Vernacular African American English: Perspectives on Its Origin

Jennifer Abercrombie
Senior Paper in Linguistics
Swarthmore College
submitted: May, 1991
Prof. Steven Piker, Advisor
Prof. Donna Jo Napoli, Department Head
Introduction:

As early as 1884, a study conducted by James A. Harrison identified grammatical features "peculiar" to the speech of African Americans (Brewer, 1974: 12). Harrison intended his study not as a definitive work, but as encouragement for further study of this dialect; unfortunately, "Harrison's call for further study of BE [Black English] was only lamely answered for more than 75 years" (Brewer, 1974: 13). In fact, through the middle of the twentieth century, the predominant view of dialect geographers such as George Philip Krapp and H.L. Mencken was that the dialect of African Americans had solely British origins. Furthermore, they claimed that this dialect was not distinct from Standard English, arguing that any differences between the two varieties were attributable to pure geographic variation (Dillard, 1972: 8-10). A notable exception to this view is the 1949 study by Lorenzo Turner who compared the creole variety of English, Gullah, spoken on the Sea Islands off South Carolina, to various African languages (Dillard, 1972: 9). While convincing in its linking of Gullah to African languages, the study did little to change the prevailing British-origin theory because Gullah was "conceded a special status," an "anomaly" of creole origin, unrelated to the dialect spoken by African Americans on the mainland (Brewer, 1974: 15).

It was not until the 1960's that the field of study truly "got off the ground." Fueled in part during this decade by the general interest in "black issues," particularly in the area of education, many new studies of the speech of African Americans were conducted, particularly of those from economically deprived urban areas. Especially notable is William Labov's 1968 study of the speech of inner-city black youth in New York City. He and his colleagues provided one of the first detailed descriptions of the black speech system as a whole rather than descriptions of individual features. He used innovative methodology, particularly in sampling procedures, and new analytic techniques such as the analysis of free variation using linguistic variables (Montgomery and Bailey, 1986: 15). During this same period, the "creolist position" also emerged at the forefront as a backlash against the British-origin theory. William Stewart and J.L. Dillard are two well-known linguists of this camp who advanced the theory of the creole origin of black speech using socio-historical data found in literary sources of the relevant time periods (Montgomery and Bailey, 1986: 13-14).

The new sources of data and theories about black speech introduced in this period have opened
the field of study wide open. Indications today are that, while much valuable research has been
done and continues to be done, much more is needed. While some general consensus has been
reached in some areas, many more issues and areas are hotly debated, oftentimes with completely
different methodology and data presented by either side.

Taking a step back from the multitude of approaches which comprise this field of study and
regressing a bit to the basics, one might inquire about the nature of the speech system under such
intense investigation. The disparity of the discipline is even reflected in the plurality of names used
to label this dialect: Nonstandard Negro English (NNE), Black English Vernacular (BEV),
Vernacular Black English (VBE), Black English (BE), Black Street Speech (BSS), etc. Currently,
however, the term Vernacular African American English (VAAE) is preferred (Baugh, 1991,
personal communication), and that is the term which will be employed in this paper. In general,
this dialect is the vernacular variety of English associated with the ethnic group of African
Americans, but the issue of its characterization goes much deeper than that.

First, it is important to remember that African Americans, while sharing the same race, do not
comprise a homogeneous sector of society; they span a wide range of social, economic, and
educational backgrounds just as their Anglo-American counterparts do (Baugh, 1986: 1). It is not
to be expected, then, that African Americans will all speak one homogeneous variety of English.
When speaking of “Black English” one must realize that this term encompasses the whole range of
dialects spoken by African Americans “from the Creole grammar of Gullah spoken in the Sea
Islands of South Carolina to the most formal and accomplished literary style” (Labov, 1972: xiii).

Vernacular African American English has a much narrower definition. Originally, this
vernacular (formerly labeled Black English Vernacular) was conceived as “that relatively uniform
grammar found in its most consistent form in the speech of black youth from 8 to 19 years old who
participate fully in the street culture of the inner cities” (Labov, 1972: xiii). One reason this
particular group of speakers was heavily targeted, at least initially, by researchers of VAAE is that
“such children and teenagers brought new problems to the school systems of the large Northern
cities” (Dillard, 1985: 198). Beyond purely educational concerns, this group was also a logical
target group for identifying characteristics of VAAE since the youths who were fully immersed in
this “street culture” would have less contact with speakers of SE than other African American
groups. Therefore, their speech would be less affected by such contact and would reflect one of
the less hybridized dialects of Black English.

The overall linguistic picture of VAAE is more complex, however, when the entire African American population is taken into consideration. It is estimated that about eighty percent of the African American population speaks VAAE (Dillard, 1972: 229). Some African Americans, such as the children of professionals who have a great deal of contact with "nonblacks", do not speak VAAE at all (Baugh, 1983: 9). Of the majority of the population who do speak VAAE, all engage in a certain amount of style-shifting from the more informal vernacular to the more formal approximation of SE depending on the social context, but with varying amounts of proficiency (Baugh, 1983: 7). This style-shifting from formal to informal speech, the range of which depends on the personal background of the individual, is not unique to the African American population, but the breadth of speaking styles actively used by African Americans is considerably greater than that of many other ethnic groups, especially those of European origin, yielding a more complicated situation in general (Baugh, 1983: 4). Thus, while eighty percent of the African American population speaks VAAE, at least in the most informal of social contexts, almost no African American will speak VAAE all the time and some African Americans do not speak it at all.

So, while the actual sociolinguistic picture of African American dialects is very complex, there is, nevertheless, a non-standard dialect of English which is characteristic of this particular ethnic group in general. The focus of this paper is not the sociolinguistic issues of VAAE as it is spoken today but, rather, the debate over its origins. To this end, the first section will present a brief overview of the creolist position and some of the difficulties and arguments against it. The next section will focus on a particular syntactic structure of VAAE, invariant *be*, including some discussion of its possible historical development. Based on the information presented in these two sections, a discussion will follow about the most promising theories currently endorsed as well as some comments about the field of study of VAAE in general.
The Creolist Position: Arguments For and Against:

There are many variations on the description of the original pidginization and subsequent creolization of the language of African slaves in the New World, but it is necessary to provide one fairly plausible scenario of this history at the onset of the discussion, from which to embark on the many questions and issues which will arise. To this end, *The Story of English* by McCrum, et al., contains a brief, well-documented overview of the possible history surrounding the linguistic developments of the African population in the New World.

According to McCrum, et al., the distant ancestor of Vernacular African American English is Sabir, the “lingua franca” of the sea-trading world. Sabir dates from the time of the Crusades and existed up through the nineteenth century. It originally evolved in the Mediterranean basin as a means of communication in multi-ethnic ship crews and would have been widely used in the trading industry by the time the slave trade became really profitable. In fact, the Portuguese had been trading in West Africa for a couple hundred years at that point, and a contemporary source attests to the fact that the West Africans living along the coast who dealt with the European traders often spoke either Portuguese or Sabir. (McCrum, et al., 1987: 197).

It is against the backdrop of this already multilingual trading industry that the many different linguistic backgrounds of the African slaves are introduced. It was a common practice to split up slaves from the same tribe and instead combine slaves from many different tribes to shackle and ship together. The reasoning behind this practice was that the risk of rebellion would be less, if the slaves could not speak each others’ languages. As a result, the slaves would have picked up a lot of the English pidgin from the sailors on the ships as a means to build communication among themselves (McCrum, et al., 1987: 200). Thus, these African slaves would have already spoke an English pidgin, at least to a limited degree, before they ever arrived in the Carribean or North America.

Once the slaves did arrive in their various destinations, their English pidgin developed into the Carribean creoles on the various islands, and the Plantation Creole of North America (of which Gullah, spoken on the Sea Islands of South Carolina even today, is its closest surviving relative) (McCrum, et al., 1987: 208). These creole varieties today form a creole continuum to which historical linguists refer when trying to trace the history of VAAE (McCrum, et al., 1987: 209). McCrum, et al., claim that this continuum “runs from the Krio of Sierra Leone [in West Africa] to
Caribbean creole to Gullah to the modern Black English of the United States (p. 209).” If this is true, then an examination of Carribean creoles should reveal something about the historical development of VAAE.

With regard to general pidginization and creolization theory, this scenario seems possible. Linguists who study pidgins and creoles are not in complete agreement as to the exact mechanisms and processes which produce these linguistic phenomena, but there are some general hypotheses in this area which have relatively wide acceptance. It is these principles which will be employed in this discussion.

With regard to pidginization, Sankoff defines a pidgin as a “contact vernacular” with a certain amount of “conventionalization,” or predetermined grammaticality, which renders it different enough from the parent language(s) so that a native speaker of a parent language could not understand the pidgin (Sankoff, 1980: 140). A pidgin arises when a means to communicate between speakers of different languages is urgently needed and there is not enough time for the speakers to learn each others’ language. Since there is not sufficient time for the speakers to become bilingual, they end up communicating through an imperfectly learned contact language which is not native to either speaker (Sankoff, 1980: 144). Sankoff argues that other factors are also involved in the development of a pidgin. For one thing, the more mutually unintelligible the languages involved in the communication situation, the more likely a pidgin will develop among the speakers. Also, some kind of social dislocation must occur in order to necessitate the development of a pidgin as a communication strategy (Sankoff, 1980:144). Finally, the contact or target language toward which the pidgin is modeled is usually the language of the people in power, although “home territory” and numerical superiority of speakers can also be factors if the power dynamics are not very clear cut (Sankoff, 1980: 140-1). If one group is clearly in a position of power, however, the advantages to be gained by learning to speak the empowered language will override these other factors.

