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On Sound Laws: Against the Neogrammarians

Dedicated to the Neogrammarian Gustav Meyer
in the most friendly and neighborly spirit.

Translated by Theo Vennemann und Terence H. Wilbur

9 The exceptionless operation of the sound laws is the only proposition the so-called neogrammarian school can consider to be its very own.* This proposition appears in writings intended not so much for the experts as for laymen and students despite, indeed, the most lively contrary arguments and without, in part, any indication that such arguments exist. Nevertheless, I would gladly follow the suggestion made by a certain party to bury the hatchet until further notice if only the two parties would oppose each other with fully unified credos, if only a single word were necessary to characterize a point of view. This is not so. The same battle continues, fought with tactics of considerable variability. The discussion does not move within well-defined paths, but meanders off into individual problems of Indo-European history. Some scholars seem to think that a middle way is possible where there is simply only a choice between yes and no. Some waver. Others keep silent.

Incidental remarks, frequently made, are not a sufficient guarantee against the danger of unjustified suspicions. I hope it will not be taken amiss when I finally express the repugnance that I have felt for the neogrammarian principle from the very first. Most of what I have to say has certainly been said before and, to a certain extent, said much better. Yet I hope to exert a salutary influence upon the viewpoint of others in this very important matter by employing schematic brevity and by emphasizing several rather neglected points. The list of references that accompanies this study was not put together according to a definite plan. It contains only what was at hand while I was working.

The nature of the proposition under consideration, as the neogrammarians themselves admit, excludes the inductive method of proof. I look upon the previous attempts to employ a deductive method as failures. They suffer from sundry and severe misrepresentations. Minimal differences are taken to be nulls, transitions to

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*Numbers in the margin refer to the pagination of the German text in this volume.

be oppositions, empirical observations to be apriori
10 principles, complicated matters to be simple ones. It is perhaps not beside the point to note that, taking into consideration the deductive nature of the following presentation, the examples used here and there only serve as illustrations. Refutation on the part of my opponents will have to be directed toward the general possibility, not to individual instances.

In the proposition 'the sound laws operate without exception', both the subject and the predicate evoke weighty doubts. If Wundt (I, p. 348-349) sees a logical postulate here, it comes about from the fact that he takes the expression 'sound laws' in the neogrammarian sense whereas what he really intends to say is something like: 'What was previously designated as sound laws are real, i.e. exceptionless laws in the sense of natural laws.' A preferable formulation would be: 'Sound change proceeds according to laws that are without exceptions.' This subsumption of sound laws under natural laws, so proudly presented in the beginning, was given up by the leaders in the neogrammarian school later on, especially after Tobler's splendid argument, which unfortunately has not been generally appreciated. It seems quite consistent to me when others, such as Körting, still retain the notion. The unexceptionability of sound laws is made untenable by the same set of facts that refutes the similarity of these laws to natural laws. The expression 'sound laws' is inappropriate in still another respect. Although, following general practice, I use the term here exclusively in the sense of laws of sound change, it can be applied with equal, or even greater, justification to the laws of sound structure. This is done by Kruszewski who, in addition, attributes absolute character to these static laws. His statements in reference to the other laws, the dynamic laws, do not always seem to me to be completely congruous.

The word 'e x c e p t i o n' expresses a completely external relationship. It contains in itself no reference to the effective forces. An unmotivated difference has been made, therefore, between apparent and real exceptions, both in general and more especially in the case
11 at hand. The exceptions that must be disregarded when considering the unexceptionability of the sound laws are: intersection with other sound laws, dialect mixture, and the effect of conceptual associations. The first of these three factors requires no closer examination for our purpose. The second will be investigated when I discuss the geographical restrictions upon sound laws. The third requires close scrutiny right now, for it stands in the foreground of neogrammarian studies. It is cited as the antithesis to the regularity of sound laws, as the 'psychological' factor in contrast to the 'physiological' factor.

It was Tobler who advanced the question of e x t e r n a l r e l a t i o n s h i p, of the rank-relationship of the two factors to one another, and who with the greatest finesse pointed out the difficulties of answering that question. First of all, there is the possibility of subordination: One factor is the constitutive or normal one. The other is the disruptive or anomalous one. The latter was then equated with the psychological factor. However, if we rely upon exterior appearance in this process, we can ask ourselves whether or not there could be evidence of cases unknown to Tobler where the isolated effects of sound laws seem to disrupt great analogical groups. In Spanish and Portuguese all the old participles in *-udo* now end in *-ido*. Could not one or the other have been retained for purely phonological reasons, for instance, *sabudo,* because *b* is akin to *u*? And have not, perhaps, such 'mechanical' causes really exerted a retarding effect in the course of this development? General reservations about conceding that capricious factors interfere with a fixed order are added to such special observations. Thus we are forced from every side to recognize that regularity is inherent in the psychological as well as the physiological linguistic principle. In other words, we are obliged to coordinate the two. The peripheries of their spheres of influence intersect each other at many points. Which one prevails over the other depends upon the immediate circumstances in every given case. For a complete solution to the problem one thing is still wanting. Tobler (1881) calls our attention to the fact that heterogeneous forces not only fail to balance each other out; they are never in a position to impinge upon each other.'

12 Advance determination of the heterogeneity of forces is scarcely possible. Only the absolute lack of relationship between the effects of these forces reveals their heterogeneity. Human will is incapable of inhibiting substantive changes to its own body. It can, however, alter reflex motions. This can be explained by the fact that these motions are nothing more than acts of the will that have become mechanical. We are considering a similar case. Analogical exceptions are impossible whenever the purely physiological cause of a sound substitution cannot be doubted, a cause such as a peculiar formation, a natural or artificial defect of the vocal organs. Wherever we *do* find such exceptions, we have to relinquish the thought of purely physiological causation. The psychological nature of one of the interacting factors gives direct evidence of the identical nature of the other. Is it possible that this is what Georg Curtius had in mind when in *Studien zur griechischen und lateinischen Grammatik* IX (1876), p. 232, he says 'under all circumstances, however, the factor that produces analogy must be very like the factor that submits to its influence'?

