Part 4: Sound change in older English

Introduction;
or,
When a knowledge of history is a dangerous thing.

With few and marginal exceptions, such as may occur when mastering the overtly codified prescriptions of an archaic standard, a human being acquires his native language in complete ignorance of its history. He does so by means of a capacity for language which is part of his biological endowment, whether we think of it in rationalist terms as a domain-specific mental module, or in empiricist terms as a set of general predispositions also applied to other cognitive domains. The linguist, in contrast, does not know what the biologically endowed human capacity for language consists of, except by more or less corroborated scientific conjecture; but, as if in compensation, he has access to a rich record of historical change in a variety of languages, of which English is a notable example. The linguist can thus observe what innovations occur frequently or infrequently in the history of languages, and under what circumstances, and from these observations he can infer hypotheses about the cognitive abilities of the speakers participating in linguistic change, whether these be children in the task of language acquisition or adults in the everyday business of language use. The relevance of diachronic evidence to the general enquiry into the human faculty for language is, in this sense, tolerably clear.

Far less clear is the relevance of historical evidence to the description of a synchronic state of a particular language. How can knowledge of Middle or Early Modern English, for instance, inform our synchronic understanding of a variety of present-day English, if such knowledge plays no part in the acquisition of that variety by its native speakers? One could conceivably answer that, when we have well-supported crosslinguistic generalizations about the typical course of change in some part of grammar, we can advance a hypothesis about how innovation may have unfolded in a language from a given point in its history, and the end-point in this hypothetical pathway of change may satisfyingly agree with an independently formulated synchronic description of the present-day state of affairs. Something of this sort is attempted, for a number of English phonological phenomena, in Bermúdez-Otero (forthcoming: §5-§6). Such arguments have a certain force, which is however tempered by the danger of a conspiracy of error: a synchronic description and a theory of change can very well support each other and yet both be false.

More troublingly, when we describe a synchronic *état de langue*, historical evidence can tempt us into anachronism. Few phonologists, for example, would nowadays be prepared to defend Chomsky and Halle's (1968: 233-35) notorious suggestion that, in present-day English, the word *nightingale* escapes a synchronic rule of Trisyllabic Shortening because the long

nucleus of its first syllable derives from an underlying representation containing a short vowel followed by /x/; it seems unlikely that Chomsky and Halle would ever have entertained this hypothesis if they had not known how the word was pronounced in Middle English, and this irrelevant knowledge lent colour to an otherwise implausible proposal. Judgement does not always come so easily, however. Similar charges of anachronism have for example been levelled against *SPE*'s rule of Vowel Shift, but the synchronic status of the latter remains disputed. Notably, McMahon's (1990, 2000: ch. 3) analysis of present-day alternations like *div*[aɪ]*ne-div*[ɪ]*nity* assumes far more diachronic restructuring of underlying representations than Chomsky and Halle's, but she nevertheless upholds the psychological reality of Vowel Shift as a set of productive stem-level phonological rules (McMahon 2007: §3); see Bermúdez-Otero and McMahon (2006: 388-90) for discussion and an alternative.

Phonologists analysing older English are no less beset by the danger of anachronism than those working on present-day phenomena. Indeed, for the former the risk is arguably twofold, since they can misrepresent their object not only in terms of what came before but also in terms of what came after: their anachronism can be *perseverative* or *anticipatory*. This predicament is starkly illustrated by the question addressed in **Donka Minkova**'s chapter. In Old English, the past-tense forms of class-1 weak verbs exhibited a characteristic alternation between thematic *-e-* and *-∅-*: e.g. *frĕm-**e**-d-e* 'performed' vs *dēm-∅-d-e* 'judged'.[1] The alternation originated in a well-known prehistoric sound change, High Vowel Deletion, which may be described as deleting short high vowels in unstressed open syllables immediately preceded by a bimoric foot (i.e. by σ́ or σ̆σ): see Bermúdez-Otero and Hogg (2003: 110-111, endnote 12) and references therein. All agree that, by Middle English, the alternation has become morphologized: unstressed vowel reduction has obliterated the distinction between the syncope-prone *-e-* of the old class 1 and the syncope-resistant *-o-* (also *-a-*) of the old class 2, so that, in Middle English, a weak verb's membership in the thematic or athematic conjugation is an arbitrary morphological fact (Lass 1992b: 127). At issue, then, is the synchronic analysis of the alternation in Old English.

