

Timofeeva, Olga. 2022. *Sociolinguistic Variation in Old English. Records of Communities of People. Advances in Historical Sociolinguistics*. Amsterdam: John Benjamins. Pp. xv + 204. ISBN 9789027211347.

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Since the publication of *LALME* (1986), there have been discussions about the feasibility of the study of Old English dialects (Hogg 1988; Lowe 2001) and attempts to apply the methods of contemporary dialectology (the fit technique developed by Angus McIntosh and associates) to Old English texts (Kitson 1990, 1993, 1995, 1996). In a similar way, Old English scholars have long considered approaching the linguistic variation in Old English from a sociolinguistic perspective and with sociolinguistic methods of analysis (the work of Toon [1983, 1992] is a good example). Olga Timofeeva's new book on sociolinguistic variation in Old English sets out to demonstrate that Old English sociolinguistics is not an impossibility in spite of the skewed nature of the evidence that has survived and the fact that the texts were written, for the most part, by individuals of the same rank: middle-aged, educated, Catholic men. According to the author, despite these obvious drawbacks, the rich Old English corpus still "awaits its full sociolinguistic appreciation" (Timofeeva 2022, 3).

The book is divided into eight chapters, without introduction and with a short Epilogue by way of conclusion. Although there are cross-references between chapters, the book reads more like a collection of papers on the topic of communities of practice in Old English and Anglo-Latin (Chapter 7 is a study of the language of William I's chancery) than a monograph on Old English sociolinguistics. The subtitle, "Records of communities and people" actually reflects more accurately the contents of the monograph. Although there are references to Mercian, Kentish and other dialects (see the chapter on diplomas (82–85) and the case study on wills (115–23)), the book focuses mainly on West-Saxon. "*Northumbrian*" and "*Kentish*" do not appear in the subject index.

The first chapter ("Sociolinguistic approaches to the study of Old English") offers a literature review of what has been done in the field along the lines of Nevalainen (2012) and (2015). Incidentally, the latter is not mentioned, although it is relevant to the study of communities of practice since it discusses the interaction between macro- and micro-sociolinguistic levels of analysis and recognises the relevance of the data provided by identifiable individuals and groups of people in a given period for studying real-time sociolinguistic variation and change (Nevalainen 2015, 246). Timofeeva briefly mentions the work done on language contact between Old English and Norse and between Old English and Brythonic (the Celtic hypothesis) and how the development of the West Saxon literary standard may have played a role in a supposed "delayed effect", which would have been responsible for the fact that some structures alleged to be of Celtic

origin did not show in the language until much later, in the Middle English period. On page 6 she refers to the “prescriptive role of the late West Saxon standard” and also to the Mercian literary language associated with the language of the Vespasian Psalter, which is—in my view—somewhat anachronistic, since there is no evidence that the scribes of the Winchester school had any intention of imposing a standard of any kind (any more than did Chancery clerks in the 15th century). The West Saxon literary standard was certainly influential (Toon 1983, 1992) and may even have reached the north (Jolly 2012; Fernández Cuesta and Langmuir 2019), but it was not a standard in the Milroyan sense of the term.

