

ON DRIFTS AND SHIFTS

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1. Introduction

In this paper we address some issues raised in William Labov's recent book¹ and in his closely-related paper on what he refers to as "the three dialects of English" (Labov 1991). At the same time we offer some comments on Lass (1992), the latest installment in our on-going debate about the English vowel shift.

We start with some notational and terminological issues. We shall use something close to Labov's preferred notation system, but only after the following brief explanation of it, and a statement of a few reservations about the wisdom of it. He finds four sets of vowels in most English dialects (1991: 7), (1994: 63):

¹ We hope that as a key speaker in *The House of Shifts*, Roger Lass will find our title gratifying. We congratulated Labov for his monumental opus at the Jan. 1996 LSA Meeting in San Diego, and in the same breath added that we would be talking about it in Poland in May. As we were working on the details of this paper, the March 1996 LSA Bulletin reached our desks with the official news that Labov's book *Principles of linguistic change: Internal factors* (1994) had been awarded the Leonard Bloomfield Book Award for 1995. These are intimidating credentials indeed: it is a stimulating book, fully deserving of its elevation to a sort of "classical status". We are no doubt foolhardy to challenge some of the basic claims made in it. As with our exchanges with Roger Lass, who described us as a voice in *partibus infidelium*, we will be iconoclastic; only the Gospels are different. Labov does not include Lass or our exchanges with Lass in his bibliography, yet he has kind words in the "Acknowledgments" which give us more than due credit ("investigations ... [which] both anticipated and illuminated my own at many points" 1994: xvii), and for the many places in the text where he cites our views favorably: e.g. "The following discussion will follow the spirit of Stockwell's simplification..." 1994: 246. We are submitting this contribution in the hope that our two separate two-way exchanges will turn into a properly rowdy triangle.

We are at an additional disadvantage in discussing Labov's book, namely that it contains 641 pages, a quantity which, if we had matched it, our editors would have rejected it and you would not have read it. This paper must therefore be thought of as comments on points of interest, and points that give us pause, in Labov's epic work.

Short		Front Upgliding		Back Upgliding		Inglinging	
i	u	iy		iw	uw	ih	uh
e	ʌ	ey	oy		ow	eh	oh
æ	o		ay	aw ²		æh	ah

Obviously there is no point in arguing about symbols, which are arbitrary. But it is not unfair to ask for a measure of consistency. For example, the /o/ of Labov's short vowels in words like *hot*, *cot*, *pot* as pronounced in most of America is not the /o/ that appears in /oy, ow/, in any dialect. Rather, the /o/ of the short vowels is, as designated by everyone else, the /a/ that appears in /ay aw ah/. On the other hand, there is no /a/ among his short vowels at all; had he used /a/ for "short o", then /ay aw ah/ would all make more sense. For these reasons, we will use the following system of vowel notation, though we do not replace Labov's symbols with these in quoting, only in discussion:

Short			Front Upgliding		Back Upgliding		Inglinging	
i	u		iy		iw	uw	ih	uh
e	ʌ	o	ey	oy		ow	eh	oh
æ	a		ay		aw	ow	æh	ah

In this slightly expanded system we have kept the caret (the vowel of *cup*, *bust*, *trump*) and have added /ow/, because it makes no sense to follow Labov in referring to the Southern States vowel of *law*, *daughter*, *caught*, *ought*, *dog*, *log* with the symbol /oh/: the Southern nucleus in these words is an upgliding diphthong like /aw, uw, ow/. It does not exist in other dialects but is highly distinctive in both Upper and Lower South, treasured and preserved like an ante-bellum artefact, conspicuous in the speech of Lyndon Johnson, Jimmy Carter, Ross Perot, Elvis Presley and William Jefferson Clinton, to name only a few prominent figures of recent years. Our other revision, the seventh short vowel, is simply for phonetic transparency. If we write /oy/ for *boy*, *coy*, *noise*, then we are uncomfortable using /o/ in *cup*, *bust*, *trump*.

The issues we address are these:

1. Should one not discriminate cleanly between implications associated with the historical use of the term "chain shift", and other types of vocalic sound change?
2. How much credence can be given to Labov's delineation of three distinctive accents of English on the basis of on-going shifts?
3. How far do spectrographic F1-F2 plots of individual speakers serve to establish dialectal norms? This question has two parts: (a) are idiolects rather

² In his book, /aw/ is located here, but in Labov (1991) it is located under /oy/.

than norms (= dialects, as generally understood) the correct basis for talking about "the southern shift" or "the northern cities shift"; and (b) can either norms or idiolects be satisfactorily characterized by F1-F2 plots?

2. Shifts vs. drifts

In order to make sense of the notion "chain shift", we think it is necessary to make a sharp clean distinction between "sound change" and "chain shift". Labov (1994) contains a clear definition of "chain shift":

- (1) A *minimal chain shift* is a change in the position of two phonemes in which one moves away from an original position that is then occupied by the other (1994: 118).

This wording presupposes that chain shifts are necessarily "drag chains", but since Labov later speculates that some on-going shifts are "push chains" (e.g. 1994: 195, the backing of /e/ and /ʌ/), this implication is probably unintended. The most persuasive causal explanation of the Early Modern English vowel shift goes against the drag chain notion. Lass, following Luick, has argued this case especially for the "upper half" of the English vowel shift.³ Labov asserts that the question of push vs. drag is an empirical issue (1994: 200), but he lets the question drop there. He notes that Ogura (1990) supports Luick, but claims to "follow the majority view" (1994: 151) that it was a drag chain. His only citation of the details that Luick (and Lass) based his arguments on is this brief sentence in a footnote:

Where ME \bar{o} was fronted to \bar{e} , the diphthongization of \bar{u} was inhibited (1994: 234, fn. 8).

