

3.06

Qualitative and Discourse Analysis

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3.06.1 INTRODUCTION: QUALITATIVE RESEARCH IN CONTEXT

For many researchers the words “qualitative method” spell out psychology’s most notorious oxymoron. If there is one thing that qualitative methods are commonly thought to lack it is precisely an adequate methodic way of arriving at findings. Indeed, for much of the twentieth century quantification has been taken as the principle marker of the boundary between a mature scientific psychology and common-sense, intuitive approaches. Since the late 1980s, however, there has been a remarkable increase in interest in qualitative research in psychology. This partly reflects a broader turn to qualitative research across the social sciences, although qualitative research of one kind or another has long been an established feature of disciplines such as education, sociology, and, most prominently, anthropology.

In psychology there is a handbook of qualitative research (Richardson, 1996) as well as a range of volumes and special journal issues whose major focus is on developing qualitative approaches to psychological problems (Antaki, 1988; Bannister, Burman, Parker, Taylor, & Tyndall, 1994; Henwood & Parker, 1995; Henwood & Nicolson, 1995; Smith, Harré, & van Langenhove, 1995). Psychology methods books and collections are increasingly serving up qualitative methods to accompany the more usual diet of experiments, questionnaires, and surveys. At the same time, an increasing permeability of boundaries between the social sciences has provided the environment for a range of trans-disciplinary qualitative methods books including a useful doorstop-sized handbook (Denzin & Lincoln, 1994) and varied edited and authored works (Bogdan & Taylor, 1975; Bryman & Burgess, 1994; Coffey & Atkinson, 1996; Gilbert, 1993; Lofland & Lofland, 1984; Miles & Huberman, 1994; Miller & Dingwall, 1997; Silverman, 1993, 1997a). These general qualitative works are complemented by a mushrooming range of books and articles devoted to specific methods and approaches.

Why has there been this increase in interest in qualitative research? Three speculations are preferred. First, there is a widespread sense that traditional psychological methods have not proved successful in providing major advances in the understanding of human life. Despite regular promissory notes, psychology seems to offer no earth-moving equivalent of the transistor, of general relativity, or of molecular

genetics. Second, as is discussed below, views of science have changed radically since the 1950s, making it much harder to paint qualitative researchers, as either antiscientific extremists or merely sloppy humanists. Third, psychology is no longer as insulated from other social sciences as it has been in the past. Of course, for much of its twentieth century existence psychology has been invigorated by infusions from other sciences such as physiology and linguistics. However, in recent years there has been increasing exchange with disciplines where qualitative methods have been more established such as sociology and anthropology. This is reflected in contemporary theoretical developments such as constructionism (Gergen, 1994) and poststructuralism (Henriques, Hollway, Irwin, Venn, & Walkerdine, 1984) that have swept right across the human sciences.

This is, then, an exciting time for qualitative researchers, with new work and new opportunities of all kinds. Yet it should also be emphasised that qualitative research in psychology is in a chaotic state, with a muddle of inconsistent questions and approaches being blended together. Much poor work has involved taking questions formulated in the metaphysics of experimental psychology and attempting to plug them into one or more qualitative methods. At its worst such research peddles unsystematic and untheorized speculations about the influences on some piece of behavior which are backed up with two or three quotes from an interview transcript. This expanding literature and variable quality creates some problems for the production of a useful overview. This chapter selects what are seen as the most coherent and successful qualitative approaches from a range of possibilities, as well as focusing on those approaches which are being used, and have prospects for success, in clinical settings. A range of references is provided for those who wish to follow up alternative methods.

3.06.1.1 Historical Moments in Qualitative Research in Clinical Settings

In one sense a large proportion of twentieth century clinical therapeutic practice was based on qualitative methods. The process of conducting some kind of therapy or counseling with clients and then writing them up as case histories, or using them as the basis for inferences about aspects of the psyche, behavior, or cognitive processes, has been a commonplace of clinical

work. Freud's use of case histories in the development of psychoanalytic thinking is probably the most influential. Although it is an approach to clinical knowledge that overwhelmingly eschews quantification, it is hard to say much about its methodic basis. For good or bad, it is dependent on the unformulated skills and intuitions of the therapist/researcher. In the hands of someone as brilliant as Freud the result can be extraordinary; elsewhere the product has often been merely ordinary. The problem for the readers of such research is that they can do little except either take it on trust or disagree. The process through which certain claims are established is not open to scrutiny. However, Freud's study of the case of Little Hans is exceptional in this regard, and so it is briefly worth considering (see Billig, 1998).

Although Freud initially based his arguments for the existence of the Oedipus complex on the interpretation of what patients told him in the course of therapy sessions, he attempted, unusually, to support this part of psychoanalytic theory with more direct evidence. He asked some of his followers to collect observations from their own children. The music critic Max Graf was most helpful in this regard and presented Freud with copious notes on conversations between his son, Hans, and other family members, as well as descriptions of dreams he had recounted. The published case history (Freud, 1977 [1909]) contains more than 70 pages of reports about Hans which Freud describes as reproduced "just as I received them" without "conventional emendations" (1977, pp. 170). Here is an example:

Another time he [Hans] was looking on intently while his mother undressed before going to bed. "What are you staring like that for?" she asked.

Hans: "I was only looking to see if you'd got a widdler too."

Mother: "Of course. Didn't you know that?"

Hans: "No. I thought you were so big you'd have a widdler like a horse."

(1977, p. 173)

Freud's fascinating materials and striking interpretations beg many of the questions that have been central to qualitative research ever since. For example, what is the role of Max Graf's expectations (he was already an advocate of Freud's theories) in his selection and rendering of conversations with Hans? How closely do the extracts capture the actual interactions (including the emphasis, nonvocal elements, and so on)? What procedure did Freud use to select the examples that were reproduced from the full corpus? And, most importantly, what is the basis of Freud's interpretations? His inter-

pretations are strongly derived from his theory, as is shown by his willingness to straightforwardly rework the overt sense of the records. Take this example:

Hans (aged four and a half) was again watching his little sister being given her bath, when he began laughing. On being asked why he was laughing, he replied. "I'm laughing at Hanna's widdler." "Why?" "Because her widdler's so lovely."

Of course his answer was a disingenuous one. In reality her widdler had seemed to him funny. Moreover, this is the first time he has recognized in this way the distinction between male and female genitals instead of denying it. (1977, p. 184)

Note the way Graf here, and implicitly Freud in his text, treat the laughter as the real indicator of Hans understanding of events, and his overt claim to find his sister's "widdler" lovely as a form of dissembling. Hans is not delighted by the appearance of his sister's genitals but is amused, in line with psychoanalytic theory, by their difference from his own. Again, the issue of how to treat the sense of records of interaction, and what interpretations should be made from them to things going on elsewhere such as actions or cognitions, is a fundamental one in qualitative research (Silverman, 1993).

Some 40 years later another of clinical psychology's great figures, Carl Rogers, advocated the use of newly developed recording technology to study the use of language in psychotherapy itself, with the aim of understanding and improving therapeutic skills. For him such recordings offered "the first opportunity for an adequate study of counseling and therapeutic procedures, based on thoroughly objective data" (Rogers, 1942, p. 433). Rogers envisaged using such recordings in the development of a scale to differentiate the styles of different counselors and studies of the patterns of interaction; for example, "what type of counselor statement is most likely to be followed by statements of the client's feeling about himself?" (1942, p. 434).

Rogers' emphasis on the virtue of recordings was followed up in two major "microscopic" studies of psychotherapeutic discourse. The first by Pettinger, Hockett, and Danehy (1960) focused on the initial portion of an initial interview. A typical page of their study has just a few words of transcript coupled with an extended discussion of their sense. Much of the focus was on the prosodic cues—the intonation and stress—provided in the interview and their contextual significance. Prosody is, of course, a feature of interaction which is almost impossible to reliably capture in *post hoc* notes made by key informants and so highlights

the virtue of the new technology. A second study by Labov and Fanshell (1977) also focused on the opening stages of a therapy session, in this case five episodes of interaction from the first 15 minutes of a psychoanalytic therapy session with Rhoda, a 19-year-old girl with a history of anorexia nervosa.

The classic example of ethnographic work in the history of clinical psychology is Goffman's study of the everyday life of a mental hospital published under the title *Asylums* (1961). It is worth noting that although Goffman was a sociologist, the various essays that make up *Asylums* were initially published in psychiatry journals. Rather than utilize tape recording technology to capture the minutiae of some social setting, Goffman used an ethnographic approach. He spent a year working ostensibly as an assistant to the athletic director of a large mental hospital, interacting with patients and attempting to build up an account of the institution as viewed by the patients. His justification for working in this way is instructive for how the strengths and weaknesses of qualitative work have been conceptualized:

Desiring to obtain ethnographic detail regarding selected aspects of patient social life, I did not employ usual kinds of measurements and controls. I assumed that the role and time required to gather statistical evidence for a few statements would preclude my gathering data on the tissue and fabric of patient life. (1961, p. 8)

As an ethnographic observer, he developed an understanding of the local culture and customs of the hospital by taking part himself. He used the competence generated in this way as the basis for his writing about the life and social organization in a large mental hospital.

3.06.1.2 Background Issues

Before embarking on a detailed overview of some contemporary qualitative approaches to clinical topics there are some background issues that are worth commenting on, as they will help make sense of the aims and development of qualitative approaches. In some cases it is necessary to address issues that have been a long-standing source of confusion where psychologists have discussed the use of qualitative methods.

3.06.1.2.1 Philosophy, sociology, and changing conceptions of science

As noted above, the development of qualitative work in psychology has been facilitated by the more sophisticated understanding of the

nature of science provided by philosophers and sociologists since the 1970s. The image of the lone scientist harvesting facts, whose truth is warranted through the cast-iron criterion of replication, has intermittently been wheeled on to defend supposedly scientific psychology against a range of apparently sloppier alternatives. However, this image now looks less than substantial (see Chalmers, 1992; Potter, 1996a; Woolgar, 1988).