The received view is that, at least during much of the recorded history of Old English, the alternation was governed by a synchronic process of syncope recapitulating the relevant effects of High Vowel Deletion. Minkova charges this account with perseverative anachronism. Following Richard Hogg's (2000) skeptical lead, she asserts that, by the ninth to eleventh centuries, syncope had become so opaque as to be unlearnable. Her various arguments include evidence that, among class-1 weak verbs, the underlying contrast between roots ending in singletons and roots ending in geminates showed serious instability. For example, a light root like /knys-/ should yield the unsyncopated past-tense form *cnys̆-e-d-e* 'knocked' (e.g. *ChronA* 0.45), whereas a heavy root like /kysː-/[2] should yield syncopated *cys̆te* 'kissed' (e.g. *MtGl(Ru)* 26.49):

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[1] All my past-tense examples are 1/3SG.PAST.IND or SG.PAST.SBJV forms.

[2] Possibly /kusː-/: cf. the noun *coss* 'kiss' < Germanic †*kuss-oz*.

(1)	input	kys:ede
	syncope	kys:de
	degemination and assimilation	kyste

In this derivation, however, degemination crucially conceals the trigger of syncope, which consequently appears to overapply. Furthermore, the underlying contrast between singleton-final and geminate-final roots was neutralized in many paradigm cells, such as the infinitive, where light-rooted verbs were subject to morphological gemination (reflecting the effects of West Germanic Gemination): e.g. /knys-/ → *cnyss-an*, cf. /kys:-/ → *cyss-an*. Thus, the underlying opposition between /knys-/ and /kys:-/ could manifest itself transparently in very few forms, like bare (i.e. uninflected) past participles: e.g. /knys-/ → *ġe-cnys-e-d* (e.g. *ClGl 3* 1392) vs /kys:-/ → *ġe-cyss-e-d* (e.g. *ÆGram* 248.1). The contrast could also be maintained in 2SG and 3SG.PRES.IND forms as long as these remained thematic, as was the norm in Anglian dialects: e.g. *cnys-e-ð* (e.g. *HomS 11.1* 146) vs *cyss-e-ð* (*Rid 63* 4); cf. athematic *cys-∅-ð* (e.g. *Conf 3.1.1* 2.8). Transparent realizations of the opposition being relatively rare, one is therefore not surprised to encounter ill-behaved forms like past-tense *cnyste* (e.g. *ÆCHom II 28* 221.15), as well as 3SG.PRES.IND *cnyss-e-ð* (e.g. *HomS 11.2* 165).

From this and similar evidence Minkova draws a radical conclusion: she suggests that the redistribution of weak verbs into two arbitrary inflectional classes (athematic or C-final stems, and thematic or V-final stems) happened not in the transition to Middle English but centuries earlier, and that it was brought about by instances of opacity like (1), among others, rather than by the merger into schwa of syncope-prone class-1 *-e-* and syncope-resistant class-2 *-o-* or *-a-*.[3] But does Minkova's claim replace perseverative with anticipatory anachronism? The answer depends on a delicate balance of argument. Certainly, Minkova's proposal makes for a phonologically transparent analysis of the Old English facts, thus amenable to implementation in a perfectly classical version of Optimality Theory. But do forms like *cnyste* attest the death of syncope as a phonological rule, or merely diagnose a localized fluctuation in underlying representations in a small corner of the lexicon? Similarly, the received view provides a unified account of past-tense forms and past participles: in heavy-rooted verbs, past participles should resist syncope when bare (e.g. *ġe-dēm-e-d*) because syncope does not apply in closed syllables, but they should become targets for syncope when inflection places the theme vowel in an open syllable (e.g. *ġe-dēm-∅-d-e*); cf. light-rooted *ġe-frĕm-e-d*, *ġe-frĕm-e-d-e*. Campbell (1959: §751(3)) reports that the predicted pattern is maintained robustly in early Mercian (*Ps(A)*) and, with few exceptions, in late Mercian too (*Ru1*). If Campbell's report proves correct, Minkova will presumably be forced to argue that her C-final stems took a past-participle suffix with two listed allomorphs: *-ed* word-finally and *-d-* before a vowel-initial suffix. In this light, do

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[3] The distinction between a [-back] theme vowel in class 1 and [+back] theme vowel in class 2 seems to be consistently preserved at least in Alfredian West Saxon: see e.g. Cosijn (1888: 187-88).

Mercian participles bear out the enduring synchronic productivity of syncope, or is the appeal to them essentially aesthetic, as so many of *SPE*'s arguments from 'missed generalization' now appear to be?

