**Against Consensus: Challenging the New Anglicists' Contentions Concerning the
Development of AAVE**

John R. Rickford
Stanford University

1. **Introduction**

The study of African American Vernacular English or AAVE has dominated the study
of Language and Society in America (SALSA, if you will). As Wolfram (2003, p. 282) has
noted, there were five times as many publications on AAVE from the 1960s to 1990sas
there were on any other variety. Much of that literature involves the issue of AAVE's
origins and development. The Anglicist or dialectologist position is that AAVE's features
come primarily or entirely from regional dialects spoken by white indentured servants and
other English settlers whom Africans encountered when they came to America, and whose
dialects they absorbed readily and completely. The opposed, Creolist position is that
although English dialects were an important input, AAVE also reflects substrate
influences from the languages Africans spoke before they were brought to America, and
the simplifying, restructuring processes associated with pidginization and
creolization.Such processes, as Hymes (1971) has noted, were characteristic of situations
in which language learners were distant from the norm of the target language. And in the
North American milieu as well as the Caribbean islands from which many of the earliest
North American slaves came, Africans were often (although not always) socially,
psychologically, linguistically and culturally distant and distinct from Europeans.

In a separate paper (Rickford, in press a), I have sketched the history of this
controversy over the past eighty years, dividing it into three main phases. My concern in
this paper is with the most recent phase, extending from the mid-1980s to the present, in
which the (neo-) Anglicists have increasingly held the upper hand. The work of Shana
Poplack and her colleagues and students (see especially Poplack, 2000; Poplack &
Tagliamonte, 2001) has been most influential in this regard. They have argued that
recordings from the African American diaspora, including African Nova Scotia English
(or ANSE) and Samaná English (in the Dominican Republic), together with the Ex-Slave
Recordings, support the Anglicist position and not the Creolist one. For instance, in her
introduction, Poplack (2000, p. 27) summarizes the papers in her edited collection as
follows:

> This research suggests that many of the features that have come to be associated with
> AAVE--e.g. *wa*s for *were*, *what* for *that*, zero plural, negative concord, non-inversion

in questions--are not simply incorrect forms that have subsequently become fossilized, as would be expected from the scenario attributing them to imperfect acquisition (e.g. Winford, 1998). On the contrary, they are regular, rule-governed parts of the grammar. In almost every case, quantitative variationist methodology has shown the system governing their use to be that attested in older forms of English. It has also shown them to differ systematically from creoles and, in one case, African languages. This lends strong confirmation to the idea that the structures, along with their variable conditioning, were already present in the English that the Africans first acquired, supporting the founder effect posited by Mufwene (1996).

And Labov (2001, p.xvi), in his preface to Poplack and Tagliamonte's (2001) book on Samaná English, urges us to a consensus on the correctness of the Anglicist position, one diametrically opposed to the creolist position he had supported two decades earlier (see Labov, 1982, p.192).

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.

In this paper, focusing on some of the features examined in Poplack (2000), I hope to argue that there are serious quantitative and qualitative barriers against any such consensus. Although a peacenik on conflicts like Iraq and Vietnam, I come to argue for conflict, not consensus, and to wage linguistic war, not peace. Labov noted in the same preface that, "the study of African American Vernacular English [AAVE] is not a field of peacable inquiry." This is quite fine by me, if the alternative is premature consensus.

One general problem with diaspora recordings made in the 1980s and 1990s is that they may not really offer us a valid and representative picture of 19th century African American speech. Poplack and Sankoff (1987) themselves admit the likelihood of internal if not external change in Samaná English, and even if we assumed little or no change over 180 years (most unlikely), these data and the ex-slave recordings, although enshrined as "Early African American English," only take us back to the early part of the19th century. This leaves the entire 17th and 18th century--the early "Early African American English" UNaccounted for, and it forces us to consider literary and sociohistorical evidence from earlier centuries. I'll return to this latter point, but I'd like to focus now on some of the quantitative analyses of specific features in Poplack 2000.

2. **Plurals**

Of the eight "Early African American English" variables examined in Poplack 2000 comparable quantitative data from creole communities exist for only two, copula absence and plural marking. Poplack, Tagliamonte and Eze 2000 (henceforth, PTE) analyze zero plural marking with the help of "Early" AAE data from their diaspora varieties, together with data from Gullah, Nigeria and Liberia. I'll reassess and extend their analysis with new quantitative data from Guyana and Jamaica, attempting to show that the situation is both more complex and more interesting than these authors suggest.

