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THE OLD ENGLISH SHORT DIGRAPHS: SOME CONSIDERATIONS

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1. The objections of Kuhn and Quirk to various reinterpretations of Old English digraph spellings, including our own, have made it clear that extensive discussion of single structure points in the overall frame of the Old English phonological system is a wasteful procedure.[1] The system and the minimal oppositions which make up the system are coexistent and difficult to discuss intelligibly without going through the circular but internally consistent process of describing the one in terms of the other. Taking our departure from a pattern whose outlines were implicit but largely unstated, we attempted to deal with one point of the structure in detail. This kind of presentation seems to have been a mistake, and to have led to some of Kuhn and Quirk's misunderstanding of OP 4. The correction of our error of judgment about presentation must, however, await further research and publication, undertaken not to prove a thesis but to arrive at the most complete, consistent, and economical interpretation of the total evidence.[2] The present article, therefore, only points out some matters of fact and clarifies the basic disagreements.

2. Retractions and emendations are given below in three groups, followed by minor matters requiring brief clarification.

2.1. The first group emends errors and inconsistencies in OP 4 pointed out by Kuhn and Quirk. For these points (149, 144, 152, 146) we are pleased to acknowledge our debt to them.

2.11. (8.3-5) Strike out the sentence 'When secondary ... identical'. Discussion here under §7.4.

[1] The article by Kuhn and Quirk containing their objections appeared → *Lg.* 29.143–56 under the title Some recent interpretations of Old English digraph spellings. All three-digit page references given by us refer, unless otherwise specified, to that article, which is abbreviated as SRI. Our article appeared as Occasional Papers No. 4, of *Studies in linguistics* (1951), to which all one- and two-digit page references refer, unless otherwise specified. In the emendations, numbers in parentheses are page and line references to our article, which is abbreviated as OP 4.

A word about the symbols used for phonetic transcription, and about the printing of symbols that represent manuscript graphs.—Our phonetic symbols for vowels are those used by George L. Trager and Henry Lee Smith Jr. in *An outline of English structure* 11 (Norman, Oklahoma, 1951). The assignment of phones to phonemes in Old English is as follows: [ɪ] and [ɨ] to /i/; [ᴇ] and [ə] to /e/; [æ] and [a] to /æ/; [ʊ] to /u/; [ɷ] to /o/; [ω] to /ɔ/; [ʊ̈] to /ü/; [ɷ̈] to /ö/. [ɑ] was assigned by us in OP 4 to /ɔ/, the phoneme which is spelled *a* in the OE manuscripts, but we have since found reason to believe it should have been indicated to be a rounded vowel in the lower back corner.

There is an inconsistency between this article and OP 4 in the representation of *ae* and *æ*. In the printing of OP 4, it was found that the lithoprinter had no italicized *æ*. He therefore used italic *a* and *e* placed fairly close together, but rather closer sometimes than others. On the title page, *æ* was printed as *ae*. Since it is well known that *æ* and *ae* alternate freely and indiscriminately in OE manuscripts, we did not expect this would cause any difficulty, but a need for the clarification of it was indicated in SRI.

[2] Kuhn and Quirk have themselves pointed this out in item 3 of their conclusion (155).

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2.12. (13.24-8) For 'the off-glide ... following consonant' read 'there was no phonemic off-glide at all, but there was a distortion of the color of the vowel caused by the kind of transition that occurred between the vowel and a limited distribution of following consonants, very much as /g/, /k/, /š/, and /ž/ in MdE [Modern English] distort any preceding nucleus toward high front color. Extensive descriptions of such distortion are given by Martin Joos, *Acoustic phonetics* (Lg. Monograph No. 23, 1948), 101 ff.' Discussion here under §6.

2.13. (15.10) For '*sceaft*' in the Anglian column read '*scæft*'. Discussion here under §9.6.

2.14. (24-6) Split change 4 into two changes, numbered 4 for 'Darkening before back vowel' and 8 for 'back umlaut'. Place this new 8 after 7, and revise the old 8, 9, 10 to 9, 10, 11. The only feature of the chronology pertinent to this discussion is the dating of *i*-umlaut earlier than palatal diphthongization, and we make no emendation or retraction on this point.

2.15. (24.15) For 'OE /i/ phoneme' read 'OE /i/ and /e/ phonemes'.

2.16. It is significant that only the error corrected by §2.12 substantively affected the core of our description. The sentence excised under §2.11 was obviously contradicted by the rest of OP 4, especially 17–19.

2.2. The second group consists of corrigenda to OP 4 as printed. They are included for completeness' sake, though they were not called to our attention by Kuhn and Quirk.

2.21. (8.7) For 'see fn. 7' read 'see fn. 8'.

2.22. (13.39) For '/y/' read '/j/'. In OE, the postvocalic semivowel /y/ is in contrast with the spirant /j/ *ge*.

2.23. (16.20) For '2.125' read '2.126'.

2.24. (20.39) Read '[fæ͡x̥t]'.

2.25. (23.3) For '(p. 350)' read '(§291)'.

2.26. (31.23) Before the sentence beginning 'Furthermore ...' insert this sentence: 'As in the case of *ælf*, it appears that the expected forms would be *ea*'.

2.27. (35.33) For 'by all means' read 'by any means'.

2.3. The third group emends the wording of secondary inconsistencies that result from the emendations made under §2.1 and §2.2 above.

2.31. (8.15-17) Strike out 'and ... ways' for consistency with §2.11.

2.32. (13.28) For 'articulation' read 'darker vowel color' for consistency with §2.12.

2.33. (13.32-4) For 'the back quality ... /rc/ was' read 'the back articulation of the consonant produced the same distortion that the back resonant articulation of /lc/ and /rc/ did' for consistency with §2.12.

2.34. (15.9) For '/yæt/ [yæt]' read '/jæt/ [jæt]' for consistency with §2.22.

2.35. (20.10, 18) For '/y/' read '/j/' for consistency with §2.22.

2.36. (24.35-6) For '/k/, /g/, and /sk/ > /č/, /y/, and /š/' read '/k/, /g/, and /sk/ > /c/, /j/, and /sc/' for consistency with §2.22.

2.37. (26.10-2) For '*ie* ... investigation' read '*ie* is /i/ in breaking and velar umlaut positions, but probably represents /e/ after a preceding palatal—i.e. not different from *e* after a palatal in the other dialects, except for a difference in spelling tradition' for consistency with §2.15.

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2.4. We attempt now to clarify the matters of the macron, the allographs of ⟨æ⟩, dating, and sources (152).

2.41. What looks at first glance to be a very serious disagreement turns into one of terminology. We are in agreement that there are two marks in the MSS, one used for nasal abbreviation and another having in some way to do with 'length'. The functional use of 'macron' in OP 4 to refer to the latter mark conflicted with paleographical tradition, which reserves it for the former. This led to a misunderstanding by Kuhn and Quirk, and we therefore regret our transparent but nontraditional use of the term.

2.42. The fact that *ae* and *ę* are in free variation with *æ* in the MSS is so well known that without comment we used *æ* in OP 4 to subsume *æ*, *ae*, and *ę*.

2.43. The dates 700–900 were not intended as outside terminal dates, since they were defined to mean 'The Old English period ... which shows the language ... in a state of clear departure from its continental roots, but before later OE changes had operated to begin the differentiation of OE from Middle English. Roughly ... 700 to 900' (3). The forms selected for illustration show no features not found in the MSS of that period.

2.44. It was not our intention to 'present new material' (152) but to present an interpretation, not itself new in its gross features nor claimed to be, of well-known material taken from standard secondary sources, with only such checking of primary sources as might be needed for corroborative evidence. What was new was the attempt to achieve structural precision, in terms of phonemic theory, in describing spellings that many observers have evidently thought represented positional variants (witness Miss Daunt's and Mossé's similar opinions).

