

In and Out in Phonology*

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1. **Introduction.** I don't know who the first person was to use the words *internal* and *external* in discussing argumentation and evidence in linguistics, but whoever it was had an axe to grind: the very choice of words shows a bias in favor of the internal ('inside'). In fact, the history of discussions about these matters is full of invidious comparisons built not only into the names *internal* and *external* but also into various alternative names (for example, Skousen's 1975 proposal to call external evidence *substantive evidence* and thereby to elevate it). Here I will ask three questions. First, where does the issue of internal vs. external evidence arise in doing linguistics? Why is it in fact a problem for linguists, particularly phonologists? I will then spend a fair amount of space discussing what counts as internal and what as external, a question sloughed off by the other contributors with brief references. Finally, what is to be done? I conclude with a few prescriptions for phonologists.

2. **Why is it an issue?** I can see at least four places in linguistics where the issue of internal vs. external considerations comes up. To begin with, it arises in the very delimitation of the tasks of linguistics, or particular subfields within linguistics, that is, in the division of labor between linguistics and other fields and in the division of labor among the various subfields of linguistics. You might wonder why there should be such a division of labor. There is, of course, the truly practical reason that everybody can't do everything. But there is a much deeper reason, which follows from the hypothesis that *genuinely different* sorts of accounts are going to be required for the various fields and subfields.

If you have a particular view about the way in which fields of inquiry should be cut up, you have already made certain (a priori) decisions about what's 'in' and what's 'out' of linguistics or your part of it. If you believe that linguistics *is* sociolinguistics or that linguistics *is* psycholinguistics, you will as a result have made decisions about what kinds of evidence will without question be relevant to doing linguistic analysis. If you believe that what is sometimes called 'phonology' is really two separate subfields of linguistics--let's call them *allophonics* and *morphophonetics*--and your interest is in the former, then you will label certain data as outside the domain of your inquiry from the very beginning of the enterprise.

Even, however, within essentially the same delimitation of the task for linguistics in general and phonology in particular, questions of what's internal and what's external arise when we're choosing among alternative sets of theoretical assumptions. In (the very frequent) situations where phenomena from some other field are brought to bear on the choice of theoretical framework for linguistics, we need, as Wolfgang Dressler pointed out quite cogently in his contribution,

linking assumptions (what Rudolf Botha calls 'bridge hypotheses') that will connect linguistics and the other field. If, for example, you are concerned about the existence of the phoneme as an entity that requires an account in a phonological description of language and you would like to bring evidence from slips of the tongue to bear on this issue, you are obliged to give some account of what connects slips of the tongue and the abstract analytic object, the phoneme, and to show moreover that slips of the tongue are in some way relevant to the analytic issue. Similar obligations ensue if you are choosing a specific feature system and you propose to appeal to articulatory, acoustic, or auditory phonetics; you are obliged to supply the assumptions that connect the various subfields of phonetics with the (again abstract) analytic framework being proposed for phonology.

A third place in which the internal/external distinction appears is in choosing, within a given theoretical framework, among alternative accounts of essentially the same facts. What I just said about choosing among theories goes in spades for choosing alternative accounts. In this case you need not only the linking assumptions and a way to determine relevance, but you also need to ascertain that the facts in question are relevant to these particular analytic choices—not just to phonology in general, but to the particular problem at hand.