This is indeed the central argument, but some further evaluation of it is surely in order, given that Labov is not persuaded by it.

Passing over, for the moment, the question of push or drag, Labov proposes a second generalization which we find difficult to comprehend:

- (2) Though these principles are stated in terms of chain shifts, I will not hesitate to use them to describe and classify individual movements where they apply (1994: 117).

But (2), which refers to individual sound changes, creates an insuperable problem for (1), because it inextricably confuses "chain shift" with "sound change". Indeed, we feel that a fair amount of the chain shifting discussion in Labov (1994) is jeopardized because of this confusion. The two points which we want to emphasize especially in this connection are:

³ Our position (Stockwell – Minkova 1988) is that it was neither push nor drag, but a complicated set of mergers and standardization.

- (a) that vocalic changes involving an element *leaving* a certain space are not necessarily, or even usually, chain shifts
- (b) that explanations for changes of this type (drifting through vocalic space) are independent of explanations for chain shifts.

To try to avoid terminological confusion, we shall speak of a spatially contiguous sequence of changes (i.e. close adjacency in the vowel space) as drifts. The name by which we choose to call these events in itself would be of little interest; however, the label and the notion of a chain shift implies causal associations and functional consequences which "ordinary" vowel changes/drifts, even extensive ones, don't have.

3. Famous drifts that are not chain shifts at all

In order to be a chain shift, on the push reading there has to be a (near) collision. Push chains require that phonemes bump like balloons, billiard-balls in a rack, on a model resembling the Brownian motion of particles. On the drag reading, there has to be evidence that some phonetic region has been vacated and further that there is something bad about having the gap that results from the evacuation, such that the language seeks to repair it. The two best-known language changes, at least in historical English (and widespread throughout Germanic), that have consistently been thought of as chain shifting are the BITE and HOUSE histories:

(3) BITE [iy > iy > əy > ʌy > ay > oy > oy > oy (>uy?)]
or:⁴

[iy > ey > əy > ʌy > ay > oy > oy > oy (>uy?)]

(4) HOUSE [uw > uw > iw > əw > aw > aw > æw > ew]
or:

[uw > ow > əw > ʌw > aw > aw > æw > ew]

The only segment of these changes that looks like it might be properly called a "shift" is the first segment, which traditionally is taken to be diphthongization of [i:] and [u:]. It has been called a "shift" because Luick thought the long vowel was bumped by [e:] and [o:] pushing from below, and having nowhere to go, it diphthongized. We have tried to show elsewhere⁵ that there were pre-

⁴ We merely allude, here, to the extended argument in Chomsky – Halle (1968), Wolfe (1972), Dobson (1957), and Stockwell (1972) concerning the first "move" in the vowel shift in words of the WIFE class. Our idea will be much closer to the *stabilizing* chain that Lass – Wright (1985: 154) speak about: "the chain effect is partly an artefact, due to a set of nuclei 'spacing themselves out' as if by a kind of mutual repulsion, rather than either the high or low ends of the continuum acting as a starting point. The controllers are still (a) a no-merger condition and (b) the shape of the vowel space; but the impulse is holistic, not local or directional".

⁵ Stockwell – Minkova (1988), where the main goal was to argue that the English vowel shift is a chimera, a mirage of mergers and splits, and not a shift at all. To our surprise, it appears that Labov did

existing diphthongal nuclei IY and UW (as in OE *stig* and OE *bugan*) with which [i:] and [u:] merged, and it is those mergers which created the possibility of subsequent drift by dissimilation, yielding (3) and (4). None of the other stages in these drifts either displaced, or merged with, any pre-existing nucleus. They therefore do not fit Labov's definition of a minimal chain-shift, given in (1) above.

A different kind of example of a sound change that has been incorrectly linked to chain shifting is the Early Modern English merger of the BAKE words with the RAIN words. When they bumped the choice was to join forces rather than fight. But if they merge, it is by definition not a chain shift. Exactly parallel in the back is the merger of the BOAT and HOPE words with the GROW words. Even Roger Lass, a staunch defender of the GVS, retracts (1992: 153) his position on the low mid and low vowels (ME a:, o:, e:) and unchains them from the changes of the high mid and high vowels (ME i:, e:, o:, u:), which we also consider more coherent looking and possibly proper candidates for the label 'shift'; or, more appropriately, in Lass's terms 'die mittelgrosse Vokalverschiebung'.

Note that we have just recategorized the two most famous chain shifts (or rather, four – two front and two back) as a subtype of ordinary Neo-Grammarian sound change. The mergers of the high vowels with pre-existing diphthongal units and the raising of the high mid vowels remain the only candidates for a mini-shift. There is more to be said about such drifts as the ones which are summarized in (3) and (4), but crucially they are not chain shifts. Nothing gets bumped or dragged, though some stages "drop off" along the way and don't continue drifting: thus we find [hʌws] *house* and [hʌyt] *height* in Virginia, Maine, Canada, Martha's Vineyard, etc.

4. A real historical chain shift?

As foreshadowed above, in "the" English vowel shift the only changes that look like they involve bumping – i.e. sequences of non-merging entities conforming to Labov's definition (1) – are the BEET and BOOT words, in relation to the BITE and BOUT words, which Lass calls "the GVS proper" (1992: 154). It is clear that [i:] and [u:] got out of the way, whether pushed or dragged (Lass 1988: 150 ff. argues that it was a temporally localizable push-chain) and whether by our suggestion of merger (Stockwell – Minkova 1988) or by some even more mysterious process of bouncing off the hard palate and diphthonging their way southward (something like this can be found in most of the 19th century accounts).

not see this paper during his extensive vowel shift research. We thought we had sent it to him, but our records of just ten years ago are about as chimerical as the shift of some 600-1000 years ago. The same volume contains a Great Vowel Shift discussion by Roger Lass, who has had more to say about the English vowel shift than anyone since Luick.

