

Explaining the Phoneme: Why (Some Of) Phonology Is Natural

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ON EXPLAINING THE PHONEME: WHY (SOME OF) PHONOLOGY IS NATURAL  
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One of the most important discoveries in the history of phonological theory is the construct that has been called the "phoneme."<sup>1</sup> In the early work in which the phoneme plays a prominent role, there are basically two positions concerning the nature of this construct. The first position, that the phoneme is a psychological or mental entity, was held by, among others, Baudouin de Courtenay (1895), Sapir (1925, 1933), and the early Prague school. Thus, Sapir (1925), for example, states that "...the objective relations between sounds are only a first approximation to the psychological relations which constitute the true phonetic pattern." The second position, taken by such investigators as Bloomfield (1933), is that the phoneme is some kind of physical entity (either a characteristic of all and only the allophones of a given phoneme, or the sum of the allophones). Twaddell (1935) challenged both of these positions, arguing that "...it is inexpedient and probably impossible (at present) to associate the term [phoneme] with a reality...because the purposes to which the term may be put in our discipline are served equally well by regarding the phoneme as an abstractional, fictitious unit." That is, the phoneme, though convenient for making statements that we would like to make about the phonological structure of various languages, has neither psychological nor physical reality. Halle (1959), in his celebrated argument from Russian voicing assimilation, takes Twaddell a step further, arguing that one aspect of phonemic theory (that which requires that it be possible to determine a phonemic representation from a given phonetic representation) "involves a significant increase in the complexity of representation." I.e., the phoneme is an inconvenient "fictitious unit," and should be banished forever from the realm of phonological theory. In this paper, I will argue that not only should the concept "phoneme" play a role in phonological theory, but it does in fact have reality of the psychological sort. After establishing this point, I will then turn to an attempt to explain why such a psychological construct should exist.

The basis for all of my arguments in favor of a psychologically real phoneme is establishing a set of psychological facts that require an explanation, and for which the existence of phonemes seems the only likely candidate. No fictitious unit, no matter how convenient, can play a role in a real explanation of real facts. Furthermore, if the facts in question are psychological in nature, then we will require constructs which are also of a psychological nature in order to explain them; one cannot explain psychological facts by appealing to, say, physical forces (aside from, perhaps, those ultimately responsible for the psychological constructs in question) any more than gravity can be explained on the basis of the shape or size of the earth.

The first kind of fact is the following often-noted phenomenon: "Although the speakers produce and the hearers experience objectively different sounds, they are not aware of that difference" (Twaddell 1935). Twaddell is referring here only to differences within a given allophone, which could presumably be explained on the basis that the differences involved are too small to be perceived (aside from cases involving free variation). But other similar facts cannot be explained on this basis, since the differences in question can be phonemic differences in languages other than in question. As Sapir (1925) puts it: "In most languages, what is felt by the speakers to be the 'same' sound has perceptibly different forms as [phonetic] conditions vary." Thus, for example, English [p] and [p<sup>h</sup>] are not perceived as being "different" sounds by native speakers, despite the fact that they are perceived as being different by speakers of languages such as Thai and Hindi--and in fact may function to signal meaning differences in these languages. The fact that speakers of these languages (and speakers bilingual in these languages and English) can routinely perceive the differences in question shows clearly that what is involved is not the physiology of the human perceptual system, but rather the neurolinguistic processing of these auditorily different sounds.<sup>2</sup>

