A problem: 'lenition' of Dravidian obstruents

The issues I raise here are neither new nor unfamiliar, but I think they are no less deserving of another airing. I suggest that what seems at first a somewhat parochial problem in the theory (or, perhaps better, methodology) of reconstruction may turn out to have wider implications.

Assume a set of languages known to be genetically related, and to have been separated for some considerable time; i.e. they are the daughters of a well-justified (and acceptably reconstructed) protolanguage. Now say that all these attested dialects share some 'common property'. The problem --- a familiar one --- is whether this property is a survival from the protolanguage itself, the result of a single innovation in 'late Proto-L', before the separation of the daughters, or the result of a series of parallel ('convergent') innovations in the daughters. Let us assume further that this is the classic situation where such questions arise: there is no compelling external evidence.

For instance: virtually all the Dravidian languages show a general distribution of obstruent phones of this type: single (short) voiceless stops occur only initially or in clusters with other obstruents, not intervocally or in post-nasal position. The reflexes of what are generally assumed to be P(proto)-D(ravidian) single voiceless stops in these positions are either voiced or fricative (or both) intervocally, and voiced after nasals. As an example, consider these forms showing reflexes of reconstructed PD *[-k-] (after Emeneau 1970:20):
No dialect has forms of the type *[^poke^], etc. in this category, which is taken to go back to PD *(^pukV^). Similar developments can be seen in the other reconstructed voiceless stops.

This intervocalic 'lenition'^5^ (the reason for the inverted commas will become clear later on) is generally traced back to some predialectal^6^ innovation; Emeneau is particularly clear on this point, since his etymological cover symbols ('*-k-' in the case above) are taken to represent "PD phones" (Emeneau 1970:1 --- my emphasis). The general pattern appears at its clearest, perhaps, in the phonetic distributions in Tamil and Malayalam, which are (rightly, I think) usually regarded as particularly 'archaic' or 'conservative' in this regard (no voiced obstruents initially in native words, for instance). Thus for Tamil, in Dravidian vocabulary, we get the following type of distribution:

(2) | #   | V   | VN  |
----|-----|-----|
    | v   | mb  |
  p- | -v- | -mb-|
    | pp- |      |
  t- | -δ- | -nd-|
    | tt- |      |
    | t-  | -ηl-|
    | tt- |      |
  tʃ-| -s- | -ndʒ-|
    | tt- |      |
  k- | -x- | -ŋg-|
    | kk- |      |

Historically, the first row in each series represents a PD */C/, and the second a */CC/ ('geminate' or 'long' or whatever, but systemically distinct from */C/, and prosodically of the type that makes a syllable consisting of a short vowel followed by it heavy). Each row may be taken, synchronically in native lexis at least, to represent one phoneme,^7^ under the usual conditions of complementary distribution, phonetic similarity, etc. That is, the obstruent system is /p ʈ ʈʃ k/, with contrasts /k/:/kk/ restricted to intervocalic position. According to the standard reconstruction, the distribution in (2) is to be traced back to this phonetic^8^ distribution in PD (here using the conventional Indological symbols):
There is no evidence for medial *[-p-]: the reason is that 'morphophonemically, *-v- takes the place of *-p- in alternation with *-pp-... It is probable that no set of phonemic correspondences can be found which would require the setting up of *-p- in contrast with *-v-' (Emeneau 1970:38). I will return to the significance of this fact below.

2 Convergence vs survival

The material set out so far raises the following questions:

(a) The convergence question --- Does lenition of single intervocalic stops go back to some pre-dialectal general lenition? Or can some of the dialects in (1) be said to exhibit the results of parallel innovations?

(b) The innovation question --- If none of the daughters of PD show a voiceless stop in intervocalic or post-nasal position, what is the justification for reconstructing phonetic voiceless originals as in (3), and attributing lenition --- whatever its exponent --- to innovation at all (whether single or multiple being beside the point there)?

First the convergence question. Most of us have a built-in, and within
limits, laudable desire for 'simplicity', i.e. for not multiplying entities. So on methodological grounds we would like to claim a single predialectal innovation, rather than parallel ones in the separated daughters. But a certain amount of tact is needed here; we have to temper our desire for simplicity (one facet of which is the avoidance of coincidence) by judgements or intuitions of the relative 'naturalness' of the processes involved. (Here 'naturalness' is defined as 'the property of being — ceteris paribus --- pretty much expectable'.)