The bottom-line status of scientific observation has been undermined by a combination of philosophical analyses and sociological case studies. Philosophers have highlighted the logical relationships between observation statements and theoretical notions (Hesse, 1974; Kuhn, 1962; Popper, 1959). Their argument is that even the simplest of scientific descriptions is dependent on a whole variety of theoretical assumptions. Sociologists have supplemented these insights with studies of the way notions of observations are used in different scientific fields. For example, Lynch (1994) notes the way the term observation is used in astronomy as a loose device for collecting together a range of actions such as setting up the telescope, attaching sensors to it, building up traces on an oscilloscope, converting these into a chart and canvassing the support of colleagues. Knorr Cetina (1997) documents the different notions of observation that appear in different scientific specialities, suggesting that high energy physicists and molecular biologists, for example, work with such strikingly different notions of what is empirical that they are best conceived of as members of entirely different epistemic cultures.

The idea that experimental replication can work as a hard criterion for the adequacy of any particular set of research findings has been shown to be too simple by a range of sociological studies of replication in different fields (Collins, 1981). For example, Collins (1985) has shown that the achievement of a successful replication is closely tied to the conception of what counts as a competent experiment in the first place—and this itself was often as much a focus of controversy as the phenomenon itself. In a study of gravity wave researchers, Collins found that those scientists who believed in gravity waves tended to treat replications that claimed to find them as competent and replications that failed to find them as incompetent. The reverse pattern was true of nonbelievers. What this meant was that replication did not stand outside the controversy as a neutral arbiter of the outcome, but was as much part of the controversy as everything else.

Philosophical and sociological analysis has also shown that the idea that a crucial

experiment can be performed which will force the abandonment of one theory and demonstrate the correctness of another is largely mythical (Lakatos, 1970; Collins & Pinch, 1993). Indeed, historical studies suggest that so-called crucial experiments are not merely insufficient to effect the shift from one theory to another, they are often performed, or at least constructed as crucial, after the shift to provide illustration and legitimation (Kuhn, 1977).

Let us be clear at this point. This research does not show that careful observation, skilful replication, and theoretically sophisticated experiments are not important in science. Rather, the point is that none of these things are bottom-line guarantees of scientific progress. Moreover, these sociological studies have suggested that all these features of science are embedded in, and inextricable from, its communal practices. Their sense is developed and negotiated in particular contexts in accordance with *ad hoc* criteria and a wide range of craft skills which are extremely hard to formulate in an explicit manner (Knorr Cetina, 1995; Latour & Woolgar, 1986). The message taken from this now very large body of work (see Jasanoff, Markle, Pinch, & Petersen, 1995) is not that psychologists must adopt qualitative methods, or that qualitative methods will necessarily be any better than the quantitative methods that they may replace or supplement; it is that those psychologists who have argued against the adoption of such methods on the principle that they are unscientific are uninformed about the nature of science.

3.06.1.2.2 *Investigatory procedures vs. justificatory rhetoric*

There are a number of basic linguistic and metatheoretical difficulties in writing about qualitative methods for psychologists. Our terminology for methodological discussion—reliability, validity, sampling, factors, variance, hypothesis testing, and so on—has grown up with the development of quantitative research using experiments and surveys. The language has become so taken-for-granted that it is difficult to avoid treating it as obvious and natural. However, it is a language that is hard to disentangle from a range of metatheoretical assumptions about the nature of behavior and processes of interaction. Traditional psychology has become closely wedded to a picture of factors and outcomes which, in turn, cohabits with the multivariate statistics which are omnipresent where data is analyzed. For some forms of qualitative research, particularly most discourse and ethnographic work, such a picture is inappropriate. This does not mean

that such research is incoherent or unscientific, merely that it should not be construed and evaluated using the family of concepts whose home is experimental journal articles. Likewise the psychological model of hypothesis testing is just one available across the natural and human sciences. Qualitative research that utilizes theoretically guided induction, or tries to give a systematic description of some social realm, should not be criticized on the grounds that it is unscientific, let alone illegitimate. Ultimately, the only consistent bottom line for the production of excellent qualitative work is excellent scholarship (Billig, 1988).

Another difference between traditional quantitative and qualitative work is that in the traditional work the justification of research findings is often taken to be equivalent to the complete and correct carrying out of a set of codified procedures. Indeed, methods books are often written as if they were compendia of recipes for achieving adequate knowledge. Sampling, operationalization of variables, statistical tests, and interpretation of significance levels are discussed with the aid of tree diagrams and flow charts intended to lead the apprentice researcher to the correct conclusion. In one sense, much qualitative work is very different to this, with the procedures for justifying the research claims being very different to the procedures for producing the work. Thus, the manner in which a researcher arrives at some claims about the various functions of “mm hm’s” in psychiatric intake interviews, say, may be rather different from the manner in which they justify the adequacy of the analysis. Yet, in another sense the difference between qualitative and quantitative research is more apparent than real, for studies of the actual conduct of scientists following procedural rules of method show that such rules require a large amount of tacit knowledge to make them understandable and workable, and that they are often more of a rhetorical device used to persuade other scientists than an actual constraint on practice (Gilbert & Mulkay, 1984; Polyani, 1958). As Collins (1974) showed in an innovative ethnographic study, when a group of scientists wrote a paper offering the precise technical specification of how to make a new laser, the only people who were able to build a working laser of their own had actually seen one built; just reading the paper was not enough.

This presents something of a dilemma for anyone writing about qualitative methods. Should they write to help people conduct their research so as better to understand the world, or should they work to provide the sorts of formalized procedural rules that can be drawn on in the methods sections of articles to help

persuade the psychologist reader? In practice, most writing does some of each. However, the difficulty that psychologists often report when attempting qualitative work is probably symptomatic of the failure to fully explicate the craft skills that underpin qualitative work.

3.06.1.2.3 Quality and quantity

There are different views on how absolute the quantity/quality divide is. Arguments at different levels of sophistication have been made for future integration of qualitative and quantitative research (Bryman, 1988; Silverman, 1993). It is undoubtedly the case that at times proponents of both quantitative and qualitative research have constructed black and white stereotypes with little attempt at dialog (although a rare debate about the relative virtues of quantitative and qualitative research on the specific topic of attitudes towards mental hospitalisation is revealing—Weinstein, 1979, 1980; Essex et al., 1980). It is suggested that quantification is perfectly appropriate in a range of situations, dependent on appropriate analytic and theoretical judgements.

In many other research situations the goal is not something that can be achieved through counting. For example, if the researcher is explicating the nature of “circular questioning” in Milan School Family Therapy, that goal is a prerequisite for a study which considers the statistical prevalence of such questioning. Moreover, there are arguments for being cautious about quantification when studying the sorts of discursive and interactional materials which have often been central to qualitative research because of distortions and information loss that can result (see Schegloff, 1993, and papers in Wieder, 1993). Some of the grounds for caution come from a range of qualitative studies of quantification in various clinical settings (Ashmore, Mulkay, & Pinch, 1989; Atkinson, 1978; Garfinkel, 1967a; Potter, Wetherell, & Chitty, 1991).

3.06.1.3 Qualitative Research and Theory

It is hard to overestimate how close the relationship is between the theories, methods, and questions used by psychologists. Theories specify different positions on cognition and behavior, different notions of science, different views of the role of action and discourse, different understandings of the role of social settings, and, most fundamentally, different objects for observation.

For both psychoanalytic theory and most of the mass of theories and perspectives that make

up modern cognitivism the objects of observation are hypothetical mental entities (the Oedipus complex, attributional heuristics). Psychoanalytic researchers have generally preferred to engage in an interpretative exercise of reconstructing those entities from the talk of patients undergoing therapy. Cognitive psychologists have typically used some hypothetico-deductive procedure where predictions are checked in experiments which investigate, say, the attributional style of people classified as depressed. Note that in both of these cases they are using people’s discourse—talk in the therapy session, written responses to a questionnaire—yet in neither case is the discourse as such of interest. In contrast, for researchers working with different perspectives such as social constructionism or discursive psychology, the talk or writing itself, and the practices of which it is part, is the central topic. For these researchers there is a need to use procedures which can make those practices available for study and allow their organization to be inspected and compared.

To take another example, behaviorist psychologists have done countless studies on the effects of particular regimes of reward and punishment on target behaviors such as compulsive hand washing. However, such studies are typically focused on outcomes and statistical associations, whereas a theoretical perspective such as symbolic interactionism or, to give a more psychological example, Vygotskian activity theory, encourage a more ethnographic examination of the settings in which rewards are administered and of the sense that those behaviors have in their local context

Without trying to flesh out these examples in any detail, the important practical point they make is that it is a mistake to attempt simply to import a question which has been formulated in the problematics of one theoretical system, and attempt to answer it using a method developed for the problematics of another. The failure to properly conceptualize a research question that fits with the research method is a major source of confusion when psychologists start to use qualitative methods.

3.06.1.4 Boundaries of Qualitative Research and Coverage of the Current Chapter

What distinguishes qualitative research from quantitative? And what qualitative approaches are there? These questions are not as straightforward as they seem. In the past the qualitative/quantitative distinction has sometimes been treated as the equivalent of the distinction between research that produces objective and

subjective knowledge—a distinction which makes little sense in the light of recent sociology and philosophy of science. Sometimes certain approaches using numbers have been treated as qualitative. For example, content analysis has occasionally been treated as a qualitative method because it is used to deal with “naturally occurring” objects such as diaries, novels, transcripts of meetings, and so on. Content analysis was meant to eliminate many of the potential “reactive” effects that bedevil social research and thereby avoid the problem in experimental and survey research, of how findings relate to what goes on in the real world; for these are records of (indeed, examples of) what is actually going on. However, in this chapter content analysis is treated as quantitative, and therefore outside the scope of this survey, on the grounds that it (i) transforms phenomena into numerical counts of one kind or another and (ii) typically attempts to statistically relate these counts to some broader factors or variables. For useful introductions to content analysis related to psychological topics see Holsti (1969) and Krippendorff (1980).

For similar reasons repertory grid analysis associated with personal construct theory, and the “Q” methodology developed by William Stephenson, have sometimes been treated as qualitative. The rationale for this was probably that they were often focused on understanding the reasoning or cognitive organization of single individuals rather than working exclusively from population statistics. However, as they involve quantitative manipulation of elicited responses from participants they will not be dealt with here. The ideographic/nomathetic distinction will be treated as orthogonal to the qualitative/quantitative one! For accessible introductions to these approaches, see Smith (1995) on repertory grid methods and Stainton Rogers (1995) on Q methodology.

