Context and Method in Qualitative Research

The Logics of Qualitative Research

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The Logics of Qualitative Research

In the Sociology Department where I work, we offer a graduate degree in Qualitative Research. This title was chosen to give a flavour of a degree largely taught by ethnographers who themselves mainly use qualitative methods. However, I feel that such a title may attract students more in terms of what it promises to avoid rather than by reason of what it offers.

‘Qualitative research’ seems to promise that we will avoid or downplay statistical techniques and the mechanics of the kind of quantitative methods used in, say, survey research or epidemiology. In fact, this is indeed the case - although we expect students to take a course in survey methods and to be aware of how the issues of validity and reliability so often posed by quantitative researchers are relevant to any kind of research (albeit in varying ways).

The danger in the title, however, is that it seems to assume a fixed preference or predefined evaluation of what is a good (i.e. qualitative) and bad (i.e. quantitative) research methodology. In fact, the choice between different research methods can depend upon quite pragmatic matters. For instance, if you want to discover how people intend to vote, then a quantitative method, like a social survey, may seem the most appropriate choice. On the other hand, if you are concerned with exploring people's wider perceptions or everyday behaviour, then qualitative methods may be favoured.

Other, less practical, questions arise when you choose between ‘qualitative’ and ‘quantitative’ methods. The researcher has to bear in mind that these methods are often evaluated differently. Table 1.1, which gives the terms used by speakers at a conference on research methods in the late 1970s, shows how imprecise evaluative considerations come into play when researchers describe qualitative and quantitative methods. In turn, these evaluative issues are linked to attempts to define mutually opposed ‘schools’ of social science.

In the 1970s, it was common to criticize what was called ‘positivism’ (Filmer et al. 1972). Unfortunately, ‘positivism’ is a very slippery and emotive term. Not only is it difficult

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to define but there are very few quantitative **[p. 13 ↓]** researchers who would accept the label (Marsh 1982). Instead, most quantitative researchers would argue that they do not aim to produce a science of laws (like physics) but simply to produce a set of cumulative, theoretically defined generalizations deriving from the critical sifting of data. So, it became increasingly clear that 'positivists' were made of straw since very few researchers could be found who equated the social and natural worlds or who believed that research was properly theory-free.[1] Moreover, even the picture of the natural sciences as indelibly 'positivist' may be wide of the mark. For instance, post-Newtonian physicists may look askance at the suggestion that there is a stable point from which to derive universal laws.

Qualitative	Quantitative
soft	hard
flexible	fixed
subjective	objective
political	value-free
case study	survey
speculative	hypothesis-testing
grounded	abstract
Source: Halfpenny (1979: 799)	

Given the nebulous character of 'positivism', quantitative research seemed to offer an easier target. The critique of purely quantitative research has a long history, beginning in the 1950s. C. Wright Mills (1959) attacked the atheoretical character of much quantitative research, which he called 'abstracted empiricism'. Blumer (1969) noted how the attempt to establish correlations between variables depended upon a lack of attention to how these variables are defined by the people being studied. Finally, Cicourel (1964), influenced by Schutz and Garfinkel, drew attention to how the choice of a purely mathematical logic can neglect the commonsense reasoning used by *both* participants and researchers. Instead of attending to the social construction of meaning,

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quantitative research, it is claimed, uses a set of *ad hoc* procedures to define, count and analyse its variables.[2]

Such critiques can be seen to lay the foundation for a school of interpretive social science with a set of emphases different from more quantitatively oriented work. Unfortunately, these foundations were, to say the least, very shaky. In the first place, just as it is difficult to find a positivist 'enemy', the critics had established a 'straw man' to represent quantitative research. Quantitative researchers are rarely 'dopes'. So, for instance, epidemiologists and criminologists are only too aware of the problematic character of what gets recorded as, say, 'cause of death' or a 'criminal offence' (Hindess 1973). Equally, good quantitative researchers are highly conscious of the problems involved in interpreting statistical correlations in relation to what the variables involved 'mean' to me participants (Marsh 1982). In these circumstances, it **[p. 14 ↓]** might be better to avoid the term 'critique' and instead talk about *some* complaints made about *some* quantitative research.

