

# LINGUISTICS

## ON THE PHONETIC SPECIFICATION OF OLD ENGLISH /r/\*

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This is a small investigation in the framework of a larger problem: the assignment of 'phonetic values' to orthographical symbols in the texts of 'dead' languages.<sup>1</sup> I will not go into the general problem in any detail, aside from some remarks on method; but the issues raised by the particular case have general consequences.

Let us begin with a definition of 'phonetic reconstruction'. Obviously this does not mean recovery of fine detail: no historical linguist is required to produce representations like 'Proto-Germanic \*[ǫ]'. But the opposite extreme, totally 'formulaic' reconstruction, is equally undesirable: if one is supererogatory, the other is irresponsible.<sup>2</sup> By 'formulaic' I mean reconstructions that make no claims beyond providing schematics for sets of paradigmatic relations,<sup>3</sup> i.e.

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\* I am grateful to John Anderson for destroying some of the worse parts of this paper. Those that remain are my own.

<sup>1</sup> Or more generally, assigning phonetic values to any reconstructed entities. The problems are pretty much the same whether we have orthographies or not; though an orthography gives us a kind of notional skeleton to hang conclusions on. This may be misleading, of course, if e.g. spelling conventions in a dead language conflict with those of the investigator's own, where both use the same alphabet. But this is not relevant to this investigation.

<sup>2</sup> Obviously reconstructions of fine detail are simply vacuous: they are impregnable statements on matters nobody can know anything about (cf. Lass and Anderson 1975: 210 - 14). Purely 'algebraic' reconstructions, on the other hand, can at least have heuristic value — often of the most important kind: cf. Saussure's prolegomena to laryngeal theory (1887).

<sup>3</sup> Formulaic reconstructions are anti-ontological in the sense typified by Meillet (1964: 42): 'les "restitutions" ne sont que les signes par lesquels on exprime en abrégé les correspondances'. See the rest of this chapter, and the discussion in Lass (forthcoming).

'abstract' representations with no phonetic content (or at least none that is taken seriously).

I take it as axiomatic that in reconstructing a system we should not be satisfied with merely saying that two phonemes \*/X/ and \*/Y/ were in opposition; we are required to make some proposals (based on whatever information or argument we can use) about the defining features. Often — perhaps usually — we do this implicitly in our choice of symbols. If we reconstruct an opposition \*/p/: \*/b/ for some stage of a language, we normally imply (at least) that the categorial representations include labiality, obstruency and noncontinuancy in both members, and an opposition of voice vs. voicelessness (or 'fortisness' vs. 'lenisness' or whatever). We might not be surprised if evidence turned up to suggest that what we thought was /p/ vs. /b/ was more likely /ph/ vs. /p/; but we would fight to the death any suggestion that it was /i/ vs. /e/.<sup>4</sup>

Any proper phonological reconstruction is in fact (categorially anyhow) really phonetic. We are committed to attempting to recover at least the basic distinctive specifications of protosegments, as well as certain nondistinctive but systematically important material. As we will see, it is not necessarily the case that we can do this: with OE /s/ there is actually too much evidence, much of it pointing in contradictory directions. This is particularly true of one piece of morphophonemic evidence, which has often (and rightly) been taken to be crucial. But this conflict — and this is the methodological point I want to make — does not surface unless we attempt a reasonably detailed phonetic specification (beyond mere distinctive features), and examine our argumentative strategies with some care. The moral is, as might be expected, that ignorance is bliss; but whether 'tis folly to be wise or not depends on one's attitude toward historical knowledge and the foundations of the craft. As far as I am concerned, at least, a known indeterminacy (even a hopelessly intractable one) is better than an imaginary certitude.