PTE begin by noting that the plural is often unmarked in the "Early AAE" varieties, much more so than in modern or contemporary AAE: 23.7% in SAM (397/1672), 26.9%

in ESR (115/427) and 36.4% in ANSE (492/1353),[1] compared with 1% to 13% for contemporary AAVE.[2] 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 (1981), Dijkhoff (1982), and Mufwene (1986), is said to involve the use of zero (e.g. dog), on non-individuated nouns (primarily generics, perceived as non-denumerable or non-countable), and on individuated nouns whose plurality is disambiguated because they co-occur with semantically plural demonstratives, numerals or quantifiers in 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 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).[3]

PTE proceed through various quantitative analyses of variable plural marking in the three "Early AAE" 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). Their 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 the number marking system in these "Early AAE" varieties 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 some cases, and I find the framing of the argumentation and facts sometimes self-serving (with respect to their over-arching "English" origins assumption). To illustrate this, let us consider Table 1, which is an

---

[1] PTE don't actually provide these percentages, although they do provide closely related "corrected means" in table 3.1 (SAM=.22, ESR=.24, ANSE=.34). I calculated the percentages by using their table 3.6 (2000, p. 97) data on the total number of plural Nouns considered in each sample, and Poplack and Tagliamonte's (1994, p. 245) data on the number of zero plurals in each sample.

[2] PTE (2000, p. 76) list the range for contemporary AAVE as "2 percent to 11 percent." I've 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 Bosto, respectively) examined in Rickford (1999, p. 264). Labov et al's (1968, p. 161) NYC peer groups had 8% zero plural, and Wolfram's (1969, p. 150) lower working class teenagers had 7.4%.

[3] A basic assumption of PTE's 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 follow the same principles. 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.

extended and modified version of their table 3.6--the culmination of their efforts to situate the "Early AAE" varieties vis a vis "Pidgin and Creole" varieties (pp. 96-98). The four data columns to the right in Table 1 incorporate data that were NOT in their Table 3.6: Jamaican (basilectal) creole data from Patrick (1994), and Jamaican (basilectal) and Guyanese (basilectal and mesolectal) data from my own fieldwork that were analyzed in the summer of 2003 by two Caribbean research assistants and myself.[4] The seven data columns to the left are identical with PTE's Table 3.6, with the following exceptions:[5]

(i) I have made <u>corrections</u> (marked with asterisks) to errors PTE made in transcribing data from other scholars' articles. For instance, they give the input probability for Gullah as .22, and for LSE as .35, but the correct figures are .78 and .30 respectively.

(ii) <u>Factor weights favoring</u> zero marking (those over .50) are indicated in bold.

(iii) <u>The language variety groupings are different</u>. PTE group 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" AAE. This strikes me as problematic in two respects. For one thing, LSE should NOT be excluded from the other Diaspora varieties, since it was settled in the 19th century in much the same way as the other settlements were. Secondly, the Ex-Slave Recordings should be separated from the Diaspora varieties, if only as a subgrouping of the "Early AAE" varieties, since the ESR speakers have lived continuously in the US, and they are themselves the exemplars of 19th century patterns.

---

[4] <u>Data Sources</u>: *Ex-Slave Recordings, Samaná English, African Nova Scotia English*, *Nigerian Pidgin English*, Poplack et al, 2000, Table 3.6; *Gullah*: Rickford, 1986, p. 51, 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 <u>dem</u>, 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 Stanford students Nadiya Figueroa (Jamaica) and J'Leise Springer (Tobago) in 2003.

[5] Another minor difference is that Table 1 begins with the phonological variables and then goes to the syntactic/semantic variables, while the order is the other way around in PTE's Table 3.6.

TABLE 1: Variable rule analysis of the contribution of recoded factors to the probability of zero plural in 'Early' AAE 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 — ESR	Samaná English — SAM	African Nova Scotia English — ANSE	Liberian Settler English — LSE	Gullah — GUL	Nigerian Pidgin English — NPE	Non-Settler Liberian English — LE	Jamaican Creole 1994 — JC94	Jamaican Creole 2003 — JC03	Basilectal Guyanese Creole 2003 — GCB	Mesolectal Guyanese Creole 2003 — GCM
Sample size (Input prob)	427 (.25)	1672 (.23)	1353 (.34)	574 (.30)*	128 (.78)*	1316 (.40)	2039* (.72)	1126 (.64)	663 (.69)	587 (.81)	613 (.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

<u>Notes</u>: 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, p. 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 –<u>Z</u> 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).