Looking at the pidginization scenario during the peak of the slave trade, it seems to fit in with Sankoff’s description of the pidginization process in general. Certainly the African slaves were in a situation of extreme social dislocation, one of Sankoff’s criteria for pidginization. Also, the Africans came from a variety of different linguistic backgrounds and were deliberately placed in groups of extreme linguistic variability, which Sankoff claims is fertile ground for the development
of a pidgin. Furthermore, given the approximately three months' voyage on the slave ships, there
certainly was not time for the Africans to become bilingual and their need to communicate with
each other was indeed urgent. Finally, Sabir is a pidgin with Portuguese as its target language,
which fits in with Sankoff's theory that the language of the people in power, in this case the
European traders, becomes the target language of the pidgin speakers.

A pidgin is necessarily more "structurally limited" than the native languages of a pidgin's
speakers due to the nature of the communicative situation under which a pidgin arises (Kay and
Sankoff, 1974: 61-62). Kay and Sankoff argue that the phonology of a pidgin is more shallow
than that of a natural language in that no changes occur in the phonology between deep and surface
structure (K & S, 1974: 62-63). They also argue that the syntax of a pidgin is also much simpler
than that of a natural language and that the surface structure of a pidgin, in general, more closely
reflects the actual deep structure (K & S, 1974: 66). In other words, fewer transformations occur
between the deep structure and surface structure of a pidgin. One possible explanation for this
simplicity is that grammatical structures which are common to all or most of the native languages of
the pidgin speakers are more highly favored to form the grammatical base of the pidgin (K & S,
1974: 67-68). Thus, there are far fewer grammatical options for a pidgin than for a natural
language.

If Kay and Sankoff are correct in their hypothesis that a pidgin more directly reflects its deep
structure in its surface realizations, then "the process of creolization of a pidgin involves the
creation of transformational machinery which moves the surface structure progressively further
away from universal deep structure" (K & S, 1974: 66). More generally, while the process of
pidginization is a process of linguistic simplification, creolization is a process of linguistic
complication and expansion. A creole comes into existence when a pidgin is learned and spoken as
a native or primary language (Rickford, 1987: 32). For example, if the children of a particular
group of pidgin speakers grow up hearing primarily the pidgin spoken around them, they will
acquire the pidgin as their native language. Bickerton hypothesizes that these children then use
their "innate bioprogram" or language faculty in the brain "to expand and develop [the] pidgin into
a functionally adequate first language" (Rickford, 1987: 2). Regardless of the exact psychological
processes involved, through its adoption as a native language, a pidgin is transformed into a creole
with the usual complicated phonology, syntax, etc. of any other type of natural language.
The linguistic situation of the creolization process often manifests itself as a creole continuum. Frequently, there is, rather than a sharply defined boundary between a creole and the target or prestige language, a "continuous transition" between two linguistic poles, one being the creole and one, the target language (Rickford, 1987:17). Along this continuum, linguists identify the "basilect," which is the creole variety furthest away from the target language, the "acrolect," which is the creole variety closest to the target language differing only in a few minor aspects of phonology and lexical items, and the "mesolect" which encompasses all of the intermediate creole varieties (Rickford, 1987: 288). According to Sankoff:

What happens during creolization, including the type and amount of linguistic variability, is of course a function of several factors: (a) the state and stability of the pidgin, or pre-pidgin continuum; (b) the accessibility of the original target; (c) the salience of the substrate languages; and (d) the relative social integration of the creolization community. (p. 155)

For example, Martinique and Haiti are often cited in the literature as places where a much firmer boundary exists between the standard language and the creole (Rickford, 1987: 22), but that is a result of the sociological factors mentioned above being much different than those found in the Caribbean, where the boundary is much less distinct.

Where the creole continuum situation obtains, both Rickford and Richard Day have developed models of this linguistic phenomena. Rickford applies Labov's property-item matrix as a relevant model of a creole continuum:

Property-Item Matrix for Continuous Transition Between Categories

<table>
<thead>
<tr>
<th>Property</th>
<th>Item</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
</tr>
</thead>
<tbody>
<tr>
<td>a</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>+</td>
</tr>
<tr>
<td>b</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>-</td>
</tr>
<tr>
<td>c</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>d</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>e</td>
<td>+</td>
<td>+</td>
<td>+</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>f</td>
<td>+</td>
<td>+</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>g</td>
<td>+</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>h</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
</tbody>
</table>

Source: Adapted from Labov (1973:344) (Rickford, 1987: 16-17)

Although some item a is clearly unique to Category X and item h is clearly unique to Category Y, the other items b-g share properties of both categories; therefore, a continuous transition, rather than a discrete boundary, exists between the two categories (Rickford, 1987: 17). When applying
this matrix to a creole continuum situation, Categories X and Y represent the basilectal creole variety and the target language. Items a-h would be speech samplings from the area under investigation which would be compared against grammatical, phonological, and morphological properties of the two languages. If such overlapping of properties was found to occur in the speech sampling, then the linguistic situation of that area would be a creole continuum. Richard Day uses a model of a string of overlapping circles to represent the creole continuum where the circle at one end is the basilect, the circle at the other end is the standard language, and the circles in between are the mesolectal varieties (Day, p. 39):

The circles in the model overlap in a continuous string, just as certain features overlap in creole varieties along the continuum. Less complex than the property-item matrix, this model emphasizes the general idea of feature-sharing rather than the specifics of it.

One final point on pidginization and (de)creolization should be mentioned before returning to the discussion of the historical development of VAAE. It is not necessarily the case that an actual pidgin will crystallize when pidginization occurs (Sankoff, 1980: 154). In other words, given the circumstances in which pidginization occurs, i.e. an urgent need for communication between socially dislocated speakers of different languages, some communication can occur without a complete set of grammatical rules developing into a fundamental language system. Concurrent with that notion, creolization can occur even without, or before, the "crystallization" of a pidgin (Sankoff, 1980: 154). As Sankoff explains it, "the object to be transformed during natization need not be a particularly 'effable' pidgin at all; native speakers take whatever is at hand as raw material"
(Sankoff, 1980: 154). With respect to the decreolization hypothesis for VAAE, this is significant in that great disagreement exists about the nature of the original pidgin the African slaves brought with them from the Old World, or even if they spoke a pidgin at all. If Sankoff’s claim is correct, this particular point of debate does not necessarily disprove the decreolization hypothesis since the possibility of creolization could not be ruled out entirely even if no pidgin ever existed in the linguistic competence of the African slaves.

On the subject of the original pidgin supposedly spoken by the African slaves, many varying viewpoints can be found. As previously stated, McCrum, et al., claim that the slaves learned Sabir while being retained in stockades in the West African slave factories and while being transported in slave ships to the Caribbean and North America. William Stewart, on the other hand, maintains that an English-based pidgin was the basis of the slaves’ early communication. Using evidence from literary sources, Stewart claims that pidgin English was spoken without huge variation across a wide area of the New World (Stewart, 1970: 359). He cites an example of pidgin English spoken by the character of a 14-year-old slave in Daniel Defoe’s 1715 novel, The Family Instructor, the setting of which is the West Indies. He then compares this dialogue to one in John Leacock’s 1776 play, The Fall of British Tyranny, in which a “Negro” from Virginia speaks, and to a description, written by J.D. Herlein in 1718, of the Dutch colony, Surinam, in South America, a chapter of which contains samples of the English dialect spoken by the African slaves there (Stewart, 1970: 358-9). Based on the similarities of the pidgin English in all three sources, Stewart concludes that, since the pidgin English spoken by Africans in the New World was very similar in such widespread locations, the African population must have learned this pidgin English “as a lingua franca in the trade centers and slave factories on the West African coast,” transporting it with them to the New World where it became a plantation lingua franca (Stewart, 1970: 359).

Joe Dillard provides a possible history of the linguistic background of the trading industry which could reconcile this apparent discrepancy between the use of Sabir or the use of pidgin English. Using two written sources from 1556, Dillard cites statements indicating that the English relied upon the Portuguese language in trading and colonizing abroad (Dillard, 1979: 261). The

Keith Whinnom in “Creolization In Linguistic Change,” 1980, questions such broad definitions of pidginization and creolization. In particular, he seems to object to equating pidginization with “any imperfect second-language acquisition” and creolization with the “primarization of any secondary language.” He would prefer to restrict the definition of pidginization to the process which “ultimately produces a stabilized pidgin” and creolization to “the primarization of an incipient, stabilized or expanded pidgin” (pp. 203-4).
English, in fact, commonly relied upon Portuguese to communicate in areas all over the globe where Portuguese traders had gone before them, West Africa included (Dillard, 1979: 262). For West Africa, specifically, Dillard cites a 1738 account by Francis Moore, among other seventeenth and eighteenth century sources, to show the presence of an established Portuguese pidgin or creole:

> When this country [the Gambia River area] was conquered by the Portuguese, which was about the year 1420, some of that Nation settled in it, who have cohabited with the Mandingoes till they are now very near as Black as they are; but they still retain a sort of bastard Portuguese language call’d Creole.. (Dillard, 1979: 263).

