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Observations on the Controversy concerning the Exceptionless Functioning of the Sound Laws*

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Writing in 1900, the German linguist E. Wechssler observed: "The struggle over the sound laws which broke out in 1876 and has been waged with especial vigor since the middle of the 1880's has now subsided. It is only infrequently now that anyone takes up this question. The results of the long debates are rather curious. In practice the sound laws have proven themselves everywhere . . . Nonetheless weighty objection against the theory have been advanced by its opponents, and these have not always been refuted, so that today it (= the theory) is no longer defended with quite the same fervor as before . . . This contradiction between the fate of the theory and its practical utilization . . . invites . . . investigation . . . " (Wechssler (1900), p. 1).

In the three quarters of a century that have passed since these words were written, interest in the controversy has not died: books (e.g., Jankowsky (1972) and Vennemann and Wilbur (1972)) and extensive articles (e.g., Hoenigswald (1977)) dealing with the controversy continue to appear. It is clear that for many the issues raised by the controversy are still important, and it is this fact that led me to undertake this investigation.

What I believe to have learned from the study is that the chief neogrammarian claim that sound laws admit of no exceptions was never particularly well supported by its proponents; the arguments were not convincing, and many facts were left unexplained. This, however, did not have the expected calamitous consequences for the neogrammarian school, because the acceptance or rejection of the claim had at best only marginal effects on the empirical work that was, or might have been accomplished by comparative linguistics during much of the past century.[1] What separated comparatists of the last century—i.e., their views on whether or not sound laws admit exceptions—ultimately turned out to be much less important than what united them—i.e., their views on the role that rules play in a speaker's knowledge of his language. There was almost complete consensus that rules play at best a marginal role as far as a speaker's linguistic competence is concerned. When linguists spoke of rules or laws they invariably envisaged formulas

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invented by scholars rather than principles that determine the form of utterances that speakers produce and understand.[2] For almost every linguist of this period a speaker's knowledge of the phonology of his language consists almost exclusively of knowledge of the phonetics of the individual words; i.e., the speaker memorizes each of the words that make up the lexicon of his language in such a way that he has no need for rules of any kind. From this perspective, the neogrammarian claim that sound laws admit of no exceptions is tantamount to the claim that sound laws affect the items in the lexicon not one-by-one but rather wholesale. This at once raises questions concerning the status of many well-known instances where change was not wholesale, where change affected one or a few lexical items. Since many of these instances were known to be the result of borrowing, it was proposed that the fact that a change did not affect the lexicon wholesale was to be taken as an indication that it was a borrowing. Since in numerous actual instances this was shown indeed to be the case, there was a tendency to feel that an empirical prediction of the neogrammarian hypothesis had been confirmed. What was overlooked was that if every instance of a change that failed to operate wholesale was to be classed as a case of borrowing, the claim that sound laws operate without exception becomes vacuous. Thus, what looked like a major victory for the neogrammarian view, on closer examination turns out to be devoid of significance. The difficulty of finding a way out of this impasse was, I believe, a direct consequence of the consensus alluded to above which relegated rules to a marginal status in a model of a speaker's linguistic competence. It was only when the consensus was destroyed with the rise of generative phonology in the 1960's that the issues could be reviewed in a new light and advances could be made towards resolving them . . . and that is both the end and the moral of the story told below.

It is a curious fact that when the neogrammarian claim that sound laws have no exceptions is normally presented in the literature it is invariably coupled with an attack on a counter-claim. The counter-claim which asserts that sound change is totally irregular is imputed by the neogrammarians to their opponents, although in actual fact such a counter-claim was never explicitly advanced by the opponents. All that they needed to refute the neogrammarian claim was some valid counter-examples, and to advance a counter-claim—especially one as obviously false as that imputed to them by the neogrammarians—was thus not only unwise, but also unnecessary. Nonetheless the presentation of the neogrammarian case has normally been structured so as to include this gratuitous straw man who is then demolished with great ease and relish. This gambit can be found in what is commonly regarded as the earliest public statement of the neogrammarian claim, Leskien's (1876) preface to his prize essay *Die Declination im Slavisch-Litauischen und Germanischen*:

