# SIX RULES OF QUALITATIVE RESEARCH: A POST-ROMANTIC ARGUMENT

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Qualitative sociology too easily succumbs to a 'Romantic' impulse. In a sociological context, the Romantic seeks to understand raw 'experience', usually by the use of unstructured interviews. Such work can lack analytic rigour, failing to distinguish the sociologist from the journalist. Following Wittgenstein and Garfinkel, the approach adopted here emphasizes the forms of representation and contexts which inform practical reasoning. It recommends a non-Romantic sociology—an aspiration curiously shared by Durkheim—and suggests some ways to pursue it.

Like their American colleagues, British ethnographers are having to adjust to an era after Garfinkel and Foucault. We see this in Peter Manning's programmatic text *Semiotics and Fieldwork* (1987) and in Michael Mulkay's analyses of scientists' discourses in his *The Word and The World* (1985). In both cases, we find a form of enthography which takes language and textuality seriously.

Such work reminds us that we share a danger of reducing questions about the practice of qualitative research to technical issues to be resolved by cookbook means. I attempt here, with a very broad brush, to raise some of the concealed analytic issues that lurk behind some apparently technical questions like observing 'private' encounters or interpreting interview data. To use a notion of Wittgenstein's, to whom I return later, a touch of 'hygiene' may be useful in clearing our minds about the nature of the phenomena that, as ethnographers, we are attempting to study.

An interesting case in point is Moerman's (1974) neglected study of the Lue tribe in Thailand. Moerman began with the anthropologist's conventional appetite to locate a people in a classificatory scheme. To satisfy this appetite, he started to ask tribespeople questions like 'How do you recognize a member of your tribe?' He reports that his respondents quickly became adept at providing a whole list of traits which constituted their tribe and distinguished them from their neighbors. At the same time, Moerman realized that such a list was, in purely logical terms, endless.

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Perhaps much more relevant to understanding this people than their abstract account of their characteristics was their actual practice in appealing to one or more characteristics in particular situations.

So Moerman stopped asking 'Who are the Lue?'. Clearly, such ethnic identification devices were not used all the time by the Lue any more than we use them to refer to ourselves in a Western culture. The issue is not who the Lue (essentially) are but when ethnic identification labels are invoked and the consequences of invoking them. Curiously enough, Moerman concluded that when you looked at the matter this way, in the context of the rules of sequencing conversation, the apparent differences between the Lue and ourselves were considerably reduced.

Reflecting the post-modern era in which we live, Moerman draws our attention to the nature of representation: its forms and, perhaps, its politics.[1] In such an era, ethnography can no longer concern itself with discovering truths which are unmediated by the situated use of forms of representation.

Yet British and American fieldwork still tends to respond, almost instinctively, to two older impulses (Silverman forthcoming). The Enlightenment urge to categorize and count is found in attempts to locate 'tribes' and cultures in classificatory schemes. Conversely, the desire to understand raw 'experience' (usually via in-depth interviews) harks back to the Romantic Movement of the nineteenth century.[2]

In this paper, I direct most of my fire at (what remains of) Romanticism in qualitative sociology. Admittedly, the crasser forms of this perspective are restricted to student essays and to some of the speeches of Prime Minister Thatcher ('there is no such thing as society' she recently commented). Nevertheless, professional sociology often still responds to the Romantic impulse, particularly in fieldworkers' commitment to the sanctity of what respondents say in open-ended interviews. We are thus sometimes left with the unappetizing choice between treating accounts as privileged data or as 'perspectival' and subject to check via the methods of 'triangulation' with other observations (Denzin 1970).

To talk about 'rules' invites charges of simplification, overgeneralization and so on. While much has had to be crammed into a small space, I hope a common thread will emerge. Throughout, I return to the situated character of accounts and other practices and to the dangers of seeking to identify phenomena apart from these practices and the forms of representation that they embody.

At least three objections may be made to this argument. Two derive from its denial of appeals to unmediated truths. First, if we reject such methods as 'triangulation', isn't there a danger of anecdotalism—simply choosing material that supports your case? Second, if we give up the quest for 'truth' how do we avoid becoming trapped by the snares of relativism and an infinite regress (see Bury 1986)? My answer to the first question is contained in part of the discussion about rule four below (for a more detailed treatment, see Silverman in press). I shall deal with the second question at the conclusion of this paper.

A third objection is potentially more telling: how original is this argument? My answer is simple—not at all (for instance, see Garfinkel 1967). The justification for writing this paper is that is seeks to *apply* the argument *constructively* to a number of analytical and practical problems that confront the fieldworker.