As the English came into more and more contact with these Portuguese descendents and native West Africans, their speech gradually came to be influenced by these groups. Again from Moore:

> The English have in the River Gambia much corrupted the English by words or Literal Translations from the Portuguese or Mundingoes [native Africans]. (Dillard, 1979: 264)

From such accounts, it seems likely that “WAPE [West African Pidgin English] supplanted ‘Negro-Portuguese’ as the lingua franca of the West African coast in the nineteenth century” (Dillard, 1979: 264). To provide evidence of an “Afro-Portuguese/English trade language among the coastal Africans themselves,” Dillard cites a report of a 1682 voyage by John Barbot which, in many places, describes the Africans as speaking Portuguese and/or Lingua Franca, and occasionally English or Dutch, all languages presumably being some kind of pidgin or creole of the standard (Dillard, 1979: 264). Dillard claims that the Lingua Franca mentioned by Barbot is none other than Sabir (p. 265). Thus, the possible reconciliation of McCrum, et al., with Stewart is that Sabir and pidgin, or creole, Portuguese influenced the pidgin English which was eventually adopted as the primary means of communication by the nineteenth century. As Dillard explains:

> It would be obviously unwise to overstate current knowledge about the language of those early seventeenth century slaves -- to attempt a reconstruction, for example. But we can at least see the general pattern into which they fit. Mixed as to tribe and African language, they resorted to those contact languages which were widespread in other parts of the maritime trade as in slavery. (p. 267)

Since Sabir and pidgin Portuguese would still have been in use during the early years of the slave trade, McCrum, et al., are not necessarily incorrect in their assertion that Sabir is the distant ancestor of VAAE. On the other hand, since pidgin English gradually replaced these other languages as the primary means of communication, Stewart is not necessarily wrong in his assertion that it was a pidgin English which the African slaves brought to the New World.
Neither view is necessarily accurate, however, if Rickford is correct in his assertion that most slaves probably did not acquire any knowledge of any pidgin before their transportation to the New World. With respect to the possible acquisition of a Portuguese pidgin by slaves in the early history of the slave trade, Rickford cites an account of “the life and trade on the Gold, Slave and Ivory coasts in the last decade of the seventeenth century,” written by William Bosnam, the Chief Factor of the Dutch West India Company. In his account, he often reports of the difficulty in trying to communicate with the “Negroes” and only once does he mention an African slave who speaks Portuguese and English pidgins (Rickford, 1987: 47). Since Bosnam’s description pertains to the port of departure of slaves for the Guiana colonies, Rickford concludes that most slaves did not speak any language but their native African one when they were shipped out to the Americas (Rickford, 1987: 47). Besides this account by Bosnam, Rickford also discusses the social stratification in West Africa during this period as another reason why most African slaves would not have learned any European-based pidgin. He points out that in the historical documents from this period, the speakers of Portuguese pidgin are either Portuguese seamen and traders, or “Africans linked to them as castle slaves, hired hands, . . . or middlemen” (Rickford, 1987: 48). Rickford claims that only these Africans who were employed by or trading with the Europeans would have had both the motivation and the opportunity to learn some European-based pidgin in West Africa; the great majority who ended up as “sale slaves” would not have (Rickford, 1987: 48-9). Furthermore, Rickford argues that the conditions in both the West African slave factories and the slave ships were such that linguistic interaction between the slaves and European-based pidgin speakers would not have been sufficient for the slaves to have acquired the pidgin themselves:

It is difficult to see how a significant proportion of a contingent of three hundred or four hundred manacled slaves could have successfully acquired the lingua franca of guards and sailors who were physically separated from them most of the time by dungeon walls or ship decks. But I wish to focus attention as well on the sale slaves’ likely preoccupation with physical survival and especially their emotional tenor and abhorrence of their enslavers. After all, consider who the speakers of the Portuguese lingua franca were from the sale slaves’ point of view: the European officials who were branding, examining, and manhandling them, the African kings and middlemen who were trading in them, and the grumetoes and castle slaves who were helping to ensure that they did not escape. (p. 49)

As evidence of the inimical relationship between the sale slaves and castle slaves/free Africans, Rickford cites examples where the second group of Africans had helped in recapturing escaped slaves and in putting down slave rebellions (Rickford, 1987: 49-50). From these various pieces of
evidence, Rickford concludes that the African slaves during this time period were too separated, both by physical constraints and psychological aversion, from all the speakers of pidgin Portuguese to have acquired it themselves (Rickford, 1987:50). Based on similar reasoning of the continued hostility and separation of the slaves from their captors, and from further documentary evidence from the early eighteenth century that slaves spoke only their native African language (such as accounts of slaves in dire need to communicate with a captor using only hand gestures), Rickford also concludes that slaves shipped from West Africa during the later time period when the English pidgin was predominant would not have acquired this pidgin prior to their arrival in the Americas (Rickford, 1987: 53-54).

Comparing the arguments of Stewart and Rickford, Rickford’s argument, in the end, appears more compelling. The documentary evidence of Dillard rather convincingly proves that first Sabir and Portuguese pidgin, then English pidgin dominated the trading industry between the Old World and the New. His evidence does not prove, nor even mention for that matter, that African slaves spoke any of these pidgins. His citations from Barbot make mention of pidgin proficiency among what Rickford has classified as castle slaves and free Africans, not among the actual sale slaves. So Dillard neither proves nor disproves either argument. In comparing Stewart’s literary evidence directly with Rickford’s, it should be noted that Stewart’s sources are all fictional while Rickford’s are all non-fictional. While one must be cautious in trusting any second-hand documentary evidence of this kind, one might be tempted to throw a little more weight to the non-fictional accounts simply because the intent of the author might be more inclined to produce reliable data. Finally, Rickford’s arguments against the interaction of the slaves and their captors seems believable, although neither linguist produces any actual evidence about language interaction in the slave factories or the slave ships. In short, while both arguments seem tenable, Rickford’s seems to have a slight advantage.

In the introduction to Language Variety in the South, Montgomery and Bailey cite Constance Weaver’s 1970 dissertation from Michigan State University, “Analyzing literary representations of recent northern urban Negro speech: a technique with application to three books,” saying, “writers of fiction are unreliable in their treatment of variable phenomena, and most of the features usually associated with black speech are variable rather than categorical” (p. 14). However, Montgomery and Bailey also caution against taking the non-fictional accounts of laymen at face value as well. They are, in fact, critical of Dillard’s use of such sources in that they find Dillard calls into question the reliability of such sources when they do not support his argument, but accepts others at face value when they do (p. 14). Montgomery and Bailey conclude, “inconsistency in handling observations of such laymen, along with the difficulties in using literature are major problems in creolists’ reconstruction of the history of black speech (p. 14).” In other words, they don’t put much stock in either kind of evidence.
If modern VAAE did not develop from the decreolization of the English trading pidgin, however, an alternate source must be found. Some linguists have argued that it has its origins in Irish English (Bailey, 1982: 237), or, more generally, in British regional dialects (Montgomery and Bailey, 1986: 10). One reason for this postulation is that, during the seventeenth and eighteenth centuries, large groups of Irish bond-servants arrived in the West Indies who would have had close social contact with the African slaves (Rickford, 1974: 106-7). Furthermore, many Scotch-Irish “Ulster-Scots” settled in the Southern United States as indentured servants and farmers, often becoming overseers of the African slaves on the large plantations (Traugott, 1972: 191). One of the most frequently cited pieces of linguistic evidence for a relationship between Irish English and VAAE is the similarity between the use of (does) be as a habitual marker in both languages (Rickford, 1986, p. 246). A possible British source of this habitual be is the Old English beon, “the be-verb that often indicated duration, that after being used in that durative sense with some frequency in OE, . . . submerged into a lower class or lower middle class social dialect, was brought to this continent [North America], and is now being observed for the first time [in VAAE] (Williams, 1986: 276).” More will be said about habitual be later; what is significant here is that it is frequently cited as a link between Irish or British dialects and VAAE. Traugott also points to the fact that multiple negation, which is obligatory in VAAE, appears optionally in some current non-standard white dialects, and was also optional in earlier standard forms of English (Traugott, 1972: 194). Additionally, Traugott argues that constructions in VAAE such as “You must didn’t read it too good,” could have their origin in Middle English which allowed a modal to be followed by another modal or by do (Traugott, 1972: 192-3). Beyond these syntactic features, there also seem to be a few phonological peculiarities which appear in both Irish English and VAAE such as “over palatalized dorsals in *card* and *guard*” which is also cited as evidence of a connection between the two languages (Bailey, 1982: 238). On the basis of these few similarities, in fact, Bailey challenges the creolists to come up with a better, more unified explanation: “They [the creolists] certainly cannot argue that Irish English COULD NOT have been the source of vernacular black English be, if the phenomenon first appeared in an area where fifty thousand Irishmen were resident, unless they give proof that the phenomenon grew up earlier in Africa; and we then need to know the exact provenience of it (p. 238).”

