

# Generative theory, language contact and Modern Hindustani: Two sides of a linguistic story

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This paper attempts to show that (1) contrary to functionalist claims, generative theory throws welcome light on some contact facts; (2) these very facts force some radical revisions in that which illuminates them; and (3) language contact may be one of our best guides in our search for the right synthesis of formalism and functionalism.

Although supporting evidence from various languages is used, the focus is on Modern Standard Hindustani (MSH).\*

## 1. Introduction.

In its short history, Modern Standard Hindustani (MSH) has been subjected to two major linguistic contact situations, with Persian and with English.<sup>1</sup> Most studies of these contacts assume some version of what might be called the non-autonomy thesis, defended by Kachru (1980) and Shapiro & Schiffman (1981), for example, and do not give due importance to those aspects of contact that follow from factors inherent in language-design.<sup>2</sup>

I begin this paper by attempting to defend the hypothesis that a contact description that does not take into account the role of Universal Grammar and of the particular grammars involved is more than likely to be not only incomplete but also inaccurate, if not misleading.

Later, I attempt to show that that which throws light on some important facts must be revised in the light of these very facts, and even radically. The Kantian core of that which illuminates, I shall show, is

\* For Mohan and Prabhu, in memory of our Singapore days.

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As 'Hindi' is still a somewhat problematic name, I use Hindustani. The reasons for that choice are, fortunately, not relevant here.

According to the thesis in question, grammar has no properties that cannot be derived from some non-grammatical considerations, typically functional in nature.

encrusted under a very heavy pseudo-neo-Kantian layer.<sup>3</sup> The purpose of that immanent critique is to attempt to liberate the potential that seems to me to have turned into a missed opportunity.

This paper is organized as follows: in section 2, I shall argue that contrary to functionalist claims, of the sort made by linguists like Kachru, Shapiro and Schiffman, Gumperz (1982), and various others who have worked on India, generative theory throws unexpected light on some contact-facts; in section 3, I shall attempt to show where the generative explanation goes wrong. In section 4, I summarize what the stories told in sections 2 and 3 seem to lead to. Although evidence for all of what I have to say can be provided from phonology, morphology and syntax, I focus on the phonological adaptation of English loans in MSH and, later, Marathi to illustrate what I want to say.

## 2. Side one.

MSH breaks up English word-initial consonantal clusters sometimes prothetically and sometimes epenthetically: it adapts a word like *station* as /ɪsteʃən/ but a word like *frock* as /frɔk/.

One could claim that this differential treatment of unacceptable clusters depends on whether the cluster begins with an /s/, except that there are near-minimal pairs like the adaptations of *station* (/ɪsteʃən/) and *slipper* (/sɪlpər/).

In Singh (1985) I argue that the distinction prothesis/epenthesis is sensitive to sonority, and if the degenerate segment, the segment that does not fit the well-formedness conditions of MSH, is more or as sonorous as the segment it is adjacent to, it is syllabified with a prothetic vowel, otherwise it is syllabified with the help of an epenthetic vowel.

The question to answer is: 'Why does MSH use two different strategies to break up word-initial consonant clusters of English words?' Although the fact that native-speakers of MSH use both these strategies in borrowing English words or in incorporating them into their interphonology<sup>4</sup> has been noted before (cf. Arun 1961, Varma 1972 and Koshal 1970, for example), no one, to the best of my knowledge, has asked, let alone answered, why only one of these strategies is not used. Let us consider the facts, given in (1) below:

(1)	Station	[ɪsteʃən]	Slipper	[sɪlpər]
	Spelling	[ɪspelɪŋ]	Sleeping-bag	[sɪlpiŋ-bæg]
	School	[ɪskul]	Frock	[frɔk]
	Sphere	[ɪsfiːə]	Fruit	[fruɪt]
	Speech	[ɪspiːʃ]	Flirt	[flɜːt]
	Spoon	[ɪspun]	Pfizer	[pʃaɪzər]
	Screw	[ɪskru]	Cloth	[klɔθ]
	Stool	[ɪstul]	Please	[pliːz]

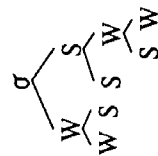
The motivation for the fact that the words in (1) are broken up is quite clear: the dialect in question does not allow any complex onset other than *tr*. What is not clear is why the unpermitted clusters are broken up sometimes prothetically and sometimes epenthetically, despite the fact that either solution can get rid of the offending clusters.

