

'EXCEPTIONS' TO EXCEPTIONLESS SOUND LAWS

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Kurt Jankowsky has recently decried the tendency to view the 'sound law' as the sole contribution of the Neogrammarians to linguistics (1972: 3, 188). In truth, of course, the concept of the sound law did not originate with the Neogrammarians themselves. It first appeared in print in 1875, but the idea had been developing for some time before that (see Graff 1932: 240ff, and Pedersen 1962: 292ff). It was Wilhelm Scherer, reviewing the German translation of a series of lectures by Whitney, who first declared (1875: 107):

"Die Veränderung der Laute, die wir in beglaubigter Sprachgeschichte beobachten können, vollzieht sich nach festen Gesetzen, welche keine andere als wiederum gesetzmässige Störung erfahren."

Leskien (1876: 3) first used the term *ausnahmslos* in this connection, and Osthoff and Brugmann repeated the claim in their declaration of Neogrammarian principles:

"Aller Lautwandel, so weit er mechanisch vor sich geht, vollzieht sich nach *ausnahmslosen Gesetzen*, d.h., die Richtung der Lautbewegung ist bei allen Angehörigen einer Sprachgenossenschaft, ausser dem Fall, dass Dialektspaltung eintritt, stets dieselbe, und alle Wörter, in denen der der Lautbewegung unterworfenen Laut unter gleichen Verhältnissen erscheint, werden ohne Ausnahme von der Änderung ergriffen" (1878: 1.XIII [emphasis in original]).

In the subsequent debate over its validity the concept was capsulized in the phrase 'die Lautgesetze wirken ausnahmslos', a slogan as imprecise as it is dramatic, and a fairer statement of the Neogrammarian position is probably that of one of their students, Bloomfield (1933: 364):

"The neo-grammarians define sound change as a purely phonetic process; it affects a phoneme or type of phoneme either universally or under certain strictly phonetic conditions, and is neither favored nor impeded by the semantic character of the forms which contain the phone."

Unfortunately, not only has this principle overshadowed the rest of the Neogrammarians' own work, its influence on other linguists has been highly exaggerated as well. In a new introduction to historical linguistics, Anttila (1972: 78) joins King (1969) and Postal (1968) not only in rejecting strictly phonetic conditioning of sound change, but also in imputing the latter view to most historical linguists. King offers his own restatement of what he terms the 'stronger' version of the 'regularity hypothesis' (H_1):

"Phonological change is regular, and its environments can be stated in strictly phonetic terms" (1969: 120).

He rejects the hypothesis expressly for its failure to account for all observable facts, and offers two counterexamples.¹ The first concerns the retention of final *-e* [ə] in Yiddish adjective endings. Middle High German final *-e* is normally lost in Yiddish: *tage* > *tag* 'days', *erde* > *erd* 'earth'. But as an adjectival ending *-e* is retained: *di groyse shtot* 'the big city', *dos alte land* 'the old country'. In King's view these facts cannot be explained in purely phonetic terms, but must be regarded as an example of morphological conditioning of a sound change: the loss of *-e* does not take place in adjectives. The second example, borrowed from Postal, is the non-insertion of epenthetic *e* in Mohawk when the sequence *-kw-* consists of the first person marker plus the plural morpheme. Thus Mohawk *kewi'stos* ~ Oneida *kwi'stos* 'I am cold', but Mohawk *ya'kwaks* ~ Oneida *ya'kwaks* 'we several exclusive eat it'. Again King concludes that the only satisfactory explanation is to assume morphological conditioning (King 1969: 123–4).

Having offered his own arguments against the regularity hypothesis, King then asserts (1969: 125–7):

"the strictly phonetic version of the regularity hypothesis has been held by the majority of the linguists working in the historical field, certainly by those in the Neogrammarian tradition"

and ascribes to this state of affairs what he considers an unfortunate consequence for historical linguistics:

"phonological change not happening to conform to H_1 is forced into categories of change such as analogy and borrowing. The latter categories, in particular analogy, thereby tend to become terminological receptacles devoid of explanatory power – catchalls for irregularities in the operation of 'regular sound laws'."

¹ I am not concerned here with the validity of these examples, which has been challenged (see e.g. Jasanoff 1971: 81–2). My reason for including a brief account of them here will soon become apparent.

Paul Postal, having quoted Bloomfield's formulation given above, makes a similar assertion as to its status (1968: 239):

"We thus see that modern linguistics is in general dominated by this traditional Neogrammarian conception of sound change. This view can be taken as the generally accepted view of the nature of sound change today, a view enshrined in our textbooks and manuals."

He likewise notes with disapproval the received view of regular sound change and analogy as mutually exclusive and opposing processes (1968: 234).

Given the qualifiers 'majority' and 'in general', a strict confirmation or refutation of the above statements would require a survey of the work of all historical linguists of the last ninety-odd years, an undertaking patently beyond the scope of this paper. I do intend to show that, at the very least, opposition among historical linguists to the idea of strictly phonetic conditioning of sound change has been widespread and continuous, in no sense isolated or restricted to certain areas (such as Romance linguistics).² Furthermore, there is considerable evidence that the stronger form of the Neogrammarian hypothesis, far from dominating current historical linguistics, is today virtually without adherents.

