

# Reflections on “On the Phonetic Rules of Russian”

Stephen R. Anderson\*  
Dept. of Linguistics, Yale University

At a time when ideas of how phonology ought to be done are clearly changing, it is important to understand the kinds of thing that induce fundamental change in our science. In that spirit, I would like to examine here what we know—and equally, what we *think* we know—about another major change that took place within the relatively recent past in our thinking about sound structure. My theme is the role which is actually played in scientific change by intellectual argument, as opposed to more opportunistic considerations, and I am afraid my ultimate conclusions are less idealistic than one might wish.

A sort of creation myth that I grew up with about the history of phonology has structuralists in America (and also in Europe) concentrating on the discovery of phonemes as minimal units of surface contrast through the 1930’s, 1940’s and 1950’s. Then at the end of the 1950’s, Morris Halle presented some data from Russian (concerning voicing assimilation) from which it was clear that such a notion of phonemes was indefensible and led inevitably to loss of generality. As a result (with allowances for entrenched prejudices and the time necessary to re-tool), phonologists re-oriented their attention toward the previously marginalized domain of morphophonemics, and phonemics was replaced by generative phonology.

To what extent does that picture correspond to reality? Let us look at the facts, historical and linguistic. Halle’s argument was first presented at the 1957 LSA meeting (Halle 1957). Thanks to the fact that in that era, meetings of the Linguistic Society were sufficiently small that minutes could be kept showing who attended, who participated in discussion, etc., we know that in the session at which this paper was given, about 160 people, including many of the major names in American Structuralist linguistics of the period, were present in the audience. The original text is no longer available, but the form of the argument was apparently the same as that which first appears in print two years later in *The Sound Pattern of Russian*:

In Russian, voicing is distinctive for all obstruents except /c/, /č/, and /x/, which do not possess voiced cognates. These three obstruents are voiceless unless followed by a voiced obstruent, in which case they are voiced. At the end of a

---

\*The preparation of this paper has been greatly aided by comments and suggestions from Mark Aronoff, Noam Chomsky, Kenneth Hale, Morris Halle, Larry Horn, Frances Ingemann, Stanley Insler, and Robert P. Stockwell; as well as the participants in the second Royaumont conference on Current Trends in Phonology, including Paul Kiparsky and Sanford Schane. None of these people are at all responsible for the use I have made of their help, of course.

word, however, this is true of all Russian obstruents: they are voiceless, unless the following word begins with a voiced obstruent, in which case they are voiced. E.g., [m'ok l,i] 'was (he) getting wet?', but [m'og bi] 'were (he) getting wet'; [žec l,i] 'should one burn?', but [žež bi] 'were one to burn'.

In a phonological representation which satisfies both condition (3) [phonemic→phonetic determinacy — SRA] and (3a) [phonetic→phonemic determinacy — SRA], the quoted utterances would be symbolized as follows: /m'ok l,i/, /m'og bi/, /žec l,i/, /žec bi/. Moreover, a rule would be required stating that obstruents lacking voiced cognates — i.e., /c/, /č/, and /x/ — are voiced in position before voiced obstruents. Since this, however, is true of all obstruents, the net effect of the attempt to meet both condition (3) and (3a) would be a splitting up of the obstruents into two classes and the addition of a special rule. If condition (3a) is dropped, the four utterances would be symbolized as follows: {m'ok l,i}, {m'ok bi}, {žec l,i}, {žec bi}, and the above rule could be generalized to cover all obstruents, instead of only {č}, {c} and {x}. It is evident that condition (3a) involves a significant increase in the complexity of the representation.

[Halle 1959, 22f]

Halle's argument is standardly adduced as having persuaded phonologists to abandon the phonemics of the time, and take up the rather different pursuits of generative phonology. The logic of the argument is that a description interposing a level of representation meeting the 'bi-uniqueness' condition between the morphophonemic and phonetic representations leads necessarily to a situation in which a regularity of the language, the assimilation of voicing in clusters of obstruents, cannot be stated in a unitary way. As I will argue below, the force of this argument rests in its focus on the need for a grammar not only to assign the correct representations to linguistic forms but also to give an account of the *rules* of the language, rules which correspond directly to components of a speaker's linguistic knowledge. The extent to which that logic was actually apparent to the linguists of the time who heard it is not obvious however, as we will see. My concern is to understand just what the relation was between this argument and the subsequent change in the subject matter of phonology.

It is reasonably clear that the role of Halle's argument cannot have come from the novelty of the facts whose analysis was at issue, since similar material had been discussed before. Halle, indeed, cites one such instance (though curiously, not one of the better known cases I will cite below: the fact that Halle mentions only Matthews in this connection, and not Bloch, Bloomfield, or some other structuralist has been adduced by some as presaging a narrowness in citation practice of which generative linguists are still accused):

"Analogous examples can be cited from many languages. An especially interesting example is discussed by G. H. Matthews, "A Phonemic Analysis of a Dakota Dialect" [*IJAL* 21:56–59 (1955)], who shows that the labial and dental nasal consonants are automatic alternants of the cognate stops as well as of /m/ and /n/, while the velar nasal is an alternant of the velar stop exclusively." [*Ibid.* p. 22, fn. 12]

In fact, the example Halle refers to is *not* analogous in the relevant details to the Russian case. Matthews shows that /b, t, k/ are replaced by [m, n, ŋ] in syllable-final position after

a nasal vowel. But he also makes it clear (Matthews 1955, p. 57, fn. 3) that “[m, n] do not otherwise occur in this position.” This is thus a case of ‘partial overlapping,’ rather than neutralization, and the example does not involve the necessary loss of a generalization in order to maintain a biunique analysis. Since the nasalization rule for stops following a nasal vowel does not involve a mixture of neutralization and allophonic effects, but can be formulated entirely as a relation between phonemic and phonetic forms, its unitary nature is not compromised by the principle requiring phonemic forms to be uniquely recoverable from the surface phonetics. Other examples that *do* have this character, however, were known at the time and will be discussed below.

