History of Englishes. New Methods and Interpretations in Historical Linguistics [Review of: M. Rissanen (1995) New directions in English historical grammar]

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NEW DIRECTIONS IN ENGLISH HISTORICAL GRAMMAR

Review Article of *History of Englishes. New Methods and Interpretations in Historical Linguistics.* Edited by Matti Rissanen, Ossi Ihalainen, Terttu Nevalainen and Irma Taavitsainen (Topics in English Linguistics 10). Berlin-New York: Mouton de Gruyter. 1992. Cloth, 799 pages, DM 298,–. ISBN 3-11-013216-8.

This is a large book.[1] It presents us with the proceedings from the sixth International Conference on English Historical Linguistics. The growth and success of this conference is well evidenced by the volumes that emerge from it. Whereas with the 1985 Conference, the ensuing publication contained twenty-one papers (Eaton *et al.* 1985), and with the next one (1987) twenty-eight (Adamson *et al.* 1990), this time we are confronted with as many as forty-eight contributions. However, quantity alone cannot be a yardstick for success. The editors do not make clear whether they have made a selection of the submitted papers, although they do state that they have been assessed. Having read through all forty-eight papers, it seems to me that a more rigorous selection could have been made. Quite a lot of the papers are of excellent quality, providing us with new ideas about methodology, new data, and new insights. A few, however, are rather meagre, and to my mind do not really deserve a place amongst the others. In this small space I cannot do justice to all the contributions; I will therefore concentrate on those topics with which I am myself most familiar, i.e. those related to morphology and especially syntax.

It must be gratifying to the organisers of this Conference, especially when one considers the occasion why it was held at the University of Helsinki (the 350th anniversary of the University), that quite a few of the contributors make grateful use of the historical language corpora that have been developed at this university, as an almost natural consequence of the scholarly work that has been conducted here in the field of English historical linguistics over the years. Dalton-Puffer, Klemola and Filppula, Melchers, Meurman-Solin and Peitsara all refer to one of these corpora (The Helsinki Dialect Corpus, The Helsinki Corpus of Older Scots, the Helsinki Corpus of English Texts), and the number of historical linguists using them is still growing, as the seventh Conference held in Valencia (in 1992) clearly showed.

The title of the book refers to new methods and new interpretations. New theoretical ideas are certainly to be found in the contributions by Labov and (James) Milroy. Milroy's views on language change must be well-known by now from earlier publications such as the article in *Journal of Linguistics* (1985) together with Lesley Milroy, and his recent book (1992). Milroy concentrates on the social facts of language change, i.e. on

how a particular linguistic variant spreads through society, and less on how this variation comes about. His main concern is with sound change, which he sees as a testing ground of historical linguistic theory. Milroy argues that there has been a general tendency among historical linguists to assume that the "message-oriented" function of language is the only relevant function, whereas it is in "listener-oriented functions ... that structural and phonetic changes are negotiated between speakers" (p. 75). In other words, in many kinds of language use, speakers do not so much aim at trying to be explicit, as to maintain "friendly relations". He also makes an important methodological distinction between 'change' and 'innovation'. A speaker-innovation may or may not enter the language system. Only a successful innovation will alter the system, and therefore constitute a change. According to this view, a language 'change' is only *socially* motivated, not structurally. Methodologically, this strict separation between innovation and change is useful because it makes us concentrate much more than before on the social motivations of a sound change. This is of course where the strength of (both) the Milroys' work lies. The idea of social networks and the importance of the *weak* links therein (rather than the *strong* ties of a speaker within a community, as Labov (1980) maintains) have shown to be a fruitful, new direction of research. It explains very nicely, for instance, why changes tend to jump from one urban center to another, and why language contact often promotes change (because in language contact situations, communities tend to have weak ties). However, there is also a certain danger lurking in this approach, which is a tendency to downtone the importance of internal factors. They seem to be only relevant with respect to innovations, if at all. It may be misleading to base general theories of change on the evidence of sound change alone. It is very likely that sounds function more strongly as markers of social identity than syntax does, especially syntax related to word order. To my mind it is quite possible that many instances of *syntactic* 'change' are structurally rather than socially motivated. If changes in the syntactic structure can be brought about by the need for more transparency or in order to solve structural problems created by changes elsewhere, then it is likely that such changes will not start off as some individual's innovations, but as the innovations of a much larger group, even though individuals make them independently of one another. In such a case the 'change' is not merely the result of the social spread of a successful innovation, but, in the first place, the result of internal factors. In other words, in these cases the distinction between innovation and change may be neutralised.

It is interesting to observe that Labov, who may be considered the 'father' of the social approach to sound change, in his contribution provides ammunition against it. Where Milroy attempts to reduce the role of internal factors in 'change' to almost zero, Labov places a direct hit at the opponents of the neogrammarian view of sound change (in which it

was viewed as basically mechanical and regular), especially at those who believed that, fundamentally, sound change proceeds word by word. Labov makes a distinction between two different types of sound change: low level phonetic sound changes, which follow the neogrammarian path (e.g. fronting, lowering of vowels); and changes where sounds are involved at higher levels of abstraction (e.g. lengthening of vowels, metathesis), which tend to be lexically diffused. This difference is very aptly expressed in the question: "is [it] sounds or words that change"? (p. 45). He adds that regular or mechanical sound change is also "characteristic of the initial stages of a change that develops within a linguistic system, without any social awareness" (p. 43), while lexical diffusion occurs in the "late stages of an internal change that has developed a high degree of social awareness" (p. 44). (It is quite clear, by the way, that these observations run counter to the ideas expressed by Milroy. At least, I take it that there must always be a degree of social awareness in the view of sound 'change' expressed by Milroy.) Labov illustrates his hypothesis about regular sound change by means of a thorough analysis of Ogura's (1986) data on the Great Vowel Shift. He examines the reflexes of ME /u:/ and /i:/ and finds that phonetic conditioning accounts for them in a very strict way: the changed segments are governed by their phonetic environment "in a manner more regular than the Neogrammarians themselves might have imagined" (p. 69). What is also interesting about Labov's new findings is that it may eventually obliterate the old distinction between dependent and independent sound-changes. It is shown here that even independent changes are influenced by their environment. We now need to find out why it is that certain consonants (or combinations of consonants) affect a particular vowel earlier than others.