Secondly, it is inaccurate to assume that quantitative and qualitative research are polar opposites. In *Interpreting Qualitative Data* (Silverman 1993; henceforth *IQD*), I sought to undermine the assumption that these two paradigms are incommensurable or, indeed, that they offer any worthwhile description of the major alternative directions of sociological research. For, of course, there are no principled grounds to be either qualitative or quantitative in approach. It all depends upon what you are trying to do. Indeed, often one will want to combine both approaches. For example, I recently tested a hypothesis by using simple tabulations on fifty-odd examples of HIV counselling 'advice packages' whose form can readily be identified and counted (Silverman 1996). This means that, if we want to understand the logic behind qualitative research, we need to recognize its points of continuity with, as well as difference from, more quantitative or 'positivistic' studies.

Thirdly, just as quantitative researchers would resist the charge that they are all 'positivists' (Marsh 1982), there is no agreed doctrine underlying all qualitative social research. Instead, there are many 'isms' that appear to lie behind qualitative methods: for example, interactionism, feminism, postmodernism and ethnomethodology (see Denzin and Lincoln 1994). This means that qualitative research can cover a vast

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range of research styles and can be used in studies without any conscious analytical perspective. For instance, in British market-research circles, ‘qualitative’ research is the latest fashion. It is seen to provide ‘in-depth’ material which is believed to be absent from survey research data. Above all, it is relatively cheap. However, all that is meant by qualitative research is open-ended interviews or ‘panel’ studies lacking a clear, analytical basis in social theory. As I argue in *IQD,* this makes any attempt to characterize qualitative research as a whole open to severe criticism (see also Atkinson 1995b). For instance, while Hammersley (1992a: 160) attempts to identify qualitative research with hypothesis-generation, more and more non-quantitative work happily *tests* prior hypotheses (for example, Silverman 1996; Strong 1979). Again, while many qualitative researchers would agree with Bryman (1988: 61) that their aim is to ‘see through the eyes’ of subjects, others would criticize such an aim as ‘subjectivist’ and even ‘journalistic’.

We should learn from the now empty debate about ‘positivism’ that such analytical differences cannot be resolved by choosing sides from spurious polarities (for example, structure and meaning; quality and quantity). For instance, it is sometimes maintained that, while positivists are concerned with ‘society’, interpretive social science focuses on the ‘individual’. But rather than choose one side of this polarity, we might look at how the individual-society opposition is actually used in everyday life.

As I suggested in *IQD,* we can find a striking example of how we all rely on the distinction between ‘objective’ and ‘subjective’ realities in Sacks's (1992) account of children's socialization. Sacks shows that participants **[p. 15 ↓]** search for underlying ‘social structures’ and try to understand the meaning of ‘experiences’. What, then, is the status of social science research which itself chooses to focus on one or other of these realities? Such a question suggests that we need to look twice at the unthinking identification of the open-ended interview as the ‘gold standard’ of qualitative research. In *IQD,* I argue that some such interviewers depend upon a nineteenth-century, romantic ontology based on the primacy of ‘experience’ and are apparently ignorant of the linguistic turn explicit in de Saussure and the later Wittgenstein. This ‘turn’ reveals the ‘public’ character of apparently ‘private’ knowledge. As Sacks tellingly remarked, if we are really interested in the contents of people's heads, we should choose to be brain surgeons rather than social scientists.

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In interviews, as Heritage (1984: 236) puts it, the mistake is to treat the verbal formulations of subjects: ‘as an appropriate substitute for the observation of actual behaviour’. Unlike the research interview, observational data are of value precisely because they focus on naturally occurring activities. What do we lose if we base our analysis purely on such data? The first thing to bear in mind is that, to become data, observations have to be recorded in some way; for example, through field notes or pre-coded schedules. Moreover, while observational data should properly include descriptions of non-verbal aspects of social interaction - what Stimson (1986) calls ‘the sociology of place and space’ - much of what we observe in formal and informal settings will inevitably consist of conversations. However sophisticated are our field notes, they cannot offer the detail found in transcripts of recorded talk.

