SURVIVAL, CONVERGENCE, INNOVATION:

A PROBLEM IN DIACHRONIC THEORY

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1   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.[1] I suggest that what seems at first a somewhat parochial problem in the theory (or, perhaps better, methodolrgy) 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.[2] 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 dis-tribution of obstruent phones of this type: single (short) voiceless stops occur only initially or in clusters with other obstruents, not intervocalically or in post-nasal position.[3] The reflexes of what are generally assumed to be P(roto)-D(ravidian) single voiceless stops in these positions are either voiced or fricative (or both) intervocalically, and voiced after nasals. As an example, consider these forms showing reflexes of reconstructed PD *[-k-] (after Emeneau 1970:20):[4]

(1)	Tamil	Malayalam	Toda	Kannada	Kodagu	Tulu	Telugu
	puxai	puxa	pax	poge	poge	puge	poga

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 etymo-
logical 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 particu-
larly 'archaic' or 'conservative' in this regard (no voiced obstruents
initially in native words, for instance). Thus for Tamib, in Dravidian
vocabulary, we get the following type of distribution:

(2)   #____   V____V   VN____V

        p-   -v-   -mb-
                           -pp-
        t-   -ð-   -nd-
                           -tt-
                           -ʈ-   -ɳɖ-
                           -ʈʈ-
        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 dis-
tinct from */C/, and prosodically of the type that makes a syllable con-
sisting 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 simi-
larity, etc. That is, the obstruent system is / p t ʈ tʃ k /, with con-
trasts / 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 Indologi-
cal symbols):

(3)   # ____   V ____ V   VN ____ V

	# ____	V ____ V	VN ____ V
	p-	-	-mp-
		-pp-	
	t-	-t-	-nt-
		-tt-	
	-	-ṭ-	-nṭ-
		-ṭṭ-	
	c-	-c-	-ñc-
		-cc-	
	k-	-k-	-ṅk-
		-kk-	

There is no evidence for medial *[-p-] :   the reason is that 'morpho-
phonemically, * -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 gene-
      ral 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 recon-
      structing *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 enti-
ties. 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 pro-
perty of being —— ceteris paribus ——— pretty much expectable'.)

Let me illustrate with a reductio. Say that in some family all the dia-
lects but one show nasalized vowels in the sequences $[\tilde{V}NC]$, $[\tilde{V}N\#]$;
and the odd one lacks these, but in cognate forms has $[\tilde{V}C]$, $[\tilde{V}\#]$.
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 com-
parative evidence, it might be interesting to see if there are any argu-
ments 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:

(4)   PD   Ta   To   Ka   Tu   Te

          *-t-   ð   θ   d   d   d

          *-t-   ḍ   ḍ~r̥   ḍ   ḍ   ḍ

          *-c-   s   s   s   dʒ   s

          *-k-   x   x   g   g   g

Toda is the one dialect in which at least some reflexes of all PD catego-
ries 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 accept-
able, 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 con-
vergent 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. $t > d > ð > \theta$

       b. $t > \theta > ð$; $t > d > ð$

That is, while spirantization of an intervocalic stop directly to a voice-
less fricative is not surprising (it occurs for instance as an allegro
rule in many dialects of English, e.g. my own as in $[p^h\iota x\iota\eta]$ '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 $[d_{\small 3}]$, let's say (or an earlier $[\textff]$, if */c/
was a stop and not an affricate) is difficult. Similarly, if $[d_{\small 3}]$ 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 deve-
lopment 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

           p̲a̲x̲   p̲o̲g̲e̲   'smoke'

           mi s̲x̲-   m̲i̲s̲u̲k̲u̲   'move'

           pɩθ̲x̲-   h̲i̲d̲u̲k̲u̲   'squeeze'

Just about all of the Toda voiceless fricatives can be explained in a
natural way as syllable-final or pre-obstruent devoicings --- 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 $[c] > [d_3] > [_3]$, with $[_3] > [\int]$ by devoicing, and then
$[\int] > [s]$. For the velars, for instance, we could take Toda $[x]$ in _pax_
and Tamil $[x]$ in _puxai_ as just possibly convergent, approaching the
same result by different pathways:

(7)   Toda :   VkV > VgV > V$_\gamma$V > V$_\gamma$ > Vx
        Tamil: VkV > 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
istelf --- 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 empi-
rical argument, depends in 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 back-
ground 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 independantly 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
loosish 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 proba-
bility 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 explica-
tive 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 documen-
ted cases of parallel innovation which cannot be pushed back to protolan-
guage 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 sys-
tems in East Slavic, Greek, and Romance; (e) the spread of the mi-presents

to thematic ō-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).[12]   But attrac-
tive as this notion may be to some (myself included), it is in our pre-
sent state of knowledge merely giving a name to a mystery.[13]

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 it-
self.*   (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 unjustifia-
ble.   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 think-
ing 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 construc-
ting 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 intervocalically, 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 appear-
      ance 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 original-
      ly non-alternating material.

The consequences of this are intolerable. The assumption (d), which is
normally implicit, but on occasion actually stated,[14] 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 multi-
plying 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 distribu-
tion 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

comment, cited earlier, on the alternation of *-v- and *-pp- 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 'dyna-
mism' 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 distri-
bution:

(8)   #____   ____C   V___V   VN___V

              k   k   x   g

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 ontologi-
cal 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 repre-
sentations for alternations,[15] and is what I referred to earlier by
saying (note 8) that a notion like 'phonemically voiceless' is meaning-
less. 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). Synchronically speaking, that is, I see no particular advan-
tage to 'process' statements except ---   within theories with certain

kinds of esthetic criteria for descriptions --- a purely instrumenta-
list 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-alterna-
tion, all we can say about the alternation is that it must always have
been there qua alternation.[16]   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' frame-
work, 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 invul-
nerable statement about either one's esthetics or one's metaphysics,
since there are clearly no empirical issues involved.[17]

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 gene-
> rally desirable that synchronic description and
> analysis should as far as possible avoid the use,
> even metaphorically, of terms and concepts more ap-
> propriate 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 men-
tion 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 histo-
rical 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 impor-
tation 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 struc-
ture 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 compara-
tive 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 ques-
tions raised in §4, especially in note 14.

The basic assumption behind IR is that we can extrapolate from the stra-
tegies used for projecting ancestors of non-identical cognates in diffe-
rent 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, æ in Old English: bæ c 'back', dæg :day' vs. dagas 'days', mann 'man'. Let us take specifically the occurrence of æ 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 (bek vs. OEast Fris mon, WFris man), but not elsewhere in Germanic (ON bak, OEN mann). 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 per-
spective, 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 heuris-
tic; 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 <u>faute de mieux</u> 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 pos-
sible sources of confirmatory or disconfirmatory material (cognate lan-
guages 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 <u>fiat</u>) of localizing the source of the alternations. Are they a mat-
ter 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 frame-
work 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 inte-
rest 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 cer-
    tain dialects (cf. Zvelebil 1970: ch. II and the etymological dis-
    plays 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 Dra-
    vidianist convention, based on the (essentially phonemic) ortho-
    graphy. 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 opposi-
    tion /p/:/pp/ intervocalically, 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 phono-
    logical 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)
    in 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 realisées dans chaque
    parler qui ne tiennent pas à des conditions propres a 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 pro-
    poses 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 morpho-
phonemic 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 *druī is not morphologically significant,
whereas that in dryge 'dry' $<$ */dru:ɣ i-/ is, because of drugian
'to dry', drugoð 'drought', etc.   But the origin of both /y:/
is the same: an originally allophonic rule, plus loss of environ-
ment and restructuring.

15.   Cf. Linell's arguments (1979: ch. 12) against the notion of 'mor-
pheme invariants'.   It can easily be extended to 'phoneme inva-
riants' 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 'converti-
bility 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 unintel-
ligible, as is 'mental derivation', etc. These are purely con-
ventionalist notions with (perhaps) some descriptive advantages.
It is probably better on issues like this to adopt an instrumenta-
list position like Cardinal Bellarmino's, rather than a 'realist'

one like Galileo's. If one wants to leave the room petulantly
claiming 'eppur si muove', that's nobody's business but one's
own.

Lass,   35

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