Thus the antithesis melts away before our very eyes and the problematic character of the external relationship between the two factors becomes clear when we unterstand their i n t e r n a l r e l a t i o n s h i p correctly. Many preparatory steps have been taken in this direction.

Although Osthoff (1878) keeps the physiological and psychological impulses in morphology rigidly apart, nevertheless even he acknowledges 'psychic influences' upon sound change in the 'Preface to Morphological Investigations'. Misteli (11.443) has exposed the contradictions in which Osthoff and Brugmann become entangled. However, I cannot agree with his ascription of phonological processes in part to physiology and in part to psychology, because his work was undertaken from an opportunistic angle that gives itself away even more obviously in the concluding passages. The indecisiveness of the neogrammarians has infected the writings of Wundt, who seems to have received special tutoring from them. While at first he does not wish to see 'more deeply seated psychological motives which are probably by far the more original ones' undervalued in comparison to the physiological conditions of

13 sound change, at a later point he speaks only about the influence of physiological factors upon sound changes. Shortly after he has asserted that 'language is dependent upon natural conditions in a manner that differs in no essential way from that of other historical developments', his discussion of physiological factors induces him to raise those factors to a status much like 'natural laws into which the various spheres of linguistic life unfortunately do not all fit with the same degree of comfort'. The difference that Wundt made between the object and the methodology of linguistics when he characterized them makes no sense to me. With astonishment I read in Brugmann (1885, p. 49) that 'among those who followed Leskien, up to the appearance of Curtius' book', the psychic nature of sound laws remained unassailed. When he said this, he forgot first of all his co-worker Osthoff. In addition, he forgot that, while Osthoff to such a great degree assumed the incapability of the vocal organs to produce certain sounds, such incapability really exists only to a small degree. I have already stated that those sound laws that analogy can disrupt are psychologically conditioned. This is confirmed by the fact that between the occurrences of the two categories no gap is to be found, only a gradation. This can be illustrated, for example, by the following series of Romance developments: *conte = comite, dunque = nunc, treatro = teatro, eglino amano = egli amano, non grieve ma lieve = non grave magis leve*. Not only are directly following sound representations anticipated, but also more distant ones. Again, to a great extent, the analogical formations are bases not merely upon an ideal juxtaposition of words, but upon an actual one. To this degree we can conceive of them as a higher order of assimilations. Not infrequently, on the other hand, phenomena in which no conceptual relationships at all are at play can be traced back to ideal juxtapositions. Then we can speak of a lower order of analogical formations. Thus, the frequency of certain sound complexes favors the new formation of identical ones (i.e. *ié = íe*, Italian *pièta*) or the frequency of a certain sound change brings about development into full generality. I expressed the notion some years ago that Italian (and general Romance) *ie, uo* = Vulgar Latin $\bar{e}$, $\bar{o}$ was originally conditioned by a following *i* or *u* as it

still is in some dialects: *vieni, buonu, buoni*. First it would have been extended by conceptual analogy: *viene, buona*, until a point was reached where no such support was necessary: *pietra, ruota*. Forms like *bene, bove* (pl. *buoi*), *nove* (in contrast to *nuovo*) would simply represent the last unconquered positions. I do not know whether my assumption of p u r e l y p h o n e t i c a n a l o g y is an entirely new thing. I would gather from Bloomfield's quotation (see below, p. 51) that Easton has come to similar conclusions in an article I am not acquainted with. In any case, I am not about to set up a new antithesis after I have been persuaded of the indefensibility of the previous one. Within the totality of analogical phenomena the activity of conceptual associations can scarcely be delimited with any certainty. In languages in which all words are now stressed upon the first syllable, it was originally only the greater number that were so stressed inasmuch as the first syllable is also the most significant. Did the majority go to work wholesale upon the minority, or, did the change progress quite gradually, taking place at each step only between conceptually related words? At times the conceptual relationship is such a general one that it is easily overlooked. Some scholars are wont to consider the devoicing of every voiced final sound, which is common to a number of languages, as a pure sound law, whereas it can be considered as such only before an unvoiced initial sound and the generalization is based upon identity in meaning. Doubts can subsist about the individual detail, but on the whole one must admit the unity of linguistic life and not imagine it to be the contention between Ormuzd and Ahriman.

When a natural scientist hears for the first time about the unexceptionability of sound laws, he probably imagines sound laws that apply at all places and at all times. When we consider the uniform basic conditions of all speech activity, such laws are not only possible, they are to be expected. Why does not sound change--for the most part at least--adhere to the same direction so that, for example, the media can develop from the tenuis or the monophthong from the diphthong and not the other way around? If that naive scientist is told that such general sound laws have not yet been discovered, that, rather, a relatively narrow

spatial and temporal limitation must be placed upon all hitherto defined sound laws, he will find that absolute necessity that would seem to be a presupposition for exceptionless laws lacking. The spatial and temporal relativity of sound laws is not a simple one. Rather it is a complicated one. If, for example, within A and B the law $(r)^a$ prevails, within C and D: $(r)^b$, on the other hand within A: $(s)^a$, within B and C: $(s)^b$, within D: $(s)^c$, the limits of the sound laws for the two different elements not only contain each other, they intersect. The relationship of the sound laws to their e x t e r n a l e x p a n s i o n is characteristically variable and fortuitous. This is the weakest position of the neogrammarians. It is on this point that they have been attacked the most vigorously. Here their defense turns into a slow retreat.