## RULE 1: DON'T MISTAKE CRITIQUE FOR A REASONED ALTERNATIVE

One of the sad things that happens to some students who take courses in social theory is that they end up being convinced that a whole series of theorists are little more than congenital idiots. Durkheim is a good example of the kind of 'straw man' figure that emerges in people's imagination. How could anybody seriously assume, for instance, that such an individual act as suicide is a consequence of social structure? Surely, such students feel, no account of suicide is adequate when it depends on the 'distortions' of official statistics and fails to refer to the motives of the actor.

Disconcertingly, however, there are curious kinds of similarities between Durkheim's account of suicide and research by Atkinson (1978) which draws on an apparently opposed theoretical perspective. Like Durkheim, Atkinson is not interested in psychological accounts of suicide which involve reference to the meaning of the act to the actor. Both are concerned with the social organization in which suicide is embedded—although Atkinson's ethnomethodological perspective locates that organization in the practical reasoning of coroners rather than in forms of social integration. Curiously, also, neither sociologist would question suicide statistics, although their reasons differ. In Durkheim's case, rates of suicide provide him with the social facts that need explanation and official statistics are the only record of such rates. For Atkinson, to question such statistics would imply that you had a *better* way of measuring suicide. This would only make an irony of real social practices (defining the nature of unexpected deaths, collating local and national statistics, recommending policies to reduce rates of suicide, etc.) all of which should be topics for sociological investigation. So, although Atkinson considerably redefines Durkheim's problematic, he is, in some ways, quite close to his position—certainly in a common opposition to the student critique (see Silverman 1985, pp. 32–33).

It is also useful to recognize the limited nature of many of the claims of the "Founding Fathers" of the discipline. For instance, again taking the despised Durkheim, it is important to note that Durkheim's polemical characterizations of 'society' have primarily a methodological status. We can read him as telling us how it is *useful* to view society, not what society *essentially* is. Thus, just as psychologists would generally resist turning 'psychological' phenomena into purely neurological processes, so sociologists would usually not want to reduce 'sociological' phenomena to the psychological dispositions of the people concerned (Durkheim 1974, pp. 24–25). In both cases, the problem is an uncritical reductionist form of thinking which fruitlessly searches for some more 'basic' level of analysis. Durkheim's solution, with which I fully concur, is to stay at one level of analysis and to see what you can say about data at that level, without seeking to resolve ontological questions about the 'essential' character of 'reality'. This may seem an obvious point but, judging by what is written in undergraduate examination answers, it does not usually lodge in students' consciousness.

A further feature of such answers is students' general horror of what they call 'positivism'. Now, of course, this may sometimes refer to certain practices, such as crude quantified 'variable analysis', which are real and, in my view, should be criticized (Blumer 1956; Cicourel 1964). But usually students use it as a 'catch-all' term which seems to encompass much of sociology. The problem that then arises is that it seems to have no referent since I cannot think of any contemporary sociologist or philosopher of science who adopts the label 'positivist'.

What seems to be happening is that students learn to use this label as a tag to refer to any practice or theory that they do not like. It serves, then, as a term of abuse and perhaps conceals that its users have no coherent alternative. This status of 'positivism' as a rhetorical device is underlined when beginning graduate students find that they lack the resources to translate their critique into a reasoned research proposal.

## RULE 2: AVOID TREATING THE ACTOR'S POINT OF VIEW AS AN EXPLANATION

How could anybody have thought this was the case in sociology? How could anybody think that what we ought to do is to go out into the field to report people's exciting, gruesome or intimate experiences?

Yet, judging by the prevalence of what I will call 'naive' interview studies in qualitative research, this indeed seems to be the case. Naive interviewers believe that the supposed limits of structural sociology are overcome by an open-ended interview schedule and a desire to catch 'authentic' experience. They fail to recognize what they have in common with media interviewers (whose perennial question is 'How do you/does it feel?') and with tourists (who, in their search for the 'authentic' or 'different', invariably end up with more of the same); they also totally fail to recognize the problematic analytic status of interview data which is never simply raw but is both situated and textual (Mishler 1986). Such analytic issues, moreover, are not even touched upon in the elegant methodological 'remedies' of survey research (Silverman 1985, pp. 156–177).

If we reduce micro-sociology to the naive interview, we lose much of the thrust of the tradition from which it emerged. You only have to look at interactionist work from the Chicago School in the 1930's and 1940's to see the presence of a much more vital approach. Using their eyes as well as listening to what people were saying, these sociologists invariably located 'consciousness' in specific patterns of social organization. For instance, Whyte (1949) showed how the behaviour of barmen and waitresses was a response to the imperatives of status and the organization of work routines. The experiences of such staff needed to be contexted by knowledge of such features and by precise observation of the territorial organization of restaurants.

