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More Concerning the Roots of Transformational Generative Grammar

Bruce E. Nevin
Cisco Systems, Inc.

1. *Introduction*

I read with interest Pieter Seuren's (b. 1934) review article on *Linguistics and the Formal Sciences: The origins of Generative Grammar* by Marcus Tomalin (Seuren 2009). Altogether Seuren's critique is informative and useful, and his discussion of how Tomalin might have done a better job is constructive and will hopefully suggest further work in this area. However, there are also aspects of Seuren's critique that call for more careful investigation.

The claim that Chomsky's (b. 1928) celebrated 1959 review of B. F. Skinner's (1904–1990) proposals about language (Skinner 1957) is "based to an overwhelming extent" on Karl S. Lashley's (1890–1958) influential talk given at the 1948 Hixon Symposium (Lashley 1951) requires substantiation. It cannot be based solely on testimony of Gardner's (b. 1943) survey of the history of cognitive psychology (Gardner 1985). Those wishing to pursue this will find that Gardner's discussion of Lashley's 1948 critique of behaviorism is on pp. 10–14 or perhaps –16. Seuren cites pages 28–29, but that is where Gardner discusses the Symposium on Information Theory, held at MIT in 1956, and depicts Chomsky's alliance with the then Young Turks of psychology.[1]

I was glad to see some discussion, however brief, of Emil Post (1897–1954), the actual though largely unacknowledged inventor (e.g., Post 1943) of the rewrite systems that Chomsky adapted for his formalization of immediate constituent analysis as Phrase Structure Grammar or PSG, and the source of the now familiar,

1. In his talk, which was shortly after published as "Three Models of Language" (Chomsky 1956), Chomsky argued that information theory has no pertinence to linguistics. Fernando Pereira, who unlike some others understands information theory quite well, proposes (Pereira 2002) to heal this spurious rift, following the lead of Harris's theory of linguistic information (Harris 1991).

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and, as it were, trademarked, terms 'generate' and 'generative'. Especially relevant is Seuren's identification (p. 100) of "the algorithmic character of […] generative grammar".[2] As John Goldsmith (b. 1951) says, "Generative grammar is, more than it is anything else, a plea for the case that an insightful theory of language can be based on algorithmic explanation" (Goldsmith 2004: 1). Maurice Gross (1934–2001) had also made this point in "On the Failure of Generative Grammar" (Gross 1979). It is also good to see Seuren acknowledge Harris's (1909–1992) programmatic definition of generative grammars of ('natural') languages in (1951: 369–370), as only a few others have done.[3]

Seuren criticizes Tomalin's lazy reliance on Chomsky's own self-assessments and references, a customary lenience among many writers (Barsky's 1997 hagiography is hardly alone in this), and he even to some degree exposes Chomsky's customary effacement of sources. This is an encouraging sign of autonomous thought. However, he reiterates a number of notions that 'everybody knows' but which can be sustained only by ignoring the relevant literature. We may take as a convenient starting place Seuren's statement, "If anything had contributed to Harris's discovery method and Chomsky's sequel to it, which led directly to TGG, it was behaviorism." Here in one brief, complex non-sequitur we have a promiscuous combination of behaviorism, discovery procedures, the unexplained assumption that one follows from the other, and the notion that Chomsky's TGG was a sequel to Harris's work. Apparently, each canard severally is something that 'everybody knows', requiring no substantiation. Let us consider them in that order.[4]

2. *Behaviorism*

My understanding of Bloomfield's (1887–1949) writings is that he referred to psychology only to put linguistics in a larger context, and the only science of psy-

2. Though Seuren's comment on p. 102 about decidability of a language, e.g., English, is hard to take seriously — how do you prove that negative?

3. Perhaps including even Chomsky, in his introduction to *LSLT* (Chomsky 1975: 11n.16), although, like everything that he has to say about his mentor, this reference is grudging and equivocal. Footnote 16 is on the statement that "The notion 'generative grammar' […] was never clearly developed."

4. There may be yet another assumption here. The notion that Chomsky's line of work followed after Harris's as a "sequel to it" apparently presupposes that Harris's work came to an end thereupon, or faded to insignificance. Seuren actually recognizes the Chomskyan ahistoricity of this 'eclipsing stance' (Hymes & Fought 1975), in the section entitled "Harris buried alive", a cute reference to a striking feature of the literature of generativist linguistics, such that even Hockett (1916–2000) in his *The State of the Art* (1968) wrote that Harris's work had come to an end or "a long silence" after 1957. But that is a story for another place.

chology seriously available at the time was behaviorism. His linguistic work, however, does not depend upon or derive from behaviorism, or from any discipline outside of linguistics. We should not forget that Bloomfield, Sapir (1884–1939), Harris, and others were conscious of establishing a new science on a firm footing, recognition of which necessarily entailed placing it in context of existing sciences and philosophy of science, as then understood. To construe these contextualizing gestures from within linguistics as formative influences from without is to ignore how working linguists actually proceeded (and proceed) with informants and their languages, of which there is ample documentation.