Martha Baudet attempts to prove exactly such African origins in her preliminary study of
common West African grammatical forms. Baudet agrees with Rickford that slaves transported to the New World probably did not speak a Portuguese pidgin but claims "there is no need to assume direct Portuguese pidgin influence if we can adequately account for the development of the Caribbean creoles without making such a strong assumption" (Baudet, 1981: 106). In this, she seems to be following Sankoff's idea that creolization can occur even when no pidgin ever crystallizes. Baudet claims that, because the African slaves were not socially integrated into the European community, they would have learned the target language imperfectly, relying more on grammatical forms from their native languages (Baudet, 1981: 107). This is, in effect, the substratum theory:

... when a group of speakers of a definite language of higher prestige managed to impose its language upon a larger mass of speakers of another language ... the speakers of language B would take over language A with some changes that reflect features of the phonology and the grammar of their own language. (Polome, 1980: 185)

Thus, Baudet is looking for evidence of a substratum of West African languages in present-day Caribbean creoles. To that end, she has done a preliminary study comparing two Caribbean creoles (Haitian and Jamaican) and four West African languages (Ewe, Yoruba, Igbo, and Twi), using one native speaker of each language (Baudet, 1981: 108). In her study, she found remarkable similarities between the languages in these grammatical forms: "the structure and word order of determiners and nominalizers; genitives; comparative constructions; serial verbs; distributive constructions; and coordinators" (Baudet, 1989: 108). Baudet argues that, although her sampling is much too small to make definite conclusions from, similarities in these grammatical forms are an especially good indication of an African substratum in Caribbean creoles because none of these forms are a common output of "universal strategies of pidgin/creole formation" and they are also generally uncommon from a global perspective of languages (Baudet, 1981: 114). For these reasons, it is unlikely that these particular forms would have manifested themselves in Caribbean creoles independently without some kind of African substratum influence.

Given Baudet's preliminary findings, it seems likely that definite links between Caribbean creoles and West African languages could be established through further research, but definite links between Caribbean creoles and VAAE must also be established for studies of West African languages to have any relevance to the study of VAAE. Dillard proposes that a relatively uniform creole, which he labels Plantation Creole, was spoken by African slaves in North America. To
prove the existence of this widespread creole in North America, he first cites numerous literary sources dated as early as the eighteenth century and representing many different states which contain examples of African Americans speaking a pidgin-like form of English (Dillard, 1972: 73-138). He also argues that the social interaction among slaves on Southern plantations would have been conducive for the development and continuation of a uniform creole. More specifically, he argues that, although the majority of plantations were small ones on which slaves would have been outnumbered by whites, there was sufficient social interaction between slaves living on different small plantations for a Plantation Creole to develop (Dillard, 1985: 109-112).

Since smallness of plantations tended to promote interaction and personal interchange between plantations, it is likely that dialect leveling between plantations was greater in the small plantations. Since the interaction patterns were largely between Blacks, it is no surprise that a relatively uniform Black dialect should develop (Dillard, 1985: 112).

To support his claim, Dillard quotes numerous ex-slave narratives which recount instances of interplantation contact among slaves under various circumstances (Dillard, 1985: 110-112). Dillard argues that the frequency and intimacy of contact between slaves on different plantations is much more crucial than the demographics of small plantations (where whites outnumbered slaves) in terms of the linguistic behavior of the slaves and that the nature of their social contacts would have favored the development and maintenance of a relatively widespread, uniform Plantation Creole (Dillard, 1985: 113).

A study by Edgar W. Schneider refutes Dillard's claims, at least in part. Schneider examined specific syntactic structures in ex-slave narratives collected by the WPA in the 1930's. Although these narratives are not ideal linguistic data since they were, for the most part, merely written down by WPA workers as the ex-slaves spoke, with very few narratives actually being tape-recorded (given the technological constraints of the 1930s). Still, this corpus is one of the best examples of what Schneider calls Earlier Black English. Schneider finds that, of the twenty syntactic structures he examined in EBE, only four are substantially different from modern VAAE: (1) the plural -s suffix is used more frequency in EBE, (2) both unmarked verbs in past contexts and present tense verb forms ending in -s are found in EBE, but not in VAAE, (3) some forms of EBE contained the perfective verbal construction is/had done pp which is not found in VAAE, and (4) us was occasionally used as a subject pronoun in EBE but is never used this way in VAAE (Schneider, 1989: 273). Schneider argues that, if the creole hypothesis is correct, then EBE should be more
closely related to Gullah than modern VAAE, yet, of the four syntactic features in which EBE and VAAE differ, only one links EBE to Gullah more closely than VAAE:

Proceeding from the four differences between EBE and Black English [VAAE] noted above, we have to conclude that in only one of these cases does EBE stand somewhat closer to Gullah than to the present dialect, namely with respect to the group of unmarked past tense verb forms. In Gullah, however, all verbs do not receive morphological marking of the past tense, whereas in EBE this applies to only 10 to 20 percent, and there is not Gullah equivalent of the past verbs ending in -s. ... With respect to the other differences noted above, EBE is even further apart from Gullah than Black English is. The verbal -s suffix, perfective structures such as had done pp, and, remarkably, also the pronoun us ... are not elements of the grammar of Gullah. (Schneider, 1989: 275).

Since EBE does not appear to be more closely linked to Gullah than VAAE is, based on the syntactic features studied, as well as the fact that EBE and VAAE share syntactic features which are not present in Gullah (such as the progressive aspect suffix -ing, the plural suffix -s, and the genetive suffix -s, to name a few) (Schneider, 1989: 275), Schneider’s evidence does not support the creolist position that VAAE is part of the Carribean creole continuum.

Schneider does not completely reject the creolist position but modifies it considerably. In his final summation of his study, he divides the features of EBE identified in the study into four categories: features which are “exclusively British-English in origin”, features “for which models exist both in dialectal British English and in African or creole languages”, features which are most likely influenced by an African substratum, and features which are “creole survivals with no close structural analogues in British regional dialects” although none of the features in this last category appear with any regularity in EBE (Schneider, 1989: 277). Furthermore, a clear majority (sixteen, in fact) of the twenty syntactic features studied fall in the first category, leading Schneider to conclude that EBE and, subsequently, VAAE, are predominantly descended from “nonstandard British and American English of the colonial period” (Schneider, 1989: 277). As for the creole forms identified in the study, Schneider hypothesizes that they are the result of “a few presumably independent creolization processes in localities with an exceptionally dense black population and on some big plantations” since these forms are, for the most part, idiosyncratic and not central to EBE grammar in general (Schneider, 1989: 277-8). While Schneider does acknowledge limited creolization in the history of Black English, he does reject the possibility that a uniform Plantation Creole ever existed, not only because EBE does not resemble a creole variety according to his data, but also because of the amount of variation found in the speech of the ex-slaves studied.
(Schneider, 1989: 278). Still, Schneider cautions against any monolithic theory of the origins of VAAE, claiming no such theory can adequately cover the complexities of the social and linguistic situation of plantation culture (Schneider, 1989: 278).
Invariant *BE*:

This discussion will focus on the grammatical feature of VAAE known as invariant *be* for the simple reason that, as John R. Rickford puts it, “No single feature of BE [VAAE] has received as much attention in the literature as the use of ‘Invariant *be*’” (Rickford, 1974: 96). Among linguists who study VAAE, invariant *be* is often cited as a unique feature of the dialect, but much disagreement exists over its actual grammatical structure and function (Baugh, 1983: 70). It is helpful, then, to first examine what structures do indeed constitute “invariant *be*” in VAAE.

Labov has labeled invariant *be* “*be*₂” in order to distinguish it from *be*₁, which is “the ordinary finite *be* which alternates with *am*, *is*, *are*, etc.” (Labov, 1972:51). In other words, invariant *be* is a separate structure which must be differentiated from “present tense concord” uses of the *be* verb in VAAE grammar which occur in the same instances of regular present tense *be* in SE (Fasold, 1972: 150). This invariant *be* must also be differentiated from occurrences of *be* in SE such as imperatives:

Be quiet!

infinitival constructions:

He wants to be president.

subjunctive:

If this be treason . . .

and the buried imperative:

If you don’t be quiet, I'm going to spank you. (Fasold, 1972: 152-3)

So where does this invariant *be* appear in VAAE? With respect to syntactic environments, according to Baugh, it is found with noun phrases:

They *be* the real troublemakers.

adjectives:

He *be* crazy when he’s been drinkin.

locatives:

They don’t *be* on the streets no more.

and with Verb + *ing*:

They *be* partying hard when Friday night rolls around. (Baugh, 1983: 73-4).

Wolfram, 1969, cites a similar distribution. To these environments, Fasold adds past participles:
Well, they *be* mixed up all kinds of way. (Fasold, 1972: 151)

With respect to the semantics of invariant *be*, there is greater disagreement. Many studies, however, do cite a habitual or durative marking function for this structure (Baugh, 1983: 70). Specifically, Labov cites its use as “indicat[ing] ‘habitual’ behavior: durative or iterative depending on the nature of the action (Labov, 1972: 51).” Fasold cites its use as “involv[ing] repeated but not continuous occurrence (Fasold, 1969: 763).” To prove the existence of this usage, both Fasold and Wolfram cite the co-occurrence of *be* with Crystal’s “frequency of occurrence” adverbs and with “when clauses” meaning “whenever”. Fasold finds that *be* occurs in these environments 36.1% of the time in his data, which he finds particularly significant in light of the fact that present-tense concord forms of *be* in his sample co-occurred in these same environments less than 1% of the time (Fasold, 1969:766-7). Wolfram finds similar results in his data: 48 out of 184 examples of *be* co-occur with frequency of occurrence adverbs and 17 out of 62 examples of “when” clauses also co-occur with *be* (Wolfram, 1969: 181-2). Based on these findings, Wolfram and Fasold concur that this use of *be* involves predication which is repeated but not continuous over time (Wolfram, 1969: 182).

