The Question of the Linguistic Reliability of the Ex-Slave Narratives

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I. Introduction

This paper intends to investigate the question of the linguistic reliability of the WPA narratives. These interviews form the basic empirical data for the seminar 'Linguistic Analyses of Ex-Slave Narratives' and therefore it is extremely important for the seminar to evaluate their usefulness for linguistic research.

The interviews with ex-slaves, which were compiled in the 1930s, form a unique corpus for the study of Early Black English (EBE) since they are so numerous and provide us with the opportunity to discover more about earlier stages of today's Black English.

Indeed, if these interviews should prove to be reliable from a linguistic point of view, we would be able to draw many far-reaching conclusions from these narratives. The information gained through these interviews would also be very helpful in helping to resolve many hotly debated questions about today's Black English (BE).

Some of the more important questions which arose among scholars of BE are the possible origin(s) of BE, the question of the former existence of a unified plantation creole and the question of a decreolization process after emancipation.

If it can be established that the WPA narratives represent Black speech in the late 19th century, an investigation of this corpus will show, whether or not there has been a unified creole spoken all over the South and whether or not this creole became decreolized. Moreover, a closer look at the interviews can reveal many features of EBE and also their possible origins. This would enable us to tell with much greater accuracy to what extent African languages might have influenced the formation of EBE and in how far some dialects of English were involved in this process.

The WPA narratives have been used by many prominent scholars studying EBE. Among those are e. g. Edgar Schneider and Jeutonne Brewer. Through these studies interesting facts about EBE were unearthed and this has certainly advanced our knowledge about earlier stages of BE to a great extent.

The interviews have also been used by many renowned historians, like for instance Norman Yetman and Vann Woodward, for whom they represent a fantastic source, providing them with new insights into the lives of African-Americans in the times of slavery. Many great books have been written in that field and much more is known about the "peculiar institution" today than twenty or thirty years ago.

In view of this it is hard to understand, why the WPA narratives had been neglected for such a long time. As early as 1946 it was claimed that they would be a "fifth avenue" to the study of Black history, but then they were virtually ignored for almost three decades.[1] It is only due to the laudable and indefatigable work of George Rawick, who edited them in three volume series, that the narratives have been brought to scholarly attention again.

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[1] Starling (1946) had expressed this optimistic view in her dissertation.

The use of the interviews as a reliable source of linguistic data has been called into question by scholars like Natalie Maynor, Joe Dillard, Michael Montgomery, and Walt Wolfram. They often claim that the linguistic reliability of the corpus has been exaggerated by some researchers. On the other hand they concede that the WPA narratives are nevertheless an important database which cannot be ignored entirely.

In my discussion of the reliability question I will start with a presentation of how the WPA narratives came into being. This entails raising questions about the interviewer, the interviewed and the interview situation. Then a brief look at editing procedures and the principles of editing will be taken. After a short comparison of the Rawick editions of 1972 and 1977/79 and a comparison of some tapes from the Archive of Folklore (AFS) with the WPA interviews, a summary of the reliability question will be given.

II. The Context of the Interviews

1. The WPA and the FWP
a) a short sketch of their history

In the time following the Black Friday of 1929 economies all over the world were in deep recession. In the United States this led to the proposition of the New Deal by president Roosevelt and this meant socialist like measures which brought relief to the poor. One of the organizations that came into existence at that time was the Federal Emergence Relief Administration (FERA). Under its auspices the Works Progress Administration (WPA; later renamed the Works Project Administration) was founded that provided work for the unemployed.

The WPA program included a theatre project, an arts project and a writers' project, which was called the Federal Writers Project (FWP). The latter supported more than 6.000 people experienced in putting words on paper. Among these were journalists, novelists, poets, etc. The FWP had an Ex-Slaves Studies division, which concerned itself with the collection of testimony from ex-slaves.

The earliest endeavours to interview ex-slaves were undertaken in 1929 at Fisk University and Southern University. These independent projects were motivated by a growing awareness that the number of ex-slaves was quickly receding and that valuable information was on the verge of being irretrievably lost. The idea to conduct interviews with ex-slaves in the framework of the FERA program was initially proposed by Lawrence D. Reddick from Kentucky State Industrial College in 1934.

This project was soon abandoned and then in 1936 the FWP started its own collection of interviews, later called the WPA narratives. From 1936-38 John Lomax, the National Advisor on Folklore, supervised the project. He was very interested in the collection of the narratives since they represented a unique treasure for folklorists. He was succeeded by B. A. Botkin, who also showed

great interest in the narratives and who clearly worked on their eventual publication. By the spring of 1939 twenty-three hundred narratives had been collected. Then the project was for various reasons discontinued.[2]

b) issued guidelines

John Lomax issued a standard questionnaire in April 1937 for the WPA narratives, which he did in order to elicit more information and to have comparable material. Following are extracts from his suggestions to the field-workers:

> "[The African-American] should be encouraged to say what he pleases without reference to the questions. It should be remembered that the Federal Writers' Project is not interested in taking sides on any question. The worker should not censor any material collected, regardless of its nature. [...] Any incidents or facts he can recall should be written down as nearly as possible just as he says them, but do not use dialect spelling so complicated that it may confuse the reader. [...] The details of the interview should be reported as accurately as possible in the language of the original statements."