In addition to these methods which are excluded as not properly qualitative, a wide range of methods have not been discussed which nevertheless satisfy the criterion of being at least minimally methodic and generally eschewing quantification. For simplicity, Table 1 lists nine methods or approaches which have been excluded, along with one or two references that would provide a good start point for any researcher who was interested in learning more about them. It would take some time to make explicit the reasons for excluding all of them. Generally, the problem is that they have not been, and are unlikely to be in the future, particularly useful for studying problems in the area of clinical psychology (e.g., focus groups—although, see Piercy & Nickerson, 1996). In some cases, the approaches are not coherent

enough to warrant discussion. In others, their central problematics are better addressed by the approaches that are discussed.

The most controversial exclusion is probably humanistic methods given that humanistic psychology developed in settings which had a broad emphasis on therapy and psychological well-being. It is suggested that the romanticism of much humanistic psychology is attractive, but ultimately unconvincing. However, it is often, quite legitimately, more concerned with developing participants’ skills and sensitivity than developing propositional claims and arguments; as such it is often offering a set of techniques for expanding human potential rather than developing methods for research. Feminist methods are excluded, despite an appreciation of the importance of feminist issues in clinical settings, because the arguments for the existence of specifically feminist methods (as opposed to theories or arguments) are not convincing. This is particularly true where such claims give a major epistemological role for experience or intuition (Ellis, Kiesinger, & Tillmann-Healy, 1997). These are topics for decomposition and analysis rather than bottom-lines for knowledge. For some arguments in both directions on this topic see Gelsthorpe (1992), Hammersley (1992), Ramazanoglu (1992), and Wilkinson (1986).

Finally, it should be stressed that qualitative work is not seen as having some overall coherence. Quite the contrary, it is fragmented and of highly variable quality. Nor is some overall coherence seen as a desirable goal. Those workers who talk of a qualitative paradigm (Guba & Lincoln, 1994, Reason & Rowan, 1981) unhelpfully blur over a range of theoretical and metatheoretical differences (see Henwood & Pidgeon, 1994).

3.06.2 GROUNDED THEORY

Grounded theory has the most clear-cut origin of any of the approaches discussed here. The term was coined by two sociologists in an influential book: *The discovery of grounded theory* (Glaser & Strauss, 1967). Some of its key features and concerns were a product of its birthplace within sociology, where it was developed to counter what the authors saw as a preoccupation, on the one hand, with abstract grand theories and, on the other, with testing those theories through large scale quantitative studies. Grounded theory was intended to link theoretical developments (conceived as plausible relations among concepts and sets of concepts—Strauss & Corbin, 1994) more closely to the particulars of settings, to ground

Table 1 Varieties of qualitative research not covered.

<i>Qualitative research method</i>	<i>Source</i>
Action research	Argyris, Putnam, & Smith, 1985, Whyte, 1991
Documentary studies	Hodder, 1994, Scott, 1990
Ethogenics	Harré, 1992
Feminist approaches	Olesen, 1994, Reinharz, 1992
Focus groups	Krueger, 1988, Morgan, 1997
Humanistic, participative research	Reason & Rowan, 1981, Reason & Heron, 1995
Life histories	Plummer, 1995, Smith, 1994
Role play	Yardley, 1995
Semiotics	Manning & Cullum-Swan, 1994

middle range theories in actual qualitative data rather than to start from preconceived hypotheses. It is important to stress that grounded theory is not a theory as such, rather it is an approach to theorising about data in any domain. Moreover, since its inception in the 1960s, work on the theory ladenness of data has made the idea of “grounding” theory increasingly problematic.

Much grounded theory research has been done outside of psychology; however, psychologists have become increasingly interested in the approach in general (Charmaz, 1995; Henwood and Pidgeon, 1995; Pidgeon, 1996; Pidgeon and Henwood, 1996; Rennie, Phillips & Quartaro, 1988), and have carried out specific studies in health (Charmaz, 1991, 1994) and clinical (Clegg, Standen, & Jones, 1996) settings.

3.06.2.1 Questions

Grounded theory is designed to be usable with a very wide range of research questions and in the context of a variety of metatheoretical approaches. Rather like the statistical analyses that psychologists are more familiar with, it deals with patterns and relationships. However, these are not relationships between numbers but between ideas or categories of things, and the relationships can take a range of different forms. In some respects the procedures in grounded theory are like the operation of a sophisticated filing system where entries are cross-referenced and categorized in a range of different ways. Indeed, this is one qualitative approach that can be effectively helped by the use of computer packages such as NUDIST, which was itself developed to address grounded theory notions.

Grounded theory has proved particularly appropriate for studying people’s understandings of the world and how these are related to their social context. For example, Turner (1994; Turner & Pidgeon, 1997) has used grounded theory to attempt to explain the origins of

manmade disasters like fires and industrial accidents; Charmaz (1991) has studied the various facets that make up people’s experience of chronic illness; Clegg, Standen, and Jones (1996) focused on the staff members’ understanding of their relationship with adults with profound learning disabilities. In each case, a major concern was to incorporate the perspectives of the actors as they construct their particular social worlds. Grounded theory methods can help explicate the relation of actions to settings (how does the behavior of key personnel in the evolution of a major fire follow from their individual understanding of events and physical positioning?); it can be used for developing typologies of relevant phenomena (in what different ways do sufferers of chronic illness conceptualize their problem?); and it can help identify patterns in complex systems (how does the information flowing between social actors help explain the development of a laboratory smallpox outbreak?).

Like most of the qualitative approaches discussed here, grounded theory is not well suited to the kinds of hypothesis testing and outcome evaluation that have traditionally been grist to the mill of clinical psychology, because of its open-ended and inductive nature. Although the researcher is likely to come to a topic with a range of more or less explicit ideas, questions, and theories, it is not necessary for any or all of these to be formally stated before research gets under way. The approach can start with a specific problem or it may be more directed at making sense of an experience or setting.

Grounded theory can be applied to a range of different textual materials such as documents, interview transcripts and records of interaction, and this makes it particularly suitable for certain kinds of questions. It can deal with records which exist prior to the research and it can deal with materials specifically collected. The processes of coding allow quite large amounts of material to be dealt with. For example, while Turner studied

a single (lengthy) official report of a major fire in a holiday complex, Charmaz studied 180 interviews with 90 different people with chronic illnesses. The requirement is only that the material can be coded.

3.06.2.2 Procedures

The procedures for conducting grounded theory work are straightforward to describe, if less so to follow in practice. Pidgeon and Henwood (1996, p. 88) provide a useful diagram to explicate the process (Figure 1).

3.06.2.2.1 Materials

In line with the general emphasis on participants' perspectives and on understanding patterns of relationships, researchers often attempt to obtain rich materials such as documents and conversational, semistructured interviews. These may be supplemented by participant observation in the research domain, generating fieldnotes which can be added to other data sets or simply used to improve the researcher's understanding so they can better deal with the other materials.

After data is collected and stored the intensive process that is most characteristic of grounded theory is performed. This involves coding the data, refining the coding and identifying links between categories, and writing "memos" which start to capture theoretical concepts and relationships.

3.06.2.2.2 Coding

Different grounded theory researchers approach coding in different ways. For example, it can involve generating index cards or making annotations next to the relevant text. The researcher works through the text line by line, or paragraph by paragraph, labeling the key concepts that appear. Charmaz (1995, p. 38)

suggests a series of specific questions that are useful for picking out the key concepts:

- (i) What is going on?
- (ii) What are the people doing?
- (iii) What is the person saying?
- (iv) What do these actions and statements take for granted?
- (v) How do structure and context serve to support, maintain, impede, or change these actions and statements?

More broadly, Pidgeon and Henwood suggest that this phase of coding is answering the question: "what categories or labels do I need in order to account for what is of importance to me in this paragraph?" (1996, p. 92).

Such coding is intensive and time consuming. For example, Table 2 shows an example by Charmaz of line-by-line coding of just a brief fragment of one of her 180 interviews. Note the way that the interview fragment is coded under a number of different topics. There is no requirement in grounded theory that categories apply exclusively.

3.06.2.2.3 Method of constant comparison

Coding is not merely a matter of carefully reading and labeling the materials. As the coding continues the researcher will be starting to identify categories that are interesting or relevant to the research questions. They will refine their indexing system by focused coding which will pick out all the instances from the data coded as, for example "avoiding disclosure." When such a collection has been produced the researcher can focus on the differences in the use of this category according to the setting or the actors involved. This is what grounded theorists refer to as the "method of constant comparison." In the course of such comparisons the category system may be reworked; some categories will be merged together and others will be broken up, as the close reading of the data allows an increasingly refined understanding.

Table 2 Line-by-line coding.

<i>Coding</i>	<i>Interview</i>
Shifting symptoms, having inconsistent days	If you have lupus, I mean one day it's my liver; one day it's my joints; one day it's my head, and it's like people really think you're a hypochondriac if you keep complaining about different ailments. . . . It's like you don't want to say anything because people are going to start thinking, you know, "God, don't go near her, all she is—is complaining about this."
Interpreting images of self given by others	
Avoiding disclosure	
Predicting rejection	
Keeping others unaware	

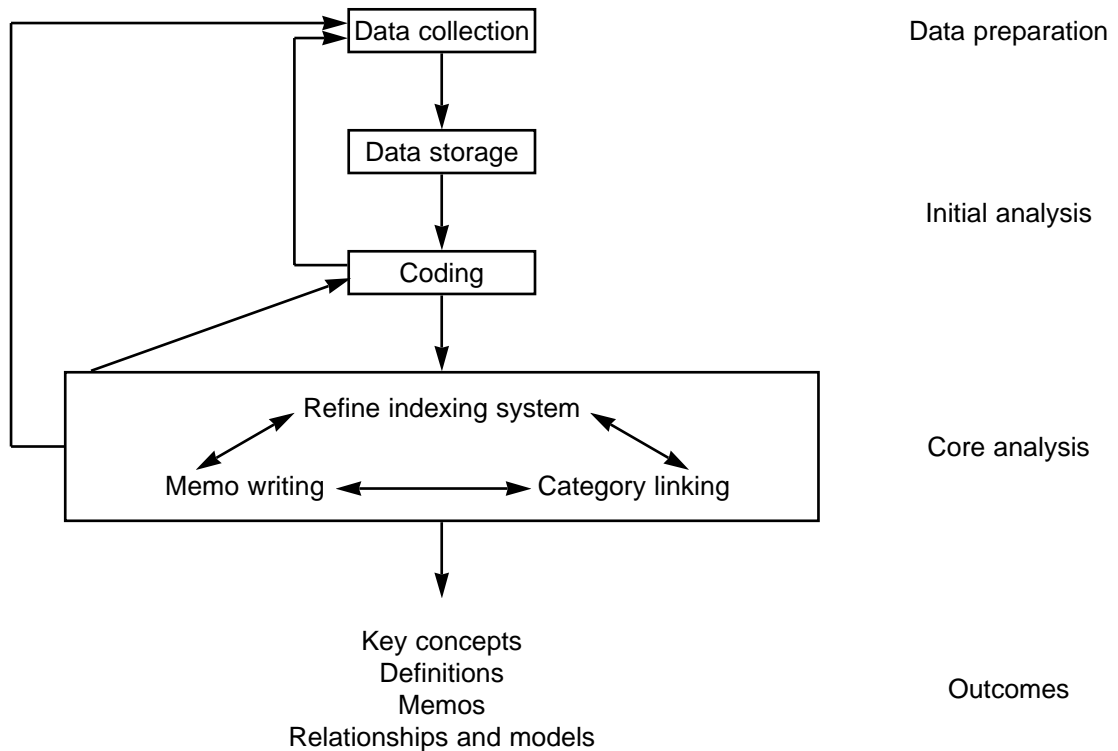


Figure 1 Procedures for conducting grounded theory research.