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Let us accept then that the goal of a phonetic reconstruction in the sense in which I will be using the term is the achievement of a categorial phonological/phonetic representation, to some degree of exactitude specifiable by a feature theory (say an articulatory one like that of Chomsky and Halle 1968). I. e., something like a 'broad' transcription within the framework of a univer-

<sup>4</sup> On purely 'abstract' grounds there would be no reason for this reaction: see Kiparsky's arguments against 'purely abstract' morphophonemics (1968). These hold *a fortiori* for historical reconstruction, where the symbols we operate with must represent something fairly close to phonetic output (Lass, forthcoming).

sal phonetic theory, such that the symbols used are not abstract counters but have some (however coarsely defined) phonetic content. We might set ourselves goals like the specification of vowel position in terms of (at least) four heights and three degrees of backness (this goes beyond Chomsky and Halle but is clearly minimal: cf. Lass 1976: ch. 4). And for consonants we would want a set of articulatory parameters specifying source and direction of air-stream, a set of positional categories from labial to glottal, a manner scale (stop to approximant), and a glottal state scale (voiced vs. voiceless minimally, probably also breathy voice, creak, etc.). Anything less than this will be defective; anything more will be overprecise.

There are two main sources for the information we need: one, which we might call 'direct' testimony (i.e. orthoëpic and similar descriptions) will not be relevant here (and is often at best only suggestive). The other source is argumentative: and there seem to be three main argument types commonly employed. I will call these (i) the Genetic Argument, (ii) the Comparative Argument, and (iii) the Phonetic Effect Argument (there are some overlaps in practice, but in general the strategies are reasonably distinct).

(i) The Genetic Argument. Here we take into account all the available antecedent material within a given language (and as a second stage, its cognates). The clearest case will be where we have an unknown segment /X/ which merges with the reflex of a known (i.e. characterized) segment /Y/, where the reflex /Z/ of /Y/ derives from /Y/ by some uncontroversially 'natural' process. In this case we argue from the naturalness of the change /Y/ → /Z/ to a value [Z] for /X/ (for an example cf. § 3 below). Under this heading we can also argue from the subsequent history of /X/ in the language in question, i.e., what sound changes it undergoes, what 'natural class' groupings it participates in, etc. This line of argument will appear under other headings as well. The direct genetic strategy is in general the least important of the three, but it does have its uses.

(ii) The Comparative Argument. Here we start from comparative method as 'extrapolation' in the usual sense. The clearest cases will be where some protosegment<sup>5</sup> has come down with a limited number of fairly similar reflexes in a given family. Let us take Proto-Germanic \*/t/ as an example, and its reflexes in three Germanic dialects: English RP, Standard North German, and Standard Swedish. In initial position before a stressed vowel we find [th] in RP, [ts] in German, and [tʰ] in Swedish (*tongue, Zunge, tunga*); intervocally we find RP [t], German [s], and Swedish [t] (*water, Wasser, vatten*: the phonetic [t:] in this form in Swedish is synchronically predictable and thus irrelevant); and finally RP [t] ~ [th], German [s], Swedish [tʰ] (*foot, Fuß, fot*). All the re-

<sup>5</sup> 'Protosegment' here means any ancestral segment (regardless of distance in time) whose value is unknown and must be reconstructed. Thus 19th-century English \*/t/ is a protosegment with respect to today's /t/.

flexes share the specifications [+anterior, +coronal, -voice], and we can assign these to the protosegment. Considering the larger picture, we can assign German [ts], [s] to a well known innovation (the *zweite Lautverschiebung*) and reconstruct [-continuant]. Further, there is no reason not to reconstruct (allophonic) aspiration (which might be relevant to the High German shift); but this may be further than we want to go.

But can we opt either for an alveolar \*/t/ or a dental \*/t̪/? On the evidence of these dialects alone we might suggest an alveolar on the grounds of majority presupposing likelihood (and [s] is also alveolar); but if we invoke more dialects this becomes uncertain (cf. the wide distribution of dental /t/ in English). So all we can reconstruct on comparative evidence — for any period of Germanic — is a voiceless coronal stop (dental or alveolar, apical or laminal, etc.) Note by the way the principle that increase of data does not guarantee increase of certainty: the opposite is probably more often the case (ignorance is bliss again).

(iii) The Phonetic Effect Argument. This is rather tricky, since it depends in principle on there being at least some (phonetic) aspects of the phonology of the language in question that are fairly well established (in the rather *Pickwickian* sense in which historical linguists sometimes use the term, 'known'). That is, we argue from the (relatively) known to the unknown, according to specific effects that the unknown segments have on the known, which resemble effects of other known segments.

