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

Encrevé severely criticizes the generative school of American linguists for, firstly, failing to treat science as a “cumulative” and “impersonal” discipline – researching, acknowledging, and discussing previous scholarship in order to benefit from all of their insight and study.

“Already by 1950 Einar Haugen had expressed his concern that the task of “scientific accumulation,” – the task of critically maintaining previous scholarship – which had been practiced on both sides of the Atlantic until the war, no longer seemed to be an obvious scientific task for the young American linguists, who tended to neglect European research. But in the 1960s, it was within American linguistics itself that the rupture took place. The cumulative task was correctly assured between linguists within the generative school itself but, up till the 1980s, the work of the “post-Bloomfieldians” was practically unknown to the great majority of Chomsky and Halle’s students.” (1)

Encrevé does not mince words:

“A good structuralist is a dead structuralist, perhaps. The new has done away with the old and cleared the path; from now on, the generativist phonologists can, if necessary, refer to such or such a pre-SPE work in a note, but they no longer submit works other than their own to discussion. A new theoretical principle has been established: either phonology is generative, or it does not exist.” (2)

Towards the end of this section, he addresses the pattern of “continual theoretical renewal” found in phonology since the 1970s. Articles devoted to “recent developments in phonology” present different material every few years, and Encrevé’s opinion on this matter is clear:

“The multiplication of theoretical frameworks has given rise to considerable dispersion, with the result that each linguist imprisons himself within a theoretical proposition, most often limiting his ambitions to just one empirical field. Debate between different theories is less and less frequent, and in most cases innovation is no longer being based on the falsification of a preceding theory… But the desire for a cumulative science is nowhere manifest. It is as if today’s phonologists have more or less forgotten that history of science is a part of science.”

2. (Re)writing history

The second section of the paper is largely devoted to a discussion of Chomsky’s scholarship, touched off by the “Note on recent history” in Bromberger and Halle (1989), as an important example of the problem mentioned in the first section:
“Some forty years later, Bromberger and Halle (1989) ended their plea in favor of a rule-ordered phonology by a “Note on Recent History,” which – *a contrario* – appears to be an exemplary symptom of that formidable amnesia concerning genesis which struck generative phonology after the publication of *SPE*, prohibiting *de facto* the accumulation of the work of the preceding generation, and which continues to have an effect on one of the fathers of the discipline, at the very moment when he considers it indispensable to take a new look at history.” (3)

According to Bromberger and Halle (1989):

“As we have seen, some six years later, by the time he was composing ‘Menomini Morphophonemics,’ Bloomfield had changed his position. The fact that he had done so, however, was totally ignored by the American linguistic community in the 1940s and 50s… The article was so unknown in American that Chomsky tells us he had not read ‘Menomini Morphophonemics’ until his attention was drawn to it by Halle at the end of the 1950s.”

Encrevé is not willing to allow Bromberger and Halle to get away with what he sees as a rewriting of history, and so he sets out to prove not only that *Menomini Morphophonemics* was not “totally ignored,” but that it is in fact extremely improbable that Chomsky was entirely unacquainted with the work when he first began working with generative analyses.

“In the first place, it is difficult to defend the idea that Bloomfield (1939) was ‘unknown’ to, or ‘totally ignored’ by, the linguists in the United States during the 1940s and 50s, when the slightest perusal of the major texts on the subject immediately shows the opposite.” (4)

It is notable that, according to Encrevé, Zellig Harris thanked Chomsky for his help with the manuscript for his *Methods in Structural Linguistics* – a work which “cites, comments on, and contests Bloomfield.” (4)

“In 1955, in what constitutes along with Harris’s *Methods* the major reference on post-Bloomfieldian linguistics, *A Manual of Phonology*, Charles Hockett borrows from Bloomfield a number of examples from Menomini, and cites MM in his (very selective) bibliography. Finally and above all, MM is quoted three times by Noam Chomsky himself in *The Logical Structure of Linguistic Theory*… The notes in which Bloomfield (1939) is quoted were written, then, either in 1955 or 1956. Whatever the case, these notes are sufficient to establish that Bromberger and Halle (1989) are mistaken…” (4)

