# Leeds Studies in English

# In reply to Dr Burton

## Michael Benskin

## 1. *Introduction*

Dr Burton has chosen a regrettable title for his paper. It is not about 'the current state of Middle English dialectology', but a critical review of a single work: the sole begetter of Dr Burton's misgivings is *A Linguistic Atlas of Late Mediaeval English*. The discipline neither stands nor falls by the publication of this atlas. Since the last century, it has been explicitly recognised that the history of the language cannot be understood without a secure knowledge of the dialects of Middle English, and there is a correspondingly extensive literature. Any review of the current state of the discipline would have to reckon not only with what has been achieved in the past, but with the contributions of present-day scholars besides the authors of the *Atlas*. Dr Burton makes no attempt to do this. To conclude, as he does, that the state of the discipline is 'parlous at best' (p. 189) does great disservice to the achievements of others,[1] and would be wholly unwarranted even if his worst suspicions about the *Atlas* were to be confirmed.

  This point will not be pursued, but it must be insisted that the *Atlas* authors have never thought of their work as sufficient unto itself, let alone as an end to inquiry. Each of us has said so, and plainly as we supposed, in the introductory sections of the *Atlas*:

> . . . it must be emphasised that what is presented in this atlas can be no more than a prelude to a very large enquiry indeed (I.vii, McIntosh).

> We reiterate: even on late Middle English, work has only begun. Like all exploratory scholarship, it is provisional, and subject to

revision (I.28b, Samuels).

> The *Atlas* is work on a large scale, and the labour of many years, but it remains provisional, not definitive (II.xiv, Benskin).

We wrote thus because we knew these things to be true, and in so far as Dr Burton complains that our work is incomplete, his complaints are superfluous. Had the necessary funds been available, the Middle English Dialect Project could have continued, and a more comprehensive job could have been done; and it would have been done against the continued carping of those who complained, almost as soon as they had heard of the work, that still the *Atlas* was unpublished, and who, though information was never withheld from them when they asked for it, were ever prepared to disparage us for 'holding back the evidence' and 'making *ex cathedra* pronouncements'.

In the following pages, Dr Burton's article will be taken on its own terms, as a selective review of the *Atlas*. His tone is avowedly combative, but he is beyond question sincere in disclaiming animosity (p. 189). In turn, I hope he will accept that there is no hostile spirit in this reply. Some of his criticisms are justified, and have long been in mind as perspectives for a revised version of the *Atlas*: it is less easy to use than it should be, additional material could well be incorporated, and we are naturally grateful for corrigenda. Much of Dr Burton's criticism, however, is misdirected or ill-informed. It is simply not true that we have suppressed evidence, isoglosses or otherwise. Sources that he impugns as primary evidence for localisation were never used as anything of the kind, and they afford no ground whatsoever for his belief that the southern material may have been misplaced by several counties; and that he cannot localise the dialect of the *Ipotis* fragment for himself, calls Dr Burton's philology into question rather than our own. Indeed, it has to be said that he would often have done better to refer to the handbooks than to hasten his difficulties into print.

## 2. *The making of the* Atlas

Samuel Johnson wrote that 'A large work is difficult because it is large, even though all its parts might singly be performed with facility'. It is also difficult

because it takes a long time: little by little, its author may depart from his early practices and intentions, so that the execution of the final parts is very different from what was accomplished at the start; and of all this, the author may be entirely unaware. These difficulties are compounded as the authors of a work are multiplied; and the *Atlas*, by the time it was published, had acquired three. In the final stages of codification, they were in Edinburgh, Glasgow and Oslo, respectively.

When the founding authors embarked upon this work, neither of them could have known how long it would take, or how large the atlas would become. Both accepted that it was a long-term project, but conceptions of 'long-term' differ: Samuels thought in terms of perhaps ten years to completion, whereas McIntosh was prepared to labour at it for so long before being finally convinced that the type of atlas he envisaged could be made at all. Neither seems to have been aware of what the other had assumed. At this remove, and given the time eventually taken to bring the northern material into publishable form, it may be hard to appreciate the circumstances in which Samuels worked; but he believed from the outset that McIntosh's work was already far advanced, and that he was therefore under great pressure to complete his own share of the survey on time.

In the event, the northern material proved intractable, even after extensive and systematic re-analysis of the sources; experiments with computational methods of reconstruction followed, which, on and off, were protracted over a decade; and the search for local documents continued almost until the work was in press. By the time the projected atlas was first brought to public notice, however, Samuels had all but completed what he set out to do. When I myself became involved, in 1969, the making of the English historical thesaurus had already occupied him for a full five years, even though much of his time was still taken up by preparing his material for conventional map-making and publication. (In the event, the methods finally adopted were anything but conventional.)

The idea that the southern material be published separately was raised at least twice, but Samuels took the view that such a course would weaken the atlas as a whole: the division between the northern and southern areas of survey was only operational, and indefensible from a linguistic standpoint. There was perhaps an argument for publishing the material from south of the Humber-Ribble line, but even here, parts of the area beyond Samuels's limit of survey were insufficiently worked out, and as late as 1983, they still remained so.

By the late 1970s, it had in any case become clear that for purely financial reasons, an atlas on the scale intended could be printed only by computational

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means, and the problems involved, both practical and theoretical, were very far from trivial; for large-scale production of the item maps, indeed, the hardware itself was not accessible until 1982. By then, Samuels's collections had been codified for nearly twenty years, but nobody could possibly have foreseen that the interval would be so long; and through all those years there were difficulties enough, without embarking on a major revision of the southern material. With hindsight, and to consider the matter in purely personal terms, Samuels ought indeed to have published separately: the southern survey would then have stood as the solid achievement of a decade, instead of being disparaged by standards that might have been attained in the course of thirty years.

The thrust of Dr Burton's criticism is otherwise to Samuels's disadvantage, because it suggests that his contribution to the *Atlas*, relative to the other authors, is less than it really was. It cannot be gainsaid that McIntosh did a lot more spadework, but the disparity is not as great as it seems. The more thorough treatment of East Anglia, and of the Midlands south of the Humber-Ribble line, is in part the virtue of necessity: as long as computational work on the northern dialects continued, and, when that led to an impasse, the search for local documents was intensified, McIntosh was free to devote his energies to the southerly parts of his domain; by 1972, if not before, the fully northern material had ceased to occupy him save incidentally. Dr Laing worked full-time for three years on Lincolnshire; Professor Celia Millward contributed notably to the analysis of the Norfolk material; and in close collaboration with McIntosh, I in turn contributed to the final revision of parts of the N.W. and E. Midlands. Samuels, by contrast, had no assistance of any sort. Nevertheless, during a sabbatical in 1980-81, he worked intensively for several months on the still-intractable literary corpus that McIntosh had amassed for the area north of the Humber-Ribble line, and he put the problems into an entirely new light.[2] The rather cautious reconstruction that appears in *Atlas* was finally my own responsibility, but it owes a great deal to Samuels's insight. Likewise, the suturing of the northern and southern surveys was very largely his work.

Samuels was responsible for nearly all of the southern analyses. The task was formidable by any standard, and given the pressure he believed to be upon him, it is hardly surprising that many of his analyses are the result of scanning. Paradoxical though it may seem, however, it is the LPs of the short texts that are the real casualties, and they are not a fair reflection of Samuels's work. Most of these are reports of local documents, and as will appear, his use of local documents was not the same as in the northern survey; he rarely attempted to record everything that was

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in them. As to his analyses of short literary texts, my own collations suggest that in expecting rather little from them, he recorded rather less. His analyses of richer sources are another matter, as will appear from the sample reproduced in Figure 1. Although Dr Burton had no reason to suspect it, therefore, his general criticism of the southern LPs is based on a sample that is seriously biased: his collations rest almost exclusively on very short texts. We do not seek to defend the omissions, though their effect, as will be explained below, is not the disaster Dr Burton implies; and many of the supposed omissions are in any case merely apparent.

Initially, Samuels's material was not prepared for publication in the form of LPs, though his original analyses were so made. For each item on the questionnaire, he compiled a separate list, showing the forms collected for that item, analysis by analysis. The following is a sample:

. . .

LP 5940 from
LP 5950 vram
LP 5960 fram
LP 5970 froo, fro, fram
LP 5980 from, fro
LP 5990 from, fro, fram

. . .

In preparing such item lists, he undoubtedly overlooked entries on his analysis sheets, which are hence missing from the *Atlas* LPs. None of these omissions, however, can have affected Samuels's localisations, because he worked exclusively from his original analyses.

For purposes of publication, the item lists remained the canonical form, and they were duly keyed into the computer. The data so registered were then corrected against the item lists, in two ways: (i) the item lists were keyed in anew, and then, by means of a computer program, collated with the original input; (ii) the emended data were then printed, and proof-read against the original item lists by Dr Laing and Dr Williamson. There seems nevertheless to have been the odd omission, though I find even that surprising. The material for the northern area of survey was entered into the computer in the form of LPs, which were checked by computer program in the same way as the item lists, and then proof-read by Dr Laing and myself against the original analyses.

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Figure 1

For the *Atlas*,\a consistent format was obviously desirable, and if that were to be accomplished, either Samuels's material would have to be converted into LPs, or the northern material would have to be converted into item lists. In terms of the relevant computational procedures, it was immaterial which we adopted; but since LPs had been recognised as useful for purposes other than dialect survey, we opted for LPs.

At this point, though none of us realised it at the time, Samuels was being put into a false position. He had not collected his material in the same way as that for the northern survey, but the main presentation now implied that he had; and the commentary on the LPs as it appears in *Atlas* was lifted wholesale, albeit at Samuels's suggestion, from a largely theoretical article reporting the later Edinburgh practice. It was also decided that the LPs in the *Atlas* should include only those items for which the record was intended to be comprehensive. For many items, however, Samuels was concerned only with certain forms: for example, whereas the northern questionnaire elicits 'answer' in whatever spelling, Samuels recorded only the *o*-, *u*- and *v*- variants, which for his purposes were diagnostic. Selective inventories like these now appear in Dot Maps (hereafter 'dm') 1115-1200 (I.541-51) and the Appendix of Southern Forms (IV.313-25), and the southern LPs give a correspondingly inadequate impression of Samuels's original analyses. Some of the reconstituted LPs, including those discussed in such detail by Dr Burton, would not have been printed had Samuels been able to see them for what they were: but for volumes III and IV of the *Atlas*, he never had proofs.[3] Had the material in these supplementary LPs been published only as part of the County Dictionary, or as modified item lists such as appear in the Appendix of Southern forms, then Samuels's work would have been shown in a much more favourable light. It would also have been much harder to criticise, something on which Dr Burton may care to reflect before enlarging his claims about the suppression of evidence.

## 3. The southern survey

Samuels's aims were threefold: (i) to establish a corpus of dialectal material for England south of The Wash, on as large a scale as the extant manuscripts would allow; (ii) to establish, so far as possible, the dialectal origins of the unlocalised literary sources; and (iii) on the basis of these localisations, to produce maps. The maps envisaged were vastly more detailed than anything attempted previously, but

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however radical the approach, Samuels's concern was to advance the subject, not exhaust it.

Localisation and mapping were operationally inseparable, and in the early stages the maps were therefore constantly evolving. When a potential source had been localised, it was incorporated in the maps, but the necessary trial mapping was itself an act of revision – a test of the configuration already established, besides a more detailed evaluation of the still-potential source. Once the configuration had become stable, however, and the density of coverage was fairly high, the work was in principle ready to be published. Beyond that, it could have continued indefinitely: until all of the extant writings had been analysed to the last syllable, there was no theoretically-determined point at which work ought to stop. The evolutionary character of the *Atlas* maps must therefore be clearly understood. The overall pattern has long been stable, but alteration in some details can be expected with the placing of any substantial text.

Samuels's immediate difficulties lay in working out an appropriate questionnaire. The early version, used also by McIntosh, proved inadequate. Samuels extended it, and as the work continued, he was increasingly able to recognise the diagnostic value of particular forms. These were progressively incorporated into his analyses, as a regular part of the questionnaire; but this enlargement, confined to the early stages, was cumulative, not a once-for-all decision. This is one source of unevenness in the southern LPs; that some of the early analyses were incomplete by Samuels's later standards had simply not been recognised. Without re-analysing the original texts, it is hard to see how this could have come to light. That an item is not recorded from a given text is not in itself ground for suspicion: it is possible to read a great deal of Wyclif without meeting 'although', and absences may cluster fortuitously, as with 'but' in the Cheshire and S. Lancashire documents (II.244).

Moreover, some of the items themselves evolved in the course of time. For example, in the northern analyses, 'ought' eventually elicited 'is under obligation' (which includes *ow(e)th* et var.), as well as the originally intended reflexes of OE *āhte*; and the former could not be reliably separated without re-analysis, given dialects having *-th* from OE *-ht*. The item had therefore to be omitted altogether from the *Atlas* LPs. Dr Burton notes the omission of *was* from LP 6310 (p. 206), and is right to do so. Samuels had initially collected only the *wes* and *wos* types, and otherwise unusual forms; but *was* gradually infiltrated the record, and that it had not been collected in the early stages was forgotten. Hence the *Atlas* record of

southern 'was' is properly a record of non-*was*, but we failed to realise it until after the work was published. So also, 'had', as we now see, was at first a record of the *hed(-)* type.

Some inconsistencies are the result of imperfect collaboration. The notes on the *Atlas* questionnaire suggest otherwise, but the suffix of the noun plural was systematically collected only for the northern area. In the south, -*es* was of no obvious value for localisation, so only the non-*es* forms were collected; the non-syllabic suffix, consonant or stem vowel + -*s*, was similarly ignored. These now inflate Dr Burton's lists of forms that Samuels is supposed to have overlooked (pp. 197-206), and they do not belong there. For 'each', Samuels did not collect the *euery* and *euerich* types at all, whereas McIntosh seems to have recorded them systematically. I certainly did so, and they therefore appear in the LPs for Bristol (9530-9610) – which discrepancy emerged only in course of writing this paper. It is no fault of Samuels that their southern distribution is now misreported, and he overlooked neither *euerych* in LP 5030 (p. 197) nor *euerych* in LP 6030 (p. 204).

In some respects, as is now clear, the Notes on the Questionnaire (I.554-6, etc.) are an inadequate guide: some of the alleged omissions are of items Samuels never intended to record. Thus the *ȝoue* that Dr Burton wishes to add under 'gave sg.' in LP 5950, has not 'slipped through the net' (pp. 174 and 203): the form in question is 2 sg. (Carleton Brown's text reads 'Þou ȝoue hym souken of þi brest'). The root vowel of strong verbs in the 2 sg. pret. is historically that of the pret. indic. pl., and 'pret. sg.' is conventionally understood to include the 1 and 3 sg. only. (OE *g(i)efan*, class V, has 1 and 3 pt. sg. *gæf/ge(a)f*, but 2 sg. *gǣfe/gēafe* and pl. *gǣfon/gē(a)fon*.) The fault here is perhaps not entirely Dr Burton's, but neither has Samuels ignored what he should have recognised as a rare form; as noted in the questionnaire (III.xviii/137) and the County Dictionary (IV.183b), 'gave pl.' was not collected for the south. So also with 'yield', corresponding to OE *g(i)eldan*, etc., a strong verb of class III. *Atlas* cites only the present stem; philologists do not normally lump together the principal parts of strong verbs, and it had not occurred to us that we should provide a note to that effect. Had past tense forms and the past participle been recorded, they would have been duly segregated: Dr Burton's *i-ȝolde* ppl. was not left out of LP 5950 by mistake (p. 203), nor does his *ȝeld* (pt. sg.) belong to LP 6310 (p. 206). Our notes on 257 'witen 1 & 3 sg. pres. indic.', however, are clear enough: it was collected only for the north, so Dr Burton could have spared us *wot* in LP 6070 (p. 204). He rightly points out that *goudes* 'goods' is missing from the same LP, but it was duly entered in the original analysis; Dr

Burton's own list omits simplex *goud* (71.25) and *good* (71.21).