The only thing that I see in Bloomfield's writings resembling a concern about conditioned responses to stimuli — which is after all what behaviorism is about — is his care not to suggest forms or paradigms to the informant, but rather to arrange situations in which forms, including members of paradigms, might naturally be spoken. But the point of this care in elicitation was not to produce 'stimuli' that 'cause' responses (the only sense that I can see in which Bloomfield might have applied behaviorism in his linguistic work).[5] Rather, it is a recognition that all people are suggestible, and that language users have an unconscious capacity to adapt to one another's usage (which is how dialects coalesce) from which the linguist-informant relationship is not immune. This caveat can be phrased in terms of cognitive psychology or indeed in terms of any psychology that recognizes human suggestibility. So, in short, behaviorism was context, not foundation. As Stephen Murray (b. 1950) found, "By the time Bloomfield became a behaviorist in psychology, he was committed to the belief that linguistics was an autonomous science [...]. Thus, one's preference for one psychological theory or another did not matter to the linguistic analysis done" (Murray 1994: 121).

There can be no doubt that the autonomy of linguistics was even more clear for Harris. It is not merely that he scarcely if ever refers to psychological research or methods (though he was hardly ignorant of these matters).[6] He did not even need it as context, and indeed all his work as a methodologist points most vigorously away from any dependencies on any prior or external notions of language. (In his later writings this is under the heading of 'metalanguage', see Harris [2002: 7–10]

5. That this notion of linear causation of behavior by the environment is an absurdity, and the astonishment that we should feel at how long it was seriously entertained, is a subject for another place. In brief, behaviorism offers an account of the coercive induction of goals or purposes, something which as a matter of principled commitment it could not recognize as either fact or theoretical construct.

6. For example: "[E]xperimental work in the psychology of perception, especially that due to Gestalt psychologists, leaves little doubt that an utterance is perceived not as an independent structure but in its relation to other utterances" (Harris 1951: 273n.27).

for an overview.) To say that "Chomsky was educated, by Harris and others, as a behaviorist" (p. 105) is mere parroting of a gross distortion first promulgated in the polemics of the 1960s. As I have reported elsewhere, Harris once told me "I don't even *like* the term 'behavior'." It is known that he had an interest in the political psychology of Erich Fromm (1900–1980), hardly a behaviorist. And again, so far as anyone in linguistics was concerned with psychology, it was as a peripheral matter, to explicate language use in context of the interrelation of personality and culture, without the 'language and mind' assumptions that came later — assumptions which could scarcely be stated scientifically in the 1940s and 1950s. As Seuren himself points out, psychology did not gain prominence as a factor within linguistics until the cognitive psychology rebellion which enabled those speculations. This ahistorical collapsing together of the interests of the two periods suggests that perhaps Seuren has fallen under the spell of Chomsky's inveterate rewriting of history.

Perhaps this explains "Tomalin's silence about […] Chomsky's conversion to mentalism" (Seuren, p. 105), since according to at least some versions of Chomsky's self-presentation there was no conversion, it was there from the beginning. Given Seuren's misunderstanding that Chomsky was trained as a behaviorist by Harris, this 'conversion' is supposedly from behaviorism to mentalism. But I know of no evidence that Chomsky paid any particular attention to theories or methods of psychology before his becoming connected in Cambridge with George A. Miller (b. 1920), Jerome Bruner (b. 1915), and others, long after he had left Penn. What he calls a 'psychological consideration' in his Master's Thesis (Chomsky 1951) is no more than the observation that in an 'analytic grammar' (which he would later call a generative grammar, that is, one going from more general elements to more particular by potentially recursive steps) the context of an element is introduced at the same time as the element itself.

> Thus one cannot really understand what a noun phrase is except in the contextual frame provided by other phrases, or until it is presented against the background of the sentence of which it is a part. But this is what has been called an analytic process. This consideration seems to have some relevance, at least on the higher levels of analysis, but, generally speaking, I do not know how much (if any) weight is to be attributed to it. (Chomsky 1951, ms. p. 8)[7]

In all of this, I cannot find any trace of operant conditioning, or stimulus-response contingencies, or any other attribute of behaviorism, nor is there a hint of such matters in *The Logical Structure of Linguistic Theory* (*LSLT*, Chomsky 1975), nor

7. All page references to Chomsky (1951) are to the manuscript, and not to the extensively revised published version (Chomsky 1979). This passage appears at the end of Section 5.

indeed in any other of Chomsky's early writings, much less in Harris's writings of any date. But in the literature of linguistics, it seems that behaviorism, empiricism, logical positivism, and the like, employed as supposed attributes of 'structuralism', have become reduced to mere pejorative labels divorced from any substantive content of the respective fields to which they refer (or to which they originally referred), and here it appears that Seuren is merely joining the others in uncritically wafting these labels about. Rather than a 'conversion' from behaviorism, then, what we see subsequent to Chomsky's engagement with nascent cognitive psychology is a continuation of his quest for a language-discovery algorithm, only now projected onto the black box of child language acquisition as an innate 'language organ'.

3. *Discovery procedures*

Chomsky (1951) refers to distributional analysis (by which he says that the elements and their combinations are identified) as a 'process of discovery'. The algorithmic bent of Generative Grammar, noted earlier, is his core interest, evident from Chomsky (1951) onward. Chomsky acknowledges (1975: 30, 33) that it was he who sought mechanical discovery procedures, not Harris, and expresses his frustration that Harris's methods did not provide them. Anything not performable by a computer running a program, in effect, Chomsky judges to be 'vague' and 'uninteresting'. In every place that I have examined where he complains about Harris's 'vagueness' or the like, it is evidently because Harris's methods cannot be reduced to an algorithm for discovery that has no need for human insight. And when Chomsky gave up on automated linguistic analysis, then since it was supposed (following Piaget) that the cognitive capacities of adult linguists cannot be ascribed to infants and children, he assigned the role of human insight to an innate language organ in children, and to linguistic intuitions in adults, the latter arising out of the former by the process of language acquisition (as distinct from language learning). Since then, of course, Piaget's views have been superseded by demonstrations of the remarkable cognitive capacities of infants, even *in utero*, and by developments in statistical learning theory.