Based on their 1968 study of VAAE in New York City, Labov, et al., describe the use of invariant *be* in different terms from Wolfram and Fasold, but only seriously contradict the other analysis in one specific area. Labov, et al., claim that this “non-future” use of *be* has many components, the most common being: “‘general’ (‘general conditions extending over a period of time, usually indefinite in force’ (Labov, et al., 1968: 231)), ‘iterative’ (‘where an event occurs habitually’ (Labov, et al. 1968: 231)), and ‘indefinite’ (‘an indefinite durative sense, or more precisely, a state of affairs’ (Labov, et al., 1968: 232))” (Wolfram, 1969: 192-3). Wolfram claims that none of these “components” ascribed to *be* by Labov conflict with his and Fasold’s view of distributive *be* (Wolfram, 1969: 193). In fact, in a later paper, Labov states that “the ‘habitual’ and ‘iterative’ meanings of *be* form the core of the semantic complex involved ... (Labov, 1982: 198).” This view seems similar to the characterization of *be* as marking non-continuous or intermittent states and actions over time which Wolfram and Fasold advocate.

The specific case which Labov, et al., cite that does contradict the “distributive” analysis of *be* is the case where *be* refers to “an instant state of affairs” (Wolfram, 1969: 193). Three examples of such a usage are given:-
(1) We shake hands. And that be₂ it.
(2) The last guy who be₂ picked, they IT.
(3) If he hit me....
He probably just hit me, 'cause he be mad right then; you know he wouldn’t hit me otherwise.
(Wolfram, 1969: 193)

Wolfram dismisses the first two examples by claiming that the events described can be seen as states which occur at different time intervals: “For example, ... whenever one plays tag, an activity which occurs at various intervals in a child’s experience, the child who is tagged last is ‘it’ (Wolfram, 1969: 194).” The third example Wolfram labels an example of would deletion³ claiming an “alternate realization” of the third example might be:

He’d probably just hit me ‘cause he’d be mad right then; you know he wouldn’t hit me otherwise.
(Wolfram, 1969: 194)

He also notes the presence of would in the following sentence as further evidence (Wolfram, 1969: 194).

For the most part, Wolfram’s rebuttal is merely an alternate interpretation of the semantics involved, and therefore not very strong counter-evidence. Without further contextual clues, it is not possible to summarily dismiss Wolfram’s claim that the events described in (1) and (2) occur repeatedly and intermittently in the experience of the speaker. That is, it is possible that the speaker of (1) is involved in some kind of occupation or other activity where the shaking of hands occurs often, and it is possible that the speaker of (2) often plays the game of tag. However, it is at least as likely that the speaker of (1) was describing an event which occurred only once and that the speaker of (2) was describing either a particular event during a game of tag, or describing a characterization of the game which is always (not intermittently) true. Without further information, neither claim can be rejected.

In the case of sentence (3), however, Wolfram’s analysis seems clearly false. He claims that the presence of wouldn’t in the next sentence indicates the presence of a deleted would in the previous sentence. This claim is not borne out by the meaning of the discourse itself. It is clear that the event of hitting described in the first sentence actually occurred, while the second sentence is conjecture of what would happen given alternative circumstances which did not actually occur. In other words, the aspect of the two sentences is clearly different, and so are the aspectual

³ Would deletion has not yet been discussed in this paper but will be addressed shortly. Basically, it is an occurrence of invariant be which derives from would be, where would is contracted to ’d and then lost altogether.
markers in each. Having dismissed the possibility that the invariant *be* in this sentence is a result of *would* deletion, it is impossible to ignore the fact that it co-occurs with the adverbial *right then*, a clear indication of an instantaneous state of affairs.

Another study by Guy Bailey and Marvin Bassett also questions Wolfram's and Fasold's description of "distributive *be*". They analyzed the data from the *Linguistic Atlas of the Gulf States*, specifically the responses of the informants native to East Louisiana, Gulf Mississippi, and Lower Mississippi (Bailey and Bassett, 1986: 159-60). Of the 106 examples of invariant *be* identified, forty-seven were of the type corresponding to Fasold's and Wolfram's category of "distributive *be*" (Bailey and Bassett, 1986: 164-5). Bailey and Bassett then independently rated each example "on a three-point scale of time reference (definite, intermittent, and continuous/stative)" (Bailey and Bassett, 1986:165). When they compared results, they found that they only disagreed in four cases which they labeled "ambiguous". Of the remaining examples, twenty-two (51%) were found to refer to continuous actions or permanent states, eighteen (40%) were found to refer to actions distributed over time, and four (9%) were found to refer to actions taking place at a definite point in time (Bailey and Bassett, 1986: 165).

Comparing the evidence of Labov, et al., and that of Bailey and Bassett, it appears that not all cases of invariant *be* do, in fact, have a "distributive" marking function as Fasold and Wolfram claim. Based on the fact that, within the LAGS corpus, more cases of invariant *be* mark continuous actions and permanent states than actions distributed over time, Bailey and Bassett argue that "extended and permanent references are also a part of the [semantic] core" of this *be* form (Bailey and Bassett, 1986: 168). Although Wolfram might be inclined to merely explain away the examples of Bailey and Bassett through an alternate interpretation of the semantics, it seems advantageous to accept these additional components into the "semantic core" of invariant *be* based on the difficulties which arose for Fasold and Wolfram from two rather "sticky" examples found by Fasold and commented upon by Wolfram.

Within Fasold's data, he found the sentences:

Every time I get in a fight, they be smaller than me.
Some of them be big and some of them be small [description of sting-ray bicycles]" (Fasold, 1969: 764).

In order to fit the semantics of these two sentences into the distributive marking which Fasold claims for invariant *be*, he posits this explanation:
Usually we can say that be characterizes predicates which are distributed over points in time, but occasionally it is the SUBJECT and not the predicate which is so distributed. In the sentence *Every time I get in a fight, they be smaller than me*, it is not that the predicate *be smaller* is true of the same persons at different points of time, but that there are various people at different times all of whom are smaller than the speaker. (Fasold, 1969:764)

In other words, Fasold is claiming that, while the state of being small is continuous for the people mentioned in the predicate, the times when the subject experiences the smallness of these various people is distributed intermittently over time. Thus, from the perspective of the speaker, the “distributive” sense of *be* is maintained. From the viewpoint of simplicity, which is the goal of any linguistic description, it seems more advantageous to accept the broader semantic core of Labov, et al., and Bailey and Bassett, than to allow the reference of *be* to “flip-flop”, so to speak, from predicate to subject, depending on the particular example at hand, as Fasold and Wolfram have opted to do. Furthermore, Fasold’s solution only accounts for apparent cases of continuous states, and not for the examples of actions taking place at a definite point in time, which both Labov, et al., and Bailey and Bassett have found in their data. Following the solution of Bailey and Bassett, the semantic core could simply be expanded further to account for these examples as well.

In short, it is obvious from the above discussion that defining the precise semantic force of invariant *be* is a difficult process. Often, it relies on the personal interpretation by the linguist of each particular example, a method which is not highly reliable especially in the absence of additional cues (adverbs of time, *when* clauses, etc.). Even though the semantic core has not yet been satisfactorily defined, however, it is clear that invariant *be* in these cases is an aspectual marker of some sort, and, until the debate on the exact aspectual category is resolved, most linguists refer to this type of *be* as distributive, habitual, and/or durative.

All the examples of invariant *be* discussed thus far have been in the present tense, but this is not the only environment in which invariant *be* is found in VAAE. It also occurs in sentences such as:

I be 12 February 7. (uttered the preceding July)

in which the event described is a single occurrence which will take place sometime in the future (Wolfram, 1969: 183). Wolfram, Fasold, and others agree that this use of *be* has a different underlying source than the distributive/durative/habitual *be* described above. More specifically, they argue that *be* occurring in a future, single-occurrence context comes from the deletion of *will*.
in some cases and from the deletion of would in others (Wolfram, 1969: 183-4; Fasold, 1969: 768-69). There are several arguments to support this view.

First of all, the contexts of this kind of invariant be clearly differ from those of habitual/durative be as already mentioned. Second:

In negative sentences, contraction of will and would (in SE), and therefore their deletion (in VAAE), is not possible, so that distributive be is clearly distinguishable from will be and would be, yielding sentences like the following:

I know I won't be able to get what I ask for, so what's the difference? (10)
It was just the way that she did things that made me think that, you know, that she wouldn't be a nice person. (90)
Well, see, I don't be with them all the time so I can't pick out one specific leader. (25).

The presence of the three different negative auxiliaries shows the three different sources of these examples: will not be, would not be, and do not be.

Third, Fasold cites an experiment conducted upon 31 black, working class informants from Detroit by the Sociolinguistics Program of the Center for Applied Linguistics. This study was designed to elicit short affirmative statements of the form I know + Pronoun + Auxiliary because forms such as will and would cannot be contracted in these types of statements. Therefore, the response of the informant would show which auxiliary s/he saw as present in the preceding statement. A sample of the test is as follows:

Tape: John can climb that tree.
Informant: I know he can.
Tape: Can what?
Informant: Climb that tree.