To return to LP 5950. Samuels certainly overlooked *ffless* 'flesh', which should have been entered as *fless*: what editors report as *ff-* is commonly *F*, and in the LPs, capitals are not normally distinguished (III.xiv-xv, etc.). The oversight is regrettable, but it may be less damaging than Dr Burton fears. The source manuscript for LP 5950 is Bishop Sheppey's collection, Merton College, Oxford, MS 248; it has non-linguistic associations with Rochester, which is where the LP is entered on the maps. At Rochester, *fless* looks isolated (IV.174c); it is less so than appears. East from Rochester, the nearest sources are entered at Canterbury, where, in Dan Michel's *Ayenbite*, will be found *uless* (LP 5890). That is the Rochester form in all save the initial consonant: *fless*, far from calling Samuels's placing into question, reinforces it. Lest Dr Burton object that the forms are not identical, be it noted that not all writers from the Land of Vee were consistent in writing *v* or *u* for historical initial *f*: the Rochester text has *vram* 'from' (x8), and a companion piece has *uendus* 'fiends';[4] Bishop Sheppey's autograph (LP 5940) shows *atte vollen* 'at the full', but *from* 'from'.[5] Still in LP 5950, *þanne* and *þan* for 'then' are ghosts: we are indebted to Dr Burton for the correction. These forms belong to the next item, 'than', from which they were duplicated in editing, but they are not in Samuels's original analysis, and so have not affected any of the *Atlas* placings.

From LP 6030, Dr Burton (p. 204) alleges the omission of *suche* and *myche*; but the LP has *such* and *suche*, *mych* and *myche*, and as explained under Editorial Practice (III.xvi), the cross-stroke through *h*, commonly otiose, was not expanded as an abbreviation. Some of the real omissions are hardly serious: Samuels missed *þan*, but not *þan*; so *whan*, but not *whan*; so *to-for*, but not *tofor*; so *ȝate*, but not pl. *ȝatis*. Other forms were recorded in the original analysis, but lost in codification: *wyrk* and *werkis* sb. 'work(s)', *ded* pl. 'did', *sawe* pl. 'saw', *synnis* sb. pl. 'sins', *ȝedyn* 'went'; so also *from* 'from', which appears in the LP as *from*. Their omission from *Atlas* is regrettable, but again, it has not affected the localisation.

In sum, Dr Burton's criticism of the southern LPs is not nearly so damaging as he has supposed. His targets are LPs based on very short texts, and these are not typical. The alleged omissions are very largely of forms or sub-items that Samuels did not intend to record: in so far as *Atlas* suggests the contrary, the Notes on the Questionnaire must be revised. Some forms significant for localisation were omitted from the *Atlas* by editorial oversight, but only one or two of those reported by Dr Burton are missing from Samuels's original analyses; it has yet to be shown that any of them could have affected the placings in *Atlas*. The real omissions are otherwise

of forms that Samuels regarded as devoid of diagnostic value, the type of variant that Dr Burton himself once advocated excluding from the record;[6] and this is so even with LP 5160 (pp. 198-99), brief notes from a defective source, which ought never to have been published.

Of the main literary sources, Samuels's analyses were not exhaustive, but he never pretended that they were; and there have clearly been some losses at the editorial stage. As a representation of distinctively local usage, however, and in terms of his own questionnaire, the general run of his analyses can be defended. LP 6400 is typical. Dr Burton notes its omission of three items, but continues: 'For diagnostic purposes, no doubt, this does not matter: there are quite enough items to place the text(s) by means of the "fit"-technique' (p. 187). This is a fair assessment, and it reflects Samuels's aims.

Samuels's work will no doubt be consolidated in due course; the last section of this paper, on the Salisbury-Wilton complex, is a token first instalment. It must be said, however, that where Samuels's analyses are less full than they should have been, the effect of re-analysis has so far been to confirm his placings, not to discredit them. An incomplete record is one of the costs of working under extreme pressure, and Samuels believed himself to be so. He sought features of diagnostic value, not so much items as particular forms, having realised early on that the standard repertoire was no sufficient basis for recovering the patterns of the past. Against this incompleteness must be set the product of intensity, Samuels's extraordinary grasp of the whole: it could never have been attained by a committee, or by an individual working piecemeal and at leisurely pace. In a long association, I have not seldom had cause to question the economy by which Samuels arrived at a placing; but laborious re-working has too often led to the same result for me to believe that his conclusions can be lightly set aside.

## 4. Documents and anchors

The use of local documents, especially in the southern survey, calls for further explanation; the account given in *Atlas* is based very much on the northern material. Originally, Samuels had not intended to incorporate local documents in his final maps. Literary manuscripts were by far the richer source, and if they could be localised, the documents would be redundant. Why enter a solitary *vch* from a local document, he held, in which the word 'each' appears only once, when a literary

manuscript from the same place has a dozen instances apiece of *vch*, *vche* and *euche*?

Dr Burton would doubtless reply that the document is an essential part of the evidence for localisation, and on that we may agree; but the maps were envisaged as a conclusion, no matter how provisional, not as a do-it-yourself kit for making an alternative atlas. The documents laid under contribution have been listed, and anyone minded to do so is free to analyse them for himself. In principle, Dr Burton is of course right to urge us to publish all the analyses. Given the manpower, we might have done so, but preparing the thousand or so LPs we did publish was the labour of several years; and the closing stages were a fearful struggle, dependent on key-to-disc operators who loathed the work and did their best to evade it altogether. Since then, the computer world has changed beyond recognition and the necessary keying-in could now be organised fairly painlessly; but the original analyses are not generally intelligible, and much editorial work would be needed to make them so.

Regardless of practical considerations, it is still questionable how far this material ought to be published: in some ways it could prove highly misleading, as appears already from Dr Burton's critique. Especially in the southern survey, the documentary analyses are very uneven. There is good reason for this: by the time southern documents in English are found in any quantity, the administrative language of the capital has already begun to infect them, and for dialect mapping they are of only limited use. Accordingly, their distinctive forms were noted, but not the commonplace, and the records so made are therefore links between forms and localities, not representations of the texts. (The same is true for some hundreds of documents examined in the northern survey: detailed notes were made on slips, but no formal questionnaire was completed.) It is unfortunate that Samuels had lost sight of this when, at a late stage and intending to harmonise with the northern survey, he added some of his documentary analyses to the maps. Dr Burton is right to observe their deficiencies: they were merely supporting evidence, selective lists that ought not to have been presented as LPs. Their inclusion is also an inadvertent breach of a strongly-held principle adopted at the very beginning of the survey, namely that any source entered on the maps had to be taken as a whole; selecting its forms to suit the prejudices of the authors would be tantamount to cooking the books. This was a further reason for excluding so many southern documents: their local element could be incorporated only at cost of submerging the regional dialect pattern in their standard or near-standard forms.

From the foregoing it will be seen that 'anchor texts' are not the well-defined

category that Dr Burton has supposed. The language of a given document may be fairly standard, but contain one or two features that are highly distinctive, and of considerable value for localisation. Should we, in the Index of Sources, have marked such a text as a primary anchor? Had we done so, would Dr Burton have accepted our classification, or would he have complained that the text could not possibly be an anchor because its language was obviously standard? How non-standard would its language have to be before he accepted it without demur? Short of printing a full analysis, and marking the one or two forms believed to be local, I see no way of meeting his likely objections. In principle, I agree with him that there is a case for presenting full analyses of all the documents; in the circumstances, the lists and annotations in *Atlas* were perhaps not such a bad compromise.

Even had we printed a complete and impeccable inventory for every document listed, the criticism would still not be at an end. Since Dr Burton will take nothing on trust, the inventories would have to be accompanied by a mass of detailed commentary: the language of a document cannot be accepted *in vacuo* as belonging to the place where the document was made, but has to be assessed in terms of what surrounds it. Dr Burton does not understand this. He quotes my account from *Atlas* I.41-2, and finds the reasoning circular (p. 173):

> How does one have 'general expectations' for a given area until one has local documents recording its language? And how does one know that such documents truly reflect the language of the area in question unless one already knows what kind of language to expect? Unless, in short, one falls back on the old generalizations (Northern, East Midland, West Midland, etc.), one has nothing whatever to go on.

Circularity troubles him, but not, it appears, regress: how does he think that those old generalisations were ever arrived at, if not by assuming at some point that associations between texts and places could be trusted?

Dr Burton does not refer here to *Atlas* I.45-7 ('Documents: date and place of origin'), where I had thought to make clear that the procedure is not circular, but reciprocal:

> Since, for the most part, regional dialect changes in an orderly way over space, the placing of a document that disrupted the

linguistic pattern already established, and did so for a large number of its features, would be inherently suspect. No matter that the document was written in that place: its *language* could not be assigned there, since it failed to cohere with the pattern established by a consensus of other documents, each of which had no less good a claim (in terms of dating clauses or other diplomatic evidence) to be considered as representing the language of that area. That people moved about is in no way contested: it is of course to be expected that some, perhaps many, local documents were written in places far removed from their writers' home ground. The question to be answered is 'Which are the likely candidates?'; and the dialect map itself is a powerful tool for identifying them.

Perhaps an example would help.[7] Consider the document that is now Durham, Prior's Kitchen, Dean and Chapter Muniments 2.16 Spec. 37, mm. 2-3. It is a Durham chancery enrollment of a letter (14 October 1454) by Robert Neville, bishop of Durham, to William, Lord Faucomberge (his brother), Sir Thomas Neville (his nephew), Robert Danby, justice, 'and other our Iusticeʒ at Duresme'; it concerns a case pending 'in our Court at Duresme', between the bishop and Richard Danyelle 'of Duresme', over a right of way in Durham. The letter was not, however, dated at Durham, but given under the bishop's privy seal 'at Auklande', the modern Bishop Auckland, nine miles to the south-south-west.

I do not believe this text to represent the language of either Bishop Auckland or Durham. Some few of its features are characteristically northern, though they are not exclusively of northern origin: *thair* 'their', *arr* 'are', *thare* 'there', *geuen* 'given', *knaw-* 'know'. Against these, *(þe) whuch* 'which' and *wul* vb. 'will' are not, so far as I know, found in northern documents by any other hands. Similarly out of place are *ichon* 'each one', *eny* 'any', *nat* 'not', *yeuen* 'given', *-eth* and *-ith* 3 sg. pres. indic.; and in varying degrees, so are many others. If this language is native to Co. Durham, then we must suppose an enclave, a disruption in the regional dialect pattern. Such things happen: colonisation is the obvious cause, though evidence for the appropriate type in medieval Durham is otherwise lacking. The supposed enclave has left no trace in the modern dialects; but such an enclave would have been rather small, perhaps too small to survive, in view of the absence of these forms from so many other documents no less strongly associated with Co. Durham.

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It could be countered that these other documents are the work of immigrants, that our enrolling clerk was the only Durham man among them. But if that be so, where does the language of all the immigrants belong? Why, if it does not belong to these parts, does it form a continuum with the documentary language of Northumberland, Yorkshire and Cumbria? I cannot see the beginnings of an answer, unless the regional dialect pattern of medieval England differed utterly in kind from any dialect continuum known today, in which case we may as well give up trying to recover it. As for the existence of a colonial enclave, the language of one clerk in an ecclesiastical bureaucracy is hardly compelling evidence.

Indeed, it is not even remotely persuasive, for it is readily explicable in other terms. The anomalous *whuch* and *wul* are attested in documents dated from more southerly parts, particularly the S.W. Midlands, where they keep company with *eny*, *nat* and the rest. These *whuch*-assemblages recur independently in many literary manuscripts as well, and are clearly genuine *états de langue*. Dialectally, they form part of a continuum, and it is not northern: quite apart from the diplomatic evidence, were this complex placed in the north, it could have no organic connection with any of the surrounding material. In the Midlands and the south, by contrast, it fades gradually into other parts of the continuum, for which the weight of the diplomatic evidence likewise points to non-northern origins.

I prefer to believe, therefore, that the Durham enrollment is the work of a man who acquired his habits of written language largely or wholly outside the Durham area. If this language is genuinely local, if the northerly-looking features are part and parcel with *whuch* and the rest, then origins in the N. Midlands are to be sought: comparable assemblages appear in one or two dialects from N.W. Derbyshire, and from the northern border between Derbyshire and Nottinghamshire. Pending more detailed analysis of other writings in this clerk's hand, there is at least a case for ascribing his language to these parts. It is possible, however, that his language is mixed, the northerly forms belonging to the Durham area, the others to the S.W. Midlands or beyond. It would not be extraordinary if a southerner, settled in Durham, had picked up a few northernisms; equally, northerners have been known to return from southern parts, and to bring largely southernised language back with them.

As it happens, there is diplomatic evidence that our man did travel: Locellus XXV nos 58 and 68, letters in the name of the bishop of Durham, are in the same hand and similar language, and dated from London. None of this affects the linguistic assessment one jot, and there is no case for regarding his southerly forms

as of London origin. These, and likewise the northern elements, are to be evaluated in terms of the whole configuration. We look for coherent distributions of the kind found in dialect atlases for living languages, and we do not assume that the great majority of the documents for a given area, London apart, were written by outsiders. If Dr Burton still thinks the procedure circular, perhaps he would refer to the writings of Hennig and Hull as noted in *Atlas* III.xiii; they were cited in the hope that they would be read.[8]

Some of the non-documentary sources used as primary evidence for localisation will be discussed in section 11 ('Salisbury and Thomas Cyrcetur') below. As will appear, Dr Burton's criticism is far wide of the mark.


## 5. *Chaucer and dialect maps*

Dr Burton complains that *Atlas* represents the Ellesmere and Hengwrt manuscripts of *The Canterbury Tales* by a single LP, and insists that it is an open question whether they are in the same hand (p. 186). True, we cannot prove beyond doubt that they are so. No more can Dr Burton prove that either manuscript is not the work of two or even a dozen scribes: if two scribes indeed wrote identically, then as a matter of definition we should be unable to tell their work apart. 'Scriptores non multiplicandi sunt praeter necessitatem'; apparently, Dr Burton thinks R. V. Ramsey's arguments[9] constitute *necessitas*. Experts in the handwriting of the period, however, have concluded that the hands of Ellesmere and Hengwrt are the same,[10] and Samuels has set out, in considerable detail, an independent case for their identity, based on dialectal criteria.[11] We built on what we believed to be the best hypothesis; we were not bound to supply people of other persuasions with the means for re-making the *Atlas* in their preferred image.

Since Dr Burton appears to believe that a user of the *Atlas* could resolve the issue merely by comparing separate LPs for Hengwrt and Ellesmere, it is hard to see that he has understood the terms of the argument at all. To say that we have 'prejudged so important an issue by creating a hybrid LP for a non-existent text' is absurd: neither the combined LP nor the *Atlas* is an attempt to answer this question, and regardless of how many scribes are involved, the dialects of Ellesmere and Hengwrt are so alike that they cannot well be separated on the maps. Indeed, when Samuels published his first account of the London dialects, he had assumed them to be by *different* hands:[12] the palaeographers' subsequent demonstration that they are

the same has had not the slightest effect on the dialectal placing. On the item maps, it is true, Ellesmere and Hengwrt could have been represented individually, with two text blocks attached to the same point; but the cartography for the London area was already sufficient of a challenge. For the dot maps, such segregation is immaterial: regardless of the number of LPs, there can be registered one dot and only one at any given point. As evidence for subsequent localisations, the use of combined as opposed to separate LPs is likewise of no effect: the criterion is *whereabouts* the relevant forms are to be found, not the number of sources used to establish their presence in any particular place. The logic of Dr Burton's last sentence in his Hengwrt-Ellesmere paragraph escapes me altogether, but in view of Samuels's published work on these manuscripts, the claim that we have suppressed the evidence wears rather thin.