> In this investigation I have proceeded on the principle (Grundsatz) that the shape of a case form transmitted to us is never based on an exception to the sound laws that are otherwise being followed. In order not to be misunderstood I would like to add: if by exceptions one understands instances in which the expected sound change did not take place for specific recognizable reasons; as, e.g., the absence of the (consonant) shift in German in the cluster *st* etc., where one rule interacts to a certain extent with another; under such circumstances there is nothing to object to the assertion that sound laws are not without exceptions . . . If, however, one admits any random accidental deviations which have no relationship one to another, then one asserts in effect that the object of the investigation—i.e., language—is not accessible to scientific scrutiny. (Leskien (1876) p. xxiv)

The same misleading construal of opposing views reappears—in rather shriller language—in Brugmann's[3] 1878 Foreword to the first volume of his and Osthoff's *Morphologische Untersuchungen*:

> Only he who holds fast to the sound laws, this main stay of our entire science, has in his researches any firm ground under his feet. However, he who without need, only in order to satisfy certain base instincts, ("nur um gewisse gelüste befriedigen zu können") admits exceptions from the sound laws that govern a dialect, he who either allows single words or word categories not to be affected by a phonetic innovation that demonstrably affects all other similar forms, or who admits a sound to arise sporadically, only in single forms, while other similar forms know nothing of the change; or, finally, who lets the same sound under identical conditions to be changed in that direction in some words, and in a different direction in others, and who moreover sees in all these highly prized unmotivated exceptions precisely what is normal, what follows by itself from the nature of mechanical sound change and even—as it commonly happens—makes these exceptions the foundation for further inferences that are supposed to destroy the consistency observed elsewhere by sound laws; such a person of necessity falls victim to subjectivity and arbitrariness; he may be able in some cases to advance rather ingenious hypotheses but none that deserve credence and may therefore, also not complain if he is met with cold rejection. (pp. xiv–xv)

In his rebuttal Schuchardt, who for a time acted as the spokesman of the opposition to the neogrammarian school, observed quite reasonably, "first, that systems of deterrence deserve no place in science and, moreover, that the proposed alternative is wrong even when formulated in less harsh terms." (Schuchardt (1928), p. 78) ("erstens, dass das Abschreckungsystem in der Wissenschaft keinen Platz verdient, und sodann, dass die aufgestellte Alternative, auch wenn sie minder schroff formuliert wird, falsch ist.") Moreover, Schuchardt pointed out that those who denied that sound laws

were exceptionless did not thereby also claim that there were no regularities to be found in sound change. "I would like to know," he wrote, "who among pre- or non-neogrammarian linguists (including my own insignificant self) is supposed to have regarded and treated sound change as chaotic." (p. 78) He remarks also that as far as he can tell "the postulation of the neogrammarian principle does not mark a turning point in the history of linguistics, following which linguistics began to advance more securely and more rapidly, nor will . . . a future generation be able to discover such a blessed turning point between Ascoli's 'Saggi ladini' and Osthoff's 'Tiefstufe im indogermanischen Vokalismus'." (p. 82)

In spite of the correctness of these remarks many later writers on the subject have continued to see the problem in much the same way as Leskien and Brugmann. Thus, Leonard Bloomfield, surely one of the most influential figures in linguistics between the two World Wars, wrote in 1932:

> If we sufficiently loosen our assumptions, we can 'explain' all facts, at the cost of abandoning all the classifications which we have made since the year 1816 or thereabouts. For instance, if we devise a consistent set of assumptions which allow of sporadic sound-change . . . we can 'explain' any and all changes of linguistic forms, but we must then abandon all results that were gained by an implicit or explicit use of the present assumption of sound-change. We could not then distinguish between sound-change and analogic change or borrowings . . . We should have to give up, to take the classical instance, the sound-change discovered by Verner. For nothing is more common than the (sporadic—MH) voicing of intervocalic spirants . . . Had Verner countenanced the possibility of a change which passed sporadically from form to form, he could never have found the correlation of pre-Gmc spirant-voicing with the place of the PIE accent. This example gets an added, if extraneous, interest from the fact that Verner, to all appearances, had not explicitly formulated the assumption of regular sound-change. (Bloomfield (1970) p. 245)

What Verner had written in the paper in which he communicated the important discovery that bears his name was:

> Comparative linguistics cannot, to be sure, rule out (the existence of) chance events altogether, but chance events en masse as here, where instances of irregular (consonant) shifts in word internal position are almost as frequent as those of regular (consonant shift), it (=comparative linguistics) can and must not admit. In such cases there must be—as it were—a rule for the irregularity; it remains only to discover it. (Verner (1876), p. 101)

This passage, however, is an embarrassment for Bloomfield and other proponents of the neogrammarian view for two reasons. First, in it, Verner expressly countenances the existence of sporadic irregularities in sound

change (his "chance events"), and yet this fact did not affect his ability to make significant discoveries, which the neogrammarians themselves widely quoted as supporting their views. Moreover, by distinguishing sporadic sound change from "chance events en masse" Verner illustrated the fact that one can admit sporadic irregularity without assuming that sound change is totally random. Since as we have seen, the possibility of making this distinction was denied by proponents of the neogrammarian views, Bloomfield could only continue to maintain these views by pretending that in spite of explicit assertions to the contrary, Verner really accepted the neogrammarian views, but had as yet "not explicitly formulated the assumption of regular sound change."

Although the case in favor of the neogrammarian postulate was, as we have just seen, not particularly strong on the face of it, it was widely accepted or acquiesced to. One reason for this is, I believe, that given the model of language that was almost universally accepted during most of the last hundred years, the neogrammarian postulate appeared to provide answers to questions that seemed otherwise intractable. It was generally believed during most of this period that sound change affected exclusively the lexical representations of words. While there was consensus that change exclusively affected the entries in the lexicon, there was disagreement about the character of the representation of these entries. Many linguists agreed with H. Paul, who believed that the speaker memorized all relevant phonetic properties of each word. He hypothesized that the speaker possesses a memory image (= Erinnerungsbild) for every word in his vocabulary and that it is these memory images "which establish the connection among the diverse physiological events and create a causal relationship between earlier and later productions of the same sound complex." (Paul (1909), p. 49)

Others like Kruszewski (1883) and, later, structuralists such as Bloomfield (1933) believed that the lexical entries contained only part of the phonetic information, and that the rest of the information was provided by allophonic rules. For example, Kruszewski distinguished the "dynamic sound laws", (his term for what other writers have called sound laws tout court) from "static sound laws", which captured other phonetic regularities. Kruszewski recognized two types of "static sound laws": the "law of the sound system" and the "law of sound sequences". The former corresponds to what in structuralist phonemics would be called the "phoneme repertory of the language". Under the heading of the "law of sound sequences" Kruszewski includes, on the one hand, the positional variants of the individual phonemes (e.g., he mentions here such facts as that "in German, aspiration accompanies not only the voiceless *k* sound, but also the entire series of voiceless plosives *k*, *t*, *p*" (p. 17)) and on the other hand, distributional restrictions on phonemes; e.g., the impossibility of having the sequence [kn] at the beginning

of French words (cf. p. 24). Kruszewski's "static" sound laws cover, thus, basically the same territory as structuralist phonemics. According to Kruszewski, the "static" sound laws are an active part of a speaker's knowledge of his language, for:

> ... all words without exception undergo what we have called here ("static"—MH) sound laws; i.e., the law of the sound system and the law of sound sequences. If a borrowed word has sounds that do not exist in the sound system of the borrowing language, they are invariably replaced by more appropriate native sounds. If the borrowed word has sound sequences that are not characteristic of the borrowing language, these are invariably replaced by sequences that conform to the laws of this language. (p. 61)

The "dynamic" sound laws, on the other hand, are historical changes that at one point in time affected the representations of lexical items, but do not do so at present. Kruszewski notes, for example, that although at one point Russian was subject to the "dynamic" laws that turn /ke/→/če/; /ro/ →/oro/; and /venC/→/uC/; in modern Russian "we have ***kegli*** and not *čegli*; ***krolik*** and not ***korolik*** (or ***korolek***); *venzel'* and not *uzel'*, etc., because in native Russian words we have these sounds and sound sequences; e.g., *ruke*, ***kroška***, *venčik*, etc." (Kruszewski (1883), p. 61)