Drew and Heritage (1992) show how this has a direct impact on the kind of data we think are relevant. Most field researchers use such data as questionnaires, interviews, observation and diaries. They thus attempt ‘to get inside the “black box” of social institutions to gain access to their interior processes and practices’ (Drew and Heritage 1992: 5). However, such studies may suffer from two problems:

- The assumption of a stable reality or context (e.g. the ‘organization’) to which people respond.
- The gap between beliefs and action and between what people say and what they do (Gilbert and Mulkay 1983; Stimson and Webb 1975).

So, even though observational data can contribute a great deal to understanding how institutions function, its observations may be based upon a taken-for-granted version of the setting in question. For instance, Strong's (1979) powerful analysis of the ‘ceremonial order’ of doctor-parent consultations undoubtedly depends, in part, upon our readiness to read his data extracts in the context of our shared knowledge of what medical consultations look like.

Consequently, ethnographic work can only take us so far. It is able to show us how people respond to particular settings. It is unable to answer basic questions about how people are constituting that setting through their talk. **[p. 16 ↓]** Detailed transcripts of conversation may thus overcome the tendency of transcribers to ‘tidy up’ the ‘messy’ features of natural conversation. However, without an analytical basis, even detailed

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transcription can be merely an empty technique. Thus we need to ask: what sort of features are we searching for in our transcripts and what approach lies behind this search?

To suggest how I would answer these questions, the rest of this chapter is devoted to two practical illustrations from research based on the recording and transcription of naturally occurring interactions. The first study I will consider is Lucy Suchman's (1987) work on human-computer interaction. Like conversation analysis (CA), I will show how Suchman's focus on the sequential organization of verbal and non-verbal interaction can give us a secure grip on the precise mechanics of organizational interaction. The second study by Clavarino et al. (1995) is concerned with doctor-patient interviews in the cancer department of an Australian hospital. It follows Suchman in using recordings as data but its particular concern is with addressing how detailed transcription can increase the reliability of qualitative data and the validity of its data analysis.

However, it should be stressed at the outset that, in selecting two studies based upon recordings of naturally occurring data, I am *not* suggesting that such a database, coupled with a CA method, is the *only* way to proceed in qualitative research. First, such a firm position would undercut all that I have said earlier about the risks of always choosing sides between the polarities that tend to organize our thoughts. Secondly, much will depend upon what we are trying to find out and what kind of data we are able to obtain.

So I have not selected these studies in order to recommend a panacea. Indeed, my own research has used ethnographic as well as CA methods in different cases. I simply intend to show the value of careful transcription in qualitative studies which choose to focus on what participants are doing. This value will, I hope, be seen in the way in which such CA-orientated work can show how people create contexts for their interactions with each other and with objects (like photocopying machines).

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People's doings: communicating with the photocopier

Suchman (1987) is concerned with the interaction between people and machines. She takes the example of a computer-based system attached to a photocopier and intended to instruct the user in the photocopier's operation. Her data derive from videos of four sessions, each of more than an hour, involving first-time users of this ‘expert system’. In each session, two novices worked together in pairs. She is particularly concerned with how interactional ‘troubles’ arise and are resolved.

In Suchman's study the computer used in the photocopier: ‘project[s] the course of the user's actions as the enactment of a *plan* for doing the job, and then use[s] the presumed plan as the relevant context for the action's interpretation’ (1987: 99; my italic). However, the problem is that ‘plans’ **[p. 17 ↓]** have a different status for computers and users: ‘While the [design] plan directly *determines* the system's behaviour, the user is required to *find* the plan, as the prescriptive and descriptive significance of a series of procedural instructions’ (1987: 101; my italic). This is shown in Suchman's (1987: 107) model of how the computer is supposed to ‘instruct’ a user:

- *Machine presents instruction:* User reads instruction, interprets referents and action descriptions
- *User takes action:* Design assumes that the action means that the user has understood the instruction
- *Machine presents next instruction*

Despite this rational model, much of the user's behaviour is unavailable to the system, for instance: ‘the actual work of locating referents and interpreting action descriptions’ (p. 107). This means that if an instruction is misunderstood by the user, the error will go unnoticed.