The four sentences in the test which dealt with be were:

(1) If he got a walkie-talkie, he be happy.
(2) He be in in a few minutes.
(3) Sometime Joseph be up there.
(4) Sometime my ears be itching.

The hypothesis was that sentence (1) was an example of would deletion, sentence (2) an example of will deletion, and sentences (3) and (4) examples of habitual/durative be (in which case, the auxiliary should be do instead of will or would). The results of the experiment did support this hypothesis: 21 informants produced would for the first sentence; 25 produced will for the second; 11 produced do for the third; and 14 produced do for the fourth. (Fasold argues that the do froms
were produced less successfully because it is a stigmatized form of VAAE not found in SE.) (Fasold 1969:770-1).

Finally, it is not necessary to formulate specific phonological rules to account for will and would deletion. In their contracted forms, will and would are reduced to 'll and 'd. In Labov's analysis of VAAE, he finds among its phonological rules, a rule which deletes -t and -d, which would account for would deletion, and a rule which deletes postvocalic l, which would account for will deletion (Labov, 1972: pp. 111-2). Thus, this explanation of be is strengthened by the fact that it falls into more general phonological patterns of VAAE without requiring specific rules of its own (Wolfram, 1969: 184-5).

While this analysis is widely accepted, not all linguists agree upon the separation of invariant be into two underlying sources. Loflin, for example, analyzes all cases of invariant be as a marker of "a-temporal tense" (Wolfram 1969:188). Loflin derives this analysis from the fact that be occurs in discourse contexts which are not specified for time-frame but could be future, present, or past depending on the time adverbs which be co-occurs with. He cites these examples:

(1) They be here tomorrow.
(2) They be here all the time.
(3) They be dancing with each other. (In this last example, the context implied past time; the informant said he went to a party where he sat out the dances in a corner; I asked him what the other people did there. Whereupon he replied [3].

Loflin calls (1) future, (2) present, and (3) past, thereby showing the occurrence of his a-temporal be in three time frames (Wolfram, 1969: 188). Wolfram rejects Loflin's analysis for two reasons. First, Wolfram claims that, by ignoring the evidence from negative and elliptical verb formations where different underlying auxiliaries appear, Loflin is failing to recognize that the grammar of invariant be is a case of neutralization, "the relationship in which one lower level unit can be the realization of two or more underlying units (Wolfram, 1969:189)." Also Wolfram claims that Loflin's example of the occurrence of be in the past is not unambiguous; if Loflin had offered examples of be co-occurring with an adverb such as yesterday or last year, his argument would be much stronger4 (Wolfram, 1969: 189-90).

So, it would appear that the analysis of Fasold, Wolfram and others which divides the occurrence of invariant be in VAAE into habitual/durative be and be from will or would deletion is

---

4Later studies such as Bailey and Bassett, 1986, and Loflin, et al., 1973, have found such examples.
the most promising. When it comes to describing the underlying deep structure of this grammatical form, however, Fasold and Wolfram are divided. Fasold argues that invariant be in VAAE is the same lexical item as occurrences of concord be in SE. The only difference is that VAAE allows nonmodal main verbs to occur without tense whereas tense is required of such main verb phrases in SE (Fasold, 1972: 177). In other words, VAAE and SE share sentences with modal main verb phrases such as:

They might be working.

and sentences with infinitivals such as:

They want to be working.

but diverge on sentences with nonmodal main verbs. The equivalent of the VAAE sentence:

They be working.

in SE is:

They are working.

SE cannot produce sentences such as:

They be working.

because SE requires tense to be marked on the main verb. VAAE does not require tense in any of these constructions according to Fasold (Fasold, 1969: 774).

There are some problems with Fasold’s analysis. First, invariant be can occur with do, which presumably carries tense. Fasold tries to account for this by claiming that do is a pro-verb which appears in every sentence and is deleted under certain conditions (Fasold, 1969: 775). However, when do occurs with verbs other than be, Fasold has claimed it carries the tense non-past. The only way to get around this problem is to claim that when do appears as an auxiliary of be, it is tenseless, but, in its other occurrences as a main verb or auxiliary to other verbs, it carries nonpast tense (Fasold, 1972: 178).

Even if Fasold’s analysis of do is accepted, Wolfram points to the fact that Fasold’s whole theory is rendered invalid if be occurs with did or didn’t which is clearly marked for past tense (Wolfram, 1969: 196). All of the possible examples of the occurrence of invariant be in the past in Wolfram and Fasold are ambiguous and not clearly past tense. Dillard, however, claims that invariant be does co-occur with the construction was Ving which would clearly be an instance of past tense (Dillard, 1985: 119). Unfortunately Dillard does not provide any examples to support
his claim. In a different study conducted with Loflin and Sobin, however, Dillard claims to have elicited the sentence:

He be makin’ hats yesterday. (Loflin, Sobin, and Dillard, 1973: 26)

It is clear that at least the possibility of the occurrence of be with the past tense exists, which puts Fasold's analysis, as it stands, in jeopardy.

Wolfram asserts that his analysis does not suffer from the same shortcomings (Wolfram 1969: 196). Wolfram sees the invariant be formed through will or would deletion as equivalent to the structures will be and would be in SE, but claims the habitual/durative form of invariant be has no exact equivalent in SE (Wolfram, 1969: 186). As evidence for his claim, Wolfram points out that both of the following sentences are grammatical in SE:

- Sometimes he’s busy.
- He’s busy right now.

In VAAE, only the first case is grammatical using the invariant be construction:

- Sometimes he be busy.
- He be busy right now. (Wolfram, 1969: 186-7)

The VAAE sentence:

He busy right now.

would be the closest equivalent to the SE version (Wolfram, 1969: 187). Although Labov, in general, finds the differences between SE and VAAE grammar to be differences in low level rules, in the case of invariant be, he sides with Wolfram, claiming it is part of VAAE deep structure which is not present in other English dialects (Labov, 1972: 55).

Perhaps an examination of the possible historical development of this particular verb form will shed some light upon its present structure in VAAE grammar. Joseph M. Williams wants to see a British source for invariant be but does not present very convincing evidence for his claim. As he himself admits, the use of invariant be in the southern British dialect of Modern English as in:

- We be ready to leave.
is merely an equivalent of is and are in Southern British English; it does not convey the same sense of habituality marked by VAAE habitual/durative be (Williams, 1975: 276). A slightly better candidate offered by Williams is Old English beon:

It may be that the durative be has its origins in the OE beon, the be-verb that often indicated duration, that after being used in that durative sense with some frequency in OE, it submerged into a lower class or lower middle class social dialect, was brought to this continent, and is now being observed for the first time (Williams, 1975: 276).

The only piece of evidence Williams presents for his claim, however, is the fact that “this invariant durative be has also turned up in Eastern Canada as well as in the American South,” and Williams finds it highly unlikely that “Newfoundland Canadian whites could have adopted it from the relatively few Blacks that settled there or that exactly the same meaning could have independently evolved in exactly the same form in the two dialects” (Williams, 1975: 276). This piece of evidence is suspect, however, in view of conflicting reports on the presence or not of habitual/durative be in the speech of white speakers in general (see Wolfram, 1974; Fasold, 1981; Bailey and Bassett, 1986). Furthermore, since this is the only evidence Williams provides, while his proposal is not impossible, he does not present a strong case for it.

Elizabeth Closs Traugott presents a more plausible history for invariant be. She points to the semantics of this verb form, i.e. description of habitual or distributive events or states, and notes its parallels in both creoles and in Old and Middle English. Habitual be seems related to creoles in that habituality is aspectual and, in creoles, generally, aspect is stressed over tense (Traugott, 1972: 191). Habitual be seems related to Old English and Middle English in that OE and ME contrasted two forms of the be verb, beo- and wes-. Furthermore, this contrast is still reflected in Scottish ENE dialects today which contrast I be and I am (Traugott, 1972: 191). Based on these facts, Traugott offers the possibility that certain English dialects in the New World influenced structures in VAAE during its decreolization process (Traugott, 1972, 190). She claims this influence is possible given the fact that “many white Southerners were Ulster-Scots who had migrated to the United States in the 1720s” many of whom became overseers on the large Southern plantations and thus would have often come into contact with the African slaves (Traugott, 1972: 190-1).

John Rickford, in a more detailed study, provides further evidence for Traugott’s view. First, Rickford looks more closely at Irish immigration patterns to the New World. In the Caribbean, Rickford concludes that the seventeenth century would have been the most fruitful period for
language interaction between Irish immigrants and African slaves based on the following information. First, the Irish population in the Caribbean drops off after the seventeenth century, so greater numbers of Irish were obviously present in the seventeenth century (Rickford, 1986: 259). Furthermore, the Irish immigrants and the African slaves often worked side by side on the Caribbean plantations creating a sort of group solidarity which is fruitful for language interaction (Rickford, 1986: 259). Rickford warns against assuming a huge Irish influence in the Caribbean, however, because the Irish would have outnumbered the Africans for one to two generations at the most, the British population was much larger than the Irish population, and the Irish of the seventeenth century were themselves learning English as a second language, since these Irish immigrants were all native Irish from the southern provinces of Ireland (Rickford, 1986: 259-60).

