R. Lass and J.M. Anderson, *Old English Phonology*.
Cambridge Studies in Linguistics 14, Cambridge University Press, 1975; xv + 326 pp; £10.25.

In the past few years historical linguistics has once again become a major centre of linguistic research, as is witnessed by, for example, the 1973 Edinburgh conference (the proceedings of which are to be found in Anderson and Jones 1974) or virtually every issue of *Language*. This offers a remarkable contrast with the decade following the publication of *Syntactic Structures*, whose wholly synchronic and syntactic orientation was quite out of sympathy with the historical discipline. The renaissance of diachronic studies is clearly associated with, on the one hand, the maturation of generative phonological theory, and, on the other, a less dogmatic approach to the nongenerative syntactic models of, especially, Greenberg (1963). In phonology, which is our sole concern here, we can observe that it is rare indeed for a synchronic phonologist of the generative school not to consider and discuss diachronic events and not to suggest ways in which the synchronic and diachronic descriptions might be relatable, with the introduction of such concepts (or fancy terms, if you prefer) as reordering, loss, restructuring, etc. (S. Anderson 1974, is to some extent a counter-example here, but that book is rare both in this respect and in its excellence).

It is, therefore, a matter of considerable interest that this book by Lass and Anderson (henceforth LA) should appear, for it presents a full-length synchronic account of a language which has already been the subject of intense study from the diachronic viewpoint. LA do not claim to cover every aspect of Old English phonology, but what they do cover they cover in detail, and furthermore that coverage extends over most of the major areas of phonological interest: thus Chapter 1 deals with Ablaut, Chapter 2 with Anglo-Frisian Brightening, Chapter 3 with Breaking and Back Umlaut, Chapter 4 with Palatalization and *i*-Umlaut; some problems concerning Old English consonants (other than Palatalization) are dealt with in Chapter 5, and there is an Appendix on Gemination. Alongside these topics LA discuss matters of more general phonological interest, as is most evidenced in their discussion of distinctive features in a preliminary chapter and in the theoretical arguments of Chapter 6, 'Epilogue'. To their credit, LA take care to interrelate the general and the specific, but it may be more appropriate in a review to separate out the two. Let us therefore firstly consider the more general topics LA discuss - the matter generative - and then move on to an evaluation of their more specific contribution to the study of Old English - the matter philological.

The first decision any generative phonologist has to make is what kind of distinctive feature system he is going to use. LA are to be commended, therefore, on facing this problem at the very beginning of their book. As on most matters, in the main body of the text LA adopt a relatively conservative line, but nevertheless they depart radically from Chomsky and Halle (1968) on one matter and less radically on another. The latter point is their replacement of [vocalic] by [syllabic], already signalled in Chomsky and Halle, and the crucial difference is that LA claim that the feature is assigned not lexically, but by a marking convention (16). The

consequences of this we discuss below. The former point is LA's classification of [j, w] as liquids, i.e., [-obstr, +cons] segments, rather than as 'glides' (13). The evidence which LA gather from Old English in favour of such an analysis is most convincing and there is clearly further evidence from West Germanic Gemination, which at its most widespread could be caused by **j*, **w*, **l*, **r*, **m*, **n*, that [j] and [w] are at least in Germanic to be so analysed (and that is all that LA claim). Even more interesting is LA's treatment of [h, ?]. LA suggest (262) that a distinctive matrix is composed of two submatrices, one defining configurational vocal-tract properties, the other localizing articulatory gestures. The first submatrix is termed [phonation], the second [articulation]. Such a division seems to be exceptionally fruitful and among its advantages it allows LA to propose that [h, ?] are segments where the [articulation] matrix has been erased, leaving them as the minimally-specified voiceless fricatives and stops resepctively, cf. (4). The ease with which we can then describe changes such as Scots [t] > [?] or Greek [s] > [h] is plain for all to see and the multitude of similar cases suggests that LA have made a most promising revision of feature theory. Note too that they have managed to do away with that most embarrassing category, the 'glides'.

Another pair of features with which it is clear that LA are uncomfortable are [anterior] and [coronal], and indeed when they discuss the patterning of Old English labials and velars (185) they almost succumb to the use of [grave]. In this respect we can note that to do so would considerably simplify their rule of Back Mutation (104). Furthermore, Back Mutation would then be even more clearly a case of assimilation. The problem here is that Back Mutation in essence assimilates a front vowel to a following back vowel but only operates where the intervening consonant is [+grave]. This relation becomes quite natural if we describe the mutation by a change (or epenthesis) of gravity. The more evidence that becomes available, the more attractive do the proposals of Ladefoged (1971) become. It is worth noting that LA are in general agreement with Ladefoged on [h, ?] but disagree with both him and Chomsky and Halle on [j, w]. But the latter pair, LA suggest (6), may vary their status from language to language.