Harris, on the other hand, was not concerned with algorithms except as they might serve as a demonstration or proof of concept. (One of the rare examples of this is Harris 1967.) As he pointed out in the 1986 Bampton lectures which became *Language and Information* (Harris 1988), there are two kinds of applied mathematics: calculational, of which there is very little in language, and the finding of mathematical objects in the world, of which there are many in language. Finding out how people process language (or how computer algorithms might, which is not the same thing) is of interest to him, of course, but a prerequisite for that is finding out the essential nature of language, its necessary properties as a mathematical object. How could the former sensibly be done prior to? the latter

He acknowledged the diversity of heuristics, hunches, criteria, and distributional methods that linguists use to wrestle their data into a coherent and useful description; the purpose of his 'methods' was not to replace these practical strategies, but rather to verify that findings, however reached, retain a valid relation to the fundamental data of native speaker judgments.[8] This grounding in primary data is in contrast to Chomsky's framing of validation procedures in terms of adjudicating among a purported huge number of alternative grammars, except that linguists in practice have already followed their informal criteria to converge on one or a few alternatives, so that there is no 'huge number' from which to choose. It is brute force computer algorithms that generate many ramifying alternatives, reasonable or not, for logical adjudication. But as Harris said in a letter of 6 February 1991 to André Lentin (b. 1931):

> I thought of my own attempts as being constructivist more than specifically intuitionist, because the reality and testability of the ultimate elements did not seem to me to be an issue in language (even if the ultimate elements are phonemic <u>distinctions</u>). But I do think that tertium non datur is untenable in any man-made or finite situation — other descriptions are always possible there. Indeed a major mistake in scientific articles is setting up an 'alternative' and proving X from non-Y.

Seuren refers (at the end of Section 2 on p. 99) to Chomsky's conversion from discovery to evaluation procedures. According to Chomsky's account in the 1975 Introduction to *LSLT*, prior to 1953 he worked on "the problem of revising and extending procedures of analysis so as to overcome difficulties that arose when they were strictly applied [as an algorithm of discovery]. While working on 'discovery procedures' for linguistics, I was troubled by a number of nagging doubts" (Chomsky 1975: 30). "By 1953, I had abandoned any hope of formulating taxonomic 'discovery procedures'…" (ibid., p. 33).[9] It was therefore not so much a conversion as

8. These are of two kinds. Judgments of contrast are based on substitutability, a distributional criterion, and Harris's criterion for transformations was the preservation of relative acceptability from one subset of sentences to another (where each is the set of satisfiers of a sentence form, that is, a sequence of form-class variables and constants).

9. There are a number of mentions of 'discovery procedures' in *LSLT* (setting aside those in the 1975 introduction, to avoid anachronism; and I have not verified the 1975 revision against the several ms. versions). Three refer to "the problem of choosing among a vast number of different grammars, each giving a different structure, and all meeting these vague and incomplete external criteria" — in other words, having given up on discovery of language structure, he at that stage takes for granted a population of diverse putative structures, and his aim is to 'discover' which is correct (Chomsky 1975: 80, 116, 177n.13). At the other places where he mentions the term, he inveighs against the notion of discovering language structures, rejecting what he says are the aims of Harris and others. On pp. 139 and 149n.21, he rejects the feasibility of such

the abandonment of a youthful project which he found intractable.[10]

In this connection, consider Seuren's footnote 5 on p. 9:

> Harris's indiscriminate use of the terms sentence and utterance should be seen in the context of his positivist behaviourism: sentences are, after all, abstractions made by linguists, whereas utterances form observable data.

Seuren is evidently unfamiliar with Harris's 1946 definition distinguishing the two terms *utterance* and *sentence* (Harris 1951: 14) or his discussion of utterance structure in terms of morphemic constructions (Harris 1951: 327, 330, 333, 372). The notion that Harris uses these two terms indiscriminately cannot have been arrived at by actual examination of what Harris wrote. Or it may be that these passages were rendered invisible or unintelligible to him by a prior assumption that the relationship of sentences to utterances is that of formal description to raw data. Assumptions can have that effect. It is true that sentences cannot be formally distinguished within the set of utterances until the linguistic analysis has been done, so to that extent, yes, utterances are 'observable data' for that analysis. Sentences are a subset of utterances, formally distinguishable as such, but utterances that are not sentences also have a formal description. Nor are such non-sentential structures limited to sentence fragments. At the very least, one would think that Seuren might see that his subsequent mention (p. 108) of entire texts or discourses as the basis for Harris 'coming upon' transformations (in the late 1930s) has some pertinence to the formal structure of utterances.

discovery procedures. At 160n.3 and 161n.9 he suggests that phonological bi-uniqueness and complementary distribution are required only if you are trying to "construct a discovery procedure for grammars" (a position from which he had somewhat retreated in Chomsky 1964). On p. 171, he suggests (equivocally, without quite explicitly saying) that immediate constituent (IC) analysis as described by Wells and by Harris (not distinguishing these) is a "practical procedure for discovering the constituents of a language." Methodologically, IC analysis depends upon native speaker judgements of successive bisections into paired constituents, whereas Harris's morpheme-to-utterance grammar depends upon the distribution of parts of sentences as compared to the distribution of those same parts plus 'expansions', as verified by substitution tests. In this, it is much closer to string analysis, and as Harris (2002) notes, transformations were evident in these relations from the outset. See Harris (2002: 3) for the recognition of transformations in the 'grammar of expansions' published as (Harris 1946), and the distinction between the latter and immediate constituent analysis (and *a fortiori* PSG).