'Sound laws operate without exception w i t h i n t h e s a m e d i a l e c t.' In the expression ' o n e a n d t h e s a m e d i a l e c t ' there is concealed an obscurity. We do not know whether we are to apprehend it *a priori* or *a posteriori*, e.g. whether we are to say 'in the dialect of Naples, the dialect of Rome, the dialect of Florence, etc., Latin *k* became *č* before *e* and *i*,' or '$\check{c} = k^{e,i}$ prevails in the speech of all southern and central Italy.' The phrase connected with it, 'in one and the same period', which can only be understood in this fashion, favors the latter formulation. Considerations of principle favor the former, and, thus, we are, as a matter of fact, accustomed to understand by 'dialect' a completely homogeneous speech community. But is there such a thing? Even Delbrück (1885, pp. 12-13), in order to find a real uniformity within the limits of which the unexceptionability of the sound laws would hold true, reaches down to the speech of the individual and, to be sure, to its normative average at any point in time. I shall not investigate further whether this limitation placed upon the neogrammarian proposition does not simply cancel it out or at least nullify its practical value. Tobler (1879, p. 46) had already said, 'the narrower the circles become, the more they approach that which is individual, and this can never be exhausted by laws'. I am not satisfied with this. The requisite uniformity does not seem demonstrable even in this case. As far as direct observation of ourselves

and others obtains, the pronunciation of the individual is never free from variations, among which I do not, of course, include modifications that are merely consequences of the biological development of the individual. Hand in hand with this endless differentiation of speech goes infinite mixture of speech: the influence of one dialect upon the other which according to the neogrammarians brings about disturbances to the unexceptionable sound laws, and the leveling of the speech of individuals which according to the same neogrammarians alone makes the unexceptionable sound laws possible: these processes of contrary effect are essentially the same. They are only mixture at different levels. It is not apparent, however, why out of the constant conflict of the centrifugal and centripetal forces, leveling of such completeness should result that no differentiations are left behind. The neogrammarians admit to quite minimal ones, but they do not take them into account. In this case, this is a grave mistake and for several reasons. In the first place, the existence of even such small differences contradicts the notion of the impossibility of differences. The latter notion is precisely what the neogrammarians postulate because their proposition does not mean that the sound laws in fact have no exceptions--e.g. this one for that reason and the other for a different reason--but rather that according to the nature of the matter they can have none at all. In regard to this point Paul (1880, p. 69) shuns strict observance. He says that it is not difficult 'to demonstrate the necessity for this consistent occurrence (i.e. of the sound laws) or, to speak more precisely, at least to draw the limitations of the deviations from such consistent occurrence so tight that our ability to discriminate breaks down.' What has been represented here as the same thing seems to me to be something quite different. Even Curtius (1879, p. 81) has conceded the existence of 'sound laws that exert themselves a l m o s t
17 with the thoroughness of natural laws'. Let us ignore the fact that the assumption of the imperceptibility of differences is simply a subjective one, for is not the infinitely small quite commonly taken into account in the discussion of historico-linguistic principles? The answer will be: yes, insofar as a cumulation of the infinitely small

takes place. Well, here we must let a pertinent observation guide us. The minimal differences that the dispute is all about represent only the lowermost of various layers of ever greater differences between ever greater speech communities, and this connection lends them real value. Even Paul (p. 37) emphasizes that 'group differences and individual differences are different not according to nature but according to degree'. Therefore, everything that holds true for the relationship between dialects on any level also holds true for the relationship between idiolects, and does so in the highest degree of restriction--or intensification. This is especially so in the following respect: a sound change often extends over a broad area, i.e. in a series of contiguous dialects. Did it develop spontaneously in each of these? No. Rather, as we can trace historically in many cases, it spread out from one point in the form of radiation. Why should a sound change have developed spontaneously in each of the idiolects that comprise a dialect? Again it is Paul who restricts the application. He does not attribute spontaneity to all members of a group, only to the majority. If he considers this majority as a regular thing, but not as an absolute prerequisite for other types of linguistic change, then I really do not know why one should not go to the same lengths with regard to sound change. Even Delbrück (1884, p. 149) says, 'that changes in pronunciation begin with the individual and spread from there to a greater and greater number of speakers through imitation'. Merlo has very cogently demonstrated the possibility of individual initiative.

According to the neogrammarians a differential between the individual members of a community can only exist in regard to the tempo at which the sound change takes place. A 'glaring contrast' is never supposed to occur. Thus, Brugmann (1885, p. 51) says that 'old and new forms can subsist alongside each other as clearly marked, therefore conscious, contrasts only inasmuch as they are used by different speech communities between which communication is much less intense than it is within each particular community'. How does this accord with Brugmann's earlier assumption of mother and daughter forms within the same dialect, indeed, in the speech of the same individuals? Old and new forms are distributed, moreover, within a

single dialect not only according to age, but also according to sex, education, temperament, in short in the most diverse manner. In regard to the manner in which a sound change is transmitted from individual to individual, from community to community, there seems to be a great divergence of opinion. I confess that in all this I do not see by any means the exclusive play of unconscious activity. Even though I do not want to compare sound laws simply with the laws of fashionable dress as F. Müller (p. 213) does, nevertheless these laws seem to me to be to a great extent matters of fashion, i.e. matters of conscious or half-conscious imitation. Although Schmidt (1885) is of the opinion that 'there prevails a general consensus, except for F. Müller, that all sound changes take place without the conscious participation of the speaker, that these changes are not fads that the individual can imitate or reject according to his fancy', there is some testimony to the contrary. Th. Benfey (1877, p. 556) says, 'this pronunciation began to acquire authority, to be considered correct and refined and, consequently, was adopted even by individuals and groups of individuals for whom the compulsion which had brought it into existence was probably quite alien'. He assumes however (p. 557), 'that the speakers had no consciousness at all of the change'. Bezzenberger (May 21): 'A sound change can also come about consciously ...For reasons of taste a more extensive group of people will adjust its speech according to the pronunciation of this individual or that small group.' Collitz (p. 321): 'It [the sound change] pleases those whose attention it
19 attracts. It becomes the fashion whether one follows it for the sake of ease, esthetics, or for any other reason, but it is not followed unconsciously.' Delbrück (1884) mentions alongside of ease also the esthetic impulse as a reason for sound change. He mentions (p. 154) a certain manner of speaking that spreads 'because it is the fashion and is considered pleasing', but considers it beyond all doubt 'that all, or almost all, of these acts are carried out unconsciously'. He advances this unconscious completion of a sound change (1885) among the arguments in favor of its regularity. I shall not go far astray if I consider the unexceptionability of the sound laws as incompatible with the share that consciousness, in my opinion, has in sound