3. The serious criticisms in SRI that require more detailed reply are itemized below, and referred to hereafter as item 3.1, 2, etc.

3.1. Failure to provide a complete frame of reference or to be aware of the 'broader implications' (145) of the solutions proposed (esp. §2, §3, §5, §12).

3.2. Failure to make phonetic interpretation and phonemic segmentation clear (esp. §2, §3).

3.3. Failure to take accurate account of the later history of the items in question (esp. §7, §8, §10).

3.4. In revising chronology, failure to take account of a 'well-known test word' (146).

3.5. Failure to take adequate account of minimally or analogously contrasting pairs and other evidence from OE scribal practice (esp. §9, §10).

3.6. Failure to interpret accurately the effects of OI spelling practices upon those of the OE scribes (§6, directed primarily at Miss Daunt and Mossé, but including §2.225 of OP 4 and requiring answer by us).

3.7. Failure to make proper assumptions about what structures are possible in historical languages (fn. 11).

3.8. Failure to interpret certain spellings in accord with highest phonetic probability (144).

4. Item 3.1 of the criticism is partially valid. OP 4 did not contain a description of the overall pattern of the phonological structure of OE, because we

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thought a case could be made without it. The case still may not be invalidated by our decision to present these details outside of the structural context. In our conclusion (35) we made it clear that we were aware of at least some of the implications of the description.

5. Since we did not describe the overall vowel system in which /æ/ was one structure point, we submit it below in the briefest possible form, knowing that a summary statement, submitted without evidence, will be subject to criticism.

5.1. We posit a system of eight simple nuclei for OE. They were /i e æ ü ö u o ɔ/. Key words are *smiþ*, *menn*, *lædder*, *kyssan*, *doehter*, *under*, *bodig*, *man*. These were the 'short vowels'. The short diphthongs were the back allophones of the three front vowels, and were phonetically central. The phonetic structure of the allophones of the three front vowels was thus:

/i/ = [ɪ]; [ɨ] *smiþ*; *ieldra*
/e/ = [ᴇ]; [ə] *menn*; *eorþe*
/æ/ = [æ]; [a] *lædder*; *eaht*, *fealdan*

We do not yet know except in part the distribution of the allophones of /i/ and /e/. What we were investigating was the distribution of the allophones of /æ/, [æ] and [a], which we referred to as the 'front' and 'back' allophones and recorded in OP 4, to avoid ambiguity with the spelling *a*, by the symbols [æ] and [æ>]. [a] is structurally a more probable way of symbolizing the phonetically central but, relative to the front allophone, BACK allophone of /æ/; it has no connection with the phonetically back, probably rounded phoneme /ɔ/, which was spelled *a*.

5.2. Besides the eight simple nuclei, we posit a set of complex nuclei consisting of these eight combined with /y/, /w/, and /h/. These were the 'long vowels' and 'long diphthongs', but if this description is accurate, there were really three kinds of 'length', 'diphthongs', or 'complex nuclei' in OE: /Vy/, /Vw/, and /Vh/.

5.3. The implications of this hypothesis (which is more readily handled by comparison with Trager and Smith's *Outline of English structure*[3]) are too considerable not to be approached by us with caution. In OP 4, we were taking simply the first step in the testing of the hypothesis, and it happened that we started in the lower left-hand corner of the vowel quadrilateral.

6. Item 3.2 results from what we consider our most serious mistake. Kuhn

[3] Op.cit. In MdE all the possible combinations of /V/ with /y w h/ are demonstrated to occur. In OE, not all can be demonstrated to occur at any one period, but granted this kind of structure it is probable that many of the blanks were filled by unrecorded alternants that occurred in actual usage. At any rate, the assignment of specific spellings to specific slots is a problem in diachronic analysis, carried out by examining (1) what the nucleus now is; (2) what it can be reconstructed to have been in Pre-OE; (3) what course of change can be charted for its passing from one of these end points to the other without crossing and without violating reasonable postulates about the nature of the spelling evidence. It is not pertinent to list here in detail the specific assignments we make of all the OE 'long vowels' and 'long diphthongs' to /Vy/, /Vw/, and /Vh/, since the immediate problem is the short digraph *æ* and the short diphthong *ea*. A sample item with its complete diachronic development will illustrate the utility of this kind of frame. Pre-OE */grṓhne/ > OE /gréhne/ > EME /gríhne/ > LME /gríhnə/ > EMdE /gríyn/ > MdE /grɨ́yn/, /grɪ́yn/ (the first one 'standard', the second in the dialect of some Philadelphians — Smith, *Lg*. 28.147).

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and Quirk write: 'The short *ea*, in breaking and velar-umlaut positions, they describe as a "back allophone" of /æ/ (spelled *e*) plus an off-glide [ə] as part of the following consonant (13)' (144). We quote the disputed passage in full, with a double bar before the portion that we now thoroughly emend (§2.12 above):

> /æ/ plus phonemic off-glide /h/ (or in whatever phonemic slot it may turn out to be necessary to write this central off-glide) can be phonetically indicated by a formularization such as [æə̯]. This is *éa*, possibly also *ǽ* (we do not wish to commit ourselves beyond the statement that at least one of them was /æ/ plus central off-glide). The off-glide is A PART OF THE SYLLABIC NUCLEUS. In the case of the back allophone of /æ/, which was *ea*, ||the off-glide which may be assumed to have produced the two-segment writing was A PART OF THE ARTICULATION OF THE FOLLOWING CONSONANT.

Note that *ĕa* is not recorded by us as [æə̯]. There is ample indication throughout the article of the precise phonetic interpretation we make for *ĕa* (11, 12, 17, 22, 24, 25, 36), and there are no two-segment transcriptions of *ĕa* given. They are all simply [æ$^{>}$]. The [æə̯] transcription for *ĕa* is the interpretation given by Kuhn and Quirk to the part of the above quotation after the double bar. Their reading of the quotation is a possible one, but the statement as it stood was ambiguous, and its ambiguity could be clarified only by an effort to interpret it in the context of our [æ$^{>}$] transcriptions and other pertinent remarks. The quotation probably represents a stage through which one passes in arriving at our present position (as represented in §2.12). Marjorie Daunt's position was this same one, demolished by SRI §3 and OP 4 fn. 6. At any rate, the retracted portion of the quotation was inconsistent with the rest of OP 4, and §2.12 above removes this inconsistency. That the rest of the description did not rest upon this sentence is evidenced by the fact that only the most minor secondary changes, §2.32 and §2.33, are required by the primary change.

6.1. We wish to show clearly how different our segmentation is from what Kuhn and Quirk think it to be. The accompanying table shows the contrast.

	Segmentation according to Kuhn and Quirk	Segmentation as based on our description	Phonemic transcription
héah	h-ea-h	h-ea-′-h	/xǽwx, xǽhx/
neaht	n-e-ah-t	n-ea-h-t	/nǽxt/
geat	ge-a-t	ge-a-t	/jǽt/
giefan	g-ie-f-a-n	gi-e-f-a-n	/jéfɔn/

In short, *ĕa* (and we see no reason not to include *ĕo* and *ĭe* in this detail) always acts as a single graphemic segment except in the situation where a palatal consonant needed to be distinguished from a velar consonant that must otherwise be orthographically homographous: thus the *eo* of *geong* bears no more relation to *eo* in nonpostpalatal positions than the *ea* of *fisceas* bears to *ea* in nonpostpalatal positions. The two *ea*'s get mixed up in items like *geat* (where **geæt* would be required for consistency's sake) simply because of the well-established

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fact in OE scribal tradition that trigraphs were not permitted (they occur only in a very few early MSS, and there sporadically).

6.2. The entire argument of SRI §3 has no bearing on OP 4, since it argues against a system of segmentation which we neither suggested nor would suggest. In fn. 6 of OP 4, we suggested the same criticism of Miss Daunt's system of segmentation that Kuhn and Quirk level in more detail under their §3.