10. As Harris had no doubt expected it to be, but would not have discouraged Chomsky from it. Experiences such as the following inform this ascription: I once proposed to Harris a project of analyzing dictionary definitions by a sort of componential analysis to extract semantic primitives, something not particularly congenial to his work. He said, "Many have tried this and failed, but you are welcome to try." On another occasion, I asked him if he thought string analysis could be done with informants, and he said no, one would have to know the language.

To look at it another way, 'grammaticality' is not limited to sentences, nor are sentence fragments merely a matter of 'performance' as a kind of degenerative phenotype of 'competence'. This is borne out by great successes of the Linguistic String Program (LSP) and the more advanced medical language processing (MLP) system[11] based in Harris's work by Naomi Sager (b. 1927) and her colleagues in the Courant Institute of Mathematical Sciences at New York University. These computer systems recognize and parse even fragmentary notes jotted by physicians and nurses, incorporating them into a structured database of linguistic information objects, an early demonstration of Harris's theory of linguistic information. Harris knew well that a grammar of sentences was only a part of the formal structure of language; for Chomsky, it is the whole picture, "all and only the sentences of the language" in the oft-repeated logicians' phrase. Nor does it follow that the results of linguistic analysis are necessarily abstractions. (I use the word 'necessarily' advisedly, because clearly for Generative linguistics the results of analysis are very abstract.) That, however, is another topic.

4. *Behaviorism*

We next come to the supposition that discovery procedures follow from behaviorism. What possible connection is there between (a) the notion that the pattern in a language is discoverable by analysis of utterances, and (b) the particular intellectual apparatus of behaviorism such as operant conditioning, stimulus-response contingencies, independent and dependent variables, and statistical analysis of discrete 'behaviors'? This is no more than a perfunctory reference to a familiar caricature called 'Behaviorism'.

The purported connection appears to be by way of another familiar whipping boy, empiricism, the view that we gain our knowledge from experience. But behaviorism does not necessarily follow from empiricism, not even from the most extreme expression of empiricism as logical positivism. Logical positivism rejects as nonsense any proposition unless it either can be verified empirically (synthetic, *a postiori* propositions) or is derived from logic (analytic, *a priori* tautologies). Its abandonment, or anyway general decline, is largely due to the difficulty of making 'observation statements' (the former) that are free from theoretical presuppositions (the latter). Here, then, we must recognize that Harris makes no claim of theory-free observation statements. Instead, he articulates a least theory, as it were, requisite for doing linguistics. And behaviorism is no part of it.

The discovery procedure canard has been debunked elsewhere, e.g., Hymes & Fought (1975), Murray (1994), Nevin (1991). The discovery assumption appears elsewhere in Seuren's article, e.g., "the levelwise induction or discovery process

11. Found at http://mlp-xml.sourceforge.net/, Sager (1985), Sager & Ngô (2002).

from sounds to phonemes to morphemes to constructions (phrases) to the highest level of sentences" (p. 8). We'll return to this, and to Positivism, in the next section.

5. ***Positivism, anti-mentalism, and the avoidance of meaning***

Moving on now to other topics, beyond those encapsulated in that brief, pungent quotation with which I began this critique of Seuren's apparent reliance on things that 'everyone knows', we should acknowledge that the positivist or logical empiricist label applies better to those who demanded that linguistic elements be constructed from the bottom up, starting from distributional analysis of phonetic observables, with no 'mixing of levels'. I have written (Nevin 1999) about the radical divergence of Harris's phonology from, e.g., Bloch's (1907–1965) layer-cake postulates (Bloch 1948), a departure made possible for Harris because he recognized that the primary data of linguistics are not the physical data of phonetic observables, but the psychological/cultural data of speakers' judgments of contrast vs. repetition. The contrasts are associated with phonetic attributes by the very act of substitution tests which establish those judgments (ideally, but not necessarily, the pair test). Thereafter, on distributional and other grounds, the systematic labeling of the contrasts with phonetic data can be carried out in alternative ways, different arrangements being freely tried and evaluated as to their usefulness for different purposes, so long as the contrasts are kept distinct and consistently represented, and the relevant phonetic facts are recoverable from that representation (so-called biuniqueness). Seuren, however, seems to accept the commonplace inference from Chomsky's (1964).[12] that Harris was also a 'taxonomic phonemicist' tarred with the same "levelwise induction or discovery process from sounds to phonemes" (p. 8, quoted earlier).

Referring to discourse analysis, Seuren says of Harris (p. 108), "being a behaviorist, he could hardly approach this question [of coherence across a discourse] from a semantic point of view." The real issue behind this rather startling non-sequitur is mentalism, which Harris allegedly ruled out in accordance with behaviorism. It is customary to see Harris and Bloomfield alike as 'anti-mentalist', and it is customarily concluded that on this principle they avoided use of meanings in linguistic analysis.

