New Perils for the Contract Ethnographer

David M. Fetterman*

The conditions of contract research may lead some workers to ignore publication rights of colleagues whose reports are of limited circulation. This type of activity may occur and be downplayed when it does occur because contract research is conducted outside the traditional academic environment. The businesslike environment, "inhuman" deadlines, and pressures to "produce" may generate abuses. Contract research, however, must be bound by the same rigorous ethical standards as research done in the traditional academic environment if free exchange of ideas and information is to occur.
ETHNOGRAPHIC EVALUATION; ETHICS; CONTRACT RESEARCH; PUBLICATION PROTOCOL; SCHOOL ETHNOGRAPHY.

The purpose of this article is to establish in print prior claim on published work and at the same time to call attention to a possible peril in contract research. Research corporations routinely publish reports of limited circulation. The fact that research reports are often of book length and not widely disseminated makes these publications vulnerable to abuse. The author has presented a case example of how this process occurred recently.

Some striking similarities between two separately published research reports warrant examination. The first was published by the author, David Fetterman, at RMC Research Corporation, Mountain View, California. It was entitled *Study of the Career Intern Program: Interim Technical Report—Task C: Functional Interrelationships among Program Components and Intern Outcomes* (September 1979). The second report was published by Ray Rist, M. Hamilton, W. Holloway, S. Johnson, and H. Wiltberger at Cornell University. The Rist et al. report was entitled *Targeting on In-School Youth: Four Strategies for Coordinating Education and Employment Training,* Interim Report No. 3 (April 1980). Ray Rist was the principal investigator. One fourth of the Rist et al. methodology section is virtually identical in content and phrasing to a section of the methodology published in the earlier report by David Fetterman. Yet there is no citation giving credit to the prior author or to RMC Research Corporation. Both materials in question are presented in the body of this discussion. This dilemma is similar to those discussed by Clinton (1976) and Colfer (1976), in contract research. An essay written by Eugene Garfield, "From Citation Amnesia to Bibliographic Plagiarism" (1980) presents an excellent discussion of the problem. The author believes that an open

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discussion of this problem may prevent a repetition of such incidents in the future. In addition, this watchdog approach is intended to ensure the viability of the contract ethnography field for future researchers.

Context

The author is currently responsible for the ethnographic component of a national evaluation of alternative high schools for dropouts. The National Institute of Education (NIE) awarded the contract to RMC Research Corporation on April 16, 1978. The first report for the ethnographic component of the study was published in September 1979 (RMC Report UR 340). The report included a detailed description of the methodology employed in the task. Ray Rist was retained by NIE to review the published report. He reviewed the RMC report in October. He presented his comments in person at RMC Research Corporation in early November 1979. The methodology section was discussed for three hours, covering each technique and how the data were analyzed, as well as the problems encountered in the study. Rist informed the president of the corporation that he was "favorably impressed" with the methodology employed by the author and the progress of the task.

In May 1980, RMC received a copy of the Rist et al. report. The study, *Youthwork National Policy Study*, is funded by Youthwork, Incorporated. The author was initially struck by the similarity between a sentence found in the Rist et al. report (published in April 1980) and a sentence from the earlier RMC report (published in September 1979). One sentence in a report, however, is certainly not significant. The possibility of independent invention exists. The two sentences are reproduced next for the reader's convenience.

> The role was more like that of a student interested in learning about how the pieces of a puzzle fit together than a traditional evaluator who enters the picture with explicit a priori assumptions about what the system is and how it works (Fetterman 1979).
>
> The role has been more that of a student interested in learning about how the various pieces of a puzzle fit together than a traditional evaluator who enters the setting with explicit a *priori* assumptions about what the system is and how it is supposed to function (Rist et al. 1980).

