

## **A Retrospective on Phonology in Chicago 1965-2004**

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### **Introduction**

I would like to start by saying that this talk is going to be a reminiscence, not a history. It will be a mixture of anecdotes about CLS and my commentary on the development of phonology over the last 39 years. In discussing the early history of CLS, I have made the perhaps unwarranted assumption that CLS archives are not neatly organized, and I did not try to search through them. There was a history of CLS prepared by officers in the 70's, but they did not consult me, and as a result they left some lacunae in the early period. Nothing in this talk pretends to be complete and accurate. I have known for quite a while that my memory is fallible. The flaw in my ruminations on phonology lies in the fact that I have not worked in phonological theory for more than ten years. I will certainly slight more recent theories. I cannot cover all of the history of phonology in one hour, and I will limit the discussion to several topics that have interested me though the years.

Since this is going to be a personal commentary, I should probably let you know the theoretical views that may color my interpretations. Except for a very brief attachment to pure generative phonology in the mid sixties of the last century, I have maintained a belief in a principled difference between pure phonology and morphophonology. The definition of morphophonology does not differ from that of Roman Jakobson, as it was taught to me by Edward Stankiewicz, but the justification comes from the work of David Stampe. In modern theoretical terms, pure phonology is that phonology based on the constraints imposed on pronunciation by the surface phonotactics of neutral speech, plus the optional processes which occur in casual speech. Morphophonology describes alternations which are not determined by surface constraints on pronunciation, because they involve lexical or grammatical conditioning.

Along with believing in the relevance of the surface phonotactics of neutral speech, I believe in a slight abstraction from that surface--the phonemic level. The phoneme is relevant to phonology in at least two ways. First of all, the phonemic level is the level of basic phonological perception. Second, at least in my opinion, the segments which spell morphemes at the basic level of representation should be identical in content to phonemes, with the possible addition of underspecified segments made from neutralized phonemes. This means I think we can and should do without abstract segments. To do that I have to allow for the fact that in morphophonology, paradigmatic patterns are sometimes as important as segments and rules.

I do believe that for phonology we need both constraints and rules, and I believe that true explanation in phonology is to be found less in the formal aspects of grammar than in relating the facts of phonology to the facts of human

perception, the physiology of speech, the acquisition of language, and language change.

That is enough about theory for the moment. Let's switch to local history. Gene Gragg and I are, I think, the only people left at the University of Chicago who attended the first regional meeting of CLS in 1965. At the time, I was a first year graduate student in Slavic Linguistics. I had never had a linguistics course in undergraduate school, so I was poorly equipped to follow theoretical papers. Since Doris Bartholemew was the organizer, I am certain that there were some very good papers in anthropological linguistics, but I may not have attended them. I remember a less than serious discussion of Chomsky's linguistics by Richard Beck, and a couple of nuns discussing the problems of teaching French to Americans. The talk that I remember most clearly was on mathematical linguistics. It was made by a young graduate student that I have been careful not to try to reidentify. He proposed to prove something with ring theory. He was writing his proof on the board when he suddenly realized that the next step in the derivation would bring him back to the equation with which he had started. He stopped, looked at the board, said: "I made a mistake," and sat down.

The next year I was taking courses with Jim McCawley, and linguistics at Chicago had become very exciting. I should remember more about the phonology papers at CLS Two and Three, but I do not. In fact I am pretty sure that I gave a phonology paper at CLS Two, on Russian accent, but I have forgotten what I said. Perhaps at CLS Two, but I think at CLS Three, David Stampe gave a talk criticizing the use of the economy metric using feature counting to account for phonological admissibility, arguing that we get ridiculous results when we trying to apply it to children in the process of learning a language. This was a companion piece to Jim Mc Cawley's CLS Three paper "Can you count Pluses and Minuses before you learn to count," which was included in *The Best of CLS* volume in 1988.

In the year 1967-8 Charles-James Bailey and I were selected chairmen of CLS. We were later joined by Alice Davison. The previous year the chairman had not shown up for several monthly meetings, and there was a long delay while we waited to see if he would come. Someone thought that having more than one chairman would solve that problem.