The raising of BÈET into BITE's position, and BOOT into BOUT's position, are the clearest instances we have of real chain shifts in the history of English. They look very systematic and persuasive, but mysteries remain about them.

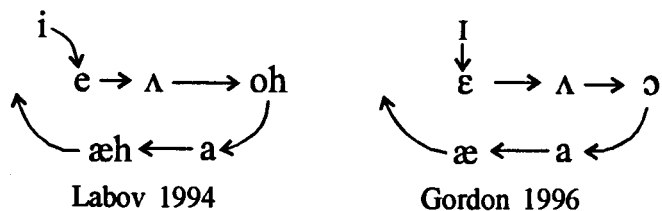
For instance, if the BEET vowel was really a "long *e*" that moved up to "long *i*", how come its short counterpart was "short *i*", not "short *e*"? Trnka (1959) argued that this asymmetry played some sort of a causal role in the drift of "long *i*" away from the other long vowels. *Mutatis mutandis*, the high back vowel scenario. But, you say, why does the asymmetry matter? The BEET vowel moved up, anyway, even if it was already a kind of I rather than a kind of E. But it matters greatly: if it was already a kind of I, and there were two high tense vowels (say, [i:] and [iy]) in that vowel space, then no chain shifting occurred of the type described by Labov, in which vowels maintain their distance and move like horses in tandem. Rather, the space got crowded, as it had elsewhere. Sometimes, as elsewhere (above) we find mergers, and sometimes a slow separation, as speakers of the language gradually equalize the distribution of functional units in the available vowel space, in the sense of Martinet or Lindblom, more recently Lass and Wright (1985).

5. A real on-going chain shift? The core of the "Northern Cities" shift

Turning back to the analysis of current, presumably well-documented instances of 'shifting', the core changes characterizing Labov's "Northern Cities Shift" are explicitly claimed to be a drag chain:

As with all Chicago speakers, we see the initial event that triggers the Northern Cities Shift – the raising of /æh/ to upper mid position (1994: 186).

Here is Labov's plot of the shift (1994: 191) (with /o/ replaced by /a/, as Gordon, below, also does), and alongside of it, the version presented by Matthew Gordon at NWAWE 25 (Oct 17, 1996):



Both plots are of course oversimplifications, but even so there are grave problems with them. To be as fair as we can in discussing the shift, we begin by quoting Labov's summary of the speech of Jackie H., whose vowel system is asserted by Labov to represent "the fullest development of the Northern Cities Shift" (1994: 191).

Of the four long upgliding vowels, /iy/, /ey/, and /ow/ remain stable on the peripheral track. The fourth of these, /uw/, shows the tendency to shift forward that is now affecting most western and northern dialects.

Forward-shifting of /uw/ and /ow/ is also true of the Southern shift, except that in all three cases it is not so much a matter of shifting as of unrounding of the first element of the diphthong, with the Southerners having gone furthest. Now continuing:

A tight group of nuclei are located in low central peripheral position: /aw/, /ay/, and /ah/ appear to share the same nucleus.

This is because they share the same starting point for the diphthong: but one glides toward high back, one glides toward high front, and one glides to schwa. One of the severe problems of all the F1-F2 plots that Labov and his colleagues favor is that they measure "points of inflection of the vowel trajectories" (1994: 165). If they measured something like midpoints of the trajectories in /aw/, /ay/, and /ah/, they would surely locate the circles quite a bit higher, with /aw/ toward the back, /ay/ toward the front, and /ah/ toward the center. But measuring *any* single point is a misrepresentation of diphthongs. Of course Labov is aware of the problem, but he does not have a good solution. He discusses the matter of "points of inflection" (referring to points at which formants start to bend, which is where he measures F1 and F2), noting that

The selection of a single point should not imply that this represents all the perceptually important information about the vowel – obviously untrue in the case of a diphthong. The one point that represents a given token of a vowel can be compared with a point for another token of the same class, so that the overall distribution of the word class or phoneme can be registered (1994: 165).

It is this second claim about comparative evaluation which we believe is seriously jeopardized by the methodology. Even directionality of glide, which is not registered in the point-plots, would be insufficient for comparative purposes, as is made clear by one of the most conspicuous differences between Southern and Northern accents. In diphthongs which move in the same direction – say the /ay/ of *bite* – the Southern glide moves in a lieisurely manner up to approximately [e] – i.e., [æe], but the Northern glide, starting at the same point, moves rapidly to at least [ɪ] or even higher,⁶ i.e. [ai]. Now continuing:

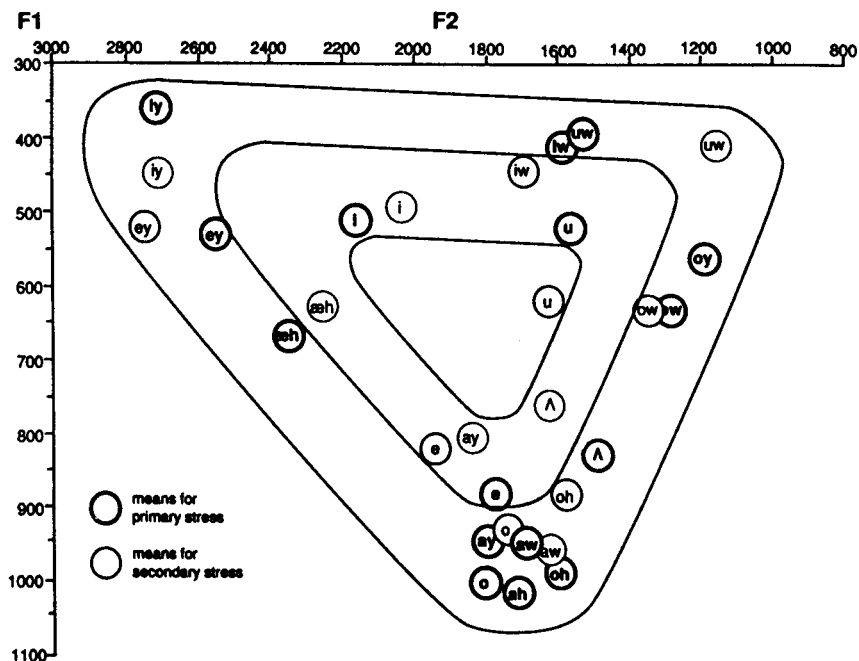
The Northern Cities pattern is shown most clearly by the positions of the primary stressed means. (1) /æh/ is raised to a mean mid position on the peripheral track; (2) the mean position of /o/ (read[a]) is to the front of the other peripheral

