



The Social Context of Kentish Raising: Issues in Old English Sociolinguistics

GRAEME TROUSDALE*
University of Edinburgh

ABSTRACT

This article considers issues in Old English sociolinguistics, in relation to specific changes affecting the low front vowels in ninth-century Kentish, as manifest in spelling variation in charters of the time. This change is referred to as Kentish Raising (Hogg 1988). It is suggested that variationist sociolinguistics is not an appropriate framework within which to explain Kentish Raising, since the nature of the data is such that a variationist approach is untenable. A reconstruction of the social, political and cultural situation in ninth-century Kent is provided, which examines Mercian influence in the period, and suggests that a Mercian-driven change from above (Second Fronting) cannot be the source of Kentish Raising. Finally, it is proposed that recent work in genetic anthropology, which seeks to discover more about the nature and extent of the continental migrations, may be useful in understanding the social context in which the varieties of Old English existed and developed.

KEYWORDS: Old English, Kentish, sociolinguistics, dialectology, variationist studies.

* *Address for correspondence:* Graeme Trousdale, English Language, School of Philosophy, Psychology and Language Sciences, University of Edinburgh, 14 Buccleuch Place, Edinburgh EH8 9LN, Scotland, U.K., Telephone: +131-650-3599, Fax: +131-650-6883. E-mail: Gracmc.Trousdale@ed.ac.uk

1. INTRODUCTION

The **purpose** of this article is to develop the debate concerning Old English dialectology **addressed** by Richard Hogg (1988), by examining the changes affecting two of the front vowels in Kentish in the ninth century. Specifically, the article attempts a **critical** analysis of the concept of Old English sociolinguistics. Is such a discipline possible? Can a relationship be established between linguistic and social variation in communities which existed over a thousand years ago, based on the evidence that remains or that can be reconstructed? I will argue **here** that **the** answer to both of these questions is 'yes', but only if the term 'sociolinguistics' is used cautiously and precisely.

In what follows, an analysis is **proposed** whereby the linguistic variation reconstructed from the extant Kentish texts can be related to the social context in which those texts were produced. But the analysis **does** not claim (as **does** Toon 1983) that such texts should be **seen** to resemble—in any way—informants in present day **urban** dialect **surveys**. Labovian speech communities, such as Kentish or Mercian, cannot be inferred from the evidence we **have**, since the definition of the speech community in the Labovian framework is of necessity very **narrow**. But the Labovian paradigm is a highly specific kind of sociolinguistics: other approaches to language, which are sensitive to the theory that varieties emerge from the social context in which language is used, can equally be described as sociolinguistic.

Any discussion of Old English sociolinguistics must be a discussion about two kinds of reconstruction. On the one hand, there is the attempt to reconstruct aspects of the linguistic system of a given variety. On the other, there is the attempt to reconstruct the social context in which the **speakers** of that linguistic system operated—as Colman (1988: 116) argues, reconstruction applies equally to Anglo Saxon society and to Old English dialects. The first part of this article is concerned with the linguistic reconstruction, and the second with the social, political and cultural reconstruction. The final part is an attempt to synthesise the discussion by reconsidering the issue of continental migration.

II. LINGUISTIC RECONSTRUCTION

An attempt to reconstruct aspects of the linguistic system of Kentish Old English is hindered by the fact that the data available is limited, both in terms of the amount of extant material, and the scope of that material. As Campbell (1959: §14-5) and Hogg (1992: §1.9) show, ninth century Kentish data are more substantial than those available for the eighth century, since the eighth century material is restricted to names in Latin charters, while the ninth century data are primarily the collection of vernacular charters from the Christ Church scriptorium in Canterbury. But **despite** this increase **after** the year 800, we are still left with a fairly small corpus. As part of an examination of the plausibility of Old English dialectology, Lowe (2001) **provides** a catalogue of the ninth century charter material, including charters written entirely in Kentish Old English, of which there are nine, and Latin charters which contain **some** vernacular data, of

which there are thirteen. Even so, this amounts to a **corpus** of about 4,000 words only, covering a period of nearly 100 years. It is hard to **justify** a quantitative analysis for data such as these. However, Toon (1983: 107-11) **does** attempt such an analysis of the distribution of spellings of the reflex of Germanic **a* before nasals in a corpus of (mostly) eighth century southern English texts. In 18 charters covering a period of 75 years, he notes 73 variants. In other words, this is, on average, less than one variant per year. There are **also potential** problems in **terms** of the nature of that limited material. Hogg (1988: 189) raises an important question: "to what extent is it plausible to consider sociolinguistic variation when the only evidence we **have** comes from the writings of a **very** narrow social group?". In other words, not only do we **have** sparse data, but the data we do **have** is both socially and stylistically highly focussed. While none of the problems mentioned above precludes a sociolinguistic analysis, they do raise serious questions about the plausibility of a quantitative sociolinguistic analysis.