Predictably, Suchman's study reveals many conflicts between the design assumptions (DA) built into the machine and user assumptions (UA). Some examples of this, based on Suchman (1987: 148-167), are as follows:

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Treat the question ‘what next?’ as a request for the next step - attended to by presentation of the next instruction.

Can ask ‘what next’ sometimes in order to know how to abort or repair an activity, e.g. where only one photocopy obtained instead of the five desired; ‘while from the point of view of the design [they have achieved] precisely what they want to do, that intent is not a feature of *their* situation’ (p. 165).

Repeat instructions either (a) where task needs to be repeated or (b) where user's action in response to the instruction is in error such as to return the system to a state prior to the instruction being given (a loop).

In the case of repeated instructions, (b) does *not* occur in human interaction. Instead, repetition of an instruction indicates that ‘the action taken in response to the instruction in some way fails to satisfy the intent of the instruction, and needs to be remedied’ (p. 148).

Users will follow instructions; where they do not, this will be detected by the machine. (But what happens when the faulted action goes unnoticed at the point where it occurs ‘because what is available to the system is only the action's effect and that effect satisfies the requirements for the next instruction’ (p. 167)?)

Can sometimes ignore instructions because of preconceptions about what is appropriate, based on prior experience.

Because of these kinds of conflicts between the assumptions of designers and users, Suchman concludes that users often fail to get what they want from the photocopier: ‘Due to the constraints on the machine's access to the situation of the user's inquiry, breaches in understanding that for face-to-face interaction would be trivial in terms of detection and repair become “fatal” for human-machine communication’ (1987: 170).

Like many studies concerned with the mechanics of organizational interaction, Suchman's findings are both analytically and practically rich. Among the practical implications of her study, we may note that:

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- It reveals the character of practical decision-making in a way relevant to the design of expert systems.
- It suggests the constructive role of users' troubles in system design, i.e. troubles arise not by departing from a plan but in the situated contingencies of action. She notes how such systems may not seek to eliminate user errors but ‘to make them accessible to the student, and therefore instructive’ (1987: 184).

Analytically, Suchman's work is important because of its focus on the precise mechanics of organizational interaction. In particular, Suchman begins by using everyday interaction as a baseline and then sees how far human-computer interaction departs from it. This means that she refuses to accept the commonsense assumption that there is a stable organizational or institutional order separate from everyday interaction.

Suchman's work on human-machine interaction thus reveals what can separate conversation analysts from ethnographers. As Maynard and Clayman (1991: 406-407) argue:

> Conversation analysts … [are] concerned that using terms such as ‘doctor's office’, ‘courtroom’, ‘police department’, ‘school room’, and the like, to characterise settings … can obscure much of what occurs within those settings … For this reason, conversation analysts rarely rely on ethnographic data and instead examine if and how interactants themselves reveal an orientation to institutional or other contexts.

Clearly, Maynard and Clayman's point should have a huge impact on how we study social institutions. So at least we can agree with the earlier critics of quantitative method that methodological issues cannot be reduced to merely technical matters.

Indeed, Suchman's focus on how rules function in human-computer interaction links with a very important debate in the human sciences about how rules function in everyday life. Suchman draws upon Gladwin's (1964) account of the navigation methods of a South-East Asian tribe - the Trukese - with no ‘rational’ Western theory of navigation. Instead, the Trukese navigate via various *ad hoc* methods (for example,

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responding to the wind, waves, the stars, the clouds, etc.). She asks how real is the contrast between Western and Trukese methods of navigation? Theories and plans don't *determine* the actions of either Western or Trukese navigators. Rather, Western navigators *invoke* a plan when asked to account for their navigation which, inevitably, depends on *ad hoc* methods (for example, accounting for disasters like the Exxon Valdez oil spill off Alaska).