7. Turning now to item 3.3, we examine the question whether the later history of the OE items in question is such as to render our description improbable for the OE period. Kuhn and Quirk's assertion that 'Miss Daunt and Stockwell and Barritt appear to be under the impression that OE *æ* and *ea* developed identically in Middle English, and that OE *e* and *eo* fell together in a similar manner' (149) is partially inaccurate, since we did not deal with the history of *e* and *eo*. We wrote that *æ* and *ea* developed identically 'when secondary developments do not intervene' (8) to split the allophones. We went on to say, however, that 'When secondary influences operate, they operate to affect *æ* and *ea* in identical ways when all other conditions are identical.' This statement we have corrected under §2.11 and, secondarily, §2.31. We were aware that the developments of *e* and *eo* in ME were NOT identical, but it was not pertinent to our discussion to say so. We assume that *e* and *eo* developed differently, that a single OE phoneme /e/ with two allophones [ᴇ] and [ə] split into two ME phonemes /e/ and /ə/, and that a single OE phoneme /i/ with two allophones [ɪ] and [ɨ] split into two ME phonemes /i/ and /ɨ/. The reason why a similar statement does not have to be made for the OE /æ/ phoneme is that it did not split in ME as /i/ and /e/ did. We posit the following simple nuclei for ME: /i ɨ u e ə o a/. Probably, judging from its later history, especially before /r/, the ME phoneme /a/ had allophones ranging along the entire bottom row. We posit complex nuclei with /y w h/ in ME just as in OE and MdE.

7.1. The question is, then, whether our description of the derivational history is contradicted by Kuhn and Quirk's evidence (149–51). Their first set of data (150), presumably not complete but a fair sampling, includes *Estharabyar*, *Trendelbiare*, *Wydebyer* (< WS *-bearu*), *Fiernham* (< WS *fearn*), *Vialepitte* (< WS *fealw-*), *Dyalediche* (< WS *Dealla-*), *Piarrecumbe* (< WS *pearroc-*), *la Hyele*, *la Hyales* (< WS *healh*).

7.11. Their argument from these items is that OE *ĕa* turns up in Southern ME spelled *ya*, *ia*, *ie*, *ye*, while OE *ǣ̆* never turns up spelled this way, and that since they were phonemically different at this stage, why do we not describe them as phonemically different in OE? Yet they would not suggest that because /f/ and /v/ are phonemically different in MdE, they were therefore phonemically different in OE. It often happens in the history of a language that a pair of conditioned allophones split into contrasting phonemes when the conditioning environments change or are lost. It also happens that one of the allophones of a phoneme may fall in with those of a different phoneme and so come into contrast with the class of which it was earlier a member. Such an instance is well validated in Proto-Gmc., where earlier /p t k/ after /s/ fell in with the new /p t k/ from pre-Proto-Gmc. /b d g/ subsequent to the time that earlier /p t k/, except after /s/, had become /f θ x/.

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7.12. There are at least two possible ways of interpreting these SME *ie*, *ye*, *ia*, *ya* spellings, and they add up to the same total: by whatever sequence of change these items arrived at their SME shape, in SME they had the same syllabic nucleus as items derived from OE *ēa*, and it does not matter in the slightest whether Kentish only differs from the other dialects in employing a special graphic device (Wyld, *History of modern colloquial English* 42 [New York, 1920]), or whether these graphs actually indicate a 'rising diphthong', i.e. /ya/ or /ye/, resulting from a shift in the nuclear center of some kind of normal OE complex nucleus like /æw/ or /æh/. Since these originally SHORT *ĕa* items cited by Kuhn and Quirk have the same nucleus in SME as originally LONG *ēa* items have (SME *diath*, *dyaþ*, *dyeaþ* < WS *dēaþ* 'death', SME *dyaf* < WS *dēaf* 'deaf', SME *lyaf* < WS *lēaf* 'leaf', etc.), it can only be concluded that the allophones of WS /æ/ have gone their separate ways under pressure of secondary conditioning influences, and that the back allophone of /æ/, when lengthened, has in this area fallen in with the complex nucleus *ēa*.[4] The details of our interpretation of these items are specified below.

WS *fearn* shows *ĕa* before a lengthening cluster. Reflexes of it may be from *ēa*, and this in spite of the three-consonant cluster in *Fiernham*, a piece of evidence which would appear to indicate some special kind of transition between the *-n-* and the *-h-* (open or 'plus' juncture?). Whether this explanation, admittedly a priori, is valid or not, the fact is that other reflexes of the same item in the same area show no evidence of a diphthong: *Ferleia* (Devon, 1086), *Fernlegh* (Devon, 1238), *Farnlegh* (Devon, 1238), *Fernele* (Devon, 1273), *Verlegh* (Devon, 1330).[5] Reflexes of *pearroc-* show a variety of spellings with none of the consistent diphthongal indications that *ēa* reflexes show: *Pedracomba* (Devon, 1086), *Parrecumbe* (Devon, 1238), *Parracombe* (Devon, 1291), *Perecumbe* (Devon, 1281), *Pearecumbe* (Devon, 1297), *Pearrecombe* (Devon, 1308), *Piarecomb* (Devon, 1303), *Parkcumbe* (Devon, 1281).[6] Some reflexes of *bearu*, *fealw-*, and *healh*

[4] Interesting corroboration of this interpretation appears Kuhn's review of Henning Hallqvist, *Studies in Old English fractured ea*, *Lg*. 26.319–23 (1950): 'In Chapter 1, Hallqvist argues that a raised pronunciation of the first element of OE "fractured" or "broken" *ea* altered the diphthong from [æa] to [ɛa], or even to [ea] in southern England. The evidence consists of three groups of spellings: (1) *ia*, *ya*, *ie*, etc., as in *-biare* < OE *bearu* "grove", or *fiern-* < OE *fearn* "fern" ...' (320). Kuhn discounts (2) and (3), saying 'The best evidence is found in the spellings of group 1' (320). He goes on: 'It is significant that the first element was raised so far as to be heard occasionally as [ɪ] or [i]' (320). Then taking up the survival of these items in ME, he agrees that 'In general the area in which evidence of a diphthongal pronunciation is strongest coincides with the area of raising ...' (321). Then 'Chapter 3 deals with stress-shifted diphthongs, OE [ǽa], which become [æá], e.g. in ME *yald-* or *yold* (< OE *eald*), *yern-* (< OE *earn*). The evidence of Devon is decisive ...' (321). We do not see how it is possible to gather from this material, added to the evidence of SME forms like *dyaf* < OE *dēaf* cited above, any conclusion except that all these SME spellings *ia*, *ya*, etc. are of the same structure in ME, regardless of diversity in source, i.e. regardless of whether they are obviously from original OE *ēa*, from lengthened *ĕa* before lengthening clusters, or from *ĕa* lengthened by processes not now described in the traditional handbooks but nevertheless lengthened.

[5] J. E. B. Gover, A. Mawer, and F. M. Stenton, *The place-names of Devon* 106 (London, 1931–2).

[6] *Ibid*. 66.