This issue of the situated nature of people's accounts directly arose in a study of a pediatric cardiology unit (Silverman 1987). When we interviewed parents after their child's first outpatient visit, most said that they had a problem taking anything in. Partly, it appeared, this was due to the way in which they had been very apprehensive of the possible diagnosis, hearing the doctor's words as containing a possible life or death sentence on their child. They also reported that one of their major problems in concentrating properly was caused by the crowded room in which the consultation took place—as a teaching hospital, several other doctors as well as nurses and researchers were present.

Although we could empathize with the parents' response, we thought it worthwhile to go back to our tapes of the encounters they were discussing. For want of any better measure, we counted the number of questions parents asked at each consultation and compared it to the numbers present in the room at the time. As it turned out, these admittedly crude figures suggested the *reverse* to what parents had told us: when there were five or more non-family members present, parents asked *more* questions than when there were less than five non-family members in the room.

As is often the case after such a counter-intuitive finding, we found quite a simple explanation. Perhaps when the senior doctor broke off the consultation to ask questions of the junior doctors present, quite unintentionally, this created a space for parents to think about what they had been told so far and to formulate their questions without being 'on stage' in direct eye contact with the doctor. This explanation was supported in another unit where parents also asked many questions but where this kind of social space was created not by teaching sessions (parents saw the doctor on his own) but by a repeat consultation the same day after the doctor had seen an ECG (Silverman 1987, pp. 91–94).

This took us back to our interview material with the parents. We were not prepared to treat what they had told us ironically, i.e., as self-evidently mistaken in the light of the 'objective' data. Such simple-minded 'triangulation' of data fails to do justice to the embedded, situated nature of accounts (Silverman 1985, pp. 105–106). Instead, we came to see parents' accounts as 'moral tales' in which respondents struggled to present their actions in the context of moral versions of responsible parenthood in a situation where (because of the risks to life and the high-technology means of diagnosis and treatment) the dice were loaded against them (Baruch 1982, Voysey 1975).

Parents' reference to the problems of the crowded consultation room were now treated not as an explanation of their behavior at the time but as a situated appeal to the rationality and moral appropriateness of that behavior. Similarly, in a study of fifty British general practice consultations, Webb and Stimson (1976) noted how the subsequent accounts of patients took on a dramatic quality in which the researcher was encouraged to empathize with the patient's difficulties in the consultation. A story was told in which a highly rational patient had behaved actively and sensibly. By contrast, doctors were routinely portrayed as acting insensitively or with poor judgment. By telling 'atrocity stories', Webb and Stimson suggest that patients were able to give vent to thoughts which had gone unvoiced at the time of the consultation, to redress a real or perceived inequality between doctor and patient and to highlight the teller's own rationality. Equally, atrocity stories have a dramatic form which captures the hearer's attention—a point that field researchers become aware of when asked to give brief accounts of their findings.

In a certain sense, once again we see how field researchers have come back, in a full circle, to a Durkheimian position. Like Durkheim, Stimson and Webb are rejecting the assumption that lay accounts can do the work of sociological explanations. Neither wants to take the actor's point of view as an explanation because this would be to equate commonsense with sociology—a recipe for the lazy field researcher. Only when such a researcher moves beyond the gaze of the tourist, bemused with a sense of bizarre cultural practices ('Goodness, you do things differently here'), do the interesting analytic questions begin.

A parallel issue arose in a study by Gilbert and Mulkay (1983) of scientists' accounts of scientific practice. As they point out, one way of hearing what scientists say is as hard data which bears on debates in the philosophy of science about the character of scientific practice. It is then tempting to treat such accounts as 'inside' evidence ('from the horse's mouth', as it were) about whether scientists are actually influenced by paradigms and community affiliations more than by sober attempts to refute possible explanations.

Confusingly, Gilbert and Mulkay's scientists used both quasi-Kuhnian and quasi-Popperian explanations of scientific practice. Understandably, however, they were much keener to invoke the Popperian ('sober refutation') account of how they worked and the Kuhnian ('community context') account of how certain other scientists worked. Were these accounts to be treated as a direct insight into how scientists do their work or how they experience things in the laboratory?

Not at all, at least in any direct sense. What this interview data gave Gilbert and Mulkay was access to the vocabularies that scientists use. These vocabularies were located in two very different discourses: a 'contingent' discourse, in which people were very much influenced by political considerations, such as institutional affiliations, ability or inability to get big research contracts, etc., an 'empiricist' discourse, where science was a response to data 'out there' in the world viewed by appropriate methods.