Chapter 1 of LA's book is a *tour de force* of abstract generative phonology in which the authors set out to construct a set of phonological rules for the Old English strong verbs in which all these verbs are derived from underlying forms containing an unspecified Ablaut vowel. Thus *rīdan* 'to ride' is derived from /rVid+an/, *ċēosan* 'to choose' from /kVus+an/ and so on through all classes (even including, in an Appendix, class VII) and all forms. Nor do LA simply confine themselves to the more regular verbs, but they consider the several variants of class III verbs and other forms such as class II verbs with [ū] in the present, e.g. *brūcan* 'to enjoy', cf. *ċēosan*, above.

Without doubt, Chapter 1 must be considered to be a striking display of intellectual talent - the problem is whether or not it is in the long run useful. On this question LA themselves have doubts, but as well as considering their self-criticism some other facets of the problem this Chapter raises may be worth pursuing. It is, for example, clear that it would be impossible for LA to propose such a great degree of abstractness if they did not permit

some very complex rules. One example from several of such a rule is their rule for Quantitative Ablaut (44):

$$\begin{bmatrix}+\text{syll}\\ [\text{SV}]\end{bmatrix} \longrightarrow \left\{ \begin{bmatrix}-\text{back}\\ -\text{low}\end{bmatrix} \quad [+\text{low}] \;/\; \left[\frac{\quad}{[\text{PP}]}\right] (\text{S})\ \text{C}\ \# \right\}$$

The problematic elements here are [SV] and [PP] - 'strong verb' and 'preterite plural' plus 'past participle'. These are clearly symbols which refer to paradigmatic entities, as LA say (236), but the fact is that within the model of generative phonology LA use paradigms have no meaningful definition. LA (238) are thus forced into a position where they need both lexical entries for the inflected forms and these forms must be rule-generated. We must surely share their unhappiness at this. And in the case of [SV] is it not worse? [SV], unlike [PP], is not conceivably a syntactic category, which may account for the redundancy of [PP]; rather it is phonological, and thus ought not to be referrable to at all.

A rather different type of rule which must be dubious is exemplified by Lowness Assignment (48):

$$\begin{bmatrix}\text{V}\\ +\text{long}\\ \alpha\text{back}\end{bmatrix} \longrightarrow \begin{bmatrix}-\alpha\text{low}\\ \alpha\text{back}\end{bmatrix}$$

By this rule [ē] > [ǣ] and [ā] > [ō]. Fair enough, it would seem, but what have the two changes got in common? As far as I can tell, they have nothing in common at all. It is only due to a spurious use of alpha variables that the rule can be formulated. It may well be true, one suspects, that such variables, even if they cannot be banned, should be more heavily restricted in their application.

At first sight, a similar problem to the above seems to arise with those class II verbs which show [ū] in the present rather than [ēo], cf. above. LA propose a minor rule called 'Class II monophthongization' (41):

$$\text{SD:}\quad \begin{bmatrix}\text{V}\\ +\text{high}\\ -\text{back}\end{bmatrix}_1 \quad \begin{bmatrix}\text{V}\\ +\text{high}\\ +\text{back}\end{bmatrix}_2 \qquad \text{SC:}\quad 1 \quad 2 \longrightarrow 2 \quad 2$$

But how do we know what is a class II verb? A simple alternative would be to have an optional metathesis rule (horrifying shades of Keyser 1975) by which [iu] > [ui]. This could then be followed by a rule deleting [i] with compensatory lengthening of [u]. As it happens, this would also help to get rid of a rather nasty Backness Switching rule (37) by which [ǣ] > [ā] and [ā] > [ǣ], for [ā] could then be derived from [aî] by the same process.

But since the verbs with [ū] are a heterogeneous collection, a more probable explanation than either above is surely one which relies on analogy. Note that class I verbs have [ī] in the present but [i] in the preterite plural and past participle. But class II verbs regularly have the [ēo] - [u] alternation. Could not, therefore, the 'irregular' class II verbs be simply the result of an analogy on class I verbs, for the [ū] - [u] alternation parallels [ī] - [i]? If this analogical explanation is correct, it is at least suggestive of a considerably less abstract synchronic phonology in which 'irregularities' are the result of (abductive) inferences from surface or near-surface patterns, rather than the obscure and apparently unmotivated introduction of new rules postulated by LA. We should note in this connection that the lone reference to analogy in this book (62) is clearly hostile, and seems to be an unfortunate product of the theory in which LA work. With the recent restoration to respectability that analogy has undergone, LA might well regret this sin of commission.

