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LETTER FROM THE EDITORS

Would you believe it? Barely half a year gone and here's another set of VIEWS. The reasons why we are going so strong are several (life's rarely simple) but the support of our colleagues near and far has played a significant role. We are, of course, talking about the response to our call for help in the last issue. This has been very encouraging and we would like to thank the following people for their donations (a few of them were just continuing a laudable tradition, too!):

Werner Abraham, Arleta Adamska-Salaciak, anon. (Italy), Rolando Bacchielli, Bernhard Diensberg, Wolfgang U. Dressler, Colin Evans, Alwin Fill, Andreas Fischer, Olga Fischer, Jacek Fisiak, Helmut Gneuss, Claus Gnutzmann, Boris Hlebec, Jonathan Hope, Yoshihiko Ikegami, Theresa Susanna Illes, Andreas Jucker, Lucia Kornexl, Ursula Lenker, Christian Liebl, Hans-Christian Luschützky, Christian Mair, Gerlinde Mautner, Gabriella Mazzon, Adam Nadasdy, Terttu Nevalainen, Winfried Nöth, Hermine Penz, Carol Percy, Reinhold Peterwagner, Helena Raumolin-Brunberg, Ernst Ritt, Gabriella Rundblad, Hans Sauer, Iris Schaller-Schwaner, Viktor Schmetterer, Edgar Schneider, Aimo Seppänen, Jenny Sheppard, Peter Trudgill.

Thanks also to Oxford University Press for buying advertising space. In order to be able to have another one of these lists in the next issue we are repeating the whole procedure in the present one. Watch for the little red slip. Those of you who forgot about it last time please give us a sign of life by sending us a comment, a contribution, a few loving lines and one of those banknote things ... If we don't hear from you at all, we will assume that we are simply contributing to your piles of half-un-wanted mail and we will strike you off our address list. If this has scared you and you are quite desperate to keep receiving VIEWS, you know what to do...

In terms of different VIEWS, the present issue brings you the promised sequel to Herbert Schendl's code-switching piece where he analyses switching strategies in mixed poems. Analysis of data is also at the centre of Karin Prögl-Rösler's contribution on generic pronominal reference in Present Day English, which has prompted a comment of a

more general kind by Henry Widdowson plus an answer by the author. Reacting to B. Seidlhofer's contribution on the (non)-native in ELT in VIEWS 5, Ute Smit points out the ideological loadedness of the matter by drawing an intriguing parallel. The remaining two contributions, though very diverse in their interests, are connected by the fact that they address the lives and fate of linguistic ideas and how they are subject to fashions, trends, methodological convenience, the social dynamics of the field, world history (WWII) and sometimes maybe sheer coincidence. Bryan Jenner has written up his detective's notes on articulatory settings, and Christiane Dalton-Puffer goes searching for meaning in word-formation theory. You may have noticed that this is the second issue in a row which contains a contribution referring to cognitive linguistics. Is this the beginning of a serious competition to our diachronic strand????

Please send contributions of the reactive and/or proactive type to:



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The Editors

Developing a meaning-oriented theory of English word-formation¹

Prolegomena = Some Ideas

Christiane Dalton-Puffer, Vienna

1. Points of departure

In the conclusion of my book on Middle English derivation I stated that in explaining the interaction of native and borrowed morphological material during the Middle English period the application of morphological naturalness parameters gave us valuable insights but that if anything had become clear during the project it was that “semantics was just as powerful a driving force behind the shifts and reshuffles in the Middle English derivational system.” (Dalton-Puffer 1996: 227). This diagnosis may generate very little surprise in those who are not professionally concerned with matters morphological and they may go on to ask exactly what semantic forces within which semantic theory I was thinking of. Those who are familiar with the current state of morphology and especially the morphological theorizing based on English, on the other hand, will be equally unsurprised by my ready admission that the semantic description of my data was based on an ad-hoc adaptation of a specific framework which, I think, I was able to put to good use while neither being wholly satisfied with its theoretical backing nor with its integration with the morphological framework.

Apart from a number of findings which are of interest for historians of English, the project raised some points which are of more general theoretical interest. It became clear to me that:

- Derivation seems to defy successful description within a strictly modular conception of grammar. This is hardly new, but worth repeating from time to time.

¹ Warning! The use of the term word-formation in the title is a concession to readability at the cost of precise expression. In the following it will become clear that I want to deal with derivation by means of affixes. This excludes conversion (zero-derivation) and compounding which are also part of word-formation in most terminologies.

- Meaning-based explanations for the changes observed in Middle English derivational morphology not only kept interfering with strictly formal morphological ones, they seemed somehow primary.
- There is a conspicuous lack of well-developed semantic theories of word-formation.
- The “meaning-factor” seems to comprise not only the semantic but also the pragmatic level, i.e. there are local “fields of derivational action” whose activity can only be explained by the fact that they belong to a clearly defined area of “real world” coherence (e.g. Middle English taxation terms in *-age*: *tollage*, *arrerage*).
- Frequency seems to play a role in the dynamics of the system but it is very hard to pin down what it does.

All this led me to the growing conviction that word-formation has to be approached via its function, via “what it is good for”. My current working hypothesis is therefore that word-formation provides the language user with a “machinery” for highlighting certain aspects of a situation, certain aspects of reality. It provides prefabricated tools for the (re)moulding, (re)casting and condensing of conceptual content. The requirements for these remoulding activities arise from the realities of language use as much as from the rules of grammar. Word-formation is a conceptualisation machine.

The aim of the research project introduced here is, then, to develop some coherent principles for an approach to English word-formation which is true to this assumption and therefore meaning-oriented.

2. Questions that have occurred to me in this context/ Problem areas which need looking into

It is true that the above-presented insights were gained while dealing with historical linguistic material, but I do not think anybody would contend they are equally valid for dealing with Present Day English. In fact I think that for the development of a semantic word-formation theory adhering to the synchrony-diachrony dichotomy is pointless and leaves us at a disadvantage. In pursuing the questions which I will present in the following, evidence from synchrony, diachrony and language acquisition will be equally admissible and necessary.

As far as I can see at the moment, the following questions will need to be discussed if we want to come up with a principled account of derivational meaning in English (and generally). Without doubt further questions will arise as we attempt to answer the following ones:

1. Which meaning units or constructs are always, typically, or rarely expressed by derivational means? Are there diachronic shifts?
2. What is the internal conceptual-semantic structure of derivational categories? Are they organised around prototypes? This also connects up to the question whether affixes are lexical items or rules and makes it necessary to consider the role of analogy. Possibly, analogy and rules are actually the same.
3. How are such derivational categories constituted, maintained and/or transformed?
4. What role does grammaticalisation play in the life of derivational categories? As this is an instance where the lexicon feeds grammar, this might indicate that we should give the lexicon a more central role in our conception of grammar.
5. How much of the combined conceptual structure of a complex word is predictable by the word-formation system and how much is context-dependent and arises through the speaker's knowledge of the referent? Are derivational categories "containers" of canonical relations which can hold between a base and an affix or between the base and the complex word and is the "final decision" as to what relation holds taken only at the moment of use? This, however, would mean a conception of grammar that draws no sharp line between the "system" and its "use".
6. How much of the conceptual structure of a base-word goes into the complex word? Is its grammatical category part of it? It may, for instance, well be that the syntactic category of the base is irrelevant at the level of word-formation. This would solve cases where it is impossible to decide whether the base of a derivative is a noun, a verb or an adjective. Such an assumption would serve to unify the description of patterns and probably allow better generalisations. This assumption would also entail another one: namely that the input to word-formation are not words (i.e. syntactic units) but "lexemes".

It is clear that in the market of linguistic theories as it is currently structured certain things are "lying around" that seem better suited to answering these questions than others and I will focus on them in due course. However, in order to put things into perspective, I would first like to stake out the geography of the field in which my plant will hopefully come to flourish.

3. Stocktaking or: a survey of English morphology in ca. 10 ½ paragraphs

3.1. Form-oriented vs. sign-oriented approaches to English morphology

Let me briefly summarise, then, (VERY much a bird's eye view) the present state of morphological theory and especially the morphological theorizing based on English (these two things are not always kept as neatly apart as one might wish). A pair of terms recently introduced by Kastovsky (1997) will give us a useful handle on this. This is the distinction between form-oriented and sign-oriented approaches to word-formation.

Judging by contributions to conferences and journals there is little doubt that the present morphological mainstream is very much of the form-oriented kind.² This orientation dates back to Bloomfield, Harris, and Hockett's Item and Arrangement and Item and Process typology of morphological models and has developed a variety of phenotypes in connection with so-called American mainstream linguistic theory. Examples are Lexical Phonology, Selkirk's word-syntax and, most topical at the moment, prosodic morphology within Optimality Theory. Since one of the basic tenets of this mainstream is the strict modularity of grammar, one might naively expect a separate morphological module. This however is not the case. Morphology tends to be dealt with either in the lexicon (especially derivation) or, more recently, done away with altogether to become part of syntax and phonology (cf. Anderson 1992). In short, and simplifying grossly, what used to be morphology becomes a set of syntactic or phonological rules or constraints or whatever the theory-specific term may be. It is noticeable that the "morphology = syntax" people are mostly concerned with inflection and/or compounding, while the "morphology in phonology" people tend to deal with (especially English) derivation. This is of course grounded in the peculiarities of the English language, especially its rich Neo-Latin vocabulary. All this, it is understood, is firmly within a view of grammar whose central component is rule-governed syntax, closely followed in importance by rule-governed phonology.

It is only fair to say that there have been syntactically-oriented approaches originating in the American mainstream which have operated with meaning elements in the shape of thematic roles and/or underlying cases but there does

² For those who believe in etymology, this is in fact exactly what the term "morphology" =

not seem to have evolved a coherent paradigm for the study of word-formation. In any case, these approaches would certainly regard complex words as a kind of “condensed sentence” and thus subscribe to the primacy of syntax.

Let me now turn to sign-oriented approaches. After 30 years, Marchand’s *Categories and Types* (1960/1969) still looms large in terms of a full-scale description of English word-formation or indeed the word-formation of any language. In its general conception the book is oriented towards an integration of form and meaning, and in the descriptive parts what is said about the meaning of various patterns and their subtypes probably outweighs the formal observations. There is, however, no coherent semantic theory to back up these observations so that the statements about meaning often appear ad-hoc and non-systematic, frequently creating the feeling that somehow significant generalisations are somewhere near the surface but are being lost. The “index of principal sense groups” added in the second edition cannot really save the day. Marchand’s central concept of the word-formation syntagma embodied in the determinant-determinatum structure is, in principle, two-sided, combining meaning and form. In its actual implementation, however, the formal-syntactic properties of the syntagma are given more prominence, with the syntactic properties of the determinatum defining the properties of the whole syntagma.³

Another, more recent, sign-oriented approach which has been spelled out in much greater theoretical detail is Natural Morphology (cf. Dressler et al. 1987). Natural Morphology is a neo-functionalist theory with a strong semiotic grounding. Essentially, it tries to make generalisations about how good morphologically complex expressions are as signs. One of the central concepts of the framework is that of diagrammaticity. In the most simple terms, diagrammaticity is about the fact that the best signs are those whose semantic compositionality is reflected in formal compositionality and vice versa. The English compounds *lightswitch* and *butterfly*, for instance, are not equally “good” semiotically because the formal compositionality of *butterfly* is not paralleled on the semantic level. The concept of diagrammaticity as such, of course, rests on meaning as much as it rests on form. In the elaboration and operationalisation of the theory, however, it turns out that meaning is taken as given, as an unanalysed entity, and much effort is then spent on working out how this meaning is reflected in linguistic form with different degrees of blurring (scale

³ The issue re-emerged later in generative theory (though with little to no reference to Marchand’s work) in terms of headedness and the ensuing bracketing paradoxes where the formal and the semantic constituency of a complex word do not coincide. This is the problem with approaches that operate with words (i.e. syntactic units) instead of lexemes or lexical stems as input to the formation of complex lexical items.

of morphotactic transparency) and what this does to the sign-quality of the linguistic expression.⁴

The bottom-line of this micro-survey is as follows: There is a clear split between form-based and sign-based approaches to morphology. But on closer inspection it turns out that in their actual implementation the sign-based approaches are much more explicit about formal matters than about meaning.

3.2. What is there in terms of word-formation semantics ?

By surveying the literature on English word-formation one might almost come to the conclusion that meaning-oriented approaches to word-formation are practically untilled soil. The picture changes somewhat once we widen our linguistic horizons.

It is tempting to speculate on the reasons for the “historical-geographical divide” between morphological theories based on English and morphological theories based on Romance and Slavic. At least two reasons come to mind. It may be that mainstream linguistics related to the English language has been so strictly modular and syntax-driven since the late fifties that morphology could not really find a place in its own right within that theoretical frame. The reason could also be that the more meaning-oriented approaches have been developed with reference to languages with morphologies much richer than English (certainly inflectional and partly also derivational). Linguists enthusiastic about form found plenty of stuff to play with in inflection so that in derivation there was some room left for the consideration of meaning (this is of course very oversimplified: currently, interesting semantically oriented morphological work is being done with reference to German inflection (e.g. Köpcke 1993).

In the study of the Romance languages Coseriu has established a school that sees the common widespread concentration on form as a consequence of methodological convenience rather than inherent priority. This basic belief is summarised in the statement: “Inhaltsfragen müssen in der Sprachwissenschaft den Vorrang vor Ausdrucksfragen haben” (Lüdtke 1994:127) and it is seen as unfortunate that in scholarly reality semantic questions are made to depend on formal ones rather than vice versa. Generally there seems to be a well-developed awareness that a general theory of word-formation and the description of the word-formation system of a specific language are not necessarily the

⁴ Dalton-Puffer (1996) is an application of Natural Morphology to data from Middle English derivation. For a semantic analysis it was necessary to turn to an unconnected, though compatible, framework. (On the parallels between Naturalness theory and cognitive linguistics see, for instance, Winters 1993).

same thing. In developing a meaning-oriented approach towards English word-formation it will be necessary to undertake a careful stocktaking of the work done in the Coseriu tradition in relation to several Romance languages.

Moving to another major group of European languages we discover another tradition of word-formation study which is strongly concerned with meaning. Scholars like Kurylowicz (1964) and Dokulil (1962; 1979) seem to have done ground-breaking work in this respect, but to the linguist who cannot read them in the original they are accessible only indirectly through more recent work such as Szymanek (1988) or Zbierska-Sawala (1993).⁵ The basic goal seems to be the establishment of a limited number of basic categories (such as Entity-Property-Action-Circumstance) which underlie naming in language or in a particular language. As far as I can tell at the moment, due attention is given to the formal linguistic expression of these basic concepts or categories so that these approaches seem to be truly sign-oriented. From this it follows that the concept of derivational category as the “meeting ground” of form and meaning occupies a central role in the approaches anchored in this tradition. Let me illustrate this with one example, Szymanek’s (1988) *Categories and categorization in morphology*. The book is, in fact, an exploration of the commonalities and overlaps between this Slavic tradition of meaning-oriented grammar and cognitive linguistics. In Szymanek’s understanding basic concepts are directly reflected in derivational categories. Simplifying his line of argument we can say, for instance, that Substance is a basic cognitive concept and “therefore” we have material adjectives of the type *wood-en*. Similarly, Agent, Instrument, Possession etc. are basic cognitive concepts which are reflected in derivational categories. Even though a slightly adapted version of the approach worked reasonably well for my own descriptive purposes in Middle English we cannot overlook its problems.

Firstly there is the status of these basic concepts. What is the concept of Result in “Resultative Nouns”, what is “Instrumental”, “Ornate”? The labels certainly come from traditional grammar, but are the constructs they refer to pre-linguistic cognitive or linguistic? Are these concepts universal or perhaps language specific? If we regard them all as universal derivational concepts, as Szymanek seems to do, we cannot explain that languages do not all exhibit the same derivational categories. What needs to be looked

⁵ Another radically meaning-oriented approach which probably has some of its roots in this Slavic tradition is that of Wierzbicka’s *Semantics of grammar* (1988). Her more recent research programme on semantic primitives attempts to establish a kind of common semantic core vocabulary presumably shared by and expressed lexically in all languages. It will be necessary to check whether these semantic primitives turn up as derivational categories.

into in a principled way, therefore, is what kinds of concepts different languages do express derivationally and whether there are any significant overlaps. It is entirely possible that some derivational categories will indeed turn out to represent universal derivational concepts. I would speculate that agent nouns are a prime candidate here.