This creates a problem in artificial intelligence systems that are 'built on a *planning model* of human action. The model treats a plan as something located in the actor's head, which directs his or her behaviour' (Suchman 1987: 3). As Suchman notes, plans neither determine action nor fully reconstruct it. Thus she argues that: 'artifacts built on the planning model confuse *plans* with *situated actions'* and proposes 'a view of plans as formulations of antecedent conditions and consequences that account for actions in a **[p. 19 ↓]** plausible way' (1987: 3). Conversely, she suggests, the successful navigation of the Trukese shows that: 'the coherence of situated action is tied in essential ways not to … conventional rules but to local interactions contingent on the actor's particular circumstances' (1987: 27-28). This implies that, in designing computers that can interact with humans, the system of communication 'must incorporate both a sensitivity to local circumstances and resources for the remedy of troubles in understanding that inevitably arise' (Suchman 1987:.28). This will mean that: 'Instead of looking for a structure that is invariant across situations, we look for the processes whereby particular, uniquely constituted circumstances are systematically interpreted so as to render meaning shared and action accountably rational' (1987: 67).

Why should we believe qualitative research?

What I have said so far can be organized into the following two assertions. First, as Suchman's discussion of rule-use reveals, analytical issues lie concealed behind apparently technical matters, such as the most appropriate research method. Secondly, these analytical issues are rarely properly identified with the tired old polarities of social science (for example, positivist/interpretive, quantitative/qualitative, structure/meaning, objective/subjective). However, if we argue for the pre-eminence of analytical issues in

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research, the implication might follow that the sole requirement for any research study is analytical integrity. This would mean that the validity of a piece of qualitative research could be settled simply by asserting its pristine, theoretical roots.

Along these lines, it is sometimes suggested that the assessment of the quality of qualitative data should transcend the conventional methodological approaches. The quality of qualitative research, it is argued: ‘cannot be determined by following prescribed formulas. Rather its quality lies in the power of its language to display a picture of the world in which we discover something about ourselves and our common humanity’ (Buchanan 1992: 133). If Buchanan is saying that the main question in field research is the quality of the analysis rather than the recruitment of the sample or, say, the format of the interview, then I would agree (see Mitchell 1983). However, Buchanan's opposition to ‘prescribed formulas’ can amount to something that might be called ‘methodological anarchy’ (see Clavarino et al. 1995).

I resist the ‘anarchic’ position on two grounds. First, it simply makes no sense to argue that all knowledge and feelings are of equal weight and value. Even in everyday life we readily sort ‘fact’ from ‘fancy’. Why, therefore, should science be any different? Secondly, methodological anarchy offers a clearly negative message to research-funding agencies; namely, don't fund qualitative research because even its proponents have given up claims to validity. Moreover, in such an environment, can we wonder that qualitative research's potential audiences (for example, the medical professions, corporations, labour unions) take its ‘findings’ less than seriously?

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However, even where questions of empirical validity are taken seriously, they are not easily settled. When presenting results, qualitative data analysts will suggest that certain patterns recur, that perhaps a particular sequence of events can be observed and that the patterns and events have the meanings ascribed to them; that is, that they are valid. In this process, there is sometimes little capacity to determine the extent to which the interpretations of the data reflect the situations or events that occurred, whether other unreported data may contradict the findings, and the extent to which the findings reflect the personal qualities of the researcher. This not only creates a degree of uncertainty about the validity of the data but it also leaves unanswered

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questions about its reliability. Here the problem is serious because some categories or observations may be of a better quality than others, yet all are rendered uncertain by the failure of the researcher to establish their credibility.

To firm up the discussion, I will take the example of a recent research study in which I participated (Clavarino et al. 1995). In this qualitative study, we attempted to address directly the issues of the validity and reliability of our observations. Moreover, as will be seen below, we showed that different analytical traditions could give us different kinds of ‘grip’ on these issues.

Validity and reliability in a study of medical interviews

Our study sought to examine the basis upon which interpretive judgements were made about the content of a series of audio-taped doctor-patient interviews between three oncologists and their newly referred cancer patients. It was during this interview that the patients were supposedly informed that their cancer was incurable.