II.1. The linguistic change

One of the linguistic changes said to **have** occurred in the ninth century in Kent is the raising of the low front vowels. Proposed evidence for this change comes for example from the Christ Church charters, where there is considerable variation in the spelling of the reflexes of the following vowels:

(1)

- (a) West Germanic **a* > /æ/ as a result of First Fronting, **restored** to /ɑ/ in open syllables where a back vowel follows: <glednes> 'gladness'
- (b) Germanic **ā* > /ɛe/: <mege> 'kinsman'
- (c) West Germanic **ai* > /æ:/: <gedele> 'distribute'

The examples in (1) come from Kentish data: a ninth century charter and the tenth century Kentish Glosses to Proverbs: So, for **instance**, West Germanic **a* develops to /æ/ as a result of First Fronting —where we would expect an <æ> spelling, as we get in West Saxon, in Kentish we sometimes **have** <e> spellings. This is **also** the case for the reflexes of Germanic **ā* and West Germanic **ai*.

In fact, in the charters of this period, there are three variant spellings of these vowels. One is the regular development of the Latin digraph <ae>, i.e. <æ>; another is the **ligature** without the hook on the *a*, **often** transcribed in the editions as <e>; and the other is <e>. Hogg (1992: 75-6) notes that the second of these graphs could be taken as an allograph of either <æ> or <e>—we cannot be **sure** which **because** of the linguistic developments in Kentish at the time. Given spellings of the type <gæfe> 'grace' (cf. West Saxon (WS) <gefe>) in an early ninth century charter (S1188; Charters are referred to **here** following the listing in **Sawyer 1968**), it is clear that **all** three graphs could potentially represent a non-low vowel.

The aim of the present article is to try to account for this variation in spelling, and to

examine how this orthographic variation might relate to a linguistic change in Kent. It is clear that we must "have respect for our scribes and the data they present us with" (Hogg 1988: 190), to argue against the "assumed inferiority of the language of scribes, which was central to the ideology of nineteenth- and twentieth-century philology" (Fleischman 2000: 38-9). The problem exists not with the notion that orthographic variation may **indicate** phonological change, but rather with the notion that the texts which are used for such analyses could be considered as **informants** in any way similar to the modern quantitative sociolinguistic **interview**. For reasons discussed below, therefore, there is no attempt to **provide** a Labovian quantitative account for this variation and change, as such a practice for the data available is impossible. More central is the relationship between Kentish Raising and Second Fronting (cf. Hogg 1988). Toon (1983) following Dresher (1980) suggests that Kentish Raising (or at least part of it, the part that affects the short vowel) is part of the process of Second Fronting. Indeed, this is **crucial** to Toon's general thesis: that sound changes of Mercian origin **filter** down into Kentish as a result of Mercian domination in the south east. Hogg (1988) considers the two changes as distinct. These two conflicting accounts are discussed in the following section.

11.2. Kentish Raising and Second Fronting

Toon's argument (1983: 150-3) rests on the quantitative distribution of <æ>, <ǣ> and <e> spellings as evidence for the raising of the short low vowel. In Kentish charters of the early to mid eighth century, there is no evidence of raising. All variants of the variable are spelled <æ>. At what Toon considers the highpoint of Mercian influence, that is in the early ninth century, there is **little** evidence of raising, but there are **some** <e> spellings (e.g. <gehueder> 'whether' S1188), perhaps indicative of the inception of the change. But from the **mid-ninth** century on, there is a great increase in the number of <e> spellings. Toon (1983: 152) reaches the following conclusions from this distribution of variants: "the Kentish were at **first** slow to learn the second fronting from the politically dominant Mercians, but then quickly imitated the speech of their masters and **fully** extended the raising to æs from all sources".

One of the problems with this account concerns the nature and extent of the sound change **known** as Second Fronting (SF). This change affected words which contained Prim. OE æ not subjected to breaking or retraction (see Campbell 1959: §164): these words, with æ and a in early OE, come to be spelled with <e> and <ǣ>, respectively. This is taken to suggest a raising of the front vowel and a fronting of the back vowel. For **some** (for instance, Colman and Anderson 1983) these two changes are part of a unified process; for others (for instance, Dresher 1980, 1990) the raising of the front vowel constitutes a **separate** change from the fronting of the back vowel.

Dresher (1990) discusses the use of manuscript evidence as part of a wider investigation into the unified nature of Second Fronting (SF). What is of **importance here** is the use of the term 'dialect' in relation to the extant manuscripts:

We can talk of a 'Mercian dialect' which is represented by a number of manuscripts sharing enough features that we can group them together in opposition to manuscripts representing the 'West Saxon' or 'Kentish' dialect. To the extent that the manuscripts differ from each other, it is useful to talk also of the dialect of a particular manuscript, as representing a subdialect of Mercian. This usage is quite conventional.