What were Harris's actual motivations? Seuren helpfully distinguishes postulates (as in Bloomfield 1926, Bloch 1948) from axioms and the 'logico-deductive

12. An implication which in correspondence with me he denied making, calling it my misreading. However, he referred me to no passage anywhere in which he might have exempted Harris from the 'taxonomic linguist' category, and I know of none; and he has told me that he feels no responsibility to correct such erroneous inferences when made by his students and followers.

method', but his leap from there to Bloomfield's (and by implication, Harris's) skepticism about meaning is ill founded. Oddly, a bit farther down on p. 108 he gives the real reason for their reservations about the use of meaning as a criterion for linguistic analysis: "In fact, whatever existed in the way of semantics […] did not provide the tools." Such tools still do not exist. Even today, the best that is available is to use one language or language-based logical system as metalanguage for another, and such usage does not amount to any system of semantic primes for a 'semantic representation'. Indeed, the religious faith of some linguists in 'logical form' as a semantics ties them, rather than the structuralists, more closely to logicism in mathematics (though even that is only an analogy), and therefore to one of the roots of logical positivism.

Edward Sapir is frequently cited as a champion of mentalism, apparently because of his essay "The Psychological Reality of the Phoneme" (Sapir 1933). Because Seuren mentions Sapir not at all, there is no convenient place here to mention that he rather than Bloomfield was arguably the more influential on Harris, for his way of working with large amounts of data, his understanding of and ability to marshal distributional relations, his profound sensibility of semantic subtleties and acute awareness of how easy it is to bulldoze them under a rubble of preconceptions, and his emergent notion of configurational grammar. I have mentioned in (Nevin 1991, 2000) and elsewhere that members of Sapir's family have said that he regarded Harris as his intellectual heir. One would think from Seuren's silence (and presumably Tomalin's) that Sapir had nothing to do with the development of formal analysis of language.

6. *The meaning of 'formal'*

Seuren distinguishes a mathematical sense of the word 'formal' (referring to a system) from a linguistic usage (referring to the form of a language as distinct from meaning). The distinction is valid, but is Seuren overstating it? A formal system (the first sense) disposes the elements of a formal 'language' of symbols (the second sense) without reference to what the symbols or the productions of the system might refer to or mean; what they mean is understood to be the interpretation(s) of the 'language' and of the system alike. Indeed, as Seuren himself says (p. 111), "A formal calculus is an algorithmically organized system allowing for derivations to be carried out solely on the strength of the symbols used and their arrangement with regard to each other." In other words, a calculus that is formal in the mathematical sense generates an 'object language' which is necessarily itself 'formal' in the linguistic sense. He says as much, but fails to connect his own dots, and inexplicably (to me) continues to ascribe the formal treatment of language to positivism and behaviorism. Again, perhaps this reflects the power of the established pseudohistorical narrative which has him in its thrall.

The connection of form to meaning is called information, and is directly related to redundancy. The passage about 'levelwise induction' that I quoted above occurs in Seuren's explication on p. 107 of his quotation from the summary final chapter of Harris (1951), where Harris talks about making elements as freely combinable as possible by defining higher-level elements in terms of lower-level elements and putting distributional restrictions into the definitions. The 'idiosyncratic jargon' of which Seuren complains is not so idiosyncratic or peculiar to a mathematician or to one familiar with information theory, and Seuren's attempted paraphrase is wide of the mark. Harris's discussion in the context and sequel to that quote is entirely about phonemic and morphemic elements, but it does clearly apply as well to word combinations in constructions, etc., as Seuren says. However, Seuren entirely overlooks the connection of redundancy to information (perhaps accepting Chomsky's fiat regarding the supposed irrelevance of information theory to linguistics). Harris's aim of achieving a representation of the information in utterances was not so clearly stated in 1946, when his first book was completed, as it is in his later writings, but it is implicit in distributionalism, and is explicit and clear e.g. Harris (1988, 1991, 2002). A statement encompassing all phases of Harris's oeuvre is given in Harris (2002), his retrospective essay that served as an introduction to (Nevin 2002a),[13] but this has apparently escaped Seuren's notice, although we know he has a copy since (Seuren 2002) also appears in that same volume. This oversight is a pity, because of its relevance to meaning and semantics, and to the unavailability of any semantic theory, representation of meanings, or other metalinguistic resources proposed to be external to language and either logically or empirically prior to linguistic analysis.

7. *Transformations, 'rules of transformation', and morphophonemics*

Seuren conflates morphophonemics with transformations, even making the unusual claim that "Bloomfield, in his *Language* of 1933, also had 'transformations'" (p. 108) because he reconstructed base forms from which he derived alternants by morphophonemic rules. Neither Harris nor Chomsky would call such derivations transformational. The origin of this peculiar confusion perhaps becomes clear at the bottom of p. 109, where Seuren says that Harris, "with Chomsky following," borrowed the notion of transformation from "the mathematical-logic literature … [b]ut instead of treating transformation as a logical entailment relation […] they transformed transformations into procedures ensuring syntactic

13. Also in the last paper written by Maurice Gross before his untimely death, also in that volume (Gross 2002), not to mention the Foreword to that volume (Nevin 2002b). See also Nevin (1991), and of course numerous other publications by Harris.

well-formedness." This cute locution, 'transformed transformations',[14] and the purported origination in mathematical logic, may be apt for Chomsky, but is quite mistaken in regard to Harris.