At this point, I'll discuss briefly, and without giving any actual linguistic forms, a problem in the analysis of Sanskrit that has exercised me for what seems like an eternity now, namely what's sometimes called the *ruki rule*. I'm going to have to call it the ruki *phenomenon*, because what's at issue is whether we are dealing with one, two, three, four, sixteen, or whatever separate cases, each requiring its own independent description in a phonology of classical Sanskrit. The ruki phenomenon involves the appearance of a retroflex /ʂ/ rather than a dental /s/ in the environment after the segments /r u k i/. Now, most phonologists hearing this for the first time are somewhat taken aback by it. It would not have occurred to most people that these segments might constitute a natural class. The literature includes quite a number of analyses which assume that these four segments¹ do constitute a natural class, as well as a number of challenges to this assumption, these giving rise to various suggested reanalyses. For example, the proposal of Allen (1954) is essentially that there are three separate rules, causing retroflexion after /r/, palatalization after /i/, and velarization after /k u/, with all three types of affected /s/ realized as /ʂ/. Vennemann ms. (1972) groups back /k u/ with palatalizing /i/ as retracting segments, versus retroflexing /r/. David Stampe has suggested to me grouping /k u r/ together as retracting segments—assuming that retroflex segments in general are classed as back—versus front /i/. And one of my proposals (Zwicky 1970) grouped the sonorants /r u i/ against the obstruent /k/. The unity of the ruki class has been defended in turn by several writers: for instance, Vennemann himself opts for a natural class position on ruki (maintaining that the four segments are united by their acoustic effect of 'lowering the frequencies of the energy concentration in a following s'), while Sommerstein (1973:53f.) argues that they are just

the [+high] segments (/s/ being distinguished from /s/ as [+high] and [-ant].) The main reason given for preferring one of these proposals over the others has to do--this should be clear from the sampling of ideas I've just given--with the *phonetic motivation* for the shift of dental /s/ to retroflex /ʂ/. In order to make sense out of this reference to phonetic motivation, we have to supply quite a few intermediate assumptions that link the content of phonological rules to particular phonetic properties--the sorts of links that John Ohala and others have tried to supply over the years. I'm not going to work through the details here, but I should point out some of the more obvious problems with the proposals I've mentioned, such as Stampe's treating postalveolar /r/ as back but palatal /i/ as front, the widespread assumption that the nonlow vowels are retracting in effect (this despite the facts that *all* vowels are articulated back of /s/ and that /a/ is further back than /i/), and the contradiction between Vennemann's lowering proposal and Sommerstein's raising analysis. The crosslinguistic survey in Bhat (1973) deepens the mystery; it identifies, as factors promoting retroflexion, (a) a preceding r, whether retroflex or not, (b) a following retroflex consonant, (c) a following back vowel, (d) implosion, (e) word-initial position, and (f) neighboring velars or palatals.

I entered this arena in (1970) with an observation, not about the phonetic motivation of the rule or rules involved in Sanskrit, but rather about their susceptibility to exceptions. I repeated some well known facts about the ruki phenomenon: There are no exceptions whatsoever involving /k/ (that is, there are simply no attested instances of dental /s/ after /k/); there are exceptions (relatively few) in the case of /r/, and a rather substantial number in the case of /u/ and /i/. Thus there is a difference in the degree of exceptionality of /k/ versus /r u i/. I assumed that, *ceteris paribus*, you would expect the exceptions to a rule to be evenly distributed across the various environments in which the rule applies. Since this is not the situation for the ruki phenomenon, I suggested that there were actually two rules, as described above.

Now I supplied no linking assumptions that would connect the distribution of exceptionality to the desirability of subsuming some phenomena in a single rule or describing it in more than one, and I can't entirely recover the steps in my reasoning, but it seems necessary to reconstruct an intermediate step using the linking assumption that rules describing a single phenomenon must correspond to some unitary mental representation. Such an assumption, which would make the various subparts of a rule not open to separate learning or variation, would provide a way of getting from exceptionality facts to a decision about the one-rule/several-rules character of a phenomenon. But now that I've stated that linking assumption, it's not at all clear that any of us would want to subscribe to it. Still, something like it is needed if the facts alluded to are to bear as evidence on the analytic issue at hand.

A fourth way in which the internal/external difference appears in linguistics is *post facto*--in attempts to explain why some description (or even whole theory) takes the form that it does. Many appeals to ease of articulation, to avoidance of ambiguity, or in general to the

large class of what have come to be called 'functional explanations' in linguistics are a posteriori (Kaye 1978): They do not claim to predict that the phenomenon in question must have occurred, but instead they claim to provide some sort of after-the-fact insight into why language should be the way it is. A rich collection of 'external' explanations has been provided (and challenged) in this area; see, for instance, many of the papers in Grossman, San, and Vance (1975).