Dr Burton's general criticism of our handling of the Chaucer manuscripts betrays fundamental misunderstanding about the aims and limitations of regional dialect survey. In principle, copies of Chaucer's writings are here of no higher status than are manuscripts of *The Prick of Conscience*, and in so far as Chaucer's *auctoritas* restrained his copyists from thorough-going translation, *The Prick of Conscience* is frequently the more valuable source. If Dr Burton were true to his convictions about the value of localised holographs, then he should be arguing not for Chaucer but for Thomas Hoccleve.

The idea that *Atlas* might 'act as a register of linguistic usage for the manuscripts of major authors' (p. 187) is similarly misguided. What possible justification is there for confining a register of their linguistic usage to those items that happen to have been used for a dialect survey? And why, given all the manuscripts of this type that are unusable for dialect mapping by reason of their mixed language, should anyone look to a dialect atlas for such a register in the first place?

## 6.  Localisation and classes of forms

The dot maps in *Atlas* commonly show the distribution not of a single form, but of a class of forms: so for 'she', dm 16 shows the combined distributions of *hee* and *he*, dm 18 the combined distributions of *hi(j)* and *hy(e)* (I.308-09). Dr Burton disapproves (pp. 179 and 183). Only the dot maps, however, invoke such categories: the separate forms are registered on the Item Maps, in the LPs, and in the

County Dictionary. If a map is wanted for some form elicited by the survey questionnaire, but is not available in *Atlas*, it can be made from either the LPs or the County Dictionary: there is no category {hij + hi + hy + hye} on the questionnaire.

Such categories are obviously interpretations, but even authors of atlases are not debarred from interpreting their material. Ours is written Middle English, and we have sought to understand it as written language. So it seems, has Dr Burton, but his view of it is not the same. For him, written forms are discrete entities: he never considers their rationale. Thus *hi* has nothing to do with *hy(e)*, and *willen* has nothing to do with *wilen* or *wyl(l)en* (p. 179). This version of graphemic theory, a pretence that there are the *figurae* and nothing more, seems to me to be an obfuscating dead end. The letters of the medieval roman alphabet are culturally-invested symbols, they have a history, and they have names. Their history informs their use, as also do their phonic implications. In ways that speech is not, writing is subject to design: analysis must take account of the doctrine of *littera*, of the conceptual categories of the designers.[13] The evolved orthographies of the later middle ages, moreover, may have extensive grammars of interchange, the cumulative and partly systematised legacies of sound-change and calligraphic development. Middle English spellings do not exist *in vacuo*: they are products of a generative system.

The *Atlas* authors have variously held that written Middle English is not phonetic transcript. The sound-pattern is not directly known, but has to be reconstructed – from, among other things, written forms interpreted in the light of the particular spelling systems to which they belong. Pronunciation is an object of discovery, not a premiss: assumptions about the way (or ways) in which a written form was pronounced, ought not to be built in to the collection of the primary evidence.[14] It does not follow that phonetic considerations are ruled out for subsequent interpretation; indeed, to advance the study of Middle English phonology was one of the reasons for making an atlas in the first place.

Dr Burton seems not to understand this. 'The theory on which the *Atlas* is founded', he claims 'insists that all spelling differences are significant' (p. 179). It is unclear to me what they are all supposed to signify, but those are his words, not ours: the theory of written language that does underlie *Atlas* is set out in I.5-7, and it is not recognisable here. Dr Burton's contribution of 'significant', indeed, is one source of his difficulties. 'What', he continues '*makes* some spellings less significant than others?' (p. 179).

Unfortunately and once again, there is no simple or programmatic answer.

For example, the writing of <y> for 'þ' is a regional trait, and it was discovered to be so largely by accident. The most economical statement of the evidence is the map.[15] Until it was drawn, we had only an unconfirmed suspicion, and it would have been no cause for wonder if a chequer-board pattern had unfolded after all. As it now appears, however, the use of <y> for 'þ', particularly in anglicana scripts, betokens a writer from northerly or eastern parts; whereas in southerly and westerly areas, 'þ' and 'y' are normally distinguished. This affords a useful diagnostic feature for textual criticism as well as for localisation. By contrast, the writing of *off* for 'of' seems to have no regional pattern at all, at least over England north of The Wash. I do not know why this should be so, and I know that it is so only because I have recorded 'of' in several hundred local documents. No doubt there are authorities who could have told me at the outset that collecting it was a waste of time, and no doubt they would have said the same about <y> for 'þ'. Other than by recording and subsequent mapping, it appears, there is no organised way of finding out which forms have cohesive distributions and which do not.

It is sometimes hard to understand the basis for Dr Burton's criticism in these matters. He says we have regarded 'very slight differences in spelling' as 'crucial', and then cites *euch* versus *uch* as one of them (p. 179). Is he unaware, perhaps, that these forms are conventionally regarded as having different etymologies, *euch* stemming from OE *ǣghwilc* et var., *uch* from OE *ylc*?[16] Does he think *eu* and *u* normally interchange as free variants in Middle English, or that the textual and regional distributions of *euch* and *uch* are the same? Admittedly the forms differ by only one letter, but then so do *twa/two* and *goþ/gon*: does Dr Burton regard these differences as very slight? And if not, why not?

From individual forms to categories once more. Philological judgement is again at issue, and again Dr Burton is uneasy: 'if we accept that we must deal in categories of forms . . . we have still to decide *what constitutes a category*' (pp. 179-83). There is no one criterion, and as to the good sense or otherwise of setting up any particular category, opinions may differ. The rationale is frequently phonological, as could (but need not) be so for the 'she'-variants *hy(e)* and *hi(j)*: it is a reasonable hypothesis that these spellings corresponded to a spoken form /hi:/, as opposed to the /he:/ implied by *he* and *hee*. Had they been more abundantly attested, as they appear to have been in earlier Middle English, local preferences for one or other spelling might have emerged; *Atlas* records them from a mere nine localities, and statistically significant patterning within the class is not to be found.

Dr Burton seems to think that *hi* of the *Ipotis*-fragment (p. 183) should be

compared only with other *hi*, and that the distributions of *hy(e)* and *hij* have no bearing on the localisation. Against this, we note that the spellings *hi, hij, hy,* and *hye*, not infrequently interchange. So LP 6510 has *hye* and *hij* for 'she', *hij* and *hye* for 'they'; LP 5890 has *hi* and *hy* for 'she'; for 'they', LP 5960 has *hi, hy,* and *hie*; many more examples could be cited, from early Middle English as well as from *Atlas* sources. These forms are not mutually exclusive; rather, they go together. The *Ipotis*-fragment has only three instances of 'she', spelled *hi, hee* and *he* respectively; 'they' is not attested. We do not think that this one occurrence of *hi* amounts to evidence that *hij* or *hy(e)* were unknown in the copyist's dialect, and in attempting to localise it, we prefer to take the distributions of these forms into account.

## 7. Localisation and the survey questionnaire

Different versions of the questionnaire were used for the northern and southern areas of survey, but that is no ground for Dr Burton's vicarious dismay: the 'initial questionnaire' needed to dispel his unease (pp. 184-85) is staring him in the face. 'Researchers who wish to establish the provenance of a text not treated in the *Atlas*' will consider first those items common to both versions of the questionnaire: there are over ninety, sufficient in most cases to determine whether the language of a text belongs to the northern or the southern sector. In no event need researchers be misled by Dr Burton's extraordinary claim that they must first know whether 'the text in question is *obviously* northern or *obviously* southern on traditional grounds' (pp. 184-85): if in doubt, they will complete the questionnaire in its full form (not two separate versions). It would be no great hardship for them to complete the full questionnaire at the outset; and to anticipate Dr Burton's likely rejoinder, whereas they have only one or two texts to analyse, the makers of the *Atlas* had some thousands.

The use of different questionnaires in making the *Atlas* was operationally sound. For example, as criteria for localisation within the southern area, the reflexes of OE $\bar{a}$ are not notably informative; as discriminants within the northern area, 'pride'/'hide'/'bride' are similarly unpromising. Now the source material has been localised, it is of course frustrating that for so many items only half the country has been mapped. (This has been a particular handicap for work on the colonial dialects, the English of medieval Ireland.) Though the founding authors may not agree, I

think it desirable to extend the analyses to include all items (including those implied in the Appendix of Southern forms) for the whole area of survey. But the work involved amounts to several man-years, and it would have been little less even had it been attempted early in the making of the *Atlas*; perhaps Dr Burton would have preferred to wait?

His other comments here are ill-informed, and the presentation is less than fair. He complains, reasonably enough, that 'hill' was not collected for the north; but when he says that *hill* and *hyll* were recorded from only seven counties, and then refers to *-i-* and *-y-* variants generally (p. 185), a reader may well infer that the southern collections are defective as well. Reference to the County Dictionary, however, will show that *-i-* and *-y-* variants were recorded copiously, from another fifteen southern counties (IV.198b-c: *hil, hill-, hille, hyl, hyll-, hylle*). In supposing that northerly 'hill' has only *-i-* and *-y-* forms, Dr Burton is merely mistaken, as he would have realised had he consulted Dr Kristensson's work.[17] Other forms are found, and they are doubtless 'worth collecting';[18] the word is one of scores that could have been used in the survey, but were not. Until such time as the material for 'hill' has been collected, we are in no position to state what forms are the rule anywhere, and it would be no service to users of the *Atlas* for us to pretend otherwise.

In his further comments on the questionnaire (p. 185), the generally delusive character of Dr Burton's reasoning emerges into harsher light. He correctly quotes us to the effect that 'most' of the first sixty-four items were collected for both parts of the survey. He thinks, however, that 'this plan appears to have been abandoned, since now, even among the first sixty-four items there are some that are collected for one part of the country only'. Not so: 'most' does not mean 'all'. If 'most' of the sixty-four items were collected for both parts, it follows, and without any intervening change of plan, that 'some' were collected for only one part. A would-be historian will have to do better than this.

For the record, in the present arrangement of the questionnaire, the guiding principle was taxonomic efficiency (so III.xi, 6.5). Of the first sixty-four items, fifteen are collected for one part of the country only, but in the remainder of the questionnaire will be found a further forty-two items collected for the whole. An atlas based on these alone might have been thought comprehensive; ours adds regional maps for nearly two hundred more, and it is found wanting in return.

## 8. Localisation, and some aspects of dialect maps

Distributions have to be interpreted, both as spatial patterns and as philological record. It is hard to see how things could be otherwise, but Dr Burton is variously ill at ease. Thus on p. 177 he writes:

> As soon as one gets to literary texts, the problems begin. One is all the time making judgements about which conflicting pieces of evidence to believe. At what point can one say, for example, 'This text has "schoy" for SHE, which is recorded only in the West Riding of Yorkshire, but I'm not going to put the text there because the other evidence favours Lincolnshire'?.

If the text is ample, if its language is internally consistent, and if each of its forms can be assigned to the same one place in Lincolnshire, then there is good reason for thinking that *schoy* may belong there as well. If, not counting *schoy*, the dialectal assemblage can be placed there and only there, then either (i) *schoy* is a Lincolnshire form not previously recognised, or (ii) *schoy* does not belong to the assemblage at all, and it is found only by accident in company with Lincolnshire forms. If *schoy* appeared only once or twice in the text, and if for other reasons a Selby (WRY) exemplar was likely, then the status of *schoy* would obviously be in doubt; and so also if *schoy* looked like a slip of the pen. But otherwise, why should we balk at regarding such a text as evidence for the use of *schoy* in part of Lincolnshire? Granted it is another county, and the county boundary is to Dr Burton what it should have been to Pigling Bland; but the only source of *schoy* recorded by *Atlas* is the Selby Court Rolls (LP 415), and Selby, on the East Riding border, is within fifteen miles of Lincolnshire. Consider also the extent of our ignorance about 'she' in these parts: at eighteen of the fifty-one LP locations in Lincolnshire, 'she' is not attested, and for anything we know, *schoy* could have been current in them all.

Similarly, the co-occurrence of *hi* and *hee* in the *Ipotis* fragment is *prima facie* evidence that there is some one area to which both forms belong. *Atlas* includes no other evidence for the existence of such an area, so when Dr Burton takes it off the map and then tries to localise it for himself, he is in difficulties. His Figure 1 'suggests that no such area exists' (p. 179) only because it excludes the evidence that such an area does exist.

The evidence for localisation is by no means always so decisive as in the

hypothetical example above. Suppose that half the forms in the hypothetical *schoy-*text had belonged to the Selby area, and the remainder to S. Lincolnshire: what should we have concluded then? Possibly that the text was copied from a S. Lincolnshire exemplar by a Selby man. On the other hand, it could be the work of a man who had lived in both places, and whose spontaneous language was genuinely mixed. Either way, it would be a poor candidate for entering on a regional dialect map. Suppose, however, that at some later stage, we discovered several more texts in the same language, but all in different hands. The chances that, say, a dozen people would combine what we had taken to be S. Lincolnshire and Selby features, in the same way and as the result of biographical or textual accident, are on the face of it rather slim. Is the apparent mixture a genuine regional variety after all? Is there any non-linguistic evidence for the local origins of these writers? Can the maps be revised to accommodate such an assemblage? Or have they been revised sufficiently between times for this new material to be incorporated without difficulty? In short, there is no simple or programmatic answer to Dr Burton's initial question. Each case must be judged on its merits, and perhaps not all scholars will agree on the verdict; but that is in the nature of historical inquiry.

The present inquiry is historical, but it involves geographical principles as well. In his attempt to localise the language of the *Ipotis*-fragment (pp. 179-84), Dr Burton finds two areas to which it could belong, but can proceed no further. 'And is it not rather disturbing', he concludes, 'that our two possible areas are on opposite sides of the country?' (p. 183). Hardly. His areas are delimited by a bare handful of forms, and divided distributions are a commonplace of linguistic geography: Dr Burton really could have read more widely before rushing these perplexities into print. Commonly (but by no means always) divided distributions are the result of innovation in some central area, the older form persisting as a relic on the margins (lateral areas). Bloomfield's *Language* (cf. p. 190) contains a useful chapter on these matters:

> The relic form . . . has the best chance of survival in remote
> places, and therefore is likely to appear in small, detached areas
> . . . Especially when a feature appears in detached districts that
> are separated by a compact area in which a competing feature is
> spoken, the map can usually be interpreted to mean that the
> detached districts were once part of a solid area.[19]

Equally accessible is the much more extensive chapter on 'Linguistic geography' in Iordan-Orr's *Introduction to Romance Linguistics*.[20] Dr Burton is not of course alone in neglecting the more specialised literature. In the past, distinguished scholars have made the most extraordinarily confident assertions as to the character of medieval English dialects, without ever, as it seems, having opened a dialect atlas for a modern language in their lives. Now, whatever its imperfections, we have a dialect atlas for the English of half a millenium ago: is it really asking too much of Middle English scholars that they should acquaint themselves with the elements of linguistic geography before they use it and find it wanting?

Lastly, a note on computational procedures. 'It must surely be possible', says Dr Burton, 'to devise some form of computer-assisted technique for comparing Linguistic Profiles and hence for locating the texts from which they were drawn' (p. 184, cf. p. 188 9.10). The one does not follow from the other (delete 'hence'), and presumably he means 'localise the dialect of the text from which a given LP was drawn': his wording is symptomatic, but let it pass. Such things can be done. Computationally, a program for collating LPs is child's play; we had one written *c.* 1977, though we never found much use for it. As to localisation, I devised a computer-based version of the 'fit'-technique in 1980 or thereabouts, and Hr Rainer Thönnes wrote the necessary programs; they were fairly straightforward, and they worked. Since then, Hr Marcel Dekker, in collaboration with Professor Anthonij Dees at the Free University of Amsterdam, has developed much more sophisticated procedures. Like mine, none of them excludes philological evaluation: we all maintain that the particular forms contributing to badness of fit must be identified and assessed before deciding between statistically similar candidates. Dr Burton will doubtless be disappointed to learn that numerical and strictly formal comparisons need not lead to the definition of one and only one likely area of origin; competing locations could emerge even on opposite sides of the country (p. 183). The only way of sparing him the agonies of philological judgement, it seems, would be to provide him with an expert system capable of bringing to bear on its comparisons all that is known about the terms to be compared: in which event, he would doubtless be complaining that the system was untrustworthy, because he could not repeat its assessments for himself.