In the section on social dialectology, reference is made to research on both Old English sociolinguistics and other periods of the language. There are also sections on Old English text types and their suitability for sociolinguistic analysis and a brief but worthwhile and informative section on research done in the field so far. The only section that I find somewhat incomplete is the one on glosses. There is no mention of the work by Pons-Sanz on Norse-derived words in the Northumbrian glosses (2000, 2004), by Kotake on Mercian glosses (2010, 2012), or recent work on various aspects of the Northumbrian glosses by Cole (2014), Fernández Cuesta and Pons (2016) and Fernández Cuesta and Rodríguez Ledesma (2020). Cole (2014) is not mentioned, even though it is a quantitative/statistical work that is carried out employing variationist methods. There are also some errors. On page 14 we read that Aldred’s earlier gloss was “used by two other scribes, Farmon and Owun, to produce an interlinear translation of the Rushworth Gospels... into the Mercian dialect.” Aside from the fact that Kotake (2016) has recently questioned that the Rushworth Gospels was a copy of Lindisfarne, there are actually two distinct sections in the Rushworth glosses, one in the Mercian dialect (Rushworth1) and another in a variety of Old Northumbrian (Rushworth2; see Fernández Cuesta and Pons-Sanz [2016, 1, 6]). The last section of Chapter 1 is on social networks, which is the focus of the monograph. Timofeeva discusses previous research on the subject, notably Lenker (2000) and refers to her own publications on communities of practice in Old English (Timofeeva 2013, 2018). Missing here—to my mind—is perhaps a more thorough description of the contents of the monograph (the seven chapters that follow) and how they are related to one another. We are told that the book is about Old English genres and registers in relation to communities and individuals, where possible, and that it deals with sociology of language and social dialectology, more concretely with correlational and interactional sociolinguistics (4). We are also informed that the book, following previous research on the Alfredian circle, is about communities that specialised “in a limited number of genres” and that each chapter deals with one genre. On page 10 we are told that “the first chapter analyses the emergence of several lexical norms in vernacular statutory and nonimaginary-narration texts... in the late ninth century and their complex association with the circle of scholars and a scribal office at the Court of King Alfred” (I suppose that Timofeeva means the second chapter on “Social networks at the court of King Alfred”). Then, surprisingly, she refers to “the rest of the book” without specifying the contents of the six remaining chapters: “The rest of the book focuses on legal genres (charters, writs, and wills) and explores their correlation with professional and lay communities within the context of such institutions as the church [sic], local courts, and the royal chancery” (10–11).

Chapter 2 (“Social networks at the court of King Alfred”) is a revised version of a previously published paper. It aims to describe King Alfred’s community of practice, composed by a cluster of Mercian scholars, in order to assess whether it throws some light on “Alfred’s authorship.” Perhaps a better way to express it would have been “Alfred’s influence,” as Alfred was not really an author but a translator of works like *The Pastoral Care*. The case study comprises two lexical features originating in the Alfredian network (*Angelcynn* and *here*) and then disseminating to other areas of the country.

Chapter 3 (“Legal Old English and its communities”) analyses legal documents from a sociolinguistic perspective. Its aim is to reconstruct social networks between legal practitioners in the Old English period. It is argued that, in the absence of letters and other genres that might reflect individual usage, legal texts such as charters can be used, since they represent oral practices “even though these practices are highly ritualised” (52). This chapter also offers information about databases that can be useful for sociolinguistic research. Based on the information provided by *The Prosopography of Anglo-Saxon England (PASE)*, Timofeeva analyses the distribution of names, occupations and gender in the database (53–58) and offers an example of how it can be used to assess the involvement (or lack of it) of scholars like Ælfric in legal matters (59). She also describes other databases which can be employed for sociolinguistic analysis (the *Anglo-Saxon Charters Project*, the *Electronic Sawyer* and the *Dictionary of Old English Web Corpus*). In this chapter (63) we are also given a brief summary of the four chapters that follow: Chapter 4 on diplomas; Chapter 5 on vernacular writs and the emergence of a proto-chancery community at the West-Saxon royal court; Chapter 6 on wills and Chapter 7 on a Latin corpus of charters from the reign of William the Conqueror. I just wonder whether it might have been clearer to place the summary of every chapter in an introduction.

