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### ***Letter from the Editors***

*So, here is VIEWS 2 (2)... or should we rather call it Vienna Journal on the Status of Old English -ian verbs? As you will notice, the debate on the fate of Germanic class II, which Roger Lass started in the last issue, has provoked another three (!! ) contributions approaching the problem from various angles. If articles on the subject keep reaching us at this rate, it might not be over-optimistic to presume that within another six or so issues, we'll have the first linguistic problem that is solved once and for all. Or shall we ... ?*

*As you may also notice, the number of international linguists discovering something Viennese somewhere deep within them is increasing steadily. Thus, we are proud that VIEWS 2(2) includes contributions by Alfred Bammesberger, Eichstätt, Veronika Knieza, Budapest, Roman Kopytko, Poznan and Roger Lass, Cape Town. - So, wherever you are, if you feel like following suit - don't hesitate to do so. We can be reached in the following three ways (mind the **new e-mail** address):*

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*The Editors*

**Note to contributors:**

We would like your contributions to reach us on disks (or via e-mail) in any standard IBM compatible word processing format (MS Word, Word for Windows, Wordperfect [for Windows], Word Star, R.T.F., ASCII ...) together with a printout showing character format, special symbols, formulae, tables etc. If you find it helpful to refer to a style sheet, we suggest that of the MLA.

**Impressum:**

**Eigentümer, Herausgeber & Verleger:** VIEWS, c/o Institut für Anglistik & Amerikanistik der Universität Wien, Universitätsstraße 7, A-1010 Wien, Austria. **Für den Inhalt verantwortlich:** Christiane Dalton-Puffer. **Redaktion:** Ute Angerer, Christiane Dalton-Puffer, Gunther Kaltenböck, Arthur Mettinger, Hans Platzer, Nikolaus Ritt, Herbert Schendl, Barbara Seidlhofer. Alle: c/o Institut für Anglistik & Amerikanistik der Universität Wien, Universitätsstraße 7, A-1010 Wien. **Herstellung:** VIEWS.

## ***Letter to the Editors:***

*Dear colleagues,*

*thank you very much for treating me to the third issue of VIEWS. The authors have made valuable contributions again and the idea to engage readers in discussions is laudable. [...]*

*I wish to commend 'It-clefts' for the width and well-groundedness of approach. I can only add that there might be a further difference between which and that which may (also) be responsible for the behaviour of these relative pronouns as noted on page 12. I have in mind the restrictiveness of that (which it shares with the homophone demonstrative determiner/pronoun) in contradistinction to the neutrality of which in this respect. I also believe that Brazil is wrong when attributing discourse features, like new and old information, to the nature (direction) of nuclear tone rather than to its location (cf. the discrepancy between Kaltenböck's statement 'We may say that there seems to be a strong correlation between the location of the nucleus and new information' (p. 20 with which I agree) and 'The fall as predominant tone on the focus is also perfectly in line with Brazil's (1975) concept of 'proclaiming' and 'referring' tones which postulates that the "choice of falling tone [...] marks the matter as new".' The following author's sentence seems to confirm my doubts: 'In our data all foci are associated with new information ... ' (p. 21). My own research into intonation has led me to a conclusion quite different from Brazil's as regards the nature of intonation.*

*After the argumentation presented on tenseness, I am even more convinced that this feature is not distinctive and, moreover, that the term is a mere construct without psycholinguistic foundation, where 'short' vs. 'long' and [+r] would suffice. Still there are some worthy observations and morphonological variations in Nádasdy's paper.*

*I subscribe to Dalton-Puffer's view that 'it would be most profitable to regard the relationship between inflection and derivation in terms of a scale where either end serves as a kind of prototypical center' (p.44). In addition to the proof adduced in the paper, it could also be reasoned that if the criterion for discriminating inflection from derivation hinges on the change of the part of speech, while there are, generally speaking, no clear-cut divisions between the parts of speech, (cf. -ing forms used as verbs, nouns and adjectives), then the former discrimination cannot be sharp either.*

*Yours sincerely*

***Dr. Boris Hlebec, Beograd***

## ***Old English ascian: word-formation and etymology***

*Alfred Bammesberger, Catholic University of Eichstätt*

1. In his discussion of the derivational patterns found in Old English weak verbs of class two, Lass (1993) rightly points out that the majority of the members in this conjugation can be analyzed as being drawn from nominal bases: OE *andswarian* ‘answer’ (< Germanic *\*andaswarōjan-*) is certainly derived from *andswaru* ‘answer’ (< Gmc. *\*andaswarō-*). In this particular instance all the available evidence definitely points in the same direction: *and-* is found in nominal compounds only and corresponds to (unstressed) *on-* in verbal formations (cf. Campbell 1959: 31). The stress on the first syllable of the weak verb *andswarian* is readily explicable on the basis of the nominal formation *\*andaswarō-*, which regularly has initial stress. And the meaning of the verb is also easily accounted for on the basis of some expression like ‘give an answer’.

2. Lass takes a sample of 205 verbs and finds only 26 “Isolates” in this list, i.e. verbs “with no obvious relations” (32). I certainly agree with this main conclusion. I want to show that the number of the “isolates” can be reduced, and therefore the conclusion of Lass’s paper that “class II is an overwhelmingly derivational category (ratio derived : nonderived = 7 : 1)” (32) is fully acceptable. I will first single out one instance of his “isolates”, namely *acsian* ‘ask’, where the etymological account can be further refined.

3. The precursor of Modern English *ask* could appear in Old English as *ascian* and (with metathesis) *acsian*. The cognate forms Old Frisian *askia*, Old Saxon *escon*, and Old High German *eiscōn* (Modern High German *heischen* with secondary initial *h-* [*~ heißen*, see Seebold 1989: 302]) clearly indicate a starting-point Gmc. *\*aisk-ōjan-* with a diphthongal root. The further etymology of this verb is not totally clear, but a few valuable indications can be found in the relevant dictionaries.

4. Sanskrit offers a verb *icchāti* meaning ‘looks for’. It is permissible to posit an underlying formation in *\*-sk-e-* for Indo-European. The root would then be posited in the shape *\*ays-* (Pokorny 1959: 16; Mayrhofer 1992: 271 posits the root with initial laryngeal *\*h<sub>2</sub>-* and gives a full listing of the etymologically cognate forms in the individual Indo-European languages). The starting-point IE *\*is-sk-e-* seems to be fully justifiable. We would expect this present in

\*-sk-e- from the zero-grade of the root \*ays- (morphologically comparable to \*pr̥sk-e- [5.] from the root \*prek-) to appear in Germanic as \*isk-; originally it should have followed the pattern of thematic verbs. But in Germanic apophony (ablaut) was a productive process. Nominal formations could be drawn on the basis of what was or could be perceived as the zero-grade of roots. Thus a zero-grade \*bid- (from the root \*beid- ‘wait’ [OE *bidan*]) could have an o-grade \*baid- (OE *bad* ‘expectation’) by its side. Although \*isk- was not etymologically the zero-grade of a root, it was quite possible that the morphological pattern of \*bid- ~ \*baid- was extended and led to the formation of \*aisk-. We could therefore imagine that there was a nominal formation of the type \*aisk-o- possibly meaning ‘the action of looking for something’.

5. On the basis of \*aisk-o- a weak verb \*aiskojan- could be formed in accordance with the pattern found in \*andaswarojan- (~ \*andaswaro- [1.]). \*aiskojan- has a formal correspondence in \*furskojan- > OHG *forscon* (MHG *forschen*), which is also based on an sk-present \*furska- < \*furhska- < IE \*pr̥sk- (cf. Lat. *posco* [Pokorny 1959: 821f.]). In both cases we seem to need nominal formations as bases for the verbs in \*-ojan-, and these nominal formations arose out of sk-presents.

6. A nominal basis of OE *ascian* is perhaps vaguely recognizable in *æsce* ‘asking, inquiry’. There may also have existed a weak verb of class 1 *æscan* (possibly pointing back to \*aisk-ijan-), but the documentation for this verb is rather weak. Since Lass accepts *wise* ‘way, direction’ as starting-point for *wisian* ‘guide’ (29), one could argue that *æsce* would be conceivable as starting-point for *ascian*. But I personally prefer the explanation given above that \*aiskojan- is based on a substantive \*aisko-.

7. From the historical point of view *ascian* is therefore by no means isolated, it rather confirms the main tenets of Lass’s argumentation. Some of the further 25 instances of “isolates” may also ultimately be explained one way or another. Thus it should be pointed out that *dogian* ‘endure’ is after all found only once, namely in the verse *Wulfes ic mines widlastum wenum dogode* (*Wulf and Eadwacer* 9). It is still worth pointing out that more than a century ago Hicketier (1887: 579) wanted to emend the form to *hogode*, which would certainly make sense (see my remarks in 1979: 37). With regard to *copian* ‘steal’ it must be stressed that this verb is a hapax. Meritt (1968: 105) commented on the gloss *conpilabat · stael copade* (Wright-Wülcker 1884: 379) as follows: “*copade* is related to *ceapian* ‘bargain, trade’ with the vowel as in *landcop* and *lahcop*”. Although the question as to how the vowel of *copian* is to be explained admittedly must remain open (cf. my remarks in 1979: 28),<sup>1</sup> the verb apparently is less “isolated” than one might have thought.

These observations can hardly be considered as in any sense settling the issues raised by *dogian* and *copian*. But the considerations here submitted confirm Lass's main argument that the verbs in *-oian-* can basically be viewed as being "derived". Details of the derivation processes involved still require further investigation.

### Note

<sup>1</sup>The further problems concerning the derivation of OE *ceapian* will be dealt with in a separate article devoted to this verb and to *cursian*; for the moment Wissmann 1938: 20-25 and Bammesberger 1984: 89f. may be consulted on the derivational pattern underlying OE *ceapian* ~ *ceap*.

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## ***Inflection, derivation and zero - or: what makes OE and German derived denominal verbs verbs?***

*Dieter Kastovsky, Vienna*

1. Since *VIEWS* is intended to be not just another journal, but a real "forum" for baked and half-baked ideas<sup>1</sup>, I could not resist the temptation to join the ongoing battle about the status of infinitival *-ian* in Old English (cf. Dalton-Puffer 1992, 1993; Lass 1993a, b; Ritt 1993). This all the more so, since this controversy provides me with a beautiful excuse for returning once again to the favourite topic of my linguistic youth: *zero*<sup>2</sup>. Moreover, it is by no means restricted to OE *-ian*, as the discussion might seem to imply, but also has ramifications for cases such as German denominal verbs in *-en* (e.g. *krön-en*, *ehr-en*, etc.) or Latin denominal verbs in *-a re* (e.g. *coron-a re*, *honor-a re*, etc.), which are exactly parallel in not containing any overt derivational affix apart from the verbal inflectional endings.

As I see it, the *-ian*-controversy consists of the following sub-issues:

1. Can inflectional morphemes also have derivational function, when no overt derivational morpheme is present in the derived lexeme in a derivationally related word-pair? Thus, assuming that *cynehelm* and *cynehelmian* are derivationally related, and that the latter is derived from the first, can *-ian* serve both as a marker of inflection (infinitive) and of derivation (verbalisation + 'provide with' as the semantics of the derivation)?
2. In what way is this situation related (or relatable) to the Modern English state of affairs where there often is neither a derivational nor an inflectional morpheme, cf. *crown* sb. : *crown* vb., *honour* sb. : *honour* vb., etc., and how is this relationship generally handled in word-formation theory?
3. Are derivational patterns always semantically coherent, i.e. does a specific suffix always have just a single meaning in order to count as properly derivational?
4. What is the history of the derivational pattern(s) under discussion? What is the status of the stem-formatives (thematic elements) in pre-OE? Are there word-class neutral roots in Germanic/Old English/Modern English/German/Latin? In what way did the overall morphological structure of the language change since the IE period?
5. How did the Modern English suffix *-en* of *darken*, *weaken*, etc. originate?

A detailed discussion of all these synchronic and diachronic problems is of course not possible here, and I will have to confine myself to a few remarks that simplify matters considerably. I will also assume familiarity with the ongoing debate and therefore refrain from continuous reference to the papers in question, but will instead simply present my own *views*.

2.1. I will begin with the synchronic-theoretical aspects of this debate. The first sub-question concerns the demarcation of inflection and derivation. Within a framework based on the distinction between lexeme, word-form and word (cf., e.g., Matthews 1974, 1991; Lyons 1963, 1977; Kastovsky 1992), appropriate for languages that have at least some inflection, inflection is usually defined as the production of the word-forms (inflectional realisations) of a lexeme, and word-formation as the creation of new lexemes. The distinction looks fairly straightforward in theory, but in practice it is by no means as clear-cut as one might want, because it of course begs the question when we are entitled to speak of a new lexeme rather than a word-form. A case in point is adverb-formation with *-ly* in English. Assuming that adverbs constitute a separate word-class, adverb formation would seem to belong to word-formation, since a typical feature of derivation is change of word-class. On the other hand, the adverbial suffix *-ly* does not add any semantic content to the adjectival basis, which would normally be the case with a derivational affix (except for transpositional suffixes such as *-al*, *-ar*, *-ic* etc. in adjectives like *musical*, *nuclear*, *atomic*). Moreover, when we compare *He smokes heavily* with *He is a heavy smoker*, it would seem that the adverb *heavily* is just a syntactically conditioned variant of *heavy* much as the accusative of a noun is a syntactically conditioned (verb-governed) variant of the nominative/unmarked form of the noun in German or OE. From this point of view, *-ly* looks rather like an inflectional suffix. In fact, both analyses are found in the literature. Marchand (1969) does not treat *-ly*-adverbs in his handbook, while Francis (1958: 283) regards adverb formation as a derivational process. A similar case can probably be made for the aspectual affixes of the Slavic languages, when one compares them with corresponding phenomena in German and English: from a German point of view, they would seem to function like lexical (i.e. derivational) morphemes creating "Aktionsarten", from an English point of view they look like grammatical (i.e. inflectional) morphemes signalling aspectual distinctions.

