (pp. 42–47). Back in 1964 the latter did not fare so well in the first dissertation Chomsky ever supervised (see Langendoen [1968, pp. 50–61], for a more measured version; see also Chomsky's [1964, p. 29] remark), especially if compared to what Goldsmith (1992, pp. 151–161) has to say about Firth's contribution.[61]

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[59] On page 8 of his book Goldsmith cites a lengthy passage from Hockett (1947b) in which Hockett gives his reasons for "removing the linearity assumption from among [his] working principles".

[60] Apart from a prominent acknowledgement (p. viii) and a listing of four publications in the bibliography (p. 364), there are references to Hockett on the following pages: pp. 3–4 (an almost page-long quote from Hockett, 1955, p. 155), p. 108, p. 275, p. 276, p. 298, pp. 336–338 notes 1, 7, and 12, p. 341, note 42, p. 353, note 8. It should be added that Harris, too, receives comparable recognition for his pioneering work (Harris, 1944, 1951a[1947]).

[61] In 1976, as a doctoral candidate at MIT, with Morris Halle as thesis director, any such proposition would have been quite unthinkable, as we may gather from what Goldsmith wrote in a letter to Encrevé in 1999:

> 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). (Encrevé, 2000, p. 81, note 39)

However, Hockett deserves to be recognized not only as a superb phonologist,[62] but also as a general linguist, indeed as a 'mentalist' that he, like the other 'Post-Bloomfieldians', was not supposed to be according to the relentless attacks on their work by Chomsky and his associates during the 1960s. In order to illustrate this latter point, I would like, with reference to Chomsky's approval of Hockett's 4-page statement of 1950, "Which Approach in Linguistics is 'Scientific'?", in his 1951 thesis,[63] quote from this as well as an even earlier paper by Hockett. Chomsky's admission, some ten years after Hockett had taken public, indeed vociferous, distance from everything that Chomsky stood for, that Hockett's work was unlike the writings of those ('Bloomfieldian') linguistics with whom he disagreed is equally worth mentioning. I am referring to Chomsky's 1976 conversations with Mitsou Ronat. When the subject was the "so-called psychological reality" of discovery procedures, Chomsky noted that, unlike his peers at the time, Hockett "put forth an explicity 'realistic' interpretation of discovery procedures in an important article in 1948, in the *International Journal of American Linguistics*" (Chomsky, 1979, p. 115). On page 118 of the same book, Chomsky again explicitly excludes Hockett from his criticism of Harris, Bloch "and others" who rejected "the realist psychological interpretation" in phonology.[64] There are two more references to Hockett's work in these conversations, when the subject of mathematical theory of communication (p. 125) and the idea of

[62] See now also Clements (2000, pp. 37–42), for an appraisal of Hockett's contribution to 'nonlinear phonology', notably in his "precocious and innovative paper" of 1942 and "his next major presentation of a feature-based model of phonemic analysis" (Hockett, 1947b).

[63] When talking about "setting up such linguistic elements as morphemes (a process of discovery) we must consider properties of the linguistic elements themselves (e.g., perhaps minimization of their number) and properties of the statements describing these elements and their relationships (e.g., perhaps minimization of their number)" (Chomsky, 1951, p. 2), he refers to "Harris (1951a), chapter 12, and Hockett (1950)" (p. 67, note 4).

[64] Apart from Sapir (1933), who was often celebrated as the hero in the generativist debate during the 1960s (cf. Chomsky and Halle, 1968, pp. 344–349, where Sapir's analysis of Southern Paiute is presented and discussed at length as a 'classic example' of "rule ordering of both a sequential and a simultaneous kind" [p. 344]), whereas Bloomfield was villified for his alleged anti-mentalism, etc. (and in the same book his 1939 paper is cited only as if Bloomfield [p. 106] had indeed not realized that "in synchronic grammars one finds numerous rules that cannot be traced directly to any sound change" [*SPE*, p. 251]). Interestingly enough, D. Terence Langendoen (b.1939), Chomsky's first official doctoral student (PhD, 1964), had this to say on the matter some ten years later:

> There is a certain irony in the fact that Halle and Chomsky (Halle, 1959, p. 14; Chomsky and Halle, 1968, p. 78) have singled out Sapir's phonological work as foreshadowing their own, since Bloomfields paper 'Menomini morphophonemica' (1939), in my opinion, surpasses anything Sapir ever wrote in phonology, and in its explicitness and rigor practically achieves the status of a *notational variant* of generative phonology [references to Bever, 1963, 1967]. (Langendoen, 1979, pp. 145–146; emphasis added: EFKK)

In his 'surrejoinder' to E. M. Uhlenbeck, Langendoen (p. 157) further adds: "I do not believe that Bloomfield's adoption of the radical behaviorism of [Albert Paul] Weiss [(1879–1931)] seriously affected the development of American linguistics. In fact, I do not believe that radical behaviorism affected even Bloomfield's work". Indeed, none of Bloomfield's followers paid any attention to this (or any other brand of) behaviorism in their positive work; cf. Harris' (1973, p. 255) affirmation:

'deep structure'' (p. 169) are discussed, but these are less important in the present context.

What I found most revealing in Chomsky's references to Hockett's work are those to his 1948 ''A Note on 'Structure'" (mentioned above) and his 1950 short ''Which Approach in Linguistics is 'Scientific'?'' (listed among the 5 entries in Chomky's (1951) Master's thesis [p. 74], and referred to with approval in note 4 [p. 67], when some methodological issues are discussed). I find these two references revealing for several reasons, not just because they are omitted in the truncated bibliography to *LSLT* in the 1975 edition (cf. p. 572),[65] which only include four other early writings of Hockett's (1947a, 1952, 1954, 1955), but because I regard them as important observations on matters of linguistic theory and philosophy of science that surely were of importance to the young Chomsky during the late 1940s and much of the 1950s, if not also the 1960s, when the topic of 'mentalism' became one of the main themes in the critique of 'Bloomfieldian' linguistics.[66]

Many years ago, I quoted from these two papers to counter Newmeyer's (1980, p. 37) argument that Hockett's (1954) ''Two Models'' paper (together with Harris's ''Transfer Grammar'' of the same year) 'uncharacteristically' contained the seeds of generative grammar (Koerner, 1989, p. 128).[67] This time, I do so in order to suggest how much Hockett was ahead of his time; without any doubt, was a very important source of Chomsky's linguistic inspiration—in addition to Harris' work, notably his

''Bloomfield's espousal of a particular current school of Behaviorist psychology as an interpretation of linguistics was arbitrary and not supportable. This was clear to almost everyone from the start, and neither of his students nor his readers took it up. (Nor did it lower his value or his standing in linguistics).''

[65] Similarly in *SPE*, only Hockett (1955) and his 1965 paper on ''Sound Change'' in *Language* 41:2 (1965), pp. 185–204 are listed (Chomsky and Halle, 1968, p. 439). The former is mentioned (p. 413) with reference to Hockett's (1955, p. 119) list of American Indian languages that lack nasals; the latter receives a positive mention because Hockett, ''unlike many writers on the subject, [...] explicitly recognizes the role that rules of the grammar play in determining the physical shape of utterances'' (p. 256).

[66] In 1976, Chomsky explains the complete absence of a cognitive psychology approach in *LSLT* ''because it seemed to [him] too audacious and premature'' at the time (Chomsky, 1979, p. 114), something which must strain credulity among those who have observed him in public debate ever since the mid-1950s.