Chapter 4 analyses diplomas, which are in Latin for the most part, but contain English words or phrases in the boundary clauses and some are summaries in English. The aim is to reconstruct the social practices of legal communities. The study is mostly descriptive, i.e. it tells us about the elements that are characteristic of these documents (invocation, dating, donor, proem, etc.). As in the case of wills, Timofeeva highlights the variation found in these formulae, for example the various dispositive verbs used, and tries to determine some conditioning factors of their use. She states that “with vernacular documents ... the overall protocol remains very similar, even though the individual formulations of the component parts differ significantly” (68). As an example, she gives charter S33 from the reign of Æthelberht of Wessex. One of the findings is that in the middle of the ninth century, at the time of the expansion of West Saxon influence, Latin templates of the West Saxon diplomatic seem to be spreading and that by the beginning of Alfred’s reign (871), they can be found in documents preserved in various archives (Winchester, Shaftesbury, Sherborne, Abingdon, and Glastonbury), suggesting that there was a monopoly of the production of royal charters by a kind of “central agency” (70). The influence of Mercian scholars and advisors at the time of King Alfred is also found in the charters from that period.

One of the most interesting contributions of this chapter is the reconstruction of the social networks involved in the production of documents from the Alfredian period and how, from the witness lists, it is possible to reconstruct social networks (the king, bishops, dukes, lay nobility), in this case with West-Saxon affiliations (73). Although

hardly new, this is interesting from a sociological point of view. Yet it would have been useful if we were told in what way “the elevation of priests from the royal household to high ecclesiastical positions” may have “promoted the social practices (including linguistic ones) of the Winchester court, both in Wessex and beyond” (74). This may have been the case, but without a comparative study of such linguistic practices (before and after the Alfredian reform) this assertion is no more than a supposition, as the author herself recognises: “Since most of the property transactions were recorded in Latin, the medium controlled almost exclusively by the clergy, it is more difficult to reconstruct the language competence of secular participants” (80).

The case study on dispositive verbs, which concludes this chapter (81–85) offers a statistical analysis of the frequency of Latin dispositive verbs (*dono*, *largior*, *do*, *condedo*, etc.) by region. The conclusion is that the overwhelming majority of charters use the first person singular, about 9% the first-person plural, some alternate between the two and only 3% use the third person (mostly in the singular). This is interesting, not only in itself, but also for comparative analysis with the language of legal documents from other periods (Spedding 2010 and Probert 2022 on Old English manumissions). The distribution by rank is also telling: the first-person plural is employed by the king and sometimes bishops, although it is not common before the Conquest (84) and there is also variation according to region and archive, which leads the author to conclude that “there are significant regional differences between the diplomatic traditions of Anglo-Saxon kingdoms at around 900” (84).

Chapter 5 on writs sets out to show how standardised features were disseminated from professional clerks to common court members and how “writs maintained Old English etiquette norms and social hierarchies” (87). The corpus for this study is formed from writs taken from Harmer’s edition of *Anglo-Saxon Writs* (1952) and uses Bates (1998) for post-Conquest writs in Latin. We are given the distribution of writs according to language and status. I do not quite understand why the two writs of Queen Ælfthryth and Queen Edith, who were crowned and anointed as queens, are classified as “non-royal.” Even though Ælfthryth’s will was issued during the reign of her son and Edith’s writs were issued during her husband’s lifetime and during her widowhood, the two ladies were certainly of royal status and there seems no doubt about the authenticity of the texts. I think that parameters for inclusion/exclusion should have been given. As in the previous chapter on diplomas, this chapter describes the protocol elements that are characteristic of this genre: salutation, addressees, notification clause, main announcement, prohibition clause, valediction.

A case study on the salutation-notification template follows, which (we are told in a footnote) is a revised version of a previously published article (Timofeeva 2019). The linguistic variables considered in the study are the adverb that follows the salutation verb (*eadmodlice*, *freondlice*), the switch from first to third person, and the use of the honorific *leof*. Timofeeva starts by analysing these formulaic expressions in Alfred’s letter to bishop Wærferð in the preface to the *Pastoral Care* to show how the same protocol (the use in the salutation of adverbs that are dependent on the status of the addressee, *eadmodlice* to superiors and *freondlice* to equals) was later adopted in writs. This is illustrated by letters from Ælfric to his superiors (patrons), where he uses *eadmodlice*, and to West Saxon thegns, who are addressed by him with formulae containing *freondlice* (94). This case study is interesting, but I wonder whether analysis

of the distribution of this feature (and of the replacement of *ealdorman* by *earl*) is enough evidence of the “standardization of linguistic practices in official correspondence and for the application of a distinctive protocol, correlated with the status of senders and receivers to each other” (98).

