

1-1-1991

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Recommended APA Citation

McCorkle, M. (1991). A Retrospective Account of Some Qualitative Research. *The Qualitative Report*, 1(2), 1-10. <https://doi.org/10.46743/2160-3715/1991.2043>

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Keywords

qualitative research

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A Retrospective Account of Some Qualitative Research

by Marguerite McCorkle

The Qualitative Report, Volume 1, Numbers 2 and 3, Winter/Spring, 1990/1991

I was asked to discuss my dissertation project of some years ago, A radical behaviorist study of "women's experience of conflict" (McCorkle, 1978). It was suggested that I consider the sort of choice points I perceived in putting together that particular work. And, of course, I will be glad to undertake the assignment. However, it certainly brings to mind many questions on my part.

By training, I have come to view my acts of professional communication as consisting of at least two aspects: 1) capturing my own thoughts in some verbal form, and 2) framing what I have to say in language common with my potential (or assumed) audience. In this case, I need to make clear that the context of my work was a mid-1970's clinical training program and its awkward relationship to the radical behaviorist community of which I was also a part. I was the only clinical student who was simultaneously pursuing interests in radical behaviorism, and so it fell to me to bring clinical issues to the one community, and a discussion of the philosophical concerns of radical behaviorism to the clinical program. I took on the role of translator. I recall the difficulties I encountered in trying to write in a language common to both groups, and it seems equally challenging to describe the issues of that time to the current reader. My plan is to provide you with a brief glimpse of the sort of work that was done by our group, and then to summarize my dissertation and highlight those choices that still might be relevant to people concerned with research practices that are currently labeled "qualitative."

Emerging from a radical behaviorist view of language was an awareness and acknowledgement of the fluidity or transitory nature of what we had to say--we tended to be interested in use, not definitions. We articulated the issues of our position in relation to the other intellectual forces that we encountered, and the particulars of what we had to say were expected to vary with our audience. It is more in keeping with the spirit of our work to offer to you an updated statement of that position, which previously has been described as a behavioral phenomenology (and sometimes is spoken of as the "Reno Method").

The most significant spokesman for this approach to psychological and philosophical inquiry was Willard Day. He and a small group of graduate students at the University of Nevada-Reno, worked over a number of years to formulate and demonstrate a radical behaviorist approach to a functional analysis of human verbal behavior. I quote from one of his recent papers:

From a professional standpoint I tend in certain respects to associate this outlook on how to conceptualize experience with a philosophical perspective that is coming increasingly to be of interest to researchers in the social and behavioral sciences and that is spoken of as hermeneutics*. In taking the realities of effective ordinary discourse as the bedrock for epistemological confidence and trust, a hermeneutical approach would not break a purported realm of experience apart from a realm of matter and then proceed to analyze these realms in

terms of analytical units that are taken to be real. Thus the world of people's experience would not be assumed a priori to be composed of ideas, thoughts, sensations, perceptions, images, and so on which can be regarded as real entities independent of social practices that have enabled us to talk and think about them.

The world of people's experience is to be known by an analysis of what they have to say about things, where what people have to say is taken in a sense to be a "text" that is then subject to analysis. Yet even though I would want to associate my own type of thinking to some extent with hermeneutics, particularly in its opposition to strategies of epistemological reduction, I have no doubt that my thought has its source much more fundamentally in the radical behaviorism** of B. F. Skinner. This is of course because my research and teaching skills have been engaged in the discussion and analysis of the implications of Skinner's thought essentially for the whole of my professional life. Indeed, it has been my own particular row to hoe to call professional attention to the important conceptual affinities that exist between radical behaviorism and phenomenological concerns, such as are characteristically involved in the analysis of conscious experience. (Day, 1987, p. 13, draft copy)

It was frequently surprising to people unfamiliar with our work to accept that we would ground our approach to the understanding of verbal interactions in radical behaviorism, especially when what we had to say seemed incongruent with their general notions of what behaviorists believed, e.g., we eliminated the then-usual subjective-objective dichotomy; we emphasized the importance of description and interpretation, and made explicit our interest in the role of the researcher.

While there were significant differences between our interests, rationale, and approach to the analysis of psychological and philosophical concepts from other behaviorists, we shared with a limited radical behaviorist community a predilection for philosophical discourse. A glimpse at the sort of territory we traveled can be seen in this brief list of some titles of Day's papers: "On certain similarities between the Philosophical Investigations of Ludwig Wittgenstein and the operationism of B.F. Skinner" (Day, 1969); "Radical behaviorism in reconciliation with phenomenology" (Day, 1969); "On the behavioral analysis of self-deception and self-development" (Day, 1977); "A behaviorist looks at the surviving work of Justin Martyr" (Day, 1984); "Hermeneutics and behaviorism" (Day, 1988).