Rejection of strictly phonetic conditioning does not necessarily mean acceptance of morphological conditioning in King's sense. While earlier analyses involving conditioning in terms of grammatical categories do exist (see 22 *infra*), they are relatively rare. Most proposals for non-phonetic conditioning have come along two major lines. The first of these, which I call the FREQUENCY argument, claims that phonemes or sequences of phonemes in very frequently used words undergo sound changes beyond the scope of 'regular' sound change. These cases usually involve the loss of phonetic material, not changes in distinctive features. The FUNCTIONAL argument asserts that a functionally important phoneme or phoneme sequence, such as an inflectional ending, is 'protected' from the operation of an otherwise regular sound change. Conversely, according to this explanation, the irregular loss of phonetic material in overused words is due not to their frequent occurrence, but to their loss of real meaning. In general, functionally important ele-

² King (1969: 134) mentions Schuchardt, and Postal (1968: 264) cites the work of Kurylowicz, but the clear implication is that these are isolated exceptions.

ments are preserved, superfluous elements are eliminated, in defiance of regular sound laws.³

It is noteworthy that, while the explanations differ, King's examples are reminiscent of those presented by other opponents of strict phonetic conditioning: they involve the retention or loss of phonetic material, not changes in features, and the phonemes affected are in inflectional affixes.⁴ We shall also see in the historical survey which follows that King and Postal are not the first to object to the explanation of sound change exclusively in terms of the opposition: regular sound change versus analogy and borrowing.⁵ It was just this search for additional factors in sound change, a search motivated originally by the need to explain a relatively small number of marginal forms, that led to the development of the frequency and functional arguments, which have since been extended to apply to a wide range of phenomena usually explained in terms of regular sound change and analogy. It is the history of this development that I will now attempt to trace.⁶

Both the frequency and the functional arguments have long genealogies, reaching back beyond the appearance of the Neogrammarian hypothesis itself. As an example of the first, we may cite Pott's approval of the etymology Ital. *andare* 'go, walk' < Lat. *ambulare* "mit zwar ungewöhnlichen, aber durch die Häufigkeit des Gebrauches von diesem Worte gerechtfertigten Buchstabenwechseln: mbl > nd" (1852: 315). Georg Curtius, who later was one of the more vocal critics of the Neogrammarians, had already stated the core of the functional argument in 1870. In 'Bemerkungen über die Tragweite der Lautgesetze' he opposes the principle of *Bequemlichkeit*, which tends to abbreviate

³ This sense of 'functional', which I borrow from Horn (1921), is distinct from the 'functional load' (*rendement fonctionnel*) of a phoneme or phonemic distinction, although both concepts have sometimes been advanced by the same linguist (see Jespersen 1904: 196; and Martinet 1952: 1-10).

⁴ There is, of course, no reason *a priori* why morphological conditioning could not apply to rules involving changes in features, but the coincidence between King's examples and those of others is striking. The same applies to those of Anttila (1972: 78-80).

⁵ As Postal points out (1968: 235n), it is not entirely certain that the Neogrammarians considered analogy and sound change as strictly opposing processes (see e.g. Paul 1920: 209ff), but the main point remains: their opponents view analogy and regular phonetic change combined as insufficient to explain all sound change.

⁶ This survey is by no means exhaustive. For further examples before 1921 see Horn 1921: 124-35. For two further examples since 1921 see Morsbach, *Grammatisches und psychologisches Geschlecht im Englischen* (1926: esp. 6 and 37), and Lejeune, *Encyclopédie française* I (1939), part 2, chap. II, 1-34-13.

utterances and blur distinctions, to that of *Deutlichkeit*, the striving of every speaker to be understood:

"Die Bequemlichkeit wird sich in solchen Silben und Wörtern am meisten geltend machen, die für die Bedeutung kein grosses Gewicht haben, in solchen aber am wenigsten, die am meisten von Bedeutung erfüllt sind" (1870: 6).

He finds a particularly clear application of this principle in the treatment of Greek *oi* before a vowel. Usually it is simplified to *o*, as in occasional *poēō* for *poiēō* 'I do, make', or in the genitive singular ending where *-oio* > *-oo* (> *ou/ō*). But in the optative it always remains: thus *poioiēn* 'I would make', *doiēn* 'I would give', etc. Curtius' explanation (1870: 25) is that the genitive singular ending is still recognizable after the loss of *i*, so the latter is expendable, while the *i* of the optative forms is essential to their characterization and is therefore retained.

The promulgation of the Neogrammarian principle did not mean the end of either the frequency or the functional argument. Thomsen supports the derivation of Fr. *aller*, It. *andare*, etc. < Lat. *ambulare* and remarks (1879: 417):

"... this verb belongs to a group of words which in every language stand, so to speak, more or less outside the sound laws, i.e., words which by reason of their frequent use [and their subsequent easier intelligibility even in slurred pronunciation] are exposed to far stronger and more violent changes than other words, and which therefore in part go entirely their own way."

