RESPONSE

NUMBER DELIMITATION IN GULLAH: A RESPONSE TO MUFWENE

MUFWENE'S (1986) PAPER IN THIS JOURNAL provides a revealing discussion of how the individuation (count/mass) and number (singular/plural) marking systems interact in English and in those varieties of Jamaican Creole and Gullah which are basilectal (ie., maximally different from English). In his concluding section, however, he ventures into a critique of my (1986) paper on the variation between plural {-Z} and {-Ø} in a mesolectal variety of Gullah which is less discerning than the rest of his paper and which obfuscates some of the relevant issues. In particular, Mufwene recalculates and reinterprets my VARBRUL results in spurious ways, and he misrepresents my original position on the matter of {-Z}/{-Ø} variation: contrary to the impression he conveys (33), I was, as a native creole speaker and creolist, quite aware of "how different from English the underlying principles of number delimitation in English-related creoles are," and I, too, was hopeful that mesolectal Gullah might still exemplify the grammatical principles of basilectal creole. Moreover, Mufwene concludes with two open questions about my analysis which require clarification (52): "Some explanation is still needed as to why the probability coefficients of the semantic factors in Rickford's analysis are in conflict with one another while they are based on apparently the same parameters. And we still need to find out how the phonological factors allegedly rule out the semantic ones as 'insignificant.'"

My reason for providing a detailed public response to Mufwene's critique, despite the delay that has occurred in the interim, is not only to set the record straight for the benefit of those undertaking work on this particular variety or variable, but also because the primary topics at issue—the value of quantitative acountability, phonological versus syntactic or semantic constraints, and insertion versus deletion rules—are relevant to the study of American dialects and sociolinguistic variation more generally. Moreover, Singler's (1988) paper on variable plural marking in Kru Pidgin English in Liberia, which makes reference to both Mufwene (1986) and Rickford (1986), illuminates some of the topics which were originally at issue, drawing on the framework of Lexical Phonology, which was less developed and less widely known when our papers were first written.[1]

SUMMARIES OF THE TWO PAPERS. In Rickford (1986) the analysis of number marking in Gullah was based on detailed examination of 128 semantically plural nouns used by Mrs. Queen, an 84-year-old black woman from a South Carolina Sea Island whom I had recorded in 1970. But it was also informed by a larger corpus of about twenty hours of Gullah speech from more than

two dozen individuals whom I had recorded between 1970 and 1984. The plural-reference nouns in Mrs. Queen's sample occurred with the English plural suffix {-Z} 23 percent of the time, and without it (i.e., with {-Ø}) 76 percent of the time.[2] In view of the close alternation between {-Z} and {-Ø} in Mrs. Queen's speech (sometimes in the same sentence, even in repetitions of the same word), and because of the fact that there were no categorical stylistic or other differences between the utterances in which these variants occurred, I treated them as instances of inherent variability in the dialect (in the sense of Labov 1969), and I began a systematic search for their variable constraints with the help of the VARBRUL program.

Suspecting (even hoping) that the variation between {-Z} and {-Ø} in Mrs. Queen's mesolectal Gullah might still show the influence of semantic/syntactic constraints generally thought to influence number marking in basilectal creole varieties, I coded every semantically plural noun in her corpus for two such constraints:[3]

a. whether it was DEFINITE OR EXISTENTIALLY PRESUPPOSED (coded as T), that is, specific in reference and assumed known to the listener because mentioned earlier in the discourse; INDEFINITE OR EXISTENTIALLY ASSERTED (coded as A), that is, specific in reference and assumed unknown to the listener because mentioned for the first time in the discourse; or NONSPECIFIC OR EXISTENTIALLY HYPOTHESIZED (coded as N), as with generic NP's; and
b. whether it was preceded by a PLURAL QUANTIFIER like 'many' or 'five' (coded as Q), the PLURAL DEICTIC MARKER *DEM* 'those, the' (coded as D); or NEITHER OF THESE PLURAL FORMS (coded as Z).

If these constraints operated as described in the creolist literature (see Stewart 1974; Bickerton 1981, 23-4, 56-8; Dijkhoff 1982; Mufwene 1986, 40), we would have expected {-Ø} most often with nonspecific nouns and/or with those accompanied by a plural quantifier or *dem*, since the presence of these plural forms is often described as rendering plural marking on the noun unnecessary.