Encrevé continues in this vein for a while, citing a good deal of evidence indicating Chomsky’s earlier awareness of *MM*, finally ending with:

“We conclude from this perusal of the texts that Halle and Bromberger have closely reread neither Harris (1951=1947), nor Chomsky (1975=1955), nor Chomsky (1964), nor even Chomsky and Halle (1968).”
To end this section, Encrevé addresses the “indirect attack on Zellig Harris” implied in Bromberger and Halle (1989):

Having read Harris (1942), Harris (1951=1947), Harris (1951b) we are left speechless. As far as the bibliography is concerned, it is particularly strange that the authors seem to be unaware that Chomsky (1979=1951) does not in fact give us a bibliography as such, but only a list of ‘references.’ Under this rubric we only find indeed the five titles which are quoted by the text, among which figures, of course, Methods in Structural Linguistics.” (6)

3. Genealogy

Encrevé next attempts to construct a genealogy of the descent of rule ordering in phonology: Bloomfield (1933), Bloomfield (1939), Harris (1951=1947), Chomsky (1951). Interestingly, he notes that Chomsky himself seems to see the shift in Bloomfield as a continuity, while Bromberger and Halle seem intent on depicting it as a rupture:

“But Bromberger and Halle do their best to dislocate it: ‘In fact, in his book Language (1933) Bloomfield fully shared the views about the irrelevance of rule order in synchronic descriptions’ (p. 66). This is followed by the quotation from Bloomfield (1933) that is referred to in Chomsky (1964): ‘The descriptive order of grammatical features is a fiction and results simply from our method of describing the forms.’ Bromberger and Halle, as we have seen, assert then, without comment: ‘As we have seen, some six years later, by the time of composing ‘Menomini Morphophonemics’ Bloomfield had changed positions.’” (8)

While Encrevé’s argument shows that the concept of rule ordering in phonology likely evolved in this fashion, it should also be noted that Chomsky’s justification for allowing rule ordering did not resemble Bloomfield’s:

“Notice above all that Chomsky attached to the expression rule ordering a referral to note 8: ‘Cf. Bloomfield (1933, 213). He regarded ordering rules as an artifact – an invention of the linguist (…) But this depreciation of the role of the 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.” (8)

“The reference to Language (1933, p. 213) is direct, and it follows that, in MM too, for Bloomfield, the sequence of ordered rules is only a ‘fiction,’ an artifact of the linguist… The descriptive order is then for Bloomfield on the same plane as the phoneme – on the very plane of the activity of the phonologist, and any attempt to separate radically MM from Language on this point is untenable.” (8)

From Anderson’s “Reflections on ‘The phonetic rules of Russian’”:

“Ultimately, I would like to say that the importance of this shift of attention from alphabets (inventories of basic representational elements) and representations based on them to rules is significant because it reflects a more profound shift in the object of inquiry, from the study of the properties of observable linguistic events, the forms, to the
study of the knowledge speakers have of their language that underlies their production and perception of such events. Rules are pre-eminently a characterization of speakers’ knowledge, while the representations are in some sense primarily a characterization of the forms. The change is thus a shift from the study of language as an external, social reality to the study of the structure and organization of an aspect of human cognition: from “E-language” to “I-language” as Chomsky has put it.” (8)

From Harris’s *Methods*:

“The work of analysis leads right up to the statements which enable anyone to synthesize or predict utterances in the language. These statements form a deductive system with axiomatically defined elements and with theorems concerning the relations among them.”