There is thus a fuzzy area between word-formation and inflection, and the demarcation simply depends on what we regard as a separate lexical item. But do OE *-ian*-verbs, and German or Latin derived verbs such as *cynehelmian/krönen/coronare* 'to crown', *arian/ehren/honorare* 'to honour', etc. represent a similar case? I don't think so.



First of all, we are dealing with inflectional languages, where the notion of paradigm plays an important role. Thus, verbs in these languages are characterised by inflectional endings in all forms, infinitive included. As Niki Ritt has pointed out, if we treat the infinitive morpheme as derivational, we would have to do the same with the other inflectional morphemes signalling person/number, tense and mood - a move which is clearly counterintuitive.

Moreover, it conflicts with the original function of the infinitive, which was partly nominal (cf. the inflected infinitive in OE). Under this analysis, we would have a morpheme that on the one hand derives verbs from nouns, i.e. has verbalising force, but at the same time provides nominal characteristics to the verb, i.e. has nominalising force, cf. also G *das Rauchen, das Essen, das Trinken*, etc.

Finally, such an analysis is not congruent with the function of the infinitive with strong verbs, where it clearly has inflectional function only, and it would have to be extended to class 1 weak verbs, where things are much messier than in class 2. Moreover, both in class 1 and class 2 of the weak verbs we find a considerable number of primary verbs (cf. 3.2. below), where again the infinitive morpheme does not have derivational force.

There is thus only one possible conclusion: *-ian* must be regarded as a purely inflectional suffix, just as the other infinitive allomorph, viz. *-an*; both are morphologically conditioned, with *-ian* marking class 2 and a subclass of class 1 of the weak verbs. They cannot at the same time have derivational force. This also means that *-ian* cannot be the source of the *-n*-suffix in *darken*, etc., especially since the infinitive morpheme was lost altogether in the subsequent history of the language.

2.2. We thus have to regard those *-ian*-verbs which are clearly denominal as derivatives without an explicit derivational marker, just as the parallel cases in German<sup>3</sup> or Latin. The inflectional endings primarily indicate person/number/tense/mood/finiteness distinctions, and secondly of course also appurtenance to the category verb, in so far as they are restricted to this category - but this is not a derivational function, at best a categorial index, much like the stress pattern *recórd* vb. ~ *récord* sb. in Mod. English. After all, the same morphemes (or rather allomorphs of the same morphemes) also occur with non-derived verbs, where they cannot have any derivational force. Synchronically speaking, such non-derived verbs are found in all inflectional classes, class 2 included, even if historically they may have originally been denominal. But the direction of the derivation may have become reversed in the course of time, cf. Kastovsky (1968: 90ff. and 3.2. below). German and Latin exhibit the same structural pattern: denominal derivation without an explicit derivational morpheme but an inflectional paradigm characterising the respective forms as verbs. This, in fact, is the only difference between these languages and Modern

English - and even this difference is not quite as striking if seen in the proper perspective. Even in English, derived verbs such as *crown*, *honour*, *clean* have some inflectional endings such as 3rd pers. sg. *-s* or preterit and the participles. Are these then derivational in the absence of a derivational morpheme? Hardly so. Thus the difference boils down to the presence vs. absence of the infinitive morpheme - semantically the most neutral morpheme in the verbal paradigm, and therefore lost early in the history of English on its way to a generally word-based morphology (cf. Kastovsky 1992). And the infinitive, as we have just seen, cannot be regarded as a derivational morpheme. Thus, there is in principle no genuine difference between Modern English affixless derivation and OE weak verbs from the point of view of derivational morphology, only from the point of view of inflectional morphology.

2.3. This brings me to the treatment of affixless derivation in general, which I will not, however, discuss at length.<sup>4</sup> Looked at from a purely formal point of view - as is done in many recent treatments of word-formation - conversion (or whatever term we may use for this process) is usually separated from suffixal derivation and treated as a phenomenon *sui generis* (sometimes even as non-derivational). In a semantics-oriented framework as the one developed by Marchand (1969) and also adopted here, affixless derivation is regarded as exactly parallel to suffixal derivation - sharing the semantic patterns. The same, incidentally, holds for deverbal nouns such as *cook*, *run*, *dump*, *whistle*, etc. According to Marchand, word-formation is based on syntagmatic structures, i.e. on a determinant/determinatum (modifier/head) construction. In suffixal derivatives, the suffix acts as head. In the derivatives discussed here, a zero suffix (zero morpheme) is usually postulated as derivative element in order to make these formations fit into the syntagmatic mould and to account for the greater semantic complexity of the derived lexeme. Consequently, the denominal verbs under discussion should also be regarded as zero derivatives, i.e. as *cynehelm/Ø-(ian)*, *krön/Ø-(en)*, *coron/Ø-(are)*, etc. And just as a suffix normally represents more than one meaning (cf. Mod. E *-er*, *-ing*, *-ise*, *-ify*), zero does the same. Thus morphological derivational patterns are usually not semantically coherent, i.e. a specific suffix does not normally have a single meaning. Whether one adopts the zero-device or whether one prefers to speak of affixless derivation is a matter of word-formation theory in general. What is relevant in our context is simply that the OE *-ian*-verbs are in no way different from German *-en-*, Lat. *-are-* and Mod. English conversion verbs.

3.1. Let me now turn to the diachronic aspect of the problem, which will (hopefully) corroborate my synchronic analysis, but will also contain a few (more or less half-baked) speculations about the morphological structure of IE and Germanic.



*landing, mowings, bedding*, let alone the semantic range of zero derivatives. Despite this multifunctionality such suffixes have always been regarded as properly derivational. I will therefore assume that the stem formatives (thematic elements) were more than mere markers of inflectional classes, i.e. they performed a genuinely derivational function.

The oldest layer of the morphological system and the incipient restructuring is still recognisable to some extent with the strong verbs, which do not have a stem-formative element. Cf., however, mixed forms such as *sittan, licgan, hebban, swerian*, where the present tense forms (and the infinitive) have a thematic element (= /ja/ of class 1 of the weak verbs) absent in the other forms, indicating that a shift from the primary (strong) verbs to the secondary (weak) verbs as the typologically dominant category was taking place at a rather early date.

3.2. This restructuring continued in Germanic and in the individual Germanic languages, presumably under the influence of the growing number of weak verbs, which historically speaking have to be regarded as a derived category, i.e. originally, all weak verbs were either denominal (desubstantival or deadjectival) or deverbal. They thus had the following structure:

$$(2) \quad [[\text{base}_{N, A, V} + \text{stem formative/theme}_V]_{V_{\text{stem}}} + \text{inflectional ending}]_{\text{word}}$$

The restructuring first of all resulted in a general shift from root- to stem-based morphology, i.e. the structure of the weak verbs with a stem-formative became the typologically dominant pattern. Accordingly, the infinitive/present tense forms of the strong verbs were also reinterpreted as stems, and also as base forms of the whole paradigm, a development which was certainly influenced (if not triggered) by the development of a binary tense system, which replaced the previous aspect system. The "present" tense came to function as an unmarked base form, and the preterit/2nd participle forms were marked by a dental suffix. This structure was also transferred to the strong verbs, where the infinitive/present came to be reinterpreted as an unmarked base, rather than being on the same level as the other forms.

At the beginning of the German period - and still in Gothic -, the stem-formatives were clearly recognisable. They could thus be regarded not only as overt class markers, but also as derivational morphemes, cf. the description in Krause (1953: 223 ff.):

- (3) a. class 1 /ja/ ~ /i/: *hugjan* 'to think' < *hugs* 'mind', *taiknjan* 'make a sign' < *taikns* 'sign', *fulljan* 'to fill' < *fulls* 'full', *lagjan* 'to lay' < *licgan* 'to lie' - semantics: causatives, intensives, iteratives, ornatives, etc.
- b. class 2 /o:/: *karon* 'to care' < *kara* 'care', *luston* 'to desire' < *lustus* 'desire', *fraujinon* 'to rule' < *frauja* 'master', *skalkinon* 'to serve' < *skalks* 'servant', *galeikon* 'to compare' < *galeiks* 'similar', *wairþon* 'to value, estimate' < *wairþs* 'worthy';

- c. class 3 \*/e:/ > Goth. /a/: *weihan* 'to consecrate' < *weihs* 'holy', *saiúrgan* 'to have sorrows' < *saiúrga* 'sorrow'; semantics: duratives, ingressives;
- d. class 4 /n/: *fralusnan* 'to get lost' < *fraliusan* 'to lose', *gawaknan* 'to wake up' < *wakan* 'to wake', *fullnan* 'to get full' < *fulls* 'full', *gabignan* 'to get rich', < *gabigs* 'rich'; semantics: inchoatives, ingressives.

Class 3 stands out in so far as the stem formative is no longer as clearly recognisable as in the other classes. Moreover, it also already contains many primary verbs, and it is therefore not surprising that this class disintegrated relatively early. It is also obvious that there is no semantic coherence within these classes, since all of them express more or less the full range of meanings that can be expected for denominal verbs<sup>6</sup>, with the exception of class 4, which seems to be restricted to inchoatives and ingressives. But this does not mean that the stem-formatives did not still function as derivational elements. As I have already mentioned, suffixes are often linked to more than one derivational meaning. The behaviour of the stem formatives is thus exactly like that of any other extremely productive suffix, and in particular like the range of zero derivation in Modern English, of which they are the antecedent. If this analysis is correct, then we still have a case of explicit derivation, where the stem-formative has two functions: it serves as a derivational suffix and at the same time as an overt class-marker indicating the inflectional class to which the respective verb belongs - but it does not have inflectional function itself, since it does not mark any verbal inflectional category such as person, number, tense, etc.

There is another interesting development, however. Already in Gothic we come across a substantial number of primary verbs in these four classes. This can be explained as follows. One part is probably not really primary; the lack of a corresponding base is coincidental and due to the restricted documentation of the language. With another, larger part, the base has not survived, so that the historical derivative has now synchronically become a primary verb. In such instances, secondary deverbal derivatives may be formed, which in turn paved the way for a reinterpretation of the direction of derivation with other, semantically similar verbs (cf. Kastovsky 1968: 90f.). This is why already in Gothic - and certainly to an even greater extent in OE - we have zero-derived nouns from weak verbs (cf. the extensive material in Kastovsky 1968). It also explains the group of so-called "co-derivatives" in Lass (1993 b), which are just instances of regular deverbal nouns from reinterpreted verbs whose denominal source is no longer transparent.

3.3. Let us now finally turn to Old English. The crucial difference between Gothic and OE is that the stem-formatives are no longer systematically recognisable, as any systematic synchronic analysis demonstrates. Due to the phonetic erosion of unstressed syllables they have either merged with the final

inflectional endings or with the preceding base. Thus the marker of class 1, /ja~i/, has produced *i*-umlaut and gemination where possible, in this manner leaving a trace in the stem and producing wide-spread morphophonemic alternations. But subsequently it was lowered to /e/ or lost altogether. Consequently, a morphological segmentation of the class 1 paradigm does not yield any recurrent element that can be identified with a stem-formative. Although in a form like *ner+ed+e* < *\*naz+i+d+a* the *-e-* of *-ed-* goes back to the stem-formative /i/, it now has to be interpreted as part of the preterit/past participle morpheme (both superficially and underlyingly), and it is absent (deleted) in instances such as *cepte*, *sette*, *sende*. In Kastovsky (1971: 100), which was based on a fairly abstract phonological framework, I had assumed that this reinterpretation took place at the end of the OE period, but I now think that it must be located at the beginning of the OE period. It is probably connected with the phonemicisation of *i*-umlaut and the fusion of the stem formative and the person/number endings in the present. After these developments, no consistent segmentation of the stem-formative was possible any longer, and its formal remnants were now regarded as part of the person/number endings or the preterit/past participle morphemes. The same holds for class 2 verbs of the type *luf+od+e*. Again, *-o-*, the formal remnant of the stem-formative, has become part of the preterit morpheme, since in the present there is no consistent counterpart.

Thus Roger Lass is right in a way when he calls *-ian* a geriatric derivational suffix as against Niki Ritt's assumption of an embryonically derivative status. But for the reasons discussed above this analysis misses an important point: *-ian* was never derivational, not even geriatrically so, unless one assumes that after the loss/merger of the stem-formative with the inflectional endings the latter assumed derivational function. Incidentally, this would of course also have to be assumed for class 1 weak verbs, where in many instances the derivational relationship with the nominal/verbal base is still transparent, although the derivational process as such was no longer productive. Such a dual function, however, does not seem to me acceptable. The only solution (at least within the framework adopted here) thus is to assume that an overt morphological marker was lost, but that the function it signalled still existed - and was not expressed overtly any longer. One way to handle this is simply to admit affixless derivation (conversion). Another is to assume replacement by zero ( $\emptyset$ ), which thus "makes up for phonological losses" (cf. Kastovsky 1980). The derivational patterns in question thus are the genuine antecedents of Modern English zero derivation. The same holds, by the way, *mutatis mutandis* also for deverbal nouns.