What is especially important for purposes of the present discussion is that for Kruszewski it is only the "dynamic" laws that are of interest for the comparatist. The "static" sound laws are outside the ken of the comparatist because they do not affect the lexical representations. The same point was made fifty years later by Bloomfield by means of the succinct formula "phonemes change" ((1933), p. 354). Thus, both structuralists and non-structuralists agreed that the changes of interest to the comparatist are changes in the lexical representation of words. These changes, however, have nothing in common with any rules that speakers of a language might know.[4] Rather the changes in the lexica of different groups occur outside the awareness of speakers, by means of mechanisms that were never made clear. Hence the sound laws that describe these changes are formulas invented by linguists to characterize the regular correspondences that obtain between the sounds of cognate words in related languages, especially in the proto-language, on the one hand, and its individual descendants, on the other. It clearly is a picture much like this that Antoine Meillet, the most influential French linguist of the inter-war period, had in mind when he wrote:

> Although it is often impossible to recognize regular (sound—MH) correspondences between two languages deriving from a common source, the correspondences between the proto-language and each of these (daughter—MH) languages obey definite rules, which, moreover, can be stated with precision. It is these (rules—MH) that are called "sound laws. (Meillet (1924), pp. 30–1)

It should also be noted that given the model sketched above, when a sound change takes place, the lexicon of the affected language after the change is distinguishably different from the lexicon before the change. Since different languages—e.g., Kwakiutl and modern German—have distinguishably different lexica, it was concluded by a common, but mistaken form of inference that a difference in the content of lexica must invariably be taken as indicating a difference in language, specifically, speakers whose lexica differ as the result of a sound change are taken to speak different languages. This conclusion accorded well with Schleicher's *Stammbaum* theory, a central tenet of which was that when a sound law affects, as it commonly does, the speech of some, but not all speakers of a language, this language is now split into two distinct languages, that from this point onward may and do go their separate ways.

Sound laws account only for a part of the regularities that can be observed between cognate lexical items of a proto-language and its descendents. At least two other mechanisms of change have commonly been recognized: analogy and borrowing. Analogy, according to Bloomfield (1933), is the basic mechanism for mastering the various grammatical patterns of a language, both morphological and syntactic.

> When a speaker utters a complex form, we are in most cases unable to tell whether he has heard it before or has created it on the analogy of other forms. The utterance of a form on the analogy of other forms is like the solving of a proportional equation with an indefinitely large set of ratios on the left-hand side:
>
> *dog*: *dogs*
> *pickle*: *pickles*
> *potato*: *potatoes*   } =radio: X
> *piano*: *pianos*
>
> (Bloomfield (1933) p. 276)

It is not difficult to imagine how analogy can affect the evolution of the form of the words of a language; e.g., a lexical item may become included in an analogy set from which—for some reason—it was previously excluded.

While analogy raises conceptual problems of its own, which cannot be discussed here, its operation and effects are so different from the other two types of change that it is plausible that change due to analogy can readily be distinguished from that due to borrowing or to sound laws. To distinguish the effects of borrowing from those of sound laws is considerably less straightforward. What made it especially difficult was that given the prevalent model of language these two types of change must directly affect the represen-

tations of lexical items. At first sight it might seem that these two types of change might be differentiated solely on a numerical basis, for a change due to the operation of a sound law affects items throughout the dictionary, whereas a change due to borrowing is usually restricted to individual items. This way of putting the problem, however, omits from consideration the possibility, which cannot logically be excluded, that sound laws might affect only one or two words or might have sporadic exceptions. In such cases, it will not be possible to decide in every case on purely formal grounds whether a given change is due to borrowing or to sound change. Cases where the source of the borrowing is known will, of course, not present any difficulty as before, but there will be difficulty with cases such as the following discussed by Bloomfield:

> ... if we examine adjacent dialects in an area, we find gradation: some have apparently carried out a sound-change, as when, say, in Dutch, some districts ... have [y:] for ancient [u:] in the words *mouse* and *house*; next to these we may find dialects which have apparently carried out the change in some of the forms, but not in others, as when some districts ... say [hy:s] with the changed vowel, but [mu:s] with the unchanged; finally we reach districts where the changed forms are lacking .... (Bloomfield (1933), p. 361)