The example under discussion is particularly instructive in that it shows that physical phonetic similarity is not always of primary importance in determining which phones are considered psychologically the "same sound." The standard textbook accounts of the articulation of these sounds describe [p] as being voiceless and unaspirated, while [p<sup>h</sup>] is said to be voiceless and aspirated; [b], on the other hand, is voiced and unaspirated. In these terms, [p] shares with [p<sup>h</sup>] the property of being voiceless, and with [b] that of being unaspirated (all three being, of course, bilabial). That is, unless there is some way of ranking these properties in such a way that a higher-ranked property counts more for determining phonetic similarity, [p] is no more different from [b] than it is from [p<sup>h</sup>]. Furthermore, as Swadesh (1934) has pointed out, there is another property that is relevant: both [p] and [b] are relatively lenis, while [p<sup>h</sup>] is fortis. Note further that if the s in an sp-initial word is removed, say by splicing it out of a taped utterance, the result is invariably perceived as having initial /b/ (cf. Ladefoged 1975). Thus, if anything, [p] is more similar phonetically to [b] than it is to [p<sup>h</sup>].<sup>3</sup> Despite this fact, I know of no researcher who has claimed that [p] and [b] are allophones of the same phoneme in English.<sup>4</sup> This represents a significant amount of agreement on this point, especially given the fact that with respect to other issues, linguists show a penchant for taking all possible positions that are even remotely plausible.

All of the above discussion is based on more or less impressionistic observations on the part of me and other linguists. One might well question these claims on the basis of a lack of any hard "scientific" evidence. Even without this type of evidence,

the arguments seem to me to be sufficiently compelling that the burden of proof would rest on those who might want to deny the existence of the putative facts in question. However, Jaeger (1980) reports on psycholinguistic research which appears to support strongly Sapir's position. I will summarize here only one of her two experiments which appear to support this position, the one which involved classical conditioning of subjects to respond to words containing [k<sup>h</sup>] (so chosen because of the large number of ways of spelling this sound). Of the four naive subjects who were eventually conditioned to respond to this sound, all four also responded to words containing [sk] in subsequent testing, while failing to respond to words with [g] and words without velar stops, at a statistically significant rate. Interestingly, two of these subjects did not notice that it was words containing [k<sup>h</sup>] that correlated with the mild shock that was being used for conditioning purposes ("galvanic skin response" was used to determine whether subjects responded even without the shock), thus providing further evidence that conscious attention to, say, spelling was not responsible for the results. Thus, there is good experimental evidence that [k] and [k<sup>h</sup>] are psychologically the same for these speakers.

Let us now turn to the second kind of fact, which, once again, Sapir (1925:25) has emphasized: "...people find it difficult to pronounce certain foreign sounds which they possess [in restricted phonological contexts--DGC] in their own language." For example, English contains nasalized vowels (before nasal consonants), but most speakers have considerable difficulty in coping with French nasal vowels, as any French teacher can attest. Similarly, Sapir points out, Nootka speakers invariably substitute n for ɲ, which occurs in Nootka, but only in song. It might be objected that this phenomenon is not psychological, but physiological, in nature, in that the vocal tract has been trained to be able to produce such sounds only in the environments that are appropriate for the language in question. While there seems to be at least some truth to this, it clearly cannot be the entire explanation. English has nasal vowels not only before nasal consonants, but also before voiceless consonants (cf. Malecot 1960), at least for some speakers. In fact, this can be demonstrated even without acoustic study for such speakers who can optionally pronounce pre-consonantal t̚ as glottal stop. In sequences such as can't go, the glottal pronunciation contains no alveolar articulation at all, and thus, obviously, no [n]. What we have here is a sequence of a nasalized vowel plus glottal stop (however much it may "feel" to speakers, including the present author, that there is an n present). Similarly, most speakers of English find syllable-initial pt clusters utterly unpronounceable, with the result that loans like pterodactyl which contain such a cluster are simplified so as to begin with t. However, such clusters do occur in casual speech, as in potato ([pt<sup>h</sup>eDo]), so we clearly are able to articulate such a sequence. It is only when we are trying to say them that they are a problem. It is neither our ears nor

our vocal tracts that are responsible for our phonological inabilities. It is the brain, apparently, that is responsible, as a result of trying to send the vocal tract instructions that are within its capabilities (although it sometimes crosses itself up, as in the case of what we might call the "potato phenomenon") or to design a system that allows for efficient phonological storage.