Let me illustrate with a reductio. Say that in some family all the dialects but one show nasalized vowels in the sequences \[ VN^C \], \[ VN^# \]; and the odd one lacks these, but in cognate forms has \[ VC \], \[ V^# \]. Could we argue for nasalization as a single pre-split innovation? Clearly the dialect that has deleted the nasals has innovated; but what about the nasalization that feeds the deletion? It may seem likely that this is a survival from a pre-separation state, but the process is so common in any case, and so well-motivated, that it would be hard to rule out convergence with any confidence.

Is the Dravidian lenition a similar case? Given only the array of comparative evidence, it might be interesting to see if there are any arguments that would enable us to decide one way or the other. If we consider the reflexes of some PD \[*-C-\] in an exemplary set of dialects, we find the following types:

\[
\begin{array}{|c|c|c|c|c|c|}
\hline
\text{PD} & \text{Ta} & \text{To} & \text{Ka} & \text{Tu} & \text{Te} \\
\hline
*\text{-t-} & \delta & e & \underline{d} & \underline{d} & \underline{d} \\
*\text{-t-} & \delta & d^- & \underline{d} & \underline{d} & \underline{d} \\
*\text{-c-} & s & s & s & \underline{d}_3 & s \\
*\text{-\k-} & x & x & g & g & g \\
\hline
\end{array}
\]

Toda is the one dialect in which at least some reflexes of all PD categories are voiceless; Tulu is the only one in which they are all voiced. This suggests --- initially --- the possibility of two lenition types, one by voicing and the other by simple spirantization. If this is acceptable, then we can perhaps argue for convergence, since we have as it were two separate routes to follow. The problem is, however, the neat typologi-
cal split Toda/Tulu, with the rest 'mixed' in that everywhere except Tulu the reflexes of */c-/ are voiceless, regardless of what else may be going on.

But there are, curiously, grounds (of sorts) for arguing that even the voicelessness of some of the Toda reflexes may reflect a development convergent with, but different in source from, the voicelessness elsewhere. The argument goes like this: first a scenario like (5a) below is extremely unlikely in intervocalic position, whereas the types in (5b) are quite acceptable:

(5) a. \( t > d > \emptyset > \emptyset \)
    b. \( t > \emptyset > d; \ t > d > \emptyset \)

That is, while spirantization of an intervocalic stop directly to a voiceless fricative is not surprising (it occurs for instance as an allegro rule in many dialects of English, e.g. my own as in \( [\text{ph}]{\text{ix}} \) 'picking'), intervocalic devoicing is highly suspect. (Dissimilations are --- ceteris paribus again --- disfavoured over assimilations.) Thus the fairly consistent \( *[c]>[s] \) is quite all right, as is Tamil \( *[k]>[x] \).

But to have \([s] \) arise via \([d3] \), let's say (or an earlier \([f] \), if \( */c/ \) was a stop and not an affricate) is difficult. Similarly, if \([d3] \) arose via \([s] \), this would also be undesirable, since it would require re-strengthening (fricative to stop), not to mention re-palatalization. So at least Tulu, for the palatals, must have had a different development from any of the others, and have started here with direct voicing. If any direct spirantizations are 'original', then Tulu had a separate development from the start.

A closer look at Toda, however, suggests something interesting: note that the reflexes of the PD intervocalic stops are 'intervocalic' only historically, due to alterations in Toda morpheme structure:

(6) Toda               Kannada
    \( \text{pa}x \)        \( \text{po}ge \)        'smoke'
    \( \text{mi}x- \)        \( \text{mi}sk\u0102 \)        'move'
    \( \text{pi}6x- \)        \( \text{hi}d\u0102 \u0131ku \)        'squeeze'
Just about all of the Toda voiceless fricatives can be explained in a natural way as syllable-final or pre-obstruent devoicing — normally consequent on loss of inflectional, thematic or other post-radical vowels. If this is the case, we might reasonably take Toda as having gone through a voicing phase (along with Tulu). We could even include the palatals, by a sequence \([c] > [\text{c}] > [\text{c}]\), with \([\text{c}] > [\text{c}]\) by devoicing, and then \([\text{c}] > [\text{s}]\). For the velars, for instance, we could take Toda \([x]\) in pux and Tamil \([x]\) in puxai as just possibly convergent, approaching the same result by different pathways:

(7) Toda: \(\text{VgV} > \text{VgV} > \text{VgV} > \text{Vg} > \text{Vx}\)

Tamil: \(\text{VgV} > \text{VxV}\)

Thus spirantization is the first modality of lenition, with voicing supervening in some cases; the retroflexes are an exception, since they seem not to spirantize anywhere.

We can say with fair confidence that there seems at least to be a basic lenition schema implemented in all these dialects (perhaps interpretable as a 'metarule' in the sense of Lass 1976:ch. 2), with very different (and hence only partially convergent) parochial manifestations. But the actual details — and even, ultimately, the problem of convergence itself — cannot be settled except by some methodological solution (Occam's Razor, judgements of probability, etc. — see §3 below). The issue is ultimately decidable only by transcendental, not empirical argument.

3 Some guidelines for convergence

The general convergence problem remains: can we in fact identify genuine examples and separate them from spurious ones? I rather doubt if this can be done by legislation; I suspect it is largely an ad hoc matter, since the availability of evidence, and therefore the groundwork for empirical argument, depends on any particular case on contingent factors (see ch. 1 above, Lass 1978a, Lass 1980:ch. 2, Appendix).

But some guidelines are possible. Our general aim of course is to rule
out convergence where possible; but this must be done against the background of (partially, perhaps largely intuitive) judgements of 'complexity' and 'naturalness'. E.g. the more 'complex' a phenomenon is, the less likely it is to be independently repeated; the less often it occurs (in general), the less likely it is to have occurred multiply in a given dialect cluster. Most judgements of this latter type, of course, are what we might call pseudo-probabilistic, since we have no real idea of the quantitative structure of the universe we are dealing with, but only looish inductive expectations derived from our particular experience. (Note how many of these I used in the preceding discussion.) But given some (ex hypothesi) epistemic validity for our inductively based probability judgements, increasing complexity and unnaturalness will require proportionately increasing evidence to support claims for convergence.

We might sum up the types of 'improbability' we have to deal with as follows:

(a) Quantitative --- the more structure is involved in a change, the less likely convergence is. Thus converging system-wide transformations ('sound shifts' properly speaking) are to be taken as survivals where possible. 9)

(b) Qualitative --- the less 'natural' a change is in terms of cross-language distribution (taking 'naturalness' either as supported by phonetic or other kinds of enabling and explicative mechanisms, or as an uninterpreted calculus), 10) the less likely convergence is. Thus if two languages devoice medial sonorants we would not like this to be convergent.

We don't, of course, always get what we want. There are many well documented cases of parallel innovation which cannot be pushed back to protolanguage survivals. Some classics that come to mind are: (a) the English and continental West Germanic chain shifts of long vowels; 11) (b) the development of high front rounded vowels by (non-assimilatory) fronting of back vowels in French, Ancient Greek, Albanian, Scots, and various NW Italian dialects; (c) the neutralization of vowel quantity with long and short vowels in complementary distribution in North Germanic (except Danish), South German, and Scots; (d) the loss of vocalic quantity systems in East Slavic, Greek, and Romance; (e) the spread of the /i/-presents
to thematic $\nu$-verbs in Slavonic and Indic; (f) the loss of the aorist/perfect distinction in Yiddish and Afrikaans, and the beginnings of the loss in German and English, (g) the loss of the dual throughout Indo-European. Cases of post-separation loss of categories (especially where diffusion is ruled out) are particularly troublesome, since here we have evidence that the category to be lost is itself a survival. Can both a category and a 'tendency to lose it' be survivals?

Certainly linguists have resorted to claims of this kind: the existence of later-to-be-realized tendencies ('seeds of destruction') actuated only at some considerable time after separation, and thus retained as 'anti-convergent' latencies, at least from a methodological point of view (cf. Labib 1975 on the loss of vowel length in IE). But attractive as this notion may be to some (myself included), it is in our present state of knowledge merely giving a name to a mystery.