I also coded Mrs. Queen's noun tokens according to two phonological factor groups previously identified as constraints on the occurrence of plural {-Z} in other dialects of American English:

c. whether the preceding phonological segment was a NONSIBILANT CONSONANT (coded as C), a SIBILANT CONSONANT (coded as S), or a VOWEL (coded as I); and
d. whether the following phonological segment was a CONSONANT (coded as K), a VOWEL (coded as *I*) or a PAUSE (coded as P).

If these constraints operated as described in the literature on American dialects (see Labov et al. 1968), we would have expected to find {-Ø} most

often with a preceding nonsibilant consonant (thus avoiding a CC sequence) and/or most often with a following consonant or pause.

I used the VARBRUL-2S program (see Cedergren and Sankoff 1974, Rousseau and Sankoff 1978) to perform a multiple regression analysis of the coded data, controlling for the simultaneous effects of all four factor groups. The results indicate that the two phonological factor groups served as statistically significant constraints on the observed variation while the two syntactic/semantic factors did not. On the basis of this evidence, I wrote the following Variable {-Z} Deletion Rule to describe Mrs. Queen's variable plural marking:

$$Z \rightarrow \langle \emptyset \rangle \; / \; \langle \tilde{}V \rangle \; \#\_\_\#\# \langle \tilde{}V \rangle$$
$$[+noun][-sing]$$

In prose terms, this rule indicates that plural {-Z} is variably deleted, moreso when the preceding and following phonological segments are NOT vowels.

Mufwene's analysis (1986) was based on 15 hours of recorded conversation with 20 or so Gullah speakers, although these data are used only selectively, and we don't get an accountable description of the occurrence of {-Ø}, {-Z} and *dem* in his corpus. A recurrent point in Mufwene's paper is that number delimitation in English differs in some significant respects from that in English-related creoles like Jamaican or Guyanese creoles and that the Gullah system appears closer to that of the latter than the former, whether or not {-Z} or *dem* is the plural marker or delimiter in question (33–34). However, the Gullah system which Mufwene describes is essentially that of an idealized basilect, insofar as its basic exponents are {-Ø} and *dem* ({-Z} is barely mentioned except as an occasional "redundant transfer" from English), and insofar as these are described as categorically constrained by semantic/syntactic factors operative in English creole basilects.

Mufwene's discussion of number delimitation in Gullah is preceded by a discussion of number delimitation in English and Jamaican Creole which draws substantially on a related distinction (introduced in Mufwene 1981) between INDIVIDUATED and NONINDIVIDUATED nouns—roughly equivalent to COUNT versus MASS and partially similar to SPECIFIC versus NONSPECIFIC respectively (Mufwene 1986, 53). Individuated nouns "refer to their denotations or subsets thereof as consisting of denumerable individuals" while nonindividuated nouns, including generics, refer to their denotations "as an 'ensemble' or part thereof " (39). Gullah nouns in their nonindividuated use are said to be unmarked for number (i.e., they take {-Ø}) but in their individuated use are described as using prenominal *dem* to indicate plurality when the noun is unquantified, as in *dem buckra* 'the whites'. When the noun is quantified by

a plural numeral or individuating quantifier (*two brudda, all de small one*) plurality is said to be unmarked (44), except in "redundant transfers" of English {-Z}:

> In cases of plurality the numeral or individuating quantifier is the only required and sufficient indicator. The {-Z} which is attached in some cases of mesolectal speech when the noun is already delimited with a numeral, an individuating quantifier, or a prenominal [dEm] is a redundant transfer from English grammar.

In the final section of his paper, where he refers to my (1986) analysis, Mufwene proposes that occurrences of the English derived plural {-Z} suffix in mesolectal Gullah be treated as the product of an insertion rule, jointly constrained by semantic and phonological factors. The major issues which he raises in that section will constitute the focus of this paper from here on.

DATA, STATISTICS, AND ACCOUNTABILITY. Comparing our respective analyses, Mufwene (1986, 48–49) remarks, "Unlike Rickford, I include no statistics; I doubt that statistics, which are essentially a refined way of representing quantitative observations as they have been guided by a particular framework of analysis, would have made a significant difference."

