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What is This?

8

An Evolving Logic of Participant Observation, Educational Ethnography, and Other Case Studies

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INTRODUCTION

The aspiration of the author in this chapter is straightforward but wide ranging: to provide a context and logic for the discussion of the genre of research that is coming to be known by such varied labels as educational ethnography, participant observation, qualitative observation, case study, or field study. For the most part, I will use these terms as synonyms.

Such an aspiration is warranted for several reasons. First, the method seems very simple, and some methodologists perceive it this way. Biddle (1967) called it "the broadest and simplest methodology used in classroom studies." The researcher goes into an educational setting with a pencil and pad, makes a few observations, takes some notes, and writes a report, a dissertation, or a book. For some it seems almost this easy; others stumble about and have incredible difficulty. Second, a growing group of researchers, evaluators, and policymakers (e.g., NIE's Experimental Schools, NIE's School Capacity for Problem Solving, Nuffield Humanities Evaluation, OECD's Center for Educational Research and Innovation, and NSF's Case Studies in Science Education) have been urging inquiry and evaluation using these techniques. Third, several major methodologists have disparaged the use of such procedures: the two most significant statements are Scriven's (1967) classic paper on evaluation and Campbell and Stanley's (1963) classic chapter on experimental research in education. The former attacked "process studies" and "noncomparative evalua-

tion." The latter took a moral stand on the design these authors call X–O, or the "one-shot case study."

The Campbell and Stanley (1963) position on the one-shot study is:

Much research in education today conforms to a design in which a single group is studied only once, subsequent to some agent or treatment presumed to cause change. Such studies might be diagrammed as follows:

XO

As has been pointed out (e.g., Boring, 1954; Stouffer, 1949) such studies have such a total absence of control as to be of almost no scientific value. The design is introduced here as a minimum reference point. Yet because of the continued investment in such studies and the drawing of causal inferences from them, some comment is required. . . .

In the case studies of Design 1, a carefully studied single instance is implicitly compared with other events casually observed and remembered. The inferences are based upon general expectations of what the data would have been had the "X" not occurred, etc. Such studies often involve tedious collection of specific detail, careful observation, testing, and the like, and in such instances involve the error of *misplaced precision*. How much more valuable the study would be if the one set of observations were reduced by half and the saved effort directed to the study in equal detail of an appropriate comparison instance. *It seems well-nigh unethical at the present to allow, as theses or dissertations in education, case studies of this nature* (i.e., involving a single group observed at one time only). (pp. 176–177; italics added)

Being faced with such an edict, and with accumulating contradictory personal research experience, posed an interesting dilemma to which we responded in various ways. In part we quoted other authorities. George Homans (1962) for example, introduced his discussion of the strategy of industrial sociological research with the epigrammatic comment: "People who write about methodology often forget that it is a matter of strategy, not of morals. There are neither good nor bad methods, but only methods that are more or less effective under particular circumstances in reaching objectives on the way to a distant goal" (p. 257). And we referred to the more devastating style and argument of Howard Becker (1970) that methodology is ∤"too important to be left to the methodologists," and to his data, a presentation of the minimal overlapping of the research methods of the chairmen of the American Sociological Association's section on methodology and the methods used by the winners of the most prestigious sociological research awards (pp. 3–7).

In addition, we have tried to confront the issue directly with students and colleagues at Washington University in St. Louis. At one point some years ago, the following question was part of a set of Ph.D. examinations there:

Gouldner's books, *Patterns of Industrial Bureaucracy* and *Wildcat Strike*, are classics in the eyes of some social scientists. The methodology seems to be what Campbell and Stanley have called the "X–O" or "one-shot case study" (attached is a quote from their

discussion). They state explicitly, "It seems well nigh unethical" to permit such research. Take a position on the apparent dilemma and indicate how you would reason through toward a solution. Illustrate with reference to the substance of Gouldner's monographs.

In an important sense, this chapter attempts to develop a broader position within education, social science, and philosophy and to join the debate in the educational research community. The goal is to isolate significant methodological issues which can be examined in some detail, both theoretically and empirically.

In a recent unpublished version of his Kurt Lewin Award address, Campbell (1974) backed off from his earlier position. In a brief paragraph he makes the following points regarding qualitative case studies: (1) "such studies regularly contradict the prior expectations of the authors" (p. 24), (2) [such studies] "are convincing and informative to skeptics like me to a degree which my simpleminded rejection doesn't allow for" (p. 24), (3) "such a study [is given] a probing and testing power which I had not allowed for" (p. 25). In a concern for the division of labor in most large-scale projects, with the resulting partial knowledge of the several specialists, he also comments: "A project anthropologist, sociologist, or historian, assigned the task of common sense acquaintance with the overall context, including the social interactions producing the measures, could often fill this gap" (p. 25). While at heart Campbell remains a quantitative experimentalist and quasi experimentalist, his more general points are "Social knowing . . . is a precarious and presumptuous process" (p. 29), and "If we are to be truly scientific, we must reestablish this qualitative grounding of the quantitative in action research" (p. 30).

My point in raising the Campbell and Stanley position and the reactions to it has to do with the difficulties in any prescription of research methods and procedures. The implications are several. First, such prescriptions are changing and evolving standards, group norms, if you like, of research communities and subcommunities within education and social science which are also evolving and changing. As such they may have a kinship with moral prescriptions. There are now a number of commentaries on the sociology of knowledge and scientific communities (Crane, 1972; Hagstrom, 1965; Kuhn, 1970; Lakatos & Musgrove, 1970; Ziman, 1968). These changing communities are one meaning of the "evolving logic" stipulated in the title to this chapter.

A second implication of the illustration, and of the title of the essay, is pedagogical. I will draw heavily on my own and my colleagues' research and methodological reflections to illustrate many of the general issues and problems. In a sense this is a reflexive mode of presentation, an attempt to try out Becker's suggestion for a "natural history." In this way a concrete, integrated, and contextual statement explicating the general arguments will be provided. Thus I believe I am stating, in a particular setting, Toulmin's (1972) more general point:

This thesis can be summed up in a single, deeply held conviction: that in science and

philosophy alike, an exclusive preoccupation with logical systematicity has been destructive of both historical understanding and rational criticism. Men demonstrate their rationality, not by ordering their concepts and beliefs in tidy formal structures, but by their preparedness to respond to novel situations with open minds—acknowledging the shortcomings of their former procedures and moving beyond them. . . . The philosophical agenda proposed here sets aside all such assumptions in favor of patterns of analysis which are at once more historical, more empirical and more pragmatic. (pp. vii–viii)

The theory under consideration in this essay is a theory of methodology within education and social science. It is a theory constructed to help solve methodological problems in studying teachers, curricula, classrooms, and schools.

Such a methodological description and analysis should culminate in a provisional codification of criteria and procedures which indicates a little about "how to do an educational ethnography or a participant observer case study," and it should present to a judge of ethnographic research proposals, or a reader of participant observer project reports, an image of a provisional set of criteria for a "good observational case study" or a "good educational ethnography."

DOMAINS OF KNOWLEDGE

General Overview

The general educational research community has only recently discovered participant observational research. A corollary to this discovery is the lack of knowledge of the substantial body of research that has been carried out with this genre of methods. In some quarters the belief seems to be that there is little precedent for such work and that there have been minimal attempts to speak to the methodological issues underlying the inquiry. To rectify this impression, four tables of references are presented here. While they are not exhaustive, they should destroy the belief regarding little precedent, and the citations are numerous enough to enable the reader to begin his own program of criticism or self-training. In rough fashion they have been grouped into four clusters. Table 1 includes general references: studies of non-Western culture, modern communities, formal organizations, and informal small groups. Mostly they have been carried out by anthropologists, sociologists, and political scientists. Table 2 presents studies of educational settings: school and community, school systems, elementary and secondary schools, classrooms, and curricula and special programs. Table 3 lists major, seminal, methodological statements from fields other than education, and Table 4 lists those from education, many of which would be described as educational evaluation.

The most obvious limitation of these tables occurs in the slighting of the traditional anthropological research. In addition, the huge case study literature in clinical and individual psychology has been omitted (e.g., Freud, Erickson, and Piaget). Finally, the related large body of literature on comparative psychology and animal ethology has not been included. For an early statement see Scott

TABLE 1
Domains of Observational Case Studies: General

Non-Western Cultures	Modern Communities	Formal Organizations	Informal Groups
Firth (1957, 1959)	Arensberg and Kimball (1940)	Arensberg & MacGregor (1942)	Becker (1970)
Malinowski (1922, 1935)	Barker & Wright (1954)	Blau (1955)	Festinger, Riecken, & Schachter (1956/1964)
Mead (1930)	Bruyn (1963)	Goffman (1959, 1961)	Gump, Schoggen, & Redl (1957)
Radcliffe-Brown (1922/1948)	Kimball and Pearsall (1954)	Gouldner (1954a, 1954b)	Homans (1950)
	Warner & Lunt (1941)	Lipset (1950); Lipset, Trow, & Coleman (1962)	Humphreys (1975)
		Polsky (1962)	Liebow (1967)
		Redl & Wineman (1957)	Whyte (1955)
		Selznick (1949, 1952)	Whyte (1953)
		Yunker (1977)	

TABLE 2
Observational Studies of Educational Systems

School and Community	School Systems and Interorganizational Educational Systems	Schools	Classrooms	Curricula and Program Evaluation	Teaching Careers and Student Teaching
Henry (1963, 1966)	CERI (1973)	Atwood (1960)	Cazden, John, & Hymes (1972)	Applegate (1971)	Becker (1951)
Singleton (1967)	Lutz (1962)	Barker & Gump (1964)	Cicourel (1974)	Beittel (1972, 1973)	Eddy (1969)
Spindler (1963)	Pohland (1970)	Becker, Geer, Hughes, & Strauss (1961)	Delamont (1976)	Berlak, A., et al (1975)	Finch (1978)
Warren (1967)	Schumacher (1975)	Charters, Everhart, Jones, Packard, Pellegrin & Wacaster (1973)	Elliott & Adelman (1977)	Eisner (1975)	Iannaccone (1963)
Wax, Wax & Du Mont (1964)	Smith (1977b)	Cusick (1973)	Gump (1967)	Easley (1974)	Sarason, Davidson, & Blatt (1962)
Wolcott (1967)		Iannacconne (1958)	Henry (1957, 1966)	Hall & Thurnau (1975)	
		Jackson (1968)	Leacock (1969	Hamilton (1977)	
		McPherson (1972)	Lipnick (1976)	Munro (1977)	
		Mercurio (1972)	Rice (1964)	Reid & Walker (1975)	
		Reynolds (1973)	Smith & Geoffrey (1968)	Russell (1969)	
		Rist (1973)	Tikunoff, Berliner, & Rist (1975)	Seif (1971)	
		Sarason (1971, 1972)		Smith & Carpenter (1972)	
		Smith & Keith (1971)		Smith & Pohland (1974)	

TABLE 2
Observational Studies of Educational Systems—Continued

School and Community	School Systems and Interorganizational Educational Systems	Schools	Classrooms	Curricula and Program Evaluation	Teaching Careers and Student Teaching
		Walker (1932)		Smith & Schumacher (1972)	
		Wolcott (1977)		Soloman (1971)	
				Stake and Easley (1978)	
				Walker (1971)	
				Wolfson (1974)	
				Wolcott (1977)	

TABLE 3
Methodological Statements on Participant Observation Field Studies: General

Papers and Chapters	Monograph and Books	Collections
Becker (1958)	Becker (1970)	Adams & Preiss (1960)
Becker & Geer (1957)	Bruyn (1966)	Casagrande (1960)
Denzin (1971)	Denzin (1970)	Epstein (1967)
Glaser & Strauss (1965)	Glaser & Strauss (1967)	Garfinkel (1967)
Gump & Kounin (1959–1960)	Junker (1960)	Habenstein (1970)
Kimball (1955)	Pelto (1970)	Hammond (1964)
Kluckhohn (1940)	Powdermaker (1966)	Jacobs (1970)
Malinowski (1922)	Walker & Adelman (1975)	McCall & Simmons (1969)
Meehl (1971)	Wax (1971)	Naroll & Cohen (1970)
Merton (1947, 1957)		Vidich, Bensman, & Stein (1964)
Scott (1965)		
Van Velsen (1967)		
Vidick & Shapiro (1955)		
Whyte (1971)		

TABLE 4
Methodological Statements on Participant Observation: Education

Papers and Chapters	Monographs and Books	Collections
Adelman (1976)	Beittel (1973)	Adelman (n.d.)
Atkin (1973)	Lutz & Iannaccone (1969)	Hamilton, Jenkins, King, MacDonald, & Parlett (1977)
Burnett-Hill (1973)	Parlett & Hamilton (1972)	Pohland (1972)
Easley (1974)		Roberts & Akinsanya (1976)
Erickson (1973)		Tikunoff & Ward (1977)
Hamilton (1976)		
Lutz & Ramsey (1974)		
Magoon (1977)		
Sindell (1969)		
Smith (1967)		
Smith & Pohland (1976)		
Wilson (1977)		

(1950), and for a recent review see Miller (1977). In fact, these tables have a personal quality, representing the gradually accumulating collection of materials I have read and found stimulating. Nonetheless, inherent in them, and well beyond, are important empirical problems in the sociology and psychology of knowledge. Genealogies, communities, and individual perspectives of these research workers and their interrelationships deserve empirical attention, much as Crane (1972) has done for mathematicians and rural sociologists.

A Personal Story

Every research worker has an interesting story to tell on the evolution of his or her own work. I believe that more of these stories need to be told if we are to have a useful and potent theory of methodology. The accounts of recommended training programs, not to mention the uncollated statements in graduate school catalogs, often have ironical contrasts with personal histories as they are recounted in various forms (Becker, 1970; Homans, 1962; Homans & Bailey, 1959; Murchison, 1961; Skinner, 1956).