Let us now turn to substantive discussion of Table 1, working downwards in the table from the first factor group to the last. PTE assert (p. 98) 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." (Emphasis added.)

This generalization at first appears to be confirmed by the non-significance of this factor group [indicated by empty square brackets] in four of the seven pidgin/creole varieties, and by the fact that a following consonant actually disfavors zero in two of them.[6] 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 applies in "none of the latter." The authors, quite remarkably, retract this absolute claim in their footnote 7, "With the exception of Gullah . . . which patterns like the Early AAE varieties." (Emphasis added.)

In fact, examining the data more closely, in light of the variationist interest in whether a following pause patterns like a consonant or vowel, in influencing final stop deletion (see Guy 1980, pp. 27-28), it is clear that the "Early AAE" varieties do not behave uniformly. In African Nova Scotia English and Samaná, pause patterns with vowel in disfavoring zero. This low pause effect is associated with Philadelphia in studies of final t, d deletion. The Ex-Slave Recordings are quite different, resembling Gullah insofar as pause and consonant pattern together in favoring zero while vowel strongly favors the retention of –z. Interestingly enough, Guy (p. 28) found this "high pause" pattern for t, d deletion characteristic not only of New Yorkers, but also of virtually all the Black speakers he surveyed, regardless of their geographical location. We don't know yet what deeper significance to attach to these sub-groupings. Is there historical significance to the fact that the two US Black varieties are high pause, and the two Diaspora varieties are low 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 are freed from the obsession with claiming only that the "Window varieties" pattern similarly, and that their patterns differ from those of the pidgin-creole varieties.

Similar complications are evident when we contrast PTE's discussion of the results for the preceding phonological segment, with the VARBRUL factor weights therein. They say, simply, and dismissively, (p. 98) 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." Well, yes, and what is REALLY interesting is that the Ex-Slave Recordings are more like Liberian Settler English, and Jamaican Creole 94, insofar as a preceding sibilant patterns with a preceding vowel in favoring –s retention. But Samaná English, Gullah, and Jamaican Creole 2003 behave differently, with a preceding sibilant similar to a preceding non-sibilant consonant in favoring zero, sometimes moreso. Once again, the most salient

---

[6] 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 does not appear to be subject to exactly the same constraints as -s marked pluralization is, especially with respect to phonological constraints. See Patrick (1994) for further discussion.

similarities and differences that the data present are NOT the ones to which the authors draw our attention, and they challenge the neat line they try to draw between the "Early AAE Varieties" and the "Pidgin-Creole Varieties."

In the case of the type of nominal reference factor group, 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 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.[7]

In the case of the animacy of the noun factor group, 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--additional evidence that it should be grouped with ANSE and SAM), supports 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, Carranza & Kendall, 1993), the constraint effects were reversed, with human nouns favoring zero plural -s marking (.57), more than inanimate ones did (.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" and "animals") in at least three cases: LSE (see Singler 1989, p. 62, fn. 19), NSLE (see Singler, 1991, p. 549) and NPE (see Tagliamonte, Poplack & Eze, 1997, p. 121).[8] 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

---

[7] And lest we take these data as "God's truth," note 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.

[8] PTE's (1997, p. 121) table 8 on NPE zero plural marking distinguishes between [-human] and [+human], while their (1997, p. 116) table 6 on NPE zero plural marking distinguishes between [-animate, -human] and [+animate, +human]. Since the sample size in both cases is identical (n=1316), might the [-human] wording in table 8 simply have been a shorthand for the [-animate, +human] distinction in table 6? Not likely, both because a data set of this size is likely to include some nouns with animal referents, and because their NPE sentence 7a (PTE 1997, p. 112) demonstrates that their data set indeed includes examples of just this type (Na de wey *got* de slip 'That's where goats sleep').