## 9.  Isoglosses  \

Dr Burton's claim that we have suppressed our own isoglosses (p. 178) is false, for the simple reason that we had scarcely any to suppress: he is mistaken in supposing them a prerequisite for localisation. In my paper from which he quotes (p. 176), isoglosses are nowhere mentioned: I referred to 'the parts of the map' where a given form was or was not recorded, and an isogloss is merely one of many schematic devices for highlighting them.[21] With very few exceptions, such isoglosses as we drew were rough working on tracing paper overlays, relevant only to the particular dialect that was being localised at the time. We used these overlays in the way I described, though they sometimes record accumulations (Dr Burton's term, p. 177), rather than exclusions. Two samples are reproduced here. Figure 2 is McIntosh's, and reflects placing within a well-differentiated area. It is the product of detailed working, though considerable knowledge had been brought to bear before pencil was so much as put to paper; in its final state, however, it is little more than a visual aide-mémoire. Figure 3 is mine, and relates to a large, inadequately documented and generally ill-differentiated area; the cross-hatchings are exclusions, the arrows mark poles of attraction. Readers may judge for themselves whether the lines drawn amount to publishable isoglosses.

Lest it be thought that these overlays represent the whole of our work on localising the sources in question, it had better be made clear that they were only a prelude to trial mapping and further detailed evaluation. Additionally, for much of the southern and some of the far northern material, modified Venn diagrams were used to establish or refine spatial ordering within local complexes; and in a limited way, computational analysis sometimes afforded further insight (so for the East Riding and Craven complexes).

The draft maps, as explained above, were in detail constantly evolving, so that even had we wished to use isoglosses in the *Atlas*, we could not have begun to draw them until the very last source had been localised. Making a comprehensive set, let alone a complete one, and then drawing to a publishable standard, would have cost at least two man-years; the dot maps and item maps, by contrast, could be run off a computer typesetter in a matter of weeks. We represented the distributions as points of occurrence, and if he wants them, Dr Burton can draw isoglosses around these points as well as I can. Neither of us can draw isoglosses without such points, and we shall inevitably differ in the way we see the spatial patterns. (Ask any psychologist: there is no need to take the word of an ex-geographer.) It was not
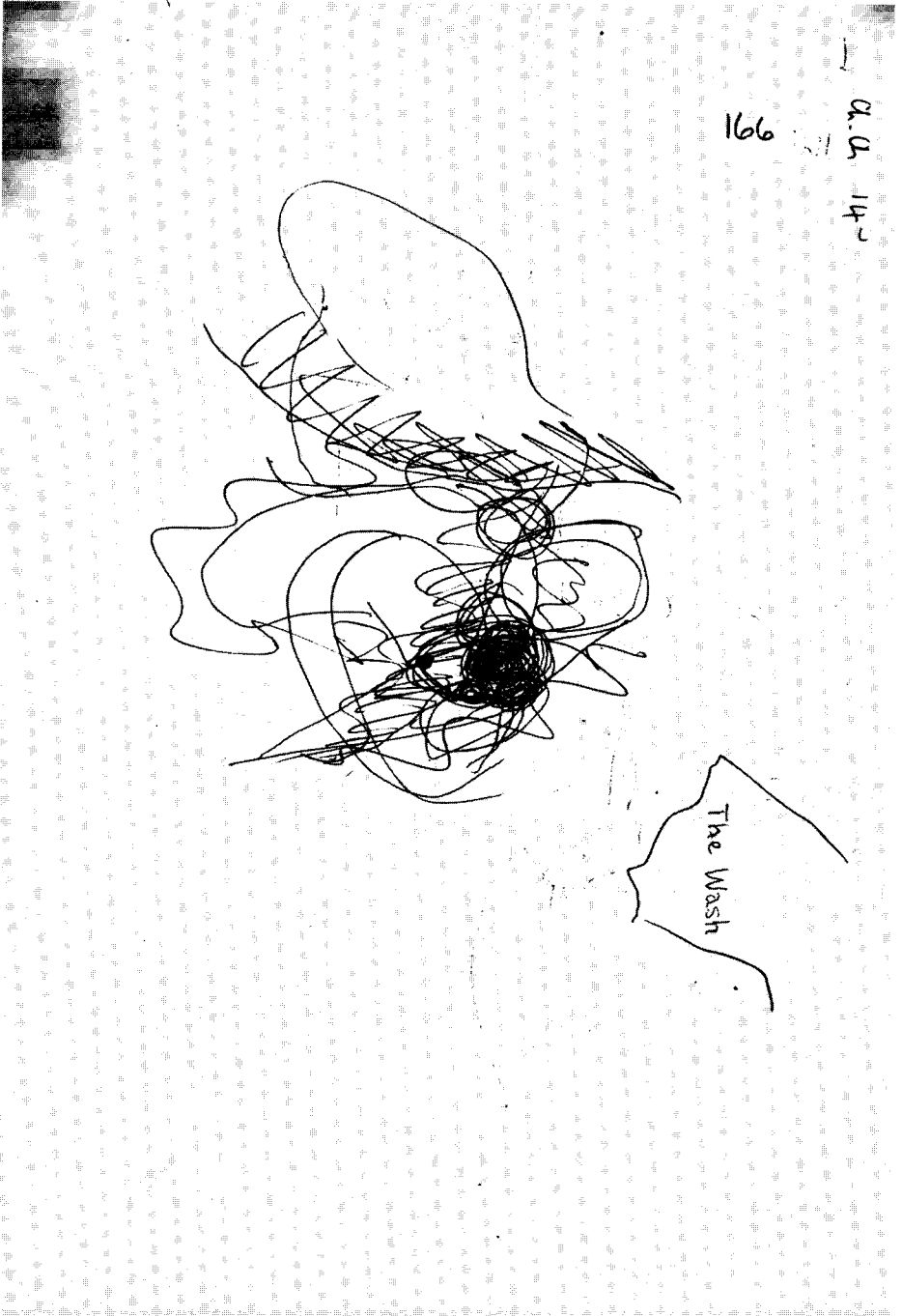
Figure 2

Figure 3

'disingenuous' to claim that dot maps are less subjective than maps showing hand-drawn isoglosses, and it is an offensively warped logic that leads Dr Burton to maintain otherwise (cf. p. 178 above). Anyone who thinks I sought to delude people into accepting the dot maps as beyond question, had better read what I wrote about the uncertainty attaching to all of the *Atlas* placings (II.ix-xiv).

Such isoglosses as we published in articles were produced *ad hoc*, hand-drawn illustrations from a time when we had no means of producing dot maps. We have used dot maps in recent publications because they present the facts of distribution, so far as we have been able to ascertain them, more directly and in more detail than isoglosses allow. Isoglosses, it must be emphasised, are merely schematic: the area in which people use a given dialect form is not impermeable, neither are its edges razor-sharp. Dot maps convey this very well. If a form is later discovered a little beyond its recorded range, to enter it on the dot map will be seen merely as filling a gap, a narrowing of uncertainty; but the psychological effect of shifting a line on the map is quite otherwise, as witness perplexed inquiries from scholars whose own work has called for minor revision of our earlier maps.

The decision to use dot maps in *Atlas* was sound, not only from an economic point of view, but in principle as well. Their present organisation, however, is another matter. In principle, for each segment of an item, the contrasting realisations should be separately mapped. Thus a map of *y* as the stem-vowel of 'it', requires at least the complementary map for non-*y*, if not individual maps for each of the non-*y* types (*i* and *e*): otherwise, there is no means of telling whether the distributions of *i* and *y* are mutually exclusive, and if they are not, how far they overlap. The dot maps in *Atlas* are not so organised, and for this I can take no responsibility. The specifications I prepared for the main series were a systematic diagnosis, based on complementary sets; but after my return to Oslo, and without a word of consultation, dozens if not scores of the relevant maps were excluded from the final copy. The necessary structural element is accordingly far to seek, and it is to be hoped that it will be restored in any future edition.

## 10. The Ipotis *fragment*

Section 6 of Dr Burton's paper reports his efforts to localise the language of the *Ipotis* fragment of Bodleian MS Eng.poet.c.3. He finds localisation impossible, and asks for a demonstration, step by step, of how the *Atlas* placing was

accomplished. His concern, however, is not so much with the *Ipotis* fragment as with the general principles involved, and he complains that the account provided in *Atlas* I.10-11 is 'too schematic to be very helpful'. On that point we are in full agreement, and had the introductory material been written in easier circumstances, a proper demonstration would have appeared. Even so, at least one scholar has put *Atlas* to effective use, as will appear from Dr G. H. V. Bunt's excellent article on 'Localizing *William of Palerne*'; as a demonstration piece, it may be consulted with advantage.[22] Of the general principles, some further account has already appeared, with illustration from the modern dialects of Schleswig-Holstein.[23] A worked Middle English example, with a step-by-step guide to the use of the *Atlas*, is now in press;[24] it is based on one of the fullest LPs in the *Atlas* (LP 576, from the Minnesota manuscript of *The Northern Homily Collection*, III.597-8). In view of this, I hope Dr Burton will be content with a selective commentary on his efforts, as opposed to an exhaustive re-working of what *Atlas* has already stated as a conclusion.

Dr Burton begins his localisation of the *Ipotis* fragment by attending to three forms that *Atlas* records only from that text: *waʒ* 'was', *fleus* 'flesh', *wyʒ-oute* 'without'. Such forms are self-evidently useless for localisation, unless close congeners are to be found.[25] This, however, is to start from the wrong end, and since uniqueness is rarity's limiting case, turning to forms that are merely rare does not of itself guarantee any advance. In this case, however, 'the next rarest forms' are by no means without value, even though Dr Burton (p. 179) could make nothing of them.

In Figure 3, he reproduces the distribution of *þat ilche* 'the same' as it appears in *Atlas*: the form is found in Essex (LPs 6260 and 6350), London (LP 6500), E. Sussex (LP 5850), and in Herefordshire (LP 7440). He does not attempt to evaluate the distribution, and takes no account of the close cognates. THIS + *ilch(e)* is found at a third point in Essex (LP 6040); it co-occurs with *þat ilche* in LP 6350 (Essex), and so also in LP 7440 (Herefordshire). The weight of the distribution is hence eastern; in the west and in Sussex, these forms are attested only as minor variants. The source of *þat/þis ilche* in Herefordshire, moreover, is Jesus College, Oxford, MS 29. This is one of a half-dozen late thirteenth-century manuscripts utilised for the *Atlas*, and as evidence for later Middle English they must be used with caution (I.25b). *Atlas* records western THAT/THIS + *ilch(e)* only from Jesus 29, and it is hence possible (though by no means certain) that such forms were no longer in use in the west by the time the *Ipotis* fragment was written.[26] This impression is borne

out by the distribution of the descendent forms, THAT/THIS + *ich(e)/ych(e)*, found also in Jesus 29. These recur in N.E. Oxfordshire (LP 6870) and N.E. Worcestershire (LP 7591), but are otherwise of easterly complexion: E. Buckinghamshire (LP 6630), Essex (LPs 6280 and 6340), London (LP 6500, beside *þat ilche*), Middlesex (LPs 6510 and 6520), Hertfordshire (LP 6620), Suffolk (LP 8380), and Norfolk (LP 4041). Although, therefore, a westerly origin for the dialect of the *Ipotis* fragment cannot be ruled out on the evidence of *þat ilche* alone, the form points much more strongly to an easterly origin than Dr Burton has supposed.

The distribution of *willen* 'will' pl. is less cohesive, but again Dr Burton ignores the congeners: *wilen* and *wyl(l)en* appear at three locations in S.W. Essex, one of which is immediately adjacent to the *Ipotis* placing (LP 6321, *wyllen*).[27] The weight of the distribution is eastern once more, and lest Dr Burton object that *willen* and *wyllen* cannot be treated as a class, be it noted that *wil(-)* and *wyl(-)* for 'will' vb. are commonly co-variant in the same hands. (So in Essex LPs 6010, 6021, 6290, ?6300, and 6360; the tally for this county would doubtless be longer were *wol(-)* and *wel(-)* less in evidence.)

The distributions of the forms for 'through', *þurw* and *þurʒ*, are too diffuse to be of value in the early stages of localisation. Both of them, however, not just *þurw* (cf. Dr Burton's Figure 3), are found in LP 6220, entered some fifteen miles north of the *Ipotis* placing. The closely related variants *þurʒe*, *þurwe*, *thurgh* and *thurwe* must also be considered. These are solidly attested in S.W. Essex and London, and set against the 'þorgh' and 'þoro(u)gh' types, reinforce the present placing.

The forms of 'she' are more difficult to evaluate. Samuels's original analysis notes *hee*, *he* and *hi,* once each. (Dr Burton rightly observes that *e* in LP 6310 is a ghost: it was produced in editing, from shorthand *he(e*, and played no part in localisation.) The distribution of forms with *h-* is not cohesive in Essex, or indeed in the south-eastern counties generally, as they are portrayed in *Atlas*. This is to be expected, however, given the history of 'she': [h]- forms generally gave way, by whatever process, to forms with [ʃ], and the geographical distribution is hence complicated by disparities of date. Ideally, *Atlas* would have been based on strictly co-eval sources, but they are not to be had in sufficient quantity; even were writings produced in the same year available for every parish, a chronological dimension would still be inescapable, given the normal co-existence of three generations (cf. *Atlas* II.xiia).

In Essex, both *h*-forms and *s(c)h*-forms are found, with *s(c)h*-forms much the

commoner. In the development of [ʃ] from [h], an intermediate stage [ç] is implied.[28] This is apparently the basis for such spellings as *ȝ(h)e*, which in the later Middle English of Essex are fairly widespread (II.14). Most of the sources for this county, therefore, exemplify a later stage of development than does the *Ipotis* fragment, and in view of the manuscript's date, *he(e)* is no bar to an Essex origin. (In the neighbouring LP 6290, *s(c)he* and *he* co-occur.) The recalcitrant form is *hi*. Its closest analogue in these parts is *hij* in N.E. Middlesex (LP 6510); *hi* and *hy(e)* are found in N. Kent. There is no case for localising the dialect of the *Ipotis* fragment in Kent, still less in the W. Midlands, and on present evidence, *hi* is to be regarded as a relict of older Essex usage. *Vices and Virtues* (*c.* 1225) has the comparable form *hie*, and its dialect is clearly of Essex origin, whether from Saffron Walden or further south. Such survival is not unparalleled; there are one or two other features that persist in later Kentish sources, long after their disappearance from Essex (so *al-what* et var. 'until',[29] also in *Vices and Virtues*).

I turn now to less ambivalent criteria. One form highly relevant for localisation is *staned* ppl. 'stoned', which Dr Burton ignores here but discusses later for other reasons: 'What happens when one meets a form outside its expected area?' (pp. 185-86). The reflex of OE $\bar{a}$, he complains, was not recorded for the southern area of survey, and therefore his questions about the status of *staned* in a text held to be from Essex cannot be answered from the *Atlas*. Bad *Atlas*.