An example would be the argument we could construct for the short low vowel represented in Old English by the graph *a*.<sup>6</sup> (This will be relevant to the problem of OE /r/, as we will see.) Was it a front [a] or a back [ɑ]?<sup>7</sup> The 'solution' proceeds this way:

- (i) Old English had two segments, written *a* and *æ*, descending from Proto-WG-\*/a/.<sup>8</sup> These were generally in complementary distribution: *æ* in closed syllables before single nonnasal consonants (*dæg* 'day'), and in open syllables if the next syllable contained a front vowel (*æðele* 'noble'); otherwise *a* before geminates (*catt* 'cat'), nasals (*hand* 'hand') and in open syllables if the following syllable contained a back vowel (*nacod* 'naked').
- (ii) For various reasons (not relevant here) we are fairly sure that *æ* represented a front vowel of the [æ] type, and that *i*, *e* represented front vowels that grouped with *æ*. Further, it seems clear that *u*, *o* represented vowels belonging to a different class (on historical grounds back), and that these

<sup>6</sup> This argument is not usually given explicitly in the literature; but it is certainly implicit in many discussions. For some comments see Lass (1976: ch. 4).

<sup>7</sup> Obviously it could also have been central [ɛ]; but the historical identification of central vowels is very tricky, and I will avoid the issue. Perhaps a better opposition would be front vs. nonfront, or back vs. nonback.

<sup>8</sup> The starred 'a' is an uncommitted symbol: it means simply a low vowel at this point.

facts (or 'facts') are crucial for characterizing *a*. The rest of the argument runs as follows:

(iii) There are *a* ~ *æ* alternations where we get *æ* before *e* and *a* before *u*: thus in the paradigm of *dæg* we find gen. sg. *dæges*, dat. sg. *dæge*, but dat. pl. *dagum*.

(iv) In this paradigm and other similar ones we also find *a* before *a*: nom.-acc. pl. *dagas*, gen. pl. *daga*. Therefore *a* classes with *u*. And since (non-morphologically) we get *a* before *o* as well (*nacod*), *a* also classes with *o*.

(v) If a specification [-back] on *u*, *o* produces *a* instead of *æ*; and if *a* also produces *a*, then *a* is also [+back]. We have a simple metaphony, such that accented low vowels have the same backness value as following unaccented vowels.<sup>9</sup>

This argument is not only from phonetic effect, but from class behaviour: since *a* produces the same effect as *u*, *o* it shares some crucial specification with them. The logical structure of this strategy may be schematized this way:

- (i) There is a segment  $S_1$  whose characterization is known;
- (ii) There is a segment  $S_j$  whose characterization is unknown;
- (iii) There is a phonetic effect  $E$  produced on  $S_1$  by  $S_j$ ;
- (iv) The effect  $E$  is typically produced on segments of the (general) type  $S_i$  by some feature or feature-complex  $F$ ;
- (v) Therefore:  $F(S_j)$ .

We shall see later what the limitations of this strategy are.

## 3

The characterization of segments written with *r* in Latin- or Greek-based scripts is always a problem.<sup>10</sup> This is due mainly to the very wide range of phone types written with *r*, even in closely related languages or dialects of the same language, and to the often ambiguous phonetic/phonological status of /r/ in many languages (e.g. in much Scots there is Auslautverhärtung for obstruents and /r/, but not for nasals and /l/: cf. Lass 1974).

The phonetic range problem is more serious. For instance, there is very little difficulty, knowing what we do of earlier and present-day languages and their orthographic traditions, in assigning a value to an *m* in an early (Latin-derived) text: it is a labial nasal, overwhelmingly (except perhaps in certain

<sup>9</sup> This is a bit oversimplified, but the outlines are accurate. For a detailed discussion see Lass and Anderson (1975: ch. II).