As Jespersen points out (1933: 196), the original version of the article without the words in brackets is a clear expression of the frequency argument, while the addition of the phrase in the 1920 reprinting tends to recast the statement along functional lines.

Curtius, in his 1885 polemic against the Neogrammarians, *Zur Kritik der neuesten Sprachforschung*, repeats in somewhat blunted form the functional argument of fifteen years before. The assertion quoted above has now become a question:

"Eine mehrfach erörterte Frage ist die, inwiefern bei den Sprechenden ein Gefühl für die Bedeutsamkeit der Laute und Silben als mitwirkend bei der Entscheidung zwischen Erhaltung und Veränderung vorauszusetzen ist" (1885: 70).

For Curtius the answer is still plainly yes, but he does not repeat the example of Greek *oi* and limits himself to not especially convincing

negative evidence concerning the resistance of root syllables to analogy (1885: 72–3). Perhaps he feels it is not necessary to repeat previous examples. If he is no longer as certain of the functional argument, he has no doubts that *some* explanation *other* than regular sound change and analogy is needed:

“Es mag hier darauf hingewiesen werden, dass es noch eine ganze Reihe von Lautveränderungen gibt, welche weder auf Lautgesetzen, noch auf rein lautlichen Neigungen, noch auf Analogiebildungen, sondern auf wiederum andern, bisher noch wenig oder gar nicht berührten Trieben des Sprachlebens beruhen” (1885: 84).

In a reply to Curtius' criticism Delbrück (1885: 25) returns to the example of Greek *oi*. He sees no justification for the functional argument, explaining the *oi* of optative forms like *doīēn* as analogical to other forms of the paradigm where the retention is regular (*doīmen*, *doīte*). The functional argument, however, had other proponents besides Curtius. In his first discussion of sound laws in 1886 Jespersen attacks the use of the frequency argument in explaining cases like *aller* < *ambulare*. To use another popular example, Jespersen contends that if *guten Morgen* were reduced to [gm̩] or [gm̩in] due to its frequency of use, then *Morgen* would be so reduced in all its occurrences. He concludes (1886: 174–5):

“Nicht die Häufigkeit, sondern die allerdings in Beziehung zu der Häufigkeit stehende *Leichtverständlichkeit und Wertlosigkeit* für die Auffassung des Sinnes des Sprechenden gestattet bei gewissen Worten und Wortverbindungen eine exzeptionelle Lautentwicklung. Hierdurch sind wir aber in offenbaren Streit mit der orthodoxen junggrammatischen Lehre geraten, dass *aller* Lautwandel nach ausnahmslosen Gesetzen vor sich geht, welche auf Bedeutung oder häufigen Gebrauch keine Rücksicht nehmen” (emphasis in original).

Regarding the corollary that meaningful sounds and syllables tend to be conserved, Jespersen criticizes Delbrück's outright rejection of the principle, but says that he cannot cite sure examples, because in most cases there are indeed forms present in which the sound is regularly preserved, and one can thus explain the others by analogy (1886: 176–8).

The frequency argument did not lack supporters either. Schuchardt, again apropos of the etymology Fr. *aller* etc. < Lat. *ambulare*, compares the development of It. *bisogna* ‘it is necessary, *il faut*’ in the Northern Italian dialects (namely *bogna*, *bigna*, *sogna*, *biso*, *bgna*, *bia*) and asserts (1891: 529n):

“Wir haben hier quantitative Veränderungen, welche ausserhalb der ‘Lautgesetze’ liegen, und ihre Ursache ist in der Überhäufigkeit des Gebrauches zu suchen; wie ich schon anderswo gezeigt habe, kann man sich in solchen Fällen nicht auf Tonlosigkeit der Wörter beziehen, da diese selbst erst eine Folge der Überhäufigkeit ist, da ferner die sonst in unbetonten Silben herrschenden ‘Lautgesetze’ nicht beachtet erscheinen, und da endlich auch betonte Wörter bei Überhäufigkeit verkürzt werden. Vgl. Tosk. *gua* < *guarda* ‘Blicke!’.”

Schuchardt is, of course, one of the most famous opponents of the Neogrammarian hypothesis, and for a full-scale theoretical discussion one may turn to his *Über die Lautgesetze* (1885). I have chosen the excerpt above because it deals specifically with non-phonetic conditioning.

As attested by the citations above, which could be multiplied, the Neogrammarian view had hardly carried the day in the debate which had reached its peak in 1885. In fact, by 1900 Wechssler begins his defense of sound laws with the following:

“In der Praxis haben sich die Lautgesetze jedenfalls bewährt: niemand möchte mehr auf ihre Anwendung verzichten, niemand will heute ohne triftigen Grund Ausnahmen von einer sonst beobachteten phonetischen Veränderung zugelassen wissen. Gegen die Theorie jedoch haben die Gegner schwerwiegende Einwände vorgebracht, welche nicht immer widerlegt worden sind, und sie wird heute nicht mehr mit demselben Nachdruck verteidigt wie früher: manchen Gelehrten gilt sie als verlassener Posten” (1900: 1).