The degenerate segment is syllabified as it is because it has to be syllabified that way. Let me explain myself. Segments have to be assigned structural homes within, let us say, possible expansions of Kiparsky's (1979) Universal Syllabic Template (U.S.T) within constraints imposed by the well-known sonority hierarchy and language-particular special dispensations.

The central claim of Kiparsky's theory is that syllabification is governed by the universal rule (2) which assigns metrical structure to strings of segments and which may be augmented by language particular rules. (2) requires an optional matching of the syllabic template (3) to the well-known sonority hierarchy (4):

- (2) Analyze a string of segments into the simplest maximal sequence of trees in such a way that the relative sonorities defined by (3) and (4) match for every pair of adjacent segments in a  $\sigma$ .

- (3) Syllabic template:



- (4) Sonority hierarchy:

Stops fricatives nasals l, r, w, y, i, o, e, a

By language particular rules is meant special dispensations of the sort that allow the strong /s/ in the onset of English words like *spot* and *school*. Language particular preferences are exemplified by statements of the sort that whereas English prefers maximized onsets, Finnish and

<sup>3</sup> The core, to unpack the metaphor a bit, is the promise that generative grammar provides an account of *potential* and the crust in which such a promise is buried is the reification of technological tautologies the generative tradition likes to refer to as linguistic theory. What is at stake is the Kantian (1929) distinction between constitutive and regulative rules, and the claim is that the generative tradition projects the regulative as the constitutive. The full implication of this will become clear as we proceed.

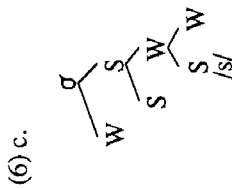
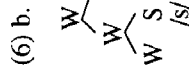
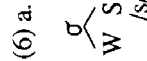
<sup>4</sup> I use the term in its standard sense - 'the phonology of second language learners.'

Sanskrit prefer maximal rhymes. A sequence like *hamstra* is, thus, syllabified as *ham\$tra* in English but as *hamst\$ra* in Finnish. All this is done, of course, within the general constraints imposed by the sonority hierarchy. An intervocalic CC sequence is treated, if it can be so treated, within these constraints, as onset or rhyme. Exhaustive assignment of consonantal clusters, in other words, is preferred to ambisyllabic assignment. The theory, as a casual inspection of (3) would reveal, makes the general claim that in the unmarked case onsets have rising sonority and rhymes decreasing sonority. The optimal two segment syllable generated by (3) is CV, the open syllable. In assigning metrical structure to strings one tries to get as many of these as one can. Maximization of onset is one of the principles that increase the number of such syllables and even in languages that like to maximize their rhymes (3) assigns a single intervocalic consonant to the onset of the second syllable.

In order to see what is going on, consider the words *station* and *slipper*. The /s/ in *station* is an S (strong) but it is a W (weak) in *slipper* as it is more sonorous than /t/ but less sonorous than /l/. Borrowing these words involves doing something with the degenerate /s/. If it is a weak /s/, the easiest way to do something with it is to construct the minimal syllabic tree necessary to accommodate it, and insert the epenthetic vowel under the empty S.



Faced with the string *station* where /s/ is, according to the standard sonority scale, stronger than /t/, there are three options (given under 6.a-c):



Given the fact that MSH does not allow syllabic sonorants, let alone syllabic fricatives, the structure provided by (6.a) cannot be used. Neither can (6.b), because the only complex onset allowed is obstruent + r. The only place where one can find a strong /s/ in MSH is in the coda (as in the words in (7)):

- (7) most (carefree), sust (slow), čust (alert).

The slightly different Eastern Hindi and Punjabi facts - they use only epenthetic vowels - can be explained by postulating a relabelling rule that relabels any word-initial consonant in a cluster as a W because there is no structural possibility of having a strong consonant in the non-branching onsets of these languages. The difference between MSH and Eastern Hindi and Punjabi is essentially the following: MSH does allow complex onsets but Eastern Hindi and Punjabi do not. MSH treats word-initial SW sequences by treating the S as an S and incorporating it into a rhyme. Eastern Hindi and Punjabi, on the other hand, relabel it as W because the total absence of complex onsets in these languages rules out the possibility of their entertaining an S, a relative value, in the onset slot.<sup>5</sup> It could metathesize them but does not, because sequences like /ts/, though unmarked as onsets according to the theory, are not permitted in MSH.