Finally, slips of the tongue and the adaptation of loan words provide evidence in favor of the psychological reality of phonemes. Fromkin (1971, 1980) reports that the output of slips of the tongue is invariably in accord with the allophonic rules of the language in question. Since, as was seen above, this kind of thing cannot be due solely to vocal tract physiology, there must be something of a mental nature involved here as well. Similarly, loan words are "fixed up" so that they obey the phonological structure of the borrowing language (cf. Ohso 1973, Lovins 1973, 1974, Bjarkman 1976, and below), as our treatment of words such as pterodactyl illustrates. This phonological structure must be of a rather abstract, psychological, sort, since the offending sequences are readily pronounceable (as long as we don't try to pronounce them!), as was demonstrated above.

We would like to be able to explain these facts, of course-- and the only serious candidate appears to be notions such as "phoneme" and "allophonic rule." Again, since the facts in question are psychological facts, the concepts used will have to have psychological content. Thus, it seems clear that Sapir was quite correct when he stated (1925:25) that "...phonetic phenomena are not physical phenomena per se..." Both phonemes and allophonic rules are psychologically real entities, although the latter, as alluded to above, have a physiological motivation.<sup>5</sup>

We are now faced with a fact that seems as puzzling as those for which we have just provided an explanation. Why do phonemes exist? I.e., what is it about the acquisition process that causes adults (and older children) to have such abstract concepts? This is a question which most linguists and psychologists have simply failed to ask, and it is easy to understand why. For those who take the "fictitious unit" position seriously (including, presumably, most behavioristically-oriented psychologists, as well as the American structuralist linguists who were heavily influenced by behaviorism), this question is utterly nonsensical. And for generative phonologists and generative grammar-oriented psychologists, who reject the distinction between phonemes and morphophonemes, it is equally impossible to ask such a question. But it is an important question to ask, if phonemes are psychologically real entities and significantly different from morphophonemes. Is there a significant difference between these two kinds of constructs? It seems clear that there is, since, for one thing, the facts discussed above do not hold for morphophonemes. No one feels that the c in electric and that in electricity are the "same sound," and no one would have any difficulty pronouncing the latter with a [k] rather than an [s] or vice versa. Some morphophonemic rules do apply to the output of slips of the tongue (such

as that responsible for the allomorphy in the English indefinite article), and play a role in nativization (at least in the later stages), but many do not, while all allophonic rules are applicable in such cases. There is also an extremely important difference from the standpoint of acquisition: there are no alternations in the case of allophonic variation to give the child any reason to go deeper than the surface form. Note that doing so would violate the Strong Alternation Condition of Kiparsky (1968) if we take this condition literally (although it seems clear that Kiparsky did not intend it to apply in the case of allophonic variation). That is, there is no evidence from the morphophonemic alternations that figure so prominently in generative phonology to suggest to the child that he or she should do anything other than take the allophones at face value. Thus, the standard generative approach to language acquisition--that the child is a "little linguist" who discovers alternations, posits abstract underlying forms and corresponding rules, and chooses the simplest system on the basis of some kind of evaluation measure--is irrelevant in this area.

Suppose, then, that the child is not only a "little generative phonologist," but also a "little classical phonemicist," in the sense that children have the ability to do the equivalent of minimal pair tests, to discover complementary distributions, etc. Is this a realistic picture of phonemic acquisition? There are a number of reasons to think that it is not. First of all, as Patricia Donegan has argued in unpublished work, what is known about the nature of memory makes it extremely implausible that something like this could be going on. But suppose, for the sake of argument, that it is physiologically possible for a child to do something like this. Is there any evidence that children do do this? If, in particular, they were actually seeking out (maximally general) allophonic rules, then we would expect them to at least occasionally overgeneralize, as they clearly do in the case of other kinds of rules. But, to the best of my knowledge, no child has ever been reported to, say, aspirate all syllable-initial stops, rather than only voiceless ones. Overgeneralization of allophonic rules never seems to occur; moreover, it is only allophonic rules which are not subject to such overgeneralization. Thus, it would appear, children are not actively seeking out allophonic rules. But if they are not, how can they end up having them?

