REVIEW ARTICLE

# Down for the count? The Creole Origins Hypothesis of AAVE at the hands of the Ottawa Circle, and their supporters

A review article on *The English History of African American English*. Edited by Shana Poplack. Malden, Mass., and Oxford, UK: Blackwell. 2000. Pp. xx, 277.

Reviewed by John R. Rickford, *Stanford University*

This volume, together with its sibling, *African American English in the Diaspora* (Poplack & Tagliamonte 2001), might be regarded by some as a one-two knockout punch to the creole hypothesis about the origins of African American Vernacular English [AAVE]. As Labov (2001) noted in his foreword to the latter book, "I would like to think that this clear demonstration of the similarities among the three Diaspora dialects and the White benchmark dialects, combined with their differences from creole grammars, would close at least one chapter in the history of the creole controversies." Some creolists, including this one, think otherwise, but all creolists should find the eight-chapter bout in this volume of interest.The editor, Shana Poplack, is professor of Linguistics at the University of Ottawa, where most of the volume's contributors were trained. In the "preamble" section of her introduction, she sets out the book's central question:

> The key – and as yet unresolved – question concerns the *differences* between AAVE and other dialects of English. Are they the legacy of an earlier widespread creole which has since decreolized, or reflexes of the acquisition of contemporaneous regional Englishes to which its early speakers were first exposed, followed by internal differentiation and divergence? (p. 1, emphasis added)

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The book's answer, as its title suggests, is that English dialects rather than creolist influences constitute the source of these differences.

Poplack and her associates are sometimes referred to as the "new" dialectologists or anglicists, to distinguish them from predecessors like Krapp (1924), McDavid and McDavid (1951), and Davis (1971), who made the initial arguments that AAVE features came from the regional dialects of English settlers. One commendable difference between the "new" anglicists and the old is that Poplack et al. acknowledge that there *are* differences between AAVE and other US English dialects (see quotation above), while the older anglicists often suggest that AAVE is merely Southern white speech moved North and West.[1]

Another difference between them is that Poplack and her collaborators depend on a "unique" data set from the African American diaspora – consisting of recordings with the (presumed) descendants of slaves and free Blacks who emigrated to Samaná in the Dominican Republic, and Nova Scotia, Canada, primarily in the 19th century. These new diaspora data have energized the field in recent years, and they do take us beyond the scattered textual attestations from earlier centuries that had been used in earlier debates. It is for this reason both surprising and disappointing that Liberia, a diaspora site that attracted far bigger numbers in the 19th century than Samaná,[2] is so completely neglected in this book. Poplack notes in Footnote 1 (p. 27) that the Liberian situation has been "extensively documented" by John Singler in a series of articles from 1986 to 1993 that she cites there.[3] However, Singler's findings – almost diametrically opposed to hers and those of her co-authors – are not discussed in this introduction, and they receive only minimal reference in ensuing chapters (only in Chapter 3). This is one of several crucial omissions that limit the persuasiveness of this book.

Another characteristic of the new anglicists is that Poplack and her collaborators provide quantitative analyses of specific variables (like the cop-

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**1.** Recall the classic articles by Wolfram (1974) and Fasold (1981), in which the relationship between black and white speech was framed as a two-part debate involving the differences issue and the origins issue.

**2.** Approximately 16,000 African Americans were sent by the American Colonization Society to Liberia in the 19th century (Singler 1989: 45). Only 200 were destined for Samaná in the initial migration of 1824, and by 1870, their number was estimated at between 500 and 600 (Poplack & Tagliamonte 2001: 16–19).

**3.** Indeed, the page (p. 4) with the footnote call ("1") and the page (p. 27) with footnote one itself are the only pages listed in the book's index for "Liberia."

ula and negation), focusing specifically on their distribution and conditioning, and comparing them systematically with "results for candidate sources, of both African and British origin" (p. 2). Speaking as a variationist, this is very commendable. However, comparisons with African candidate sources in this volume are rare, limited almost entirely to Igbo in Chapter 3 on plural marking.

One respect in which the new anglicists resemble the old is that they both seem to regard the Africans who came to America as blank slates, whose original languages had virtually no impact on the acquisition of the new ones they encountered in America, and on whom the features of whatever English dialects they encountered could be endlessly and easily transcribed. As Tagliamonte, Poplack and Eze put it, "[t]he results of this research permit us to reaffirm and even strengthen our earlier contention that the grammar of plural marking in Early Black English . . . owes little, if anything, to the influence of either African languages or English-based creoles" (1997: 127). In reading this, one recalls the arguments of Frazier (1939: 21) who argued that the catastrophic experience of slavery precluded the retention of Africanisms, and resulted in the wholesale acculturation of Africans in the US to European and American norms. However, Frazier's arguments have been significantly discredited by later research, from Herskovits (1941) to Gomez (1998).

Another respect in which the new anglicists resemble the old is that they still appear to be arguing against a "widespread" creole hypothesis, of the kind postulated by Stewart (1967, 1968) and Dillard (1972). That is essentially a straw man now. Rickford (1997) and Winford (1997/1998) both agree that there could not have been a "widespread" plantation creole everywhere Africans settled in the thirteen original American colonies, since the demographic and sociohistorical conditions for creole formation were not generally favorable in the Northern and Middle colonies. The creolist position has been framed since the late 1990s in terms of possible *creolist influences*. These could have come from creole varieties that developed in the deep American South – the source of *most* of America's black population from 1750 to 1900. And they could also have come from creole varieties imported via creole-speaking slaves from the Caribbean – who represented a significant part of the founding Black population in places as far apart as South Carolina and New York (Rickford 1997: 239–245).

In the rest of her introduction, Poplack discusses:

(i) the reliability and validity of the data used in this book (1.2–1.5, pp. 3–16);

(ii) the methodological assumptions and principles that guide the analysis of those data (1.6–1.9, pp. 14–20), and
(iii) the chapters that follow, grouped according to the linguistic levels (morpho-phonological, morphosyntactic, syntactic) of their variables (1.10–1.13, pp. 20–27).

With respect to (i), Poplack begins by isolating the "diachrony problem" – the fact that the origins debate requires a comparison of contemporary AAVE with earlier stages, but that reliable and valid data on such stages are difficult to come by. In particular, textual evidence – including literary attestations and personal correspondence from the 18th and 19th centuries, and ex-slave narratives written down by WPA interviewers in the early 20th century – is open to at least two criticisms: it underrepresents vernacular speech, and its transcriptions are inaccurate. Early 20th century recordings of interviews with ex-slaves in five states (see Bailey et al. 1991) are potentially more reliable,[4] but Poplack notes that their representativeness has been called into question, and they are limited to "only a few hours of audible speech" (p. 4).

As it turns out, several of the contributors in this volume do make use of the 20th century Ex-Slave Recordings (ESR). But other textual data, in particular the extensive evidence of travelers' accounts, courtroom testimony, literary renditions, newspaper and other mass media from the 17th to 19th centuries (see Brasch 1981) are not mentioned, along with letters from slaves, migrants, and overseers (Montgomery & Fuller 1996; Van Herk & Poplack 2003). And the evidence of over two dozen volumes of written ex-slave narrative data (see Rawick 1972, 1977, 1999) is ignored. One problem with this strategy is that it sidesteps data that have been central to the origins debate (e.g., Stewart 1967, 1968; Dillard 1972; Brewer 1974; Schneider 1989). A still bigger problem is that it violates the principle (see Labov 1972) that a good data set should have complimentary strengths and weaknesses. The recorded 20th-century data are potentially high in reliability, since they offer long stretches of actual speech that researchers can listen to repeatedly, code in relation to linguistic and non-linguistic constraints, and analyze with the help of VARBRUL and other programs. The written 17th- to 19th-century data are less *reliable* in those respects,

4. Wald (1995), however, highlights the role of ideology and bias in transcription (the fact that "creolists" and "anglicists" tend to find more "creole" and "anglicist" features in the texts, respectively). These factors – and their effect on the reliability and validity of the Ex-Slave Recordings – are not acknowledged by Poplack.

but there are ways of assessing their reliability (Baker & Winer 1999; Rickford 1986b), and the textual data are potentially more *valid* insofar as they take us farther back in time, and were contemporaneously generated. (That is, unlike the Diaspora recordings, which are referred to throughout the volume as evidence of "Early African American English," and purport to tell us about late 18th- and early 19th-century speech, but was actually made in the late 20th century.) Ignoring the 17th to 19th-century textual evidence is, as I have noted elsewhere "... reminiscent of the drunk who lost his wallet in a dark field, but was looking for it under a street light two blocks away because the light there was better" (Rickford 1997: 233).

Poplack goes on to discuss the "transplanted" African American Diaspora data upon which she and her collaborators rely as an alternative to older written texts. The Nova Scotia data (African Nova Scotian English, henceforth ANSE) come from two communities – Guysborough, settled in 1783 by house slaves and other Blacks from the Northern US who had been "loyal" to the British side in the American War of Independence; and North Preston, populated primarily by the descendants of refugee field slaves from the US North and South who migrated there in 1815 in the wake of the War of 1812. The Samaná data (Samaná English, or SamE[5]) are from an English-speaking enclave in the Dominican Republic settled by ex-slaves sailing from Philadelphia, New Jersey and New York starting in 1824.[6]

Poplack addresses various criticisms that have been made or could be made against the use of these diaspora data as evidence of earlier AAVE. She acknowledges that "language internal evolution" in the Diaspora communities between the 19th century and now is a potential problem, but she considers potential external influences on ANSE and SamE a bigger threat. Against this, she argues that the linguistic, social and topographic isolation of these diasporic communities makes surrounding influences unlikely. Moreover, since the external Canadian influences on ANSE would have been different from the Spanish/Caribben influences on Samaná, structural commonalities between them would argue for descent "from a common stock" (p. 10). A second critique is

**5.** Although this book's contributors use "SE" as their abbreviation for Samaná English, I will use "SamE" instead throughout this review, even in quotations, because "SE" is too well-established for "Standard English," and may lead to ambiguity or confusion.

**6.** For more details about these diaspora communities and their settlement than this book provides, see Poplack and Tagliamonte (2001).

that, particularly in Samaná, where Poplack and other White North Americans recorded the data, they may have failed to elicit the vernacular. Poplack's retort is that Dawn Hanah and other African Americans who recorded in Samaná (like Charles DeBose) were also outsiders. However, it remains true that Hannah (1997), an African American who subsequently married a local Samanán (of African American descent), *did* record higher (i.e., more vernacular) frequencies of copula absence in Samaná than Poplack and Sankoff (1984) did. And Poplack's argument that differences in *frequency* elicited by different interlocutors could not affect constraint hierarchies is challenged by recent data in Alim (2004), where it is shown that the copula absence of black teenagers in Sunnyside, California was affected both in frequency *and* constraint hierarchy by the ethnicity and familiarity of their interlocutors.[7] Finally, in response to critiques that the Diasporic communities might not be representative of earlier Black populations in the US (particularly under-representing the South), Poplack acknowledges here (p. 13) that the exact provenance of some Diaspora settlers is difficult to pinpoint, and (earlier) that Southern sources are amply represented. Her recurrent point, however, is that the strongest refutation of these and other critiques are the parallels in constraint hierarchies between both diasporic data sets and the independently collected and analyzed Ex-Slave Recordings, and the parallels between them and British origin varieties.

In relation to issue (ii), the methodological assumptions and principles that guide the analysis of the data, Poplack emphasizes that the book draws on the perspectives of variationist linguistics and the principles (albeit not the exact practices) of the historical comparative method. This means that the book's authors give primary attention to the conditioning rather than the rates of linguistic variation, that they emphasize cross-variety comparison, including regional non-standard regional white British and Canadian varieties and creoles as well as diaspora and other varieties of AAE in their search for "diagnostic"

7. In conversations with unfamiliar Black interlocutors, the Sunnyside teenagers' copula absence was higher than in conversations with unfamiliar White interlocutors (37% and 11% respectively). Their Following Grammatical constraint hierarchy was also different: Black interlocutors: __NP (.31) <__ADJ (.40)<__LOC (.55)<__VING (.64) <__GON (.92)White interlocutors: __ADJ (.39) <__LOC (.45) <__NP (.47)<__VING (.59) <__GON (.96). Eerily enough, although it may be sheer coincidence, the Black interlocutor ordering is the same one that Hannah (an "unfamiliar Black" researcher) found in her Samaná data (1997), and the White interlocutor ordering is the one that Poplack and Sankoff ("unfamiliar White" researchers) found in their Samaná data (1987).

features "which have a unique association with a source variety" (p. 18), and that they are careful to look for non-independent interactions among putative constraints.

I have no quarrel with the principles themselves, but I would like to express a few reservations about the ways in which these principles are adumbrated and implemented. First, the claim that *rates* of variable usage are irrelevant, as Poplack suggests, flies in the face of earlier work by Poplack and her colleagues (e.g., Poplack & Sankoff 1987: 303), who often include statements that the lower frequencies of copula absence and other variables show that "Early AAVE" was closer to standard English, or further from creole than contemporary AAVE. Secondly, Figure 1.1, (p. 16), which illustrates the "cross variety comparison" of this book, has *one* oval for "English based creoles" (and *none* for African languages), but *four* for colonial and contemporary English varieties ("Colonial and pre-colonial English," "20th century standard English," "20th century nonstandard English," and "20th century British source vernaculars"). This reminds us forcefully that the primary orientation of this book is "anglicist." Finally, Poplack's dismissal of creolist claims as "impressionistic" and lacking in "scientific proof ..." (p. 17) is contrary to the spirit of scientific objectivity and fairness that she otherwise strives to convey in this introduction, which sets the stage and tone for the rest of the book.

Rather than discuss the editor's introduction to the various parts and chapters of the book, I will instead go on to discuss the parts and chapters themselves.

Part I, "Morphophonological Variables," includes two chapters, one on copula contraction and absence by James A. Walker, and one on plural marking by Shana Poplack, Sali Tagliamonte, and Ejike Eze. I will review these chapters in much greater depth than the others, and this for two reasons. First of all, I was able to test their analyses through replications using original African American and Caribbean data of my own. Secondly, the variables with which these chapters deal are the *only* ones – of the six examined in this book – for which comparable quantitative data exist in English-based pidgins and creoles and Liberian Settler English.[8] Clearly, without comparable data of this type, it is difficult to use constraint hierarchies and other machinery of the variationist enterprise to decide whether contemporary or diasporic AAVE follows the

**8.** A third variable of this type is past tense marking, but this is examined in Poplack and Tagliamonte (2001), not in this book.

patterns of English dialects or pidgins and creoles. Poplack herself notes (p. 17) that cross-variety comparison is hindered by the dearth of quantitative studies of English based creoles.

*Copula Absence:* Walker's chapter focuses on copula contraction and absence, a variable repeatedly studied in AAVE, and one with parallels and potential origins in English-based creoles. Walker has two main goals: (1) to show that "there has been an unjustified focus on following grammatical category" and an unjustified neglect of preceding grammatical category (i.e., subject type) in studies of the copula in AAVE and English-based creoles (EBCs); and (2) to argue that a previously unexplored constraint – prosodic phrasing – is as significant as following grammatical category, and offers "a more meaningful linguistic explanation" for copula variability (p. 36).