3.06.2.2.4 Memo writing

Throughout this stage of coding and comparison grounded theorists recommend what they call memo writing. That is, writing explicit notes on the assumptions that underly any particular coding. Memo writing is central to the process of building theoretical understandings from the categories, as it provides a bridge between the categorization of data and the writing up of the research. In addition to the process of refining categories, a further analytic task involves linking categories together. The goal here is to start to model relationships between categories. Indeed, one possibility may be the production of a diagram or flow chart which explicitly maps relationships.

As Figure 1 makes clear, these various elements in grounded theory analysis are not discrete stages. In the main phase of the analysis, refining the indexing system is intimately bound up with the linking of categories, and memo writing is likely to be an adjunct to both. Moreover, this analysis may lead the researcher back to the basic line-by-line coding, or even suggest the need to collect further material for analysis.

3.06.2.2.5 Validity

There is a sense in which the general methodological procedures of grounded theory are

together designed to provide a level of validation as they force a thoroughgoing engagement with the research materials. Line-by-line coding, constant comparison, and memo writing are all intended to ensure that the theoretical claims made by the analyst are fully grounded in the data. That, after all, was the original key idea of grounded theory. However, some specific procedures of validation have been proposed.

Some grounded theorists have suggested that respondent validation could be used as a criterion. This involves the researcher taking back interpretations to the participants to see if they are accepted. The problem with such an approach is that participants may agree or disagree for a range of social reasons or they may not understand what is being asked of them. Other approaches to validation suggest that research should be generative, that is, facilitate further issues and questions; have rhetorical power, that is, prove effective in persuading others of its effectiveness; or that there could be an audit trail which would allow another researcher to check how the conclusions were reached (Henwood & Pidgeon, 1995; Lincoln & Guba, 1985).

3.06.2.3 Example: Clegg, Standen, and Jones (1996)

There is no example in clinical psychology of the sorts of full-scale grounded theory study

that Charmaz (1991) conducted on the experience of illness or Glaser and Strauss (1968) carried out on hospital death. Nevertheless, a modest study by Clegg et al. (1996) of the relationships between staff members and adults with profound learning disabilities illustrates some of the potential of grounded theory for clinical work.

In line with the grounded theory emphasis on the value of comparison, 20 staff members from four different residential settings were recruited. Each member of staff was videotaped during eight sessions of interaction with a single client that they had known for over three months. Each staff member was subsequently interviewed about their work, their relationship with the client, a specific experience with the client, and their understanding of the client's view of the world. These conversational interviews were tape recorded and transcribed, and form the data for the major part of the study.

The study followed standard grounded theory procedures in performing a range of codings developing links between categories and building up theoretical notions from those codings. The range of different outcomes of the study is typical of grounded theory. In the first place, they identify four kinds of relationships that staff may have with clients: provider (where meeting of the client's needs is primary); meaning-maker (where making sense of the client's moods or gestures is primary); mutual (where shared experience and joy at the client's development is primary); companion (where merely being together is treated as satisfying). The authors go on to explore the way different settings and different client groups were characterized by different relationships. Finally, they propose that the analysis supports four propositions about staff-client relationships: some types of relationship are better than others (although this will vary with setting and client); staff see the balance of control in the relationship as central; families can facilitate relationships; professional networks create dilemmas.

3.06.2.4 Virtues and Limitations

Grounded theory has a range of virtues. It is flexible with respect to forms of data and can be applied to a wide range of questions in varied domains. Its procedures, when followed fully, force the researcher into a thorough engagement with the materials; it encourages a slow-motion reading of texts and transcripts that should avoid the common qualitative research trap of trawling a set of transcripts for quotes to illustrate preconceived ideas. It makes explicit some of the data management procedures that

are commonly left inexplicit in other qualitative approaches. The method is at its best where there is an issue that is tractable from a relatively common sense actor's perspective. Whether studying disasters, illness, or staff relationships, the theoretical notions developed are close to the everyday notions of the participants. This makes the work particularly suitable for policy implementation, for the categories and understandings of the theory are easily accessible to practitioners and policy makers.

Some problems and questions remain, however. First, although there is a repeated emphasis on theory—after all, it is in the very name of the method—the notion of theory is a rather limited one strongly influenced by the empiricist philosophy of science of the 1950s. The approach works well if theory is conceived in a limited manner as a pattern of relationships between categories, but less well if theories are conceived of as, say, models of underlying generative mechanisms (Harré, 1986).

Second, one of the claimed benefits of grounded theory work is that it works with the perspective of participants through its emphasis on accounts and reports. However, one of the risks of processes such as line-by-line coding is that it leads to a continual pressure to assign pieces of talk or elements of texts to discrete categories rather than seeing them as inextricably bound up with broader sequences of talk or broader textual narratives. Ironically this can mean that instead of staying with the understandings of the participants their words are assigned to categories provided by the analyst.

Third, grounded theorists have paid little attention to the sorts of problems in using textual data that ethnomethodologists and discourse analysts have emphasised (Atkinson, 1978; Gilbert & Mulkay, 1984; Silverman, 1993; Widdicombe & Wooffitt, 1995). For example, how far is the grounding derived not from theorizing but from reproducing common sense theories as if they were analytic conclusions? How far are Clegg's et al. (1996) staff participants, say, giving an accurate picture of their relationships with clients, and how far are they drawing on a range of ideas and notions to deal with problems and work up identities in the interview itself?

Some practitioners are grappling with these problems in a sophisticated manner (Pidgeon & Henwood, 1996). As yet there is not a large enough body of work with clinical materials to allow a full evaluation of the potential of this method. For more detailed coverage of grounded theory the reader should start with the excellent introductions by Pidgeon (1996) and Pidgeon and Henwood (1996); Charmaz (1991) provides a full scale research illustration of the

potential of grounded theory; Rafuls and Moon (1996) discuss grounded theory in the context of family therapy; and, despite its age, Glaser and Strauss (1967) is still an informative basis for understanding the approach.

3.06.3 ETHNOGRAPHY AND PARTICIPANT OBSERVATION

Ethnography is not a specific method so much as a general approach which can involve a number of specific research techniques such as interviewing and participant observation. Indeed, this has been a source of some confusion as rather different work is described as ethnography in anthropology, sociology, and other disciplines such as education or management science. The central thrust of ethnographic research is to study people's activities in their natural settings. The concern is to get inside the understanding and culture, to develop a subtle grasp of how members view the world, and why they act as they do. Typically, there is an emphasis on the researcher being involved in the everyday world of those who are being studied. Along with this goes a commitment to working with unstructured data (i.e., data which has not been coded at the point of data collection) and a tendency to perform intensive studies of small numbers of cases. Ethnography is not suited to the sorts of hypothetico-deductive procedures that are common in psychology.

Two important tendencies in current ethnographic work can be seen in their historical antecedents in anthropology and sociology. In the nineteenth century, anthropology often involved information collected by colonial administrators about members of indigenous peoples. Its use of key informants and its focus on revealing the details of exotic or hidden cultures continue in much work. Sociologists of the "Chicago School" saw ethnography as a way of revealing the lives and conditions of the US underclass. This social reformism was married to an emphasis on understanding their participants' lives from their own perspective.

3.06.3.1 Questions

Ethnography comes into its own where the researcher is trying to understand some particular sub-cultural group in a specific setting. It tends to be holistic, focusing on the entire experience participants have of a setting, or their entire cosmology, rather than focusing on discrete variables or phenomena. This means that ethnographic questions tend to be general: What happens in this setting? How do this group understand their world? In Goffman's

(1961) *Asylums* he tried to reveal the different worlds lived by the staff and inmates, and to describe and explicate some of the ceremonies that were used to reinforce boundaries between the two groups. A large part of his work tracked what he called the "moral careers" of inmates from prepatient, through admission, and then as inpatients. Much of the force and influence of Goffman's work derived from its revelations about the grim "unofficial" life lived by patients in large state mental hospitals in the 1950s. In this respect it followed in the Chicago school tradition of exposé and critique. Rosenhan's (1973) classic study of hospital admission and the life of the patient also followed in this tradition. Famously it posed the question of what was required to be diagnosed as mentally ill and then incarcerated, and discovered that it was sufficient to report hearing voices saying "empty," "hollow," and "thud." This "pseudopatient" study was designed with a very specific question about diagnostic criteria in mind; however, after the pseudopatients were admitted they addressed themselves to more typically ethnographic concerns, such as writing detailed descriptions of their settings, monitoring patient contact with different kinds of staff, and documenting the experience of powerlessness and depersonalization.

Goffman's and Rosenhan's work picks up the ethnographic traditions of revealing hidden worlds and providing a basis for social reform. Jodelet (1991) illustrates another analytic possibility by performing an intensive study of one of the longest running community care schemes in the world, the French colony of Ainay-le-Château where mental patients live with ordinary families. Again, in line with the possibilities of ethnography, she attempted to explore the whole setting, including the lives of the patients and their hosts and their understandings of the world. Her work, however, is notable for showing how ethnography can explore the representational systems of participants and relate that system to the lives of the participants. To give just one small example, she shows the way the families' representation of a close link between madness and uncleanness relates to practices such as taking meals separately from the lodgers.