Secondly, such an approach is limited in the sense that while being very useful for the “canonical” derivational types (such as de-nominal Material adjectives like *wood-en*, or de-adjectival Nomina Essendi such as *small-ness* etc.), it cannot account equally well for the more marginal types of word-formation or for newly emerging ones, which are semantically more specific and thus closer to the lexical end of the inflection-derivation-lexicon continuum. Diminutives or formations like *lemon-ade*, *cherry-ade*, would be examples for that. In short, it is much harder to postulate basic cognitive concepts for them. (Can there be a basic concept Soft Drink??) Neither can such an account cater for the subtle differences between more prototypical and less prototypical derivation (cf. Dressler 1989): more prototypical derivation is “more lexical” and has naming as its primary function (e.g. de-adjectival *small-ness*) while less prototypical derivation is more focused on syntactic recategorisation and has to do with recasting content into different (thematic?) roles (e.g. de-verbal abstract nouns like *writ-ing*).

In spite of the problems just sketched I do not think that such directly concept-based approaches to word-formation are entirely misconceived and should therefore be discarded but I think they need to be fitted into a larger framework which regards language as a whole as a conventionalised part of cognition.

4. Word-formation in a cognitive perspective

In the first section of this contribution I formulated my functionalist understanding of word-formation as a basic working hypothesis. Looking at cognitive linguistic theories as they are currently being developed, these seem to offer interesting connection points to my understanding of word-formation. In the remainder of this contribution I would like to outline very briefly where I think lies the potential of such an approach.

An important question which arose in the previous section was the relationship between language and cognition. It seems to me that cognitive scientists have by now reached a broad consensus about this relationship: as cognition and concept formation are instruments for structuring experience in general and as there is evidence that animals have concepts, we cannot assume that concepts are per se tied to language. Cognition is the basis of language and not

vice versa (Barsalou 1997). This does not preclude the assumption that language plays a special part in the cognition of humans.

In cognitive linguistics itself three main approaches or views are discernible at present: the experiential view, the prominence view and the attention view (Ungerer & Schmid 1996: xi). As always, a particular view has its particular strengths in explaining particular parts of reality. I will try to characterise each of them very briefly because each of them seems to bear on the questions I have formulated at the outset.

The Experiential view started out in cognitive psychology. Its classic field is the study of categorisation. The meaning attributes of concepts/words elicited from speakers show that these attributes go far beyond inventories developed on a strictly logical basis. It is held that these attributes reflect the way language users experience and interact with the world. It is held further that our shared experience of the world is stored in our language(s). Studies within this view are mostly concerned with what has traditionally been called lexical semantics: the study of lexical items, lexical categories, prototypes etc. There is also a strong strand in the study of metaphors, that is, with the way(s) in which basic experience is brought to bear on less basic experience. Stances developed within this view relate to my question of what meanings are actually expressed derivationally. It would also be interesting to follow up the thought in how far derivational processes are actually metaphors themselves.

On a superficial level the Prominence and the Attention views of linguistic structure are very similar: both are concerned with the fact that from the potentially endless number of attributes present in an object or a situation only a small number eventually surface in linguistic expression. (By “linguistic expression” I here understand form-meaning conjuncts.) It is generally assumed that the same attribute reduction is characteristic of concept-formation, which again points us to the question of whether linguistic semantics and concepts are really two different things....

As far as I can see at present the Prominence and Attention views seem to be differentiated by the fact that the prominence view seems to depart from prominence relations which are simply “there” or naturally given, such as the prominence of movement over stasis. Studies within this view are typically directed towards the description of relations (which can be spatio-temporal (prepositions) or syntactic (subject/object)). The attention view seems to be directed towards the structuring of events (verbs) and towards the question of why and how some stages of events typically surface linguistically and why others do not. Both views are relevant for answering the questions formulated in section 2: what is expressed derivationally as such in the language system and also how derivation operates within a text.

I am well aware that all this is rather vague. The fact is that at the present moment very little can be said about word-formation in the cognitive paradigm because very little seems to have been done. The two studies which relate the Slavic meaning-oriented tradition to the cognitive approach (Szymanek and Zbierska-Sawala) have already been mentioned. Apart from that, I am aware of one derivation study done within the Langackerian framework of Cognitive Grammar, which, interestingly and tellingly, also has a Polish author (Gorska 1994).

Cognitive linguistics is a discipline in flux and in development; as far as I can see at the moment Langacker's Cognitive Grammar⁶ is the most comprehensive and coherently formulated theory of linguistic structure within the cognitive paradigm. On the following page or two I would therefore like to highlight some aspects of the theory which seem promising in pursuing the questions which bother me. (Following our basic cognitive need to categorise we might say Cognitive Grammar subscribes to the Prominence view but like all categorisations this one, too, does not tell the whole story.)

- Cognitive structure does not surface directly as linguistic expression. The speakers of a specific language construe extralinguistic reality in certain conventional ways. This body of conventions makes for a separate, language-specific, semantic structure.

The meaning of an expression [=a linguistic form; CDP] is not given solely (if at all) by the objective properties of the situation it describes – rather it is a function of how speakers construe the situation and structure it by means of specific images. Semantic structure is therefore language-specific to a considerable degree, for the choice of images is a matter of linguistic convention. (Langacker 1990: 56)

Grammar thus is nothing but the specific conventional imagery of a language. One short example will have to make the point: take the language-specific choice of whether to mark animacy morphologically (or not). For the speakers of a language which has animacy marking the decision of whether an entity is animate or not is not only largely prefabricated but also foregrounded and salient. For the speakers of a language which does not have obligatory animacy marking the distinction stays in the background. This does not, of course, amount to saying that the distinction does not exist for those speakers and that it cannot be expressed linguistically. Langacker's view is thus not Whorfian, or only very weakly so.

These considerations bear directly on my question 1, which is concerned with the problem whether there are any candidates for the status of “derivational universal”. In other words, whether we find any cross-linguistic pref-

⁶ Langacker 1987, 1990, 1991.

erences for expressing certain meanings morphologically rather than otherwise. Plurality, for instance, seems to be a possible candidate here. In word-formation proper the expression of agenthood might also be one. But a comparison of totally unrelated languages might surprise us yet.

Given this outlook on meaning in language it is clear that also within one language two linguistic expressions can designate the same objective situation but differ semantically because they structure the situation through different images. This bears on my question 5.

- In cognitive grammar there is no sharp dividing line between specifications of an entity or event which are linguistically relevant and those that are not. “Any facet of our knowledge of an entity is capable in principle of playing a role in determining the linguistic behaviour of an expression that designates it.” (Langacker 1990: 4) Specifications which are linguistically relevant are thus relevant by convention (and conventions may change) - and here is where grammaticalisation (my question 4) comes in. I assume that the fact of there being a conventionalised linguistic expression for a certain meaning or meaning-complex underlines its relevance to the speaker(s) although this relevance may simply be a historical fact which has been preserved in linguistic structure. Can we indeed postulate that? Many of the canonical word-formation categories (Agent nouns, Nomina Essendi) seem to have been there “forever” - though a contrastive approach integrating many unrelated languages may teach us otherwise. But I think that even within the relative shortness of our attention span we can find support for our relevance argument. The independent development of a specific word-formation pattern to designate “can V/can be Ved” in a variety of European languages at the same time that *-able* sprang to life in English may well prove to be a case in point (cf. Dalton-Puffer forthcoming a and b).
- This encyclopedic outlook on meaning also has its impact on how we view the generation of meaning in a derived word (as sketched in question 5). It is entirely compatible with the grammaticalisation view sketched above that the dividing line between linguistic and extra-linguistic specifications, while being flexible in principle and diachronically, is clear-cut at a certain given point in time. Such a view, however, is not compatible with my speculations about the genesis of the meaning of a derived word as sketched in question 5 since that postulates that language use in a specific situation (i.e. pragmatics) plays a role in this process (is a *toaster* a gadget or perhaps the person responsible for producing toasted bread at breakfast-time in a hotel?). Such a view of the construal of derivational meaning, however, does not in principle constitute a problem for Langackerian cognitive grammar as it takes a non-modular view of grammar and makes the integration of pragmatics its

declared programme (Langacker 1990: 105f). The details of such an integration have, to my knowledge, not been spelled out and will present a problem or two in terms of modelling. But the foreseeable problems do not exonerate us from making a step into this direction.

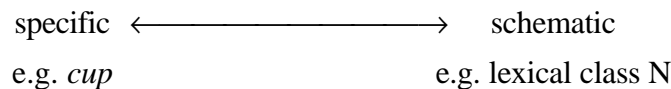
- A view of language that does not peripheralise pragmatics has three other facets which are highly interesting in dealing with word-formation and derivational categories. Firstly, such a view allows language use to feed back into the conventionalised core of the system and thus to change it (cf. the grammaticalisation issue), secondly it does not operate with ideal speaker/hearers but allows for the differential linguistic knowledge of different speakers. This differential knowledge depends on the speakers' degree of mastery of and familiarity with linguistic units; it can change over time and allows us to cater for questions of language acquisition as much as for stylistic variation (e.g. the preponderance of Neo-Latin word-formation in English scientific texts).⁷ Thirdly, since language use takes place at concrete points in time these instances of language use may be repeated at short intervals, at long intervals or not at all. Thus instances of language use sum up to type and token frequency effects. This is a dimension which has played practically no role in linguistic theory building over the last decades, but which cannot a priori be discounted because "frequency is not part of the system". Cognitive psycho-linguists are in fact producing the first empirical results which strongly suggest that frequency IS part of the system (Baayen 1996, 1997).
- In Cognitive Grammar the linguistic symbol has two sides, meaning and sound. (Few will fail to be reminded of Saussure here and also of functional approaches....) The minimal (but not the only) symbolic unit is the morpheme. It is in this context that I think my question 2 (internal structure of derivational categories) could be pursued.
- Unfortunately, derivational categories are not neat, disjunct entities. In this they are of course similar to most other linguistic and extra-linguistic categories. And so it is typical of many areas of derivation to be characterised by non-unique relations between the two poles of meaning and form: there often are multiple exponents for one function as well as multiple functions of single exponents. At least this is so when we describe derivational function

⁷ This knowledge differential was also one of the basic and unsolved problems of my Middle English study when a decision had to be made about the status of the newly borrowed lexical material. How do we account for bilingual and multilingual competences in the description of one language system? Clearly the status of Romance loans was a different one to speakers with different educational levels.

with the means available to us up to now, that is, in a relatively detailed, well-specified fashion. Differences in the degree of specification can of course be found and some categories are therefore more clear-cut than others. But these differences have not been part of a theory so far.

- Cognitive Grammar, on the other hand, allows for symbolic units to vary along a “specificity parameter” which is embodied in a scale running from

(1)



I would therefore like to pursue the idea and speculate that a specific type of derivation (such as the derivation of agent nouns) is present in the grammar as a symbolic unit which is both complex and schematic (cf. Langacker 1990:17). To account for multiple exponents we could therefore postulate that the phonological side of this schema is maximally general, the semantic side perhaps less so and this may vary from one derivation-type to another. That is to say, some are more schematic than others. It is almost trivial to state that deverbal abstract nouns such as: *specify-specification* are evidently more schematic than a formation of the *cherryade* type. It would also be interesting to check in how far this variation along the specificity parameter might correlate with Dressler’s distinction between more and less prototypical derivation.

5. Conclusion

This contribution contains loose ends and inconsistencies and maybe they are even more maddening to me than to the reader. At the hub of the problem lies the question (grossly foreshortened in my exposition) of whether linguistic form enters into direct connection with concepts or whether we ought to assume a kind of interface that “translates” pre-linguistic concepts into linguistic ones. In short, the question is whether we need semantics as something that is distinct from cognition. My exposition in section 4 has been biased in so far as it has largely followed the ideas of Cognitive Grammar, and Langacker is a believer in semantics. A closer inspection of connectionist models may, however, create a rather different perspective. I am quite simply miles from having gained clarity on this issue. I hope that my colleague Arthur Mettinger’s cognitivist treatment of contrastivity will help me to make headway in this area.

It needs to be said, though, that apart from its factual and intellectual complexity the whole issue is also “politically fraught”: for one thing abolishing semantics to the linguist is a drastic, amputation-like measure, secondly, and

more seriously, abolishing semantics seems to lead us straight into embracing a Whorfian standpoint and thus into an infringement of a central dogma of modern linguistics. Ever since I personally can remember this has been a no-go area for modern, democratic intellectuals but it may be time to venture into it in order to re-examine the dogma - if only for reconfirmation.

Let me restate, then, the main goals of the project whose prolegomena you have just been reading. The more immediate and narrower goal is to give a semantically principled account of Modern English derivation. In how far the scope can or indeed should be extended to conversion and compounding, i.e. to word-formation as a whole, will be a matter of how it all develops. In a wider sense I hope to make some headway in finding out which concepts form the basis of derivational categories in a broader sample of languages and to contribute to a word-formation theory which rests on a coherent theory of meaning. In the end this should enable us to come up with a truly sign-oriented theory of word-formation.

I am aware that much of what I have said is very general - I hope it is not so general as to be vacuous. What I hope to have shown, though, is that in crossing over between different linguistic schools and traditions we detect a web of ideas which might be brought to bear fruitfully upon word-formation. As we all know, the proof of the pudding is in the eating. Watch this space.

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Articulatory Setting: the genealogy of an idea

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Acknowledgments

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1. The Idea

In 1964 there appeared a book entitled *In Honour of Daniel Jones*, edited by David Abercrombie and others. (Abercrombie et al, 1964) It contained a set of papers, offered originally to Daniel Jones in 1961 on the occasion of his 80th birthday by a number of distinguished scholars, colleagues and associates. It was then published as a tribute to his great contribution to the teaching of phonetics over a period of more than 50 years. Among these papers was one entitled 'Articulatory Settings' by Beatrice Honikman. (Honikman 1964)

The paper is an odd one to find in a collection devoted to Daniel Jones since it outlines an approach to phonetic description and foreign language teaching which seems to be the antithesis of everything that he stood for. While Jones devoted his life to the phoneme and to ever more precise descriptions of the individual sounds of many varieties of English and other languages, Honikman's paper represents an attempt to embark upon a different course and to describe a set of general articulatory features or habits which would permit very broad non-segmental characterisations of different languages and varieties.

The paper claims that 'where two languages are disparate in articulatory setting, it is not possible completely to master the pronunciation of one whilst maintaining the articulatory setting of another'. (74) The implication of this is that it would be better to spend less time teaching or studying segments and much more on defining and teaching features of the general articulatory posture of particular languages. The paper then goes on to describe the respective set-

tings of English and French and to show how such descriptions may be used in practical language teaching.

The paper has made some impact over the years on pronunciation teaching and has from time to time been cited as a useful source for a productive alternative approach. (cf. MacCarthy 1978, Jenner and Bradford 1982, Jenner 1987, Dalton and Seidlhofer 1994.)

Unfortunately it contains no references to any other works and has therefore been treated by most British (and American) phoneticians since that time as completely original. For instance, Abercrombie 1967, Crystal 1969, Gimson 1970, O'Connor 1973, Trudgill 1975, Esling 1978, Wells 1982, Jenner 1984 and Roach 1991 all attribute the idea, or at least the term, to Honikman and no-one else. That is to say, all of these authors either refer directly to her article or, at least, include it in their bibliographies, and give no other sources under the heading of 'articulatory setting'.

Two citations will serve to illustrate the point. In Abercrombie (1967) we find:

For a very full and clear account of adjustments of this sort, and how they characterize particular languages, see 'Articulatory settings' by Beatrice Honikman (1964)
(93 note 3)

Roach (1991) offers the following:

Differences between languages have been described in terms of their **articulatory settings**, that is, overall articulatory posture, by Honikman (1964). She describes such factors as lip mobility and tongue-setting for English, French and other languages.
(131 - 2)

In the case of the Abercrombie citation, it should be remembered that he was a co-editor of the book in which the paper originally appeared, so he might be expected to attribute some importance to it. He does, incidentally, also make a passing reference to Sweet (1902) to whom he attributes some brief discussion of 'some of the factors involved in voice quality.' These, he informs the reader, may be found in the *Primer of Phonetics* on page 72. It is a matter of considerable regret that he did not turn the page and find, on page 74 of the same work, a discussion of *Organic Basics*, which begins:

Every language has certain general tendencies which control its organic movements and positions, constituting its organic basis or *basis of articulation*.
(my italics) (Sweet, op. cit. 74)

I shall return to Sweet below but this reference may already suggest a problem with Honikman's paper.