change. Just consider the influence of the school even in those places where public instruction is very limited in scope. Think of the widespread inclination of the uneducated to talk in a genteel fashion among provincials. Isn't Berlin's *j* for *g* advancing further and further into Central Germany in perfect goosestep? In M. Trautmann's detailed study (1880, p. 214 ff.) we can see that uvular *ρ* has been more and more widely adopted, replacing alveolar *r* over the years in Germany and France. Just this fact had been mentioned previously by Brugmann as an example of 'blind', i.e. unconscious action of the sound laws. By the bye, I permit myself one question: Schmidt spoke later on about blindly operating sound laws. Just how does Brugmann dare to say that he has always found the expression ambiguous? Fashionable sound change, that is to say, more or less conscious or, even better, voluntary sound change frequently has attendant innovations. The sound change can suffer false application. It can even be increased by one degree. It can produce parallel sound changes. If, in the end, as can be attested historically, any particular peculiarity of pronunciation of a really style-setting personality, a prince, a courtier, an actor is copied voluntarily in his own circle, or, if a teacher imposes his own pronunciation upon his pupils, it cannot be disputed that the impulse
20 for sound change is one of personal choice. Individual sound change can without further ado be attributed to personal choice. For this reason, it does no good to limit the unexceptionability of the sound laws to the speech of individuals as Delbrück does. In short, I agree completely with Bloomfield (p. 178) when he remarks about our question in the spirit of Whitney: 'The word "inviolable" or "infallible" in matters of grammar is always to be deprecated, if for no other reason than the one that the c o n s c i o u s w i l l of any language-user undeniably stands above phonetic facts.'

Before I go on to the next aspect of the neogrammarian proposition, let me add a postscript to the preceding section. As I said, I assume language mixture even within the most homogeneous speech community. Paul does so only in the case of ethnic mixture, which he considers to be something quite exceptional. I must reject this latter notion. On the one hand, the fluctuation of population in every fairly

large urban center is usually such that one may indeed designate it as a mixture even in the narrow sense. Remote dialects, far from being 'unable to develop differences perceptible as such', leave their unmistakable imprint upon the dialect of the population center. Indeed, this dialect occasionally even loses its original character completely in such a manner. (Just as, for example, the popular speech of Rome today is Tuscan, which it definitely was not five hundred years ago.) Especially we must not underestimate the linguistic influence of Jewish portions of urban populations even where they constitute only small minorities. On the other hand, the only case in which Paul (1880, p. 71) assumes speech mixture, namely 'where in consequence of special historical causes fairly large groups of people are torn loose from their homes and are thrown together with others', is not so exceptional either. Going back from the formation of the Romance-speaking nations to the very beginnings of the Roman nation we find an almost unbroken series of mixtures of the most varying kinds that not only Romance grammar, but also Latin grammar must take into account. Paul (1880, p. 72) thinks that he must better define the expression 'dialect mixture' as 'the borrowing
21 of a word from a foreign dialect'. We can, to be sure, adopt foreign words, but we can also adopt a foreign manner of pronouncing common words. It is a well-known fact that Germans fall into a Yiddish way of speaking when they have a great many dealings with Jews. If, in consequence of this, the Yiddish pronunciation of a word often heard from the mouth of a Jew becomes fixed in the speech of a German, e.g. *Persent = Perzent*, we can hardly speak of a loanword. If it is true that the initial sounds of French *haut, gâter, goupil* were influenced by German *hoch, wüsten, Wolf* in the mouths of romanized Teutons, these, too, have nothing in common with ordinary loanwords. The causes of these events are, of course, obscure. In the case of the last word, the Teutons' love of hunting could have been the decisive factor, just as the urban Roman, perhaps, borrowed his *vulpes* and *lupus* as whole words from some chase-loving Italic speakers.

'The sound laws operate without exception w i t h i n t h e s a m e p e r i o d.' This is only a supplementary definition. Within temporal limits that can be determined only after the fact, a sound law is carried to completion within the entire expanse of the speech community and throughout the entire extent of the speech material. I have just discussed the correctness of the first point. I shall dispute the validity of the second point below. But first I would add a general remark about transitional stages. There has been an attempt to make the proofs of the two points less easily refutable--whether it concerns the first point or the second--by suspending the law of the unexceptionability of sound laws for transitional stages. This cannot be permitted. Every stage of a language is a transitional stage. One stage is just as normal as any other. What holds true for the whole holds true for each part. I cannot conceive of language as a mixture of complete and incomplete sound laws. This would mean mixing teleological notions into scientific consideration. When I speak about transitional stages, it is only in a relative sense, only in relationship to later, already established facts. We have no right to designate any present-day state-of-affairs as a transitional stage.

Even if someone should be of the opinion that the difference between the neogrammarians and their opponents in regard to the external expansion of the sound laws lies in the presentation rather than in the apprehension, he will not gainsay a factual difference between the two points of view regarding the i n t e r n a l e x p a n s i o n of the sound laws, which I shall now discuss.

'In the case of sound change within the same dialect all individual cases where t h e s a m e p h o n e t i c c o n d i t i o n s are present are treated in the same manner.' If we take into consideration, however, all the cases where a sound occurs and if we ask which ones among them show the same phonetic conditions, that is to say, which require the same treatment, i.e. lack of change or change into the same sound, we shall find no answer. Since there is a series of categories of phonetic environments such as accent, position within the syllable, nature of the directly following sound, of the directly preceding one or of the one after the following one, etc., in every single case a complex of conditions becomes evident. If

we compare the complexes of conditions of all cases with one another, we shall find that each one is different from the other except for homonyms. And homonyms are least suitable for the illustration of the effects of sound laws. Therefore, at best a notion of p a r t i a l equality of the complexes of conditions can be considered relevant. But which elements, and how many of them must be equal in order to outweigh the partial inequalities? With what aids are we to distinguish the essential conditions from the accidental ones or conditions in the strict sense of the word and concomitants? We are forced to admit that 'the same phonetic environment' is only to be abstracted from each sound law itself, that the application of such an environment as a premise is impermissible, that it has no place at all in the definition of the unexceptionability of the sound laws. Even if we decided to speak about the equality of phonetic environments among all of the individual cases of a sound law, we would not expect to find it among all of the individual sound laws of a dialect. We see, for example, that within the category of the directly following sound (more
23 exactly, of the consonant following a vowel) the four liquids are distributed in the following fashion: after the one vowel *l, r, n,--m,* after the second *l,r--n, m,* after the third *l, r--n--m.* In other words the partial equality extends, beyond the combinations, to the individual categories: *n* acts in the cited examples first as a dental liquid, then as a nasal, finally as a dental nasal. Not infrequently we come upon sound laws where even that relative uniformity of conditions cannot be verified. We can confront these rather cloudy 'sound laws' with very clear cases of 'sporadic sound change'. Stressed *a* in present-day literary Portuguese has become *o* in one case only, in *fame = fome.* Only unstressed *a* is subject to the influence of a following or preceding labial. (For example, Colloquial Portuguese *fanforrice, charomela.* See Jules Cornu, p. 340.) However, the influence of a following labial and that of a preceding one taken together are strong enough to assimilate a stressed *a* too, but only in this isolated word of extremely frequent use. (Among others, not in *fava,* and, owing to reduplication, not in *mama.*) A neogrammarian would certainly hang