3.4. Finally a remark on the origin of Mod. E. *-en* as in *darken*, *fasten*, etc. Christiane Dalton-Puffer relates it to the infinitival *-ian*-morpheme. For the

reasons given above I have my doubts. Why should a grammatical morpheme, that is progressively getting lost, adopt a derivational function? More seriously, why should it be preserved in this derivational function, when everywhere else *-en* is lost? The solution has to be sought elsewhere. OE already had an *-n*-suffix forming denominal verbs, cf. *berht-n-ian* 'glorify' < *berht* 'bright, brightness', *lac-n-ian* 'to heal' > *læce* 'physician', *þreat-n-ian* 'to threaten' < *þreat* 'threat'<sup>7</sup>. Whether this was exclusively due to misanalysis of zero-derived verbs whose base ended in *-n* such as *fægen-ian* 'to rejoice', *open-ian* 'to open', *tacen-ian* 'make a sign', as suggested in Marchand (1969: 271, following Raith 1931), or whether these were at least partly also relics of the Germanic class 4 verbs would need additional investigation. But Christiane Dalton-Puffer is certainly right in assuming that continuous reanalysis of stems ending in *-n* contributed to the growing productivity of this suffix. In view of the discussion above, however, we do not need to involve the infinitive ending *-ian* in this process; *-n-* had already been a suffix in OE and simply continued to be productive on a moderate scale, but eventually only with adjectives as bases. This simply corroborates the picture we already have: after the demise of the stem-formatives, the preferred derivational pattern producing verbs in the Germanic languages is zero (or affixless derivation). This is of course more obvious in English (and Afrikaans), where verbal inflection has been reduced to a minimum. But it holds for Modern German as well, and it held for OE. Explicit, suffixal derivation is the exception, even with the Latinate vocabulary, where *-ate*, *-ify*, *-ize* have become only moderately productive. This, I think, is the major riddle behind all this. Why is it that we have so many overt suffixes for deverbal derivation, but so few (if any) for denominal verbs? Maybe someone else has some interesting *views* on this, for I don't, at least not at the moment.

4. My thanks go to Christiane Dalton-Puffer, Roger Lass and Niki Ritt for opening this discussion and prompting me to formulate more coherently some ideas that I had cursorily expressed here and there, without realising that this topic deserved a more thorough investigation. I have only scratched the surface in this contribution, and much I think has still to be done to sort out the function and structural value of the Germanic stem-formatives. Also, my separation of inflection and derivation may be too rigid, and a more flexible framework might be more appropriate. But at the moment I cannot reconcile this with my other theoretical assumptions about the functioning of word-formation and its interaction with other modules - at least in the Germanic languages, which are the subject of this discussion. On the other hand, it may be exactly shifts like the one discussed here that can only be understood if we revise our conception of the interaction of word-formation and inflection as part of an overall morphological component.

## Notes

<sup>1</sup>Cf. Roger Lass's remarks at the beginning of his contribution to this volume. I am grateful to the editors for having provided me with a copy of Roger Lass's reply included in this volume, which greatly helped to make my own views on this issue more precise.

<sup>2</sup>Cf. Kastovsky (1968, 1980). Some of the problems discussed in the ongoing controversy had also been touched upon in these two publications, although not in greater detail and within a much more limited framework than is available today (cf. esp. Kastovsky 1968: 43, fn. 65; 82, fn. 9; 1980: 233 and fn. 32).

<sup>3</sup>Note that in treatments of German word-formation the infinitival *-en* is also sometimes interpreted as a derivational morpheme, cf. Fleischer (1976: 314-315): "Das Verb...verfügt heute nur über ein einziges Verbalisierungsmorphem, das *-(e)n* des Infinitivs...Wohl aber ist zu unterscheiden zwischen Ableitungen lediglich mit Hilfe des Verbalisierungsmorphems *-en* einerseits und solchen mit Hilfe von Erweiterungen dieses Morphems (*-el-n*, *-ig-en* usw.) andererseits.

<sup>4</sup>For a more detailed treatment, cf. ch. 1.4. of my dissertation (Kastovsky 1968), as well as Kastovsky (1980, 1986).

<sup>5</sup>Such an analysis would in fact provide some morphological support for the Nostratic hypothesis according to which IE and Semitic are genetically related.

<sup>6</sup>Cf. Marchand (1969: 368ff.) for a corresponding survey of zero derivation.

<sup>7</sup>Note that Roger Lass (1993 a: 29f.) wrongly lists some verbs containing this suffix among his pure *-ian*-derivatives, e.g. *diht-n-ian* (: *diht*), *droht-n-ian* (: *droht*), *wit-n-ian* (: *wite*). Similarly he includes some verbs with the suffix *-s-* among his *-ian*-derivatives, e.g. *ben-s-ian*, *cnyl-s-ian*, *coc-s-ian*, *hlyn-s-ian*, *ier-s-ian*, *mær-s-ian*, *met-s-ian*, *swin-s-ian*. In each of these, derivation is effected by the suffix *-n/-s-*, and not by the infinitive morpheme.

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## *The post-conquest lexical elements in the Peterborough Chronicle*

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### 0. Introduction

If one is interested in the chronology of lexical borrowing into English during the period following the Norman Conquest and related questions, one will find the *Peterborough Chronicle* a valuable source. Three parts of it were written after 1066, and although they were composed in different parts of the country and at different dates, all three of them are more or less contemporary with the events their annals relate to. **Part I** deals with the period from 1070 to 1121; **Part II**, the First Peterborough Continuation, covers the period from 1122 to 1131; and **Part III**, the Second Peterborough Continuation, that from 1132 to 1154. In **Part I** the time span between the events and the dates of their description is not greater than fifty years, and in **Parts II** and **III** roughly twenty years. Apart from that, however, **Parts II.** and **III** are also important in that both seem to be original compositions<sup>1</sup>, and in that they can be exactly located to Peterborough. Thus, the post-conquest lexical data these three portions contain will serve as a valuable source both for the chronology of the introduction of foreign vocabulary into the English language and its phonological shape as represented in its mid-twelfth century written form.

This contribution, then, intends to raise a few of the questions that the Peterborough Chronical might help to answer. Apart from the two place-names mentioned in list (1) below, the borrowings to be dealt with include only common words (noun, adjectives, verbs)<sup>2</sup>, since the the type of French influence evident through them is also the one which is treated most extensively, especially in textbooks.

### 1. The material

Normally, French words borrowed into English are classified according to their subject matter, e.g. architecture, legislation, etc. This approach allows one to relate the French influence on the lexicon to the direction of the political, cultural and religious impact of the Norman Conquest. Treatments of this type are Mary Serjeantson (1935) and Cecily Clark (1952), and the editor's notes in the edition of the *Peterborough Chronicle* (1958). Interestingly, however, a thorough philological analysis of the individual elements of the text has never

been carried out, although its importance has continuously been stressed. In particular, no complete list of new words was ever compiled. The one below draws on the efforts of several twentieth-century scholars, all of whom seem to have overlooked one or the other example<sup>3</sup>. It differs from earlier lists such as the one by Wright in that it does not only include loanwords in the narrow sense of the term but also cases of semantic borrowing, which are notoriously difficult to establish of course.

(1) Word-List

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<i>abbot, -rice</i>	<i>cuntesse</i>	<i>offrede</i>
<i>acorded, -e, -on,</i>	<i>curt</i>	<i>pais</i>
<i>Aduent</i>	<i>dæcne</i>	<i>Pasches</i>
<i>anno</i>	<i>dubbade</i>	<i>prior</i>
<i>Bataille</i>	<i>duc</i>	<i>prisun</i>
<i>calicen</i>	<i>emperice</i>	<i>priuilegies</i>
<i>cancelere</i>	<i>fals</i>	<i>procession</i>
<i>canonie</i>	<i>iustise</i>	<i>rentes</i>
<i>cantelcapas</i>	<i>iudeus</i>	<i>rice</i>
<i>capelein</i>	<i>laces</i>	<i>soalice, -scipe</i>
<i>capitele</i>	<i>legat</i>	<i>standard</i>
<i>cardinal</i>	<i>luna</i>	<i>tenserie</i>
<i>carited</i>	<i>Malueisin</i>	<i>Theophanie</i>
<i>castel, -men, worc</i>	<i>market</i>	<i>tresor</i>
<i>cellas</i>	<i>martir</i>	<i>tur</i>
<i>clerc</i>	<i>messe</i>	<i>uuerre, wyrre</i>
<i>concilie</i>	<i>miracles</i>	<i>uuerrien</i>
<i>corona</i>	<i>muntis</i>	<i>(treson)</i>
<i>crucethur</i>	<i>Natiuited</i>	

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This list, then, could be taken as a starting point for the investigation of such matters as the chronology of reception and/or changes in meaning, stylistic value, etymology, phonology and spelling.

## 2. Chronology

The investigation of the chronology of loans from French was facilitated by the publication of the New English Dictionary (NED). Also Otto Jespersen's first attempt (1905) to establish the process of borrowing was based on the NED volumes which were available then, i.e. A - J. When the dictionary was finished Jespersen's investigation was completed by A. Koszal. It considered only the first hundred entries of each of the letters and only words which were quoted at least five times (Jespersen 1946:87). A different method was applied by A.C. Baugh in 1935. He selected pages with special numbers, e.g. the ones

ending in 5. F. Mossé (1943) counted the French words found in the volume for the letter A only, and in 1956 G. Herdan counted the French elements among the first hundred words. X. Dekeyser (1986) counted loans using the Middle English Dictionary (MED). In the following table the results from the various countings are presented and held against the distribution to be found in the *Peterborough Chronicle*.

(2) Chronology of the French borrowings

	Jespersen	Koszal	Baugh	Herdan	Dekeyser	PChr
before 1050	2	-	2	2	-	2
1051-1100	2	1	-	-	-	-
1101-1150	1	2	2	1	-	8
1151-1200	15	11	7	14	8	11

Although there is some variation, the figures in the traditional studies seem to be similar in proportion at least. All in all, they support Jespersen's remark that "the linguistic influence [of French on English] did not begin immediately after the Conquest" (Jespersen 1946:87). It is indeed evident that during the first hundred years the French influence on the English lexicon was small, while the influx of French borrowings is characteristic of later periods, especially that after the thirteenth century. This contrasts in an interesting way with the figures derived from the *Peterborough Chronicle*. Particularly, there are more French loan words in the first hundred years than would have been expected, even if - following Jespersen's principles - only such words were considered which are part of present-day vocabulary.<sup>4</sup>

(3) New words in the parts of the Peterborough Chronicle

in I., II., III. abbot, acorden, castles, canceler, clerics

in I. and II. Aduent, capitele, dæcne, tur

in I. and III. uerre (wyrre)

in II. and III. sotlice, sotscipe

only in I. capelein, corona, dubbade, luna, Natiuited, procession

only in II. calicen, canonie, cardinal, cellas, concilie, duc, fals, legat, laces, market, Pasches, prior, Theophanie

only in III. carited, cuntesse, curt, emperice, iustise, miracles, pais, prisun, rentes, standard, tenserie, tresor, uerrien

A comparison of the three parts with regard the new words sheds an interesting light on the process of borrowing. Though the Chronicle is uniform regarding both author type (all parts were written by monks) and purpose (all parts are historic narratives) they differ greatly in their usage of the new words. In particular, if a new word was introduced in an earlier part, it will not necessarily be repeated in a later one. The majority of pieces of new vocabulary seem to be unique to the part of the Chronicle that contains them.

There are five words in the list which occur in all three parts. Of those *abbot* is new only graphically/phonetically, the OE form being *abbod*. Of *clerc* and *castle*, on the other hand, only the meanings are new: *clerc* now meaning "secular priest" as opposed to "munuc" (the OE meaning had been "priest" generally); *castle* now means "fortress", while in OE it had meant "mountain village". Thus, only *acorden* and *canceler* represent entirely new elements.

Another interesting point highlighted by the use of new words is that **Part II**, which is a Peterborough text, shows a greater affinity to **Part I** (which was probably composed in Kent), than to **Part III**, the other Peterborough Continuation. There are four words shared by **Parts I** and **II**. Of those *Aduent* and *capitele* count as new, the other two represent slightly altered forms of words which were otherwise known in OE: *dacne* (= OE *diacon*), *tur* (= OE *torr*). **Parts II** and **III** share only the pre-conquest loan *sot*, which apparently counted already as a native word and could combine with English formative elements: *sotscipe* in **Part II** and *sotlice* in **Part III**. The only word that might be considered as shared by **Parts I** and **III**, finally, is *uuerre* (in **Part III**), which appears as *wyrre* in **Part I**<sup>5</sup>

It is furthermore noteworthy that the words which occur in their usual French form in **Part I**, i.e. *Natiuited* and (*mid*) *procession*, appear in their Latin form in **Part II**, namely as *Natiuitas* (three times) and (*mid*) *processionem* (three times). The classification of *corona* in **Part I** is questionable: in one instance, when it represents the direct object of the sentence it takes the form *coronan*, usual for Latin words in Old English (cf. *papa* - *papan*). Should *corona* be taken to represent a French element at all? Or is it still a Latin word? The MED seems to be slightly inconsistent in this respect. Thus, it excludes *luna* for being a Latin item but includes *corona* as French, although formally there does not seem to be any difference between them.