The interest in the coincidental phrasing would have diminished immediately, however, had the similarities not continued in the ensuing pages. The most striking similarities between the reports are found in the methodology chapters. Fetterman's pages 13 to 18 (published September 1979) are presented in the following section alongside pages 25 to 29 of the Rist et al. report (published April 1980) for the reader to compare and evaluate. The author's primary interest in this endeavor is to establish in print prior claim to particular research and to discourage such negligence by others, rather than to chastise any individual.[1]

Report

The author's text is presented first, on the left, and the Rist et al. text is on the right for comparison by the reader. The most unsettling component of this

comparison is the fact that certain words and paragraphs have been systematically altered or deleted. The words selected for alteration or deletion are only those that are study specific. For example, the word "interns"[2] has been changed to "students"; the paragraph beginning, "Participant observation was conducted at the site as described earlier," was deleted; similarly, the paragraph beginning, "In the study, the author made use of a minimum of three to five key informants per site," was deleted; and so on.[3]

In addition, the author has recently become aware of the fact that this same portion of the text can be found in a second earlier report of Rist's in a less complete form (see pages 18 through 20 of Ray Rist et al., *Education and Employment Training: The Views of Youth*, Youthwork National Policy Study, Cornell University, November 1979). This report was published two months after the author's report was published and one month after Rist officially reviewed the author's report.

Study of the Career Intern Program: Interim Technical Report—Task C: Functional Interrelationships Among Program Components and Intern Outcomes, David Fetterman (September 1979)

Participation Observation

Ethnographers attempt to immerse themselves in an environment to understand the situation or the system—allowing impressions and patterns to emerge from participation with, and observation of participants. Ethnographic field work is guided by grounded theory (see Glasser, 1967). This involves developing testing hypotheses and theories by interacting directly with the empirical reality observed. Field work of this type, according to Malinowski (1961), can only be done through long months of residence at the local scene. Of necessity, the field work conducted for this study was done on a very different schedule. It was admittedly too brief, but the amount and quality of data that were collected suggest that Malinowski's position may be overstated for studies of American subcultures (if site visits are spread out over a period of time).

As Pelto (1970) writes:

Targeting on In-School Youth: Four Strategies for Coordinating Education and Employment Training. Interim Report No. 3, Youthwork National Policy Study. Ray Rist et al. (April 1980)

Participant Observation

Ethnographers attempt to immerse themselves in an environment to understand the situation or the system—allowing impressions and patterns to emerge from their participation with, and observation of people in their natural settings. Ethnographic field work is guided by grounded theory (cf Glaser and Strauss, 1967). This involves developing hypotheses and analytical frameworks by interacting directly with the empirical reality. Traditionally, field work of this type was only assumed possible through a long and intensive period of residency on site. Of necessity, the field work conducted in this present study was done on a somewhat different schedule. An important variation from the classical model was that the observer was not "in residency," but was a member of the local community who visited the site on an average of one day per week. The amount and quality of data that were collected over the months and months of these weekly visits suggests, though, that the traditional model may be overstated for study of American organizations when conducted by Americans themselves. As Pelto and Pelto (1970:92) have written:

> Every individual is a participant observer—if not of other cultures, then at least of his own. But the typical nonanthropological resident in a foreign community returns to his native haunts with a very unsystematic and incomplete picture of the scene he has observed. Field work requires much more than simply "being there" and passively watching what people are about. Often the fieldworker, in observing a particular pattern of behavior or an event, needs to find out a great deal more about that event than he is able to observe firsthand. His personal theoretical frame of reference suggests to him sets of questions to ask; relationships of this event to other types of data must be explored, and a host of other materials must be considered in order to make individual observations useful. In cases where the fieldworker feels that a significant block of information is available to him simply through his observation of a particular type of event, he may nonetheless need to devise ways of ensuring the representativeness and objectivity of his observations in a series of repetitions of the given event. By structuring observations and systematically exploring relationships among different events—through interviewing, watching, and perhaps administering "tests"—participant observation can be converted to scientific use.

Over time, repeated patterns of behavior emerge and are identifiable, even if observation is noncontinuous.