Attached to this document were notes from an unnamed editor on the rendering of dialect:

> "Simplicity in recording the dialect is to be desired in order to hold the interest and attention of the readers. It seems to me that readers are repelled by pages sprinkled with misspellings, commas and apostrophes. The value of exact phonetic transcription is, of course, a great one. [...] Present day readers are less ready for the overstress of phonetic spelling than in the days of local color. [...] Truth to idiom is more important, I believe, than truth to pronunciation. [Now follows a long list of word-spellings not to be used. They will be discussed in context later]"[3]

Henry G. Alsberg, the director of the FWP, likewise made suggestions on how to conduct the interviews. Here follows a short excerpt from a memorandum issued in July 1937 and sent to all state offices of the FWP:

> "The specific questions suggested to be asked of the slaves should only be a basis, a beginning. The talk should run to all subjects [...]. We suggest that each state choose one or two of their most successful interviewers and have them take down some stories *word* for *word*. [...]. *All stories should be as nearly word for word as possible.*"[4]

All the material presented here suggests that the heads of the project were very much interested in the exact rendering of the speech and stories of ex-slaves, although one has to bear in mind that probably the story was more important to them than the linguistic reliability. Moreover, it remains to be seen in how far the interviewers and their supervisors were willing or even able to follow the guidelines.

a) basic questions about the conductors of the interviews

Now we have to address the question of the reliability of the interviewers. It seems most important to me that we know about the actual people involved in the interviews since their biasses and idiosyncracies would greatly influence the linguistic value of the material. Unfortunately we know next to nothing about most of the interviewers and thus I have to confine myself to asking questions

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[2] The proceedings and the history of the FWP are well recorded in Yetman (1967) pp. 540ff and Brewer (1991) pp. 155f.

[3] Both quotations from Rawick (1972), vol. 1, pp. 174ff.

[4] Rawick (1972), vol. 1, p. 173. The italics are part of the original text.

without being able to give definite answers. Some facts about the field-workes can be inferred from the way they wrote the texts, but all in all this is more conjecture than anything else. Nevertheless, it is my opinion that the questions posed here have to be asked if one wants to investigate the linguistic reliability of the narratives from a particular interviewer.

Of great importance to me seems the question of the personal and educational background of the conductors of the interviews. Knowing about the background we could answer questions like what opinions they held about Blacks and what previous experiences they had had with Blacks. Some interviewers obviously seem to have been very condescending towards their informants which resulted in flawed interview techniques and thus in very unreliable narratives. The historian Blassingame thus states: "Many of the interviewers refused, initially, to accept the 'wrong' answers. This was especially the case when the former slaves described their masters as cruel and said that life on the plantation was characterized by unusually hard work."[5]

Woodward, another historian, also claims that "In that climate of the race relations the White interrogators adopted a patronizing or at best paternalistic tone and at worst an offensive condescension. They flouted very nearly every rule in the handbook of interview procedure. [...] the questions were leading, the answers routine or compliant and the insensitivity of the interrogator and the evasiveness of the interrogated were flagrantly displayed. [...] The interrogator regularly got what he asked for."[6]

Most of the distortions resulting from this would be affecting the content of the narrative and not necessarily the phrasing of the sentences. On the other hand it can be assumed that an interviewer with such a mentality would also be more prone to 'adapt' the language of his informant to what he would regard as appropriate Black speech.

Another important question is, of course, the method by which the informants were chosen, since this greatly affects the representativeness of the idiolects for the entire Black speech community. Unfortunately it seems to have been the case that the people to be interviewed were not chosen on the basis of achieving representativeness but rather because of previous personal acquaintance and proximity to the living place of the interviewer.

There are two reasons for this: First there is the question of money. The interviewers were for the most part previously unemployed and relatively poor people and thus their motive for participating in the interviews was to earn money. Taking this into consideration, it makes perfect sense that the FWP people chose informants who were easily accessible, moneywise and in terms of distance.

The second reason that led the interviewers to select informants on the basis of personal acquaintance was that it must have been difficult for a White approach Black people and to single out interesting interviewees, in these times of racial tensions.

A very positive aspect of this is that personal acquaintance would have resulted in a less tense interview situation and thus the information elicited, linguistic or not, should be more reliable.

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[5] Blassingame (1977), p. XLV.
[6] Woodward (1974), pp. 473f.