Dresher (1990: 152)

These subdialects are therefore grouped together depending on what variants of a given variable they happen to display. So, for instance, Dresher (1990: 155) has a range of Mercian dialects (that is, individual manuscripts or groups thereof) depending on how Second Fronted they are:

(2)

Mercian Dialects

- a. Lacking both $\text{æ} > \text{e}$ and $\text{a} > \text{æ}$ (or ea):
Rushworth Gospels = 'north' Mercian
- b. Having both $\text{æ} > \text{e}$ and $\text{a} > \text{æ}$ (ea): *Epinal Glossary, Corpus Glossary, Vespasian Psalter* = 'west' Mercian
- c. Having $\text{æ} > \text{e}$ but not $\text{a} > \text{æ}$ (or ea): *Royal Glosses, Life of St. Chad*
- d. Having $\text{a} > \text{æ}$ (ea) but not $\text{æ} > \text{e}$: *Omont Leaf*

Hogg (forth.) shows that SF is consistent only in the *Vespasian Psalter*, which has been considered as representative of an emerging literary standard for ninth-century Mercian (though the concept of a 'standard' for such a variety is questionable —with which other texts can it legitimately be compared in order to identify it as a standard?). Certainly in comparison with those other texts often considered Mercian — Epinal, *Corpus*, *Rushworth 1* — it is anomalous as far as SF is concerned: the invariability of SF in the *Vespasian Psalter* is in marked contrast to the patterns in the other texts, where only about one in ten of the variants is raised. This anomaly is at the heart of the concern expressed by Hogg (forth.) about the very nature of Mercian.

It is therefore not possible to make the generalisation that SF is a Mercian feature which would have been a target sound change for the upwardly mobile Kentish of the ninth-century. The extant evidence would actually suggest that the geographic spread of SF is limited to the West Midlands. Because we cannot imbue the literary dialects of Mercian and Kentish with territorial significance, it is reckless to assume that a linguistic feature associated with one manuscript reflects the language of the whole of a disparate set of speakers which have been grouped together by historians as the Mercians. This issue is raised by Toon, and discussed further in section 2 below.

Hogg's analysis (1988: 193-8), by contrast, suggests that the pattern in Kent is a separate development from SF, and provides some statistics to illustrate this point. As noted above, the numbers involved for some of the charters are very small. For instance, the charter S1510 (845x853) has only four variants for etymological *e, two spelled with <æ>, and two spelled

<e>. With data this paltry, a quantitative analysis is doomed. But this should not detract from Hogg's main thesis, which is that Kentish Raising was a process which — since it involved both the short and the long low vowel — was therefore distinct from SF:

Consider, however, the **possibility** of a Mercian (-trained) scribe familiar with or speaking a dialect with the different change of Second Fronting [...] **If** in the early ninth century Second Fronting has already taken place but Kentish Raising is **just starting** to **take** place, then when the scribe **hears** a **slightly** raised Kentish ϵ this will **still** be **sharply** distinguishable from the product of the Second Fronting of ϵ . Therefore he will be strongly motivated to **write** α rather than e, in order to distinguish the Kentish sound from **its** Mercian equivalent.

Hogg (1988:197)

In other words, this is a question of salience. Salient phonological **features** are most likely to be reproduced or noted by orthographic variability. This analysis is useful in terms of explaining scribal habits—it shows what a Mercian or Mercian-trained scribe might do in order to represent a local Kentish change. But it **does** not explain what might **have** brought about this change in the first place.

In summary, Toon's account suggests a Mercian influence which might not be justifiable, and Hogg's account explains the orthographic variation but **does** not account for the motivation for the phonological change. Since both Toon and Hogg invoke sociolinguistic phenomena to account for the patterns they observe, it is important to address the issue of sociolinguistic variation in detail. The remainder of the article therefore explores the possibility of Old English sociolinguistics.

11.3. The impossibility of Old English variationist sociolinguistics

Toon (1983) attempts to account for variation in early English texts within the quantitative paradigm. In the orthographic variation in eighth and ninth century Kentish texts, he **sees** a reflection of the rise and **fall** of Mercian influence in the far south east of England at that time: there is a correlation between what Toon argues are Mercian variants in the Kentish charters, and Mercian power in the Kentish kingdom. Toon himself acknowledges that there are problems with the application of the quantitative paradigm to Old English data. For instance, he notes (1983: 65) that we know very little about the scribes (for instance, where they were born, how mobile they were) and their writing styles (i.e. "which social or stylistic registers their writing represents"). He **also** acknowledges that the data are **sparse** and, crucially, non-representative. **A** further problem is the **difficulty** we **have** about localising texts.

11.3.1. Texts as informants

In his discussion of the extant charters from the Anglo-Saxon period, Toon refers to such texts repeatedly as "informants" (1983: 42, 66). This is problematic, since the linguistic information provided by the charters is nothing like the information provided by an informant in a modern

quantitative sociolinguistic survey. Even with the most favourable interpretation, the charters are monostylistic, failing to display the idiolectal variation which typifies informant style-shifting in a modern variationist interview. Then there is another problem concerning orthographic variation between the different scribes at Christ Church. Brooks (1984), in his analysis of the ninth century charter material, was able to identify a number of scribes who had written (or at least contributed to the writing of) more than one charter, and made a number of observations on the standard of their work. Thus, while Brooks' scribe 4 (writing in the 820s and 830s) was evaluated as "the best of the contemporary Christ Church writers" (Brooks 1984: 198), scribe 7 (possibly the principal scribe soon after 855) has "particularly unattractive" writing. Brooks' analysis of this hand is as follows:

A characteristic of this scribe is the unchecked proliferation of misspellings, reflecting the usages of his native Kentish [...]. Since his script is so distinct from that of earlier Canterbury writers, it seems reasonable to suppose that he was a Kentish cleric who had been trained in a different centre where there was still some command of grammar but no attempt to each any standard pronunciation or spelling.