My own papers attempted to move towards a dialogue with family and systemic therapists and behaviorally-oriented clinicians, and frequently focused on research practices, especially those appropriate for analyzing "talk," for example, "Radical behaviorism and systems theory: Implications for clinical and research practices" (1980); "Metaphor and technology: What computers can and cannot do in the analysis of verbal behavior" (1982); "What they are saying about how women (and men) talk" (1983); "What are looking for? (Reflecting upon our research biases)" (1986).

In brief, this orientation derived from a particular (and peculiar) understanding of the writings of Skinner, and was used as an analytic framework for psychological and philosophical discourse. The specific work that needed to be done varied over a period of time, as different audiences and/or various issues came to the fore or were retired. It should be said that while we were exceedingly excited about our approach, we were frustrated that much of the time we needed to

dispel certain preconceptions of others regarding what Skinner or we held to be central to our conceptual framework.

Moving to my initial work along these lines, I offer the abstract to my dissertation (McCorkle, 1978) as an introduction to the type of discussions we generated, and as a text of the period:

This project is an attempt 1) to articulate an alternative approach to psychology that is outside the current mainstream orientation, and 2) to develop research methods that are compatible with that approach. The conceptual basis of the proposed orientation is radical behaviorist, especially as it relates to scientific methodology.

The central focus of the dissertation lies within the realm of the philosophy of psychology. Psychology, in spite of itself, is largely connected to philosophical issues. The current practices of psychologists leave these issues unarticulated and functioning as assumptions. The presuppositions under question have to do with the view of psychology as a natural science, which leads to notions about the proper subject matter of psychology, its appropriate methodology, and to ideas about what constitute legitimate data. The proposed orientation suggests that a psychological position based on a different set of propositions would legitimize other forms of investigation, allow for a different conception of data, and hopefully will lead to an articulation of important questions concerning both the behavior and experience of human beings.

Within this project, the proposed methodology is aligned with a "human science" value-orientation to psychological research. Chapter I provides an historical perspective of the human science view, and some of the recurring issues involved with that point-of view are presented. Chapter II outlines a theoretical rationale for a methodology based on radical behaviorism. Important concepts considered from a radical behaviorist perspective include: knowing, discrimination, and description. Chapter III surveys similar non-experimental methods applicable to psychological research, and some of the basic contrasts between radical behaviorism and ethology, phenomenological psychology and "naturalistic" research are presented. Chapter IV introduces the topic of a demonstration study, to follow in Chapter V--"women's experience of psychological conflict," particularly those conflicts that have variously been described in recent years as role conflicts, identity problems, or conflicting self- concepts as female. Both theoretical and "experiential" material have been included. Feminist positions based on Freudian, Marxist, and Jungian theories are presented, in addition to more directly personal statements selected from women's diaries. Chapter V presents an example of how radical behavioristic research may be approached with an emphasis on analyzing the research procedures which resulted from assuming the proposed orientation. The major purpose of the demonstration study was to contribute to the development of methods appropriate to a study of verbal behavior (i.e., communication) consistent with radical behaviorist epistemology. A second, underlying concern of the researcher was to provide examples of the relevant behavior, histories, and environmental conditions which seemed relevant to the participants' particular experience of "conflict." (vi-vii).

In developing my argument in Chapter I, I sketched, historically, certain recurring criticisms of the structural and reductionistic (and essentially quantitative) framework prominent in what came to be the mainstream view of modern social scientific inquiry. The particulars of this

historic tradition may not be of interest here, except to say that they represented our position in contrast to the 19th century experimental paradigm, and aligned us with an alternative tradition, citing the works of Dilthey, Brentano, Husserl, Giorgi, Skinner, and Day, among others. In general, these authors were considered in light of their interests in science as a human activity, and with their understanding of the role of description, analysis and interpretation of human activities within context.

The point of Chapter I was to lay out a theoretical rationale for the proposed research practices. The concepts and supporting arguments were discussed within a radical behaviorist framework. While a number of topics were considered, central to the orientation was our notion of scientific knowledge, by reference to what we mean by "knowing"; what radical behaviorists mean by 'the analysis of contingencies,' 'causation,' and 'control'; and a discussion of the role of the observer (elimination of the objective-subjective dichotomy).