⁶ See Kurath and McDavid (1961), *passim*.

⁷ The vowel being referred to is the low central vowel of American *pot*, *hot*, *cot* which would be something close to IPA [a]; the vowel referred to by /oh/ would be approximately [oə].

low central vowels⁷ (3) /oh/ is in peripheral low back position behind it; (4) /i/ has shifted back along the nonperipheral track (and not down, as in other speakers' systems); (5) /e/ has moved both down and back, to the lowest position in the nonperipheral track, adjacent to both /o/ and /ʌ/; (6) stressed /ʌ/ has shifted back to the peripheral track, to the position formerly occupied by /oh/.

The facts alleged above are represented in our Figure 1, from Labov (1994: 192).



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Figure 1. Merger of short /o/ and long open /oh/ for long-distance operators.

Remember that this is the “fullest” development of the Northern Cities shift. That is, it doesn’t get any worse than this, so the norm in the Northern Cities can be assumed to resemble Classical Kenyon-Knott Middle America (CKKMA) even more than this does.

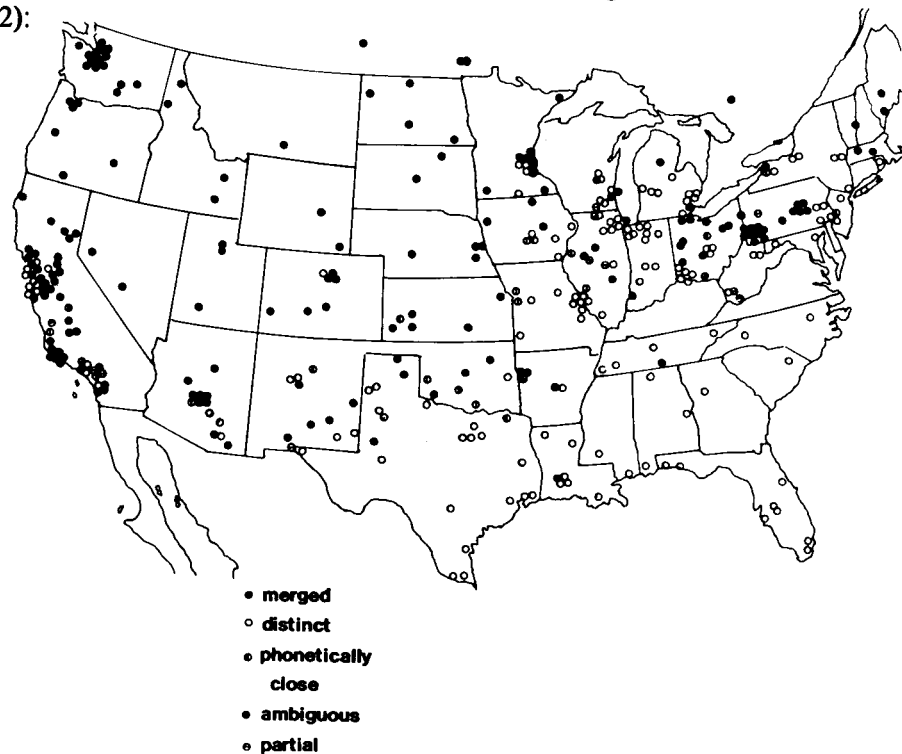
The Northern Cities shift in fact fails to resemble CKKMA at only two points: the position of /æh/ and the position of /e/. They appear to have switched places, almost, except that Labov has drawn the peripheral boundary in such a way as to guarantee that /æh/ is on the [+PERIPH] track and /e/ is on the [-PERIPH] track. This, however, is a bit of a cheat: /æ/ is surely on the [-PERIPH] track in CKKMA. In order for it to move, it must first lengthen, as Labov himself asserts repeatedly – i.e., [æ] – [æə], [æə]. But the lengthening

is unconditioned and still allophonic in the north-central states where it occurs most commonly. It still counts as a “checked” vowel, like [æ] in other dialects. This is quite unlike the corresponding historical phenomenon in British English (resulting ultimately in /ah/). On the U.S. NE Seaboard, the same development is largely conditioned by environments similar to the British ones. In NYC, Connecticut, Philadelphia, etc., there are small numbers of real contrasts like *can* (v) vs. *can* (n), all details widely discussed at least since Trager (1930), most recently by Labov extensively in his book, and by Kiparsky (1989). When such contrasts arise, the new /æh/ has joined the long vowels and behaves exactly as long-ago argued by Stockwell for in-gliding vowels (most recently Stockwell – Minkova (1988), with references to earlier work), namely it dissimilates by raising the first element of the diphthong. The other point, the extreme lowering and centralizing of the “short e”, is surely not motivated by pressure from the “short i”, which is in a quite normal [+HI, -PERIPH] position. Furthermore, the diagrams of both Labov and Gordon indicate that fronting of “short o” (i.e. the [a] of *cot*, *hot*, *not*) is part of the shift: but the F1-F2 plot of Jackie H.’s vowels gives no clue that even minimal fronting has taken place.