Brook (1984: 171)

While we can ignore Brooks' prescriptivism here, he raises an important point about style, and this is compounded by our lack of knowledge about the provenance and status of the scribes themselves.

Also, in the charter corpus, there is not really a range of 'informants' at all: while the function of the charters does vary to some extent (some are wills, some are royal grants, some are private agreements between members of the nobility etc.), the function of the charters is hardly sufficiently heterogeneous to allow the texts as a whole be classified as a range. Certainly, the range of charters is not of the same type, nor of the same degree, as the range of informants of a typical modern quantitative survey. The crux of the problem with Toon's analysis is summed by the following comment: "Real data produced by native informants are to be preferred over any reconstruction" (1983: 65). But this is not —despite what Toon claims— what we actually have when we examine a Kentish charter. All historical phonology requires reconstruction, and texts simply cannot be considered members of a speech community: to do so would be an instance of Conceptual Inertia, "the straightforward application of the linguistic concepts or grammatical categories of a modern language to the data of an older stage of that language" (Fleischman 2000: 39). Fleischman's analysis of text languages is particularly relevant to the present discussion: while she suggests that the 'native speakers' of a text language are the texts themselves, she also urges caution in treating texts as informants, since they do not represent the idiolect of any language user, but rather "the language of an author/composer filtered through one of more textual copies and subject to greater or lesser modification in the process" (Fleischman 2000: 46).

II.3.2. The Mercian speech community

The notion of the Mercian and Kentish speech communities is central to Toon's thesis. The applicability of this concept to the linguistic situation in Anglo-Saxon England is questionable. Indeed, Toon himself draws attention to issues surrounding the association of linguistic with social categories in Anglo-Saxon England when he discusses tribal organisation and political structure in light of the Tribal Hierarchy, which

... calls into question two standard assumptions of English historical linguistics: first, the geographical orientation of the dialectology proposed for early English, with its oversimplified division of the country into Northumbrian, Mercian, West Saxon, and Kentish; and, second, the further assumption that each of the four regional varieties was a single, homogeneous speech variety. Tribal organization is not a likely foundation for widespread linguistic homogeneity.

Toon (1983: 25)

Toon's main conception of a historical speech community seems to be the following:

It is more realistic to speak of a speech community which loosely shares a set of typical features but within which there are subsystems in competition. The second fronting, for example, is a feature of Mercian Old English. To admit this is not to admit that second fronting was uniformly implemented in the language of all of the members of the Mercian speech community. Its implementation would be evident as a wave moving through the community and variously affecting speakers and even intruding into other 'dialects' (as the second fronting did into Kentish).

Toon (1983: 61)

Middle English evidence, however, suggests that Second Fronting barely spread out from the western fringes of Mercia. Toon also believes (1983: 65) that the data he analyses allows us to "monitor the diffusion, throughout a whole speech community, of linguistic features characterizing a politically dominant group".

One factor which is crucial for the existence of a (Mercian) speech community—in the Labovian tradition in which Toon's work is located—is shared norms. In other words, for a Mercian speech community to exist, there must be evidence that variants are (subconsciously) evaluated in the same way by different members of the group. This is what is claimed by Toon (1983: chapter 3) in his discussion of the development of West Germanic *a before nasals: the diffusion from <a> to <o> over a large area of southern England throughout the period of Mercian domination is a result of speakers responding to the prestige (presumably) of Mercian pronunciations. I assume that this is to be classified as a change from above. For instance, Toon's claim that "Mercian political domination could effect linguistic change in Kent" (1983: 118) would suggest that the change is effected by dialect contact, introduced by a higher status social group, both of which are symptomatic of change from above, rather than change from below (on which see further Labov 1994: 78).

The problem is that there is **insufficient** evidence for these **claims**. In order to **prove** that there are shared norms among the Mercian speech community, there must be evidence of different groups evaluating the variants differently in different styles. We would need to see that in careful styles, speakers in the Mercian speech community had a higher proportion of variant *x* than they did in more casual styles. This is the only evidence for the **existence** of the **kind** of speech community which Toon employs in his work. To argue that the Mercian speech community is simply a **small** group of texts which show vaguely similar distributions of variants over a **large** period of time suggests a misconception of what constitutes a speech community in the quantitative paradigm. Indeed, Toon himself recognises problems with his data:

Certainly, the data of this study cannot be **controlled as** tightly **as** that which is **collected by** trained **linguists** in the contemporary social **setting**. We cannot know who the informants were, where they came from, how **long** they had **lived** where they **wrote**, or which social or stylistic **registers** their writing represents. Further, the data are sparse **and** do not represent a cross section of the speech community.