The issue of statistics—anathema to linguists in general for some time, and a concern raised by students of American dialects more recently (see Holm 1984, 302)—is one that I'll comment on specifically below, but I want to lead into it by discussing the more general issue of accountability to data.[4]

To his credit, Mufwene's analysis is based on a recorded corpus, from which he selects illustrative examples of modern spoken Gullah. He is also commendably aware of variation in the data and critical of approaches which abstract away from it. But he sometimes makes stipulative generalizations and draws distinctions between hypothetical grammatical and ungrammatical sentences which are not in accord with observed data. For instance, he claims (34), "While in the context of today's Gullah [hi sʌn dɛm/nɛm] and [hi sʌnz] are both grammatical, *[hi sʌnz dɛm/nɛm] is not grammatical." But MIXED intersystemic variants of this general type are familiar from other continuum situations (eg., Edwards 1974, 14, reports occasional occurrences of lower mesolectal *unuaal* along with basilectal *unu* and higher mesolectal *you all*), and my Gullah corpus reveals several counterexamples to Mufwene's specific claim. The following combine {-Z} and post-nominal *dem,* as in Mufwene's asterisked example:[5]

1. De white man who did own dat place give *our grandparents dem* dat plot. (Mrs. Queen 15-II-534)[6]
2. But what I know is wha' *de fellows dem* tell me. (FM 14-II-283).

The following combine {-Z} and pre-nominal *dem*:[7]

3. I show *dem ladies* wha been hey de odda day. (EH 9-II-251)
4. *Dem two lil boys*—dey look like twins, ain'it? (BR 11-I-63)

The following combine {-Z} with BOTH prenominal and postnominal *dem*:

5. You know how *dem boys dem* meet up *dem girls dem.* (JH 4-I-684)
6. Dey used to mos'ly live mos' off de farm dem, an stuff like dat, *dem days dem.* (FM 6-I-186)

Furthermore, my Guyanese Creole corpus contains similar examples (hence the "Noun (#Z)## dem" table 2 column heading in Rickford 1986, 47), showing that intersystemic mixtures of this kind occur quite generally.

Moreover, although Mufwene recognizes variation between *dem*, {-Z}, and {-Ø} in modern Gullah, and although he provides a number of interesting generalizations about basilectal constraints on their occurrence, he does not provide details about the distribution of these forms in the recorded data of his informants, that is, whether they use any or all of them in the semantic subcategories he postulates as constraints. (Since, by his own subsequent admission [1988, 113], no Gullah or other creole speaker uses basilectal forms or follows basilectal principles 100% of the time, the speakers in his sample must vary, as those in mine do, and our understanding of this variation requires more detail than he provides.) Even without statistics, an accountable description of this sort can be an asset to understanding the course of dialect variation and change and facilitating comparison with other communities. For instance, Bickerton's (1973) observations on copula variation in Guyanese Creole indicated whether *Ø, a, de,* or inflected forms of *be* were used by each informant in various environments, but not how often each occurred. These qualitative data were critical in charting the nature of copula/auxiliary variation in the mesolect and revealing parallels with what had earlier been hypothesized by Stewart (1970) for Gullah.

Beyond using a recorded corpus and providing qualitative reports about the distributions of forms, there is the issue of QUANTITATIVE ACCOUNTABILITY—providing statistical observations of the frequency with which all variants occur. When Labov (1969, 737–38, n. 20) introduced the principle of accountability as a basis for the study of linguistic variability, it was THIS kind of accountability that he had in mind:

> ANY VARIABLE FORM . . . SHOULD BE REPORTED WITH THE PROPORTION OF CASES IN WHICH THE FORM DID OCCUR IN THE RELEVANT ENVIRONMENT, COMPARED TO THE TOTAL NUMBER OF CASES IN WHICH IT MIGHT HAVE OCCURRED. [emphasis Labov's]

As Labov (1969) goes on to say, this principle is important for increasing intersubjective agreement and for minimizing the influences of the linguists' theoretical preconceptions and the tendency to dismiss unwanted variants as examples of "dialect mixture" (random shifting between ideal-

ized invariant dialects). Many sociolinguists and students of language variation have since accepted and worked with this principle. This is not to say that everyone must, but, speaking to the quotation from Mufwene with which I began this section—the difference which statistics and the accountability principle offer is the possibility of increasing the precision, reliability, and validity of our analyses, and perceiving systematic conditioning, ongoing change, and interdialectal relationships where these might otherwise be invisible. Labov's own work, and variationist studies of the past two decades (see NWAV volumes from Bailey and Shuy 1973 to Ferrara et al. 1988), exemplify and validate this point.