onto a *fomentar* or *fomite* before he would admit so much. For an analogical relationship, compare French *buvons* for older *bevons* alongside *devons.* The axiom 'same cause, same effect' (we label as cause in sporadic sound change that which is, strictly defined, only constant environment) cannot be invoked here in favor of the doctrine of the unexceptionability of sound laws. It is a question of partial equality of sets of causes where, in addition, the equality is partial in varying degrees. The labial factor is not present in the individual labials to the same degree. It is found, for example, to be present to a greater degree in *m* than in *b.* Therefore, a whole series of accessory conditions are at work in the labialization of the neighboring vowel. Delbrück (1885, p. 18) admits the existence of completely isolated cases of sound change which 'do not accord with the concept of law.' How does this jibe with the neogrammarian proposition that all sound change is subject to exceptionless laws? Up to this point in our discussion of the equality of phonetic conditions we have assumed a temporally defined transverse section of language. Now the question is: Do the phonetic conditions of a sound law--whatever their form might be--remain constant over a period of time? I would like to answer that here and now with a concrete example.

Modern French *e*--sometimes open, sometimes close, the difference is not essential here--in *chef, fève, pré, tel, mer, nez, ème, lène = caput, faba, prato, tale, mare, naso, amat, lana,* corresponds to Gallo-Vulgar Latin $\bar{a}$ (classical $\check{a}$ and $\acute{a}$ before a single consonant). The following consonant appears to be a matter of indifference. This was not so in Old French. This fact is reflected in present-day orthography: *chef,* etc., but *áime, láine.* If now $\bar{a}$ before *m* and *n* has become *e* passing through the stage *ai,* could this not likewise have happened before the other consonants? And if *chaif, faive, tail, mair* were originally pronounced, it is conceivable that in a more recent period *chaif, faive, tel, mer* occurred. Thus, different stages of conditions were present with respect to the monophthongization of the *ai* that developed from $\bar{a}$. Otherwise we have to assume different sound laws. In any case, a difference is concealed behind present-day identity. If a sound law is extracted from the comparison

of two series of phonetic forms that are separated from one another by a long and opaque stretch of time, what guarantee is present that the situation is not the same in regard to this sound law too? Just consider any particular group of related dialects. You will see how the conditional environments of the sound laws change from place to place. You will, as it were, perceive the spatial projection of temporal differences. The principles of continuity and identity are contradictory to the assumption of a series of different laws. But how does this reflect upon the unexceptionability of sound laws? If the differences between two conditional environments are really only temporal-spatial variants of a single one, may we not consider either of the two to constitute the exceptional case, depending upon which way we are looking? This internal expansion of the sound laws can easily be understood if we assume phonetic analogy. I touched upon this point above when I called into question the presence of a dualism in linguistic life by illustrating with one example how a combinatory sound change becomes an unconditional one. Even the greatest distance between the initial restrictions imposed upon a sound change and the final ones need not strike us as strange, when we consider that conceptual analogy often has a most extensive effect although it too started from a very restricted segment in the language. Attestations of this can be found especially in the history of Romance participles. I do not even consider it impossible that a whole sound law could develop from a single exchange of sounds produced by conceptual analogy. By no means am I saying that the original causal conditional environment spilled over simultaneously in every direction by the operation of phonetic analogy. A sound change can progress hesitantly from one similar environment to the next, for example, by joining forces with another sound change as when *-ol- = -al-* in conjunction with *-or- = -ol-* leads to *-or- = -ar-*. In *Gröbers Zeitschrift* V (1881, p. 319) I have asserted that wherever *s* has become *h* in all positions, this weakening must have first occurred as a combinatory weakening. Thus the bridge between intervocalic *h = s* and initial *h = s* can be discovered in initial *h = s* after a vocalic final (*-aha-: -a ha-: -t ha-*,

with generalization from the voiced to the voiceless sound, while in the final devoicing law mentioned earlier the direction was the opposite.) However, I cannot expatiate any further upon this metamorphosis of sound laws, which as far as I know has never been made the object of general discussion, but stress it all the more emphatically. Even in the realm of mechanical sound change -- to make use of neogrammarian terminology--I find things that are quite different from mere, self-contained processes of sound change that can be wrapped up in rigid formulas. I see here the lively endless interplay of innumerable impulses in the midst of which individual items stand out in more vivid outline.

While the neogrammarians make the unexceptionability of sound laws dependent upon e q u a l i t y o f p h o n e t i c c o n d i t i o n s, which in my opinion does not exist at all, at the same time they treat with indifference the immediately obvious d i f f e r e n c e b e t w e e n w o r d s. 'In the process of completion of sound change it is not at all conceivable that different ways were taken in different words' (Brugmann, 1885, p. 51). This is defended in the following manner: 'The kinetic feeling is not formed for every single word in particular; but wherever the same elements are repeated in the flow of speech, their production is regulated by the same kinetic feeling. Therefore, if the kinetic feeling is displaced by the pronunciation of an element in any particular word, this displacement is also decisive for the same element in another word' (Paul, 1880, p. 69). I consider this wrong, at least in the absolute form in which it is asserted. Here Paul practices the procedure--which is criticized by so many and even by himself, as far as I can see, in the chapter about sound change-- of isolating the consideration of the individual sound from that of the word in which it occurs. The change of a sound, its progress in a certain direction, whereby naturally the inevitable effect of purely physiological change is ignored, consists of the sum of microscopic displacements. It is, therefore, dependent upon the number of repetitions. If *x* requires 10,000 repetitions to become *x'*, these repetitions are to be counted within individual words, nevertheless. An *x* spoken one time each