Another point is that the interviewer would know more about the specific elements of the idiolect of an informant, which could prove to be of great help when he transferred the notes he had taken during the interview to a fully written narrative.

The question of the interviewers' competence is a much debated one and is answered differently by several scholars. Whereas Yetman and Dillard criticize the sometimes conspicuously awkward interview techniques and the linguistic training of the interviewers, Schneider points out that the staff of the FWP "consisted of trained and experienced writers whose profession qualified them fully to write down the text of an interview on the spot, presumably in shorthand."[7] It should also be added that the fact that the interviewers had received no linguistic training can be viewed positively since that made them linguistically unbiassed.

To my mind, the competence of the interviewers can be rightly called into question. But this does not necessarily mean that the interviews are as unreliable as they are sometimes portrayed. Rather this should make us look make more carefully at the interviews when we want to elicit specific information from them. Moreover, there certainly have been many interviewers who were not only interested in what they did but who also proceeded with great skill.[8] A certain drawback is the fact that some forms in EBE which resemble forms in SE but have their own and entirely different meaning were often interpreted wrong by the interviewers.[9] This leaves the modern researcher with the task to re-interpret such forms very carefully. All in all, for the selection of a linguistically more reliable corpus from the interviews it is necessary to evaluate the interviewers according to the points raised above.

b) basic questions about the informants

Here we have do deal we the people on the other side of the table, viz. the interviewed Blacks. Obviously their former experiences with Whites greatly influenced their behavior towards the White interviewers. But since the questions of age, sex, and race will be addressed under 2c a few remarks about the general situation will suffice.

It is well known that the Jim Crow Laws and the racially hostile environment of the 1930s made life hard for the Blacks. Therefore it can be assumed that they were very guarded when they met a White person, especially in such a formal context as an interview. Personal acquaintance with the interviewer was very important since that, presumably, would have eased tensions and resulted in more reliable interviews as pointed out above.

Beside the matter of personal acquaintance, former experiences with Whites also influenced the reliability of the infomation given to the interviewer to a large extent. But in contrast to the sparse knowledge we have about the conductors of the interviews, the narratives tell us quite a lot about the

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[7] cf. Yetman (1967), pp. 551f. cf. Dillard (1993), p. 225. cf. Schneider (1989), pp. 49f.

[8] cf. Brewer (1991), pp. 160ff.

[9] e. g. the interpretation of "she been" as past perfect rather than as remote past, which is unique to BE.

people interviewed, enabling us to resolve the questions asked above, although 'reading between the lines' will very often be necessary, too.

c) the interview situation

This section deals with the problems that arose in every interview without paying particular regard to the individual circumstances.

The first question to be addressed here is how the interviews were recorded. For those interviewers who knew shorthand there should have been enough time to transcribe everything their informants said. But since the interviews took sometimes rather long it is unlikely that this was often the case. If the interviewers, some of whom had been professional journalists, had really written down everything 'word for word,' as they had been requested to do, this would have had several consequences. Firstly it would have produced a very long text. Secondly this would have made the interview situation so formal and would have been so intimidating for the interviewees that they might have refused to answer any more questions (not even the most insensitive interviewer would take that risk). Thirdly it would have preoccupied the interviewer's attention to such an extent that it would have impaired his ability to conduct the interview (another thing a good interviewer would never allow to happen). Therefore it seems clear that in most cases only notes were taken down, which were intended to help the interviewer to write the narrative up at home. Besides, not all of the interviewers would know how to write in shorthand.

Rawick (1977) supports my view, although his emphasis lies on the fact that this procedure makes the narratives less reliable: "The narratives were taken down in pencil or pen, most often after the interviews, from memory or scattered field-notes."[10]

Now I want to investigate more thoroughly questions of race, sex and age that may have influenced the interviews.

It is obvious that interracial and intersexual factors lead to a possible intimidation of the informants. A result of this intimidation was certainly code switching. The interviewees tried to put on their linguistic 'sunday clothes' which casts further doubt on the overall reliability of the narratives.

The situation the Blacks faced was tense. Most of them were very old and largely depended on the dole for living. Therefore, if a White person came and said he was working under a government program, the elderly Blacks tended to misconceive the real power of the interviewers and thus were often very carefully choosing what to say and how to say it. Proof for this contention can be found in the fact that the interviewers were often asked to raise the pensions of the informants, something that was not at all in the interviewers' power.[11]

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[10] cf. Rawick (1977), vol. 1, pp. XXXf.
[11] Rawick (1972), p. XXXII. Blassingame (1977), pp. XLIII ff.