There are three other papers placed in the same section ('Theory and methodology'), which deal with smaller or more personal points. Blake pleads for the recognition of the importance of translations (especially from Latin) on the development of the English language. Mitchell questions the usefulness of modern linguistic theories for the study of Old English syntax ("are the Emperor's new clothes really there?" (p. 98)). I appreciate Mitchell's own method of studying OE syntax, in so far as it aims at giving an increasingly better description of OE, to further our understanding of its literature. I also agree with him that in historical language study the data are of the utmost importance, since we lack native speaker intuitions. Ideally, therefore, any study undertaken on an aspect of OE syntax should deal with all the data available. With the help of computer corpora this will indeed become feasible. We must not forget, however, that even 'all the data' does not represent all the data. To set up hypotheses about the structure of OE, we must therefore also rely on theories that are formed in order to understand language structure. I suspect that Mitchell's rejection of the "new clothes" is in fact a refusal (unwillingness, inability?) to understand why such theories are resorted to

in the first place. These 'new' historical linguists are not primarily interested in reading the literature of the OE period but in the questions: what makes the syntax of OE tick, how did OE children acquire their language, what rules and principles underlie the complexity of the data? It is by working within a restrictive theory of grammar that we may hope to acquire more insight into the *system* of OE; that we may explain why certain surface phenomena are the way they are, or why they may have changed. (I hope my discussion of Ogura's paper, below, will show how the theory may be applied with success to account for surface data.) In this way, we may even predict the occurrence of certain constructions that have not been noted before, and which often are only found because one specifically looks for them. That these theories are themselves not yet perfect, is only to be expected, but that does not mean we should reject the use of them. It is only by providing better descriptions *and* better theoretical explanations that we may hope to acquire a fuller understanding of OE syntax in its totality.

The last contribution in this section is on analytic versus synthetic developments in language. Danchev contests the idea that English is becoming more and more analytic. This is a common point of view among English historical linguists, and Danchev's contention therefore may come as a bit of a surprise. However, when we look at his definition of 'analyticity/syntheticity', matters soon become clear. Danchev does not confine these notions to the area of morphology, as is usually done, but includes phonology and the lexical level. The basic idea behind the opposition is then that analyticity implies a one to one relation between form and meaning (where meaning may be structural or lexical, I take it), and syntheticity a one to several relation. It seems to me that the definition broadened in this way becomes actually quite vacuous. To what end would one like to use these definitions? How do they clarify the situation? When Danchev gives as an example of phonological syntheticity the development of a long vowel in OE *gos*, due to the loss of a following nasal before a spirant (p. 31), I can see that two elements have merged into one, but I don't quite see that the result is a 'one to several' relation. What do the 'several' elements (i.e. the original short vowel and the nasal) stand for in terms of (structural) meaning? When it comes to examples of lexical syntheticity, we are presented with instances of lexical derivation and compounding, or blends such as *smog*. In fact the notions have now become so general that each language shows analytic as well as synthetic developments. In other words we have lost a useful term for describing general, unidirectional developments in the morphological make-up of languages.

The other part of the title of this volume, 'new interpretations', is also well represented. Quite a number of the papers present us with new data which lead to new interpretations (Dalton-Puffer, Denison, van den Eynden, Koopman, Melchers, Romaine, Schendl, Ukaji). There are also

papers which offer new interpretations based on 'old' data (Anderson, Boas, Colman, van Kemenade, Lass, Lutz, McMahon, Minkova and Stockwell, Moessner, Poussa, Ritt). Finally there are papers where the data provides the core and the interpretation goes no further than the data (Curry, Klemola and Filppula, Meurman-Solin, Nunnally, Ogura, Peitsara, Schneider). The third group of papers is the easiest to deal with, since the value lies exclusively in the type of question(s) asked and the methodology used. The results vary enormously depending on whether one starts with a hypothesis, i.e. the wish to sift through the data in order to find an answer to theoretically potentially interesting questions. This is not always the case. Sometimes one feels the data are presented almost mechanically, as if the researcher had no idea what (s)he would like to do with them. Klemola and Filppula look at the subordinating uses of *and* in the history of English, more precisely at non-finite clauses introduced by *and*, traditionally termed 'absolute constructions'. They note that these absolute constructions occur both with and without *and*, but they confine themselves to the former. This is an example where to my mind the data have run away with the researchers, because they give no good reasons why the *and* and *and*-less constructions should be treated separately. Presumably, one can only know the reasons by looking at them both. Especially the infinitival *and*-type looks rather similar to the *and*-less type, and both tend to occur only in formal (Latin-influenced) language. The non-infinitival types are further subdivided into whether the predicate consists of a participle, a noun, adjective etc., but this does not provide any further insight into the construction, either synchronically or diachronically. What is of interest is that the non-infinitival types occur mainly in narrative texts. As the origin for the use of *and* there, reference is made to Jespersen's suggestion of Celtic influence, but unfortunately this is not further investigated historically as to type or provenance of texts.