It has therefore been widely accepted that Beatrice Honikman invented the idea of 'Articulatory Setting', although Laver (1994) reminds us that there were references to this phenomenon in much earlier British writers, such as

Wallis (1653) and Wilkins (1668). (399) He also states the following (Laver 1980):

The originator of the term ‘articulatory setting’ was Honikman (1964:73). But while the term was new, the general concept was not. (12)¹

He then goes on to point out that a number of 19th century phoneticians (Sweet, Sievers, Storm, Jespersen and Viëtor) were interested in the topic, but significantly - as we shall see below - makes no reference to any writer on the subject between these late 19th century scholars and Honikman’s 1964 paper.

2. Grounds for Suspicion

In 1988, during a visit to the Charles University in Prague, my attention was drawn to a work by two East German phoneticians, Arnold and Hansen (1975), entitled *Englische Phonetik*. This contained a short section on ‘Artikulationsbasis’ which outlined, with reference to differences between English and German, the same kinds of general articulatory features as Honikman had described in her 1964 paper.

Arnold and Hansen’s book contains references to a number of earlier sources which deal with the same subject but, significantly, made no direct reference to Honikman’s paper. This cannot have been because western sources were difficult to obtain, since their very full bibliography does include, amongst others, Abercrombie 1967, Abercrombie et al (1964) which is where the Honikman paper was published, Gimson (1970), and O’Connor (1973).

There are at least two possible interpretations of this situation: either a) the two sources arrived at the same conclusions independently - which would be rather odd in view of the fact that the East Germans claim no originality for their version of the idea; or b) both drew on an earlier common source. The German authors give us clear information concerning their sources, but Honikman does not.

The fact that they cite the book in which Honikman’s paper appeared, but do not mention her in their discussion of *Artikulationsbasis*, suggests that the latter interpretation is more likely and that, unlike many British writers, Arnold and Hansen knew that the idea did not begin with her paper.

¹ Others have written summaries of the general history and development of the idea. (cf. Laver 1978, Kelz 1971.) My concern is more specific: what was the ancestry of the particular version of the idea which Beatrice Honikman used? Laver’s bibliographies have been very useful in suggesting possible lines of enquiry, although the German tradition is underrepresented. Kelz’s paper was less helpful in that the dates given in his bibliography are not always accurate.

In this situation the researcher is in a position analogous to that of the genealogist confronted with two individuals sharing an unusual family name but with no apparent connections between them. The genealogist's approach in such circumstances is to find out as much as possible about the history of each of the individuals, however irrelevant it may initially appear, and to work backwards until a plausible common ancestry can be established.

Viewed in this way Arnold and Hansen's book appears to have a respectable ancestry, while Honikman's paper has 'no known parents'. One is therefore forced to examine the circumstances in which it was written, to look for other publications by the same author and to examine her professional history, in the hope that this may provide clues to possible sources and influences.

Before embarking upon that, however, it will be enlightening to examine the ancestry of the German source, to establish likely candidates for the desired common ancestor. This will involve, in various ways, a consideration of the effects of political circumstances on the pursuit of scholarship and the interchange of ideas. It may also force us to reconsider the relative importance of a number of key figures in the history of phonetics.

3. The German Tradition

Arnold and Hansen offer the following definition of 'Artikulationsbasis':

Die Grundhaltung der Sprechwerkzeuge in Sprechbereitschaft und die Besonderheiten der Bewegungsart der aktiven Sprechwerkzeuge nennt man *Artikulationsbasis*. Im wesentlichen handelt es sich dabei um die Grundhaltung des Unterkiefers, der Lippen und der Zunge..... Jede Sprache hat ihre besondere Artikulationsbasis, die man sich als Ausländer im allgemeinen nur schwer aneignet, so daß beim Sprechen ein fremder Akzent' entsteht, an dem man bald als Fremder bzw. Ausländer erkannt wird. (21)

They go on to give a more detailed description of the particular features of English and German settings, and direct the reader to a work by Vockerath (1925) for fuller discussion of the differences between German and English, and to Zacher (1960) for a comparison of the settings of German and Russian.

The work by Vockerath is a doctoral dissertation from the University of Greifswald in north-eastern Germany. An electronic mail survey enabled me to establish that there are still copies of this dissertation in university libraries as far afield as Ann Arbor (Michigan), Pennsylvania, Illinois, Yale, and Konstanz. I was also advised that it is probably still retained in some university libraries in eastern Germany (including Greifswald) where pre-war collections survived both the bombing and the 'cleansing' of the Cold War. The appeal received a total of 10 replies, none of them from the United Kingdom. Subsequent searches of university libraries also suggested that in all probability there

are no copies now available in the UK. The copy which I ultimately consulted was discovered in the reserve stack of the library of the University of Vienna.

Vockerath (1925) represents a serious full-length attempt to describe the general differences in articulation between German and English and to assess the relevance of this to the teaching and learning of English pronunciation in German speaking countries. There is much in the book which strikes us as quaint or even dangerous today, such as sections where he claims that the German skull is of a different configuration from the American skull, or that the British articulatory setting is a reflection of the British propensity for diplomacy!

On the other hand, it was the largest single discussion of the topic until Laver (1980), and Vockerath is entirely honest about his sources, quoting extensively from Sievers (1893, first edition 1881²), Viëtor (1923, first edition 1884) and, above all, Sweet (1902/1906, first edition 1890), and Sweet (1877) to whom he gives credit for first developing the concept of ‘organic basis’.

The work by Zacher (1960) is, in some ways, even more interesting. It is an ideologically purified outline of phonetic theory applied to the description of German. It was published in Leningrad in 1960 by a German scholar working in the Soviet Union, and opens with rousing quotations from Marx, Engels and Lenin. It then outlines the development of phonetic science, attributing its origin solely to the work of the ‘Russian’ (actually Polish) scholar Baudouin de Courtenay. Ferdinand de Saussure and Trubetzkoy receive an almost grudging mention for contributions to other aspects of linguistic science.

The great names of 19th century phonetics elsewhere in Europe (Sweet, Jespersen, Viëtor, Sievers, Passy) are simply not discussed and Sweet does not even figure in the bibliography.

This would perhaps not matter if Zacher had not included a very full and clear contrastive discussion of *Artikulationsbasis und phonetische Basis*. (46f.) This contains a thorough characterisation of the differences between German and Russian, and a shorter description of the features of the English setting.

This particular section is presented without any indication of sources for more detailed discussion, although we do find the names of Viëtor (1923, 1st edition 1884) and Sievers (1893, 1st edition 1881) in his bibliography. These, it should be noted, are precisely those sources which are both listed **and cited** by Vockerath.

I think there can be little doubt that Zacher used a German source for the idea of *Artikulationsbasis*, and that that source was either Vockerath directly

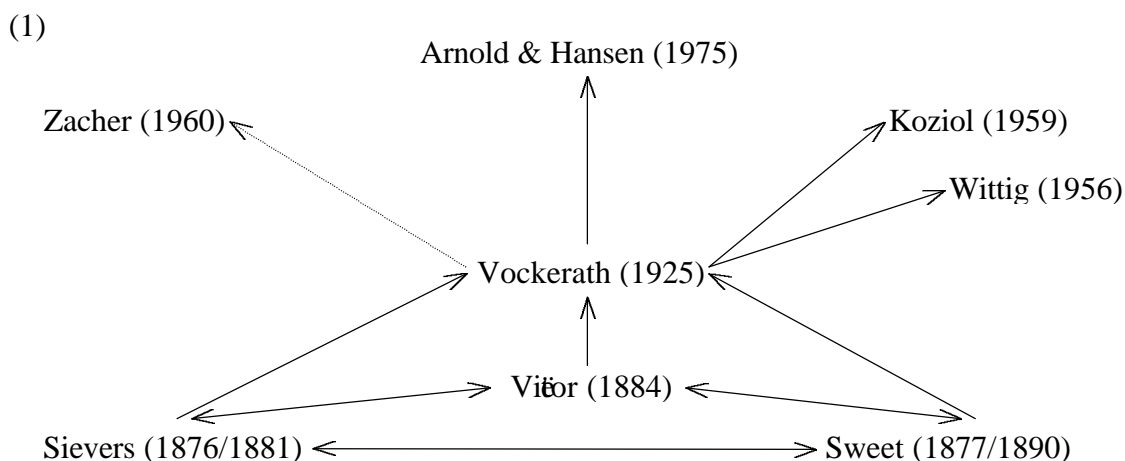
² Sievers wrote of the concept in an even earlier work (Sievers 1876). This is not the source used by Vockerath, however.

or one of the earlier German sources (Viëtor or Sievers) whom Vockerath used. For ideological reasons, however, Zacher perhaps felt it was inappropriate to mention them.

Just to underline how widespread the idea was in the German-speaking world we may also note that *Artikulationsbasis* is also used by Wittig (1956) in a description of the articulation of American English, in which he contrasts the American habits not only with those of German but also with British English. Wittig glosses *Artikulationsbasis* as ‘organic base’ (sic), which suggests that he was aware of the work of Sweet, even though Sweet does not figure in his bibliography. None of the German sources is listed either, which suggests that for him the term was so well-established that it no longer required any justification. *Artikulationsbasis* is also discussed, albeit briefly, in *Die Aussprache des Englischen* by the Viennese phonetician Herbert Koziol. (Koziol 1959) After 10 lines of description of the English setting, he also notes that the idea was derived from Vockerath (1925).

4. The German Family Tree

A clear pedigree is now beginning to emerge for the notion of *Artikulationsbasis* in the German-speaking world:



With the exception of Sweet and Viëtor none of these is included in Laver’s 1980 and 1994 bibliographies which are in every other respect definitive. Honikman (1964) is for him, as for all other British authors, the only source mentioned after Sweet. We therefore need to examine this apparent gap (from 1902 until 1964) against a wider historical background. In particular, we need to examine the development of British phonetics in the 20th century. We will also need to reassess the role of Daniel Jones, some aspects of the history of the Phonetics Department at UCL and the movements of Beatrice Honikman.

5. The British Tradition

Henry Sweet

It is generally agreed that the founding father of phonetic science in Britain was Henry Sweet. His influence extended far beyond Britain, and in some respects he was taken much more seriously – at least in university circles – in Germany than he was in his own country.

Sweet produced 2 major phonetics textbooks (*A Handbook of Phonetics* 1877, and *A Primer of Phonetics*, 1890, second edition 1902), both of which make reference to the *organic basis* of pronunciation, and offer short comparisons of English and French, with a passing reference to German.

In the *Primer* the difference between English and French is described as follows:

In English we flatten and lower the tongue, hollow the front of it, and draw it back from the teeth, keeping the lips as much as possible in a neutral position. The flattening of the tongue widens our vowels, its lowering makes the second elements of our diphthongs indistinct, front-hollowing gives a dull resonance which is particularly noticeable in our **I**, its retraction is unfavourable to the formation of teeth-sounds, and favours the development of mixed vowels, while the neutrality of the lips eliminates front-round vowels.....

In French everything is reversed. The tongue is arched and raised and advanced as much as possible, and the lips articulate with energy. French therefore favours narrowness both in vowels and consonants, its point-consonants tend to dentality, and, compared with the English ones, have a front-modified character, which is most noticeable in the **I**, while the rounded vowels are very distinct.

Similar descriptions are found in the *Primer of Spoken English* (1890), *The Sounds of English* (1908), and the article on ‘Phonetics’ in the 11th edition of the *Encyclopædia Britannica* (1911).

The number of sources, and the long time-span between the earliest and the latest of these, suggest that Sweet was very committed to the idea throughout his long career and looked upon it as a fundamental general principle both in phonetics and in language teaching.

In Sweet’s opinion, the work undertaken by himself and his German colleagues in the course of the 19th century had led to a state of affairs where the segmental descriptions of languages had been largely taken care of, although there was still work to be done on the kind of notation to be used in transcription. Quite early in his professional life he wrote:

The whole subject of intonation, especially, requires to be thoroughly investigated by a thoroughly competent observer, which I am very far from being, my natural ap-

itude and my training being equally defective. It is in this branch, *in the study of voice-timbre and of synthesis generally*, that the work of future phoneticians must be concentrated. (Sweet 1877, p x)

In both the *Primer* and the *Handbook* we find that *organic basis* is dealt with under the general heading of *synthesis*, and it is quite clear from all the references that he felt the most important and perhaps the only important work for his successors was in these essentially suprasegmental areas. Indeed, it is almost ironic, in view of subsequent developments, to find the same claims and statements still being made in the *Encyclopædia Britannica* article of 1911-2 years **after** the first edition of Daniel Jones' *Pronunciation of English* (Jones 1909). This work of Jones established the atomistic approach which was totally to dominate British phonetics for the next 60 years.

Sweet was also (in the 1880's) a founder member of the organization which ultimately became the *International Phonetic Association*. He collaborated closely with scholars in various European countries: Passy in France, Sievers and Viëtor in Germany, and Jespersen in Denmark among others. Academic interchange was very common in the latter part of the 19th century and relations between Britain and Germany seem to have been particularly cordial. Sievers, for instance, spent 2 years as a Lector in Liverpool and Sweet was awarded an honorary doctorate by the University of Heidelberg in recognition of the importance of his pioneering work.

Unfortunately Sweet seems to have enjoyed very little popularity in his own country. For that – if G.B. Shaw is to be believed – he had only himself to blame. In the preface to *Pygmalion* (1916 – less than 4 years after Sweet's death) Shaw writes:

(Sweet) was about as conciliatory to conventional mortals as Ibsen or Samuel Butler. His great ability as a phonetician (he was, I think, the best of them all at his job) would have entitled him to high recognition, and perhaps enabled him to popularize his subject, but for his Satanic contempt for all academic dignitaries and persons in general who thought more of Greek than of phonetics. Once ...I induced the editor of a leading monthly review to commission an article from Sweet on the special importance of his subject. When it arrived, it contained nothing but a savagely derisive attack on a professor of language and literature whose chair Sweet regarded as proper to a phonetic expert only ... When I met him afterwards, for the first time in many years, I found to my astonishment that he who had been a tolerably presentable young man, had actually managed by sheer scorn to alter his personal appearance until he had become a sort of walking repudiation of Oxford and all its traditions ...The future of phonetics rests probably with his pupils, who all swore by him ... He was not in the least an ill-natured man: very much the opposite, I should say; but he would not suffer fools gladly; and to him all scholars who were not rabid phoneticians were fools.

Whoever Sweet's pupils were, they seem to have disappeared without trace. Possibly they fell during the First World War, or perhaps their very as-

sociation with Sweet counted against them. Whatever the case, there was to be no new mention of organic or articulatory basis in British publications for many years.

Dumville (1909) includes a whole chapter (10 pp.) on *Organic Basis* in which he describes the broad differences between English and French. He also warns that

....it is useless, nay harmful, for a person wishing to obtain a correct pronunciation of a foreign tongue to rely on books giving a so-called 'phonetic' transcript which is nothing more than the spelling of the foreign words in such a way that the learner merely transposes the sounds of his mother tongue into the words of the foreign language. Although the differences of articulation are often very slight indeed, the fact that they occur *in almost every sound* makes the general effect of a word or sentence quite distinct from that produced by a native. (my emphasis)

Although Dumville's work was reissued in 1926, it was to be the last reference to bear the hallmark of Sweet. From then on there was a new emphasis dominated entirely by the interests and methods of Daniel Jones.

Jonesian Phonetics

Jones made his mark very quickly. He was appointed to a lectureship at University College London in 1907. In the same year he became joint editor, with Passy, of *Le maitre phonétique*, and Assistant Secretary of the International Phonetic Association. In 1909 he published the first edition of *The Pronunciation of English* – a work that went through several editions and was still widely used in the 1960's. In 1914, after a second book, he was appointed Reader in Phonetics. As the senior member of his department he was presumably exempt from military service and was able to remain active throughout the first world war. By 1921 he was Professor of Phonetics – a position he retained until his retirement in 1949.