Section 5.4.4. (“Writs by other social leaders” 98–101) analyses examples of writs made on behalf of queens in order to assess whether these documents follow “the protocols and conventions established in male legal correspondence.” The writ of Queen Edith contains all the expected formulae, as does that of Queen Ælfthryth (100). The only difference is the choice of adverb (*eadmodlice* instead of *freondlice*), which is interpreted tentatively as a change in diplomatic etiquette due to the fact that the Queen’s status has changed from that of queen consort to dowager queen (100). This is of course anecdotal. The motives could have been to show respect to one of the recipients of the letter (Archbishop Ælfric) on the part of a woman who was now spending most of her time at a nunnery.

Section 5.4.5 has the suggestive title “The incredible stability of royal writs” (103). Yet, shouldn’t we expect legal language to be stable and formulae to be adapted accurately from one language (in this case Old English) to another one (Latin and Anglo Norman), particularly in a period of transition? Initial formulae, which are the elements analysed in this chapter, tend to be relatively stable, whereas variation is generally found in the body of the will before the general acceptance of more standard forms. The function of these formulae is precisely to provide textual stability and avoid ambiguity. The idea is to possess a stable record of the legal act in question, as people need to trust that the text will be understood in the future. As Kopaczyk (2013, 184–85) states, “The requirements of legal discourse prevent lexical creativity and promote stability in order to inhibit manifold interpretations of the text.”

One of my criticisms of this chapter is that some of the conclusions do not follow from the evidence provided in the study. From the analysis of opening formulae in writs of royalty and ecclesiastics, Timofeeva concludes with a series of suppositions, which might contain elements of truth, but for which no evidence is given: “The emerging administrative channels *would* disseminate verbal and non-verbal practices of this small community of royal scribes to their colleagues down the chain of authority, from kings to chief magnates to smaller landowners to shires to hundreds. Eventually, the scribes at the royal court and at local assemblies *would constitute* a discourse community. Without necessarily knowing each other, *they would know of each other and of the shared accepted practices within the community of scribes... the community would also include* lay peripheral members...” (106, emphasis mine)

In the coda to chapter 5, we are told that Anglo-Saxon bureaucratic structures proved very resilient after the Norman Conquest, which is not surprising. They continued to be used because the incomers recognised them, and they were available as a ready-made bureaucracy. We are also told that “Official notices switched to English (now for good) in the first decades of the fifteenth century, as the royal chancery had abandoned the Latin medium although the new English template took some time to achieve a standard form” (107)¹. Yet Benskin’s study (2004), founded on an extensive and detailed

¹ It is not clear what the author is referring to by *official notices* (writs?). The term covers so many different kinds of texts as to be almost vacuous.

survey of the sources, demonstrated that Chancery was nowhere near abandoning Latin in the fifteenth century. Fisher et al. (1984) may have been the source of the error, but their flaws were exposed by Benskin (2004), as has also been recognised by later scholarship (Corrie 2006; Sobecki 2020, 256 n. 17).

Chapter 6 (“Wills: Variation by archive and gender”) is a study of Anglo-Saxon wills that correlates linguistic variation in this text-type with the variables *region* and *gender*. This chapter is an earlier version of Timofeeva (2022), recently published in *Studia Anglica Posnaniensia* (in the reference section of the monograph it appears as “forthcoming”).