Participant observation was conducted at the sites as described earlier. Specific activities included informally interacting with interns' parents; "hanging out" in the hallways or the side of the building with interns and staff; going out with intern friends to their "hang outs" in the street, attending Pentacostal church services with interns; being invited to wrestling matches; dancing at a CIP disco; participating in a spelling bee in class;

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doing assignments; and teaching a class* at one of the programs.

Nonparticipant observation was characterized by simply observing interns and staff interacting in the classrooms, hallways, the cafeteria, and outside the building.

Key Informant Interviewing

"Working with informants is the hallmark of ethnographic field work," according to Spradley and McCurdy (1972). The difference between a respondent and an informant is that a respondent will respond to specific question (usually honestly) whereas an informant answers specific question and then supplies additional, unsolicited information both related and unrelated to the question), giving the researcher a broader view of the situation. Spradley and McCurdy (1972) explain the process and difficulties in selecting informants:

> The ethnographic field worker must locate helpful people, win their cooperation, and establish a close, personal relationship with them. This task is not simple, because it involves a *basic conflict*. On the one hand, the ethnographer establishes a *relationship of trust* with his informants. It is desirable that this be productive and beneficial to both parties. Often it is marked by friendship. On the other hand, the ethnographer seeks to know things that informants may be *reluctant to reveal*. Indeed, they may perceive that the researcher is asking them to tell secrets about other people to whom they are loyal. At the very least, they will be asked to talk about what they know in a manner that is new to them. Some of the ethnographer's questions may be embarrassing; others are outright stupid.

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This basic conflict is exacerbated when one is perceived as an evaluator. Gen-

role as discussed earlier. Generally, working alone rather than in teams creates a less threatening atmosphere more conducive to gathering data. Some informants are better than others, some individuals have had a great deal of experience in their current social situation and know their culture or sociocultural system well, e.g., the school. Another important characteristic of a good informant is his/her willingness and ability to talk or communicate. Many interns share selections of their poetry, assignments, or segments of their diary as a way of communicating (in a nonanalytic manner).

In this study, the author made use of a minimum of three to five key informants per site in order to increase the reliability of the obtained data. Four procedural stages were employed to maximize the utility of the data collected from them. Developing a good rapport was the first step. It was undertaken in order to decrease fabrications. Asking what the informers believe and what they think others in their group believe rather than only asking about their personal opinions was a second step. Asking the same question during successive interviews to check for consistency was a third step, and triangulation, which is discussed later, was a fourth step taken in this study to increase the reliability and validity of the information. Key informants who cooperated in the study included directors, conselors, instructors (e.g., staff members), intern leaders, popular interns, secretaries, janitors, and community members (e.g., clergy and merchants).

Informal and Structured Interviews

Informal interviews. Many of the data were collected during informal interviews with interns and staff members, at lunch, or after school. The purpose of using informal interviews was to collect data in normal, "natural" settings. Information collected in the natural setting is more likely to reflect real conditions and constraints operating on the individual. This approach avoids many of the problems associated with role playing as

erally, working alone rather than in teams creates a less threatening atmosphere which is more conducive to gathering data. The value to a researcher in having a key informant is that this person knows their setting from the inside and has had experience in their current social situation, e.g., the school. Another important charcteristic of a good informant is his/her willingness and ability to talk or communicate.

Informal Interviews. Many of the data were collected during informal interviews with students and staff members during lunch or after school. The purpose of using informal interviews was to collect data in normal "natural" settings. Information collected in the natural setting is more likely to reflect real conditions and constraints operating on the individual. The approach avoids many of the problems associated with role playing. In

discussed earlier. In addition, this approach mitigates many of the problems that exist in the laboratory setting where artificial stimuli (stimuli isolated from the context in which an individual would actually be operating) produces an artificial response (a response that reflects the artificialness of the laboratory). Informal interviews were conducted at staff members' houses, an American Indian graveyard, a bar, in the author's car, a coffee shop, hotel rooms, "fronts," classes after school, staff offices, in the streets, and in interns' homes, among other places.

[Site specific section deleted-Ed.]