After the death of Sweet, British phonetics revolved almost entirely around Jones. He retained the editorship of *Le maitre phonétique* until 1950, by which time it was almost exclusively devoted to his own preoccupation with the precise segmental description and transcription of languages. He was Secretary of the IPA from 1938-1949 and was elected President in 1950.

He also dominated the work of his own department at University College London, and the style of phonetics teaching and research in other parts of London University and beyond. The 'London School of Phonetics' – devoted to segmental matters – became firmly established.

The more general articulatory aspects of languages, and the other topics on Sweet's agenda, were effectively buried. In particular, the pedagogically useful

contrastive dimension implicit in the work of Sweet and his contemporaries seemed now to be a taboo subject.

Jones did list in his bibliographies a number of authors who had written about the ‘basis of articulation’ (Dumville, Viëtor, Sweet, Sievers) but he never discussed the notion himself. His bibliographies, moreover, had a different purpose both from those of the German tradition and from the kind of bibliography which is conventional today. Whereas German scholars, from the mid 19th century onwards, discussed each other’s ideas and attributed terminology to their original sources, Jones typically makes no textual reference to any other scholars. His bibliography is simply an annotated list of ‘books recommended for further study’.

Moreover, British phonetics seems to have become extremely insular during this period. For example, although academic contacts with Germany were very limited in the post-war years, it is nonetheless surprising that Jones was apparently unaware of work being done there, since he travelled quite widely in Germany, spoke German well and also published editions of several of his works in Leipzig. (e.g. *An Outline of English Phonetics*, 2nd edition 1922.) It would also be surprising if copies of the various early German sources referred to above (e.g. Viëtor, Sievers) were not held in his departmental library since they are still to be found in the main library of UCL in very early editions.

It is also remarkable how little new work on English phonetics was actually published in Britain between the two world wars. The sole exception is, of course, Jones himself, but his contemporaries seem to have been so overawed by his eminence that they felt that there was nothing worth saying.

Throughout this period, then, British phonetics, and the International Phonetic Association which was controlled from University College London, pursued a policy of narrow atomistic segmental description. This phoneme-dominated approach continued more or less uninterrupted until the work of the Edinburgh school (Abercrombie 1967, Laver 1968, 1980, 1991) revived British interest in the subject of ‘settings’ and voice quality.

Beatrice Honikman

Beatrice Honikman came to Britain from South Africa in 1932, according to the records of the IPA, to take up a position in the School of Oriental Studies, where she specialized in the phonetics of African languages. She remained at SOS (later SOAS) until well after the end of the second World War, and is referred to by Daniel Jones (*Maitre phonétique* 1947) as ‘helping to keep the Association going during the war years’. The last years of her professional life

(until the late 1960's) were spent in the Department of Phonetics at Leeds University.

She worked with Jones on several projects during her London years. Jones thanks her for assisting with the revised (1949) edition of *The Pronunciation of English*, and this acknowledgment is repeated in the 1956 edition. In the 1959 edition she is again thanked for 'a number of helpful suggestions'. She receives a further acknowledgment in the 11th edition of the *English Pronouncing Dictionary* (1956) for undertaking 'a great part of the arduous task of calculating the number of words recorded.' We may therefore assume that she was very much a phonetician in the Jonesian segmental mould.

With the exception of her work in bringing Liliás Armstrong's study of Kikuyu to press (Armstrong 1940), her only published work was the single paper on 'Articulatory Settings'. The subject of the paper does not seem to have been reflected in her phonetics teaching, either in London or subsequently at Leeds, and was not followed by any further reflections on the subject, journal articles or conference presentations.

Why, then, did she choose to write this paper as her only contribution to the literature of phonetics, and what were its antecedents? It is apparent from the very first paragraph that there must have been antecedents. Honikman writes:

..there is an elusive aspect of articulation which, up to the present, *if not totally neglected*, has not received the attention it merits. (my emphasis)

This suggests that Honikman was aware that the phenomenon which she re-named 'articulatory setting' had been described in the literature before 1964. The question is, which – if any – of the earlier publications was she actually familiar with? This is a particularly significant question in view of the profound influence of Daniel Jones and his own neglect of any of the more general features of pronunciation.

There are several possible candidates among the works already mentioned. From the very early sources we should consider Sweet (1877, 1890 and 1911), Viëtor (1884, available in an English edition from 1889), and Dumville (1909/1926). Honikman was already 27 years old when she arrived in London and had presumably completed her basic phonetic training in South Africa. It is entirely possible that Sweet's works were still in use at that time and that the reputation of Jones was not yet as considerable there as in Europe. Sweet would, in that case, be the most likely source.

Honikman's early interests, however, were in African languages, and it is equally likely that she had encountered none of these works before coming to London.

A further possibility is that the matter of articulatory basis cropped up from time to time in discussions with one or more of her contemporaries in London in the 1930's. An obvious candidate for this might be David Abercrombie, who was at the London School of Economics from 1934 onwards, and who subsequently became interested in matters of voice quality and settings. It could even be that it was Abercrombie who suggested this to her as a possible topic for her paper.

A third possibility, of course, is that she genuinely stumbled upon the idea as a result of her own practical experience at some point in the late 1950's and then rediscovered one or two earlier references, such as Sweet or Viëtor.

Apart from the German sources listed above, the idea of articulatory basis was still being discussed by prominent phoneticians in other European countries. For instance, Malmberg (1963) writes:

The term *articulatory basis* is often applied as a convenient, but not strictly scientific label for all the articulatory habits which characterize a language....One language has a predilection for front articulations....., another for back articulations...In several languages the lips play a great part and rounding of the lips is employed to distinguish one vocalic timbre from another. (71)

Malmberg's book, incidentally, was available in a French edition as early as 1954, and he devotes almost 2 pages to the subject, in the course of which he makes a brief but pointed comparison of the different settings for French and English. He also comments on the bad pronunciation which French and English speakers use when trying to speak each other's languages, precisely because their articulatory bases are so different.

Malmberg's comparison of English and French is very reminiscent of the writings of both Sweet and Dumville and suggests that the idea was so familiar outside Britain that it no longer needed to be attributed or justified. Honikman's paper, which appears to derive at least in part from experience in teaching French pronunciation to English speakers, may therefore be viewed as simply another contribution to an established tradition.

It is quite plausible that Honikman, as a speaker of French and a teacher of introductory courses in phonetics, would have been familiar with the French version of Malmberg's introductory book, and had derived her interest – and even some of her ideas – from there. But that gets us no farther in the search for ancestors: if Honikman was using Malmberg, then who was Malmberg referring to?!

The clue to this may lie indirectly in another publication of Malmberg's. In 1968 he edited the second (revised) edition of the *Manual of Phonetics*. An earlier edition, edited by Louise Kaiser (1957) had contained a number of papers on aspects of phonetics which were no longer fashionable, including an article on Slavic languages (Hála 1957) which made extended reference to their

‘Articulatory bases’. Although Malmberg removed all trace of this from his revised edition, the earlier version had been a standard phonetics reference book and it is likely that both he and Beatrice Honikman had used it. Honikman’s article refers briefly to the articulatory setting of Russian and, apart from the work by Zacher referred to above, there seem to be no other sources for this.

None of this is conclusive proof of an ancestor and much of it must remain speculative. It does suggest, however, that Beatrice Honikman – rather than producing a startlingly original paper – was simply continuing and developing a tradition which had survived explicitly elsewhere in the world, and particularly in German speaking countries.

The tradition which had been silenced in Britain by the preoccupations of Daniel Jones was to resurface in Edinburgh under the influence of David Abercrombie, who – from about 1958 onwards – became interested in general matters of voice quality and settings (cf. Laver 1991: 383). Its application has been greatly developed in recent years in the work of Trudgill (1974), who applied it to the description of non-standard varieties, and, above all, Laver (1980, 1991, 1994) who has developed a full descriptive framework for the classification of voice qualities.

Honikman’s paper contained little that was really new or original (apart from the term ‘setting’) but, by virtue of being published in a collection that was bound to achieve worldwide prominence, has served as a very useful catalyst for phoneticians who wished to go beyond the phoneme and escape from the confines of the Jonesian view of the world.

Indeed, it seems entirely possible – with the benefit of hindsight – that the Jones *Festschrift* was used by the editors, and the authors whom they commissioned, to mark the end of an era and break out in new directions.

Seen in this way, Honikman’s paper was part of a concerted move away from the phoneme-dominated approach and should be set alongside many other developments which started, or were revitalized, in the 1960’s. Catford (1964) (which also appeared in Abercrombie 1964), Laver 1968, and Abercrombie 1967 all contributed to the renewed interest in voice quality and settings, which culminated in Laver’s 1980 study. The pioneering work on parametric description, also encouraged by Abercrombie, culminated in Ladefoged’s definitive papers of 1972 and 1980 in this area.

All of this work came from scholars who had either taught or studied in Edinburgh. Much of it, however, was already implicit in the 19th century tradition initiated by Sweet and Sievers. We are therefore indebted to Beatrice Honikman, not so much for an original idea (which it was not) but for restoring it to respectability.

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Sexism in Language – A study on pronoun usage in generic contexts

Karin Prögler-Rössler

Over the last few decades feminists' claims relating to language have been discussed extensively. Feminist linguists have argued that language reflects as well as recreates sexism in our society. Accordingly, numerous proposals have been made to change the English language (as well as other European languages), and 'guidelines for non-sexist language usage' have been published by world-wide organizations like UNESCO (e.g. Hellinger / Bierbach 1993) as well as by individual textbook divisions, professional societies, publishers of newspapers and magazines, religious confessions, and other institutions (e.g. *Guidelines for Inclusive Language* 1981; Miller / Swift 1981). One major point of feminist criticism has been the use of *man* in reference to all people and the use of the pronouns *he/his/him* in generic contexts. It is argued that this usage equates the male sex with the norm of humanity whereas the female sex is presented as deviant from and inferior to this norm. Numerous studies on the understanding of 'generic' *he* have shown that people usually think of a male person when reading or hearing the pronouns *he*, *him*, or *his*, even if they appear in generic contexts. Harrison (1975), for example, was concerned with generic masculine terms in science textbooks, Eberhart (1976) tested elementary school pupils' understanding of generic words, MacKay / Fulkerson (1979) did a number of tests with university students including response-time measurement in their analysis. In addition to the finding that the use of the generic masculine tends to conjure up male imagery, Martyna (1980) concludes that females have difficulties in imagining themselves in generic roles when the pronoun *he* is used. Thus, women are 'alienated' as well as made 'invisible' through the use of the generic masculine.

Like most proposals for change these feminist efforts have not only met with approval. One major argument against changing sexist language has been the claim that not language itself is sexist but the people who use it. Thus, it has been argued that trying to change language is just a waste of time and effort. In opposition to this, feminist linguists hold that not only the usage but also the structure of English incorporates sexism. They argue that a language where one and the same word is used to refer to the male sex in particular and to humanity in general is "structurally sexist" (Miller 1982, 18) because it

equates the male sex with the norm of humanity. In other words, in the English language the two sexes are not treated equally: the male is 'privileged', while the female is discriminated against.

A similar argument against the proposed changes is the claim that language does not interrelate with society, so that changing sexism in language is useless. Questioning this, feminists have justified their concern with language with the assumption that language and society do not function independently but influence each other in a number of ways. A weak version of the Whorfian Hypothesis – that the language we use influences the way we perceive our environment – is widely accepted. Although opponents to the feminist movement deny the existence of such an influence with regard to sexism, even they generally acknowledge that language is one way of transmitting culture from one generation to the next. Conservatives like the philosopher Roger Scruton¹ hold language sacred as patrimony inheritance which must not be tampered with. But aren't the social values that are thus transmitted only as good as the society they emerge from? Even though they include highly valuable wisdom, they naturally also transmit a number of prejudices. While the wisdom has to be preserved and enlarged, what has turned out to be hampering prejudice has to be eliminated. Sexist attitudes which are reflected in the language we inherited from our parents certainly belong to the latter category. Many linguists argue that the influence of language on attitudes and social behaviour is especially strong with children. Since children have still got limited experience of the real world, they establish their value system mainly according to what they are told. Karsta Frank (1992) describes language as a social looking glass, through which children perceive the world around them. Among other things, Frank comprehends sexist language as a means to transfer sexist perception and thinking to future generations (145). From early childhood the structure and usage of our language teaches us that the male is the more important, the more 'general' sex, that men are the norm, women the 'others', distinguished from and by implication inferior to the norm. For those who want to keep women 'humble' and inferior, such a language might be a useful tool, but if we talk to our children about sexual equality, we run the risk of contradicting ourselves by a sexist usage of our language.

In my own study I have been concerned with measuring the success of feminists' efforts as regards the use of pronouns for generic reference. Having tested today's British pupils and students' pronoun usage in generic contexts as well as the mental imagery connected with this usage, I compared my results

¹ Scruton, Roger. 'How Newspeak Leaves Us Naked.' *The Times*, 2 Feb. 1983; quoted in Cameron 1985, 88.

with those of Wendy Martyna's similar study in the late 1970s. In keeping with Martyna's findings I had expected female informants to use more non-sexist *he* than male informants. I had also expected *he* to be used more often with nouns denoting a stereotypically male profession (e.g. *lawyer*) than with 'neutral' nouns such as *student*. Furthermore, it had been hypothesized that nouns such as *lawyer* would conjure up more male imagery and nouns like *typist* more female imagery than other nouns, and that because of the traditional usage of *he* as generic pronoun males would be more likely to perceive themselves in generic roles than females.

1. Method

The survey was done with fifth grade pupils (aged 14-15) of a Nottingham Comprehensive School and first year Science students (aged 18-19) at Nottingham Trent University. Altogether 121 pupils and students participated in the study. Since it was not possible for the observer to be present when the pupils did the test and filled in the questionnaire, the survey was carried out by the pupils' English teacher, who was carefully instructed on how to present it to the teenagers. At university I personally handed out the tests and questionnaires in the last ten minutes of an introductory lecture. The students were asked by their teacher whether they would like to stay on and participate in the study, they were, however, assured that this was not part of the course requirements and that they could leave if they wanted to. Thus the test and questionnaire were filled out anonymously and (at least for students) voluntarily. In order to prevent the informants from guessing the concern of the test they were told that pupils' and students' abilities in completing a number of sentences meaningfully in a stress situation were going to be compared. The time taken by the individual informants was measured.

As the aim of the survey was to elicit spontaneous pronoun usage as well as people's explanations of their own usage, the study consisted of two parts. A test of 30 separate sentences with gaps to be filled contained twelve investigation sentences where a generic pronoun had to be used (e.g. *When the teacher asked the question, not a single child raised _____ hand*). Four types of generic antecedents were distinguished:

(1)

- A: mixed-sex, distributive (*everybody, no one, everyone*);
- B: neutral (*student, child, cyclist*);
- C: predominantly female (*typist, receptionist*)²
- D: predominantly male (*lawyer, judge, banker*).

² The third sentence of this category had to be ruled out because of ambiguity.

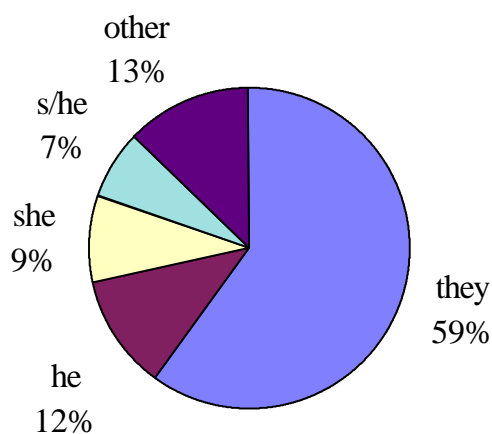
In two sentences a pronoun had to be chosen in accordance with a noun specified for number and sex in order to insure that the informants were capable of using personal pronouns correctly (e.g. *Every mother loves _____own child.*). The rest were filler sentences constructed to distract the reader's attention away from pronouns (e.g. *She is studying _____ a doctor.*). In those sentences where the pronoun was not a possessive, the verb directly following the pronoun was either a modal or also left to be filled in. (*Before a judge can give a final ruling, _____ must weigh the evidence. / No one steals food if _____ not hungry.*) The reason for this precaution was the assumption that third person singular -s in the verb directly following the pronoun would bar the occurrence of *they* and thus reduce the respondents' choice.