The arguments presented at that time were formulated to address both behaviorists and other psychologists with a greater commitment to experimental methods; as differentiated from our group, where we viewed ourselves as more interested in and sensitive to the philosophical implications of the works of B.F. Skinner (e.g., *Verbal Behavior*, 1958; and *About Behaviorism*, 1974) and to issues related to the functional analysis of verbal behavior (which includes non-verbal behavior and an interest in meaning.)+ Fundamental to our work was a major interest in how language functioned, and regardless of the particular philosophical issues under scrutiny, our analytic framework continued to call attention to language use. This focus would show up in our work as careful attention to the details of what people said or wrote; and you would find that our work includes the extensive use of transcripts and the citation of much longer quotations that this generally seen in academic journals.

Chapter III was my least favorite section, and it generally reflects the demands of my dissertation committee. I do not mean that I reacted against the discipline of comparing our evolving approach with other professional viewpoints; it was rather that I might have selected other positions for such a comparison, probably outside of psychology. However, the committee wanted me to place my work on the professional scene as they saw it, and in complying with their request discussed my approach in relation to 1) ethology, 2) the so-called "naturalistic" research used in the general psychological and social psychological community, which essentially meant non-experimental, and then with 3) the phenomenological method advocated by Giorgi (e.g., 1970), which was of interest to our group.

After months of correspondence between Giorgi and Day, Giorgi spent a week with our group, which allowed for a fairly in-depth exploration of the two approaches. While there were some theoretical clashes, the research practices showed considerable similarity. In addition, our interaction was beneficial to us as students, in that it increased our familiarity with Husserl, Heidegger, Gadamer, and other European scholars and the intellectual traditions from which their thinking was emerging. I think that in general this increased our sense that some of the constraints and frustrations we were encountering within our small arena were not unlike the dissatisfactions being expressed across many disciplines. Certainly by the mid- to late 1970's the call for new methods had become commonplace. In a subtle but pervasive way, we developed and maintained a certain openness with regards to how various researchers talked about their

search for alternative modes of inquiry. I in no way intend to minimize the important differences that arise from taking various theoretical positions, but at the same time, there is no need to ignore strong similarities that can be seen in the research practices themselves, regardless of the words researchers might use in explaining and justifying their methods.

Chapter IV functioned as a review of the literature for the specific topic of my demonstration study--"women's experience of psychological conflict." That is, the first chapters of the dissertation were essentially theoretical and provided a rationale for alternative research approaches. Chapter V was to allow me the opportunity to design a research strategy that was consistent with the theoretical position I wished to maintain. Chapter IV, through its presentation of sampled literature, provided my readers with two alternative approaches to the topic of women's "conflict." This general topic had been receiving considerable attention in the popular press at the time, and the academic community was not far behind.

Since I was turning away from traditional psychological formats in developing my research, I did not review studies undertaken from an experimental orientation except to refer the reader to a review article which included 148 references to then-current studies concerned with a re-evaluation of prevalent social views toward women (Mednick & Weissman, 1975). Instead, I presented three theoretical positions relevant to the "psychology of women." Essentially, the reader was presented with a summary and discussion of Psychoanalysis and Feminism (Mitchell, 1974), in which Mitchell describes those Freudian concepts she finds necessary to foster relevant professional analysis; in addition, she presented alternative interpretations of Freud's work from within the psychoanalytic community. A second framework for analyzing our social-psychological environment was discussed via a review of Charnie Guettel's book *Marxism and Feminism* (1974). Thirdly, a theoretical position based on the work of C. G. Jung was considered, *The Way of All Women--A Psychological Interpretation* (Harding, 1933; 1970), where the discussion focused on an analysis of the relationships between men and women. In concluding that section of the literature review, I made a particular choice that had considerable impact on the shape of the subsequent research. I presented it this way:

That each of the positions contributes something to our professional understanding of women's psychological experience is only the first step. Questions remain as to which portions of these sophisticated theories would actually be useful in accounting for the "psychological realities" in the lives of many women. In attempting to design research projects in which some of the issues may be confronted we may find it important to distinguish between research efforts directed towards (1) refining our professional verbal capabilities by undertaking the empirical analysis of the theoretical-conceptual language (professional verbal behavior) relevant to an understanding of "women's issues," or (2) assessing more directly the descriptions provided by women when they attempt to verbalize their problems, questions, or issues related to their particular experience of "being a woman." (McCorkle, 1978, pp. 119-120)