The situation for North America is much different than for the Caribbean. While some native (southern) Irish did immigrate all the way to North America in the seventeenth century, the data is very sketchy (Rickford, 1986: 255). The immigration in the eighteenth century by the Ulster-Scots (from the northern province, Ulster, Ireland) is better documented (Rickford, 1986:259). The nineteenth century once again sees a huge immigration of native Irish but Rickford argues this period is least likely for language contact, since, in the nineteenth century, African Americans and Irish immigrants were in competition for the same, low-skill jobs and thus were in opposition to each other (Rickford, 1986: 258). Rickford concludes, then, that both the southern Irish of the seventeenth century and the Ulster-Scots of the eighteenth century could have come into language contact with African slaves.

Next, Rickford points to the differences in dialect between the native Irish from southern Ireland and the Ulster-Scots from northern Ireland. Specifically, habitual be is more common in the Ulster-Scot dialect, while do (be) is more common in the southern variety (Rickford, 1986: 262). This matches the Caribbean evidence better than the North American evidence. In Caribbean creoles, invariant be is not found, but the mesolectal habitual marker is [doz] which could have been the influence of southern Irish speakers in the seventeenth century (Rickford, 1986: 261-2). Furthermore, since no Ulster-Scots immigrated to the Caribbean, one would not expect to find be in Caribbean creoles. However, do more commonly appears with be than with other verbs in the southern Irish dialect, but doz more commonly occurs with other verbs than with be in Gullah and Caribbean creoles, so the syntactic match is not perfect (Rickford, 1986: 264). Finally, with
respect to North America, an explanation is needed for why *does be* does not occur in VAAE, when the seventeenth century southern Irish immigrants to North America probably came in closer social contact with the African slaves than did the eighteenth century Ulster-Scots (Rickford, 1986: 264). These facts lead Rickford to reject a direct diffusion from Irish-English as the origin of invariant *be*.

Instead, Rickford suggests that a decreolization explanation better accounts for more of the available data. He argues that invariant *be* in VAAE results from the loss and replacement of the habitual marker *does*, found to co-occur with *be* in “an earlier American plantation creole”6 (Rickford, 1986: 265). Furthermore, Rickford claims the emergence of invariant *be* in VAAE should be seen as a later step in the decreolization of the basilectal creole habitual marker *(d)a*, with the emergence of *does* being an intermediate step (Rickford, 1986: 265). He outlines the process in four steps:

(18) Habitual aspect with a prepositional phrase or locative:

Stage 1: *He *(d)a de [de] in the bed.* (basilect)
Stage 2: *He does de in the bed.* (hab. *(d)a→does*)
Stage 3: *He does be in the bed.* (loc. cop. *de→be*)
Stage 4: *He ¢ be in the bed.* (does →¢; *be* ‘habitual’) (Rickford, 1986: 266)

Stage 1 is the basilect which contains both the basilectal copula *de* and the basilectal habitual marker *(d)a*. Although it is not the only possibility7, Rickford claims *(d)a* is probably a convergence between English habitual *do* and the West African substrate which had aspect markers of habituality with often similar phonetic representation such as *(a)maa* in Yoruba, *-na* in Ewe, *de* in Ibo, etc. (Rickford, 1986: 269).

In Stage 2, the lexical item, *does*, from the superstrate (English) replaces the basilectal habitual marker. This change fits well into creole theory in general, which states that, in decreolization changes, new forms take on already-established functions (Rickford, 1986: 277). *Does* is clearly taking over the function of habitual marker, not retaining its English verb status, since it remains invariant, i.e. is never inflected as verbs are, in the mesolect (Rickford, 1986: 269).

In Stage 3, the lexical item, *be*, replaces the basilectal copula. Rickford claims it is introduced

---

6 The existence of a unified plantation creole in North America is in great dispute and was discussed earlier; this debate, however, is not particularly relevant here.

7 Rickford notes that *(d)a* could have resulted simply from the creole universal of marking habituality. (p. 266)
at this point merely as a syntactic link between *does* and the predicate, and is “semantically ‘empty’” (Rickford, 1986: 270).

In Stage 4, which has occurred only in the creole predecessors of VAAE (but is possibly occurring presently in SIC), *does* is lost and *be* assumes the role of habitual marker. Rickford claims this deletion can easily be accounted for through phonological rules, although the realization of the non-standard character of *does* on the part of VAAE speakers might also be a factor (Rickford, 1986: 270). Namely, he claims that there is a phonological rule, common in VAAE and West Atlantic creoles, but not in “white colloquial varieties of English,” which deletes the initial voiced stop of a tense/aspect auxiliary (Rickford, 1986: 270). Once the initial *d* is lost, the remaining schwa syllable, in its “phonologically reduced form,” is lost through further “creole phonological reduction rules” (Rickford, 1986: 272). Furthermore, Rickford cites evidence of the reinterpretation of *be* as the habitual marker as *does* is being lost, claiming the process is currently occurring in Sea Island Creole (Rickford, 1986: 271). In the early 1970’s, Rickford found speakers on the Sea Islands who regularly used *does* as a habitual marker, but these speakers were all over sixty. In contrast, Rickford found that the youngest generation used either zero or *be* as the habitual marker (Rickford, 1986: 271-272). Rickford argues that, because *does* is being lost through phonological deletion, younger speakers do not hear it often enough and instead interpret the *be* which remains as the habitual marker (Rickford, 1986: 272).

Clearly Rickford’s analysis has similarities with Traugott’s. Traugott claims that certain varieties of English influenced the decreolization of VAAE in the New World; Rickford claims specifically that the basilectal habitual marker *(d)a* was influenced both by the West African substrate and the English habitual *do*, and that the replacement of *(d)a* by *does* is an influence of habitual *do (be)* in the Southern Irish dialect. Although Rickford’s analysis is more useful than Traugott’s in that it identifies actual stages in the process of decreolization and specific superstrate influences during these stages, Rickford’s analysis is also more susceptible to criticism than Traugott’s for the same reason. Namely, Rickford explains the selection of *does* as the habitual marker replacement in Stage 2 as an influence of the superstrate. On the other hand, he does not ever actually explain the selection of *be* in Stage 3. Given the explanation for Stage 2, it would seem logical to posit a similar explanation for Stage 3, except that this would not fit the data; the Ulster-Scots did not migrate to the Carribean, and Stage 3 is attested for in the Carribean.
Rickford does claim, however, that invariant *be* in the Ulster-Scot dialect did influence the reinterpretation of *be* as the habitual marker in Stage 4 (Rickford, 1986: 272). This ties in nicely with the fact that invariant *be* emerged as the habitual marker only in North America and not in the Carribean. Still, the selection of *be* in the first place (in Stage 3) remains unaccounted for, which seems like a weak spot in Rickford’s analysis.
Conclusion:

In the introduction to his book on Black Street Speech, John Baugh notes an interesting analogy concerning the nature of linguistics:

Linguistics has occasionally been referred to as the physics of social science, because spoken utterances can be recorded and analyzed as physical commodities. This physical measurement can in turn be controlled under laboratory conditions with high levels of accuracy (Baugh, 1983: 3).

The point seems obvious, but sometimes, amidst the eloquent theories of language function and theoretical constructs of the language faculty itself, the hard concreteness of human speech itself can be overlooked. Once an utterance is spoken in natural circumstances, it must be taken into account as a solid piece of data and any theory, no matter how eloquent, which cannot account for the utterance, cannot be accepted as true.

An utterance not spoken under natural circumstances is more problematic, however. Certainly, it can still be analyzed with the same rigor, but some schools of linguistics would call its true value as data into question. Particularly in the field of sociolinguistics which is concerned with speech as social behavior, the environment in which speech occurs is as important as the actual speech itself. On the other hand, in areas of linguistics such as syntax which are focused more upon the theoretical model of language (such as that envisioned by Noam Chomsky), the environment in which speech is elicited has no bearing on the analysis. These two models of language, language as social behavior versus language as an innate faculty of the human brain, mark a great dividing line in the overall field of linguistics creating two opposing camps with differing goals, approaches, and methodology. Thus, linguists with the latter focus do not have to concern themselves with the methodological problems which plague the sociolinguist such as overcoming the observer’s paradox in order to record ordinary, casual speech in the lives of the speakers.

Historical linguists, however, do not have to concern themselves with these finer points over the elicitation of speech because they simply do not have the benefit of actual human speech samples to analyze. They have to use whatever sources of data are available, being very careful to take the limitations of the data’s usefulness into account. The ideal situation would be for the historical linguist to step back into time with a tape recorder and collect speech samples from the time period under investigation. The reality is, of course, far from the ideal. Given the fact that audio recording is a relatively recent technological innovation in the course of human history, the
next best evidence for the historical linguist is written records of a language. Much of the historical
development of a language group for which much written documentation exists, such as the Indo-
European language family, can be reconstructed with fairly high success. However, without
written evidence of earlier stages of a language, the process of historical reconstruction is much
less certain.

As is clear from the discussion of the creole hypothesis, not many written examples of earlier
forms of VAAE exist. Therefore, its origins and subsequent historical development are quite
uncertain, as is also obvious from the previous discussion. In the absence of many written
samples, which are already at least a step removed from the ideal of actual recordings of speech,
linguists studying the historical development of VAAE have had to turn to many varied, even less
ideal, sources for evidence.

