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SYMPOSIUM 5: Phonetic Explanations in Phonology

Chairman: *J.J. Ohala, U.S.A.*

Panel members: *E. Fischer-Jørgensen, K. Kohler, L. Goldstein*

1. Chairman's opening remarks

The purpose of this symposium is to demonstrate that aspects of the structure and behavior of speech, i.e., its phonology, can be explained by reference to phonetic facts. Although this is a position with a respectable history (Passy 1890; Rousselot 1891; Grammont 1933) it has recently become controversial due to some well-articulated challenges which argue that there are important limitations on our ability to explain phonological facts (Lass 1980; Anderson 1981; Ladefoged 1983). A few preliminary remarks may help to eliminate some misunderstandings and thus to channel the arguments on this issue into productive directions.

First, it must be said that the commitment to search for explanations in any area of science is an act of faith. There is no guarantee that the search will be successful. In the history of science, failures to discover explanations are much more abundant than successes. Nevertheless, the appetite for understanding the workings of the universe, i.e., to reduce the mysterious to the familiar, has been stimulated in us by the classical Greek philosophers. They believed that by the application of rational means of inquiry it was possible to penetrate the seeming chaos of the universe and to discover a system - some small set of primitive entities and principles - from which the behavior of the universe could be derived. Other philosophical stances are possible, e.g., a belief in the inscrutability of the universe, that humans should try to achieve a harmonious union with the world rather than try to dissect it or to force it to reveal its secrets. It seems to me there is no way to show that one of these philosophical positions is better than the other. Thus it seems fruitless to attempt by persuasion and argument to make an advocate of one of these views renounce their faith.

Second, the explanations offered are *partial* explanations. As Hermann Paul (1880) noted, speech and language exist in three domains: the physical, the psychological, and the social. It follows that linguistic phenomena, like virtually every interesting topic of scientific study, exhibit behavior that has multiple determinants. Our situation in phonology, then, is similar to that of the medical researcher who ventures to show a causal relationship between heavy smoking and lung cancer. Lots of factors undoubtedly contribute to the development of lung cancer: diet, life style, environmental factors, here-

dity, etc. The causal connection between smoking and cancer can only be established on a statistical basis where all these other factors have been controlled, conceivably by being neutralized by varying randomly over the sample studied. This is, in fact, one of the ways such epidemiological studies are done. Thus, faced with an individual who was a heavy smoker and had lung cancer, can the epidemiologist say that the person contracted lung cancer because of the smoking? Not with as much certainty as one can establish for a large sample but nevertheless with a probability that is high enough that people will act on his advice (by giving up smoking, for example).

So, in our phonetically-based explanation for sound patterns we are on the safest ground when we find the same pattern in many diverse languages where the psychological and social forces vary in random ways. We can still offer plausible explanations for isolated cases but run a greater risk - an acceptable risk, many would maintain - of being wrong.

The third point is to address Roger Lass's (1980) criticism that the fact that we can't predict language change - sound change, as it concerns us - , means we are not a true science like the natural sciences such as physics and chemistry which are capable, according to him, of formulating deductive nomological (law-like) accounts of the behavior in their domain of interest. This claim is based on two misconceptions. Anyone who has ever done an experiment in a physics class knows that the measurements of phenomena never come out exactly the way the 'laws' say they should. This is true no matter how careful the operations are done. Physical laws, then, as many modern philosophers of science admit - and as the history of science demonstrates (where the laws get revised or replaced from time to time) - are a fiction. This does not mean that the theories of physics or chemistry don't explain phenomena; they do, but, as mentioned above, they are partial probabilistic or statistical explanations. Furthermore the ability of the physicist, for example, to predict the future is on a par with linguists' ability to predict the future. If a physicist were challenged to predict the trajectory of a billiard ball it would be necessary to impose a host of restrictions on the event: to specify that the ball would be hit (a social fact, as it were), how it would be hit (force, angle), the conditions of the respective surfaces of the stick, the ball, the table, the side cushions, etc., that there would be no air currents impinging on the ball, that the table would remain stationary (and not upset by an earthquake), etc., and even then the prediction would not be exact because 'unpredictable' factors almost always crop up, e.g., a piece of the ball being chipped away when it is hit. If a linguist had the luxury of being able to set down comparable conditions, e.g., monolingual speech community, no orthography, a listener learns the pronunciation of words from a single, non-redundant utterance, etc., then it might very well be possible to achieve some success in predicting language change. If physics and chemistry seem to do a better job at their predictions than linguists do, it may be because (a) their public relations effort is more intense, (b) they have been

practising their science for a longer time and hence have accumulated more useful knowledge, etc. I don't think there are any *inherent* differences between the disciplines in their potential for achieving deductively-based (but not nomological) explanations for phenomena.