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show results in SME which are predictable, within our frame, only in terms of what regularly developed from OE *ēa* in that area. It is therefore our assumption that a diachronic descriptive statement must be made, applicable to items of this sort in this area, to the effect that the loss of *-u*, *-u/w-*, and *-h* produced doublets of which one alternate had compensatory lengthening of the preceding nucleus. This does not accord with any presently accepted view, but only, we believe, because the necessity for making such a statement did not exist in the frame of reference that treated OE *ĕa* as a diphthong. This leaves *Dealla*, which we cannot now explain. We expect that the same kind of thing is involved as was described by A. A. Hill in his discussion of the place name *Yapton.*[7]

We agree that, in this area, 'it is necessary to depend largely upon place names for evidence of local pronunciation' (150). We think, however, that a variety of spellings reflecting a single OE source should be weighed carefully. For example, a large number of the items derived from OE *ĕa* in this same area, including numerous examples of the items cited in SRI (150), are spelled in ways that would be difficult to interpret as diphthongs: *Bara* < WS *bearu* (Devon, 1265), *Parrecumbe* < WS *pearroc* (Devon, 1238), *Falepitte*, *Valeputte*, *Falewill* < WS *fealu* (Devon, 1321, 1339, 1249, 1254), *Caldecumba*, *Chaldecumbe* < WS *ceald* (Devon, 1167, 1244), and so on at great length.[8]

7.13. Since these ME reflexes of OE *ĕa* in length-conditioning environments are not in contrast with the reflexes of *ēa*, but have in fact fallen in with them, it would seem to be difficult to say that these items (150) represent anything except instances of lengthening, regular or analogical, with entirely normal subsequent histories. In short, these and similar items do not show that *ĕa* was a diphthong, but only that *ēa* was one, with which we are in complete agreement.

7.2. The real test of our description is not whether items can be found (whether among place names, which have always constituted a special problem because of the conservatism that marks their spelling, or in unequivocal sources like the Katherine Group of MSS) that seem to offer difficulties in accounting for their particular shapes. The real test is rather whether, anywhere in a given dialect area of ME, sources can be shown to reveal a regular three-way contrast among the reflexes of OE *ǣ̆*, *ĕa*, and *ēa*. It is a question of how many oppositions were maintained in the structural system.

7.21. The SRI evidence from MS Bodley 34 of the Katherine Group is of this latter type. Out of many hundreds of forms that could have been chosen, the list that appears in SRI (150) is short and selective, accompanied by the qualification that Kuhn and Quirk 'do not wish to suggest that these spellings are distinguished with perfect consistency, although the regularity of the MS Bodley 34 seems extraordinary when one compares it with most MSS of the 13th century' (150-1). What, then, DOES the list suggest? It suggests that one return to the MS and arrange the spellings according to the number and type of orthographical contrasts that were maintained to see if a three-way contrast among the reflexes of OE *ǣ̆*, *ĕa*, *ēa* can be shown to have existed. We find that it is not possible to arrange them so as to show such a three-way contrast. Presumably most

[7] *Lg.* 28.278 (1952).

[8] Gover et al. 32, 66, 314, 310, 60.

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scholars would agree, either as regards MS Bodley 34 or other ME MSS, (1) that all the *a* spellings for the stressed vowel would represent 'short *a*' or 'long *a*', which we would write as /a/ and /ah/; (2) that all the *e* spellings would represent 'short *e*', 'long open *e*', or 'long close *e*', which we would write as /e/, /eh/, and /ih/; (3) that the *ea* spellings would ordinarily represent 'long open *e*', but sometimes would represent one of the other nuclei already listed, though certainly NOT still ANOTHER nucleus (Kuhn and Quirk do not 'propose to interpret the *ea*-spellings of the Katherine Group as diphthongal' [151], which we interpret to mean that they would agree with most scholars in assigning these *ea*'s to one of the nuclei listed above, however labeled); (4) that the *ei* spellings would represent the obvious diphthong, which we would write as /ey/. If we are right in assuming that most scholars would agree that we have listed the maximum number of phonemic contrasts that can be assumed from orthographical evidence of the type suggested in SRI from MS Bodley 34, it evidently follows that the reflexes of OE *ĕa* fell in with various pre-existent nuclei from non-*ĕa* sources —specifically, the same ones that OE *ǣ̆* reflexes fell in with except under the secondary conditioning influence of lengthening, which split the allophones— and therefore do not constitute a separate structure point. If they do not constitute a separate structure point, then Kuhn and Quirk's assertions about contradictory derivational evidence (150-1) are not valid.[9]

[9] Of the sources listed in fn. 19 of SRI, we have examined both Mack and Einenkel closely, and d'Ardenne cursively. All of them lead to the same result, that no pattern of contrasts emerges which separates out OE *ĕa* reflexes as phonemically distinct. Like OE *ǣ̆* reflexes, they are spelled *e*, *a*, and *ea*, though more often *ea* than OE *ǣ̆* reflexes are because of the lengthening clusters (earlier breaking clusters) which caused them to fall in with *ēa*.

Mack has conveniently listed, with a careful etymological breakdown, a vast number of the correspondences between OE *ǣ̆*, *ĕa*, *ēa* source items, in all environments, and their spelling in *Seinte Marherete* (*EETS*, OS 193; London, 1934). We cite a number of these below, with page and line references, for the reader to examine. The examples have not been verified beyond Mack's text, and the etymologies are hers. Even if Mack is not accurate in every detail, the evidence still is quite clear.

OE *ǣ̆* (Vespasian Psalter *e*, Mercian *e*): *feder* 4.16, *fedres* 4.1, *feader* 18.29, *efter* 2.3, *et* 34.21, *þet* 20.2, *berninde* 12.3, *bearninde* 42.13, *hwet* 8.23, *hweat* 30.7, 32.2, *ber* 46.15, *bree* 12.18, *wes* 2.19, *hefde* 4.20, *wecchinde* 36.19, *wesch* 44.8, *beað* 44.4, *feat* 40.18, *weater* 44.2, *wleatewile* 28.24, *stealewurðe* 36.18, 38.6; Mack calls the following forms 'retractions' from OE *ǣ̆*: *war* 38.10, *unwarre* 32.16, *þat* 52.28, *blackre* 24.22, *attri* 32.15; OE *ǣ̆* after palatal consonants: *scher* 50.29, *biȝet* 4.4, *forȝet* 22.5, *ischepen* 46.21, *frumscheft* 46.20, *schefte* 26.25, *nebschafte* 10.11, *schape* 10.10, *schal* 6.16 (Mercian retraction of *æ* to *a* before *l*); deriving from Mercian forms with 'back umlaut' of *ǣ̆*: *heatele* 14.30, *eateliche* 28.11, *teaperes* 42.13, *nease* 20.27; due, according to Mack, to analogies of various sorts: *þeauie* 32.21, *gleadien* 48.24, *glede* 16.13, *heatieð* 38.35, *geapede* 20.35, *dearie* 38.25, *fearen* 44.22, *forfeare* 18.14.

OE *ĕa:* OE *ea* + *r* + *C*: *bearmes* 52.12, *dear* 38.25, *earme* 28.14, *hearm* 20.5, *wearð* 10.2 (elsewhere *warð*), *smertliche* 50.28, *scherpe* 50.29; before lengthening groups: *bearn* 24.29, *bearnes* 52.17, *eardið* 22.16, *bern* 38.31, *berd* 20.23; *a* is found uniformly after *w* except in *wearð* 10.2; *smeortliche* 22.8 (perhaps through influence of *smeorten*); OE *ea* + *r* + *c*, *k* (with Anglian 'smoothing'): *sterke* 36.18, *sterclukest* 32.33, *sperclede* 20.28, *merke* 12.12 (perhaps ON *merki*), *stearc* 20.33; OE *ea* + *r* + *C* + *i*/*j* (Anglian *e*; WS *ie* > *i*, *y*): *merren* 10.4, *derue* 28.15; *ideruet* 36.21, *snercte* 42.15, *ȝerde* 26.23 (before lengthening cluster); from Anglian *æ* (umlaut of unbroken *a*): *charden* 8.12, *awariede* 12.17, *dearne* 18.25, *dearnliche* 32.15; OE *ea* + *h* + *C* (Anglian *æ* by 'smoothing'; LWS *e*): *feaht* 52.27, *feht* 4.3; *mahte* 16.18,

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7.3. All of SRI §8 is devoted to proving that in ME the reflexes of OE *e* and *eo* were members of different phonemes. We are in complete agreement that the ME reflexes of *e* and *eo* were different phonemes. But the split of OE *e–eo* into two ME phonemes does not prove they were two phonemes in OE, nor does it mean that *æ–ea* had to split in the same way. The examples of §8 pertain to the *e–eo* split, not to *æ–ea*.