The position that we appear to be driven to is that such constructs are in some sense innate, since we "know" (in the funny Chomskyan sense) things that we have not learned. This is the position taken on at least partially independent grounds in the theory of "natural phonology" (cf. especially Stampe 1973, Donegan and Stampe 1979): children don't actively acquire phonemes or allophonic rules at all. Rather, these mental-but-physiologically-motivated rules (=Stampe's "natural processes") are built in (innate), and if we are not forced by the language we are acquiring to unlearn them, they survive to adulthood. Thus,

for example, French speakers are forced to unlearn vowel nasalization, but as learners of English, we were not. If we suppose further that children have the ability to abstract away from the effects of these processes--i.e., that any phone that can be derived via a natural process from another will be treated psychologically as the latter (as long as they are not contrastive, and even then in certain cases, I would argue)--then we will have an explanation for the existence of both phonemes and allophonic rules.

But is there any independent reason for believing that this is in fact the case? To my mind, the evidence adduced by Stampe (1973) and Donegan and Stampe (1979) would by itself be sufficient to render this position eminently plausible. However, judging from the phonological literature, I am somewhat in the minority in this respect, and I would like to consider briefly here a kind of evidence which I find extremely compelling, but has only recently come to light. The phenomenon in question is what I have termed "impossible nativizations," and have discussed in more detail elsewhere (Churma 1984). As is well known, syllable-initial sr and sl are disallowed in many dialects of English. There are basically two approaches toward describing such constraints, the "morpheme structure rule" approach of, e.g., Halle (1959), and the "morpheme structure condition" approach suggested first by Stanley (1967). The latter approach would require something like rule (1a), which simply states that the offending sequences are not permitted, while the former would require either (1b) or (1b').

- (1) a. \*#sr; \*#šl  
 b. s ---> š / # \_\_\_ r;      š ---> s / # \_\_\_ l  
 b'. l ---> r / #š \_\_\_;      r ---> l / #s \_\_\_

Note that there is nothing internal to modern English that would allow a choice between the latter two possibilities; in fact, it is just such kinds of cases that have been said to provide support for the condition approach. But if we examine what happens (and, more importantly, what does not happen) when loan words are nativized, we find that changes such as those in (1b') are utterly impossible. (The number of speakers who have now heard me make this claim is well into triple figures; the probability of them all agreeing about this--and I have had no objections--by chance, assuming only 100 speakers, is  $(1/2)^{100}$  or one in over 1,200,000,000,000,000,000,000,000,000,000,000!). Sri Lanka can come out with initial /šr/, but not \*/sl/, and Schlitz is pronounced by some speakers with initial /sl/, but everyone rejects the possibility of fixing it up by changing the initial cluster to /šr/. In order to explain the (impossible) nativization facts, it appears that we must accept (1b) as a psychologically real rule. But the child has no more reason to choose (1b) over (1b') (or (1a), for that matter--which makes no predictions at all about how nativizations will behave) than the linguist does. Unless (1b) is in some sense innate, that is, it will be impossible to explain

why acquirers of English invariably end up with this rule, rather than some other rule which accounts equally well for the data and appears to be just as phonetically plausible. (The phonetic motivation for this rule appears to be an assimilation with respect to point of articulation, at least for my articulation of /r/. But note that (1b') would be an assimilation of exactly the same type.)

Even in the face of this and the other evidence adduced in favor of natural phonology, one might still feel somewhat uncomfortable positing innate constructs, especially ones as specific as rule (1b). Derwing (1977), in fact, questions "the value of any linguistic theory that attempts to invoke 'innateness' as an explanatory vehicle" on the basis that doing so "does not provide any positive insight into either its nature or development, but is rather tantamount to an admission of failure to explain it." (He is referring here specifically to natural phonology.) To be sure, one must be careful about abusing this notion, but it seems to me equally undesirable to go too far in the opposite direction. Surely no one would question the value of a biological theory that proposed that the reason why birds have wings is that this is part of their genetic makeup. The reason why such a theory is acceptable is that there are good reasons for believing it to be true, and we should clearly evaluate linguistic theories on the same basis: does the evidence support the claim about innateness or does it not? It would be just as wrong to claim that something that is innate is not, as to claim that something that is not innate is. There is, of course, nothing to prevent the biologist from asking further why wingedness is innate to birds, and indeed one might well propose an explanation based on such concepts as evolutionary advantage--nor is there anything to stop the biologically oriented linguist from asking an analogous question.