The first goal is addressed primarily in the first part (2.2) of the chapter, in which the literature on zero copula and the creole-origins controversy is reviewed. If we go through each of the datasets referred to there, comparing the *range* between the values of the factors most favorable and least favorable to copula absence within the following grammatical and subject type factor groups, as in Table 1, we find that, in more than two thirds of all cases (13 out of 19), the range for following grammatical category is greater than for subject type.[9] The relative ranges within two factor groups is a common measure of their relative significance, and the data in Table 1 therefore provide no basis for the claim that the greater focus on the former in the literature is unjustified. It should be added that new data in Walker and Meyerhoff's (2004) paper on copula absence in Bequia, St. Vincent also point in this direction: the ranges for following grammatical category are .91 and .80 for 3rd singular and "other" persons, respectively, while the corresponding ranges for subject type + preceding segment are .26 and .01.

Walker's argument that Following Grammatical Category has been given excessive attention is not made on the basis of data like these (whose existence

9. In all of the six cases in which subject type is more significant, this seems to be due to distinctions between "I," "he/she," and other kinds of subject pronouns that are not usually made in studies of the copula, and that have no theoretical significance in themselves. Moreover, as Walker himself notes, his Table 2.3 (which provides four of the instances in which subject type is more significant) includes *am* as well as *is* and *are*, a copula variant that is normally excluded from consideration in AAVE studies since it shows very high rates of contraction and little or no zero copula.

**Table 1.** Comparisons of the VARBRUL factor weight *ranges* for Subject Type versus Following Grammatical Category in studies of copula contraction and zero surveyed in Walker's Chapter 2. In highlighted cases, the range for Subject Type exceeds that for Following Grammatical.

| *Study | W's table# | Variety | CONTRACTION Subj Type | Foll Gram | ZERO/DELETION Sub Type | Foll Gram |
|---|---|---|---|---|---|---|
| Baugh 1980 | 2.2 | AAVE/Cobras | .85 | < .88 | .71 | < 1.000 |
| P & S 1987 | 2.3 | Samaná | **.85** | > **.66** | **.84** | > **.76** |
| Hannah 1997 | 2.3 | Samaná | .78 | < .85 | **.85** | > **.81** |
| P & T 1991 | 2.3 | ANSE | **.76** | > **.63** | **.75** | > **.33** |
| Singler 1991 | 2.4 | LSE/Carolina | 33% | < 56% | 50% | < 72% |
| | 2.4 | LSE/Al&Slim | **.98** | > **.87** | .67 | < .87 |
| Winford 1992 | 2.6 | Trinidadian | .53 | < .77 | .25 | < .88 |
| R & B 1990 | 2.8 | Barbadian | .63 | < .75 | .65 | < .69 |
| Weldon 1996 | 2.8 | Gullah | .46 | < .64 | .22 | < .73 |
| Rickford 1996 | 2.8 | Jamaican | no data | no data | .47 | < .56 |

*In "Study" column, B = Blake, P = Poplack, R = Rickford, S = Sankoff, T = Tagliamonte.

and implications go unnoticed), but on the basis of the charge that its effects are "notoriously inconsistent" (p. 49). He tries to establish this primarily by showing that the relative orderings of a following Locative and Adjective are often reversed. But the variability of Loc/Adj orderings is old news (see Rickford et al. 1991: 121), and AAVE/Creole zero copula studies show a remarkable consistency in ranking these factors intermediate between a highly favoring __*gonna* and __Ving and a disfavoring __NP, a point that Walker himself concedes in reporting similar findings in his Samaná and ANSE data: "*As in previous studies*, Ving and *gonna* favor both contraction and zero, while NP disfavors and ADJ and LOC have intermediate effects" (p. 64, emphasis added). Sharma and Rickford (to appear) show that this ordering holds true more generally for eight different groups of AAVE speakers, and seven different groups of creole speakers. The AAVE and Creole speakers combined score 0.976 for the consistency of their following grammatical category orderings, as measured by Cronbach's alpha (Cronbach 1951), a measure in which a coefficient of .80 marks the threshhold above which items in a set are taken to correspond very closely to the same pattern.

The "inconsistency" argument is bolstered, perhaps unintentionally, by Walker's use of statistics that have been superseded by subsequent work. For zero copula rankings by following grammatical category in Jamaican, for instance, he uses Holm's (1984) analysis of Baba Rowe's Anansi stories in De Camp (1960), which pattern like this (most zero copula > least):

1. Adj (66%) > *Gonna* (32%) > NP (22%) > V-*ing* (17%) = Loc (17%)

This seems quite different from Labov's (1969) finding for AAVE in NYC:

2. *Gonna* > V-*ing* > Adj/Loc >NP

and from Baugh's (1980) reanalysis of Labov's NYC Cobras data, reported as:[10]

3. Adj (1.00) > Loc (.68) > *Gon(na)* (.60) > V-*ing* (.40) > NP (.00)

However, the Jamaican statistics in Holm (1984) had been questioned in Rickford and Blake (1990) for possibly including incommensurate variants (e.g., continuative *a* and *de* which precede invariant verb stems, versus zero and inflected copula forms, which precede Verb + *ing*), a possibility which was confirmed in my detailed (1996) reanalysis of every single copula token in De Camp's (1960) data set, the corrected statistics then (p. 360) yielding a zero copula ordering that was more similar to the classic AAVE pattern:[11]

4. *Gwain* (100%) > V-*ing* (86%) > Adj (79%) > NP (28%) > Loc (18%)

Moreover, Baugh's (1980) analysis was actually completed earlier (1977) than his (1979) thesis, and it made use of a non-application probabilities variable rule model that tended to polarize constraints since one factor in each group

**10.** For NP, I use Baugh's data for NPs without determiners rather than for NPs with determiners (.74). I do so because it is based on many more tokens (126 vs. 36, Baugh 1980: 90), and if the two categories were combined, as they are in everyone else's analysis, the resultant value would be much closer to that for bare NP than for Det + NP.

**11.** Walker (p. 48) states that my (1996) study of JC excludes *a*, *de* and *go* "following Winford (1992: 26)" rather than Rickford and Blake (1990), which he cites. And he sounds sceptical about whether the enhanced match with the AAVE pattern is cause rather than effect, suggesting that he may not have fully grasped the analytical point. He also calls into question, in footnote four (p. 68), the generalizability of my (1996) analysis, since it is "based on less than two hours of recorded speech from one speaker of JC." But this is precisely the same data pool (from DeCamp 1960) upon which Holm (1984) was based, and no similar demurrals were expressed when Holm's statistics were introduced. Finally, the number of copula tokens in my (1996) JC study (368) is in fact comparable to the number in Walker's analysis of ANSE (465), and the results line up with those I obtained from two other Jamaican speakers originally examined in Rickford (1991), referred to in my (1998) paper, and subsequently published as Rickford (1999).

had to be assigned the value of zero. This was superseded in variationist linguistics by the logistic model, the one used in Baugh's thesis, where the zero copula ordering for *is* (p. 178) was as follows, again much more similar to Labov's baseline AAVE pattern:

5. *Gon(na)* (.69) > V-*ing* (.66) > Adj (.56) > NP (.32) > Loc (.29)

Walker is indeed right that the fluctuation in __Adj/__Loc orderings, like some other aspects of the hierarchy of factors in the following grammatical category, is not adequately explained by any existing hypothesis or decreolization model. But that point has been made before (Mufwene 1992; Poplack & Tagliamonte 1991: 322–323; Rickford 1998: 181–183); and the fact remains that by the end of the chapter, he has come no closer to explaining this fluctuation either. Walker's chapter certainly does *not* demonstrate that the Following Grammatical Factor's effects are "epiphenomena of constraints dictated by prosody," as Poplack claims in her introduction (p. 21). I'll return to this point below.

Walker is also right that the subject type constraint has been relatively neglected, but that's partly because it's not usually *the* most dramatic factor in copula absence (again, see Table 1), and because no one had any potentially interesting explanations for it. He also neglects to mention that, despite my generally pro-creolist stance, I was the first to draw attention (1998: 183–185) to the fact that in *creole* data sets, NP subjects are typically more favorable to zero copula than pronoun subjects, the opposite of what is attested for AAVE. He does mention that "the one factor group for which the SamE and ANSE results do not parallel those of modern AAVE" is subject type, insofar as "NP subjects disfavor contraction and favor zero" (p. 39). But he omits to mention that the favoring effect of an NP subject on zero copula in Samaná and ANSE had also been noted in Rickford (1998: 184). This was one of several kinds of evidence presented there that these "early" African American English varieties "more closely resemble ... EBCs [English-based Creoles] than ... AAVE" – the situation that, as Walker himself notes, would obtain "if AAVE did indeed descend from a creole" (p. 39).[12]

12. Before moving on to the prosodic part of the chapter, two small points about the data in Section 2.2 remain to be made. The first is that in presenting my Jamaican data for zero before __*gonna* in Table 2.8, Walker lists the values as " – " (indicating "no data") rather than 1.00 or 100% (a categorical knockout variant). This error is easy to understand because my (1996) Table 7, does not include __*Gwain/Gonna* as a factor. But Table 6 and Footnote 7 therein show that __*Gwain* yields 100% copula absence. A second point is that in discussing

In the final sections of the paper (§§2.3–2.6), Walker addresses what is really new and quite exciting about his paper – the claim that *prosodic structure* provides a better explanation for copula contraction and absence than any of the traditional constraints.[13] By prosodic structure, Walker means the framework developed by Nespor and Vogel (1986) and others in which speech is perceived as occurring in "hierarchically arranged chunks" (Nespor and Vogel, p. 1), from the phonological utterance (U), at the top, to the syllable (σ), at the bottom. The units most relevant to Walker are the prosodic word (ω) – "the right edge of a lexical category (N, V or A)" and the phonological phrase (φ) – "the right edge of its maximal projection" (p. 50). In the following sentence from Inkelas and Zec (1993: 218), reprinted in Walker (p. 51), *Tom*, as both a noun and a Noun Phrase, is both a prosodic word (ω) and a phonological phrase (φ), but since auxiliary *iz* is an unstressed function word, it is not a prosodic word by itself and has to be grouped with the prosodic word *complaining*, thus forming the phonological phrase that constitutes the VP:

(1) $[(\text{Tom})_{\omega}]_{\varphi}$ $[\text{ iz } (\text{complaining})_{\omega}]_{\varphi}$

Walker's hypothesis is that the probability of contraction and deletion of auxiliary/copula *is* (as we'll see below, he excludes *are* from analysis) is conditioned by the prosodic structure of preceding and following constituents. In particular, he hypothesizes that:

Table 2.3, Walker argues (p. 41) that it shows "more similarities than differences" between the Samaná English results of Hannah (1997) and Poplack and Sankoff (1987). But I see more differences than similarities. For instance, in the hierarchy of following grammatical elements, Preceding Phonological environment is significant in Hannah's study but not in Poplack and Sankoff, while the reverse is true of Following Phonological environment, and so on. And the claim (ibid.) that style "can affect the overall rate of zero without affecting the factors conditioning its variability" (p. 41, attributed to Rickford & McNair-Knox 1994) is one that I would now have to withdraw or modify in light of the East Palo Alto data discussed in Alim (2004). (See footnote 7 above.)

**13.** The discussion in the rest of this section has benefited from collaboration with Julia Sweetland; lead author of our NWAV-29 presentation (Sweetland, Rickford & Hsu 2000). Julie wrote her first Stanford University linguistics qualifying paper on the subject as a graduate student in 2001 under my supervision, providing a prosodic analysis of data from the East Palo Alto AAVE project that Faye McNair Knox and I initiated in 1986. Since the discussion of Walker's prosodic analysis in this review article includes several points not in the NWAV presentation, or in Julie's qualifying paper, however, my coauthors should not be held responsible for it.

(2) "The first prediction . . . is that preceding elements which are prosodically simple (i.e., nonbranching: proclitics and simple Phonological Phrases [φ]) favor contraction more than those which are prosodically complex (complex Phonological Phrases, Intonational Phrases). ... the second prediction is that ... complex preceding and following prosodic constituents favor zero more than simple ones." (p. 56)

Table 2 helps to clarify and exemplify what counts as prosodically "simple" or "complex," according to whether the elements precede or follow the copula or auxiliary.

The first problem Walker encounters in trying to test his hypothesis is a series of overlaps between the factor groups whose independent effects he is trying to isolate and compare. For instance, preceding "personal pronouns" (in the subject type factor group) are almost all "proclitics" (in the prosodic factor group), and they end in vowels (in the preceding phonological factor group). When overlaps of this type exceed 95%, the VARBRUL program simply cannot disentangle independent factor group effects in a reliable manner (Guy 1988: 131). Some of the overlaps in Walker's data are 99% and 100% (see the cross-tabulations in his Tables 2.12–2.14). To eliminate these overlaps, he is forced to collapse factor groups, ending up with mega groups like "Preceding Prosodic/grammatical context and phonological segment" whose individual factors include "Proclitic Personal Pronouns ending in a vowel or r." But mega factors like these represent a mishmash of elements from different levels of the grammar. They therefore have no theoretical status. They also produce recurrent data gaps and make it difficult to extricate the effects of phonological, prosodic and grammatical constraints in the analysis.

To understand this more fully, consider Table 3, which reproduces the results for contraction and deletion of *is* that Walker found in his ANSE and

**Table 2.** Prosodically *simple* & *complex* elements preceding and following *is*, based on Walker's (2000) account.

| | | |
|---|---|---|
| SIMPLE: | Preceding: | Proclitics (unstressed pronouns), e.g. he's complaining |
| | | Simple (non-branching/one word) φ, e.g. John's complaining |
| | Following: | Simple (non-branching/one word) φ, e.g. he's coming |
| COMPLEX: | Preceding: | Complex (branching) φ, e.g. The old man is coming |
| | | Intonational Phrase, e.g. The answer – you know – is coming |
| | Following: | Complex φ, function word after is, e.g. He's gonna come |
| | | Clause-final function words: The place where John is |
| | | Intonational Phrase, e.g. The guy is – I think – coming |

SamE data.[14] Looking first only at contraction, and at preceding elements, Walker reports that "[p]rosodically complex elements tend to disfavor the contracted form: a preceding IP disfavors contraction highly, while proclitic personal pronouns favor contraction almost categorically, and all other categories disfavor" (p. 61). This seems true enough at first glance, since the probabilities for prosodically "complex" factors (shaded) in the "contracted" columns of Table 3 are all considerably below .50, which in VARBRUL results, indicates disfavoring effects. However, in the sixteen (unshaded) cells for "simple" preceding elements in the "contracted" columns of Table 3 (recall from Table 2 that "proclitics" are also "simple" prosodic elements, although not in themselves "Simple Phonological Phrases" (φ)) only *three* values (in boldface) show the expected favoring effect, and the high values for "Proclitic personal pronouns ending in a vowel or r" (.97, .91) could be attributed to the pronominal or phonological effect rather than the prosodic one. The values in the other eight simple/unshaded cells for which there *IS* data (five have "no data") are not only all below .50, and therefore *dis*favoring to contraction (contrary to Walker's first prediction in (2)), but they also vary widely, from .04 to .48, suggesting that the non-prosodic factors are playing key roles.