Not only in Chapter 1, but throughout their book, LA exploit a decision to permit nonspecification of certain features in the lexicon, that is, the freedom to postulate what we might call systematic archiphonemes. We have already mentioned this in connection with the feature [syllabic], but LA also suggest, for example, that Old English /f, s, þ, x/ be unspecified for [voice] in underlying structure. The justification for such a move is: (a) that there is no *a priori* case for picking either [+voice] or [-voice] as basic; and (b), that this allows a generalization about voicing in Old English to include reference to word-boundaries (178). This may seem useful, but the implications of such a decision are that one can have a segment characterised as only, say, [+obs, -cons, +back, +low], which is in fact LA's analysis of an underlying 'laryngeal' used in the generation of certain strong verbs (222-3). But do we really wish to have such an abstract analysis? The evidence of *brūcan*, above, suggests not. Interestingly, if fully specified entries only are permitted in the lexicon, then the degrees of abstractness LA advocate will be unattainable. Archiphonemes may be useful in diachronic linguistics, cf. here the descriptions of the Indo-European 'laryngeal' system, but their introduction here seems to me to smell suspiciously of a confusion between diachronic and synchronic aims.

The above criticisms of the synchronic rules proposed by LA, which to some extent overlap with their own criticisms in the 'Epilogue', are, it ought to be noted, not unambiguous evidence of the inadequacies of this work. Rather, they show how provocative LA's rules are in engendering a genuine inquiry into the validity of this type of synchronic description. But perhaps most interesting of all is the question they themselves ask: 'is the mere fact that a phonological solution works any guarantee that it is correct?' (226). By taking some interesting examples from earlier in the book, LA come to a highly speculative and tentative conclusion 'that paradigm-forming rules are restricted in their scope solely to the paradigms they produce, and do not constitute "generalizations" about the phonology of a language beyond the paradigms which are their domains' (231). If this claim is correct, and it seems to me that there is considerably more than a germ of truth in what LA say, then the implications for synchronic phonology are immense. Let me briefly, therefore, cite an example in support of LA.

It is well-known that the paradigm of class I weak verbs in Old English shows an alternation between geminate and nongeminate consonants, e.g. *fremme* 'I perform', *fremeþ* 'he performs'. This suggests a synchronic rule of gemination - for details see Appendix II of LA's book. But elsewhere, namely in certain noun classes, we find geminate consonants in non-alternating positions which are still due to gemination (rather than being underlying) geminates, for example *hyll* 'hill'. Now precisely in such forms it is probably impossible to apply a synchronic rule of gemination, since no underlying string can both contain the post-root /j/ necessary to cause gemination and the zero inflexion, i.e. /hul+j/ is an impossible underlying form, as LA make clear in their discussion of /j/. Nor can we posit an underlying form such as /hul+j+a/, for that would give surface *[hylle]. It looks as if such non-alternating instances of gemination must have been lost synchronically, which would strongly support LA's proposal.

From the synchronic phonologist's point of view, this book probably raises more questions than it answers - it certainly is doubtful whether LA have provided us with a satisfactory account of the synchronic phonology of Old English. But in a very real sense that scarcely matters, for the questions that LA raise are considerably more interesting than any answers which a book of this scope could have hoped to provide. Furthermore, in the final chapter of this book, the 'Epilogue', LA themselves make it clear that the success of this book is to be measured by the interest and the importance of the questions that it poses. There can be little doubt that in this respect LA have achieved their aim. But what kind of reception does this book deserve from the traditional diachronic linguist? To take an analogy which I suspect may appeal to the authors, the traditional linguist has by and large considered generative phonology to be parasitic on traditional studies, whereas what we need is a symbiotic relationship. Perhaps, to coin a phrase, we need a generative philology rather than a generative phonology.

The first point to be made is that LA, unlike too many generativists, are exceedingly careful with their data: very few mistakes occur, and of these fewer are serious, although we should mention *pleġan* for *plegan* 'to play' (135, 180). This seems to arise from a quite unacceptable account of Anglo-Frisian Brightening, by which *-an* is derived via /æn/ < /an/. There is sufficient evidence from Palatalization, apart from the form *plegan* itself, to show this is wrong, cf. lack of Palatalization in *cempa* 'warrior', which their account cannot explain. Secondly, LA are always willing, as we have already said, to take account of the fine detail of data, which can all too easily wreck a more simple-minded analysis. Thirdly, LA's usefulness from the traditional point of view may too often seem trivial, as when they suggest that *n* in *singan* 'to chant', etc. may not have been a velar or when they suggest that Old English *r* was uvular (which may not be correct, but at least attempts a real solution at this nasty problem), but in every case what is important is the new insights and perceptions which flow from LA's approach. Such (apparently trivial) points are worth arguing about, for they force one to think about hypotheses which were rapidly attaining the status of fossilized truth or dogma.