7.4. The set of ME spellings cited in SRI §10 bears out our position, as altered by §2.11 above, that Kuhn and Quirk may possibly be ignoring intervening sound changes that caused *ĕa* under specific conditions to fall in with *ēa* and yield reflexes identical with those of the complex nucleus it had become under those conditions. Note that the examples *biarn*, *wiarþ*, *getiald*, *sialde*, *syelde* show the vocalic nucleus before a possible lengthening cluster (/rθ/ was a lengthening cluster when the voiced allophone [ð] was conditioned by a voiced environment; thus *wiarþ* presumably shows a lengthened vowel by analogy with other forms in the paradigm that required the voiced allophone of /θ/). The *ie* in *sielt-* is a spelling not of *ĕa* but of *ĭe*, which is a different problem; the *y* of *cyealf* may as easily be said to indicate the scribe's desire to indicate the palatal quality of the initial consonant as 'to indicate a more palatal first element' (153) in the vowel. Furthermore, 'the scribe frequently replaced *ēa* (WGmc. *au*) with a similar range of spellings' (153). Except for *sielt-* and *cyealf*, these items evidently show reflexes of *ēa* and fail to present any problem to our interpretation of OE *ĕa*. There would appear to be no reason why lengthening should effect *ĕa* and *ǣ̆* identically (153): this is one of the ways in which phonemic splits occur, only in this instance both allophones, when lengthened, fell in with pre-existent complex nuclei. Where these complex nuclei fell together, as they did in some dialects but not in others, lengthened *ea* and *æ* also fell together.

7.5. We will leave to Mossé the problem of answering the first paragraph of

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2.16, *strahte* 20.34, *arahte* 2.15, *waxeð* 24.32; Anglian *a* + *l* + *C* (WS *ea*): *alle* 4.7, *fallen* 32.2, *halt* 48.3; Anglian *al* + *C* + *i/j* (Anglian *æ* alternating with *e*): *welle* 14.6, *smellen* 10.26, *auellet* 26.7, *melten* 14.33, *helde* 4.20, *welden* 4.28, *wealden* 14.18, *wealde* 48.31, *wealdest* 22.15, *afeallen* 24.4, *mealteð* 36.9.

OE *ēa*: *bileaue* 24.32, *deade* 2.20, *deað* 2.3, *earen* 4.7, *heaued* 44.15, *leat* 30.3, *beleue* 6.25, *beteð* 12.16, *deð* 12.13, *bred* 20.18; OE *ēa* + *ȝ*, *h*, *c*, Anglian and LWS *ē*: *deh* 2.17, *dreh* 4.6, *ehnen* 20.2, *heh* 8.10, *hec* 34.14, *steah* 2.5, *beah* 50.21; OE *ēa* + *i/j* (non-WS *ē*): *heren* 36.2, *nede* 38.18, *schene* 10.11, *leue* 12.3.

OE *ǣ*[1] (WGmc *ā*): *dede* 52.2, *strete* 40.29, *forletest* 14.16, *drede* 12.11, *hwerto* 38.21, *wepnen* 32.32, *beren* 52.11, *breken* 44.16, *cweþen* 8.13, *isehen* 16.28, *read* 14.10, *reade* 14.22, *þear* 14.10, *fearlac* 22.5, *unmeaðliche* 34.28, *reasde* 24.10; OE *ǣ*[2] (OE *ā* + *i/j*): *biteache* 46.12, *cleane* 32.1, *gleam* 20.32, *heale* 14.21, *healent* 2.18, *heaþene* 14.28, *leade* 32.24, *leaf* 8.10, *ear* 16.3, *bitechen* 12.7, *glem* 28.31, *flesch* 16.5, *unwreste* 32.11, *lesteð* 2.23, *heþene* 4.16, *ilened* 2.23, *lest* 30.18, *lef* 28.8; with shortening of *ǣ*: *ledden* 46.5, *þreste* 20.28, *lefdi* 12.4, *leafdi* 28.8 (8 times), *wreððe* 42.31, *wreaððe* 22.11, *wraððe* 16.2, *fleschliche* 34.22, *wreastlin* 32.26, *totweamde* 40.24, *healewi* 32.16, *earst* 32.14, *hahte* 4.30, *lahte* 20.29, *bitahte* 4.26, *rahte* 24.11, *bitaht* 6.25.

OE *e* remained *e*: *astenche* 28.24, *besmen* 12.17, *biset* 10.23, *helde* 4.20, etc.

None of these lists are complete. We make nothing of them except (1) that the regularity is not especially astonishing when one looks at them to see if *ea* can be isolated out as representing a separate and distinct structure point, and (2) that they certainly do not reveal a pattern of contrasting reflexes of *OE ǣ̆* and *ĕa*.

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153, since we agree with Kuhn and Quirk that *i–ie,io* and *e–eo* split apart in ME. The phoneme which resulted from OE *ie, io* (ME /ɨ/) and the phoneme which resulted from OE *eo* (ME /ə/) were not by any means identically distributed from dialect to dialect, any more than they are today. Only on one point is an observation required: the fact that one set of allophones of a pair of phonemes fall together in a specific situation while the pair of phonemes remain in contrast in other situations does not require any explanation of why the phonemes remained distinct, contrary to Kuhn and Quirk's assumption (153.11-4). In many dialects of modern American English, the allophones of /e/ and /i/ before /n/ and /ŋ/ have fallen together so that there is no contrast between them: thus *pen–pin*, etc. But the contrast between /e/ and /i/ elsewhere is not inhibited by the reduction of opposition between them in these specific environments. The reduction of opposition between OE *io–eo* is exactly parallel, and is, in fact, difficult to explain otherwise.

7.6. We have spent a disproportionate amount of space in discussing the derivational evidence, because our earlier treatment of it was too brief. But while Kuhn and Quirk are right in asking a closer study of it, the study only reveals that the derivational evidence is, as we originally wrote, noncontradictory to our description.

8. A correction of a detail of the chronology which did not pertain to this problem was made under §2.14. The criticism summarized under item 3.4 is built around the item *cȳse*.

8.1. Kuhn and Quirk consider *cȳse* a test case. There is no form **cīese* in the MSS, even though the item occurs in some MSS at a date before *ȳ* or *ī* had REGULARLY replaced WS *īe*. The replacement had begun to occur, but the process was gradual, and, for instance, in Wright's *Vocabularies*[10] the following 10th- and 11th-century spellings with *īe* appear (all of which were later spelled with *ȳ* or *ī*), even though in the same vocabulary *cȳse* is spelled with *y*: *cīepeman, pīe, flīete*. These happen to be the only items in this vocabulary which are expected, etymologically, to have *īe*. There are none that show *īe* changed to *ȳ*. Since **cīese* is the critical step in the reconstruction for proving the chronological sequence that Kuhn and Quirk consider likely, it is strange that **cīese* does not turn up in a MSS evidently written before the late WS change of *īe* to *ȳ* or *ī* had become general. We are not questioning the traditional dating of the change from *īe* to *ȳ*. We are merely pointing to a specific piece of data in which one might legitimately wonder why **cīese* does not occur instead of *cȳse*.

8.2. If *cȳse* is a test case, it is a curiously circular one, since no matter how often it appears in the MSS it is an isolated item which has no etymological parallels throughout every step of its reconstruction. Unique etymologies are not ordinarily used to establish sound laws if the sound laws thus established contradict laws which are needed to describe a set of nonunique items. There is no question but that *cȳse* is a bona fide WS form. The question is whether items like *giest, ciefes, ciele, cietel,* and *scieppan*, which in the traditional frame can be explained by placing *i*-umlaut EITHER before OR after diphthonging by the initial palatal (since *ǣ̆* was subject to *i*-umlaut but *ǣ* was not), do not outweigh the

[10] Thomas Wright, *Anglo-Saxon and Old English vocabularies* 1.258–83 (London, 1884).