### 3. Motiviation of loans

When words are borrowed from one language into another the usual reason is that new concepts are introduced by one group of speakers and together with the new notions their labels are also adopted. *Duc* or *tenserie* would represent such cases. Another reason for borrowing is related to social considerations. Even if a language has adequate names for a concept, new ones may be taken over from a foreign language, because they count as socially more prestigious. The borrowings then survive either as synonyms or even replace the original native words. Cecily Clark noticed such examples in the *Peterborough Chronicle*, where especially in **Part III** French words had already got the upper hand and ousted the Old English synonyms from the text of the chronicle. As is normal in such cases, the native words managed to survive for

some time in the Middle English period as part of regional vocabularies, which becomes evident from the MED. Thus, the notion of "treasure" is expressed exclusively by *tresor* in **Part III**, whereas in **Parts I** and **II** it was expressed by *gærsum* (a Scandinavian borrowing), *maðum* and *sceatt*. In a similar way, *corona* replaced *cynehelm* in **Part I**. In **Part III**, we find *pais* instead of earlier *frið* and *grið* (the latter again a Scandinavian word). *Curt* replaced *hired*, which, however, was preserved in the compound *hiredclerc* (where *clerc* still preserved its older, more general meaning). Though *prisun* occurs six times in **Part III**, in the Annal for 1137 it appears side by side with OE *cweartern*. Interesting is the case of the pair of the French/Latin *miracles* and native *wundor*, which represent antonyms in the text: *miracles* having a 'positive' meaning, while *wundor* appears in the meaning of 'horrible deed' (cf. also *Sir Gawain and the Green Knight*).<sup>6</sup>

The influence of French words on the vocabulary of religion was great as well. However, while some old words gave place to French ones (e.g. OE *fulluht* to *baptism* [not noted in the *Peterborough Chronicle*]), or were re-borrowed in French forms (OE *engel* - ME *angel* [again not attested in the *Peterborough Chronicle*]), the native names of the great church holidays remained in use. Thus, *Christmas*, *Easter*, *Twelfthnight* were the usual words, while *Advent*, *Pasches*, *Theophany* remained only marginal.

#### 4. Glossing of new items

It is commonly assumed to have been a usual practice of Middle English scribes to gloss new French words in English. There is, however, only one example of this in the *Peterborough Chronicle*:

- (4) 1095: Malueisin ðet is on Englisc yfel nehhebur...

The other two examples with explications contain such French words which represent notions unknown in earlier times, and thus could have no native names. In these cases it is the notion itself that had to be explained in English.

- (5) 1137: hi læiden gældes on the tunes æure um wile 7 clepeden it tenserie.  
1137: crucethur ðat is an ceste...

In the same annal another torture implement is also explained in a similar way though the word for it was native:

- (6) 1137: lof 7 gri ðat wæron rachenteges....

#### 5. Synonym pairs

Another stylistic device of the Middle Ages was the use of pairs of synonyms to express emphasis. In Middle English texts it became more and more frequent to pair an English with a French word, as e.g. *longages and speche*, *longages and tunges* in Trevisa's account of the English language from 1385.

In the Peterborough data *meres and laces* is the only potential example. But although Behrens added *laces* to his list of Early French loans, the word was current all through the Old English period. Thus, the modern form *lake* seems to be a direct and regular development of OE *lacu*, and the word does not really belong here.

## 6. Further points of interest

The list below entails a number of interesting questions which I intend to investigate by means of a detailed analysis of the corpus:

(7)

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### 1. Pre-Conquest borrowing from Latin

- a) with a sound change in Middle English: *abbot, dæcne, messe, muntres, tur*
- b) with a change of meaning in Middle English: *castel, cleric, fals, offrede, rice*

### 2. Pre-Conquest French borrowings

*market, sot -lice, -scipe*

### 3. post-conquest Latin words

- a) *anno, aduent, cardinal, legat, martir, pasches, prior, Theophanie*
- b) with a French ending: *iudesu, miracles, priuilegies*

### 4. post-conquest French borrowings

- a) in Anglo-Norman form: *calicen, cancelere, capelein, capitele, cantelcapas, carited*
  - b) *acorde, Bataille, cellas, corona, concilie, crucethur, cuntesse, curt, dubbade, duc, emperice, iustise, Malueisin, Natiuiteð, pais, prisun, procession, rentes, standard, tenserie, tresor, uuerre, uuerrien.*
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A phonetic description of these early examples would be important for the study of the development of French words in English. The same is true of the analysis of their spelling forms. Also, some changes of meaning need to be dated, as M. Serjeantson, who dated the appearance of the meaning "wealthy" to the thirteenth century, stresses in his discussion of *rice*.

Though it is often assumed that the etymologies of the French words will have been adequately treated by lexicologists, some cases are far from being well established, as becomes obvious if one compares English dictionaries to French ones. Thus *sot* is derived from a VulgLat. *\*sottus* by Anglisticists (Holthausen 1963, Serjeantson 1937), while French authorities agree that it is of unknown origin (see Meyer-Lübke 1935). Similarly, *rice* counts as a Celtic borrowing in the Germanic languages, but in French it is regarded as a loan from Germanic.<sup>7</sup>

Finally, the last item of list (1) above, i.e. *treason*, which I put in brackets for good reasons, needs to be commented on. The word was included among the French words by Hall (1920), who accepted a fairly adventurous reading of

an ambiguously written line in the annal for 1135 which had been favoured by nineteenth century and early twentieth century scholars. Thus, Plummer read

(8) 1135: ða westre sona ðas landes

Nowadays this is normally read

(8') ða ðestre[en] sona...<sup>8</sup>

Before the modern reading came to be accepted, however, some scholars suggested the line to be divided into

(8'') ða wes treon a ðas landes

because of the faulty form of the third graph which is too short for a "thorn" and too close to the previous word, so that it was read as a "wynn" (Bradley 1917). It is on the basis of this idiosyncratic reading only that *treson* can be assumed to be attested in the *Peterborough Chronicle*.

## 7. Summary and outlook

The Peterborough word-list offers ample material for a varied analysis of early examples of French lexical influence. The relationship between French and Anglo-Saxon, and especially their mutual influence on one another during the Middle English period requires a careful interdisciplinary treatment, combining English and French historical linguistics. This seems to be the only way to arrive at reliable results. Hopefully, by these means more satisfactory answers will be obtained to problems still under discussion concerning the French influence on the English language.

## Notes

<sup>1</sup>Though there is some indication that **Part II** might have been drawn from some central source [Plummer 1899:xlivff]

<sup>2</sup>The post-conquest lexical elements of the *Peterborough Chronicle* can be divided into three groups:

- a) common words (nouns, adjectives, verbs). Phonologically, this group can be divided into items that show characteristic Old French or Anglo-Norman features and such that appear in their mediaeval Latin forms.
- b) personal names which were not represented in pre-conquest documents. Again, these can vary in their origin: some names are Germanic, e.g. *Henry*, others, e.g. *Roger*, were brought from the Continent by the Normans.
- c) Continental topographic names, mostly French place names, e.g. *Capum* (= Caen).

<sup>3</sup>Some examples even escaped the notice of the editors of the Middle English Dictionary e.g. *luna*.

<sup>4</sup>It may be noted here that the numbers are at odds with J. Wright claim that there were only fourteen French words in the Peterborough Chronicle (cf. Wright 1923:89). This may be due to the fact that he didn't include semantic loans, though.

<sup>5</sup>The two earliest French place-names: *Bataille* (1094) and *Malueisin* (1095) occur in **Part I**.

<sup>6</sup>The Peterborough examples throw new light on M. Serjeantson's (1937) claim that foreign borrowings first appear as additional synonyms and fully replace or take up special stylistic functions of older words only later. While the remark may be true, if one considers languages in total, replacement might be the first and only step where individual texts are concerned.



<sup>7</sup>The confusion goes back to a carelessly formed sentence in Barbara Strang's textbook (1970). Additionally, of course, *rice* is interesting phonologically (shortening of the originally long Old English vowel) and with regard to the date of the change of its meaning.

<sup>8</sup>cf. Clark 1959.

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## *Linguistic pragmatics and the concept of 'face'*

*Roman Kopytko*

The two main objectives of this paper are (1) to show that a coherent pragmatic theory based on (a) philosophical non-essentialism and (b) non-categorical and non-modular pragmatics may incorporate the notion of "face" with no reservations. This view I advocate in Kopytko (in press); (2) by implication to claim that the notion of "face" is incompatible with (a) philosophical essentialism and (b) categorical and modular pragmatics; (for the opposite view (cf. Brown and Levinson 1988)).

### 1. Non-essentialist approach

A successful and final definition of any concept in linguistics (as well as in the humanities or social sciences) will most likely, never be achieved. There are two major reasons for this. The first one is that there are no clear and discrete boundaries between concepts and their scope cannot be easily determined (e.g. the scope of context). The second reason is the conscious or 'tacit' belief of some linguists in Aristotelian essentialism, criticized by Popper (1945, 1957). For a new critique of essentialism and rebuttal of essentialist sociolinguistics (cf. Janicki 1989). Popper states Aristotle's view as follows: "Aristotle saw the ultimate aim of all inquiry in the compilation of an encyclopaedia containing the intuitive definitions of all essences, that is to say, their names together with their defining formulae; and that he considered the progress of knowledge as consisting in the gradual accumulation of such an encyclopaedia." (Popper 1945: 12).

This refers particularly to so called *real definitions* (cf. Runes 1977), which are not conventions for introducing new symbols or notations, as syntactic or semantic definitions are. Rather, they are propositions of equivalence (material, formal etc.) between two abstract entities (propositions, concepts etc.) of which one is called the *definiendum* and the other the *definiens*. Not all such propositions of equivalence, however, are real definitions, but only those in which the *definiens* embodies the "essential nature" (*essentia*) of the *definiendum*.

Popper (1945, 1972) claims that 'what is' questions, such as 'what is freedom?', 'what is science?' (or in our case 'what is pragmatics?', 'what is context?') will never receive any one unambiguous answer. Popper (1945)

states that the Aristotelian essentialist view of definitions is wrong. And there are two reasons for this. One is that there are absolutely no grounds for treating 'intellectual intuition', though extolled by Aristotle, as the way to establish truth, or true knowledge. The other reason is that defining terms always leads to an infinite regression of definitions (e.g. defining pragmatics in terms of context).

Janicki (1989) adduces many examples of essentialist thinking in linguistics. For example, linguists insist on defining 'language' or 'communicative competence' claiming that inadequate definitions hamper the progress of linguistics and implying, unjustifiably, that the desired, adequate definitions may be formulated in future. However, such a possibility seems to be unlikely, and terminological disputes (as well as the inadequacy and indeterminacy of definitions) will never be resolved.

## 2. Non-modular pragmatics

The unreliability of quantitative methods in pragmatics is not due to their internal defects or deficiencies but rather to the inherent features of pragmatics, which (unlike grammar) is not a special-purpose modular system. Pragmatics is not a module - i.e. a system of interacting subsystems that have their own properties or, in other words, a system analyzed in terms of functional subassemblies (modules) and then synthesized using these modules. (cf. Fodor 1983, Longley 1989). For Fodor (1983), the paradigmatic example of a modular system is the linguistic system. A grammar, i.e. a code, relates phonetic representations of sentences to semantic representations of sentences. Wilson and Sperber (1986: 68) assume that there is of necessity a connection between the modularity of the linguistic systems and the fact that it incorporates a grammar or code. Thus, to make the claim that pragmatics is a module is equivalent to claiming there is a pragmatic code. Such a pragmatic code, analogous to a grammar, would enable speaker-hearers to recover the intended interpretation of utterances. But, unfortunately, this is not what actually happens in the process of pragmatic interpretation since it is highly context dependent. Different contextual assumptions lead to different pragmatic interpretations. Wilson and Sperber (1986: 89) claim that in some cases there is no algorithm (i.e. an unambiguous statement of the actions required to solve a problem in a finite number of steps (cf. Longley 1989: 12), for selecting the appropriate set of contextual assumptions that the hearer should use to decode the speaker's intentions. By extension, if there is no algorithm for identifying contextual assumptions, there can be no algorithm for recovering the intended overall interpretation of an utterance. According to the code model, the hearer should be able to reproduce exactly (through a decoding process) the thought that the speaker encoded into an utterance.

However, as Wilson and Sperber (1986: 69) claim "in the case of implicit import, figurative interpretation and stylistic effects, such reproduction is rarely intended or achieved". And further "the existence of indeterminacies in interpretation suggests a fundamental inadequacy in modular approaches to pragmatics. Where indeterminacy is involved, it seems that the most that communication can achieve is to bring about some similarity between thoughts of communicator and audience".

The fundamental claim of non-modular pragmatics is that there are always alternate ways to interpret contextual cues (cf. Gumperz 1982). Accordingly, the speaker's interpretation of the hearer's intention may prove to be totally or partially incorrect, whereas in modular pragmatics decoding procedures guarantee the recovery of the correct, i.e. the intended interpretation.

According to Fodor (1983) scientific theorizing is an example of a non-modular process. Scientific theorizing based on a non-demonstrative inference process consists of hypothesis formulation and confirmation. Both processes are informationally unencapsulated. That is, they have free access to contextual information. By contrast, grammatical processes have a very restricted access to contextual information (i.e. they are unaffected by the hearer's non-linguistic beliefs).