There were two important innovations within the Chicago Linguistic Society that year. We decided to shift the monthly meetings to the University of Chicago, and we published the proceedings of the fourth regional meeting of CLS. In both cases the idea came from Bailey. For the previous two years, the monthly meetings had been held at the University of Illinois at Chicago. By 1966-7, as the talks got more oriented toward generative grammar, practically no one came to monthly meetings except people from the University of Chicago. Bailey complained about the fact that all of us were leaving Hyde Park for what was de facto a meeting of U of C linguists, but he hesitated to do anything to offend other linguists in the area. In my first year at Chicago we had met at the downtown campus of Northwestern University--in a smallish room in a neighborhood with next to no legal parking. I had been present when the decision

to switch to UIC had been made. What I remembered was that a linguist from UIC had extended an invitation, pointed out the obvious benefits of UIC over Northwestern downtown, and the members present voted to make the change. I suggested that we simply put it to a vote, and that is what we did, at either the first or the second meeting of '67-8. That was the beginning of the process whereby a regional Chicago Linguistic Society became a part of the University of Chicago.

Bailey also complained about the fact that there were few opportunities to publish papers on generative grammar, and that the publishing process was far too slow for a field developing as fast as generative grammar. Papers at that time circulated in mimeographed or Xeroxed copies. In pre-Kinko days, Xeroxing was expensive. In the previous year Bailey had suggested that we publish an informal collection of linguists' papers from the U of C, and have it copied by University Microfilms and sold by individual order. We did that, we called it the *Chicago Journal of Linguistics*, and there were two issues.

With these two issues in hand, we wanted to go to the University to ask for aid to publish the proceedings of the upcoming regional meeting. Eric Hamp was at that time the Chairman of the Linguistics Department. For some reason, the students in the department were afraid of him. I was not in the linguistics department--I was in Slavic--and it had never occurred to me to be afraid of Eric (I at that time called him Mr. Hamp). I was designated to talk to him. He asked me to find out how much it would cost, which I did. He then took the issues of *The Chicago Journal of Linguistics* to the Dean of Humanities, Robert Streeter, and asked for some arrangement to finance the publication. The Dean gave Eric permission to use money from his (Eric's) personal research account, as long as it was paid back. We requested and got more-or-less camera-ready copies of the papers from CLS 4, and with minimum editing (with typewriters and white-out, remember) we got the volume out by the third week of June in 1968. We owe a debt of gratitude to Eric Hamp and Robert Streeter, and to the administrative flexibility of the University of Chicago.

This kind of publication, particularly with the rapid turnaround, was unknown at that time and CLS was off and running. CLS 5 was a huge conference, and produced one of the most influential volumes in CLS history.

Before returning to phonological theory, I would like to add one sociological anecdote from CLS 5. Anyone who was in linguistics in the sixties must remember that there was a real antagonism between American phonemicists and generative phonologists. Part of this may have derived from Chomsky's personality; part may have been from the general rebellious environment of the sixties. At any rate, conferences which included linguists from both camps could be embarrassingly nasty. It was common to have generative phonologists rework old data from phonemicists' articles and announce that theirs was the only way it should be done. Most of Chomsky's criticism of phonemics, for example, was based on data from articles by phonemicists who were well aware of those problems.

At CLS 5, Jim McCawley presented a generative reworking the treatment of Tübatulabal length in Swadesh and Voegelin's (1939) "A Problem in

Phonological Alternation." His first two sentences are illustrative: "This paper is largely an exercise in lily-gilding. It will be a restatement and revision of an analysis for which I have always felt nothing but awe." After the talk an elderly gentleman I did not know got up to comment. It was Carl Voegelin. I do not remember the exact words, but the substance of his comment was that when he and Morris Swadesh wrote the original analysis they did not have the theoretical apparatus to adequately describe the alternations, and that he considered Jim's analysis superior to theirs.

To return to theory: It is fair to say that generative phonology in the early days at CLS was primarily a reaction to the theory of *The Sound Pattern of English* (Chomsky and Halle 1968). The reports of the theory had begun to reach us in 1966, with Theodore Lightner's dissertation (1965) on the generative phonology of Russian. According to hearsay from people who were at MIT at the time, Lightner was the one who convinced Halle that extreme abstract underlying forms and rules which recapitulated history were justified by the criterion of economy. Lightner himself visited Chicago the next year and laid out his theory. As a Slavist, I was appalled. I was not alone. As I recall, by the time everyone had read SPE, no one that I knew at Chicago accepted it. Most of us still accepted the basic premises of generative phonology, and were willing to criticize SPE within that theory, but the publication of SPE marked the beginning of a widening gulf between Chicago and MIT in phonology.