One could argue that this sort of relabelling also goes on in languages that do not have any complex W's at all - i.e. no complex onsets and no complex rhymes. Kaye & Lowenstamm (1982) mention three such languages: Desano, Dida, and Maori. Loan words in these languages, like Eastern Hindi and Punjabi, are likely to show up with an epenthetic vowel.

Let me summarize the structure of what I have said so far. I have argued, perhaps redundantly, that no linear analysis can easily and satisfactorily account for the facts of (1), though such analyses are possible for loan-words in Eastern Hindi, Punjabi, Indonesian, Japanese and Spanish. That alone would be enough to make my point. I have, however, tried to show that the framework unutilized to explain the syllabification of English words in MSH leads to revealing formulations of some puzzling contact facts.

We have seen that some of the apparently arbitrary prosodic aspects of English loans in MSH and of the interphonology of native speakers of MSH learning English as a second language can be adequately accounted for only in terms of the metrical theory of the syllable of the sort proposed by Kiparsky. Such an account is quite explanatory and provides a fairly precise characterization of the role played by universal and language particular (L<sub>1</sub>) factors in shaping the syllables of borrowed words or interphonology syllables (cf. Tarone 1976). Our search for an explanatory account of the prosodic aspects of English loans in MSH and of the shape of syllables in the interphonology of native-speakers of MSH learning English leads us to the conclusion, for which substantial evidence exists, that a restrictive, well-articulated theory of phonology can provide a very satisfying account of facts assumed to be beyond its explanatory reach. 'Sociolinguistic' or 'language in context' investigation of English loans in MSH cannot even ask the question such a theory answers.

<sup>5</sup> By allowing relabelling to take place under specific conditions, we in fact restrict the power of these rules. Stowell (1979) argues in a similar vein.

3. Side two.

Having argued, in deliberately partisan tones, that generative theory can say some very revealing things about language contact, I now turn to the second side of the story.

The problem is that while the theory defended in section 2 provides an illuminating account of the facts considered, it raises a series of other questions that must be answered. Let us see in some detail precisely where the explanation I defended rather vigorously in section 2 leads us.

The first problem is that there does not seem to be any essential difference between a phonological rule that inserts a vowel before the more sonororous consonant and a strategy for adapting words from another language that does the same thing, because they both have to do with syllabifying an initially non-syllabifiable sequence, and that a reasonable explanation for why these rules and strategies look like they do is not available within the sort of framework espoused there.

The first conclusion is important because even some of the most insightful studies of loan phonology, such as Kaye and Nykiel (1979), leave the impression that these strategies are somewhat arbitrary.

But, more importantly, consider some synchronic facts of Turkish:

(8) Turkish data:

8.a Some Non-alternating Nouns		
gloss	absolute	dative plural
hand	el	eller
horse	ata	atlar
ball	topa	toplar

8.b Some Alternating Nouns

chest	koyun	koyuna	koyunlar
son	ogul	ogla	ogullar
time	vakit	vakte	vakitler
mouth	alin	alna	alinlar
root	cezir	cezre	cezirler
intelligence	akil	akla	akillar

8.c Some More Non-alternating Nouns

sheep	koyun	koyuna	koyunlar
woman	kadin	kadina	kadinlar
sea	deniz	denize	denizler

8.d Some Non-alternating Nouns with Final Clusters

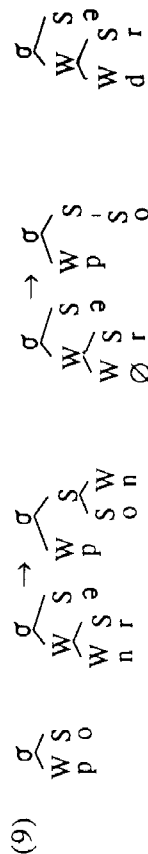
steep	sarp	sarpa	sarplar
harsh	sert	erte	sertler
folk	halk	halka	halklar
upper-surface	üst	üste	üstler
love	aşk	aşka	aşklar

Given (i) that final clusters are preserved only when followed by a vowel-initial suffix (8.b) and (ii) that not all final clusters are disallowed, how are the facts of (8.a-d) to be stated? One crypto-taxonomic type solution would be to postulate a rule of epenthesis and mark the items in (8.d) as lexical exceptions; another such solution would be to postulate a rule of syncope and mark the items in (8.c) as exceptions.