2. Summary of symposium papers

As the complete texts of the symposium papers were printed in Cohen and Van den Broecke (1983), only brief summaries of these papers will be given here. These and the discussion which follows are paraphrased; use of first person pronouns does not indicate direct quotations.

Ohala: Part of the 'lore' of phonology that has accumulated over the past two centuries or so has been an intuitive expectation for the favored direction of sound change. To the extent we can make give these intuitions an empirical, phonetic base, we should be able to do a better job at reconstructing linguistic history.

Sound changes of the sort, back velars > labials, e.g., Proto-Indo-European *gwi-wo- 'living' (cf. English *quick*) > Greek *bios*, can be explained by the acoustic-auditory similarity of the two sounds. But this leaves unexplained why the substitution is usually asymmetric, i.e., why labials do not often change to back velars. The answer, I suggest, may lie in the same perceptual factors which cause asymmetries in the confusion of letters of the alphabet in a visual identification task, where, e.g., 'E' is misidentified as 'F' more often than the reverse. Viewers may miss the 'foot' of the E and therefore report it as the letter which is graphically equivalent to the E minus the foot, i.e., an 'F'. Although when viewing an 'F' they might miss some of its features, too, these would not lead to an 'E' percept. Moreover, they are unlikely to 'add' missing features. Back velars may thus have some 'extra' acoustic feature which is absent in labials; failing to perceive this would lead listeners to think they had heard a labial, but labials would not be misidentified as back velars by the same process.

Dissimilation, which operates in the reverse direction of the more common assimilation, is due, I propose, to the listener invoking - inappropriately - perceptual rules he has developed to discount the effects of assimilation in speech. Normally these perceptual rules would operate to factor out non-distinctive phonetic features, e.g., the anticipatory labialization of the vowel before a labial or labialized consonant. In the case of Latin /Kʷiŋkʷe/ > Italian /tʃiŋkʷe/, listeners apparently took the labialization on the first syllable to be entirely a manifestation of anticipatory assimilation to the second /k/ and they therefore factored it out of their lexical representation of the word, which, of course, would be the basis for their pronunciation of it. Various bits of support for this hypothesis can be found in the circumstances under which dissimilation occurs as well as in some laboratory studies (Ohala, 1981). The subtle prejudice that exists against dissimilation - since it seems to contradict the more common and phonetically motivated process of

assimilation - can be dismissed: most assimilatory processes are the product of the speaker; dissimilation is the product of the listener 'second guessing' the speaker.

Fischer-Jørgensen: The traditional descriptive system for vowels, high vs. low, front vs. back, rounded vs. unrounded, and tense vs. lax, has usually been defined primarily in articulatory terms. This system has been criticized by Wood (1982), among others, for being articulatorily inaccurate or vague and for failing to account for certain interactions beteen vowels and consonants. Ladefoged (1980) finds a need for the traditional descriptive system but bases it on acoustic-auditory dimensions, not articulatory. I believe both of these views are wrong insofar as they discard the traditional articulatory-based system. Admittedly, the notion introduced by Daniel Jones (for pedagogical purposes) that it is the 'high point of the tongue' that is being described by these terms, 'front, low' etc., is open to criticism. But if we treat these terms as characterizing the overall location of the upper tongue surface with respect to the palate, then they are physiologically more accurate and they succeed, where the other systems fail, in giving an insightful characterization of such sound patterns as umlaut and vowel harmony. In these latter two processes, moreover, tongue position behaves independently of lip rounding, which they would not do if these had only acoustic-auditory correlates. Certainly acoustic-auditory correlates for vowels are needed e.g., they play an important role in vowels' role in sound symbolism (Fischer-Jørgensen, 1978), but not to the exclusion of the quite useful traditional articulatory correlates.