While one might suspect that Wechssler has overstated the case in order to increase the impact of his own defense, it is hardly credible that he would have made such a statement had the Neogrammarian view ‘dominated’ historical linguistics of the time, and we shall see that his view of a disparity between theory and practice is echoed by linguists of later periods. Wechssler himself upholds the validity of sound laws, rejecting both frequency and function as factors in ‘sporadic’ changes like *aller* < *ambulare*. He claims that these are due to enclisis or proclisis and thus phonetically conditioned by accent, even if the precise details remain to be worked out in some individual cases (Wechssler 1900: 134).

Both the frequency and functional arguments continue into the 20th century, and the impression that the Neogrammarian view is losing ground is confirmed by the fact that proponents of the two aforementioned arguments now tend to address themselves to each other, as if the existence of exceptions were no longer in doubt, but only the matter of how best to explain them. Gauchat rejects Wechssler's criticism and maintains (1905: 219n):

"Comme un mot que nous écrivons très fréquemment prend caractère de sigle, de même nos organes acquièrent une certaine paresse à prononcer un mot mille fois répété. Ainsi il arrivera à un grammairien de dire part 'cipe, mais pas de dire al 'bi pour alibi, char 'vari pour charivari."

This view is upheld by Morf in his review of Gauchat (1905: 448n).

Meillet in turn argues against this explanation. He cites the *koiné* forms *triánta* 'three hundred' < *triakónta* and *penténta* 'five hundred' < *pentēkónta* and the Latin names for the divisions of the *as*: *dextans* '5/6' < *de sextans* and *dorans* '3/4' < *de quadrans*. In his view neither the Latin nor the Greek forms

"ne se laissent pas expliquer par la simple application des lois phonétiques ordinaires et présentent des raccourcissements. Dans l'exemple latin et dans l'exemple grec, on voit qu'il s'agit de mots formant des groupes définis de formes analogues, et où la suppression d'une partie ne nuisait pas au sens" (1913: 27).

He expressly denies the effect of frequency of use, which he says tends rather to fix the correct form in the mind. Meillet sees part of the explanation in syntactic terms:

"L'un des principes essentiels de l'alteration est sans doute l'abrègement qui est la conséquence naturelle de l'union avec un autre mot."

Thus Lat. *seniorem* has two reflexes in French: *sieur* before proper names and *seigneur* elsewhere. Note that for Meillet this is syntactic, not phonetic conditioning — *seniorem* is stressed in both instances. But he also gives cautious assent to the argument for function as a factor in such changes:

"Une circonstance qui favorise éminemment la mutilation et dont l'effet vient s'ajouter à celui de l'abrègement est que les mots accessoires de la phrase n'attirent pas l'attention du sujet parlant ... et qu'en insistant sur ces mots, on ferait venir à la conscience de l'auditeur un élément inessentiel des choses exprimés par l'ensemble de la phrase" (1913: 27).

Thus far the examples cited of the frequency and functional arguments have dealt with a rather limited number of phenomena, in some cases with the very same material. But as I indicated earlier, the history of these two hypotheses has not been that of retreat before the Neogrammarian principle, but rather of elaboration and generalization. Perhaps the most ambitious and comprehensive statement of the functional position is that of Horn, *Sprachkörper und Sprachfunktion* (1921).

He first recalls the Neogrammarian dichotomy of regular sound change and analogy. Citing previous opposition to this view, some of which we have just seen, he then goes on to propound the functional argument as given previously: functionally important sounds tend to be preserved, and functionless parts of speech to disappear, in defiance of 'sound laws'. He realizes the obvious contradiction of the Neogrammarian principle, but suggests (1921: 3):

"Sollte es nicht möglich sein, durch Beachtung dieses Zusammenhangs alte Schwierigkeiten zu meistern, denen die Wissenschaft mit Lautgesetz und Analogie vergeblich beizukommen suchte?"

For Horn the functional argument applies not just to a few unusual contractions and expressive forms, but to a wide range of data which, in his view at least, cannot be explained in terms of sound laws and analogy. He supports this contention with some 120 pages of examples, from which I have chosen two. As an example of an irregular loss due to weakening of function, he cites the 'endless datives' of Old English in such forms as *tō dæg* 'today', *æt hām* 'at home' and *þy seofodan dæg* 'on the seventh day'. His explanation is that when the locative sense is expressed by a preposition or the instrumental form *þy*, just in those cases the final *-e* of the noun is lost, because it is superfluous (1921: 51–2). The corollary that functionally important sounds are irregularly preserved is exemplified by the Old English first singular present ending *-u*. Normally final *-u* is lost in Old English after a heavy syllable. Yet one finds the verb forms *bindu* 'I tie', *helpu* 'I help', not **bind*, **help*. Horn is well aware of the explanation of these forms by analogy with those with a preceding light syllable, where the retention is regular: *beru* 'I carry', *nimu* 'I take'. He reasons that if this were due to analogy, the same thing should happen in nouns, where we find instead the phonetically regular reflexes: fem. sg. *gifu* 'gift' but *ār* 'honor', neut. pl. *fatu* 'vessels' but *word* 'words'. Therefore he believes that the final *-u* in verbs has been retained due to its important function as the marker of the first person singular: "Die Funktion war stärker als das Lautgesetz" (1921: 23–4).

