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# *Life is out there: a comment on Griffin*



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**ABSTRACT** Open-ended interviews remain the default data generation technique for qualitative psychology and sociology. This commentary raises questions with Griffin's understanding of naturalistic materials and the emic/etic distinction. It reiterates problems in the use of open-ended interviews, and repeats the case for more considered support for their use.

We are delighted to comment on Christine Griffin's thoughtful and wide-ranging article (henceforth BDBT). Our commentary raises questions on Griffin's understanding of naturalistic materials and the emic/etic distinction. It reiterates problems in the use of open ended interviews, which remain the default data generation technique for qualitative psychology and sociology. We also repeat the case for more careful support for the use of interviews. Although there is much to agree with, and many issues that are too complex or too peripheral to address here, we will comment on some major problems with BDBT's representation of general issues in social research method and our work in particular. We will attend first to those matters and then briefly consider some of the broader issues BDBT raises.

## *The place of interview research*

A casual reader of BDBT would come away with the impression that there has been a methodological putsch in social science. They might assume that audio and video records of talk have been marched into centre stage in the journals while downtrodden interview researchers are now banished to the margins where they struggle for publication. They would be wrong. For example, in our recent discussion of interview research (Potter and Hepburn, 2005) we documented the central place of interviews in qualitative research in the discipline of

psychology. We noted that standard methods handbooks present interviewing as the default choice for virtually every perspective (phenomenology, ethnography, grounded theory). The situation in sociology is similar. For example, in 2004 the journal *Sociology* published some 56 substantive articles – of these, 20 used interviews or focus groups (often with little justification) and just three used naturalistic data (working with the loosest of criteria).

Strong arguments for the virtues of naturalistic data have been developed in discursive psychology. However, they have been developed to counter a well-established interview orthodoxy. They are not intended to bar the use of interviews.<sup>1</sup> Note also that the majority of contemporary psychology works with experimentally generated materials of one form or another – naturalistic data has hardly sneaked into the back row of this theatre, let alone got anywhere near the stage. Studies using naturalistic data in the discipline of psychology are a small proportion of a small proportion. The situation in discourse studies (as a field, and a journal) is undoubtedly different; however, BDBT is more concerned with interviews in psychology and sociology so we will stay with that broader question.

A number of points in BDBT require clarification.

### *Naturalistic records*

The issue of ‘natural data’ or ‘naturalistic records’ is a subtle one. The (conceptual) ‘dead psychologist test’ was designed to provide a clear test for research materials generated primarily through interaction with the researcher and those materials that (ideally) would have been generated irrespective of the researcher’s activities (Potter, 2002). If the researcher was taken ill that morning, interviews and focus groups (and experiments, surveys, and questionnaires) would fail to be done, and so are not naturalistic; phone calls between friends, family mealtimes, relationship counselling sessions, police interviews, records of Parliamentary debates (amongst many other things) would carry on more or less as before. Note that Griffin is simply wrong, and particularly misleading, when she suggests that naturally occurring talk is ‘usually taken to mean talk that is informal and outside the context of situations with a declared purpose and a particular venue’ (p. 248). In discursive psychology, for example, the majority of work has used naturalistic materials from institutional settings such as helplines, counselling and therapy, police interviews, and neighbour mediation (see, for example, contributions in Hepburn and Wiggins, 2005, forthcoming). A much smaller proportion has used everyday talk – and even there it would not be considered to be ‘outside of a particular venue’.

As Griffin rightly points out, naturalistic materials can be affected by their involvement with research process (traditionally ‘reactivity’), hence the word ‘naturalistic’ rather than ‘natural’. Reactivity presents challenges that can be met in practical, theoretical, and analytic ways (e.g. by acclimatization or by studying orientations to the recording process as Speer and Hutchby, 2003, have done). DP generally has a nuanced approach to the nature of data, evidence, fact and reality (Edwards, et al., 1995; Potter, 1996).

Naturalistic material is constituted as such at least partly through the analyst's stance to it. For example, an open-ended interview can be used in, say, an ethnographic mode with the aim of identifying the 'views' or 'meanings' of a social group such as 'young people', as is the case in BDBT. However, the interview can be treated as an interactional event in all its institutional and normative particulars. Perhaps this is what Griffin is arguing – if so we are very much agreed. Work taking such a naturalistic approach to method is discussed in Potter and Hepburn (2005).