<sup>10</sup> More accurately, segments written with graphs normally transliterated as *r*; e.g. Greek ρ, corresponding graphs in Cyrillic, Carolingian Miniscule, Insular hands, etc. The identification as 'r' is usually based on (*inter alia*) relation to the Latin or Greek alphabet, historical source (e.g. IE \*/r/), later developments, and systemic function.

sandhi positions) a bilabial. An *n* is more difficult: it might be dental or alveolar, or (before the appropriate consonants) palatal, velar, or even retroflex. But *r* is much worse: given only a sample of the values represented in modern West Germanic dialects, the reasonable candidates — in the absence of special evidence — would be at least the set [r ʀ ʁ ʎ ʁ R]. In fact all of these (except perhaps [R]) and more can be found as dialectal norms in English, which poses special problems for us here.

I now take up the central question: what was Old English /r/? (I will use the representation ‘/r/’ since there is no doubt of the position of this segment in Old English phonology: it is a phoneme by any definition, and functionally the nonlateral congener of /l/). I will begin by looking at some typical handbook descriptions, and then attempt to show what kinds of arguments are used, and where they fail. Here are some typical ones:

- (i) /r/ was a “dental liquid” (Campbell 1959:§ 50).
- (ii) “*r* was strongly trilled with the tip of the tongue” (Moore and Knott 1955: 14).
- (iii) “*r* was trilled in all positions as in Modern Scotch ... In West Saxon, and Kentish, and parts of ... Mercia, it was reverted ... which accounts for breaking taking place before *r*+consonant more regularly in WS. Ken. than in Anglian ...” (Wright and Wright 1925:§ 7).
- (iv) “*r* avait une prononciation qui nous est mal connue ... devant une voyelle un *r* lingual roulé comme en italien, ou peut-être déjà un *r* spirant comme dans l’angl. mod. *red* ... devant une consonne et en position finale, un *r* rétroflexe comme dans certaines dialectes de l’anglais moderne” (Mossé 1945:§ 14).
- (v) “*r* initially was probably trilled, as in Scots ... Finally and before a consonant it was probably made with the tip of the tongue curved back, as in south-western dialects of Mn.E. and in American ...” (Sweet 1957:3).
- (vi) “OE *r* may well have been strongly trilled as in Mod. Scots, but the same symbol was used for the fricative (‘burred’) sound in some positions, notably before consonants and finally ...” (Quirk and Wrenn 1955: 15).
- (vii) “Das *r* des Ae. war wahrscheinlich cerebral, d. h. wurde mit stark zurückgebogenen Zungenspitze gesprochen, wie z. T. noch heute im Englischen. Nur so erklärt sich nämlich phonetisch die Brechung vor *r* ...” (Brunner 1965: 146).
- (viii) “Auf präpalatale (retroflexe) Artikulation schließen wir für das ae. /r/” (Pilch 1970: 55).

This would make an interesting study in the transmission of scholarly traditions. The dates of the sources I used, many of which are later editions of early works, are a bit misleading: it is clear that the two notions (a) that /r/ was tril-

led in at least some positions and (b) retroflex in others, descend from Sweet.

There are thus two questions to be answered: (a) was there only one phonetic value for OE /r/, or were there two? and (b) in either case, can the value(s) be established? As far as I can tell, the two-value analyses (e.g. Sweet, Wright, Mossé) are based on two assumptions, only one of which is tenable:

(i) [r] is in some sense a ‘normal’ value for *r*;

(ii) breaking is crucial for determining at least one value of OE /r/.

Assumption (i) seems to be without support: certainly lingual trills (as opposed to fricatives, taps, or approximants) are not typical for English (even Scots), nor for Germanic in general (except in special uses, emphasis, the stage, or singing: e.g. German speakers with [R] or [ʁ] sing classical music and read poetry with [r]). It is true that in many Germanic dialects there is a difference between initial and postvocalic /r/ (to get on to the problem of two values), but this is not normally a difference in place of articulation. Typically we get either presence vs. absence (as in non-rhotic dialects of English), or strong (consonantal) vs. weak (vocalic) reflexes, as in Standard German, Danish, and many dialects of Swedish.

But certainly we do not get alveolar vs. retroflex, or (in general) anterior vs. nonanterior, etc. In fact there seems to be no reason at all for suggesting a difference in phonetic value for the two environments: it is rather the case that in OE initial /r/ is non-evidential, since it doesn’t do anything, while postvocalic /r/ can be evidential because it has effects. This is not the same as arguing for two different kinds of /r/ (phonemic or allophonic): it is simply that one environment is richer in evidence than the other. In this case Occam’s Razor alone precludes any ‘two-value’ analysis.