Goldstein: It is a very old idea that the 'seeds' of sound change may be found in synchronic variation in speech. But since the sound changes that recur in unrelated languages proceed in certain favored directions, it follows that synchronic variation is also constrained in its directionality. A proper account of the latter can explain much about the former. I have explored this notion as it applies to (unconditioned) vowel shifts, about which the following two generalizations seem valid: front vowels shift only along the height dimension and back vowels along the height and the front-back dimension. I hoped to find the explanation for these patterns in the constraints on the conversion from articulation to sound. To do this I used a synthesizer which takes articulatory parameters as input, including one which specifies tongue body center. A variety of vowel types were synthesized and for each type, 100 tokens in which the tongue body center was perturbed along a radius 2 mm from the target position for that vowel type. These perturbations were intended to represent the kind of variability vowels are subject to in normal speech. When plotted on the F_1 vs. F_2 space, the spectra of these perturbed vowels showed that, as predicted, the front vowels' acoustic perturbation was primarily along the 'height' dimension and the back vowels along both height and front-back. Central vowels showed no such directionality.

Kohler: I wish to stress the need to introduce the time dimension into phonological analyses and to illustrate this by reference to the so-called

'voiced- voiceless' opposition in obstruents. Although usually thought of as an a-temporal feature at a static point in a segment chain I suggest that it is better characterized as a 'fortis-lenis' distinction which can be defined by a single physiological property: greater vs. lesser physiological effort and thus by faster vs. slower articulatory movements. This results in a host of measurable phonetic differences: fortis consonants will have longer closure duration, shorter VC transitions and longer CV transitions, etc.; lenis consonants, the reverse. All of these patterns can be illustrated in a detailed phonetic analysis of minimal pairs such as the German *leiden* vs. *leiten*. The shorter closure duration of lenis consonants may make actual voicing more likely. The longer closure of fortis consonants leads to a higher oral pressure and a passive cessation of voicing. Of course, voicing may also be actively controlled, and languages may differ in the ways they implement this opposition. Sound changes of the sort p/b > b/β, etc., suggest that the fortis-lenis opposition may be preserved in spite of being shifted on the fortis- lenis continuum.

3. Discussion

Fischer-Jørgensen: (to Ohala) Why – in your paper – do you reject explanations for sound change based on 'ease of articulation'?

Ohala: It is a matter of research strategy. We know so little about the 'effort' involved in articulating sounds that it is a notion that is *too easy* to invoke and, frankly, it is a notion that has often been abused. We should exhaust the explanatory principles that are known and testable before using this 'wild card'.

Fischer-Jørgensen: (to Ohala) I think your account of dissimilation is an interesting one but I do not think it can explain all types of dissimilation, e.g. those involving n/l and r/l. In those cases too many similar sounds in a word --often these are borrowed words-- confuses the listener.

Kohler: (to Ohala) I agree with that. Although I find it neat that you associate assimilation with articulation and dissimilation with perception, I do not think it can handle all cases. Examples of the sort French *marbre* to English *marble* seem to point to the articulatory difficulty of pronouncing (what must have been) two rolled *r* in a short interval.

Goldstein: (to Ohala) One thing I like about your approach is that, insofar as the directionality of sound change is explained, there is no suggestion made that the later state is any better than the original, that is, that it somehow improves the language.

Ohala: (to Goldstein) If you had specified a greater perturbation from the

vowel targets, say 3 mm instead of 2 mm, would you have obtained more fronting of the back vowels?

Goldstein: A little more, but not an appreciable amount.

Fischer-Jørgensen: (to Kohler) The different rates of movement you found for fortis and lenis obstruents in VC transitions is quite well documented, but I'm not so sure about the CV transition. Why, in any case, should it be slower for fortis than lenis? The evidence for this when C is initial is at best equivocal and at worst does not support your claim.

Kohler: Part of the basis for that claim was the finding by Öhman in 1966 of greater EMG activity in the lips following /b/ than /p/, however, I should perhaps have made a more careful distinction between initial CV transitions and those which are intervocalic. It was the latter that formed the bulk of my studies.