Toon (1983: 65)

A variationist analysis of Old English data requires a **relaxation** of the constraints associated with the early Labovian conceptualisation of the speech community. Sociolinguists' understanding of what the speech community actually is has of course changed since Labov's work in New York City in the 1960s —for an analysis of current approaches to the speech community, see Patrick (2002). But the methodology employed by Toon relies heavily on the notion that it is possible to show that a Mercian (and presumably Kentish) speech community exists, and its shape is derivable either from linguistic evidence —**which** would **make** the analysis horribly circular —or from **external** historical evidence, on which see **further** below. Such a view is made clear by comments such as “**the** charters offer a cross section of the Mercian speech community over an extended period of time” (Toon 1983: 145). It is impossible to proceed along Labovian lines —**which** it seems to me is what Toon **does**. He is right to suggest that any linguistically significant variation which we find in the extant **manuscripts** demands analysis. **And** he is **also** right to suggest that we contextualise the variability in its social context. But a variationist approach is untenable given **the** nature of the data.

III. SOCIAL, CULTURAL AND POLITICAL RECONSTRUCTION

The discussion so far has **been critical** of the methodological approach adopted by Toon (1983): I believe that the model chosen **cannot** work given the data available. However, this is a matter of debate, so let us **allow** that a Labovian approach is possible, and that it **is** possible, in **principle**, to suggest that Mercian political power could change Kentish pronunciation, similar to the way in which contact with General American speakers brought about the reintroduction of rhoticity into the lects of speakers **from** New York City. And let us go even **further**, and accept that Kentish Raising of the short vowel might be **part** of Mercian Second Fronting. In order for

this to occur, we would need to **propose** a 'change from above', brought about by fairly sustained contact between Kentish and Mercian speakers, which then percolates down whatever hierarchy is proposed for Kentish society. Unfortunately, even this will not work, as can be shown by a reconstruction of the social and political scene in eighth and ninth century Kent.

The ninth century in Kent saw a general shift in the balance of power from Mercian authority towards that of the West Saxons. But recent historical research has indicated that the means of imposing that authority were not consistent —**nor** always successful — throughout the period of Mercian control. Kent was undoubtedly a **critical** political arena. As Keynes (1993: 112) argues, control of Kent was important in a number of ways. Kentish centres of trade maintained crucial links with the continent, Kentish mints supplied the coinage with which such trade could be carried out, and Canterbury provided the ecclesiastical **focal** point for southern England throughout most of the period. But control of the trade, of the production of money and of the clergy was exercised in different ways by different authorities at different times. What follows is an exploration of Mercian, West Saxon and Kentish social, political and cultural influence in the period.

III.1. Mercian authority in Kent

Toon (1983) relies heavily on the notion of Mercian influence for his interpretation of the sound changes affecting Kentish in the ninth century. But as King (1992: 24) argues, there are certain claims made by Toon concerning the political situation in eighth and ninth century Kent which require closer scrutiny. A pattern of unbroken Mercian 'direct rule' in Kent is a myth: in the early to mid eighth century, Æthelbald, while he was accepted by Eadberht II and Æthelberht II of Kent as their overlord, had a policy of **minimal** interference (cf. Brooks 1984: 111), which was to be echoed in Cenwulf's government at the beginning of the ninth century; in 757, the murder of Æthelbald allowed for a brief period in which Kentish rule of Kent was established, which again was echoed at the end of the eighth century on the death of Offa.

But even during the mighty Offa's rule, the situation in Kent for the Mercian authorities was not straightforward: Brooks (1984: 112-3) suggests there was significant resistance by the Kentish dynasty and nobility, to the extent that Kent was by and large independent for nearly ten years, from 776 to c.785. The establishment of an archbishopric at Lichfield is also interpretable in a number of ways. Toon (1992) suggests that it is indicative of Offa's power, and the extent of Mercian influence, since the king was able to persuade the Pope to create a third archiepiscopal see. But Brooks (1984) suggests that the decision made at the Synod of Chelsea in 787 might also be seen as a sign of weakness on Offa's part, as an admission of his **inability** to maintain control. Offa's elevation of Hygeberht to Archbishop at Lichfield as a result of animosity between the king and Jænberht in Canterbury could just as easily be viewed as the act of a ruler who could no longer wield the same kind of control over Kentish affairs.

We might conclude from this that the attitude of the Mercians towards the Kentish was somewhat supercilious, on which see further Colman (2004). Certainly, the level of antagonism

between the Mercians and the Kentish would be unlikely to be one in which the former was emulated by the latter in any way. Offa's methods are fairly well documented: "he disposed of land in Kent without reference to local kings; he used the Canterbury mint as his own; and his name was inscribed on coins issued by the Archbishop of Canterbury" (Hodgkin 1952: 387). It is hard to see how this kind of behaviour on the part of the Mercian authorities could engender favourable attitudes (crucial to the likelihood of any linguistic accommodation, a critical factor in the transfer of linguistic features from one community to another in the kind of dialect contact scenario Toon envisages).