[-human], this new factor tends to have the statistical weight of the inanimates.[9] But it may be theoretically and historically important to observe the distinction, and to provide data on all three categories. [10] 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 pp. 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 1): .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, p. 90) explain the animacy effect in NPE as a possible transfer from Igbo, the first language of most of their informants, and they cite Welmers (1973, p. 220) as showing that "bare inanimate nouns have generic reference, while bare human nouns receive a singular interpretation, unless plurality is otherwise specified." But the primary distinction Welmers 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": (16) ú.mù. é'wù 'goats' (lit. 'PL. goat').[11]

Finally, even granting this one case--generously, two--where the constraints on zero marking in the "Window Varieties" differ from those in pidgins and creoles, this does not argue decisively against creole ancestry or <u>for</u> English ancestry. Despite PTE's insistence on the importance of constraint hierarchies, NOT A SINGLE ONE of the constraints on zero plural that they report 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 1, 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 <u>ten car</u>, <u>plenty cow</u>), 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.)

---

[9] 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 a "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."]

[10] See Rickford (in press b) for further discussion.

[11] An additional distinction between personal and non-personal nouns, is relevant in Igbo.

Consequently, PTE's conclusion that plural marking inthe "Early AAE" varieties shows no pidgin-creole connection and can be completely attributed to English is not supported.

3. **Wh-questions**

For a second feature, let us consider Auxiliary Non-Inversion in Direct Questions, explored by Gerard Van Herk in the Poplack (2000) volume. In the interest of space, I'll skip my remarks on his analysis of Yes/No questions (but see Rickford, in press b for this) and focus only on his discussion of Wh-questions with do-support. Referring to the Samaná data on non-inversion in Wh-questions. Van Herk observes (p. 180) that the 39% SAM non-inversion rate is "far less" than the categorical non-inversion that an (idealized) creole diagnostic would lead us to expect. But it's also far MORE than the standard English prohibition on non-inversion in Wh-questions would lead us to expect. [12] McWhorter (2000, p. 401) 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 presentation, 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.

And given the evidence of studies such as Williamson (1972), I would have to agree.[13] 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 without overt auxiliaries, as in "What you looking at?" and "What you say?"

The heart of Van Herk's chapter is pp. 181-192, where he presents quantitative data on the conditioning of non-inversion in "Early" AAE (SAM and ANSE), using constraints identified by Ellegård (1953), Stein (1988), Kroch (1989) as significant in the rise of do insertion in Early Modern English. Van Herk summarizes his findings (p. 192) 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 SAM, with modals and copulas. These parallels to the complex system of Early ModE question formation are striking, and are beyond coincidence.

---

[12] 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) uninverted 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?"

[13] 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.

These findings are indeed striking, and I agree with Van Herk'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" (p. 192)--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 Van Herk 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 Van Herk is really comparing are the factors that promoted the rise of periphrastic do in Modern English questions like (1), 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 Middle English system of question formation involving lexical verb inversion (2) where an overt auxiliary was not already present:[14]

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

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

But of course, do insertion questions like (1), 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 Van Herk (cf. "What did you say"? p. 184), as they would be by any modern researcher. So what we're actually comparing are the factors that promote "inversion" (via do insertion) in Early Modern English with the factors that promote "non-inversion" in "Early" AAE. There is something a little disingeneous about this, even granting Van Herk's point that "both EModE do and "Early" AAE non-inversion preserve Subject-Lexical Verb-Object order" (personal communication).

We should also remember that, historically, these constraints on question formation don't apply to questions like (3) which don't involve do support, for example, sentences that already have a copula or auxiliary. These retain in Modern English the auxiliary inversion they already had in Middle English:

(3) ...have we not cast oute devyls? (Kroch, 1989, p. 216, as cited in Ellegard, 1953)

Since the "Early" AAE corpus includes many questions with NON do-support auxiliaries I am 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 "By 1700, this new form [question formation with periphrastic do] had largely, though not entirely ... replaced the original usage" (Kroch, ibid.), I am wondering why we would expect it to have influenced the patterns of question formation among African Americans who emigrated to Samaná and Nova Scotia in the late **18th** and early **19th** centuries. These questions leave me somewhat skeptical about the English influence on question formation in "Early" African American English.

---

[14] Both examples are from Kroch (1989, p. 216), and ultimately Ellegård (1953).