Tore Janson: Although I am in general sympathy with the purpose of this symposium, I am surprised at the title 'Phonetic explanation in phonology'. This is a contradiction in terms: it is *phonology* that should provide generalizations or explanations for phonetic facts. If phonetics and phonology do not always dovetail, it may be that the phonologist has disregarded certain phonetic facts or that phonological theories are imperfect. But the basic facts, which need to be explained, are phonetic.

Kohler and Fischer-Jørgensen offer solutions to the perennial problem of the relation between phonetic substance and phonological features. But trying to find a set of features which are universal, phonetically realistic, able to capture the distinctions made in all languages, etc., might be conflicting aims. A basic universal set of features may be possible but these will have to be supplemented by 'extra' features as needed.

Ohala's and Goldstein's contributions are explanations for sound change, not strictly speaking, explanation in phonology. Phonologists often cite data from sound change but this data is at heart phonetic. Also, it should be kept in mind that what they address is sound change at the very initial stages. Subsequently sound change becomes embedded in the grammatical system of the language and its further progress depends more on morphology than phonology. A desirable (if long-range) goal for phoneticians working on sound change would be rank ordering of potential changes from more to less expected.

Lehiste: (to Kohler) I question your assumption that V + C is some kind of basic building block in phonological structure and acts as a whole in manifesting the fortis/lenis distinction. How would this accommodate fortis/lenis distinctions in syllable initial position?

Kohler: The pre-vocalic consonants behave in a different way from the postvocalic ones, so it will be necessary to give a different account for the former.

Kenneth Stevens: À propos of Janson's remarks, I think it can be said that phonetics has at least provided phonology with a set of distinctive features. What we are doing here today is trying to discover ***relations*** between those features. E.g., there is a sense in which /u/ is a *labial*, in which /i/ is a *coronal* or *palatal*, etc. We need to find out how one feature can help another and (in sound change) gradually take over for another.

G. Heike: In accord with Kohler's findings, 23 years ago I studied the German dialect of Cologne and found that in intervocalic position there was no difference between stops in voicing, intensity of burst, or aspiration but they did differ in duration. Changing this duration by tape splicing could change a lenis stop into fortis.

À propos of Goldstein's study, I would prefer to see more work with *dynamic* articulatory models to answer phonological questions. The German 'r' in post-vocalic position has many variants: from a uvular trill to a glide. All variants have in common a dorso-velar closing gesture. The dynamic gestures in speech would make a better focus for phonological theory than discrete segments.

Björn Lindblom: (To Goldstein) Some work that Johan Sundberg and I did some years back with an articulatory model also revealed an asymmetry in the propensity of vowels to shift along the front-back dimension. We observed from X-rays that the physiological rest position has the tongue in a more or less front position. Thus when we made peripheral vowels [i, e, a, o, u] and let them glide to the neutral position, the front vowels changed along the height dimension but the back vowels exhibited fronting.

René Gsell: On the subject of phonetic explanation for sound change, it should be noted that the essence of language is change. The code of the speaker may differ from the code of the hearer and so there must be a shifting between the realization of one and the identification of the other. Thus there arise allophones in synchrony which leads to phonological change diachronically. But the selection of one variant as the norm is a social fact and cannot be explained phonetically. Phonetics only explains the 'birth' of the variant, not its subsequent fate in the language.

(to Ohala) The shift of labialized velars to labials may be due to acoustic-auditory reasons but I do not agree that the shift of palatalized labials to apicals has the same explanation.

Vicky Fromkin: From the title of this symposium I had hoped that there would be a great deal of discussion on phonology. I think we've had very

little discussion on phonology--only some *aspects* of phonology. There are some tremendously exciting theoretical approaches being proposed currently. It would be interesting to find out whether phonetic evidence can be found for some of these, e.g., metrical phonology and autosegmental phonology.

Ohala: The evidence discussed for the spreading of features throughout a word does have relevance to autosegmental notation. There is, in fact, quite a bit of phonetic literature already accumulated on this and other points which have a bearing on these newer approaches, e.g., Öhman (1966), but there seems to be little interest or attention given to it by the proponents. The phonetic literature is quite vast and the data in it obtained through much hard work and ingenuity; it deserves to be mined for its phonological relevance.

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