The Turkish type of facts have, in the past, led to another type of solution of phonological problems I shall refer to as *the naming solution*. The name used in this case was 'conspiracy' (cf. Kisseberth 1970).

But to come back to epenthesis, Pyle (1974) correctly observes that the principle involved is rather simple: clusters are permitted only if the first consonant is more vowel like than the second. His locution 'more vowel-like' translates naturally into 'more sonorous' given sonority hierarchies of the type one now takes for granted.

That rules such as epenthesis have something to do with syllable structure is further corroborated by diachronic facts. Consider Old French consonant epenthesis (cf. Walker 1970). The epenthetic consonant can be said to come about because Old French does not allow *mr, nr*, etc. as syllable onsets or consonants to be dominated exclusively by *S*. Consider the entirely representative case of *pondre < ponere*. The post-syncope derivation is sketched out in (9):



The reasons for which the derivation given in (5) is required are: (i) *nr* is not a possible onset in Old French, (ii) *r* cannot be syllable in Old French and (iii) moving a segment to close an initially open syllable leaves a structural hole, at least in a language that prefers, like Old French, to maximize its onsets rather than its rhymes.

I take it that I have succeeded in showing that there is no essential difference between a phonological rule that inserts a vowel before the more sonororous consonant and an adaptation strategy that does the same thing, because they both have to do with syllabifying an initially non-

syllabifiable sequence, and that a reasonable explanation for why these rules or strategies look they do is needed.

What I would like to suggest is that these adaptation strategies are not merely natural extensions of phonological rules of the type needed to account for the Turkish facts: phonological 'rules' of the type in question are repair strategies for rendering unsyllabifiable strings syllabifiable.

What are called phonological rules, in other words, are merely positivistic descriptions of the effects of these repair-strategies (for a demonstration of the entirely secondary, derivative status of 'the rules of epenthesis', see Singh 1987 and Piggott & Singh 1984, and for defence of the position that the construct 'phonological rule' is largely dispensable, see Singh 1987 and 1990).

It is impossible not to point out that the connection the analysis proposed here establishes between phonology, interphonology, and loan-phonology amounts to a fundamental critique of some of the basic assumptions of standard generative phonology.

Although generative phonologists have often used borrowing as evidence for motivating proposed rules of phonology, they have not bothered to ask the question: 'Why do some rules of phonology not play a role in language-borrowing?'

The relationship between phonology, loan-phonology, and interphonology, in other words, has been typically seen as unidirectional, and borrowing evidence has been treated only as icing on the cake. I am, on the other hand, suggesting that although strategies that play a role in the adaptation of foreign words are unquestionably active 'rules' of synchronic phonology, those 'rules' that do not play such a role are perhaps *not* phonological 'rules' at all but only rules frozen in the lexical/morphological structure.

Alternations involving sounds divide themselves into two categories: global and local. The former are governed or motivated by well-formedness conditions (cf. Singh 1987) and the latter constitute part of the morphological operations which in some theories are seen as triggering them (cf. Ford & Singh 1983). Thus, a rule like  $r \rightarrow \emptyset / \#$  in British R.P. is a global rule because it is governed by the WFC that disallows  $r$  in R.P. rhymes.

A rule like German Umlaut, on the other hand, is part of the morphology of German (cf. Janda 1982). It is interesting to note that only 'governed' rules are used to nativize foreign words (cf. Hyman: 1970, Kaye & Nykiel 1979, amongst others). Alternations such as English Velar Softening (cf. Chomsky & Halle 1968), Spanish Diphthongization (cf. Harris 1978) and German Umlaut do not ever play a role in loan-phonology. They do not because phonological adaptation is just that - phonological.