The theory of SPE involved the belief in an economy criterion for phonology which was little more than counting features to compare grammars. Qualifications were made for as yet undiscovered universals which would limit the criterion, but almost everything odd in SPE could be traced to some device which saved features. Crucially, features in rules counted the same as features in the dictionary. This meant that any rule that could eliminate an underlying segment could save a very large number of features in the lexicon. No restrictions were made on the degree of phonological abstraction. It was allowable to divide the vocabulary into those which followed a set of rules and those that did not, so that a subset of the vocabulary would have fewer underlying segments. Phonological grammar was seen as a set of rules and underlying forms. Rules applied cyclically, and there were no restrictions on rule order. The phonemic level was specifically rejected, as was the claim for a difference between morphophonology and pure phonology. The issues raised in SPE have remained relevant throughout the history of CLS.

## **1. Natural Phonology**

One of the early alternatives to SPE phonology offered at CLS was the theory of Natural Phonology, developed by David Stampe. I played a role in this movement, and that is reflected in my biases, listed above.

A very important difference between Natural Phonology and SPE phonology was in the stated goals of phonological theory. Rather than trying to explain how people arrived at the most efficient formal grammar to account for language data, Natural Phonology focused on relating the facts of synchronic

phonological grammars to a whole range of external phonological phenomena-- acquisition, borrowing, historical change, casual speech, and language games.

Stampe started with the observation that all the processes that are observed in pure phonology in languages can be found in historical change, and these in return can be observed in the phonology of children acquiring a language, or in casual speech phenomena. This led him to the conclusion that there is a set of natural sound changes that apply in all these areas of phonology. These sound changes are presumably the result of the difficulties imposed on speech production by human physiology and neurology. In the spirit of SPE, which asserted that what is universal in language does not "count" against the economy criterion, Stampe came to a conclusion which would have been impossible for Halle and Chomsky--that natural processes, operating without constraint, do not have to be learned or acquired, and therefore to not count against the economy of the grammar. (We never really counted things, but the point was that these rules did not make the grammar more complex.)

Stampe argued that children learn pure phonology in a different way from morphophonology. They learn to articulate the surface forms, in the process overcoming the minimum number of constraints imposed on them by the physiology and neurology of speech, and the remaining processes which implement phonological alternations are simply left over, by default, without being learned. To use a concrete example, in a language like Russian that exhibits assimilation of voice within consonant clusters, children do not learn to assimilate. They never learn to produce mixed voice clusters. In this sense they have an easier time than speakers of some other language which does have mixed voice clusters. It therefore makes no sense to say that Russian phonology is more complex in this particular area, because it has a rule that the other language does not. Since Russian is easier to learn in this respect, its grammar should be simpler.

Morphophonology is a different matter. Alternations which do not correspond to leftover constraints of neutral speech, but which are implemented in neutral speech, must be learned conceptually. They therefore do make the grammar more complex, and count against the economy of the grammar in the way that SPE says.

There are obvious parallels in historical linguistics. The most common kind of historical change is sound change, which adds a rule to the grammar. This caused a problem for Kiparsky (1968b), who wanted to argue that historical change is most often a simplification. For Stampe sound change is a simplification of the phonology--a response to innate conditioning of speech production. On the other hand, all of Kiparsky's changes which did involve simplification--rule loss, rule generalization--applied with the morphophonology, where this is a simplification.

There are a very large number of articles in early CLS volumes which deal with Natural Phonology, including a special volume (*Natural Phonology*) devoted to the issues. Among the articles are:

In *Natural Phonology*, CLS 1974: Introduction, Dressler, Lee and Howard, Lovins, Rhodes  
CLS 5 (1969): Stampe  
CLS 7 (1971): Darden  
CLS 8 (1972): Miller, Rhodes, Stampe  
CLS 9 (1973): Miller  
CLS 11 (1975): Bjarkman  
CLS 14 (1978): Bjarkman  
CLS 18 (1982): Bjarkman  
CLS 19 (1983): Darden  
CLS 21 (1985): Darden

## 2. Unspecified Features

Early generative phonologists went to extreme lengths to eliminate features from underlying forms. Much of the effort was spent in eliminating segments from the underlying system. I have a distinct memory of Arnold Zwicky at CLS disavowing an analysis of umlaut in German which included a lexically conditioned rule which fronted vowels in non-alternating forms with no phonological environment. The sole purpose of this rule was to eliminate front rounded vowels from underlying forms. Lightner went to similar lengths to "predict" palatalization of consonants in Russian. He treated palatalization as the result a process which applied before underlying front vowels. Unfortunately there are stems in Russian which end in palatalized consonant, and which retain the palatalization before endings which even Lightner treated as having initial back vowels. To eliminate these palatalized consonants from underlying forms, he added a morphological rule which palatalized dentals and labials at the end of stems which belong to the third declension. He did not even mention the stems of this type which had switched out of the third declension, but still kept the palatalized consonant before back-vowel endings (cf. Darden 1971).