But this does mean, even more strongly, that assumption (ii) is well taken: if we are to find out anything at all about OE /r/ it will probably be from breaking (plus perhaps something from genetic argument; comparative argument is difficult, as we will see in § 5). At any rate, the assumption of a single value squares with the available evidence, and we can aim for establishing this.

First, let us consider the possible genetic argument. Since e.g. Proto-Gmc \*/r/ is as much of an unknown as Proto-IE \*/r/ or our target, OE /r/, there is nothing to be gained here. But there is one source of genetic information: the development of some OE /r/ from Proto-Gmc \*/s/ via Verner’s Law and rhotacism. There is no reason to believe that Proto-Gmc \*/s/ was anything but an [s], i.e., [+ant, +cor, +cont, +strid]; and since the /r/ from \*/s/ (e.g. in *curon* ‘they chose’ <\*/kuzún-/ <\*/kusún-/) is not distinct from original /r/ (in OE: it is apparently briefly distinct in Runic Old Norse, where the reflex of \*/z/ < \*/s/ has a special representation), we can argue that original /r/ must also be at least [+ant, +cor]. That is, since \*/z/ < \*/s/ passed to /r/, the simplest assumption is that there was a minimal feature change, [+obs] to [–obs] (cf. Lass and Anderson 1975: ch. V). And if rhotacism is simply the lenition

of \*/z/, the most reasonable value for /r/ would be a voiced coronal approximant, i.e., something like [ʀ] (I assume that [ʀ] can be taken as a 'weakened [z]').

So the only (antecedent) genetic argument leads to a coronal. Before we consider the breaking arguments, however, there are a few (later) phenomena in the history of Old English that also point in this direction. They are:

(i) Late OE lengthening. Short vowels lengthened before clusters of nasal plus homorganic voiced stop, /ld/, and /r/ plus [d l n ð]. If this is basically a homorganic rule, as the nasals and /l/ would suggest, then the /rC/ environments should be homorganic too: it would be odd for a rule to operate before [mb nd ŋg], before [ld], and before (e.g.) [k d k l k n], etc. (For details see Campbell 1959: § 283).

(ii) Assimilations in /Cr/, /rC/ clusters. In a series of sporadic changes (Campbell 1959: § 484), clusters of /r/ and another (preceding or following) consonant become /rr/ or /CC/. The clusters involved (in graphic representation) are: *hr* > *rr*, *lr* > *rr*, *nr* > *rr*, *rp* > *rr*, *lr* > *ll*, *sr* > *ss*. If *hr* is not phonemic /xr/ but a sequence of voiceless and voiced /r/ (cf. Icelandic *hr*, *hn*), then all of these clusters involve /r/ plus a coronal. If at least all cases of /rr/ from /Cr/, /rC/ are cases where /C/ is coronal, then this suggests a value of [+cor] for /r/; certainly a set of fusions of this kind involving [nk kθ], etc. seems rather odd.

## 4

But the central argument is clearly that from the sound change known as 'breaking'. There is a good deal of controversy about just what this was, but I think a good case can be made for the traditional view that it was a diphthongization, specifically epenthesis of a back vowel between front vowels and certain consonants or clusters (cf. the discussion of the literature in Lass and Anderson 1975: ch. III).

Breaking shows up typically in alternations of single-graph and digraph spellings in certain environments: e. g. *æ* ~ *ea*, *e* ~ *eo* (and sometimes *i*, *e* ~ *io*). The environments are basically as follows:

- (i) Before *h* (= /x/): *beran* 'bear' vs. *eoh* 'yew' (< \**bera-*, \**ex-*)
- (ii) Before *rC*: *bær* 'he bore' vs. *wearp* 'he threw' (< \**bar*, \**warp*)
- (iii) Before *lC*: *belig* 'belly' vs. *healp* 'he helped' (< \**balj-*, \**halp*)

The traditional explanation and description of this process can be represented by this passage from Lehnert (1965: 51–2):

Zwischen den kurzen palatalen Vokalen *æ*, *e*, *i*... und folgenden gedeckten velaren Konsonanten *χ*... *r*, *l* oder einfachem *h*... schob sich ur-ae. ein Überlangslaut *u* ein, so daß die Kurz-diphthonge *æu*, *eu*, *iu* entstanden...