An acceptance of the central hypothesis of this evaluation of the explanation defended earlier - the hypothesis that there is no essential

difference between phonology, loan-phonology and interphonology - leads, it is easy to see, directly to the rejection of the traditional distinction between 'external' and 'internal' evidence (cf. Kenstowicz & Kisseberth 1977) and paves the way for a phonology that may turn out to be of some use (cf. Singh & Ford 1985 and Singh 1986).

The issue, it should be obvious, is the place of morphonology in the grammar. Consider a list of some allegedly problematic examples of interlingual morphological and morphonological errors.

(10) a. *Dutch Inter-German* (Jordens 1977)

- (i) *verstehbar* (for *verstandlich*, understandable)  
(cf. German *furchtbar*, *wunderbar*, etc.)
- (ii) *geistlich* (for *geistig*, spiritual)  
(cf. German *kostlich*, *ensetzlich* etc.)  
(cf. Dutch *geestelijke*)  
(cf. German *geistliche*, 'pertaining to the church')

- b. (i) *Dutch Inter-German* (Singh & Martohardjono 1988)  
*marcheerd* (cf. Dutch *herlezen*)
- (ii) *English Inter-French* (cf. Singh & Martohardjono 1988)  
*cynicisme* (for Fr. *cynisme*) (cf. *catholique/catholicisme* vs. *fanatique/fanatisme*)

Were it not for cases like (10b) one could say that evidence clearly indicates that the role of  $L_1$  in shaping interlanguage is confined to those of its rules that are needed to account for its global alternations, alternations that are independent of its morphology. The rules needed to account for the local, morphologically dependent alternations of  $L_1$  or the ones needed to account for its word-formation processes do not play such a role. Interference, in other words, is caused only by across-the-board phonological rules of  $L_1$ . So called morphophonemic rules of  $L_1$  do not cause it, and morphological interference from  $L_1$  seems not to exist, as word-formation errors in intermorphology are the results of illegal extensions of  $L_2$  word-formation rules. As we shall see, one can actually say that the apparently problematic cases are not problematic at all.

As far as the morphological and morphonological cases are concerned, the only ones that call for comment are the cases in (10b). They are often presented as examples of  $L_1$ -induced problems. In the case of (10bii) also, it is possible to argue that the error is induced by  $L_1$  (cf. English *cynicism*).

The traditional classification of these errors is that whereas the examples in (10b) are instances of interference, others show 'overgeneralization' that is  $L_2$ -induced. The examples that I have

provided parenthetically suggest quite strongly that they are actually all overgeneralizations induced by L<sub>2</sub> and that the grammars of L<sub>1</sub>'s involved have nothing to do with them, unless one considers the notion of 'seeing structural relatedness' à la Jordens or Kellerman (1978) a grammatical notion, and makes the availability of evidence that the misapplied suffix is available in L<sub>2</sub> a condition on learner's competence à la Adjémian (1983), both dubious moves, though incorporating factually correct observations. Adjémian's move is only an indirect way of saying that an interlingual morphological error must be licensed by L<sub>2</sub> morphology. If there are formal similarities in the relevant lexica and in the bits of morphology involved, cases involving them, would, of course, appear more frequently than otherwise, but, obviously, for reasons that do not have anything to do with grammar. The fact that *faux-amis* cause more problems than real ones is not a module-particular grammatical fact but a general, global principle, perhaps of memory.

Since the Dutch-German case is often cited as an example of L<sub>1</sub>-induced morphological and morphonological errors, it will be worth our while to look at it carefully. Jordens gives examples of attested errors which could be ascribed to interference. He explains these by saying that the learners have transferred the Dutch equivalents of these forms; where Dutch has weak declension German requires middle. The alternative explanation, unlike direct transfer, takes into account the learner's active hypothesis testing: these errors could be seen as being produced by the overgeneralization of existing German patterns. It is interesting to note that *geistliche* does exist as a lexical item in German, but that it has taken the semantically narrow meaning of 'pertaining to the church.'

The best generative account of the generalization that only certain kinds of phonological rules can cause interference is proposed by Rubach (1984). He argues that, by and large, only post-cyclic rules can cause interference. The limitations covered by the 'by and large' of his explanation are taken care of by stipulative additions to the principle that "postcyclic rules may cause interference" (p. 42). The modified principle is formulated by Rubach as follows:

- (11) Postcyclic rules which are automatic and context-sensitive may cause phonological interference (p. 44).