We think different, unrelated events have been collapsed into this “chain” by Labov. Consider the *cot-caught* merger, represented by Labov as /oh/ – /o/. Many things remain to be discovered about this merger. Milroy (1995: 438) points out that it is a well-known merger in Belfast, that “it is a commonplace in western and central Scots dialects and widespread in Canada.” Milroy goes on to observe that “an origin in Scots and Scotch-Irish immigration to North America, through Pennsylvania, is surely rather likely” (1995: 438). The evidence is that this merger began in the western part of Pennsylvania, around Pittsburgh, and that it spread from there west to the Pacific, north to Canada, and south to the Mexican border (though only in the far west).⁸ The merger has long been completed (70 to 90 years, probably longer; Labov (1994: 317) cites evidence that it began in the early 19th century, though the evidence is insufficient to be taken as conclusive on the dating) for nearly all speakers in the Rocky Mountain states and on the west coast except for coastal Northwest and older generation San Francisco. It is the norm throughout the rest of California regardless of settlement history (large numbers from Oklahoma and Missouri, for example, who could not have had the merger when they left the midwest; large numbers from Manhattan, who also could not have had the merger natively). This merger is, furthermore, a merger in a proper sense, not a chain of events, and demonstrably independent of other events because, as just noted, virtually all western accents have it but do *not* have fronting of the resultant “short o” (i.e. low central unround vowel lacking distinctive length),

⁸ It also spread eastward in Pennsylvania, as documented by Herold (1990). The facts for Western Pennsylvania are documented by Wetmore (1959).

and do *not* have lengthening and raising of [æ]. The latter change is a north-ern-tier-of-states event, not extending to the general Rocky Mountain area which regularly has the “short o” merger. The areas that have ash-raising are genuinely northern: Chicago, Madison, Ann Arbor, Syracuse, Detroit, etc. What remains mysterious about the *cot-caught* merger is the explosiveness of it west of the Mississippi and Missouri rivers. Examine Figure 2, from Labov (1991: 32):



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Figure 2

Notice that the black dots, representing merger, are heavily concentrated in western Pennsylvania (the ones in eastern Pennsylvania are the result of recent independent merger: see Herold (1990); the ones in New England are a different, unrelated, merger, to a low back slightly rounded vowel which has never spread outside eastern New England). The area where this merger arose first in America, as we noted above, was around Pittsburgh. It spread westward through northern Ohio, and through central Ohio along the National Road; and then comes the mystery. It peters out through Indiana and Illinois, in spite of the fact that the Road went right through to Vandalia, Illinois, about 75 miles short of St. Louis, the intended terminus. But then the merger rises sharply to almost

total domination west of the Missouri all the way across the states. Outside of the Upper North,⁹ only the Upper South, the Lower South, and the eastern parts of Iowa escape this merger. The Upper and Lower South have a phonetically sharply distinct manifestation of what Labov writes as /oh/, as we noted earlier: they have /ow/, which is much more different from /a/ than /oh/ is, and this probably serves to block the merger. The merger is not uncommon in several areas of the Upper North that are included in the Northern Cities shift, in spite of Labov's assertion to the contrary. He asserts that, in the Northern Cities shift, “long open o and short o remain distinct” (1991: 14). But his own long-distance phone survey reflects many instances of merger, especially around eastern Minnesota, and a scattering around the Great Lakes. Given the current distribution, one would like to speculate that dialect geography has a partial explanation: the merger traveled from Pittsburgh with westward settlement across Ohio, and down the Ohio River as well as out across the National Road from Wheeling, spreading northward and westward up the Mississippi and Missouri rivers. The problem with that is, Missouri itself should be a merger area, which it clearly is not, and neither is eastern Iowa; and nowhere south of the Ohio is the merger to be found (one would think they got off the river rafts on only the north bank of the river). But there is no doubt about the rest of the west. The merger is total, in our observation, and occasional non-merger western speakers have the contrast only by hypercorrection. Older generation San Francisco and Seattle speakers sometimes have the contrast, but not younger generation speakers anywhere in the west. It has to be accepted as the new CKKMA norm, the least areally-marked pronunciation of American English, geographically most widespread, and numerically most common.

The raising of the low front vowel therefore cannot be an initial event in a drag chain of which the *cot-caught* merger is one of the dragees. If it is a chain at all, it is a push chain in which the merger is first. We also doubt that /æh/ raising to /eh/ can be blamed for the front reflexes of “short /o/”. First, the raised and lengthened reflex of historical “short ash” is essentially universal throughout the northern tier over to the Rockies, but the really noticeable front reflexes of “short /o/” (such that *pot* can be heard as close to standard *pat*) are found only in younger generation speakers of the northern cities, generally working class individuals, and the pronunciations are not yet to be heard among “standard” speakers. There is no chronological evidence presented by Labov, or known to us, to suggest that the fronting of “short /o/” to something like

⁹ We use the very persuasively justified terminology of Carver (1989). While his work is based on DARE, which maps lexical variation, the coincidence of his isoglosses with what we know about accentual differentiation in the same areas, including Labov's excellent information about the Northern Cities shift, is so nearly total that different terminology would, in our view, require much richer justification than we are prepared to supply, nor have we found it elsewhere.

[a] was the pressure which led "short ash" to protect itself by diphthonging and raising, because this fronting is very recent and very restricted by comparison with the generality of both the changes at the extremes of the putative chain, namely *cot-caught* merger and ash raising.