Another topic for ethnographic work has been the practice of psychotherapy itself (Gubrium, 1992; Newfield, Kuehl, Joanning, & Quinn, 1990, 1991). These studies are interested in the experience of patients in therapy and their conceptions of what therapy is, as well as the practices and conceptions of the therapists. Such studies do not focus exclusively on the interaction in the therapy session itself, but on the whole setting.

Although ethnography is dependent on close and systematic description of practices it is not necessarily atheoretical. Ethnographic studies are often guided by one of a range of theoretical conceptions. For example, Jodelet's (1991) study was informed by Moscovici's (1984) theory of social representations and Gubrium's (1992) study of family therapy was guided by broader questions about the way the notion of family and family disorder are constructed in Western society. Ethnography has sometimes been treated as particularly appropriate for feminist work because of the possibility of combining concerns with experience and social context (e.g., Ronai, 1996).

3.06.3.2 Procedures

Ethnography typically involves a mix of different methods with interviews and participant observation being primary, but often combined with nonparticipant observation and the analysis of documents of one kind or another. Such a mixture raises a large number of separate issues which will only be touched on here. There are whole books on some elements of ethnographic work such as selecting informants (Johnson, 1990), living with informants (Rose, 1990), and interpreting ethnographic writings (Atkinson, 1990). The focus here is on research access, field relations, interviewing, observing, fieldnotes, and analysis (see Ellen, 1984; Fetterman, 1989; Fielding, 1993; Hammersley & Atkinson, 1995; Rachel, 1996; Toren, 1996; Werner & Schoepfle, 1987).

3.06.3.2.1 Access

Research access is often a crucial issue in ethnography, as a typical ethnographic study will require not only access to some potentially sensitive group or setting but may involve the researcher's bodily presence in delicate contexts. Sitting in on a family therapy session, for example, may involve obtaining consent from a range of people who have different concerns and has the potential for disrupting, or at least subtly changing, the interaction that would have taken place. There are only restricted possibilities for the ethnographer to enter settings with concealed identities, as Goffman did with his mental hospital study, and Rosenhan's pseudopatients did. Moreover, such practices raise a host of ethical and practical problems which increasingly lead ethnographers to avoid deception. Access can present another problem in sensitive settings if it turns out that it is precisely unusual examples where access is granted, perhaps because the participants view them as models of good practice.

3.06.3.2.2 Field relations

Field relations can pose a range of challenges. Many of these follow from the nature of the participation of the researcher in the setting. How far should researchers become full participants and how far should they stay uninvolved observers? The dilemma here is that much of the power of ethnography comes from the experience and knowledge provided by full participation, and yet such participation may make it harder to sustain a critical distance from the practices under study. The ethnographer should not be converted to the participants' cultural values; but neither should they stay entirely in the Martian role that will make it harder to understand the subtle senses through which the participants understand their own practices. Field relations also generate many of practical, prosaic, but nevertheless important problems which stem from the sheer difficulty of maintaining participant status in an unfamiliar and possibly difficult setting for a long period of time. At the same time there are a whole set of skills required to do with building productive and harmonious relationships with participants.

3.06.3.2.3 Fieldnotes

One of the central features of participant observation is the production of fieldnotes. Without notes to take away there is little point in conducting observation. In some settings it may be possible to take notes concurrently with the action but often the researcher will need to rely on their memory, writing up notes on events as soon as possible after they happened. A rule of thumb is that writing up fieldnotes will take just as much time as the original period of observation (Fielding, 1993). In some cases it may be possible to tape record interaction as it happens. However, ethnographers have traditionally placed less value on recording as they see the actual process of note taking as itself part of the process through which the researcher comes to understand connections between processes and underlying elements of interaction.

Ethnographers stress that fieldnotes should be based around concrete descriptions rather than consisting of abstract higher-order interpretations. The reason for this is that when observation is being done it may not yet be clear what questions are to be addressed. Notes that stay at low levels of inference are a resource that can be used to address a range of different questions. Fielding argues that fieldnotes are expected:

to provide a running description of events, people and conversation. Consequently each new setting observed and each new member of the setting

merits description. Similarly, changes in the human or other constituents of the setting should be recorded. (1993, p. 162)

It is also important to distinguish in notes between direct quotation and broad précis of what participants are saying. A final point emphasised by ethnographers is the value of keeping a record of personal impressions and feelings.

3.06.3.2.4 Interviews

Ethnographers make much use of interviews. However, in this tradition interviews are understood in a much looser manner than in much of psychology. Indeed, the term interview may be something of a misnomer with its image of the researcher running through a relatively planned set of questions with a single passive informant in a relatively formal setting. In ethnography what is involved is often a mix of casual conversations with a range of different participants. Some of these may be very brief, some extended, some allowing relatively formal questioning, others allowing no overt questioning. In the more formal cases the interview may be conducted with a planned schedule of questions and the interaction is recorded and transcribed.

3.06.3.2.5 Analysis

There is not a neat separation of the data collection and analytic phases of ethnographic research. The judgements about what to study, what to focus on, which elements of the local culture require detailed description, and which can be taken for granted, are already part of analysis. Moreover, it is likely that in the course of a long period of participant observation, or a series of interviews, the researcher will start to develop accounts for particular features of the setting, or begin to identify the set of representations shared by the participants. Such interpretations are refined, transformed, and sometimes abandoned when the fieldwork is completed and the focus moves on to notes and transcripts.

Fielding (1993, p. 192) suggests that the standard pattern of work with ethnographic data is straightforward. The researcher starts with the fieldnotes and transcripts and searches them for categories and patterns. These themes form a basis for the ethnographic account of the setting, and they also structure the more intensive analysis and probably the write-up of the research. The data will be marked or cut up (often on computer text files) to collect these themes together. In practice, the ethnographer is unlikely to attempt a complete account of a

setting, but will concentrate on a small subset of themes which are most important or which relate to prior questions and concerns.

The analytic phase of ethnography is often described in only the sketchiest terms in ethnography guidebooks. It is clear that ethnographers often make up their own ways of managing the large amount of materials that they collect, and for using that material in convincing research accounts. At one extreme, ethnography can be considered an approach to develop the researcher's competence in the community being studied—they learn to be a member, to take part actually and symbolically, and they can use this competence to write authoritatively about the community (Collins, 1983). Here extracts from notes and interview transcripts become merely exemplars of the knowledge that the researcher has gained through participation. At the other extreme, ethnography blurs into grounded theorizing, with the notes and transcripts being dealt with through line-by-line coding, comparison of categories, and memo writing. Here the researcher's cultural competence will be important for interpreting the material, but the conclusions will ultimately be dependent on the quality of the fieldnotes and transcripts and what they can support.

3.06.3.2.6 Validity

One of the virtues of ethnography is its rich ecological validity. The researcher is learning directly about what goes on in a setting by observing it, by taking part, and/or by interviewing the members. This circumvents many of the inferences that are needed in extrapolating from more traditional psychological research tools—questionnaires, experimental simulations—to actual settings. However, the closeness of the researcher to the setting does not in itself ensure that the research that is produced will be of high quality.

The approach to validity most often stressed by ethnographers is triangulation. At the level of data this involves checking to see that different informants make the same sorts of claims about actions or events. At the level of method, it involves checking that conclusions are supported by different methods, for example, by both interviews and observation. However, triangulation is not without its problems. Discourse researchers have noted that in practice the sheer variability in and between accounts makes triangulation of only limited use (Potter & Wetherell, 1987) and others have identified conceptual problems in judging what a successful triangulation between methods would be (Silverman, 1993).

3.06.3.3 Example: Gubrium (1992)

There are only a small number of ethnographies done in clinical settings or on clinical topics. For instance, many of the clinical examples in Newfield, Sells, Smith, Newfield, & Newfield's (1996) chapter on ethnography in family therapy are unpublished dissertations. The body of ethnographic work is small but increasing rapidly. I have chosen Gubrium's (1992) study of family therapy in two institutions as an example because it is a book-length study, and it addresses the therapeutic process itself rather than concentrating solely on the patients' lives in hospital or community care schemes. However, it is important to stress that Gubrium's focus was as much on what the therapy revealed about the way notions of family and order are understood in American culture as in therapeutic techniques and effectiveness. He was concerned with the way behaviours such as truancy take their sense as part of a troubled family and the way service professionals redefine family order as they instigate programmes of rehabilitation.

Gubrium's choice of two contrasting institutions is a commonplace one in ethnography. The small number enables an intensive approach; having more than one setting allows an illuminating range of comparisons and contrasts. In this case one was inpatient, one outpatient; one more middle class than the other; they also varied in their standard approaches to treatment. The virtues of having two field sites shine through in the course of the write-up, although it is interesting to note that the selection was almost accidental as the researcher originally expected to be successful in gaining access to only one of the institutions.

The fieldwork followed a typical ethnographic style of spending a considerable amount of times at the two facilities, talking to counselors, watching them at work in therapy sessions, reviewing videos of counseling, and making fieldnotes. The study also drew on a range of documents including patients' case notes and educational materials. In some ways this was a technically straightforward setting for ethnographic observation as many of the participants were themselves university trained practitioners who made notes and videos as part of the general workings of the facilities.

One of the striking differences between this ethnographic study of therapy and a typical process or outcome study is that the therapy is treated as a part of its physical, institutional, and cultural contexts. For instance, time is spent documenting the organization of the reception areas of the two facilities and the way the counselors use the manner in which the families

seat themselves in that area as evidence of family dynamics. Gubrium writes about the important role of tissue boxes in both signaling the potential for emotional display and providing practical support when such display occurs:

I soon realized that tissues were about more than weeping and overall emotional composure during therapy. Tissues mundanely signaled the fundamental reality of the home as locally understood: a configuration of emotional bonds. For Benson [a counselor] their usage virtually put the domestic disorder of the home on display, locating the home's special order in the minutiae of emotional expression. (Gubrium, 1992, p. 26)

The ethnographic focus on events in context means that therapy is treated as a product of actual interactions full of contingency and locally managed understandings. It shows the way abstract notions such as family systems or tough love are managed in practice, and the way the various workers relate to each other as well as to the clients. It provides an insight into the world of family therapy quite different from most other styles of research.