The cyclic account, however, faces some serious problems, even if the rather dubious status of the cycle and the host of problems associated with it are ignored (cf. Kiparsky 1982). Consider, for example, the proviso concerning automaticity and absolute neutralization. They require the same sort of interpretive power that things like the Alternation Condition do: there is, for example, no way of finding out, short of inspecting, to use Kiparsky's (1982) words, every derivation, if a given rule meets the automaticity condition. If automaticity is what is at

stake, it should be formalized in the grammar rather than left as a formally uninterpretable footnote to cyclicity.

The observation that certain things about the sound pattern of a language can cause problems of the sort seen in examples like MSH /*stésən*/ and /*frøk*/ despite the fact that they are not satisfactorily described in terms of what the generative tradition calls phonological rules, presents both empirical and conceptual problems for most contemporary theories of phonology.

It is quite clear that Classical Generative Phonology (CGP) or its offshoots can offer no truly satisfying account of facts of the sort we are concerned with. All that it can do is attribute the insertion of epenthetic vowels in unacceptable clusters to some derivative extensions of phonological rules without assigning them any status whatsoever within synchronic phonology. Weinberger (1990), for example, calls them loan-phonology rules but has little to say about their relationship with phonological rules! Whether these ill-understood derivative extensions will or can appeal to syllable and word-structure constraints is hard to say, for such constraints have no place or status in CGP. If they are allowed to appeal to these constraints, they must, within the CGP frame of reference, be seen as doubly unusual *vis-à-vis* phonological rules.

The problem with CGP and its offshoots is that they see adaptive mechanisms of loan-phonology as derivative extensions of what they call phonological rules rather than see the latter as limited versions of processes the speaker can be said to have. Weinberger (1990), for example, uses the notion 'repair strategy', first proposed, to the best of my knowledge, in Singh (1981), without relating it to what he, like others, call phonological rules. The point, made originally in Singh (1985), is that repair-strategies are not derivative extensions of phonological rules: the latter are merely descriptions of particular cases of repair. We shall return to the matter later.

I must make it clear that what is being argued against is the construct or device called phonological rule as it is used in contemporary phonological theories. As the phonological competence of native speakers must be described in terms of what Kant (1929) called *constitutive rules*, there is a sense in which any mechanisms postulated to account for that competence can be called rules. That sense of 'phonological rule' is not at issue here.

What *is* at issue is the mechanism X → Y/Z (X becomes Y in the context Z), a mechanism that allows CGP to treat uniformly both dynamic, live, phonotactically governed and processural alternations and static, dead, phonotactically ungoverned and non-processural ones. Phonological theories utilizing this notion of phonological rule seem to me to make a crucial mistake: they do not draw the obvious conclusion that if every language has a rule of epenthesis, for example, then none really does, and that if things are factored out all that is left for 'phonological rules' to describe is non-automatic morphonology. The

possibility that Weinberger (1990) invokes, for example, is not a real one (what if Kannada and Spanish did not have, he asks, any epenthesis rule?).

Actually, the situation, as I pointed out in Singh (1987), is much worse. Almost all contemporary theories of phonology, particularly the off-shoots of CGP, use both phonological rules and well-formedness conditions, ignoring Postal's (1968) redundancy objection. Postal objected to phonotactic conditions because he thought they were unstateable and redundant (for a detailed discussion, see Singh 1990). Although his unstateability argument was shown to be fallacious by Sommerstein (1977), his redundancy objection still applies to most contemporary theories of phonology. The way to meet it, I argue in Singh (1987 and 1990), is to eliminate 'phonological rules'.

That mistake has recently been recognized under what is called the principles and parameters view, but most principles and parameters models of phonology seem either reluctant to give up the idea of language-particular phonological rules, chiefly because of their reluctance to give up the idea that morphonology is a part of phonology (cf. Singh 1987), or simply use the word 'parameter' for 'rule'. The impact of parametric reinterpretation of traditional typological concerns of linguistics is clear neither in syntax nor phonology, chiefly because empirical and conceptual constraints on precisely what constitutes a parameter remain far from clear. The reification of 'parameters' as explanatory devices seems much too premature to me, certainly in phonology. Even if the use of parameters in phonology can be constrained, it is clear that no truly principles and parameters account is going to be possible until the assumption that morphonology is part of phonology is given up entirely (cf. Singh 1987). As long as it is not given up, all one can have is a principles-parameters-rules account. It is, at any rate, premature to refer to this research program as a theory, though its promise must be duly acknowledged.