THE PRIMACY OF PHONOLOGICAL VERSUS SEMANTIC/SYNTACTIC CONSTRAINTS. There is a common preconception among those who work on English creoles that grammatical variables in these varieties are constrained by semantic and syntactic factors but not phonological ones. There is a related preconception, perhaps dating back to the Labov/Stewart controversies in the 1960s about the analysis of the copula, that anyone who suggests that grammatical variables in Vernacular Black English are phonologically conditioned is "anti-creolist."

Although Mufwene holds neither of these preconceptions in their simple forms—he is willing to admit that the presence and absence of plural {-Z} in mesolectal Gullah might be jointly conditioned by phonological and semantic/syntactic factors (50)—he disputes my conclusion that the phonological factors are primary, specifically examining and reinterpreting the VARBRUL results on which I base my conclusion. In this section I will summarize his versions of my arguments and evidence, give his comments and interpretations, and then offer rebuttals and explanations to clarify my original position.

Mufwene begins by presenting the results of the VARBRUL run in which all four factor groups were included. The "absence probability coefficients" (APC's) calculated by VARBRUL represent the independent effect of each factor on the Variable {-Z} Deletion Rule shown earlier; probabilities over .5 favor rule application, those under .5 disfavor it, and those at or around .5 have little or no effect.[8] From these results, shown in table 1, it is clear that the factors in the phonological factor groups constrain plural marking as we had expected them to (see 151–52 above), but that those in the semantic and syntactic factor groups do not—definite nouns rather than nonspecifics most favoring {-Z} absence, and the presence of *dem* disfavoring rather than favoring {-Z} absence (instead favoring "redundant" marking with {-Z} as in examples 1–6 above). However, contrary to what Mufwene suggests (50), this was not my reason for regarding the latter as insignificant. Nor was it

TABLE 1
Absence Probability Coefficients in VARBRUL Four-Factor-Group Run

Semantic Factor Group
Definite/Exis. Presup. = .685; Indefinite/Exis. Asserted = .299; Non-Specific = .519
Syntactic Factor Group
Preceding Plural Quantifier = .553; Preceding *dem* = .426; Neither Plural Form = .521
Preceding Phonological Segment Factor Group
Nonsibilant Consonant = .652; Sibilant = .564; Vowel = .291
Following Phonological Segment Factor Group
Consonant = .594; Vowel = .284; Pause = .633

because there was any inherent "conflict" between the VARBRUL results for the syntactic and semantic factor groups, as Mufwene also suggests ("Rickford does not explain what accounts for the conflict between the first two computations; after all, the parameters selected for the first computation seem very similar to those selected for the second!" [51]). The conflict which Mufwene sees here is more apparent than real, a function of erroneously assuming that the factors in the first two factor groups could be automatically equated with each other, and with his own subcategories (49) as shown in table 2.

TABLE 2
Mufwene's Assumed Cross Factor-Group Equivalences

Factor Group 1		*Factor Group 2*		*M's subcategories*
T (Exis. Presup.)	=	D (*dem* Present)	=	Individuated
A (Exis. Asserted)	=	Q (Quant. Present)	=	Individuated
N (Exis. Hypoth.)	=	Z (No *dem*/Quant.)	=	Non-Individuated

To understand the nature of the conflict which Mufwene sees here, note that if the correspondences between the factors in the Semantic and Syntactic Factor Groups in table 1 were exactly as set out in table 2, their probability coefficients and orderings within each group should be identical—which, as table 1 reveals, is not the case. For instance, T equals .685—MOST favorable to {Z} absence in the Semantic Factor Group; while D equals .426—LEAST favorable to {Z} absence in the Syntactic Factor Group.

However, other correspondences besides those in table 2 are possible, and do in fact occur. For instance, T may be equivalent to Z, as in *plant de odda tings* (where the noun is definite or existentially presupposed, but preceded neither by a plural quantifier or by *dem*), which occurs ten times, and T may be equivalent to Q, as in *de two oldest one* (where the noun is existentially presupposed and preceded by a plural numeral or quantifier). The mistake which Mufwene makes here is that while D in his second Factor Group definitely implies T in the first (noun phrases with prenominal *dem*

ARE definite in reference), the reverse is not necessarily true (noun phrases can be definite by virtue of occurring with prenominal *de* rather than *dem*). Other occurrent correspondences besides those in table 1 include: A = Z and N = Q. Clearly, Factor Groups 1 and 2 are NOT simply restatements of each other, and the fact that they yield different results is therefore NOT surprising. (Note, in any case, that if these Factor Groups had been restatements of each other, this would violate the VARBRUL requirement that factor groups be orthogonal; see Guy 1988, 126).