Let us look, then, somewhat more carefully at the argument. It is worth pointing out that the argument given above appears to have another strike against it: since most of data discussed come from English, we are essentially positing a universal on the basis of data from a single language. In fact, however, there is nothing wrong in principle with such an argument, as I will now attempt to demonstrate. I give in (2) an outline of the single language/universal argument:

- (2) a. If a set of speakers S "know" X, but can't have learned X, then X is innate to S.
- b. All of the English-learning children studied to date seem not to have learned phonemes and allophonic rules, and all English-speaking adults (and older children) seem to "know" these things.
- c. Therefore, these things are innate to the children studied.
- d. If X is innate to S, then X is innate to all humans.

(2c) follows from (2a) and (2b) by modus ponens, and the

conclusion that "these things are innate to all humans" would follow from (2c) and (2d), again by modus ponens. There is thus nothing at all suspect about the form of such an argument; we have here an impeccable double application of modus ponens. If it fails, then, it must be because one of the premises is false. (2a) seems to me to be thoroughly plausible, and establishing the truth of (2b) has been the focus of this paper up until now. If we accept these premises, then we are bound by simple logic to accept the conclusion (2c). Before turning to (2d), I would like to compare briefly this argument with a typical Chomskyan argument for innateness of some syntactic principle. From the standpoint of form, there would be no difference between the two kinds of arguments, the Chomskyan argument again depending on a double application of modus ponens. Premise (2a) would also remain unchanged, but (2b) would replace "seem not to have learned" by "cannot have learned." This is an important difference, especially since this claim is typically made without any reference to the actual facts about acquisition. The second conjunct of (2b) would also presumably be more problematic in the Chomskyan argument, since many researchers would not accept the claim that adults "know" what they are claimed to "know." Thus, not accepting the Chomskyan conclusion need not entail rejecting the conclusion of the present argument.

But (2d) must also be true in order for the argument to go through, and it seems clear that it is not. Musical ability, for example, is not the kind of "X" that we would like to generalize about on the basis of a small number of individuals. We can't all be Beethovens, and many of us can't even carry a tune, but if we examined a statistically freaky group of individuals and found that they all had substantial musical talent, (2d) would enjoin us to conclude that all humans do. Unless there is a crucial difference between language abilities and musical abilities, then, the argument is in trouble. But there does in fact seem to be such a difference: it is a truism that all normal individuals end up by acquiring remarkably similar grammars of the language(s) they are exposed to. Furthermore, the unanimity concerning the phonetically counterintuitive grouping of [p] and [p<sup>h</sup>], without regard to the language spoken by ancestors, provides strong evidence that there are no relevant genetic differences with respect to the acquisition of this aspect of language. That is, it seems that a weaker version of (2d) is likely to be true:

(3) If X' is innate to S, then X' is innate to all humans.

If "X'" is taken to be "phonemic acquisition ability," this is an extremely plausible hypothesis, and the desired conclusion is correspondingly likely. Note that showing that one of the premises on which this argument rests would seriously weaken the conclusion (though it would not, strictly speaking, falsify it), since it is hard to see what other kind of evidence could be provided for it. Note further that extending the range of linguistic

phenomena to include other abilities such as syntactic ones (and I find such an extension quite plausible, although I will not press the case here) would render a Chomskyan argument legitimate--as long as premise (2b) (the problematic one) can be shown to be true.

Some apparent substantive problems have also been brought up in the literature. First of all, different languages have different rules which act to remove the same phonetic difficulty (Dinnsen 1980), and show what we might call "suballophonic" differences (i.e., different languages have phonetically different vowels that we would write as /u/, etc.--cf. Ladefoged 1983). This creates the problem of explaining why, if the processes in question are innate, speakers of one language choose one in order to alleviate the phonetic difficulty in question, while speakers of other languages choose different ones, in the first kind of case, and why different languages have different "suballophonic" rules in the second. A related objection has been raised by Manaster-Ramer (1982), who points out that different languages may treat the sound in different ways when nativizing it. Thus, for example, German invariably converts English interdental stops to the corresponding dental stop, while in French the alveolar fricatives are substituted. (In fact, if my informal observations are to be trusted, even dialects of the same language may differ in this way, since Canadian French speakers show the dental stops.) Again, we would like to know why German speakers apply their innate despirantization rule, while French speakers "prefer" to use their backing rule, despite the fact that both rules are innate to both sets of speakers.