Our examples show that the speaker's phonological competence is larger than the 'competence' that can be rationally reconstructed on the basis of language internal distributional regularities. It is here that the people of Tasmania take their revenge against SPE (cf. Chomsky & Halle 1968:4).<sup>6</sup> The trouble with the generative enterprise, in other words, is that it takes its distributional picture to be the picture of the speaker. That is what allows Weinberger (1990) to speak of the transfer of not only the rule of epenthesis but also of the "method for determining the quality of the epenththesized vowel" (p. 163).

Our response must construe phonology as both of *the speaker* and of *the language*, to use Stampe's (1987) conceptually rich locutions.

<sup>6</sup> My metaphor here is based on the following statement by Chomsky & Halle (1968:4): "For example, if only inhabitants of Tasmania survive a future war, it might be a property of all their existing languages that pitch is not used to differentiate lexical items."

Whereas SPE and its offshoots want to characterize the speaker as a derivative extension of phonology empirically deduced from distributional regularities, we must, it seems, accept the Stampean view that the speaker is larger than his 'phonology', and see the phonology of a language as a constrained derivative delimitation of the speaker. These limitations are what he has to learn, or, more precisely, what he has to learn are the conditions that the inevitable skewed distributional regularities of his language exhibit. They meet James' (1990) demand for what he calls 'behaviourally motivated structural teleology'. Having learnt them, he has recourse to a universal set of repair strategies whenever these conditions run the risk of being violated, and his bag of tricks also contains things his language never gave him the opportunity to use. The ability to tell us what to do despite a lack of experience is, after all, the essence of (Kantian) constitutive rules. It is also important to point out that the WFC's of his language that constitute the essence of its phonology are not all language-specific primitives: some of them are merely individual instantiations of universal phonotactic well-formedness conditions. These can and should be factored out. What the speaker has to learn is the set of WFC's that are truly language-particular or idiosyncratic. He learns them by finding out that the processes he is endowed with must be inhibited or suppressed under these conditions, but the empirical content of his knowledge is those conditions and the acquisition of phonology ( $L_1$  or  $L_2$ ) is the acquisition of those conditions. My plea to Stampeans would be that WFC's are as close as we can hope to get to formulating the knowledge that the speaker can be legitimately said to have. It is obvious that what happens is that the processes get delimited qua processes in ways we cannot even begin to describe. The WFC's, I am willing to admit, may describe only knowledge accessible to consciousness.

It is also possible that *acquisition* is the wrong metaphor because the learner doesn't actually *acquire* them. He merely *discovers* them; but having *discovered* them, he must *keep track of them*. Notice that one reason the construct *phonological rules* has had unwarranted success is that one can speak more easily of *acquiring* a rule: it is something you can add to your regulative bag of tricks. But this Chomskyan systematic ambiguity cannot, as we have seen, be sustained for long. The speaker does acquire *constitutive* rules but they have nothing to do with what most phonologists have called 'phonological rules'.

#### 4. *Summing up.*

In section 2, I argued that the view that generative theory is incapable in principle of handling the messy facts of language contact is quite erroneous. It can, I tried to show, not only handle some of those facts, but also help us analyze them quite insightfully. As a matter of fact, the

formal aspects of linguistic contact remain quite obscure under other lights.

Consider, for example, Pandharipande's (1981) claims regarding the phonology of loan-words in Marathi, a first cousin of MSH. She claims that loans are never completely nativized in Marathi, that there is a hierarchy of loans from Sanskrit, Persian, and English, and that this hierarchy is explainable on the basis of the attitudes of Marathi-speakers towards Sanskrit, Persian, and English. These claims are quite extraordinary, but apparently not to someone not interested in theoretically guided grammatical inquiry.