Next, a few words about why linguists are so exercised about these matters, especially why phonologists are (it is almost impossible to look at work in phonology without an issue of external validation of some sort appearing). I would like to say that the problem in phonology is that there are simply *so many* alternatives available within almost any existing theoretical framework, and consequently that it's very hard to believe that the sort of evidence usually classed as internal could decide among the alternatives.

The sort of evidence that is without question internal deals with *alternation* and *co-occurrence* in the forms of linguistic units--the alternative forms of morphemes, words, and possibly phrases (in the case of phonology), as well as the restrictions on the occurrence of phonological units, within morphemes, words, and phrases, with respect to one another. Almost always we are dealing with what are in fact a finite number of items (granted, a rather large finite number), distributed within domains that are also finite, so that in most cases that have been considered in the phonological literature it would actually be possible to list all of the alternative forms, and list all of the relevant domains, and stop there. Such an analysis wouldn't be in any sense explanatory, but it is at least conceivable.

What I'm suggesting, then, is that there comes a point--and it may come fairly soon if you're hard-working--at which you've essentially exhausted all available evidence of the alternation and co-occurrence types. All the relevant facts are probably in hand, but your descriptive framework still provides a very large number of alternative descriptions. That is, I see the need for so-called external evidence in phonology as one arising from a real crisis in analysis.

Halle (1978) has maintained that if you look hard enough, then simplicity in the special technical sense of generative grammar will in fact decide such issues for you. In this paper Halle discusses issues in the description of three languages (Maori, Turkish, and Finnish) and claims that analyses which had been argued for on so-called functional grounds in fact would be supported by technical simplicity arguments alone, and consequently that the functional considerations were irrelevant for the purpose of deciding among alternative analyses. Halle here introduces a consideration that I haven't yet officially discussed, namely, some brand of simplicity. Simplicity considerations are *not* raw facts about the phonology of a language; we're not talking about the number of ways in which a particular morpheme can be pronounced or what the privileges of occurrence of some phonological unit are with respect to its neighbors.

3. **What's in and what's out?** Halle's claim leads me to a discussion of what will count as internal or external, beyond facts of alternation and co-occurrence. Before leaving these latter sorts of facts, I should say that I'm not concerned here with the issue of how you get facts about alternation and co-occurrence--whether they are obtained by observation, by introspection, by experimentation, or by elicitation, and what particular methods, within those four general types, are employed. I'm assuming that these facts are available to us, however they may be discovered, though I do point out that such 'facts' are in fact actually already low-level generalizations of some sort.

I will also sidestep serious discussion of two cases, productivity and variation, which seem to be problematic for anyone examining internal/external evidence. It is very hard to determine whether the productivity of some generalization should count as internal evidence--it is often used without comment, but you might reasonably feel that such considerations go beyond records of actual linguistic behavior and examine *potential* linguistic behavior, thus manipulating subjects in a quasi-experimental way. In the case of variation, other than that within the speech of a single person, the problem is the familiar one of whether it is an individual linguistic system that is being described, or a system shared by a social group. I'm going to have to assume that some sort of pronouncement by fiat has been made in these two cases.

Let me begin with *systemic considerations* that can play a role in evaluating alternative analyses. Here I would like to take seriously the idea (enunciated by Chomsky on many occasions) that a linguist's grammar is a *theory* of a language or part of a language. In fact, I'm going to borrow from discussions in the philosophy of science a list of considerations which are often used in choosing among alternative theories within some scientific field; I will treat them as considerations which can be used in choosing among alternative descriptions of some aspect of a language (this discussion will exhibit a general indirect influence of Botha's work).

The first of these I include for completeness: the degree to which a theory, or in our case a description of some aspect of a language, is *explicit*. If it's not sufficiently explicit it will be disfavored with respect to alternatives, other things being equal.