The study by Peitsara on the development of the *by*-agent in English is very carefully set up and presents some interesting findings. The study is limited to two prepositions, *of* and *by* (I don't quite see why *by* is described as "primarily instrumental" (p. 379), it clearly was locative to start with). Peitsara comes to the conclusion that *by* was replacing *of* much faster than has generally been assumed (the usual cut-off point given for the predominance of *of* is 1600): already in late ME the use of *by* was 77 percent as compared to 23 percent *of*. She also finds differentiation according to text type, *of* occurring far more heavily in religious and instructive works, and being rare in legal and in popular narrative texts. The reasons for this are not further investigated. The author also considers the influence of the overall functional load of both prepositions. Since *of* had a much heavier functional load than *by*, it might be expected that this contributed to the eventual selection of *by*. This is an important question and it is a pity that it is not really developed. Although the various functional uses are counted (but not, strangely enough on the basis of the corpus, but on the

basis of the OED), no descriptions of these functions are given, and therefore we get no idea how close some of the *of*-functions are to some of the *by*-phrases. Since the change from *of* to *by* seems to take place by lexical diffusion, this is an important point. In this connection, the section on the preference shown by some verbs (or rather participles) for either *of* or *by* is useful. Some important parameters emerge here, such as the influence of the origin of the verb (native or French/Latin), and of the semantics of the verb. Concerning the latter it seems clear that *of* is preferred after verbs that express a stative, non-physical action. It would be interesting to compare these lexical findings to the meanings of *of* and *by* in their other functions (see above), to see whether there is a connection there, so that we might get clearer answers about the pathway of lexical diffusion.

The paper by Meurman-Solin on the morphology of verbs in Middle Scots is another example where good use has been made of a corpus. The facts are well represented and all the possible constraints (linguistic and extra-linguistic) influencing the choice of variants have been examined, such as the distance between subject and verb; the position of the subject and its presence/absence; the type of subject (nominal, pronominal, person (1st/2nd/3rd) and number (sing./pl.)); the influence of anglicisation, text type and the audience it was written for. This study comes up with clear answers to straightforward questions (although it is a pity that these questions and answers are not further embedded theoretically, which would give them more depth).

Not such good use of a corpus is made in another corpus-based investigation by Curry into *do*-variants in questions and negatives in the writing of Jane Austen. First of all, the subject does not really lend itself to a study of the constraints on variants, since to all intents and purposes *do* is the norm in both types of clauses in the two investigated genres (letters and a novel) in Jane Austen (this fact in itself is of course useful to know). Curry herself notes that none of the parameters that Kroch (1985 [sic], this must in fact be 1989) mentioned as determining the absence or presence of *do* in questions play a role in Austen (p. 710, and note 6). On the whole, the treatment of the very few cases where *do* does or does not occur as expected is rather superficial and the explanations given are quite ad-hoc. It is not clear to me, for instance, that the examples in (5) (p. 709) deviate from PrDE: *Does Susan go?*, sounds to me as natural in PrDE as *Will Susan go?* If the difference depends on context, it should have been discussed. There is also a general tendency of *hinein*-interpretation. An example is the interpretation of "declarative inversions" (*How came she to think of asking Fanny?*) as signalling "real or mock formality". It seems to me much more telling that all examples of declarative inversions contain the verb *come*, but no further investigation is made concerning that fact.

The other data-oriented studies I will discuss according to topic in order to highlight the relations between them and papers on a similar subject. Four contributions that can fruitfully be discussed together are

van Kemenade, Koopman, Ogura and Ukaji, all dealing with questions of word order. Essentially, van Kemenade's paper deals with the history of modals, but the important point she makes is that there are two kinds of modals in OE, which, she thinks, can to some extent be differentiated by word order. She starts off with a recapitulation of Lightfoot (1979) on the history of the English modals, where he makes a distinction between changes leading up to the reanalysis of the modals and those resulting from it. (Strangely enough she leaves out Lightfoot's change (iv) (1979: 105), which deals with structure and word order and which plays an important part in her story: it was the changes in word order which obliterated the earlier structural distinction between Aux-modals and Main Verb-modals (cf. p. 287).) Van Kemenade notes that Lightfoot's idea of a cataclysmic change does not square with the facts, since, after the reanalysis of Verb to Aux, we are still confronted with modals that display non-auxiliary behaviour. She wishes to show that it is reasonable to assume that already in OE the modals occurred in two different structures: root (deontic) modals, which are main verbs, appear in control structures (NP$_i$ [PRO$_i$ NP V$_{inf}$] V$_{modal}$), and epistemic modals, which are auxiliaries, are placed in the INFL node.[2] In the course of the ME period the difference between these structures became opaque (due to the loss of the V-second Rule and developments in negation) so that slowly more and more modals adopted the Aux-structure.

The idea of two structures is appealing. It is far from easy, however, to prove that there are indeed two different structures. What evidence is there that the modals occur in another structure beside the, for main verbs, usual control structure? Van Kemenade puts forward three kinds of evidence, of a morphological, syntactic and semantic nature. Morphologically the modals already were isolated in their class of preterite-present verbs (but cf. Colman's interesting contribution on this topic), which made them less verb-like. It was already shown by Warner (1990) and Denison (1990) that there is structural evidence in OE (especially in the use of modals in impersonal constructions, where the modals lack a syntactic subject) that some modals could be used in an epistemic sense. Van Kemenade tries to find further syntactic evidence for the existence of epistemic modals. This is difficult because both epistemic and deontic modals may occur with infinitival constructions (cf. note 2). The author's approach to this problem is semantic. She notes examples in OE where the verb, according to her, expresses epistemic modality. Two of them ((9a) and (b) on p. 295) are quoted here,

(1a) þonne magon hie ryhtor cweþan þæt þæt wæren þa ungesælgestan
     then   may   they righter say   that that were   the unhappiest
     'then they may (have the power, are in a position to) say more truthfully that those were
     the most unhappy ones'   (Oros, 113, 18)
   b) ealle hie þæt anmodlice wilnodan þæt hie his word gehyran moston
      all   they that unanimously desired that they his words hear   might
      'they all unanimously desired that they might (would have the opportunity to) hear his
      words'   (Bl. Hom. 219, 34)