Descriptions like this can be multiplied from the literature. But there is an obvious problem: why is the set /r l x/ 'velar' or 'back'<sup>11</sup>? This does not square, it would seem, with the evidence for the coronality of /r/ cited above (nor with the belief in coronality apparently held by many scholars who nonetheless take /r/ to be 'back').

John Anderson and I (Lass and Anderson 1975: ch. III) constructed a rather elaborate explanation for this grouping of /r l x/; and, more to the point, we also proposed a new and contentious characterization of OE /r/. This was based almost entirely on a phonetic effect argument, though it did draw to some extent on comparative material as well. I will restate the argument briefly here, as I propose to criticize it in the following section.

Basically we argued that if (a) breaking before /r l/ and breaking before /x/ are 'the same process'; and if (b) /x/ is clearly a velar fricative; then there must be some specific phonetic feature common to /r l/ and a velar, within a feature-framework such as the generally reasonable Chomsky and Halle one which we used. Given that in this framework, /x/ is [+obs, –ant, –cor, +high, +back, +cont, –voice], the requisite specification must lie in this set. Old English phonology in general seems to allow us to rule out everything except [+back], since other segments possessing these specifications in varying combinations do not cause breaking. And the choice of backness (aside from mirroring the vague traditional intuition that this is what counts) is also at least (weakly) 'explanatory', since a back consonant is *a priori* likely to have some interesting effect on a front vowel transition.

Given what we know of present-day dialects of English, our argument continues, /l/ is no problem: at least in the relevant environments it was a velarized [ɫ].

The next stage is to find some 'typical' back *r*, preferably one widely attested in Germanic, and specifically in English. And here again the answer is obvious: a uvular, either [R] or [ʁ]. At least in Northern England and Scotland there is a reasonable distribution of [ʁ] or some other uvular continuant as a reflex of OE /r/ — Northumberland and Durham mainly, with pockets in the West of Scotland, and Aberdeen. In Germanic at large, some form of uvular for Proto-Gmc \*/r/ occurs in every dialect except Icelandic.

Therefore, we concluded, OE /r/ was uvular (whether fricative or approxi-

<sup>11</sup> Further evidence for backness as crucial comes from the (related) process called 'velar umlaut', where in some dialects *ea*, *eo* etc. spellings appear before following back vowels: e.g. *feola* 'many', *sioððan* 'after', *heofon* 'heaven', *ealu* 'ale', etc. If *u*, *o*, *a* are back, then with the addition of /x/ we have a set of four 'known' back segments involved (cf. Lass and Anderson 1975: ch. III, § 7).

mant or trill is beyond recovery); and thus not only do the breaking environments form a natural class (all [+back]), but so do the breaking and velar umlaut environments (/u o a  $\text{K}$   $\text{t}$  x/).<sup>12</sup>

## 5

This account, though we liked it at the time, is not convincing. There are, first of all, some historical difficulties which make the argument from geographical distribution rather tenuous, and cast doubts on the notion of 'survival' of uvular *r* that underlies the (*post facto*) genetic part of our argument. For one thing, the main area where we now get [ɣ] < OE /r/ — the North-east of England — is precisely that area where the 'naturalness' of the OE /r l x/ grouping is most suspect. In Old Northumbrian (cf. Campbell 1959: §§ 143 ff.) it is in fact the case that breaking of [æ] < \*/a/ was regular only before /x/; before /r l/ the result was retraction, not breaking. Now certainly retraction is expectable in a 'back' environment; but if this is indeed the common factor, it is strange that the one known back segment /x/ — from which the argument to /r l/ was extended — should not behave like the other two.

In fact, what we have is a problem in OE vs. NE distribution: the most regular breaking occurred in West Saxon and Kentish, not in Anglian, yet all the dialects that now have uvular *r* are of Anglian (specifically Northumbrian) origin. The West Saxon derived dialects (those of the South-west and SWML) typically show retroflex reflexes of OE /r/, a fact which will later prove to be a red herring (§ 7 below).