My own experience in participant observation of natural settings began in discussions with Laurance Iannaccone and W. W. Charters, Jr. From them I came to know Homans' book *The Human Group* (1950), and soon I was into the literature of the overlapping groups of social scientists in the Society of Fellows at Harvard and Warner's Yankee City group. From Iannaccone and Charters also I learned of Robert Merton's *Social Theory and Social Structure* (1957) and the case study work of a cluster of his students, Blau, Gouldner, Selznick, and Lipset. A major center of activity existed in Chicago: Blumer, Hughes, Becker, Bruyn, Geer, Goffman, Lortie, Strauss, and the Waxes. Harvard, Chicago, and Columbia traded some people back and forth over the years. Some of these individuals began studying educational settings and problems. Most notable were Kimball and his students, Iannaccone and Hill-Burnett and their students. At Stanford, Spindler was training such people as Wolcott, Singleton, and Warren and developing his highly regarded series of monographs on education and culture. Recently several of these strands have become institutionalized in the Council on Anthropology and Education of the American Anthropological Association and in Division G, Social Context of Education of the American Educational Research Association, as earlier some had flowed together in the founding of the Society of Applied Anthropology.

My own background—a mixture of Minnesota dustbowl empiricism in the Psychology Department and a kind of clinical educational psychology in the College of Education's Psycho-educational Clinic—had acquainted me with *none* of the anthropological and sociological investigators but with a variety of researchers doing case studies of individuals. That line of case study work has intersected very little with the more sociological and anthropological case studies. At one point (Smith, 1972) I informally tried to check the references to Piaget's clinical method in the various methodological statements by participant

observers. There were none. More recently, people like Gardner (1972, 1973) have been bridging those domains. Finally, and again personally, an interest in the arts and aesthetic education raised the possibility of some additional kinds of case studies. Once again the ubiquitous Becker was already there empirically ("The Dance Musician," 1966) and theoretically ("Art as Collective Action," 1974). Beittel's seminal case studies (1972, 1973) on producing art seem unknown to most educationists. Hamilton and his colleagues (Hamilton, Jenkins, King, MacDonald, & Parlett, 1977) have produced a reader in educational evaluation, *Beyond the Numbers Game,* whose saucy and irreverent continuity tells a part of the "illuminative evaluation" story—complete with a manifesto and a prediction of a paradigm shift in educational evaluation.

These personal socialization sequences and casual observations of evolving networks, invisible colleges, and gradual institutionalizations deserve more formal attention than the informal curiosity which can be satisfied by reading prefaces, footnotes, and references and engaging in casual conversations concerning struggles for an intellectual perspective, for a peer reference group, and for legitimacy.

In summary, these comments are small but important examples of the evolving nature of scientific ideology and practice. They make the points that moral principles ("It seems well-nigh unethical") in research are norms of communities and subgroups of scholars in the social sciences and education, and that deviancy from community standards has some parallels to deviancy in other groups (Becker, 1966; Festinger, Schachter, & Back, 1950; Schachter, 1953). They also suggest that methodological pluralism has some benefits as well as hazards. For this chapter they indicate the labyrinthian and somewhat tortuous route one investigator took in searching for a theoretical-methodological rationale to solve the problems he was confronting. Traveling that path has helped to focus many of the more general perspectives of this essay.

Reference to a half dozen of our observational studies will occur throughout this essay, and a brief introduction to these should facilitate the analysis. They can be grouped into three periods: initial forays, a CEMREL period, and a recent period. Substantively they reflect a research serial which might be called an evolving ethnography of schooling. Theoretically they represent a cluster of middle-range theories which might one day cumulate in a general theory of education.

Early on, the Office of Education, in its small contract program, supported three of our studies: *The Complexities of an Urban Classroom* (Smith & Geoffrey, 1968), *Anatomy of Educational Innovation* (Smith & Keith, 1971), and *Patterns of Student Teaching* (Connor & Smith, 1967b). *Complexities* was an attempt to look at how a middle-class teacher coped with a group of lower-class children in a sixth-and-seventh-grade classroom of an urban elementary school. Procedurally it was our first attempt to implement a qualitative participant observer methodology. Through a series of commonsense decisions and lucky

accidents we hit on the "inside/outside stance" of William Geoffrey, the teacher, insider, and true participant, and Smith, the outsider, nonparticipant, and observer. We developed a device which we called "interpretive asides," which called for the inclusion of insights in the observational record. We split our records into in situ "field notes" and out-of-setting "summary observations and interpretations." The latter we dictated into a portable stenorette while driving to and from the school, which involved a half hour of critical time each way. Leaving the site, the percepts and ideas would be popping in a thousand directions. Capturing these before they were lost seemed essential. Early the next morning, on the way in, the residue that remained could be commented on when one felt fresh and relaxed. Some of the results of the study which still seem important were concerns about the way the school year began; the teacher's role in that beginning; the development of the social structure of the classroom, especially the roles played by individual children; the conceptualization of teacher as decision maker and actor; and the characteristics of children from an inner-city neighborhood as these presented themselves to a teacher whose responsibility was to help the children learn.

From this class of Geoffrey's we moved to Kensington and the study of the first year in the life of an innovative suburban elementary school. *Anatomy* became a study of issues in innovation and organizational structure and process, a story of a group of educators attempting to build and implement the new elementary education. The significant results were descriptions and analyses of organizational development, of formal doctrine, and of the alternative of grandeur as a strategy of innovation. Considerations included the key themes of open-space building design, democratic administration, team teaching, and individualized curriculum and instruction, major elements in the new elementary education.

William Connor and I, intrigued with the unusual pattern of student teaching at City Teacher's College, spent a semester following a dozen apprentices around. Their "two by two" apprenticeship program included two weeks in a kindergarten, two weeks in a first grade, two weeks in a second grade, and so on through the eighth grade. Methodologically we became more serious about interviewing and began to think about triangulation and what we later came to see as an elaboration of the Campbell and Fiske (1959) multitrait, multimethod approach to valid data. Substantively, a variety of issues arose—"the nine trials phenomenon," aspects of anxiety, an analogy to psychomotor skills, and a model of socialization into the teaching profession.

The CEMREL period reflected an early commitment by Wade Robinson, the president of the Regional Laboratory, to the possibilities of alternative modes of educational inquiry and their relationship to educational practice. I spent a decade there, mostly with half-time appointments and released time from Washington University. One study continued directly the earlier line of work. Pat Brock and I began and still have several unfinished drafts of *Teacher Plans and*

Classroom Interaction. We wanted to attack issues in the intellectual life of classrooms, we wanted to continue and extend the micro analysis of classroom discourse, we wanted to critique the stance of various in-vogue systems of classroom observation—Flanders, B. O. Smith, Taba, Medley and Mitzel, and Bellack, we wanted to move toward quantification, and we wanted to remain with a processual rather than a structural analysis. Consequently we tape-recorded a full semester of her first-hour Science I class, we took field notes along the way, and we have her daily pre- and poststatements about plans and results. CEMREL published our major methodological statement *"Go Bug Go": Methodological Issues in Classroom Observation* (Smith & Brock, 1970).

However, most of the CEMREL work was a series of forays into formative and summative evaluation using qualitative observational procedures, sometimes independently and sometimes triangulated with experimental and survey procedures in a three-legged evaluation model. The initial example (and perhaps most important) of these was "Education, Technology, and the Rural Highlands" (Smith & Pohland, 1974), a study of a computer-assisted instruction program. A series of papers (Smith & Pohland, 1974, 1976) grew out of that work. Substantively we were into community analysis, interorganizational theory, and the wonders of technology—both its doctrine and its realities. Methodologically we came to terms with "standard" participant observation technique, a phenomenon which does not exist, in our view. We elaborated the multimethod, multitrait position. And we tried to reanalyze and synthesize the Glaser and Strauss (1967) grounded theory position with our own.

Pat Carpenter and I spent a year in the formative evaluation of a social exchange token economy program in an urban school. In *General Reinforcement Project: Qualitative Observation and Interpretation* (1972), we raised critical issues in the implicit value stand of a behavior modification position, the simplistic doctrine of token economies and the complex behavior of the teachers, and some similarities and differences with Kounin's (1970) classroom management position.

Finally we were involved in several studies related to the Aesthetic Education Program. Sally Schumacher and I spent a year on *Extended Pilot Trials: A Description, Analysis, and Evaluation of the Aesthetic Education Program* (Smith & Schumacher, 1972). Later I observed a weeklong workshop for administrators (Smith, 1974), began toying with a variation of Piagetian clinical method (Smith, 1975b) as an evaluation technique, and took the observational role of learner in *Mrs. Kaye's Drawing Class: Some Theoretical Thoughts on Curriculum, Teaching, and Learning* (1975a). The perspective of the pupil seems an unexploited stance.

Our major recent activity has been an involvement in Robert Stake and Jack Easley's *Case Studies in Science Education* (1978), supported by the National Science Foundation (NSF). We studied the Alte School District (Smith, 1977b) an older suburban, upper-middle-class school district with the reputation of

having a good science program. Key issues arose in district history, in the politics of curriculum change, in the nature of the strong teacher (as well as the prima donna syndrome), and in the conceptualization of curriculum at the district level. Currently we have two studies planned and underway: ''Improving Urban Education: Federal Policy in Action'' and ''Kensington Revisited: A 15-Year Follow-Up of an Innovative School and Its Faculty.'' The former is an attempt to describe and understand a major effort in knowledge development and utilization in urban education. The latter is a simple ''What's happened?'' inquiry at the innovative Kensington School, including an attempt to find the original faculty, who are now scattered about the country, and to inquire into their current views of educational innovation and change. Both of these investigations are being supported by the National Institute of Education.

In summary, outside the dominant educational psychological paradigm in educational research, a large body of research exists within the qualitative, ethnographic, participant observational genre. Its roots lie especially in anthropology and in several traditions within sociology. A brief overview of one educational research practitioner's use of these methods suggests its applicability to a broad array of problems within education—schools, classrooms, curriculum development, and evaluation.

COGNITIVE PROCESSES IN EDUCATIONAL ETHNOGRAPHY

A number of stories can be told regarding the intellectual processes in doing educational ethnography, participant observation, and other case studies. On several occasions symposia at APA, AERA, and AAA, and in appendices to books and technical reports, we have tried to speak to issues in the cognitive processes involved in qualitative observational research. These attempts have usually been reflexive, that is, they have grown out of our musings and reflections as we have tried to use the methods in particular projects. In an important sense they have been attempts to indicate the dynamics of ethnography by attending to the creative processes in learning from a field work project. In this section I will summarize these thoughts. Once again this is a personal statement; it should be read as a semiintegrated collection of hunches and hypotheses open to more careful tests by empirically oriented students of social science methodology. How one does that testing is an interesting commentary on one's assumptions, practices, and theory of methodology.

Preliminary Phases

Origins of Problems. The vagaries in the origins of our research problems are captured best in the title of a recent short paper, ''Accidents, Serendipity, and Making the Commonplace Dramatic'' (Smith, 1978). The general point is that the problems are all around; they pass by the investigator in varying guises and for the most part need only to be recognized for their possibilities. A brief

example or two must suffice. William Geoffrey was an MA student in a summer school course entitled "The Classroom as a Social System." As one of the activities each student took the Minnesota Teacher Attitude Inventory (MTAI) as part of a discussion of teacher variables in the classroom system. As Geoffrey turned his paper in he commented that this was how he felt but it had little to do with the way he taught. Such needling provoked a conversation, led to an invitation to see what it's like in an urban classrom, and eventually developed into *Complexities*. While observing Geoffrey's class I met an apprentice who was in this "funny two by two" student teaching program which was so different from the "regular" program at Washington University. Two programs which had common goals but such different structures just had to be interesting, so Connor and I set out on *Patterns*. When Cohen and Shelby approached me to get involved studying the Kensington School, it looked like a beautiful opportunity to see the origins of an elementary school faculty peer group, a phenomenon that was a very important part of Geoffrey's life at the Washington School. *Anatomy* grew out of that.

It might be argued that the three illustrations suggest an absurd model for the origins of research studies. Be that as it may, that's the way it seems to happen. It might be argued also that this is an applied extension of Underwood's (1949) old notion of "I wonder what would happen type" origin of research problems.

The Intuitive Feel of the Problem.   When funding agencies, colleagues at other universities, practicing professionals (superintendents, principals, teachers), or graduate students considering dissertations raise with me the possibility of "the study of X, Y or Z," there occurs an almost immediate perceptual reaction-evaluation that it is or is not a good problem. Strangely perhaps, almost as an animal sniffing the air in an unusual setting, it comes out silently to myself as "It smells like a good problem." I have the impression that I could, at a minimum, defend it, or at a maximum get truly excited by it and be willing to commit one or more years to working on it. Usually the perceptual reaction is accompanied by a feeling of "Why didn't I see that or think about it before?" I don't understand the dynamics of the reaction, but it happens. It seems functional.

Guiding Models and Images of an End in View.   Usually very quickly also there comes to mind a particular piece of research which captures the essence of what seems implicit in the problem and which serves as a kind of guide for what might be done. *Guide* is perhaps too limited an image, for not only does it give direction to the intellectual work but it brings a kind of confidence to the task; it legitimates the activity: "If we can do it as well as Jones did, it will be a worthwhile contribution." Several brief illustrations come to mind. In *Complexities,* a "simple" image dominated our orientation: Do an educational case study that would fit the half dozen presented and analyzed in Homans's *Human Group*. The logic was simple—a teacher and a classroom are just another group, as was the Irish farm family, the street corner society, or the bank wiring group.

Further, Homans provided methodological and procedural guidelines, a conception of social science theory, a view of explanation, and a set of concepts and hypotheses appropriate to a middle-range theory of groups (and possibly a basis for a more general and abstract theory of sociology). In *Alte* the model was McKinney and Westbury's (1975) attempt to consider a school district and its curriculum from a historical perspective. We were on the hunt for a way of looking at science education in the Alte schools. Currently we have begun a new study entitled "Improving Urban Education: Federal Policy in Action." As soon as the label was generated it immediately raised an association with a study I'd casually known from years ago, "Project Camelot." A search in the library found it unavailable, but its author, I. L. Horowitz, had co-authored another book with J. E. Katz, *Social Science and Public Policy in the United States* (1975). A quick skimming (one section dealt with "Project Camelot") provided an initial guiding model.