The first claim is empirically false for Marathi is full of completely nativized Sanskrit words. As far as the other two major claims are concerned, Singh & Lele (1985) suggest that what is construed as the hierarchy of nativization is in fact a function of two rather simple facts: 1) that borrowed words must first be assigned to a morphological category and a declensional class, if one believes in them, within that category, and 2) that borrowed words that do not have a svarbhakti vowel, the crux of the hierarchy of nativization, because of prosodic reasons, have it for entirely morphological reasons. I can provide only one example here to show that the alternative is fraught with dangers because it does not exhaust the possibilities provided for by the autonomy theory. It claims that English *bus* and *doctor*, when borrowed into Marathi, do not take the locative suffix *-t* as it would create unacceptable codas. The solution suggested is the use of post-position *madhe*. Native speakers of Marathi could, potentially, render the unacceptable \*/bəs/ and \*/dɔktər/ acceptable by inserting an epenthetic vowel in the unacceptable coda, but, it is claimed, they do not do this because their attitudes are such that they want not to treat English words in ways they treat their native lexical stock. The existence of words like /postət/, 'at the post-office', casts very serious doubts on the power attributed to these attitudes.

Looking at this power from the other side – the social side, that is – it has failed despite the many efforts on the part of some native speakers to replace these words with Marathi words. The conclusion: the native speaker of Marathi uses the prosodic principle of svarbhakti – as does the native speaker of Hindustani, Turkish, Persian, and Walpari (cf. Singh:1981b) – to incorporate words from whatever source just as he uses it to syllabify unsyllabifiable morpheme concatenations that arise in his native sub-lexicon. Of course, he does so if, and only if, morphology forces his hand.

The point I want to make is not that the analysis in question is wrong, but that all such analyses are caught on the horns of a dilemma. Their less than sophisticated functionalism, unfortunately, guides them away from asking questions the answers to which may lead to a theory of society. Although the trap such an orientation sets up is not easy to

avoid, one should not abandon modesty and proceed to reduce society to attitudinal grids and multivariate matrices.

In section 3, I argued, equally vigorously, against what I consider to be some of the structuralist limitations of generative linguistics. Like all other structuralist enterprises, it ignores the only real evidence there is. To set up a rule of umlaut in a synchronic phonology of German, for example, amounts, to put it rather bluntly, to nothing more than a demonstration of one's ability to read German.

It is somewhat ironic that analyses based on such concretist assumptions are generally referred to as abstract. There's nothing abstract about the claim that an umlaut is an umlaut. It is simply a literate tautology. It is, in fact, the so called concrete analyses that are based on the abstract assumption that native speakers can treat superficially similar things rather differently, some in phonology and some in morphology. The so-called abstractness problem is, however, not my concern here, (cf. Singh 1986 for some further discussion).

My indictment of Kachru-Pandharipande-Schiffman style of work on linguistic contact does not, however, entitle generative linguistics to close the curtain that can bring in some light to help us verify its hard-earned, insightful grammatical findings. The fact that the best the contact-specialist can do is to say things like "-wala is used as a cushion for easing the shock of borrowing" or the fact that even Naro (1980), in his extremely sympathetic review of Sankoff (1978), is forced to fault Kiparsky only for the fact that social variables have to be learnt speaks very highly of the possibilities in the generative enterprises.

But, I believe, the revolution is bound to be seen as a missed opportunity as long as the generative grammarian restricts himself to ways that require reification and positivist tricks.

Notice that the generative enterprise is quite unusual in this respect. It does not follow what Erikson (1963) calls the familiar course of inquiry: the course that looks at the margins to see what the centre may look like. It looks instead at the centre to project what can be done about the margins. Whereas it is quite normal for other explanatory enterprises to pay more attention to the abnormal in order to discover the simple laws man and nature live by, it is standard practice in generative linguistics to look at the normal only.<sup>7</sup>

There is no methodological quarrel with characterizing the abnormal in terms of the normal, a theoretical necessity, but what is disturbing is that the abnormal is rarely, if ever, allowed to bring to bear on the normal. Consequently, some crystals, as Freud would have said, never reveal their structure.

<sup>7</sup> 'Abnormal' here is intended to include all so-called 'external evidence', the rôle of which is discussed in, for example, Bertinetto (1992) and Singh (1988).



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