It might appear that these facts are the type of counter-example that the opponents of the neogrammarian claim might adduce in order to refute it. There is, however, another way of looking at facts such as these. We may assume that the neogrammarian claim is correct. In that case there is only one way of interpreting the above facts, namely, the odd distribution of the vowel in the different dialects must, then, be due to borrowing. Bloomfield is quite explicit on this point:

> Under a hypothesis of sporadic sound-change, no definite conclusions could be drawn, but under the assumption of regular sound-change, distributions of this sort can at once be interpreted: an irregular distribution shows that the new forms, in a part or in all of the area, are due not to sound-change but to borrowing. (Bloomfield (1933) p. 362)

Bloomfield adduces external evidence proving that the distribution in the Dutch example under discussion is in fact the result of borrowing (See *op. cit.*, pp. 329–31). And it is no doubt impressive that the claim should be supported under these far from self-evident conditions.

It can, however, not be overlooked that the procedure adopted by Bloomfield makes it in principle impossible to discover counter-examples to the neogrammarian claim, for given Bloomfield's procedure, once it is discovered

that a sound change has exceptions it is classified as a borrowing. The result of this is that the neogrammarian claim that sound laws have no exception now becomes a tautology, true by definition, rather than an empirical proposition.

It is apparently in this way that the neogrammarian claim is understood by Hoenigswald (1977). Noting that in Old Persian certain lexical items show the correspondence IE *g gh d dh*=OPers. *d*, whereas others show instead the correspondences IE *g gh*=OPers. *z* and IE *d dh*=OPers. *d*, Hoenigswald comments:

> We are free to consider the replacement of *g gh d dh* by *d* as well as the replacement of *g gh* and *d dh* by *z* and *d* respectively, either as examples of sound change along two lines of descent (true Persian and Median), or as an example of dialect borrowing interfering with the true line of descent for Old Persian. If this is how we reason, there is no occasion for surprise in Leskien's formula that sound change occurs without exception, since it is a tautology .... (Hoenigswald (1977) p. 181)

Hoenigswald is puzzled by the fact that it has "been possible again and again to raise the question of the regularity of sound change with the thought that not only is it a matter of observable fact (rather than of definition), but also that sound change can be irregular and that Leskien was wrong" (p. 181), and he finds it "odd that Leonard Bloomfield ... may have continued to regard this 'assumption' as material rather than definitional" (p. 183).

Like the proponents of the neogrammarian position Hoenigswald denies that sporadic sound change other than that resulting from borrowing has ever been shown to exist:

> Advocates of 'sporadic' sound change ... will have their claim neither proved nor faulted until they can specify the circumstances under which competing regularities are to be interpreted neither as (a) 'analogic' ... nor as (b) representing different channels of transmission, including the one dubbed the main channel, but instead as (c) 'sporadic' sound change. If this is only a case of labelling, in all sobriety, regularities of nonanalogical origin for which we cannot name or care to construct a reasonable historical background (the Medes or the Scandinavians) 'sporadic' means no more than 'somehow competing' and is noncommital. It is, however, sometimes said that such processes tend to have special phonetic properties; reportedly they are distant dissimilations and assimilations, metatheses, and haplologies, and the like. In actual fact, however, these so-called minor sound changes seem to offer not more difficulty to interpretation than the major ones such as, say, ordinary and 'regular' contact assimilations ... As a matter of fact, the 'exception' to the more ordinary sound laws are often far richer than what we have here. This, however, is not even the issue. It seems that sporadic sound change is either a contradiction in terms or merely a traditional and not particularly well chosen collective designation for other than main-channel material. (pp. 182–3)