Neither discourse conveyed the 'true' sense of science—there is no more an essential form of scientific practice than there is a single reality standing behind 'atrocity stories'. Everything is situated in particular contexts. Scientists, for instance, Gilbert and Mulkay note, are much more likely to use a 'contingent' discourse in a discussion at a bar than in a scientific paper. So the issue ceases to be 'What is science?' and becomes 'How is a particular scientific discourse invoked? When is it invoked? How does it stand in relation to other discourses?'.

## RULE 3: RECOGNIZE THAT THE PHENOMENON ALWAYS ESCAPES

Webb and Stimson, like Gilbert and Mulkay, remind us of the occasioned, situated nature of lay and sociological seeing, saying, and doing. In this sense, the link with Durkheim is clearly broken. Given patients' and scientists' skillful invocation of discourses in appropriate social contexts, Durkheim's faith in a stable reality, separate from people's seeing, saying, and doing, is misplaced. Clearly, the botanist classifying a plant is engaged in a less problematic activity than an anthropologist classifying a tribe.

In both the studies I have been discussing, the researchers disabused us of our commonsense assumptions about the stable realities of particular collectivities. So patients, conceived as a stable phenomenon, escaped the Webb and Stimson study and scientists, treated as a collectivity having stable goals and practices, also escaped in Gilbert and Mulkay's work.

A recent paper by Woolgar (1985), in the main concerned with 'artificial intelligence,' notes how participants themselves may be reluctant to treat their own activities as instances of particular idealized phenomena. Like Gilbert and Mulkay, Woolgar was interested in the sociology of science. Yet, he reports, that, when he tried to get access to laboratories to study scientists at work, each laboratory team would uniformly respond that, if he was interested in science, this really was not the best place to investigate it. For whatever reason, what was going on in this laboratory did not really fit what scientific work really should be. On the other hand, the work being done at some other place was much more truly scientific. Curiously, Woolgar tells us that he has yet to find a laboratory where people are prepared to accept that whatever they do is 'real' science. He was perpetually being referred to some other site as the home of 'hard' science.

Like 'science', Woolgar also found that 'artificial intelligence', conceived as an indisputably 'real' phenomenon, was also perceived to be 'elsewhere'. As each new test of what might constitute 'real' AI appeared, grounds were cited to find it inadequate. The famous Turing test is now largely rejected because even if a hearer cannot tell the difference between human reasoning and AI, a machine may only be 'simulating intelligence' without being 'intelligent'. Even machines which successfully switch off televisions during commercials will not be recognized as an example of AI since, it is held, this is a response to changes in the broadcast signal rather than in program content. Hence the search for 'genuine' AI, Woolgar argues, has generated a seemingly endless research program in which the phenomenon always escapes.

These kind of studies point to the way in which idealized conceptions of phenomena become like a will-o'-the-wisp on the basis of systematic field research, dissolving into sets of practice embedded in particular milieux. Nowhere is this clearer than in the field of studies of the 'family'. As Gubrium and Holstein (1987) note, researchers have unnecessarily worried about getting 'authentic' reports of family life given the privacy of the household. But this implies an idealized reality—as if there were some authentic site of family life which could be isolated and put under the researcher's microscope. Instead, discourses of family life are applied in varying ways in a range of contexts, many of which, like courts of law, clinics and radio call-in programs are public and readily available for research investigation.

In pediatric clinics, doctors, parents and sometimes children themselves invoke notions of the family as a way of accounting what they and others might be up to. For instance, a pediatric cardiologist may appeal to a child's 'enjoyment of life' as a way of persuading parents to avoid risky surgery on a Down's Syndrome child with a heart defect, or a mother may invoke her respect for her teenage diabetic child's need for autonomy in response to a perceived charge of being a 'nagging' parent (Silverman 1987, Chs. 6 and 10). Above all, within what Strong (1979) refers to as the 'bureaucratic' format of British National Health Service medicine, all parents but especially mothers are to be treated as having an exemplary character. However, as Strong shows, outside the consultation, for instance in discussions with medical colleagues, moral character work is routinely enacted.

If the family is present wherever it is invoked, then the worry of qualitative researchers about observing 'intimate' family life looks to be misplaced. Their assumption that the family has an essential reality looks more like a commonsense way of approaching the phenomenon with little analytic basis. Even on this basis, however, such researchers have failed fully to appreciate the richness of commonsense practices.

Finding the family is no problem at all for laypeople. In our everyday life, we can always locate and understand 'real' families by using the documentary method of interpretation (Garfinkel 1967) to search beneath appearance to locate the true reality. In this regard, think of how social workers or lawyers in juvenile or divorce courts 'discover' the essential features of a particular family. Yet, for sociologists, how we invoke the family, when we invoke the family, and where we invoke the family become central analytic concerns. Because we cannot assume, as laypeople must, that families are 'available' for analysis in some kind of unexplicated way, the 'family', conceived as a self-evident phenomenon, always escapes.