Although there is a small number of wills made on behalf of women from this period (seven in total, which renders the *gender* variable susceptible of sociolinguistic analysis), caution should be exercised. In the Anglo-Saxon period wills were intended to be records of an oral act and we have no way of knowing whether they were written down after the fact or written ahead of time to be read out. None were written by the testators and, while oral traces persist, it would be hard to state categorically that they represent the actual words the testator spoke. Although testators would have been intimately involved in the content of their testaments, we need to take into account the various linguistic layers in this text-type, where testators, legal professionals, and copyists might all have been involved to a greater or lesser extent (Moore 2002; Fernández Cuesta 2014). This is particularly relevant in a period where, as the author says, “Only twenty-two wills (about a third of the surviving corpus) are preserved in manuscripts written before 1066, with many among them being copies of earlier documents. The majority are even later copies, surviving in cartularies, whose contents are abbreviated, and witness lists truncated” (109).

In the following pages (112–14) we are offered a description of the types and protocol of wills. Section 6.5 is a case study on the distribution of dispositive verbs, their complementation patterns, and the adverbial phrases *æfter POSS dæge* vs. *ofer POSS dæg* (“after his/her death”) (115) in wills before and after 900, considering the “archive” where they are kept.² The conclusion is that Bury St Edmunds stands out by showing a great deal of variation between individual wills and also within the same will. The results, when considering the variation of dispositive verbs by region, are interesting and show in a statistically significant way that *ge(unnan)* is characteristic of legal texts, whereas *sellan* is mostly found in other genres such as poetry and religious language (119). The distribution of *unnan* + genitive and innovative *unnan* + accusative shows that there is a preference for the older construction with genitive in the archives of Christ Church, Canterbury, whereas in Bury St Edmunds we find the newer construction with accusative as early as 942. Yet, it should be taken into account that if the will referred to is that of Bishop Theodred (Whitelock 2011 [1930], 2), the date of 942 is based on the date when one of the estates mentioned in the text was granted to him by the king. Theodred’s last signature occurs in 951, and *PASE* has his death at 951x953, so while the will must clearly have been written between 942 and 953, the date of 942 should at least be qualified and not relied upon to underpin suppositions about dates of language change. Moreover, this

² It is worth noting that the right translation is “after his/her day” rather than the often used, but less accurate “after his /her death,” which is the one used by Whitelock (2011 [1930]). In “The art of dying” (2022) Timofeeva alternates between “death” and “day” (“after one’s death” / “after my/his/her day” (122)) without explanation.

will is preserved in the Sacrist's register (14th century). Timofeeva concludes that "This Bury innovation is quite remarkable given that legal texts tend to be very stable in their use of grammar and vocabulary... and it may be the case that here regional innovation overrides genre convention" (120). Yet, the question remains whether the later copying of wills may be responsible for the choice of construction (I think that the possibility should at least be mentioned). Furthermore, Timofeeva states that the new construction with accusative probably originated in the spoken language, which might have been the case, although wills are not the only legal documents that originated as testimonies of oral pronouncements. On the other hand, the idea that this feature may have been perceived as a West-Saxonism and as such rejected by the scribes of other dialectal regions (122) is more far-fetched (is there any evidence?). Similarly, one should be careful in concluding that women's wills tend to favour the innovative construction, as the results are not statistically significant (123).

As regards section 6.6. ("Variation by gender," 123–26), it is concluded that women's wills are characterised by the use of formulae that request royal protection, and that they make use of more positive politeness strategies (humility formulae and the modal *motan*). The author hypothesizes the "[e]xistence of a template (probably also an oral one) ... in female wills, at least in Wessex, that reflected contemporary expectations of social hierarchy, gender roles, and politic behaviour" (126). I wonder whether there is enough evidence to suggest this. Since 38% of the male wills also included a plea for support or protection, then it was not a gendered characteristic, but had more to do with the absence of an executorial function or a formal probate process, although it cannot be denied that the complications of women's land holding and the property laws of the day made it practically expedient for women to have the backing of both king and church, and more so than for men. Comparison with studies of wills and testaments from the Middle English period (in Latin, French and English) would have been welcome here (see Spedding 2010). Similarly, it is also claimed that the reason why anathemas and the use of stronger and more emotional language are more elaborate in women's than in men's wills may be due to the fact that women felt that they were more vulnerable and had to protect the rights of their children against those who might try to violate their last decisions and post-mortem arrangements. Although it is certainly a possibility, I think that the sample is too small, a fact that the author acknowledges in Timofeeva's "The art of dying" (2022, 127). Unless a number of wills and these and other parameters are taken into account, the interpretation (women were more insecure about having their rights respected than men) is no more than a plausible hypothesis. It also assumes that women were responsible for the way in which their wills were drafted or that there was a template especially devised for women (see above). As Timofeeva herself states, curses become more numerous in wills of the eleventh century, so that it is possible that the wills analysed are following that trend. Without a study that considers other variables and, especially a time dimension, we simply do not know.