Foreshadowed Problems.   One of Malinowski's major contributions to the logic of ethnography was his distinction between "foreshadowed problems" and "preconceived solutions." As he put it a half century ago:

> Good training in theory, and acquaintance with its latest results, is not identical with being burdened with "preconceived ideas." If a man sets out on an expedition, determined to prove certain hypotheses, if he is incapable of changing his views constantly and casting them off ungrudgingly under the pressure of evidence, needless to say his work will be worthless. But the more problems he brings with him into the field, the more he is in the habit of molding his theories according to facts, and of seeing facts in their bearing upon theory, the better he is equipped for the work. Preconceived ideas are pernicious in any scientific work, but foreshadowed problems are the main endowment of a scientific thinker, and these problems are first revealed to the observer by his theoretical studies. (Malinowski, 1922, pp. 8–9)

In a sense, Malinowski's statement calls the investigator to an awareness of the key problems, issues, and debates in that corner of the intellectual world in which the setting and the problem lie. The foreshadowed problems represent initial and partial analyses of the problem, the tenor of thinking of people who are working in related and relevant areas, and provisional modes of thinking. By way of illustration, at one time we joked about what one needed to know before starting a theory-generating observational study. Since we had just finished an educational psychology text (Smith & Hudgins, 1964) this seemed an appropriate spot to begin. The advice became, "Go write an ed psych text, then you are ready for a classroom observational study!" In retrospect, the truth in that advice seems to be in the residual questions after one has tried to read and synthesize several hundred research references (not to mention those that didn't make the bibliography). In the best sense these were Malinowski's foreshadowed problems.

Equally important for me has been the experience of teaching in an education department. My students have been undergraduates just moving into the teacher education program and experienced teachers in the M.A. and Ph.D. program in elementary and secondary education, administration, psychology, and guidance. As they raise their most perplexing questions, I store them away to ask the data from Geoffrey's class, from the Kensington School, from the Alte District, or wherever else we have been or are contemplating entering. In working with students as they try to learn the methodology of educational ethnography, a major problem arises if they do not have a wide background of problems generated by attempts to puzzle through large amounts of related but difficult-to-integrate literature, by an array of personal experiences or by difficult questions posed by colleagues. Dealing with that is one of the more intractable problems in teaching and learning participant observation.

Competing Theories.   Many students and critics have found the conception of foreshadowed problems not adequate to their concerns for selectivity of data, seeing what one wants to see, and implicit theoretical biases. One of our further procedures has been to describe the tenor of the theoretical concerns that we have "gone in with." The best illustration of this comes from *Complexities*. In our educational psychology text we drew heavily on such theorists as McClelland, Homans, and Skinner. A more precise conceptualization of that position can be seen in the table of contents of that book, and contrasts and similarities can be seen in the table of contents of *Complexities* and, perhaps even more significantly, *Anatomy*. While elements of continuity exist, the real world of the classroom and school pulled us some distance from our initial stance. This acquaintance with prior theory reflects not only the early position of Malinowski but a similar more recent statement by Diesing: "The prospective field worker will acquaint himself with a variety of theories (the more the better) that may be applicable to his case" (1971, p. 142). Now, in long retrospect, the point of emphasis is slightly different. I believe we were implicitly running several alternative general theories against each other, that is, putting them in competition. In a sense we were unwittingly initiating an ethnographic paradigm for falsification. In addition, we were not only selecting from the several stances but also moving toward the beginnings of our own position.

Discussions with colleagues (Nolan, 1975) suggest a further illustration. In a project involving the genesis of a community college, one alternative would be to enter this deliberately from the point of view of a sociologist such as Parsons and to look to the resolution of the functional problems and pattern variables and further clarification and development of that point of view. In keeping with the principle of competing theories, one might become as well versed in Homans's brand of interaction theory, Merton's functionalism, and March and Simon's organizational theory. As the events play themselves out in the natural setting, particular hypotheses within one position or another will be found to be less tenable than others. These beginnings of falsification, in the context of compet-

ing theories, seem to be the latent logical thrust of the Malinowski, Diesing, and Becker rules of thumb. Stated alternatively, approaching a setting with several competing theories, to each of which one is only partially committed, allows one to explore more fully the conceptual realities of the events in the setting. As events occur which the several theories omit, neglect, or speak to only mini- mally, the generation of one's own position comes to the forefront.

Thinking during Data Collection

While we are in the field observing directly, informally talking with and listening to participants, collecting and reading documents, a variety of discrim- inable intellectual operations seems to be occurring.

Immersion in Concrete Perceptual Images. One of the exciting and often unexpected events for a novice field worker comes from his/her immersion in concrete perceptual images. The day-in, day-out involvement in the setting produces an ocean of images of the phenomenon, a wealth of particulars— people, situations, events, occasions, and so on. The human condition, in all of its varied, idiosyncratic, unusual, mundane, exotic aspects, plays itself out before one's eyes. The potency of this overwhelming flood of unorganized data to disturb one's cognitive map of structures, hypotheses, and point of view cannot be overestimated. One sits in wide-eyed and ''innocent'' wonder and tries to capture, as much as possible, in the field notes and the summary observations and interpretations the drama going on.

The Interpretive Aside. Along the way, a variety of ideas, insights, and interesting associations of ideas, events, and people arise. We tend to jot these down into the notes as ''(Obs- ...)''. They seem to ''pop out'' in the normal give-and-take of observing and talking with people in the setting. Often they have a free-associational quality (''reminds me of...'') and sometimes they are simple perceptual comparisons or contrasts. This technique or procedure we sort of ''fell into'' while beginning *Complexities*. It seemed sensible to make at least a quick note of the ''insights'' or ''bright ideas'' that seemed to be arising effortlessly along the way. My hunch is that many get lost if not jotted down at the time. Later we found these to be very useful in that part of the analysis process we have called ''generating concepts and hypotheses.'' Students whose notebooks are full of these seem to move the analysis along much easier than those whose notes are more limited.

Conscious Searching. Concomitantly with the almost unconsciously deter- mined items of the interpretive asides there is the omnipresent question, ''What does it all mean?'' This is a search for overall patterns, for broad themes which seem to break the phenomenon into large chunks or domains. This is an active searching for order. Sometimes, as with the historical emphasis in the case study of science education in the Alte District, it came early, from reading Toulmin in general and Westbury in particular. It seemed to ''keep working for me'' in the sense of methodologically guiding me toward interesting data and, substantively,

in turning up interesting problems and perspectives. It became a major theme of the final report. Further, it left me with a bit of unresolved tension: ''Next time, or soon at least, I want to do a *for real* historical study.'' That brings us back full circle to a foreshadowed problem (in this instance, a mix of problem and procedure) and means that the next study is already cracked open enough to have a beginning point of attack. Such events remind me of Henry Murray's concept of serial, a longer-term unit in a theory of personality. He illustrates with friendships, marriages, and careers. More recently Beittel (1973) has adapted the concept for studying creativity in drawing. His argument is that understanding the production of any one piece of art requires knowledge of prior work and intended future work, an artistic serial. The parallels to cumulative research seem obvious. The conscious searching for patterns, in effect, is not only within the single project but also throughout the series of projects over time.

The conscious search for analytical or interpretive meaning moves concurrently with data collection. Glaser and Strauss (1967) have called the process ''theoretical sampling'': ''data collection for the purpose of generating theory whereby the analyst jointly collects, codes, and analyzes his data and decides what data to collect next and where to find them, in order to develop his theory as it emerges'' (p. 45). A number of concepts are subsumed under the generic concept of theoretical sampling. Among these are saturation, slices of data, and depth. By saturation, Glaser and Strauss mean that no additional data are found which contribute to the properties of the categories under consideration. This is a useful but tricky concept. It assumes that one knows in advance or along the way what the key categories are and where the locus of information is. We have not found it quite that easy. As we work in a particular context or setting we try to exploit that setting for all of the information and all of the ideas we can find. In a sense, we keep looking until we can generate no more ''insights'' and ''interpretive asides.'' It is at that point that we tend to quit. In this situation our experience has been that beyond the initial focus, the narrative or story line soon carries us into a whole variety of other problems and issues that we had not anticipated in our preliminary entrance to the problem. This is moving from the foreshadowed problems into the theoretical issues. The ''Rural Highland'' project was a beautiful example of this. Initially we had conceived the project in terms of the psychology of mathematics teaching. As we began carrying out our observation, however, the exigencies of the situation shifted the focus of investigation to the politics of education, interorganizational issues, the problems of introducing sophisticated 20th-century technology into an underdeveloped region, and the like. In a very real sense, the twin concepts of saturation and flexibility run parallel courses in the field work.

As is becoming explicit in the several items in this discussion of cognitive processes, data analysis occurs throughout the project, and also the social context of the project impinges on the intellectual aspects of the work. In our reflections on the *Alte* case study (Smith, 1977b), we raised the idea of ''project press'':

''the short time line on CSSE produced enormous pressure to move quickly, to begin intensive attempts at conceptualizing early, to seek workable outlines. This produced a series of stresses, some of which were toward conceptualization and interpretation'' (p. 128). We were on a one-semester, portal-to-portal time line: enter in January, final report due the first of June. We were finished (in several senses) on the first of July. Some of our colleagues in the other case studies were on shorter time lines—a month's observation and interviewing, with varying deadline dates. In every project we have been on, deadlines were posed by aspects of the projects themselves, by contracts for final reports, by AERA presentations, by new activities, by recurring responsibilities at the Graduate Institute of Education. These have exerted a press to finish particular pieces of work, and variations on the form of the intellectual processes of data collection, analysis, and write-up have developed. These were felt most intensely while still in the setting.

In one form or another these cognitive activities, along the way, appear in accounts of most field workers. They are vivid and potent experiences which contrast dramatically with images created by Campbell and Stanley's brief account of the one-shot case study. They seem more akin to an extended and sometimes multiple time series quasi experiment.

Final Analysis and Writing

The overall image I'm trying to arouse in this section on cognitive processes is intended to be one of creative thinking, the generation and construction of concepts, perspectives, and theories from an initial set of problems, through a long period of sought and unsought percepts and experiences in the field setting, to some final kind of order which appears as a written report. Its open-ended quality frightens some novices and critics and exhilarates others.

The Case as an Instance of a Class of Events. Eventually, if not early on, comes one of the most difficult and elusive problems, locating the case as an instance of a more general class of events. I think this gives researchers from other traditions great difficulty with the early stages of participant observer research, particularly as done by neophytes (e.g., doctoral students), because they cannot specify their problems clearly enough. The outsider seldom is content with ''a description and analysis of X,'' for X is usually a particular concrete setting (e.g., Geoffrey's class, the first year of the Kensington School, science education in the ALTE School District). In a hypothesis-testing sense, there is no problem. The difficulty that neither the student nor the critic perceives is the theoretical complexity in coming to grips with what is the substantive problem in the investigation.

We tried to address the issue in the project entitled ''Mrs. Kaye's Drawing Class'' (Smith, 1975a). In coping with the ''problem'' under investigation, I constructed an abstraction ladder of one element, the kind of learning under consideration. At the most concrete level, I, as student-participant-observer, was

learning to sketch wine bottles with charcoal. Sketching wine bottles is one instance of charcoal drawing, which is an instance of one kind of representational drawing. Learning representational drawing is an instance of a broader class of learning artistic skills, which in turn is an instance of an even broader class, productive learning, wherein one produces something. In that study, I eventually decided that I wanted to work at the level of a theory of productive learning, interrelated with conceptions of curriculum and teaching. Alternatively, that research might have been conceptualized as a study in representational drawing or a study in artistic skills.

I believe this kind of process occurs throughout social science but that it is neither well recognized nor dealt with effectively in most observational research, or for that matter in other genres of educational research. Paul Meehl speaks to a similar problem in *Clinical vs. Statistical Prediction* (1954). Prediction, for Meehl, requires that an individual event be put into a class of events for which probabilities exist. Predictions of whether Jane Doe will go to a movie on Friday night will vary, depending on whether you know frequencies of Friday night movie attendance, of women attending Friday night movies, of women having dates, or of women with a recently broken leg attending Friday night movies. Jane Doe could be classified correctly into any one of those cells, but each would give a different probability. Other instances come to mind. Some years ago, in reading Skinner's *The Behavior of Organisms* (1938), one of the most brilliant and stunning, and, in hindsight, possibly misguided illustrations of this occurs in the first 60 pages, Chapters 1 and 2. For those interested in a model of theory building, his discussion of the experimental box as ''environment,'' of white rat as ''organism,'' of bar press as ''operant,'' and of operant as ''spontaneous behavior'' is breathtaking.

For our purposes, Diesing (1971) captures best the importance of the issue under examination:

If I had to generalize at this premature stage, I would be inclined to point to the problem of the One and the Many as the essential problem of scientific method. Any scientific account of human society must somehow deal not only with the uniqueness of which human history and individual life histories consist, but also with the regularities of various sorts that appear in history. If I were to work out this problem in detail to determine how adequately various methods deal with it, case study methods would come out on top. They include both the particular and the universal within science instead of consigning the particular to intuition, practical application, or history; they exhibit the universal within the particular instead of segregating the two in one way or another; and they move from particular to universal and back by gradual steps rather than in one grand jump. (p. 296)

As we have indicated throughout this essay, such thoughts are continually salient. In the final analysis, if not before, the researcher must stake out the domain in what often seems like the shifting sands of multiple levels of abstraction across domains of theory.

Skimming the Cream: An Initial Overview.   In the appendix to our study of the extended pilot trials of an aesthetic education program (Smith & Schumacher, 1972) we used a metaphor we called "skimming the cream." In a sense this is another perspective on what kind of a case one has. Here the procedure is more inductive and more "quick and dirty." In my view, metaphors from other occupations, life-styles, and eras probably do not carry the full meaning one intends, but they help. One of the consequences of research in a bureaucratic organization is being faced with deadlines. A second is limited time to do a task. The situation was this. During our last week of data collection we had to make a brief presentation of results to the several parties of the larger project we were investigating; these individuals were making decisions regarding the form the project would take in the succeeding years. The tactic we adopted was a simple one. In a local coffee shop, for a period of a couple of hours, we asked ourselves: "What are the major things we have learned from our year in the field?" As we brainstormed these ideas, with no reference to our file drawer of notes, interpretive asides, or summary interpretations (some of which were still untyped on tapes because of organization resource problems), we gradually accumulated a list of ideas, findings. We pushed and pulled on these until they gradually fell into reasonable broader topics and differentiated outlines. Points of debate were joined and countered with images recalled from classroom observations and informal conversations (interviews) with children, teachers, and supervisors. The most intriguing methodological question this raises is suggested by the "skimming the cream" metaphor. In this simple procedure, can the really significant rich items be obtained? Do the labored procedures suggested by Becker et al. (1961) in the *Boys in White* analysis and by Glaser and Strauss (1967) in their constant-comparative method yield more creative, more comprehensive, or more reliable theory and interpretations? Our guess is that the differences in creative propositions are probably minimal. Some comprehensiveness is probably lost by cream skimming. The reliability of interpretation, or perhaps better, the confidence in the interpretation, probably drops off more sharply. For students of methodology this is obviously a testable empirical problem.