Seuren is here referring to Carnap's (1891–1970) notion of 'rule of transformation' (Carnap 1934). It is clear that Chomsky, in his 1951 *Morphophonemics of Modern Hebrew*, assumed Carnap's usage as the fundamental meaning of the term:

> The statements have the form of rules of transformation. Given a sequence of a certain shape, they direct you to alter the shape in a specified way. If the directions are followed, any sequence of morphemes, properly selected from M and U, will be transformed step by step into a sequence of phonemes. (Chomsky 1951: 22)

> Here it will be shown how sequences of morphemes (of [words in the word classes] M and U, the basic words) are transformed by the morphophonemic statement into their constituent phonemes. Adjunction of this set of examples to the previous pair gives a complete exemplification of the transformation of all possible sentences into phonemic sequences. (Chomsky 1951: 59)

It need hardly be asserted that *Morphophonemics of Modern Hebrew* (Chomsky 1951) is a nontransformational grammar, but if there were any doubt Chomsky himself calls it "a (nontransformational) generative grammar of Hebrew" in the 1975 Introduction to *LSLT* (Chomsky 1975: 37). The basic program is given in this rather optimistic statement:

> [B]eginning with the sentence as the fundamental unit, the grammatical statements will progresssively transform it into its more and more simple constituents, until all sentences of Modern Hebrew, an actual spoken language, are represented in terms of phonetic units. (Chomsky 1951: 3)

There are no syntactic operations here concerning the structure of sentences, only lexical and sublexical rules. The rules in this grammar are 'rules of formation' and 'rules of transformation' in the sense of Carnap (1934), but they are not syntactic transformations.

For Chomsky at this early stage, the distinguishing of sentences in the set of utterances is a given (as indeed it was given in Harris 1941). "The fundamental linguistic fact is the sentence" (p. 9). It has been so ever since in Generativist linguistics. The syntax is presented as

S = ES C ES

14. A similar locution occurs as the title of Lin (2000), which is referenced in Lin (2002) in the same above-referenced volume.

where C is defined by a list of conjunctions and ES ('elementary sentence') is defined in a sequence table such as that given by Harris in *Methods* (19.31 and its appendix). This is clearly a notational equivalent to a set of rewrite rules in a phrase-structure grammar.[15] Tables (rules) presented later for the elements in that table are unabashedly a statement of morphophonemics, as announced in the title.

A salient feature by which it is identified as a *generative* grammar is that these statements are applied in a strictly regulated order. Bloomfield called this 'descriptive order', a usage continued by Harris in (1951) and elsewhere. Bloomfield (1933: 213) introduced the distinction between descriptive order and 'actual sequence' or structural order thus:

> The 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*).

Chomsky later inveighed against this distinction, attributing it to 'anti-mentalism' (Chomsky 1964: 70n.8) and affirming the psychological reality of rule ordering. 'Descriptive order' is unarguably of a different logical type, however, from structural order (rule ordering is 'meta' to the rules that are ordered, which in turn are 'meta' to the 'actual sequences' in the output of the rules), and while structural order is invariant under different analyses (with the well-understood exception of Harrisian long components, which have no relevance for Chomsky), descriptive order may change sharply under different analyses. Bearing witness to this are the enormous differences in the main table of rule ordering given in the published version (Chomsky 1979b) as compared with that in Chomsky's original thesis (Chomsky 1951). The descriptive order of rules was radically changed while still accounting for the same 'actual sequences' of linguistic elements. [16]

Koerner (2002 and elsewhere) questions the credibility of Chomsky's claim that in 1946–1951 he had no knowledge of Bloomfield's use of ordered rules, and in particular that he was unaware of his 1939 "Menomini Morphophonemics". A

15. Plus a system of indices on syntactic categories. Chomsky (1979: 112), in a later insertion into the translation of Chomsky (1977: 123), draws a contrast with "the framework of segmentation and classification that was later constructed, in somewhat different terms, as the theory of phrase structure grammar." The latter proved amenable to Chomsky's Carnapian alternative definition of transformations.

16. There are great differences in style, presentation, notation, organization, and maturity of discourse pervading the entire work, accomplished, we are told, between June and December of 1955.

copy of the Festschrift containing Bloomfield (1939) has evidently been in the University of Pennsylvania library since about 1940 (Koerner 2002: 228n.24). The précis of Bloomfield's treatment of base forms and descriptive order for morphophonemics given in Harris (1951), e.g., at p. 237 for the Menomini, must have come to Chomsky's notice while he was writing what amounts to a restatement of Harris's linguistic analysis of Hebrew.[17]

Contrary to Seuren's assumption, Harris's notion of transformation did not originate with Carnap and the logicians, it is a straightforward application of set theory and linear algebra to the data of language, as he plainly stated in (Harris 2002: 5–6) and elsewhere.[18] The concept of transformation (linear mapping) in abstract algebra applies in a straightforward way to mappings from subset to subset within the set of sentences. This is obvious to a mathematician, if not to the average linguist. The mapping or transformation is established when there is a 1–1 correspondence between the members of the two subsets (e.g., for each sentence of the form N t V A-*ly* there is a corresponding sentence N t V *in an* A *manner*), and a property of any member of one set holds for the corresponding member of the other. The criterial property for transformation is "acceptability or normal discourse-neighborhood" (Harris 1968: 195).[19] Thus *Albert objected mildly* and *Albert objected in a mild manner* are both easily said in a broad range of contexts, but the sayability of *vacuum objected mildly* is limited in the same ways as *vacuum objected in a mild manner*, and so on for other members of the two subsets ("for