A very important consideration has to do with the *fit* between some proposed description and existing descriptions, or between the proposed description and the remainder of the theory within which the description is embedded. This particular consideration is almost invariably considered to be internal, and it's quite frequently used. You will find discussions in phonology, for example, of some analysis with the observation that it would require extrinsic ordering but that the theoretical framework within which the writer is working disallows extrinsic ordering of phonological rules, so that this analysis is disfavored with respect to alternatives consistent with universally determined rule application. That's a reference to a fit of a particular proposal with the surrounding theory. Similar remarks apply for analyses that require cyclic application of phonological rules. As it happens, both extrinsic ordering and cyclic rule application are

relevant in the case of the ruki phenomenon. My earliest analysis (Zwicky 1965) required both features. As a result, it is favored or disfavored depending on its fit with quite general assumptions concerning the nature of phonology.

The next two considerations make a tight pair. The first has to do with the *completeness* or *exhaustiveness* or *comprehensive-ness* that we require of an account, and the second with *the degree to which it's confirmed* by evidence within the language (including the extent to which there are 'independent' lines of evidence). The idea here is that we favor the descriptions which cover all of the potentially relevant facts (or at least as many as possible) and we favor those which have lots of confirming evidence within the language and relatively little disconfirming evidence. I don't have much now to say about such considerations, though I must mention a horrible problem in this area, namely that of distinguishing counterexamples from true linguistic exceptions. This is the type of problem that does not arise for physicists in the corresponding situation; for them, there are counterexamples and that's it. As you all know, languages have real exceptions, plenty of them in fact. But when we are talking about what confirms or disconfirms a description or theoretical proposal we somehow have to distinguish the plain old linguistic exceptions from true anomalies.

Next we come to *simplicity* in a nontechnical sense (the sense in which simplicity is treated in the philosophy of science, not necessarily the sense of simplicity to which Halle alluded, though they are not unrelated). I refer here to considerations having to do with the number of primitive concepts or terms, with the number of hypotheses, with the relative internal complexity of hypotheses, and so on. It's almost impossible to find an argument in phonology that can be unpacked without some reference to simplicity in a nontechnical sense, and the importance of simplicity in this sense has nothing to do with whether or not an analyst subscribes to something like the Chomskyan evaluation metric. Consider the ruki phenomenon. What, after all, would be wrong with saying there were four different rules? Why is one better than four or two or three or sixteen? Unpacking this *why* leads you to a set of assumptions about the desirability of relatively simple accounts. What would be wrong with saying that the rule(s) take /s/ as basic and derive /r/ from it? After all, the set of non-ruki environments is small in comparison to the set of ruki environments (see footnote 1). However, the relevant Sanskrit forms fall into two main classes—those showing /s/ in non-ruki environments and /r/ in ruki environments, and those showing /r/ in all environments—so that if /s/ is taken as basic for the first class of forms, some way must be found to distinguish alternating forms with putative basic /s/ from nonalternating forms (which always have /r/), for the reverse-ruki rule must apply only to the former set. The upshot is that the basic-/r/ analysis will necessarily be more complex (in one of a number of different possible ways) than the basic-/s/ analysis.

Now I must observe that as I continue through this list of considerations relevant to choosing linguistic analyses, it becomes less and less clear that we are dealing with matters that are in fact internal to linguistics, or to phonology. Appeals to simplicity could

be claimed to be appeals to particular metatheories of linguistics, to those favoring certain kinds of descriptions and theories over others. And it could be claimed that metatheoretical preferences are not matters internal to linguistics (much less phonology), but rather are principles of philosophy, consequently outside the domain of linguistics itself.

When we come to the next consideration, (Popperian) *falsifiability*, doubts rise still further. The proposal here is that the description or the theory that makes the strongest possible claims is to be preferred: the one that makes the largest number of predictions, of the most varied sorts, and that countenances the narrowest range of facts. Here the feeling that we are appealing to a metatheoretical principle outside the domain of linguistics becomes even stronger.