6. Another real on-going chain shift? The Southern shift

Here we are at a disadvantage because Labov does not give us anyone like Jackie H. to illustrate his claims. However, RPS is a native Upper South speaker, educated in the Lower South, so we can rely on first-hand knowledge about and observations of southern speech. Labov's description of the Southern Shift (1991: 28) asserts that /iy, ey, ow, uw/ "become increasingly centralized". The vowel means were plotted for Memphis by V. Fridland and presented at N.W.A.V.E. 23, (1996). Her F1-F2 plots certainly show a very striking fronting for /uw/ and /ow/, but they show /oh/ (as she writes it) consistently remaining low back, from speakers over 65 down to an 11-year-old, male and female. The reason is that the /oh/ is not an /oh/ but an /ow/. The centralizing of /uw/ and /ow/ are really simply unrounding of the first segment of the diphthong, thereby spreading the distance between the starting point and the finishing point, i.e. dissimilation, of a sort. But it really just unrounding, and that is what brings Southern States English into a degree of resemblance to Southern British and Australian, which have massively unrounded the first segment of /ow/ and /uw/ (for Australian, in this respect, see Wells (1982: 596); for London see Wells (1982: 304)).

7. A critique of Labov's chain-shifting principles¹⁰

Labov's principles, now well-known, are repeated here for convenience:

I. In chain shifts, long vowels rise (1994: 116).

Labov reports that this principle has no clear exceptions in his data (1994: 116), and further that this principle is so solid that "falling" is predicted not to occur, ever, in long vowel shifts (1994: 120). And there are no totally clear counter-examples in English,¹¹ though some of the facts in the north of England are either neutral or negative. In general Labov's principle is a reasonable char-

¹⁰ It will be clear by now that this paper is not intended as a devastation of Labov's views. We take quite literally his statement that "The principles presented here are designed to generate further interest in the problems involved" (1994: 291).

¹¹ We are content to argue about vowel shifting on the basis of the richness of the English data alone. We are uncomfortable with Labov's willingness to assume identical phenomena based on reports across a wide range of languages of the world, in many of which or even most of which there is nothing approaching the level of reliability of the data found in the *Survey of English Dialects* or the two centuries of massive philological excavation that has taken place on English.

acterization of a genuine drift for which we have no better account than this simple statement – except that it has nothing to do with chain shifts. It is the normal direction of long vowel drifts. Moreover, nobody has an explanation *why* the correlation between length and raising should exist; after all, it is a well-known fact that low vowels are inherently longer in duration than the high vowels (Lehiste 1970: 18).

II. In chain shifts, short vowels fall (1994: 116).

The second principle has little of the empirical value of the first. The three most stable short vowels in the history of English are the three front vowels that we will write simply as I E Æ. Phonetically they are now, for the most part, [i e æ]. Except for Lass, most historians have assumed that I E Æ had these values in Anglo-Saxon times: but it is possible that they had the values [i e æ] and that "long vowels" were simply identical to these vowels but longer. The lowest one has been subject to lengthening (more correctly, it has developed an in-gliding complex nucleus which should be written, Labov and Trager-Smith style, ÆH) in the environments [-s, -f, -nC, -θ], with retraction to AH as in *grass, graph, can't, bath* in many British accents (originating in the South of England in the mid-18th century) and raising to EH in the Middle Atlantic Seaboard, including Philadelphia, New York, Connecticut, Rhode Island, and eastern Massachusetts and Maine, with minimal and near-minimal contrasts having developed in pairs like *can* (v) with Æ and *can* (n) with ÆH, or *ash* vs. *ask*. As noted earlier, it has also been subject to across-the-board¹² raising and lengthening in the northern tier of states from Cleveland westward, with no minimal sets (unlike Middle Atlantic Seaboard) – i.e., all Æ's sound like the contrastively lengthened vowel of the mid-Atlantic area. These observations have been replicated now many times and make up the central part of what Labov calls the "Northern Cities Shift".

The point of these observations is that short vowels, in general, do NOT fall, in the history of English. They also do not participate in chain shifts at all, as independent nuclei. Labov cites two cases on which Principle II (the lowering of short vowels) is based; North Frisian and the Romance language of Vegilote (1994: 122). The North Frisian case involves a single short vowel change, /i/ → /a/, and though it indeed may be part of a complex picture, the case for the interrelation between events in the long vowel system and this isolated short vowel change has not been made (1994: 126). The Vegilote case is also suspect: Labov admits that Vegilote shares with ÆH in American Eng-

¹² That is, to Neo-Grammarians sound change, sweeping and completely regular. The Middle Atlantic Seaboard development is lexically variable (see Trager 1940, Kiparsky 1989, Labov 1994). But it is not a chain-shift, just a single general phonetic change.

lish, lengthening, raising, and breaking of the short vowels (1994: 128). Moreover, it is far from clear that the existence of /iɛ/ and /uɔ/ as a subsystem justifies the treatment of all other vowels as 'short'. The fact that the diphthongs were 'smoothed' to /i/ and /u/ respectively, might well suggest that this language has no quantitative distinctions, though it has simple and diphthongal vowels.¹³ Labov himself identifies problems with Principle II: London working-class speech has [ɪ], [e⁺], [ɛ] for the front short vowels.¹⁴ The Australian English and New Zealand English shifts are even more damaging (1994: 138).

A well-documented counter-example which Labov has not included in his list is the South African 'stabilizing chain' (Lass and Wright (1985), Lass (1987: 304-307)). What happens there is very similar to the New Zealand shift: centering of the high front vowels, raising of the low and mid front vowels. This chain in fact looks identical to the traditional formulation of the English vowel shift, with some variability built in:

- (5) bit ⇒ [bit]
 bet ↑ [bet ~ bɛt]
 bat ↑ [bæt ~ bɛt]

So far, just within English, we have three (or four) counterexamples, and no supporting evidence for Principle II. To quote Labov: "It is possible that there are NO (emphasis RS, DM) general constraints on the movements of short vowels: we must end the survey of completed changes by concluding that this is a possibility" (1994: 138). Why then does Labov even try and pursue the hypothesis that short vowels should fall? There appear to be two answers: (1) he needs it because he attributes the distancing from each other of the first and second constituents of diphthongs (Principle IIa, to which we return, below) to lowering of the first element; and (2) he has a mistaken analysis of the history of the short back vowels.