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evidence of *cȳse* in a frame where *i*-umlaut must be stated to precede diphthonging by the initial palatal, leaving *cȳse* without parallels. Our position is that by describing certain phonological patterns more rigidly we necessarily find that a few items traditionally easy to explain become difficult, but that such difficulties are justified by the precision with which the phonological structure can be described and related to its living derivative.

Luick[11] was aware that the *ie* cases did not really parallel the explanation of *cȳse*, but neither he nor later scholars questioned Sievers' reconstruction[12] of the intermediary stages, especially the form **cīese*, because in their frame of reference the form posed no problem. Nevertheless, the traditional placing of diphthonging after the initial palatal in a chronological position before *i*-umlaut affects only *cȳse* and therefore cannot be proved right or wrong, much less be used as a test case.

Late WS developments have no bearing on our argument that *cȳse* requires explanation by a sequence of sound laws that have been tailored chronologically to fit only the one item. It is clearly neater to be able to treat *cȳse* like *scieppan*, *giest*, etc., and the traditional frame succeeds in doing this. Nevertheless, a scholar puzzling over *cȳse* has only two facts to work with: the pre-OE reconstructed form **kǣsi/j* (phonemically, as analyzed by us, */kǽhsi/), which is morphophonemically unique, and the OE spelling *cyse* (phonemically, as analyzed by us, /cíhse/ in early OE, /cíyse/ in later OE). Since its history is unique, the only 'explanation' for it is the reconstruction of an intermediary stage */céhse/. The language is full of items like this which show no direct parallels. OE *dyde* 'did' is one, MdE /rúm/, /rúwm/, /rów m/ (all shapes of *room*, the third one heard from a Richmond, Va., speaker by a qualified observer) is another. In a language such as English with an overall pattern that permits idiolectal and dialectal variation, within the limits of the pattern, in the selection of the phonemic shape of syllabic nuclei, the eventual survival of more than one alternate shape of a given item is expected. To suggest this is not to deny the principle that sound change occurs regularly and systematically; it is merely to suggest that 'regularity' is to be defined in terms of the PATTERN of change.

9. The criticism of item 3.5 begins with the mistaken assertion that 'Stockwell and Barritt maintain that there are no minimal pairs in Old English distinguished by *æ* and *ea*' (154). §§2.22 ff. of OP 4 list ORTHOGRAPHIC minimal pairs and explain why they do not represent PHONEMIC contrasts. Our assertion was that it would not 'be possible to find numerous examples, within any single dialect, of paired items which contrast by virtue of the *ea*–*æ* distinction alone' (27).

9.1. Before we can handle the orthographically contrasting pairs of 154–5, it is necessary to describe a point of methodology at which Kuhn and Quirk are at odds with us: repeatedly Kuhn and Quirk refer to 'what SOUNDS [emphasis ours] the digraph spellings represent' (143) and appear to assume that the spellings of OE MSS can give evidence about PHONETICS (thus: '... short diphthongs were phonetically distinct from short vowels' [156]—a statement with which we agree,

[11] *Historische Grammatik der englischen Sprache* §197 (1921).

[12] Miscellen zur angelsächsischen Grammatik, *PBB* 9.206 (1884).

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for different reasons and with specific limitations as stated here and in OP 4). With us, it is a procedural assumption that in describing a historical language, one works first from the orthography to find the PHONEMIC CONTRASTS that are represented, and from this, plus etymological evidence, especially the evidence of the living derivative of the historical language, one works out a phonemic structure which then enables him to make reasonably probable guesses about what the phonetic facts may have been in order to be consistent with the particular types of structuring they appear to have had.

9.2. The chasm between these two approaches cannot be bridged by discussion: they are different, and ultimately cannot both be valid. Either a scholar assumes that medieval scribes were something like phoneticians and could therefore, in a free spelling system, be trusted to spell with only a small margin of error, or he assumes that these were native speakers struggling along with an orthography which (1) monolinguals did not introduce; (2) was not ideally suited to the structure of their language; (3) was, from the earliest dates that it remains to us, already bound loosely to a number of traditionalized spellings. We believe the last three conditions to have held for OE, and we have proceeded accordingly. The reverse assumption, that the OE manuscripts are to be used essentially as informants or as the records of methodical field workers, while not explicitly stated by Kuhn and Quirk, is implicit in such statements as these: 'We may point out that the use of *e* for a back sound, *a* for a more raised and fronted sound, is not in keeping with the usual practices of Anglo-Saxon scribes' (144); 'Miss Daunt and Mossé are not clear as to what sounds the digraph spellings represent' (143); '... if OE *ea* represents a back allophone of /æ/ ... it is strange to find it replaced by ME *ia*, *ya*, *ie*' (150) [Kuhn and Quirk have here ignored intermediate developments: lengthening (which produced a complex nucleus) and the subsequent stress shift within the nucleus]; 'ME scribes did not treat diphthongized and undiphthongized results of WGmc. *a* alike—evidently because they did not sound alike' (151) [§7.21 above shows this differential treatment to be less than decisive; the quoted lists (fn. 9) indicate that only a methodology that places the irregularities of manuscript phonetics before patterned contrasts could arrive at this conclusion]; 'While WS *e* remained in the spelling, the WS short *eo* assumed a number of forms ... *eo*, *u*, *o*, sometimes *ue* and *oe*, these variants being usually interpreted [we assume also by Kuhn and Quirk] as representing a front-round vowel' (151) [Why front-rounded? Why not simply some vowel for which no symbol from the Latin alphabet was readily adaptable—say /ə/?]; 'Certain of the forms ... give such indications of phonetic values as to preclude the possibility that *ea* and *eo* were either allographic variants of *æ* and *e* or minute allophonic variants of /æ/ and /e/' (153) [Nothing in OP 4 indicated that these allophonic variants were 'minute'; phonetically they were probably fairly considerable in order to be noticed when the language was reduced to Latin orthography]. Our disagreement with scholars who weigh heavily what they consider to be phonetic evidence from the MSS does not mean that we think of MS data as anything but primary evidence. The only question is whether one way of analyzing and interpreting this primary evidence is more fruitful than another.

9.3. If one assumes that there are many traditionalized, and, as it were, petri-

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fied spellings in the medieval MSS (and we do not see how one can assume otherwise), then he must examine exceptional spellings which contradict the phonological structure that seems probable from the evidence of the majority spellings and etymological evidence to see whether they are (1) numerically inconsiderable in comparison with the regular patterns of the majority spellings; (2) found in items that represent special situations, not on the phonological level, that would possibly generate unpredictable spelling variants; (3) in variation, even sporadically, with the structurally expected spelling. If one finds these conditions fulfilled, he may then write off such spellings as representing inconsistencies petrified by tradition, of the type to be expected from the scribal predicament described above in §9.2.