In 798, Cenwulf of Mercia, having failed to persuade Pope Leo to establish primacy of London above Canterbury following the abolition of the Lichfield archbishopric, invaded Kent and captured Eadberht Praen, who had emerged as the Kentish king on the death of Offa. It was perhaps the manner in which Cenwulf exercised his control of Kent which led to the disintegration of Mercian authority following his death in 821. Keynes (1993) suggests that Cenwulf asserted his authority forcibly in 798, but thereafter ruled from a distance, with the Kentish nobility forming a distinct secular hierarchy. The nature of Mercian influence during this period is summed up in the following assessment:

It seems unlikely, in other words, that Mercian interests in Kent had ever extended to colonization, or that Mercian control had ever involved columns of occupying troops marching up and down the streets of Canterbury; it may have been sufficient for Cenwulf to rely on the presumption in Kent that any challenge to his authority would meet with a sharp response, as in 798.

Keynes (1993: 117-8)

This contrasts markedly with the behaviour of West Saxon rulers. Where Cenwulf had rarely, if ever, maintained a personal presence in Kent, Egberht and Æthelwulf journeyed there frequently, as well as making a member of their immediate family king of Kent; and where Cenwulf had shown little involvement in the organisation of the Kentish nobility, Æthelwulf was keen to promote their interests, promoting some of them to the position of ealdorman (Keynes 1993: 119-120). West Saxon rule of Kent was much more 'hands on' than was Mercian rule. This is all highly problematic for Toon's account, where his analysis of eighth and ninth-century charters leads him to the conclusion that "the Kentish were at first slow to learn the second fronting from the politically dominant Mercians, but then quickly imitated the speech of their masters and fully extended the raising to any *æ*s from all sources" (1983: 152). His thesis—brilliant though it is for trying to emphasise the social context of historical sound change—has a number of problems, both in principle and in application. In the final section of the article, I therefore discuss an alternative source for Kentish Raising—the Frisians.

IV. THE CONTINENTAL FACTOR

The continental influence on the development of the Old English dialects has **been** a topic of debate in the **past** (e.g. Bremmer 1981, DeCamp 1958, Nielsen 1981, Samuels 1971). The nature and extent of continental influence has **been** and continues to be an issue for historians and historical linguists.

IV.1. The Franks and the Frisians

There has **been some** discussion of Frankish influence in early Kentish history. Susan Kelly makes the following observation about the Roman mission:

Augustine and his **companions** were **already** worried about linguistic **difficulties** before they arrived in England. In order to **alleviate** their anxieties, Pope **Gregory arranged** that they should be joined by Frankish priests who would act as interpreters. Much ink has **been** spent on the question of the mutual **intelligibility** of Frankish and English; alternatively, **it is** possible that mercantile and political **contact** between Kent and Gaul and the **presence** of Franks at **Æthelberht's** court had familiarised Franks with English (and **some** Anglo-Saxons with Frankish).

Kelly (1990: 58)

Frankish influence is **also** discussed by Brooks (1984), who suggests that **it** was likely that there were **some** Franks among the early settlers of Kent, based on archaeological **evidence** uncovered in sixth century graves, which contained Frankish **jewellery** and weaponry; and by Colman (2004) who discusses currency changes in Kent in light of trading **links** between Kent and the Franks.

The relationship between English and Frisian has **similarly been** a **source** of debate for **some** time. We can trace the history of this idea from the seventeenth century on: as Nielsen (1981: 40-3) points out, Franciscus Junius and Janus Vlitius had assumed a **close** connection between English and Frisian. In the **later** nineteenth century Siebs refers to a variety he **calls** *englisch-frisisch*:

... eine sprache, wie sie durch die **summe** gemeinsamer **lauterscheinungen** der ags. und frs. mundarten repräsentiert wird, und wie sie geraume **zeit** vor der colonisation Britanniens —**vielleicht** im 2. oder 3. **jahrhundert** n. Chr.— bestanden haben **dürfte**”

[a language, through which is represented the **entire** common phonetic **forms** of the Anglo-Saxon and the Frisian dialects, and which must **have** existed for **some** considerable time before the colonisation of the British **isles** —**probably in** 200-300 AD] (Siebs 1889:7, **my translation**).

As Nielsen (1981: 43) argues:

the **reason** for his [Siebs'] dislike of the term '**anglofrisisch**' is that it suggests a closer **affiliation** of Frisian with **the** language of the Angles than with the languages of the emigrated Saxons and the Kentish Jutes ... Siebs **reaffirms** his view of the relationship between English and Frisian many years **later** (cf. 1930: 70 where the following English-Frisian innovations are cited: **Gmc. a** > **æ/e**; **Gmc. ē** (> WG **ā**) > **æ:/ē**).