But independently of the validity of Matthews’ analysis as a precedent, there were actually several examples that had been discussed in the literature before Halle’s paper that involve facts whose logic is entirely parallel to that of Russian voicing assimilation. It is instructive to look at the treatment they received, because it shows us something about the extent to which linguists of the period held to the principles of their theory.

One way of dealing with such facts is illustrated by Bloomfield’s discussion of some facts in Menomini. As in Russian, we have an apparent generalization which (when applied to morphophonemic forms) involves a mixture of phonemic and sub-phonemic effects. But instead of concluding that this showed the inadvisability of phonemic representations, Bloomfield interprets the facts as showing that the allophonic variation is probably really phonemic too, after all. “If it looks like a phoneme, walks like a phoneme, quacks like a phoneme, it must be a phoneme.” [with apologies to Walter Reuther]

If postconsonantal y, w, or any one of the high vowels, i, ī, u, ū, follows anywhere in the word, the vowels ē and ō are raised to ī and ū, and the vowel o in the first syllable of a glottal word is raised to u : mayīčekwa? *that which they eat*, cf. mayēček *that which he eats*; ātεʔnūhkuwεw *he tells him a sacred story*, cf. ātεʔhnōkεw [...] Since ū occurs only in this alternation, it is not a full phoneme.

[Bloomfield 1939, §35]

“Not a full phoneme.” What does that mean? In the inventory of “the actual Menomini phonemes,” the element ū appears in parentheses, and is identified as a “semi-phoneme” (Bloomfield 1939, §5). Bloomfield must have been somewhat uncomfortable with this analytic result, because in a later treatment, his posthumously published grammar of Menomini (edited by Hockett), he gives some rather marginal arguments that ū is a phoneme after all.

Since the occurrence of ɹ is normally confined to the forms in which it replaces ɔ under the regular alternation of 1.8 [referring to the rule above — SRA], it might be viewed as a mere positional variant of ɔ. In this alternation, however, the difference of ɔ and ɹ is parallel with that of e and i, two sounds which unmistakably figure as separate phonemes. Moreover, the difference of ɔ and ɹ is maintained by persons in whose speech this alternation has lost its regularity. Also, the sound of ɹ (and never of ɔ) is used in a few interjections: capuq

‘splash!’, kuh ‘stop it!’. A contrast of o and u appears in the foreign words coh ‘Joe’, cuh ‘Jew’.

[Bloomfield 1962, §1.16]

Fairly clearly, the invocation of such marginal evidence (the speech of individuals who do not really control the phonology of the language, ideophonic elements, and the pronunciation of a synchronically non-Menomini word) represents quite a reach.

A somewhat different response, and a real triumph of honesty in dealing with such facts, is illustrated by Bernard Bloch. In discussing an example from English that is logically just like Halle’s, he notices exactly the point Halle makes: that an apparently unitary rule must be broken in two as a result of the requirements for a phonemic representation. But does he conclude that phonemes should be discarded? Not a bit of it. He concludes that science has saved us from a seductive but ultimately false generalization. I cite the relevant passage at length to show Bloch’s train of thought in the matter.

The pairs of words *bit bid*, *bet bed*, *bat bad*, *but bud*, *bite bide*, *beat bead*, etc. have respectively the same vowel phoneme, but exhibit a regular and fairly constant difference in the length of the vowel allophones. This difference is summarized in the well-known habit of English pronunciation, that vowels and diphthongs (and also liquids and nasals) are longer before a voiced than before a voiceless consonant. The alternation between longer and shorter allophones runs through the whole phonemic system. The vowel of *pot* is affected by the same automatic alternation: in the pairs *pot pod*, *cop cob*, *font fond*, the vowel of the first word is regularly shorter than that of the second; and there is nothing, so far, to show that a pair like *pot pod* is not in every way comparable to *bit bid*.

In my speech, *bomb* is different from *balm*, *bother* does not rime with *father*, and *sorry* does not rime with *starry*: the vowel quality is the same in all these words, but in the first word of each pair the vowel is short (just as it is in *pot*), and in the second noticeably longer. Since the difference in length cannot be explained as an automatic alternation (like the difference in *bit bid*), we conclude that *bomb* and *balm*, *bother* and *father*, *sorry* and *starry* have different vowel phonemes; and we naturally identify the vowel of *bomb*, *bother*, *sorry* with the phoneme of *pot*. The vowel of *balm*, *father*, *starry* appears also in *alms*, *palm*, *pa*, *star*, *card*. Again, there is nothing, so far, to show that the phonemic organization is in any way abnormal. But now comes a hitch.

In the sentence *Pa’d go (if he could)*, the utterance fraction *pa’d* must be analyzed, according to what we have just said, as containing the phoneme of *balm*. In the sentence *The pod grows*, the utterance fraction *pod* must be analyzed, again according to what we have said, as containing the phoneme of *pot*. But *pod*, with a vowel quantity distinctly longer than that of *pot* (just as the vowel of *bid* is longer than that of *bit*), is phonetically identical with *pa’d*!

[...] The apparent intersection of the phonemes of *pot* and *balm* reveals the fact (which otherwise could scarcely have been suspected) that the analysis we

have made is faulty [...] If we say that *pa'd* has the phoneme of *pot*, *bomb*, *bother*, *font* and the like, we must necessarily classify the vowels of *pa*, *balm*, *father*, *alms*, *card*, and so on in the same way: that is, we must deny the obvious fact that in the dialect here considered *bomb* and *balm* are different, and the pairs *bother* and *father*, *sorry* and *starry* do not rime.