But this word does not contain OE $\bar{a}$: its stem vowel is OE $\bar{æ}$ < WGmc. *ai* + *i/j* (OE *stǣnan*)[30] and *staned* shows the characteristically East Saxon development. This $\bar{a}$ for OE $\bar{æ}$, whether the umlaut of WGmc. *ai* or the reflex of WGmc. $\bar{a}$ (West Saxon $\bar{æ}$, Anglian $\bar{e}$), has long been recognised: so W. Heuser, *Altlondon mit besonderer Berücksichtigung des Dialekts* (Strassburg and Osnabrück, 1914), pp. 37-42; K. Luick, *Historische Grammatik der englischen Sprache* (Leipzig, 1921 etc.), §362; R. Jordan, *Handbuch der mittelenglischen Grammatik* (Heidelberg, 1925), §50 ('Ostsächsisch $\bar{a}$ aus $\bar{æ}_1$ und $\bar{æ}_2$', cf. §33 Anm. 1). Nor is such information confined to books in German: see, for example J. and E. M. Wright, *An Elementary Middle English Grammar* (Oxford, 2nd ed. 1928), §52 n. 2; F. Mossé, *A Handbook of Middle English* (Baltimore, 1952), §28 Remark IV; K. Brunner, *An Outline of Middle English Grammar* (Oxford, 1965), §11.2. It is even recorded in *Atlas*, where *staned* from the *Ipotis* fragment is duly listed (IV.317a).[31] And Dr Burton tells us that he has discussed the dialect of this text at seminars in seven universities (n. 9), and still nobody has enlightened him. Here indeed is cause for concern, if the common knowledge of a generation ago is shrouded in such

obscurity.    \

The combination of *staned* with *þar* 'there' (consider also *þar*- and *þare*), reduces the eligible area to London and the south-west quarter of Essex. South Cambridgeshire is a marginal possibility, but is excluded by *þat ilche*, the distribution of which has been noted above.

Samuels's narrower placing within this area appears to have rested mainly on two features listed in the Appendix to the County Dictionary, but excluded as a matter of policy from the LPs (IV.311). The first is *fol* 'full' (IV.313c, LP 6321; cf. dm 1125); the second is initial *v* for historical *f*, as in *vyfte* 'fifth', *vor* 'for', *vair* 'fair' (IV.322a, LPs 6040 and 9250: cf. dm 1180). On present evidence, I do not think that Samuels's conclusion can be far mistaken. Forms omitted from his analysis, but which corroborate this placing, are: *þise* 'these' (LP 6250); *þanne* 'then' (LP 6321); *an* 'and' (LPs 6250 and 6300, cf. *Atlas* IV.313a); *fro* 'from' is widespread. It is uncertain whether *goud* in the fragment is scribal (578, rh *rod* 'rood'), or preserved from the exemplar; the form belongs to S. Essex rather than London.[32] Dr Burton corrects the transcript from which we worked, reading 614 *on-bounde* instead of *vnbounde* (p. 205); this points likewise to S. Essex (cf. *Atlas* IV.316a, dm 1148, and LPs 6300 and 6321). The other forms of the *Ipotis* fragment are consonant with the present placing, though as diagnostic criteria their independent value is slight.

To insist that such a placing is geographically impeccable would be absurd, as the *Atlas* authors have repeatedly emphasised. Localisation consists in identifying that sector of the dialect matrix to which the whole assemblage of forms can plausibly be assigned. As a general principle, localisation is much more difficult with sparse LPs, like that for the *Ipotis* fragment, than it is with ample records. Such distinctive features as do appear must be recognised for what they are, and their congeners taken into account. Inevitably, philological judgements have to be made. It is arguable, on present evidence, that the dialect of the *Ipotis* fragment should be placed further to the south-west, on the London-Essex border – as Samuels readily concedes. Neither of us, however, can place it outside this part of the matrix. It is hardly a matter for reproach that Dr Burton cannot place it at all. He is trying to remake the *Atlas* for himself, but he lacks the necessary knowledge, and he really cannot expect to acquire it inside a couple of years.

## 11. Salisbury and Thomas Cyrcetur

A substantial part of Dr Burton's critique rests on an examination of two rather exiguous LPs which in the *Atlas* are entered at Salisbury (pp. 171-73 and 199-201). Because the manuscripts from which they are derived have non-linguistic associations with Salisbury, he imagines that they were used as anchors for the southern configuration. Having shown these associations to be very weak, and then finding no linguistic grounds for assigning these LPs to Wiltshire at all, he concludes that they may have been misplaced by several counties (p. 173). If the anchor texts are here so wildly misplaced, he continues, the Wiltshire configuration cannot be trusted; and it is an obvious inference that the *Atlas* maps generally may be so much in error as to be worthless.

In reply, therefore, it had better be stated categorically and at the outset that neither of the LPs in question, 5390 and 5400, was ever used as an anchor of any sort: Dr Burton's argument (p. 173) is based on entirely false premisses.

Nevertheless, his discussion of these LPs calls for comment on several points, and an explanation of how this part of the dialect continuum was reconstructed is obviously in order. It will be convenient to begin with LP 5400.

Dr Burton makes much of this LP, and in Appendix 2 (No. 15) prints part of the text from which the LP was derived. He follows it with the LP, and then complains that of the eleven items it reports, only three have forms in the cited text. 'One must assume', he writes, 'these items, which do not appear in the nominated poem, are taken from other English writings in the same hand' (p. 201). They are. Folios have a verso as well as a recto, and whereas the designation 'f. 5r' excludes f. 5v, 'f. 5' does not. On the verso of f. 5 in this manuscript will be found a further fourteen lines of verse. *Atlas* errs in describing the text as 'poem' instead of 'poems' (better, 'verses'); and the forms of the creed were included as well, which should have been made clear. At the time the Salisbury Cathedral manuscripts were examined, access was restricted and the authorities refused Samuels permission to make photographs. The reference in *Atlas* depends on notes which at the time they were made were not envisaged as canonical description, and, yes, ideally we would have gone back to Salisbury; but could anyone capable of collation really be in doubt that LP 5400 draws upon all of these texts?

Times have changed, and Dr Burton has apparently fared better at Salisbury than Samuels did; but it may be noted that what he presents as a single poem (p. 200) is treated by Brown's *Register* and *IMEV* as two (four lines on the Seven

Deadly Sins, ten on the Commandments);[33] the verses on f. 5v are styled 'Op*era* misericordie sp*iritu*alia' (eight lines) and 'Op*era* misericordie corporalia' (six lines).[34]

Dr Burton impugns the ascription of LP 5400 to Salisbury on the grounds that the writer of the source text, Thomas Cyrcetur, although a canon of Salisbury, was born in Cirencester, educated in Oxford, and held various ecclesiastical offices in Somerset and Dorset, as well as in Wiltshire: 'there is no reason', concludes Dr Burton, 'to suppose that he spoke or wrote like a native of Salisbury' (p. 173). On the face of it, his argument is sound, and had LP 5400 been used as an anchor, then the indictment would be serious indeed; but LP 5400 was never so used. Moreover, to the limited extent that Cyrcetur's language is represented by LP 5400, he writes very much as could be expected for a native of Salisbury: the LP was entered at Salisbury only because its language did not *dis*agree with the provenance of the manuscript and with Cyrcetur's residence there. We are grateful to Dr Burton for pointing out the biographical details, and agree that Emden's *Biographical Register* – which work, incidentally, appeared some years after Samuels made his notes from Cyrcetur's text – ought to have been consulted.[35] Nevertheless, the fact remains that if LP 5400 is removed from the map, the dialectal configuration is unaltered. As it stands, LP 5400 contributes nothing to the Salisbury record that either (i) cannot be found in the other Salisbury LPs, or (ii) can be judged absent from Salisbury on the basis of the LPs entered in the surrounding area.[36] The texts may very well have been misplaced, but their language, in so far as it appears in LP 5400, has not.

Had we known of Cyrcetur's shifting abode, LP 5400 would no doubt have been jettisoned, for the texts yield so few forms that their language would not have been thought worth re-assessment. My own examination, prompted by Dr Burton, confirms that it is not out of place at Salisbury (cf. note 36 below, p. 258); whereas collation with the LPs entered about Cirencester gives no ground for thinking that it belongs to Cyrcetur's birthplace. It remains uncertain whether the language of these texts is in fact Cyrcetur's own, or whether, if the verses are copies, he reproduced some local writer's forms. From R. M. Ball's study, which in any case appeared too late (1986) for *Atlas* to benefit from it (cf. Dr Burton's n. 5), it appears that Cyrcetur has left little else in English beside tags embodied in Latin sermons. Ball quotes 'of mornyng & of sorwyng' (p. 224), from Salisbury Cathedral MS 174; and from Salisbury Cathedral MS 126 (the source of LP 5400), 'al þat brekuþ spousoode & her meynteners' (p. 226 n. 142) and '[he] haþ no wyl for [to] be hool þat wol not schew ys sor' (p. 227 n. 144). These could likewise be of Salisbury origin, though

that is not to say that they are so: on present evidence, Oxford is little less likely than Salisbury as the place of origin for the language of all these texts. It remains to be seen whether such other English as is in Cyrcetur's hand displays features that point decisively to one or the other, instead of being conformable with both.

I turn now to LP 5390. Dr Burton failed to find *good-* in the source text, and rightly so: *Atlas* errs in that *goodnes* 'goodness' belongs not here, but to LP 5400.[37] Even supposing, however, that *good-* did belong to LP 5390, Dr Burton would still be far mistaken in postulating, simply on the strength of this form, that the LP represents a mixed language containing a 'midland/northern' layer (p. 172). Both east and west of Salisbury, and in uninterrupted spatial contact with it, are entered LPs containing *good(e)* for 'good' (cf. map 139, II.283): so LP 5340, at Shaftesbury in Dorset, and LP 5520, at Winchester in Hampshire. These lie within twenty miles of Salisbury; and given Dr Burton's obsession with the county as a category of linguistic coherence, it should be noted that twenty miles is less than half the distance from Salisbury to the northern border of the county in which that city lies.

Further perusal of the map will show that *good(-)* is well-recorded across most of southern England. This appears to have escaped Dr Burton, whose report of the distribution (p. 171) is seriously misleading; even if he intended to exclude simplex *good(e)*, it is still curiously selective.[38] As to his further commentary, we should perhaps make clear that *Atlas* was not intended as a philological encyclopaedia, or as a replacement for the handbooks and philological literature. Dr Burton asks, reasonably enough, whether inflected or compound forms of 'good' are sufficiently attested in manuscripts to be used in dialectal analysis; but he is hardly fair to reproach *Atlas* for not providing the answer. To anyone who has read very much Middle English, or even scanned the appropriate articles in *MED*, the answer will of course be obvious: *Sidrak and Bokkus* notwithstanding, these forms are very common indeed.[39]

Dr Burton claims that three other forms in LP 5390 are not attested in Wiltshire, and that its placing at Salisbury is therefore indefensible. He is here mistaken in principle, as well as in philological detail. The forms at issue are (1) *vram*, (2) *eh*, and (3) *hiwel*:

(1) *vram* 'from'. First, the vowel: *fram* is solidly attested in south Wiltshire, and the adjacent parts of Somerset (II.127). In Salisbury, the absence rather than the presence of *a*-forms would therefore be surprising. Secondly, the consonant: the

writing of *v* for historical initial *f* is widely attested over southern England (dm 1180). Salisbury lies in uninterrupted contact with several places to which LPs containing the feature are assigned: these lie in Wiltshire (LPs 5331, 5320, and 5411) and Hampshire (LP 5530), at distances from about seven to twenty miles, and in Dorset (LP 5350) at a range of about thirty miles. Once again, therefore, it would be surprising if the feature were absent from Salisbury. Accordingly, regardless of the local origins of LP 5400, *vram* for 'from' at Salisbury is hardly an unreasonable extrapolation. The writing of *v* for initial *f* is of variable incidence, even in those texts which contain it (cf. p. 218 above); and that 'from' so written has not been recorded from other sources in the Salisbury area, may reflect no more than the hazards of survival attendant on any historical corpus.

To follow Dr Burton in excluding initial *v* in 'from' as even a potential feature for Wiltshire (p. 171), is to insist beyond reason that 'chaque mot a son histoire', or that no one written form can be connected with any other. Clearly, assumptions about their history ought not to be built irrevocably into the collection of the Middle English forms, but it does not follow that every word or form must have a history that is unique in all respects. Neither does it follow that, in seeking to understand a given form, we cannot compare it with others: Middle English spellings are the rule-bound products of generative systems. The rationale for this spelling – the voicing of initial [f] – is after all sufficiently attested in the modern dialects. Of the nine locations in Wiltshire reported by the Leeds *Survey of English Dialects*, eight show [v] in 'from'; and only two show [f] at all, these lying in the north-central and south-western parts of the country.[40]

(2) *eh* 'each'. Dr Burton notes that the form is unique to this manuscript (p. 171), and certainly it is not recorded elsewhere in the *Atlas*. The neighbouring LP 5380 (Emmanuel College, Cambridge, 27), however, likewise shows *h* from OE final *c*̇: so *ih, Ih* and *yh* for 'I', beside *-lih* for the adverbial suffix '-ly'; and further scrutiny of its source manuscript shows *eh* itself, struck through by the scribe but intended apparently for 'each'.[41] The origin of *eh* no doubt lies in *ech* rather than *ich* or *vch*, and *ech(e)* is indeed the usual form for 'each' in the Salisbury area (*Atlas* II.55). If, therefore, the rationale for the spelling be considered, *eh* in LP 5390 is not nearly so isolated as Dr Burton imagines. Even supposing that the form were unparalleled, however, his seeming objection to origins in Wiltshire would still have no force. By Dr Burton's reasoning, *eh* could belong nowhere at all, because no matter where it is placed, it will not be attested in any other source for the county in question; whereas

no matter how idiosyncratic it may be, the form must surely belong somewhere. The unique example, we are to suppose, is no example at all.

(3) *hiwel* 'evil'. Like *eh* above, the form is confined to this LP, at least so far as the *Atlas* record extends; and no Wiltshire texts, observes Dr Burton, have forms with initial *h* or even initial *i* (p. 171). Unhistorical initial *h*, however, is characteristic in Wiltshire writings (cf. IV.320a and dm 1172), and its lexical incidence is very variable: that it should turn up in 'evil' in a text thought to be from this area, is hardly a matter for surprise. *Atlas* reports 'evil' with *h-* in only five LPs, assigned respectively to Gloucestershire (LP 7020), Kent (LP 5882), Suffolk (LPs 6140 and 8480), and Worcestershire (LP 7721); and in every one of them, *h*-forms are co-variant with *e-*, *i-* or *y*-forms. Dr Burton now reports that the source text for LP 5390 contains *ivel* as well.

Similarly, the writing of *w* for *v* (or of *v* for *w*) appears in various LPs assigned to Wiltshire, but again, its lexical incidence is very variable, and accidents of sampling have to be taken into account.[42] Indeed, although *w* for *v* is found in many other parts of England as well (cf. IV.322b and dm 1182), the County Dictionary reports *w* in 'evil' from less than a dozen texts, and in all but two of these it occurs beside forms with consonantal *u* or *v*.[43]

So far, *hiwel* in Wiltshire is hardly out of place, but Dr Burton objects also to the vowel of its stem: even *i*-forms, he complains, are not found in Wiltshire. Indeed, *i*-forms are generally thin on the ground: the County Dictionary shows them in only eleven LPs (IV.163a), and consideration of the consonant after the initial vowel may suggest why. Mostly this consonant appears as <u>, which is the normal ME correspondent of medial [v]; and if the vowel preceding is written <i>, the result is a minim cluster that, especially in informal hands, may prove less than transparent at the reader's first glance. The use of medial <v> instead of <u> is one remedy, and in some localities it is the normal practice, but it runs counter to the mainstream of scribal convention. Without offending against any positional rule, however, <i> in minim clusters can perfectly well be replaced by <y>, which normally has the same phonic implications. Forms of 'evil' written with initial <yu> will be found in 102 LPs; initial <iv> appears in just one (IV.163a).