Another factor concerning race was the condescension with which many Whites confronted their Black informants. This, and the fact that almost all these Blacks had been slaves, resulted in their 'puttin' on ole massa,' something that has affected the linguistic reliability to some extent. Accordingly Woodward (1974) states that in his opinion the interviews which were conducted by Blacks were much more to be trusted.[12] But this statement can claim no general applicability. Schneider (1989) has convincingly shown that some Black interviewers were more prone to use Standard English (SE) forms instead of dialectal forms, because they wanted to prove their ability to write SE and held the BE forms in low esteem.[13]

Certainly it was also of importance which sex the interviewers and the informants belonged to. Intersexual factors have surely affected the way the informants told their stories and which stories were told. Brewer (1980) compares two interviews with the same informant, Susan Hamlin, Charleston S. C., by two different interviewers. One of them was August Ladson, a Black male, and the other was Jessie Butler, a White female. Whereas the Ladson interview has many gruesome details and contains much dialect, the Butler interview contains less dialect and paints a rosier picture of the 'peculiar institution.' The conclusions we can draw from this are manifold. Here we can see how the factor race and the factor sex influenced the interview, helping us to rate its reliability. But it remains to be proven in how far the above results can be adduced to test the reliability of other interviews. Personally I regard this as a next to impossible task.

Age is the last factor to be looked at in this context. It should suffice to say that the age of the informants sometimes resulted in their being hard to understand or even to follow. Besides, the fact that the difference in age between the interviewer and the interviewed was so great has surely added to the complication of the interview situation and affected the linguistic reliability of the narratives.

Concludingly it can be said that only a thorough knowledge of the participants of the interviews and of the respective interview situation will help us in rating the reliability of the interview. From what was said above, it is clear that the narratives are no verbatim accounts of the interviews, which again raises the question of their reliability.

III. The Question of Reliability

1. Editing

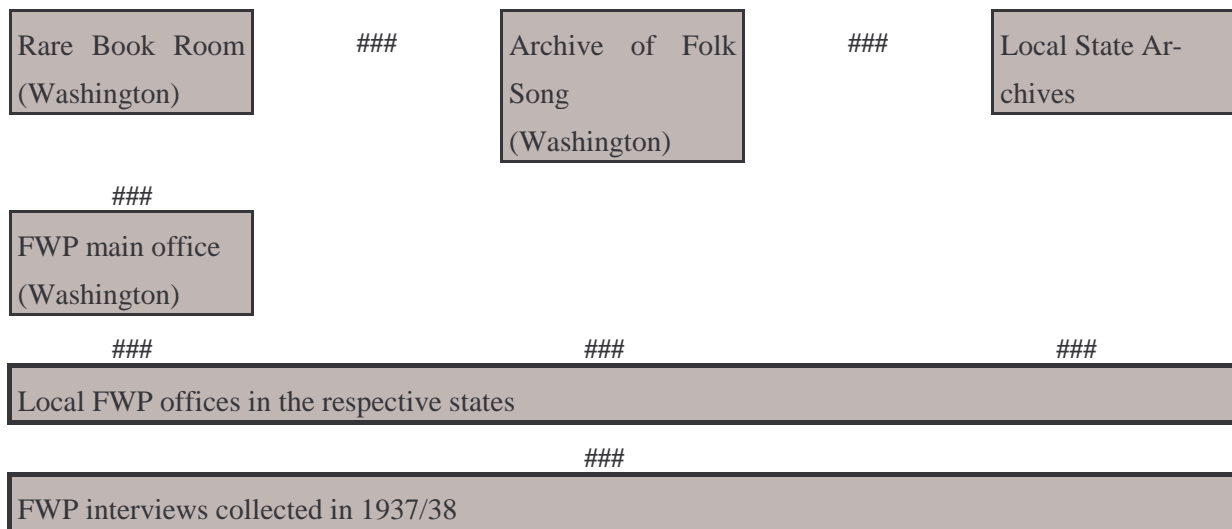### a) the interviews' way from the field-workers to Washington

Since we have no immediate knowledge about the writing up of the interviews, we can only surmise how they came into existence. Blassingame (1977) claims that sometimes "a staff person composed or

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[12] Woodward (1974), p. 474.
[13] Schneider (1989), pp. 11 and 58f.

rewrote the story from the notes of the interviewer."[14] But I suppose that in most cases the field-worker did this himself. In his writing up of the interviews he had four pillars on which he could build the narratives: his **notes**, his **immediate memory** of the interview, his **knowledge of the idiolect** of the informant and his **stereotypical beliefs** about BE in general.[15] Schneider (personal conversation) therefore labeled them as "note supported mental protocols." Bearing this in mind, one can say that the sources the interviewer drew on were pretty reliable, though not exactly verbatim as has been shown above.