When we reach the consideration of *plausibility*, we are explicitly referring to principles outside our domain. What is at issue here is the plausibility of a description (or whole theory) with respect to some other theory, whether the latter is actually formulated explicitly or not. In linguistics such considerations appear as references to phonetic plausibility (to articulatory mechanisms or to perception), to the learnability of some proposed system, or to the facts of historical change, for instance. We ask questions like the following. Is this description plausible with respect to historical change? Could it have arisen by known forms of historical change? Would future historical development shed some light on the nature of the present system? Is the particular description plausible with respect to what we know about how poetic forms work in the language in question? Or with respect to how language play operates there? These are matters that you have heard discussed in some detail by earlier contributors.

They are also situations in which we are desperately in need of linking assumptions. The most extensive discussion of these questions that I know of (focussing on historical change, language play, and acquisition) is Churma (1979) (see also the useful brief surveys in Sommerstein 1977:sec. 9.2.2). Churma's work brings us little cheer, for one of its lessons is that it is very hard to supply linking assumptions that both (a) are plausible on their own grounds as assumptions about some domain other than language structure, and also (b) do the work of linking linguistics with the other domain. The linking assumptions have to be sufficiently strong actually to bridge the two domains, but they also have to be independently credible.

In the case of acquisition, for example, the first linking assumption you are tempted to make is that children hit on mental representations for various aspects of their language and then don't give them up. That particular linking assumption would permit many familiar arguments connecting the facts of acquisition with adult language structure to go through, but the linking assumption is quite incredible, in my opinion. Trying to refine the assumption in such a way as to make it believable--while still connecting the facts of language acquisition to the nature of the adult system--is very difficult.

I should point out that plausibility considerations play a role not only with respect to linguistics and the allied disciplines, but also in treating some subarea of linguistics as against others. Suppose, for example, we're working within a theory in which allophonics and morphophonemics are distinguished and we're talking about morphophonemics. Then questions of phonetic motivation are at several removes from our domain of inquiry, being directly relevant to allophonics rather than morphophonemics. As far as morphophonemics is concerned, phonetic plausibility is an external consideration, and we need the same kinds of linking assumptions that we need if we are going to connect phonology to, say, aphasia studies.

As it happens, these last observations are germane to the ruki problem. There is every reason to think that the ruki phenomenon is morphophonemic. To begin with, the alternations in form involve two segments that are clearly different phonemes in Sanskrit (they are in contrast in non-ruki environments). Next, Kiparsky (1973:61-3) and O'Bryan (1974) have observed that the alternation between /s/ and /ṣ/ is exceptionless (subject to a general proviso I'll mention in a moment) when the /s/ is suffix-initial;² the many lexical exceptions all involve the failure of retroflexion within a morpheme. The retroflexion(s) will then make crucial reference to morpheme boundary (in Kiparsky's treatment, to the derived character of forms) and would as a result be classified as morphophonemic. It also appears that /s/ and /ṣ/ within morphemes are not distributed by rule but simply supplied as part of lexical entries.

O'Bryan notes further that all remaining apparent exceptions to retroflexion are forms in which a retroflex continuant, /ṣ/ or /r/, follows /s/ within the same word. There are then *no* lexically marked exceptions, and my (1970) argumentation is quite beside the point (even if a credible linking assumption could be pressed into service).

Bear with me for one more chapter in the ruki story, for not all the mysteries have been solved. O'Bryan concludes, correctly, that her arguments permit the ruki phenomenon to be described by a single rule. But they don't require it to be so described. What licenses the description of morphophonemic alternations with one rule, two, or sixteen? Phonetic plausibility--which figures so prominently in the literature I cited earlier largely *because* it is problematic--will have little to say in the matter. There is the fact that /r u k i/ are equally exceptionless in retroflexing morpheme-final /s/, but that fact is not of much moment: we don't expect many instances of lexical exceptionality in conditioning (as opposed to undergoing) morphophonemic rules, so that the exceptionlessness of every Sanskrit morpheme in the environment of the rule is not surprising (there *are* cases of exceptionality in conditioning--see Coats (1970) and the survey discussion in Kenstowicz and Kisseberth (1979: 394-400)--but they are not common).