In *hiwel*, the minim cluster does not arise, and so also in *brid* 'bread', *deilich* 'daily', *war-þifus* 'forgive us', and (Dr Burton's addition, p. 199) *ivel*. In *þynhc* 'thing', by contrast, <y> replaces <i> before the double minim. The correspondence of the stem vowel in this scribe's *hiwel* to the stem vowel of other

scribes' *yuel*, whether or not they are of Wiltshire origin, is sufficiently clear: orthographically, <i> and <y> are positional variants, according to a rule that has long been common knowledge.[44] If systematic equivalences like these had to be spelt out individually every time a form was evaluated for dialect mapping, the descriptive matter in the *Atlas* would be without end: the work could never have been published. Does Dr Burton really believe that we should have made a record of every decision that was made along the way?

Excluding those from Salisbury, seven of the LPs entered in south and mid-Wiltshire contain 'evil', and six of them show *yuel(-)*. Salisbury itself appears to lie at the edge of this domain: south of the city, *yuell-* appears in S.W. Hampshire (LP 5530), but the W. Hampshire LPs otherwise show initial *e-* (cf. IV.162b-163b, dm 977 and dm 978). On this evidence, *i-* or *y-*forms could well have been current in Salisbury itself. The Salisbury LPs that Dr Burton does not contest show only three occurrences in all, one of *euel* (LP 5371) and two of *vuele* (LP 5380, early fourteenth century).

## 12.   The Salisbury-Wilton complex

It will be obvious from the county lists in vol. III that for these parts there are relatively few sources that can be localised by non-linguistic evidence;[45] the present configuration, sparse by comparison with other areas, is accordingly tentative. Even so, the Salisbury complex is better founded than these lists suggest. The following account is based on an independent re-working of the material, but its conclusions are the same as those arrived at by Samuels some thirty-five years before.[46]

Firmly associated with Wilton, three miles to the north-west of Salisbury, is Part III of the composite manuscript British Library Cotton Faustina B III (fols 194r-274v). It includes two works in English, *The Life of St Editha* (otherwise the *Chronicon Vilodunense*, fols 194r-258r) and *The Life of St Etheldreda* (fols 260r-274v); they are in the same hand, which is responsible also for the intervening Latin account of Wilton Abbey and its foundation (fols 258r-259v). Horstmann, who edited both saints' lives, judged them to be by the same author, who, 'without doubt', was a religious at Wilton Abbey. These conclusions are confirmed by our independent scrutinies of the texts. His belief that the manuscript is from the author's hand, however, and his assumption that the dialect of these works belongs to Wiltshire, cannot now be sustained.[47] The language is mixed, predominantly of a

W. Midland type, but containing forms apparently of S. Wiltshire origin both in rhyming and non-rhyming position. The appearances are most economically saved by supposing that the originals were, as Horstmann believed, composed in a Wiltshire dialect, but that the texts in Faustina are the work of a copyist from N. Warwickshire or S. Staffordshire. In view of the contents of the manuscript, the copyist worked most probably at Wilton, though for present purposes it matters only that the authorial dialect can be localised. The distinctive forms of this dialect, so far as they can now be recovered, are at least potentially criteria for recognising other material as belonging to the Wilton area.

It could of course be objected that a S. Wiltshire dialect need not belong to Wilton, and that the nuns of Wilton Abbey – perhaps among them the copyist of the saints' lives – need not all have come from local families. Indeed not: but a preliminary hypothesis that the relict forms in the saints' lives do belong to that area is not unreasonable. If the distinctive forms among these relicts are found otherwise only in sources that (i) cohere dialectally, and (ii) cannot be localised outside the Wilton area, then there will be a case for believing such sources to represent the local dialect. That case will be the stronger if (iii) they can be interpolated into this part of the configuration without introducing unconformities: it is axiomatic that, in default of evidence to the contrary, the dialects belonging to any gap in the matrix will be of transitional type with respect to the dialects surrounding it. If, in addition, some of the sources thus localised have (iv) independent and extra-linguistic associations with the area in question, then the reconstruction will gain in its historical as well as in its formal coherence.

Well-established in the rhymes of both the *Chronicon* and *St Etheldreda* is *-nasse* '-ness';[48] writings containing this form are extremely rare. It is found in Emmanuel College, Cambridge, 27 (LP 5380), and in all three hands of Salisbury Cathedral 39 (LP 5371-3); it is not otherwise known from southern sources, and is hardly to be found elsewhere.[49] Two of the Latin texts in Emmanuel 27 associate the manuscript with Salisbury, and one of them, an inventory of the altars, must have been composed either in the cathedral itself, or by someone who had intimate knowledge of it.[50] The manuscript that is now Salisbury Cathedral 39 was bequeathed by Thomas Cyrcetur (*ob.* 1452), whose career has already been noticed (pp. 241-42 above); but though he may have brought the book with him from elsewhere, he may equally have obtained it at Salisbury while canon residentiary there.[51] For neither manuscript is the evidence of local origins compelling, but the fact remains that Salisbury and Wilton are the link between the sources of *-nasse* in

southern dialects.

Rarer even than *-nasse* is the writing of *h* for OE final *ċ*. This feature, as noted above (p. 244) is shared by Emmanuel 27 (LP 5380) with Salisbury Cathedral 82 (LP 5390),[52] a volume bequeathed by a chancellor of Salisbury Cathedral in the earlier fourteenth century.[53] Thus in Emmanuel 27, two extreme rarities co-occur, and in the southern dialects they are known otherwise only from sources which, like Emmanuel 27, have non-linguistic associations with either Wilton or Salisbury.

One rare form does not of itself establish that the sources containing it must be from the same area: rare though the form may be, there is no *a priori* guarantee that its distribution is cohesive. The sources containing it can be presumed to belong to the same area only if they are alike in respect of their other dialectal characteristics. If they are, it will be a corollary that the distribution of the rare form that they share is cohesive. Then, if one of the sources containing the rare form can be localised independently, an approximate place of origin for the whole complex will emerge. These principles can be applied to the present material, but instead of a decisive argument, mutual reinforcement is the result. Some of the sources yield very little: Salisbury 82 contains but the *Pater noster*, and even the texts in Emmanuel 27 amount only to 116 lines of verse. Dialects similar in respect of fifteen or twenty items may appear increasingly unlike with examination of the next twenty (cf. the account of Cyrcetur's language, p. 242 above); present comparisons fall short of any ideal simply for want of text. Similarly, the rhyme-forms and in-line relicts in the saints' lives provide only a limited basis for comparison, and there is no guarantee that the authorial dialect belongs to Wilton.

So far as the evidence goes, however, these sources are dialectally coherent, an impression that is strongly reinforced by comparison with the other material from the southern counties. Moreover, the co-occurrence in this complex of two extreme rarities cannot be dismissed lightly. Most texts afford the opportunity of writing *-nasse* for '-ness', and *h* for OE *ċ*: the rarity of these forms cannot be accounted for by appeal to defective source-material, but is a fact of language resting on a solid statistical base. Further, they are linguistically independent of one another: the writing of *-nasse* for '-ness' in no way affects the writing of *-lih* for '-ly' or *sueh* for 'such'. Hence their combined value for localising a source in which they co-occur, is greater by far than the value of either of them individually: the probability of fortuitous co-occurrence is not their separate probabilities of occurrence added together, but those probabilities multiplied.

Even discounting Emmanuel 27 (LP 5380), there is still a case for localising

the dialect of Salisbury Cathedral 82 (LP 5390) in the Salisbury area. Consider the distributions of the following: *i* or *y* written for OE *ēa* (IV.317b and dm 1159), so *brid* 'bread' (of the nineteen other sources listed in *Atlas*, five are from Wiltshire and five from Hampshire, and one from Somerset near the S.W. Wiltshire border); initial *h* written unhistorically (IV.320a and dm 1172), so *hiwel* 'evil', *hure* 'our' and *hus* 'us'; *w* written for earlier ME *v*, and of *v* for earlier *w* (IV.322b and dm 1182), so *hiwel* 'evil' and *vonhnic* 'dwelling', cf. *wader* 'father' and *war-* 'for-'. These distributions overlap in an arc through W. and S. Wiltshire into mid-Hampshire; and they overlap just possibly in Norfolk as well, in the neighbourhood of LP 4665, which provides *Atlas*'s only E. Anglian evidence for *i/y* from OE *ēa*. Norfolk is the less likely place of origin given *i/y* as the stem vowel of 'evil', for as a Norfolk feature it appears only in LP 4663, entered to the east of LP 4665 (cf. dm 978); and LP 5390's *vram* (< *fram*) 'from' excludes Norfolk decisively (dm 174). One or two other features, though not confined to Wiltshire and Hampshire, corroborate the western placing: *nc/nk* for historical *ng* (IV.321b), so *þynhc* 'thing' and *Brunk* 'bring'; *an* 'and' (IV.313a); and the *i-* prefix in the ppl. (IV.324b and dm 1195), so *i-do*. Not in the LP is *deilich* 'daily', with which compare the S.E. Wiltshire pl. *deies* (LP 5411 and IV.149b-c).[54] As an assemblage, even these few forms cannot be reconciled with a placing very far removed from Salisbury.

Certain features in the *Chronicon Vilodunense* and the *Life of St Etheldreda* reinforce this complex. The following, however, are merely summary notes; a lengthy monograph would be needed to evaluate the author's rhyming practices and all the copyist's dialectal conversions.

(i) Not recorded in *Atlas* is the writing of *k* for historical *g*, a feature uncommon in texts from the southern counties. Salisbury Cathedral 82 has *kultes* sb. pl. 'guilts' and *kilt* 'transgress'; Canterbury Cathedral Lit. D.13 (66), the source of LP 5420 (mid-Wiltshire) has *kif* 'give' and *knawen* 'gnawed'. In *Chron.*, and apparently relict, are *kete* 'get' inf. 2616, *(y-)kete* ppl. 2692, 2693 and 3360, and *kat* 'got' 4377 (rh.).

Possibly related are spellings indicating the voicing of historically voiceless stops: *g* for historical *k* (so *golde* 'cold' 3354 rh., *leyge* 'like' 4325, 4328, 4331 and 4332, *legenyd* 'likened' 1128), *d* for historical *t* (*y-graundyd* 'granted' 809, *fedrus* et var. 'fetters' 4415, 4420, 4435, 4469, 4508, *y-fedryd(e)* 'fettered' 2311, 2860 and 3769, *hondyng(e)* 'hunting' 4453 rh. and 4492 rh.), and *b* for historical *p* (*crebulle* 'cripple' 4321, cf. *crepull* 4364).

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(ii) The writing of *i* or *y* as the reflex of OE *ēa* has been noted already, in Salisbury Cathedral 82; the weight of the distribution lies solidly in Wiltshire and Hampshire. In *Chron.*, the feature appears once only, in the spoiled rhyme *dyde* 'dead' (OE *dēad*): *redde* ppl. 'read' (OE *rǣdd*) 3267. Here, *dyde* is unlikely to be a copyist's substitution, erroneous or otherwise. First, 'dead' in non-rhyming position is regularly *ded(e)* (20 exx). Beside *dedde*, these forms appear also in rhyme, as do *deyd(e)* and *deydde*, noted in non-rhyming position twice only (3583 and 3876). Secondly, the only other word that the copyist could reasonably (albeit wrongly) have intended is 'did', and 'did' is regularly *dud(-)* (161 exx), with variant *ded(-)* (25 exx); *dyd* appears once only (578).

(iii) *fur(-)* 'for(-)': Salisbury Cathedral 39, hand B, *furȝeuenasse*; Emmanuel 27 *furlete*; LP 5300 (W. Wiltshire) *furgon* and *furbarnde*. Cf. *fur* 'for' in *Chron.* 4166, which is unlikely to reflect the copyist's language: *for(-)* is regular, and though *fur(-)* is recorded from S. Warwickshire, it seems not to have been current in the north of the county (cf. *Atlas* IV.313b).

(iv) In the authorial dialect of *Chron.*, *i* or *y* from OE *-ian* inf. is indicated by rhyme, *redy* 'ready': *helpy* inf. 'help' 4754. OE had *helpan*, not *\*helpian*, but the late ME dialects preserving infinitival *-i-* variously extend it to strong verbs and Romance loans. The feature is widespread (cf. *Atlas* IV.324a-b), but characteristic in Wiltshire and the counties bordering it. To the Wiltshire sources (11 of 15 LPs) should be added the Wilton deposition of 1376 (Harley Charter 45 A 37: Morsbach, *Originalurkunden* pp. 1-3), *wytnissy*.

(v) *Chron.* has occasional *f* for *v*, so *fanysshede* 'vanished' 3335 (beside 8x *vanysshed*), *feynne* 'vain' 4173 rh. (cf. *veyne* 4206 rh.). In 'vouchsafe', *f-* is regular (10 exx). *St Eth.* has *fanysshede* 650 (beside *wanyssede* 651, *vanysshede* 847); and *fexst* 'vexed' 315 rh.

    *Chron.* has the reverse spelling, *v* for *f*, in *vondeden* 'found' 2561, *vylette* 'fillet' 2657 rh.

(vi) *Chron.* has occasional *w* for *v*, so *y-weylled* 'veiled' 625 (*veylled* 623, *veylle* sb. 4624), *a-wowe* 'avow' 864. *St Eth.* has *wanyssede* 'vanished' 651.

    The feature has already been noted from Salisbury Cathedral 82, in *hiwel* 'evil', and *war-þifus* 'forgive us' and *wader* 'father' (both with *w* < *v* < *f*). So also

hand A of Salisbury Cathedral 39 has *weawe* 'few' (*w- < v- <f-*).

The reverse spelling, with *v* for *w*, is much less common (cf. *Atlas* IV.322b, and IV.81a under *v*). It appears in hand A of Salisbury Cathedral 39, so *vanne* 'when' (*v < w < OE hw-*) beside *wanne*; Salisbury Cathedral 82 has *vonhnic* 'living' (OE *wunung*).

A further manuscript having associations with Salisbury is Lincoln College, Oxford, MS Lat. 129 (E), which came to notice after the southern part of the survey was in press. Most of the manuscript appears to be in the hand of Thomas Schort, chantry priest and perhaps schoolmaster as well.[55] It is not known where he was born or brought up. He wrote at least part of this compilation at Bristol, in 1427-28, though if the wording of fol. 97r is autobiographical, Bristol was not his home town ('Y haue dewllyd att bristow þis þre ȝere, and as myche more as fro myȝelmasse hedyr-to' (fol. 97r/77). Schort became chaplain of a chantry in Salisbury Cathedral in the 1430s, and was resident there until perhaps 1445, when he settled in Malmesbury or nearby; he died in 1465, possibly in London. His written language, which may not be all of a piece, has yet to be properly analysed, but *-nasse* in the *vulgaria* of fols 92-99 should be noted (*byttyrnasse, swetenasse* fol.93v/31). Its status here is uncertain, but again it occurs in a manuscript associated with Salisbury; and forms like *dryng* 'drink' fol. 94r/36, *sowgyng* 'sucking' fol. 93v/28, point in the same direction. A more detailed examination, it is hoped, will be the subject of a separate paper.

## 13. Appendix 2 of Dr Burton's paper

Most matters of substance arising from this part of Dr Burton's paper have been discussed elsewhere. The remainder are somewhat diverse.

First, in view of his other criticism, my statement of responsibility for the various parts of the *Atlas* (III.xi) should be perhaps enlarged. All of the SOU analyses are Samuels's, except for LPs 9130 and 9530-9610 (which are mine), one or two contributed by McIntosh (there is no list for these), and a very few explicitly acknowledged as the work of other scholars. McIntosh was responsible for the bulk of the NOR LPs, except (i) those for Lincolnshire (which are Dr Laing's), and (ii) most of the LPs derived from local documents belonging north of the Humber-Ribble line (which are mine), and (iii) such LPs for which other people are explicitly

acknowledged. Of the items discussed in Dr Burton's Appendix 2, Nos 1-5 are McIntosh's; Nos 6-11 (and probably the error in No. 3) are mine, and Nos 12-19 Samuels's.