Another, related aspect of cream skimming involves the preparation of initial statements of parts of the report. Here we have been caught with limited time to do the report—two months instead of a year. Essentially we have picked up on the brainstormed issues, returned to the notes to check them, elaborate them, and refine them. This is in marked contrast to alternative procedures of quick total review of all notes and careful page-by-page reading with cumulating analysis, or careful reading, categorizing, and tabulating as done in the constant-comparative approach. The empirical question remains. Does the quick and dirty cream skimming yield 20, 50, 70, or 90% of the total from the more systematic analysis?

Developing Individual Sections.   From one perspective, the methods of data analysis I use seem terribly inefficient and unsystematic. After such processes of

skimming our recollections for key items, and after the files have been organized into a chronology with separate sections for my field notes, my summary observation notes, the notes of colleagues and assistants, and the various kinds of documents, and after I have done some reviewing of the specific contents of the overall project, I usually start back at the beginning of the notes. I read along and seem to engage in two kinds of processes—comparing and contrasting, and looking for antecedents and consequences.

The essence of concept formation is—and somehow I'd never quite seen it back in the days when I was administering the Wechsler and WISC—"How are they alike, and how are they different?" As items appear in the perceptual images, as verbal comments are recorded, as situations appear, as events come and go, one asks a simple two-sided question: How are they alike, and how are they different? The similar things are grouped and given a label that highlights their similarity. The different things are grouped, insofar as possible, and given labels. There always is a large "miscellaneous" category of items which seem important but which do not fit anywhere. The *seem* is critical. There is always a hunch lurking behind the *seem* and, given more data, more time, and more thought, the pieces find a place in relation to one another. Earlier we called this the "jigsaw puzzle analogy" (Smith, 1967). This metaphor reflected not only the multiple pieces of a jigsaw one was trying to put together, but the very important aspect of actually shaping the individual pieces themselves. In time, these similarities and differences come to represent clusters of concepts, which then organize themselves into more abstract categories and eventually into hierarchical taxonomies.

Concurrently, a related but different process is occurring. Some time ago, I was impressed by Robert Merton's insight into social theory and social structure. The item that came to represent the totality was the beautiful label "latent and unanticipated dysfunctional consequences." My psychological background had urged a search for causes, for reasons, for determinants of a child's reading problem, for disaffection in school, for family difficulties. The conscious search for the consequences of social items, in all their combinations—latent and manifest, anticipated and unanticipated, functional and dysfunctional—seemed to flesh out a complex systemic view and a concern for process, the flow of events over time. In addition it seemed to argue for a more holistic, systemic, interdependent network of events at the concrete level and concepts and propositions at an abstract level. Zetterberg's argument for multiple ways of ordering propositions—inventories of determinants, inventories of consequences, chain patterns, and finally axiomatic formats—blended the theoretical with the concrete flow. At a practical level, while in the field, the thinking, searching, and note recording reflected not only a consciousness of similarities and differences but also an attempt to look for unexpected relationships, antecedents, and consequences within the flow of items.

These twin processes seem to capture the best of concept formation, in contrast to concept attainment, and of hypothesis development, in contrast to

hypothesis testing. For us, they specify, almost operationally, the meaning of Glaser and Strauss's beautiful idea and label, "the discovery of grounded theory" and Diesing's equally apt term, "patterns of discovery."

There is a social dimension to these. No project occurs in isolation, at least when one studies social phenomena in one's own society. The importance of this for the gradually developing analysis and the progressive refocusing of data collection seems obvious to most field workers, and it is underestimated by most investigators using more set designs, whether experimental, quasi experimental, or survey. One illustration conveys the general point. As we were observing events at the Kensington School and talking casually with Professor Edwin Bridges, he took items about the fervor of the participants, the total time commitments, and the enthusiasm, and suggested that they sounded like Hoffer's (1951) true believers. Immediately the relevance of the more general conception was obvious. Later, Pat Keith found extensions to Klapp's (1969) crusades. Every project we have undertaken has always rippled in and out of other events in our professional lives. Such experience makes the "one-shot case study" label a serious misnomer. Everyday in the field is a new quasi experiment, guided and enriched by an intellectually stimulating environment of persons, supportive and critical; of ideas, mundane and all encompassing; of chores and opportunities. These events play in and through the field experience.

Collapsing Outlines.   In every participant observer study we have undertaken we have experienced a further phenomenon, one we have called "collapsing outlines." As we have begun analyzing the data, usually in terms of the foreshadowed problems which initially guided our entry and data collection, we have come upon interpretative asides and latent theoretical issues which seem a vital part of the setting and our understanding of it. As pieces are developed, we keep making tentative outlines that put some larger meaningful and logical order into the interpretation. Invariably the tentative outlines collapse in the face of more complex data and ideas. This seems another, later stage item in the definition and redefinition of the problems as a theoretical issue. Empirically, the dated sequences of outlines as we grappled with the meaning of our problem would be a helpful addition to this discussion.

Collapsing outlines seems very similar to a number of accounts of creativity in various artistic disciplines (Beittel, 1972; Gribble, 1970; Housman, 1933). While most artists begin a work with an initial idea, the gradually developing picture, poem or novel seems to develop something of a life of its own. Resolutions to particular problems create structures which not only constrain future decisions but also often suggest options which the individual creator had not perceived earlier. So it seems with problems, data, and analysis in participant observation. The initial problems are jarred by the interpretative asides. Recalcitrant pieces of data and negative instances are there and demand to be integrated. Finally, and most devastatingly, large, reasonably intact outlines tend to collapse because the weight of the data and the developing ideas in the analysis are too

much for the earlier formulated conceptual structure. Eventually we have an outline which holds. It has a structure reflecting three major dimensions: integrity, complexity, and creativity. By integrity I mean it has a theme, a thesis, a point of view. The pieces fit together as an interrelated part-whole relationship. By complexity, I mean the outline has enough discriminable pieces to cover the major themes and the minor nuances, the large elements, and the nooks and crannies necessary to do justice to the system under study. Finally, by creativity, I mean the outline conveys some novel and important ideas to some relevant audience—the people in the system, the educational research community, and/or some practitioner who is teaching, administering, or working in the educational community.

Summary

As should be obvious by now, we have wrestled with and been guided by a number of field workers: Homans, Malinowski, Becker, Bruyn, Glaser and Strauss, Denzin, McCall and Simmons, Whyte, Iannaccone, Van Velsen, and others. All of them kept speaking to and answering questions we kept running into. Perhaps the most amazing experience has been rereading some of the early favorites (e.g., Homans and Whyte) and finding how much Homans and Whyte had learned and had to teach the second and third time through, several years later.

CLUSTERS OF MULTIPLE DIMENSIONS OF PARTICIPANT OBSERVATION

To this point I have argued that the educational research community exemplified by Campbell and Stanley has focused on the experiment and the quasi experiment as the dominant mode of knowing. I have suggested that such a view may represent the norms of the major community in education and psychology but that other social science communities—in anthropology and parts of sociology, especially—have developed their own traditions. Within education, a smaller subcommunity has evolved which has attracted a number of practitioners, researchers, and theorists and has begun to institutionalize itself more formally. More particularly I have described in some detail one researcher's attempt to implement, through a series of studies, this strand of participant observer research. Now, I intend to distance myself from those particular experiences and formulate a more abstract structure for analyzing and evaluating research work within this genre. This task seems both difficult and necessary. Regarding the difficulties, early on we thought that there might be a unitary phenomenon called ''standard participant observation procedures'' which could be used as a paradigm for analyzing and eventually judging any particular piece of educational ethnographic research. Paul Pohland and I (Smith & Pohland 1974, 1976) disabused ourselves of that hope. We argued then that on such dimensions

as emphasis on the descriptive narrative, generation of theory, verification of theory, and quantification, important participant observer studies varied quite markedly. As a consequence, I am more inclined to ask for a clear statement of actual procedures and an intellectual cost/benefit analysis of those procedures for the problems at hand and the purposes in mind. In this section, four related clusters of concerns will be used to chart the multiple dimensions of participant observation.

While these clusters of concern pattern themselves like a piece of woven cloth, for analytical purposes they can be broken into warp, weft, colors, and textures. In order of increasing abstraction they include these levels: (1) data, (2) descriptive narrative, (3) analytical-theoretical-interpretive, and (4) metatheoretical. The necessity of such efforts arises in the needs of various individuals and groups who must make decisions in training students, in reviewing project proposals, and in judging research reports for publication.

The Data Level

In analyzing and judging a piece of observational research, a series of questions can be raised concerning the data and how they were collected. The resulting cluster of dimensions—direct on-site observation, freedom of access, intensity of observation, triangulation and multimethods, sampling, and attention to muted cues and unobtrusive signs—seems to comprise the major conditions assuring valid data.

Direct On-site Observation.   At the simplest and most basic level, participant observation, in the sense of ''being in'' or ''living in'' the setting, involves the researcher directly in the social behavior under study. Being on site is the sine qua non of ethnographic research. It distinguishes the research from surveys, from interview studies, from laboratory studies, from testing studies. Such direct on-site observation assumes several conditions in social life. For instance, individuals in institutions, organizations, and groups often mask what is happening in the setting, for a number of reasons. That is, they create formal doctrines, develop facades, or perhaps ''wallpaper over'' significant issues and events. On such occasions any researcher faces major problems at the data level in regard to what is ''really'' going on. This masking cannot be done so easily when one is observing directly at meetings, talking informally to participants at coffee klatsches, and taking part as people relate to one another in situ. While useful for many purposes, questionnaires, tests, and formal reports of events, insofar as they attest to social behavior and events, are indirect observations of those events and are susceptible to all kinds of distortions, both conscious and unconscious. The makers of K, L and F scales on the MMPI and other tests are wont to indicate this possibility when individuals fake good or fake bad, both consciously and unconsciously. In other settings and on other occasions individuals have been known to ''stonewall,'' to lie, and to develop fictional reports to hide organizational realities.

The magnitude of importance of these points is so fundamental yet so "obvi-ous" that it seems unnecessary to elaborate further. It emphasizes and reem-phasizes our continual preoccupation with valid data of social events. In part our later discussion amounts to means to control or lessen possible difficulties in learning from such simple direct observation.

Freedom of Access.   Conversations with observers—particularly short-term evaluators of educational programs— indicate a concern that they are being steered toward particular teachers, or classes, or schools or that they do not have access to other particular settings, people, or events. Usually these issues are settled at the time of entry; usually they are an interrelated function of the initial foreshadowed problems, the boundaries of the system under study, the more general purposes undergirding the research, and the evolving social relationships of the researcher in the setting. For instance, in *Complexities* the formal agreements that I had with the superintendent and the principal of the Washing-ton School were that I would be in Geoffrey's class and go with him wherever his duties took him—lunch duty, yard duty, and so on. I was not to go into any other classroom unless invited. Our basic interest was his classroom, and that's where I spent most of my days. However, the clique of teachers with whom he associated included the eighth-grade "teacher in charge" and the clerk in the main office. Geoffrey set up a coffee bar where a number of teachers congregated and gossiped before school and at recess. We lunched usually with a group of the men teachers. I became good friends with several teachers and we chatted on several occasions through lunch and through their free period when the children were with the physical education teachers. While the focus was on Geoffrey's class, I was socialized more generally into the faculty of the Washington School. At Kensington, our agreements were such that all meetings—general, faculty, teams, PTA, and so on—were open to us, as well as all classes, curricular areas, and lessons. We were free to talk with anyone who was willing to talk to us. More recently in the Alte School District, in our study of science curriculum (Smith, 1977b), some of the staff had been in class with me at the university. We had discussed participant observer research in detail. When the project began they opened conversation with comments such as "You'll proba-bly be interested in some of this..." and brought out stacks of curriculum guides, reports, newsletters, and so on. Once again, as part of the informed-consent procedures, teachers willing to participate, after a discussion of the project, signed consent forms describing the kind of access we hoped for.

The main point: In studying schooling most field workers negotiate broad access relevant to the questions under study. Insofar as the relations among problem, purpose, settings, and events have been perceived initially, they are able to gather relevant data. A subcondition we try to emphasize is the freedom to come to classes, meetings, and other events unannounced or without prior arrangements. Partly this represents convenience in maximizing use of time. Partly it broadens the basis of seeing normal or usual events and increases the

validity of the data we are obtaining. When access is limited, for whatever reasons, questions arise regarding the adequacy of the data.

Intensity of Observation. Invariably one of the first and most critical questions regarding the validity of the data concerns the intensity of observation of the system under study. In general, field work is a labor-intensive research mode. In our own work on *The Complexities of an Urban Classroom*, I was there, "almost all day, every day for a semester," sitting in the back of the classroom, taking notes. Geoffrey, as teacher-researcher, was there all day everyday. In *Anatomy*, the intensity of involvement was recorded in a footnote to the study:

During the study, school was in session 177 days from September to June. The workshop had involved four weeks in August. The observers have field notes from 153 *different* days at the school or in the district and 247 total entries. The latter indicates the overlap when both of us were in the field. Although it is possible to speak of 247 man-days of observation, this is faulty in the sense that some of the entries reflect part days and some reflect early morning to mid-night days. One of our colleagues phrased it colloquially but cogently when he commented, "You were all over that school." The intensity of involvement is a key issue in the validity of the data. (Smith & Keith, 1971, p. 10)

Several implications follow upon the issue of intensity of observation. First, the possibility of individuals "faking" their behavior, intentionally or unintentionally, seems less probable. The multiple actors are caught in a thick web of historical and contemporaneous interconnections. As observer, I kept listening and looking for offhand comments, raised eyebrows, hints that any one was behaving uncharacteristically. Reactions from pupils who had been in class the prior year, from teachers a grade below or a grade above, and from staff friends were constantly being scanned at the odd moment and setting of lunch or recess. Becker (1970) makes similar points in contrasting field work with the laboratory experiment. In the latter setting, the "subject" is totally removed from the social constraints of real life and is susceptible to experimenter intentions and nuances of experimenter behavior, as Orne (1962) and other have argued. In such a context the "pristine" qualities of laboratory data and results take on other shades of meaning.