It is noted that the modal in (1) "does not appear to have its own subject" because thematically speaking *hie* is not the subject of the modal but of the infinitive (p. 295). However, this interpretation of (1) and of its structure depends completely on one's reading of the modal verb. To my mind the context does not exclude the possibility of a deontic meaning, 'have the power' in (1a) and 'have the opportunity' in (1b). (The evidence from impersonals, quoted above, is much stronger because there it can be shown that there is no syntactic subject.) However, van Kemenade goes on to use (to my mind potentially ambiguous) examples like (1) to show that there may be structural differences on the surface. Surface differences would, of course, prove most strongly the existence of two different syntactic structures. Van Kemenade notes that the control structure of root modals may be obliterated by Verb (V)-raising. This helps us towards a distinction, because only main verbs can undergo V-raising. In other words modals that do not show the evidence of V-raising must be epistemic. V-raising in OE is an extremely complicated matter since the rule allows many subtypes, and there is as yet no agreement about which types of V-raising apply to OE (the choices are V-raising to the left, V-raising to the right, VP-raising, possibly followed by inversion, extraposition etc.). In other words, when we look at the surface structure, it is difficult to tell in many cases what (types of) rule(s) has/have applied. To cut matters short, van Kemenade comes to the (tentative) conclusion that absence of V-raising is shown by the surface order XVv (where V = infinitive and v = finite verb), and that therefore such orders imply (again tentatively) the presence of an epistemic modal.

Koopman, in his paper on the distribution of verb forms in OE subordinate clauses, provides (he refers to an earlier version of van Kemenade's paper) some interesting comments on van Kemenade's proposal that XVv order indicates the presence of an epistemic verb. He writes on p. 328 that there are "pairs of sentences which differ only in the form of the verbal cluster and are otherwise identical" (quoting *Cura Pastoralis* 293.5 and 293.8). It is unlikely that here we are dealing with a root modal in the vV order (which shows V-raising) and an epistemic modal in the Vv order, because both clauses mean the same. Another piece of evidence is that genuine auxiliaries do occur in vV clusters (and they are not usually considered to be raising verbs, (see (18) p. 328)), and that raising verbs appear in Vv clusters (see (19), p. 328). Finally Koopman's data indirectly show that the Vv = epistemic-hypothesis is untenable. He examines the internal order and the position of the verbal cluster in subclauses. Table (7) on p. 323 makes clear that in final position the order Vv is much more frequent (the average is about 80 percent) than vV (about 20 percent) in clusters with modal verbs. This would mean in van Kemenade's terms that the ratio of epistemic vs root modals is about 4:1. This seems to me highly unlikely considering the general assumption that root modals were still the common type in OE and that epistemic modals were a new development.

Koopman's study, in fact, shows something rather different, and that is that the *position* of the cluster within the clause is crucial and not the type of verb present in v (the finite verb). He is interested in the distribution of V and v in subclauses in the light of later developments (in main clauses the rule is already vV). In general, there are as many Vv as vV clusters in subclauses in OE, and there do not seem to be any diachronic developments (see table 1, p. 320). However, as soon as one looks at the position of the cluster, there seems to be a clear split: vV is most common in non-final, and Vv in final position (see his table 6). It should be noted that these word order data are very carefully collected and that interfering factors are well taken care of. This is quite unlike some statistical studies of word order, where often things are heaped together with as a result a very unreliable picture. What comes to mind, for instance, is Bean's (1983) account of word order in coordinate clauses. She has carefully separated these from other main clauses (cf. Mitchell's warning in 1964), but she does not consider that there might be coordinate clauses on two different levels: (i) attached to a main clause, or (ii) to a subclause (and, in the case of (i), whether the main clause is introduced by an element that may cause inversion). It is little wonder that she finds such a high proportion of OV (and VS) structures in these clauses (which leads her to conclude, among other things, that OE was a V-third language).

Another interesting finding in Koopman's contribution concerns the type and distribution of extraposed elements. True SOV languages (like e.g. Dutch and German) do not as a rule have extraposed objects, only PPs and adverbial phrases may be extraposed. In OE, however, objects are frequently extraposed (no diachronic development has been noted) and, when they are, they occur mainly after vV clusters not after Vv. What could be the explanation for these distributional patterns? On this aspect the paper is less strong, which is perhaps not surprising considering the many types of rules that have been suggested for the distribution of word order in OE (see also above). Koopman suggests that the reason why SvVX (X = object) is far more frequent, may be because this order can be the result of either the extraposition of X or of verb-fronting of the whole cluster. It is not clear, however, that the possibility of there being two rules to achieve the same result must necessarily lead to a higher frequency of that particular order. Moreover, the evidence for something like verb-cluster fronting is not very strong. In addition it still does not explain the low frequency of SVvX. Koopman writes that SVvX must always involve extraposition, since "there is no way the verbal cluster can be moved" (p. 330). But is that correct? Given the immense number of possibilities made available by the combinations of all kinds of rules (which gives one an inkling that something is wrong) there is no reason why the movement of the cluster could not be followed by an inversion rule, which Koopman uses elsewhere in combination with V-raising; then we would also get SVvX as a result. It is clear that the proposed rules are too unrestricted.

I have no solution to this problem either, but I would like to make a few simple remarks. Koopman notes that SvVX is more frequent, and so is vV when it is next to the subject. It is quite striking, and presumably no coincidence, that this vV order is precisely the order that becomes the norm in main clauses. Stockwell (1977) has shown that the SvVX order in main clauses in OE was effected by 'exbraciation'. Exbraciation is a process whereby the object is moved out to the right. This could be the result of so-called 'afterthought', of a number of extraposition rules present in OE, as well as of abduction. Since the order in main clauses was regularly SvO when there was only one verb involved, due to the V-second rule (and the percentage of one-verb-clauses was high because the auxiliary system had not fully developed yet), the language learner would have good reasons to abduct SVO as the underlying order. This process of abduction could ultimately then also lead to the spread of Sv(V)X in subclauses. Koopman's contribution may therefore have shown that subclauses in OE either have underlying SOV, which results in final Vv orders, or they have abducted the surface order of the main clause, resulting in non-final vV orders.