The facts in question appear to me to be quite genuine, but not, at least in principle, unexplainable in terms of the theory of natural phonology. Before turning to an examination of possible explanations, I would like to give some attention to Dinnsen's discussion, which is marred by an apparent failure to distinguish between explanation and prediction. He maintains that in order to qualify as an explanation, a phonetic account must explain why one change occurs to the exclusion of others. It is not enough, for Dinnsen, to show that the process in question serves to remove a phonetic difficulty, as long as there is another way of removing that difficulty.<sup>6</sup> For me, this is all that is necessary--if we understand why something occurs, then we have explained it. We can't predict when or if another language will respond to this same difficulty in the same way, but prediction is not explanation. If we required that all explanatory accounts make predictions, then we could never explain a past event (which would make the practice of conducting autopsies in order to determine the cause of death a prime candidate for the Golden Fleece Award!). We may not be certain that we have the correct account, but neither can we be certain that our predictions will be borne out. This kind of uncertainty is simply a fact of life for all of science.

However, some of the uncertainty about which phonetic process

will occur in which language will quite possibly be removed by future advances in our understanding. I will mention two areas in which such an advance seems likely. The first is the traditional notion of the "basis of articulation" (which Ladefoged in fact mentions in a pair of sentences but otherwise ignores). It has often been maintained, for example, that the French basis is relatively far forward in the mouth and with significant lip rounding (cf. Schourup 1983 for a survey and some extremely interesting and promising discussion of this notion). This could be taken as a valid explanation for why French has as many front rounded vowels as it does--including, perhaps significantly, the hesitation vowel. For the present, this is really no more than a promissory note, but I find Schourup's arguments concerning the relevance of the basis to natural phonology in particular quite suggestive. This could also obviously play a role in explaining the cross-linguistic differences discussed by Ladefoged, so it is difficult to understand why Ladefoged did not discuss it any more than he did. Similarly, the borrowing differences may also yield to explanation in such terms.

To Schourup's discussion I would like to add one observation which, like so much of the evidence for the basis, is anecdotal in character. It has often been maintained that when one speaks a second language with a reasonably accurate pronunciation, it is necessary to "warm up" after not having spoken this language for some time before the limits of accuracy can be attained. My own experience confirms this contention, and it seems that the basis of articulation could provide an explanation for why this is so. If we suppose that it is necessary when changing from one language to another to also change bases, and that this latter task is relatively more difficult as the length of time during which a basis has not been used increases, then this phenomenon is just what we would expect.

The other area, in which some progress is now being made, is the difference in prosodic structure that is found from language to language (cf. especially Donegan and Stampe 1983, Selkirk 1984). That is, it may be that the processes that are innate vary not only with respect to the basis of articulation, but also with respect to what kind of prosodic system is present. Alternatively, it may be the case that all processes are innate, but each requires a "trigger" in the basis and/or the prosodic system in order to be activated. This approach bears obvious similarities to the parametric approaches advocated in various other contemporary theories, in which the parameters, like the basis of articulation and the prosodic structure of the language being acquired, must be learned, but most of the rest of the linguistic system is built in. In sum, it seems likely that phonology is not quite as simple as most current theories make it out to be.

One final apparent objection will be considered here, that of Anderson (1981). Anderson begins and ends his article with quotations from Donegan and Stampe (1979); a passage from the conclusion is given below (the passages in single quotes are taken from

Donegan and Stampe):

"Language is not 'governed by forces implicit in human vocalization and perception.' Language is not simply a 'reflection of the needs, capacities, and world of its users'... In this sense, phonology is clearly not 'natural.'" (Anderson 1981:535).