Moreover, it is quite invalid to argue, as Mufwene does, that combining the results for "equivalent" factors in Groups 1 and 2 would support his Individuated/Non-Individuated hypothesis ("together they average to a higher probability coefficient for INDIVIDUATED nouns than for NONINDIVIDUATED NOUNS" [50]). This is so not only because the linguistic equivalences in table 2 aren't automatic or exclusive, but also because the required computations are statistically impermissible. Mufwene doesn't provide the actual computations which he thinks support this argument, but it appears that he merely added the probabilities for Existentially Asserted (.299),[9] Existentially Presupposed (.685), Preceding Plural Quantifier (.553) and Preceding *dem* (.426), on the assumption that they are all "Individuated," and divided the total by four to get an average of .491. This is less than the mean of .520 which can be derived for "Non-Individuated" if you add the coefficients for "Existentially Hypothesized" (.519) and "Neither Quantifier nor *dem*" (.521) and divide by two. However, one simply cannot—on either linguistic or statistical grounds—combine probability coefficients across factor groups like this. These reconstituted data are invalid, and the argument which they are intended to support is therefore spurious.

But if the syntactic and semantic factors groups are not ruled out because their probability coefficients are internally or externally conflicting, how do they come to be ruled out by the phonological factor groups (the first of Mufwene's two open questions [52])? The answer was given in my original article—by the VARBRUL multiple regression procedure which estimates the statistical significance of each factor group in accounting for the observed variation (50). But in order to answer Mufwene's question adequately, it may be helpful to expand on the explanation there, and in fact to provide a detailed account of how Mrs. Queen's data were prepared for and processed by VARBRUL.

In order to see whether the Syntactic and Semantic Factor Groups really played a significant role in Mrs. Queen's plural marking, and whether the factors within each group favored, disfavored, or had little effect, I started by preparing a "token file" for Mrs. Queen's data as in table 3, coding each token or example of a semantically plural noun according to whether it was

TABLE 3
Some Examples From Mrs. Queen's Token File

Example	*Variant*	*Sem*	*Syn*	*PrePho*	*FolPho*
. . the other things and . . .	1	T	Z	C	I
two o' my brudda gone	0	A	Q	V	K
dey raise hog	0	N	Z	C	P

marked with {-Z} (coded as "1" in the Variant column) or unmarked (coded as "0"), and according to the factors represented (see p. 149 above for codes). On the basis of all her 128 coded noun tokens, I then prepared a "cell file," indicating the relative frequency of tokens with {-Ø} in all occurrences of each environment or cell (consisting of one factor from each factor group), as in table 4. This cell file is the input data which the VARBRUL computer program uses to estimate probability coefficients for each of these factors (T, Q, C and so on), representing its independent contribution to the overall probability of rule application (see Rickford 1986, 50 for the relevant logistic formula).

TABLE 4
Sample Cell Entries in Mrs. Queen's Cell File

#tokens w. 0	*Total tokens*	*Percent*	*Cell or environment*
3	3	100%	T Q C K
0	3	0%	A Q V I
6	8	75%	N Z C I

In order to estimate which factor groups as a whole significantly constrain the observed variation, VARBRUL 2S also contains a regression procedure in which more and more of the constraint data ("step-up"), and less and less of it ("step-down") is considered. For simplicity, we'll discuss just the step-down procedure, in which the program, following the level-four run in which all four factor groups are included, proceeds to successive runs in which each factor group is systematically excluded and discarded if its removal does not have a significant statistical effect (as measured by a chi-square figure significant at the .05 level or less) on the program's ability to account for the variation in the data, as measured by changes in the maximum likelihood figure obtained for each run (see Sankoff [in press] for further discussion). In our case, the syntactic factor group (DQZ) was selected to be discarded on step-down at level three, where only three factor groups at a time were being considered; when the constraint information from this factor group was suppressed, it made very little difference to the program's ability to account for the observed variation—its omission resulted in a chi-square probability figure of .868, which was far from the .05 threshold, and higher than the corresponding chi-square figures when any of the other groups was excluded. The semantic factor group (TAN) was selected to be thrown out at level two,

when only two factor groups at a time were being considered; its omission resulted in a chi-square probability figure of .158, which was closer to the .05 threshold than the syntactic factor group, but still outside the level of statistical significance, and much weaker in its effect than any of the phonological factor groups. The phonological factors were retained, because their removal always had a statistically significant effect on the analysis, the following phonological factor yielding a chi-square probability value of .019 when omitted, and the preceding phonological group yielding a corresponding value of .009 when omitted.[10]