When we concentrate only on the cells that allow us to isolate the effect of prosodic factors, we get mixed results. For instance, in ANSE, the contraction probability for "'Simple' φ, Noun ending in V/r" is .42, more favorable to contraction than the .22 weight for 'Complex' φ, (Noun) ending in V/r," and thus interpretable as supporting Walker's hypothesis (although the .42 figure should ideally be over .50). But the ANSE contraction probability for "'Simple' φ, Noun ending in C" is .04, *lower* than the corresponding .12 weight for "'Complex' φ, Noun ending in C," the opposite of what Walker's hypothesis would lead us to expect.

14. Walker excluded *are* from consideration because *are* contraction is restricted to postvocalic environments, and because of the difficulty of distinguishing *are*-deletion from *(r)*-deletion. However, Wolfram (1974) has demonstrated that the variable contraints on the two processes are different, and Rickford et al. (1991) show that, notwithstanding the fact that *are* contraction is not possible after consonants (in "the men're tall", the "re" segment is syllabic), the contraction and absence of *are* patterns similarly to the contraction and deletion of *is* in terms of most of its preceding and following constraints, phonological and grammatical. Considering *are* along with *is* allows one to state the variable constraints once instead of twice, and the expanded data pool increases the robustness of the analysis.

**Table 3.** Factors contributing to *is*-contraction and absence ("zero") in African Novia Scotian English (ANSE) & Samaná English (SamE)*

| | *Contracted* | | *Zero* | |
|---|---|---|---|---|
| | ANSE | SamE | ANSE | SamE |
| Input: | .874 | .564 | .241 | .166 |
| Total N: | 465 | 556 | 334 | 287 |
| **Factor groups** *and Factors* | | | | |
| **Preceding prosodic/grammatical context and phonological segment** | | | | |
| *Proclitic Personal Pronoun ending in V/r* | **.97** | **.91** | .33 | .39 |
| *Proclitic Personal Pronoun ending in C* | *no data* | *no data* | *no data* | *no data* |
| *Proclitic, Other Pronoun ending in V/r* | *no data* | .45 | *no data* | 0% |
| *Proclitic, Other Pronoun ending in C* | .04 | .21 | **.94** | **.97** |
| *'Simple' φ, Other Pronoun ending in V/r* | **100%** | .48 | .24 | **.93** |
| *'Simple' φ, Other Pronoun ending in C* | *no data* | *no data* | *no data* | *no data* |
| *'Simple' φ, Noun ending in V/r* | .42 | .41 | **.66** | **.64** |
| *'Simple' φ, Noun ending in C* | .04 | .10 | **.68** | **.78** |
| *'Complex' φ (Noun) ending in V/r* | .22 | .21 | **.63** | **.73** |
| *'Complex' φ, (Noun) ending in C* | .12 | .07 | **.85** | **.88** |
| *Intonational Phrase* | .01 | 0% | – | *no data* |
| **Following prosodic/grammatical context** | | | | |
| gonna *in a 'Simple' φ* | *no data* | *no data* | *no data* | *no data* |
| *Verb +* ing *in a 'Simple' φ* | .76 | **.83** | **.75** | **.92** |
| *Adjective in a 'Simple' φ* | .41 | .54 | **.67** | **.65** |
| *Participle in a 'Simple' φ* | .28 | .33 | .19 | **.81** |
| *Locative in a 'Simple' φ* | .38 | **.66** | 0% | .45 |
| *NP in a 'Simple' φ* | .27 | .36 | .35 | .21 |
| gonna *in a 'Complex' φ* | **.94** | **.97** | **.64** | **.94** |
| *Verb +* ing *in a 'Complex' φ* | 100% | **.89** | **.94** | **.90** |
| *Adjective in a 'Complex' φ* | .44 | .20 | .24 | .45 |
| *Participle in a 'Complex' φ* | .43 | .42 | 0% | .54 |
| *Locative in a 'Complex' φ* | .70 | .46 | .51 | .23 |
| *NP in a 'Complex' φ* | .62 | .47 | .30 | .53 |
| *Intonational Phrase* | .05 | 0%/KO | *no data* | *no data* |
| **Following phonological segment** | | | | |
| *Consonant* | [ ] | **.62** | [ ] | *excluded* |
| *Vowel* | [ ] | .45 | [ ] | *excluded* |

*Source: Walker (2000a: 62–63, Tables 2.15 and 2.16), as adapted in Sweetland, Rickford & Hsu (2000). Computational methods: Labov Contraction (C + D/F + C + D) and Labov Deletion (D/C + D), where C = Contractions, D = Deletions, and F = Full Forms.[15] Probabilities in bold (over .50) indicate they favor rule application (contraction or zero). Square brackets represent instances in which the effects of a factor group were statistically insignificant, as with the following phonological segment in ANSE. For that same factor group, "excluded" refers to an instance in the SamE data in which "following phonological segment interacted so much with the other two factors that it was impossible to obtain a valid result" (Walker, p. 63). Shaded cells are prosodically complex; unshaded cells are prosodically simple.

Other portions of Table 3 show similar problems. With respect to the "preceding prosodic/grammatical" element results, Walker notes that "those categories that favor contraction disfavor zero, and vice versa" (p. 61). This is generally true, but the fundamental hypothesis that prosodically complex elements favor zero more than simple ones (Walker's second prediction) is not upheld, since there are zero-favoring values (above .50, in boldface) throughout the prosodically simple cells (unshaded) as well. In fact, of the eleven unshaded preceding context "zero" copula cells for which we have data from ANSE and SamE, as many as seven are above .50, indicating that they favor deletion or zero; and three of those do so at almost categorical rates (.94, .97, .93).

In discussing the results for *following* prosodic/grammatical context, Walker says first that it was "also selected as significant in both ANSE and SamE" (p. 61). But in fact, from the bigger *ranges* reported for this factor group compared with the preceding factor group in three of the four cases in Tables 2.15 and 2.16 (.75 vs. .70 for zero in ANSE, .77 vs. .71 for contraction in SamE, .73 vs. 58 for zero in SamE), the following prosodic/grammatical context is *more* significant, contrary to Walker's gripe about the relative attention paid to subject type versus following grammatical environment, and in line with the indications of earlier studies, shown in Table 1 above. Walker goes on to admit, in a refreshingly candid way, that "an interpretation of the results is not immediately apparent." (p. 64). But he still attempts to discredit the validity of the following grammatical category, by arguing (ibid.) that the fluctuations in the relative orderings of a following locative and adjective are systematically related to their prosodic complexity. This is true for the ANSE contraction data, as we can see from Table 3 (Adj .41 > Loc .38 for simple φ, but Adj .44 < Loc .70 for complex φ). But it's *not* true for the SamE data: Adj < Loc for both simple (.54 < .66) and complex (.20 < .46) φ in contraction, and Adj > Loc for both simple (.65 > .45) and complex (.45 > .23) φ with respect to zero copula.

What Walker does NOT say – and the omission is striking – is that his prosodic hypotheses (see 2 above) fail badly for the following grammatical/prosodic context. Contraction is *not* favored by simple over complex phonological phrases when the grammatical category is held constant. In fact, for ANSE, the reverse is true for *all* of the five categories for which we have comparable data (e.g., .62 for __NP in a complex φ vs. .27 in a simple φ), and this is also the case for three of the five categories in SamE. Similarly zero is favored by complex over simple phrases in only four of the ten comparable categories for which we have data in ANSE and SamE. In fact, the only following category effect that is consistent, and in line with predictions previously established in the literature, is the following grammatical one, which Walker summarizes as "V-ing and gonna favor both contraction and zero, while NP disfavors and and ADJ and LOC have intermediate effects" (p. 64). Contra Poplack's claim, there is no evidence here for the following grammatical effect being an "epiphenomenon" of the prosodic constraint (p. 19).[16]

In his conclusion (§2.6), Walker suggests that the consistent favoring of contraction and zero by __V-*ing* and __*gonna*, and its disfavoring by __NP "in every study" (p. 66) might reflect the basic distinction between the auxiliary and copula. He calls for further study of the semantic, syntactic and prosodic correlates of this distinction. On this point, I would concur. The distinction itself is one that has been long noted, and we have begun to investigate it more closely in recent work (Sharma & Rickford to appear).

Walker also suggests that the Following Grammatical Category is not a well-defined factor, repeats the (unsupported, as noted above) claim that "many of the purported grammatical effects are due to prosody" (p. 67), and proposes that we extend our study of copula variability beyond AAVE and English-based creoles to include other dialects of English (e.g., Canadian English, as studied by Walker & Meechan 1999). In the final sentence of his chapter, he concludes that this extension "could form the basis of a truly comparative approach, one that might provide reliable evidence for the origins of zero copula in AAVE" (p. 68). I am not opposed to studying copula variability in other dialects of English, and welcome the proposal for all its potential insights. But the single-mindedness with which Walker advances this proposal that we look only to English varieties for insight is symptomatic of the fundamental limitation of this book's Anglophiliac orientation. One reason for our initial interest in following grammatical environment is the evidence

**16.** In his (2000) thesis, written after the chapter in this volume although both works have the same date, Walker attempts another method of pinpointing the prosodic effect that is more theoretically coherent, and less subject to the massive interactions between factor groups evidenced in the chapter under review here. Following Inkelas and Zec (1995), it takes into account the prosodic configuration of sentence as a whole, dividing sentences (pp. 88ff.) according to whether a Phonological Phrase boundary intervenes between the copula and the subject (Type 2) or not (Type 1). Although he does not include following grammatical category in the new analysis, Walker finds a consistent effect in ANSE and SamE in which Type 1 prosodic structures sentences favor contraction and Type 2 favor zero. But in a replication with AAVE data from East Palo Alto, California, that did include following grammatical environment, Sweetland, Rickford and Hsu (2000) found that following grammatical was significant, and Type 1/Type 2 sentence prosody was not.

that the form and absence of the auxiliary and copula were crucially determined by this factor in the African languages spoken by the ancestors of today's AAVE and English-based creole speakers, and in the creole varieties themselves (Holm 1976, 1984; Baugh 1980). Given the evidence that L1 language transfer *does* influence second language acquisition (Odlin 1989; Sarhimaa 1999), why would we look only to English dialects, and not to further study of the West African languages that were the primary first languages of African American and Caribbean slaves? This one-sided research strategy is all the more incomprehensible because, as Walker himself notes (ibid.), the feature in question – copula absence – is *not* attested as a productive process in the history of English. This preference for tracing AAVE phenomena to English even when English provides no clear historical models for it is also manifested in the chapter on zero plurals, to which we now turn.

*Plural marking:* Chapter 3, by Poplack, Tagliamonte and Eze (hereafter PTE), is an amalgam (with much of the original wording and many identical tables) of two earlier articles. They are Poplack and Tagliamonte (1994), which focused on plural marking in SamE, ESR and ANSE, and Tagliamonte, Poplack and Eze (1997), which provided a complementary analysis for Nigerian Pidgin English (NPE). The updated integration of these articles yields a relatively compact chapter that fits seamlessly with the rest of the volume. But some valuable details from the source articles are lost in the process.

From Poplack and Tagliamonte (1994), we lose, for instance, an explicit definition of the "comparative reconstruction" that Chapter 3 (p. 74) and others claim to be providing.[17] We also lose some of Singler's (1989, 1991) Liberian (Settler) English [LSE] data that were included in the earlier article, along with a potential explanation (pp. 247–248) of why the authors group LSE

17. Their definition is as follows: "Comparative reconstruction involves comparing features (in this case, patterns) of (putatively) related varieties and drawing conclusions about their common ancestor" (Poplack & Tagliamonte 1994: 248). One can agree with this in general, while noting that a crucial element in conventional Comparative Reconstruction is the reconstruction of proto-language forms and features (a step that Poplack and her co-authors usually omit), and that comparative reconstruction as practiced in historical-comparative linguistics more commonly and confidently involves the reconstruction of phonemes and lexical items than of morphology and syntax. As Sihler notes, "a generally accepted reconstruction of affixes tends to take shape much more slowly and tentatively than the reconstruction of phonology and lexicon. … And reconstructing anything like an explicit proto-syntax is very questionable" (2000: 149–150).

with the "Pidgin/creole varieties" rather than with the other "Early AAE" varieties (SamE, ESR, ANSE), despite the fact that it is another diaspora variety of African-American English that was transplanted to Liberia at about the same time as its counterparts to Samaná and Nova Scotia (p. 248). I will return to this point in discussing the crucial Table 3.6 with which Chapter 3 concludes. But I would urge anyone seeking a deep understanding of the variables investigated in this chapter to read the authors' source articles as well as each of the quantitative analyses of plural marking referred to in this chapter, including Patrick et al. (1993), and its (uncited but in some ways more important) successor, Patrick (1994).[18]

PTE begin by noting that the plural is often unmarked in Early AAE, much more so than in modern or contemporary AAE: 23.7% in SamE (397/1672), 26.9% in ESR (115/427) and 36.4% in ANSE (492/1353),[19] compared with 1% to 13% for contemporary AAVE.[20] But they conclude that this greater non-standardness is not indicative of a creole legacy, because the "Early AAE" varieties do not appear to follow the constraints that govern plural marking in creoles.

The creole system, following Alleyne (1980), Bickerton (1975), Dijkhoff (1983), and Mufwene (1986), is said to involve the use of zero on non-individuated nouns (primarily generics, perceived as non-denumerable, e.g. *dog*), and on individuated nouns whose plurality is unambiguous because of semantically plural demonstratives, numerals or quantifiers within the NP (e.g., DEM/TWO/PLENTY *dog*). The only semantically plural nouns in English-based creoles that are said to require marking with the basilectal pluralizer

18. I am grateful to Peter Patrick and John Singler for discussion and clarification of many of the intricacies of plural marking in their Jamaican and Liberian data sets, especially in comparison with the analyses provided by PTE for NPE and "early AAE."

19. PTE do not actually provide these percentages, although they do provide closely related "corrected means" in Table 3.1 (SamE = .22, ESR = .24, ANSE = .34). I calculated the percentages by using their Table 3.6 (2000:97) data on the total number of plural nouns considered in each sample, and Poplack and Tagliamonte's (1994:245) data on the number of zero plurals in each sample.

20. PTE list the range for contemporary AAVE as "2 percent to 11 percent" (2000:76). I have extended this to 1% to 13% to include individual plural absence data for two of the six East Palo Alto, California speakers (Paula Gates and Foxy Boston, respectively) examined in Rickford (1992). Labov et al.'s (1968:161) NYC peer groups had 8% zero plural, and Wolfram's (1969:150) lower working class teenagers had 7.4%.

*dem* are individuated nouns that are not so disambiguated, specifically, nouns preceded by the definite article or a possessive (e.g., DE *dog dem,* ME *gyal dem).*[21]

The chapter proceeds through various quantitative analyses of variable plural marking in the three "Early AAVE" varieties, compared with mesolectal Gullah, mesolectal Nigerian Pidgin English, Liberian Settler English, and Liberian English (a continuum from Liberian Interior English to Liberian standard, excluding Kru Pidgin English and Liberian Settler English). In addition to an NP Constituency factor group that operationalizes the creole predictions outlined above, the authors consider "semantic classification" (for the commonly reported claim that English nouns of weight and measure often occur bare), "animacy of the noun" (inanimate nouns favor zero marking in NPE, as in its primary substrate, Igbo), and phonological constraints (preceding and following phonological segments, found to be significant in contemporary as well as "Early" AAE). The conclusion is that the Early AAE varieties differ from the pidgin/creole varieties in showing no significant animacy effect, a reverse generic effect (less zero marking with generics rather than more), and a significant effect of following phonological segment, and that their number marking system must therefore be English- rather than creole-derived.