We are left, then, with the other alternative. By classifying the vowel of *pod*—and consequently also the vowels of *rob*, *nod*, *bog*, *fond* and the like—as members of the phoneme of *balm*, we destroy the neat parallelism of the pairs *bit bid*, *bet bed*, *bite bide*, *pot pod*: the words in the last pair, instead of exhibiting shorter and longer allophones of the same phoneme, have different phonemes. But by sacrificing this symmetry we are able to account for all of the facts of pronunciation, which is surely the more important requirement. The resulting system is lopsided; but the classes it sets up are such that if we start from the actual utterances of the dialect we can never be in doubt of the class to which any particular fraction of utterance must be assigned.

[Bloch 1941]

These reactions are among the more principled. In fact, when we look at the examples that began to accumulate by the 1950's to the effect that phonemic representations had properties that led to incorrect or otherwise deficient analyses, linguists of the time found various ways to preserve their principles in the face of the apparent facts. On an issue other than bi-uniqueness, this can be illustrated from reactions to the famous example of *writer/rider*, where the surface contrast is in the “wrong” place as illustrated in (1).

(1) [rajDə] ‘writer’ vs. [ra:jDə] ‘rider’  $\stackrel{?}{=} /rajtr/$  vs.  $/rājtr/$  or  $/rajtr/$  vs.  $/rajdr/$

One possible way to deal with such a situation is to force the theory to provide the correct result. When the principles lead to absurdity, adapt the principles so that they will yield what you know intuitively to be correct. An example of this approach is provided by Harris’ (1951) procedures of ‘re-phonemicization.’ An alternative is to follow the principles consistently, and if they lead to absurdity, then deny the facts. With respect to the specific facts in (1), this is illustrated by Householder’s (1965, p. 29) conviction that “I can tell you from experience that you will, if the words are in fact consistently distinguished, invariably find one or more of several other differences [between the flaps of *writer* and *rider*].” That is, even though all of the apparent evidence suggests that the difference between *writer* and *rider* (in the relevant dialect) is a matter of the quantity or quality of the stressed vowel, a sufficiently assiduous search for phonetic detail will uncover some basis for assigning the difference to the medial consonant (where it intuitively ‘belongs’) and treating the patent vowel difference as allophonic.

Principled discussion in the 1940's and 1950's of facts that were embarrassing for phonemic theory did not in general consider, as Halle did, the possibility that the appropriate conclusion to be drawn was that the basic premises of structuralist phonology were misconceived. I know of one place in the literature, a paper of Eric Hamp's on Celtic, where an

example is discussed that is logically like Halle’s, and where the suggested conclusion is precisely that one ought to study the “morphophonemic” representations, that the “phonemic” ones get in the way.

From these phonetic oppositions it is evident that without the use of morphophonemes the list of phonemes previously stated must be expanded to include at least /bb, dd, gg/, or else a very restricted gemination phoneme [...]. The following pairs show sample contrasts which lead to the point just made:

[tórreðeðebbá:s] *Torret eo he baz* ‘Her stick is broken’  
 [tórreðeðebá:s] *Torret eo e baz* (<pa:z) ‘His cough is broken’  
 [...]  
 [pérrinalénnellízer] *Perrin reads her letter*  
 [emápe:rollénnelí:zer] *Emañ Per o lenn e lizer* ‘Per is reading his letter’

Transcribed in phonemic terms—without morphophonemes in the left-hand column, with morphophonemes in the right-hand column, but without fully exploiting /:/ as a component—these sequences look as follows:

/tored ew e bba:z/	/tored ew eS ba:zL/:
/tored ew e ba:z/	/tored ew eL pa:z/;
...	
/perin a len e lli:zer/	/perinL aL len eS li:zer/:
/emá pe:r o llen e li:zer/	/emá pe:r oPL len eL li:zer/

[...] Thus, by employing morphophonemes we not only gain great structural advantages for purposes of morphological statement [...]; we also dispose of phonetic elements in such a way as to reduce the overall stock of phonemes and to simplify the statement of their distribution.

[Hamp 1953, pp. 244f]

Hamp’s point was that if we treat “morphophonemes” as part of the phonological representation, rather than limiting this to bi-unique phonemes, the result is an overall simplification. That is, since the morphophonemic regularities must be stated in any event, the requirement that phonological representations be bi-uniquely related to phonetic form results in a complication of the statement of the language’s phonology. But as far as I know, few if any linguists followed Hamp on this point: while interest in morphophonemics was substantial (and growing) during this period, this was taken to be a supplement to, not a replacement of, the strictly phonemic analysis.

When we ask why Halle’s argument should have been so earth-shaking, it is hard to say. Not only did it not involve a completely novel complex of facts, it is not even the case that it shows biunique phonemic analyses in general to lead to loss of generalization. This is a point that several authors have made, with respect to theories like that of Trubetzkoy.

In (2) I offer the outlines of an analysis of Halle’s Russian example within a theory that employs archiphonemes in positions where phonemic contrasts are neutralized:

- (2) a. Paired obstruents are underlyingly [+Voice] or [–Voice]; unpaired obstruents are unspecified for [Voice].