The interviews were recorded, first, to provide insight into the nature of the interaction between the oncologist and the patient in the context of the disclosure of bad news. The second purpose was to determine whether or not it is possible to extract reliable information from such interviews. Two independent transcriptions were performed. In the first, an attempt was made to transcribe the talk ‘verbatim’, that is, without grammatical or other ‘tidying up’. The second transcription was informed by the analytical ideas and transcription symbols of conversation analysis (see Heritage 1984; Silverman 1993). This provided additional information on how the parties organized their talk.

Using the first transcription, three independent coders, who had been trained to be consistent, coded the same material. Inter-judge reliability was then estimated. For instance, inconsistencies among coders may reflect some ambiguity in the data and some cross-over with the type of information being requested in another coding

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category. What the presence of multiple coders does do, however, is enable the researcher to detect instances of ambiguity, inconsistency and simple coding errors.

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In the first stage of the data analysis, the interviews provided the source for developing a coding scheme. Based upon the research objectives, a series of 24 issues were identified which reflected a range of factors relevant to assessing the quality of life of the patients. For reasons of space, we only deal with the topic of death and dying here.

In 21 of the 22 interviews, all three coders agreed that the patient had been told that the condition was incurable. For one case (W008), however, there was a discrepancy involving the interpretation of the interview data.

> W008 - male, aged 64 at entry to study, was diagnosed with small cell lung cancer one month prior to interview, the disease had spread by the time of initial diagnosis. This patient died almost exactly twelve months after diagnosis.

Two of the three coders believed that the patient had not been told that the condition was incurable. The discrepancy appears to have occurred primarily because the word ‘incurable’ was not used by the doctor. In addition, one of the two coders believed that there was some ambiguity attached to the doctor's statement in relation to the cancer. The following is a transcript of the relevant text:

[Ex.1, W008, p. 9] (D = doctor; P = patient; W = wife)

D: There's somewhere about 15% chance of being around in two years' time, with the disease under control. What that comes to long term is very hard to know. And with that, what we try and do is shrink this away, preferably totally away, and then probably also offer you some radiation to that area after that.
P: Mmm.
D: Uhm, and then watch, wait and see. And it really depends on what happens with time after that and how everything goes. It's very hard to be absolutely dogmatic about any predictions with these things. But despite all of those things, in the majority of people the disease does come back, even from the beginning.
P: Yes.
. . .
D: And if it does come back, we can try other drugs which may control it for a little while, but generally all that you can try and do is control the symptoms.
P: Yes Mm.
D: Uhm, the first time gives us the best chance for a longer survival, hopefully long term, but the odds are generally against that. But *if* [our emphasis] we do nothing for these sorts of diseases, it kills you within a couple of months.
W: The breakdown could be that quick could it?
P: Mmm
D: Well actually it's quite spectacularly fast.

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In Extract 1, one coder believed that the doctor's use of the word ‘if’ (line 18) may have created some ambiguity in the mind of the patient. Given the degree of intercoder agreement where the information in the transcript is apparently unambiguous, this ‘if’ together with the lack of the word ‘incurable’, created disagreements between coders. Based upon these coding practices and this initial transcription, we can only clarify this ambiguity by **[p. 22 ↓]** observing that the wife's comment (line 20) seems to reveal an understanding of the seriousness of the disease. Can a re-transcription, according to CA standards, tell us how this doctor and patient heard each other's talk?

We might initially think that this is a fruitless path to follow. After all, CA insists that it has no entry into people's ‘minds’ and, therefore, makes no claims to show us what people are thinking. However, CA does claim to reveal what people are *doing* and to reveal that doing in observable stretches of talk-in-interaction. Here is our re-transcription of the relevant passages. Extract $_{1*}$ begins slightly before Extract 1 and ends earlier:

Ex.1*
[D talking about poor prognosis if ‘the disease has spread outside the chest’]

1 D: Now for the more limited disease like yourself
2 P: mm=
3 D: =those odds are improved
4 P: mm
5 D: such that there's somewhere about 15 per cent chance (0.5) of being around in two years' time
6 P: mm=
7 D: =with the disease under control.
8 P: mm
9 D: What that comes to long term is very hard to know.
10 P: Mm
11 (0.6)

[D outlines treatment possibilities] . . .