The argumentation is quite exhaustive, and the authors make a commendable attempt to scour the history of English for sources of zero marking, and to draw on most of the existing quantitative analyses of plural marking in English-based pidgins and creoles (of which more exist than for any other area of the grammar). However, I find their verbal interpretations out of synch with the statistics in several cases, and I find the framing of the argumentation and facts frequently self-serving (with respect to their over-arching "English"-origins assumption). To illustrate this, let us consider Table 4, which is an extended and modified version of their Table 3.6 – the culmination of their efforts to situate "Early AAE vis-à-vis Other Comparison Varieties" (pp. 96–98). The four data columns to the right in Table 4 incorporate data that were not in their Table 3.6 – Jamaican (basilectal) creole data from Patrick (1994), and Jamaican

**21.** A basic assumption of this chapter, as of most of the quantitative research that has been done on plural marking in creoles since the mid 1980s (e.g., Rickford 1986; Singler 1989, 1994), is that mesolectal plural marking with *-s* instead of *dem* should be subject to the same constraints (or at least, this possibility should be empirically investigated). This assumption – in line with Bickerton's (1975) hypothesis that basilectal grammatical constraints continue to manifest themselves in mesolectal forms – may be particularly questionable in the case of pluralizing *dem* and *-s*, as Patrick (1994) suggests.

(basilectal) and Guyanese (basilectal and mesolectal) data from my own fieldwork, analyzed in the summer of 2003. The seven data columns to the left are identical with PTE's table 3.6, except that:[22]

(i) Corrections to errors PTE made in transcribing data from other scholars' articles are indicated with asterisks; e.g., they give the input probability for Gullah as .22, and for LSE as .35, but the correct figures are .78 and .30 respectively (Rickford 1986b: 51; Singler 1989: 55); and they list the sample size for LE as 571, but it is 2039 (Singler, 1991: 562).
(ii) Factor weights favoring zero marking (those over .50) are indicated in bold.
(iii) The language variety groupings are different. PTE include Liberian Settler English with the Pidgin-Creole rather than the Diaspora varieties, grouping Samaná English, African Nova Scotia English, and the Ex-Slave recordings together as "Early AAVE." This strikes me as problematic in two respects. First, LSE should *not* be excluded from the other Diaspora varieties. It was settled in the 19th century in much the same way as the other settlements, and Singler (1989: 48) makes arguments similar to those made by Poplack and Tagliamonte (1994) about the settlers having been an enclave community, relatively resistant to linguistic and cultural influences from surrounding communities. One possible reason why these authors want to separate LSE from the other diaspora varieties is that LSE differs in showing a stronger generics effect on plural absence, and a weaker numerics effect. However, the similarity between the LSE overall rate of plural marking (.30) and that of the other diaspora varieties (compare ANSE's .34) and its sharp difference from the rate for non-settler Liberian English (72%), which is clearly more like NPE and the creole basilects, argues, along with sociohistory, for the inclusion of LSE in the diaspora group. Second, the Ex-Slave Recordings should be separated from the diaspora varieties, if only as a subgrouping of "Early AAE," since the ESR speakers have lived continuously in the US, and they are themselves the exemplars of 19th-century patterns (as against their descendants in the case of the diaspora varieties). The ex-slave recordings were generally made earlier than the diaspora recordings (1935–1944 vs. 1980s), and the interviewees in these cases were born between 1844 and 1861. Despite problems of representativeness and their relative 'lateness' in the chronology of the African

22. Another minor difference is that PTE's Table 3.6 lists the syntactic/semantic variables first and the phonological variables next, while the order is reversed in my Table 4.

presence in North America (which begins in the 17th c.), these ex-slave recordings are the closest thing we have to a genuine "Early African American English" in *audio recorded* form, and we should set them apart from the others for historical comparison. In view of these considerations, I have separated the varieties into two broad categories: "Pidgin -Creole" on the right, and "Early AAE" on the left, the latter subdivided into "Ex-Slave Recordings" and the three Diaspora Varieties (SamE, ANSE, LSE).

Let us now turn to substantive discussion of Table 4, starting with the phonological factors.[23] PTE assert that:

> Where the Early AAE varieties differ from the English-based creoles is with regard to the following phonological segment. In each of the former (*but none of the latter*), we observe the by now familiar effect: consonants favor zero realization. (p. 98, emphasis added)

This generalization at first appears to be confirmed by the non-significance of this factor group in four of the seven pidgin/creole varieties, and by the fact that a following consonant actually disfavors zero in two of them.[24] But note that the Following Phonological Segment is also insignificant in one of the "Early AAE" varieties (LSE), and that one of the pidgin-creole varieties does show the favoring effect of a following consonant which PTE claim to apply in "none of them," – a point that the authors acknowledge, quite remarkably, in their Footnote 7 ("[None ...] With the exception of Gullah ... which patterns like the Early AAE varieties."). In fact, examining the data more closely, in light of the variationist interest in whether a following pause patterns like a consonant

**23.** Notice first the empty square brackets for factor groups found to be statistically insignificant in VARBRUL. An alternative tradition, attested in PTE's Table 3.1, is to include probability coefficients (or factor weights) even for non-significant factors, so that readers can see whether the trends point in the same direction. As Poplack and Tagliamonte note in discussing the equivalent of Table 3.1 there (Table 4): "Factor weights enclosed in square brackets were not selected as significant . . . Although there are not enough data to rigorously establish statistical significance for these factors, we include them because they suggest further confirmation of the remarkable similarities across varieties" (1994: 241). This cannot be done with Table 4 (= their Table 3.6).

**24.** The cases in which the following consonant does not have a favoring effect are both basilectal creoles (Guyanese and Jamaican). These varieties show a higher proportion of *dem*-marked plurals (*de boy dem*) than the mesolectal varieties do, and *dem* marked pluralization is not subject to exactly the same constraints (especially phonological constraints) as *-s* marked pluralization, as Patrick (1994) has shown.

**Table 4.** Variable rule analysis of the contribution of recoded factors to the probability of zero plural in Ex-Slave Recordings, African American Diaspora varieties, and pidgin-creole varieties. Adapted and extended from Poplack, Tagliamonte and Eze (2000), Table 3.6.

| | 'Early' AAE Varieties | | | | Pidgin and Creole Varieties | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ESR | Diaspora Varieties | | | Data presented in Poplack (2000) | | | Additional data | | | |
| | *Ex-Slave Recordings* | *Samaná English* | *African Nova Scotia English* | *Liberian Settler English* | *Gullah* | *Nigerian Pidgin English* | *Non-Settler Liberian English* | *Jamaican Creole 1994* | *Jamaican Creole 2003* | *Basilectal Guyanese Creole 2003* | *Mesolectal Guyanese Creole 2003* |
| | ESR | SAM | ANSE | LSE | GUL | NPE | LE | JC94 | JC03 | GCB | GCM |
| Sample size | 427 | 1672 | 1353 | 574 | 128 | 1316 | 2039* | 1126 | 663 | 587 | 613 |
| (Input prob) | (.25) | (.23) | (.34) | (.30)* | (.78)* | (.40) | (72%) | (.64) | (.69) | (.81) | (.32) |
| **Following Phonological Segment** | | | | | | | | | | | |
| Consonant | **.53** | **.62** | **.71** | [ ] | **.61** | [ ] | [ ] | [ ] | .41 | .42 | [ ] |
| Vowel | .37 | .46 | .41 | [ ] | .30* | [ ] | [ ] | [ ] | **.56** | .46 | [ ] |
| Pause | **.65** | .43 | .46 | [ ] | **.60** | [ ] | [ ] | [ ] | **.54** | **.62** | [ ] |
| **Preceding Phonological Segment** | | | | | | | | | | | |
| Non-sibilant consonant | **.58** | .55 | [ ] | **.64** | **.65** | [ ] | **.72** | **.57** | **.52** | [ ] | **.57** |
| Sibilant consonant | .27 | **.56** | [ ] | .37 | **.59** | [ ] | .21 | .38 | **.66** | [ ] | .40 |
| Vowel | .45 | .42 | [ ] | .46 | .27* | [ ] | **.63** | .38 | .44 | [ ] | .50 |
| **Type of Nominal Reference** | | | | | | | | | | | |
| Generic | .27 | [ ] | .44 | **.59** | [ ] | **.57** | **.65** | [ ] | [ ] | [ ] | [ ] |
| Def & Indef | **.58** | [ ] | **.52** | .41 | [ ] | .47 | lower | [ ] | [ ] | [ ] | [ ] |
| **Animacy of Noun** | | | | | | | | | | | |
| [–anim.] | [ ] | [ ] | [ ] | [ ] | – | **.54** | **.67††** | .52† | [ ] | **.52** | **.53** |
| [+human] | [ ] | [ ] | [ ] | [ ] | – | .38 | .33 | .38 | [ ] | .33 | .27 |

Notes: Input probabilities (corrected means) reflect the overall likelihood of rule application in each sample. Figures marked with an asterisk * have been corrected from Poplack et al. (2000: 97) Table 3.6 to reflect data presented in original articles. Factor weights in **bold** are greater than .50 and favor rule application (zero plural). Factor weights in plain text are less than .50 and disfavor rule application (retention of –Z or _dem) Square brackets [ ] indicate that the factor group was not statistically significant (p. > .05)

† These data are not available in the original article; they were computed more recently (11/2003) by Peter Patrick (p.c.).

†† .67 in Singler's NSLE data is for [–human], including both animals and things (inanimates).

Data Sources: *Ex-Slave Recordings, Samaná English, African Nova Scotia English*, Nigerian Pidgin English, Poplack et al. (2000: Table 3.6); *Gullah*: Rickford (1986: Table 3); *Liberian Settler English*, Singler (1989: Table 8 – converse of values calculated to get zero-marking); *Non-Settler Liberian English*: Singler (1991: Table 36.2 – converse of values calculated for zero-marking); *Jamaican Creole English94:* Patrick (1994: Table 1 – converse of values calculated for zero-marking; figures are for -Z only, not *dem*, tabulated separately; also, "Human" contrasted with 6 other factors; most are "higher" for zero plural, but not Weight/Measure N's (.23) and "Time/Day" tokens (.34). *Jamaican Creole English '03, Guyanese Basilect, and Guyanese Mesolect:* data on these varieties were transcribed from 1990s recordings, and tabulated by John Rickford with the help of Nadiya Figueroa (Jamaica) and J'Leise Springer (Tobago) in 2003.

or vowel in influencing final stop deletion (see Guy 1980: 27–8), it is clear that "Early AAE" is not uniform. In African Nova Scotia English and Samaná, pause patterns with vowel in disfavoring zero. Only a following consonant, threatening to create a dispreferred consonant cluster if a plural *-s* is retained, disfavors plural. This low pause effect is associated with Philadelphia, in studies of final *t, d* deletion. The ex-slave recordings (ESR) are quite different, resembling Gullah insofar as pause and consonant pattern together in favoring zero while vowel strongly favors the retention of *-s*. (In these varieties, a following vowel favors *-s* retention, perhaps by allowing the *-s* to resyllabify and serve as the onset of the following word, something which a following pause or consonant does not.) Interestingly enough, Guy (1980: 28) found this "high pause" pattern for *t, d* deletion characteristic not only of New Yorkers, but also of all the Black speakers he surveyed, regardless of their geographical location.[25] (The only exception was Black speakers from Detroit examined by Wolfram 1969.) We do not know yet what deeper significance to attach to these sub-groupings. Is there historical significance to the fact that the two Diaspora varieties (SamE and ANSE) are low pause, and the two non-Diaspora varieties (ESR and Gullah) are high pause? And could the Samaná low pause effect represent a retention from Philadelphia, the city from which most African American emigrants sailed to Samaná? These are some of the intriguing historical questions the data raise once we free ourselves from a narrow focus on similarities within the "Early AAE varieties" and differences between them and the pidgin-creole varieties.

Similar complications are evident when we contrast PTE's discussion of the results for the second factor group, *preceding phonological segment*, with the VARBRUL factor weights therein. They say, simply, and dismissively, that "Preceding phonological segment is selected as significant in most varieties; these share a variable process of consonant cluster simplification, though they handle epenthetic vowel insertion after sibilants differently" (p. 98). Well, yes, but what is *really* interesting is that the Ex-Slave recordings are more like Liberian Settler English, and Jamaican Creole94, insofar as a preceding sibilant patterns with a preceding *vowel* in disfavoring *-s* deletion. But Samaná English, Gullah, and Jamaican Creole 2003 behave differently, with a preceding sibilant similar

**25.** New AAVE data on plural absence constraints in Princeville County, North Carolina (Rowe 2004) are similar too, with a following pause (.52) patterning more like a following Consonant (.61) than a following vowel (.38). (In Rowe's variable rule analysis, the effects of a following semivowel, .59 and nasal, .70 are computed separately.)

to a preceding non-sibilant consonant in favoring zero, sometimes more so. (The non-significant factor weights for ANSE show it to be weakly similar to Gullah and SamE in this regard.) Once again, the most salient similarities and differences the data present are *not* the ones to which the authors draw our attention, and they challenge the neat line they draw between "Early AAE" and "Pidgin-Creole Varieties."

In the case of the third factor group, *type of nominal reference*, one is at first tempted to concede PTE's primary point – that two of the pidgin/creole varieties (Nigerian Pidgin English and Non Settler Liberian English), do show high generic effects, while the opposite situation obtains in the Ex-Slave Recordings and African Nova Scotia English (weakly so in the latter case – note the small 8 point range between generic and non-generic). LSE patterns with the pidgin-creole varieties on this factor. But before we enshrine the stipulative/descriptive claim that creole generics always favor zero plural, note that in *five* of the *seven* pidgin-creole samples, this factor group has no significant empirical effect, as is also true in Samaná. And note too that the generic effect is only evident in the *African*-based pidgin-creole varieties, where one might expect continuing influence from co-existent African languages.[26]

In the case of the fourth factor group, *animacy of the noun*, the favoring effect of inanimates on zero marking in virtually all the pidgin-creole varieties in which this constraint was investigated, and its insignificance in ESR and *all* of the diaspora varieties (including LSE – more evidence that LSE should be grouped with ANSE and SamE), seems to support PTE's claim that we are dealing with different kinds of conditioning here. But a few matters remain to be clarified. First, note that animacy has no significant effect in at least one pidgin-creole variety, Jamaican Creole'03, and that in an earlier Jamaican Creole data set not tabulated here (Patrick et al. 1993), the constraint effects were reversed, with human nouns favoring zero plural *-s* marking (.57), more than inanimate ones (.49). Secondly, PTE characterize the relevant distinction as being between humans and inanimates, but their factor weight data actually distinguish between humans and non-humans (the latter including "things"

**26.** Lest we take these data as "God's truth," note too the powerful effect of recoding in three of these cases. After converting the significant six or seven level "NP Constituency" factor group into the two level "Nominal Reference" factor group (plus the conversion of Animacy from a three level to a two level factor group) in JC'94 and JC'03, NP constituency dropped out of significance. Judging from a comparison of PTE's Tables 3.1 and 3.6, this also appears to have been true in their Samaná English data.

and "animals") in at least three cases: LSE (see Singler 1989:62, Fn. 19), NSLE (see Singler 1991:549) and NPE (see Tagliamonte, Poplack, & Eze 1997:121).[27] The distinction is not likely to make a significant statistical difference, because in the GC and JC data sets (and presumably others), plural nouns referring to inanimates are 10 to 22 times more numerous than nouns referring to animals, so when these two categories are combined as [–human], this new factor tends to have the statistical weight of the inanimates.[28] But it may be theoretically and historically important to observe the distinction, and to provide data on all three categories. For instance, since the animacy hierarchy is "human > animal > inanimate" and since "noun phrases higher in animacy have the [number] distinction while those lower in animacy do not" (Comrie 1981:178, 182), one might expect that nouns referring to animals would favor zero at a rate intermediate between those referring to humans and inanimates. But in all four of the Caribbean data sets in which animacy is significant, nouns referring to "animals" show the *highest* propensity for zero *-s* marking (contrast the lower weights for humans and inanimates in Table 4): .77 in GC mesolect '03 (n = 23), .84 in GC basilect '03 (n = 21), .82 in JC basilect '94 (n = 44, Patrick 1994, and p.c. 2003), .85 in JC basilect '93 (Patrick et al. 1993).