## RULE 4: AVOID CHOOSING BETWEEN ALL POLAR OPPOSITIONS

The philosopher of science T.S. Kuhn (1970) has described sociology as preparadigmatic and, hence, in a state of competing paradigms. As I have already implied, the problem is that this has generated a whole series of sociology courses which pose different sociological approaches in terms of either/or questions.

Such courses are much appreciated by students. They learn about the paradigmatic oppositions in question, choose A rather than B and report back, like a parrot, all the advantages of A and the drawbacks of B. It is hardly surprising that such courses produce very little evidence that students have ever thought about anything—even their choice of A is likely to be based on their teacher's implicit or explicit preferences. This may, in part, explain why so many undergraduate sociology courses actually provide a learned incapacity to go out and do research. Learning about rival 'armed camps' in no way allows you to confront field data. In the field, material is much more messy than the different camps would suggest. Perhaps there is something to be learned from both sides, or, more constructively, perhaps we start to ask interesting questions when we reject the polarities that such a course markets?

We have already seen how the opposition between 'structure' and 'meaning' is not very instructive in a range of settings from families, tribes, laboratories and coroners' courts. As I will argue shortly, the same might be said about the analysis of interview data. Does this tell us simply about people's experiences (and thus about 'meaning')? Or are interview responses instances of collective phenomena, such as moral forms and structures of narration? In this case, as Durkheimian 'collective representations', interview data tell us about structures. So the student question: are you concerned with structures or meanings, is not very helpful to the field researcher who, necessarily is concerned with both. Here, as elsewhere, attempts to place fieldwork on one side or another of competing paradigms are misplaced.

Another area in which the 'purity' of particular models may be invoked arises in the decision to use or to avoid quantitative methods. In British sociology of the 1970's, the word got about that no good qualitative researcher would want to dirty her or his hands with any techniques of quantification (Silverman in press). Yet although many of the criticisms of survey methods in the 1960's were well placed (Cicourel 1964), so were some of the survey researchers' suspicions about field research. We are all familiar with the case study report that advances its argument on the basis of 'a good example of this is . . .' or 'X's comment was typical'. Of course, these are 'good' or 'typical' examples because the researcher has selected them to underline the argument.

Just choosing examples of phenomena stands in the way of both rigorous and lateral thinking. Yet, if you are trying to get some feel about your data as a whole or are actively pursuing deviant cases, it may sometimes be very useful to use certain quantitative measures, however crude they may be. For instance, in the study of a pediatric cardiology clinic mentioned earlier, I observed that consultations with parents of Down's Syndrome children seemed very different in character to other consultations with parents of children who also had suspected congenital heart disease. To pursue my hunch, I examined closely the form of the doctor's initial 'elicitation' question. Simple counting then revealed very nicely the way in which the usual question ('A well child?' or 'Is s/he well?') was transformed ('How is s/he?') to parents of children with an additional handicap present (Silverman 1981).

This apparently trivial finding proved to be crucial in an analysis of how primarily 'social' rather than 'clinical' categories came to be central to the formulation of Down's Syndrome children with heart disease and the way this tied in to the doctor's policy of surgical nonintervention. Moreover, not only was I happier because I could account for all my data, instead of using selected examples, but I was able to do this by counting in terms of the language used by the participants rather than imposing my own categories on to the data prior to counting.

Categories abstracted from the business of daily life usually impose a set of polarities (or continua) with an unknown relationship to that business. One obvious example of such *a priori* polarized theorising is in the abstract models of decision-making found in the polarity of rational/non-rational action. As Anderson, Hughes and Sharrock point out, such models, in their classical Weberian or social-psychological (e.g. Cyert and March 1963) form, fail to address 'the essentially socially organized character of the discovery, recognition, determination and solution of problems' (Anderson *et al* 1987, p. 144).

Using materials from audio-tapes of business negotiations, Anderson *et al.* show that the parties' focus on problems and provision of candidate solutions is embedded in how they play with the sequencing rules of natural language. For instance, a transition point to a next speaker or a next topic may not be accepted and so a party can avoid a commitment until more is known of the other party's game. Equally, requests for clarification both buy time and give the ball back to the first speaker in a three-part sequence (clarification request / clarification / response).