Consider the latter point first. Labov takes both the "short *u*" changing to MnE [ʌ] in *cut*, *but*, *flood* (after shortening), and the "short *o*" changing to BE [ɒ] and AE [ɑ] as evidence of falling. But examine these cases more closely. There were three short back vowels in Old English, namely [u ʏ ɑ]. The lowest one may have been [ɒ] – the evidence being that when it was lengthened it was unambiguously rounded, as in words like Mercian *ald* becoming ME *old*, OE *camb* becoming *comb*. In Middle English there are only two short back vowels, *u* and *o*. The short low vowel written ⟨a⟩ merged with the low front

¹³ It should be fairly common, in fact, to find languages which distinguish phonetically steady-state and diphthongal vowels, but which have no phonemic quantitative distinctions: e.g., Spanish, Bulgarian, Polish.

¹⁴ This case is presented by Labov as 'upward compression of the phonological space used by the short vowels', not a 'shift' since there are 'no entering and leaving phonemes' (1994: 138). We agree that this is not a shift, but we find it difficult to understand why 'upward compression' of the (front) vowel space should take place, given Labov's other principles.

vowel written ⟨æ⟩. The remaining non-high short back vowel had no competition in the articulatory area all the way from low central to mid back, since the ⟨a⟩ must have had low front values, not low central, in order to merge with EY shortly after it lengthened (the "long A" merger with EI), see also Lass (1976: 105-129 and the references there). Whatever the exact value of the non-high short back vowel was in Middle English, it had become [ɒ] by the time of the earliest commentaries, and there it remains in British English today, having unrounded to [ɑ] in American English.¹⁵

This tendency toward unrounding is so strong in American English that one variety of American English, now probably the majority variety, from the Plains through the Rockies and the West Coast, close to a "standard" pronunciation followed by more speakers than any other variety in the English-speaking world, simply has no round vowels at all. In BOOT it has a diphthong which may be written ɪW, in PUT a high central unround vowel ɪ, in BOAT and FOUR a diphthong ʌW, and COT and CAUGHT are merged to A, the vowel of FA-THER, CALM, POT, HOT, LAW.

So far, the story of the short back vowels is clearly not a history of "falling". The case that looks most like "falling" is "short *u*". But the change of "short *u*" to [ʌ] is simply unrounding, with the apparent "falling" being predicted by any theory of marking, preference, or optimality: namely, elimination of the highly marked vowel that results from unrounding of short *u*, that is disfavoring of [ʊ] (Round) or [ɪ] (Unround).

We conclude that Principle II, as stated, is extremely dubious. How does it fare in IIA, wherein Principle II is taken as the operational factor? We turn to that question.

IIA. In chain shifts, the nuclei of upgliding diphthongs fall (1994: 116).

To discuss this principle, we need to insert some theoretical underpinning. In the phonetic domain, there are two, and only two, parameters along which improvements or any kind of change, for that matter, can be made. One can articulate more clearly, improving the PERCEPTUAL EASE parameter, but at the cost of greater articulatory effort being expended (more DISSimilation); and one can articulate in a more casual manner with less articulatory effort (more ASSimilation), achieving improvement on the ARTICULATORY EASE parameter. For short they can be called CLEAR and LAZY¹⁶ respectively. They

¹⁵ The standard accounts of Early Modern English include (sporadic) unrounding for British English too, and even fronting (Wyld 1956: 240-242). Lass reports much more widespread unrounding in Scots, cf. *Tam, Rab*, and 19th c. Scots spellings *bannet, gat, lobster, pat, tap*, etc. (1976: 139, fn. 3).

¹⁶ The term LAZY in this sense is Robert Kirchner's, though the concept as described here has been around for a long time, popularized by Martinet (1955), used by Stockwell – Minkova (1988, 1976), and earlier as well as by Labov from around 1969.

are motivated by opposite cost/benefit ratios. Improved signal quality is expensive but more likely to be understood. Degenerate signal quality is cheaper but less likely to be understood. The relation between these two we can call "dynamic tension".

The basic idea of IIA is that diphthongs like IY and UW are pessimal diphthongs from the point of view of CLEAR, and optimal from the point of view of LAZY. AY and AW are optimal on CLEAR, but pessimal on LAZY. Given the principle of irreversibility of sound change, these principles make familiar predictions about the English vowel shift. But actually the principles represent an excessive extrapolation from the facts.

First, if they are mid upgliding diphthongs, they do not fall. EY and OW have been almost totally stable in the history of English, although EY has begun drifting recently in the way predicted by CLEAR (see the Cockney data from Wells (1982) below, and our remarks in Section 6); to improve CLEAR the nucleus can centralize or lower the onset). OW has also begun drifting in the way predicted by CLEAR and by the general tendency of back vowels in English to lose their rounding, i.e. it has become [ʌw] (the representation [ou], implying a fully rounded onset, is misleading both for RP and for American English, though of course in some northern accents of Britain it does have a fully-rounded onset, and little or no glide).

Second, we have argued above that these changes are not chain shifts but drifts. Even so, it is clear that simple "falling" of the nucleus is not an accurate description. IY and UW are "falling" at first because they start out high. The only thing they *can* do for a while is fall. A better characterization of the whole sequence, both front and back, is dissimilation of the two constituents of the nucleus, a characterization which Labov also finds useful. A nice instance which only partially conforms to Labov's IIA but rather better to the dynamic tension hypothesis is the "Cockney Shift" described by Wells (but note – it is not a shift, but parallel drifts, in that the entering (target) position is empty and the leaving position, having been emptied, remains empty).