Pragmatic interpretation seems to resemble scientific theorizing in the sense that, as Wilson and Sperber (1986) claim, the speaker's intentions are not decoded, but non-demonstratively inferred by a process of hypothesis formation and confirmation, with free access to contextual information. Thus the hearer attempts to formulate the most plausible hypothesis about the speaker's intentions. Unfortunately, this hypothesis (as is the case when a scientist proposes a hypothesis) may prove to be wrong. It will be assumed in this paper that the non-modular approach to pragmatics is correct. The existence of indeterminacies in pragmatic interpretation cannot be easily overlooked. Formal pragmatists who assume a modular interpretation of pragmatics analyse only a very restricted range of data. The most important consequence of a non-modular approach seems to be the question of the unpredictability (probability) of pragmatic interpretation. In that sense 'non-modular' implies 'unpredictable'. Even the most cursory examination of ordinary conversation will point to numerous instances of incorrect interpretations of the speaker's intentions due to contextual indeterminacies (both intended and unintended). Such a view of pragmatics seems to be consistent with (and explanatory of) the failure of the quantitative paradigm in pragmatics.

### 3. Non-categorical pragmatics

Pragmatic constraints on linguistic behavior, as exemplified by Grice's conversational principles or Leech's (1983) politeness principle differ clearly from the kind of rules formulated for the study of grammar. This is so because pragmatic phenomena (e.g. formality, politeness etc.) must be described in terms of continuous and indeterminate values due to their non-discrete, scalar and fuzzy nature. It may be claimed that pragmatic processes are essentially indeterminate and non-categorical, in contradistinction to a view of categorical grammar described in terms of discrete and determinate categories. (Also in sociolinguistics variable rules and implicative scales have been used to account for sociolectal variation in quantitative and scalar terms). Grammatical description (e.g. in transformational terms) assumes the discreteness and determinacy of grammatical categories, phenomena and processes. That view of grammar was challenged by Ross' (1972) study of 'squishes' in syntax which points to scalar and fuzzy phenomena in grammar. Nonetheless, Leech (1983: 71) claims that "the grammatical system can tolerate a considerable amount of fuzziness without failing to operate as an essentially discrete system".

The psychological notion of a CATEGORY was investigated by Rosch (1977). Rosch's categories are defined by reference to prototypes - i.e. the most 'typical' instance of a class or category of things, regarded as an abstraction based on shared features or functions of the members of the class or category (cf. Reber 1988). For example, a robin is regarded as a more 'typical' bird than a chicken or a penguin. This, however, does not mean that the robin is the prototype of the category *bird*, only that it is closer to the abstract, prototypical bird.

The notion of a prototypical category can be applied to syntactic and phonological categories e.g. some verbs are more 'verby' than others or some consonants are more 'consonantal' than others. However, one must also recognize the individual features which assign features to make a category discrete. As a result, a considerable amount of indeterminacy may follow (e.g. Labov's experiment pointed to the uncertainty about what objects could be called *cups*). In conclusion, pragmatics seems to be essentially a non-categorical system; grammar and semantics have been considered to be categorical and determinate (although the latter claim can be undermined by numerous counterexamples).

### 4. Pragmatics and philosophy

A question of considerable philosophical import is: what kind of world (in the Popperian sense) are we studying when we study language? Or, specifically relevant to the present study, to what world do pragmatic phenomena belong?

Popper (1972: 106), in his epistemological theory of human knowledge distinguished three worlds (as specific domains of knowledge: (1) the world of physical objects or physical states; (2) the world of states of consciousness, or mental states, or perhaps behavioral dispositions to action; (3) the world of objective contents of thought, especially of scientific and poetic works of art. The evolution of human knowledge (an object of the world of objective contents) Popper attributes to the development of the descriptive and argumentative functions of language.

Leech (1983) expands on the Popperian hierarchy of 'three worlds' by adding a world of societal facts (or institutional facts) which place it between the second (subjective) and the third (objective) worlds. Thus, Popper's third world became in his schema a 'fourth world'. There is a strict correlation between the four functions of language and the four worlds, because Poppers functions of language provide the means of transition from one world to another. Thus, the expressive function may evolve in a purely physical sense (*world 1*) and is correlated with the mental (subjective) *world 2*. The signalling function is correlated with societal (intersubjective) objects and states - (*world 3*). The intersubjective world of social fact is the prerequisite of the descriptive function of language. The objective *world 4* evolves out of the descriptive function of language.

In the intersubjective *world 3* a number of individuals from the same social group can confirm, mutually, the meaning of some phenomenon which is external to all of them. Thus, the signalling function of language establishes a reality beyond the individual, i.e. the intersubjective world of social facts.

Popper's 'objective knowledge' (i.e. knowledge existing independently of any knower, e.g. mathematical knowledge, knowledge stored in libraries, scientific knowledge) belongs to Leech's (*world 4*). Leech (1983: 55) claims that a linguistic theory properly regarded as a *world 4* theory about *world 4* phenomena because language exists in an autonomous world which cannot be reduced to a *world 3* of social phenomena, a *world 2* of mental phenomena or a *world 1* of physical phenomena. Historically, the post-Bloomfieldian structuralists claimed that linguistics is a *world 1* (physical) phenomenon. Language as a mental phenomenon has been advocated by Chomsky (1965, 1975). Language as a social phenomenon is exemplified in the linguistic theories of Saussure (1959), Firth (1957), Halliday (1978). Although Saussure's (1959: 14) observation that language is "outside the individual who can never create nor modify it by himself" indicates that Saussure had some thoughts about the objective existence of language, i.e. as a *world 4* phenomenon. According to Leech's hierarchy of worlds pragmatics is a social phenomenon (i.e. a relation of language to society) belonging to the intersubjective *world 3*.

## 5. On 'face'

Brown & Levinson's (1988: 58) construct of the Model Person (MP) is endowed with two special properties - *rationality* and *face*. The idea of a social construct is often used as an aid in the analysis and understanding of social phenomena. It is a deliberate abstraction of reality which focuses on particular aspects and ignores others. Its function is heuristic not descriptive (i.e. to suggest possible explanations or eliminate others rather than to describe or explain facts). In fact, logical or social constructs are useful fictions which unfortunately always distort the 'reality' of the phenomenon represented. The MP is reduced to two special properties, *rationality* and *face*, assuming that they are two independent and homogeneous modules (i.e. independent of other human faculties and psychological factors: personality, conventionalized behavior, attitudes, emotions, feelings or external social pressure in the form of norms, values etc.). The view of mind as an aggregate of independent modules underlying a deterministic model of human behavior (action) (although it sometimes may be a useful fiction) is less convincing for the purposes of this work than a more plausible alternative in the shape of a network of interdependent and interacting relationships. Therefore, I am inclined to believe that *rationality* and *face* are under constant pressure both from within - psychological factors - and from without - social factors - which in fact accounts for the deviations of human action from an ideal type, a model or a social construct. (Or, rather, for the failures of expectations and predictions based on such concepts). In connection with this problem, Popper's (1957: 141) opinion on adopting the method of logical construction in the social sciences cannot be overlooked: "I refer to the possibility of adopting, in the social sciences, what may be called the method of logical or rational construction, or perhaps the 'zero method'. By this I mean the method of constructing a model on the assumption of complete rationality (and perhaps also on the assumption of the possession of complete information) on the part of all the individuals concerned, and of estimating the deviation of the actual behavior of people from the model behavior, using the latter as a kind of zero co-ordinate. An example of this method is the comparison between actual behavior (under the influence of say, traditional prejudice, etc.) and model behavior to be expected on the basis of the pure 'logic of choice', as described by the equations of economics".

As can be seen, Popper's view of the use of the method of logical construction in the social sciences attaches more importance to the assessment of the deviation of the actual behavior of people from the model behavior than to the deterministic, predictive value of the model itself.

## 6. Face as self-image

B & L (1988: 61) define 'face' as the public self-image that every member (of a society) wants to claim for him or herself, consisting of two related aspects: the *negative* and *positive face*. Similarly, Goffman (1967: 5) sees *face* as an image of self delineated in terms of approved social attributes - albeit an image that others may share, as when a person makes a good showing for his or her profession or religion by making a good showing for him or herself. In addition, Goffman defines face as a positive social value a person claims for himself.

The face is self-image, i.e. the self one supposes oneself to be; the 'imagined self', and especially its content, is not an objective category, but rather a subjective one, i.e. a person's real self is frequently (or perhaps always) to some extent incongruent with his or her self-image. (Not necessarily as a result of neurosis). The discrepancy between the 'idealized self' the 'real self' and the 'public self-image' may be very great. Thus, there may be problems with self-perception or attribution of social characteristic motives or personality features to oneself or to another person. Another problem is the concept of self and its content, especially, the social self, which reflects the interface between society and self (cf. Reber 1988). Two important questions to be analyzed include (1) those aspects of self that are largely determined by societal values and social influences and (2) the aspects of one's self or personality which are readily perceived by other people in social interactions. In connection with the second point a very interesting issue seems to be that of an intentionally false 'public self-image'. Therefore, the inescapable conclusion is that a distinction should be made between the public and the private self-image, which may differ considerably or even be diametrically opposed to one another. In addition, the public perception of a person's public self-image (i.e. offered to the public as his/her real self) may be different. The sense of the social self given in (1) above is very important for those who believe that politeness phenomena are, first of all, determined by societal values rather than by psychological or ethological factors as B & L want it.

To conclude, the idea of face as a self-image can hardly be represented as a generative, deterministic system; on the contrary, at a deep psychological level, the self-image is a constant source of indeterminacy, unpredictability, misunderstandings, and other psychologically motivated surprises in social interaction. The important fact here is not that interlocutors possess faces, but rather that they possess different faces (also dynamic and changing faces), which makes the claim of rational predictions of (S's) or (H's) behavior mostly theoretical, at best probabilistic.



## 7. Face as 'wants'

B & L (1988: 62) redefine 'face' as 'wants' in the following way:

- the *negative* face: the want of every 'competent adult member' that his actions be unimpeded by others;
- the *positive* face: the want of every member for his wants to be desirable to at least some others.

Thus, B & L go one step further than Goffman in their reductionist approach to 'face' by reducing a person's public self-image to some basic (universal) wants of human nature (of ethological origin). B & L do not elaborate on the notion of want (which is from a non-essentialist point of view very laudable) neither do they situate their concept in any psychological model or theory of basic human needs. The problem of whether and to what extent, if at all, a reductionist approach is justified in the social sciences, will be handled at later stage of this discussion. Now, however, I intend to concentrate briefly on the 'indeterminacy' of human wants, and its theoretical consequences.

The value of the vague concept of want in a predictive, deterministic system is very suspect. Again, we would have to assume that the two wants are encapsulated in two modules which exist in "splendid isolation", independent of other wants, whatever they may be, (e.g. the want to be challenged, opposed or dominated may not be so infrequent or necessarily pathological as it may seem) resistant to all internal and external (social) pressures, a perfect deterministic system of depersonalized rationality.

Furthermore, the question of reduction of face to wants seems also to be controversial, and in need of justification. Face is more than just want, if it is that at all. It is also (or perhaps primarily) a social value and norm. Face is a value in itself. Someone (an actor) is afraid of losing face because of a probable loss of reputation, respect, social prestige or any kind of social punishment. The frustration of his individual want which may be a blow to his positive self-image, may still be less harmful than the distortion of his public self-image and its social consequences.

The indeterminacy of social encounters, especially of face-work (cf. Goffman 1967) may be illustrated by the example of two politicians from opposing parties discussing an urgent and controversial social issue. How much attention will they pay to one another's face wants? Will they reach some kind of agreement or part as deadly enemies after having thrown dirt at one another's face? Probably neither one of them nor an observer can predict the course of argumentation, or the motives, the psychological factors etc., which may influence the results of the discussion. Speaking of politicians, two candidates in a presidential campaign can hardly count on their face wants to be satisfied by their opponents. Thus, there seem to be social situations when

satisfying an actor's face want may be suspended (for political reasons or some group or individual interests), although it is not a matter of necessity but rather a possible deviation which depends on a number of factors (values, morality, motives etc.).

A more moderate view of face as want and face as social value (norm) could be that they are two sides of the same coin. However, that probably would be only an approximation of another 'construct' or 'ideal type' (in the Weberian sense).

## 8. Face and essentialism

The reduction of politeness to face, face to self-image, self-image to want seems to be an attempt at grasping the essence (or true nature) of the defined terms. According to Popper (1945: 32) methodological essentialists claim that the aim of science is to reveal essences and to describe them by means of definitions. In contrast, methodological nominalists aim at describing how a thing behaves in various circumstances (rather than finding out what a thing really is) and whether there are any regularities in its behavior.

There are serious methodological problems in the essentialist approach e.g. (1) the problem of distinguishing 'true' essential definitions from 'false' ones. That is, how do we know that the true essence has been grasped in a definition. To account for that, some kind of intellectual intuition has to be postulated. However, people may differ in their opinions based on intellectual intuition. A definition or a theory that somebody may feel, intuitively, that it must be true or that it is 'self-evident', for somebody else the same definition or theory may seem to be false. And (2) the problem of avoiding an infinite regression of definitions. A very good example is that of face defined as self-image, self-image as want and want as ... ? B & L stop their definitions at the level of 'want'. A true essentialist (consciously or unconsciously) would probably like to know the meaning of 'want' in the B & L theory of politeness. Another connection of B & L's theory with essentialism is via the Weberian *Zweckrational* model of individual action (they reject the *Wertrational* model). According to Weber *Zweckrational* (instrumental action) is an action in which the actor not only compares different means to a goal but also assesses the utility of the goal itself. In *Wertrational* (value rationality) the actor takes the goal as an end in itself and may not even compare different means to that goal (cf. Abercrombie et al. 1988). For Weber the types of action he postulates (in addition to the two types just mentioned he proposed *traditional* and *affectual* action) are *ideal types*. He also admits that it is empirically possible for actions to be a mixture of one or more of the types. B & L's reductionist bias (i.e. in this case, basing their theory on one type of action-*Zweckrational* model) will undoubtedly fail to account for the more complex cases of a mixture of one or

more of the types. There are also problems with Weberian ideal types. Firstly, ideal types are hypothetical constructions (they may have some explanatory value). Secondly, the precise relationship between ideal types and the reality to which they refer remains obscure. Thirdly, Weber argued that ideal types were not models to be tested. (Although some claim that Weber himself often implicitly used ideal types as tested models). Thus, there still remains the question of testing (or even the possibility of testing) of the *Zweckrational* model of action.