- b. Finally and before another obstruent, [Voice] is neutralized (i.e., specifications of  $[\pm\text{Voice}]$  are deleted in these positions).
- c. Obstruents not specified for [Voice] are pronounced
  - i. voiced, if followed by another obstruent pronounced voiced;
  - ii. otherwise, voiceless

Here the representation that results from the neutralization rule (2b), but which has not yet been specified phonetically by the rule for pronouncing under-specified elements, (2c), meets the conditions of phonemic theory. Such a representation can be uniquely recovered from the phonetic form in light of the rules of distribution of phonemic elements (including archiphonemes) in Russian. But notice that voicing assimilation is stated by the pronunciation rule (2c) in a way that doesn't involve the kind of invidious bifurcation Halle objected to.

In fact, the kind of analysis one might offer in a contemporary auto-segmental framework, as outlined in (3), is quite similar. Here the representation that results after the de-linking rule (3b) applies, but before spreading rule (3c) and default insertion of [-Voice], again meets the conditions of phonemic theory. We thus cannot really assume that facts of the sort discussed by Halle necessarily entail the impossibility of phonemic representations.

- (3) a. Assume privative [voice] phonologically;
- b. De-link [voice] finally and before another obstruent
- c. Spread [voice] leftward from an obstruent to a preceding obstruent.

To summarize what we have seen thus far, we can conclude that Halle's argument when it was presented in 1957/1959 was of a sort that had been offered in substance before; and that in any event, it did not really suffice to prove its point in a fully general form. So why, then, did it have such a major effect, apparently, while other similar cases had little or no effect? In a book I wrote some years ago, I argued that the special force of Halle's argument came from the fact that it was embedded in a theory of rules, not just representations and the alphabets of elements that compose them.

[T]he effectiveness of Halle's argument [...] lay in the emphasis it put on the centrality of rules in a phonological description. Note that the entire argument rests on the observation that, in certain situations, a level meeting the conditions of bi-uniqueness requires some unitary regularity of the language (here, voicing assimilation) to be split up into two effectively unrelated rules. Now in a theory (such as American structuralist phonemics) in which only the representations of forms have 'real' status, such an argument is nonsensical or at best irrelevant: the principles relating one representation to another (the rules) are simply parts of the definitions of individual elements of representations, and have no independent status whatsoever in the grammar. If they can be formulated in a simple

and concise way, so much the better; but the notion that the elements of representations themselves should be chosen for the convenience of the rules was inconceivable.

The immediate consequence of Halle's discussion was a change in phonology in the direction of much more abstract representations than those permitted within a theory which concentrated on biunique phonemics. But it must be emphasized that this move was, in an important sense, an ancillary consequence of a more fundamental reorientation in phonological research: a shift from a concentration on the properties of phonological representations and their elements to a much greater stress on the rules of a grammar. Naturally, concern with questions of representations and their nature did not disappear overnight. Nonetheless, the recognition was dawning that rules as well had to be taken seriously as part of a grammar if language was to be examined as a complex cognitive system rather than an inventory of phonemes, morphemes, words, and constructions. Since the study of rules, their properties, and their organization into linguistic systems was virtually unexplored territory, this reorientation had a much more important effect on the nature of phonological research than the mere fact that generative underlying representations are more abstract than biunique phonemic ones.

[Anderson 1985, pp. 321f]

Halle's innovation, on this view, was the focus he put on the need to get the *rules* right in the statement of a language's phonology, and not simply to provide the right representations. Notice that this is a way in which his argument differs from Hamp's: both argue that the morphophonemic representation is where the interesting regularities are to be found, and that these should be related directly to surface phonetic forms. But Hamp stops there: he does not actually formulate any rules. His discussion treats Celtic mutations as systematic correspondences between morphophonemes and their pronunciation, but does not cite specific rules that could not be (properly) formulated if the description is limited to a set of phonologically basic elements (phonemes and/or morphophonemes).

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.

Now during the heyday of American structuralism, it was pretty much out of bounds to study internalized knowledge: all there was to study was the observable external form. But by the 1950's the world was gradually coming to be more receptive to talk about minds,

and so such a shift was at least logically possible. The link between rules and individual cognition is quite explicit, at least by the time of *SPE*:

The person who has acquired knowledge of a language has internalized a system of rules that determines sound-meaning connections for indefinitely many sentences. [...] [W]e use the term ‘grammar’ to refer both to the system of rules represented in the mind of the speaker-hearer [...] and to the theory that the linguist constructs as a hypothesis concerning the actual internalized grammar of the speaker-hearer.

[Chomsky & Halle 1968, pp. 3f]

On Halle’s own account, “[t]he essential novelty of [...] *SPE* [...] derives from its basic assumption that phonology is an aspect of the knowledge that speakers/hearers have of their language” as opposed to “the view dominant in American linguistics in the 1940s and 1950s, which regarded the task of linguistics as that of assembling inventories of elements—phonemes, morphemes, immediate constituents, and so on—and constructing a taxonomy of these entities without special concern for the status of the entities or a search for rules.” (Halle 1998, p. 539) And this, of course, was the innovative change of focus that resulted from Halle’s argument about Russian voicing, on the standard historical view of the situation.

It would be really nice to be able to show that this link between Halle’s argument, with its central attention paid to getting the rules right (not just the representations), and a mentalist conception of language as knowledge, was what persuaded people of the correctness of the generative position. But to show that, we need to be able to establish that anyone (except perhaps Chomsky and Halle and some of their immediate colleagues and students in Cambridge) actually understood the argument in that way. When I first asked Morris Halle about the reaction to his 1957 LSA presentation, he told me a story that would certainly seem to support such an impression:

I seem to recall from the [1957 LSA] meeting that Bloch spoke up afterwards and called me a ‘mentalist’, which was not meant to be complimentary. [...] As I remember the meeting, I responded by remarking that I did not understand what was wrong with being a mentalist; like most people including Bloch I assumed that there was such a thing as the human mind and that, moreover, I thought it resided in people’s heads; I concluded that if Professor Bloch preferred another part of the anatomy he should name his candidate and we could discuss it. There was a big laugh at that and the meeting ended on this note. Since I came out the winner so decisively, the story may well be what Goethe in his autobiography called ‘Dichtung’ rather than ‘Wahrheit’. At any rate, if you retell it you should label it as potential apocrypha, since I have no one[’s] corroboration for it.