Moreover, PTE (2000:90) explain the animacy effect in NPE as a possible transfer from Igbo, the first language of most of their informants, and they

**27.** PTE's (1997:121) Table 8 on NPE zero plural marking distinguishes between non-humans [–human] and humans [+human], while their (1997:116) Table 6 on NPE zero plural marking distinguishes between inanimates [–animate, –human] and humans [+animate, +human] with no apparent provisions for animals [+animate, –humans]. Since the sample size in both cases is identical (n = 1316), might the non-human [–human] wording in Table 8 simply have been a shorthand for the inanimate [–animate, [VIP'] –human] distinction in Table 6? Not likely, both because a data set of this size probably includes some nouns with animal referents, and because their NPE sentence 7a (PTE 1997:112) demonstrates that their data set indeed includes examples with animal referents (*Na de wey got de slip* 'That's where *goats* sleep').

**28.** To illustrate: The GC basilect '03 data on zero plural marking distribute as follows: Nouns with a "human" [+animate, +human] referent: .33 (n = 105); Nouns with an "animal" [+animate, –human] referent: .84 (n = 21); Nouns with an "thing" [–animate, –human] referent: .52 (n = 461); When the latter two are combined into a new [–human] factor group (n = 482), its weight is .54, considerably closer to the "thing" factor weight (.52) than the "animal" factor weight (.84), because of the statistical predominance of "things" in the new group. [The data for GC and JC in Table 1 represent the "human" and "thing" factor group weightings in varbrul runs in which a three-way distinction was drawn between "humans," "things," and "animals."]

cite Welmers as showing that "bare inanimate nouns have generic reference, while bare human nouns receive a singular interpretation, unless plurality is otherwise specified" (1973:220). But the primary distinction Welmers (ibid.) actually draws is between inanimates and animates (not humans), his first example of an animate noun requiring specification with the plural marker being the word for "goat": *ú.mù. éwù* 'goats' (lit. 'PL. goat').[29] And if the significance of animacy in number marking is related to West African substratal influence, is this not more plausible in the pidgin-creole varieties in contact with African languages (NPE and NSLE) than those no longer in such contact (e.g., the Caribbean varieties – cf. McWhorter 2000:398–399)? Might its relevance in the Caribbean and NSLE have something to do with the greater significance of animacy for pluralization with *dem* than pluralization with *-s*, a point noted by Singler (1991:553) and Patrick et al. (1993)? Or might the interaction of animacy with plural marking in the creole varieties as a group have something to do with universals rather than West African influence, given Comrie's (1981:182) discussion of this as a "universal" phenomenon, and the tendency of creoles to favor universal constraints? These are all sub-issues for future research, but ones that will require attending to the distinction between inanimate, human, and animal nouns.

Finally, even granting this one case – generously, two – where the constraints on zero marking in "Early AAE" differ from those in pidgins and creoles, this does not argue decisively against creole ancestry or for English ancestry. To begin with, recall that the rate of zero plural marking in "Early AAE" is, on the authors' own admission, high (23–34%), higher than the rates reported for contemporary AAVE (2 to 13%), and higher than those reported for other non-standard English dialects (1% in white Nova Scotia English, according to Poplack and Tagliamonte [1994:248]).[30] Secondly, and more significantly given PTE's insistence on the importance of constraint hierarchies,

**29.** An additional distinction between personal and non-personal nouns is relevant in Igbo.

**30.** Miller (1999:209) reports rates of zero plural marking for Augusta, Georgia, that are higher than those normally reported for (non-creole) varieties in the US or England, although still lower than those for "Early AAE" in Table 1: 17.5% for Whites, and 22.7% for Blacks. But his dataset includes irregular or mutation forms like *men* and *mice*, which are normally excluded in the variationist analyses of the plural considered in PTE and this review. I am grateful to Crawford Feagin and Michael Montgomery for drawing my attention to Miller's detailed and highly informative study.

*not a single one* of the constraints on zero plural reported as characteristic of historical or present day varieties of English – the definitive/indefinite effect, the individuation/saliency effect, the collectivization or hunting animal effect ("we bagged three elephant"), and the lexical effect of nouns of weight, measure and money ('twenty mile, five dollar") – was found to be operative in the Ex-slave Recordings, Samaná English, or African Nova Scotian English. Outside of the semantactic and phonological factors in Table 4, the only constraint that seemed to have some effect, the functional disambiguation tendency to avoid plural marking on the noun when plurality is marked elsewhere in the Noun Phrase (e.g., by a plural numeral or quantifier as in *ten car*, *plenty cow*), could have come from English *or* from pidgins and creoles. (And its much greater proclivity in pidgin-creole varieties than in other varieties of English would argue for pidgin-creole influence.) Consequently, PTE's concluding statements (pp. 98–101) that plural marking shows no pidgin-creole connection and that it can therefore be completely attributed to English are not supported.

*Negation.* Chapter 4, by Darrin M. Howe and James A. Walker (hereafter "H&W") is the first of two chapters in Part II that deal with morphosyntactic variables – in this case negation. Considering in turn several uses of *ain't*, negative concord, negative inversion and postposing, the authors conclude that Early AAE is more similar to White vernacular and other English dialects than it is to English-based creoles [EBCs], and that "at least as far as negation is concerned, early African Americans simply learned and spoke the colonial English they were exposed to, apparently without approximation or creolization" (p. 136). In some cases, as with negative concord to verbs outside the clause ("Well isn't nobody *wouldn't* go out"), negative postposing ("We had *no home*"), and negative inversion ("*Can't* no one get there"), the differences between Early AAE and what has been reported for EBCs is categorical or *qualitative*. Creoles reportedly lack all of these features, although it must be admitted that there are few descriptions of negation in English-based Creoles, especially ones that are quantitative, data-based, and accountable to variability. Sometimes, the differences among the relevant varieties are *quantitative*, as when the nearly categorical rate of negative concord to indefinites within the same clause in contemporary AAVE (98%), compared with its somewhat lower rates in Early AAE (66%–89%) and white nonstandard English (75% to 81%) is taken as indication of a relatively recent (sometimes labelled "spectacular") development in AAVE. This is an argument that I found persuasive (see Rickford & Rickford

2000: 157) when I first read it in Howe (1997), but about which I am now much more sceptical (see below).

In keeping with the principle emphasized in the introduction to this book (e.g., pp. 14, 18), H&W establish a metric of significance in which similarity of conditioning ranks highest in comparisons between varieties. As they say, "mere presence, or even ... overall rate of occurrence, reveals little about the underlying grammar" (pp. 110–111). But in fact their argumentation in relation to some features rests entirely on "presence/attestation" or "rate/percentage" arguments. One larger problem with this chapter (not in itself the author's fault) is that we have so little quantitative data on negation in the creoles – so little to compare the Early AAE and contemporary AAVE data with. Another is that while descriptions and data from earlier studies that appear to support the authors' arguments are cited, those that do not support the author's arguments are not cited. A third is that we have to assume that the Early AAE data H&W present are a valid and reliable representation of African American usage 100 or more years ago, without the variability that interlocutor shifting, change over the lifespan, and other factors may have introduced.

The authors' several discussions of *ain't*, the negation feature explored at greatest length, illustrate some of these problems. In their Table 4.2, H&W compare the relative frequency of *ain't* in present perfect contexts (i.e., over all the contexts in which *ain't* or *haven't/hasn't* were used), and report that it is highest in "Early AAE" (SamE 80%, n = 15; ESR 90%, n = 10; ANSE 100%, n = 4), somewhat lower in Weldon's (1994) contemporary AAVE data (63%, n = 32), and lowest of all in Feagin's (1979: 226) Southern White Nonstandard English data (31%, n = 127). One would think that this evidence [that the feature is relatively weak in white vernacular English, strong in AAVE, and even stronger in a putatively "earlier" form of AAE] would support the creole origins hypothesis, or at least cast doubt on an English origins hypothesis. But the "small number of tokens" (4 to 15) in the Early AAE data is said to "prevent conclusive interpretation" (p. 114). And, dismissing the percentage data altogether (despite the stronger token count in AAVE and SWNE), the authors conclude with a "presence" argument: "Since creoles, non-standard English, Early AAE and AAVE all use *ain't* for *have + not*, these findings are silent with respect to the creole origins hypothesis" (p. 114). In subsequent tables and subsections, however, H&W have no problem basing their conclusions on cells with small tokens (cells with 0, 1, 2, 3, 4, 5, 9, 11, and 12 tokens in Table 4.5, for instance).

Another problem with the discussion of this feature is that no creole data are introduced. As far as I know, no fully comparable published creole data exist – a problem for many of the crucial comparisons in this chapter. The closest I found is Bickerton's (1975: 93) data on the frequency with which various negatives are used by twelve Guyanese mesolectal creole speakers. Ignoring the ten tokens of incommensurate forms (*na*, *hadn, neva*, *didn* and *doon*) used for "had/have not" in Bickerton's Table 3.8 leaves fifteen tokens of *en* and four of *havn*. The resultant *en* percentage of 79% (15/19) is strikingly similar to the 80% H & W report for SamE.

In their discussion of the use of *ain't* for *be* + *not*, H&W do consider conditioning, specifically, distribution across past and present contexts for *be* as a copula and auxiliary (Table 4.5).[31] Their data clearly indicate that *ain't* for *be* + *not* is *not* a past tense marker in Early AAE, occurring 0% in auxiliary contexts, and only 1% to 14% in copula contexts. This is said to be consistent with the generalization that "the use of *ain't* in the past-copula environment is not a feature of any dialect of English" (p. 116), in contrast with creole *ain't* which is characterized as tense neutral, equally capable of occurring in the present or the past (p. 116). For the latter claim, Winford (1983) and Bickerton (1975) are cited. But a check of these sources reveals a very different picture. Winford states that *eh* ("ain't") is used for past in Trinidad Creole [TC] only before non-state bare stem verbs, where it's equivalent to "didn't" (*The girl eh lie* 'The girl didn't lie') and he says explicitly that in past tense environments before V*ing*, NP, Adj, Adv, or Prep Phrase, "the form used to express negation in TC is invariant /woz/ (1983: 203)." This is confirmed by Pyne-Timothy (1977: 111–113), whose TC copula examples with *en* (*He en going home* 'He is not going home'; *She en pretty* 'She is not pretty') are all translated as "is not," and who translates English "was not" with TC *wasn* Asin Winford (1983), Pyne-Timothy's (p. 111) examples of *en* before verb stéms are given past or present perfect interpretation, however (*He en write the letter* 'He has not written the letter'; *He en go home* 'He did not go home'). Bickerton's (1975) description of Guyanese Creole [GC] *en* is a bit more complicated. On the one hand, Bickerton does say that "As a Have/be' negator, *en* is employed indifferently with past and non-past reference" (1975: 99) But on the other, he explicitly includes "have" as an English equivalent, not just *be*, and some of his examples show *en* being used for "didn't" (as in TC), and "don't." In fact, Bickerton has only one clear example of *en* = 'was not' (3.85 *Anyway three or four months pass and me*

31. They also consider distribution across stative and non-stative aspects, but by their own admission (pp. 118–119), this distribution is meaningless, since "copulas are by definition ... always stative," and auxiliaries are for other reasons also "aspect neutral."

*and R.* EN *talking*), making his GC data comparable to what H & W report for Early AAE. In sum, when we consider conditioning by tense, especially in copula environments, the creole data on *en* = *be* + *not* show it to be *more* similar to Early AAVE than any other reported English dialect, directly contradicting H&W's conclusion (p. 119) that the evidence supports an English rather than creole origin.

In discussing the use of *ain't* for "didn't," the authors note that it occurs about 40% of the time in Weldon's (1993) contemporary AAVE data, and much less (2%–6%) in Early AAE. On this basis they conclude (p. 120) that "the relative prominence of *ain't* for *didn't* in modern AAVE is a recent and spectacular development." Labov et al.'s (1968: 256) AAVE data, not cited by H&W, show this feature to have been comparably high (32%–50%) among adolescent peer group members in New York City almost 40 years ago, which would presumably count as "recent" too, supporting H&W's argument that relatively high rates of ain't for "didn't" are a modern phenomenon. But other "Early AAE" and creole evidence is more ambiguous. Schneider (1989: 201) does not tell us how often *didn't* occurred, but he does report 22 instances of *ain't* for "didn't" (from 18 informants) in his 19th century "Earlier Black English" slave narrative corpus, slightly more than the twenty tokens reported by H&W (ibid.) for ANSE (5), SamE (11) and the ESR (4) combined. H&W also don't consider the possibility that adult speakers in their "Early AAE" corpora may have reduced their usage of this stigmatized feature as they grew older and more sociolinguistically aware (cf. Bailey 2002: 327–328). And they seem unjustifiably sceptical of a creole origin for the feature,[32] given its occurrence in creoles at rates comparable to those in contemporary AAVE. There are no directly comparable studies, but if we again remove incommensurate forms (*neva*, *na*, and *doon*) for "did not" in Bickerton's (1975: 93) Table 3.8 of Guyanese mesolectal speakers, we are left with 14 tokens of *en* and 28 of *didn't*, yielding an *en* percentage of 33%, comparable to that of contemporary AAVE. H&W's inference (pp. 120–121) that AAVE *ain't* for *didn't* could not have come from creole influences and that it was probably inherited by blacks from whites (rather than vice versa!) does not seem justified, the more so because only three examples

32. H&W critique my (1977: 203) phonological derivation of *ain't* from *didn't* via a general rule by which initial voiced stops are deleted in creole preverbal tense-aspect markers. They argue (p. 120) that this cannot be considered "simplification," but ignore the fact that I recognized this as well, pointing out (ibid.) that it is a fairly complex rule.

occur in Feagin's (1979: 215) Alabama English data, and one of them is, by her own admission (ibid.), questionable.

Finally, returning to the discussion of clause-internal negative concord with indefinites, H&W show (Table 4.8) that the contemporary AAVE rate in Labov et al.'s (1968: 277) NYC data is higher (98%) than in Early AAE (66%–89%) or white non-standard vernaculars (75% to 81%), and on this basis view it as "another development of contemporary AAVE, rather than a prior creole legacy." But had they chosen to use Wolfram's (1969: 156) study of AAVE in Detroit, which they cite but do not use as data base, they would have had to report lower contemporary AAVE rates (77.8% for the Lower working class, and 54.7% for the Upper working class). And in Schneider's (1989: 197) "Earlier Black English," rates of 93.9% are reported for this variable too. In sum: the percentage differences displayed in H&W may well be statistically significant, but they disappear when we consider other attestations of contemporary AAVE and Early AAE.