**4 Is innovation necessary? Processes vs. states**

Now to question (b) from §2. So far I have argued pretty much along 'classical' lines, i.e. the way historical linguists tend to argue, and the way I was brought up to do; I have been fairly self-consciously traditional and un-eccentric. But I now suggest that we may be dealing with a pseudo-issue, non-answers to a non-question. Let us return to our Dravidian examples: what warrant do we really have for assuming that there ever was in fact a 'process of lenition' as an innovation ('added rule' or whatever) in Dravidian? The data in actuality support nothing but the non-existence of intervocalic voiceless stops in PD itself. (Note that I do not say 'presence of lenition': see below.) The voiced stops and fricatives in forms like Kannada poge, Tamil puxai, are given by Emeneau as reflexes of PD phonetic *-k-: but on the evidence available, this is, despite its apparent plausibility, quite unjustifiable. At least its only justification is a strong, implicit, very common, and untenable a priori assumption.

This assumption, which seems to me to vitiate much reconstructive thinking and practice in historical linguistics, can be seen clearly if we attempt to reconstruct the argument leading to Emeneau's PD voiceless...
stops in non-initial positions. (I know I run the risk here of constructing a straw man, as we all do in 'rational reconstructions' of other peoples' inexplicit performances, but I think the danger is slight in this case.) The argument probably goes like this:

(a) All the Dravidian languages show some version of a set of obstruent alternations that shows up most clearly, perhaps, in Tamil.

(b) We get voiceless single stops only initially, fricatives or voiced stops intervocally, and voiced stops after nasals.

(c) This alternation must go back to an original non-alternating state in which the ancestral form (as suggested by the appearance of 'weak' forms in weak environments) is voiceless and a stop.

(d) Because (a priori assumption): the source of an alternation (universally) is the action of innovatory rules on originally non-alternating material.

The consequences of this are intolerable. The assumption (d), which is normally implicit, but on occasion actually stated, entails that any language showing any alternation at all stems from one with none, and that therefore all natural languages with morphophonemic alternations or allophony go back to protolanguages without them. (Talk about multiplying entities.) Even if this were the case, it would make such stages of languages the province of some discipline other than linguistics as we know it (Lass 1977:9-13).

But the claim that all alternations go back historically to original non-alternations merely reflects a prejudice, if one that has become something of a 'hard-core' dogma. On evidence like that presented by Dravidian, the only thing we can justifiably reconstruct is a distribution of phone types (or several distribution-types in different proto-dialects). This would perhaps best be done in terms of a polysystemic array, with three at least partially distinct obstruent subsystems. There is no warrant for assuming any kind of 'process' in PD — either in terms of a synchronic alternation, or of an innovation at some period after an original period of non-alternation. (In fact Emeneau's
Lass, 26

... comment, cited earlier, on the alternation of *-v- and *-££- ought to have prepared us for this.)

Indeed, since synchronic 'process' or 'mutation' rules are in any case merely metaphorical restatements of distributions of items, based on particular (non-empirical) theoretical assumptions, their epistemic status is at best foggy. This reflects in fact on the notion 'historical process' as well, in a rather complex way, as we will see.

Granted, a polysystemic statement can be said to lack either the 'dynamism' of a process statement, or its factitious resemblance to the proper statement of a genuine historical change. Nonetheless, it says about as much as can be safely or informatively said about situations like the Dravidian one, either historically or synchronically (perhaps an insight of prosodic phonology that we have neglected too long).

Say for instance that a language has, like Tamil, the following distribution:

(8) # C V VN

To claim that \([x]\) and \([g]\) must be 'from underlying /k/, acted on by voicing and spirantization rules' is simply to make an assertion, with as far as I can see no particular claim on anyone's credulity.