The intensity of observation, in the sense of length of time in the field, interacts with the conceptual, theoretical stance one takes. In most of our work we have had a concern for processes over time, a theory of action. Early on, it was more implicit; more recently we have been trying to make it explicit. In regard to our data collection, we have tried to be around for a period of time that reflects commonsense boundaries—a semester, a year, the life of a project, and so forth. I suppose such units are comparable to the annual cycle in the life of a primitive group. Schools, in part, have an annual or semiannual opening or beginning; an establishment of order, structure, and routines; a long steady state period; and a closing or termination of the year. By observing throughout such a

cycle one is privy to the special problems of phases in the social activities of the system, and one sees particular actors—administrators, teachers, pupils—coping with each other at critical, different points in time. Early on we had an intuitive feel that these data were more critical for teachers and administrators in our classes, in the sense of giving them help in thinking about their problems. Now we see this as a part of a larger theory of action conception.

The main point is that there is a need for a statement of the intensity of observation so that readers can assess the credibility of the results. A secondary point is a caution in moving toward a simple checklist-type judgment. The amount of time interacts with the scope and focus of the problem. A tertiary point is a common concern for adequacy of data across modalities of social science.

Qualitative and Quantitative Data. Traditionally, participant observers and educational ethnographers have shied away from quantitative data—from tests, questionnaires, or structured interviews. In my own case, early on I deliberately did so because I did not want to defend what we were doing in terms of the canons of the quantitative test literature. I had spent a year attending to the validity of children's personality and adjustment tests (Smith, 1958). I thought I was on to a very different kind of problem; I wanted it judged on another set of criteria. Now I think many of the arguments about qualitative and quantitative data are pseudo issues. Some field workers, such as Blau (1955), count kinds of interaction to make specific points. Others, such as Becker et al. (1961), quantify field note records regarding particular issues; on occasion we have taken issue with such procedures (e.g., Smith & Geoffrey, 1968, pp. 255–256, the two realities problem).

Some educational anthropologists (Cazden, John, & Hymes, 1972; Cicourel, Jennings, Jennings, Leiter, Mackay, Mehan, & Roth, 1974; Erickson, 1975), pursuing more specific substantive problems in the "new ethnography" rather than the broader, more holistic study of a group or a community, have moved to audio and video tapes of classroom events and the beginnings of quantitative analyses of these. From these records, they reconstruct the implicit meanings in speech and nonverbal behavior. My guess is that this tradition will gradually merge with the time-sampling tradition of study begun in child development by Goodenough (1928), Olson (1930), and others. In addition it will probably merge with the large literature on quantitative observation of teaching and classroom events, work well summarized in Dunkin and Biddle (1974) and Simon and Boyer (1967). A quick, nonquantitative survey of Dunkin and Biddle's book on teaching and Cicourel et al.'s (1974) book on language and school performance suggests a minimal overlap in reference citations. The work of Barker and his colleagues (1954, 1964) stresses direct observation of social settings and quantified records, but it too has remained as a nearly independent tradition. I find these to be puzzlements in the social science of knowledge.

Triangulation and Multimethods. Even though one tries to stay at the data level, the phenomena keep dragging one back to one's purposes and one's more

general conceptions. Briefly I have indicated some of the dimensions of partici-
pant observation at the data level which enable one to analyze any piece of work
and to begin to see the methodological structure of any project. A further
argument needs to be entertained—that is, the nature of combining and synthesiz-
ing the multiple kinds of data implicitly and explicitly raised in the discussion.

Our attempts to deal with the issue have been mainly an outgrowth of
Campbell and Fiske's (1959) multimethod, multitrait approach to construct
validity in psychological measurement and Denzin's (1970) concept of triangula-
tion. By triangulation, Denzin means the use of multiple kinds of data brought to
bear on a single problem or issue. At an initial level, all of our studies combine
direct observation interviews and conservations and document analysis, all of
which we bring to bear on the issues at hand. A similar but more sophisticated
analysis is made by Campbell and Fiske (1959). Their argument is simple and
powerful. In checking the validity of psychological tests to give a picture of an
individual, one needs multiple instruments dealing with multiple traits. The
pattern of intercorrelations allows one to detect reliability coefficients and two
kinds of validity coefficients: convergent coefficients, where the several instru-
ments measuring the same trait should be high, and divergent coefficients, where
the test formats are similar but the traits are different and the coefficients should
be low. At the time of their writing, and probably now as well, the psychometric
data available on most instruments was severely limited.

In our work we think we are pursuing a similar paradigm, even though our
data are qualitative and even though they are more a patchwork of partially filled
cells and inferences. Figure 1 presents our elements of a multimethod, multiper-

1. Methods
    1.1 Observation
    1.2 Informal interviews
    1.3 Documents: lesson materials, computer printouts, et cetera
2. Persons
    2.1 Pupils
    2.2 Cooperating teachers
    2.3 Principals
    2.4 Other teachers
    2.5 Multiple incumbents of multiple positions in multiple organizations
3. Situations
    3.1 Pupils at terminals
    3.2 Classroom teaching: announced and unannounced visits
    3.3 Multiple parts of the curriculum, in addition to arithmetic
    3.4 Multiple schools
    3.5 Multiple organizations
    3.6 Multiple parts of the country
4. Variables
    4.1 Individual: schemas, traits, motives
    4.2 Group: classroom interaction, activity, sentiments
    4.3 Organizational: schools, universities, R & D, Title III

Figure 1. Elements of a multimethod, multiperson, multisituation, and multivariable
matrix. (Smith & Pohland, 1974).

son, multisituation, and multivariable matrix. The picture of the phenomenon—for instance, Geoffrey's classroom, the innovative Kensington School, the computer-assisted instruction in the rural highlands, or science education in Alte—involves the interrelation of data from different sources (observation, interviews, documents), different people (teachers, administrators, school board members), different situations (classrooms, schools, board meetings), and different variables and concepts—systems, norms, interactions, individual schemas.

When the items fit, agree, or are congruent, the picture evolves confidently. In the Alte High School, as I was trying to come to grips with the nature of social science, it was revealing that most of the faculty had degrees in history, that the psychology courses (and instructor) were not listed as part of the social science department, and that the social science department office sign said "History Department." Such items led to several interpretations about a dimension, degree of breadth, in social science at Alte High School. The central thrust of the multimethod approach seems to be an argument for internal consistency; the data hang together, the correlations would be high if data were scaled and quantified.

When the data do not hang together, one cannot throw out the tests or items and go back to the drawing boards. The problem is more difficult. Usually it sends one back for more data. Is an individual lying, seeing only part of the system, or ignorant of a whole set of events? Is the principal conveying his own wishful thinking of what science education is in his building? Is the faculty sketching out "what they are reaching for," "trying to do," rather than the day-to-day realities? Or is it, as the test makers might see it, that there is another trait or variable involved? In an important sense, one begins to reconstrue one's conception of the phenomenon. In a brief but powerful statement, Charters and Jones (1973) caution about the risk of appraising nonevents in program evaluation. Specifically, they ask: "When has an innovation been adopted?" Their analysis produces a conceptualization of four levels of organizational activity: institutional commitment, structural context, role performance, and learning context. Such a conception puts order into the discrepant data one finds in formal documents and institutional plans, in discussion of administrative strategy and tactics, in teachers' day-to-day activities, and in pupil activities. The new conception sends one back for more data to check its adequacy. If the distinctions are sound, the data should diverge, just as correlations should be low on tests measuring traits which are independent (e.g., intelligence and weight).

Attempts to triangulate or to build multimethod matrices with qualitative data often result in congruencies which strengthen the validity of the picture one is drawing. When the data do not converge then one checks the points with more data, reconstrues the phenomenon, that is, makes more subtle distinctions than one began with, and then one goes for more data to recheck the new descriptive model, conceptual system, or interpretation. Van Velsen (1967), in discussing the final report and its credibility, argues for inclusion of lengthy excerpts from

field notes. These notes, which include some context beyond the central point entered by the author, permit the reader to regroup and reorder them for her or his own analytical and verificational purposes. Van Velsen's label for this is "situational analysis."

In short, these investigators are arguing for a common criterion—data from multiple methods brought together on common issues and presented in a way that the reader can perceive each source clearly and can begin to weigh the overall credibility and significance of the analytic interpretation.

The Sampling Problem at the Data Level. Whenever one cannot be everywhere in the system all of the time, one is faced with the sampling problem just as other social scientists doing more quantitative survey or experimental research are faced with the problem of sampling. Our comments here intertwine with earlier remarks (e.g., intensity of observation) and with later remarks on the descriptive and theoretical purposes of the particular projects. In the case study of science education in the Alte School District we were trying for a descriptive, analytic account of the nature of science education (math, natural science, and social science) in kindergarten through grade 12. Even for a small district, a hopelessly large task. With the problem stated—"What is the nature of science education in the district?"—a tentative sampling plan, Figure 2, was conceived.

SAMPLING DOMAINS

1. Principals

2. Schools and teachers

3. Classes and teachers by weeks:

	Cycle 1	Cycle 2 Social	Cycle 3	Cycle 4 Alternative
High School	Science	Science	Math	high school
Junior High	Team 1, 7th	Team 2, 8th	Team 2, 7th	Team 1, 8th
Elementary	School A	School B	School C	School D

4. Elementary Curriculum Committee: Science, Math, Social Studies

5. High School committees preparing for North Central Evaluation: Policy, Math, Science, Social Studies

6. Special circumstances:
   a. Citizens, school board members
   b. Knowledgeable professionals who have contacts in the district
   c. Faculty meetings, PTA meetings, etc.
   d. Special events, programs, activities

Figure 2. An early sampling plan for the Alte study (Adapted from *Science Education in the Alte Schools,* by L. M. Smith (NSF Case Studies in Science Education), 1977).

The logic of the plan was quite simple and was built on conventional wisdom about schools in general and Alte in particular. I talked with each of the principals. Typically, this was in the form of an open-ended oral history interview: What's science education like in your building? This usually had several components: an hour of taped comments; a walk through the building with

running comments on facilities, materials, and staff; an armful of documents; and initial meetings with particular teachers by chance and design. In effect they conveyed, at least initially, their perspective of science education—curricularly and organizationally, hopes and realities, problems and successes. It seemed that I needed to see teaching and learning in process; consequently I devised a possible way of cycling through schools, levels, and domains of science during the semester of field work. Early on I found there were curriculum committees and, even more importantly, by chance, that Alte High was in its self-study year, getting ready for a North Central Evaluation. I was permitted to and did join some of those 7:15 a.m. discussions. A number of special circumstances also arose. I interviewed people with special perspectives on the district—long-term residents, parents, school board members, professionals who enter the schools for various reasons (psychologists, university professors, etc.), and districtwide central office personnel. Some of these were early ''Tell me about science education in Alte,'' and some were more exit interviews. The latter started with ''Tell me'' and soon evolved into a series of ''How about . . .?'' wherein I raised interpretive hypotheses. The major point here, however, is that we had a rough initial image of the territory. We tried, through interviews, observation, and document analysis, to cover that territory.

Time restrictions in Alte caused us to modify in several ways the more formal plan outlined in Figure 2. For instance, the classroom observations shifted to an initial concentration on the junior high school—every science, math, and social studies teacher was observed at least once, and a number were observed several times. Usually these were intertwined with brief to several-hour interviews. Most of the elementary school observations were concentrated in two schools. At the high school I observed several teachers in each department—usually a mix of those I had known and not known. Typically these were of the unannounced ''May I sit with you this morning?'' type. No request was refused; often there were comments such as ''Today's a lab . . . a review . . . a film'' and so forth. I said ''Fine'' and went in to see what was happening. Usually I was given a copy of the text or lab materials, and a brief comment or two locating the lesson in the broader perspective of the course, department offerings, or grade-level sequences. Similarly, I hunted documents—high school yearbooks, curriculum guides, reports of curriculum committees, and, significantly, the *Alte School News*, a newsletter to the citizens of the community. Some of these I scanned, some I read intensely with particular questions in mind. Again they presented a sampling problem.

Most participant observers don't speak to the issues of sampling. In our view it lurks behind every decision the investigator makes when he elects to be here or there, to spend more time here rather than there, and decides what array of documents to read, of people to interview, of settings to hang around. At the data level, the question is always, ''Have I seen the nooks and crannies of the system as well as the main arenas, to give a valid picture of the system?'' The main criterion we strive to meet is to know the total system better than any participant,

who is often restricted to a particular niche or position in the system. As we learn more about the system at its concrete level, we find areas we hadn't anticipated, and we find we have to shift plans. This shift in plans has been called "progressive refocusing" by Parlett and Hamilton (1972) and speaks to the flexibility of the methodology. Similarly, Glaser and Strauss (1967) have spoken of "theoretical sampling." In their discussion, they are attending to the problems of the kinds of ideas and theory being generated and the shift in activities at the data level as the more abstract problems under investigation are clarified. My central point is that the participant observer, in a procedures chapter or a methodological appendix, needs to convey to the reader his initial plans and intentions, the changes along the way, and the final resolutions and accompanying reasons. In this manner, the reader, or the skeptic who wants to replicate, can see clearly how and why he did what he did.