[Halle, personal communication, 4 May, 1998]

Unfortunately, the story really *is* too good to be true, because according to the proceedings of the 1957 Chicago LSA meeting as published in the *LSA Bulletin* (Number 31,

Supplement to *Language* volume 34), Bloch did not participate in the discussion of Halle's paper. The only other occasion that either Halle or I have been able to identify where this exchange could have taken place was in the discussion of Frances Ingemann's paper ("A Dimensional Approach to Speech Synthesis") at the same meeting, discussed by both Bloch and Halle (in that order). The subject matter of Ingemann's paper dealt with synthesizing speech using phonetic parameters such as place, manner and voicing, and would not seem to have provided any stimulus to Bloch to criticize Halle's views on phonemics. At any rate, Ingemann has no recollection of any such exchange following her paper, and adds "that the tone of the exchange Halle reported does not sound much like the 1950's. In the 50's people were relatively polite. It wasn't until the 60's that people became much more impolite and confrontational. Maybe he is remembering an exchange that took place later." (personal communication, 28 August, 1998).

The point of this discussion is not at all to question the veracity of anyone's unguarded recollections of events more than forty years ago. What I would like to establish, rather, is that *if* (what I feel to be) the real significance of Halle's argument had been appreciated on its original presentation, phonemicists like Bloch *ought* to have responded by labeling it as an instance of (the then widely anathematized 'error' of) "mentalism." The fact is that they apparently did not. While there is abundant evidence of a reaction against the *technical* proposal of Halle's paper, a rejection of bi-unique phonemic representations in phonological analysis, there is very little reason to believe that adherents of the dominant structuralist view appreciated the more profound *conceptual* implications of this argument.

Chomsky, in turn, has a slightly different view of what happened:

[Y]ou're right that 'similar situations had certainly been analyzed before,' and it's also correct to say that the importance of Morris's observation was that it 'was embedded in a program for studying languages as systems of knowledge, not just collections of forms.' I'd put it a bit differently: it was embedded in a point of view that took the study of language to be on a par with the study of organic chemicals, or the visual system, or other parts of nature.

[Chomsky, personal communication, 23 May, 1998]

I have no doubt that those who were directly involved in generative work at MIT at the beginning of the 1960's had such a perception that their approach to language was based on a notion of 'science' that was qualitatively very different from that of their structuralist predecessors; and this sense of such a different point of view has been confirmed to me by various people who arrived at MIT before I did (in 1966). But I think it is very hard to find real evidence that linguists in general suddenly realized, on the basis of Halle's argument or other similar presentation, that they had not really been doing scientific work, and thus should revise their basic assumptions about the nature of linguistics. It seems quite true to say that the shift from the study of forms to that of grammars is a change in focus from epiphenomena to a coherent and principled natural phenomenon, but to say that is to characterize the *effect* of the shift in focus, not its *cause*. It still does not really tell us how and why the change in question took place.

The change in basic approaches toward a science of language to which Chomsky refers seems to have been quite gradual, and very much a consequence of specific, persuasive presentations on his part. That is certainly the view suggested by Halle:

[E]verybody also agreed that this redundancy in the statements was undesirable, that it indicated some flaw in the theory. This seems to me to be supported by the reaction of the audience at the Third Texas Conference (May 1958) following the presentation of this example in Noam's paper. Perhaps they were not being consistent, but they understood that these were relevant data.

I think that Noam's presentation of my example at Texas is what made the difference politically. As you will recall he did an absolutely first-rate job there: he convinced Sledd (and perhaps a few others). He frightened Stockwell into switching sides and others into respectful neutrality or silence. After that, MIT linguistics was taken very seriously everywhere, even in places where everyone was opposed to us. In view of this the text of my LSA paper is only of academic interest, for it played a secondary role in the development of the field. What affected the field was not my LSA paper, but Noam's repeated discussion of the facts that I had gathered.

[Halle, personal communication, 13 May, 1998]

Halle here suggests that Chomsky's presentation at the Texas conferences persuaded people of the correctness of his (Halle's) logic. But is that actually the case? While the papers given by Chomsky at the third and fourth Texas conferences were widely cited as providing arguments for the new point of view, it is much less clear that the important structuralist figures who attended these conferences were really attuned to the aspects of these papers which are most important in retrospect, or that they were actually convinced of much.

Let us see what Chomsky (1962) actually said. It seems to me that his presentation here of the argument, while quite persuasive in retrospect, is not phrased so as to make the real importance (the need to shift from a study of forms to that of rules and linguistic knowledge) stand out. It is possible to read these passages simply as urging the need to abandon proceduralism as the underlying epistemology, rather than as a call for a basic re-orientation in terms of the object of inquiry in linguistics.

The central methodological concern in recent American linguistics has been the precise definition of such notions as Phoneme, Morpheme, and Immediate Constituent. Almost without exception phonemes have been thought of in relatively substantial terms as (in one formulation) certain classes of sounds, while morphemes are taken to be certain classes of sequences of phonemes, and constituents, certain classes of sequences of morphemes. The methodological problem for linguistic theory, then, has been to provide the general criteria for making these classifications, and the goal of the linguistic analysis of a particular language has been to isolate and list the particular classes, sequences, sequences of classes,

etc., which are the phonemes, morphemes, constituents of these languages. A linguistic grammar of a particular language, in this view, is an inventory of elements, and linguistics is thought of as a classificatory science.