Such a claim first of all confuses a relation between items with a 'thing' (an underlier: cf. Linell 1979:ch. 12), and reifies the thing rather than stating the relation. This adds an unwarrantable ontological complication (if you take it seriously): the notion of 'things changing into other things'. This is the problem produced by the otiose move of having phonetically specified common underlying representations for alternations, and is what I referred to earlier by saying (note 8) that a notion like 'phonemically voiceless' is meaningless. If a phoneme isn't a 'thing', but a class-name for a set of alternants, then it can't be 'voiceless' (unless you're speaking in metonymy). Synchronously speaking, that is, I see no particular advantage to 'process' statements except within theories with certain...
kinds of esthetic criteria for descriptions — a purely instrumentalist or conventionalist one.

The same thing can be said, to a certain extent, in a comparative-historical perspective. If no language in a family shows non-alternation, all we can say about the alternation is that it must always have been there qua alternation. And of course 'alternation' here is not an 'observational' term, as decades of habit have led most of us to treat it, but a theoretically loaded one: all we 'observe' is a distribution. Synchronically, to assign a distribution of this kind to a 'process', with all the extra machinery and (in a 'realist' framework, anyhow) ontological complication, is in any case not to take an 'intellectual risk', produce a 'strong hypothesis', or anything with that kind of metascientific glamour. It is simply to make an invulnerable statement about either one's esthetics or one's metaphysics, since there are clearly no empirical issues involved.

One could do worse at this juncture than quoting an elegant bit of wrist-slapping by R.H. Robins (1970[1957]:196):

"It is an unsuitable metaphor to say that one sound operates at a distance over intervening sounds to exert a force on another sound, and change it from something which in fact it never was (in the words concerned) into something else. It is indeed generally desirable that synchronic description and analysis should as far as possible avoid the use, even metaphorically, of terms and concepts more appropriate to the diachronic study of the history and development of languages and linguistic features."

(Robins cites Hockett 1954:210-34 in support; and we might also mention Allen 1951).

And, oddly, the same thing holds in cases like this for historical statements of 'origin' (turning on its head the usual accusation that synchronic process phonologies illegitimately incorporate or ape historical change). That is, the weakness of the notion 'synchronic process' must cast doubt on the viability of the notion 'diachronic process' as well —- in cases where the actual change of X into Y can't be shown to have occurred. And this is manifestly the situation with the Dravi-
dian material discussed here. All 'dynamism' or 'historicity' in these cases is specious, since both history and basic reconstructive technology in fact can only tell us that a particular static distribution of item-types has always been the case.

None of this of course is to be taken as implying a simple-minded 'fact' vs. 'hypothesis' distinction, or of undervaluing hypotheses, hypothetico-deductive method, etc. Rather it suggests a reasonable limitation on the interpenetration (confusion?) of theories of synchronic structure and theories of change. In this instance, rather than the usual importation of metaphors of change into synchronic description, we have the synchronic bias toward unique underliers for sets of variants imported into history, creating pseudo-innovation. As far as theories of structure and theories of change are concerned, my own contention is that these are neither the same thing, nor particularly closely related; but this requires separate argument.

5 Appendix: a note on internal reconstruction

This paper has dealt with what is essentially a problem in internal reconstruction, since the question has been: what are we reconstructing? It was internal, because in fact all the comparative data pointed in the same direction, pretty much, and what was really going on was (implicitly) an internal reconstruction of a putative Dravidian lenition on the basis of the synchronic distribution of obstruent phones in Tamil.

Internal reconstruction (henceforth IR) is the bastard child of comparative method, and shares all of its difficulties and few if any of its virtues. I have discussed this matter in considerable detail elsewhere (Lass 1977), but a few remarks are apposite now, because of the questions raised in § 4, especially in note 14.

The basic assumption behind IR is that we can extrapolate from the strategies used for projecting ancestors of non-identical cognates in different languages to the projection of ancestors of non-identical cognates in the same language; i.e. all non-suppletive allomorphs of one morpheme are 'cognate', and hence can be used as the basis for a projective 'tri-
angulation'. This is, however, a viable procedure only under the assumption that all non-identities must go back to original identities, and I have argued that this is untenable, since it produces alternation-free languages as outputs, and hence violates uniformity principles.