9.4. We attempted to show the special situations that created several of the apparent contrasts, but Kuhn and Quirk found these arguments (OP 4, §§2.22 ff.) too weak (fn. 31). The methodology within which the arguments fit was possibly not clear. They admit ('naturally there cannot be many' [154]) that the contrasts are numerically inconsiderable in comparison with the patterned spellings. Finally, several of their contrasts are not as real as they appear to be. The whole group with *ll* and *rr* in the oblique cases (*steal–steallas, weal–weallas, wear–wearras, fear–fearras*, 154–5) are probably subject to an explanation that is supported by their frequent occurrence in the MSS with final *rr* and *ll* spellings: namely, that the *ea* in the singular is the result of a spelling analogy, no more, with the allophonically significant spellings of the oblique cases, just as the phonemically nonsignificant final *rr* and *ll* probably represent spelling analogies with the phonemically significant double intervocalic consonants. In the oblique cases the phonemic double consonants condition the appropriate back allophone of /æ/. *Stæl* and *steal*, *wæl* and *weal*, *wær* and *wear*, *fær* and *fear*, etc. must then be assumed all to have had the front allophone of /æ/. The *cwealmes–ælmesse* pair contrast phonemically by virtue of an open juncture in *ælmesse* between /l/ and /m/ (an assumption that is purely descriptive and is neither supported nor denied by the reconstructed etymon **alimosina*) as opposed to close transition in *cwealmes*. While we do not expect this kind of argument to find ready acceptance from Kuhn and Quirk, at least Quirk, in his article On the problem of morphological suture in Old English (referred to in SRI fn. 28), has found it necessary to deal with transition phenomena in terms not very dissimilar to these. In spite of Kuhn and Quirk's scorn (fn. 31) for our discussion of the metathesized forms (§2.222), it still seems to us that these were traditionalized spellings, though there is possibly no way ultimately to prove the opinion. *Fersc* is also a metathesized form (OHG *frisc*, OI *friskr*, Lat. *fresca*), and neither it nor *mersc, elfan, selfum*, etc. (155) bear on *æ–ea*. We see no convincing reason to believe that items like *hneapedun* beside *hneapade* represent anything but SPELLING analogy within the paradigm. Similarly *steaþul–steaþelas, -feara–fearende*. There apparently are such things as orthographical irregularities, especially where allophonic differences are involved, which simply do not reflect the structure of the language.

9.5. We conclude, then, that most of the infrequent *æ–ea* spelling contrasts can be presumed to be nonphonemic, and that those few which remain, in the

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very nature of the orthographic situation, are insufficient evidence to destroy a structural hypothesis consistent with all but a fractional segment of the graphic evidence and fully consistent in its relation to both earlier and later stages of the language structure.

9.6. Kuhn and Quirk state that we presented 'unreliable new evidence' (153), but in describing the details of our unreliability they become involved in a contradiction: 'Anglian *gæt* is extremely rare in the early texts' (152); but 'In words of the type represented by *cæster*, *gæt*, and *sceaft* [which we revise to *scæft*], 8th century Anglian texts regularly have *æ* (also spelled *ae* and *ę*) [note that Kuhn and Quirk recognize the nonsignificance of this free variation; see §2.42 above] or *e*' (fn. 25). We interpret the phrase 'words of the type ... regularly have' to be a very satisfactory equivalent for our 'statistically predictable' spellings. Of the 'four forms in which [they] can have little confidence' (152), then, *sceaft* was an error, *gæt* is right in line with the statement of their fn. 25, *cæster* they admit to be 'typical in an 8th-century Mercian or Northumbrian text' (152), and our description of **cæfl* holds just as well for the attested form *cæfli* (their fn. 25), though we picked the wrong form to cite and appreciate the correction.

10. Turning to item 3.6, we observe that Kuhn and Quirk's comparison of Irish-English spelling habits, while it may adequately denude the arguments of Daunt and Mossé, is not directed sharply enough at the real point of this kind of argument: 'On the basis of what evidence there is about how OE was reduced to [Latin] writing ..., some degree of bilingualism on the part of the person or persons who did it must be assumed' (34). Once granted this, the consequences described by us inevitably follow: allophones may turn up fairly systematically distinguished in the orthography.

11. Item 3.7 apparently arises from Kuhn and Quirk's failure to recognize that our discussion was a normal use of the scientific criterion of simplicity (6). In this sense, simplicity is defined, and recognized, in terms of the number of hypotheses that are required to describe a set of data. Complexity of explication has nothing to do with the simplicity of the description in these terms. It requires at least one more hypothesis to describe the history of English within the traditional frame than it does within ours, namely that a syllabic structure with a four-way set of distinctions—V, V:, VS, V:S—changed to a syllabic structure with a two-way (or at most three-way) set of distinctions. We did not say that a four-way set was impossible, or that a language might not have a unique feature, but only that the description was complicated by the addition of one more hypothesis if such a structure was assumed.

12. We move on to item 3.8. We have touched on this matter of phonetic analysis earlier (§9.1, §9.2), but would like to describe here the specific limitations that we feel must be put upon any argument that works directly from symbols to phonetic values.

12.1. By analysis of the graphemics of the manuscripts, by which we mean an analysis of the regularly represented orthographic contrasts, one determines how many phonemes are needed to describe the structure. On the basis of what one knows about the phonetic values allocated to these graphemes in related orthographies, one then makes a guess at what general position and type of

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articulation the graphemes may have represented. At this point one cuts loose from the orthography and goes to etymology (by which we mean all that is known about the history of a set of items down to and including their structure today). In terms of the later history of the items in which a given position and type of articulation may be assumed, by analysis of the graphemics, to have been represented, one determines what various possible sequences of change could result in the shape or shapes of those items as they appear in the living language. One discovers that several sequences of change could have been possible, but not all of them would have been equally probable. Furthermore, one sometimes has evidence from the living dialects to indicate in what direction a given set of changes must have gone. One further knows that certain types of variations are more common than others in the living language, and without evidence to the contrary, one assumes that these are NOT innovations but are in fact the very kinds of things that have been going on in the language as far back as our evidence goes.

12.2. Within such a procedural frame, it is clearly impossible to place as much weight upon the presumed phonetic values of orthographical segments as several of Kuhn and Quirk's arguments appear to. Such phonetic interpretations are low-priority evidence compared with what can be inferred from the structural implications of a language's later history.

13. In conclusion, we respect the force of Kuhn and Quirk's arguments. We feel that they have done us a service in showing the need for refinement in several details of our interpretation and for clear explication of others. We do not feel that, even in terms of their frame of reference, their evidence was secure enough for them to reject our description so completely. Our own frame of reference is such as to suggest that the traditional position, represented by Kuhn and Quirk, is not a hypothesis of structure which shows meaningful relations between the LANGUAGE and its derivatives and ancestors, at all. Rather it suggests that the traditional position embodies, fundamentally, an acceptance of the idea that for historically recorded utterances, the spellings ARE the language system, rather than the idea that those spellings only symbolize the historical language more or less accurately—for the vowels, probably less rather than more.

The article which follows this one, and replies to it, makes a number of statements which cannot be allowed to stand unquestioned. This note, its numbering in accord with the sections of the following article to which reference is made, does not modify the present article at any point. It merely touches upon those points in the following reply which we feel need discussion.

§2.12. We thought, from our specific mention of MdE /g/, /k/, /š/, and /ž/ as consonants which distort any preceding nucleus toward high front color, that the analogy would be clear: in some dialects of MdE this phonetic distortion has been identified as phonemic by the speakers, while in others it has not: /búš/ > /búyš/, /ǽš/ > /ǽyš/, /lég/ > /léyg/, etc. In OE the distortion that was noticed was associated with certain consonant clusters, was transcribed first by bilinguals, and later became phonemic in certain of the distorting environments, the lengthening clusters (falling in, like the /Vy/ nuclei above, with preexistent complex nuclei).

§§5.1–3. It would be remarkable if OE *a* were not slightly rounded at the time of lengthening in the environment of lengthening clusters, since it regularly fell in with long nuclei with a rounded first component, and since OE 'long *a*' also developed as though it were

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rounded: OE *stān* /stɔ́hn/ > ME *stoon* /stóhn/ > MdE /stówn/; OE (Merc.) *ald* /ɔ́ld/ > OE after lengthening *āld* /ɔ́hld/ > ME *oold* /óhld/ > MdE /ówld/. Those *a*'s that remained unlengthened during EME times fell in which /æ/ from the low front and resulted in only one, certainly unrounded, low vowel for ME.