Furthermore, almost every approach to linguistic theory has attempted to formulate these definitions in such a way as to provide an essentially mechanical method that an investigator might use, in principle, to isolate the phonemes, morphemes, and constituents in the analysis of a particular language. This interest in a discovery procedure for linguistic elements has motivated the insistence on strict separation of levels, bi-uniqueness of phonemic transcription (often with the additional and stronger requirement that if one occurrence of a physical event is assigned to a particular phoneme on one occasion, then every occurrence of that event must be assigned to the same phoneme — I will refer to this below as the strong form of the bi-uniqueness principle), phonemic identifiability of morphemes, and many other widely held doctrines.

Neither the conception of a grammar as an inventory of elements nor the requirement that there be a discovery procedure for elements of the inventory is very easy to justify. A grammar of a language should at least be expected to offer a characterization of the set of objects that are sentences of this language, i.e., to enable its user to construct a list or enumeration of these utterances. It is not at all clear how an inventory of elements provides this information (just as it is not clear what the user of a traditional grammar brings to its paradigms and examples that enable him to produce new sentences and reject nonsentences). And as soon as we attempt to give a rigorous account of the process by which a grammar generates sentences, a variety of new considerations come to the fore. We find that some of the requirements that have been imposed on linguistic elements (e.g., the bi-uniqueness condition for phonemes) lead to extensive and unnecessary complication of the grammar, that certain others (e.g., the requirement of phonemic identifiability of morphemes) become entirely superfluous, while others (e.g., strict separation of levels) become almost unstatable. We find that there is strong reason to allow considerations of the form in which grammars are given and considerations of complexity of grammar to be basic factors in the selection of a set of phonemes, morphemes, etc., in analysis of a particular language. Consequently, we are led to abandon the attempt to define such terms as “phoneme,” “morpheme” in general linguistic theory with no reference to the grammars in which these terms will appear. [pp. 125f]

[presentation of Halle’s Russian example. . . ] It is clear that this complication of the grammar is entirely pointless. But it is just this that is required by the widely accepted strong form of the bi-uniqueness principle. . . On the phonemic level[. . .]it seems that the view of a grammar as an inventory of elements can be profitably replaced by the conception of a grammar as a device of a specified form . . . which characterizes the utterances of the language. Instead of viewing the phonological section of the grammar as an inventory of phonemes, constructed by

procedures of analysis based on such rather arbitrary principles as bi-uniqueness, we can construct the simplest possible grammar of the appropriate form, and consider the phonemes of this language to be the elements that appear in the representation of utterances on the appropriate level in this grammar. [p. 134]

When we read this today, *we* can certainly see the logic: if we maintain a bi-unique conception of phonemic representation, we are unable to provide a correct account of the regularities of the language, and thus are forced to mis-represent what it is a speaker knows about his/her language. The force of the argument arises from the notion that the linguist must do just this: give an account of a speaker's knowledge, not simply an analysis of the observable linguistic forms in some (actual or potential) corpus in terms of a basic alphabet of potentially contrastive elements. But it is highly questionable whether the other participants in the Texas conferences followed the logic that linked Halle's Russian example with such fundamental issues. Chomsky's point sounds at first blush as if it concerns the mechanics of a linguistic description, rather than the basic object of inquiry in the field.

It is certainly the case that the issue of mentalism came up at this conference, but only in a rather marginal way, and not at all as a focus. Here is the most explicit instance of this in the discussion at the third Texas conference:

Hill: If I took some of your statements literally, I would say that you are not studying language at all, but some form of psychology, the intuitions of native speakers.

Chomsky: That is studying language.

Long: I agree with Chomsky and Harris here. Language goes on in the brain, not merely in the throat.

[Hill 1962, p. 162]

In context, Hill clearly took his characterization of Chomsky's interests to be a *reductio*, but the issue is dropped immediately after this exchange, and the role of 'mentalism' only resurfaces later as a quite different question. That is, the issue of mentalism was only discussed at any length in connection with Chomsky's remarks about the importance of studying a speaker's "intuitions." That seems to have been interpreted in the sense of "vague, unscientific feelings about the world," not in the more central and principled sense Chomsky intended. As such, a number of speakers expressed reservations about the utility of scientific research based on "mere intuitions." It is fairly clear that the notion of a speaker's intuitions as a way of describing cognitive organization, an aspect of knowledge, was not the way the expression was understood by the audience (at least that part of it recorded for posterity in Hill 1962).

In any event, Halle's argument about Russian only comes up once as far as I can see in this discussion, and there only in the following exchange, in connection with the validity of discovery procedures for an analysis:

Winter: Is it appropriate to tie up this problem with the examples your paper gives from Russian?

Chomsky: As long as you don't ask me too much about them, since I am not an expert.

Winter: When Halle presented these examples at the last meeting of the Linguistic Society, my chief reservation was that I was not quite sure how he arrived at his units in the first place.

Chomsky: No, I don't think he is either. [!]  
[Hill 1962, p. 171]

The conversation then goes off into a discussion of how to justify particular procedures for arriving at an analysis. This latter procedural point was what people actually heard: a situation reminding one of a famous Gary Larson cartoon about what portions our dog hears out of what we say.

The following year, at the infamous 4th Texas conference (infamous because the proceedings were never published, though rumor has it that discussion was dominated by a battle royal between Chomsky and the structuralists precisely about phonology), Chomsky's presentation makes the point somewhat clearer. This is in connection with an early formulation of the distinctions that would later become the famous "three degrees of adequacy."