Michiko Ogura seeks to answer the question why the order *to cwæd him* (or a full nominal phrase instead of *him*) is impossible in OE. She approaches this problem mainly from a 'surface' point of view. She finds that the other five orders logically possible with these three elements all occur; so this one is the only exception. She next looks at other verbs involving the element *to* in preverbal position, and finds thirty-four instances in her corpus (not counting ms. variants), of which most occur in glosses. Of these instances *to* is almost always part of the verb, i.e. a prefix or particle rather than a preposition. She concludes therefore that with simplex verbs (i.e. where *to* is part of a prepositional phrase) the above order is prohibited, in order to avoid confusion between *cwedan to* 'speak to', and *tocwedan* 'forbid'. It seems to me that here the use of a theoretical frame-work would have led to a more satisfactory (because more basic) solution for why the order under discussion is not found. It would take me too long to explain this in detail, I will therefore confine myself to a rough description. When we look at the five possible orders,

(2) (a) *cwæd to him* [regular SVO order in main clause]
     (b) *to him cwæd* [regular SOV order in subclause]
     (c) *cwæd him to* [as (1), with *him* in clitic position, on the left of *P*]
     (d) *him to cwæd* [as (2), with *him* in clitic position, on the left of *P*]
     (e) *him cwæd to* [as (1), with *him* in clitic position, on the left of periphery of VP]

we find that the word order rules for OE (including the rules for clitic pronouns), such as proposed e.g. by van Kemenade (1987) account for all five orders (as indicated in (2) by square brackets) and exclude the order *to cwæd him*. In the unattested order, the clitic *him* is found in the right periphery of the VP, which is not a possible clitic position. When we look

at the data provided by Ogura in table 1 (pp. 375–6), we may note that the
grammar used in (2) seems to account very nicely for the data found. We
would expect the orders found in (c) to (e) to be only possible when the
dative is a pronoun (clitic). This is exactly the distribution found in table
1: (a) to (e) are all common when the dative is a pronoun; when the dative
is a full noun phrase only (a) and (b) are found with an aberration of only
0.8 percent in prose and 1.2 percent in the glosses (the latter may well be
due to slavish translation).

A last article dealing with word order is by Ukaji. It discusses the odd
position of the negative in the construction *I not say*, found in early
Modern English. He argues quite convincingly that this word order forms
a 'bridge construction', between an earlier type *I say not* (where the nega-
tive *not* is still in its original position as a 'secondary' negative) and the
later development *I do not say* (where *not* still follows the finite verb –
thanks to the introduction of *do* – but precedes what is semantically the
main verb).[3] As evidence for his hypothesis, he shows with the help of his
corpus (which comprises a large number of texts covering the fifteenth to
the eighteenth century) that *I not say* is not part of the successive linear
development in which the other (diachronic) negative orders are all in-
volved, but that it is coexistent with two of these orders, when they were in
the process of transition. Secondly, he observes that *I not say* does not
occur with auxiliary verbs, such as *have, be* and the modals. Since they do
not evolve from *I could not* to **I do not can*, he argues, they have no need of
the bridge construction. I am not sure whether this second argument
holds, because it can also be said that these auxiliaries were by this time
(cf. van Kemenade above) already a separate category, no longer verbal.
The existence of 'bridge' phenomena in itself is of great interest if we want
to acquire more insight into processes of syntactic change. The process
reveals not so much an internal restructuring as a kind of 'surface' adapta-
tion made by the speaker in order to smoothen a process of (internal)
change. Ukaji points to another such bridge phenomenon, the change of
the numeral system from *three and twenty* to *twenty three* via *twenty and
three*. Another that can be suggested for the history of English is the
development from the opaque *one the best knyght* 'the very best knight', to
the partitive *one of the best knights* via *one of the best knight*. These two
examples of bridge phenomena, however, both concern rather small sub-
parts of the grammar. Here, surface adaptations are perhaps more under-
standable and tolerable. I'm not sure whether this process is as likely to
occur in the very basic parts of grammar. The examples provided by Ukaji
suggest that another factor may also have been responsible. All the in-
stances he gives under (6) to (14) (p. 454) of *not* followed by the main verb,
seem to be examples of main verbs which are non-dynamic. They can all
be replaced (except, I think, (13)) by a negative + adjective/noun: e.g. *that
I not herd* > *that I am not-aware of; not yet crept out of the shell* > *not-out
of the shell; who not needs* > *who has no-need; I not repent me* > *I am not-*

repentant etc. In other words they can be replaced by elements which have the negative immediately before them.

There are a number of articles that deal with cases. Two are concerned with the genitive case, others deal with the semantic roles expressed by cases in OE. I found the contribution by Boas on the historical development of English genitives rather disappointing. He uses the development of the genitive to start off a diatribe against generative grammar, especially where it attempts to describe and explain linguistic change. This is of course legitimate, but I would expect a critic who demolishes something so severely, to put up a new building in its place. Nothing much comes of that.