3.06.3.4 Virtues and Limitations

Much of the power of ethnographic research comes from its emphasis on understanding people's actions and representations both in context and as part of the everyday practices that make up their lives, whether they are Yanomami Indians or family therapists. It can provide descriptions which pick out abstract organizations of life in a setting as well as allowing the reader rich access. Ethnography can be used in theory development and even theory testing (Hammersley & Atkinson, 1995). It is flexible; research can follow up themes and questions as they arise rather than necessarily keeping to preset goals.

Ethnographic research can be very time consuming and labor intensive. It can also be very intrusive. Although Gubrium was able to participate in some aspects of family therapy, this was helped by the sheer number of both staff and family members who were involved. It is less easy to imagine participant observation on individual therapy.

One of the most important difficulties with ethnographic work is that the reader often has to take on trust the conclusions because the evidence on which they are based is not available for assessment (Silverman, 1993). Where field notes of observations are reproduced in ethnographies—and this is relatively rare—such notes are nevertheless a ready-

theorized version of events. Descriptions of actions and events are always bound up with a range of judgments (Potter, 1996a). Where analysis depends on the claims of key informants the problem is assessing how these claims relate to any putative activities that are described. Ethnographers deal with these problems with varying degrees of sophistication (for discussion see Nelson, 1994). However, some researchers treat them as inescapable and have turned to some form of discourse analysis instead.

For more detailed discussions of ethnography readers should start with Fielding's (1993) excellent brief introduction and then use Hammersley and Atkinson (1995) as an authoritative and up to date overview. Two of the most comprehensive, although not always most sophisticated, works are Werner and Schoepfle (1987) and Ellen (1984). Both were written by anthropologists, and this shows in their understanding of what is important. Newfield, Sells, Smith, Newfield, and Newfield (1996) provide a useful discussion of ethnography in family therapy research.

3.06.4 DISCOURSE ANALYSIS

Although both grounded theorizing and ethnographic work in clinical areas has increased, the most striking expansion has been in research in discourse analysis (Soyland, 1995). This work is of variable quality and often done by researchers isolated in different subdisciplines; moreover, it displays considerable terminological confusion. For simplicity, discourse analysis is taken as covering a range of work which includes conversation analysis and ethnomethodology (Heritage, 1984; Nofsinger, 1991), some specific traditions of discourse analysis and discursive psychology (Edwards & Potter, 1992a; Potter & Wetherell, 1987), some of the more analytically focused social constructionist work (McNamee & Gergen, 1992), and a range of work influenced by post-structuralism, Continental discourse analysis, and particularly the work of Foucault (Burman, 1995; Madigan, 1992; Miller & Silverman, 1995). In some research these different themes are woven together; elsewhere strong oppositions are marked out.

The impetus for discourse analysis in clinical settings comes from two directions. On the one hand, there are practitioner/researchers who have found ideas from social constructionism, literary theory, and narrative useful (e.g., Anderson & Goolishian, 1988; White & Epston, 1990). On the other, there are academic

researchers who have extended analytic and theoretical developments in discourse studies to clinical settings (e.g., Aronsson & Cederborg, 1996; Bergmann, 1992; Buttny, 1996; Edwards, 1995; Lee, 1995). Collections such as Siegfried (1995), Burman, Mitchel, and Salmon (1996), and Morris and Chenail (1995) reflect both types of work, sometimes in rather uneasy combination (see Antaki, 1996).

This tension between an applied and academic focus is closely related to the stance taken on therapy. In much discourse analysis, therapy is the start point for research and the issue is how therapy gets done. For example, Gale's (1991; Gale & Newfield, 1992) intensive study of one of O'Hanlon's therapy sessions considered the various ways in which the goals of solution focused family therapy were realized in the talk between therapist and client. However, some conversation analysts and ethnomethodologists resist assuming that conversational interaction glossed by some parties as therapy (solution focused, Milan School, or whatever) must have special ingredient X—therapy—that is absent in, say, the everyday "troubles talk" done with a friend over the telephone (Jefferson, 1988; Jefferson & Lee, 1992; Schegloff, 1991; Watson, 1995).

This is a significant point for all researchers into therapy and counseling, so it is worth illustrating with an example. In Labov and Fanshel's classic study, the therapy session starts in the following manner:

- Rhoda: I don't (1.0) know, whether (1.5) I-I think I did- the right thing, jistalittle situation came up (4.5) an' I tried to uhm (3.0) well, try to (4.0) use what I- what I've learned here, see if it worked (0.3)
- Therapist: Mhm
- Rhoda: Now, I don't know if I did the right thing. Sunday (1.0) um- my mother went to my sister's again.
- Therapist: Mm-hm
- Rhoda: And she usu'lly goes for about a day or so, like if she leaves on Sunday, she'll come back Tuesday morning. So- it's nothing. But- she lef' Sunday, and she's still not home.
- Therapist: O- oh.
(1977, p. 263)

Labov and Fanshel provide many pages of analysis of this sequence. They identify various direct and indirect speech acts and make much of what they call its therapeutic interview style, particularly the vague reference terms at the start: "right thing" and "jistalittle situation." This vagueness can easily be heard

as the 19-year-old anorexia sufferer struggling to face up to her relationship with her difficult mother. However, in a reanalysis from a conversation analytic perspective, Levinson (1983) suggests that this sequence is characteristic of mundane news telling sequences in everyday conversation. These typically have four parts: the pre-announcement, the go ahead, the news telling, and the news receipt. For example:

- | | | |
|----|---|-----------------|
| D: | I forgot to to tell
you the two best
things that
happen' to me
today. | preannouncement |
| R: | Oh super = what
were they. | go ahead turn |
| D: | I got a B+ on my
math test ... and I
got an athletic
award | news telling |
| R: | Oh excellent.
(Levinson, 1983, p. 349—slightly
modified) | news receipt |

A particular feature of preannouncements is their vagueness, for their job is to prefigure the story (and thereby check its newsworthiness), not to actually tell it. So, rather than following Labov and Fanshel (1977) in treating this vagueness as specific to a troubled soul dealing with a difficult topic in therapy, Levinson (1983) proposes that it should be understood as a commonplace characteristic of mundane interaction.

This example illustrates a number of features typical of a range of discursive approaches to therapy talk. First, the talk is understood as performing actions; it is not being used as a pathway to cognitive processes (repression, say) or as evidence of what Rhoda's life is like (her difficult mother). Second, the interaction is understood as sequentially organized so any part of the talk relates to what came immediately before and provides an environment for what will immediately follow. The realization of how far interaction gets its sense from its sequential context has critical implications for approaches such as content analysis and grounded theory which involve making categorizations and considering relations between them; for such categorizations tend to cut across precisely the sequential relations that are important for the sense of the turn of talk.

The third feature is that the talk is treated as ordered in its detail not merely in its broad particulars. For example, Levinson (1983) highlights a number of orderly elements in what we might easily mistake for clumsiness in Rhoda's first turn:

R's first turn is ... formulated to prefigure (i) the telling of something she did (*I think I did the right thing*), and (ii) the describing of the situation that led to the action (*jistalittle situation came up*). We are therefore warned to expect a story with two components; moreover the point of the story and its relevance to the here and now is also prefigured (*use what I've learned here, see if it worked*). (1983, p. 353)

Even the hesitations and glottal stops in Rhoda's first turn, which seem so redolent of a troubled young person are "typical markings of self-initiated self-repair, which is characteristic of the production of first topics" (Levinson, 1983, p. 353). This emphasis on the significance of detail has an important methodological consequence—if interaction is to be understood properly it must be represented in a way that captures this detail. Hence the use of a transcription scheme that attempts to represent a range of paralinguistic features of talk (stress, intonation) on the page as well as elements of the style of its delivery (pauses, cut-offs).

A fourth feature to note here is the comparative approach that has been taken. Rather than focus on therapy talk alone Levinson is able to support an alternative account of the interaction by drawing on materials, and analysis, taken from mundane conversations. Since the mid 1980s there has been a large amount of work in different institutional settings as well as everyday conversation, and it is now possible to start to show how a news interview, say, differs from the health visitor's talk with an expectant mother, and how that differs in turn from conversation between two friends over the telephone (Drew & Heritage, 1992a).

A fifth and final feature of this example is that it is an analysis of interaction. It is neither an attempt to reduce what is going on to cognitions of the various parties—Rhoda's denial, say, or the therapist's eliciting strategies—nor to transform it into countable behaviors such as verbal reinforcers. This style of discourse work develops a Wittgensteinian notion of cognitive words and phrases as elements in a set of language games for managing issues of blame, accountability, description, and so on (Coulter, 1990; Edwards, 1997; Harré & Gillett, 1994). Such a "discursive psychology" analyzes notions such as attribution and memory in terms of the situated practices through which responsibility is assigned and the business done by constructing particular versions of past events (Edwards & Potter, 1992b, 1993).

3.06.4.1 Questions

Discourse analysis is more directly associated with particular theoretical perspectives—

ethnomethodology, post-structuralism, discursive psychology—than either grounded theory or ethnography. The questions it addresses focus on the practices of interaction in their natural contexts and the sorts of discursive resources that are drawn on those contexts.

Some of the most basic questions concern the standardized sequences of interaction that take place in therapy and counseling (Buttny & Jensen, 1995; Lee, 1995; Peräkylä, 1995; Silverman, 1997b). This is closely related to a concern with the activities that take place. What is the role of the therapist's tokens such as "okay" or "mm hm" (Beach, 1995; Czynewski, 1995)? How do different styles of questioning perform different tasks (Bergmann, 1992; Peräkylä, 1995; Silverman, 1997b)? What is the role of problem formulations by both therapists and clients, and how are they transformed and negotiated (Buttny, 1996; Buttny & Jensen, 1995; Madill & Barkham, 1997)? For example, in a classic feminist paper Davis (1986) charts the way a woman's struggles with her oppressive social circumstances are transformed into individual psychological problems suitable for individual therapy. While much discourse research is focused on the talk of therapy and counseling itself, studies in other areas show the value of standing back and considering clinical psychology as a set of work practices in themselves, including management of clients in person and as records, conducting assessments, delivering diagnoses, intake and release, stimulating people with profound learning difficulties, case conferences and supervisions, offering advice, and managing resistance (see Drew & Heritage, 1992a). Discourse researchers have also been moving beyond clinical settings to study how people with clinical problems or learning difficulties manage in everyday settings (Brewer & Yearley, 1989; Pollner & Wikler, 1985; Wootton, 1989).