in 10,000 different words would not become *x'*. I will not deny that a word that has been spoken 10,000 times can favor the development of the sound *x* to *x'* in a word spoken only 8,000 times, etc. The greater or lesser frequency in the use of individual words that plays such a prominent role in analogical formation is also of great importance for their phonetic transformation, not within rather small differences, but within significant ones. Rarely-used words drag behind; very frequently used ones hurry ahead. Exceptions to the sound laws are formed in both groups. It is an old experience that in all languages the most common words show the greatest inclination to emancipate themselves from the sound laws. (And it is just from these words that one would expect obedience to
27 the sound laws.) In consequence of this fact they create serious difficulties for interpretation. (I call to mind the Romance words for *to go*.) They have been compared to small coins that, as they pass from hand to hand rapidly, are soon worn thin. This splendid observation has not been pursued in recent times. Rather, it has for the most part been ignored. Kruszewski (1887, p. 162) has expressly called attention to this although I find his suggestion far from satisfactory. He says, 'If *gosudaŕ* becomes *sudaŕ* and in the end *sŭ*, if *babuška* becomes *bauška*, *pravo* becomes *pra*, *wasza miłość* becomes *waszmość*, *waść*, *trzeba* becomes *trza*, *podobno* becomes *pono*, *człowiek* becomes *człek*, *proszę pana* becomes *pšpana*, etc., we must keep in mind that these words in the greater number of cases are spoken quickly, without accent, and in association with other words.' All languages provide examples of this sort, especially in titles and greetings. I call your attention to Hungarian *alá szolgáj = alazatos szolgája*, *tejes* or *téns = tekintetes*, Spanish *usted = vuestra merced*, Colloquial German *g'Morgen*, etc. In several cases we have, of course, enclisis and proclisis. However, stresslessness is not a sufficient explanation inasmuch as the same changes do not always take place in the unstressed syllables of unit words. Rumanian *ună* becomes *uă*, *o*. Even without accent the dropping away of *n* between vowels is otherwise unknown. Romance *cas (ca)* develops from *casa* in proclisis. Does, however, the syncope of pretonic *a* take place according to sound law? What is more, there arises the

question of whether that stresslessness is not itself a consequence of over-frequent use. If I say *g'Morgen* for *guten Morgen*, the adjective is deprived almost completely of its meaning, but only in consequence of the incessant repetitions. The fate of Latin-Romance *ille* appears to me in no other light. As the final cause of all such conceptual and phonetic weakening, I must take over-frequency all the more into consideration since it goes to work even where no association with other words is present. In *guten Morgen* not only the first word, but even the second word is garbled (*g'Moin*, *g'Mõ*). If we trace the development of speech within relatively small groups that are cemented together by very definite interests, we see that it is just the most meaningful words that are most readily subject to phonetic change owing to their being constantly repeated. Notice, for example, how abbreviation and fanciful distortion of technical terms is favored at games. It seems as if alongside the principle of least effort there is another impulse, the avoidance of monotony. One can condense this observation into an experiment. Have somebody who does not know why you ask him to do it repeat a single word thirty, fifty, or eighty times in a row and you will find very marked variations in pronunciation. Writing provides an analog for this group of phenomena. The same cluster of signs will be represented more carefully or more carelessly depending upon whether they turn up in rarer or commoner words, words that are less familiar or more familiar to the writer and to the reader-- and this comes about unconsciously, to be sure. There is always talk about the principle of least effort whenever the causes of sound change are being debated. What is more natural than making things easier whenever over-frequency provides the strongest impulse for this and wherever the danger of misunderstanding is least? I am returning to the previously mentioned extension of the sound change *h = s* from intervocalic to initial position. In Yakut--Delbrück (1885, p. 15, referring to Boehtlingk's *Jakutische Grammatik*, p. 62) calls it to our attention-- we find not only medial but also initial intervocalic *s = h*; however, only in a single case has *s* developed into *h* regardless of position, in the one

word *suoch* 'no'. Is it not possible that initial *s* = *h* will be extended to less common words starting from this word? As a rule only preconsonantal *s* becomes *h* in Andalusian. As I have noted in *Gröbers Zeitschrift* V (1881:319), the tendency for further application appears to occur first of all in final position (*loh amigos*, alongside *los amigos*), but then to turn up in *no heñó*, *sí heñó*. In negation and affirmation many a thing happens that otherwise does not. From an Italian one rather frequently hears a whispered *si* or simply *s* instead of *sì*, and the sound law responsible for the disappearance
29 of *n* in Spanish and Italian *no* is still not clear, at least to me. Whenever it is a question not of indigenous, but of transplanted sound change, the old pronunciation will remain longest precisely in the most common words. Kolosov (1877) considers the change of *ë* to *i* to be an original general feature of the Novgorod dialect. In some places *e* has completely replaced this *i*. In others it is maintained only by old people while *chlib*, *sino*, etc. seem ridiculous to young people. Here again *i* is found exceptionally alongside the usual *e* (*chlib*, but *seno*) and in another place the opposite case is true. It is easy to understand why the old sound is kept in the word for bread. Other cases are not so obvious. Dialect mixture is not to be denied, but I do not know how--in a case where individual words have not been borrowed--it can be considered as only an apparent exception to phonetic regularity. It must be explained why in the one word the traditional and in another word the new sound wins out. What is possible in the case of such a mixture is generally possible. Delbrück (1880, 1884) agrees with Brugmann (1885) completely in the opinion that a sound change does not begin in specific words and then become transmitted to other words. Delbrück (1884:155) adds 'that this is really the case is demonstrated not only by experience with popular dialects,' --the facts mentioned above speak against this--'but also by the consideration that only with the presupposition of a uniform and consistent pronunciation of sounds is the acquisition of a foreign language to be explained.' I cannot refute this argument, because I do not understand it. That rarely-used words easily have an archaic appearance is likewise

well-known. The question arises as to whether still other gradations within the total speech material are not conceivable with reference to the occurrence of sound change. Delbrück (1884, p. 160) has hinted at the possibility--to be sure, in order to reject it--'that every alteration of sound begins with a specific word and spreads from this very word, that is, for example, from
30 one substantive to another, from there to adjectives and participles until it reaches the verb.' Given the gradual spread of the sound change, could not the thought arise that conceptual analogy works against sound laws only in individual cases, but rather works together with the sound laws as a general rule.