Boas concentrates on Lightfoot's (1980) treatment of the genitive. He detects quite a number of weaknesses in this analysis, the main ones being: (i) Lightfoot's disinclination to attribute the change (from genitive to periphrastic form) to foreign influence; (ii) his ignorance of a number of distributional facts of OE; (iii) his use of X-bar theory for OE; (iv) his belief in autonomous syntax. Although I quite agree with Boas that data is not Lightfoot's strongest point (cf. also Fischer and van der Leek 1981, and other reviews of his 1979 book), and I concede that Lightfoot's presentation of the genitive data is not at all complete, in Lightfoot, at least, we are given a well-ordered discussion of the (incomplete) facts (so that they are indeed falsifiable). In Boas, instead, the discussion consists mainly of reactions to Lightfoot's proposals. Only at the end, in two pages, are we given an idea of how the development of the genitive can be accounted for within a syntactic framework developed by Boas in 1975. However, the description of this framework is so general, that the reader gets no idea how it handles the facts of the genitive. With respect to (i) (and (iv)), for instance, Boas writes, "An adequate explanation of the changes from Old English to Middle English and Modern English presupposes a linguistic theory that not only encompasses potential bilingualism and its creolizing effects on the languages in question, but also provides for the means to formally represent these effects on the phonological, syntactic and semantic level. This requires, however, giving up the autonomy and priority of syntax and the strict separation of levels and components as postulated in Chomskyan generative approaches" (p. 235).

A theory that encompasses the effects of language contact would be fine, but I, for one, have no idea how this could be done. Lightfoot is surely correct when he says that 'borrowing' is a chance element, and, although we might be able to describe the circumstances under which borrowing is more likely, I do not see that we will ever be able to incorporate it into our theory. It also seems from the above quote that Boas does not want to maintain a distinction between the theory of grammar and the theory of change. I think it would be methodologically sound to maintain that distinction because otherwise too many unlike quantities have to be considered simultaneously, with the danger that nothing will be discovered.

With respect to the influence of French on the development of the periphrastic genitive, Boas himself does not show how this influence could be formally represented in his theoretical framework, nor does he give evidence that French did influence the development. He merely writes, "Traditionally, this spread has been attributed to the influence of French *de*" and "this French ... influence ... was surely part of the creolization process that affected late Old English" (p.233). This is surely too easy. French may have had some influence, but it is also true that the periphrastic genitive already made its appearance in OE (cf. Mitchell 1985: 548, Mustanoja 1960: 74ff.). It is quite likely that the loss of inflections was the more immediate trigger of the change. It has been frequently noted that syntactic borrowing is more likely to take place if the borrowing language has a need for the foreign construction and can fit it into its own system (cf. Aitchison 1981: 119ff.; Fischer 1992: 19ff.; Sørensen 1957: 133). In this light Mustanoja's remark that the *of*-periphrasis is commoner in the plural than in the singular is interesting, because the plural marker was phonologically weaker than the singular one. Finally, other Germanic languages not influenced by French independently developed a periphrastic genitive using a partitive preposition such as *van* in Dutch, and *von* in German. French is therefore more likely to be a means to an end than a cause.

Concerning points (ii) and (iii), Boas objects to Lightfoot's use of X-bar theory because it cannot account for the fact that in OE the genitive could appear in pre- as well as post-modifier position. He writes that in OE there is "not sufficient motivation for distinguishing structurally between prenominal specifiers and postnominal complements as required by X-bar theory" (p. 231). Although it is true that the distribution in OE is complicated, more so than Lightfoot supposes, I doubt whether it could not be structurally described. Mitchell (1985: 548ff.) is more optimistic than Boas thinks he is (but more study of the facts is undoubtedly necessary), and Nunnally's contribution in this same volume also makes clear that structural parameters are involved. Nunnally's figure 1 (p. 362) for the position of the genitive in translated OE prose shows that the majority of genitives are preposed when no other qualifier is present (postposition is only found in 3.3 to 3.7 percent of cases). Parameters that influence the position of the genitive are whether or not the genitive is qualified, whether or not it is coordinate, and (Mitchell adds) whether or not the governing noun is qualified. Nunnally adds two other factors that tilt the scales towards postmodification: when the genitive has partitive meaning and in order to acquire the stylistic effect of parallelism. Nunnally concludes that variation is not free but functionally conditioned, and that the postposed variant must be considered "the marked member" (pp. 366–67). Boas solution to the problem of the variable position of the genitive in OE is to assume that OE was a non-configurational language. This, however, raises a host of other problems and does not do justice to many

of the regularities in word order that have been found in OE.

A difficult and relatively unexplored area of OE grammar is the relation between syntactic case and the expression of semantic roles. Schendl makes a brave attempt at opening up this field in his "valency description of Old English possessive verbs". His contribution presents an elaborately worked out proposal for about one hundred and forty OE verbs expressing 'alienable possession'. I particularly welcome such an attempt because it shows how difficult it is to fit the nitty-gritty of language into some pattern. Schendl takes the verb as the central part of the proposition (as is usual), upon which the complements (the cases) are directly dependent. The valency of verbs is seen as a function of their verbal meaning. The meaning of the verb decides which semantic relations are or need to be expressed. These relations may then be bundled in different ways into semantic roles. These roles are expressed by cases. Through the combination of relations into roles we get a fine-grained picture by means of a limited number of basic elements. It is clear from the above that Schendl's line of approach is semantic. This has its advantages in that we can observe independently how case forms are tied to semantic roles (which might also help syntacticians to make a more well-informed decision about which cases are structural and which inherent). To my mind, however, it would also pay to consider the whole problem simultaneously from a semantic *and* a syntactic point of view. The problem I have with Schendl's description is that it takes no account of the form of the case that the semantic relations/role may take. Thus, in his scheme, the same relations are sometimes expressed by one case sometimes by another (e.g. the 'objective' role with the verb *gewinnan* is expressed by the accusative, with the verb *beheawan* by the instrumental/dative case (see (11)–(12) on p. 425) and with the verb *þolian* by the genitive (see (8) p. 423)). We have to assume that it was possible for OE children to learn this system. If indeed the objective role can appear in all these cases, we have to assume that children learn about the use of case verb by verb, in other words that there is no system. This seems to me highly unlikely. I would like to start from the assumption that the relation between role and case is fairly stable (perhaps less so for the nominative and accusative case, which already in OE could be regarded as structural, cf. Fischer and van der Leek 1987; van Kemenade 1987: ch.3). This would mean that from the semantic side an attempt must be made to bring down the number of possible semantic roles. Only more investigations of this kind will decide whether this is possible.