In short, it was the data themselves which indicated minimal effects from the semantic/syntactic factor groups and maximal effects from the phonological factor groups. (Interestingly enough, Singler, 1988, 345—including Mufwene's Individuated/Non-Individuated among his subcategories—reports similar results for Liberian Pidgin English.) This result is not one I had hoped for—I controlled for these particular semantic/syntactic factor groups precisely because I felt, like Mufwene, that basilectal constraints might continue to play themselves out in this area of the mesolect as they seem to do elsewhere. But Mrs. Queen's plural marking data simply do not support this hypothesis, and we cannot continue to insist on it on philosophical or other grounds in the face of empirical reality.

DELETION RULE—OR INSERTION? The other aspect of my analysis which Mufwene questions is my decision to handle Mrs. Queen's variation in this area of the grammar by means of a rule which variably deletes an underlying or obligatorily inserted plural {-Z} rather than a rule which variably inserts this plural marker. This is, of course, another issue on which "dialectologists" and "creolists" are often stalemated, but my final decision to provide a deletion rule for Mrs. Queen was not the result of any philosophical preconception, but the result of a systematic attempt to consider arguments for and against each approach.

As Mufwene notes (51), the fact that "{-Z} is missing in the overwhelming majority of cases" does argue for the insertion rather than the deletion analysis, but he fails to note that I had made the same point, with more supporting argumentation, in my (1986) paper (53):

> The preponderance of {-Z} absent forms in Mrs. Queen's speech . . . makes us uncomfortable about suggesting that the suffix is underlying. . . . It is "cheaper" to account for the occasional occurrences of a feature by the application of a grammatical rule inserting it than to account for the nonoccurrences by the prior application of grammatical rule insertion, followed by the application of a phonological rule which has the effect of wiping out the newly inserted feature more often than not.

What seemed to me to militate decisively against the insertion analysis, however, was the fact that "the only alternative to a phonological {-Z}

deletion rule . . . is a grammatical {-Z} insertion rule with phonological constraints, and it is difficult to see how this is possible in any framework in which the phonological component is interpretive and subsequent to the grammatical one." That is, in most theoretical models since Chomsky's standard theory (1965), the phonological component operates on the out put of the syntactic component rather than vice versa, so that it is possible for phonological rules to take grammatical information into account, but not for grammatical rules to take phonological information into account. Given this restriction, the only way to account for the phonological conditioning of Mrs. Queen's use of plural {-Z} in the established generative models of the early 1980s was to have "plural" {-Z} obligatorily inserted in the grammatical component, but variably deleted within the phonological component.

However, in Rickford (1985), written, despite the date (see n. 1 below), AFTER Rickford (1986), I did note (119, n. 5) that

> Once we can adopt a model in which grammatical insertion can be phonologically conditioned (Kiparsky's lexical phonology is a possible candidate), I would be happy to accept a plural *-s* insertion rule for Mrs. Queen. This would have the additional advantage of matching the diachronic development of Mrs. Queen's grammar (and that of Gullah as a whole) more accurately.

I have since been assured by linguists working within the Lexical Phonology framework (Sharon Inkelas, Paul Kiparsky, and K. P. Mohanan, personal communications) that it is indeed possible to have phonologically constrained morphological insertion rules in this framework, and Singler (1988) has proposed a Lexical Phonology analysis for variable plural marking in Kru Pidgin English (KPE) which is easily adaptable to our Gullah data. In his analysis, there is a variable morphological rule of plural {-Z} affix-insertion, phonologically constrained by the preceding segment, and a subsequent postlexical {-Z} deletion rule which is also variable, phonologically constrained by the following segment.[11] The ordering of individual factors within the preceding and following phonological factor groups is somewhat different in KPE and Gullah, but the fact that these are the significant factor groups is the same.[12]

While Lexical Phonology and other frameworks remove the theory-internal barriers against an insertion analysis for plural {-Z}—or at least make it possible to have an analysis which combines variable insertion AND deletion, additional research in this area which takes other, theory-external considerations into account is certainly warranted. Such considerations should include the following conditions which Labov (1984) identifies as favoring analyses of zero as the result of a productive deletion process:

a. Quantitative phonological conditioning
b. Absence of hypercorrection
c. Uniform distribution in the community
d. Fine grained style-shifting
e. A high degree of learnability
f. Integration into the grammar.