The major difference between 1 and $_{1*}$ is that $_{1*}$ records considerably more ‘mms’ from P. Such ‘mms’ can be heard as ‘response tokens’ which indicate to other speakers that one is monitoring their talk by identifying a potential turn transition point but returning the turn to another. The positioning and presence of such response-tokens (RTs) can be interpreted as having a significant bearing on how parties monitor each other's talk. Do these response-tokens suggest ambiguity about how P is monitoring D's talk?

As Peräkylä and Silverman (1991) have suggested, in an information-delivery format where a professional, like this doctor, delivers information to a client, minimal response-tokens from the information-recipient will allow the information-deliverer to continue through multiple turns. The absence of response can be treated by the information-deliverer as indicative of lack of uptake by the information-recipient.

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Notice first how P provides two RTs (both ‘mm’) at lines 2 and 4. These two RTs are positioned during a turn from D which implies a relatively favourable prognosis (‘limited disease’, ‘improved odds’). Hence, so far, we have no reason to suggest that P is hearing D's talk as indicating that his condition is incurable. However, on lines 6, 8 and 10, we also find RTs from P even when it turns out that this means only a 15 per cent chance of survival in the context of ‘long-term’ uncertainty. None the less, D has not so **[p. 23 ↓]** far suggested ‘incurability’ and, therefore, P's RTs are not indicative that he has receipted anything more than items like ‘15 per cent chance’ and ‘uncertainty’.

However, following D's outline of various treatment possibilities, the position clarifies:

Transcript 1* (continued)
24 D: But er despite all of those things, in the majority of people the disease does come *back*
25 (0.8)
26 D: even from the beginning.
27 P: Yes.
28 D: And (0.4) if it does come back, we can try other drugs which may control it for
29 a little while
30 P: mm um
31 D: but generally all that you can try and do is control the symptoms.
32 P: Yes mm.
33 D: Uhm, the first time gives us the best chance for a *longer* (0.5) survival,
34 hopefully long term
35 P: hhm
36 D: but the odds are generally against that.
37 P: Yes um (0.4)
38 D: But if we do nothing for these sorts of diseases, it kills you within a couple of months.
39 P: Yes.

After D has offered a statement which approximates to ‘incurability’ (‘in the majority of people the disease does come back’), the continued re-transcription (line 25) reveals a 0.8 slot which P does not use to receipt this information. Now it appears that P can be heard actively to avoid receipting the category ‘incurability’.

Presumably, D monitors P's (lack of) response in the same way because he continues by upgrading his statement (‘even from the beginning’). Now, if P were still to say nothing, his silence would be distinctly noticeable possibly marking (to D) some psychological category like ‘denial’. However, at this point, P *does* provide an RT (‘yes’, line 27). Its presence here, immediately after D has conveyed ‘bad news’, and in an upgraded form (‘yes’ rather than ‘mm’) strongly suggests that P has receipted the seriousness of his condition.

Both this ‘yes’ and P's ‘yes mm’ after further ‘bad news’ (line 32) are available in the original transcript (Ex. 1). However, the re-transcription gives additional evidence of P's receipt of what D is saying about his incurability. On lines 37 and 39, P is heard to say ‘yes’ after D has said that ‘the odds are generally against [long-term survival]’ and that

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‘if we do nothing for these sorts of diseases, it kills you within a couple of months.’ P, as well as his wife, receipts a bad prognosis.

However, we might also note that, despite the clearly ‘bad’ implications of what D is saying, D does not directly say ‘you will die.’ There is an element of ambiguity present here and we may speculate that D's failure to come out and directly say to P that his condition is terminal functions to manage different kinds of response to ‘bad news’. By not being totally ‘up front’ about **[p. 24 ↓]** P's prognosis, while none the less making an ‘honest’ statement, highly implicative of ‘bad news’, D preserves P's ‘choice’ about how to respond.