Figure 1 shows which way the interviews then took:[16]

| Rare Book Room (Washington) | ### | Archive of Folk Song (Washington) | ### | Local State Archives |

| FWP main office (Washington) |

| ### | | ### | | ### |
| Local FWP offices in the respective states |

| ### |
| FWP interviews collected in 1937/38 |

This figure clearly shows that the editing and selection of narratives was possible on several distinct levels, on each of which different preferences were made. How this affected the relaibility of the interviews shall be looked at next.

b) adherence to the guidelines/editing

It is an undoubted fact that most of the narratives were edited before they came from the field-workers to Washington. Therefore Dillard jokingly called them a collection of "fakelore."[17] There are two obvious reasons that can help to explain why some people in the FWP were prone to editing.

First we have to take into account politics. The American South was at that time still weakened by the Civil War, not only economically but also as regards political influence. Thus many Southerners were eager to make sure that nothing should implicate their still precarious position. Throughout the 19th century the sometimes gruesome tales from fugitive slaves had been used in the North to propagandize against slavery and the Southern way of living. Small wonder then that often stories relating violence and cruelty of the owners towards their slaves were omitted. This can be

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[14] Blassingame (1977), p. XLVIII.

[15] This will be further investigated under the next point under the key-word of "literary tradition."

[16] For this figure I am indebted to my fellow student Ruth Huber who kindly permitted me to print it. The arrows read: "copies sent to"

[17] Dillard (1987), p. 226.

proven easily by comparing interviews in the Rawick 1972 edition with their respective counterparts in the edition from 1977/79.

Secondly there is a very strong literary tradition prescribing how to represent dialect in written language, which has got nothing to do with linguistic accuracy. "I have seen an instance in which the pencil-writing interviewer, with some obvious eye-dialect, writes *I noes*. The colored pencil editor has "corrected" this to *I know*. The deficiencies of the first are as obvious as those of the second."[18]

Dialect in literature tends to be an exaggeration of some characteristic features of a dialect. Also an oversimplification and deletion of features can be observed. Since most of the interviewers can be assumed to have read at least some of these literary products, the forms encountered there may well have influenced the way in which the narratives were written down and also how they were "corrected" as was shown above. It is also thinkable that some interviewers/editors consciously added dialectal features to the speech of a Black informant, in order to make the narratives comply with their condescending attitudes towards Blacks and EBE.

The consequences for the linguistic reliability of the interviews are not to be taken lightly. Dillard, e. g., concludes: "We might ultimately be able to utilize the ex-slave narratives for what I think they are: representative examples of the literary tradition of black speech" and Ken Lawrence, who had helped to edit the Missisipi supplement series, states: "None of the dialects can be considered authentic."[19] In addition, Walt Wolfram shows that some dialectal features were edited in, as, for instance, verbal -*s* endings, the use of *am* (mostly in Texas) and the use of *us* for the 1st person plural.[20] In general, a tendency to represent EBE as less standard and efforts to increase the readability of the narratives can be observed. But this should not lead us to discard the interviews as linguistic evidence altogether. Even if some features have been edited in or out, the editors did not invent any forms. There were only changes in terms of frequency but not in the quality of the forms. Moreover, we have evidence that there are certain characteristic features in the narratives which were not edited in and thus represent a valuable database for the study of EBE.

As a final point, I want to cite some contemporaneous statements from FWP employees and interviewers. This serves to illustrate a few of the points raised above and to shed more light on the individual circumstances.

In a letter from the Supervisor of Assignments and Files to the State Director of Missisipi the former reveals great zeal to comply with the official guidelines, therefore potentially providing us with linguistically reliable interviews:

> "Please note attached ten autobiographies. All of these are in the same phraseology as recorded by the field-workers. No revision has been attempted other than a partially standardized form of spelling. [...] No doubt the Folklore editor will change a number of our colloquial spellings. Our policy in editing has been to use contractions of words in a number of instances. This is really in

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[18] Dillard (1987), pp. 227f.

[19] Dillard (1987), p. 229 and Rawick (1977), vol. 6, p. XCIV. The underlining is part of the original text.

[20] Wolfram (1990), pp. 122ff.

line with our Negroes mode of speech. [...] The sequence of the narrative has not been altered in an effort to produce a more interesting or redable story. [...] I have made no revision of form as I did not want to risk a stilted, formal, or unnatural narrative. [...].“[21]

The State Director of South Carolina, on the other hand, seems to be keen on deliveringblameless interviews to the Washington office:

"Please follow carefully the instructions, originating in Washington, which have been sent to you from time to time, particularly those relating to modified dialect. Consult the list of tabooed words and do not use them. Our desire is for easy reading and a complicated dialect does not produce that result.“[22]