Given the kinds of weaknesses in data marshalling and argumentation noted above, the single biggest value of this chapter, I think, will not be the specific conclusions it reaches, but its demonstration of the potential value of negation as a new site for the study of the anglicist/creole hypothesis, and the need for quantitative studies of variability in Caribbean and other creole negation systems, comparable to the work H&W (and more recently Walker 2005) have already pioneered on this variable in Early AAE.

*Was leveling.* Chapter 5, by Sali Tagliamonte and Jennifer Smith (hereafter "T&S") examines the use of *was* where Standard English normally requires *were*, as in *you was* (only 2nd person *singular* contexts were considered), *we was*, and *the books was*. Like other authors in the volume, T&S conclude that leveled *was* in "Early AAE" derives from a British English rather than creole source. Their data sets are, however, relatively unique, as two varieties of "Early AAE" – from the North Preston (NPR) and Guysborough (GYV) black enclaves in Nova Scotia – are compared with white Nova Scotian vernacular English (NSVE) in Guysborough Village, and with (white) British English from Buckie (BCK), Scotland. Although the three Nova Scotian communities are all descendants of US migrants who fled to Canada after the American Revolutionary War (1775–83) or the War of 1812, the whites reportedly came mainly from the American north, while the blacks came primarily from the American south. The significance of this is that the American south had been peopled primarily by British "northerners" (from northern England, Ireland and Scot-

**Table 5.** Constraints on *was* leveling in the four communities under consideration

| | Northern British (Scotland) | "Early" African America English in Nova Scotia enclaves (ANSE) | | *White* vernacular English in Nova Scotia |
|---|---|---|---|---|
| | Buckie | Guysborough | N. Preston | WNSVE |
| 2nd person singular | ✓ | ✓ | ✓ | X |
| NP > Pronoun | ✓ | ✓ | ✓ | ✓ |
| Negation | ✓ | ✓ | ✓ | X |

Source: Tagliamonte and Smith's Table 5.4 in Poplack (2000:162)

land), while the American north had been peopled primarily by British "southerners" (from East Anglia, South and West England).[33] Buckie is of interest because it represents a British "northern" area. If the present day "Early" AAE communities share linguistic conditioning of *was* with Buckie, rather than with whites in Guysborough Village, this might be taken to suggest that the blacks who fled to the Nova Scotia enclaves two centuries ago had been linguistically influenced by the British "northerners" with whom they had been in contact in the American south.[34]

This is precisely what T&S conclude after examining variable constraints on *was* leveling, shown schematically (without the quantitative data) in Table 5.

The only constraint the four varieties share is a preference for *was* leveling with a full NP rather than a pronoun subject. This constraint has been attested throughout Britain for several hundred years. It would therefore have been present in the speech of both British northerners and Southerners who settled in the US and Canada, and in the speech of any Africans and African

33. The British northerners who went to the southern US did so primarily in the 18th century, while the British southerners who went to the northern US did so primarily in the 17th century. The situation is probably more complicated than T and S suggest, however, since the 18th-century migration from Britain was far more voluminous than the 17th-century migration (nearly 250,000 vs. ca. 75,000, Table 5.1, p. 149). Some of the 18th-century British southerners immigrated to northern areas (like Pennsylvania) directly, and others undoubtedly made their way north before their descendants migrated to Canada in the late 18th and early 19th centuries, so the US north/south and British south/north correlations may not be quite that precise. We crucially need more information about the demographic sources of the White loyalists who migrated to Guysborough Village. Poplack and Tagliamonte (2001:Chapter 3) are very informative about the sources of the Black loyalists who went to Nova Scotia, but much less so about the White loyalists.

34. But see Montgomery (n.d.) for a critique of the relation that T & S attempt to make between Buckie and Nova Scotia.

Americans they influenced. By contrast, the favoring effect of second person singular *you* as subject, rather than pronouns from other grammatical persons (e.g., *we*), is a feature long associated with northern British varieties, and sure enough, Table 5 shows that Buckie and the "Early" AAE Nova Scotia enclaves with putative British northerner influence share this constraint, while White NSVE, with British southerner roots, does not. The white NSVE also distinguishes itself from the other varieties in *not* favoring *was* in negatives; although this constraint "is not mentioned in the historical dialect literature" (p. 162), the authors suggest that it might have been a conditioning factor in earlier varieties of British English.

The strategy of using white varieties like Buckie and WNSE as foils for comparison with the two "Early" AAE Nova Scotia varieties is a clever one, and, from the evidence of Table 5, a successful one for supporting the author's conclusions. But Table 5 plays fast and loose with the quantitative evidence on which it is based (Table 5.3, p. 160). In the table header for T&S's Table 5.3, the authors indicate that "factor groups selected as significant" appear in bold. "Grammatical person" is not a bolded factor group for either of the Early "AAE" varieties in that table, so even though their factor weights are higher for 2nd person singular than for other grammatical persons, the difference is not statistically significant. In fact, if the authors had followed the convention of NOT including non-significant factor weights, the cells for this factor group in Table 5.3 would have appeared with empty square brackets (as in Table 4 above, based on PTE's Table 3.6). In the case of "Subject Type," the feature weights for North Preston are also non-significant (not bolded) in T&S's Table 5.3.[35] Strictly following the evidence of their own quantitative data, Table 5 should be revised to look like Table 6, which is considerably less supportive of the author's conclusions. The "Early AAE" varieties no longer pattern with Buckie in showing the Northern British 2nd person-singular effect, and N. Preston now appears as different from Buckie as White Nova Scotia English, failing to show a significant effect for *two* of the three constraints that Buckie speakers favor.

There are two other major weaknesses in the authors' argumentation. The first is in the discussion of their Table 5.3 evidence that females favor *was* leveling more than males do in three of the four varieties (N. Preston is the excep-

35. Cells in which "knockouts" (100% or 0% effects) forced the authors to use percentages rather than variable rule weights are also unbolded (because they did not go through the variable rule regression procedure), but those are clearly significant, and will be treated as such in this discussion. Examples are the cells for type of subject in BCK and NSVE.

**Table 6.** Significant Constraints on *was* leveling in the four communities (revised)

| | **Northern British (Scotland)** | **"Early" African America English in Nova Scotia enclaves (ANSE)** | | **White vernac English in Nova Scotia** |
|---|---|---|---|---|
| | Buckie | Guysborough | N. Preston | WNSVE |
| 2nd person singular | ✓ | X | X | X |
| NP > Pronoun | ✓ | ✓ | X | ✓ |
| Negation | ✓ | ✓ | ✓ | X |

Source: Tagliamonte and Smith's Table 5.4, revised to reflect the data in their Table 5.3

tion, with males favoring *was* with a probability of .54, while the corresponding figure for females is .45). Although T&S note that the effect is statistically significant only in Buckie, they continue to attach some importance to the slight statistical preference of females for *was* in Guysborough enclave (.07) and Nova Scotia Vernacular English (.04), stating (ibid.) that "[t]his direction of effect reveals that the use of *was* is not stigmatized, and suggests that it is an inherent part of each of the varieties" (p. 161). Beyond the fact that the observation is not statistically unjustified, I do not know of any well-established sociolinguistic principle to support it. It certainly is not the case that anytime women appear to favor a variant we can infer that the variable is *not* socially stigmatized in their speech community (contrast Escure's 1991, 2001 findings in Belize). And how social evaluation gender preferences might relate to evidence that a feature is an "inherent" part of a variety is unclear.

Another weakness of this chapter is the author's argumentation *against* the possibility that *was* leveling in the "Early" AAE varieties might have a creole source. The only creole study they cite is Bickerton (1975), from which they extract the generalization that in decreolization, "*was* appears first as an irregular lexical insertion, while *were* is acquired later in direct proportion to increasing acquisition of Standard English features" (T&S, p. 144). This statement appears to be the basis of their assumption that "the use of *were* in a decreolizing variety would be conditioned primarily by extra-linguistic constraints, particularly sensitivity to the standard language" (ibid.) (here is where the gender issue apparently comes in), and that the feature would have no internal constraints. But if you return to Bickerton's study, you'll see that he never looked at constraints on *was* leveling. His Table 4.1 (p. 115) shows that in the outputs of 30 Guyanese mesolectal speakers, there were 284 tokens of *was*, and 26 tokens of *were*. But he gives no information about which of those tokens were standard (e.g., *was* with third singular subjects) or nonstandard, and whether they were conditioned by grammatical person, NP versus pronouns, negation, or any of the constraints

T&S considered. To my knowledge, no one has carried out quantitative *was*-leveling studies in any anglophone creole community comparable to the one done by T&S and the many carried out elsewhere in North America (see Wolfram & Thomas 2002: 75). This fact highlights the need for such studies to be done. But until they are, there is no basis for T&S's conclusion that *was* leveling in "Early" AAE "can hardly be seen as the result of decreolization" (p. 161).

*Auxiliary non-inversion in direct questions.* Chapter 6, by Gerard Van Herk (hereafter "VH"), deals with non-inversion of the auxiliary in direct questions in "Early" AAE, as in "He *don't* know the pastor?" (#21, p. 184) and "What Ella *must* have done with it?" (#27, ibid.).[36] DeBose (1996), finding this characteristically creole feature in Samaná English [SamE], concludes (p. 7) that SamE is more divergent from Standard English than modern AAVE is. VH draws on data from SamE too, as well as ANSE.[37] But he introduces quantitative data to show that non-inversion in these "Early" AAE varieties is favored by many of the same factors that favored non-inversion in Early Modern English, and concludes that the parallels are "beyond coincidence" (p. 192).

Before getting to the parallels with Early Modern English, VH discusses some methodological issues. He argues that we should only consider questions that have an overt verbal auxiliary or copula, and are invertible, as in the following SamE examples from page 179:

(3) To who *was* they going? (#16)

(4) Where your riches *is*? (#17)

In particular, he excludes questions like (5) and (6), in which "it is impossible to determine whether the original auxiliary was inverted or not prior to deletion" (p. 178):

36. I thank Gerard Van Herk for helpful email exchanges after I sent him a draft of my review of his chapter, but must add that he does not agree with or endorse all my views. Indeed, we continue to disagree (cordially) on several matters, especially those in the paragraph following example (10), while agreeing on the need for a fuller future study of question formation in creoles and "Early" Black English that would take into account "non-inverted" sentences and semantic/pragmatic factors not yet considered.

37. VH considers Ex-Slave Recording (ESR) data at the beginning of his chapter, but since the total number of countable tokens is small (8), and they are always inverted, they have nothing to contribute to the study of variable constraints, and are quickly ignored.

(5) And where you-all come from? (SamE, #11, ibid.)

(6) What we going to do? (SamE, #13, ibid.)

Zero auxiliary cases like these account for 438 of the 1,038 questions remaining in VH's corpus once 2,305 "Don't Count" tokens like repetition requests ("Eh?"), tag questions, fragments ("The hotel"?) and so on are removed. So the study is based on just 600 tokens (592 once the 8 invariant ESR tokens are excluded), from an original data pool of 3,343.

I understand the preference for excluding cases like (5) and (6) based on indeterminacy, but given the high rates of zero-marked verbs and copula/auxiliary absence in creoles and vernacular varieties, I believe that this strategy will also artificially reduce the percentage of non-inverted questions, and also remove them from consideration when relevant constraints are being considered. Even in colloquial Standard English, as in these examples from the Stanford Switchboard corpus,[38] tense marking on the verb shows that *do*-support (and "deletion") never took place, and that we are dealing with sentences which, without question intonation, are structurally identical to non-inverted declaratives:

(7) Oh, you *found* that out tonight?

(8) He *lives* in Cleveland?

To exclude cases like these from the count of non-inverted questions seems wrong, and this must be even more the case with English creoles and vernaculars in which *do*-support is weak or non-existent.[39]

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**38.** The Stanford Switchboard Corpus is a selection from the larger corpus of 2,400 telephone conversations in English between adult strangers that was recorded in the US in the early 1990s, and made available through the Linguistics Data Consortium at the University of Pennsylvania. Stanford undergraduate Linguistics major Tommy Grano and I used a parsed subset of these for our analysis. We are grateful to Tom Wasow for assistance.

**39.** *Do*-support does not occur as a productive rule until the upper mesolect, for instance in the Guyanese creole continuum (Bickerton 1975: 91). Unstressed *does* (sometimes represented in the literature as *doz*) and *did* occur there in affirmative, indicative forms as iterative/habitual and anterior/past markers, respectively, and they cannot be invered to form questions (He *does* go deh regular. **Does* he go deh regular?). Questions are formed by rising intonation: "He *does* go deh regular?" My claim is not that creoles would give us more sentences like (7) and (8) in which tense marking indicates that *do*-support did not occur, but that since the prospects of *do*-support occurring are lower in creoles and other vernaculars, we must be prepared to rethink the status of sentences like (5) and (6), VH also notes (pp. 179–180) that while inversion is always required in Wh-questions in Standard English, non-inversion in Yes/No questions does occur, as in:

(9) It's near Billings Bridge? (Ottawa English, #18, ibid.)

But he does not give us an estimate of how often such non-inversion takes place in Standard English, and the Crowley and Rigsby (1987) article he cites in this connection does not do so either. On the basis of nearly 2000 Yes/No "count" questions in the Switchboard corpus, Tommy Grano and I found that the percentage of clearly non-inverted questions in modern US English is only 12% to 14%.[40] This is comparable to the 19% non-inverted rate in Yes/No questions in ANSE, but much lower than the corresponding 69% rate in Samaná English. Unless the rate has changed dramatically over the past two centuries, colloquial metropolitan English is an unlikely source for the Samaná patterns.

A similar, even stronger argument applies to the Samaná data on non-inversion in Wh-questions. VH observes (p. 180) that the 39% SamE non-inversion rate is "far less" than the categorical non-inversion that an (idealized) creole diagnostic would lead us to expect. But it is also far *more* than the standard English prohibition on non-inversion in Wh-questions would lead us to expect.[41] McWhorter is quite pointed about the significance of these examples:

> The crucial cases are those identified by, for example, DeBose (1996) in Samaná English, "Why I didn't see you?" ... and copula final sentences like "From where you is" and "Where you was?" Judging from Van Herk's pre-

which represent in these varieties the basic way of marking questions (usually with rising intonation).

**40.** Twelve percent (223/1892) if we followed Van Herk's guidelines, and excluded the 46 tokens like (7) and (8) that did not include an overt auxiliary; fourteen percent (269/1938) if we included them. Note that this count excluded cases of absent or deleted auxiliaries, where we cannot be sure whether they would have occurred inverted or non-inverted if they had been present. And note that the 1200 *Wh*-questions we examined in this corpus follow VH's predictions perfectly, insofar as they are all inverted; but the 19 Wh-questions without overt auxiliaries (e.g. "Where you at") were excluded from consideration. I am grateful to Tommy Grano for his meticulous analysis and quantification of these data.

**41.** See the preceding and the following footnotes. And note that we are referring to non-inversions with the auxiliary present, with clause initial *wh*. For instance, Bolinger's (1957) non-inverted or "plain assertive" Wh-questions include examples in which the Wh-word is clause final: "He did it *when*?" and "They got *who* to help them?"