Today it would be a quaint idea to propose counting feature specifications in the lexicon, but the spirit of SPE lives on as a genuine belief that predictable features should be left out of underlying forms. The notion goes back to Trubetzkoy's archiphoneme. Trubetzkoy used archiphonemes to express the loss of distinctiveness of a otherwise distinctive feature in a position of neutralization. This was clearly a part of the surface phonemic representation, and was tied to the sign value of surface segments. Jakobson, who codified the idea of phonemes as sets of distinctive features with binary values, left features unspecified when the values of those features were redundant in the system as a whole, but elected not to use archiphonemes to express syntagmatic neutralization. His argument was that a /t/, if it is distinct from /d/ in the system, is still a /t/ when voice is neutralized in word-final position. I am unaware of any persuasive evidence that Jakobson was wrong; that is, of evidence that an archiphoneme in the strict Trubetzkoyan sense acts differently because it is an archiphoneme, as opposed to because of what it is phonetically in the environment where it occurs. There are,

however, cases where two or more segments neutralize as a phonetic segment which is not identifiable as any of the neutralized segments. In this case we may have to use an archiphoneme because we have no other reasonable choice.

Chomsky and Halle had a different level of lexical representation--the morpheme. They took the position that every feature value which could be predicted at that level should be left empty. They used both the syntagmatic redundancy of Trubetzkoy and the paradigmatic redundancy of Jakobson to fill in feature specifications. In this system, unspecified features never acted differently from specified feature values--i. e. zeros were never distinctive. For many modern phonologists that restriction no longer applies. I am viscerally suspicious of such practice, but in some cases it may be justified. For instance, in a vowel harmony system where the harmony is not purely phonological, but where either all the affixes or a coherent subset of the affixes are subject to harmony, we may legitimately say that we do not know what feature to fill in for the affix vowels before the harmony applies. Thus if a set of affixes consistently agree with the last vowel of the stem for the feature [+/- back], we have no apparent basis to choose either value of the feature for the underlying representation. We not only could, but may be required to leave it unspecified. It is important to note, however, that these unspecified vowels are not Trubetzkoyan archiphonemes--even if he says they are. These are slightly abstract morphophonemes. The unspecified value of the feature [back] signals that these vowels harmonize, while others do not. This use of underspecification is effectively used in Manuela Noske's (1995) dissertation from the U of C.

However, the use of underspecification to signify an alternating segment is potentially the first step on a slippery slope. I have read accounts where modern phonologists give Trubetzkoy credit for a use of archiphoneme that Morris Swadesh should be blamed for. Swadesh used a small capital F to symbolize the irregular voicing of the final constituent in the plural of nouns like *wife*. There is an elegant but to my mind awful way of representing this with unspecified features. One can spell the plural as: <waif-z> where F represents a labial continuant with no specification of voice. Since the underlying representation of the plural morpheme must be voiced (note *cores* /korz/ vs. *coarse* /kors/), the voice of the suffix can spread to the F, satisfying the constraint against mixed-voice clusters at the end of a word. This to me is awful--because it represents as phonologically regular what is clearly exceptional behavior. Plus in this case I would definitely want to say that it is the lexical item that is irregular, not the final segment of the word.

There is another type of argument for underspecification which is not based on any redundancy in the phonology of individual languages, but on predictability based on how phonological theory is presented--rightly or wrongly. Since Jakobson, phonological features have mostly been treated as having only two values--plus and minus. Given that way of presenting the facts, we can always elect to write down only one of those values, and have a redundancy rule change all the zeros to the other value. Used in this way, this theory has its roots the use of m's and u's in Chomsky and Halle's appendix to SPE. John Goldsmith

(1990:245), in presenting this theory, is properly careful in pointing out the difference between this kind of redundancy and others, and overtly ties this to language typology. He properly asserts that this entails an assumption that all features are what Trubetzkoy called privative oppositions, with one of the values marked, the other unmarked. I have no interest in doing this, but I have no objection, as long as the zeroes are filled in before any phonological rules apply.