Muted Cues and Unobtrusive Signs.   Close observation in a setting produces what Andrew Halpin (1966) has called "muted cues" and what Webb, Campbell, Schwartz, and Sechrest (1966) have called "unobtrusive measures." When the clerk in the Washington School sat silently "working" within earshot while I had my entry conversation with the principal, and two weeks later she thought it would be a good joke on the principal to make out a central office timecard for me to sign first and then for him to sign, I began to form an image that she had more to do with the social structure of the school than the formal organization chart indicated. In spite of my initial anxiety, it turned out to be a good joke, she thought, and thereafter she cheerfully volunteered neighborhood and school stories and items "for your book": items from the mailman who walked through the community daily, from local shopkeepers, and from parents, not to mention items from downtown office personnel that came by her desk. In effect, the muted language, the unobtrusive traces of social life, suggest a host of important, often unverbalized issues that enhances the quality of data available to the directly observing inquirer, in contrast to the inquirer not in the setting. In project proposals, which are based on some pilot observations, I look for data reflecting muted cues and unobtrusive measures. Final reports which don't contain such data seem less significant than those that do.

Summary.   In the analysis of the structure of participant observation at the data level, my contextualist bias has encroached to demand that other elements and the multiple interrelations among the elements be stressed. Careful analysis of the project's data does begin to locate it in a position from which judgments of quality can be initiated.

The Descriptive Narrative: A First-Level Interpretation

In considering the multiple dimensions of participant observation, we have been inquiring first into the adequacy of the data generated by the researcher, in the course of which we have commented necessarily but fragmentarily on the uses to which the data are put. Since first reading Homans's *The Human Group*

(1950), I have been struck with the impact of his descriptive accounts, in everyday language, of a group, organization, or society under consideration, and with his attempts to conceptualize these events in more abstract concepts and propositions. Most of the groups we have studied have evolved over time, and we have usually wanted to grapple with the change processes in the group during these periods. The latent agenda seemed to be a belief that the process issues (in contrast to the structural) are more critical for teachers, school administrators, and curriculum developers, our usual audiences. This has led us to argue for "telling the story" of the characters, settings, incidents, and on occasion the drama of conflict, crisis, and denouement. More recently we have been persuaded by White's (1963) analysis of the logic of historical narrative that there is not "a" narrative or "the" narrative; rather, multiple narratives might be written.

White's Analysis.   In his "The Logic of Historical Narration" (1963), White makes a half dozen points which clarify the nature and assumptions of the descriptive narrative in participant observation. He sets the stage with a definition of history:

. . . every history is a history of some entity which existed for a reasonable period of time, . . . the historian wishes to state what is literally true of it in a sense which distinguishes the historian from a teller of fiction or mendacious stories, and the task of the narrator is to give a connected account of the entities' development in time. (p. 4)

Participant observers, particularly those of us interested in processes of education, should have little difficulty with this initial statement.

Next White distinguishes "a chronicle" from "a history." The former is "a conjunction of non-explanatory empirical statements which expressly mention that subject and which report things that have been true of it at different times" (p. 5). A history is distinguished from the chronicle in that a history employs the notion of explanation. At this level the chronicle is close to our data level. His comments regarding facts in the narrow sense, statements about conditions and statements about events, follow upon this. For White a chronicle is true insofar as its component statements are true. Even as chronicles move toward historical narrative, he expressly indicates that this requires no necessary commitment to a covering law or regularity model of explanation. Nor, in his judgment, is it a commitment to explanation by causes.

Third, he embarks on a major discussion of the meaning of one history being better than another, when both are "true" in the sense of the truth of the items in the chronicle. He comments that historians have developed reasonably clear statements of alternative positions regarding this level of their histories. The list of labels includes subjectivism, essentialism, big battalion history, encyclopedianism, and scientism. Again they seem congruent with various stances on ethnography and participant observation.

Fourth, he argues that calling one history a superior or better history than

another piece of work involves more than judgments of truth of facts and truth of causal statements. Finally, regarding historical memorability, he leaves the analyst with a relativistic and contextualist problem: ''I know no rock or historical practice or usage upon which to rest some definition of historical memorability'' (p. 26). In my judgment, that takes the participant observer, insofar as his descriptive narrative is like an historical narrative, back to a broader contextualist stance of his purposes, his priorities, his problems, and his situation, and the purposes, priorities, problems and situations of his multiple audiences. In a sense this is every researcher's concern. In another sense, I am arguing that the participant observer has a special and potentially very powerful stance toward these issues.

Beittel's Analysis: Presentational Modes.   Explicating Kenneth Beittel's approach to participant observation constitutes an exercise in the totality of the methodology. However, his point of view regarding the several narratives which might be written is central to his analysis and provides a complement as well as supplement to White's historical analysis of narrative. In contrast to most participant observers, Beittel focuses on the individual rather than the group. The individual in this case is in an interesting setting, a basement laboratory in the art education building at Pennsylvania State University, and he is involved in ''the making of art,'' a series of drawings using pencil, charcoal, and ink. The laboratory permits time-lapse photographs, notes on the process, and interviews stimulated by the photos. The two-hour weekly sessions continue for ten weeks. Some individuals return for several semesters. Beittel's major assumptions are twofold: ''. . . to study the making of art one must move as closely as possible . . . to the creating stream of consciousness, and [secondly] . . . a special participant observer role is essential to this closeness'' (1973, p. 8). At the data level, Beittel has (1) mute evidence, that is, the pictures themselves; (2) iconic representations, videotapes and motion pictures of the artist in action; (3) process representations of the evolving art work; that is, the time-lapse still photographs of the developing picture; and (4) the notes kept by the observing researcher and recordings of the interviews between the observer and the artist.

From these data, Beittel argues that several kinds of narratives can be developed. Several of these remain very close to the data. The ''first-person singular narrative'' is an attempt to ''reunite the available information on a drawing's evolution as though the artist were thinking to himself as he works'' (1973, p. 34). His examples, such as this one of Larry's, graphically convey the portrayal: ''Let's see . . . I'd like to try to get that bad downtown Baltimore feeling. I can see it. How to begin? Like I'm right in the middle, everything stronger than me. But how to get it on the page? What line to hang it on? They need to fan out like. Guess I'll just jump in. Stronger, bolder strokes. There . . . better watch how I mix black and white paints. I'll lose that grunginess of outdoors against that black, black inside feeling—the bar, the strip show. The black's gotta count . . .'' (1973, pp. 36–37). But as gripping as the narrative is, it loses for Beittel some of the broader context, both in the long-term serial

perspective and in the host of additional elements of meaning and feeling about ''downtown Baltimore.''

Beittel tries next for what he calls a ''multiple consciousness narrative.'' The artist, Beittel, an assistant, and a visiting psychologist all have taken part in the participant observation of the drawing process. The descriptive narrative produced by these multiple consciousnesses stays at the educated layman's level as the several individuals come to grips with the events and conditions of ''a unique time-space artistic process.''

Beittel labels a third kind of narrative ''literary psychology.'' It represents an observer with a particular theory in mind which guides his perceiving and reporting but which still remains couched in the language of the general culture. Frequently used theories are those of Freud, Jung, Langer, and so forth. This mode of narration gives Beittel great difficulty. His problem, so it seems to me, is his own creativity. Consider, for example, the following comment:

My procedure, in actual practice, has been that of coining new terms and labels to aid my perception and description of the individual case, arriving at these neologisms as inductively as possible. Since, however, I use the new terms for more than one case, they function somewhat like principles from psychological theory. In truth, they function more usefully when I discuss the problem of representing individual cases in the abstract, or in the general. When I actually speak of a given individual, the terms do not occur as readily. In this book, for example, I have spoken of ''artistic causality,'' ''idiosyncratic meaning,'' and ''intentional symbolization.'' (1973, p. 44–45).

If I read him correctly, Beittel has the beginnings of major sensitizing concepts which reflect both inner and outer perspectives and which have considerable power in thinking about artistic creativity. Informally we have tried to test this potency hypothesis by equating artistic creativity, teacher creativity, and researcher creativity and looking for analogues to Beittel's concepts in these other domains. But my major point is that the generation of these concepts plays back into his observing and narrating. As a consequence, the literary psychology narrative begins to take on a considerably more theoretical appearance.

In what he calls ''historical and interpretive modes twice removed from the artist's stream of consciousness,'' Beittel distances himself in time and specific occasion to develop narratives which I am more inclined to call analytical or theoretical accounts. They continue to have a curious blend of the artist and the observer. In essence he tries to draw out the ''artist's conceptualization about making art'' as the artist reflects on what he's done. The analogue seems to be similar to our several attempts to capture a teacher's theory of teaching or a principal's theory of administration, that is, to determine what are the working concepts and propositions. Finally, in some if not most or all artists, Beittel pushes toward what the depth psychologists are prone to call more basic or fundamental levels of functioning, and what he calls ''the artist's superordinate concepts on the making of art or idiosyncratic artistic myths.'' These are the

more all-inclusive accounts of the meshing of an individual's life and his art. His major examples are autobiographical, ''the Bach-music landscape theme'' and ''the river theme.'' In time they stretched over decades; in space they ranged from the Susquehanna River to the battlefields of France; and in meaning they captured the full reaches of an evolving life perspective.

Finally, he takes a major position on the observer's relation to the individual(s) he studies. This he labels the ''formative hermeneutic mode,'' a stance which seems close to a Rogerian counseling relationship. Understanding and helping intertwine. The narrative takes on an action perspective.

This commitment to a narrative as one of the outcomes of participant observer research has further major implications on the kind of sociological and psychological theory that one generates and uses. I'm reminded of Becker's arguments in the preface to *Sociological Work* (1970), wherein he sees society as collective action, people doing things together, and sociology as a study of the forms of collective action. Any talk of structures and functions must come back eventually to individuals doing something together. Similarly, Homans's (1950) early admonitions about the ''big'' words of sociology—status, role, culture, authority—must eventually lead back to human beings doing things in particular times and places. In psychology, Henry Murray's (1938) concerns for persons, situations, actions, proceedings, and serials, not to mention needs and presses, reflect a point of view, a theory, which is compatible with results presented in narrative form. More recently, Sarbin (1977), in the *Nebraska Symposium on Motivation*, has argued that psychological theory needs to be reconstructed around a root metaphor or world view of contextualism which implies, for him, a dramaturgical perspective, units built on historical events and interacting individuals, and concepts such as scenes, plots, roles, and actors if one is to deal with creativity, novelty, and change in the human condition. By implication he is arguing that other theories are less amenable to these issues.

Summary.   This discussion of the descriptive narrative has shown how complex and problematic is what we once thought was a necessary but simple task—telling the story of the case under investigation. By appealing to White's analysis of the historical narrative and Beittel's imaginative set of alternatives, we now see a fuller set of distinctions about the narrative. The implications for the kind of psychological and sociological theory are also apparent. The data problems, the metatheoretical dilemmas, and the theoretical stance all contribute a context to the narrative. The individual ethnographer in doing his work is faced with a series of contingent decisions. He who would judge a particular piece of ethnography faces a task no less complex. To think otherwise is to make a serious mistake.

The Theoretical-Analytical-Interpretive Level

In the discussion of clusters of dimensions which can be used for analyzing and evaluating participant observer research, the argument has proceeded from

the concrete to the abstract. A number of concerns were raised regarding the quality of the data. At the next level of abstraction, it was argued, most participant observers want to ''tell the story'' of a group, an organization, a community. To some, such as Erickson (1975), Lévi-Strauss (1963), and Wolcott (1975a), this is the core definition of ethnography. Most researchers, however, want to move more abstractly into what is typically called theory, analysis, or interpretation. At this point, the field splits apart and almost all commonality is lost. For purposes of clarity I propose to describe briefly the evolving position we have taken in much of our work, and then to indicate alternative positions. The arguments for the pros and cons of those positions involve a series of metatheoretical dilemmas, controversies which have plagued social science and philosophy for a long time and which seem to have no clear solutions.

In recent years we have been urging our students to make explicit the theory that is implicit in their studies; that is, we have asked a student, Jones: ''What is Jones' theory of . . .?'' Over the past few years this has yielded Wood's (1977) theory of localist educational task groups, Yunker's (1977) theory of professional socialization of police officers, Finch's (1978) theory of the teacher and the change process in schools, and Wolfson's (1974) and Lipnick's (1976) theory of post–Bar Mitzvah religious education, to list just a few.

Asking the question, What is your theory of . . .? forces the inquirer to attend to issues such as: What is meant by theory? What are the differences between generating and verifying (proving, falsifying) theory? What stance—inner or outer—do you take? What is meant by explanation? What are the boundaries of your case? Of what is your case an instance?

I do not believe that these questions are inapplicable to other research modes, but educational ethnography or participant observation research is an innovative and evolutionary development in most schools of education. The questions take on different meanings in this new context. Trying to justify a different set of activities demands a consciousness of issues that most others can handle by assumption or by appeal to the status quo. Further, in making a judgment of the adequacy of a piece of qualitative research, one important criterion is theoretical coherence of one's point of view. Such internal consistency runs through these metatheoretical issues.

In our first attempts at participant observation (Smith & Geoffrey, 1968; Smith & Keith, 1971), we were confident that what we sought was the beginnings of a theory of classroom teaching in the one instance and a theory of educational organizational innovation in the other. We were operating well within the logical positivist's conception of theory. Some of this had roots in Feigl (1945) and the early debate regarding operational psychology, Bridgeman (1927), and Boring (1945). Zetterberg's *On Theory and Verification in Sociology* (1965) is a simple, coherent account of this position: Concepts are abstractions of reality; some are categorical or class labels, and others are dimensions or variates. Propositions are relations between two or more concepts. Those propo-

sitions that are tentative are hypotheses, and those that are more strongly corroborated are laws. When two or more propositions are joined together, one has a theory. Zetterberg described several variations—inventory of antecedents, inventory of determinants, chain structures, and axiomatic formats. We have developed this position in considerable detail in Chapter 1, "The Nature of Classroom Microethnography," in *Complexities*.

Such a conception led us in several directions. We developed glossaries of concepts with theoretical and operational definitions and pictorial models of miniature and middle-range theories. It led us to a conception of explanation, which later we found formalized in Hempel's (1965) covering law model, both deductive nomological (DN) and inductive statistical (IS). The conception legitimated our emphasis on theory generation in the case study, with later verification/falsification in more classical experimental and quasi-experimental designs. Figure 3 depicts this sequence.