I shall not offer an answer to these questions here, but will merely note that alternatives between the received view and Minkova's radical revisionism remain to be explored. A flavour of the possibilities may be gathered from the account of the morphophonology of Old English neuter *a*-stem nouns given in Bermúdez-Otero and Hogg (2003: §3) and Bermúdez-Otero (2005), which is cast in a stratal-cyclic version of Optimality Theory. In this account, High Vowel Deletion, as applied to neuter *a*-stem nouns, dies a long torturous death, its throes spanning most of the recorded history of Old English: it breaks into two processes, apocope and syncope, which retreat at different speeds from wider to narrower and more specific morphological domains, amidst much restructuring of underlying representations. The account draws its strength from the kind of interlocking argumentation adumbrated above: detailed synchronic accounts of particular dialects (Owun's, Alfred's, Ælfric's) fall into a diachronic series that accords well with what we know independently about the life cycle of phonological processes (Bermúdez-Otero 2007: 503-504, forthcoming: §5-§6). Yet, of course, this accord may well depend on a conspiracy of mutually supporting errors: Richard Hogg, ever the Humean skeptic, would have delighted in pointing out this possibility.

The linguist's knowledge of history enables him not only to understand a synchronic state in terms of what preceded and followed it—with the advantage of privileged insight balanced by the risk of anachronism—but also to see a sequence of changes as arranged in a language-specific, coordinated, directional pattern, one that can span centuries, even millennia. Such visions of 'drift' (Sapir 1921: ch. 7) are even more of a mixed blessing, for reasons that we shall discuss presently; but they have a long tradition in English historical phonology. Indeed, two of the discipline's classic objects, the Middle English Length Adjustment and the Great Vowel Shift, both arguably discovered or invented by Karl Luick in the late nineteenth century, are precisely of this sort. Their status, however, has famously aroused vehement disagreement in recent decades, with Lass taking Luick's side against Minkova and Stockwell's deflationary revisionism: on the Middle English Length Adjustment (Luick 1898), compare for example Lass (1974, 1987, 1992b) with Minkova (1982) and Minkova and Stockwell (1996); on the Great Vowel Shift (Luick 1896), compare Lass (1988, 1989, 1992a) with Stockwell and Minkova (1988a, 1988b) and Minkova and Stockwell (1990).

In his chapter, **Nikolaus Ritt** bravely wades into these tumultuous waters. In the history of English during the last millennium, Ritt detects a sustained tendency for consonants to be lenited and for vowels to be strengthened, and he claims that it is the stress-timed rhythm of the language that has imparted this direction to its phonological history. This sort of argument faces two challenges. The first is to show that the purported pattern lies in the facts and not in the analyst's eye; the criteria for weighing counterevidence bristle with difficulties. Even if Ritt's pattern of vowel 'strengthening' is limited to stressed vowels, for example, it is unclear how Trisyllabic Shortening (if it ever was a sound change at all; cf. Minkova and Stockwell 1996,

Bermúdez-Otero 1998) and Pre-Cluster Shortening can be counted as instances of it, although Ritt wishes to include them because they fit nicely with his rhythmic rationale. Similarly, if diphthongizations count as strengthenings, what are we to make of smoothings like those of Middle English /ai̯/, /au̯/, and /ɔu̯/ in the Early Modern period, of /aɪ/ in the South of the USA, of /eə/ in many British dialects, and of the triphthongs /aɪə/ and /ɑʊə/ in RP? Indeed, the pattern that Ritt lays most emphasis on, and the one most directly connected with his purported rhythmic engine, could perhaps be defined more narrowly as an apparent trend for duration to lose its rôle as an acoustic cue of contrast: this would cover consonant degemination in Middle English (the subject of Derek Britton's chapter) and the qualitative decoupling of long and short monophthongs (i.e. /i/≠/iː/ > /ɪ/≠/i(ː)/).[4] Even then, however, problems arise, for, through Prefortis Clipping (Wells 1990), duration has become—or at least currently remains—the main phonetic cue to laryngeal contrasts among obstruents in non-foot-initial position: e.g. *neat* [nit] vs *need* [niːd̥].