Another set of questions are suggested by the perspective of discursive psychology. For example, Edwards (1997) has studied the rhetorical relationship between problem formulations, descriptions of activities, and issues of blame in counseling. Cheston (1996) has studied the way descriptions of the past in a therapy group of older adults can create a set of social identities for the members. Discursive psychology can provide a new take on emotions, examining how they are constructed and their role in specific practices (Edwards, 1997).

From a more Foucaultian inspired direction, studies may consider the role of particular discourses, or interpretative repertoires in constructing the sense of actions and experiences (Potter & Wetherell, 1987). For example, what are the discursive resources that young

women draw on to construct notions of femininity, agency, and body image in the context of eating disorders (Malson & Ussher, 1996; Wetherell, 1996)? What discourses are used to construct different notions of the person, of the family, and of breakdown in therapy (Burman, 1995; Frosh, Burck, Strickland-Clark, & Morgan, 1996; Soal & Kotter, 1996)? This work is often critical of individualistic conceptions of therapy.

Finally, discourse researchers have stood back and taken the administration of psychological research instruments as their topic. The intensive focus of such work can show the way that the sort in idiosyncratic interaction that takes place when filling in questionnaires or producing records can lead to particular outcomes (Antaki & Rapley, 1996; Garfinkel, 1967b; Rapley & Antaki, 1996; Soyland, 1994).

Different styles of discourse work address rather different kinds of questions. However, the conversation analytic work is notable in commonly starting from a set of transcribed materials rather than preformulated research questions, on the grounds that such questions often embody expectations and assumptions which prevent the analyst seeing a range of intricacies in the interaction. Conversation analysis reveals an order to interaction that participants are often unable to formulate in abstract terms.

3.06.4.2 Procedures

The majority of discourse research in the clinical area has worked with records of natural interaction, although a small amount has used open-ended interviews. There is not space here to discuss the role of interviews in discourse analysis or qualitative research generally (see Kvale, 1996; Mischler, 1986; Potter & Mulkay, 1985; Widdicombe & Wooffitt, 1995). For simplicity discourse work will be discussed in terms of seven elements.

3.06.4.2.1 Research materials

Traditionally psychologists have been reluctant to deal with actual interaction, preferring to model it experimentally, or reconstruct it via scales and questionnaires. Part of the reason for this is the prevalent cognitivist assumptions which have directed attention away from interaction itself to focus on generative mechanisms within the person. In contrast, discourse researchers have emphasised the primacy of practices of interaction themselves. The most obvious practice setting for clinical work is the therapy session itself, and this has certainly

received the most attention. After all, there is an elegance in studying the “talking cure” using methods designed to acquire an understanding of the nature of talk. However, there is a danger that such an exclusive emphasis underplays mundane aspects of clinical practices: giving advice, offering a diagnosis, the reception of new clients, casual talk to clients’ relatives, writing up clinical records, case conferences, clinical training, and assessment.

Notions of sample size do not translate easily from traditional research as the discourse research focus is not so much on individuals as on interactional phenomena of various kinds. Various considerations can come to the fore here, including the type of generalizations that are to be made from the research, the time and resources available, and the nature of the topic being studied. For example, if the topic is the role of “mm hms” in therapy a small number of sessions may generate a large corpus; other phenomena may be much rarer and require large quantities of interaction to develop a useful corpus. For some questions, single cases may be sufficient to underpin a theoretical point or reveal a theoretically significant phenomena.

3.06.4.2.2 Access and ethics

One of the most difficult practical problems in conducting discourse research involves getting access to sometimes sensitive settings in ways which allow for informed consent from all the parties involved. Experience suggests that more often than not it is the health professionals rather than the clients who are resistant to their practices being studied, perhaps because they are sensitive to the difference between the idealized version of practices that was used in training and the apparently more messy procedures in which they actually engage. Sometimes reassurances about these differences can be productive.

Using records of interaction such as these raise particular issues for ensuring anonymity. This is relatively manageable with transcripts where names and places can be changed. It is harder for audio tape and harder still with video. However, technical advances in the use of digitized video allow for disguising of identity with relatively little loss of vocal information.

3.06.4.2.3 Audio and video recording

There is a range of practical concerns in recording natural interaction, some of them pulling in different directions. An immediate issue is whether to use audio or video recording. On the one hand, video provides helpful information about nonverbal activities that

may be missed from the tape, and good quality equipment is now compact and cheap. On the other hand, video can be more intrusive, particularly where the recording is being done by one of the participants (a counselor, say), and may be hard to position so it captures gestures and expressions from all parties to an interaction. Video poses a range of practical and theoretical problems with respect to the transcription of nonvocal activity which can be both time consuming and create transcript that is difficult to understand. Moreover, there is now a large body of studies that shows high quality analysis can, in many cases, be performed with an audio tape alone. One manageable solution is to use video if doing so does not disrupt the interaction, and then to transcribe the audio and work with a combination of video tape and audio transcript. Whether audio or video is chosen, the quality (clear audibility and visibility) is probably the single most consequential feature of the recording for the later research.

Another difficulty is how far the recording of interaction affects its nature. This is a subtle issue. On the one hand, there are a range of ways of minimizing such influences including acclimatizing participants and giving clear descriptions of the role of the research. On the other, experience has shown that recording has little influence on many, perhaps most, of the activities in which the discourse researcher is interested. Indeed, in some clinical settings recordings may be made as a matter of course for purposes of therapy and training, and so no new disruption is involved.

3.06.4.2.4 Transcription

Producing a high-quality transcript is a crucial prerequisite for discourse research. A transcript is not a neutral, simple rendition of the words on a tape (Ochs, 1979). Different transcription systems emphasize different features of interaction. The best system for most work of this kind was developed by the conversation analyst Gail Jefferson using symbols that convey features of vocal delivery that have been shown to be interactionally important to participants (Jefferson, 1985). At the same time the system is designed to use characters and symbols easily available on wordprocessors making it reasonably easy to learn and interpret. The basic system is summarized in Table 3. For fuller descriptions of using the system see Button and Lee, (1987), Ochs, Schegloff, and Thompson (1996), and Psathas and Anderson (1990).

Producing high quality transcript is very demanding and time consuming. It is hard to give a standard figure for how long it takes

Table 3 Brief transcription notation.

Um::	colons represent lengthening of the preceding sound; the more colons, the greater the lengthening.
I've-	a hyphen represents the cut-off of the preceding sound, often by a stop.
↑ <u>Already</u>	up and down arrows represent sharp upward and downward pitch shifts in the following sounds. Underlining represents stress, usually by volume; the more underlining the greater the stress.
settled in his = Mm = own mind.	the equals signs join talk that is continuous although separated across different lines of transcript.
hhh hh .hh	'h' represents aspiration, sometimes simply hearable breathing, sometimes laughter, etc.; when preceded by a superimposed dot, it marks in-breath; in parenthesis inside a word it represents laugh infiltration.
P(h)ut	
hhh[hh .hh] [I just]	left brackets represent point of overlap onset; right brackets represent point of overlap resolution.
.,?	punctuation marks intonation, not grammar; period, comma and 'question mark' indicate downward, 'continuative', and upward contours respectively.
()	single parentheses mark problematic or uncertain hearings; two parentheses separated by an oblique represent alternative hearings.
(0.2)(.)	numbers in parentheses represent silence in tenths of a second; a dot in parentheses represents a micro-pause, less than two tenths of a second.
°mm hmm°	the degree signs enclose significantly lowered volume.

Source: Modified from Schegloff (1997, pp. 184–185).

because much depends on the quality of the recording (fuzzy, quiet tapes can quadruple the time needed) and the type of interaction (an individual therapy session presents much less of a challenge than a lively case conference with a lot of overlapping talk and extraneous noise); nevertheless, a ratio of one hour of tape to twenty hours of transcription time is not unreasonable. However, this time should not be thought of as dead time before the analysis proper. Often some of the most revealing analytic insights come during transcription because a profound engagement with the material is needed to produce good transcript—it is generally useful to make analytic notes in parallel to the actual transcription.

3.06.4.2.5 Coding

In discourse research the principle task of coding is to make the task of analysis more straightforward by sifting relevant materials from a large body of transcript. In this it differs from both grounded theory and traditional content analysis where coding is a more intrinsic

part of the analysis. Typically coding will involve sifting through materials for instances of a phenomenon of interest and copying them into an archive. This coding will often be accompanied by preliminary notes as to their nature and interest. At this stage selection is inclusive—it is better to include material that can turn out to be irrelevant at a later stage than exclude it for ill-formulated reasons early on. Coding is a cyclical process. Full analysis of a corpus of materials can often take the researcher back to the originals as a better understanding of the phenomenon reveals new examples. Often initially disparate topics merge together in the course of analysis while topics which seemed unitary can be separated.

3.06.4.2.6 Analysis

There is no single recipe for analyzing discourse. Nevertheless, there are five considerations which are commonly important in analysis. First, the researcher can use variation in and between participants' discourse as an analytic lever. The significance of variation is

that it can be used for identifying and explicating the activities that are being performed by talk and texts. This is because the discourse is constructed in the specific ways that it is precisely to perform these actions; a description of jealousy in couple counseling can be assembled very differently when excusing or criticizing certain actions (Edwards, 1995). The researcher will benefit from attending to variations in the discourse of single individuals, between different individuals, and between what is said and what might have been said.

Second, discourse researchers have found it highly productive to attend to the detail of discourse. Conversation analysts such as Sacks (1992) have shown that the details in discourse—the hesitations, lexical choice, repair, and so on—are commonly part of the performance of some act or are consequential in some way for the outcome of the interaction. Attending to the detail of interaction, particularly in transcript, is one of the most difficult things for psychologists who are used to reading through the apparently messy detail for the gist of what is going on. Developing analytic skills involves a discipline of close reading.

Third, analysis often benefits from attending to the rhetorical organization of discourse. This involves inspecting discourse both for the way it is organized to make argumentative cases and for the way it is designed to undermine alternative cases (Billig, 1996). A rhetorical orientation refocuses the analyst's attention away from questions about how a version—description of a psychological disorder, say—relates to some putative reality and focuses it on how it relates to competing alternatives.