Of more general interest, however, than this nibbling away at received, but dubious, opinion are LA's assaults on major problems of Old English phonology. As was almost inevitable, LA address themselves to the interpretation

of the socalled 'short digraphs', that is, the use of *ea, eo* to represent developments of historically short vowels as well as the long diphthongs derived from the Germanic diphthongs. Ever since Daunt (1939), Old English scholars have argued over the status of *ea, eo*. Despite numerous attacks, the traditional view, that in such cases *ea, eo* represent short equivalents of the long diphthongs, has remained dominant. LA now suggest (75-83) that *ea, eo* always represent diphthongs, as in the traditional accounts, but that there is no phonetic distinction of short vs. long. This clearly answers the major criticism levelled against the traditional view, namely that it required a systematic distinction which was generally foreign to the Germanic languages and unsupported by general linguistic principles.

In order to account for the fact that in Middle English the two types of diphthong regroup into short and long monophthongs according to their original source, LA suggest that the phonological rules which produced the 'short diphthongs' were lost, thus reversing the merger which they allege had taken place. But despite an attempt by LA (110-11) to argue to the contrary, it does not seem to be the case that their theory will meet certain dialectal facts of Middle English, most especially in those dialects where *eo* undergoes rounding to [ø]. The subsequent unrounding to [e], which then allows LA to hypothesize the reversal of the merger, is so much later than the development of *ea* > *a* (the equivalent stage with the low diphthong) that the two events should be collapsed into one. Further, LA must crucially rely on instances of *ea, eo* before [x] being treated as long except when morphological considerations apply, cf. (108, n.1), but examples such as Old English *weaxan*, however complex, appear to count against them. Since in any case acceptance of LA's proposal relies on an act of faith - the belief in rule loss without relic forms - it can scarcely be acceptable.

LA are slightly more successful in their discussion of the status of breaking and back umlaut (102-7). Traditional handbooks are notoriously reticent in typologically relating sound changes to one another, but LA show that back umlaut must be related either to *i*-umlaut or to breaking. Quite correctly, they resist the temptation to relate the two umlaut processes, unlike the situation in, for example, Old Norse. But they do show very clearly the phonological near-identity of back umlaut and breaking. So far so good, but they go too far when they posit one synchronic rule to reflect both processes (104). There are at least three objections to the rule. Firstly, it is excessively cumbersome, although this could be mitigated by the judicious use of [±grave]. Secondly, LA agree that breaking must precede a rule retracting [æ], but back umlaut follows the same rule. To get round this LA exclude [æ] from back umlaut, which denies the facts of Old English, especially in the Vespasian Psalter dialect but everywhere theoretically. Thirdly, there are many instances where a front vowel due to *i*-umlaut is later subject to back umlaut (due to a variety of unstressed vowel alternations and morphological transfers). Thus back umlaut is later than *i*-umlaut, but the latter follows breaking. The problem here is even more extensive, undermining as it does several generative assumptions, but it is in any case sufficient to refute LA's proposal.

Much of this review has been spent in criticizing individual features of LA's book, but it would be wrong to conclude by damning it with faint praise. The reviewer hopes that he is not so egocentric as to write a review of this length for a book which he considers to be of merely passing

interest. Rather, the length of this review suggests the importance of the book. LA may be wrong, as I tend to think they are for much of the time, but their wrongness, if that it be, results in a book which is immeasurably more interesting than one which either naively followed the more dogmatic precepts of generative phonology or was content to repeat uncritically the products of traditional research, however right or wrong either of these lines may be. There is a danger that the book will be perceived as falling between two stools: the generative phonologist will not read it because it is so detailed and complex an examination of a far-off language of which he knows little; the traditional Old English philologist will not read it because it is written within a theory of which he disapproves or, and much worse, of which he is ignorant. That would be more than unfortunate. Both types of reader would, if he approached this work with an open but critical mind, find much to interest and stimulate him.

As is usual in this series, the publishers have served the authors well, but there are two serious faults in its production where the blame must fall at LA's feet. Campbell (1959) is, of course, a major source of reference, and therefore it would have been better if LA had systematized their references to the book by referring to it always either by page or by section. This they do not do, and thus the occasional loss of a vital '§' can cause problems as in (137). Secondly, LA provide a useful Appendix of rules - why then are they not in the order LA argue for in the book, but rather in what might be called a partially-ordered set? But thank you, on the other hand, for a fine set of indices.

Richard M. Hogg,
Department of Historical English Language,
University of Lancaster.

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