The problem is that formal devices of this sort typically have a further history. People who value underspecification, seemingly for its own sake, can use this as a distinctive element in their phonology. There is an example of this in CLS 22 (Sohn 1986). In this system it is easily possible to select a single vowel and leave all its features blank, with only a V on the CV tier, and fill in the features later. Starting from Archangeli's (1984) suggestion that the most frequent inserted vowels could be treated as featureless syllabics, Sohn offers a treatment of Korean [i] as a featureless vowel. Korean [i] is often deleted by phonological rule, and is inserted to break up clusters in borrowed words. The result is an elegant formal representation of the process. However, I think that when we do this, we should ask ourselves what we are saying about the phonology of the language and phonology in general. We could argue that the most prototypical vowel should be featureless, but we cannot argue that the most prototypical vowel is the vowel which is most easily deleted, and the one which is inserted to break up unpronounceable clusters. That vowel is far more likely to be the most unvocalic of all vowels, the vowel which is perceptually closest to nothing at all.

### **3. Phonemics**

The rejection of phonemics in SPE was based a variety of arguments, the most important of which was Halle's (1959) "proof" of the impossibility of maintaining the phonemic level in generative phonology. At the Fourth Regional Meeting of CLS, but unfortunately not in CLS 4, David Stampe argued for the reality of the phoneme and phonemic level, based on how phonology must be learned, and on how rhyme works. David Johns' paper in CLS 5 (1969) showed that Halle's 'proof' is not relevant for Trubetzkoy's phonemics, and offered counter arguments for many of Chomsky's other arguments against the phoneme. Halle could still argue that Trubetzkoy's phonemic level was irrelevant for his theory.

Many people have argued that underlying forms are no more abstract than phonemics in non alternating forms (Stampe at CLS 4, Kiparsky (1968), Shane (1974) in CLS *Natural Phonology* . Darden (1971) offered historical evidence for the relevance of a change from phonetic to phonemic status, and more external evidence for phonemics in CLS 21 (1986). In that article I argued, I hope cogently, that Halle did prove that for his theory neither the phoneme nor the phonemic level were relevant. However, if one can show evidence that the phoneme and/or the phonemic level is important in language, then it is Halle's theory that is in trouble, not the phoneme.

Most modern theories have the output of one level which is close to phonemics, but may be more like SPE "systematic phonetics." If phonemics is

primarily a level of perception, theories that (metaphorically or not) start from morphemes and progress to phonetics may not pass through the precise phonemic level. We may need grammars of perception.

In OT, the constraints that limit segment inventory should produce the inventory of phonemes in the language

#### **4. Distinctions between Morphology and Phonology**

The postulation of the phonemic level more or less goes along with the recognition of a difference between phonology and morphophonology, although phonemicists have defined morphophonemics in different ways.(cf. Darden 1979 in CLS 15). The reverse, however, is not so obvious. One can fail to believe in the relevance of a phonemic level, but still posit a theoretical difference between pure phonology and rules or alternations governed in part by lexical and grammatical conditions. Halle's argument against phonemics was based on the fact that a single phonological process--voicing assimilation in obstruent clusters in Russian--implemented both changes of one phoneme into another and changes which determined the allophones of a single phoneme. The input was therefore more abstract than Jakobsonian phonemics, while the output was less abstract, including allophones. For Jakobson (and for me) the process is unambiguously purely phonological, with no grammatical or lexical conditioning, so this phenomenon was irrelevant to the issue of morphophonology.

Arguments for the lack of a distinction in rule type were generally based on claims that morphophonological rules could apply both before (the norm) and after purely phonological rules, preventing a separation of the two types in generative grammars of actual languages. Kiparsky (1968b) provided what seemed to be a spectacular example from a Swiss German dialect. Some Swiss German dialects at a time had a change whereby /o/ was lowered before nonlateral coronal consonants. This was apparently an allophonic alternation. Kiparsky argued that the rule that implemented this alternation had to apply before ablaut in the dialect of Kesswil. Not only was this an instance of an allophonic rule applying before a clearly morphologically conditioned rule, but this arrangement produced a new phonetic entity, a low front rounded vowel. This dialect is offered by Hock (1986: 154) as an example of a new phone produced by analogy.

Judith Moses (1982), in CLS 18, argued convincingly that: (1). The change was not reordering, but restructuring, with the low rounded vowel put in the underlying form, while a new /o/ was introduced before coronals, and (2) A low front rounded vowel occurs in this dialect from a sound change, so the phonetic vowel was not new.

All the practitioners of Natural Phonology, which played a large role in the history of CLS, either explicitly argue for or assume a distinction between morphophonological and phonological rules. Most theories have developed something like this distinction. It may called cyclical versus post cyclical rules, lexical versus non lexical rules, morpheme level rules versus word level rules.