To recapitulate: our aim was to determine whether we could improve the reliability and validity of qualitative analysis.[3] First, we sought to identify and to explain differences between trained raters using rough transcripts. Next, drawing upon the transcription symbols and concepts of conversation analysis, we sought to reveal subtle features in the talk, showing how both doctor and patient produced and received hearable ambiguities in the patient's prognosis. This involved a shift of focus from coders' readings to how participants demonstrably monitor each other's talk. Once we pay attention to such detail, judgements can be made that are more convincingly valid. Inevitably, this leads to a resolution of the problem of inter-rater reliability.

What both analyses suggest is that continuing and detailed attention needs to be paid to the quality of the qualitative data, and the ways in which judgements are made about its content. This means that, despite the employment of two very different approaches, our analysis stayed on a consistent path.

First, unlike some qualitative researchers, we took seriously the criteria of reliability and validity. Secondly, both methods we used seek to build a consensus among a research team through identifying and resolving hard cases. Inter-rater agreement focuses on interpretations of given transcripts, while conversation analysis treats the establishment of an agreed transcript as a crucial first task for a research team.

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Conclusion

Towards the beginning of this chapter, I showed how we often make problematic assumptions about the differences between qualitative and quantitative work. As Table 1.1 showed, it is often claimed that qualitative work is ‘soft’, ‘subjective’ and speculative’, while quantitative research is ‘hard’, ‘objective’ and ‘hypothesis-testing’. Yet the two qualitative studies reviewed undoubtedly seek to be ‘objective’ and ‘hard’, while, in some respects, the work on medical interviews involved hypothesis-testing. These two studies do more than show how qualitative research can be ‘objective’ by means of careful transcription. They also offer an alternative way forward which, by their common focus on what participants are doing, allows us to question the assumption that the open-ended interview is the ‘gold standard’ of all qualitative research.

The last thing I want to do, however, is to impose conversation analysis as the only acceptable method of doing qualitative research. As already noted, everything will depend upon the research problem being tackled. Moreover, thoughtful researchers will often want to use a combination of methods. Thus I am not trying to argue that there is only one analytically sound or rigorous method in qualitative research. As later chapters show, using **[p. 25 ↓]** observation, interviews, and textual as well as conversation analysis, qualitative methods may be employed to pursue diverse theoretical ends. So the multiple logics of qualitative research emerge from their relationships with the general purposes of research projects. In this way, my discussion has sought to counter the tendency to divide research methods into mutually exclusive epistemological camps.

However, this benevolent neutrality towards the varying logics of qualitative research co-exists with an appeal to two very strong principles. First, researchers always need to address the analytical issues that may lie concealed behind apparently straightforward issues of method. Secondly, qualitative research's concern for an ‘in-depth’ focus on people's activities (or representations of those activities) is no warrant for sloppy thinking or anecdotal use of ‘telling’ examples. We owe it to ourselves and our audiences to generate reliable data and valid observations.

If there is a ‘gold standard’ for qualitative research, it should only be the standard for any good research, qualitative or quantitative, social or natural science. Namely, have

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the researchers demonstrated successfully why we should believe them? And does the research problem tackled have theoretical and/or practical significance?

Notes

1 Ironically, the most recent attempt to revive ‘positivism’ as a term of abuse is to be found in the attacks by Garfinkel and his followers on what they see as the ‘positivist’ elements present in conversation analysis (e.g. Lynch and Bogen 1994).

2 As Robert Dingwall (personal communication) has reminded me, it is worth noting that Cicourel (1964) also provides a chapter devoted to a critique of qualitative research. For instance, the claims that open-ended interviews offered direct access to ‘experience’ were opposed by Cicourel who showed that commonsense reasoning entered into the understandings of both interviewees and researchers.

3 For a more radical discussion of how the issue of the coding of observations can render analysis problematic, see Garfinkel (1967).

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