The summary descriptions of the southern material in the Repository List, etc., are nearly all by Samuels, variously amplified by Dr Laing. The bulk of those for the northern literary material were compiled from McIntosh's notes by Dr Laing and me; the descriptions of the sources for Lincolnshire are all Dr Laing's, and most of the short notices pertaining to northern documents were written directly by McIntosh. The longer notices in calendar form, whether for northern or southern documents, are mostly mine, and the medieval Hiberno-English[56] entries are wholly so; these were collated with photographs wherever possible. All references to printed works were checked against the British Museum (Library) Catalogues, by Dr Laing and myself.

My impression is that the individual practices reflected by the NOR LPs are consistently the same; Dr Burton's assessment here seems right. Many of my analyses from original documents replaced those McIntosh had made from printed versions; and inadvertently, I duplicated several of his analyses made from originals. Collation shows that we missed the odd form; but unless we both miss the same things, I do not think that re-analysis will have much effect on the other LPs. Dr Laing's analyses of the Lincolnshire sources, nearly all of which are literary manuscripts, were extremely thorough; again, there is little to be gained by re-analysis. Samuels's practice differed, and the SOU LPs examined by Dr Burton are not representative (cf. sections 3 and 4 above). The main run of the SOU LPs could well be consolidated, but re-analysis can be expected to confirm rather than invalidate the southern placings.

'Re-analysis' and not 'checking', note. Dr Burton's own efforts are confined to just a few texts, and very short ones at that: had he any real experience of the work, his comments might have been more circumspect. To check an analysis is as much as to do it again, and the second time is no quicker than the first. If discrepancies between the analyses are to be resolved, it will be necessary to note folio numbers or line references for every form throughout both analyses, which though time-consuming, is excellent practice (cf. Figure 1). Dr Burton suggests this labour of 'checking' could profitably be shared amongst volunteers from all over the world. Perhaps; but there were precious few volunteers to help with the making of the *Atlas*. As for postgraduate students, finding supervisors competent to 'check' their work (p. 188) may be less easy than Dr Burton seems to imagine.

Most of Dr Burton's corrections to the LPs depend on his examination of the manuscripts, whereas our own work was done very largely from microfilms or photographs; for local documents, the usual medium was latterly photocopies rather than photographs. In photograph, and in photocopy *a fortiori*, parts of a text may be absolutely illegible, even though recoverable from the original; and where the original is merely hard to read, the facsimile may be inscrutable. This is one reason for some of the *Atlas* omissions. Doubtful readings were generally excluded as a matter of principle; such as we printed are duly annotated ('?'). Ideally, we would have worked from the originals; Dr Burton's expenses for his own little jaunt (p. 193) will show him one reason why we could not.

Dr Burton's view that all analyses should have been done from facsimiles if not the originals, is obviously sound, particularly so in the case of local documents. Editions of these are usually the work of historians, and for philological purposes they are seldom trustworthy. No. 13 looks to involve a particularly bad case: Dr Burton's criticism is obviously justified. No. 5 is a different matter. LP 306 depends on a transcript made for the Ann Arbor *Middle English Dictionary*. The 'several errors' Dr Burton finds in the LP are not ours, but the readings of the transcript. (I am grateful to Dr Laing for checking this.) There are only three in all, two of them queried in the LP itself. His 'several' omissions likewise amount to three; but though *oure* was missed, *oure* was recorded; and 'worshipful' adj., as will appear from the scant record in IV.295a-b, was not collected systematically, though this should have been stated in the item note (I.xxb-c, IV.xxb-c). Is *Atlas* really the worse for relying on the transcript as opposed to entering nothing at all?

As to No. 11, it is hard to see what could be gained by adding it: there is not nearly enough to decide between Broncroft and Worfield as its place of origin, if indeed it belongs to either, and its few distinctive forms are already on the map in those parts (so *wes* 'was', *-et* ppl., and *y-* ppl. prefix). As will be seen from sections 11 and 12 above, its value for dialect mapping bears not the least comparison with that of No. 14 (LP 5390).

Dr Burton's criticisms of LP 5950 (his No. 16) have been largely dealt with, but one point remains outstanding. He rightly notes the change of language at fol. 166v in the source manuscript (p. 201): the summary description in *Atlas*, though not in error, ought indeed to have made clear that only the first four poems (Brown Nos. 35-38) were laid under contribution. In his commentary, however, Dr Burton makes altogether too much of this. It is not 'a very great pity that the poems on fols 166v-67r were not analysed', and for the very good reason that they were so. No

LP was printed for them, because they, in common with a vast amount of other material we analysed, were not used directly as sources for the maps. Their dialect is clearly of a N. Midland type, but these texts reveal too little of it for it to be placed at all narrowly. Since the areas to which it could belong are already well-documented, nothing was to be gained from trying to incorporate it. The first four poems, by contrast, were marginally useful additions to the otherwise meagre record for N. Kent. Dr Burton is otherwise on shaky ground. 'Scholars who accept without query', he says, 'the impression given by the *Atlas* entry that all these poems are from Kent are going to be horribly mistaken' (p. 175). Hardly. If the scholar is concerned with where the poems were copied, then he will not be led astray. It is central to part of Dr Burton's critique that all of them are in the same hand, and he apparently accepts that the first four belong to Kent, which is where the manuscript was made: is he asking us to believe that Bishop Sheppey's compilator must then have travelled to the N. Midlands to copy the poems on fols 166v-67r? As to scholars who are likely to be 'horribly mistaken' in respect of the language, it is difficult to see whom Dr Burton has in mind. Nobody could gain from *Atlas* the impression that the forms of fols 166v-67r belong to Kent, and that they do not appear in the Kentish record would give pause to anyone who analysed these texts for himself. Could anyone capable of such analysis fail to recognize that their language is not of a piece with the others? Even Dr Burton, driven back to his favoured 'traditional grounds', could see that this language is from northerly parts; the scholar who uses *Atlas* as a means to localisation will look to origins in N. Nottinghamshire or the adjacent parts of Lincolnshire and the West Riding, conceivably to the Craven district of N.W. Yorkshire, or possibly to one or two places between.

Lastly, LP 6310. In his corrections on p. 206 (No. 19), Dr Burton tells us that 'In fact' the transcript of the source text has six occurrences of *was*, and gives the line references. I count eight (add lines 600 and 601), though I may of course have missed some.

## NOTES

For help in various matters connected with this paper, I am indebted to Dr Margaret Laing (Edinburgh), Dr Kari Anne Rand Schmidt (Oslo), Dr Richard Beadle (Cambridge), and Professor M. L. Samuels (Glasgow). Dr Burton was kind enough to send us an advance copy of his own paper, and the editorial board of *Leeds Studies in English* has shown every consideration; but through no fault of theirs, this reply has been written under great pressure, and the other authors of the *Atlas* have had little opportunity to comment on the text.

[1] It is perhaps invidious to cite examples, but Dr Gillis Kristensson's *Survey of Middle English Dialects 1290-1350* cannot pass unnoticed: completed are *The Six Northern Counties and Lincolnshire* (Lund, 1967), a *vade mecum* since my undergraduate days; and *The West Midland Counties* (Lund, 1987).

[2] See 'The Great Scandinavian Belt', in A. McIntosh, M. L. Samuels and M. Laing, *Middle English Dialectology. Essays on some principles and problems* (Aberdeen, 1989), pp. 106-15.

[3] I may add that I was never sent proofs of any sort, and that the publishers even refused me an advance copy of the work.

[4] C. Brown, *Religious Lyrics of the XIVth Century* (Oxford, 1924), no. 37. Included in LP 5950.

[5] So McIntosh's analysis, on which the LP depends.

[6] Pp. 17-18 of 'Misgivings about Middle English dialectology' (draft, privately circulated, Adelaide 1990).

[7] One such is in press: see section 10 of 'Some new perspectives on the origins of standard written English', in the Proceedings of the Colloquium on Dialect and Standard Language, Royal Netherlands Academy of Arts and Sciences, 15-18 October 1990, edited by J. B. Berns and A. van Leuvensteijn (forthcoming).

[8] W. Hennig, *Phylogenetic Systematics*, second edition (Urbana, 1979), esp. pp. 21-22; D. L. Hull, 'Certainty and circularity in evolution', *Evolution*, 21 (1967), 174-89.

[9] R. V. Ramsey, 'The Hengwrt and Ellesmere manuscripts of the *Canterbury Tales*: different scribes', *Studies in Bibliography*, 35 (1982), 133-54; and 'Paleography and scribes of shared training', *Studies in the Age of Chaucer*, 8 (1986), 107-44.

[10] A. I. Doyle and M. B. Parkes, 'A paleographical introduction' to the facsimile edition of the Hengwrt MS of *The Canterbury Tales*, *Variorum Edition of the Works of Geoffrey Chaucer*, I, edited by P. G. Ruggiers (Norman, Oklahoma, 1979), pp. xix-xlix.

[11] M. L. Samuels, 'The scribe of the Hengwrt and Ellesmere manuscripts of *The Canterbury Tales*', in *Studies in the Age of Chaucer*, 5 (1983), 49-65. Reprinted in M. L. Samuels and J. J.

Smith, *The English of Chaucer and his Contemporaries* (Aberdeen, 1988), pp. 38-50.

[12] Cf. pp. 87-88 of 'Some applications of Middle English dialectology', *English Studies*, 44 (1963), 81-94.

[13] Exemplary analyses appear in Hreinn Benediktsson's introductions to *Early Icelandic Script as Illustrated in Vernacular Texts from the Twelfth and Thirteenth Centuries* (Reykjavik, 1965), and to his edition of *The First Grammatical Treatise* (Reykjavik, 1972).

[14] It might have turned out, for example, that *life*, *lijf* and *liyf* were used indifferently all over the country; but if these forms had been lumped together at the outset, the predominantly Central and S.E. Midland distribution of *lijf* could not have emerged (IV.209c and 318b). When the suffix '-ful' was added to the survey questionnaire, the contrast between *-fol* and *-ful* was the only point of interest; but in the northern area of survey, as it now appears, the contrast between *-l* and *-ll* may be a better guide to local origins (cf. I.516 and 542, dm 947-48 and 1125; IV.303a-b and 313c).

[15] M. Benskin, 'The letters <þ> and <y> in later Middle English, and some related matters', *Journal of the Society of Archivists*, 7 (1982), 13-30 (p. 15). The regional pattern would have been more clear-cut had textura <y> been excluded (cf. row 4 in the plate). I have examined well over two thousand local and state documents since the map was drawn, and though minor revisions are necessary, the main pattern is confirmed. There is further evidence that 'þ' and 'y' were confused in southerly writings of the thirteenth and early fourteenth centuries, though later writers from the south nearly always distinguish them.

[16] Cf. A. Campbell, *An Old English Grammar* (Oxford, 1959), §§90, 719 and 725; R. Jordan, *Handbuch der mittelenglischen Grammatik*, third edition (Heidelberg, 1968), §167; *Þe Liflade ant te Passiun of Seinte Iuliene*, edited by S. R. T. O. d'Ardenne, EETS, os 248 (London, 1961), p. 89; *MED* s.v. *ēch* pron.; etc.

[17] Kristensson, *Six Northern Counties* . . . , pp. 108-09 and 116-20; Kristensson, *West Midland Counties*, pp. 87-88 and 95-99.

[18] In view of Dr Burton's strictures about categories of forms (pp. 179 and 183), it would be interesting to know why he thinks that these *-i-* and *-y-* variants form a single class, and by implication, that they are not worth collecting.

[19] Chapter 19, 'Dialect geography'. Citations are from the twelfth impression (London, 1973), pp. 334 and 340.

[20] Revised edition by R. Posner (Oxford, 1970), pp. 144-278. On the 'Neo-linguistic school' and lateral areas, see pp. 276-77. For Old English, see especially D. DeCamp, 'The genesis of the Old English dialects: a new hypothesis', *Language*, 34 (1958), 232-44 (reprinted in *Approaches to English Historical Linguistics*, edited by R. Lass [New York, etc., 1969], pp. 355-68). ME *hi(j)* and *hy(e)* 'she' would be normal reflexes of OE *hī* or *hīe*; the distribution of the OE forms is similarly divided, between the W. Midlands (Vespasian Psalter Gloss) and Kent (Kentish Glosses).

See Campbell, *An Old English Grammar*, §703.

[21] Properly, they are heteroglosses. The relevant cartography is therefore less straightforward than it may seem. Note, for example, that a map showing the ME distributions of the $\bar{o}$- and $\bar{a}$-reflexes of OE $\bar{a}$, would need at least two heteroglosses, not one: the northern limit of $\bar{o}$ is not the same as the southern limit of $\bar{a}$. Such overlaps are not exceptional, but the norm.

[22] *Historical Linguistics and Philology*, edited by J. Fisiak, Trends in Linguistics Studies and Monographs, 46 (Berlin and New York, 1990), pp. 73-86.

[23] M. Benskin, 'The numerical classification of languages, and dialect maps for the past', in *Distributions spatiales et temporelles, constellations des manuscrits. Études de variation linguistique offertes à Anthonij Dees à l'occasion de son 60me anniversaire*, edited by P. van Reenen and K. van Reenen-Stein (Amsterdam and Philadelphia, 1988), pp. 13-38.

[24] M. Benskin, 'The "fit"-technique explained', in *Regionalism in Late Medieval Manuscripts and Texts. Proceedings of the Fifth York Manuscripts Conference*, edited by F. Riddy (Woodbridge, 1991), pp. 9-26.

[25] In view of the *Atlas* placing, cf. *witʒ-outin* in LP 6321. For 'with', an item not included in *Atlas*, the *Ipotis* fragment has *wytʒ* (554), beside *wyʒ* (four exx).

[26] Madan dated the hand as 'early fourteenth century': so J. D. Sutton, p. 115 of 'Hitherto unprinted manuscripts of the Middle English *Ipotis*', *Publications of the Modern Language Association of America*, 31 (1916), 114-60. The hand of Jesus 29, 'not essentially different from a twelfth-century hand', is accepted by most authorities as belonging to the second half of the thirteenth century: cf. N. R. Ker, *The Owl and the Nightingale. Facsimile of the Jesus and Cotton manuscripts*, EETS, os 251 (London, 1963), p. ix. Ker regarded it as contemporary with the hand of the Cotton *Owl and the Nightingale*, for which he thought Wright's date, 'probably a little after A.D. 1250', on the early side (cf. pp. xvi and xvii n. 3). On this evidence, the *Ipotis* fragment could well be later than Jesus 29 by two generations, though it is not impossible that they are co-eval.

[27] These forms are found also in five Norfolk LPs, and one LP apiece for Hertfordshire, Suffolk, Surrey, and Worcestershire (which last is the only western occurrence). Likewise eastern is *wyllyn/wil(l)yn*, found in four Norfolk LPs, three from Suffolk, and one each from Ely and E. Sussex. (For both types, there are also one or two northerly occurrences.)

[28] Cf. *MED* s.v. *she*. There is an extensive literature, but see now D. Britton's detailed and judicious review, 'On Middle English *she, sho*: a Scots solution to an English problem', *North-Western European Language Evolution*, 14 (1991), 3-51.

[29] *Atlas* IV.273b. For a map, see M. L. Samuels, *Linguistic Evolution* (Cambridge, 1972), p. 102.

[30] See J. Bosworth and T. N. Toller, *An Anglo-Saxon Dictionary* (Oxford, 1898), s.v. *stǽnan*.

In *OED*, the verb \is *steen*, not *stone*. Further, cf. S. Feist, *Vergleichendes Wörterbuch des gotischen Sprache* (Leiden, 3rd edn. 1939), s.v. *stains*; W. P. Lehmann, *A Gothic Etymological Dictionary* (Leiden, 1986), S130. The inf. *\*stainjan* is not attested; Ulfilas shows *staineiþ* 2 pl. pres. indic., *stainjam* 1 pl. pres. indic. (John 10. 32-33), *stainiþs* ppl. (II Cor. 11. 25).