Such a large-scale extension of the functional argument as represented by Horn's book did not go unnoticed, and it called forth a response by Hirt, who summarily dismisses the idea (1927: 1.129–30):

"Dass, wenn die Endung oder ein Vokalwechsel wie der Ablaut zum Ausdruck einer Bedeu-

tung, also eine Funktion geworden ist, dass sich dies dann ausdehnt auf analogischem Wege, das kommt oft genug vor. Aber ich bestreite es, dass irgendein Laut oder Silbe deswegen erhalten bleibt."

Sturtevant (1924: 52) is not so ready to reject the functional argument out of hand:

"It is quite possible for the requirement of intelligibility to hinder the spread of a new articulation. This does not mean that a speaker says to himself: 'I will not pronounce thus and so, because I may not be understood'. He will at first use the pronunciation as well where it causes the loss of a useful linguistic distinction as elsewhere, but when he is not understood and is asked to repeat his sentence, he will substitute the alternative pronunciation."

Like Jespersen, however, Sturtevant cannot offer compelling examples for this half of the functional argument because the possibility of analogy is always present as well, and he concludes that the two factors probably complement each other.

We have seen that as early as 1900 Wechssler viewed the Neogrammarian hypothesis as a 'deserted post', although the regularity of sound change was still respected in practice. By 1932 Graff is prepared to go further:

"Today it would be hard to find a linguist who still believes in the theoretical identity of the laws of sounds with those of nature or in their exceptionless application, although in practice it is taken for granted that the established correspondences in the history of sounds have to be respected if etymologizing is to be given any credit" (1932: 241-2).

The reference to the 'identity of the laws of sounds with those of nature' is unjust, because the Neogrammarians themselves make it clear that sound laws are not 'laws' in the sense of physical laws (see e.g. Paul 1920: 68). But the rest of the statement squares well with what other historical linguists have said, before and since. Other of his views on the subject of sound change have a familiar ring as well:

"Yet it has never been shown that those sounds of a word which at a given time were felt to be the carriers of some semantic part or whole were eliminated while this feeling was still alive. Besides, it is inaccurate to oppose as essentially different the effects of analogy to those of the so-called phonetic law . . . In every language we meet with the phenomenon of very advanced phonetic corrosion or contraction in words and formulas of especially frequent occurrence . . . *s'il vous plaît* > [splɛ], *guten Morgen* > [moɪn]" (Graff 1932: 245-8).

Charles Bally (1932: 21) recounts a whole series of phenomena

which he feels fall outside the scope of sound laws and analogy:

"La linguistique historique commence à reconnaître que les changements phonétiques ne sont pas des phénomènes de nivellement qui passent uniformément sur toute la surface de la matière phonique. Les mots soumis à une 'loi phonétique' ne la subissent pas tous de la même manière, mais diversement selon la catégorie à laquelle ils appartiennent, ou (ce qui revient au même), selon le rôle qu'ils jouent dans le discours. Un mot autonome ne change pas comme un mot agglutiné dans un groupe (cf. 'Cela peut être vrai' et 'C'est p't-être vrai'), un mot simple comme un mot analysable dans ses parties, un mot proprement dit comme un mot grammatical (cf. 'calque' et 'quelque' prononcé souvent 'quèque', 'de lait' et 'de les' devenu 'des'), un mot usuel comme un terme technique et rare, un mot onomatopéique comme un mot qui a perdu ce caractère (pipare: pépier vs. pipiorem: pigeon), un mot désignant une notion pure et simple comme un mot chargé d'affectivité (cf. 'le cochon domestique' et 'ce cochon de domestique'), etc. etc."

Bally believes that all these deserve more careful study and cites the works of Horn and Jespersen which we have quoted as first steps in that direction. Several of his examples, however, are clearly open to another explanation – namely, morphological conditioning of sound change in terms of syntactic function along the lines of King, and in view of Bally's reference to 'catégorie' and 'le rôle dans le discours' it seems likely that the latter's views lean in that direction, although obviously they would not be stated in generative terms.

Jespersen, reviewing in 1933 his remarks on sound laws of half a century before, feels his earlier comments have been fully vindicated. He refers to the works of Horn, Graff and Bally as recent support for his viewpoint and repeats some of Horn's examples. He concludes with evident satisfaction (1933: 223-4):

"Heutzutage sieht man ohne weiteres ein, dass es unrichtig war, den Satz aufzustellen: 'die Lautgesetze wirken ausnahmslos'. Falsch ist auch der Gegensatz Lautgesetz und Analogie: die beiden Vorgänge sind nicht absolute Gegensätze, da es Zwischenstufen gibt, und jedenfalls können sie selbst beide zusammen, die Möglichkeit der sprachlichen Änderung nicht erschöpfen – nicht einmal wenn man Entlehnungen als dritten Faktor mit einbezieht. Viele lautliche Änderungen sind nicht gesetzmässig, brauchen aber deshalb nicht als analogisch bezeichnet zu werden: umgekehrt gibt es psychologisch bedingte Änderungen, die durchaus nicht unter den Begriff 'Analogie' fallen."