> sentation, these sentence types are found in early AAE ... but the paper does not cite evidence of such sentences in any white varieties spoken by whites in contact with American blacks in the past or present, which we can take as indicating that there is no such evidence. (2000: 401)

And given the evidence of studies such as Williamson (1972), I would have to agree.[42]

The heart of VH's chapter is pp. 181–192, where he presents quantitative data on the conditioning of non-inversion in "Early" AAE (SamE and ANSE), using constraints identified by Ellegård (1953), Stein (1988), Kroch (1989) as significant in the rise of *do* insertion in Early Modern English. VH summarizes his findings as follows:

> In both Early ModE and Early AAE, non-inversion is most likely with negative questions. In affirmative questions, it is more likely in yes/no than in *Wh*-questions. In *Wh*-questions, it is more likely with causatives. In the remaining *Wh*-questions, an easily processed Subject-lexical Verb-Object order is maintained through non-inversion with transitives, and in SE, with modals and copulas. These parallels to the complex system of Early ModE question formation are striking, and are beyond coincidence. (p. 192)

These findings are indeed striking, and I agree with VH's call for studies to see whether similar factors constrain non-inversion in English Based Creoles [EBCs], "given the undisputed contribution of English to both AAVE and EBCs" (ibid.) – something that other contributors to this volume seem to forget. The fact that quantitative studies of question inversion have not yet been done in EBCs, and other vernacular varieties of English (a point VH notes, p. 193) does limit the comparative diagnosticity of these findings.

But we should also be clear about the kind of Early Modern English "non-inversion" under consideration. What VH is really comparing are the factors that promoted the rise of periphrastic *do* in Modern English questions like (10), which, he argues is "a form of non-inversion," since it preserves the affirmative SVO word order in the main clause, in contrast with the Old and

42. The only Wh-question "non-inversions" we found in Williamson (1972), whose point is that question non-inversion is widespread in White colloquial and literary English, were examples with deleted auxiliaries, as in "What you looking at?" and "What you say?" That is, they did not include overt auxiliaries. By contrast, as David Sutcliffe (personal communication) has noted, "all 31 creoles in Hancock (1987) have non-inversion after WH, while apparently no non-creolized mainstream varieties of English have this, anywhere."

Middle English system of question formation involving lexical verb inversion (11) where an overt auxiliary was not already present:[43]

(10) Where *doth* [Aux] the grene knyght [S] holde [V] hym [O]?

(11) How great and grevuous tribulations [O] s*uffered* [V] the Holy Appostyls [S]?

But of course, *do* insertion questions like (10), are, from the modern/current perspective, and from the perspective of the entire sentence, inverted forms. Similar questions in "Early" AAE, are counted as inversions by VH (cf. #28, "What did you say?" p. 184), as they would be by any modern researcher. So what we are actually comparing are the factors that promote "inversion" (via *do* insertion) in Early Modern English with the factors that promote "non-inversion" in EAAE. There is something a little disingenuous about this, even granting VH's point that "both EModE *do* and EAAE non-inversion preserve Subject-Lexical Verb-Object order" (personal communication).

We should also remember that, historically, these constraints on question formation do not apply to questions in which *do* support or insertion do not occur, for example, sentences that already have a copula or auxiliary. In Modern English, these retain the auxiliary inversion they already had in Middle English:

(12) ... *have* we not cast oute devyls? (Kroch 1989: 216; from Ellegard 1953)

Since the "Early" AAE corpus includes many questions with NON *do*-support auxiliaries, I am a little unclear about why we should expect to find the constraints that applied to *do*-insertion in Early Modern English applying to EAAE questions with other auxiliaries.

Finally, given the fact that "[b]y 1700, this new form [question formation with periphrastic *do*] had largely, though not entirely ... replaced the original usage" (Kroch 1989: 216), I am wondering why we would expect it to have influenced the patterns of question formation among African Americans who emigrated to Samana and Nova Scotia in the late 18th and early 19th centuries. These points leave me somewhat skeptical about the English influence on question formation in "Early" African American English, even as I am struck by the quantitative evidence of parallels between EAAE and EME, and impressed by VH's openness to potential parallels in EBCs and to potential in-

43. Both examples are from Kroch (1989: 216), and ultimately Ellegård (1953).

fluence from "universal tendencies" in language acquisition (nicely pursued on pp. 194–195).

*Relativization strategies.* Chapter 7, by Gunnel Tottie and Dawn Harvie (hereafter "T & H"), examines relativization strategies in "Early" AAE, including variation between *that*, *what*, *who*, and *which* as relative markers, and especially zero, as in "... that's all Ø we got" (ANSE object relative, #31a) and "They has a fella here Ø has a property" (SamE, subject relative, #31b).

T&H essentially seek to show that the relativization strategies in ANSE, SamE and the ESR are no different from those attested either now or in the past in British and white American English varieties, whether they involve the use of non-standard *what*, relatively high rates (11–44%) of subject relativizer deletion, or specific constraints on zero relatives. The latter include the grammatical category, adjacency and humanness of the antecedent NP, and the category membership of the subject of the relative clause. Since these factors have been found relevant in quantitative studies of zero relatives in other English dialects, the authors conclude that "these varieties of Early AAE are descended from the same genetic stock, and that this stock is English" (p. 225). Although they have no quantitative data on relativization in creoles ("to our knowledge there are no quantitative accounts of relative markers in Caribbean creoles", p. 22), T&H also assert (ibid.) that the possibility that the Early AAE relativizer system parallels or derives from creoles is slim.

There are several positive aspects of this chapter. The lead author, Gunnel Tottie, has been working on zero relatives in British and American English for over a decade, and her publications are regularly cited in the (burgeoning) literature on this topic (e.g., in Guy & Bayley 1995; Lehmann 2001). Relativization, as the authors point out (p. 198), has figured minimally in the AAVE origins controversy. There is only one quantitative, variationist study of relative markers in contemporary AAVE (p. 200), and even that is based on a data pool of only 56 restrictive relative clauses. (Non-restrictive relative clauses do not allow zero, and are excluded in all variation studies.) It is good to have the data on relativization in ANSE, SamE and ESR provided in this chapter to add to that in Schneider (1989) and Tottie and Rey (1997) on the ex-slave narratives and recordings.

The information T&H provide about the relatively high frequency of *what* relativizers and subject zeroes in earlier and some present-day varieties of English is also interesting, eye-opening even, given the tendency to think of these as primarily if not exclusively characteristic of AAVE or creoles. Zero subject relatives, they note (pp. 202–203), were the predominant type in Middle English, and even though they declined in Early Modern English, and are now usually rarer than zero object relatives, they occur 14% of the time in contemporary Dorset (England), 24% of the time among lower class speakers in Ayr (Scotland), and 35% of the time in Appalachia (USA). Occurrences of *what* as a relative marker date back to Middle and Early Modern English, and it is also reported (pp. 203–205, without statistics) to be the preferred or predominant relativizer in Eastern parts of England, including Norfolk, Suffolk, and East Anglia. David Sutcliffe (personal communication) also report that WHAT relatives occur in London vernacular speech.

Against these plusses must be arrayed several significant minuses. The first is that the data reveal so much variability between ANSE, SamE, and ESR, that "Early" AAVE does "not seem to share a common system of relativization" – precisely the critique that T&H level (p. 201) at English based creoles, but with *no* quantitative evidence. For instance, as the authors themselves note:

> We see that the three relative markers *that*, *what* and zero predominate in each of the varieties of Early AAE, but that overall, a different one is favored by each: *that* in ANSE (43%), *what* in SE (53%), and zero in ESR (59%). (p. 211)

Moreover, in the discussion of zero relatives, which occupies the bulk of the chapter, at least one of the varieties, and sometimes two, show no statistical significance for the constraint under discussion, unlike the remaining two (or one), as shown in Table 7 below. To give yet another example, their Table 7.11 is supposed to show that the "Early" AAE varieties pattern alike, and with Appalachian English (AppE), in favoring zero subject relatives in existential constructions, with possessive *have/got*, and in *it/that* clefts. But the percentage of zero relatives that occur in existential constructions ranges from zero in SamE to 13% in ESR, 21% in ANSE and 40% in AppE, and for Possessive *have/got*, the percentages are equally variable: 8% in ESR, 13% in ANSE, 26% in AppE, and 63% in SamE. The most one can say is that the construction types "are all attested in Early AAE,"[44] which is not a very strong claim when it comes to determining historical relationships.

The second major weakness is statistical. This is the only data-based paper in the volume that does not use VARBRUL or any other multivariate analytical procedure to estimate the independent contributions to rule application of

44. The authors qualify this statement as follows: "although SamE has no instances of *there* constructions, and *it/that* clefts are absent from the ESR sample" (ibid.).

the various constraints considered. The authors explain (p. 211) that "making separate analyses of subject and non-subject relatives [required because they involve different constraints – JRR] results in some cells becoming too small to support a variable rule analysis." So the crucial data tables for the analysis of zero relative markers in non-subject function (7.5–7.8) and subject function (7.9–7.10) all consist of percentages of zeroes rather than probabilistic varbrul weights. But these tables are not accompanied by any chi-square or other measures of statistical significance, and when I calculated them myself,[45] I was surprised to discover that only five of the eighteen data distributions for ANSE, SamE and ESR in Tables 7.5 to 7.10 were statistically significant, as shown in Table 7.[46]

What this means is that the observations in the accompanying text regarding the effect of specific factor(s) must be regarded as vacuous, or at best suspect, awaiting confirmation from additional data. This is especially true for Tables 7.7, 7.8, and 7.10, where none of the statistical distributions is significant. The authors do admit that the humanness of the antecedent NP shown in Table 7.8 exhibits only weak or non-existent effects on zero non-subject relatives, but in the other cases they seem unaware of the limitations of their data. With respect to Table 7.7, for instance, they say that personal pronoun subjects in the relative clause "clearly favor zero [object] relative for SE and ESR," (p. 216) but as Table 7 shows, their Table 7.7 does not offer statistical support for this claim.[47] Similarly, in the discussion of adjacency effects on subject relatives, shown in their Table 7.10, T&H say that "in ANSE and ESR adjacency favors zero relatives. In S[am]E the effect seems to be reversed but notice that numbers are low (p. 219)" But as our Table 7 shows, the chi-square values for the data distributions in all three of the "Early" AAE varieties on this feature are non-significant.

**45.** I used Catherine Ball and Jeffrey Connor-Linton's Georgetown Linguistics chi-square calculator, available at: www.georgetown.edu/faculty/ballc/webtools/web_chi.html

**46.** In Tables 7.7 and 7.9, the bottom row(s) with missing data or only 1 to 2 tokens per cell, were omitted. Including them in the chi square calculations would of course have made it more difficult if not impossible to achieve statistical significance.

**47.** However, the $p \leq .10$ values for the corresponding SamE and ESR cells in Table 7 show them to be closer to the significance cut-off value ($p \leq .05$, or $\alpha^2 = 3.84$ in this case) than any of the other non-significant cells in this table.

**Table 7.** Chi square and significance assessments of the data in T&H's Tables 7.5–7.10

| Tables | ANSE | SamE | ESR |
|---|---|---|---|
| 7.5 | $\alpha^2$ = 0.56, p≤ 1 (Not Sig.) | **$\alpha^2$ = 10.7, p≤ .01 (Sig.)** | $\alpha^2$ = 3.17, p≤ 1 (Not Sig.) |
| 7.6 | **$\alpha^2$ = 4.3, p≤ .05 (Sig.)** | $\alpha^2$ = 1.97, p≤ .2 (Not Sig.) | **$\alpha^2$ = 16.5, p≤ .001 (Sig.)** |
| 7.7 | $\alpha^2$ = 0.01, p≤ 1 (Not Sig.) | $\alpha^2$ = 3.55, p≤ .10 (Not Sig.) | $\alpha^2$ = 3.71, p≤ .10 (Not Sig.) |
| 7.8 | $\alpha^2$ = 0.01, p≤ 1 (Not Sig.) | $\alpha^2$ = 0.44, p≤ 1 (Not Sig.) | $\alpha^2$ = 1.63, p≤ 1 (Not Sig.) |
| 7.9 | $\alpha^2$ = 0.14, p≤ 1 (Not Sig.) | **$\alpha^2$ = 16.8, p≤ .001 (Sig.)** | **$\alpha^2$ = 9.96, p≤ .01 (Sig.)** |
| 7.10 | $\alpha^2$ = 1.01 p≤ 1 (Not Sig.) | $\alpha^2$ = 0.61, p≤ 1 (Not Sig.) | $\alpha^2$ = 1.61, p≤ 1 (Not Sig.) |

Turning now to the tables which included some significant effects, we should note that although "definite NPs show a lower-than-average incidence of zero in S[am]E" (p. 214) in Table 7.5 (for object relatives), and although this accords with two other studies of English, it is, as T&H admit, contrary to the findings of at least one earlier study of English. It's also contrary to the findings of Wasow and Orr (2004), who examine 4387 relative clauses from the spoken US Switchboard corpus, and report a statistically very significant effect (**$\alpha^2$ = 366, p ≈ 0**) of the determiner type of the antecent NP, with the determiner type *most* favoring zero (at 60%) being *the*.[48] So between the opposing evidence of different studies, there is no single "English" pattern with respect to this constraint.

In the case of Table 7.6, which does show a significant positive effect of adjacent antecendent NPs for zero object relatives in ANSE and ESR, this finding is corroborated by all the studies of British and American English cited by T&H (dating from 1957 to 1995). It is also corroborated by Wasow and Orr (ibid.), who show additionally that the likelihood of a zero object relative declines steadily from about 50% when there are no words between the antecedent NP and the relative clause (compare 50% in ESR, 84% in ESR), to almost 0% when there are seven or more intervening words. But this may be a universal rather than a language or family-specific effect (cf. Hawkins 2001, 2004), of little use for untangling genetic or historical relations.

In the case of Table 7.9, dealing with the effect of the grammatical category of the antecedent NP on zero *subject* relatives, the statistical distributions of the SE and ESR data are highly significant, showing a strongly favoring effect (69% in ESR) for indefinite NP heads. But a relatively small sample of 27

48. The zero-favoring percentages for the other types are: 14% indefinite NPs ("a" or "an"), 38% no determiner, 50% other determiner). I am grateful to Tom Wasow, my faculty colleague at Stanford, for sharing these data with me and for related discussion.

Guyanese Creole subject relatives that I examined in connection with this review also showed a statistically significant (**$\alpha^2 = 10.81$, $p \leq .01$**) effect for this factor group,[49] with zero being most highly favored (77%) with indefinite NP heads. This suggests that a creole or universal effect for the patterning of the "Early" AAE data examined by T&H cannot be ruled out. The failure to consider any quantitative relativization data from creoles, or at least to be cautious about ruling out creole parallels and influences in the absence of relevant evidence, is the final weakness of this chapter that offsets its relative strengths.

*Sociohistory.* The eighth and final chapter in the volume, by Salikoko S. Mufwene (hereafter "M."), is entitled "Some sociohistorical inferences about the development of African American English." M. is one of only a handful of linguists involved in the AAE origins debate who has taken the time and trouble to research the sociohistorical context, and, as always, his contributions to this vital and understudied area are welcome.