One way of categorizing the different kinds of evidence used by linguists to investigate the
history of VAAE is to look at synchronic versus diachronic approaches to the problem. A
synchronic approach involves only the present time frame. It looks at present-day speech to find
clues to the past. A diachronic approach involves a length of time extending back into the past. It
looks for clues in the past to explain changes in language over time. A synchronic approach has
the advantage of using actual speech samples as data, but has the disadvantage of not investigating
the past directly, only inferring it from the present. On the other hand, a diachronic approach does
look directly at the conditions of the past, but cannot use the ideal linguistic data, actual speech,
and relies on alternate forms of evidence.

An example of a synchronic approach is Martha Baudet’s study. She examines present-day
Caribbean creoles and West African languages in an attempt to prove their relatedness. All the
studies of invariant *be* in present day VAAE are also synchronic studies. Although none are
presented in this paper, other synchronic approaches examine present day Caribbean creoles,
Gullah, and VAAE, to find evidence of their relatedness in a creole continuum. Finally, although
also not presented in this paper, synchronic studies have compared VAAE to Southern nonstandard
English dialects spoken by whites to see how much they diverge and how much they share in
common. All of these studies use actual speech as their primary evidence.

Of the diachronic approaches presented here, quite a few use socio-historical facts as evidence.
For instance, Rickford describes the social stratification of the European traders, the free Africans,
and the African slaves in his analysis of the overall linguistic picture of the slave trade; Traugott and Rickford also document Irish immigration to the New World to determine the influence of Irish English on the speech of African slaves; Dillard examines the demographics as well as the social interaction on southern plantations to prove the existence of a Plantation Creole. Most of this sort of evidence is culled from existing historical documentation for the time period under investigation. When such documentation exists, the researcher must take care not to immediately accept the information it contains at face value. Such documentation is always recorded from the point of view of the person writing it down and is, therefore, colored to some extent by that person’s individual perception. If the researcher is careful to take this bias into account, however, such documentation can often be a fruitful source of information.

Other diachronic studies in this paper have employed literary sources, both fictional and non-fictional, as evidence. Dillard looks for descriptions of the speech of African slaves in non-fictional accounts of voyages during the slave trade to prove the existence of a trading lingua franca with pidgin characteristics. Rickford also cites similar non-fictional accounts to prove most African slaves did not have proficiency in such a pidgin. Stewart, on the other hand, looks to fictional sources, citing the dialogue spoken by African and African American characters as being a reasonable facsimile to actual speech during the same time period. As discussed earlier, literary evidence can be very unreliable, particularly when speech, directly quoted from it, is used as data. Phonological transcriptions are the most accurate written representation of speech, but even when written down by trained linguists, such transcriptions can still contain errors if a sound is not heard and judged correctly by the linguist. Written representations of speech recorded by laymen with no linguistic training using a standard alphabet are bound to lack the accuracy and precision of phonetic transcription. Literary sources are still valuable in that they indicate differences in language noticeable to the ordinary listener, but it is questionable, at best, whether representations of speech found within such sources can be assumed to accurately portray the actual linguistic situation.

One notable exception is Schneider’s study of the WPA ex-slave narratives. Although these narratives are examples of laymen attempting to record speech using a standard alphabet, they are a more promising source of data than most literary sources because the interviewers were specifically asked to render the informant’s speech as accurately as possible (Schneider, 1989: 45). Still,
Schneider does not automatically accept the entire corpus as a reliable representation of EBE. Instead, he ranks the accuracy of each interviewer by analyzing how each records the feature of pronominal subject/copula agreement and uses only the texts of interviewers who do not "correct" this feature. That is, lack of concord between pronominal subjects and the copula is characteristic of EBE, so if an interviewer writes down non-concord forms instead of changing the form to SE (where subject and copula agree in number), Schneider takes this as evidence that the interviewer was genuinely interested in accurately recording the speech of the informant (Schneider, 1989: 54). Because Schneider takes into account the potential flaws of his literary corpus and tests its accuracy before embarking on an analysis of it, one does not need to be as wary of his evidence as in most cases where literary evidence is employed.

The diachronic studies searching for British/Irish origins of VAAE have probably the most reliable evidence at hand since, as noted previously, the Indo-European language family has the most written documentation of earlier stages. Williams and Traugott have numerous texts written in Old English and Middle English at their disposal in which they can locate examples of particular grammatical forms upon which VAAE could have been modeled. While there are appreciable differences between the language a person uses when speaking and the language used when writing, the writing of a native speaker of a language is still much closer to the actual spoken language at the time than, for instance, the dialogue found in a literary work. In fact, samples of writing during earlier stages of a language's history are the most reliable source of data for the historical linguist.

That, then, is a brief examination of various synchronic and diachronic approaches to the problem of ascertaining the origin(s) of VAAE, as well as the types of evidence utilized. With the multitude of approaches and evidence available, it is not surprising that so much debate flourishes around the question VAAE's origin(s). For instance, when Baudet's synchronic study of West African languages and Carribean creoles points to a substratum of African languages for VAAE, while Schneider's diachronic study of Earlier Black English, Gullah, and present day VAAE point to primarily British origins for VAAE, it is difficult to reconcile the two differing conclusions. The situation is analogous to comparing apples and oranges. Different kinds of data and arguments over the same point can be taken into account, but cannot be compared directly which is one difficulty in assessing the overall picture of the origin(s) of VAAE.
Another difficulty of an almost opposite nature is that the same piece of evidence can be used to support differing, even opposing, views depending on how it is interpreted and worked into the framework of the argument. For example, Williams uses the fact that grammatical forms characteristic of VAAE are found as far north as New Foundland, Canada, to argue against a pidgin/creole origin for VAAE. He claims that since so few slaves ended up that far north, it is more probably that these forms emerged as a result of the influence of British speakers. Dillard, on the other hand, uses this same fact to support the creole hypothesis. He claims that the presence of these forms in Canada is evidence of a very widespread Plantation Creole across North America. The evidence is the same, but the underlying assumptions are different which makes two different interpretations possible.

To take a more complex example, consider the relationship of VAAE to nonstandard white speech in the southern United States with respect to the question of the origin(s) of VAAE. Some linguists argue that the speech of African Americans in the South is a separate entity from the speech of their Anglo-American counterparts while others argue that the speech of the two groups is the same -- any differences found are attributable to factors other than race such as regional variation and socio-economic standing (Fasold, 1981: 163). (Moderate, or intermediate, positions are also argued for, but, for the sake of simplicity, only the two extreme positions will be considered here.) As for the origin of VAAE, some linguists argue for the creole hypothesis and some argue against it. When these two issues are considered together, an interesting picture emerges:

Wolfram (1971) has pointed out that either position on one of these issues can be held concurrently with either position on the other. There are four possible combined positions. The first position (different +, creole +) is that modern black English is substantially different from any variety of white English because it developed from the Plantation Creole and no white dialects did. The second position (different -, creole +) would be that, although there was a Plantation Creole, decreolization has proceeded so far that no significant differences between Southern black and white speech survive when the social status of the speakers are comparable. The third position (different +, creole -) would be that, although there was no Plantation Creole, black and white language systems have so diverged over the years because of racial discrimination and segregation that the two varieties are now substantially different systems. The fourth position (different -, creole -) is that there was no Plantation Creole and there were no other factors to cause substantial speech differences, so that in fact there are none. (Fasold, 1981: 164)

All four positions are possible because the assumptions underlying each one are different. Thus, evidence which proves the dialects in the South to be the same, for example, could be worked into
a framework which either supports or refutes the creole hypothesis.

These two factors, the utilization of different kinds of approaches involving evidence which cannot be directly compared, and the utilization of the same evidence to support differing points of view based on different underlying assumptions and interpretations of the evidence, are the two main difficulties in the field of VAAE studies. Of course, these differences can arise in many different ways: sociolinguistics versus “Chomskian” linguistics, diachronic study versus synchronic study, second-hand literary evidence versus actual early texts, etc. With respect to the historical origins of VAAE, differences such as the latter two are more central to the problem given the nature of historical linguistics, but in other areas of VAAE studies, the first division is much more central. The decision of whether to study VAAE as just a language system, ignoring its place and function within society, or to study it as a social dialect, locating it within the dynamics of society, has great implications for the approach taken and methods employed by the linguist. The issue is further complicated by the politically-charged nature, in this country, of issues having to do with racial distinctions. In fact, this may be one reason why very few analyses of VAAE as purely a language system unto itself have been undertaken; linguists perhaps shy away from the option of ignoring VAAE’s place and function in society, because it is a politically-charged topic, the societal implications of which are not always easy to ignore. This creates an unfortunate imbalance in the study of VAAE, in general, which, hopefully, will be rectified in the future.

To return to the question of VAAE origins, however, it is clear that neither the creole hypothesis, nor the British/Irish origin hypothesis are without their difficulties. The lack of entirely convincing British models for certain grammatical forms of VAAE, such as invariant *be*, seems particularly damaging to the British influence hypothesis while Schneider’s study of Earlier Black English, which fails to find entirely convincing signs of decreolization between EBE and VAAE, seems particularly damaging to the creole hypothesis. The best solution, at least at the present time, and one advocated by many in the field in one way or another, is a somewhat modified creole hypothesis which acknowledges a British/Irish influence upon VAAE as well (Baugh, 1983; Traugott, 1972; Wolfram, 1971; Fasold, 1981; Rickford, 1986). Hopefully, further research will show whether this is, indeed, the case.
BIBLIOGRAPHY