Following the completion of the test, the informants were asked to reflect back on their own pronoun usage in a questionnaire. From each group of antecedents one noun was chosen and for each the informants were asked to explain their pronoun choice and remember the images they had in mind when completing the test. They were also asked what other pronouns could be used with reference to the same noun and which form they would 'normally' use.

2. Results

Figure 1 below shows the pronoun usage in the test without distinguishing the individual noun groups. Pupils and students, males and females taken together, *they* reached a score of 59.7%, *he* was used in 11,7% of all answers, *she* in 9%, and double pronoun constructions (*he or she; he/she; she/he; she or he* in the diagram shortened to *s/he*) in 6,8% of all answers. 12.8% of the answers contained other or no pronouns.

Fig. 1: Pronoun Usage in General



It is interesting to note that traditional grammars establish the pronoun *he* to be the ‘best’ in all the sentences used in the test. In their *University Grammar of English*, Quirk / Greenbaum (1973), for example, do not discuss the generic masculine but in the *Workbook* they give “*himself*” as the only solution in the sentence “Everybody clings to this illusion about (everybody)” (1974, 37). Somewhat more up to date, Leech et al (1982) do mention the problem of the generic masculine. However, they do not find the sentence “Everyone can vote as *they* wish” acceptable in written English. Double pronoun constructions are considered “awkward”. As the only form which is grammatically correct and stylistically acceptable is “Everybody can vote as *he* wishes”, they advise the reader to avoid the problem by pluralization (178). The pie chart above shows that the percentage of actual usage of the ‘grammatically correct’ form *he* in the test is very low (12%). In contrast to this, nearly 60% of all pronouns used in the test are ‘incorrect’ and another 7% of all answers are ‘stylistically awkward’ according to the grammatical standards of traditionalists. Since all informants used correct pronouns in the two sentences where the sex of the antecedent was specified, there is no reason to doubt their linguistic competence.

Contradicting the hypotheses that females use more non-sexist alternatives to ‘generic’ *he* than males and students more than pupils, the differences of pronoun usage between the four groups of informants were minimal. Only a few differences could be observed, none of which is statistically significant. The nature of the antecedent, however, did play an important role in the informants’ pronoun choice.

Figure 2 below presents the overwhelming majority of *they* in a different light. In Group A (mixed-sex, distributive antecedent), B (neutral antecedent), and D (stereotypically male antecedent), *they* was used significantly more often than any other pronoun, reaching its highest score (86.5%) with A. Only with stereotypically female nouns (Group C) did the female pronoun *she* surpass *they*.

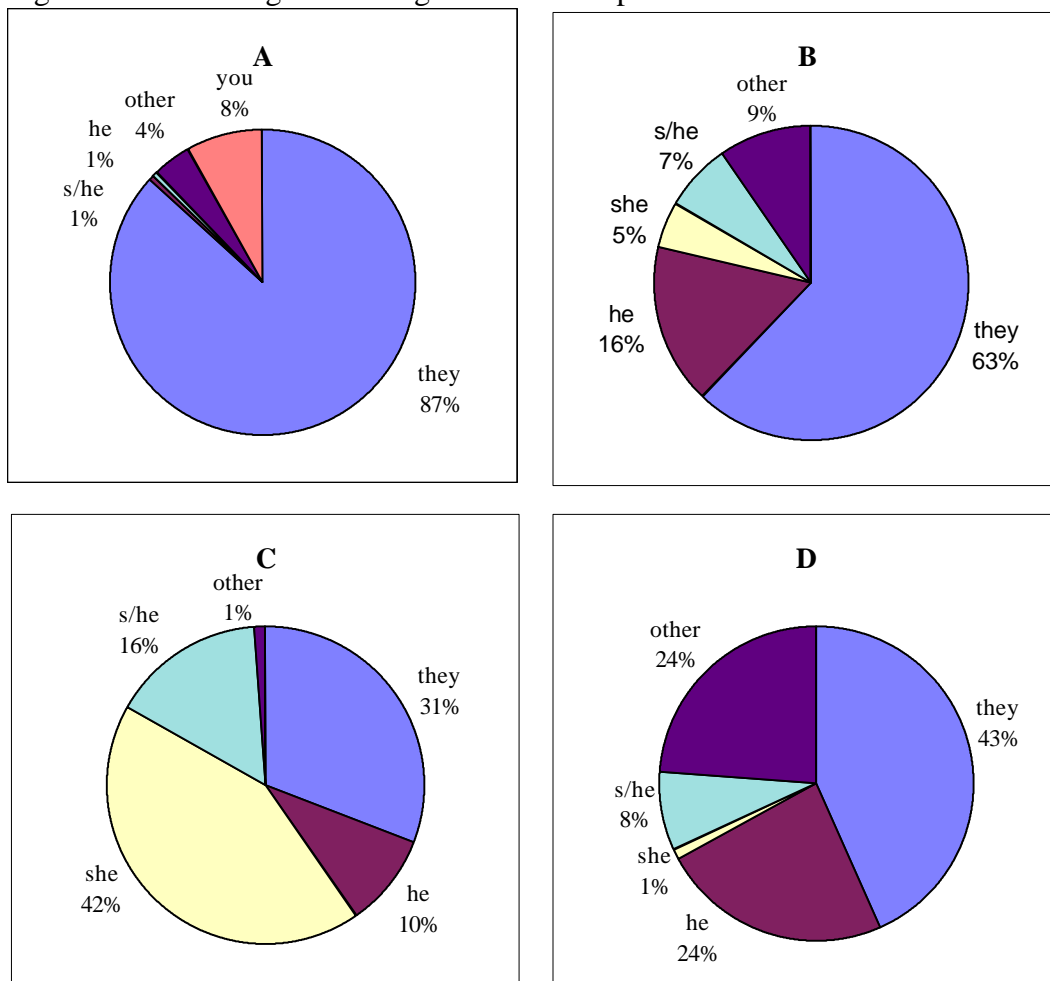
Compared to the high use of *they*, double pronoun constructions are not favoured by the informants. Even though these forms cannot be said to be grammatically incorrect and have thus been suggested in all guidelines, their use in this test was rather low. Only with stereotypically female nouns, double pronoun constructions were used in more than 10% of the answers. Thus, double pronoun constructions were used more often than *he* only in sentences containing ‘predominantly female’ nouns (16% vs. 10%).

The pronoun *he* does not occur in the test in more than one quarter of the answers in any noun group, reaching its highest score with stereotypically male nouns (24%). Even though this confirms the hypothesis that *he* is used most often with stereotypically male nouns, this figure is still surprisingly low com-

pared to the use of *they* in the same noun group (43%). Furthermore, it is interesting to see that with stereotypically male nouns the amount of answers without pronouns ('other') is relatively high.

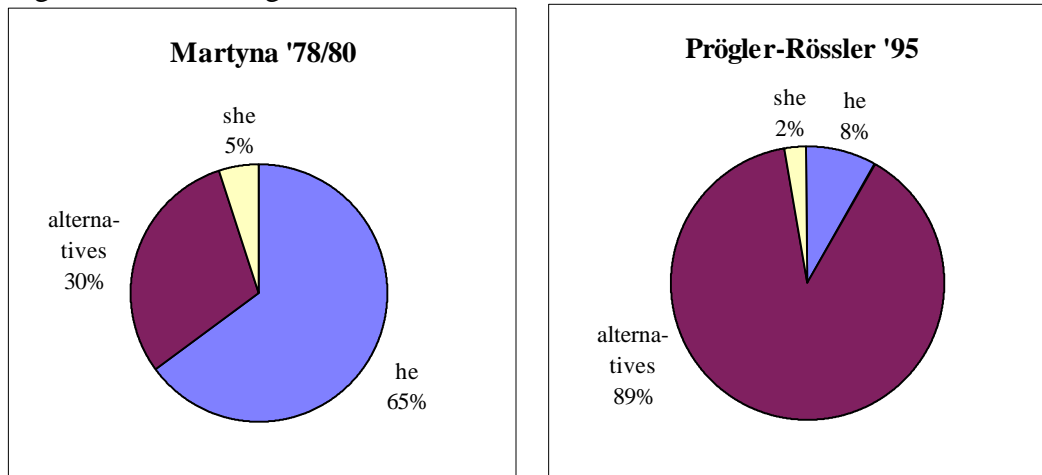
Except for sentences containing mixed-sex antecedents like *everybody*, *nobody*, etc. *she* was used in all noun groups. However, its use did not exceed 5% in Group B and 1% in Group D. As hypothesised, *she* was used most often with stereotypically female nouns (42.7%). Note that this percentage is a lot higher than that of *he* in relation to stereotypically male nouns (23.7%).

Fig. 2: Pronoun Usage according to Noun Groups

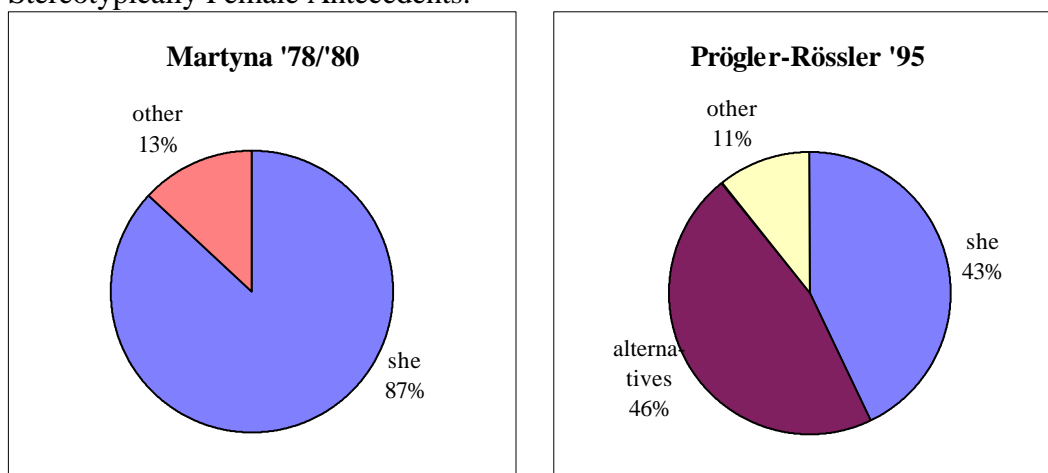


The significance of these results becomes clear when compared to the results of Martyna's study (reported in Martyna 1978 and 1980). Figure 3 demonstrates a shift from 'generic' *he* towards non-sexist alternatives.

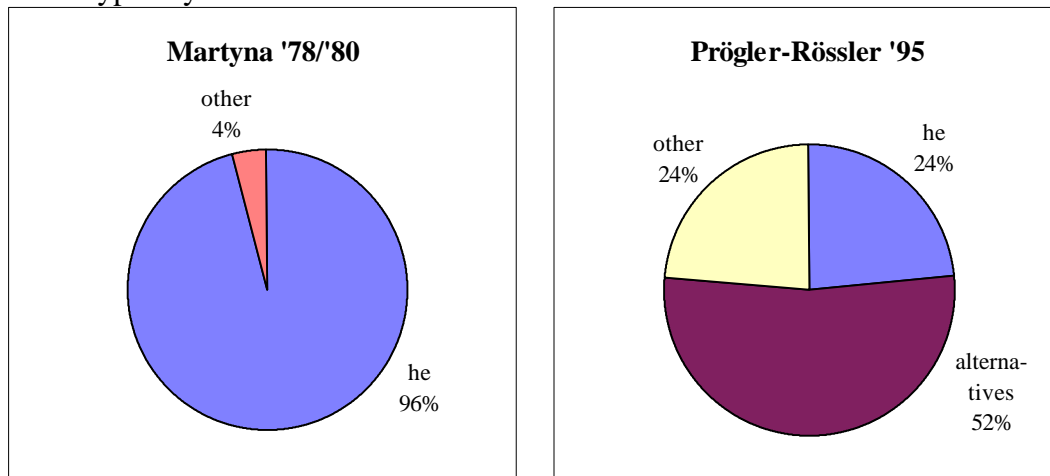
Fig. 3: Pronoun Usage with Neutral Antecedents; USA 1978/80 vs. GB 1995



Also with sex-related antecedents (groups C and D in my survey) alternatives were used significantly more often in my survey than in Martyna (1978/80). Figure 4 shows that the usage of these sex-specific pronouns has decreased in comparison to the results of 1978/80. Note that the difference is most obvious with the use of *he* (96% vs. 24%). The pronoun *she* with stereotypically female nouns was also used only half as often as in the older study. Thus there is a clear trend towards non-sexist language usage even with stereotypically female or stereotypically male nouns. The comparison of figures 3 and 4 shows that the trend towards non-sexist language usage, though significant in all noun groups, is strongest with neutral nouns.

Fig. 4: Pronoun Usage with Sex-Related Antecedents; USA 1978/80 vs. GB 1995
Stereotypically Female Antecedents:

Stereotypically Male Antecedents:



Concerning the imagery connected to generic roles, there is a similar shift in responses. Martyna (1978/80) found that males tended to use 'generic' *he* because they hold images of male persons or self-images in their minds, whereas females use it "for lack of a better word", because it is the only grammatically correct pronoun in generic contexts (136-37). Martyna concludes that the use of *he* as generic pronoun makes it difficult for women to see themselves in generic roles. My own findings are more complex and in many ways they form a clear contrast to Martyna's.

As my questionnaire did not exclusively ask for imagery but also for other reasons for the pronoun choice, only few respondents mentioned imagery at all. Furthermore, the multiple choice form also offered the possibility "*thinking of anything in particular*" which certainly was the easiest answer and was chosen by 44% of all informants. Despite this large number of neutral answers, some interesting aspects can be extracted from the rest, especially when the individual groups of informants, nouns and pronouns are compared. In contrast to Martyna's (1978/80) findings, female respondents in the present survey did not report less female imagery or self-imagery than males. To the contrary, female self-imagery was more often mentioned than male self-imagery (15% vs. 6%).

As expected the antecedents considerably influenced the informants' imagery. Sex-stereotypical terms conjured up more sex-specific imagery than 'neutral' terms, as sex-specific imagery was strongest with the stereotypically female term *typist* (49% female imagery) and practically absent with the distributive *everybody* (0% male/female imagery). As regards self-imagery, it is not surprising that the informants could imagine themselves most easily in the role of a *student* (23%) since all of them were either pupils or students. Simi-

larly, *everybody* triggered an expectedly high percentage of self-imagery (19%). The most interesting result with regard to self-imagery is the fact that none of the informants imagined themselves in the role of the *typist*, whereas 15% could see themselves as *lawyers*. The most plausible explanation for this striking contrast is the difference in the social prestige of these two professions, which is obviously still present. It seems logical that young people prefer to imagine themselves in future roles of high social prestige.

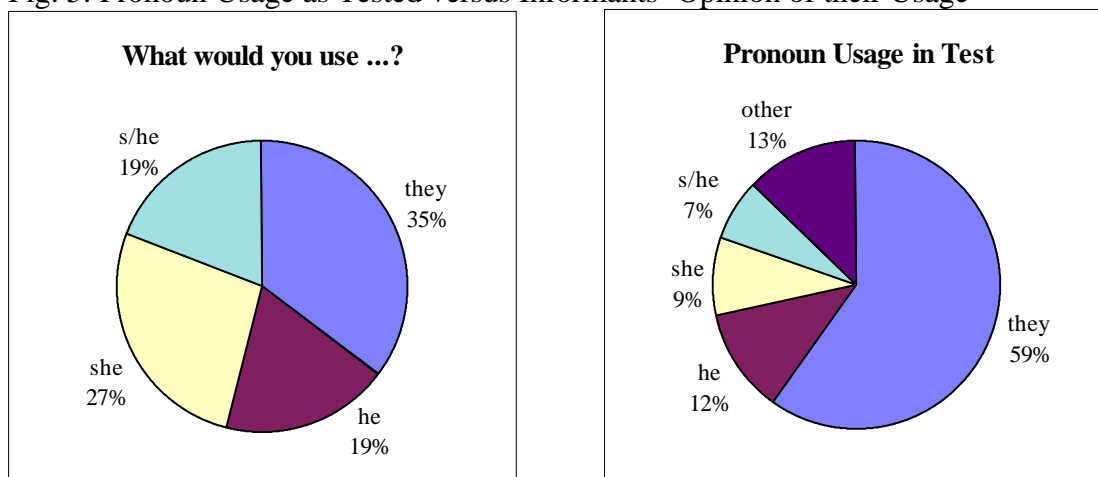
Reported imagery correlated most clearly with the informants' own pronoun choice. Especially sex-specific imagery was closely connected with the use of *he* and *she*. 88% of those informants who used the pronoun *he* in one of the generic test sentences, said that they had done so because they had been thinking of a man.³ 82% of those who used *she* stated that they had been thinking of a woman. The use of double pronoun constructions was related with female imagery in 26% of the cases. With the pronoun *they* sex-specific imagery occurred rarely (5-7%), most people (54%) stated that they had not been thinking of anything in particular. The comparison of the pronoun *they* used in singular, generic contexts with double pronoun constructions shows that the latter are more suitable to make women 'visible' in language. Furthermore, self-imagery was reported most often by those informants who used double pronoun constructions (19%). Even though double pronoun constructions seem to favour self-imagery, *they* obviously is the preferred alternative. The reason for this preference can be seen in the answer of one female pupil who stated that she used *they* because it is "nice to be remembered", others probably wanted to avoid the 'clumsiness' of double pronoun constructions.