Case Studies and/or Expressive Autobiographic Interview

> Key informant interviewing frequently becomes so important in anthropological fieldwork that extensive personal documents are collected from a small number of persons with whom the anthropologist has especially good rapport. The anthropologist is attracted to collecting extensive materials from persons who are unusually eloquent and sensitive in their presentation of personal and cultural data. Thus, in most cases, life histories represent the exceptional rather than the representative or average persons in the community. In spite of this fact, the richness and personalized nature of life histories afford a vividness and integration of cultural information that are of great value for understanding particular life ways. (Pelto 1970, p. 98–99)

Case studies of individual intern's background and progress through the program continue to be compiled to document the types of interns in the program and their development while in the program. Expressive autobiographic interviews have been used thus far to develop the case studies. The expressive autobiographic interview according to Louise Spindler is:

> a cross between a structured interview and a chronological autobiography. The respondent is asked

addition, this approach mitigates many of the problems that exist in the laboratory setting where artificial stimuli (stimuli isolated from the context in which an individual would actually be operating) produce an artificial response (a response that reflects the environment of the laboratory). Informal interviews have also been conducted at the homes of staff members, bars, sitting on the hood of a car, a coffee shop, staff offices, and on the streets.

Biographical Case Studies. Case studies of individual students, their background and progress through the program, continue to be compiled. These studies document the development of students as they progress through the program. Expressive autobiographic interviews have been extensively used thus far to develop the case studies. The expressive autobiographic interview according to Louise Spindler is:

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> to tell the story of his or her life but intervention by the anthropologist at critical-points . . . relevant to (specific topical points of interest) . . . turns the autobiography to relevant considerations and permits an economy of time that is not possible with the full autobiography. (Spindler & Spindler, 1970, P. 293).

Two interns per cohort per site have been tracked (with two alternates per cohort per site in case of unforeseen problems). The data collected about these individuals have been integrated into the study rather than portraying their lives and progress in the more common narrative format. The type of information gathered regarding the case studies has been outlined and is presented below.

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The data collected from these intensive interviews with the youth have been integrated into the present study by means of illustration. In this manner, the impacts of programs can be portrayed in the lives of individual youths.

Conclusion

The conditions of contract research may lead some workers to ignore publication rights of colleagues whose reports are of limited circulation. The conditions of dissemination of findings and communications within the network of contract research are conducive to abuse. Research practices and findings in their formative stages are discussed in informal as well as formal contexts. These reports are often of book length and contain the results of intensive fieldwork done under difficult and sometimes threatening conditions—as in the instance of the Career Intern Program in which the author is currently working as an ethnographer.

This type of activity may not only occur but be downplayed when it does occur because contract research is conducted outside the traditional academic environment. The businesslike environment, "inhuman" deadlines, and pressures to "produce" may generate abuses. Contract research, however, must be bound by the same rigorous ethical standards as research done in the traditional academic environment if free exchange of ideas and information is to occur.

Endnotes

1. It is important that no individual be unduly chastised for such behavior. The pressures and reinforcements for such behavior abound in research. In fact, as a colleague informally shared, "given the reinforcement contingencies that exist in academia, what is truly surprising is how rare it is." This environment also fosters a reluctance to confront such behavior. The act and any open response are often classified as taboo behavior. This article follows the work conducted in *Science* magazine over the last six months regarding the same type of behavior. The aim is to sufficiently document the case to allow independent readers to assess the situation. In this regard, a few additional details may be useful to the reader. The author notified the second party