Horn in 1921 had extended the functional argument far beyond its previous bounds. Three decades later, Martinet, starting from another point of view, generalized the functional argument along somewhat different lines. There can be no doubt, however, that he is expressing the same basic functional argument. Witness the following description:

"We do not mean hereby that the phonetic trends launched by, say, a strong stress accent will develop blindly, ruthlessly destroying any piece of linguistic machinery that happens to be in their way. If the vocalism of unstressed endings plays in the economy of the language too important a role to be wiped out, speakers may be induced to save some of their articulatory energy for the final syllable of a word. When, as it seems, prehistoric Latin developed a word initial stress, wide-spread blurring of vocalic timbres took place in medial syllables, but final syllables, in which lay the expression of most morphological categories, were hardly affected" (Martinet 1952: 31).

Martinet elaborated his views in a succeeding article. Since they seem to have been misinterpreted,⁷ I will quote his opening remarks at some length (1953: 1):

"The basic assumption of the functional approach to diachronic phonology is that the distinctive role played by a phonemic opposition is one of the features involved in its preservation or eventual elimination. In the traditional jargon the proposition might be summarized thus: phonetic laws *do not* work blindly . . .

The blunt rejection of one of the assumed neo-grammarian tenets might mislead some readers into believing that these lines are what they are not, namely, one more attempt to prove that there are exceptions to phonetic laws, and that these exceptions are determined by the peculiar function of the specific words in which they are found to occur. E.g., phonetic deterioration should be more rapid in words and formulas with little communicative importance (a good many of the so-called allegro forms). The phonemic make-up of some words might be strengthened when they correspond to concepts heavily laden with affectivity. Such exceptions do exist, and they certainly deserve more attention than has been given them in some linguistic quarters. But they should be studied less with the intention of using them as war-machines against the views of scholarly opponents than as phenomena whose observation should lead toward the establishment of new principles of phonological explanation. Yet in spite of their considerable theoretical and practical importance, they are to be conceived as somewhat marginal in comparison with the cases where, in a given environment, all the realizations of a given phoneme are submitted to the same, usually gradual shift. We shall be concentrating here on 'regular sound laws', purposely disregarding such deviations as would neither weaken nor strengthen our case for the role of function in sound change.

It should be clear that diachronic phonemic theory is based upon the assumption that, apart from well defined cases, the meaning, function, or use of a given word cannot influence the phonetic evolution of its phonemic components. It is clear that if, in synchronic descriptions, we are able to ascribe all the sounds of a language to a definite number of phonemes, it is because all the realizations of a given phoneme in a given context are, as a rule, found to shift in the same direction and at the same rate of velocity."

Obviously Martinet feels that to repeat examples like *aller* < *ambulare* would be gratuitous. He is seeking a much wider application. He dis-

⁷ Postal (1968: 237) cites Martinet as a supporter of the Neogrammarian hypothesis and quotes the section beginning: "It should be clear that . . .". The preceding context and the careful qualifier 'apart from well-defined cases' show that Martinet does not mean 'exceptionless' when he says 'regular', and that it is such a 'well-defined case' of non-phonetic conditioning that he proposes to pursue.

cusses the merits of Germanic umlaut for his case (1953: 3-4) and rejects it, because in some cases the conditioning element is still present after the umlaut (e.g., Germ. *Kraft*: *kräftig*), and he wants an unequivocal case of functional, not phonetic conditioning. He chooses instead the 'infection' of Old Irish consonants: e.g., the *-r* of nom.-acc. sg. *fer* 'man' has the neutral or a-quality because it was once followed by the endings **-as*, **-an*; the *-r* of the voc.-gen. sg. *fir* has palatal quality because of the former endings **-e* and **-i*, and that of dat. sg. *fiur* has u-quality because the dative ending was once **-u* (example from Thurneysen 1946: 97). In a lengthy discussion which we need not repeat here, Martinet seeks to disprove the common assumption that *every* consonant was conditioned by a following vowel⁸ and to show instead that the change, though thoroughgoing and systematic (thus 'regular'), only occurred in those places where it was necessary to preserve grammatical distinctions threatened by the loss of medial and final syllables. He summarizes (1953: 5):

"Anyone inclined toward functional explanations would consider infection as a clear case of the preservation of distinctive features that were threatened by impending syncope or apocope. Had the last syllables of Proto-Irish words disappeared without leaving any trace, all the cases of the singular of the Old Irish equivalent of Latin *vir* would have been identical; actually we have three distinct forms: nom.-acc. *fer*, voc.-gen. *fir* and dat. *fiur* mainly distinguished by the coloring of *-r*."⁹

I have now discussed two elaborations of the functional argument, but thus far the frequency argument has remained restricted to what Martinet rightly terms 'marginal' cases. This gap has recently been filled by Mańczak's *Le développement phonétique des langues romanes et la fréquence* (1969). He first enumerates various exceptions to 'sound laws', claiming, like Schuchardt before him, that *atonality* is not sufficient to explain sound changes like Lat. *illum* > Fr. *le* or *illam* > *la*, since regularly *illum murum* would yield **eumur* and *illam matrem* likewise **ellemère* (1969: 1ff). He acknowledges his debt to Schuchardt, Pott and others, but he wishes to establish a broader basis for his analysis. He takes as his starting point Zipf's Law, which he reformulates as follows (1969: 18):

"Les éléments linguistiques dont la fréquence d'emploi augmente subissent, en général, une diminution de leur volume."