Nagucka's paper deals with "the use of *to* and *for* in Old English". The questions it asks are in some way related to the ones in Schendl's paper, but the whole set up is far from clear. She is concerned about questions of government and theta-assignment in relation to prepositional phrases. What is the role of P(reposition) in OE? Does it assign case, does it assign a theta-role to the following NP? And, if it does, what do we do with

instances where the same P assigns different cases, or where the same P assigns different semantic roles? What is the relation between the use of P and morphological case? These are in themselves legitimate questions, but the way in which they have been treated does not really throw light on the problem. The examples used are plucked from all kinds of texts so that we do not get a coherent picture of any developments that might be taking place here. In addition, the discussion of the examples themselves is not convincing. On p. 354, Nagucka observes that there are contexts in which the PP alternates with bare N in the dative, but to my mind the contexts are clearly different: in (10) *to ... bærnete* after the verb *beran* is clearly locative (which explains the use of P), while in (11), *þam drihtealdre* used after *beran* expresses a benefactive function. In example (15), *Her Seuerus feng to rice*, *to* is said to assign case to *rice*, but no theta-role, because that must have been assigned by the verb, witness examples such as (16), *Her Seuerus onfeng rice*. Nagucka does not seem to have noticed that in (16) a different verb has been used.

Another topic that is usually well represented in books on English historical syntax is developments in relative constructions. Three papers are concerned with this. Two of these are data-oriented. Schneider deals with the loss of case-marking in wh-pronouns in early Modern English. He is especially interested in the type of constraints that have operated here. The investigation is very carefully set up. It makes use of the so-called VARBRUL program to test which input factors contributed to a given variant's distribution, and what the strength is of each individual factor. The program works well, the results are clear (though not very staggering). He finds three parameters influencing the choice of relative marker in descending order of importance. (i) clause type: objective *who* rather than *whom* is much more frequent in interrogative than in relative clauses; (ii) *who* is far more frequent with preposition stranding than when the preposition precedes; (iii) *whom* is more frequent in more formal language. It is interesting to note that Shakespeare is clearly ahead of his (near-)contemporaries in his use of *who* for *whom*, the difference being 13.5 percent to zero (with the exception of Marlowe (3.6 percent) and Congreve (5.2 percent), who lives later). Concerning the choice between *whose* and *of which*, here clause type (always *whose* in interrogative clauses) and antecedent (±human) are crucial, although there is also room for personal preference. He concludes that *whom > who* is an overt change from below; the desire to have an unmarked *who* in preverbal position is slowly gaining on ideas of correctness. Correctness did not play a role in the *whose/of which* choice; rather, this involves a restructuring of the semantic/grammatical system of English.

The constraints and development in the choice between *whose* and *of which* support a more general tendency in English, which Poussa has termed the 'Great Gender Shift'. She notes in a stimulating paper (which sets out to explain why *this/that* – in contrast to *these/those* – can function

only as *social* deictics with reference to persons in PrDE) that there has been a shift in the English pronoun system. In OE singular pronouns were distinguished for gender, in the plural they were not. Gender gets lost due to morphological attrition, except in the personal pronouns. Here, however, grammatical gender slowly turns into natural gender. The natural gender distinction becomes gradually the main distinguishing force. Since the personal pronoun *it* is closely associated with *this/that, this/that* becomes typically non-human, and can no longer be used with human reference. In the plural this did not happen, presumably because there never was a distinction there in the first place. This development finally opens up the way for the comic/dishonourific use of *this/that* in PrDE (as noted above). The same force also influenced the *whose/of which* choice, *whose* becoming strictly + human, and *of which* - human.

Van den Eynden starts off her study "Relativization in the Dorset dialect" with a quote from William Barnes (1801–1886): "Whereas Dorset men are laughed at for what is taken as their misuse of pronouns, yet the pronouns of true Dorset, are fitted to one of the finest outplannings of speech that I have found" (p. 532). This tells us something about the linguistic perception of William Barnes (the vicissitudes of whose work on the South-western dialect are further discussed by Bernard Jones in this volume) and on the interest that the Dorset pronoun system has for historical linguists. Van den Eynden provides a synchronic analysis of relative clauses and, where possible, their historical analogues, with the ultimate aim of acquiring a better understanding of the directionality of linguistic change. She also compares the Dorset situation to surveys of neighbouring dialects and Standard English. Her survey thus differs from some of the other corpus studies in this volume in that it poses these larger questions and is not restricted to a narrow interpretation of the data. Dorset relative constructions are of interest in that they are to some extent a reflex of an earlier stage. Thus, *that* is still more frequent than *wh*; the use of *who* is very low (which corroborates the hypothesis that *who* entered the language via more complex styles, cf. Romaine 1982); there is more frequent use of resumptive pronouns (cf. also Moessner below) and of 'relative concatenated constructions' (also called successive cyclic wh-movement) of the type *the place that you say that is your birthplace* (she gives examples both with and without retention of the complementiser *that*). Finally she notes the use of 'connecting' *which*, which was prevalent especially in the 15th century. This use may have resulted from an internal development (cf. Reuter 1938), or (the more commonly accepted hypothesis) it may have been influenced by Latin/French. Its presence here may offer support for Reuter's view. It is also quite possible, however, that the native and Latin types can and must be distinguished. The example that van den Eynden gives of connecting *which* (*But then when my gran died,* **which** *we courted for six years, when my gran died, (...) hm I got married* p. 533), is quite different from the Latin type discussed below, in that

which is used as a very loose (adverbial) connector, and there is no longer any antecedent to speak of. In the Latin type, *which* normally refers back to something in the previous discourse, and can usually be replaced by *and this*. There is clearly room for further research here.