of this serious matter in the second week of June 1980. The author received mailed correspondence approximately one and a half weeks later, which suggests that the delay was due to the postal system. The author, however, decided to pursue other channels at this time to respond to the matter. The author also notified the second party of his actions. The second party assumed personal and professional responsibility for not providing "a citation as to the source of that bit of material." The author found the response unsatisfactory, continued to pursue the appropriate channels, and continued to maintain correspondence with the second party regarding the author's actions. The second party called the author in early July 1980 only after receiving the author's decision to pursue additional channels of recourse (e.g., professional journal). The second party's explanation at that time was that it was "a simple omission of reference." The author explained why the response was still unsatisfactory: the behavior occurred twice (and in a more complete fashion the second time); long selections were appropriated verbatim without permission; minimal but systematic changes were made; the individual personally reviewed the specific work in question for over three hours; and the selections involved one fourth of the second parties' methodology section in a totally different and separate study. The most recent explanation offered by the second party is that "it was an omission of a footnote" in his second report. The footnote would have explained how the material was taken in toto from the author with minimal but systematic alterations. This is precisely what the author requested that the second party use in all future reports in which the author's material was to be used by the second party. The author brought some of the problems with this after-the-fact explanation to the attention of the second party. It is the author's understanding that, minimally, it is customary to request permission for such an unusual use of an author's material. When the author asked whether the lost footnote explanation also applied to the 1979 report, the first time the author's material was used in an unauthorized and unacknowledged manner, the second party did not recall ever using a footnote or reference and offered no explanation for that occurrence. Finally, when the author queried the second party regarding how the material in question (a significant portion of its methodology) could be placed in the final version of the first report but not be included in the draft, considering the research had already been conducted, no response was forthcoming.

2. The Career Intern Program is an alternative high school for dropouts and potential dropouts. Program participants (or students) are called interns to avoid a potential stigma associated with the term student.
3. The only citation in this section of the text found in Rist's list of references is Pelto and Pelto 1970 (which should read Pelto 1970 to be accurate); all other citations in this section were not listed in the reference list (e.g., Glaser and Strauss 1967; Spradley and McCurdy 1972; Spindler and Spindler, 1970; as well as Fetterman, 1979).

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Editor's Note

David Fetterman's article was submitted by him on June 6, 1980. We received it on June 12. On June 18 we sent it for review to three readers in accordance with the editorial policy for *AEQ* which we set in 1977 (From The Editor, 8:192). On June 18 we also sent a blinded copy of the paper to Ray Rist and invited him to let us have his views on it *before* we made a decision to publish (or not). Rist did not reply to us. On July 7 we received a letter from Fetterman telling us that Rist had called him to ask him to modify his paper, but that Fetterman felt it should be reviewed as submitted. Subsequently, the reviewers' comments came in strongly in favor of publication, and no one recommended against publication. We concurred that the paper raised an important general issue, and on August 20 we wrote Fetterman advising him that we were accepting his paper and suggesting some minor revisions concerning style and presentation. At that time we again wrote to Rist, this time informing him of our decision to accept Fetterman's piece and soliciting a reply to be published with the article. Subsequently (September 8), Rist wrote us for the first time and submitted his reply, which is printed next. Professor Rist's reply was then sent to Fetterman, whose answer follows it. (C.H.)

Shadow versus Substance: A Reply to David Fetterman

Fetterman's piece is an unfortunate but poignant example of the meanness of spirit that has overtaken so many involved in educational research.

In his effort to build a straw man challenge to his academic integrity through insinuation and innuendo, what he has in fact accomplished is quite the opposite. He has taken an error made without malice or intent on our part and sought to parade his own moral virtue.

We know that Fetterman has sent this piece to, among other places, at least one other journal (which declined to publish it) and to one quasi-governmental agency. It was done, as he later wrote to us, because "I did not receive an immediate response from you." Our explanation and apology were written and posted the same day we received our first communication from him. But Fetterman has seemingly not been interested in either our apology or explanation. He has cast many lines into the water, hoping from one to get a bite. That the present journal bit hook, line and sinker is not the issue, only that Fetterman has been out to score points, not to resolve the situation.

What we have freely and willingly admitted to Fetterman is that 1) we have used his materials (because we thought them appropriate), and 2) we had intended to do so, and that 3) in either drafts two or three, the indication that we were using approximately two pages of his text and three quotes *in toto* (save some minor changes, e.g., the word "intern" to "student") was omitted and never again picked up by the time Fetterman received the fourth version of the report. We have freely acknowledged that when one writes a report by commitee (there were five of us involved and the report totaled 241 pages), omissions such as these can occur. But for Fetterman to perhaps enhance his *vita* in this way is unwarranted.