There is less agreement as to the general nature of the morphophonological rules, whatever they are called. Lexical Phonology (Kirparsky 1983) treats them as a subset of phonological rules, even to the point of using them as justification for abstract phonological segments. At the other end of the spectrum Natural Generative Phonology (Hooper 1979) treats them as having no phonological content whatsoever, except as (phonologically) spelled alternants. From my own work (Darden 1979, 1985, 1989) I would say that changes in morphophonological rules are governed by principles of historical morphology, such as reduction of allomorphy, increase in transparency of the relationship between alternants, and sharpening of morphological function. However, phonological class matters. The substance on which these changes operate is the phonemic system, or the system of distinctive features in the surface. This means that it is proper to describe the rules as changes in phonological features. The question as to whether we ever have to mention anything less abstract than phonemics or distinctive features is open. Several people, including Hooper (1974) have argued that changes in superficially phonological rules can be determined by morphological principles, with the result being a morphophonological rule. This amounts to allowing analogy to operate on allophones. The rule, as it results, however, does not refer to allophones.

I still like what I said in 1989, which is that phonology makes words pronounceable, while morphophonology makes them grammatical. However, that does not mean that a single rule might not do a little of both. As I have gotten older, I have become less sure that there is a crystal clear distinction between phonology and morphophonology. At least I have trouble making that distinction in specific cases. This is particularly true of boundary phenomena, which I will discuss next.

## **5. Boundaries**

Phonologists have always been uncomfortable with boundaries in pure phonology. Boundaries are obviously morphological, but they seem often to affect segments in a phonetic way. For instance, without boundaries, we would have to treat aspiration of English stops as distinctive. There have been some modern attempts to treat this as syllable initial rather than word initial aspiration, but that just moves the problem to a higher level. We still need the word boundaries to account for the syllable boundaries. Phonemicists very early on decided that they needed at least word boundaries for their systems to express what they wanted to express.

Nevertheless, there have been reasoned attempts to claim that any alternation governed by a boundary is morphophonemic. This is true of Natural Generative Phonology. One can claim that children learn words in isolation, where word boundaries are phonological pause, and then transfer the pre- or post-pausal phenomena to connected speech by a kind of analogy. This works for things like devoicing at boundaries, but does not work so well for phenomena at internal boundaries, or external word sandhi, where word boundaries seem to be positive environments for strong assimilation in connected speech. This is

exhibited in external sandhi in Sanskrit, Krakow Polish, and Slovak. In these languages, voiceless obstruents in word-final position are voiced before initial sonorants in the following word, while voiceless obstruent plus sonorant is allowed in word internal position. Since this is a phenomenon of connected speech, it is hard to argue for an analogical development from words in isolation.

There are two attempts to handle these phenomena in the CLS Natural Phonology volume. Venneman (1974) tries to explain external word sandhi. Working within the theory of Natural Generative Phonology, he argues that words are stored in their grammar in a phonetic, rather than phonemic form. He suggests that the word-final obstruents are phonetically weakened before pause, and that children learning the language transfer these weakened obstruents into connected speech. These phonetically weak consonants are more susceptible to assimilation of voice. In CLS 12 (Darden 1976), I used a Russian casual speech example to argue that Vennemann was wrong. If we trust the descriptions of casual speech, Russian shows stronger assimilation across word boundaries in clusters that are created by elision of word-final schwa than in similarly created clusters within a word. Since the elision is a phenomenon of connected speech, it cannot be due to pronunciation in isolation. In addition, Polish and Slovak proclitics were subject to the strong assimilation over a broader area than where we find assimilation across word boundaries, and proclitics are not learned in isolation. Unfortunately, even though I think Vennemann is wrong, I have no explanation for the phenomenon. I have thought of using forced syllable boundaries at external and internal word boundaries, even before vowels and sonorants in Slavic, but that doesn't quite work either. Slovak and Polish have voicing of a proclitic that, by the time it came in contact with a sonorant in the following word, consisted of the single consonant *s*-. It could not have been in syllable-final position at any time in its history. The sandhi may be historically connected to weakening of syllable-final obstruents, but it must have been generalized as a rule triggered by a boundary.