As for the interpretation of /x/ and /h/, we are quite agreed with Kuhn and Quirk that the initial and final *h*'s of *hēah* represent members of the same phoneme, which we write /x/. We cannot imagine what possible reason they have for assuming that the initial one was phonetically [h] and the final one was phonetically [x]. We assume they were both voiceless spirants of the [x] or [ç] type (backer or fronter according to the environment), but if there was any difference in the FORCE of articulation (as Kuhn and Quirk's symbols [h] initial and [x] final imply), then surely the initial was the more forcefully articulated, since it survived, while the intervocalic and final did not (except insofar as our hypothesis of voicing, e.g. /níxt/ > /níht/ > /níyt/ > /nɨ́yt/ > /nə́yt/ > /náyt/ 'night', is right).

The items cited by Kuhn and Quirk are not difficult to phonemicize in this frame: /blówwən/, /knéw/ (with /ww/ in oblique cases—possibly also finally—an inelegant bit of distribution, but semivowels often are different from consonants in distribution), /xǽwwan/, /lǽhwede/, /níwwe/, /sɔ́wwən/, /léwxt/, /líwxtən/, /nǽhx/, /θúhxte/, /wóhx/, and /wrɔ́hx/. The additional items cited in fn. 9 give pause at only one point: final /ww/. Perhaps *cnēow* was /knéww/. The others are /bű́y/, /bű́je/ (see §2.22 of the present article: /j/ later > /y/), /hǽw/, /hǽww/, /hű́jd/, /hű́yd/, /sǽh/, /sǽww/, /séw/, /séww/, /θéh/, /θéw/, /wǽyj/, /wǽw/, /wéh/, /wej/.

§§7–7.6. To say that 'apparently [we] agree with [their] contention that *æ* and *ea* ... did not develop identically in Middle English' is a misleading oversimplification of the area of agreement: we hold that *æ* and *ea* did develop identically except when secondary influences operated such that one set of allophones fell in with one phoneme complex, and the other with another. There was no split of *æ–ea* as there was of *e–eo*.

§7.12. The explanation offered here for the lack of uniformity in ME spellings of Southern place names is an example of one of the reasons why several scholars find that they can no longer operate satisfactorily within the traditional frame of reference of historical English phonology. Rather than assume that 'the Southern dialect area was not uniform' (an assumption which, without a frame of reference of the overall-pattern type used by Trager and Smith for MdE, amounts to the same thing as saying that the dialect cannot be described except by listing—that there is no pattern), or that scribal mixture explains the lack of uniformity, we prefer to assume that OE dialects (and ME, for that matter) were much like MdE dialects insofar as 'uniformity' and selection from an overall pattern are concerned, and that they are describable in similar terms. For this kind of description, see Henry Lee Smith Jr., review of Jones, *The pronunciation of English*, *Lg*. 28.147, as well as Trager and Smith, *Outline of English structure*.

§7.12, third paragraph. Is it not true that the 'regular' development of OE *ēa* is to MdE /iy/?—That is, we are surely agreed that *ēa* was a diphthong in OE; and since all its modern reflexes that are not explained by secondary laws (shortening, influence of /-r/ and /-l/, etc.) are surely diphthongs (/iy/ as in *Easter*), is there any reason to assume that they were not diphthongs all along?—OE /æw/, LOE /æh/, ME /eh/, LME /ih/, EMdE /iy/, MdE /iy/.

§7.21, first paragraph. Fortunately Kuhn and Quirk attach a footnote to their statements that 'macrons and circumflexes [and acutes?] which modern editors and grammarians place over the long diphthongs will not be found in the MSS' and in the footnote admit that acute accents do appear in the MSS, though, they say, sporadically. We are compelled to suggest that these scholars actually count the number of occurrences of such accents (over ALL vowel symbols) and correlate these with the etymological evidence for 'length'. They will find, especially from the 9th century on, that they occur far too frequently to be called 'sporadic' and that they are better than 85% 'right' in the sense that they correlate with the etymological evidence for 'length'.

§7.21, second paragraph. Throughout several draft versions of our Considerations we have been unable to set up any list from MS Bodley 34—evidence first introduced by Kuhn

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and Quirk and quite obviously taken from the same introductory material of Mack that ours is—that was deemed a fair presentation by them. In despair we merely listed data from Mack's phonology untouched, unreorganized, and without any slant whatever. If the obvious inconsistencies which appear in the MS can be written off as 'scribal errors', 'eccentricities', and 'admixture of Southwestern forms', then we have no common ground for discussion.

§8.1. We consider the argument about transition texts to be a completely circular begging of the question. There are no specific times at which a language can be said to be in a state of transition. Change in language, we assume, is nearly constant in rate and, even if change is not quite constant, it most certainly is NOT a sequence of several static periods linked by transition periods.

§8.2. We suggest emending 'in correct sequence', which prejudges the case, to 'in the traditional sequence'. We also maintain that the 'ambiguous' item is *cȳse*, not the considerable number of items like *giest*. We do not claim to have 'explained' *cȳse*, nor to have presented a sound-law for it. We merely say that it presents difficulties far too severe to entitle it to the status of a 'test word'. Finally, **kerfan* would become *ceorfan* by either their chronology or ours, and the apparent contradiction offered to our hypothesis by this and similar items is resolved when it is remembered, as we have pointed out (OP 4.16), that graphic triphthongs were not permitted. We have stated our belief (OP 4.24 as corrected by §2.15 above) that after a palatal consonant and not in a breaking environment the *i* of *ie* indicated ONLY that the preceding consonant was one of the palatals, just as did the *e* of *eo* in *geong*. In short, *ceorfan* ought to be spelled something like **cieorfan* or **ceeorfan* if the orthography were consistent with itself. The phonemic development of **kerfan* to *ceorfan* was, then, */kə́rfən/ > /cə́rfən/, which allophonically became [cə́rvən] by breaking. We have, therefore, not abandoned 'test-words': just one test-word, *cȳse*.

§9. The conclusions of Kuhn and Quirk about minimal pairs are questionable. Spanish is a highly inflected language, but we have no trouble finding minimal pairs for the vowels in great numbers. The vocabulary of OE, while preserved only in part, is certainly preserved in quite a large part—there is no difficulty in listing numerous minimal pairs for all but one (/ö/) of the OTHER vowel contrasts. Finally, minimal pairs are only one way of approaching the matter of phonemic contrast: the obverse of minimal contrast is complementary distribution, and there is further the matter of pattern congruity in the distribution. The latter criteria have been utilized by us more fully than they have in the traditional analysis of OE phonology.

§§9.2–3. We would accept Kuhn and Quirk's description of how one deals with a MS text as an adequate description of our own approach to such a text. The key procedure is that the scribes' 'system of transcription must be puzzled out before anything can be done with the evidence'. This is precisely what we believe they have failed to do.

§9.4. We answer the question of how the front allophone got into *steal* etc. by pointing out that it was there all along: there is no evidence that at the time of breaking 'the nominative singular also had the double consonant'—phonetically or phonemically; the erratic spelling with and without doubling with *ea* is easily understood as spelling analogy within the paradigm. Prejunctural double consonants need not be posited for any stage of the reconstruction of Proto-English or Proto-West-Germanic.

As for the matter of *fersc*, we can only point out that the order of vowels and consonants, especially with /r/, is not so certain a thing as our opponents' irony suggests. Take MdE *pretty* as an example. We know of many nonlinguists who speak this item very clearly with the structure /pɨ́rtiy/ and yet, apparently bound to the spelling, insist, when it is called to their attention, that they are saying /prítiy/, to rhyme with /grítiy/. Other equally relevant examples appear in Martin Joos' discussion of *r*-color, *Acoustic phonetics* 92 ff. (Baltimore, 1948).

§9.5. Our opponents have picked the one pair, /o/ and /ö/, that cannot be supported by numerous contrasts, a fact which we readily acknowledge. To this we reply: we would set up /ö/ as a phoneme only in the earliest period, as the stage between the /o/ in umlaut position and the /e/ which /ö/ became after unrounding.

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