Taking into consideration what has been said, the doctrine of the unexceptionability of sound laws can just as little be demonstrated by deduction as by induction. Whoever adheres to it must confess to it as to a d o g m a . And it is called a dogma incidentally in G. Meyer's obituary for Georg Curtius and very expressly in Bloomfield's study devoted to the question itself. Dogmas can attain scientific status only by means of 'false analogy' and the fruitful *tertium comparationis* will be found in the salutary effect. As a matter of fact, Bloomfield says bravely, without fear of saying too much, that even if the doctrine of the inviolability of the sound laws should turn out in the end to be wrong, this fact would not detract from its value as a method, for it stood the test by its results. The alliance of correct results with possibly faulty premises contradicts scientific thinking. It is just as impermissible to identify a scientific procedure directly with a scientific theorem. On this point a great number of linguists, more or less consciously, might very well agree with Bloomfield and differ only inasmuch as the excellence of the method excludes for them any doubt about the validity of the doctrine. All that I can admit is that the doctrine is quite an absolute and simple one. That is why it is so very easy to operate with. One likes to strengthen the infallibilistic principle apagogically. Thus Paul (1880, p. 1) is of the opinion that whoever rejects it--he grants it, to be sure, 'no more than the value of an hypothesis'--'relinquishes completely the possibility of raising

grammar to the rank of a science.' According to Kruszewski the neogrammarians force upon us the necessity of
31 'assuming exceptionless laws or conceding the absence of all sound laws.' I must remark first that high-handed intimidation deserves no room in science and secondly that the insisted-upon alternatives, even if less harshly formulated, are wrong. I would like to know who among the pre- and non-neogrammarian linguists, including my own humble self, has ever considered and treated sound change as chaotic. (I even find that expression in Kruszewski.) It seems very much beside the point that Bloomfield stands up for sound laws in the broadest sense. (He will have nothing to do with unexceptionability.) It is true that I have not read Easton's pessimistic statements to which he refers. The basic error in Bloomfield's case and in the case of others is quite deeply rooted, in the presupposition that there exists any area at all, or even that such can be posited, that is subject to no laws. Within various categories of phenomena the connecting regularity is graduated in the most varied manner, depending upon the greater or lesser complexity of conditions, all the way from the chance-element of a gambling game right up to the fixed order of the mechanical universe. A general survey of the field in which we intend to work must always give us indications of the regularities that we are to expect. The gambler who pursues his luck with a mathematical system has given no heed to the true nature of the game. I find it even more remarkable that the psychological bases of sound change, the social character of a language, the fluid borders of its spatial and temporal variations can be perceived with such lucidity and, at the same time, that unexceptionability of the sound laws can be defended so staunchly. The neogrammarians confuse 'the very simple concept of law with that of the complex effects that are produced by many laws that variously operate simultaneously together' (Merlo, p. 159). The formal defects of the neogrammarian dogma revealed above do not allow me to confront it with my own point of view in a contradictory formulation. I shall not say, 'The sound laws have exceptions.'
32 If, however, the statement is, 'There is no sporadic sound change,' then I shall express myself positively. 'There is sporadic sound change.' I would go one step further. If

I were obliged to include the notion of 'unexceptionability in my credo, I would relate it to the occurrence of sporadi sound change, rather than to sound laws, in the sense that every sound change in some phase is sporadic. If we wish tc characterize different points of view with an antithetical manner of expression, we can speak of absolute and relative conformity to the laws.

That those of us who attribute a further sense to the expression 'sound laws', which has unfortunately become established in our parlance, have on this account no more trouble when it comes down to practice, i.e. to the particular practice of explicating words and forms, has yet to be demonstrated as a virtuous, but superfluous, deed. Some have been of the opinion that greater rigor has been introduced into scientific research with the infallibilisti doctrine. They have, in doing so, started out from an incorrect generally held opinion. Rigor can manifest itself not in the object, but only in the subject, not in the setting up of a more rigorous law, but in the more rigorous observation of that law without which there is no science and which in turn is sufficient for all of science: the law of causality. This more rigorous observation comes about automatically in the steady course of scientific progress. Only step by step does science replace description with explanation. Thus, in linguistics we were too much occupied at the beginning with the collection of facts to tackle the investigation of causes on a broad front. To consider a temporary waiver of claims in this direction as a denial of the principle of different causes for different effects seems to me to be a violent imputation. For the rest, we must be permitted even today to point out any particular deviation from a recognized sound law and to be silent about the cause of this deviation rather than to chance a poorly founded supposition. Those errors against which the neogrammarians have railed so stridently are either ones that have long since been corrected or they are regressions from which no science is free and which deserve to be forgiven precisely by the neogrammarians because of their own frequent practical offenses against their own doctrine, or they are not errors at all. As far as I am concerned, the doctrine of the unexceptionability of sound laws is nothing but a hindrance

to the further development of the science in the sense of the law of causality. The sound laws are elevated to such heights that the desire for transcending them is far weaker than if they had only the value of great regularities. And yet in any case they are only empirical laws and, as Wundt also stresses, these laws must be transformed into causal laws. Is it not a remarkable inconsistency of the neogrammarians that they neglect to comprehend the sound laws themselves, but insist that the exceptions be understood? And that they see these for the great part in the effects of conceptual associations and, while doing so, disregard other factors such as language mixture? This seems dangerous to me especially in regard to the Romance dialects as they are transmitted to us in medieval manuscripts. In short, the positing of the neogrammarian principle does not mean for me a revolution in the history of linguistics with which the science began to progress with greater rapidity and assurance. I do not think a future generation will be able to find such a beneficial turning-point between Ascoli's *Saggi ladini* (1873) and Osthoff's *Tiefstufe im indogermanischen Vocalismus* (1876).