The research program that I will outline now is motivated by the following two questions:

1. What kind of 'equipment' or capacity must be attributed to the mature speaker of language to account for his ability to understand new sentences as he does, and to freely produce new sentences which are similarly understandable to other speakers;

2. What kind of properties must be attributed to an organism capable of achieving the state described in the answer to 1 in the time available with the information available.

[...] A formalized grammar of a language provides a (partial) answer to 1; a formalized general theory of linguistic structure, I believe, provides a partial answer to 2.

[Chomsky 1959]

Even at this point, though, it is not clear that the message got through about the connection between arguments like Halle's and the mentalist focus on rules as opposed to representations. The only reminiscence of this meeting that I have been able to develop is from Bob Stockwell: since he is generally portrayed as one whose 'conversion' at these conferences was centrally important to the subsequent success of generative views in the academic marketplace (Newmeyer 1986, p. 30), one might expect his to be the most congenial interpretation to be found among the students of Hill and other American structuralists who dominated the conference. But what he saw as the take-home point had nothing to do with mentalism: rather it was a matter about procedures and the validity of a certain kind of representation.

I think I remember differently from the reconstruction you have suggested the sense of what was important, in the sense of ‘what was heard’, at the 4th conference discussions of phonology. The issue over and over was ‘mixing of levels’. Halle’s point, as it was represented by Chomsky there, or at least perceived to have been focused in this way, was that mixing of levels is okay, not only okay but absolutely necessary to get a decent description of the Russian facts. Many of the English problems that today would be handled easily by reference to boundaries (therefore mixing levels) could still then be handled (sort of) at a phonemic level by positing ‘plus juncture’, the phonetic correlates of word boundaries. But that will not work for the Russian case.

The Russian case breaks down ‘separation of levels’, and thereby opens up the possibility of a rich derivational phonology in which reference can be made to whatever linguistic phenomena one can show are relevant to a description. That is what I think was heard. So in a sense it WAS just the character of phonemic representations that was at issue, not so much the necessity for rule systems in general but from what range of phenomena could the rules legitimately draw their characterizations of, for example, environmental effects. Rule systems of some sort were not in doubt, were they, given early Bloomfieldian descriptions and Hockett’s characterization of rules and processes in phonemics? It was a question of allowable depth. The inference was taken (by some of us, at least) that this question had now been answered, that there were no necessary separation-of-levels barriers.

[Stockwell, personal communication, 25 May, 1998]

As late as 1965, when Householder presented Chomsky and Halle with a debating platform for use in going through the bases of alternative approaches to phonology, it is clear that at least a significant fraction of the field did not (and perhaps could not) understand the notion that linguistics might have speakers’ knowledge, not the properties of linguistic forms, as its proper object. Fred Householder was certainly a very intelligent man, and an experienced linguist, but the very idea of linguistics as the study of an aspect of the mind was quite incomprehensible to him. In discussing the claim of Chomsky (1964) that “A grammar that aims for descriptive adequacy is concerned to give a correct account of the linguistic intuition of the native speaker” Householder (1965, p. 14) finds that “[o]nly ... ‘observational adequacy’ is intelligible (at least to me) ... it is sheer braggadocio to talk about descriptive adequacy, even if one knew how to discover what a ‘correct account of the linguistic intuition of the the native speaker’ is.”

By the mid to late 1960’s, as new generations of students appeared whose training originated in the work of Chomsky, Halle, and their colleagues at MIT, we can see that the basic point about the central importance of rules, the need to get those right because they are really what language is all about, came to be more generally appreciated. But recall that the persuasiveness of Halle’s original argument really rests crucially on one’s willingness to take seriously the need to get the rules right. If it took ten years or so after Halle’s original presentation for this to become a generally accepted notion, it is clear that whatever

was responsible for the rise of generative phonology, it probably was not the logic of Halle's conclusion about the obstructive role of phonemes in a descriptively adequate account of Russian voicing assimilation.

So what in fact *did* happen to change the direction of phonologizing in the early 1960's? A part of the responsibility undoubtedly should be laid to a principle that "*plus c'est la même chose, plus ça change.*" That is, by the end of the 1950's, phonemic theory had increasingly become a settled discipline within which only quite minor adjustments seemed necessary (or possible). With little left to do, new generations of students inevitably looked for new challenges—and new approaches that would provide them. While the fundamentally distinct scientific premises of the new theory of generative grammar may have been apparent to its originators, students did not have to appreciate these differences to see that something quite new and different was going on, and that they could make real contributions to it.

Generative grammar was not the only alternative to classical American structuralism that arose around this time, and we must ask why this point of view (rather than, say, that of Stratificational Grammar) prevailed so decisively. The answer comes in part from clear arguments offered by the early developers of generative phonology, but also to a non-trivial extent, I'm afraid, from a much more superficial consideration: the fact that nothing succeeds like success. Halle offers this analysis:

In my opinion, the reason that generative phonology caught on had mainly to do with the fact that Noam and I (and soon also our students, Lightner, McCawley, Schane, Kiparsky, etc.) showed that there were interesting results to be obtained by taking rules seriously, results that could not be obtained within the confines of structuralist phonology.