Concern with rhetoric is closely linked to a fourth analytic concern with accountability. That is, displaying one's activities as rational, sensible, and justifiable. Ethnomethodologists have argued that accountability is an essential and pervasive character of the design and understanding of human conduct generally (Garfinkel, 1967c; Heritage, 1984). Again an attention to the way actions are made accountable is an aid for understanding precisely what those actions are.

A fifth and final analytic consideration is of a slightly different order. It is an emphasis on the virtue of building on prior analytic studies. In particular, researchers into interaction in an institutional setting such as a family therapy setting will almost certainly benefit from a familiarity with research on mundane talk as well as an understanding of how the patterning of turn taking and activities change in different institutional settings.

The best way to think of these five considerations is not as specific rules for research but as

elements in an analytic mentality that the researcher will develop as they become more and more skilled. It does not matter that they are not spelled out in studies because they are separate from the procedures for validating discourse analytic claims.

3.06.4.2.7 *Validity*

Discourse researchers typically draw on some combination of four considerations to justify the validity of analytic claims. First, they make use of participants' own understandings as they are displayed in interaction. One of the features of a conversation is that any turn of talk is oriented to what came before and what comes next, and that orientation typically displays the sense that the participant makes of the prior turn. Thus, at its simplest, when someone provides an answer they thereby display the prior turn as a question and so on. Close attention to this turn-by-turn display of understanding provides one important check on analytic interpretations (see Heritage, 1988).

Second, researchers may find (seemingly) deviant cases most useful in assessing the adequacy of a claim. Deviant cases may generate problems for a claimed generalization, and lead the researcher to abandon it; but they may also display in their detailed organization precisely the reason why a standard pattern should take the form that it does.

Third, a study may be assessed in part by how far it is coherent with previous discourse studies. A study that builds coherently on past research is more plausible than one that is more anomalous.

Fourth, and most important, are readers' evaluations. One of the distinctive features of discourse research is its presentation of rich and extended materials in a way that allows the reader to make their own judgements about interpretations that are placed along side of them. This form of validation contrasts with much grounded theory and ethnography where interpretations often have to be taken on trust; it also contrasts with much traditional experimental and content analytic work where it is rare for "raw" data to be included or more than one or two illustrative codings to be reproduced.

Whether they appear singly or together in a discourse study none of these procedures guarantee the validity of an analysis. However, as the sociology of science work reviewed earlier shows, there are no such guarantees in science.

3.06.4.3 **Example: Peräkylä (1995)**

Given the wide variety of discourse studies with different questions and styles of analysis it

is not easy to choose a single representative study. The one selected is Peräkylä's (1995) investigation of AIDS counseling because it is a major integrative study that addresses a related set of questions about interaction, counseling, and family therapy from a rigorous conversation analytic perspective and illustrates the potential of discourse work on clinical topics. It draws heavily on the perspective on institutional talk surveyed by Drew and Heritage (1992b) and is worth reading in conjunction with Silverman's (1997b) related study of HIV+ counseling which focuses more on advice giving.

Peräkylä focused on 32 counseling sessions conducted with HIV+ hemophilic mainly gay identified men and their partners at a major London hospital. The sessions were videotaped and transcribed using the Jeffersonian system. A wider archive of recordings (450 hours) was drawn on to provide further examples of phenomena of interest but not otherwise transcribed. The counselors characterized their practices in terms of Milan School Family Systems Theory and, although this is not the starting point of Peräkylä's study, he was able to explicate some of the characteristics of such counseling.

Part of the study is concerned with identifying the standard turn-taking organization of the counseling. Stated baldly it is that (i) counselors ask questions; (ii) clients answer; (iii) counselors comment, advise, or ask further questions. When laid out in this manner the organization may not seem much of a discovery. However, the power of the study is showing how this organization is achieved in the interaction and how it can be used to address painful and delicate topics such as sexual behavior, illness, and death.

Peräkylä goes on to examine various practices that are characteristic of family systems theory such as "circular questioning," where the counselor initially questions the client's partner or a family member about the client's feelings, and "live open supervision," where a supervisor may offer questions to the counselor that are, in turn, addressed to the client. The study also identifies some of the strategies by which counselors can address "dreaded issues" in a manageable way. Take "circular questioning," for example. In mundane interaction providing your partial experience of some event or experience is a commonplace way of "fishing" for a more authoritative version (Pomerantz, 1980). For example:

- A: Yer line's been busy.
 B: Yeuh my fu (hh)- .hh my father's wife called me

In a similar way, the use of questioning where a client's partner, say, offers their understanding of an experience "can create a situation where the clients, in an unacknowledged but most powerful way, elicit one another's descriptions of their inner experiences" (Peräkylä, 1995, p. 110). In the following extract the client is called Edward; his partner and the counselor are also present.

- Counselor: What are some of things that you think Edward might have to do. = He says he doesn't know where to go from here maybe: and awaiting results and things.
 (0.6)
 Counselor: What d'you think's worrying him.
 (0.4)
 Partner: Uh::m hhhhhh I think it's just fear of the unknown:n.
 Client: Mm[:
 Counselor: [Oka:y.
 Partner: [At- at the present ti:me. (0.2)
 Uh:m (.) once: he's (0.5) got a better understanding of (0.2) what could happen
 Counselor: Mm:
 Partner: uh:m how .hh this will progre:ss then: I think (.) things will be a little more [settled in his =
 Counselor: [Mm
 Partner: = own mi:nd.
 Counselor: Mm:
 (.)
 Client: Mm[:
 Counselor: [Edward (.) from what you know:
 ((sequence continues with Edward responding to a direct question with a long and detailed narrative about his fears))
 (Peräkylä, 1995, p. 110)

Peräkylä emphasizes the way that the client's talk about his fears is elicited, in part, through the counsellor asking the partner for his own view of those fears. The point is not that the client is forced to reveal his experiences, rather it is that the prior revelation of his partner's partial view produces an environment where such a revelation is expected and nonrevelation will itself be a delicate and accountable matter. In effect, what Peräkylä is documenting here are the conversational mechanisms which family therapists characterize as using circular questioning to overcome clients' resistance.

3.06.4.4 Virtues and Limitations

Given the variety of styles of work done under the rubric of discourse analysis it is difficult to give an overall summary of virtues and limitations. However, the virtue of a range of

studies in the conversation and discourse analytic tradition is that they offer, arguably for the first time in psychology, a rigorous way of directly studying human social practices. For example, the Peräkylä study discussed above is notable in studying actual HIV+ counseling in all its detail. It is not counseling as recollected by participants while filling in rating scales or questionnaires; it is not an experimental simulation of counseling; it does not depend on *post hoc* ethnographic reconstructions of events; nor are the activities immediately transformed into broad coding categories or used as a mere shortcut to underlying cognitions.

A corollary of this emphasis on working with tapes and transcripts of interaction is that these are used in research papers to allow readers to evaluate the adequacy of interpretations in a way that is rare in other styles of research.

Studies in this tradition have started to reveal an organization of interaction and its local management that has been largely missed from traditional psychological work from a range of perspectives. Such studies offer new conceptions of what is important in clinical practice and may be particularly valuable in clinical training which has often been conducted with idealized or at least cleaned up examples of practice.

Discourse research is demanding and requires a considerable investment of the researcher's time to produce satisfactory results. It does not fit neatly into routines that can be done by research assistants. Indeed, even transcription, which may seem to be the most mechanical element in the research, requires considerable skill and benefits from the involvement of the primary researchers. Analysis also requires considerable craft skills which can take time to learn.

With its focus on interaction, this would not necessarily be the perspective of choice for researchers with a more traditional cognitive or behavioral focus, although it has important implications for both of these. Some have claimed that it places too much emphasis on verbal interaction at the expense of nonverbal elements, and broader issues of embodiment. Others have claimed that it places too much emphasis on the importance of local contexts of interaction rather than on broader issues such as gender or social class. For some contrasting and strongly expressed claims about the role of discourse analysis in the cognitive psychology of memory, see papers in Conway (1992).

An accessible general introduction to various practical aspects of doing discourse analysis is provided in Potter and Wetherell (1987; see also Potter, 1996b, 1997). Potter and Wetherell (1995) discuss the analysis of broader content

units such as interpretative repertoires, while Potter and Wetherell (1994) and Wooffitt (1993) discuss the analysis of how accounts are constructed. For work in the distinctive conversation analytic tradition Drew (1995) and Heritage (1995) provide clear overviews and Heath and Luff (1993) discuss analysis which incorporates video material; Gale (1996) explores the use of conversation analysis in family therapy research.

3.06.5 FUTURE DIRECTIONS

The pace of change in qualitative research in clinical settings is currently breathtaking, and it is not easy to make confident predictions. However, it is perhaps useful to speculate on how things might develop over the next few years. The first prediction is that the growth in the sheer quantity of qualitative research will continue for some time. There is so much new territory, and so many possibilities have been opened up by new theoretical and analytic developments, that they are bound to be explored.

The second prediction is that research on therapy and counseling talk will provide a particular initial focus because it is here that discourse analytic approaches can clearly provide new insights and possibly start to provide technical analytically grounded specifications of the interactional nature of different therapies in practice, as well as differences in interactional style between therapists. There may well be conflicts here between the ideological goals of constructionist therapists and the research goals of discourse analysts.

The third prediction is that the growth of qualitative work will encourage more researchers to attempt integrations of qualitative and quantitative research strategies. There will be attempts to supplement traditional outcomes research with studies of elements of treatment which are not easily amenable to quantification. Here the theoretical neutrality of grounded theory (ironically) is likely to make for easier integration than the more theoretically developed discourse perspectives. The sheer difficulty of blending qualitative and quantitative work should not be underestimated—research that has attempted this has often found severe problems (see Mason, 1994, for a discussion).

The final prediction is that there will be an increased focus on clinical work practices embodied within settings such as clinics and networks of professional and lay relationships. Here the richness of ethnographic work will be drawn on, but increasingly the conversation analytic discipline of working with video and

transcript will replace field notes and recollections. Such work will have the effect of respecifying some of the basic problems of clinical research. Its broader significance, however, may depend on the course of wider debates in Psychology over the development and success of the cognitive paradigm and whether it will have a discursive and interaction based successor.

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