17. Barsky (2007: 148) quotes a letter

> that Harris wrote to Bernard Bloch […] on December 19, 1950…: "A student of mine, A. N. Chomsky has been doing a great deal of work in formulation of linguistic procedures and has also done considerable work with [Nelson] Goodman [(1906–1998)] and [Richard Milton] Martin [(1916–1985)]. Last year I [gave] him the morphological and morphophonemic material which I had here …"

Koerner (2002) suggests that Chomsky may well have also relied upon Harris (1939a, b, 1941) for the historical perspective on the morphophonemics of the language. Chomsky had his own familiarity with the data, of course, being steeped in Hebrew studies from an early age, but familiarity and even fluency are not the same thing as cogent linguistic description, not even for those who believe that the latter discloses the mental representation of the former.

18. As noted earlier, we know that Seuren has a copy of this essay. As to chronology and precedence, Leigh Lisker (1918–2006) reported that Harris was teaching transformations to him and his fellow students as early as the late 1930s (e-mail message to Bruce Nevin, 1 March 2000, as reported in Nevin 2002b: x, n.3). He didn't begin developing transformations as a separate grammatical system until after the completion of Harris (1951) in 1946 (Harris 2002: 4).

19. The criterion stated in (Harris 1957) is word co-occurrence. In (1965) it is preservation of relative acceptability. Harris (2002: 5) says that he used the latter prior to 1954.

the satisfiers of the two sentence-forms"). Like the pair test (and other substitution tests) for phonemic distinctions, this is "an unambiguous, cross-culturally valid, and effective behavioral test" (Chomsky 1975: 82n.6) — *pace* the tendentious word 'behavioral'.

It was Chomsky who replaced these set-theoretic and algebraic concepts with Carnap's notion of 'transformation rule'. Doing so is more or less inevitable, given his insistence on representing the internal structure of sentences by phrase-structure trees, and it is a necessary presupposition for 'recasting' transformations as operations on those abstract structures.[20] But even before those developments, (Chomsky 1951) attests his prior commitment to an algorithmic treatment of grammar rather than an algebraic treatment of language. Harris's transformations are a property of language, Chomsky's are a formal device for representing that property (more or less) by rules that 'enrich' a phrase-structure grammar. Rules of grammar may be widely variant in form, as a matter of notation and metagrammatical superstructure (and indeed have widely varied in the history of Generative Grammar), but transformations in the algebraic sense are variable only insofar as language varies, changes, and evolves. Seuren's statement (p 109) that "in or around 1957, Harris's and Chomsky's notions of transformation are roughly identical" is true (if at all) in only the most superficial possible sense that, as he says, "predicate-argument relations remained constant through transformations." I say "if at all" because Harris's 'incremental transformations' of course

20. The word 'recasting' is from Chomsky (1979a: 112), in a passage that we will come to presently. Harris understood well what Chomsky was doing. "[T]he tree representation there could be considered a representation not so much of the sources of the sentence as of the ordered choices to be made in that system for producing the given sentence" (Harris 2002: 6). Chomsky has repeatedly claimed that Harris never read his work. Goldsmith provides the most recent statement of which I am aware:

> Noam Chomsky (p.c.) emphasizes that the growing consciousness of a conceptual difference (in my terms) was entirely on his side, in that he is 'sure that Harris never looked at my 1949, 1951 work on generative grammar', and that 'it's next to inconceivable, for example, that Harris looked at my Ph.D. dissertation or LSLT', and that Chomsky and Harris did not discuss this material during the 1950s. In nonlinguistic areas, their close relationship 'from the '40s continued without change […] until the late 1960s, and ended for the usual reasons. There was no break of any kind'. (Goldsmith 2005: 720)

This is controverted by Chomsky's own acknowledgements in his 1975 Introduction to *LSLT*: "While working on *LSLT* I discussed all aspects of this material frequently and in great detail with Zellig Harris, whose influence is obvious throughout. (Chomsky 1975: 4). Harris's student and friend Bill Evan has told me that on a visit to the Harrises at Princeton, where they lived while his wife the physicist Bruria Kaufmann was assistant to Einstein, he found Harris and Chomsky "going at it hammer and tongs" with the manuscript of *LSLT* (Chomsky 1955) spread out on the kitchen table. One is left to wonder what "the usual reasons" might be.

add to the predicate-argument relations found in their argument, while preserving them. But for Harris, the preservation of ‘predicate-argument relations’ is an outcome of applying the distributional criterion for transformation (preservation of relative acceptability, or of context for full acceptability), whereas when transformations are defined as deformations of abstract structures, it is the properties of those structures, such as ‘predicate-argument relations’, which are criterial. In a footnote at that same place, Seuren says “Chomsky’s statement (1964: 62) that Harris’s 1957 notion of transformation is symmetrical is thus clearly false.” But a Harrisian transformation is an equivalence relation under the stated criterion. An equivalence relation is necessarily symmetrical; a ‘transformation rule’ operating on either PSG trees (nested constituency labelings) or predicate-argument dependency trees need not be.