The consequence of this choice was the methodological move of allowing women to speak for themselves, which I did first by a review of literature typical of autobiographies or private journal entries. I made the decision to ground my work in what these women had to say about important aspects of their own lives, and not to base my work on professional conceptualization about women's experience handed down by prominent social scientists. Along these lines, I

introduced passages from Hannah Tillich's book *From Time to Time* (1973), Anais Nin's *The Diary of Anais Nin* (Vol.1, 1931-1934), and from *the journal of Carolina Maria de Jesus* (Moffat & Painter, 1975) -- a black, Brazilian woman born to an illiterate farm worker mother; who with two years of school and three children to raise, wrote a book to sell as her only way out of the favela, the slums of Sao Paulo.

The second kind of literature reviewed was not formally "theoretical," but rather was considered as examples of the kind of verbal behavior one might expect from a person trying to describe this or her own experience. This literature was presented in support of the notion that we, as professionals, have much to learn from simply talking to people. I argued that research undertaken with this approach in mind would produce a line of research remarkably distinct from research designed to test (the goodness of fit of) our theoretical models.

For the purpose of demonstrating research methods, three women were interviewed for ten, 1-hour "self-investigation" interview sessions; the sessions were tape-recorded and observed by at least one other investigator from behind a one-way mirror. The observer made notes of the major topics discussed, tracked the approximate time spent in various segments of the conversation, noted the introduction of specific questions by the interviewer, and was free to add any of her own questions or thoughts as they occurred during the session. These notes formed the first outline for a "catalogue" of each session, and functioned as the initial index to the taped interactions. Immediately following the interview sessions, a "debrief" session was held between the interviewer and the observer; these sessions were also recorded. The "debrief" procedure was used to allow the researchers an opportunity to verbalize their perceptions, concerns, questions, etc. Within a day or two of the interview, the research team met to construct the session summary (i.e., the catalogue). In general, two or three re-listenings of the tape were necessary prior to constructing the catalogue entry. The tape for a given session was reviewed and notes were taken; descriptions of the session were written with the aim of maintaining as much of the original text as convenient. For the purposes of the demonstration study, only some sections of the interview were transcribed for further analysis, although the tapes were available and other selections could be transcribed subsequently.[^] This was seen essentially as a way of collapsing or condensing the data.

Because of the reflective nature of my dissertation, the description of each procedural step was accompanied by some commentary pointing to how the researcher's behavior was engaged in generating or transforming the data. For example, the process of creating a transcript calls on the researcher to organize and interpret the originally auditory phenomena; such actions as supplying punctuation or other notation imposes considerable structure on the verbal material. While such commentary may seem utterly obvious and unnecessary by today's standards, at the time and place of my original work, such an examination of the interaction between the researcher and data was unexpected.

The results of this work, from my point of view, consisted of (1) the development of a theoretical rationale for alternative sets of investigative procedures, which was necessary to justify a range of practices that did not fit the then-prevalent view of professional research; (2) the presentation of interesting texts of women discussing situations in their own lives that could be construed as relevant to the professional understanding of "sex role conflicts," (3) the discussion of proposed

methods for handling tape-recorded verbal material, where maintaining the tapes (and transcripts) and allowing for on-going access and repeated (long-term) analysis of such material was the goal, rather than generating or testing hypotheses with regard to the obtained material.

The conclusions I reached about research methods at the end of my dissertation project are summarized below. I shall not attempt to update, refine, or amend what I had to say at that time, although much has occurred during the intervening years. It is certainly possible that the reader may be confronted with the certain aspects of a radical behaviorist position with which they will be uncomfortable. I hope, however, that we may discuss those issues at another time.

Attention should be called in particular to the following methodological implications of the preceding theoretical rationale....