BDBT argues that discursive psychologists are advocates of a 'dead social scientist approach' (p. 266) and that they wish to avoid 'contaminating the field' (p. 253). This is critique by innuendo, and stands history on its head. In the 1980s the discourse analytic work that evolved into discursive psychology primarily worked with open-ended interviews. It criticized the idea that the interviewer should be passive and make minimal contributions and advocated 'a much more interventionist and confrontative arena than is normal' and suggested interviews could be 'an active site where the respondent's interpretative resources are explored and engaged to the full' (Potter and Wetherell, 1987: 164). Our dissatisfaction with interviews, then, did not arise because interviews are insufficiently neutral. On the contrary, it arose because of the difficulty of achieving the desired *activity*.<sup>2</sup> The excitement of working with naturalistic materials came from *this* dissatisfaction rather than a nostalgic positivistic wish for neutrality. Moreover, it is the *pull* of naturalistic material as an extraordinarily rich topic of study rather than the *push* of problems with interviews that has sustained the research. Given that naturalistic materials are both powerful and analytically tractable, the question becomes: why have interviews remained the default choice for qualitative researchers and why has there been so little justification of that use?

### *The emic/etic distinction*

We do not want to devote much space to this. However, the discussion is confused and misleading and needs clarifying. The emic/etic distinction was developed by Pike (1954) building on notions from linguistics. In linguistics, phonemic analysis focuses on sound differences that distinguish meaning for a particular speech community while phonetic analysis focuses on technical sound differences that can be identified by a trained researcher. To talk about emic and etic approaches, then, is not to say that one is necessarily better than the other; that depends on the job that is to be done.

In discursive psychology this distinction has sometimes been used to highlight different ways of treating psychological notions. For example, take the topic of 'noise'. An etic approach is characteristic in the psychology of perception where features of noise are distinguished and identified by the researcher and then used in different sorts of stimuli in experimental studies. Such work can be important in dealing with topics such as hearing loss, and to be against such 'etic' work in principle would be somewhat odd. However, DP offers a more 'emic'

alternative which starts with the way notions such as noise are constructed and deployed within particular settings. For example, Stokoe and Hepburn (2005) consider noise reports in calls reporting abuse to a child protection helpline and in calls complaining about the actions of neighbours. They show how noise is constructed in subtly different ways to fit different actions: complaining and reporting.

It does not make sense to say that research interviews are 'more "etic"' (p. 249) than naturalistic materials. For example, interviews can be analysed for their local sense-making procedures, for how issues such as 'prejudice' are constructed and managed for instance (Edwards, 2003; Clarke et al., 2004). And when BDBT proposes to 'challenge the notion that research interviews inevitably and primarily produce material with "etic" qualities' (p. 249) it is not clear who is being attacked; but this is certainly not a claim we have developed.

### *The limitation of interviews*

As our article on problems with qualitative interviews in psychology (Potter and Hepburn, 2005) was picked out for particular criticism, it is worth considering its lessons for the current argument. First, let us emphasize what it was not. Despite the impression given to the contrary it was not a blanket attack on the use of interviews in psychology (the field under discussion in the piece). We argued that the use of qualitative interviews in psychology is often flawed, and that it would be fruitful to gain more understanding of the operation of qualitative interviews using the resources of contemporary interaction analysis. The discussion was designed to generate a more critical approach to the choice of interviews (particularly when working with naturalistic materials can be so fruitful) but also to support better interview research. The discussion worked through a series of problems with the design, conduct, analysis and representation of interview studies. We emphasized the way the active role of the research interviewer was often missed in current research, but were not thereby suggesting that participants were merely passive. Despite lip service often paid to the idea that the interview is an interactional event, its interactional nature is often all but lost in the research process.

BDBT repeatedly claims that research interviewees are treated as passive in DP so it is important to rebut it clearly.<sup>3</sup> DP treats both interviewer and interviewee as actively engaged in a range of practices. Again, for us the activity of interviewees is not a problem that will somehow disrupt an ideal research process. The problem is that the activities of interviewee (like those of the interviewer) are often bound up with interview-relevant practices (following social science agendas, managing footing, constructing appropriate stake and interest) which are frequently difficult to identify and analyse. There is a pervasive failure to address those practices in contemporary interview research. That is a point we document at some length in Potter and Hepburn (2005).