The most common reason given for the use of non-sexist alternatives to 'generic' *he* was the wish to be *politically correct* or *not sexist*. Some of the answers are interesting in respect to the grammatical correctness of *they* with a singular, generic antecedent. 16% of those who had used *they* with the antecedent *everybody* stated that this was what they had learnt to use in school. Since *everybody* is at least semantically plural, this figure may not seem surprising. However, the same reason was given by 12% of those pupils and students who had used *they* with *student* as well as with *typist* and even by 6% of those who had used it with *lawyer*. However, the comparison of the respondents' actual pronoun usage in the test with their answers to the question "What would you normally use to refer to 'everybody' / to 'student' / to 'typist' / to 'lawyer'?" suggests that the respondents were not so sure about the grammatical correct-

³ Interestingly, male imagery was the only reason given for the use of *he*. No one commented on the grammatical correctness of the form and no one ticked 'because this is what we learnt to use at school'.

ness of ‘singular’ *they* after all. The pie charts in Figure 5 show striking differences.

Fig. 5: Pronoun Usage as Tested versus Informants’ Opinion of their Usage



In the questionnaire, only 35% of the informants claimed that they would ‘normally’ use *they*. In the test, however, *they* was used in 60% of all investigation sentences. In the informants’ opinion of their own pronoun usage, ‘sex-exclusive’ pronouns occur more often than they actually appeared in the test. Double pronoun constructions also scored higher in the informants’ opinion than in their usage as it had been tested. Generally, from the usage as tested to the opinion given in the questionnaire, a great decrease of *they* in favour of all other forms can be noticed. Obviously, the respondents thought that they had used *they* more often in the test than they would do normally. There are several possible reasons for this discrepancy. On the one hand, it seems possible that the informants guessed the aim of the survey and wanted to please the observer by using non-sexist forms in the test. On the other hand, they did not use double pronoun constructions more often than they thought they normally would, to the contrary, they used them less. Why did they think they would please the observer by using *they* but not by using double pronoun constructions? Since no logical answer can be found to this question, it is necessary to consider other possible reasons for the difference shown in Figure 5. Not only does the pronoun usage as tested have to be questioned but also the informants’ statements in the questionnaire. Thus it is also possible that the informants used *they* more often than they thought they did or than they wanted to admit. The fact that this alternative is still not widely accepted by grammarians may have played an important role here.

3. Discussion

The analysis of the questionnaire has shown that the overwhelming use of 'singular *they*' in the test as well as the use of double pronoun constructions were motivated mainly by the notion of political correctness. The concept of 'politically correct' and 'non-sexist' language has come into discussion and has been gaining importance only within the last few decades. This appears to be the most likely reason for the differences of pronoun usage in this survey compared to the survey of Wendy Martyna (1978/80) as shown in Figure 3.

The fact that the pronoun *she* was not used more often in 1995 than in 1978/80 can be interpreted in different ways. Martyna (1978/80) bemoaned this small percentage of *she* because her findings led her to interpret it as an indication that females had difficulties in seeing themselves in generic roles. In contrast to this, the present survey could not find any significant difference in the imagery reported by female vs. male informants, so that there is no longer any indication of a disadvantage of women and girls as far as imagery is concerned; a fact which seems to contradict Martyna's conclusion. However, the difference of the results of the two studies in the field of imagery clearly shows a relation with the difference in pronoun usage. The strong correlation of the pronoun *he* with male imagery and the pronoun *she* with female imagery which was found in the present study confirmed Martyna's results. Thus the high usage of *he* in Martyna's study explains the dominance of male imagery reported by her informants. It seems that the change of pronoun usage in generic sentences has also brought about a change in the imagery connected with generic roles. This also means that the disadvantage of women and girls as far as imagery is concerned, which had been found in 1979, no longer exists. The fact that this was achieved mainly by a higher usage of the sex-indefinite pronouns *they / their / them* in singular generic contexts and not by a higher usage of female pronouns shows that it is not necessary to use 'generic' *she* in order to produce female imagery in equal numbers with male imagery.

The fact that the use of non-sexist alternatives is still comparatively low with antecedents describing stereotypically female/male professions confirms the assumption that the awareness of sexism in sex-stereotypes is still rather low. This is to say that choosing a female pronoun for a stereotypically female antecedent is not experienced by the subjects as a sexist choice, even though it reinforces sex-stereotypes most women want to get rid of. Furthermore, the choice of sex-specific pronouns for generic roles is induced by sex-related imagery. These results suggest that the change documented in Figure 4 is not due to a change in the imagery conjured up by stereotypically female/male nouns but rather to a growing habit of using non-sexist alternatives. As with neutral

antecedents, the most common reason for choosing 'sex-inclusive' pronoun forms was the wish not to be sexist/to be politically correct. The change in pronoun usage seems to have started out with neutral nouns and to be expanding on to nouns which name stereotypically (fe)male professions. The pronoun *they* which is already used very often with neutral generic nouns, seems to be finding its way into sex-related clichés.

The equal pronoun usage of females and males in this study can be explained in two possible ways. Firstly, it has to be noted that both sexes used non-sexist alternatives overwhelmingly often, which means that it is mainly the male score which is surprising. Treating women and men equally does no longer seem to be a concern of females alone, at least in this young generation of speakers. Maybe young males want to show their old-fashioned elders that they are going with the time. Secondly, this study dealt with people in whose public lives no obvious discrimination of females can be found. Both, grammar school and university are attended by roughly equal numbers of female and male students. Furthermore, the rate of female teachers in the 'new' universities is already comparatively high. This means that female pupils and students are not (yet) in a situation where they experience discrimination of women. On the contrary, since in school girls are frequently considered to be the 'better' pupils, they hold a good position and are often even envied by their male colleagues. In their free time young people tend to be among themselves again. In their cliques, they are all equal and most of the time they are concerned with getting admiration from the members of the opposite sex rather than fighting against them. In that situation, young people may well believe that sexism is a thing of the past. Young women may not see any need to fight for their rights any more. Thus, the concern both sexes put on non-sexist language may simply be to get rid of something which is long outdated and which, among themselves, does not exist any more.

Unfortunately, the restriction of this survey to young people does not in itself allow for predictions concerning pronoun usage in the future. This study does not give insight into possible changes within the language of one and the same person, as they grow older. Thus, whereas discrimination of the female sex may not be a relevant subject for teenagers, it may well become important later on. When young people start to work, more often than not they are still confronted with sexist structures and attitudes. Therefore it is difficult to say whether these young people who contributed to the survey will keep their non-sexist attitude and language usage. It may well be that in ten years' time, when these young women will have found out that the 'real world' is not all that good yet, their concern about sexism will surpass that of their male colleagues.

It may also be that young men will to some extent adapt to their new environment and to their older male colleagues who still hold more sexist attitudes. In addition, they may lose their interest in sexual equality in their new environment. For these reasons it could well be that the pronoun usage of the same women and men when tested in ten years' time would be quite different. In order to find out whether there really would be such a change and what exactly would be the results, it would be very interesting to follow one group of informants from university into the work place and test them again after a few years of work (and/or family) experience.

In summary, it can be stated that feminists' efforts to change sexism in language has already borne fruit. Young people nowadays seem to accept that it is important to treat women and men equally, in language as well as in society. The present survey, especially in comparison to Martyna (1978/80), shows a clear trend towards non-sexist language. The speed with which this change seems to be taking place confirms feminist language policies and contradicts those who argue that language change cannot be induced. Nevertheless, the relation between guidelines for non-sexist language usage and the change in pronoun usage is not as simple as might be expected. Especially the high usage of 'singular *they*' is surprising insofar as guidelines for non-sexist usage are still rather careful about suggesting this grammatically 'incorrect' form. Some guides do not or only marginally mention this (popular) solution. Instead they list several possibilities of avoiding pronouns and using double pronoun constructions. Since double pronoun constructions did not appear to be very popular among the informants of this survey, a direct relation of the students' non-sexist pronoun usage with 'guidelines for non-sexist language usage' is doubtful. However, the two factors are related indirectly through the growing popularity of the subject of non-sexist and politically correct language. On the one hand, guidelines are a result of this popularity and, on the other, they certainly increase it. Whether or not the pupils and students tested in this survey have come into contact with guidelines for non-sexist pronoun usage, the ideas have reached them, be it through the media, or through their teachers. It is the popularity of these ideas which has influenced their usage. Naturally, the suggestions of guidelines are not put into practice one by one. Rather, people pick out what seems to be useful and practicable. One of my informants stated that she used *they* "because it's easy to remember". This statement indicates that extensive guidelines with numerous suggestions can be quite confusing and impractical in usage. We have to accept that language users are generally 'lazy'. This means that they do not want to think about different alternatives every time they would have used 'generic *he*'. The search for a 'new' singular, generic pronoun, which started as early as the eighteenth century (see Baron

1981) and is still going on, shows that there is a need for one simple word which can be used in all generic sentences. As the usage of ‘singular *they*’ with reference to singular generic nouns is increasing, this form seems to be fit to fill this gap. The fact that the verb directly following the personal pronoun *they* does not take third person singular *-s* is no hindrance for its usage. Even though *they* does not seem to be used as a singular pronoun grammatically (*No one steals food if they are not hungry.*), it can very well function as semantically singular as the following example shows:

A pair of jeans has been left in the tumble drier. *The owner* is asked to call at the office, where they will be given back to *them*. (Note put up at the launderette at Clifton Campus, Nottingham Trent University, in June 1995)

One possible reason why double pronoun constructions are disfavoured in comparison to *they* is that they are not suited for all generic sentences. Thus, ‘singular *they*’ certainly is the most easy and elegant solution to the problem of generic pronouns.

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Comments on **Sexism in language – A study of pronoun usage in generic contexts**

Henry G. Widdowson

Linguists are sometimes inclined to speculate without adequate reference to the evidence of empirical data, and so this paper by Karin Prögler-Rössler is to be welcomed. Here is a well designed study on a topic of major sociolinguistic concern which elicits data, displays them clearly, and then draws attention to their significance. The explicitly stated results and discussion are of interest in themselves, but even more so (for me at least) are the implicit assumptions behind them, and the way they provoke reflection on issues concerning this kind of enquiry in general. It is this aspect of the paper that I should like to comment on.

There are two questions that are always problematic about language data: where do you get them from, and what are they evidence of. You can get data from three kinds of source: intuition, elicitation, observation. The last of these is currently much in vogue. The electronic processing of vast corpora of actually occurring language reveals actual facts of usage, and these are then usually adduced to discredit intuition. What people think about the language they use turns out to be wrong because it does not match up with their actual behaviour. If you want real language data, consult a concordance. This sounds convincing on the face of it. You can certainly count on the computer to provide you with reliable observational data. But it does not invalidate intuition because it is not the same kind of data. There is a good deal about a language which its users know but which does not show up in behaviour, and simply is not observable. One obvious example is intertextual knowledge. In using language on a particular occasion, I will assume familiarity with other usages, and so produce elliptical expressions, leaving my interlocuter to fill in the blanks. Thus I might produce something like “Many’s the slip...” or “Spare the rod...”, assuming

that these will be referred to the full forms in the recipient's mind ("twixt cup and lip", "Spare the rod, and spoil the child") (cf. Aston 1995). The point is that we need to make allowance for a knowledge of language (as of other things) that we do not necessarily act upon, but which we assume to be recoverable when relevant. Observation does not invalidate intuition: it simply yields a different kind of data.

And the same is true of elicitation. Here subjects are prompted to provide data within the controlled conditions of a contrived context. In such circumstances, their attention is focused and their responses likely to be, in part at least, a function of introspection not generally present in unprompted behaviour. Labov's observations about the style variation under different elicitation conditions are obviously relevant here (Labov 1972). In reference to Prögler-Rössler's paper, it is difficult to accept, therefore, that her survey elicited "spontaneous usage". One cannot conclude that what her subjects were prompted to produce was necessarily what they would spontaneously produce in naturally occurring usage. This does not invalidate her findings as such, but it raises the question of what they are evidence of. They certainly show that her subjects are aware of the shift to political correctness in the use of non-sexist pronouns, and what acceptable behaviour should be. But this is consistent with the Labovian style shift towards what are conceived of as correct norms of behaviour. What they cannot show is what these subjects' actual usage is. To find that out you need to turn to observation as a source of data.

I would suggest, then, that the data elicited here do not serve as reliable evidence for usage. But are they evidence of any underlying change in attitude, a shift in imagery? Again, I think, not necessarily. Take the case of the so-called female and male nouns specified in the paper. We should note, to begin with that these are not morphologically marked for sex, unlike **postman**, **charwoman**, **poetess** and the like, and would not be sexually differentiated in their denotation. So they are not semantically male and female nouns as such. But the point at issue is that they are customarily differentiated in pragmatic use without this semantic warrant. The question is whether this referential convention reflects the way the referents are conceived, and, in the case of the present paper, whether an elicited variation reflects a change in conceptualisation. It is important to note that it is the generic use of pronouns that is being elicited here, so what the subjects are being asked is not what pronoun they would use in reference to particular persons but in reference to a role, an abstract classification. Typists (in the USA and UK at any rate) are in general women, and judges in general men. Since generic reference deals with generalities, one might argue that the normal and unmarked reference for typist would be **she**,

and for judge **he**, and this pragmatic convenience carries no necessary conceptual significance, and would allow for a pronominal shift for particular referents without ambivalence. The case is different with the 'neutral' nouns like **student** or **child**, because, as a matter of fact, their referents are of both sexes, so whichever pronoun is used for generic reference is bound to be marked. So the question arises as to what would motivate subjects to go against the normal pragmatic grain and choose a marked or counterfactual mode of reference by referring generically to the role of judge, say, as **she** or **they**? It is possible that in using these terms they do actually cast the role in a neutral or female image in their minds, but then we would need some evidence other than the language to demonstrate this. For it could also be that subjects are conforming to what they think is expected of them, and fitting their response to what they take to be the requirement of the study. Or it could be that they are being perverse, making reference to the world as they would like it to be, rather than as it is, in order to make a point about the kind of sexist society we live in. At all events, a conclusion about imagery cannot be read off from the pronouns that were elicited. We need evidence beyond these linguistic data.

But we do need these data too. The merit of this paper is that it demonstrates the value of elicitation as a source, and sets us thinking about how we can use data as evidence in sociolinguistic enquiry of this kind.

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Two Remarks on H.G. Widdowson's comments

Karin Prögler-Rössler

ad spontaneous pronoun usage

As H.G. Widdowson appropriately criticises, the term 'spontaneous' is misleading in reference to pronoun usage in my study. From this kind of data one cannot draw any conclusions concerning "naturally occurring usage". Never-

theless, it is important to keep in mind that the informants were not told that sexism was under investigation. Thus, if the high use of non-sexist alternatives to 'generic' *he* in the test was prompted by the informants' awareness of sexism in language, this awareness was not raised by the test but already present in the pupils and students' minds. It is therefore legitimate to conclude that the same awareness will produce similar pronoun usage on other occasions of the same degree of formality.

ad imagery

H.G. Widdowson rightly notes that no conclusion about imagery can be read off from the pronouns used in the test. It is probably not clear from my article that this is not what I was doing. Conclusions about imagery were made on the sole basis of the informants' answers to the questionnaire. The results were *then* correlated with the respondents' pronoun usage in the test as well as with other factors. There is, however, a general problem in the investigation of imagery, namely that none of us is trained in the art of mind-reading. So if we want to know what our informants were thinking of when *using* a certain pronoun, we have to ask them (*Was there any particular image in your head?*) and confide in what they tell us. Unreliable as this kind of data might be, it is the best we can get.¹ Since Martyna (1978/80) used the same kind of questionnaire to elicit imagery in relation with the use of generic pronouns, a change in this field can reasonably be assumed.