4. **Relativization**

Some of these barriers to our accepting the "new consensus" are qualitative rather than quantitative, and I do want to give a few more qualitative examples. But in view of the excessive praise for its number crunching that has been heaped on Poplack's volume, I can't help making a final quantitative comment, which is that in at least two papers (the ones on <u>was</u> levelling and relativization), the crucial supporting data turn out to be almost completely non-significant. Note for instance, Table 2, which provides chi-square measures for the major tables in Tottie and Harvie's paper on relativization. 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 the various constraints considered. So the crucial data tables for the analysis of zero relative markers in non-subject and subject function (Tables 7.5-7.10) all consist of percentages of zeroes rather than probabilistic VARBRUL weights. These tables are not accompanied by any chi-square or other measures of statistical significance, and when I calculated them myself,[15] I was surprised to discover that only five of the eighteen data distributions for ANSE, SAM and ESR in these tables were statistically significant, as shown in Table 2.[16] What this means is that the observations in the accompanying text regarding the effect of specific factors must be regarded as vacuous, or at best suspect, awaiting confirmation from additional data.

TABLE 2: Chi square and significance assessments of the data in T&H's Tables 7.5-7.10

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

5. **Sociohistory**

For some closing examples of "qualitative" barriers to a new consensus, let's turn now to "Sociohistory," considered by Salikoko S. Mufwene (herafter "M") in the eighth and final

---

[15] 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

[16] In Tables 7.7 and Tables 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.

chapter in Poplack (2000).

One of M's arguments is that a Gullah or Caribbean-like creole did not 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 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 "The 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).[17] M feels that the segregationist Jim Crow laws of the late 19th century (see 32) "forced African and European Americans to live in separate neighborhoods and not to use the same public facilities. All these changes entailed limited interaction between African and European Americans, thus providing the first socio-economic ecology for linguistic divergence between the vernaculars of the two races" (p. 248).

There is much in these general conclusions--and their supporting details--with which I would agree, in fact, have already agreed. For example, the conclusion that 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 outside of the Gullah area interacting freely and living and speaking in parallel ways until the end of the 19th century is far too rosy, and significantly under-represents the divisive effects of slavery, the formidable institution that dominated the lives of Blacks (and Whites) for the preceding two hundred years. For instance Tate (1965, pp. 10-11)--a source Mufwene cites but doesn't really quote from-- provides evidence of the increasingly repressive slave codes that developed in Virginia and virtually every other North American territory, from the mid 17th century to the mid-18th century:

> Besides the widening gap in the length of service demanded of white and Negro servants, a few other distinctions began to appear in these years [mid 17th century] to the disadvantage of the black man. These restrictions bear some of the marks of racial prejudice. Negroes were excluded, for instance, by a statute of January 1639/40 from the requirement of possessing arms and ammunition. ... Once the law of 1660/61 had admitted the possibility of life servitude, there followed a period lasting down to about 1675 or 1680 during which a number of laws confirmed or defined further the Negro's lower status. More and more, these differentiations cut

---

[17] 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.

the Negro "apart from all other servants and gave a new depth to his bondage"[Quotation at end is from Handlin and Handlin, p. 209].

Tate also testified (1965, pp. 34-35) to the fact that most slaves were field hands rather than household workers and personal servants:

> The largest proportion of Negroes--men, women and children--were field hands, assigned to growing tobacco and the other marketable crops the colony [of Virginia] produced. This was the real purpose for which slavery had evolved, and it represented the institution in its most impersonal, burdensome, and typical form. ... A smaller but still significant number of slaves fared somewhat better as household workers and personal servants ...

And Tate (1965, pp. 34-35), quoting from Smyth (1773, pp. 84-5), also noted the frequency with which Blacks were overworked and mistreated:

> He [the slave' is called up in the morning at daybreak, and is seldom allowed time enough to swallow three mouthfuls of homminy, or hoecake, but is driven out immediately to the field to hard labour, at which he continues, without intermission, until noon. ... About noon is the time he eats his dinner, and he is seldom allowed an hour for that purpose. ... They [i.e. the slaves] then return to severe labour, which continues in the field until dusk in the evening, when they repair to the tobacco houses, where each has his task in stripping allotted him, that employs him for some hours.