If in comparative reconstruction all the members of a family show an alternation, there is no warrant for arguing from this to an original non-alternation (e.g. an ablaut-free PIE is not really warrantable, except as a necessity forced on one by an assumption). In the case of IR this means that if we are operating with a single language, no alternation can confidently be traced back to an identity without comparative or prior genetic (documentary) evidence; i.e. while comparative method succeeds by virtue of being what it claims to be ('comparative'), IR is vacuous if it is what it claims to be ('internal').

To clarify: consider the near complementary distribution of the graphs a, e in Old English: beac 'back', deag 'day' vs. dagas 'days', mann 'man'. Let us take specifically the occurrence of e before single non-nasal consonants and a before nasals (this is only part of a complex alternation pattern, but will suffice for illustration: for details, Lass & Anderson 1975: ch. II). Under the assumption that such complementations must reflect innovatory change, we reconstruct a single segment underlying the alternation in Old English, and a rule causing a (context-sensitive) split.

But we cannot argue seriously for this without invoking comparative evidence: when we do this we find that a similar alternation occurs in Old Frisian (hek vs. OEast Fris man, WFr is man), but not elsewhere in Germanic (ON bak, OEN man). This supports the original reconstruction. But what if all the Germanic dialects showed a front/back alternation here? In that case the split would have to be pushed back to PGmc or some earlier period; but even this couldn't be justified unless there were evidence for identity in some other IE dialect group. Thus in this case IR by itself tells us precisely nothing. It is only 'safe' in a comparative perspective. In which case it might --- given that we were starting from Old English or Old Frisian as a source for investigating Germanic --- furnish a useful heuristic (when you find an alternation, look for an identity somewhere else). But it is not an independent source of historical information.
Comparative reconstruction, however, precisely because of its wider perspective, is closer to independent; the perspective helps us to guard against the temptations of parochial error. So from the point of view of isolating and ordering innovations, IR is at best an ancillary heuristic; ancillary not only to comparative reconstruction, but to external history as well.

The only cases where IR is (perforce) an 'independent' technique are those where both cognate languages and documentary history are lacking. E.g. in a language isolate, we use IR faute de mieux to produce some history; in such a case, the less documentation we have, the more we need IR, and of course the less we can trust it. That is, in the cases where we need it most, we can't test it against either of the two possible sources of confirmatory or disconfirmatory material (cognate languages and a text tradition). But if the technique is not in itself reliable, we must be most skeptical about its viability in precisely those cases where it is most necessary to use it.

There is another problem with IR, which I looked at in detail in my earlier paper (Lass 1977). It is in principle impossible, in the absence of external evidence of some kind, to tell in any particular case whether an 'internal reconstruction' is in fact historical or synchronic. This is because the procedures of IR are identical to those of any other form of abstract morphophonemic or phonological analysis (a point noted as early as Bloomfield 1939, though he did not worry about it). That is, if you begin simply by trying to find unique underliers for alternating pairs or n-tuples of segments (whether phonemic or phonetic), and writing rules to generate the alternants, there is no way (other than by fiat) of localizing the source of the alternations. Are they a matter of synchronic 'structure', or of the historical origins of that structure, or just things that were always the case, and hence neither? Once again, there is no way of making such decisions within the framework of a given theory; what you set out to do tells you in the end what you've done. Whether a given procedure of reducing an alternation to underlier-plus-rules gives history, 'structure', or nothing of interest outside the theory under whose control the operation is performed is a contingent matter, undecidable without recourse to some external standard of judgement or source of information.
NOTES

1. I am grateful to Sidney Allen and Richard Coates for comments on an earlier draft; and to Ron Asher for discussion of matters Dravidian.

2. The term is borrowed from evolutionary biology. As an example, the possession of mammary glands and hair by monotremes, marsupials, and placental mammals is a survival; but the similar dentition in placental and marsupial carnivores is an instance of ('adaptive') convergence.

3. The exceptions are normally the reflexes of geminate stops in certain dialects (cf. Zvelebil 1970: ch. II and the etymological displays in Emeneau 1970). The rare cases of post-nasal voiceless stops also seem to go back to geminates (Raja 1969). I will not be concerned with these (aberrant) cases here. All these remarks are restricted of course to native lexicon.