The second challenge for an account of drift is to devise a non-teleological, causal engine that will start the drift on schedule, keep it going as generations of speakers succeed each other over the centuries, and wind down at the appropriate point. Here Ritt appeals to the popular view that English is (and presumably has long been) a stress-timed language (Pike 1945; Abercrombie 1965, 1967), and one can see how the need for segments to stretch or contract in response to the demands of accentual isochrony would interfere with the ability of duration to cue phonemic contrast. The appeal is not without difficulties, however. First, Abercrombie's claims are certainly false (see the review of the literature in Bertinetto 1989): stresses are on average no less irregularly spaced within utterances in so-called stress-timed languages than in so-called syllable-timed languages (Roach 1982, Dauer 1983), and both language types exhibit a small compression effect whereby the average duration of an Abercrombian foot containing *n* syllables is smaller than *n* times the average duration of a monosyllabic foot (Dauer 1983); the most that can be said for English is that a full-vowelled syllable does significantly increase in duration when immediately followed by another full-vowelled syllable (Bolinger 1981). Some phoneticians have argued that stress-timing does exist either as an auditory illusion (Lehiste 1977) or a higher-level Gestalt (Dauer 1983), but it is hard to see how a perceptual illusion could drive Ritt's drift. Indeed, the properties that have been taken to reflect the rhythm of 'stress-timed' languages are also present, though to a lesser degree, in 'syllable-timed' languages: e.g. Italian, often mentioned as a prototypical 'syllable-timed' language, has vowel lengthening in stressed open syllables, a reduced vowel inventory in unstressed syllables, and stress shift to alleviate clashes. If this is true, then the hallmarks of so-called stress-timing are in fact soft universals and need not be seen as the outcome of language-particular drift.

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[4] The dating of the latter is the subject of a long-running disagreement between Stockwell (e.g. 1964) and Lass (e.g. 1989), intimately connected with their differences over the Great Vowel Shift.

Be that as it may, what is arguably most important to Ritt is the logic of non-teleological explanations of drift in general. His key insight is the idea that one feature of a language may form part of the environment within which variants of other properties are historically selected, with the process of selection itself taking place in language use by adults and language acquisition by children. This is in exact correspondence with theories of biological evolution where the gene is the unit of selection (Dawkins 1976): in such theories, other genes within the genome of an organism form part of the environment where alleles compete, their competition playing itself out through the building, survival, and reproduction of phenotypic vehicles. The same logic has been deployed in syntax: see, for example, Hawkins's (1990) parsing-driven account of how the presence of prepositions triggered a drift from SOV to SVO in the Indo-European family.

If it can be so very difficult merely to decide when a change happened, or whether an apparent trend is a fact or an illusion, then the utmost circumspection recommends itself in talk about the causes of particular sound changes. **Derek Britton**'s chapter bears ultimately on this question: it addresses the causes of the shortening of geminate consonants in Middle English. Here, Britton takes issue with an influential paper by Kurath (1956), whose account of degemination laid emphasis on the claim that Middle English Open Syllable Lengthening rendered consonantal length non-contrastive, at least in the North Midlands and the North, where all short vowels were targeted. As Britton notes, Kurath was forced to posit a different and tortuous path towards degemination in the South Midlands and the South, where the high vowels /i/ and /u/ resisted lengthening. But one can go further, I think, and attack Kurath's approach to degemination both on empirical and on theoretical grounds. Empirically, the premise of his argument collapses if revisionist historians are right in claiming that so-called Middle English Open Syllable Lengthening was really no such thing (Minkova 1982, Bermúdez-Otero 1998)—though Britton himself does not take this view. Theoretically, one can argue that Kurath did not even begin to explain the shortening of geminates because there is no necessary connection between a feature becoming allophonic and its being lost: for example, vowel length was phonemic in Middle High German and became predictable in Middle Bavarian, but of course Middle Bavarian retains long and short vowels on the surface (see e.g. Seiler 2005).

It is difficult to decide to what extent this last objection bears on Britton's own claims, for his own contribution concerns the functional load of the singleton-geminate opposition in the Middle English lexicon: he shows that, even at a relatively early point, before Open Syllable Lengthening and before Schwa Loss, the number of minimal pairs distinguished by consonant length was extremely small. But Britton treads far more carefully than Kurath. He regards the shortening of geminates as a phonetically driven change occurring 'under conditions of reduced articulatory energy'. Presumably, therefore, he must be read not as stating that the low functional load of the singleton-geminate contrast brought about degemination, but rather as suggesting that a potential systemic obstacle in the path of phonetically driven reduction was nearly absent, and absent in fact far earlier than Kurath allowed. We do not really know the

extent to which low functional load can favour articulatory reduction leading to neutralization, but Silverman (in press) makes a nuanced plea 'to reintroduce Martinet's proposals into modern discourse on phonological theory'.

This introduction has highlighted how much of the phonology of older English recedes beyond our grasp, certainly now, perhaps forever. Throughout his career, Richard Hogg preached and practised the double self-denial required in this situation: he constantly drew our attention to the limits of the knowable, whilst refusing to relax the strictures of uniformitarianism to suit our ignorance.

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