Given this, one might expect that the body of the paper would contain, at least in part, a critique of Donegan and Stampe's position. Aside perhaps from the claim (p. 512) that "if we were to attempt to impose phonetic naturalness as a defining condition on the class of objects we study in phonology, we would not be left with very much" (made without discussion of or even reference to the claim to the contrary made by Donegan and Stampe 1979:128 that "this includes far more than it excludes"--a sentence which immediately follows one of those quoted by Anderson), however, it does not. What Anderson has actually done is argue (and demonstrate, I feel) that what Donegan and Stampe would call morphonology is not natural. I do not see why Anderson felt compelled to demonstrate this, since no one, to my knowledge, has ever denied it, certainly not Donegan and Stampe. One could conceivably construe the body of Anderson's paper as a critique of the "Natural Generative Phonology" of Theo Vennemann and his students (though not a very good one, I would maintain), but since he does not refer to any of their work, this seems unlikely. Anderson has succeeded in demolishing his straw man, but the non-effigy is still very much alive; at least in the area of allophonics (and probably in all of automatic phonology), phonology, contrary to Anderson, is natural. Moreover, if we accept the distinction between phonology and morphonology, we can maintain that all of phonology is natural.

#### NOTES

1. The "phoneme" I am referring to here is the classical one of the American structuralists which is a unit of surface contrast. This is the same phoneme which Halle (1959), and Chomsky in later work, have attacked. Lest it be thought that I am being inconsistent (or have changed my mind!), I still feel that Halle's argument was successful to a limited extent (cf. Churma 1983). In particular, his argument appears to legitimately refute theories which require a rigid prohibition against "mixing of levels," such as the structuralists he was criticizing. But it did not show that the phoneme has no role to play whatsoever in linguistic theory, only that it does not exist as some identifiable level in a generative derivation (cf., for example, Schane 1971).
2. There are two different senses of "perceive" that the English language does not allow us to readily distinguish. In one sense of this word, speakers do in fact perceive such differences,

since, as Sapir has pointed out, they can notice foreign accents that make use of the "wrong" allophones. Nevertheless, they are not consciously aware of them, will deny that they exist, and fail to make use of them when performing such tasks as designing orthographies. It is in this sense that they cannot perceive these differences.

3. Swadesh (1935), in his critique of Twaddell's monograph, argues that [p] is slightly less lenis than [b], and that this is why the former is grouped with [p<sup>h</sup>]. Even so, as Twaddell (1936) points out in his reply to Swadesh, [p] is phonetically more similar to [b] than it is to [p<sup>h</sup>], if we accept Swadesh's account of the facts.

4. Some, such as Twaddell and the Praguians would set up a third phoneme or "archiphoneme." Ladefoged (1975) seems to be suggesting that the orthography is responsible for our perception that [p] and [p<sup>h</sup>] aren't really different (and cf. Ladefoged 1983 for further argument along these lines). This position is extremely implausible on several counts. First of all, the orthography does not cause us to feel that other sounds that have the same spelling, but are phonemically distinct, are not different (see below for further discussion). Secondly, preliterate children who invent orthographic systems after having learned only the names of the letters and how to make them invariably use the symbol for the voiceless sounds to represent post-s stops (cf. Read 1971). Finally, the experimental evidence discussed by Jaeger (1980) indicates that spelling cannot explain this phenomenon (see below).

5. It is worth pointing out that, while phonemes are relatively accessible to consciousness, allophonic rules, like other linguistic rules, are not.

6. Dinnsen does not go into much detail about what would replace phonetics as an explanatory device, giving only a sketchy account of his theory of "atomic phonology" which is apparently intended to give a vague idea of what would be involved in a true explanation. What is more, he says nothing about the severe problems faced by the theory that Donegan and Stampe (1977) have pointed out.

#### REFERENCES

- Anderson, S.R. 1981. Why phonology isn't natural. LI 12:493-539.
- Baudouin de Courtenay, J. 1895. An attempt at a theory of phonetic alternations. In E. Stankiewicz (ed.), A Baudouin de Courtenay anthology. Bloomington, IN: Indiana University Press.
- Bjarkman, P. 1976. Natural phonology and loanword phonology (with selected examples from Miami Cuban Spanish). University of Florida dissertation.
- Bloomfield, L. 1933. Language. New York: Holt, Rinehart and Winston.