In this context it is now time to talk about the official list of "taboo“ words already referred to under II. 1b. This list is an obvious encouragement to alter the interviews. Although it was meant only to increase readability, it undoubtedly was misused by some interviewers and editors. Marjorie Woods Austin, an interviewer from Mississipi, protests against this: "Never in my life have I ever heard a negro say de for the. [...] However, since "de“ seems to be part of Washington's idead, fine, I am using it--under protest. [...] I have not used 'mammy' as of your correction because none of the negroes have used the word.“[23]

c) the Rawick edition of 72 vs. the edition of 77/79

After having investigated the editing process and the motives for it, I shall nowlook more closely at specific examples. The 1972 edition of The American Slave consist of the interviews Rawick found in the Rare Book Room in the Library of Congress in Washington. Later he surmised that there might be additional interviews in the state archives which, for some reason or other, were not sent to Washington (cf. Figure 1). He succeeded in finding thousands of additional pages which he later published in his supplemental volumes in 1977/79. The narratives he found were not only new ones but also different versions from some that had already been found in Washington. It is a widely accepted fact that the narratives in the 1977/79 edition are earlier in date, i. e. they are less edited than those published in the 1972 edition. The latter contain more dialect and less cruelties and are therefore supposed to be linguistically less reliable.[24]

In order to establish differences between the interviews in the 72 and the 77/79 edition and to look at the possible implications on the study of EBE this could have, I have compared some findings in Schneider (1989), who used the 1972 edition as a basis, to my own findings, using the 77/79 edition. I chose to investigate the use of relative pronouns in ten Texas narratives from the 77/79 edition with Schneider's findings for the use of relative pronouns in twelve Texas narratives.[25] Ten narratives I used were only different versions from the interviews Schneider looked at, leaving only

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[21] printed in: Brewer (1980), pp. 41f.
[22] printed in: Rawick (1977), vol. 2, pp. 323f. The underlining is part of the original text.
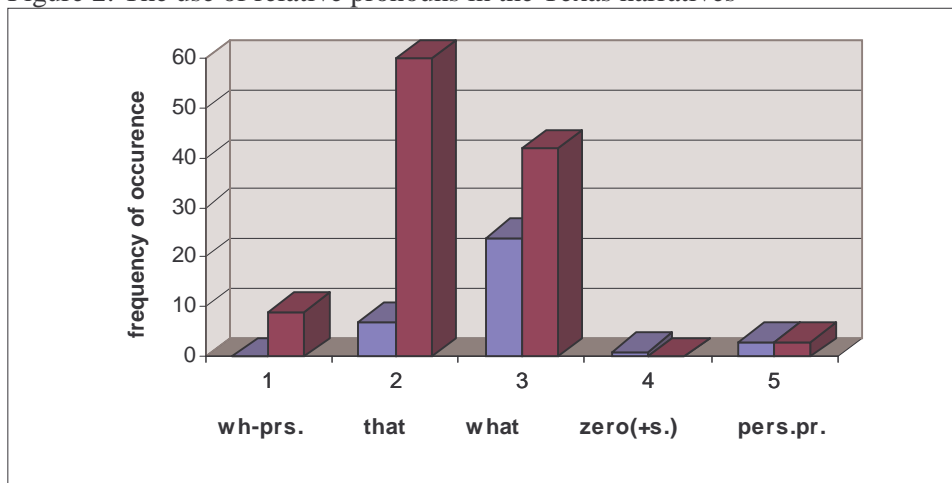[23] printed in: Maynor (1988), pp. 112f.
[24] cf. e. g. Montgomery (1991), pp. 175ff and Maynor (1988), pp. 113ff.
[25] The narratives I used from the 1977/79 edition were those of James Brown, Amos Clark, Carey Davenport, Mandy Hadnot, Rosina Hoard, Mrs. Thomas Johns, Cinto Lewis, Hiram Mayes, Elsie Reece, and James W. Smith.

two which do not correspond. Therefore a significant difference between the two results would suggest that much editing has taken place. On the other hand, if the differences should be only slight, it can be maintained that, although the respective interviews differ greatly in length and some in content, their linguistic content was not changed too much, i. e. this would be proof of their linguistic reliability.

I did not take into account the use of the *zero pronoun* in sentences where it does not refer to the subject because the exact number of these were very hard to establish in the narratives I looked at. In Schneider (1989) this form of the *zero relative pronoun* was the dominant form used by the informants. It was used 27 times by ten speakers. In my interviews I found only six instances of this in six different narratives. This result is possibly very revealing but I chose not to include in my figure since I was not sure I had found all the instances.

Figure 2: The use of relative pronouns in the Texas narratives

The figure reads: # of occurences of the respective relative pronouns in the two samples. The left column always denotes Schneider's findings and the right column my own findings.