The history of this blinding sophistry that has brought such wide circles into confusion is noteworthy. It is rooted in the earlier point of view that separated speech from human beings, that attributed to it an independent life. It first appeared upon the stage in romantic-mystical garb and then in rigorously scientific costume. The theory of the unexceptionability of sound laws which, if it did not originate with August Schleicher, certainly has been proclaimed in his spirit, pokes its head out of that period into our own day like an ancient statue. Our day ascribes to linguistics the character of an historical science
34 [*Geisteswissenschaft*]. It does not view language as a natural organism, but as a social product. In Paul's *Principles of language history,* where he has penetrated so deeply into the nature of language, this neogrammarian doctrine strikes its falsest note. To be sure, it turns up in that work in a very moderated form. In general, the acrimony with which this position was first asserted, when the disciples were at pains to put it over, had to be dropped. Therefore, it is not without difficulty that

formulations can be discovered in the numerous corollaries and expositions of the neogrammarian theoretical proposition that are contradictory to the proposition itself. The best critique for it would perhaps consist of the naked confrontation of the multitudinous forms that it, despite its absolutistic claims, has assumed from Osthoff right down to Delbrück. Its widespread dissemination is no argument in its favor. Only a few adhere to it because they have developed it spontaneously or have tested it thoroughly. Most people have adopted it on account of its already noted methodological convenience. It fits well into the emphasis science today places upon its handwork. What W. Scherer so appropriately called 'mechanization of methods' reduces the demands upon independent thinking to a minimum and thus makes possible the participation of an extraordinary number of actually incompetent people in 'scientific' work.

I should be very sorry indeed if I have given offense where I have merely wished to be as precise and as definite as I could be and had to be in the interests of the matter at hand. I should regret this all the more since so many cordial ties bind me to the neogrammarian school--as the dedication indicates--and I regard very highly the achievements of individuals--aside from what is specifically neogrammarian. Lynch-mob tactics that still besmirch the most recent annals of our science seem to cause some of us to assume an attitude of exaggerated restraint. A spirit of conciliation is a fine handmaiden for scientific research. But it must relate to personal rather than to factual matters. A person who out of a conciliatory heart wished to amalgamate two mutually contradictory etymologies or wanted to remain undecided between them would be generally criticized. Why should we adopt a different stance when it is a question of principles that bear upon so many matters, as if such principles did not belong to the realm of science but to that of arbitrary choice?

Those who declare that repeated and thorough discussion of linguistic principles is gratuitous and boring do no justice to these principles. In opposition to these persons--and thus at least in respect to one aspect of the disputed question--I join hands with those from whom

that question separates me. I do not intend to return to the differences in practical consequences that exist between the neogrammarians and the rest of us. They are evident only to a limited extent. However, the neogrammarians not only give a precept, but they assert it as a fact, a fact that in general would characterize the whole of linguistic life. What difference could it make whether Romance *andare* came from *adnare* or *addare* or *ambulare* or from a Celtic verbal stem, whether in this dialect *l* became *r* or in that one *r* became *l*, etc.? What meaning do all the thousands of etymological and morphological correspondences, the thousands of sound laws have as long as they remain isolated, as long as they are not absorbed into higher relationships? They serve in part, and only in an auxiliary function, for the clarification of the migration of peoples and cultural relationships. But first they must be assimilated within the science itself. We must learn to find the general rule in the specific detail. Accordingly the recognition of a fact that dominates all of linguistic life is of much greater importance than the discernment of particular linguistic phenomena.

This question about the value of principles is closely connected with the question about the position of linguistic science in the community of sciences. Thus, the two questions are treated by Brugmann (1885, pp. 1-41) under one title. I stand in complete opposition to him in regard to the latter question, too. I do not believe
36 that the reconciliation that he fervently desires will be possible until we have divested ourselves of the label 'philogogy'. The classification of the sciences has to result from the observation of things and not from the definition of names, least of all names originally used with indeterminate meaning and, therefore, of such constantly shifting usage, names that we have inherited from times when science had scarcely yet emerged. Why in all the world can we not decide to talk about linguistic science, literary science, and cultural science? I hold the opinion that any language is much closer to any other language--however far apart they may stand--than language is to literature, even if these two may be the language and the literature of the same

nation. Coherence of research methodology is of greater import than any other connection between heterogeneous objects of investigation. It does not matter how lively the interrelationship between linguistic science and literary science may be. The one at best will always play the role of an ancillary science in regard to the other. In vain do I look about in others fields for an analogy to explain what is meant by the term 'philology'. Does one, for instance, combine the flora and fauna of a given region in a particular discipline? If we intend to consider each of the different 'philologies' as a practical study, as a kind of sentimental national folklore, I have nothing against it. I cannot concede to Brugmann (1885) that, for example, Indo-European linguistics is not a branch of general linguistics but a branch of Indo-European philology. To elevate the borders of language groups to major divisions of science I consider as all the more improper since genetic relatedness and unrelatedness in a number of cases have not yet been determined but are themselves still objects of investigation. Brugmann (1885) and most others with him do not give much consideration to comparisons between unrelated languages. Their judgment must, in logical consequence, be extended to comparisons between historically unconnected phenomena in related languages, such as Brugmann has collected in his admirable essay, *Zur Frage nach den Verwandtschaftsverhältnissen der indogermanischen Sprachen [The question of the genetic relationships of the Indo-European languages]* (1883). I, on the contrary, consider investigations such as August Schleicher undertook some decades ago about zetacism to be extremely profitable. Linguists should follow the example of natural scientists and more frequently take casual walks about the world, exploring this or that phenomenon or group of phenomena. By doing so, they could cast light not only upon many individual facts but, above all, upon the general rules. If, according to Brugmann, the results that comparisons between unrelated things produced would benefit only general linguistic theory, just this fact would be for me a confirmation of their supreme value, for the separation that is made between the individual linguistic sciences and general

linguistic theory seems to me to be the least justified of all. Each of these sciences merges with general linguistic science. Each must be absorbed into it and to an even greater extent the more their scientific nature increases, the more they strip off everything that is empirical and fortuitous. We must never lose sight of what is generally true, even in the most exacting particular study; we must immerse ourselves in science only in order to transcend it; we must serve science only in order to master it.

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