The fact that the imagery reported in my questionnaire correlated strongly with the pronoun usage in the test suggests that the two factors are related. There are three possible forms such a relation might take: either a change in the usage of generic pronouns causes a change in imagery, or imagery is responsible for pronoun usage, or a third factor, say the awareness about sexism (in society and in language) or actual changes in society, influences both. Pupils and students' answers to my questionnaire suggest that these three ways of interaction combine. Whereas the first can be assumed on the basis of previous studies on the understanding of generic pronouns (mentioned in my article), the second seems to have been an important factor with the sex-stereotypical nouns *typist* and *lawyer* in my study. In addition, statements like "*There are many female lawyers nowadays*", which was given as an answer to the question *Why did you chose this word? (she)*, hint at a possible influence of changes which are going on in society.

¹ Note the difference from studies that tested the understanding of *given* pronouns (e.g. Harrison 1975, Eberhart 1976, MacKay / Fulkerson 1979, and Martyna 1980 – see article for references), where different measures offer themselves.

‘To London fro Kent / Sunt predia depopulantes’: Code-switching and medieval English macaronic poems

Herbert Schendl

0. Introduction

In the first part of this paper published in VIEWS 5 (Schendl 1996) it was argued that the linguistic study of the numerous mixed language texts from earlier periods “should be of major interest for historical linguistics [...] as written testimony of early English bilingualism and language contact and [...] could also add the still lacking diachronic dimension to modern studies of code-switching” (50). The main aim of the first part was to illustrate the great variety of such mixed texts with regard to text type and genre, but also to give a first illustration of the different syntactic types and functions of code-switching (CS) in these texts.

This part will concentrate on one particular genre, namely **mixed or ‘macaronic’ poems** (for a discussion of this literary genre and its supposed origin and history see Delepierre 1852; Morgan 1872; Lazzarini 1982; for an account of English macaronic poetry see Wehrle 1933). The approximately 80 poems investigated for the present study date mainly from the 13th to the 15th centuries,¹ – a period in which Latin was still widely used as a written and spoken High language; longer verse pieces, such as *Piers Plowman*, have not been included in the present study, though they will have to be investigated for a comprehensive study of CS in poetic texts (for Latin insertions in *Piers Plowman* cf. Sullivan 1932; Alford 1992; Machan 1994).

Modern studies of bilingual discourse have shown that a large variety of extralinguistic factors can motivate or trigger switches. CS in poetry may follow at least partly specific poetic conventions, which may differ in a number of ways from the strategies used in other contemporary written texts, and even

¹ The term ‘poem’ as used here will include carols and hymns. - After the 16th century, macaronic poems seem to have become more artificial; this is especially reflected in the increasing (often exclusive) use of intraword switches, i.e. Latin endings are added to English word stems. This stage seems to correlate with the increasing disappearance of Latin as a ‘living’ language of literature and scholarship and its growing restriction to a small number of written functions, respectively to a school language.

more so from those followed in speech – though we evidently do not have any early spoken data of CS. Switching in poetry is most likely a deliberate and conscious choice of the author, while CS in spontaneous conversation is often an unconscious process; furthermore, spontaneous switching seems to be more frequent in informal situations, while medieval poetry can only rarely be classified as informal, though it may be less formal than prose writings. Finally, poetic CS is no less evidence of insufficient language competence than other forms of CS are.

A clear majority of the poems in the present corpus switch between English and Latin, normally with a quantitative predominance of the English material over the Latin, though there are also pieces where the two languages are fairly balanced. Only few poems show French/English or French/Latin switching, or use the three languages English, French and Latin.

Let me begin with a look at the structural patterns of switching, before some functional aspects of poetic CS will be discussed in chapter 2.

1. Structural patterns

In a discussion of structural patterns of switching and switch boundaries, one has to distinguish between metrical aspects and syntactic ones, though the two are certainly interdependent and cannot always be neatly separated. The corpus analysis has so far yielded the following general tendencies:

(i) In the majority of poems, there is a clear correlation between metrical factors and switch sites; i.e., switches frequently coincide with verse lines and half-lines; as a result, poems tend to show more regular switch patterns than written prose or speech (cf. examples (1) to (4)). However, there are a number of poems where switches occur irregularly, i.e., where the switch points do not coincide with metrical units, cf. (7), (23), (25).

(ii) In spite of this metrical influence, the syntactic patterns seem to be quite similar to those found in studies of modern bilingual speech; i.e., switches tend to be more frequent at major constituent boundaries, such as NP, VP, PP, though switches within these constituents do occur. Thus it seems justified to assume that metrical considerations would not regularly override possible syntactic constraints, even though individual cases of ‘poetic licence’ may occur.

(iii) There is only a very small number of cases in which switching results in ungrammatical constructions.

(iv) Intrasentential switches are clearly more frequent than intersentential and tag-like switches.

1.1. Languages and metrical patterns

Examples (1) to (4) illustrate both the combination of languages used and the correlation between metrical patterns and switching sites. Neither here nor in the following discussion will the material be classified according to date or languages used, though these parameters have to be taken into account in any detailed study of CS.

The first instances of systematic switching between Latin and the vernacular date from the Old English period, e.g. in the last lines of the *Phoenix* and in the so-called *Macaronic poem*. A few lines of the latter are quoted under (1). The 31 lines of this poem show a regular change between the Old English half-line and the Latin one and are connected by alliteration, which supports the smooth integration of the two languages.

- (1) *The OE macaronic poem* (Corpus Christi College, Cambridge, 201)²

Geunne **þ** on life *auctor pacis*
 sibbe geslā, *salus mundi*,
 metod se mæa *magna uirtute*,
 [May he grant you in life the giver of peace
 peace and happiness, the saviour of the world,
 the famous lord with great fame,]

The highly artistic mixing of three languages in the 14th century poem *On the times* has already been briefly discussed in the first part (Schendl 1996: 2.2.3): the changing distribution of the languages per half-line and their connection by (internal and end) rhyme help to “establish a harmonious integration of the three disparate languages” (Schendl 1996: 60). (There are, however, also a number of switches within individual half lines in this poem, i.e., switch point and half line do not always coincide.)

- (2) *On the times* (BM Royal 12 C xii, ‘R’, ¹14th c.)

Quant houme deit parleir, *videat que verba loquatur*;
Sen covent aver, *ne stulcior inveniatur*.
Quando quis loquitur, bote resoun reste **þ**rynne,
Derisum patitur, and lutel so shall he wynne.
En seynt’ eglise *sunt multi sepe priores*;
 Summe beoþwyse, *multi sunt inferiores*.

² Switches are indicated by italics and - in the case of multilingual poems - by bold type. Translations of Latin and French passages are given in square brackets and are underlined, respectively in bold type. ME passages have only been translated if thought to offer some difficulty to the modern reader.

[**When a man has to speak**, let him take heed what words he utters;
It is necessary to have understanding, lest one be considered more foolish.
When anyone speaks, unless there be reason in it,
He meets with scorn, and so he shall gain little.
In holy church many are often superior in position;
Some are wise, many are inferior.]

Example (3) provides a late 13th/early 14th century instance of French-English switching, where one language is maintained over two lines. While the language pattern is FF - EE - FF - EE, the basic rhyme scheme is *abab*, i.e., the rhymes help to closer connect and integrate the two languages.

- (3) *On the King's breaking of the Magna Charta* (Cambridge, St.John's College 112, e. 13th c.)

*Nostre roy de Engleterre,
Par le conseil de sa gent,
Wolde a nywe laghe arere,
And makede a muchel parlement.
Tuz y vindrent, les evekes
E le baruns ensement,
And alle iswore þat þr were,
And hulde taperes ytent.
[Our king of England,
On the advice of his people,
Would set up a new law,
And summoned a great parliament.
All came there, the bishops
And the barons likewise,
And all who were there took an oath,
And held lighted tapers.]*

The poem under (4) provides an example of French-Latin switching. The first four lines of the five-line stanza form a quatrain, and switches occur after every half-line. The fifth line is completely in Latin. The alternation between stressed and unstressed syllables, however, “frequently does violence to the normal accentuation of French words in the whole poem” (Aspin 1953: 108).

- (4) *Against the king's taxes* (BM Harley 2253, ¹14th c.)

*Une chose est countre foy, unde gens gravatur,
Que la meyté ne vient al roy in regno quod levatur.
Pur ce qu'il n'ad tot l'enter prout sibi datur,
Le pueple doit le plus doner et sic sincopatur.
Nam que taxantur regi non omnia dantur.*

[One thing is dishonest, whereby the people are oppressed,
That not half (the tribute) reaches the king which is raised in the kingdom.
Because he does not receive the tax completely just as it is given to him,
The people must pay the more, and thus they are cut short.
 For all that is levied is not given to the king.]

The above overview of metrical patterns in bi- and multilingual poems has emphasized the frequent correlation between switch points and metrical patterns. The often highly artistic switching patterns should, however, not distract from the fact that syntactic factors such as constituent structure are also of major importance for the placement of switch points. Let us take a closer look at these syntactic factors in the following chapter.

1.2. Syntactic patterns

The establishment of possible syntactic constraints on CS as well as the relative frequencies of switching sites and switched constituents have been a major research topic for many years (cf. Timm 1975, Pfaff 1979, Poplack 1980, Berk-Seligson 1986, Clyne 1987, Jake 1994, Mahootian 1996, etc.; for a recent survey see Muysken 1995). According to the syntactic nature of the switched units, three types of switching have generally been distinguished, though their definitions are not always consistent: (i) intersentential switches, (ii) tag-switches, (iii) intrasentential switches. Intersentential switches will be defined here as switches between sentences or independent clauses, intrasentential switches as those between or within the constituents of a sentence, including dependent ('embedded') clauses (cf. Myers-Scotton 1993: 4; Romaine 1995: 122f.). Only general trends will be discussed here, since a detailed statistical analysis and the establishment of a frequency hierarchy of switched constituents will form the topic of another paper.

1.2.1. Intersentential switches

Intersentential switches involve sentences and independent clauses, i.e., syntactically rather independent units, so that their integration into the text does not present any particular difficulties for the bilingual speaker. They occur both in the main text of poems and – fairly frequently – as title, burden or refrain.

The three independent clauses in (5) (French – English – Latin) are unconnected, while the two clauses in (6) a. and b. are connected by a coordinator; this coordinator is in the language of the second clause in (6a), but in the language of the preceding clause in (6b):

- (5) **Soyez permanent et leal!** / Loue me so þt I it fele, / *Requiro*. [**Be constant and faithful!** / Love me so that I feel it, / I ask (you)] (*De amico*, Cambridge, Univ. Gg. IV.27, ¹15th c.)
- (6) a. In erthe be peas to man also / *Et gaudium sit angelis*. [And joy be among the angels.] (J. Ryman, *Now the Most High*, Cambridge, Univ. Lib. Ee. 1.12, e. 15th c.)
- b. *Homo proponit*, / oftymes in veyn, / But *deus disponit*, / the boke telleth pleyn. [Man plans...But God decides] (*The Battle of Barnet*, Trinity College, Cambridge MS 601, 15th c.)

1.2.2. Tag-like switches

The absence of tags and tag-switches in the corpus could be due to their rather informal nature, which may not be compatible with the poetic form or genre. However, there is a small number of interjections, all of them French phrases in an otherwise English utterance, cf. under (7); for possible functions of such switches cf. 2.1. and 2.3.

- (7) Sire Emer de Valence, gentil knight and free, / Habbeth y-suore oht that, *par la grace Déee!* / He wollith ous delyveren of that false contree, yef hii conne. (*Execution of Simon Fraser*, BM Harley 2253, ¹14th c.)

1.2.3. Intrasentential switches

Intrasentential switches constitute the largest number of switches in the corpus (for statistical information on their distribution in some modern data see Pütz 1994: 242). Since they involve the matching of the syntactic rules of two or more languages, they are linguistically more complex than the other two types – though it is controversial whether they are typical of or restricted to fluent bilinguals (cf. Poplack 1980: 581; Pütz 1994: 279ff.). In the present corpus, they mainly occur between the major sentence constituents, such as NP, VP, PP, but sometimes also within these constituents, i.e., between minimal constituents – a tendency which corresponds to results from modern data. In the following, a few general tendencies in intrasentential switching will be illustrated and briefly discussed. The corpus analysis has yielded clear differences in the frequencies of the switched constituents, respectively particular switch points; some of these seem to mirror syntactic constraints established and extensively discussed for living languages (cf. 1.2. above). However, syntactic constraints are increasingly understood as probabilistic rather than categorical (cf. Muysken 1995) so that frequency hierarchies of switched constituents in

older texts may yield results comparable to modern studies. – The following discussion will be descriptive and surface-oriented.

1.2.3.1. **Noun phrase:** The present data does not provide any instances of switching between a pronominal subject or object and the verb. This corresponds to the strongest of the five syntactic constraints on switching points given by Timm for modern data (1975: 477-480), though there are counterexamples even of this constraint in the secondary literature (cf. Sankoff & Poplack 1981). For Latin as a pro drop language, the absence of a switched pronominal subject is not surprising, though Stolt (1964: 279) gives an example from Old High German - Latin mixed texts of the type *ih consentiebam*. A point worth mentioning is the very small number of unambiguous single noun switches attested in the corpus; this differs both from their predominance in ME and EModE business texts (Wright 1992, 1994) and in living languages (cf. e.g., Poplack 1980: 603; Pütz 1994: ch. 8). It must be pointed out, however, that a clear differentiation between borrowing and single noun switches is often almost impossible for Middle English texts, which show an enormous number of French and Latin borrowings, many of which are not integrated phonologically and/or morphologically.³ - In the present corpus complex subject or object NPs are more frequent than single noun switches:

- (8) *Angelus consilij* / Was borne of þ̄ blessyd ladye, [The angel of council] (Oxford, Ashmole 189, 15th c.)

Postmodification: There are a number of switches between an English noun and a postmodifying Latin element; the postmodifier can be a genitive, as in (9), a non-finite clause, or a finite relative clause as in (10):

- (9) *Þ̄ster nyth, and comth þ̄ day* / *Salutis*; [Dark night, and comes the day Of salvation] (*Hymn to Mary*, BM Egerton 613, m. 13th c.)
- (10) *Fro the fynd he vs schyld,* / *Qui creauit omnia*. [Who created everything] (BM Sloane 2593, 15th c.)

1.2.3.2. **Verb phrase:** In a few cases the whole VP is switched as in example (11), where an explicit English subject NP is found in the preceding clause:

³ The distinction between switching and borrowing has been extensively discussed in the secondary literature on CS, and a number of different criteria have been proposed, none of which seems unproblematic for corpus languages; see Myers-Scotton (1992, 1993: 163-207); Romaine (1995: 142-161); for a survey of recent research and different approaches see Pütz (1994: 209-220).

- (11) The childe fellyd alle the fendys pride, [...] and with the blode of his dere syde *Soluit a pena miseris*. [Freed the miserable from punishment] (Oxford, Bodl. Lib. Arch. Selden B. 26, 15th c.)

In a few cases the subject slot is not overtly filled, since Latin as a pro drop language does normally not provide an explicit subject personal pronoun, cf. under (12):

- (12) *Exortum est* in loue and lysse: [(He) came into existence] (Oxford, Bodl. Lib. Arch. Selden B. 26, 15th c.)

Equally rare is switching of a single finite verb, cf. the Latin examples under (13) and (14); in (13) both the preceding subject NP and the following adverbials are in English, in (14) the verb is preceded by a French subject NP and two English adverbial phrases:

- (13) *Aungellys exaltant*, bothe lowde and hih, (*Coronation of the Virgin II*, BM Harley 2255, 15th c.)
- (14) *Ma tresduce et tresamé* / Night and day for loue of þe / *Suspiro!* [My most sweet and most beloved / Night and day for love of thee / I sigh!] (*De amico*, Cambridge, Univ. Gg. IV.27, 15th c.)