I can imagine facts that would bear on the question, but I don't believe they are available in Sanskrit to point one way or the other. Consider the suffixes affected by retroflexion, and recall that we are dealing with a morphophonemic rule. Suppose that Robinson (1975), Skousen, and the Natural Generative Phonologists (among many others) are

correct in believing that morphologized rules are ordinarily learned in connection with specific morphological categories, and not as phonologically conditioned operations; in this case, there would be a set of rules, each referring to a specific affected /s/-initial morpheme. If different /s/-initial morphemes were subject to different morphological conditions on retroflexion, we could ask whether *each* of these distinct morphophonemic rules applied identically after the four segments /r u k i/, or whether there were differences between one rule and another with respect to the effect of these four segments; a simplicity argument could be constructed for or against the unity of the ruki phenomenon in these circumstances. But, alas, the retroflexion has no conditions of the appropriate type. It seems irritatingly free of morphological conditions as well as lexical exceptions.

One final shot, harking back to my (1970) squib. The retroflexion of /s/ after /k/ is 'surface true', in the sense of NGP: There are simply no /ks/ sequences within words in classical Sanskrit, while there are /rs us is/ sequences. In a desert of usable facts, this remains--/k/ is different from /r u i/ in its combinability with /s/, while there seem to be no significant morphological or phonological similarities among the four segments. I view this as a standoff, perhaps an everlasting one, given the unavailability of true native speakers for the collection of possibly relevant external evidence.

4. **What is to be done?** Now to turn to some prescriptions and warnings. My first two prescriptions have to do with plausibility considerations that are 'external' but within linguistics.

The first is the prescription not to assume that phonology is parallel to syntax. We have no right to assume that the principles of argumentation that are appropriate in syntax necessarily carry over to phonology, that decisions about theoretical matters such as rule ordering in one domain carry over to the other, or that the formalisms appropriate for one domain are appropriate for the other. Let me say a few words about each of these points in turn.

In Zwicky (1973) I pointed out that there is an asymmetry in argumentation between phonology and syntax. If you have shown, for example, that certain classes of English subjectless imperative sentences ought to be analyzed with underlying structures having a second-person subject, you're entitled to infer that other imperative sentences also have such underlying structures, even though the particular sentence you're looking at might not have the kind of evidence that led you to this analytic decision in other cases. In phonology, as a rule, you can't do that. You're not licensed to move from a demonstration that a particular instance of the diphthong /ai/ has some underlying representation (let's say /ī/) to the position that all instances of the diphthong /ai/ have this underlying representation. The difference between syntax and phonology in this case has to do, I claimed, with a difference in the nature of the domains, syntax being infinite in the appropriate sense, phonology finite. In any case, I think it's fairly easy to see that some of the arguments you can make in one area do not carry over into the other.

With respect to the rule ordering issue, a case against language-particular ordering conditions for individual pairs of syntactic rules has been made fairly strongly (see especially Pullum 1976), though the issue is hardly closed. The parallel demonstrations in phonology are much weaker, in my opinion. In particular, examples of counterfeeding order in allophonics seem to be fairly numerous and hard to analyze away. My guess is that language-particular rule ordering might be eliminable in syntax, but probably not in phonology.

As for formalism, there's absolutely no reason to think that the formalisms suitable for, say, unbounded movement rules in syntax are going to have anything to do with those appropriate for phonology. Imagine alternative pronunciations of sentences in which some phonological segment could appear in either of two positions, one an indefinite number of segments away from the other, but which are otherwise identical; I know of no such examples. In general, if you import syntactic formalisms wholesale into phonology what you get is usually nonsense. In fact, I'd hope that the two components would have quite different internal organization, since that would lead to a more falsifiable general theory.