BDBT offers some interesting observations about what happens in a sequence from one of the author's interviews. Whether these are convincing or not, we offer three observations.

First, we note that the analytic claims Griffin offers include little on the issues of gender, class, ideology and consumption that have been threaded through her previous work. That is, BDBT is not showing how to move from this interview to broader analytic conclusions. Far from showing up the virtue of working with interview material, then, it starts to show how hard it can be to work with.

Second, it was difficult to assess even these specific claims because of the representational practices used in BDBT. The form of transcription renders the interaction as virtually play-script, making it hard to assess even the limited claims made. We asked Griffin if she would provide a sound recording of the interaction so we could produce a transcript that captured more of the interaction. This might have clarified some of the analytic claims and perhaps allowed us to build on her analytic claims or develop some alternative possibilities. She declined, citing space and ethics. This is unfortunate.

Note also that the article makes much of various non-vocal elements of the interaction, but provides only broad brush researcher stipulations about what they were. Analysing gestures and movements without a record of such things makes the research claims dependent on the interviewer's (uncheckable) impressions and memories. We suggest that if the researcher is going to work with non-vocal elements of interaction (in interviews or other settings) some kind of visual record is essential.

To take one example where a more careful transcript might have made a difference to the claims offered, BDBT treats the 'laugh' reproduced on line 457 as 'indicating a moment of troubled interaction' (p. 258). This might be so. However, laughter can be doing all kinds of different things and, crucially, is different when shared and when other parties avoid joining in (Glenn, 2003). Without more information about the delivery and uptake of the laughter it is difficult to make much of the (already vague) analytic claim offered. We should emphasize that our interest in the audio and desire for an adequate transcription does not flow from a yearning for a more positivistic universe but from a recognition of just how complex, interesting and difficult interview interaction is. Put another way, BDBT stresses the importance of seeing the interviewee as active and the whole interview as a relational encounter – yet its representational practice precisely wipes out much of the rich information that might allow access to this very thing.<sup>4</sup>

Third, it is not clear to us the virtue of generating material in the way advocated by BDBT. Why produce materials that are, as we noted in the piece BDBT is partly a response to (Potter and Hepburn, 2005), flooded by social science agendas and researcher categories, where participants work with a range of different interview-related orientations to stake and interest, and where the parties shift between complex research-related footing positions? What is the special magic the interview provides that makes the very complex analytic task of dealing with those endemic and probably inescapable interview features worthwhile? BDBT does not show the added value that comes from the researcher managing the interaction over and above the sorts of materials that come from naturalistic conversations between girls and women (as offered by Coates, 1996, for example). Although BDBT makes suggestive comments about an offer of hand cream

and the waving of a Brazilian flag why not focus on material which might well involve such things but does not revolve around an interviewer? The interview is the default approach for qualitative social science – but BDBT does not offer a good case for it remaining as that. It is hard to resist the suspicion that in many cases interviews have been done because the researcher has not considered any alternative approach, or has assumed that access will be hard to deliver.

Ultimately, however, there is no conflict between us. We have pressed the virtues of working with naturalistic material but equally recognize that high quality interview work is important, interesting and possible. Our aim has been to improve interviews where appropriate and encourage a shift to working with naturalistic material where they provide something more.

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#### NOTES

1. We have both published interview studies in the last few years (e.g. Hepburn, 2000; Clarke et al., 2004) and have long experience of interview work.
2. None of this is to say that interesting research using active interviews is not worthwhile. We should emphasize again, we are not anti-interview, just against bad interviews.
3. BDBT quotes Auburn and Lea (2003) as saying the material on which they based their recent study is from a prison offender programme rather than an occasion 'where the talk is generated *by and solely* for the consumption of the researcher' (2003: 282, Griffin's emphasis). She takes this as indicative of a general stance towards interviews, which she glosses as the 'simplistic view' that 'the researcher and the research project [have] overwhelming dominance over the research encounter, relegating other participants to relatively passive "feeder" roles' (p. 250). This is a highly misleading picture of Auburn and Lea's rather straightforward claim and the more general DP approach to interviews. Griffin misses the point in her discussion of Auburn and Lea (2003) that their object of analysis is as much the psychologists who construct particular individual cognitive models from the talk of sex offenders. Doing their own interviews with sex offenders would make it much harder to develop the arguments that they do.
4. For further examples and argument of this kind see Benwell and Stokoe (2006) and West (1996).

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