Popper (1945: 292) in discussing Weber's sociological method noticed that "he (i.e. Weber) advocated for the social sciences a 'method of intuitive understanding'; and his 'ideal types' largely correspond to the essences of Aristotle and Husserl". Finally, and most importantly, B & L's construct MP (model person) is another example of an 'ideal type' (i.e. a speaker of a natural language reduced to two 'special properties' *rationality* and *face*) with a very dubious methodological status. Thus, B & L seem to adhere very strongly to the option of methodological essentialism, and as a result their theory suffers from all the deficiencies connected with essentialism as presented in all of Popper's major works.

The juxtaposition of wants vs. norms and values in B & L's approach to the respect for 'face' and especially, their preference for the treatment of face as 'want, is one more effect of the essentialist bias. All this is due to the definition of value they adopted, namely a definition expressing belief in absolute values. As phrased by B & L (1988: 62) "... the *wertrational* model (which would treat face respect as an unquestionable value or norm) fails to account for the fact that face respect is not an unequivocal right". Thus, they seem to adhere to the Platonic tradition of seeing values as essences rather than to the Aristotelian tradition, which (after Aristotle) maintained that values are not essences but, rather, they are defined by human interest. J. Dewey frequently criticized the theories propagating the existence of absolute and universal values. Plato introduced the distinction between intrinsic and extrinsic (instrumental) values, i.e. the distinction between values in itself, or as an end or for its own sake, and values as a means to something. (cf. Runes 1977). Plato also proposed intermediate values that were held to have both characteristics. J. Dewey, with his concept of the means - end continuum, held all values to be of the intermediate, extrinsic-intrinsic type. (cf. Reese 1987). In conclusion, it may be claimed that a choice of definitions for some basic concepts in a proposed theory (a choice based on some kind of unreliable 'intellectual intuition', on values, or on some other unspecified preferences) not only does not introduce any 'truth' about the defined concept but rather distorts it by means of the essentialist bias (i.e. a rather arbitrary process of discovering 'essences'). To complete the theme of values in this short presentation it should

be noted that the axiological theory is abundant in numerous hypotheses concerning values. According to Reese (1987) depending upon theory, values have been viewed as *cognitive* or *non-cognitive*, *absolute* or *relative*, *objective* or *subjective*, *natural* or *non-natural*, *essentialist* or *existentialist*, and *justifiable* or *non-justifiable*.

## 9. Social vs. individual values

Another possible distinction is that between social and individual values. The link between social and personality system is achieved by the internalization of the values through the process of socialization. The interplay between the social and individual values and the possible instrumental treatment of values may account for the dynamic aspect of a speech encounter (i.e. its constitution, negotiating terms (roles) etc.). It may also contribute to the indeterminate and unpredictable aspects of speech encounters e.g. due to the internal conflict between social and individual values. Inadequate internalization and socialization of values is a constant source of unpredictable human behavior. Even if we admit that the individual deviations (or the dynamic aspects) from social norms in speech encounters can be alternatively or complementarily explained in terms of wants rather than norms and values, such a reductionist approach cannot help the 'cause of determinism' because indeterminism and unpredictability will be always 'there' (i.e. in the social sciences and perhaps, although less conspicuously, in the natural sciences) independently of the methodological approach. Social norms (values) are inmates of *world 3* (cf. Popper 1976: 194). To reduce them to the objects of *world 2* would raise more problems than it would solve. If we adopt B & L's definition of social values (norms) in absolute terms, it seems that the predictive force in terms of values (still probabilistic) in absolute numbers will be greater than that in terms of individual, psychological wants where the level of predictability (due to individual deviations from the expected social norm) will be frequently lower than the expected norm. In other words, norms as frequencies of occurrence are to some degree predictable; individual behavior is much less so.

Another difference between wants and values is that wants are to some extent, according to B & L, inherited genetically, e.g. the wants of negative and positive face (at the most abstract level, i.e. the want to be unimpeded in one's actions and approved of by others respectively); values on the other hand are acquired in the process of socialization. The complex phenomenon of politeness cannot be accounted for solely in terms of inherited wants (of ethological origin) because it is also or perhaps mainly a product of social interaction and social values, which cannot be easily reduced to inherited wants without a considerable social residue (i.e. a set of social factors responsible for the origin as well as the shape of politeness phenomena in the

social interaction). Furthermore, politeness as a social fact cannot be reduced, unless we reject the Durkheimian *dictum* of the irreducibility of social facts. Such an act deserves some kind of justification rather than an *ad hoc* decision.

## 10. Face and 'mutual knowledge'

The assumption of 'mutual knowledge' of (S's) and (H's) face wants is a *conditio sine qua non* of the minimization of an FTA (face threatening act) in a speech encounter. Actually, such 'mutual knowledge', is hardly ever attainable. That is so, because face wants are not directly accessible (as psychological entities) and changeable. That is, in the former case (S) has to rely on his guesses, deductions or beliefs about (H's) face wants; or to make another assumption, that (H's) face wants are the same as those of (S); and, in the latter case, the static character of face wants has to be challenged by the dynamic development of face wants due to social or economic pressure. Thus, paying respect to (H's) 'old' face wants may often result in an FTA. In sum, it seems to be doubtful that the assumption of 'mutual knowledge' (and associated with it - as a consequence, the idea of predictability & determinism) can be positively postulated without accounting for its dialectical opposite, namely 'mutual ignorance' of face wants, which accounts for the deviations from the expected pattern of linguistic behavior and the undesirable perlocutionary effect of FTAs.

In conclusion, it may be claimed that the indeterminate and non-deterministic nature of the concept of face (and its functioning in the process of verbal interaction) does not justify the inclusion of it in a system of categorical, modular and predictive pragmatics. On the contrary the socio-psychological concept of 'face' seems to be rather closely associated with the coherent pragmatic theory based on (a) philosophical non-essentialism, (b) non-categorical and non-modular pragmatics and (c) non-deterministic probability.

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## *Old English Class II: more VIEWS*

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Ypde swa þisne eardgeard ælda Scyppend  
 oþþæt burgwara breahtra lease,  
 eald enta geweorc idlu stodon  
 -*The Wanderer*

### 1. Preliminaries

I'm delighted that *VIEWS* is doing what its editors so bravely and imaginatively decided to do: encouraging the discussion of ideas in progress. There's far too little of this; most of us tend to wait till our work has reached a gemlike Virgilian perfection before being willing to give birth in public. (I hope mine are not a case of *parturiunt montes ...*). So I'm grateful to the editors for providing a forum (and indeed, to carry on the Roman image, acting as gladiators in the process) for some half-baked ideas about OE *-ian* (or more accurately about weak class II). I hope now to bake them a bit more.

Both Niki Ritt and Christiane Dalton-Puffer (*VIEWS* 2.1) responded as I'd hoped, and took my contribution in the right spirit. What I was saying of course is not 'OE *-ian* is a derivational suffix, and therefore class II is a derivational class'. It was rather more along the lines of 'class II is odd; what would happen if we suggested that ...?' And I've now seen what happened. The questions raised in the replies are fundamental ones, and there's yet more mileage to be got out of *-ian*; and I have some answers to some of the objections, as well as some further implications to draw out. This will not be a full-scale argument, but a consideration of various important points raised in the two replies.

### 2. The funniness of class II

Niki Ritt in the last sentence of his comment (1993: 39) has caught the essential point: that there's something funny about class II. At least it's very different from class I (classes III, IV in OE are so tiny they're hardly worth looking at in this connection; but I suspect some odd claims could be made about Old High German class III, if it's as full of inchoatives as I think likely). It is at least, as Ritt says, 'more derivational' than has usually been suggested, or is ever pointed out in the standard handbooks. If I've made this point, I'm happy.

The real thrust of my paper was not actually a consideration of synchronic status (though it may have looked that way), but at least covertly historical. This is unsurprising, because I don't really believe in the hard form of the Saussurean dichotomy, and I have some doubts about the general usefulness (or even theoretical coherence) of the notion of 'synchrony' in linguistics. It's possible, and even desirable, to see languages as always coming from wherever they were, and dragging all kinds of interesting historical detritus with them; a language, like a work of art, is in a sense an 'intertextual' object, always in dialogue with its own past. I think one can say this as a working linguist, without getting into neo-Humboldtian mysticism about *energeia* vs. *ergon*; languages are always pretty much both, but more the former, in the sense that their structure is never 'finished'. I return to this in the final section.

### 3. Semantic incoherence, morphology, and the status of *-ian*.

The charge (Ritt 1993: §2) that class II overall is not semantically coherent (even if 'derivational') is correct; again, given the history this is precisely what I'd expect. The situation is not dissimilar in Latin: *amare*, *cantare*, *plantare*, *crepare* 'rattle', *domare* 'tame', *lauare* 'bathe', *secare* 'cut', for instance are not the same semantic type; *-a-re* is not a 'derivational marker' in the same sense that the largely inchoative *-esc-* is. (The fact that verbs in *-esc-* always, as far as I know, take the infinitive marker *-ere* is of interest in relation to some remarks to be made below, in that a particular type of derivation, here explicitly marked, seems to require membership in a particular inflectional class.) The IE \*/-ja:-/ derivations are a rather unspecific set; the point is that they're derivations, mainly denominal, but not semantically specific like the Sanskrit *-yá-* causatives or much of Gmc class I weak (e.g. the causatives off pret sg strong verb stems like *settan*, *drenčan*, etc.).

OK, I admit that taking '*-ian*' as the topic was a cheap trick, in that I silently extended it to the rest of the verb. But in fact (*pace* Ritt 1993: §2), it's not just the infinitive that's peculiar; so is much of the rest of the morphology, e.g. the back vowel in 3 sg and the thematic preterite even after heavy stems. Class II is still recognizable as a class, even if some of its forms may on occasion be homophones of forms of nouns. I take it that this is at least a partial answer to Dalton-Puffer's (1993: §4) support of Ritt's remarks, where she says that 'once the verbs are not in the infinitive, *-ian* is gone too, and other grammatical morphemes ... step into its place'. By and large these look funny too.

The possible homophony between between *ar-a* 'of honours' and *ar-a* 'honour!' (Ritt 1993: §2) is beside the point, which is after all morphological 'deviance' as an INDEX of the character of this class as a whole. Homophony in any case (especially in a language that allows some zero-derivations, but also



in general) is of no particular interest in such connections, because at least in a text there will nearly always be some kind of cues that will disambiguate, especially if the homophones belong to different grammatical classes.

Class II however is not ‘embryonically’ derivational (Ritt 1993: §4) but the opposite; perhaps I didn’t make this clear. To the extent that it is ‘derivational’ (the scare-quotes suggest that I don’t of course believe that *-ian* = *-estre!*), it is so not as a foetus that failed to reach term in Middle English, but rather as a tottering geriatric. Because of its origin it is IN FACT derivational or ‘secondary’ to begin with, as the IE \*/-ja:-/ conjugation, its parent, is. Its derivationalism is inherited, but not fully functional, like wisdom teeth (which however some people can still bite with).

It is also not to the point that there are problems with apparent direction of derivation in some cases, e.g. those cited in Ritt’s examples (4, 5), where e.g. in *andsæc* ‘denial’ and *andsacian* ‘deny’ the nouns could be taken as ‘relatively straightforward action nouns’, and hence deverbal. By this stage in the history of class II one would expect derivations in the ‘wrong’ direction anyhow; what seems important to me is that these are in the minority, as far as I can tell, and the majority is the reflection of the original (basic) status of the class. (One still has to do the real statistics, of course; I shirked that boring but important job in the interests of getting something in print quickly.)

Ritt (1993: §3) also suggests that given the overall semantic incoherence, it might be better to view much of class II’s derivationalness as being, rather than ‘directional’, a matter of both verbs and nouns being ‘derived from syntactically neutral roots’. I did in fact raise this possibility in my paper (notes 4, 5), and it deserves consideration. The only reason I did not make a stronger case for it is that this kind of derivational relationship seems much less Indo-European in ‘feel’ than directionality: the ‘neutral root’ kind of morphology is much more Finnic or Semitic in type. Synchronically, however, this may well be as good an approach as mine.

#### 4. Embryos and old men

I never meant to imply, of course, that Dalton-Puffer was being ‘careless’ or ‘wrong’ when she said what she did; especially as she was looking at things from the Middle English end, rather than from the Indo-European, as I was. Different beginning points produce different perspectives. From the fish point of view, the three ossicles of the mammalian middle ear are old gill arches; from the reptile point of view two of them (incus and malleus) are shifted relics of pieces of jaw architecture; from the mammal point of view they are a chain of bones that conduct sound from the eardrum to the cochlea. All of these characterizations are of course ‘correct’.

To keep to the biological imagery, as I intend to do even more below, consider her comment (1993: §6) on the unidirectionality suggested by Ritt's embryo image. She sees rather a bidirectional movement in the history of class II, a shift from derivation to inflection, which she takes (rightly) as my main point, but in Middle English a movement in the opposite direction, as the detritus left behind by the decay of OE verb morphology allowed cooptation of the fragmentary *-n-* in old *-n-ian* (for instance) for new derivational purposes (so that the *-n-* becomes part of the root in *darken* and the like: this kind of 'demorphologization' is interestingly discussed in Hopper 1990). She suggests that inflection and derivation should be seen as related on a kind of scale (presumably nondirectional with respect to time), 'where either end serves as a kind of prototypical center', and affixes could move about on the scale over time. I couldn't agree more. In fact this says at least part of what I may have been struggling for in a better way than I managed to.