Realizing that a major criticism of the frequency argument in its previous

⁸ See e.g. Thurneysen 1946: 97.

⁹ As Martinet rightly points out, if one takes into account the nasalizing action of the acc. *fer* on a following word, there are actually four distinct forms.

form was the unsystematic, *ad hoc* quality of its application, Mańczak attempts to pin down the notion of frequency with some statistical support. Using word frequency lists for French and Spanish, he isolates from the first 6,000 words all those in which irregular sound changes are said to have occurred (the latter based on the standard etymological dictionaries and other secondary works). He finds that in French and Spanish respectively, 86% and 89% of such words are in the first thousand, and several of the words which lie beyond the second thousand were undoubtedly once of more frequent use, e.g. Fr. *sire*, Sp. *hidalgo* (1969: 20–22). Encouraged by these results, Mańczak then applies his formula to a large number of phenomena in Romance historical phonology in some fifty pages of examples, one of which will serve as an illustration. He notes the irregular development of the imperfect endings in the various Romance languages: e.g., in Provençal one would expect in the first person singular **-iva* or **-éva* and in Spanish **-iba* or **-éba*, but one finds in both standard languages *-ía*. Mańczak sees this as originating in overused forms and draws support for this interpretation from Provençal dialect studies in which some forms in *-v-* are found. In such cases the ratio of forms without *v* to those with *v* is highest in verbs such as 'be' and 'go' and falls off in less common verbs (1969: 72–4). Mańczak is aware of Horn's work and praises him for his breadth of vision in subsuming all such changes under one cause, but naturally he argues against the role of 'function'. He reasons that if languages tend to eliminate superfluous sounds, then irregular sound changes should be as common in words like 'institute' or 'professor' as in 'go' or 'good morning'. If in fact the overwhelming majority of such changes occur in the most commonly used forms, then the only correct explanation is frequency (1969: 83).

Besides the frequency and functional arguments, whose history I have just sketched, there also exist pre-generative analyses involving morphological conditioning of sound change in terms of grammatical categories or syntactic function. Meillet sees irregular contractions due in part to close syntactic union (see 11 *supra*), and some of Bally's descriptions of non-phonetic conditioning lend themselves to a similar interpretation. An even clearer example is provided by Jakobson's article on 'The Phonemic and Grammatical Aspects of Language in their Interrelations'. As the title implies, Jakobson wishes to re-emphasize the basic unity of language, which he feels has been given too little attention by some researchers. Like many before him, he deplores the Neo-

grammarians conception that 'grammatical analogy is an irregularity, an infraction of regular sound laws' (cf. the statements of Graff and Jespersen 15, 17 *supra*). Jakobson's own view is that:

"As far as the so-called conflicts between sound changes and grammatical analogy are concerned, they are simply phonemic changes grammatically limited, or in other words, phonemic changes affecting not the general pattern, but only the special pattern of certain grammatical categories" (1949: 113).

He gives as an example the depalatalization of final consonants in Russian. When the final consonant is part of an inflectional ending, it is depalatalized: /dást/ 'he gives', /idút/ 'they go', /za stalóm/ 'at the table'. Other final palatalized consonants remain: (1) in isolated forms like /jést/ 'there is', /avós/ 'perhaps'; (2) in the nominative singular of nouns with zero ending like /pút/ 'path', /l'ós/ 'elk'.¹⁰

As I indicated when I began, the above survey is not meant to be a representative sampling of opinion. I believe it does prove beyond any doubt that opposition to strictly phonetic conditioning of sound change has been widespread and continuous from the time it was first proposed to the present day. Moreover, in the absence of a statistical survey, I believe there is considerable indirect evidence to suggest that the stronger form of the regularity hypothesis has been almost entirely abandoned. We have already seen the statements of Wechssler in 1900 and Graff in 1932 to this effect, corroborated by Martinet's matter-of-fact acknowledgement of exceptions to sound laws in 1953. Similar recent statements are not hard to find. Fourquet, in reasserting the validity of exceptionless sound laws, feels the theory is in sore need of defenders (1964: 642):

"Depuis 60 ans, la phonétique historique a mauvaise conscience: elle continue, faute d'autre chose, à employer une méthode fondée sur un principe auquel elle ne croit plus."

Ladd (1965: 97–8) takes it as an acknowledged fact that 'some changes are regular, and some are not'. As he sees it:

"The problem is not so much: 'Is sound change regular?' as: 'Why is sound change as regular as it undoubtedly is, and what are the limits of this regularity?'"

¹⁰ For a further example of syntactic conditioning see Rohlfs, *Historische Grammatik der italienischen Sprache* (1949: 1.516–17).