The rare use of the relative pronouns *zero (+subj.)* and the *personal pronoun* as a relative pronoun seems to be quite similar in both cases. Also the use of the *wh-pronouns* is so rare in both corpi that there seems to be no real difference. Perhaps they have been consciously edited out to make the narratives look more dialectal but since they are not the dominant forms this is negligible. The differences in the uses of *that* and *what* seem to be more significant. The much higher frequency of these forms in my corpus corresponds to the much lower frequency of the pronoun use of *zero (-subj.)*, which is not included in Figure 2. It is to be noted, however, that all these forms are correct in SE (although th usage of *what* is often incorrect). It can be surmised that the replacement of *that* and *what* by the *zero pronoun (-subj.)* was intended to make it look less standard. All in all, my findings do not support the contention that the changes that were made affect the linguistic reliability of the interviews at large. There have been obvious changes in the frequency of the forms but the use of the markedly non-standard forms of zero (+subj.) and and the use of the personal pronoun as a relative pronoun is hardly changed. Thus it can be stated that, although the interviews from the 1977/79

edition undoubtedly underwent less revision, the interviews from the 1972 edition can yet yield linguistically reliable results.

2. The Tapes from the Archive of Folk Song (AFS)
a) reliability

The tapes from the Archive of Folk Song are recordings with ex-slaves from the 1940s and 50s. The equipment used weighed, in the first years, approximately 500 lbs. and later 250 lbs. The recording material were aluminium discs, which meant that the sound quality got worse every time the tapes were played. Therefore, only 24 out of the 31 interviews available are of a quality that permits their transcription (=> ~30.000 - 35.000 words of transcript). Thus they form a very small corpus in which the idiolects play an overimportant role. This has to be borne in mind when they are compared with the written narratives.

The linguistic reliability of the recordings is the best we can have. Although some points can be raised that diminish their overall reliability somewhat John Rickford rightly states: "The tapes are the closest we can get to ultimate reliability, since we can listen and relisten, count and recount, note the specific points on which investigators seem to disagree and attempt to resolve them."[26]

The interview situation was undoubtedly much more intimidating than in the other interviews since the massive hulk of the machine could not be overlooked making the interviewee more guarded. But Paul Escott claims that "the recordings suggest that many informants did not feel highly constrained or uncomfortable, but all of them knew how to assuage white feelings, if necessary."[27]

A last point concerning the reliability of the tapes is their poor recording quality. On some points even experts who have heard the tapes over and over again do disagree and Rickfrod admits that "When I received copies of the reel-to-reel tapes and began checking the sutdents' transcripts and tabulations against the tapes, mistakes occurred so frequently that I was forced to abandon the exercise."[28] But this is not a major obstacle since we now have excellent transcripts of the tapes, although standardization of forms in doubtful cases was the rule among the transcribers.
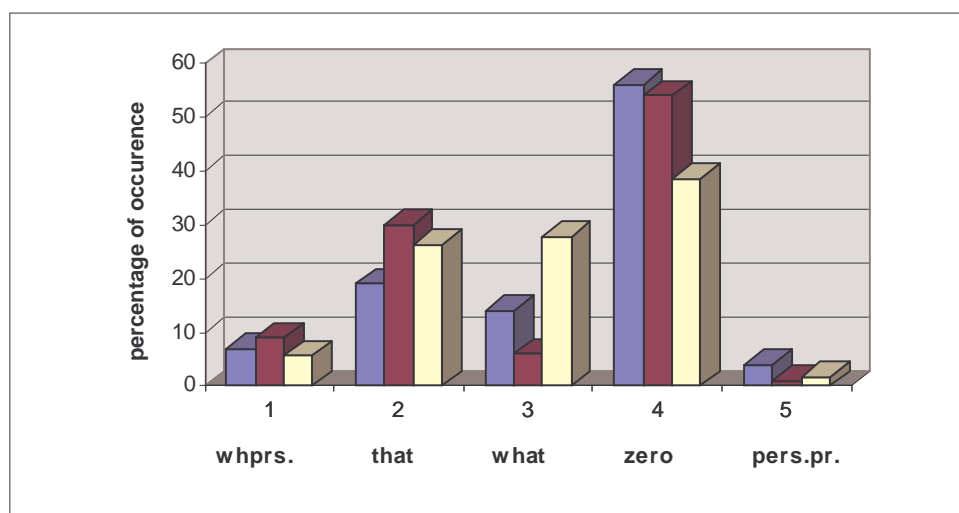
b) comparison with the interviews

The tapes have frequently been used to test the linguistic reliability of the WPA narratives. Figure 3 shows a comparison drawn by Montgomery (1991):

Figure 3: Pronoun use in 11 Rawick interviews/11 tapes/according to Schneider (1989)

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[26] Rickford (1991), p. 195.
[27] Escott (1991), p. 132.
[28] Rickford (1991), p. 195.