It is important to note, in any discussion which involves a comparison of English and Frisian, that Frisian is generally later attested than varieties of English are: as Bremmer (2001) suggests, given that the Frisian writing tradition begins with Psalter Fragments c.1200, much of the evolution of Frisian prior to this date is of necessity conjecture. A discussion of the phonology of Old Frisian (OFr.) is provided by Nielsen (2001). His description of the development of Gmc. *a, *ā and *ai (cf. (1) above) in OFr. is as follows: Gmc. *a merges with the reflex of Gmc. *e (cf. OFr. *stef* 'staff' and *bera* 'to bear'); Gmc. *ā and *ai merge (along with four other Gmc. vowels) to OFr. *ē* (OFr. *dēd(e)* 'deed', *dēla* 'deal'). Bremmer (2001: 602) reconstructs a Proto-Frisian (Proto-FR.) phase on the basis of the development of Insular North Frisian (InsNFr.), which emerged following the emigration of a group of Frisians to the islands off the Schleswig-Holstein coast during the eighth century:

It is especially InsNFr. that allows us to reconstruct a Proto-Fr. phase because this dialect shares a number of phonological and morphological innovations and generalized tendencies with Frisian as it was (and is) spoken between the Rhine and the Weser ... At the time of the emigration, these features must already have been a prominent feature which (together) separated Frisian from the adjacent dialects of Low Franconian and Saxon. They include: ... (2) fronting of a > æ (spelled <e>) in closed syllables (with some restrictions, e.g. Gmc. **staŋ* > OFr. *stef* 'staff').

There is thus some debate concerning the reflex of the short vowel: Nielsen takes the <e> spelling to be an indication of a merger with a mid vowel, while Bremmer argues that the spelling indicates a fronting, but not necessarily a raising. (See also Boutkan 2001, who discusses various issues surrounding the development of OFr. /e/, as well as some evidence for both /æ:/ and /e:/ in OFr). DeCamp's (1958) position is that Frisian was of central importance to the genesis of Old English dialects and this was particularly the case in Kent, but only to the extent that Frisian continued to exert an influence after the initial settlements: he argues, therefore, for an insular, rather than a continental, source for the developments of the OE dialects. One of the central arguments put forward by DeCamp is that the consensus of modern historians is that there were "no migrations of entire tribes" (DeCamp 1958: 233), so that Kent was colonised by peoples from a range of locations on the continent. He suggests however that mass migration is not necessary for linguistic features to move from one area to another, and that the spread of a change may be propagated by imitation, usually from a "superior to an inferior culture" (DeCamp 1958: 233). As evidence against the continental origin of Old English dialects, DeCamp notes, for instance, that raised variants of Gmc. *ā (> /E:/) and Gmc. *a (> /æ/ through First Fronting) --(1a) and (1 b) above— are rare in the earliest Kentish documents. If the raising had indeed begun on the continent, there would have been some evidence of raising in the seventh and eighth centuries. De Camp was of the opinion that the conquest of Britain did not involve "a transfer of entire continental nations, each with its own culture and language" (DeCamp 1958: 237). He views the migrations as more piecemeal, and the migrants as a rather less cohesive group than is presented in traditional accounts. Innovations, therefore, merely

spread along trade routes, which would allow for the diffusion of so-called 'Kentish Raising' far beyond Kent. Indeed, DeCamp suggests that the change affecting the short vowel may have spread as far as the Wash. Certainly we do not need to assume extensive Frisian presence in Britain. But I am not sure about DeCamp's claims regarding migration. Certainly, I do not think that there is consensus amongst anthropologists about the nature of the migrations. DeCamp's theories accord with the processual school or New Archaeology views of the 1960s and 1970s—the adoption of new cultures (and therefore linguistic varieties, being a part of culture) could occur through trade or "by the influx of a small ruling elite with minimal or no impact on the gene pool (the 'elite dominance' model of Renfrew 1987)" (Weale *et al.* 2002: 1008). I turn finally to some research by genetic anthropologists in this regard.

A study was carried out into present-day genetic evidence for earlier mass migration, that is, in the Anglo-Saxon period (Weale *et al.* 2002). The researchers took buccal swab samples of 313 males from seven towns in an east-west transect from East Anglia to North Wales. In addition, DNA samples were collected from 94 males in Friesland in the northern Netherlands, and 83 males from Norway. The sample consisted of males on the grounds that the "non-recombining portion of the Y chromosome and the mitochondrial genome are useful sources of data because they provide exceptionally detailed high-resolution haplotypes, allowing fine definition of the underlying gene genealogies" (Weale *et al.* 2002: 1009). Haplotypes are a set of closely linked genetic markers present on one chromosome, which tend to be inherited together. The main findings of the study were as follows:

- "Little genetic differentiation exists among the Central English towns" (Weale *et al.* 2002: 1017).
- The two North Welsh towns are highly divergent, both from one another, and from the Central English towns.
- "No significant differences in haplotype frequencies exist between Friesland and any of the Central English towns" (Weale *et al.* 2002: 1017).
- The Norwegian samples were significantly different from all of the Central England towns apart from Bourne (whence few samples were collected).
- When the results from all the Central English towns are combined, they show that the Central English males are more closely related genetically to the Frisians than they are to the North Welsh or to the Norwegians.