RP	PopLon	Cockney	Conform Labov
iy >	iy >	əy	+
ey >	ʌy >	ay	-/+
ay >	ɑy >	ɔy	-
ɔy >		oy	-

III. In chain shifts, back vowels move to the front (Labov 1994: 116).

We have nothing of interest to say about this principle. It appears to be a valid instantiation of a more general principle which says, "Relieve the overcrowding,

especially in the back." It seems to have little if any application to English. The one well-known drift of this type, in the North of England where "long o" went through fronting and then raising, ending up as IH (in words like *boot*), was clearly isolated, not part of any chain shift (indeed, where it occurred, the regular back-vowel change shift did not take place). But examples like Swedish (1994: 131) and Akha (1994: 132-133) seem convincing.

8. Phonological space

In his "Overview of the Issues" Labov claims that he is introducing "a new conception of English phonological space, and a new conception of the feature *peripherality* shared by other Germanic and Baltic languages that show the diphthongization of [i:] and [u:] to [ii] and [uu]. Both front and back vowel spaces are divided into two regions of phonological space: a peripheral region, near the outside of the vowel space, and a nonperipheral one, closer to the center" (1994: 32). He then makes what we take to be one of the two central claims of the book:

It is then proposed to modify the basic principles of chain shifting to assert that tense vowels rise along the peripheral path, and lax vowels fall along the non-peripheral path.

He quotes us as having stated slightly different principles (Stockwell 1978) and he tries to reconcile his with ours (esp. 1994: 246-247):

The V of Vh and nonhomorganic Vy/w drifts upward.

The V of homorganic Vy/w drifts downward (Labov 1994: 219, quoting Stockwell 1978).

His reconciliation consists, in part, in formulating the following rule:

{G8.1} VOWEL SHIFT

[z high] →	[z + α high]/	_____ αperi βround [βround] +str +long
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In order to get the right results in the two constituents of the diphthong Labov must add a rule which operates on the output of this rule to generate "dissimilation effects", to which we give the label CLEAR, i.e. maximization of the distance between the onset and coda of the diphthong (discussed above).

Unfortunately this resolution puts Labov into a famous and familiar trap pointed out in Dobson (1956), in Stockwell's (1972) critique of Chomsky and Halle's (1968: chapter 6), and in Cercignani (1981). The trap is that the inherited

EY from Norse words (like *they*) and Mercian words (like *day* in the form *dei*) would necessarily have merged with the "long I" in its descending path. Labov is well aware of the problem. His defense against this trap is that the new diphthong was descending along a [-PERI] track while the inherited thing was [+PERI]. This view is exactly equivalent to the Chomsky-Halle view that the difference was between inherited [ēy] (note the long vowel, necessarily peripheral in this context) vs. [ey] (note the short vowel) from [iy], and we think both versions of this view are completely unpersuasive. The effort to discriminate between peripheral and non-peripheral "tracks" seems futile as a solution to this problem, even though "peripherality" has great utility as a phonological feature, because the merger-avoidance strategy has simply changed from "front vs. central" (the Dobson-Stockwell-Cercignani strategy) to "more front vs. less front" (i.e., peripheral vs. non-peripheral tracks), which is hardly more than a terminological quibble.

Later, in discussing Sievers and the question, Why is this kind of vowel shifting a specifically Germanic tendency?, Labov asserts that "Germanic uses a version of phonological space with peripheral and nonperipheral tracks both in front and in back, whereas other languages do not" (1994: 221). Now, there are two things wrong with this formulation. First, it is a tautology. If you take vowels which are inherently defined by virtue of their being nonperipheral, as long as they retain the feature of nonperipherality but simply move up or down, saying they move in a nonperipheral track is saying only that they are non-peripheral. No new information is added. Second, saying that Germanic uses this kind of phonological space whereas other languages do not is exactly equivalent to saying that (some) Germanic languages have a phonological feature [+/-PERI].¹⁷

Summarizing, there are devastating problems with either Labov's version of English chain-shifting or with chain-shifting of the types described by Jespersen, Lass, Luick, Donegan, Stampe, or Stockwell (1978: 246-247). It is because of these problems that we wrote (Stockwell – Minkova 1988) an account of the English vowel shift which entirely dispensed with the notion of "shifting" at the crucial stages, but rather looked around for a pre-existing nucleus for the shifter to merge with. We have already noted that Labov did not see this paper, no doubt because of our own carelessness. We argued then, and would now, that the historical events in questions were not the consequence of chain

¹⁷ Labov asserts that "Stockwell treats the peripheral/nonperipheral distinction as a formal device rather than a substantive description" (1994: 220). This is more than a little mysterious, since Labov himself uses peripherality as a phonological feature in the formal rule cited above. He also asserts that Stockwell's account does not become "engaged with the specific structure of phonological space exemplified here" (1994: 220). This is true if and only if Labov does not view distinctive features as substantive descriptions. We assume he would not wish to subscribe to this purely terminological revisionism. He would presumably wish to sort out the points in phonological space into contrasting distributional entities, like most other linguists.

shifts as technically defined by Labov but lexical mergers (resulting from various drifts) which spread and (sometimes, as in the GVS) got standardized. Our position, and the moral of this paper, is closest to, but not identical with, that of Liberman (1995), which we quote below:

The great Vowel Shift and its analogues did not have a "beginning", that is, there never was a time when long (bimoric) vowels began to diphthongize (for whatever reasons). Bimoric vowels always had diphthongal variants, even if some of them – perhaps under the influence of Latin orthography – were associated with, and perceived as, monophthongs... Old long vowels and diphthongs were bimoric, while the diphthongs that arose by later shifts were monomoric (they appeared on the ruins of mora counting). They were no longer divisible, and gliding became their distinctive feature... (1995: 222-223)

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