Moessner is concerned with the development of what she calls 'functional amalgamation' in relative clauses, i.e. how and when the double functions of the relative pronoun ((i) as a connector between two clauses, and (ii) as the identifier of the syntactic function that the relative has within the subclause) are becoming concentrated into one form. The first part of the paper provides a typology of early Modern English relative types, the second part deals with the/their diachronic development. The types and properties which Moessner distinguishes in a table on p. 342 with respect to the presence or absence of functional amalgamation seem to me to be not well considered. One problem is that the author does not distinguish between 'true' resumptive pronouns and 'pseudo' ones. When there is a true resumptive pronoun as in her examples (1) and (2), (e.g. *And both like serpents are,* **who,** *though they feed/ On sweetest flowers, yet* **they** *poison breed* p.337), there is indeed no functional amalgamation because *who* can be said to provide the connection and *they* the syntactic function in the relative clause. However, when the (resumptive) pronoun belongs to another subclause, *not* the relative clause, then it makes no sense to talk about absence of functional amalgamation, because the two elements are not a function of one clause. This is the case, for example, in her type III (p. 339): *the winter's wind,/* **Which** *when* **it** *bites and blows upon my body,/ Even till I shrink with cold, I smile.* Here *it* is not strictly part of the relative clause; it functions as subject of the temporal subclause, and cannot be left out. This relative construction is in fact quite normal from a modern English point of view (*which* leaves a trace after *smile* and is moved to the COMP position of the same clause), except that *smile* does not normally take an object.[4]

A similar usage of *which* can be noted in another example given by Moessner: *Which when Beelzebub perceiv'd, ..., with grave/ Aspect he rose* (Milton *Paradise Lost* II, 299ff., Moessner p. 340). This one is clearly different from the above in that it is not a normal PrDE construction; here *which* moves to the COMP position of a higher clause. For this last example, I would prefer to follow van der Wurff's terminology (which is based on a clearer understanding of the structure involved). Van der Wurff (1990) calls this a construction with only one gap (his type B) to distinguish it from the parasitic gap construction, which has two gaps (his type A). Moessner's type V is an example of van der Wurff's type A: ... *she whom all men prais'd, and* **whom** *myself,/ Since I have lost* e, *have lov'd* t (p. 341), where *t* indicates the real gap, and *e* the parasitic one.

The problem with Moessner's analysis is that it mixes up two separate developments in the history of English relatives: the use and decline of resumptive pronouns, and the rise of constructions with a gap in an

adverbial subclause as discussed by van der Wurff. They are only connected in so far as gaps in adverbial subclauses are sometimes filled by resumptive pronouns (in order to make the clause structure less dense, cf. Franz 1909: 307). Van der Wurff has shown that the rise of constructions with a gap in adverbial clauses is due to the influence of a Latin (and possibly French) formal written style. This is the usual interpretation, as acknowledged, but rejected, by Moessner (she refers to Abbott 1897: 169ff., Franz 1909: 307ff., Görlach 1978: 118). Resumptive pronouns, on the other hand, are a feature of Old English (cf. Abbott 1897: 169). They decline through changes in the relative pronoun system and through the rise of a written standard. Because Moessner mixes up these two developments in her typology of relative clauses, she can come to the conclusion that all types already existed in OE, and that there was no Latin influence involved in their development. This position seems to me untenable.

I realise only too well that there are a large number of professional and insightful articles that I have not had room to discuss here, either because they deal with a rather peripheral topic or because their subject is phonological or morphophonological. I do not want to leave unmentioned, however, the original paper by David Denison on the development of what he has termed the 'information present': the use of the present tense in a past tense context as in *Jim tells me that the forecast is bad*. He manages to connect the use of this present in English with the rise of the indirect passive, and produces some very interesting data from other Germanic languages that lack both the indirect passive and the information present or, like English, have both.

All in all, this book should be welcome to students of (English) historical linguistics. It is very broad in its range, providing a good idea of what is happening in the field. The book is well presented. Apart from a subject and author index, it even provides us with a list of titles of textual sources used in the articles. It is a pity, though, that no indication is given of the affiliations of the authors.

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Notes

1. I am very grateful to Wim van der Wurff and Willem Koopman for their careful reading of the text and their suggestions towards improvement.
2. There is some confusion in van Kemenade's article about the different types of modal that exist. Van Kemenade makes a distinction between deontic modals, which have main verb characteristics, and auxiliaries, which have epistemic meaning. Denison (1990) gives a three-way distinction: dynamic root modals, deontic root modals and epistemic modals. Deontic modals already have modal meanings, dynamic modals do not. The meanings that

van Kemenade gives for the 'deontic modals' on p. 293 (i.e. *cunnan* 'know', *willan* 'want' etc.) are in fact the ones that belong to the dynamic modals. In addition, only the dynamic modals are clearly different in structure in that they take NP or a clause as complement. Deontic and epistemic modals only take infinitival complements.

3. He also mentions other explanations given for this order, such as the belief expressed in Söderlind (1951) and Traugott (1972) that it was used for emphasis.

4. The syntactic analysis of this sentence depends rather heavily on one's interpretation of it. The complete sentence in Shakespeare (*As you like it* II,1,5) reads as follows:

Here feel we not the penalty of Adam,
The seasons' difference [= change], as [= such as] the icy fang
And churlish chiding of the winter's wind,
Which when it bites ...

To my mind, *which* cannot really be interpreted as a loose connector here ('and this'), as it can in the example from Milton given below. *Which* has a clear antecedent (*the winter's wind*); the interpretation of *which* as *and this* would not fit the syntax of the passage. Also there is a connection between *smile* and *which*, which again is not present between *which* and *rose* in the Milton example. For these reasons I would prefer to interpret *which* as the object of *smile* ('at which I smile').

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