Foner (1975, pp. 205-207) is replete with details of increasingly repressive slave codes enacted in South Carolina and other states from the 17th century to the 18th:

> [S. Carolina]: In 1696, ... the South Carolina Assembly passed an especially barbaric slave code that established a despotic control over every aspect of the slave's life. ... the code stipulated that slaves needed written permission to leave their masters' residences; slave-owners were required to make regular searches of slave quarters for weapons; and slaves who ran away or struck their masters faced severe penalties, among which were whipping, branding, slitting the nose, and castration. More specifically, no master or overseer was allowed to give his slaves leave on Sundays... or at any other time to go out of his plantation without a pass, ... and any person seeing a slave out of his master's plantation without a pass was empowered to correct the miscreant by whipping, not exceeding twenty lashes. ... For lesser offenses, such as stealing or killing cattle... for the first offense the slave was to be branded with an "R" on the right cheek; for the second, with an "R" on the left cheek and whipped up to forty lashes; for the third offense, he would be executed. ...

> The act of 1696 remained, with some slight amendments, the slave code of South Carolina until 1740 [the year after the Stono River slave insurrection of 1739]. The major provisions of the new slave code were directed to making sure that the slave "be kept in due subjection and obedience." The law outlawed all assemblies of slaves, forbade the sale of alcohol to them, and prohibited them from learning to write so as to curtail forged passes. ...

And these could be supplemented with the numerous first person accounts of brutality toward slaves recounted in Weld (1841).

Partly because of these kinds of legislation and this kind of mis-treatment, many Blacks and Whites led divergent and non-interacting lives, and whether because of limited contact or limited inclination/motivation, their religion, music, folk-culture and world-view often differed. Note, for instance, this 1842 remark by the Reverend C.C. Jones, quoted in N. Jones (1990, p. 21):

> 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 themselves. 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.

And note too this remark from Kulikoff (1986, p. 351), referring to the late 1700's: "White observers agreed that the music, dance and religiosity of black slaves [in the Chesapeake] differed remarkably from those of whites."

If these kinds of separation and differentiation, induced by slavery, were not enough to yield the kind of restructuring associated with pidginization and creolization, they were certainly enough to inscribe different Black/White identities long before Jim Crow laws came into effect, and they might have been enough to produce Black/White vernacular differences long before Jim Crow laws and the urban residential segregation of the 20th century brought new divergences in their wake, quite apart from the creole influences introduced by Caribbean slaves brought up to the American colonies in the 17th and 18th centuries (Rickford, 1997).

Finally, speaking of African American Diaspora varieties, M concludes (p. 247) that "nothing has been found so far which suggests that AAVE was more creole-like at the beginning of the nineteenth century." But this ignores the contrary indications of the OTHER 19th century Diaspora variety, Liberian Settler English [LSE]. M acknowledges in footnote 12, the greater heterogeneity and creole-like nature of the Liberian Settler evidence, but he suggests that "some of the features have their origins in Kru Pidgin English"--a suggestion that Singler emphatically rejects (personal communication, but see also Singler 1997, 1998), since the Liberian Settlers and the Kru had little contact. Indeed, in his (1998) paper, Singler shows that some of the features that Myhill had thought of as 20th century inventions, using the Ex-Slave Recordings, were attested as 19th century features of American AAVE based on the evidence of Liberian Settler English.

6. Conclusion

In this paper, I have argued that while Poplack and her colleagues have made helpful contributions to the debate on AAVE's origins, we should resist any premature consensus on their conclusion that AAVE is almost entirely derived from English, or (based on Mufwene's arguments especially) that modern AAVE only developed in the 20th century, when Jim Crow laws and migration to northern ghettoes kept Blacks and Whites apart. I have tried to show that there are empirical (both linguistic and statistical) weaknesses in the new Anglicists' arguments, and that what we know of the sociohistorical relations

between Blacks and Whites from the 17th century on does not support the rosy view of social relations and linguistic transmission that the Anglicist view assumes. Sociohistory indeed is one of the weak links in modern sociolinguistics, and one of the areas in which creole studies and the study of AAVE offers fertile fields for new research by bright students like those at SALSA. To advance the debate on AAVE's origins, we also need more research on the extent and role of creole influences introduced to North America by Caribbean slaves, more quantitative analyses of pidgin-creole varieties to permit fruitful comparison with AAVE and other varieties for which quantitative (including variable rule) analyses exist, and more analyses of West African patterns to see whether these might underlie some of the characteristics of modern AAVE that we attribute so readily to English influence. Once the lacunae represented by these areas have been filled, we will be in a better position to reach a satisfying consensus.