Hoenigswald is correct that "advocates of 'sporadic' sound change" will not have their claim proven unless they can show that a specific exception is due neither to analogy nor to borrowing, but to something else. But he is mistaken in assuming that this something else must be an otherwise phonetically unprecedented type of process. There, surely, are other ways in which the challenge may be met. In fact it is not difficult to meet this challenge provided that the assumption that sound change affects only the lexicon is abandoned and replaced instead by the view now generally held among generative phonologists of all persuasions that attributes to rules a major role in phonology. Like the Kruszewski (—structuralist) model, the generative phonology model distinguishes between phonological information that is directly given in the lexical representation of items, and phonological information that is provided by rules, but unlike the former it sees no reason to restrict language change to processes that affect the lexicon directly. In the generative phonology model each of the two main components can undergo change independently of the other. Thus there can be change in the representation of lexical items and there can be change in the rules; in particular, new rules can be added. It is the latter type of change that traditionally has been presented as being due to the operation of sound laws. Change in the representation of lexical items, on the other hand, is due to other factors: analogy, borrowing, and other irregular adjustments in the lexicon, which, as Bloomfield aptly noted, is the repository of all idiosyncratic data about words. It is the latter type of change that is commonly designated as 'sporadic'. For example, in contemporary English the change from [ɛ] to [iy] in the pronunciation of the word *economics* is a 'sporadic' sound change. Since in the generative phonological view sound laws are phonological rules that play—or once played a role in the language under discussion, sound laws must be subject to the same constraints and limitations as phonological rules. These narrowly restrict the kinds of change that a given sound can undergo, and determine what changes may be subsumed under a single law. It is the constraints that provide an answer to the question, with which we have been concerned above, whether sound laws admit exceptions. This question now reduces to whether or not phonological rules admit exceptions. Our experience with phonological rules has not been extensive enough to allow us to offer more than an educated guess as to the answer, and that is that phonological rules do in fact admit exceptions.

NOTES

1. I follow here the lead of P. Kiparsky (1974). While Kiparsky's conclusions emerged from a study of the views of Bopp and of certain scholars who challenged these

views (in particular, Benfey), they are fully in accord with the results of my own readings in the (later) polemical writings of the neogrammarians and their opponents. Because of the great interest of Kiparsky's remarks I quote them *in extenso*: "Since so many advances in comparative grammar were made by the neogrammarians, it is natural to think of them as the initiators of the revolution in the field. But the revolution was begun in the decade *before* the neogrammarians made their appearance. Furthermore, the new paradigm of historical explanation was in the 1870's accepted by linguists who stood on both sides of the controversy about the neogrammarian theory of sound change. There exists a most regrettable myth that the belief in necessarily exceptionless sound change accounts for the discovery of the many new sound changes in the 1860's and 1870's. For example, Bloomfield (1933: 355) attributes Grassman's discovery to this belief, and similarly Verner's discovery (1933: 359). But neither Grassman nor Verner in fact believed that sound changes could not have exceptions. And why did linguists who specifically rejected the neogrammarian theory of sound change once it had been formulated in 1870's, such as Collitz, Ascoli, J. Schmidt, discover sound laws which were just as significant as those discovered by neogrammarians, such as Brugmann, Osthoff, and Leskien? Neither side in the disagreement over the sound change issue seems to have had a bad effect on the linguists who embraced it. In fact, the two groups of linguists scarcely differed in the kind of actual historical linguistics they did. It is hard to see how a theory with so few actual consequences for comparative work should be considered a "breakthrough." A more important change in the field was surely the acceptance of the new paradigm of historical explanation. This far exceeds the sound change debate in productivity and concrete consequences, and is a far more likely historical source of the new discoveries in phonology. The disagreement between the neogrammarians and their contemporary opponents seems utterly insignificant compared to the rejection by both groups of the Bopp-Schleicher-Curtius paradigm." (340–1)
2. The inability of the nineteenth century linguists to think of phonological rules of significant complexity as constituting an essential component of a speaker's linguistic competence makes it rather unlikely that T. Vennemann (see Vennemann and Wilbur (1972)) is correct when he attributes to Schuchardt discoveries about how phonological rules might be generalized in the course of time. In order for Schuchardt to have thought of this he must have had explicit ideas about the nature of the rules to be generalized. Since there is no trace of such an idea in Schuchardt's writings—as Vennemann himself points out—it is groundless to attribute to him a discovery that crucially presupposes this idea.
3. Although signed also by Osthoff the preface was the work of Brugmann alone. See Jankowsky (1972), p. 124 n.
4. A notable exception to this is Jakobson (1929), who attempted to show how changes in the allophonic rules of a language could affect its phonological system. Although it was of the greatest interest and dealt with questions of vital concern, Jakobson's work was all but ignored for the first twenty years after its publication. Though more widely quoted since then, it has yet to have the impact on the practice of comparative linguistics that it would seem to deserve. I have attempted to sketch the main theses and results of this work in Halle (1976).

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