[67] This claim that these two linguists were not aware of the significance of their thinking is, I submit, little else but a rhetorical trick, when claims to 'originality' are advanced. Other such examples can be found in Chomsky's work (e.g., 1975, p. 51, note 45) and more explcitly in Barsky (1997, p. 55) according to which Chomsky regarded Bloomfield's Menonimi paper of 1939 as so ''completely inconsistent with Bloomfield's other writings'' that he ''believes'' that this was ''one of the reasons Bloomfield decided to publish it in Europe.'' In his 1996 São Paulo lecture on ''Knowledge of History and Theory Construction in Modern Linguistics'', Chomsky went further and used the term 'schizophrenia' twice to describe the distance between Bloomfield's (1933) book and his 1939 article (Chomsky, 1997, p. 108). Cf. also Halle's claim (e.g., Bromberger and Halle, 1989, p. 66, p. 68, note 16) that Bloomfield's Menonimi paper of 1939 was atypical for Bloomfield and therefore ignored by his followers. Similarly, Newmeyer (1996, p. 14) says this about Jakobson (1948): ''The most noteworthy feature of 'Russian conjugation,' from the point of linguistic historiography, is its 'un-Jakobsonianness'.'' The 1986 edition of Newmeyer's 1980 no longer contains this argument concerning Hockett's and Harris' 'untypical' 1954 papers (and a variety of other spurious claims), subsequent to his reading of Murray (1980) and Koerner (1983).

Methods, which remained the most frequently cited publication even in the 1975 edition of *LSLT*.

Outlining the "task of the structural linguist, as a scientist", Hockett emphasized that the linguist must go much beyond classification and the mere accounting for all the utterances which comprise the corpus of a language at hand. Indeed,

> [...] the analysis of the linguistic scientist is to be of such a nature that the linguist can account also for utterances that are not in his corpus at a given time. That is, as a result of his examination he must be able to predict what OTHER utterances the speaker of the language might produce [...] (Hockett, 1948b, p. 269; emphasis in the original)

And as if to anticipate the basic ideas of Chomsky's much later talk about 'mentalism', 'cognitive psychology', and the 'language acquisition device', Hockett continues his argument in the next paragraph with the following observation (pp. 269–270):

> The analytical process thus parallels what goes on in the nervous system of a language learner, particularly, perhaps, that of a child learning his first language. The child hears, and eventually produces, various utterances. Sooner or later, the child produces utterances he has not previously heard from someone else.

However, the essential difference between the child's acquisition of the language and the linguist's procedure lies in the fact that

> [...] the linguist has to make his analysis overtly, in communicable form, in the shape of a set of statements which can be understood by any properly trained person, who in turn can predict utterances not yet observed with the same degree of accuracy as can the original analyst. The child's 'analysis' consists, on the other hand, of a mass of various synaptic potentials in his nervous system. The child in time comes to BEHAVE the language; the linguist must come to state it.

In the final analysis, a 'linguist as scientist' must "determine the structure actually created by the speakers of the language", not impose one, for "a language is what it is, it has the structure it has, whether studied and analyzed by the linguist or not" (pp. 270–271). Hockett, who had studied both Chinese (cf., e.g., Hockett, 1950b) and several Amerindian languages (cf., e.g., Hockett, 1948a, c), certainly knew what he was talking about.

Since Chomsky in 1951 makes explicit mention of Hockett's note on "Which Approach in Linguistics is 'Scientific'?", one final quote would not be out of place here. Referring to what he believed to be the unquestionable promise of 'immediate constituent' analysis (cf. Wells' 1947 outline), Hockett observed that it is "not an analytical technique, but a hypothesis about the nature of talking and hearing language", conceding at the same time:

> The problem is to develop techniques by which the hierarchical structure of the utterances of a language can be revealed and stated. A child learning to speak has such a technique; our objective techniques are as yet quite faulty, but at least they are good enough to reveal this very important feature of linguistic structure. (Hockett, 1950a, p. 56).

5. Coda

Looking back at the development of American linguistics of the past fifty years, one may wonder how far the field has really progressed, how many true insights into the nature of language have been gained since the 1950s. Opinions on this may differ, but it should have become obvious from the present incursion into the history of morphophonemics that there were many important pre-1951 findings in this area. A central challenge to the writer of the history of any scientific discipline lies in reconciling its practitioners' rhetorical claims of revolutionary turns and paradigmatic incommensurabilities with evidence that, in hindsight, suggests more continuity and cumulative advance (or in some cases even regression) at the substantive levels of methodology and genuine understanding. The present study suggests how the collective forgetting of certain works, possibly as a result of their having been ahead of their time, but more likely by other disciplinary, institutional and social or personal factors that drove the rhetoric of revolution, has brought about a distortion of history that is in all of our interests to rectify.

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