The low frequency of switches between Aux and the non-finite V (cf. example under (15)) seems to mirror one of the syntactic constraints proposed e.g. by Timm (1975):

- (15) ~~It~~ ye wolde of mine stat *Audire* [Hear] (*Responsio*, Cambridge, Univ. Gg. IV.27, 15th c.)

There are a number of switches between the verb and a nominal or sentential object; in (16) the governing clause is in Latin, the complete object clause (including the subordinator) in English; in (17) the Latin object NP is dependent on an English verb:

- (16) *Ysayas cecinit* / ~~It~~ a chylde schalle be borne. [Isaiah sang] (*A maid hath borne*, Oxford, Ashmole 189, 15th c.)
- (17) ‘Thou shalt conseyye this sam day *Saluatorem mundi*.’ [The Saviour of the world] (Oxford, Bodl. Lib. MS Engl. poet.e.i, 15th c.)

1.2.3.3. **Adverbial phrase:** Clearly the greatest number of intrasentential switched constituents are Adverbial Phrases. In this function a variety of syntactic constructions and semantic classes occur in the corpus. (18) provides an example of a switched Latin temporal PP:

- (18) He brynge vs alle to good ende *In die nouissima*. [On the last day] (BM Sloane 2593, 15th c.)

In general, the whole PP is switched, and switches within the PP only occur when the dependent constituent is a quotation, a title, etc., as in “Aske fo-

ryeuenes of thi trespas, With *Parce mihi, Domine*” (‘With Spare me, o Lord’). This corresponds again to a tendency found in modern data, cf. Sankoff & Poplack (1981). However, switches after the preposition have sometimes been found in data outside the present corpus, as the following instance from a 16th century poem illustrates. It should be noted that the switch occurs within a biblical quotation:

- (19) Whan Chryst sayeth, ‘*Uenite*; / Ye blessed chyldren, come to me / Into *vitam eternam*. [When Christ says, ‘Come; You blessed children, come to me Into the eternal life.] (Huntington Library, R. Kele, *Christmas carolles*, c. 1550)

Well attested are Latin finite and non-finite adverbial clauses, cf. (20) and (21); the latter, however, are functionally not always clearly distinguishable from postmodifiers:

- (20) Sette hym *ut sedeat in principibus*, as he dyd before, [that he may sit with the Highest] (*Ballade set on the gates of Canterbury*, J. Speed Davies MS, m. 15th c.)
- (21) ‘Here, tretour, thou xalt abyde, *Ferens mortis tedia*.’ [Enduring the pains of death] (BM Sloane 2593, 15th c.)

Another frequent type of switched adverbials are Latin NPs in the dative or ablative case, cf. (22):

- (22) Than these profetes prechyd afor / That a chald xuld be born / To beye that Adam hadde forlorn / *Sua morte propria*. [Through his own death] (BM Sloane 2593, 15th c.)

It should be evident that the isolated examples given above do not adequately reflect the sometimes very complex switching patterns. This is illustrated by the example under (23):

- (23) *Regnum Anglorum regnum Dei est*,
As the Aungelle to seynt Edward dede wyttensse.
Now *regnum Sathane*, it semethe, *reputat* best.
For *filii scelerati* haue broughte it in dystresse.
[The kingdom of the angels/English is the kingdom of God
As the angel bore witness to saint Edward.
Now the kingdom of Satan, it seems, accounts best.
For the accursed sons haue broughte it in distress.]
(*Ballade set on the gates of Canterbury*, J. Speed Davies MS, m. 15th c.)

So much for an illustration of some tendencies with syntactic switching patterns. Let me finish with a brief look at some functional aspects of poetic CS.

2. Functional aspects

Much recent research on CS has focused on the discourse functions and the pragmatic aspects of switching (cf. Romaine 1995: 161ff.). There are evidently a number of factors which limit a functional-pragmatic approach to older poetry: firstly, the lack of information on extralinguistic factors, such as author, addressee, situation, event, etc.; secondly, the specific nature of poetic texts, though medieval and Early Modern poems provide a fair number of direct speech and even dialogues; thirdly, the general artistic function of poetic switching, which may override more specific functions. The conventionality of this form and the author's pleasure in playing with language are aspects which must have played a considerable role, especially in the case of regular alternation between languages. There is also evidence that such poems were sometimes written by students and monks as a sort of poetic practice. Another general function of Latin-English switching in medieval poetry - and in other mixed written texts - may have been to indicate membership of the educated social class, much in the same way as conversational switching can function as "an overall discourse MODE" (Poplack 1980: 614). Poplack has rightly emphasised that "[t]he very fact that a speaker makes alternate use of both codes, itself has interactional motivations and implications, beyond any particular effects of specific switches.[...] It is then the choice (or not) of this mode which is of significance to participants rather than the choice of switch points." (1980: 614). - But apart from such general functions of poetic switching, CS may also serve more specific functions within a poem, such as to set a scene, to make the extralinguistic context more obvious, etc. Most of the functions illustrated below have been established by Gumperz (1982: 75-84) for living languages. Again, the following discussion does not make any claims for completeness, but only tries to emphasise the functional importance of poetic switching and existing correspondences to CS in living speech.

2.1. Quotations

Most of the switches in the corpus are quotations, especially from the Bible or other religious texts - though religious quotations in ME poetry are also frequently in English, cf. Smyth (1911). They often serve more specific functions within a specific poem, such as providing the refrain or setting the motto or burden for an otherwise monolingual poem. - In a number of cases, switching occurs even within the quotation, cf. (24), with the source of the quotation ('sayethe dyuyne Scrypture') being explicitly stated; this pattern or strategy can be interpreted as a kind of 'flagging' device (cf. Poplack 1988: 229f.).

- (24) “*Omne regnum in se divisum,*” sayethe dyuyne Scrypture, / “Shall be desolate”
[Every kingdom divided in itself,] (*Ballade set on the gates of Canterbury,*
John Speed Davies MS, 15th c.)

An interesting function of quotations is shown in (25), where they are used to set a certain scene or background, and thus imply information which is not explicitly expressed. In this poem on *The death of the duke of Suffolk*, seven of Suffolk's ecclesiastical supporters are introduced and characterized by the first words of various parts of the Office of the Dead (i.e. psalms, antiphons, and responses), which they have to perform on the execution of the duke.

- (25) *The death of the duke of Suffolk* (BM Cotton Vesp. B.xvi, m. 15th c.)

Pray for this dukes soule þt it might come to blis,
and let neuer such another come after this! [...]

“Placebo”, begynneth the bisshop of Herford.

“Dilexi, for myn auancement”, saith þ bisshop of Chestre.[...]

“Ad Dominum cum tribularer”, saith þ abbot of Gloucestre.[...]

“Si iniquitates”, saith þ bisshop of Worcetre,

“For Iac Nape soule, *de profundis clamaui*”.

Switched direct speech can also be used to characterise a speaker as a member of a certain linguistic or social group. In the *Song on the Flemish resurrection*, for example, some French knights begin with a French sentence or phrase, but then switch over to English, cf. (26). Though this is not done consistently throughout the poem, it clearly sets the French knights and court apart from the Flemish rebels. This rendering of quotations mirrors observations in modern data that quoted messages are not always reported in the original language.

- (26) “*Sire Rauf Devel,*” sayth the Eorl of Boloyn, / “*Nus ne lerrum en vie channon ne moyne,* / Wende we forth anon ritht withoute eny assoygne.” [We will not let alive chaplain nor monk,] (BM Harley 2253, ¹14th c.)

2.2. Reiteration

Reiteration, i.e., translation into or paraphrasing in another code, has been found to be a frequent reason for switching in living languages, either for the sake of clarification, or to “amplify and emphasize a message” (Gumperz 1982: 78). This strategy has been illustrated in the religious texts and sermons in the first part of this paper (Schendl 1996); it is equally used in poems, cf. (19) above and (27):

- (27) The sunne þt euer shyneþ bryȝt, / the sterre þt euer yeueth his lyȝt / *Semper clara*.
[Always bright.] (Oxford, Bodl. Lib. Arch. Selden B. 26, m. 15th c.)

2.3. Interjections

The few interjections in the corpus are French ones in an otherwise English text. Their discourse function seems similar to that of some quotations, namely to add to the characterization of a speaker, such as the French knight talking to his king in *The song of the Flemish resurrection* (28):

- (28) Tho suor the Eorl of Seint Poul, *Par la goule Dé!* / We shule facche the rybaus
wher thi wille be, / Ant drawn hem with wilde hors out of the countré (BM
Harley 2253, ¹14th c.)

2.4. Word-play

Switches may also function as word-plays or provide a kind of folk etymology for a preceding word (cf. also McClure & McClure 1988: 41f.). In a Latin poem from the middle of the 13th century, e.g. the names of four greedy Anglo-Norman brothers, *Robert, Richard, Gilbert* and *Gefrei* are 'explained' by the phonetically similar French words and phrases *robbur, riche hard, gilur* 'guiler', and *jo frai* 'I will do', cf. (29):

- (29) Gilebert non sine re *gilur* appelatur. [Gilbert is, not without reason, called guiler.] (BM Harley 978, fol. 123)

2.5. Rhyming function

In many cases a switch provides the rhyme, a fact which may have motivated switching to a certain extent - though this hardly seems to be an independent function. The examples quoted so far provide ample instances of rhymes between the same language and of rhyming between different languages.

3. Conclusion⁴

The two parts of this paper should have illustrated a number of points, though further research, including quantitative analyses, will have to substantiate some of the hypotheses:

⁴ This conclusion is largely based on ideas jointly elaborated in a paper with Laura Wright, cf. Wright & Schendl (1995). I would like to thank Laura Wright for her share in the elaboration of these points.

(i) CS in written texts was not an exception but a widespread specific mode of discourse over much of the attested history of English. There are certainly still many more texts which have not yet been studied under this aspect - both in Britain and on the continent, since mixing between Latin and the respective vernaculars seems to have been a more or less pan-European phenomenon.

(ii) Switching occurs across domains, genres and text types - business, religious, legal and scientific texts, as well as literary ones - and 'o-ems should be seen in this larger context. Differentiation according to these textual parameters is necessary for any further research.

(iii) The theoretical framework provided by modern research on CS helps us to see medieval mixed texts in a new light, since there are evidently a number of parallels between older written and modern spoken data, which may reflect universal tendencies of switching. This does certainly not mean that we should force prefabricated grids on older data.

(iv) The history of English has too often and for too long been written as the history of the literary language as evidenced by monolingual sources. However, even these cursory remarks should have shown that the detailed study of older mixed language texts, in particular of switching patterns and strategies, could add an important dimension to the study of the history of English - a history which is much less monolingual than older research has often made us believe.

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Looking beyond the native / non-native contrast in ELF teaching:

Some thoughts on Barbara Seidlhofer's re-interpretation of an FL(E) teacher's double thought

Ute Smit

In her contribution to VIEWS 5 (1&2), Barbara Seidlhofer has done the unthinkable in the EFL trade. She has dared to question the ultimate aim accepted as part and parcel of EFL learning, namely **native** communicative competence, as well as the superiority of the native EFL teacher. In doing so, she has not only ventured an idea sacrilegious to many, but she has also done something much more fundamental: she has attacked one discriminatory “n-covered in our otherwise so politically correct academic world.

This specific “ism” has been so far below the level of consciousness that I am not aware of ever having heard a term for it - let me call it “native-teacherism” then. By analogy with the definition of “racism” given in the *Cambridge International Dictionary of English* (1995:1166),¹ I'd describe native-teacherism as

the belief that language teachers' qualities are influenced by their L1 background and that the teachers of L1 backgrounds other than the language they teach are not as good as those who teach their own L1, which results in unfair treatment of the former group.

In a time when all such “isms” are being uncovered and rejected, it comes as a surprise that this one has not yet been universally denounced and rejected. Native and non-native teachers of English alike have accepted and also actively supported the hegemony (to use one highly fashionable term) of the native language teacher. While some individuals might have voiced slight criticism by pointing to positive aspects of being a “non-native” (see e.g. Seidlhofer, p. 75), this has most often been seen as light weight compared to the un-

¹ In this dictionary racism is defined as "the belief that people's qualities are influenced by their race and that members of other races are not as good as the members of your own, which results in the other races being treated unfairly."

alterable shortcoming of not being a native. No matter how hard you might try, you'll never get rid of this birthmark.

Isn't it amazing and shocking at the same time, how similar this train of thought is to hard and fast racial thinking? Birth is seen as the deciding factor for whether you can become part of the ingroup or have to stay outside forever, and for whether you can or cannot "do it"; the only difference being that the derogatory term is not "native", but "non-native". It is really high time that this view got questioned and critically examined.

This Barbara Seidlhofer (1996) has done, albeit with a different purpose in mind. Judging from her argumentation she is not primarily interested in uncovering injustice and discriminatory practices; she rather concentrates on the group's defining purpose, namely (foreign) language teaching, and dissects the nature of this profession with the aim to show up a number of misconceptions of what language teaching is all about and of the relevance of the teacher's "native-ness".

Before I venture my own point here, I'd like to state explicitly that I fully support her argumentation concerning our profession, i.e. her crucial plea for regarding ourselves as instructors rather than informants, as FLE (= foreign language which is English) rather than EFL teachers, and as double thinkers in the positive, cumulative sense. I can also only underline the relevance of teacher education instead of teacher training with the aim to educate teachers who are aware of their professional abilities, and not only their linguistic ones.

Where I would like to differ, though, is with regard to the importance attached to the nativeness factor. In her attempt to dismantle the seemingly all overriding superiority of the native, as opposed to the non-native, teacher, Seidlhofer stresses the advantages of being non-native. Such a teacher shares with her pupils, firstly, their first language and, secondly, the experience of learning English as a foreign language or lingua franca. Due to this twofold cultural link - linguistic culture and language learning culture - the non-native teacher is presented as better equipped than the native could ever be.

When I link this back to my starting point of "native-teacherism", this train of thought seems quite similar to the "Black is beautiful" movement, i.e. it reminds one of the classic self-assertive move of suppressed groups. These movements do, of course, have a point. Self-assertion is a necessary step towards questioning the suppressing group's hegemony and towards establishing a self-defined, endonormative identity. At the same time, though, these movements actually perpetuate the importance attached to the division they are fighting against. By claiming that Black people should be proud of their blackness, the racial argumentation is not dismantled as illogical, inconclusive and

not fitting to our modern world, but is re-implemented, albeit under changed conditions. We are faced with an antithesis here, not with the synthesis.

Similarly antithetical, it seems to me, is the plea for non-nativeness for ELF (=English as a *Lingua Franca*) teachers. It attaches crucial relevance to the teachers' own language background, to her own linguistic competence. It is thus a continuation of the old argumentation, not a refutation thereof. This, however, I would claim cannot get any support from Barbara Seidlhofer. In her article she makes it very clear that her aim is not to attack native in order to defend non-native EFL teachers, but to dismantle the belief traditionally put in the teacher as informant and as provider of native communicative competence; as teacher of English, "which happens to be a foreign language".

In other words, she is actually striving for synthesis. In order to get there, though, I would argue that it will be necessary to transcend the limits set up by the criterion of (non)nativeness and identify it as a pseudo-argument (cf. Seidlhofer 1996).

- Firstly, in order to be able to "share their students' cultural and foreign language learning background" (p. 70), non-native and native teachers alike may have gained relevant personal experience, be it by having lived in a similar cultural setting and/or by having experienced foreign language learning themselves.
- Secondly, how effective and successful the education is that teacher trainees get (p. 72-3) is not dependent on their (non)nativeness.
- Thirdly, native and non-native teachers can gain the "significant distance" (p. 76-7) to the language as this is very much a question of education and training: both native and non-native teachers can develop "experience-near" and "experience-distant" abilities.

With regard to all three points, there is no direct link between (non)nativeness and teaching. This Seidlhofer (1996:77) acknowledges herself in stressing that "you don't have to be a native speaker to know how best to do it", to which I would add "but you can be one."

In conclusion, it seems to me that (non)nativeness has no direct bearing on the abilities Seidlhofer puts forth as decisive for a good ELF teacher. Since these factors are directly relevant and responsible for a teacher to become a good teacher of a foreign language, which - in our case - happens to be English, I'd therefore suggest to drop the criterion of (non)nativeness altogether.

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