References

Alleyne, M. (1980). Comparative Afro American. Ann Arbor: Karoma.

Bickerton, D. (1975). Dynamics of a Creole System. Cambridge: Cambridge University Press.

Bickerton, D. (1981). Roots of Language. Ann Arbor: Karoma.

Comrie, B. (1981). Language universals and linguistic typology : Syntax and morphology. Oxford: Blackwell.

Dijkhoff, M. (1982). The process of pluralization in Papiamentu. In Norval Smith (ed), Amsterdam Creole Studies (pp. 48-61). Amsterdam.

Ellegård, A. (1953). The auxiliary do: The establishment and regulation of its use in English. Gothernburg Studies in English. Stockholm: Almqvist and Wikwell.

Foner. P. S. (1975). History of Black Americans, vol. 1: From Africa to the emergence of the cotton kingdom. Westport (Conn.), and London: Greenwood Press.

Guy, G. (1980). Variation in the group and the individual: The case of final stop deletion. In William Labov (ed), Locating language in time and space (pp. 1-36). New York: Academic Press.

Handlin, O., & Handlin, M. F. (1950). Origins of the Southern labor system. *William and Mary Quarterly, 3rd series, VII* (April), 199-222.

Hymes, D. (ed) (1971). Pidginization and Creolization of Languages. Cambridge: Cambridge University Press.

Jones, N. T., Jr. (1990). Born a child of freedom, yet a slave: Mechanisms of control and strategies of resistance in antebellum South Carolina. Hanover (N. H.): University Press of New England.

Kroch, A. (1989). Reflexes of grammar in patterns of language change. *Language Variation and Change, 1.3*, 199-244.

Kulikoff, A. (1986). Tobacco and slaves: The development of Southern cultures in the Chesapeake, 1680-1800. Chapel Hill (N. C.) & London: University of North Carolina Press, for the Institute of Early American History & Culture, Williamsburg, Virginia.

Labov, W. (1982). Objectivity and commitment in linguistic science: The case of the Black English trial in Ann Arbor. *Language in Society*, *11*, 165-201.

Labov, W. (2001). Foreword. In S. Poplack & S. Tagliamonte, Eds., African American English in the Diaspora (pp. xiv-xvii). Malden (Mass.) & Oxford: Blackwell.

Labov, W., Cohen, P., Robins, C., & Lewis, J. (1968). A study of the Non-Standard English of Negro and Puerto Rican speakers in New York City. 2 vols. Philadelphia: US Regional Survey.

McWhorter, J. (2000). Strange bedfellows: Recovering the origins of Black English. *Diachronica XVII*: 2, 389-432.

Miller, M. I. (1978). Inflectional morphology in Augusta, Georgia: A sociolinguistic description. Chicago: University of Chicago dissertation.

Mufwene, S. (1986). Number delimitation in Gullah. *American Speech 61*, 33-60.

Patrick, P. (1994). Functional pressures on plural-marking in Jamaican patwa. Paper presented at the Society for Caribbean Linguistics 10th biennial conference, University of Guyana, Georgetown, August 1994.

Patrick, P., Carranza, I. & Kendall, S. (1993). Number-marking in the speech of Jamaican women. Paper presented at the 22nd Annual Conference on New Ways of Analyzing Variation (NWAV-XXII), University of Ottawa.

Poplack, S. (ed) (2000). The English history of African American Vernacular English. Malden (Mass.) & Oxford: Blackwell.

Poplack, S. & Sankoff, D. (1987). The Philadelphia story in the Spanish Caribbean. *American Speech 62*, 291-314.

Poplack, S. & Tagliamonte, S. (1994). -S or nothing: Marking the plural in the African American diaspora. *American Speech 69*, 227-259.

Poplack, S. & Tagliamonte, S. (Eds.) (2001). African American English in the Disapora. Malden (Mass.), & Oxford: Blackwell.

Poplack, S., Tagliamonte, S., & Eze, E. (2000). Reconstructing the source of Early African American English plural marking: A comparative study of English and Creole. In S. Poplack (ed), The English History of African American English (pp. 73-105). Malden (Mass.), & Oxford: Blackwell.

Popper, K. (1960). The logic of scientific discovery. London, Hutchinson [Translation of Logik der Forschung, 1934.]