In other words, there is a clear indication of a common male line of descent between inhabitants of Central England and Friesland. The question is, of course, whether mass migration is necessary to produce such a pattern. Weale *et al.* consider their findings to be striking, "given the high resolution and rapid mutation rate of the Y chromosome haplotypes on which those findings are based" (Weale *et al.* 2002: 1018). To best account for their data, Weale *et al.* (2002: 1018) estimate that "an Anglo-Saxon immigration event affecting 50%-100% of the Central

English male gene pool at that time is required". This does not have to be a single event, of course: a more gradual migration, lasting several generations, is more likely—but the key issue is that "the same degree of admixture" (Weale *et al.* 2002: 1018) is required, irrespective of whether this happens in one go, or over an extended period. While mass migration is not proved conclusively by their results, the background migration rate would have to be very high to result in the same patterns: while a rate of 0.3% would negate the need for a mass migration, it would also mean that "one in six of today's Central English males descend from Frisians—or a population identical to the Frisians—who emigrated to England after the invasion and that an equal proportion of Frisians must also be descended from the English in a like manner" (Weale *et al.* 2002: 1018).

Such a view on the migrations has not gone unchallenged by other geneticists, however. A more recent survey (Capelli *et al.* 2003) suggested quite different Y chromosome patterns across the British Isles. They compared data from a wider range of British sites with Norwegian (Bergen and Trondheim), German (Schleswig-Holstein), Danish and Irish (Castlerea) samples, and noted a high degree of similarity between the Danish and North German Y chromosomes, meaning that it was not possible to distinguish the genetic contributions of these two groups to the British Isles. They also discovered that the greatest German/Danish influence was not in the far south-east, but in Norfolk (Norwich) and Yorkshire (York), and that there was no evidence for complete population replacement anywhere in the British Isles. Crucially, their comparison of a Frisian genetic sample with that of the Schleswig-Holstein group suggests that "the Frisians were more 'Continental' than any of the British samples, although they were somewhat closer to the British ones than the North German/Denmark sample" (Capelli *et al.* 2003: 983). Further genetic evidence relating to the nature of the migrations is clearly needed, since the finding of Weale *et al.* (2002) and those of Capelli *et al.* (2003) provide quite different accounts of the nature and extent of Anglo-Saxon, Frisian and Danish settlement patterns. But both of these surveys suggest that the Frisians may have had a significant role to play in the migrations. Now if there was, as Weale *et al.* suggest, a mass migration from Friesland to Britain, and if there is also much more evidence to suggest a close Kentish and Frisian connection historically, I think that DeCamp possibly came to the right conclusions for the wrong reasons. His dating of Kentish Raising is very early (late seventh century for the short vowel), much earlier than Hogg's (whose theory requires Kentish Raising to postdate Second Fronting), which is in the mid-ninth century, a view shared by Campbell (1959: §290); and the degree of spread of that change is also debatable. My view is that the actuation of the change may well have been in the late seventh or early eighth century. At that point we are probably more likely to be talking about sporadic innovations rather than embedded changes. Certain genetic evidence suggests the possibility of significant—if not tidal—waves of Frisian immigration spreading throughout the south-east of England and thence northwards, from the initial fifth-century migration onwards. The embedding of that change is likely to have been significantly later, at the point where it took on a social meaning, perhaps as a marker of Kentish identity in opposition to Mercian interference. As

Samuels argues, dialect **features** of varieties in periods **later** than the invasion "must always be considered as potentially relevant to **the** invasion period" (1971: 5), and "the late appearance of correspondences **does not prove** that there was no original connection" (1971: 4).

Perhaps few would consider Old English sociolinguistics to involve the discussion of Y chromosome haplotypes; and certainly this **genetic** evidence must be treated cautiously, for many reasons. It is not introduced **here** to suggest **anything** about the **genetic** encoding of language, or —**much** worse— to suggest that certain **genetic** types **have** specific linguistic properties. Nor can any firm conclusions be drawn from what **is** an on-going debate among **genetic** anthropologists, so **it is necessary** to acknowledge that the claims made **here** are provisional. Rather, the **genetic** evidence **is** adduced as an attempt to establish in more detail the nature and extent of settlement patterns **in** the migration period, since this kind of knowledge —**involving** a discussion of migration patterns from the continent, based on data collected from a range of sources, be they **genetic** anthropologists, historians, or numismatists— is crucial for our understanding of the cultural context in which varieties of Old English existed.

Acknowledgements

I am very **grateful** to audiences at research seminars in the English Language departments of the Universities of **Sheffield** and Edinburgh, as **well** as to Fran Colman and Katie Lowe, for their very **helpful** comments on aspects of this article. **All shortcomings** are my own.

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