[personal communication, 4 May, 1998]

It is reasonable to observe that a great deal of interesting and productive work followed close on the heels of Halle's proposal to abandon bi-unique phonemic analyses in favor of (what had been called) morphophonemics, but at least in this case, the story is a bit more complicated than simply one of the triumph of intellectual virtue. When we examine work of the sort Halle refers to, including early MIT theses in phonology such as those of Foley (1965), Harris (1967), Kiparsky (1965), McCawley (1965) and Schane (1965), as well as Halle's own work on Russian and on Latvian (e.g., Halle & Zeps 1966), there is a common thread apart from the focus on rules in descriptions. In each case, these works draw heavily on the existing results of historical linguistics, either directly (as in Kiparsky's case) or as a source of antecedent analyses whose bearing on synchronic phonology is quite direct in a 'morphophonemic' context. In contrast, within the assumptions of bi-unique phonemics, this rich source of knowledge had to be excluded more or less on principle as irrelevant to phonological analysis. The new line on synchronic phonology could thus draw immediately on a wealth of well developed material, producing significant results within a short time and providing a generation of graduate students with a relatively direct recipe for making interesting, novel, and theoretically interesting contributions to a new and rapidly growing enterprise.

It is important to understand the content of our creation myths, since they tell us something about the structure we actually give to our world. On the other hand, it is also important not to confuse them with explanations of how the world actually came to be the way we find it. In the end, I conclude that Halle's argument about Russian voicing assimilation did not itself persuade the linguists of the time to drop their externalist presumptions, their phonemes and their exclusive focus on representations so as to become mentalists focusing on rules as the expression of internalized knowledge. But on the other hand, it is exactly in the context of that development that we still have to see the logical force of the original argument. We really only come to appreciate the sense of this important argument after the shift in point of view that it supposedly produced has been achieved. And that shift was undoubtedly the result of considerations only tangentially related to the persuasiveness of the argument.

I do not find it particularly satisfying to discover that the field changed character fairly rapidly for reasons that were primarily pragmatic, rather than purely principled. But on the other hand, I think that the same thing has been responsible, on a smaller scale, for a number of the changes we have seen since then (and probably many times before). I submit that the wholesale abandonment of the formalist concerns that grew out of *SPE*, issues such as rule ordering and the role of formally defined notational conventions in producing an explanatorily adequate theory of grammar, in favor of the study of auto-segmental representations did not come about because autosegmentalists solved the earlier problems, or even showed that they were misconceived. Rather, this happened because auto-segmental work developed an impressive array of fascinating results within a rather short time, especially in an area that had previously been rather poorly understood (the study of tone). As in the case of the early years of Generative Phonology, there was a large existing corpus of good work that had previously seemed intractable to incorporation into our understanding of phonological structure: in this case, the descriptive literature on tonal phenomena (and later, syllable structure). Once the path to this rich vein of potential results (and prospective theses) had been shown, phonologists (especially those of a new generation) rushed enthusiastically to pursue it, abandoning any previous theoretical pre-occupations. It was only much later that it was shown that some classical chestnuts of earlier work, like the analysis of length, could really be solved in auto-segmental terms in ways that were just not available otherwise. I think we can all think of other examples of the same thing, but I refuse absolutely to speculate on the relation of these remarks to the role of Optimality Theory in current phonologizing.

## References

- Anderson, Stephen R. 1985. Phonology in the twentieth century. Chicago: University of Chicago Press.
- Bloch, Bernard. 1941. Phonemic overlapping. *American Speech* **16**. 278–284.
- Bloomfield, Leonard. 1939. Menomini morphophonemics. *Travaux du cercle linguistique de Prague* **8**. 105–115.
- Bloomfield, Leonard. 1962. *The Menomini language*. New Haven: Yale University Press. [Charles F. Hockett, ed.].
- Chomsky, Noam. 1959. The transformational basis of syntax. unpublished paper given at Fourth Texas Conference.
- Chomsky, Noam. 1962. A transformational approach to syntax. *in Hill* 1962.
- Chomsky, Noam. 1964. The logical basis of linguistic theory. Proceedings of the ninth international congress of linguists, Cambridge, Mass., August 27–31, 1962, ed. by H. G. Lunt. 914–978. The Hague: Mouton.
- Chomsky, Noam & Morris Halle. 1968. *The sound pattern of English*. New York: Harper & Row.
- Foley, James. 1965. *Spanish Morphology*. PhD thesis. MIT.
- Halle, Morris. 1957. On the phonetic rules of Russian. unpublished paper read to the Annual Meeting of the Linguistic Society of America, Chicago.
- Halle, Morris. 1959. *The sound pattern of Russian*. The Hague: Mouton & Co.
- Halle, Morris. 1998. The stress of English words 1968–1998. *Linguistic Inquiry* **29**. 539–568.
- Halle, Morris & Valdis Zeps. 1966. A survey of Latvian morphophonemics. *Quarterly Progress Report of the Research Laboratory of Electronics, MIT* (83). 105–113.
- Hamp, Eric P. 1953. The morphophonemes of the Keltic mutations. *Language* **27**. 230–247.
- Harris, James. 1967. *Spanish Phonology*. PhD thesis. MIT.
- Harris, Zellig. 1951. *Methods in structural linguistics*. Chicago: University of Chicago Press.
- Hill, Archibald A., ed. 1962. *Proceedings of the third Texas conference on problems of linguistic analysis in English*. Austin: University of Texas Press.
- Householder, Fred W. 1965. On some recent claims in phonological theory. *Journal of Linguistics* **1**. 13–34.

- Kiparsky, Paul. 1965. Phonological Change. PhD thesis. M.I.T. published by Indiana University Linguistics Club.
- Matthews, G. H. 1955. A phonemic analysis of a Dakota dialect. *International Journal of American Linguistics* **21**. 56–59.
- McCawley, James D. 1965. The Phonological Component of a Grammar of Japanese. PhD thesis. MIT.
- Newmeyer, Frederick J. 1986. *Linguistic theory in America*. 2nd edn. Orlando: Academic Press.
- Schane, Sanford. 1965. The Phonological and Morphological Structure of French. PhD thesis. MIT.