Our conception of theory also led us to see educational principles as one piece of social science and social science as part of the larger general fabric of natural laws produced by science. In turn, this was part of a lawful determined universe. This point of view was influenced, illustrated, and legitimated by several books of George Homans: *English Villagers of the 13th Century* (1941), *The Human Group* (1950), *Sentiments and Activities* (1962), *The Nature of Social Science* (1967), and *Social Behavior: Its Elementary Forms* (1961). All in all, this conception provided a very powerful, integrated stance.

In recent years, this intellectual structure has begun to unravel. The unraveling has not been the simple pulling of a single thread; the fabric was snagged in multiple places and in multiple ways. Early on, it was a major puzzlement in *Complexities*.

A serious discontinuity exists within educational psychology. The language of learning theory—Hull, Mowrer, Skinner, or other behaviorists—used to analyze the behavior of children cannot easily be used by the teacher to analyze and alter his own behavior. As we see it, the problem focuses on the pupil as an object, a complex of operant and respondent behavior controlled by the environment, a part of which is the teacher. The child's "rationality" and autonomy are minimized as the program and the reinforcing contingencies are accented. The teacher, however, usually is implored to be rational, to plan carefully, to meet the child's needs, and so forth, as though the locus of control lay within himself. The teacher who thinks about his own behavior as a series of operants has difficulty in synthesizing these positions. We believe the issue lies fundamentally in the heart of contemporary social science theory, and we do not propose anything like a basic solution. Rather, we are going to present a way of talking about teaching that has seemed "comfortable" to us at this fundamental level. It has provided a congruence between the experience of observing and participating in teaching and the language available for describing teaching. As we meet the traditional problem areas of educational psychology we will try to rephrase them from this point of view. (Smith & Geoffrey, 1968, pp. 87–88)

This puzzlement is in part the genesis of this chapter and the innumerable activities between then and now.

Snags occurred in reading Bruyn's *Human Perspective* (1963); he makes a case for participant observation as *the* method of sociology, based on the central criterion that it respects the nature of the subject matter, the human condition. He contrasts it with the "traditional empiricist" position. I found myself doing what I thought was participant observation, yet doing it from a rationale which he saw as a polar opposite. Table 5 is from his account.

TABLE 5
The Human Perspective: Methodological Dimensions

	Inner Perspective (*Participant Observer*)	Outer Perspective (*Traditional Empiricist*)
Philosophical background	Idealism	Naturalism
Mode of:		
Interpretation	Concrete procedures	Operational procedures
Conceptualization	Sensitizing concepts	Formal concepts
Description	Synthesis	Analysis
Explanation		
Principles	Telic	Causal
Models	Voluntarism	Determinism
Aims	Sensitively accurate interpretation and *explanation* of man's social and cultural life	Accurate measurement and *prediction* of man's behavior

Source: *The Human Perspective in Sociology: The Methodology of Participant Observation,* by S. T. Bruyn (Englewood Cliffs, N.J.: Prentice-Hall, 1966, p. 49).

The Homanian rationale is heavily on the side of traditional empiricism. In addition, Bruyn argued from a broader position in the humanities. The Aesthetic Education Program (Smith & Schumacher, 1972) literature became increasingly salient. The central point, however, remained—Homans and Bruyn, the major exponents of case studies, qualitative observation, and generation of grounded theory, were operating from highly divergent metatheoretical positions.

Quandaries occurred in trying to formalize a concept of explanation. As I read Hempel (1965) and saw the linkages to earlier reading of Feigl (1945), I cheered at the more philosophical underpinnings of Homans and Zetterberg. The major villains that Hempel was flogging were Scriven and Dray, so I went back to their original papers (Scriven, 1959; Dray, 1957). In his characteristic style, a style not known for understatement, Scriven (1959) blithely stated his thesis:

Such, in brief, is the argument that ties together the certainty of explanations with the possession of laws and the possibility of predictions. In its most convincing form, it is due to Professor C. G. Hempel. I refer to it as the deductive model of explanation because it proposes as a criterion for good explanations the deducibility of a statement of the facts to be explained from statements of the antecedent condition and relevant laws. *I have the greatest respect for its powers, its interest, and its adherents, but I shall argue that it is wrong, not only in detail but in conception.* (pp. 444–445; italics added)
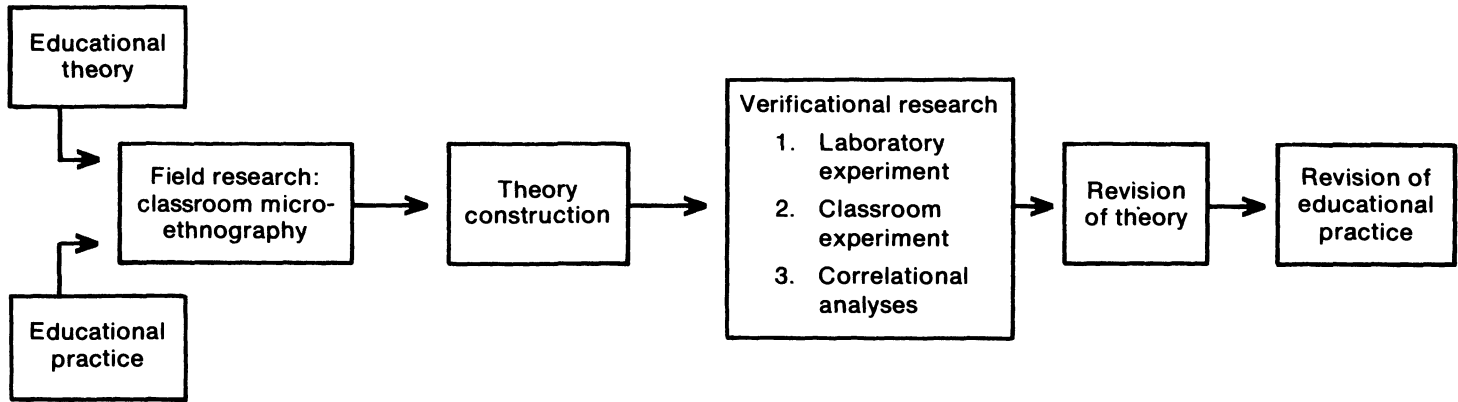
Figure 3. A process model integrating educational research styles, educational practice, and educational theory. (From *The Complexities of an Urban Classroom,* by L. M. Smith and W. Geoffrey. New York: Holt, Rinehart & Winston, 1968, p. 246.)

On the one hand it is reassuring to find philosophy of science, like other intellectual domains, riven with conflicting positions among major theorists. On the other hand, this is troublesome when one is trying to put one's intellectual house in order. And particularly it is troublesome when one is trying to justify a departure from the norms of inquiry in the educational research community.

In part, too, the unraveling occurred as I read more intensively in the philosophy and social science of science—particularly the splendid series by Toulmin and Goodfield (1961, 1963, 1965). The diversity of what was and is scientific method and theory is startling to behold. Stories that I knew before as isolated fragments from high school and college science were elaborated and arranged into a complex design. Variations in inquiry across subject matter areas and across centuries were pieced together. The incredible celestial forecasting of the Babylonians was dust bowl empiricism at its best. The invention of theory by the Greeks spanned a multitude of "interpretations" of celestial affairs before the current synthesis, or present-day common sense. The discovery of time, the Darwinian revolution, the implication for "the present state of nature and humanity as temporary products of a continuing process developing through time" (Toulmin and Goodfield, 1965, p. 246) challenged the fixity of the 17th-century religious-scientific world view. This broader history intermingled with Snow's *The Two Cultures and the Scientific Revolution* (1959), Kuhn's *The Structure of Scientific Revolutions* (1970), and Ziman's *Public Knowledge* (1968), all accounts of scientific communities and how they function.

At a personal level, I found myself challenged by Professor Beittel, both in conversation and in coming to terms with his provocative work on the production of art. He was doing his own variant of participant observer research; he had read our work as carefully as anyone, and he was chastising us for not seeing the full implications of what we were doing. Our lack of vision concerned the incompatibility of our stance at the root metaphor level of contextualism, and the very important problem of individual creativity. In an important way he provided an artistic educational instance of Bruyn's more general sociological perspective. This he did while we were doing an evaluation of an aesthetic education program and more generally starting a series of inquiries into psychological aspects of aesthetic education (Smith & Schumacher, 1972; Smith, 1975a & b, 1977a). It was timely, troublesome, and stimulating to be told that the world view, the root metaphor, the major polar principle, was incompatible with the logic of the methodology we were using and with the central procedural activities in which we were engaged.

Then there came an internal tearing apart of the fabric by the dominant group of educational researchers. As an educational psychologist, I have been taken aback with a number of my most respected colleagues who have expressed a malaise with the theoretical outcome of traditional educational and psychological research, even when done at a highly sophisticated level (Cronbach, 1975; Jackson; Jenkins, 1974; Sarbin, 1976.) The major statement, however, is Cronbach's (1975) address on receiving the APA Distinguished Scientific Award.

He makes several points skeptical of scientific theory in education.

First, Cronbach observes that the aptitude treatment interaction problem seems unsolvable as regards more complex and higher order interactions: "Once we attend to interactions, we enter a hall of mirrors that extends to infinity" (p. 119). Second, he suggests the time element—which decade the data were gathered—as a source of interaction of great importance to educational affairs. Many empirical propositions, especially those involving open systems, have a short half-life. Once again he develops a powerful metaphor to make his point: "It is as if we needed a gross of dry cells to power an engine and could only make one a month. The energy would leak out of the first cells before we had half the battery completed. So it is with the potency of our generalizations" (p. 122). Third, this view has major consequences for his early work on construct validity, in which he and Meehl imported an epistemological rationale from a logical positivist view of social science as a part of natural science (Cronbach & Meehl, 1956). Fourth, as he shies away from "enduring theoretical structures" and "theoretical palaces," he moves toward "interpretations in context." These are more heavily descriptive and more sharply tied to local situations. He, too, accents the analogy to historical inquiry and to the generation of a perspective or point of view. Finally, as he argues that broad and enduring theories about man in society are unlikely on the horizon, he sets forward two goals for the educational inquirer: "One reasonable aspiration is to assess local events accurately, to improve short-run control. . . .The other . . . is to develop explanatory concepts, that will help people use their heads" (p. 126). More and more Cronbach seems to be moving toward what Bruyn (1966) calls "concrete universals," meanings in the local culture, and "sensitizing concepts," which link the scientist's world to the conventional world.

In an interesting sense these more recent educational psychology points of view seem to come full circle, back to Scriven's early confrontation with Hempel. In his conclusion, as he argues for truisms as grounds for historical explanation, Scriven quoted, then rejected, Boswell: "Great abilities are not requisite for an Historian. . . ." Rather, Scriven (1959) felt: "To get the facts ready to one's hand, to avoid invention in reporting them, to penetrate their meaning and illuminate their presentation it might well be said that these *are* tasks to tax the greatest powers of the human mind"(p. 471). Cronbach, referring to Scriven, made his final point:

> The special task of the social scientist in each generation is to pin down the contemporary facts. Beyond that, he shares with the humanistic scholar and the artist in the effort to gain insight into contemporary relationships, and to realign the culture's view of man with present realities. To know man as he is is no mean aspiration.

To someone who has been arguing for "theory generation" as the goal of qualitative field research, this is not a happy state to be in.

Finally, I have been strongly influenced by what I've come to call the Hirst

(1966), O'Connor (1973), and Struthers (1971) debate, the degree to which educational theory is mostly scientific, mostly ethical, or some combination of the two. The position one takes on this metatheoretical issue determines quite strongly the logic of the argument one derives from one's data and analysis. If the key terms of the theory—for example, education, curriculum, and teaching—contain value statements in their very structure (as Peters, 1965; Scheffler, 1971; Green, 1971; and Gowin, 1976, among others, argue) then an ''objective,'' ''scientific'' theory of education is impossible. An educational theory which has ethical components in its core concepts is a very different theory from one which is ''scientific.'' Such a view ripples through such practical items as making recommendations from research studies, dealing with multiple and sometimes conflicting values in evaluation, analyzing divergent interests in the politics of education, and making discriminations among such activities as teaching, instructing, and indoctrinating.

In conclusion, the more involved I became in observational studies, in reading methodological rationales of other observational inquirers, in keeping up with the activities of educational and social science colleagues, and in trying to ground the theoretical rationale in more philosophical conceptions, the more difficulties and irresolvable dilemmas arose. Nonetheless, as I read observational reports, the criteria I tend to focus on at the theoretical level are these: First, insightful distinctions, that is, novel concepts, propositions, and perspectives that tell me something about the phenomenon that I did not know before. Second, clear definitions of new concepts, at the semantic or theoretical level and at the operational, index, or concrete example level. Third, a cumulating glossary of these ideas within a specific project and across projects, that is within the investigator's research serial. Fourth, the interrelations of ideas into patterns or concatenations, as Kaplan (1964) calls them, or more abstract formal deductive systems, as Hempel (1965) calls them. Fifth, I want the findings to be useful, that is, helpful in solving problems when I'm working in the same broad domain, either as a researcher or a practitioner. All this seems to be a way of saying that the theory should be novel, comprehensive, internally consistent, and functional, a reasonably conservative view of theory.

Metatheoretical Issues: Assumptions in the Phrasing of Results

To ask what has been learned from an observational project is to pose the theoretical-analytical-interpretive issue in a slightly different form. To have a rationale for the manner in which one phrases those results moves one into a series of metatheoretical dilemmas. To be called on to judge observational research proposals, as one might do on an NIE panel, or to judge reports of observational projects, as one might do as a book or journal editor, demands a stand on these issues.