In turn, these sequencing rules are enacted in the context of a set of 'business' relevances which, as Anderson *et al.* show, depend on the display of 'competitiveness' coupled with a form of 'urbane affability' that takes for granted the reciprocity of personal and commercial relevances. Anderson *et al.'s* analysis reveals 'what adopting a businesslike attitude to the solution of routine problems means as an observable, interactional feature of daily life' (*ibid.* p. 155). In doing so, it emphasizes rules three and four: not only does it reject prior polar oppositions (say, between rational and non-rational elements in negotiation) but it also shows how 'business' disappears as a single phenomenon. As Anderson *et al.* note, 'business' life is interwoven with social life: the purely 'rational' cannot be filtered out from the social.

## RULE 5: NEVER APPEAL TO A SINGLE ELEMENT AS AN EXPLANATION

A further parallel between qualitative and quantitative work is that multi-factorial explanation is likely to be more satisfactory than explanations which appeal to what I have called a 'single element'. Just because one is doing a case study, limited to a particular set of interactions, does not mean that one cannot examine how particular sayings and doings are embedded in particular patterns of social organization. Despite their very different theoretical frameworks, this is the distinctive quality shared by, say, Whyte (1949) and Moerman (1974). The classic case of this is found in Mary Douglas' (1975) work on a central African tribe, the Lele.

Douglas noticed that an anteater, that Western zoologists call a 'pangolin', was very important to the Lele's ritual life. For the Lele, the pangolin was both a cult animal and an anomaly. It was perceived to have both animal and human characteristics—for instance, it tended only to have one offspring at a time, unlike most other animals. It also did not readily fit into the Lele's classification of land and water creatures, spending some of its time on land and in the water. Curiously, among animals that were hunted, the pangolin seemed to the Lele to be unique in not trying to escape but almost offering itself up to its hunter.

Fortunately, Douglas resisted what I called earlier the 'tourist' response, moving beyond curiosity to systematic questioning. Many other groups who perceive anomalous entities in their environment reject them out of hand. As Douglas suggests, you can see the logic of this response. To take an anomalous entity seriously, might cast doubt on the 'natural' status of your group's system of classification.

The classic example of the rejection of anomaly is found in the Old Testament. Douglas points out that the reason why the pig is unclean, according to the Old Testament, is that it is anomalous. It has a cloven hoof which, following the classification system, makes it clean but is does not chew the cud—which makes it dirty. So it turns out that the pig is particularly unclean precisely because it is anomalous. Similarly, the teachings on inter-marriage work in relation to anomaly. Although you are not expected to marry somebody of another tribe, to marry the offspring of a marriage between a member of your tribe and an outsider is even more frowned upon. In both examples, anomaly is shunned.

However, the Lele are an exception. What this suggests to Douglas is that there may be no *universal* propensity to frown upon anomaly because it de-naturalises conventions of classification. If there is variability from community to community, then this must say something about their social organization.

Sure enough, there is something special about the Lele's social life. Their experience of relations with other tribes has been very successful. They exchange goods with them and have little experience of war. Now what is it to relate with other tribes? What does that involve doing? It involves crossing a frontier or a boundary. But what do anomalous entities do? They cut across boundaries. Here is the answer to the puzzle.

Douglas is suggesting that the Lele's response to anomaly derives from experiences grounded in their social organization. They perceive the pangolin favorably because it cuts across boundaries just as they themselves do. Conversely, the Ancient Israelites regard anomalies unfavorably because their own experience of going across boundaries was profoundly unfavourable. The Old Testament reads as a series of disastrous exchanges between the Israelites and other tribes. Perhaps the similar experience of British sociologists with other groups (particularly the State bureaucracy and the media) explains why the subject in Britain for many years located itself around warring 'armed camps'? And again, the less apparent doctrinal battles in North American sociology, suggests a more peaceful relation with the outside world.

Douglas' study of the Lele exemplifies the need to locate how individual elements are embedded in forms of social organization. In her case, this is done in an explicitly Durkheimian manner which sees behavior as the expression of a 'society' which is the 'hidden hand' constraining and forming human action. Alternatively, Atkinson's and Anderson *et al.*'s work indicates how one can follow Rule 5 and avoid single element explanations without treating social organisation as a purely external force. In this case, people cease to be 'cultural dopes' (Garfinkel 1967) and skillfully reproduce the moral order.

Durkheim's contemporary, Saussure, provides a message appropriate to both these traditions when he reminds us that no meaning ever resides in a single term. This is an injunction equally relevant to Douglas' structural anthropology as to Atkinson's (1982) interest in the sequencing of conversation. So we can take Saussure's message out of context from the kind of linguistics that Saussure himself was doing and use it as a very general methodological principle in sociology. What we are concerned with, as Saussure (1974) showed us, is not individual elements but their relations. As Saussure points out, these relations may be organized in terms of paradigmatic oppositions (Ancient Israelites, British sociologists, etc.) or in terms of systems of relations which are organized in terms of what precedes and what follows each item.