Rhodes (1974) tried to account for the acquisition of internal boundaries which are stronger than simple morpheme boundaries--such as the boundaries between prefixes and stems, and in suffixes with apparent stronger boundaries, such as the German suffix *-chen*.. He has essentially an analogical explanation, whereby people learn the unaffixed forms, and transfer the boundary to the prefixed forms, but in the synchronic system he has an argument that boundaries are 'administered' transderivationally.

Level ordered morphology and lexical phonology tried to eliminate boundaries using 'edge' and the derived environment condition on lexical rules. This obviously works only for cases where a boundary blocks a process.

Harmonic phonology and OT seem to have resurrected word boundaries in phonology. Noske's harmonic treatment of North German *x/ç* alternations in CLS 26 is essentially identical in its use of boundaries to that of Rhodes in *Natural Phonology*, although she is not concerned with how such a situation develops.

Boundaries will remain a theoretical problem. There are phenomena, such as *r*-insertion at boundaries in English, which do not seem to be explainable in

purely phonological terms, but which act in every way like phonological rules. Can the brain (unconsciously) organize articulation in units of words, and if so, is there any 'pure' phonology?

## **6. Abstractness**

Abstract segments in phonology have been a recurrent issue at CLS, thanks largely to the stubbornness of Bill Darden (1976, 1979, 1981, 1985). Apparent cases for abstract segments are, to my knowledge, always based on a situation where a historical change has eliminated a segment, but traces of that segment are left in the alternations of the language. Either two segments have merged, and a single segment has different alternational behavior in different morphemes, or a single segment has changed (e.g. a sonorant has become an obstruent) and this segment has behavior in all relevant environments that would make more sense if it still had the phonetic characteristics of its earlier incarnation. The argument is that in the first case, the description of the two different patterns of alternation is made more economical when two segments are posited for underlying forms, and in both cases the alternations become more 'natural' with a different set of features in the underlying segment.

My objection to the first type of case is that it postulates an unwarranted extra step in the acquisition of the language. In order to posit the abstract segment children must learn the different alternation patterns. If they have learned the alternation patterns, they do not need the abstract segment.

David Stampe in an (unpublished) talk at CLS 4, "Yes Virginia ...," argued that children's initial forms could not be more abstract than the phonemic level, and that to justify a more abstract representation of a morpheme, there had to be phonological alternations of that morpheme. This eliminates the possibility of absolute neutralization, which is necessary for abstract segments. Kiparsky (1968) in "How Abstract is Phonology" said something similar in same year, but by the time he qualified it--particularly for lexical phonology--he allowed the same abstract segments as SPE. (cf. Kiparsky 1983).

The CLS Natural Phonology Volume (1974) has several papers (Hooper, Hudson, Vennemann) arguing for extreme concreteness of basic representation. Shane, in that volume, has an intelligent survey of the issues, with a concrete solution to the English vowel shift. Darden (1976, 1979) criticized the extreme concreteness of Natural Generative Phonology. Hudson (1982) has a reply in CLS 18.

The issue will not go away. We might all agree that recapitulating the great English vowel shift in Modern English phonology is ridiculous, as well as unenlightening, but there are other languages for which the use of abstract segments is less obviously wrong (e.g. Yawalmani). Morphological conditioning is always less elegant looking than phonological conditioning, as long as abstract segments are an acceptable theoretical device, some linguists will opt to use them.

## **7. Rule Order**

Rule ordering was a very important topic in 70's. Kiparsky (1968) defined feeding, counter feeding, bleeding, counter bleeding orders. His first suggestion was that unmarked order was based on maximum application, so feeding, counterbleeding were unmarked. He described rule loss and rule reordering as types of language change. He further argued that all changes in order were from marked to unmarked order. By Kiparsky (1971) he had changed his mind and opted for transparency and elimination of allomorphy over maximum application as motivation for changes in rule order. The unmarked orders in this schema were feeding and bleeding. The general conclusion was that both marked and unmarked orders were possible, and grammars had to specify that order.

Anderson (1974) proposed local ordering, by which he meant different orders in different derivations of same language. Kaise (1976) in CLS 12 offered examples of local ordering in Greek dialects. Bley-Vronman in *The Best of CLS* (1988) presents evidence that in the interaction of umlaut and syncope in Icelandic--an example used by Anderson, there must be two applications of umlaut, rather than two possible orders of syncope and umlaut in different paradigms. Darden (1978) in CLS 14 argued that all of Anderson's examples, analyzed correctly, were subject to an interpretation with reapplication, rather than reordering.