The chapter on wills finishes with a brief case study ("Sociolinguistic outliers", 134–38) which analyses two wills that do not conform to the norms and conventions of the genre and "can, arguably, be called sociolinguistic outliers" (134). The will of Leofgifu in favour of Bury-St Edmund is addressed to a queen, possibly Emma, as *hyre leuedi* (135) and uses other unconventional formulae that are associated with writs rather than with wills. According to Timofeeva, the reason that the donor violated the norms of the genre

in this case may have been her close relation with Queen Emma (she might have been in her service) at a time when the queen, now a dowager, was in a more vulnerable position (136). Yet a simpler reason might be that whoever wrote the will was used to writing writs, not wills. This case study shows the importance of considering the sociohistorical context in the interpretation of the choice of linguistic features used in texts and, in this sense, it could be considered a case study in “textual/literary pragmatics or discourse analysis” rather than a “sociolinguistic case study.” And the same applies to the analysis of Mantat’s will. “Sociolinguistics” is frequently used as a cover term for various disciplines such as discourse analysis, ethnography of speaking, geolinguistics, etc. (Trudgill 2003, 123), but it is important to make clear which approach and method is being used in each case.

The last chapter (“Mixed-language practices of William I’s chancery”) aims at documenting the changes in the royal chancery and the local courts during the first two decades after the Norman Conquest. It analyses the result of the involvement of Norman scribes in the English administration by studying the office of chancellor between the 1060s and 1100. Timofeeva’s corpus comprises sixty Latin writs produced in England between 1070 and 1087. The documents are written in Latin, which could lead one to question the suitability of this study in a monograph entitled *Old English Sociolinguistics*. The justification may reside in the interest of examining the multilingual situation in England in the late 11th century (a transition period) and the fact that half of these documents contain at least one English (or French) term (143).

The case study (“English terms in Latin acta of William I,” 143–52) is the revised version of a published paper (Timofeeva 2017). An interesting result, something that is also relevant for Old English, is that these Anglo-Latin documents contain lexemes that are not attested (or are very rare) in the Old English record (144). There is also a discussion about whether certain terms from English should be considered loanwords or instances of code-switching between Latin and Old English. Timofeeva analyses the degree of integration of the lexical items in the recipient language, i.e. whether they take the Latin case endings/gender or the English ones (148), as well as the possible reason for borrowing from Old English (150), aside from filling lexical gaps.

In sum, this is in many ways an informative monograph and definitely worth reading. The main problem is that it reads more as a collection of papers with a common thread than a work that has been planned as a monograph from the beginning. In the same way that there is no proper introduction explaining how the various chapters are connected, there is no conclusion. In Chapter 8 (“Epilogue”) we should have been offered a summary of the main findings, but instead Timofeeva discusses the methodology that should be employed when approaching Old English from a sociolinguistic perspective (175). This information should perhaps have been placed in an Introduction (or in the first chapter), after the literature review rather than in the Epilogue. In addition, some of the conclusions are quite obvious: “[T]he episodes of political centralisation were correlated with linguistic focusing and supralocalisation. Conversely, the times of political fragmentation and instability were correlated with divergence of common linguistic practices” (175). There are also various references to works which, again, would be better placed in the section on literature review (176). Finally, the Epilogue (three pages) ends too abruptly, and I wonder if the final paragraph about Ælfric’s lexical development is an appropriate one with which to conclude the book.

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