- Churma, D. 1983. A further remark on the 'Halleian Syllogism.' *Lingua* 59:345-54.
- Churma, D. 1984. Impossible nativizations as phonological evidence and the explanation of constraints on phonological structure. *JL* 20:223-7.
- Derwing, B. 1977. Is the child really a 'little linguist'? In J. Macnamara (ed.), *Language learning and thought*. New York: Academic Press.
- Dinnsen, D. 1980. Phonological rules and phonetic explanation. *JL* 16:171-91.
- Donegan, P. and D. Stampe. 1977. On the description of phonological hierarchies. In S. Fox, et al. (eds.), *CLS book of squibs*. Chicago: Chicago Linguistic Society.
- Donegan, P. and D. Stampe. 1979. The study of natural phonology. In D. Dinnsen (ed.), *Current approaches to phonological theory*. Bloomington, IN: Indiana University Press.
- Donegan, P. and D. Stampe. 1983. Rhythm and the holistic organization of language structure. In J. Richardson, et al. (eds.), *Papers from the parasession on the interplay of phonology, morphology, and syntax*. Chicago: Chicago Linguistic Society.
- Fromkin, V. 1971. The non-anomalous nature of anomalous utterances. *Lg.* 47:27-52.
- Fromkin, V. 1980. *Errors in linguistic performance*. New York: Academic Press.
- Halle, M. 1959. *The sound pattern of Russian*. The Hague: Mouton.
- Jaeger, J. 1980. *Categorization in phonology*. University of California (Berkeley) dissertation.
- Kiparsky, P. 1968. How abstract is phonology? In O. Fujimura (ed.), *Three dimensions of linguistic theory*. Tokyo: TEC Company.
- Ladefoged, P. 1975. *A course in linguistic phonetics*. New York: Harcourt Brace Jovanovich.
- Ladefoged, P. 1983. The limits of biological explanation in phonetics. *UCLA Working Papers in Phonetics* 57:1-10.
- Lovins, J. 1973. *Loanwords and the phonological structure of Japanese*. University of Chicago dissertation.
- Lovins, J. 1974. Why loan phonology is natural phonology. *Natural phonology parasession*, 240-50. Chicago: Chicago Linguistic Society.
- Malecot, A. 1960. Vowel nasality as a distinctive feature in American English. *Lg.* 36:222-9.
- Manaster-Ramer, A. 1982. *How abstruse is phonology?* University of Chicago dissertation.
- Ohso, M. 1973. *A phonological study of some English loan words in Japanese*. Ohio State University Working Papers in Linguistics 14:1-26.
- Read, C. 1971. Pre-school children's knowledge of English spelling. *Harvard Educational Review* 41:1-34.
- Sapir, E. 1925. Sound patterns in language. *Lg.* 1:37-51.

- Sapir, E. 1933. The psychological reality of phonemes. In D. Mandelbaum (ed.), Selected writings of Edward Sapir. Berkeley, CA: University of California Press.
- Schane, S. 1971. The phoneme revisited. Lg. 47:503-21.
- Schourup, L. 1983. The basis of articulation. Ohio State University Working Papers in Linguistics 25:1-13.
- Selkirk, E. 1984. Phonology and syntax: the relation between sound and structure. Cambridge, MA: MIT Press.
- Stampe, D. 1973. A dissertation on natural phonology. University of Chicago dissertation. [Published in 1979 by Garland Publishing.]
- Stanley, R. 1967. Redundancy rules in phonology. Lg. 43:393-436.
- Swadesh, M. 1934. The phonemic principle. Lg. 10:117-29.
- Swadesh, M. 1935. Twaddell on defining the phoneme. Lg. 11:244-50.
- Twaddell, W.F. 1935. On defining the phoneme. Language Monograph No. 16.
- Twaddell, W.F. 1936. On various phonemes. Lg. 12:53-9.