[31] Of eight other LPs containing <a> for OE *ǣ*, four are from S.W. Essex (including LP 6321, mapped directly adjacent to the *Ipotis* LP), two are from N.E. Middlesex, one from N. London, and one is from just outside Essex in S. Cambridgeshire (LP 6180).

[32] Note *goud*(es) occurs in the source of LP 6070 as well, and should be added to *Atlas's* record.

[33] C. Brown, *A Register of Middle English Religious and Didactic Verse*, Part 1 (Oxford, 1916), p. 449. C. Brown and R. H. Robbins, *The Index of Middle English Verse* (New York, 1943), nos 2776 and 3731.

[34] Brown, *Register*, ibid.; *IMEV*, 3263 ('three couplets') and 645.

[35] Samuels's information came from E. M. Thompson in *Englische Studien*, 1 (1877), 214-15.

[36] Entered otherwise in Salisbury is *buþ* 'are'. Not in the Salisbury LPs, which except for 5371 are in any case exiguous, are *þey* 'they' and *hem* 'them'; but these are well attested in the Salisbury area. So is *ask-* 'ask-', for which see also *aske* (IV.122c-123a).

Contra Dr Burton (p. 172), *mannus* 'man's' is unlikely to be peculiar to this text. Inflected forms have not normally been recorded, unless the simplex was lacking; where they are recorded, the inflexion is commonly replaced by a trailing hyphen. For *-us* in the gen. sg., compare the suffix of the noun pl. (IV.105b); see LPs 5311 (which has the abbreviated form *mannus* gen. sg.), 5411-12, 5460, and 5371 (which last is entered at Salisbury).

For *wen* 'when', cf. *wenne* in LP 5361, immediately W. of Salisbury, and *(when)* entered in Salisbury itself (LP 5371); also *(wen, wenne)* in LP 5530, in uninterrupted contact *c.* fourteen miles to the south, in Hampshire.

'Church' is ill-attested in the region; LP 5380 (Salisbury) has *churiche*, and LP 5400's *chyrch* is somewhat isolated. To the E., however, at Winchester, is found *-chyrch[e]* (LPs 5500 and 5511); to the S.W., at Southampton, is found *chyrch* (LP 5541); all are in open contact with Salisbury. Seven miles to the north, LP 5411 has *chirche*.

Capitalised *Hy* 'I' is not otherwise reported in the County Dictionary, though *hy* is noted from one Essex LP (IV.203b). Unhistorical initial *h-*, however, is characteristic in writings from S. Wiltshire, and since *y* for 'I' is there common, *Hy* is hardly to be wondered at.

For 'pride', the record in *Atlas* is generally thin; the form in LP 5400 may or may not have been Salisbury usage, though it affords no grounds for placing the LP there. At Salisbury itself (LP 5371), and immediately to the north (LP 5411), 'pride' shows *uy* or *u*. LP 5400's *pryde* is matched by LP 5450 (N.E. Wiltshire); cf. *pride* in LP 5420 (mid-Wiltshire), and *pryte* in LP 5511 (Winchester).

To the Appendix of Southern Forms (IV.313-25), LP 5400 contributes the following: *fur* 'for(-)' (IV.313b, cf. occurrences at Winchester [LP 5511] and in W. Wiltshire (LP 5300]); *y* for WS *ie* (IV.319b, s.v. *hyldron*, cf. Winchester *hyldyr, yldiste* [LP 5500]); unetymological initial *h* (IV.320a, well attested in S.W. Wiltshire); *-ngg*, in *comyngg* vbl. sb. (IV.321b-322a, cf. entries for Wiltshire LPs 5311, 5420 and 5450, and Hampshire LPs 5511 and 5530); the sporadic feature *scl-* for historical *sl-* (IV.322a, cf. LPs 5311 and 5430, both Wiltshire); *t* for *d* (IV.320b, cf. Winchester LP 5511). Only *ff* for medial *v* (IV.322b) is isolated at Salisbury, and its distribution is not cohesive (cf. E. Somerset LP 5200).

Dr Burton's text and the creed provide further forms that may bear on localisation. Not attested in Salisbury, though perhaps not out of place there, is *mon* 'man' (IV.28b-c); cf. *monnes* g. sg. in LPs 5313 (W. Wiltshire) and 5511 (Winchester). Nine Wiltshire LPs show *fro* 'from' (IV.50a); so also, within range of Salisbury but outside the county, do LPs 5340 (Dorset), 5480 and 5500 (Hampshire). Not predictable in the Salisbury area is *worchepe* vb. 'worship', unrecorded from the rest of the southern counties and the S.W. Midlands (IV.292b); this form would be an oddity wherever the assemblage were localised.

37  The analyses for the two LPs were entered on the same sheet of paper, a practice followed very rarely; the error arose in editing them for publication. The original versions were from the texts printed in *Englische Studien*, 1 (1877) 214-15, which may account for one or two further deviations from Dr Burton's transcript; additional notes were made from the manuscripts, whereas the analyses ought to have been done anew.

38  Omitted are *good-* (in the guise of *goodes, goodes,* or *goodis*) from Buckinghamshire (LP 774), Gloucestershire (LPs 9530 and 9590), Hertfordshire (LP 6530), and Kent (LP 5900). See County Dictionary IV.187b, s.v. 'good *sb.*'

39  See further the entry for 'good' in Notes on the Questionnaire: 'In late LPs, the sub-category '*sb*' is regularly invoked' (III.xxi and IV.xix, s.n. 139) – which, to apply Dr Burton's own intuitions, indicates that for most of the corpus, it was not.

40  *Survey of English Dialects. Basic Material*, IV: *The Southern Counties*, Part III, edited by H. Orton and M. F. Wakelin (Leeds, 1968), p. 959 (item VIII.2.11). It has been of some interest to examine the Wiltshire distributions in course of writing this paper, though it must be emphasised that they have played no part at all in the making of the *Atlas*.

41  Fol. 162v, 'Al fram eh vuele þinge : me schulde ie*sus* þat may'. (For the text, cf. *Cambridge Middle English Lyrics*, edited by H. A. Person [Seattle, revised edition 1962], no. 34 on p. 28.) To LP 5380, add also *sueh* 'such' (from fol. 163r, text not in Person); and delete *such*, which was misread from *sueh* in the analysis.

42  To the list for Wiltshire in IV.322b, add LPs 5313 and 5380, both with *v* for *w*.

43  IV.162-63. The two are LP 5010 (Cornwall), and LP 6321 (Essex); for the latter, Samuels's

original analysis records the word itself only once. The other occurrences are in Berkshire (6751), Essex (6130), Norfolk (4620 and 4665), Oxfordshire (6820), Somerset (5620), Suffolk (8491), Surrey (5740), Warwickshire (4285).

44 See, for example, *Fourteenth Century Verse and Prose*, edited by K. Sisam (Oxford, 1921), p. 276; J. Wright and E. M. Wright, *An Elementary Middle English Grammar*, second edition (Oxford, 1928), §9.

45 Local documents from Salisbury itself were not identified. Probably from Wilton, three miles to the north-west, there are the four Frye letters of *c.* 1401, entered as LP 5361; these are very short, and only two are in local language (E 28/29/50, by Robert Frye's mother Anneys, and E 28/29/64, by T. Symme). Robert Frye was a clerk to the Privy Seal, and so resident at Westminster; the language of his own letters (E 28/29/ 11 & 55) is anonymously southern. (On Frye's career, see A. L. Brown, *The Early History of the Clerkship of the Council*, University of Glasgow Publications, NS 131 [Glasgow, 1969], pp. 18-20.) A better source is the Wilton deposition of 1376, British Library Harley Charter 45 A 37, for which see L. Morsbach, *Mittelenglische Originalurkunden von der Chaucer-Zeit bis zur Mitte des XV. Jahrhunderts* (Heidelberg, 1923), pp. 1-3; its forms could well have been entered on the maps.

46 Unless otherwise stated, I have used Samuels's inventories of forms, and in their original state, not as codified LPs.

47 'Beide Legenden sind Werke desselben Dichters, in demselben Dialecte, in demselben Metrum und derselben Strophenform geschrieben. Der Dichter war ohne Zweifel einer der Geistlichen der Abtei von Wilton, der während der Regierung Heinrich's V. lebte und schrieb (c. 1420). Der Dialect ist der westsächsische von Wiltshire . . . Das Ms. ist wahrscheinlich die Originalschrift des Dichters selber, der auch die nicht seltenen Correcturen zu verschiedenen Zeiten eingetragen zu haben scheint . . .' C. Horstmann, *Altenglische Legenden. Neue Folge* (Heilbronn, 1881; reprinted Hildesheim, 1969), p. 282. As Horstmann noted and as Dr Laing confirms, the last twenty-two lines of *St Etheldreda* (1110-31) are by a different hand. For the text of *St Editha*, see *S. Editha sive Chronicon Vilodunense*, edited by C. Horstmann (Heilbronn, 1883). The Latin text of fols 258r-59v ('Ista fundatorum sunt nomina Cantarie priore Ellendinie i(d est) Wyltonie') is printed on pp. 111-13.

48 Occurrences have been recorded as follows; the list is intended to be full, though it may not be exhaustive. I am indebted to Dr Laing for various notes on *St Editha*, made from a microfilm of the MS; these I have supplemented from a close reading of Horstmann's edition. For *St Etheldreda*, I have had to rely solely on Horstmann. References are by Horstmann's line numbering.

*St Editha.* In rhyme: *clannasse : Emperasse* 1734, *was : heuynasse* 1864, *heuynasse : grace* 4779, *was : honestnas* 2279, *godenasse : was* 4347. (Probably original as well, though the rhyme does not prove *a*, is *ryȝtwysnasse : lasse* 527.) Cf. the spoiled rhymes *godenes : was* 818,

*forȝeuenesse* : *grace* 1990, *sekenesse* : *place* 3059; the self-rhymes *swetnesse* : *gladnesse* 2587 and *godenesse* : *sekenesse* 4359, and (apparently also translated) *godenesse* : *wesse* 'was' 822 (*wes(se)* is not attested in Wilts, Hants, or Dorset). As a by-form, *-nys(se)* is apparently confirmed by *y-wys* : *cherysshenys* 1147. (For *-nisse* and *-nys(se)*, see *Atlas* IV.314c: they are well-attested within range of Salisbury, in Hampshire and W. Wiltshire.) The in-line usage is *c*. 30x *-nesse* : 10x *-nes*, with 2x *-nasse* (*bleynasse* 2937, *wittwnasse* 4437).

*St Etheldreda*. In rhyme: *clannasse* : *place* 222-24; cf. the self-rhyme *lustynasse* : *clannasse* 318-20. Spoiled are *godenesse* : *was* 233 (presumably from *-nasse*, as *Editha* 822); and *falsnesse* : *case* 133, *y-wys* : *godenesse* 263, *ywys* : *honestnysse* 916 (presumably from *-nys(se)*, as *Editha* 818 and 1147). The in-line forms are *clannasse* 212 and *heuynasse* 343 and 931, against 15x *-nesse* (*mountenesse* 8, *witnesse* 183, *godenesse* 276, 297, 815 and 938, *mekenesse* 326, *sekenesse* 343, 604, 893 and 895, *weketnesse* 364, *derkenesse* 367, *sca[r]senesse* 611, *gladnesse* 784).

[49] Salisbury Cathedral 39 (Samuels's analyses): hand A (fols 129r-39v) *wodnasse* x 2, *wytnasse*, *wyldurnasse*; hand B (fols 140r-42v) *furȝeuenasse*, *sweftnasse*; hand C (fols 143r-7?v) *gladnasse*, *wytnasse*. Emmanuel 27 (my analysis, from Dr Laing's transcript) *witnasse* 111v and 162rb, *forȝiuenasse* 162ra.

Otherwise, Samuels recorded *-nasse* only in hand B (fols 7-86) of Bodley Tanner 201, the dialect of which he assigned to Herefordshire (LP 7391). In the original analysis, *unclannasse* 85v is the only instance noted. It is unclear whether this is carried over from an exemplar, or whether it should be added to the list in *Atlas* IV.314; *-nesse* appears to be the scribe's regular form. Sample readings of *MED*'s articles on the common '-ness' words have failed to increase the tally (note that the lists at the head of these articles record by no means all the variants appearing in the citations); the suffix is not independently treated.

Comparable formally but not dialectally is *-nas*, recorded from two Cumbrian documents (*Atlas* IV.305b).

[50] Emmanuel College, Cambridge, MS 27, whence LP 5380, is composite: see M. R. James, *The Western Manuscripts in the Library of Emmanuel College. A Descriptive Catalogue* (London, 1904), 22-27, where the contents are described as 'mostly of cent. xiii'. The three parts it now contains may be of diverse origins. The English items (fols 57v, 111v, 162r-63r) are all in Part II (fols 13-194), and apparently additions of the early fourteenth century. A possible connection of Part II with Salisbury, whether city or diocese, appears the text of fols 172v-75r, 'statuta dominorum episcoporum Sarum'. The front flyleaf is an independent document and clearly of Salisbury origin: Dr Richard Beadle, to whom I am grateful for re-examining the manuscript on my behalf, regards it as the kind of waste vellum commonly picked up locally and used to protect the outermost leaf of a manuscript proper. The text comprises two lists, (i) of the altars in Salisbury Cathedral, and (ii) 'nomina Regum in ecclesia Sarum', ending with Richard II. (For the text, see

James, p. 23.)

A connection with Chichester is suggested by a dedication to Sompting Church, *anno* 1246, added to the Kalendar at the end of the volume (fols 244-45); but this is in Part III, and therefore no evidence for the origins of Part II. Neither need it go against Salisbury as the place where Parts I-III were bound together: as a verdict on the whole volume, James's 'Probably from Chichester' (p. 22) goes beyond the evidence.

N. R. Ker rejected ascriptions both to Salisbury and Chichester (*Medieval Libraries of Great Britain* [London, 1964], p. 339); but he was concerned only with the holdings of named libraries, and his strictures can preclude neither city as the manuscript's place of origin.

[51] E. Maunde Thompson, *Catalogue of the Manuscripts in the Cathedral Library of Salisbury* (London, 1880).

[52] *MED* lists no such variants under *ēch* pron. For the 1 sg. pers. pron (s.v. *ich*) it reports only very early instances, viz *Ih* from the Lambeth Homilies (*c.* 1225) and the Cotton *Owl and the Nightingale* (*c.* 1250), and suffixed *-ih* in *natih* from *St Juliana* (*c.* 1225). I have not checked all of the remaining vocabulary in which such spellings might be found. The forms in Emmanuel 27 are listed on p. 244 above. The *Pater noster* in Salisbury Cathedral 82, according to Maunde Thompson's text, has additionally *ehd* 'increased' (OE *ēcan/ȳcan*: cf. MED s.v. *ēken*).

[53] Henry de la Wyle, chancellor [1313-1327]: so E. Maunde Thompson, *Catalogue of the Manuscripts in the Cathedral Library of Salisbury* (London, 1880).

[54] *Atlas* has no other record of *deies* or the like from the southern counties. The proper comparison is obviously with the singular, in which *ei/ey* is doubtless more widespread than in the plural (OE *dagas*), but SOU LPs record only 'days'. The point here is that *deilich* is at least consistent with a Salisbury placing.

[55] For Thomas Schort's career, see pp. 302-04 of N. Orme, 'A grammatical miscellany of 1427-1465 from Bristol and Wiltshire', *Traditio*, 38 (1982), 301-26; I am grateful to Professor Orme for bringing the manuscript to my attention. A detailed account of the manuscript appears in N. R. Ker, *Medieval Manuscripts in British Libraries. III Lampeter-Oxford* (Oxford, 1983), pp. 630-37. Citations of text are from Professor Orme's paper, by MS folio and item number.

[56] *Atlas* I.288a and III.696c err in glossing 'MHE' as 'Middle Hiberno-English': English was not a language of Ireland until the late twelfth century.