Montgomery is of the opinion that these figures indicate that the Rawick interviews have been tampered with to an extent that makes them unreliable. He suspects they were approximated to SE when they were written down. He therefore concludes: "[...] their value may well be limited to that of literary dialect."[29]

But these figures can also be interpreted differently.It can be seen easily that all three corpi agree that the *wh-pronouns* are not often used, that *that* occurs at a frequency of ~ 25%, that the *zero-pronoun* is the dominant form and that the use of a *personal pronoun* for a relative pronoun is very rare. The only significant difference in frequency is the use of *what*, which is very restricted in the tapes and more frequent in the Rawick interviews and in the results of Schneider. In the latter two, *what* and *that* are used approximately with the same frequency, whereas their frequencies differ considerably in the tape sample. To me that seems to be rather irrelevant. Of course, it can be claimed that the more frequent use of *that* reflects an approximation to the standard use but that does not affect the reliability of the narratives very much since the use of *that/what* has never been part of any theory on BE or EBE. The conlusions drawn from this feature are minimal and, more or less, of no great interest.

Besides, it has to be taken into consideration that the sample of eleven Rawick interviews is almost ridiculously small. How can they ever be seen as representative of the whole corpus of the Rawick narratives? Also the sample of eleven tapes is too small, but this is excusable because the number of tapes available is only 24. Nevertheless, it has to be stated that the idiosyncracies of an informant play a far too great role in such small corpi and that any statistical differences do not reveal much about the actual usage of relative pronouns by Black speakers in these times. On the contrary, the fact that the figures corresponded to a very large extent should be taken as a confirmation of the reliability of the Rawick interviews.

3. Summary of the Reliability Question

First I want to look at the representativeness of the collection of narratives. The number of ex-slaves interviewed amounts to 2% of the whole number of ex-slaves still living at that time. This is

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[29] Montgomery (1991), p. 188.

statistically a very large number and thus the interviews can be seen as being representative for EBE, although the process of choosing the informants did not comply with "normal" statistical procedure.

Woodward pointed at the fact that the narratives overrepresent males, urban residents and house servants.[30] This is obviously true and leaves us with a quizzical problem. However, this affects only the reliability of our results and not the reliability of our source and shall therefore be left unanswered now. Similarly, the question of how representative an interview is of a single idiolect is also very important. But the answering of this question would occupy too much space and lead us too far away from our topic. Therefore, it is left out, too.

Brewer makes a strong case for the reliability of the interviews by stating: "First, the narratives contain examples of lexical items which dialect geographers have found significant in their studies of regional dialect. Second, such linguistic characteristics as the variable use of be suggests the WPA interviewers were attemptimg to record what was related to the interview rather than to standardize it to preconceived linguistic stereotype. [...] Third, the narativess contain linguistic characteristics found to be significant in recent sociolinguit studies of present-day BE."[31]

On the other hand, Montgomery casts doubt on Brewer's very optimistic evaluation by pointing out that we indeed have multiple versions of the same account and that the narratives do not contain features characteristic of spoken language like false starts, interruptions, sentence fragments etc. For him the obvious fact that the interviews were edited renders them valueless for serious research. This point of view seems to be extreme as well, although the questions he raises are worth asking.

A very convincing argument concerning reliability was forwarded by Schneider (1993).[32] He looked at the three-verb cluster *have/be + done + verb-pp*, which was widespread in the Middle English period but later became restricted to the north of England. It was transplanted to the United States and has been recorded in Southern dialects in particular. Although this three-verb cluster is very infrequent in modern BE it often and exclusively appears in WPA narratives from the early-settled states South Carolina, North Carolina and Georgia. This is an extraordinary result because it makes sense linguistically and extra-linguistically and no kind of editing could have achieved that.

What can be deduced from all the facts given above is, that we should not insist on the reliability of the individual instance or even the individual interview. Despite of this, it can be assumed that the general frequencies of the corpus are trustworthy. It is impossible to say that the interviews as a whole are reliable or are not reliable. Rather we deal with a continuum of reliability. Some narratives are more to be trusted than others and there the need arises to evaluate the individual interviews in advance before adding them to a research corpus. To my mind, this is of extreme importance. However, if we look too much at an interview before we do research with it there is the danger that we might move in a circle, i. e. that we, consciously or not, choose the interviews that fit

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[30] Woodward (1974), p. 472.
[31] Brewer (1980), p. 50.
[32] for the following cf. Schneider (1993), pp. 214f.

our prospective results best. Therefore, we should exclude the features from our research that we looked at to decide upon the reliability of an interview, as it has been done by Schneider (1989).

Another point to be taken into account is that not all issues are equally suited for research. That is the reliability of our results not only depends on the reliability of our material but also on the questions we ask and how we ask them. Certainly, points of grammar can be investigated with much greater reliability than the phonology of EBE since sentence structures are not as likely to be changed as the spellings of individual words. Especially such major questions as the assumed existence of a plantation creole extending all over the South were successfully challenged by using the WPA narratives as data.

Even someone who calls the linguistic reliability of the ex-slave narratives into question has to admit that they are the most reliable, representative and exhaustive source we have. The interviews have many limitations as has been shown above but, to my mind, no serious study of EBE can afford to ignore them.

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