From Hockett’s “A note on ‘structure’”:

“The essential difference between the process in the child and the procedure of the linguist is this: the linguist has to make his analysis overtly, in communicable form, in the shape of 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 varying synaptic potentials in his central nervous system. The child in time comes to behave the language; the linguist must come to state it.”

Would Hockett have considered a derivational, rule-based phonological analysis part of “game-playing” linguistics? It would seem that having “cognitive plausibility” as a primary criterion for judging linguistic analyses would put this squarely on the side of “game.” However, while a derivational analysis leads easily into characterizing a speaker’s knowledge (as Anderson points out), an analysis that uses ordered rules need not be a cognitive one (or even one that aspires to some sort of cognitive plausibility). As Bloomfield does, it can be noted that a derivational account is merely a “fiction” (and, Encrevé notes, the usage of ‘fiction’ at the time referred to an abstraction by the linguist).

Encrevé says:

“We should emphasize that all the terms used are in perfect continuity with *Language*: Bloomfield proposes neither there nor here a grammar of the speaker’s competence, but a generative grammar whose goal is descriptive. In Chomsky’s (1964) vocabulary, he does not aim for ‘explanatory adequacy’ but only ‘observational adequacy’ and ‘descriptive adequacy.’” (10)

And, on the subject of psychological reality and descriptive accuracy:

“Sapir’s and Bloomfield’s phonologies are, first of all, linguistic propositions, and their divergences as to the psychological reality of the objects that construct are secondary with regard to the convergence of their theories and their specifically linguistic practices.” (11)

4. A.N. Chomsky, a post-Bloomfieldian generativist phonologist
Despite the thoroughness with which Encrevé addressed the issues of Chomsky’s scholarship and the evidence concerning his awareness of MM, he almost dismisses these issues to address this point:

“Does the repeated assertion concerning Chomsky’s long ignorance as to the very existence of Bloomfield (1939)—which, after all, is a problem of no consequence—not come to cover up the incontestable dependence of Chomsky (1951), and so of generative phonology itself, on Methods, and more generally on Harris, and still more generally on American structuralism on a whole, pretendly confined within its behaviorist taxonomies?” (12)

Encrevé points to “The Mopho-phonemics of Modern Hebrew” as the “true inaugural text of generative and transformational linguistics,” and if it had been published earlier, in 1955-1956, when Chomsky submitted The Logical Structure of Linguistic Theory to the MIT Press, this link would have been more explicit. Even further, it would have “permitted the Bloomfieldians, who generally received Syntactic Structures favorably, to recognize the continuity between their work and generative phonology, instead of soon seeing in the latter only an unacceptable attack against the phonemic perspective.” (14)

5. New phonologies

The final section discusses the link between post-SPE and pre-SPE, particularly how one of the weaknesses of the SPE model, would have been clearer had MMH been the inaugural text for generative phonology – Chomsky’s need to refer to “long components” to explain the grammar of Hebrew indicated how unsuited a linear model was to explaining nonconcatenative morphologies. This was remedied, post-SPE, through autosegmental phonology, which was better equipped to deal with suprasegmental features and non-linear morphologies.

Encrevé again emphasizes the importance of keeping phonology a cumulative science:

“The ‘new phonologies’ cannot be constituted theoretically as such without having theoretically constructed ‘the anteriority’ of the precedent theories – that is, without providing the history of phonology, to make it once again the cumulative science it was up to the 1960s. It is not a matter of writing the dead history of concepts, but rather the living history of their transmission or their non-transmission.” (16)

And, finally, some advice from Encrevé:

“The new phonologies of today and those to come soon tomorrow, which will consider the most advanced point of today’s research as out-dated and out-moded, have no chance to reap fruitful innovations unless they thoroughly nourish themselves with the work effected up till now, and understand the reason for the obstacles placed in the way. I suggest that young phonologists pay particular attention to the way the question of the cognitive reality of linguistic models has been treated and the increasing confusion at MIT, during the 70s, between ‘the model of reality and the reality of the model.’” (16)