The Middle English evidence cited by both Ritt and Dalton-Puffer, in particular the 'freeing' of *-(n)-en* < *-(n)-ian* to attach itself to later derivations of different types, is crucial to getting the full mileage out of another remark of Ritt's, which may help clarify the kind of argument I was aiming at.

## 5. The non-integrity of the sign and the possibility of history

Ritt notes (§4) that as various new derivations arose that were more 'speaker-friendly' or semiotically 'efficient', this makes it unsurprising that our suffix 'went down the drain of linguistic evolution'. He then says that 'this view is not compatible with the established integrity-of-the-linguistic-sign theorem, but what the heck'. This is too good to be a mere throwaway line, and deserves to be an epigraph. My only disagreement is with the idea that this notion of integrity is in fact 'established'. On the contrary, it's dead wrong. If it were really the case, there could probably be no linguistic history. I would rather claim (and cf. Lass 1990) that at any given time a good part of any linguistic system does not consist of 'signs' at all, but of junk: bits of historical garbage, strings of material that participate in no semiosis at all, but are simply open back doors for change to sneak in through.

It's time for a small attack on semiotic imperialism anyhow, in the interests of making history. There is a Peircean (and now neo-Peircean) view that just about everything in the organic world is 'semiosis' (for a gross example see Sebeok 1991, especially chs. 1, 7-8). Sebeok argues that semiosis is an 'original' and virtually defining property of life. E.g. self-replication

requires communication in the form of a readable code, the eukaryotic (nucleated) cell results from a symbiosis of prokaryotes, who had to talk to each other so that certain primordial bacteria knew that they ought to become mitochondria or chloroplasts, the immune system works by cells presenting protein fragments on their surfaces so that antibody-making cells can read them, etc. All well and good, and probably true, if not very informative.

But to say that living systems (or languages) 'are' sign systems does not mean that everything in them is a sign actively engaged in being one, and that everything emitted is a sign, and any receiver must take every piece of any message as a sign. (We've known at least since the early days of information theory how important redundancy is). It is for instance well known that of the three nucleotide bases that make up every coding gene-fragment (exon), the third is (relatively) redundant: the first two are enough to specify for protein synthesis, and the third has considerable freedom to mutate without compromising the output. Further, exons are separated by non-coding 'dead' sequences (introns), which are clearly non-semiotic, in that they do not produce messages that are translated by messenger RNA (which in fact snips them out). Similarly, retroviruses like HIV have certain nucleotide sequences that 'communicate' in the sense of defining them as HIV or whatever, others that vary quietly without compromising identity, and still others that are hyper-variable (Eigen 1993), and mutate wildly without making HIV anything except HIV (though they do help it to outflank the immune response).

In addition, most genes exist in multiple copies, only one of which is expressed; this junk DNA (which may comprise half or more of a genome) does nothing except mutate cheerfully without any phenotypic expression: though it may be used later as a source of variability, if selective pressures should favour its potential expression.

Linguistic systems not only have a 'semiotic' core of active signs, but huge amounts of redundancy, junk, etc., historical detritus like class II (cf. Lass 1986 on the IE consonant-stems and the decay of ablaut, Lass 1990 on the formerly aspect-marking garbage that was cobbled into the Germanic strong verb system, Lass 1991 on the origins of the Germanic dative). There is actually nothing at all peculiar in this view: it lies at the heart of the useful Praguian distinction between 'centre' and 'periphery' in linguistic systems, and is there for anyone to see who isn't deafened by semiotic buzz-words or attractive but useless visions of systems 'où tout se tient'.

The presence of junk that in itself is neutral, but is both indexical of evolutionary history and possibly suggestive of future developments, is simply a property of all historically evolved systems that replicate imperfectly and are subject to selective pressures that favour some (types of) variants at the expense of others. It is in fact an unsurprising property if one assumes a strong

element of contingency in both the generation of variants and their selection (which is certainly the case in the organic world: cf. Gould 1989), and if one assumes that the carrying capacity of these systems for all kinds of nonfunctional material is indeed massive. What Konrad Lorenz (1973: 19) says of the biologist holds for the historical linguist as well: he 'muß sich bei seinen Reduktions- und Erklärungsversuchen mit einem sehr großen Rest des historisch nicht Rationalisierbaren abfinden'. As he says later (1973: 25), one finds in any organism (and this could be said for languages as well, without of course making the vulgar error of considering them to 'be' organisms) 'eine Menge Baumerkmale, die Überbleibsel einer "Anpassung von gestern" sind'. These remnants are 'ein großes Glück für den Forscher, der nicht nur die Spielregeln der Evolution kennen lernen, sondern auch ihren speziellen Gang erforschen will'.

The point is that 'dead' material of various kinds can be recycled, exapted (Gould & Vrba 1982, Lass 1990) for various purposes, even ones conceptually quite different from what they were originally 'intended' to do. Dalton-Puffer's scenario for the class II *-n-ian* types is a case of precisely this kind of thing: exaptation of a semantically empty or 'useless' category, an intron or third nucleotide, for something new. Verbs like *darken*, etc. are conceptual innovations, cooptations of old material that could not have occurred if it were still 'semiotic' in any serious way. The linguistic sign does not have integrity (or alternatively not everything in a language is a sign), and this is one of the most potent sources of novelty in historical development.

I agree with both of my critics that class II is not a 'pure' example of derivation (or anything else); this very fuzziness is what allows (pieces of) it to be re-used; *darken* and the like are in principle very similar to using a no-longer read book to prop up a table leg, and more of linguistic history consists of such ecologically sound moves than perhaps one normally thinks.

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# ***My/mine, thy/thine: aspects of their distribution in Early Modern English***

*Herbert Schendl*

## 0. Introduction

It is a well-known fact that the EModE attributive possessive pronouns ("determiners") of the 1st and 2nd persons singular have two variant forms each, namely *my/mine* for the first person and *thy/thine* for the second. The origin of this variation is basically phonological, since the *-n* disappeared before consonants in the course of the ME period, while it was retained before vowels (and partly before ⟨h⟩, cf. Jespersen, *MEG* II, 16.211). This development was largely completed by the end of the ME period, though there is some inconsistency in the use of the variant forms before vowels towards the end of ME.

## 1. The situation in EModE

1.1. Most handbooks state that at the beginning of EModE the distinction between *my/thy* and *mine/thine* "is pretty consistently observed in the standard language" (Barber 1976: 207), while both forms occur before ⟨h⟩. But there are still rare occurrences of *mine/thine* before consonants in the 16th century, e.g. in Tyndale's translation of the Bible and in J. Heywood (cf. Spies 1897: 18f.). Jespersen's claim that the analogical extension of the *n*-less forms before vowels and ⟨h⟩ began only "from the end of the 16th c." (*MEG* II, 16.212) is certainly not correct. Graband (1965: 252) states that the use of the variant forms gets frequently mixed up in the 16th century and that *my* and *thy* are increasingly found before vowels and ⟨h⟩.

1.2. Shakespeare's inconsistent use has frequently been mentioned, though not really systematically studied (cf. Franz 1939: 292ff., Scheler 1982: 40). Abbott (1870: 160) established a rule that in stressed position the *n*-less variants are preferred before vowels - though there are some counterexamples (cf. also Schmidt 1962, s.v. *mine*).

1.3. According to Barber (1976: 208), *my/thy* and *mine/thine* are "in free variation" before vowels by 1600. The *n*-forms continually recede during the 17th century, till they have virtually disappeared from standard literary prose by 1700, except as conscious archaism (cf. Barber 1976: 208). Some authors

point out that with certain words such as *own*, *eye*, *ear* the *n*-forms are more frequently used and survive longer.

1.4. In most traditional accounts euphony, stress patterns or simply 'free variation' are given as explanations of the seemingly irregular distribution of the variants before vowels and ⟨h⟩. On the whole, however, the factors leading to the choice of a given variant are not discussed in any detail. The distribution and development of the variant forms of the two possessives still awaits a detailed investigation.

## 2. Aims and methods

2.1. In recent years variationist approaches have increasingly been applied to historical linguistics (cf. Romaine 1982, Rissanen 1986, Raumolin-Brunberg 1988, etc.). Empirical studies based on large stratified diachronic corpora have shown that phonological, morphological and syntactic variation in historical texts frequently correlates with variables such as style, register, text type and genre. The traditional concept of 'free variation' is no longer considered as a sufficient explanation of corpus-internal variation.

2.2. The present study re-investigates the variation of the attributive possessives *my/mine* and *thy/thine* in a variationist framework. Based on an extensive corpus of EModE texts it has two main aims:

- (i) to provide detailed information on the frequencies of the variants *my/mine* and *thy/thine* before vowels and ⟨h⟩ in a variety of texts from the 16th and early 17th centuries;
- (ii) to investigate the correlation between the observed changing variation and linguistic and extralinguistic factors. Among the latter, style, text type, and the possible influence of spoken language deserve special consideration (cf. Rissanen 1986: 98f.).

The quality of this type of research depends heavily on the nature of the corpus, which should include a variety of different styles, genres, text types, etc. (cf. Nevalainen and Raumolin-Brunberg 1989). With the variants studied here a difficulty lies in the restricted occurrence of the forms in specific text types, many of which do not or only rarely use possessives of the first and second persons singular. This partly accounts for the present emphasis on drama, poetry, letters and certain novels. The present corpus has, however, still obvious gaps in regard to informal non-literary prose texts. But even this limited corpus has shown tendencies of distribution which cannot be explained as chance variation.

2.3. The corpus investigated is listed under (1). (For reasons of space no bibliographical information on the texts can be provided. They have been consulted in generally accepted standard editions.) Apart from the one text by

Caxton from 1490, the texts are grouped into three periods of 50 years, (i) 1500-1549, (ii) 1550-1599, and (iii) 1600-1649 (though at present the last text of period 3 dates from 1633).

(1) Texts analysed

**Period 1 (1500-49)**

(W.Caxton, *Eneydos* (1490))  
 Anon., *Everyman* (c.1512)  
 J.Heywood, *A Dialogue of Proverbs* (1546)  
 H.Medwall, *Fulgens and Lucrece* (1512-16)  
 Sir Th.More, *Letters* (1529-35)  
 J.Palsgrave, *Acolastus* (1540)  
 J.Skelton, *Magnificence* (1515/16)

**Period 2 (1550-99)**

Anon., *Arden of Feversham* (1592)  
 Th.Deloney, *Jack of Newbury* (1597)  
 R.Greene, *Friar Bacon & Friar Bungay* (1594)  
 J.Lyly, *Euphues* (1578)  
     *Mother Bombie* (1594)  
 Th.Nashe, *The Unfortunate Traveller* (1594)  
 G.Peele, *The Old Wives' Tale* (1591-94)  
 Sir Ph.Sidney, *Letters* (1568-86)  
     *Certain Sonnets* (c. 1580)  
 Th.Whythorne, *Autobiography* (c. 1676)

**Period 3 (1600-49)**

J.Donne, *Letters* (1600-1630)  
     *Songs and Sonnets I* (1633)  
 Th.Heywood, *A Woman Killed With Kindness* (1607)  
 T.Middleton, *A Trick to Catch the Old One* (1608)  
 Beaumont & Fletcher, *The Maid's Tragedy* (c. 1610)  
 J.Webster, *The Duchess of Malfy* (1613)  
 Ph.Massinger, *A New Way to Pay Old Debts* (c. 1625)  
 J.Ford, *'Tis Pity She's a Whore* (1625)

### 3. Empirical study

3.1. The figures under (2) present the absolute and relative frequencies of the *n*-variants *mine* and *thine* before vowels in the texts of the three periods. They do not take into account any phonological, morphological or stylistic factors. The table differentiates between the frequencies of TOKENS and TYPES of the adjectives and nouns which immediately follow the possessive; of these two frequencies, the latter, i.e. the type frequency, is certainly the more informative one and will be referred to except when stated otherwise. However, the fact that the relation between token and type frequencies changes over time is an important indicator of the increasing constraints on the *n*-forms before certain lexemes.