An example that Saussure himself gives shows the importance of organization and sequence in social phenomena. The 8:15 train from Zurich to Geneva remains the 8:15 train even if it does not depart till 8:45. The meaning of the train—its identity—only arises within the paradigmatic and syntagmatic (or rational) demands of the timetable.[3] Let me illustrate the significance of this with an example drawn from a further case-study. Dingwall and Murray (1983) were concerned with how medical staff responded to patients presenting themselves at a British 'casualty' or emergency hospital unit. They note that Jeffery (1979) suggests that patients are typified by staff as either 'good' and 'interesting' or 'bad' and 'rubbish'. The former might be patients who tested the specialized competences of staff; the latter might be patients with trivial complaints and/or responsible for their own illnesses.

Dingwall and Murray argue that Jeffery's polarity inadequately spells out the system of relations in which these labels are embedded. They note, for instance, that children often have trivial complaints for which they themselves are responsible and yet are not usually defined by staff as 'bad' or 'rubbish' patients. Drawing upon McHugh's (1970) treatment of deviance, Dingwall and Murray suggest that casualty staff assign such labels only after assessing whether the patient is 'theoretic' (i.e., perceived to be able to make choices) and the situation is 'conventional' (i.e., that if offered a choice for the patient to make). On this basis, Dingwall and Murray offer a $2\times2$ table which reveals the staff's decision-making rules. So, in a conventional situation, a patient who does not co-operate with staff is normally defined as 'bad'. Children, however, because they may be perceived as pre-theoretic, will not find that such behavior leads to this label. Similarly, in a situation offering no choice (i.e. 'non-conventional'), patients will be labelled as 'inappropriate' ('theoretic') or 'naive' ('pre-theoretic').

Indeed, as Dingwall and Murray show, the attribution of deviance to a patient arises only with one of three 'frames' that shape the perceived clinical priority of a presenting patient. This 'special' frame sorts out patients according to their moral worth (e.g., as 'bad', 'inappropriate', 'naive' or simply a child). It co-exists with a clinical frame, according to which patients are judged simply by whether they constitute an 'interesting' case, and a bureaucratic frame in terms of which 'routine' patients, without perceived deviant characteristics or special clinical interest, get routine treatment.

Just as Douglas discovered that the pangolin's anomalous characteristics were the key to unravelling the social organization of the Lele, so the anomaly created by children who break rules and yet are not treated as 'bad' patients' shows the complexity of decision-making in a hospital setting. In both cases, the importance is revealed of avoiding single element explanation and of focussing upon the processes through which the relations between elements are articulated.

## RULE 6: UNDERSTAND THE CULTURAL FORMS THROUGH WHICH 'TRUTHS' ARE ACCOMPLISHED

I referred earlier to my preference for working with 'naturally occurring' data. This seems logical if your interest is in the practices through which phenomena like 'families', 'tribes' or 'laboratory science' are constructed or assembled. Despite this, however, many ethnographers move relatively easily between observational data and data that are an artifact of a research setting, usually an interview. Elsewhere, I have pointed out the difficulties this can create, especially where 'triangulation' is used to compare findings from different settings and to assemble the context-free 'truth' (Silverman 1985 pp. 105–106).

However, there are two dangers in pushing this argument very far. First, we can become smug about the status of 'naturally-occurring' data. I have already referred to Hammersley and Atkinson's (1983) observation that there are no 'pure' data; all data are mediated by our practices of reasoning as well as those of participants. So to assume that 'naturally-occurring' data are unmediated data is, self-evidently, a fiction of the same kind as put about by survey researchers who argue that techniques and controls suffice to produce data that are not an artifact of the research setting.

The second danger implicit in the purist response is that it can blind us to the really powerful, compelling nature of interview accounts. Consider, for instance, the striking 'atrocity stories' told by mothers of handicapped children (Baruch 1981) and their appeal to listeners to hear them as 'coping splendidly' (Voysey 1975). There are powerful cultural forms at work in such 'moral tales'. Consequently, the last thing you want to do is to treat them as simple statements of events to be triangulated with other people's accounts or observations. For the fact is that, as societal members, we can see the 'good sense' of such tales. In many respects, an 'atrocity story' is no less powerful because there is no corroborating evidence. It reveals the 'moral work' involved in displays of 'responsible' parenthood, particularly, as in Baruch's study, where that responsibility had to be demonstrated in the context of potentially unintelligible, high-technology medicine.