Further, the same part of England where we get uvulars (especially Northumberland) is one of the few areas outside of Ireland and the West of Scotland where we find postvocalic and preconsonantal *clear* /l/ in Modern English; thus the very 'relic' property of uvular *r* that we used as an argument for a uvular in Old English tells against OE [ɣ] in the same environment. And in Germanic at large, *clear* /l/ — even palatalized, as in North German — is at least as widespread as uvular *r*.

So at the very least any 'continuity' arguments for an OE uvular are dubious; the 'naturalness' of the /r l x/ grouping has to be sought somewhere other than in a uvular articulation for /r/. The problem now is that while a

<sup>12</sup> There is a further (weak) argument based on the unique behaviour of /r/ in the West Germanic Geminatio. If /r/ is a uvular, it is not only [+back], but according to our interpretation of uvulars also [+low] (Lass and Anderson 1975: 'Preliminaries'). If this is so, it is the only [+low] consonant in the OE inventory, which suggests a reason for its failure to geminate after a short vowel and before the [+high] \*/j/ that otherwise causes gemination (e.g. *fremman* < \*/framjan vs. *herian* < \*/harjan: cf. our Appendix II).

value [ɣ] for OE /r/ is a sufficient condition for the observed class grouping, it is not a necessary one. There are other possibilities, with similar supporting arguments.

## 6

In our account of OE /r/, Anderson and I decided on uvular articulation because we assumed that if OE /r/ was back, it had to be uvular. We failed to see the value of a suggestion made by Richard Hogg (1971) that OE /r/ was — at least in the relevant environments — a velarized [ɣ]. Such a segment would of course group naturally with the [ɣ] that almost everybody agrees was the value of OE /l/, at least preconsonantly. Our problem was that while Hogg's arguments were very tempting, they were entirely historical and 'phonological'; we had no evidence of the existence of velarized /r/ in any modern dialect, and Hogg's suggestion seemed to be an ad hoc device, less plausible than uvular articulation (Hogg in fact mentions our uvular proposal in a note — 1971: 64 — and points out that this value would be consonant with his own analysis of breaking.)

But it turns out that many dialects of modern English have just the kind of /r/ that Hogg proposed for OE: though this is not very well known, and is as far as I know only described in an important and little known paper by Elizabeth Uldall (1958). In this paper, Uldall points out the existence in a number of American dialects of what she calls a 'molar *r*': an approximant of the general [ɣ] type, produced with the sides of the tongue raised toward the upper molars.<sup>13</sup> This is effectively, in fact, a 'velarized *r*' or even a 'velar *r*' — at least as velar as the usual type of dark *l*.

Could OE have had a molar *r*? If it did, then it would be exactly equivalent to [ɣ]: the class involved in breaking would be [x  $\neq$  ɣ], which would not only share the specification [+back] as [x  $\text{K}$  ɣ] did, but would all be [+high] as well. And from a genetic point of view, we would still have a widespread present-day segment type that meets the articulatory specifications necessary for natural-class grouping with [x ɣ]. The structural description of the breaking rule would actually be the same regardless of whether /r/ was uvular or molar ([+high] is redundant for all phonemic consonants in OE — at least in our analysis — since there are no underlying palatals). Thus the breaking rule

<sup>13</sup> As an example of the usefulness of reading, I was unaware until I read Uldall's paper that my own /r/ is of this type. A further 'naturalness' arguments for (at least) preconsonantal and postvocalic velarization of /r/ comes from another observation of Elizabeth Uldall (personal communication): in many dialects of English not only /l/ but all coronals are velarized in these positions, e.g. in *cease* [si: s], etc. (Though by habit we do not either talk about or listen for 'dark *s*', 'dark *d*', etc.).