How well did Chomsky at that time understand the algebraic basis of Harris’s notion of grammatical transformation? The evidence is equivocal. Chomsky (1979a: 112) acknowledges that during his work on Hebrew, which he says he began as an undergraduate, “Harris’s early work on transformations was then under way and as a student of his I was familiar with it, but I did not see then how this work could be recast within the framework of generative grammar that I was trying to work out.” This supports the interpretation of Chomsky trying to fit transformations, an observed property of language, into his algorithmic project. This passage does not appear in the original interview as published in French translation,[21] where he (1977: 122) is quoted only as saying “les transformations n’existaient pas encore”. Strictly speaking, this is quite true, transformations in Chomsky’s sense did not yet exist, although Harris had been teaching his students transformational analysis for more than a decade when Chomsky (1951) was submitted for a Master’s degree. Not long prior to that interview he had recognized the distinction more explicitly: “In *LSLT*, transformations are understood in a very different sense; it probably would have been preferable to select a different terminology instead of adapting Harris’s in this rather different context” (Chomsky 1975: 43).[22] In the event, Chomsky has dropped the term and his subsequent reformulations of theory have invented new terminology for what continue to be Carnap-style transformation rules applied to abstract representations of structure.

There is a relationship between Harris’s transformations and morphophonemics, but only insofar as the phonemic shape (or phonological representation, if you prefer) of words is subject to change as transformations place them in new

21. This is one of a great many more or less extensive passages that were not a part of the interviews on which the book is “based,” as evidenced by the French publication.

22. Rather than ‘adapting’ it may be that ‘usurping’ would be a more apt term.

contexts. Beginning in Harris (1969)[23] the paraphrastic or non-information-bearing transformations (roughly equivalent to the reductions in Operator Grammar) are referred to as extended morphophonemics ('change purely of phonemic sequences'). It is unlikely that Seuren knew that, and highly improbable that he used it to justify the conflation of morphophonemics and transformations; and to have done so would anyway have been anachronistic for the historical survey that he was reviewing. What is of interest and value is to note that Harris arrived at this, not by assuming Carnapian 'transformation rules' in advance (as Chomsky did, suitably to his algorithmic approach), but rather in the course of presenting the observed properties of language as a mathematical object. It is illegitimate to presume that the constructs of logicians and mathematicians are properties of language prior to or as a basis for determining what the properties of language are.[24] After a succinct summary of the relation of his mature theory of language and information to the views of logicians and others, Harris makes this important distinction as follows:

> The intent of this work, however, was not so much to arrive at such conclusions, as to arrive at them from first principles, from a single method which is proposed here as being essential because of the lack of an external metalanguage. The issue was not so much what was so, as whether and how the essential properties of language made it so. (Harris 1991: 29n.6)

Chomsky instead rushed to apply the mechanics of Post production systems (Post 1943) to a somewhat simplified form of the immediate constituent analysis of Bloomfield,[25] which is based on psychological intuitions of divisibility, but certainly not to the more subtle word-expansion grammar of Harris (1942), which is based on substitution tests (a distributional basis that is 'mentalistic' insofar as it is grounded in native speaker judgments). He subsequently applied 'deformations' to this pre-baked formalism so as to represent transformations, a property of language revealed to him by Harris, and took off thence in his own direction, by his own acknowledgement never really understanding what Harris was doing or why.[26]

23. Or with the morphophonemic transformations φ_m in (Harris 1968) and certain earlier papers.

24. Thus also Frege's (1848–1925) 'laws of thought' more properly are 'laws for thought', since thinking is demonstrably not always and automatically logical, else logicians would be out of work.

25. Perhaps in Rulon Wells's (1918–2008) reformulation (Wells 1947).

26. For example, his ignoring of the principle of analogic extension articulated in (Harris 1957, 1965, 1969) and elsewhere, his consequent conviction that Harris "provides no means for the

8. *Concluding remarks*

Despite the shortcomings of Tomalin's book that Seuren reports, and those which I here ascribe to Seuren's review article, both Tomalin and Seuren have performed valuable spade-work in the largely unexamined history of formal analysis and formal systems within the more general development of linguistics. The criticisms that Seuren and I have each severally levied should not, and indeed cannot overshadow the abundance of useful information that they, Tomalin and Seuren, have disclosed. The disclosures may not always have been as intended, but doubtless others will likewise attribute unintended consequences to what I have written here. Hence, I offer these remarks as an *amicus curiae* contribution to the ongoing collective adjudication of the history of our field.

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generation of new structures" (Chomsky 1975: 43), and his attribution of this to Harris's alleged "unclarity concerning the 'realist interpretation' of linguistic theory" (ibid.) which he espouses. By this, he means the psychological reality of the constructs of linguistic analysis (ibid., p. 36), evidently not accepting that language has an observable reality apart from the psychology of its users. The latter was Harris's object of study.

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27. Professor Stephen Bennett Johnson's middle initial was printed as M on the title page and cover of this work because the publisher automatically inserted the name of an author already in their database. The middle initial B appears in the table of contents. They have assured us that this will be corrected with a new printing of this second volume.

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Author's address:

Bruce E. Nevin
101 Chase Road
Edgartown, MA 02539-7401
U. S. A.

e-mail: bruce.nevin@gmail.com