4. Emeneau writes -k- for Tamil and Malayalam, but this is a Dravidianist convention, based on the (essentially phonemic) orthography. I replace his phonemic representation with a phonetic one, since that's what's at issue here. The form in (1) means 'smoke'/ 'tobacco'.

5. I consider both voicing and spirantization, fairly conventionally, to be forms of lenition. Cf. Lass & Anderson 1975: ch.V.

6. The best I can do for an equivalent to voreinzelsprachlich, which we ought perhaps to use as a technical term.

7. [v] is a problem, since if there are any that can properly be taken as allophones of /p/, they overlap with /v/, which occurs freely both initially and medially. There seems to be no opposition /p:/ /pp/ intervocally, but only /v:/ /pp/. See below.
8. Not 'phonemic', which in this sense is meaningless (see below). Reconstruction is in any case primarily a phonetic, not a phonological operation. Cf. Lass 1978a.

9. The two polar types are survival and genuine convergence. But somewhere in the middle is the rather intractable problem of 'diffused' change, where a change travels along a geographical gradient (e.g. the isogloss pattern resulting from the High German obstruent shift). I would tend to take diffused innovation as a separate and essentially non-convergent category.

10. On interpreted vs. uninterpreted versions of naturalness, see Lass 1980: ch. 2 and Appendix.

11. The diffusional account of the high-vowel diphthongization part of these shifts (involving spread of rules from continental West Germanic to England in the late Middle Ages or early Renaissance) is untenable on a number of grounds. Cf. Lass 1978b:120f.

12. Cf. Meillet's dictum (1921:65): 'Quand une langue se différencie en parlers distincts, celles des innovations réalisées dans chaque parler qui ne tiennent pas à des conditions propres à ce parler [whatever that means] sont ou identiques ou du moins orientées en une même direction'.

13. This idea is an old and pervasive one (though not in linguistics); it can be traced back at least to the theory of creation in St Augustine's De genesi ad litteram (esp. V, VI). Augustine proposes that God's act of creation as Logos was to endow matter with the potentiality for unfolding transformation by infusing into it rationes seminales. These are a sort of archetype with a time-fuse, that can be activated in future, but exist at any one time primarily as latencies. (This produces, incidentally, a theology that can cope with both evolution and special creation: a fact that seems to have been unaccountably neglected in the Darwinian debate).
14. The most extreme statement I know is Marchand's (1956:246): 'If one or more phonemes regularly alternate, under any conditions whatever, with one or more other phonemes in the same morpheme, these phonemes must have derived from the same phoneme or group ... if two allomorphs are cognate, they must stem from one and the same morpheme of a previous stage of the language, existing in one phonemic shape'. This is meant to apply only to morphophonemic alternation; but as I have argued elsewhere (Lass 1977: 11f) it must extend to allophony as well, since morphophonemic alternations are only contingently morphophonemic (at least in historical origin --- when they have one). That is, those with histories stem from original allophonic alternations where the relevant phonemes happen to have been in morphologically relevant positions, and later restructuring has led to phonemic contrast. Thus in Old English, i-umlaut produced \[y(:)\] < \*[u(:)] before */i(:), j/ independent of morphology; but the /y:/ in dry 'magician' < OIr *drūi is not morphologically significant, whereas that in drūge 'dry' < */drui:i-/ is, because of drūgian 'to dry', drūgoō 'drought', etc. But the origin of both /y:/ is the same: an originally allophonic rule, plus loss of environment and restructuring.

15. Cf. Linell's arguments (1979: ch. 12) against the notion of 'morpheme invariants'. It can easily be extended to 'phoneme invariants' as well, if one wishes.

16. This view was taken in fact in the 19th century by Caldwell, the pioneer of Dravidian linguistics, who assumed that the 'convertibility of surds and sonants' is a primitive feature of Dravidian (Caldwell 1875).

17. That is, there is no ontological question involved, since the notion of a 'phonetically specified common underlier' is unintelligible, as is 'mental derivation', etc. These are purely conventionalist notions with (perhaps) some descriptive advantages. It is probably better on issues like this to adopt an instrumentalist position like Cardinal Bellarmino's, rather than a 'realist'
one like Galileo's. If one wants to leave the room petulantly claiming 'eppur si muoye', that's nobody's business but one's own.
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