- (1) The presentation of any verbal material whatsoever...is made largely for the purpose of shaping desired discriminative behavior in the careful reader, and of influencing the reader's subsequent verbal behavior so that it functions effectively within relevant professional discourse.
- (2) The primary purpose of research is to put the researcher in a situation such that professionally interesting behavior can be directly observed by the researcher.
- (3) The adaptive function of the direct observation of behavior [or of texts] by the researcher is focused on the capacities of the observation to shape new, professionally relevant discriminations in the behavior of the researcher.
- (4) The primary professional obligation of the researcher who has acquired new discriminative repertoires as the result of the direct observation of behavior is no more that simply to respond verbally under the control of what has been observed....
- (5) The professional training of radical behaviorists leads them to bring to the behavior of direct observation a particular kind of conceptual equipment, which is the product of their professional training. Salient features of this conceptual equipment are: (a) a tendency to make sense of observed behavior in ways that avoid mentalism; and (b) a focal interest in spotting the functioning of environmental variables as they act to control behavior.
- (6) In informing others of the steps taken in the course of a program of research, the primary focus of attention is on the behavior of the researcher as the program happens to have been actually carried out....Thus reports of research are largely descriptions of the researcher's behavior throughout the course of the research and of the environmental circumstances [the context] that have acted to control his/her behavior. (McCorkle, 1978, pp. 68-69)

Essentially, what I think we want from research is a richer understanding of some aspect of our culture, environment or experience. I am asking for a descriptive account from colleagues who are positioned to make some sort of contact with the world that I am not. A report of your research activities should include a description of what you saw, what you did, what conceptual framework you brought to your endeavor, in what way you want the data to function, and any other information regarding the context of your work which will allow the reader/listener to make sense out of what you are doing.

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Footnotes

*(re: Footnote 7 from Day's paper, 1987) "Trying to be simple about the matter, I sometimes say that `hermeneutics means the study of what is involved in interpreting written texts'" (Day, 1986). Zukier (1987) has recently scanned the evolving connotations of the term as follows: "[Traditionally,] Hermeneutics is the theological discipline of interpretation of the divine scripture. In the 19th century Dilthey used the term to characterize a theory of interpretation of cultural and symbolic products, such as signs and rituals, art and literature. Contemporary hermeneutics, reinvigorated by the work of Gadamer (1975), has evolved from a method of textual interpretation into a general philosophical theory of understanding" (p. 707). These remarks come from a review by Zukier of the book *Narrative Psychology* by Theodore R. Sarbin (1986). Within this context Zukier observes that Sarbin hopes to amplify: "*Narrative Psychology* contains very little theoretical background or argument; it is, rather, a collection of 15 chapters by various contributors, which are presented as illustrations of research within the hermeneutic framework. Sarbin proposes the hermeneutic, or narrative, approach as the general `epistemological crisis' and from the `disillusionment' with `traditional research'....Sarbin offers the conventional hermeneutic critique of 'mainstream' science, adapted for psychology. The crisis stems from the low predictability and lack of generality of the traditional research findings" (p. 708). For an excellent overview of the way in which hermeneutic stances are currently being adopted in philosophy and social science see Woolfolk, Sass, and Messer (in press). For a note on affinities between hermeneutics and contemporary radical behaviorism see Day (in press).

** (re: Footnote 8 from Day's paper, 1987) The simplest thing to do here is to let Skinner speak for himself in this regard. He puts the matter in a very straight forward and non-technical way in Chapter I on "The Causes of Behavior" of his book *About Behaviorism* (1974). For my own involvement in the matter, I have discussed in detail Skinner's concentration on the inclusion of private events as part of the field of behavior analysis in distinguishing radical from methodological behaviorism in my paper "On the Difference between Radical and Methodological Behaviorism" (Day, 1983). I have discussed the investment by radical behaviorism in the analysis of private experience in my papers on "Radical Behaviorism in Reconciliation with Phenomenology" (1969), "Methodological Problems in the Analysis of Behavior Controlled by Private Events" (1971), "Analyzing Verbal Behavior under the Control of Private Events" (1976), and "On Skinner's Treatment of the First-Person, Third- Person Psychological Sentence Distinction" (1972).

#Within the field of psychology, this view of science was institutionalized as experimental psychology, and may be referred to as methodological behaviorism. For the purposes of intellectual discourse, it must be differentiated from radical behaviorism, a philosophical position based largely on the writings of B.F. Skinner and Willard Day, and other behaviorally oriented psychologists whose work can be found in the journal *Behaviorism*. Historically, the terms and labels used to differentiate these approaches to research have varied, but certain elements or themes re-emerge. Over time, the issues have been argued with reference to the Wundt-Stumpf controversy (Leibzig vs. Wurzburg schools), reductive vs. phenomenological research, experimental vs. empirical research, structural vs. functional explanations, etc.

+ With regard to the current reader, this radical behaviorist interest in language may be said to have many similarities with an interest in the pragmatics of human communication.

^ A detailed transcript of one of the interviews was provided so that the reader would be familiar with the sort of conversations that constituted the data.

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