Rickford, J. R. (1986). Some principles for the study of Black and White speech in the South. In M. B. Montgomery & G. Bailey (Eds.), *Language Variety in the South* (pp. 38-62). Tuscaloosa (Al.): University of Alabama Press.

Rickford, J. R. (1990). Number delimitation in Gullah: A response to Mufwene. *American Speech 65*, 148-163.

Rickford, J. R. (1991). Representation and reliability of the ex-slave materials, with special reference to Wallace Quarterman's recording and transcript. In G. Bailey, N. Maynor, & P. Cukor-Avila (Eds.), The Emergence of Black English: Text and commentary (pp. 191-212). Amsterdam: John Benjamins.

Rickford, J. R. (1997). Prior creolization of AAVE? Sociohistorical and textual evidence from the 17th and 18th centuries. *Journal of Sociolinguistics, 1*(3), 315-336.

Rickford, J. R. (1998). The Creole origins of African-American Vernacular English: Evidence from copula absence. In S. Mufwene, J. R. Rickford, G. Bailey, & John Baugh (eds.), *African-American English: Structure, history, use* (pp. 154-200). London: Routledge.

Rickford, J. R. (1999). African American Vernacular English. Oxford: Blackwell.

Rickford, J. R. (In press a). The Anglicist /Creolist quest for the roots of AAVE: Historical overview and new evidence from the copula. In J. M. Fuller & L. Thornburg (Eds.), *Studies in contact linguistics: Essays in honor of Glenn G. Gilbert*. Bern & New York: Peter Lang.

Rickford, J. R. (In press b). 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, MA, and Oxford, UK: Blackwell. In Journal of Pidgin and Creole Languages.

Sabino, R., Diamond, M., & Cockcroft, L. (2003). Language variety in the Virgin Islands: Plural markings. In M. Aceto & J. P. Williams (Eds.), Contact englishes of the Eastern Caribbean (pp. 81-94). Amsterdam: John Benjamins.

Sabino, R, Diamond, M. & Oggs, A. (2004). A quantitative study of plural marking in three non-urban African American language varieties. Paper presented at the Third conference on Language Varieties in the South (LAVIS III), University of Alabama, Tuscaloosa.

Singler, J. (1989). Plural marking in Liberian Settler English, 1820-1980. *American Speech 64*, 40-64.

Singler, J. (1991). Social and linguistic constraints on plural marking in Liberian English. In J. Cheshire (ed), English around the world: Sociolinguistic perspectives (pp. 545-562). Cambridge: Cambridge University Press.

Singler, J. (1997). The configuration of Liberia's englishes. *World Englishes 16*, 205-31.

Singler, J. (1998). What's not new in AAVE. *American Speech 73*, 227-56.

Smyth, J. F. D. (1773). Smyth's travels in Virginia, in 1773. *Virginia Historical Register VI (April 1853)*, 84-5.

Tagliamonte, S., Poplack, S. & Eze E. (1997). Plural marking patterns in Nigerian Pidgin English. *Journal of Pidgin and Creole Languages 12*(1), 103-129.

Tate, T. W. (1965). The Negro in eighteenth-century Williamsburg. Williamsburg, VA: Colonial Williamsburg. [Distributed by University Press of Virginia, Charlottesville, VA].

Weld, T. D. (1969 [1841]). Slavery and the internal slave trade in the United States; being replies to questions transmitted by the Committee of the British and Foreign Anti-Slavery Society. 1840. New York: Arno Press & the New York Times.

Welmers, W. E. (1973). African language structures. Berkeley: University of California Press.

Williamson, J. V. (1972). A look at the direct question. Studies in linguistics in honor of Raven I. McDavid, Jr. (pp. 207-14). Tuscaloosa (Al.): University of Alabama Press.

Winford, D. (1998). On the origins of African American Vernacular English--A creolist perspective. Part II: Structural features. *Diachronica XV*.

Wolfram, W. (1969). A sociolinguistic description of Detroit Negro speech. Washington, DC: Center for Applied Linguistics.

Wolfram, W. (2003). Re-examining the development of African American English: Evidence from isolated communities. *Language 79.2*, 282-316.

Wolfram, W., & Thomas, E. R. (2002). The development of African American English. Malden (Mass.), & Oxford: Blackwell.

Department of Linguistics
Stanford University
Stanford, CA 94305-2150
rickford@csli.stanford.edu