Although (a) is clearly present in the case of Mrs. Queen's data, and while I argued in Rickford (1986) that conditions (b) and (d) were also satisfied to some extent, the evidence on conditions (c), (e), and (f) remains to be gathered.

One other relevant consideration—raised in my original article, taken up in Mufwene (1986), and even more critical now if a postlexical deletion rule is to form part of the analytical apparatus—is whether the rule deleting plural {-Z} applies only to this morpheme. At the postlexical level in Lexical Morphology, morpheme boundaries are nonexistent, having been removed by a Bracket Erasure Convention which applies at the end of every level of the lexical phonology (Kiparsky 1982, 5; Mohanan 1986, 24). As a result, a postlexical deletion rule constrained by following phonological environment should not be restricted to plural {-Z} but should be applicable to all instances of final /z/. Singler (1988, 350) claims that such is the case in KPE, but the evidence for Mrs. Queen's Gullah is less clearcut. While possessive and adverbial {-Z} absence is about as frequent in Mrs. Queen's data as plural {-Z} absence, and while it appears to be similarly favored by a following nonvowel, monomorphemic forms lose their final /z/ much less frequently, and the effect of following environment is minimal. Furthermore, alternative nonphonological explanations for the variable absence of final {-Z} in the adverbials and possessives are possible. These considerations militate against a general postlexical deletion rule for {-Z} or final /z/, but the data base for these other features in Mrs. Queen's recorded speech is relatively small (13 to 24 cases, contrasted with 128 for the plural), and corroborative evidence from other speakers seems to be required.

SUMMARY AND CONCLUSION. Three aspects of my (1986) analysis of plural marking in mesolectal Gullah which Mufwene (1986) calls into question are the value of statistics, the relative significance of phonological versus semantic constraints, and the necessity of representing Mrs. Queen's variation between {-Z} and {-Ø} by a deletion rule rather than an insertion rule. With respect to the first aspect, I have defended the use of statistics as an integral part of providing a reliable and accountable description of variability in mesolectal Gullah. With respect to the second, I have explained that the phonological factors were statistically significant while the syntactic and semantic

ones were not, and I have provided additional details about the VARBRUL multiple regression procedure which was used to establish this. Additionally, I have shown that Mufwene's attempt to equate factors in my syntactic and semantic factor groups with each other and to recompute their probability coefficients accordingly is invalid, and the argument his recomputations were intended to support (i. e., that the Individuated/Non-Individuated distinction significantly constrains Mrs. Queen's plural marking) is therefore spurious. With respect to the third issue, I concede that it is indeed now possible to have a phonologically constrained variable {-Z} insertion rule, thanks to such frameworks as Lexical Phonology, but I indicate some theory-external issues which need to be considered in relation to this issue.

The need for additional research on number delimitation in mesolectal Gullah and other intermediate pidgin-creole varieties is quite clear. On the one hand, Mufwene has illuminated the semantic bases on which the individuation and number systems operate in basilectal Gullah and other Atlantic creoles, revising and extending earlier analyses by Stewart, Bickerton, and Dijkhoff. On the other, Singler and I, independently conducting VARBRUL analyses of number marking in Gullah and Kru Pidgin English, have found phonological constraints to be paramount, contrary to our own expectations and those of most creolists. I, for one, hope to extend my analysis of number delimitation in Gullah to a number of other speakers, including Mufwene's categories but retaining the Labovian principle of quantitative accountability, as Singler has done for Liberian Pidgin English. In turn, I hope that Mufwene will go beyond the schematic description of idealized basilectal and acrolectal systems which he has so far provided and attempt a more (quantitatively) accountable description of the mixed, intermediate systems which, in the Sea Islands as elsewhere, are the norm rather than the exception.

NOTES

1. This paper has benefitted in part from discussions with John Baugh, Sharon Inkelas, Paul Kiparsky, K. P. Mohanan, Angela Rickford, and Arnold Zwicky. I wish to thank them for their input while absolving them of responsibility for any claims or ideas represented herein.