(2) Absolute/relative frequencies of *mine/thine* before vowels

TEXT	TOKEN		TYPE	
	Abs.	Rel.	Abs.	Rel.
<b>Frequency</b>				
<b>Period 1 (1500-49)</b>		%		%
Caxton <i>En.</i> (1490)	26	90	16	84
<i>Everyman</i> (1512)	7	78	6	75
Medwall <i>Fulg.</i> (1512-16)	36	100	13	100
Skelton <i>Mag.</i> (1515/16)	21	100	13	100
More <i>Let.</i> (1529-35)	25	100	9	100
Palsgrave <i>Acol.</i> (1540)	54	100	22	100
Heywood J. <i>Dial.</i> (1546)	45	96	19	90
<b>Period 2 (1550-99)</b>				
<b>Group 1</b>				
Nashe <i>Trav.</i> (1594)	5	10	2	6
Peele <i>Wives</i> (1591-94)	4	21	2	14
Deloney <i>Jack</i> (1597)	4	17	2	22
Sidney <i>Let.</i> (1568-86)	8	30	1	7
<i>Son</i> (c. 1580)	10	63	3	38
<b>Group 2</b>				
Lyly <i>Euph.</i> (1578)	113	72	35	56
<i>Bombie</i> (1594)	13	59	6	43
<i>Arden</i> (1592)	15	50	7	59
Greene <i>Friar</i> (1594)	15	71	5	45
Whythorne <i>Aut.</i> (1576)	26	68	19	63
<b>Period 3 (1600-49)</b>				
Donne <i>Let.</i> (1600-30)	21	38	4	14
<i>Son</i> (1633)	16	100	9	100
Heywood T. <i>Wom.</i> (1607)	13	25	3	8
Middleton <i>Trick</i> (1608)	12	26	5	26
Beaum/Flet <i>Maid</i> (1610)	38	49	11	28
Webster <i>Malfy</i> (1613)	15	38	4	15
Massinger <i>Debts</i> (1625)	12	19	3	8
Ford <i>Whore</i> (1625)	19	36	10	29

3.2. There are obvious differences between the three periods in the use of the *n*-variants before vowels: in period 1, i.e. up to 1549, the relative type frequency is between 75% and 100%, with 100% in four texts including the private letters of Thomas More to his daughter and wife (1529-1535). Of these earlier texts, *Everyman* (ca 1512), has the lowest frequency with 75%. The language of *Everyman* is usually termed 'natural' and seems to be on a lower level of formality than most of the other texts of period 1. (For a discussion of the relation between level of formality and spoken language, see Rissanen 1986: 98f.)

The data seem to indicate that the *n*-forms were the stylistically neutral forms before vowels in this first period. Further texts closer to spoken language

will have to be analysed to decide whether the *n*-less variants were a feature of colloquial or spoken language as the data from *Everyman* might imply.

In period 2 (1550-1599), there is a sharp decline in the frequency of the *n*-forms before vowels in *all* texts, though with obvious differences between different text types.

A first group of texts shows frequencies of *n*-forms between 6,5% (Nashe, *The Unfortunate Traveller*, 1594) and 22% (Deloney, *Jack of Newbury*, 1597); this group also includes Peele's drama *The Old Wives' Tale* (about 1593, 14,3%). All these texts have traditionally been classified as informal.

The language of Sir Ph. Sidney's *Certain Sonnets* (1580) has been described as the "language of ordinary prose and of everyday polite conversation" (Ringler 1962: lii f.) - though, of course, even natural poetry is never identical with real speech. This alleged closeness to speech correlates with a relatively low frequency of 37,5% of *n*-forms. In Sidney's private letters from between 1568 and 1586, however, there is a (further) sharp decline of *n*-forms to a mere 6,7%. This difference between the letters and the sonnets is too great to be mere chance variation. Together with the figures of the second group of texts from period 2 this allows of only one interpretation: the *n*-less forms before vowels had acquired a stylistic marking as 'informal', at least by 1570/80.

The second group of texts of period 2 has clearly higher frequencies of *n*-forms, namely between 40% and 55%. This group comprises drama and higher prose, i.e. Lyly's *Euphues* (1578, 55%), his comedy *Mother Bombie* (1592, 43%), the anonymous *Arden of Feversham* (1592, 41%), and Greene's *Friar Bacon* (1594, 45%). This is no surprise in the case of Lyly, with whom there is also a difference between the stilted prose of *Euphues* and the language of the comedy *Mother Bombie*.

A word must be said about the last text listed under group 2 in period 2, Whythorne's autobiography, which was written in the late seventies, i.e. about the time of *Euphues* and Sidney's texts: its high frequency of *n*-forms (about 63%) would group it with Lyly's prose. One possible explanation of this frequency is the age of the author. Sidney, for instance, was then in his twenties, while Whythorne was approaching fifty, and from his date of birth (1528) still belongs to period 1. (I.e. we may have to reckon with the influence of the variable 'age' in our interpretation of the data, though a formal style is clearly compatible with the genre 'autobiography'.)

Period 3, i.e. the first decades of the 17th century, shows a further decline of the *n*-variants. In all six dramas the frequency is - sometimes clearly - below 30%, in two cases (Heywood, Massinger) it is even below 10%; i.e. the frequencies of *n*-less forms in the drama of period 3 approach those of informal prose in period 2!

As with Sidney in period 2, the most informative difference is again between the letters and the poetry of one and the same person, namely of John Donne. While in Donne's letters the *n*-forms amount to only 14%, they are exclusively, i.e. with 100%, used in his poems. The type : token relation in the two text types from Donne is also quite revealing (see under (3): The relation is 1 : 5,25 for the letters and only 1:1,78 for the sonnets; i.e. the letters show a relatively frequent use of a small number of lexemes after the *n*-forms, while this is not the case in the sonnets.

(3) Type:token relation of *n*-forms in J.Donne

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Letters:	4:21,	i.e.	1:5,25
Sonnets:	9:16,	i.e.	1:1,78

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The difference between Donne's letters and sonnets must be interpreted differently from that in Sidney's writings. They most likely indicate that the *n*-forms had become marked as 'poetic' or 'archaic' by 1630 at the latest.

The figures from period 3 also underline the well-known conservative character of the language of the *Authorized Version*, where the *n*-forms are regularly used before vowels (and ⟨h⟩) (cf.Emma 1964: 52) - this distribution did no longer correspond to actual usage in spoken language and in normal prose.

So much for the distribution of the variant forms before vowels.

3.3. The distribution of the variants before ⟨h⟩ will be discussed only briefly. In this case the initial grapheme ⟨h⟩ is taken as a criterion of inclusion, i.e. at this stage no attention is paid to etymology or pronunciation. The figures under (4) show very clearly that the frequency of the *n*-variants before ⟨h⟩ never approximated that of the *n*-forms before vowels. This fact is not made clear in most handbook accounts, where vowels and ⟨h⟩ are normally taken together. In period 1 - and partly also in 2 - lexemes of Germanic and French origin occur with the *n*-variants, whereas in period 3 the *n*-forms become increasingly restricted to a small number of words of French origin, such as *honour*, *honesty*. - The problems connected with the pronunciation of ⟨h⟩ in words of Germanic and French origin in EModE will not be discussed any further here (cf. Luick §§ 729, 790f.).

(4) Absolute/relative frequencies of *mine/thine* before ⟨h⟩

TEXT	TOKEN		TYPE	
	Abs.	Rel.	Abs.	Rel.
<b>Period 1 (1500-49)</b>		%		%
Caxton <i>En.</i> (1490)	3	18	2	18
<i>Everyman</i> (1512)	0	0	0	0
Medwall <i>Fulg.</i> (1512-16)	4	33	2	33
Skelton <i>Mag.</i> (1515/16)	0	0	0	0
More <i>Let.</i> (1529-35)	1	25	1	25
Palsgrave <i>Acol.</i> (1540)	4	10	4	24
Heywood J. <i>Dial.</i> (1546)	0	0	0	0
<b>Period 2 (1550-99)</b>				
<b>Group 1</b>		%		%
Nashe <i>Trav.</i> (1594)	3	7	2	11
Peele <i>Wives</i> (1591-94)	4	21	2	14
Deloney <i>Jack</i> (1597)	1	2	1	9
Sidney <i>Let.</i> (1568-86)	0	0	0	0
<i>Son</i> (c. 1580)	1	7	1	14
<b>Group 2</b>		%		%
Lyly <i>Euph.</i> (1578)	9	15	2	10
<i>Bombie</i> (1594)	3	6	2	13
<i>Arden</i> (1592)	0	0	0	0
Greene <i>Friar</i> (1594)	3	17	3	30
Whythorne <i>Aut.</i> (1576)	2	13	2	17
<b>Period 3 (1600-49)</b>		%		%
Donne <i>Let.</i> (1600-30)	0	0	0	0
<i>Son</i> (1633)	0	0	0	0
Heywood T. <i>Wom.</i> (1607)	3	4	1	5
Middleton <i>Trick</i> (1608)	5	16	1	8
Beaum/Flet <i>Maid</i> (1610)	11	17	3	15
Webster <i>Malfy</i> (1613)	2	5	1	8
Massinger <i>Debts</i> (1625)	0	0	0	0
Ford <i>Whore</i> (1625)	1	2	1	10

3.4. The textual distribution of the *n*-variants before vowels has turned out to be quite revealing. Any study of the development of the variants, however, also has to analyse the individual instances of the forms in each of the texts, to discover possible phonological, morphological, lexical and stylistic constraints on the distribution of the variant forms.

As mentioned in the introduction, the origin of the *n*-less variants is phonologically conditioned, and this phonological factor still largely determines the basic distribution of the variants in period 1: i.e. we have *-n* before vowels, (and partly before ⟨h⟩), *n*-less forms before consonants. This segmental phonological factor seems to be supplemented by suprasegmental factors of rhythm and stress at least in period 2. The tendency observed in

Shakespeare that the stressed forms before a vowel are *n*-less (cf. 1.2. above) is basically valid for the metrical examples of the corpus in period 2, though the tendency does not work the other way round (i.e. we also find the *n*-variants in stressed position).

Apart from this phonological factor, a morphological one (which is to a certain extent connected with the stress patterns) seems to have had some influence on the choice of variants, namely the number of syllables of the lexeme following the possessive.

- (5) Relative frequency of polysyllabic lexemes (tokens)  
 A: Percentage of polysyllabic lexemes after 1/2sg attributive possessives  
 B: Percentage of *n*-variants before polysyllabics

<b>TEXT</b>		
<b>Period 1 (1500-49)</b>	<b>A%</b>	<b>B%</b>
<i>Everyman</i> (1512)	78	71
Medwall <i>Fulg</i> (1512-16)	64	100
Skelton <i>Mag</i> (1515/16)	48	100
More <i>Let</i> (1529-35)	36	100
Palsgrave <i>Acol</i> (1540)	31	100
Heywood J. <i>Dial</i> (1546)	34	94
<b>Period 2 (1550-99)</b>		
<b>Group 1</b>	<b>A%</b>	<b>B%</b>
Nashe <i>Trav</i> (1594)	51	0
Peele <i>Wives</i> (1591-94)	26	0
Deloney <i>Jack</i> (1597)	29	0
Sidney <i>Let</i> (1568-86)	41	0
<i>Son</i> (c. 1580)	19	0
<b>Group 2</b>	<b>A%</b>	<b>B%</b>
Lyly <i>Euph</i> (1578)	48	47
<i>Bombie</i> (1594)	36	25
<i>Arden</i> (1592)	50	40
Greene <i>Friar</i> (1594)	33	29
Whythorne <i>Aut</i> (1576)	47	17
<b>Period 3 (1600-49)</b>	<b>A%</b>	<b>B%</b>
Donne <i>Let</i> (1600-30)	61	6
<i>Son</i> (1633)	31	80
Heywood T. <i>Wom</i> (1607)	57	7
Middleton <i>Trick</i> (1608)	60	9
Beaum/Flet <i>Maid</i> (1610)	46	28
Webster <i>Malfy</i> (1613)	56	5
Massinger <i>Debts</i> (1625)	35	0
Ford <i>Whore</i> (1625)	57	20

The figures under (5) show that period 1 clearly differs from periods 2 and 3 in this respect. In period 1 the frequency of the *n*-variants before

polysyllabics is - except for *Everyman* - clearly higher than the total percentage of polysyllabics in the construction 'possessive + adjective/noun'; in periods 2 and 3 this relation changes drastically, i.e. the percentage of the *n*-variants before polysyllabics is clearly lower than the total percentage of polysyllabics in this construction in a text; this means that polysyllabics are an environment that evidently selects or prefers the *n*-less variants.

Towards the end of period 2 and in period 3, another morphological - or rather lexical - factor gains ground: the distribution of the *n*-variants is increasingly restricted to certain lexemes such as *own*, *eye*, *ear* etc., most of which are monosyllabic.

The growing restriction of the *n*-variants to certain lexemes becomes evident in the growing gap between the relative frequencies of token and type in the texts of periods 2 and 3 as against period 1, see under (2); i.e. the smaller the percentage of types in relation to tokens, the smaller the number of lexical items which collocate with the *n*-variants.

It is more difficult to say anything definite about the influence of stylistic factors within the individual texts, such as differences between dramatic characters according to social position, situation, verse vs. prose passages etc. But even here certain tendencies seem to become apparent towards the end of period 2, which might indicate a conscious use on the part of the author. Thus, for instance, in Peele's *Old Wives' Tale*, *my own* is preferred in the more realistic frame story, while the phantastic, sometimes pathetic characters of the main plot prefer *mine own*. In investigating this sort of stylistic marking, we must, however, be aware of the very small number of instances on which such conclusions are based.

#### 4. Conclusion

The present study has shown that the distribution of the variants *my/mine* and *thy/thine* before vowels cannot be regarded as "free variation". In period 1, the distribution of the variants is basically phonologically determined (*n*-less variants before consonants, *n*-variants before vowels). In regard to the distribution of the two variants before vowels, the *n*-variants are evidently the unmarked forms. (Whether the frequencies of *Everyman* already point to a colloquial or 'informal' marking of the *n*-less forms before vowels remains to be seen.) In period 2, suprasegmental and morphological factors increasingly seem to influence the use of the two variants before vowels, with a growing stylistic marking of the *n*-less forms as 'informal'. In period 3, the *n*-variants before vowels become stylistically marked as 'poetic' or 'archaic', except for a small number of lexically determined instances; in turn, the *n*-less variants now become the unmarked ones even before vowels. If these tendencies could be confirmed on the basis of a larger and still more stratified corpus, the

development of the variant forms *my/mine*, *thy/thine* would correspond to the familiar pattern of a change from below within a variationist approach.

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