Root Metaphors.   Implicit in the structuring of one's results are more general assumptions, stances, and perspectives, analogies and metaphors from which

individuals operate and which guide thinking over troublesome spots and implicitly make some events but not others problematic. Stephen Pepper (1942), the philosopher-aesthetician, speaks of these as root metaphors. He argues that four or five of these have currency in contemporary thought: mechanism, organicism, formism, and contextualism. To conceive of human events in terms of clocks and pendula or billiard tables, balls, and cues, is to adopt a mechanistic root metaphor. Behaviorism, in his view, is a psychological theory grounded in a metaphor which solves many problems but has more trouble coping with problems of creativity, of opinion, of choice, of tragedy. Organicism or growth metaphors undergird many counseling and personality theories (e.g., self actualization). Formism or structural metaphors give legitimacy to trait theories and classical individual-difference psychology. Contextualism raises images of historical happenings, events, contexts, and syntheses. Goffman's (1959) brand of symbolic interactionism and Burke's (1945) grammar of motives would be others. In brief, the hypothesis I'm proposing is that a critic, judge, or evaluator brings one or another of these root metaphors to his task, just as the inquirer brings one or another to his task. Most participant observers are probably contextualists. When mismatches occur, the critic or judge defines almost unconsciously the inquiry out of the domain of legitimacy.

Inner vs. Outer: Observer Stance and Theoretical Perspectives.   When Geoffrey and I first began thinking about what we were doing methodologically, we began to talk about our "inside-outside" stances. This seemed to make uncommonly good sense in understanding what was going on in the classroom. I was the objective outsider; he was the insider, privy to the entire system. Later we were to find that Gold (1958) and Junker (1960) had a taxonomy of such roles, that Bruyn (1967) spoke of involvement and detachment, and that Powdermaker had a book entitled *Stranger and Friend* (1966). She stated the task for the outsider in lucid prose in the preface:

To understand a strange society, the anthropologist has traditionally immersed himself in it, learning, as far as possible, to think, see, feel, and sometimes act as a member of its culture and at the same time as a trained anthropologist from another culture. This is the heart of participant observation method—involvement and detachment. Its practice is both an art and a science. Involvement is necessary to understand the psychological realities of a culture, that is its meanings for the indigenous members. Detachment is necessary to construct the abstract reality: a network of social relations including the rules and how they function—not necessarily real to the people studied. (p. 9)

Once again, we find that the kind of "learnings" one desires from one's inquiry keep tugging at the kind of data. For Powdermaker, "meanings for indigenous members" and "construct the abstract reality" constitute the twin goals. Others (Bruyn, for instance) stay more with the meanings for the participants, while some (Homans, for instance) are more behavioristic natural scien-

tists looking for general truths and hypotheses. I have never lived comfort-
ably with the dichotomy between the inner perspective—the world as viewed
from the point of the agent—and the outer perspective—the agent viewed as one
part of the natural world.

A resolution or synthesis, if it be that, seems awkwardly simple, perhaps
overly simple. It lies in the continual transformation of the internal perspective
through knowledge from the external perspective. For instance, in our early
study of an urban classroom we approached the setting, in part, as descriptive
behaviorists. The analogue between the Skinner box and the classroom box was
very real. Equally, Geoffrey's point of view, especially his decision making,
seemed very critical. Our hope was, and is, that insofar as we could say some
important things about the regularities of his classroom, regularities that were
generalizable to other classrooms, other teachers could be taught to think and act
in terms of these regularities. Their internal perspectives would be enhanced by
such knowledge.

This same position is reflected in a point of view about consulting which
derives in part from Gouldner's (1961) comments on the theoretical requirements
of applied social science. First, one needs to carefully understand the client's
purposes, internal perspective, and statement of the problem. Second, these ideas
must be translated into one's own theoretical point of view and solved in one's
own terms. Third, the ''results'' must be translated back into a framework useful
to the clients. Usually a fourth step occurs, one of mutual adaptation. The
experience teaches both parties things they hadn't known before—enlarges and
differentiates their repertory of ideas.

If I read the Toulmin and Goodfield (1961) point of view correctly, they are
arguing that the conventional wisdom, the common sense of an era, has evolved
over the recorded history of man. As they say in regard to the fabric of the
heavens, ''Common sense is a powerful mould. . . . One century's common
sense is an earlier century's revolutionary discovery which has since been
absorbed into the natural habits of thought'' (1961, pp. 15–17). People with
commonsense ideas that heavenly bodies are of different orders, such as stars,
planets, and meteors; that atoms are not the ultimate particles sought by the
Greeks; and that human history antedates Adam and Eve are in some important
respects different from people of earlier generations. The most important aspect
of this difference is that outer perspectives developed by cumulating applications
of intelligence and rationality to problems have created social structures which in
turn create individuals with different internal perspectives. Scriven (1972) takes
an even stronger position and argues for sidestepping the entire debate: ''I want
to suggest that once again we should be willing to forget the dichotomy of
external/internal, subjective/intersubjective, and think of these as claims that
both require and refer to internal states *and* external ones'' (p. 114). All of these
arguments seem to concur with, but go a step further than, Campbell's discussion
of qualitative knowing, which we cited earlier.

Miniature Theories vs. General Theories.   Because case studies of classes or schools often have a holistic or systemic quality, they tend to get caught in dilemmas regarding the scope of the theory. Various labels have been coined, such as miniature theories, middle-range theories, and substantive theories, to contrast with abstract theory, formal theory, or general theory (Glaser & Strauss, 1967; Merton, 1957; Zetterberg, 1965). In our work we have tended to solve the problem in several ways. First, we accent the building of pictorial models of smaller pieces of the educational world under study. These might be described as miniature theories of pupil roles, such as court jester, or pupils on contract (Smith & Geoffrey, 1968), or miniature theories of facade and individualized curriculum and instruction (Smith & Keith, 1971). The major concepts within these miniature theories become parts of larger and more abstract clusters within the same research study. Increasingly, and with some difficulty, we are attempting to link them across studies. For instance, in the *Alte* investigation in 1977 I picked up the concept "idiosyncratic styles of teaching," which had come up almost tangentially a decade before in our apprenticeship study (Connor & Smith, 1967; Smith, 1972). The hope is that these larger integrations will culminate in a more general theory of education.

Pattern Explanation vs. Deductive Explanation.   If one can believe the philosophers of science (Hempel, 1965; Kaplan, 1964; Scriven, 1958, 1959), a large controversy exists in the phrasing of abstract social science results. We got mixed up in it in several ways. First, the Homanian approach, which is essentially grounded in a deductive nomological rationale, was one of our guides. Second, Becker's (1958) early paper on problems of inference and proof in participant observation suggested a four-step procedure: (1) selection and definition of problems, concepts, and indices, (2) checking the frequency and distribution of phenomena, (3) construction of social system models, and (4) presentation of results. It, too, was influential and pushed us toward patterns, configurations, and concatenations. Early on, Geoffrey, Keith, and I struggled with the various diagrams, models, and miniature theories in *Complexities* and *Anatomy*. We tried, unwittingly at times, for more abstract, deductive, covering law statements and at other times for more concrete, systemic, configurations or patterns. The dilemma has remained. We have found each mode to be informative at different points as we reached for understanding.

Educational Rather than Social Science Theory.   Traditionally, and particularly in its research efforts, education has borrowed from the social sciences. The methods, measures, apparatus, and ideas of anthropology, sociology, and particularly psychology have been brought to bear on the teacher, the classroom, the school, and the curriculum. At this time it seems appropriate to ask whether education has profited substantially from the stepsister relationship, and whether it may now be time to take seriously such terms as teaching, curriculum, steering group, lesson, and recitation and to build a genuinely educational theory with core primitive terms, derived terms, and sensitizing concepts of its own. Partici-

pant observer research seems uniquely suited to this task because of its efforts to understand events in a culture and system from the point of view of the practitioners in the system. Ferreting these out, codifying the shades of meaning, building them into configurations and propositions, using them to solve significant problems seems a worthy objective. A vigorous application of such a criterion by journal editors or funding committees would change the nature of educational jargon and maybe eventually the phenomena themselves.

Social and Educational Theory: Natural or Artifactual.   Basic to the formulation of one's results is a belief concerning the formulation of social science generalizations as natural science laws or as more artificial or artifactual principles dependent on human institutions which are constructed for individual and collective purposes. In a sense this seems a form of emergence, that is, living organisms and events are different in some important ways from inorganic events, and human beings are different in some important ways from other animals. As one moves into social events of human beings, the openness and indeterminancy loom larger and larger. Natural law seems less and less applicable, so the argument goes. Within educational theory Cronbach (1975) and Gowin (1976) have voiced a variant of this more explicitly than most of their fellows.

The is/ought problem is solved classically by splitting science and ethics, with science trying to describe the world as it "really is" and with ethics trying to clarify the good or the ideal, the world as it "should be." As social theory is conceived more toward the artificial or artifactual, it often takes on aspects of practical theory or a theory of action (Schwab, 1969, 1971, 1973). This raises old arguments that are sometimes posed as historical explanation vs. scientific explanation. Dray (1957) and Scriven (1958, 1959, 1972) have had a running argument with Hempel (1965) for several decades over these issues. In a recent paper, within a long series of essays attacking the more formalist position of the logical positivists, Scriven (1972) argues again the nature of historical knowledge as the prototype for education in particular and social science in general. He is referring to "weak knowledge claims . . . a knowledge claim of a rather different kind from the usual ones." He argues:

One would thus expect it to be the norm in history and, if this approach is logically sound, it does something to rescue history from the choice between (usual conceptions of) the Scylla of science and the Charybdis of literature. We can, for instance, answer the question, what do we learn from history, without having to produce absurd or trivial laws, or bare particulars about the past, or murmur mysteriously about deepening our understanding of man. What we most importantly learn from history is a range of possibilities—not of probabilities, not of certainties. And of course these are not mere possibilities, of the kind that one has in mind when one says, Oh, anything is possible! They are significant possibilities, ones that have shaken empires or cabinets before and may do so again for all that we know to the contrary. They are thus most deserving of our respect, and with our knowledge of them we can plan more rationally for the future. If we

wish to make a certain outcome more likely, then we can try to bring about those conditions which on previous occasions, not demonstrably irrelevant to the present case, did bring about that outcome. If we wish to prevent an outcome, what we can do is to make it less likely; we try to eliminate those circumstances which have in the relevant past brought about this result or repeat those circumstances that have previously frustrated it. We rarely have much idea about how much effect such actions will have, speaking precisely, but we sometimes know that they have a ''good chance,'' or are a ''desperate hope,'' and to suppose that we shall ever be much better off than that in human engineering a history-conscious world is pipe dreaming. The sad thing is that we could have done so much and done it so much better if we had been willing to learn the lessons from history that are there to be learned, instead of going in search of some Holy Grail whose contents would give us the same kind of predictive reliability in history that we have in astronomy. (p. 115)

If I understand him correctly he is arguing the more general epistemological case of knowledge in a theory of action.

In summary, educational research workers differ on a number of often implicit dimensions and configurations, which might be called metatheoretical issues. A half dozen of these seem particularly important:

1. The root metaphor within which one works—mechanical, organic, formal, or contextual.
2. The inner or outer perspective one chooses, that is, a stance from the subject's point of view or the outside observer's point of view.
3. A theory which is more limited in scope and time to a local context versus one that is more general.
4. A level of abstraction that is more descriptive and concrete or more abstract and interpretive.
5. A model of explanation that is more covering law versus one that is configurational or contextual.
6. A theory that is more action oriented and more ethical versus one that is more descriptive and analytical.

It is my contention that a reader of participant observer reports, as he positions himself at one or another of the poles in these metatheoretical dilemmas and as he treats these positions as value laden—good, appropriate, desirable—will make varying judgments on the quality of any particular piece of research. It is my contention also that while these issues separate various ethnographic researchers from each other, they also represent major differences between the ethnographer and the larger community of educational researchers. As such they are problems needing more general attention in the educational research community.

CONCLUSIONS

This essay has been directed largely to the educational research community,

perhaps most thoroughly represented by AERA. Most basically, the hope has been to widen and deepen the discussion of a broad methodological stream of inquiry—educational ethnography, participant observation, and other case studies. More by implication than direct analysis, the assumption is that the dominant paradigm—experimental, quantitative, positivistic, and behavioral— has been too restrictive to cope with the ideas, the problems, and the interests of what is called education and of people who call themselves educators.

More positively, the analysis and its several lines of argument have tried to make several major points. A large, interesting, and provocative literature, both substantively and methodologically (Tables 1–4 and the reference list) exists within this field study tradition. As the activities reflected in those tables engendered conversations and communications, individuals began to coalesce in conferences, groups, communities, and invisible colleges. Witness, for example, the Cambridge Evaluation Conferences of 1972 and 1975 and the recent publication of *Beyond the Numbers Game* (Hamilton et al., 1977), a reader in alternative methods of educational evaluation. The elements in a theory of research methodology, as items or patterns, can be viewed as group norms or mores of those evolving communities. Over time, the larger research communities and subcommunities evolve and change, as in the development of Division G, Social Context, in AERA and the Council on Anthropology and Education in AAA. In time, the theory of research methodology, as with any theory, evolves and changes, perhaps on occasion dramatically enough to be labeled a revolution, as Kuhn calls the paradigm shifts.

Second, a reflexive overview of the cognitive processes in field work suggests a perspective on the methodology. It is one person's idiosyncratic "how to do it." It, like the methodology, builds on one participant's actions and observation of the process, reading about other persons' observations, and then describing, analyzing, and interpreting that experience. It is its own kind of grounded theory of methodology. Experientally, the phases and the discriminable items within the phases are very real. They have a public quality in being sharable, communicable, and meaningful to other field workers. They seem to help our students in learning to do similar work. They seem to lead to knowledges and understandings that are useful to a variety of persons engaged in educational activities.

Third, and more specifically within the educational ethnographic, participant observation tradition, the essay presents a patterned analysis of the genre of research. The major domains considered were at four levels of abstraction, data, descriptive narrative, theoretical, and metatheoretical. The framework presents a way of talking about any piece of research in the tradition. Such a perspective is one step toward a guide to evaluation. Audiences can discern relative kinds and amounts of attention to data, to narrative, to theory in a piece of work. If they value portrayals more than conceptual models, for instance, then one piece will be judged better than another; conversely, if they value model development and they find only a portrayal, the evaluation will be different. Each of the levels of analysis contains several discriminable subissues. All of these are linked in

multiple and sometimes contradictory ways to other research traditions in educa-
tion, social science, and philosophy. Overall the framework suggests the kinds of
things the practicing inquirer might consider as he is developing his own line of
research. Particular positions of the author were presented in considerable detail
as one configuration of possibilities. For now that seems enough.

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