It should be noted that Mufwene and I have corresponded privately about theoretical and methodological differences between our approaches (in a candid but constructive spirit, I should add), and some of our publications already take this correspondence into account. Mufwene saw my (1986) paper, originally written for a 1981 conference, while it was in press, and revised his own paper, first written in 1984, after our correspondence. In turn, my (1985) paper, actually written AFTER the 1986 paper despite the publication dates, responds (119, n. 9) to some aspects of Mufwene's 1984 typescript.

2. There was also one occurrence of postnominal *dem* (*de masa dem*), accounting for the remaining 1% of her total (see Rickford 1986, 47).

3. According to Mufwene (1986, 57, n. 21), Mrs. Queen is acknowledged by me to be "rather mesolectal." Although I did classify Mrs. Queen as "mesolectal," and still do, too much should not be made of her mesolectal status, because purely basilectal speakers of Gullah (or other Atlantic English-based creoles) are either rare or nonexistent, and also because the distribution of Mrs. Queen's variants, in this as in other subsystems, is closer to basilectal than acrolectal norms. As I note elsewhere (Rickford 1986, 47-48), her 76% frequency of Noun -Ø plurals resembles the more nearly basilectal usage of Nani in Guyana (74%) much more than the corresponding 6–13% frequencies reported for Northern speakers of Vernacular Black English.

4. Compare Joos (1950, 703): "All phenomena . . . which we find we cannot describe precisely with a finite number of absolute categories we classify as non-linguistic elements of the real world and expel them from linguistic science." And Gleason (1961, 393): "Descriptive linguistics is an either-or proposition, and its methods are applied only when the data can be so quantified."

5. Parenthesized codes after each example represent the initials or name of the speaker, the tape number, side and counter number at which the example occurs.

6. The postnominal *dem* in Mrs. Queen's example is plainly of the plural (more than one grandparent) type rather than the associative plural (one or more grandparents in association with specified others) type, since the speaker is referring to the single plot of land her grandparents owned.

7. Note the following additional examples from Mufwene himself (1987, 96): *dem dreams, dem bushes, dem riddles*! Incidentally, contrary to the impression Mufwene gives in citing these examples, the plural *dem* restriction to [+human] nouns noted by Cunningham (1970, 29) and myself (1986, 48) was clearly restricted to postnominal rather than prenominal *dem*. Example (6) above is only a partial counterexample, since it involves both prenominal and postnominal *dem*.

8. Contrary to what Mufwene suggests (49) in introducing these results, the probability coefficients for each factor group do not represent separate, alternative computations, but the results of a single run in which all four groups were considered simultaneously, and their independent effects factored out. This clarification should also help to correct Mufwene's mistaken impression (evident in the final paragraph, 50) that the contributions of the syntactic/semantic constraints and the phonological ones have not been jointly examined.

9. The probability coefficient for existentially asserted NP is indeed .299, as indicated here and in Mufwene (1986, 49). The .200 figure in my paper (1986, 60, n. 19) is a mistake which crept in somewhere between my original manuscript and the final proof.

10. Following the convention of using as a basis for discussion the VARBRUL run with the significant factor groups, I included the results of the level-four run given above as table 1 only in a footnote, and provided in the body of my paper (51) the results of the level-two run which included only the factor groups Preceding Phonological Segment (Nonsibilant Consonant = .654, Sibilant Consonant = .587, Vowel = .271) and Following Phonological Segment (Consonant = .609, Pause = .604, Vowel = .297). However, the coefficients for each factor differ only minimally from those in the four factor run, as comparison will reveal.

11. It isn't possible to handle both the following and the preceding phonological

constraints in a single lexical insertion rule, because the following segment constraint involves application across words and requires a postlexical rule (see Mohanan 1986, 10, Singler 1988, 348).

12. Basically, {-Z} absence is favored by a preceding nonsibilant consonant in both of our data sets, but a preceding sibilant is mildly favoring and a vowel strongly disfavoring in my Gullah data, while a preceding sibilant is strongly disfavoring and a vowel mildly disfavoring in Singler's KPE data. In terms of following segment, a vowel strongly disfavors {-Z} absence in the Gullah data but has exactly the opposite effect in the KPE data. As Singler himself admits (1988, 347), the motivation for the latter effect is unclear.

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