Fetterman's position is further eroded by the fact that he received the report directly from us. He appears to imply that he came across the report in some indirect manner. We have not tried to hide it from him or any of his colleagues. Indeed, we even paid the postage. Had we sought to abscond with his two pages of text, we would not have then taken the effort to mail him and others at RMC copies of the report.

As noted, the error was just that. That Fetterman seeks a moral crusade over what he views as "negligence" is, of course, his choice. That the facts do not match his fervor is but a further contribution to the erosion of the research community.

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Protocol and Publication: An Ethical Obligation

The issue has been to establish in print prior claim to a published work, and at the same time to call attention to a possible peril in contract research. My article was written to that end. This communication is a response to Rist's essay. "Shadow versus Substance," and a brief discussion of a few issues raised by this experience.

First, the decision to publish the article was not a simple one. Many factors were weighed. The pros and cons were analyzed and reanalyzed regarding the moral issue, individual careers, and the future viability of a newly emerging field. I knew from the beginning that this was a no-win proposition. Contrary to the charge that this will enhance my vita, I recognized that the publication of this article would have anything but a positive effect on my career, as Rist pointed out in our last phone conversation.

Second, I initiated specific steps to resolve this problem because I did not receive an immediate response from Rist. I did not, however, decide to publish the article for this reason. In fact, I informed Rist that, despite the two instances in which he used my material in an unauthorized, unacknowledged manner, I was more than willing to withdraw from all steps taken upon receipt of a satisfactory response. In addition, I agreed to discuss the use of pseudonyms with the editor before making my final decision. It was the opinion of the editor and reviewers (paralleling the position held by the editor of *Science* in a similar situation) that the "article raises an important and general point which of course must be documented in order to be made." In fact, I informed Rist of each measure taken and of each agency notified to resolve the problem as per my obligation. The decision to publish the article was based on the following set of circumstances:

1. Regarding Rist's first report, no explanation to date has been offered for the unacknowledged use of three pages of my material essentially verbatim.

2. Rist personally reviewed my published report, asking in detail about each technique used in the methodology section.
3. Rist's explanation regarding his second use of five pages of my material was unsatisfactory, dismissing the issue in a cursory fashion as an "omission."

These questions were not addressed in his explanation:

1. Systematic alterations were made throughout both of his reports in which my material was used (e.g., the word "intern" was systematically changed to "student"), site-specific paragraphs were deleted, and so on. It is unusual to make systematic alterations when quoting material.
2. Contrary to standard publishing policy, the material was not indented or set off in any way in either of his reports. Rist did not cite in his bibliography, with the exception of Pelto, those references found in the portions of my work used in his reports.

I continue to believe it is important that no individual be unduly chastised. These actions, however, should not be condoned. Moreover, this behavior should be discussed openly. The pressures and reinforcements for such behavior abound in research. The underlying problem touched on by the article and Rist's reply is that our professional environment generally fosters a reluctance to confront such behavior. Open confrontation of these problems is often classified as taboo behavior, an unwritten code of the academic brother or sisterhood. The case study presented is only a symptom, not the cause, of a much greater problem facing the modern research community.

David M. Fetterman

INFORMATION FOR AUTHORS

Manuscripts (*in triplicate*) should be addressed to the Editor of the *Anthropology & Education Quarterly,* Box 19, Teachers College, Columbia University, New York, N.Y. 10027. All papers appropriate for the AEQ are sent anonymously to readers. Only the title should appear on the manuscript itself; all identifying material (name and affiliation) should be restricted to a cover page which will be removed when the paper is sent for review. All identifying references and footnotes should appear on a separate removable page.

An abstract of 50–75 words must accompany each article, followed by a list of not more than five words or phrases under which the article can be indexed.

In all other aspects as to style and reference, manuscripts must follow the style guide of the *American Anthropologist.*