Kenstowicz and Kisseberth (1970), in CLS 6 and in *The Best of CLS*, argued that there are relations between rules which cannot be handled with rule order. They described situations in which only the output of a rule is subject to another rule, and even more complex relationships. Since this kind of complex relationship entails that not all potential inputs to a rule undergo the rule, the rules in question must be morphophonological. Darden (1977) in CLS 13 has another example--most definitely from **morphophonology**. This kind of relationship is rare, and any historical linguist would expect. Extrinsic rule order is the expected result of relative chronology. Any other systemic relationship requires a more complex history. There is certainly no reason to think that speakers are not capable of learning such arrangements, particularly since this is part of morphophonology. What interests me is that if relationships other than rule order can be and are learned by speakers, then we have an alternative to abstract segments. If in Yawalmani a /ō/ which ablauts with /u/ triggers high-vowel harmony, while a /ō/ which ablauts with /o/ triggers low-vowel harmony, then we can simply say that there is a learned relationship between the rules. (cf. Darden 1976, 1981).

Even for pure phonology, the problem of rule order remains unsolved. Optimality Theory is just beginning to deal with the problem. For OT anything but transparent order makes constraints without rules impossible, and to my knowledge they have not gone though all the old evidence for nontransparent order and said how they would handle it. For my own theory it is counterfeeding order that causes problems for pure phonology. Counterfeeding creates surface exceptions and is expected to be either unstable or impossible as purely phonological rule. Neither bleeding nor counterbleeding creates exceptions, although counter bleeding order often creates new phonemes. Neither seems to

be diachronically unstable. For my article in CLS 21, I did try to find all putative examples of counterfeeding order. I found only one such case that I would describe as purely phonological<sup>1</sup>.

## **8. Constraints and/or Rules**

With the success of Optimality Theory, constraints have come to play a dominant role in phonology. The idea is of course much older. One could even look at descriptive phonemic as a constraint-based (rather than process-based) phonology. Natural Generative Phonology treated surface phonotactics and syllable structure as a constraint. I would certainly agree that treating the surface phonotactics of neutral speech as a target or filter for speech processes is a good idea. I would prefer positive constraints, rather than negative ones, but that may be a matter of taste. A constraint-based phonology fits nicely into a theory of acquisition. Children have to filter adult speech through the system that they have learned to produce as they gradually overcome the constraints placed on them by the limits of neurology, perception, and maturation.

I have seen a lot of theories come and go. Proponents of a new theory emphasize what their new devices can do that previous theories could not, but they rarely go through the entire range of phenomena of language and compare their theory to older theories. I think that OT may be guilty of overstating the importance of constraints.

I find it very strange to claim that constraints can replace rules, as opposed to providing a target for those rules. I have never been a fan of economy for economy's sake, and this I think is a false economy. Speakers have to know precisely which processes are used to meet constraints. One of the strong arguments for constraints was the occurrence of "conspiracies," where more than one rule operates independently to create a surface configuration. In a case like that speakers must not only know which process to apply, but the conditions under which each of the processes operates. Overall, I think that Harmonic Phonology, as developed by John Goldsmith, better fits the facts of pure phonology. It has the proper combination of surface constraints with rules.

Constraints based on neutral speech phonotactics make good sense, precisely because there is a target structure which can serve as a filter. Such a system does not fit so well for casual speech, where there is no stable surface structure. In casual speech there is a tension between fidelity to the neutral speech form (not the underlying form) and universal tendencies imposed by the conditions of speech production. This seems to be basically the same phenomenon, but it is less easily viewed as a constraint.

Morphophonology poses a particularly difficult problem for OT, since by definition morphophonological rules have phonological "exceptions." They do not function to meet surface constraints. It might seem that we could produce a surface description of the distribution of alternants created in morphophonology-- simply by overtly recognizing the morphological component of their

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<sup>1</sup> John Goldsmith offered another candidate at the conference.

conditioning. This was basically what item-and-arrangement morphology did. The target structure would then be the input to the pure phonology. However, we would run into the same problems as we did with pure phonology. Nontransparent rule order would make a single surface filter impossible, although we might work around rule order by making the distributional constraints more complex. What we could not avoid is the problem of matching alternants to each other. We cannot simply say that /k/ is not allowed before -ity in English, nor can we simply list the consonants which are allowed (excluding /k/). We need to specify that morphemes that end in /k/ in some environments have /s/ in that position before -ity. For morphophonology, we cannot even appeal to the naturalness of the substitution. In formal terms Goldsmith's Harmonic Phonology handles this set of rule rather well, again using both rules and target structures, but metaphorically, he treats this as a subset of phonology.

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