that we formulated (Lass and Anderson 1975: 90, 104-5) is neutral with respect to any positional feature assigned to /r/ except [+back]; and it clearly doesn't matter whether this is a primary or secondary articulation. The skeleton of the rule would in any case still be<sup>14</sup>:

$$\emptyset \rightarrow \left[ \begin{array}{c} \text{V} \\ +\text{back} \end{array} \right] / \left[ \begin{array}{c} \text{V} \\ -\text{back} \end{array} \right] - \left[ \begin{array}{c} +\text{cons} \\ +\text{back} \\ -\text{cont} \\ <-\text{obs}> \end{array} \right] < +\text{cons} >$$

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But there is still another possibility which has been traditionally invoked, as we saw earlier: that OE /r/ was retroflex in the breaking environments (Sweet, Mossé, Quirk and Wrenn) or everywhere (the Wrights, Brunner, Pilch). We can reject a two-value analysis on grounds of simplicity (e. g. [r] ~ [ʀ]); but what about a retroflex in all positions?

This hinges on whether diphthongization is a likely effect for a retroflex consonant to have on a preceding front vowel. According to the detailed survey by Bhat (1973), this seems unlikely. The most characteristic effects of retroflex consonants on vowels are retroflexion and centralization (Tamil, Armenian and others); in only one case (Tulu) do front vowels become back (and then only if a labial precedes). In Tamil, palatalization of Proto-Dravidian\*/k/ before front vowels is blocked if a retroflex follows (which suggests centralization). And to Bhat's data we can add the effect of retroflex consonants in Swedish, which is lowering of preceding mid front vowels (cf. Eliasson 1975). Overall, then, the best documented effects of retroflex consonants do not include diphthongization: Brunner's claim that a retroflex /r/ is necessary for OE because "Nur so erklärt sich ... phonetisch die Brechung vor r" cannot be accepted.

If there were sufficient attestation of retroflexes causing diphthongization, it would be possible to group them with velars in a fairly natural way — though not on articulatory grounds. That is, acoustically they are 'low tonality' segments (in Jakobsonian features [+flat]); transitions to them show a characteristic drop in the second and third formants. And velars are also low tonality, though in a slightly different way ([+grave]); in fact they are known to have the effect of retroflexing preceding nonretroflex consonants (as in the famous

<sup>14</sup> This is a simplified version of the final rule, which incorporates 'velar umlaut' as well (104-5). But the main features of the structural description are the same, and the preliminary version is clear. The specifications in angled brackets account for the fact that /r l/ require a following consonant to cause breaking, but /x/ doesn't.

'ruki' phenomena in Sanskrit and elsewhere). So there is some case to be made for retroflex/velar as a 'natural' grouping, but not, unfortunately, with respect to diphthongization (whereas velars are implicated in this in English: cf. Lass 1976 :ch. 7). Finally, it seems unlikely that low tonality *per se* is involved in the OE breaking, since labials (also [+grave]) do not cause it, and in some cases even inhibit it (cf. the common failures in *melcan*, *delfan*, *self*: Lass and Anderson 1975: 92).

I conclude that breaking was articulatorily motivated, not an acoustic or perceptual change. And if this is so, then there is a fairly strong argument against a retroflex OE /r/, which would cause us to invoke acoustic factors. Since the articulatory explanation works perfectly well for /l x/, it would be supererogatory to introduce a different motivation to defend a (not well supported) retroflex /r/: and this would get us back to the undesirable notion that there were 'two kinds' of breaking.

We are left then with two possibilities: uvular or molar. Is there any way of choosing? If both the prior-genetic and comparative (current distribution) arguments are weak (the latter weaker), and we can produce only sufficient conditions and no necessary ones, the answer is no. It appears that no matter what argumentative strategies we bring to bear on the question, we are faced with two possible values, and no way of deciding between them. The only possible criterion might be 'degree of class-cohesion': if /r/ were molar, then the breaking environments would all have two specifications in common ([+back, +high]) rather than just one ([+back]). But the gemination argument (cf. note 12) might weight against this.

I would claim that no matter how refined our arguments may get, we are faced with an insoluble indeterminacy. And further, that every time we come across this kind of situation we will be faced with the same problem. That is, whenever we have a phonetic effect that can be due to two or more segment types; and when in addition both types occur in daughters of the language we are interested in; then if these two factors coincide it will be impossible in principle to decide which of the types the segment being reconstructed belonged to.

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