Such a perspective derives from two very different but equally neglected sources. Wittgenstein (1968) implies that we should not treat people's utterances as standing for their unmediated inner experiences. This is particularly striking in his famous discussion of statements about pain (paragraphs 244–246, 448–449). Wittgenstein asks us what does it mean when I say "I'm in pain"? And why is it that we feel unable to deny this assertion when someone makes it? In our community, it seems, we talk about pain as if it belongs to individuals. So, in making such an assertion or seeing its good sense, we reveal what our community takes for granted about private experience (but not private experience itself). So Wittgenstein makes the point that we are talking about what is appropriate to a communal language-game. Just as I have argued that 'the phenomenon always escapes', so, for Wittgenstein, there is no direct route to what we might choose to call 'inner experience'.

A second source for understanding the public sense of interview accounts is to be found in Mills' (1940) discussion of 'vocabularies of motive'. Mills reminds us that, for sociological purposes, nothing lies 'behind' people's accounts. So when people describe their own or other's motives, the appropriate questions to ask are: when does such talk get done, what motives are available and what does 'motive talk' do—conceived as a performative (Austin 1962)? As Gilbert and Mulkay (1983) were to argue many years later:

> the goal of the analyst no longer parallels that of the participants, who are concerned to find out what they and others did or thought but becomes that of reflecting upon the patterned character of participants' portrayals of action (p. 24).

Conceived in this way, interview data becomes a fascinating resource for analytically sensitive case study work. With a little lateral thinking, it is also possible to derive from this approach practical as well as analytic insights. For instance, given the cultural compunction for parents, particularly mothers, to display their 'responsible parenthood', can this be incorporated into medical consultations?

In the study of the pediatric cardiology unit (PCU), it would have been tempting to follow other researchers (e.g. Byrne and Long 1976) and to suggest that the problems derive from doctors' inadequate communication skills. Our experience, however, was that the constraints of the setting and of the task at hand (speedy diagnosis and treatment) meant that the first outpatients' clinic had no space for many of these moral issues and that, in any event, many parents needed time to come to terms with what they were being told. If time was allowed to pass (when, for instance, parents had faced the questions of other anxious relatives and had consulted books or the family physician) and the family was invited to revisit the hospital, things might turn out differently. Such a clinic was indeed established at the PCU and the constraints further altered by informing parents in advance that their child would not be examined this time. An evaluation study indicated that, in the eyes of the participants, this was a successful innovation (Silverman 1987, pp. 86–103). Yet at no point had we sought to teach doctors' communication skills. So the sociological truism: change the constraints of the setting and people will behave differently, had paid off in ways that we had not foreseen—people responded to the new setting by innovating themselves, parents bringing their children along to see the playroom and to discover that the ward was not such a frightening place after all.

## CONCLUSION

Although the discussion of the policy input of one qualitative study has exceeded my brief, I hope it has introduced a positive note into these observations. Reviewing my first five rules, I could not fail to notice the uniformly *negative* form in which they are couched—as if research were all a matter of what you must not do. Of course, I intended throughout to convey a sense of the good things that research can do. I tried to convey this in the examples of successful case studies and, above all, in my implicit appeal to lateral thinking. If the world is divided into two sorts of people: those who make such a statement and those who don't, then I am firmly with the latter group. Perhaps, as Douglas implies, we have something to learn from the Lele.

Part of what we might learn is living with uncertainty. Curiously, the critics of such apparently disparate theorists as Garfinkel, Derrida and Foucault have one argument in common. If everything is discursive, how can we find and secure ground from which to speak? Are we not inevitably led to an infinite regress where ultimate truths are unavailable (see Bury 1986)?

Three responses suggest themselves. First, isn't it a little surprising that such possibilities should be found threatening when the natural sciences, particularly quantum physics, seem to live with them frequently and even ingeniously? Second, instead of throwing up our hands in horror, why not marvel at the elegant solutions that societal members use to remedy this context-boundedness? For practical actions, the regress becomes no problem at all. Finally, like members, why not use practical solutions to practical problems? For instance, count where it makes sense to count, use the constant comparative method where appropriate and so on.

The worse thing that we can imply to our students is that in this post-Romantic, post-Modern age, anything goes. The trick is to produce intelligent, disciplined work on the very edge of the abyss.

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## NOTES

1. For other examples see Prior (1987) on a sociology of the mortuary and Bloor on medical decision-making (1976).
2. See also Hammersley and Atkinson's (1983) critique of 'naturalism' among ethnographers and Silverman's rejection of the technique of 'triangulation' (Silverman 1985).
3. The work of Pierre Bourdieu (1977) and Ernesto Laclau (1977) and (1981) bears close scrutiny by ethnographers interested in the role of time and discursive practice in classification.

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