The second lesson is quite similar to the first: We have no right to assume (certainly not ahead of time) that allophonic processes are parallel to morphophonemic rules--that argumentation appropriate in one domain carries over to the other, that theoretical assumptions such as those about ordering carry over, or that formalisms appropriate for one will do for the other. The rather large number of people who have concerned themselves with distinguishing morphophonemics from allophonics (Skousen, Linell, Dressler, Stampe, and many many others) have been listing ways in which allophonic processes and morphophonemic rules always or sometimes are different from one another. If we take even a bit of this work seriously, then parallels in argumentation, theoretical assumptions, and formalisms cannot be assumed. And, as I pointed out earlier, if we want to import considerations appropriate to allophonics into discussions of morphophonemics (or vice versa, for that matter) we must supply linking assumptions to relate the two.

The third prescription I feel silly giving, but the fact is it's so often not been taken that the waters of phonological argumentation have been considerably muddied. This is the prescription to survey the **full** set of alternation and co-occurrence phenomena to be analyzed in a language. It's astonishing the extent to which intricate analyses with far-reaching consequences are proposed on quite incomplete sets of what count for phonology as primary data. Even such a work as *The Sound Pattern of English*, which takes on a very wide range of phenomena (morphophonemics and allophonics together, with a very heavy emphasis on morphophonemics), misses a great many relevant allophonic phenomena, and even some of the more prominent morphophonemic alternations. It has no treatment of the analysis of the regular English inflectional suffixes, that classic of beginning linguistic courses. Yet settling on a description of these facts is not an easy matter, being tied up in very complicated ways with other aspects of English phonology and morphology (see Zwicky (1975) for more discussion of these issues than anyone might want). Now I would give *SPE* high marks on coverage of the facts--it would be easy to cite much worse examples, including some from my own

work—but there are still significant gaps. I am surprised that phonologists generally don't adopt the strategy of actually listing, ahead of time, all of the phenomena they believe might be relevant to their analyses.

Finally, I would like to repeat the prescription that linguists, and particularly phonologists, supply the assumptions that bridge the subparts of linguistic description and that connect linguistics to the allied disciplines. I don't think my 1970 discussion of ruki and exceptions is at all extraordinary in its failure to show how the proffered data are relevant to the analytic issue. Nor do I think that such potential lines of evidence should just be eschewed on the grounds that they're 'external'; as I argued at the beginning of this paper, I think phonological analysis needs all the help it can get. But it needs real underpinning, not cardboard props.

Notes

*This paper grows out of Zwicky (1981) and material prepared for a seminar on methodology and argumentation at the Ohio State University in the spring of 1981. It was originally presented at the University of Michigan in May 1981, as part of a series on evidence in phonology organized by Richard Rhodes and Peter Benson; I have not tampered much with its conversational tone. This is the version of 27 August 1981.

¹Actually, these four segments represent a good many more. '/u/' stands for a short vowel phoneme and a long one; for /o/, which functions like /a + u/ in Sanskrit; and for /aw/, which functions like /a: + u/. '/i/' similarly stands for /i i: e ay/. '/r/' stands for syllabics, both short and long, as well as for a nonsyllabic retroflex segment. /k/ before /s/ is the product of neutralizations affecting all palatal and velar stops (/c c^h j j^h k k^h g g^h/) and certain instances of the fricatives /ṣ ṣ̃/. /s/ remains after nasals, the vowels /a a:/, labials, dentals, and /t̪/ (the neutralization product of /t̪ t̪^h d̪ d̪^h/ and of the remaining instances of /ṣ ṣ̃/); the liquid /l/ does not occur before /s/.

²Readers who know Sanskrit might wonder about ~~stem-initial~~ /s ~ s̃/ alternations in compounds and prefixed forms; they are remarkably irregular. Kiparsky (1973:84-5) argues that in classical Sanskrit these alternations are to be described separately from the ruki phenomenon proper (in some cases the alternation appears even in non-ruki environments).

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