M. lays out at the start (p. 234) the positions he will defend in the chapter:

(1) AAVE did not develop from a creole, either American or Caribbean.
(2) This doesn't preclude influence from Caribbean slaves imported in the 17th–18th c.
(3) But such influence was not necessary for AAVE to have the features it now does.
(4) Influence from African languages was more important, especially in the 18th century.
(5) 17th–18th-century colonial English was central in the development of AAVE, Gullah, and Caribbean Creoles.

49. The relative clauses were extracted from texts in Rickford (1987), using the line index of selected grammatical features at the end of the book (pp. 327–332), and following T&H's criteria for including and excluding examples. This process yielded twenty-seven subject relatives, with an overall zero relative percentage of 44%, and nine non-subject relatives, with an overall zero relative percentage of 56%. I also did a quantitative analysis of the constraints considered by T&H in this chapter. Although the relatively small data pool achieved statistical significance only for zero subjects in relation to the grammatical category of antecedent NP, data on the other constraints usually pointed in the same direction as T&H's constraints too. For instance, adjacency favored zero subject relatives 48% of the time (11/23), versus 25% (1/4) for non-adjacent subject relatives. There is clearly a need for a more substantive variationist study of relativization in English based creoles, but this fledgling quantitative study of Guyanese Creole accorded with the "Early" AAE and other English results, contrary to what one might have predicted from reading T&H's chapter.

Taking (1) and (5) together, M.'s argument is that a Gullah or Caribbean-like creole failed to develop in the American colonies outside of the South Carolina and Georgian coast because the necessary ecological conditions did not exist. Focusing on the Chesapeake (Virginia and Maryland), for instance, he argues that in the 17th century the number of Africans (who for the first few decades were indentured servants rather than slaves) was limited, their proportions to Whites low, and their contacts with them in small homesteads rather than large plantations likely to have provided ample opportunity for them to learn White vernacular English. Even in the 18th century, when the numbers of Blacks grew significantly, they never represented more than 38% of the Chesapeake population, and "[t]he relative integration of Blacks and poor Whites – both living primarily on small land holdings – favored the development of similar Black and White vernaculars ..." (p. 246).[50] M. feels that "the first socioeconomic ecology for linguistic divergence between the vernaculars of the two races" outside the Gullah area did not exist until the segregationist Jim Crow laws of the late 19th century "forced African and European Americans to live in separate neighborhoods and not to use the same public facilities" (p. 248).

There is much in these general conclusions – and their supporting details – with which others would agree, in fact, have already agreed. For instance, the conclusion that the sociohistorical conditions were not conducive to the development of a widespread, basilectal creole outside of Gullah territory is endorsed by Rickford (1997) and Winford (1997). But the picture of Blacks and Whites interacting freely and living and speaking in parallel ways until the end of the 19th century is too rosy, and under-represents the divisive effects of slavery, the formidable institution that dominated the lives of Blacks (and Whites) for the preceding two hundred years. Tate (1965) – one of Mufwene's sources – Weld (1969), Hast (1969) and Foner (1975), among others, provide evidence of the increasingly repressive slave codes that developed in Virginia and virtually every other North American territory, from the late 17th century to the mid-18th century, of the fact that most slaves were field hands rather than household workers and personal servants (Tate 1965: 34–35), of the frequency with which

**50.** A central assumption of M.'s – one which I support – is that "the time and extent of divergence of African American vernaculars from their white counterparts of the same regions are inversely proportionate to the degree of social integration of the speakers in the majority and/or politically-dominant population" (p. 236). In short: The *more* Blacks and Whites were socially integrated, the less likely their vernaculars would diverge. And the *less* they were socially integrated, the more likely their vernaculars would diverge.

Blacks were overworked and mistreated, of the extent to which many Blacks and Whites led divergent and non-interacting lives,[51] and of the ways in which their religion, music, folk-culture and world-view often differed.[52] If this were not enough to yield the kind of restructuring associated with pidginization and creolization, it was certainly enough to inscribe different Black/White identities before Jim Crow laws came into effect, and it might have been enough to produce Black/White vernacular differences long before such laws and the urban residential segregation of the 20th century brought new divergences in their wake.

M. argues, that "any divergent speech patterns would have been recorded in writings" (p. 238) of the 17th century. Maybe.[53] But both for North America and the Caribbean we have precious little historical or textual documentation for the 17th century, certainly much less than for the 19th century (Rickford & Handler 1994: 223, Tate 33–34). M. (p. 244) points to an 18th century commentator (Jones 1724/1956) who says that "slaves born in Virginia 'talk good English, and affect our language, habits, and customs," but he does not cite comments by other contemporary observers (e.g., J. F. D. Smyth, and the Reverend James Marye, Jr., cited in Rickford 1997) that suggest the opposite. Finally, speaking of African American diasporan varieties, M. concludes that "nothing has been found so far which suggests that AAVE was more creole-like at the beginning of the nineteenth century" (p. 247). But this ignores, as McWhorter (2000: 419) notes, the prevalence of copula absence in AAVE, which is paralleled in the Caribbean creoles, and as Sharma and Rickford (to appear) note (contra McWhorter), cannot be attributed to universals of sec-

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**51.** Note, for instance, the following 1842 remark by the Reverend C. C. Jones: "Persons live and die in the midst of Negroes and know comparatively little of their real character. The Negroes are a distinct class in community, and keep themselves very much to themsevles. They are one thing before the whites, and another before their own color. ... It is habit – a long established custom, which descends from generation to generation". (quoted in Jones 1990: 21)

**52.** As Kulikoff notes, referring to the the end of the 18th century: "White observers agreed that the music, dance and religiosity of black slaves [in the Chesapeake] differed remarkably from those of whites" (1986: 351)

**53.** Note, for instance, Armin Schwegler's work on Cuban Palo Monte ritual speech (essentially restructured KIKONGO), which was essentially unattested, and considered *non-existent* until 1998, when he discovered *fluent* speakers of it in contemporary Cuba, in the midst of Havana, Cienfuegos and other urban areas! See Fuentes & Schwegler 2005.

ond language acquisition. Moreover, ANSE and SamE, unlike contemporary AAVE but more like the Caribbean creoles, permit some copula absence with first person subjects (see Poplack & Tagliamonte 1991: 321), and favor copula absence with NP rather than Pronoun subjects (Rickford 1998: 184). The first person effect is especially interesting because Kautzsch (2003: 104), after citing and discussing examples from earlier sources of AAVE, concludes that "zero-copula after first person subject seems to have been a fairly stable variant up to the beginning of the twentieth century, at least in same states of the Old South (AL, NC, SC and VA)[54] M.'s statement also does not fit the contrary indications of the *other* 19th-century diaspora variety, Liberian Settler English [LSE]. M. acknowledges in Footnote 12 (p. 257) the greater heterogeneity and creole-like nature of the LSE evidence, but he suggests that "some of the features have their origins in Kru Pidgin English" – a suggestion that Singler emphatically rejects (personal communication, and see Singler 1997, 1998), since the Liberian Settlers and the Kru had little contact.

Turning now to (2) and (3) in the five-point position statement above, M. carefully and diplomatically does not preclude Caribbean English influence on the Black vernacular(s) that developed in Virginia and other North American colonies in the 17th and 18th centuries, but the bulk of his argumentation is against it. One of his arguments is that the Caribbean slaves were newcomers to a North American ecology in which African-imported and "US"-born slaves had already established themselves, so why would the latter "have shifted to the Caribbean vernaculars or taken them as their models?" (p. 240). However, the situation in the 17th and early 18th century was quite unlike the situation today, where WI immigrants to New York and Los Angeles are relatively small and relatively unintegrated segments of the Black community. As such, they hardly impact the local African American vernacular. As I've noted elsewhere (Rickford 1997), Caribbean slaves often represented substantial segments of the early North American slave population, so much so that in some cases we would have to consider them the linguistically influential "founding population" (cf. Mufwene 1996).[55] This is something that we will want to re-

54. I am grateful to David Sutcliffe for reminding me of the discussion of zero with 1st person forms in Kautzsch (2003) and for other comments on a near-final draft of this review article.

55. Compare the following quotes: "During the first half of the eighteenth century, thousands of slaves were transported to New York from Barbados and Jamaica. Relatively few were imported directly from Africa prior to 1750 ..." (McManus 1966: 24) "These first Black

search more carefully in the future, distinguishing between different locales and decades in North America, and taking advantage of the new slave trade database and publications of David Eltis and his colleagues (e.g., Eltis 1999; Eltis et al. 1999). But it is clear that slaves from the Caribbean *were* the first black populations in some regions of North America, so the question is how later arrivals of Africans and African Americans adjusted to them rather than vice versa.

Mufwene also argues that the vernaculars of these early Caribbean imports would not have been very creole-like (basilectal) either because the imported people had not been in Barbados and St. Kitts (two key source colonies) long enough to acquire the local vernacular, or because they came from "small farms" and homesteads in which Black/White contact was extensive, and the conditions for creolization poor. This is a valid consideration, although the sub-argument that the Barbados slaves "probably" came from small farms "which went out of business because they could not cope with the competition from large plantations" (p. 239) is not supported by any evidence, and the limited documentary evidence we have of Barbadian slave speech in North America – e.g., the speech of Candy and Tituba at the Salem Witch Trials in 1692 – is quite basilectal. M. admits (p. 239) that Candy and Tituba "spoke creole/pidgin-like idiolects", but observes that "we don't know for sure that these were not interlanguages, nor how representative they were of Barbadian slaves in general." The interlanguage rebuttal strikes me as terminological hair-splitting. The representative-ness argument is, however, more substantial. We may never have enough documentary evidence to settle the issue (but for some relevant sociohistorical and textual considerations, see Rickford & Handler 1994).

Overall, M.'s position on the possible impact of Caribbean slave imports, which is quite nuanced, is perhaps most fairly characterized by his conclusion that:

> Caribbean English vernaculars would have influenced the development of African American English (AAE) varieties either by favoring those options which they shared with the Virginian vernacular or by adding to variation

Carolinians, scarcely more than a thousand in number, came from the West Indies, and most were retained as slaves by a small number of aspiring white immigrants from Barbados ..." (Wood 1975: 130) "Until the mid-1670s, when slaves were first shipped from Africa, most of the Chesapeake's blacks came from Barbados and from other Caribbean colonies or from the Dutch colony of New Netherland ..." (Davis 1986: 8)

> ... But in no case could we assume that Caribbean slaves brought with them an already developed basilect from which AAE would have developed. (p. 240)

But I have reservations about at least one other assumption in this section, his claim that "Africans intended to speak ... the vernacular of their new ethnographic ecology. ... [they] knew that their adaptive success depended largely on how closely they approximated the diffused or less-focused target ... to which they were exposed" (p. 236). Maybe so. But we have to be careful not to see indentured servants or slaves through the lens of modern-day immigrants or job-candidates, who seek to maximize their success through mastery of the "language" of the marketplace. Certainly slaves did not have access to the same kinds of resources, and improvements in their mastery of the pre-existing vernacular would have redounded as much if not more to their *owner's/employer's* benefit as their own. Baker's (1990) suggestion that slaves from different regions might instead have been trying to create a "medium of inter-ethnic communication" – which M. explicitly rejects as "mistaken" – is quite plausible. In any case, M. speaks here and elsewhere about the *intentions* of slaves and contemporary peoples with greater confidence than he or any of us should.[56]

In relation to M.'s fourth point, about the relative importance of slaves who came directly to North America (or virtually so) from Africa, and about the importance of African substrate influence, I do not have any substantive disagreement. Although the Caribbean slave input (itself relatively fresh from Africa) was more important in some North American colonies during some of the 17th century phases, we are in complete agreement that by the (mid) eighteenth century, the African component was more substantial and potentially influential. M. does seem a bit equivocal about such influence, hypothesizing on the one hand that "African languages influenced Africans in North America to re-articulate and integrate English features in North America in somewhat different ways from their European counterparts" (p. 255), and on the other, appearing to endorse (on the same page), the claims of Poplack and the other contributors to this volume that "Early" AAE was virtually identical to White settler English in its features and constraints.

**56.** The other point is on page 253, where he says, "[p]erhaps the late nineteenth century was also the time when African and European-Americans were *particularly eager* to identify some linguistic peculiarities as ethnic markers and thus made divergent selections of features from the pool of variants they had shared until then" (emphasis added).

In any event, the great virtue of M.'s chapter is that it opens this and other sociohistorical issues (like the role of the 19th-century domestic slave trade, p. 247) for serious (re)consideration. The issues here are no more settled than the linguistic ones with which we have wrestled for four decades, but the prospects of shedding new light on them via library research are excellent. This will, I hope, attract more sociolinguists to the enterprise.

## Concluding remarks

*The English History of African American English* is a welcome book, which makes a useful contribution to the long-standing origins debate about this variety. Like the "dialectologists" who preceded them, the contributors to this volume believe that AAVE was shaped primarily by the dialects of English settlers whom African slaves and indentured servants encountered in America. But unlike their predecessors, the neo-Anglicists are more willing to concede contemporary Black/White differences, they draw on refreshingly new data sources (African American diaspora data from Samaná and Nova Scotia that, together with data from the Ex-Slave Recordings, are referred to as "Early African American English"), and they employ accountable methods of analysis drawn from modern quantitative sociolinguistics and variation theory. In her introduction, the editor provides a helpful discussion of some of the theoretical and methodological issues in the origins debate, and she articulates the principles that guided the analysis in the volume. In the chapters that follow, we are treated to intense quantitative analysis of six features of "Early African American English", and to a thought-provoking discussion of the sociohistorical context in which AAVE and EAVE emerged. Whether the features under analysis are perennials, like copula absence and plural marking, or relative newbies, like relativization and question formation, readers come away in each case with the sense that we have gained new information and/or new perspectives.

At the same time, despite the several positive reviews that have appeared in the five years since *The English History of African American English* was published, this book does have serious. Ironically, some of the most egregious are in its quantitative analyses – the aspect of this book that has received the highest accolades. If you carefully work through each of the tables in this book, assessing its logic and significance, and comparing it closely with the accompanying text, you will find, as I have indicated at various points in this article, that substantial claims are sometimes made on the basis of data that are statistically insignificant (e.g., *was*-leveling, relativization), that do not support the verbal argumentation (e.g., copula absence), or that point in directions opposed or tangential to those for which the contributors argue (e.g., plural marking). The fact that comparable quantitative data are limited or unavailable for most of the features considered in this book is not the contributors' fault, but what *is* available is sometimes misrepresented (e.g., negation), and when additional data from AAVE or English-based creoles *is* introduced (e.g., with respect to copula absence, plural marking), the book's findings are only partly replicated, and sometimes disputed or flatly refuted.

Far from constituting a knock-out punch to the creolist hypothesis, the argumentation and evidence in this book often fail to hit their mark. Instead of closing off debate, this volume should spur researchers to renewed scrutiny of its data and analyses, to the collection and examination of new data (from modern creole conversations and recordings to be sure, but also from texts and manuscripts of American, British and Creole English), and to consideration of some of the data types and sources Poplack and her collaborators exploit marginally or not at all. The latter include West African and other possible substrate languages, additional phonological, grammatical and lexical features, and the kind of inter-generational analysis, showing trajectories of change, exemplified in Wolfram and Thomas (2002). Ultimately, we also need – *all* of us – to be more than overarching anglicists or creolists. We need to be like the referees in the ring rather than the pugilists, willing to recognize hits and misses on either side.

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