Ladd's conception of the problem is not new. DeGroot (1948) likewise believes that it is not the exceptions to phonetic laws which need explanation, but rather their remarkable degree of regularity. Ladd and DeGroot differ in their conclusions (see Ladd 1965: 99ff, and DeGroot 1948: 197ff), but what concerns us here is their common assumption that exceptions to sound laws need no further demonstration. How are we to reconcile the statements of Graff and Ladd with those of Bloomfield and others, quoted by Postal (1968: 236-9) as modern defenders of the Neogrammarian position?

I believe the answer is that all these linguists accept what King calls the 'weaker' form of the regularity hypothesis (H_2) (1969: 121):

"Phonological change is regular, but its environments cannot always be stated in strictly phonetic terms."

King himself accepts this view:

"Phonological changes do indeed apply to large classes of lexical items. Often they are context-free, frequently they occur in purely phonetic environments, and they apply across the board without regard for grammatical category in many cases."

He concludes his chapter on sound change (1969: 139):

"In short, we try to render the simplest account of the facts. If a change has a purely phonetic environment, the simplest account involves writing a rule with a purely phonetic environment. If the change cannot be stated in purely phonetic terms, we still render the simplest account we can."

But is this not the same attitude that Wechssler refers to when he says that no one will admit exceptions to an observed phonetic change 'without a cogent reason'? I call attention to the fact that Wechssler and Fourquet, who are genuinely defending the stronger hypothesis, refer to 'exceptionless sound laws', while other alleged supporters of the Neogrammarian position speak only of the 'regularity of sound change'. This is not merely a difference in terminology. It is the difference between H_1 and H_2 . The reason most historical linguists show no signs of a 'bad conscience' is that the supposed disparity between their theory and practice does not exist. As opponents of H_1 from Schuchardt (1885) to Adrados (1967) have insisted, rejection of the stronger regularity hypothesis does not deny the regularity of most sound

change, and thus there is no inconsistency in continuing the practice of looking for phonetic conditioning wherever possible. For some time now, the principal issue has not been the existence of exceptions to regular sound change, but the amount of weight to be accorded them and the proper explanation for them where they are admitted. Some researchers will more readily accept non-phonetic conditioning than others, and many would probably prefer to substitute 'usually' for King's 'frequently' and 'in most cases' for 'in many cases'. No doubt most would reject the frequency and functional arguments for the bulk of Mańczak's and Horn's examples in favor of an explanation in terms of regular sound change or analogy. But given their remarkable staying power over the last century or more, it is certain that the frequency and functional arguments will continue to find proponents, and if the majority of historical linguists reject such explanations for specific cases, it is on their individual merits, not because the linguists believe in the absolute regularity of sound change. In the words of Ladd (1965: 95):

"One may well query individual explanations of this kind – by their nature they are generally *ad hoc* – but one cannot reject them out of hand."

It is doubtless a reflection of this attitude that Hoenigswald, quoted by Postal as a supporter of the stronger hypothesis, does not criticize Martinet in this regard in reviewing *Économie des changements phonétiques*. Hoenigswald does not reject the functional argument because it contradicts H_1 , but does recall (1957: 576) that in its previous incarnations the functional argument "has left other active scholars cold because it is circular and unspecific". Similarly, King's own explanations in terms of morphological conditioning have not received the treatment one would expect from a field 'dominated' by the stronger regularity hypothesis. Campbell in his review (1971: 199-200) notes the previous existence of similar analyses, including Jakobson's, and quarrels only with King's rejection of the functional argument, a view which Campbell supports. Jasanoff (1971: 82) rejects King's examples, but leaves open the possibility of such conditioning. All this suggests that it is H_2 , not H_1 , that has dominated historical linguistics, and that attacks on the stronger hypothesis are largely without targets.

There is a fundamental difference in the sort of stance one may take toward H_1 and H_2 . The first is an 'all or nothing' proposition: one either affirms strictly phonetic conditioning or one does not. The sec-

ond admits of degrees. One may limit non-phonetic conditioning to marginal cases like expressive forms, formulaic contractions and discontinuous transmission; or one may admit non-phonetic conditioning – in the guise of function, frequency or grammatical category – on a very large scale. Therefore in suggesting that most recent historical linguists have held the weaker hypothesis, I am not claiming a unity of outlook, nor proposing that King's formulation is necessarily that of the majority. My investigation *has* led me to conclude that, aside from a few exceptions like Fourquet's article, recent debate has concerned not the choice between H_1 and H_2 , but rather the proper sense of H_2 .

Recalling the criticisms of Jespersen, Jakobson, King and others, one might object that, even if most historical linguists have held only the weaker regularity hypothesis, they have still tended to pursue their research only in terms of analogy and regular sound change. It is questionable whether one may justly attribute such a method to the Neogrammarians (see fn. 5). Counterexamples in more recent historical linguistics are common enough. For analyses involving the role of function in language change (in a sense quite different from that of Horn or Martinet), one may turn to among others Benveniste's *Origines de la formation des noms en indo-européen* (1935), Kurylowicz's *The Inflectional Categories of Indo-European* (1964: esp. 148ff) or Watkins' *Geschichte der indogermanischen Verbalflexion* (1969: esp. 81ff). Thus historical linguistics turns out to be dominated neither by the stronger regularity hypothesis nor by an exclusive reliance on analogy and regular phonetic change.

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