Different visions: a rejoinder to Henwood, Potter and Hepburn

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In BDBT I engage with 'an argument in favour of the analysis of "naturally occurring talk" (or 'naturalistic records') in preference to material derived from research interviews or ethnographic observation, and a related argument in favour of discursive psychology approaches (henceforth DP) derived from conversation analysis (henceforth CA) as the most suitable means of analysing the interactional aspects of talk' (this issue, p. 247). In particular, I interrogate some of the assumptions about research, the research process and the position of the researcher and the researched on which these preferences are based, and the implications for the practice of qualitative psychology and sociology. I argue that research interviews and observational methods, for all their undeniable difficulties, retain a number of advantages for those researchers who wish to adopt a more 'engaged' approach to the research process, and for those with an interest in making connections between people's talk and other activities in research encounters and the broader discursive (i.e. ideological) context.

In questioning some of the implications of a preference for the analysis of 'naturalistic records' using a CA/DP approach, I certainly would not view this as reflecting what Potter and Hepburn refer to as a 'methodological putsch in social science' (p. 276). Far from it, these debates have been the concern of a relatively specific, though influential group of qualitative social science researchers, which is certainly not to diminish the importance of the issues at stake. I am grateful to Karen Henwood, Alexa Hepburn and Jonathan Potter for their thorough and detailed engagement with the points I raise in BDBT. I found much to agree with in both their responses, and much to ruminate on for future exchanges. I will confine myself here to clarifying a couple of points relating to the Potter and Hepburn response, and expanding on some of the points raised by both pieces. For example, I very much welcome Potter and Hepburn's clarification of the 'pull' and excitement of working with naturalistic materials as an important 'draw' for those discursive psychologists who prefer to work in this way, rather

than being influenced primarily by the 'push' of a cogent critique of the difficulties inherent in research interviews.

My discussion of the 'dead social scientist test' was not intended to equate Potter's argument with a wish to avoid 'contaminating the field': I was careful to differentiate the DP perspective on this issue from the naturalistic tradition emerging from ethnographic research (see Hammersley and Atkinson, 2003, for a fuller discussion of this approach). Since what is sometimes termed the 'critical turn' in the social sciences during the 1980s and 1990s, a third approach has begun to emerge. Rather than aiming to minimize the impact of the researcher, such work has openly acknowledged and even embraced a detailed exploration of the impact of researcher on researched and vice versa as part of the analytic process (see Jorgenson, 1991; Aull Davies, 1999; Foley, 2002, for examples of 'critical' and 'reflexive' ethnography from the anthropological tradition). This has much in common with Karen Henwood's point that research interviews can enable discourse to be treated as both topic *and* resource for analysis, although there is undoubtedly an urgent need for more good examples of this sort of analysis.

In qualitative psychology, with its interest in the detailed analysis of language as a social practice, this type of approach has resulted in a focus on the diverse and fundamental ways in which the very questions asked by researchers in interview encounters will inevitably incline 'participants' to respond in particular ways. Jonathan Potter, Alexa Hepburn and Karen Henwood, amongst many others, have been at the forefront of this type of work.

Unfortunately I only have space here to engage with a few of the specific points made by Potter, Hepburn and Henwood. First, I would like to clarify a number of apparent misunderstandings in the Potter and Hepburn piece. My definition of 'naturally occurring talk' as 'usually taken to mean talk that is informal and outside the context of situations with a declared purpose and a particular venue' (p. 248) was a paraphrase of Stephanie Taylor's definition in the Open University text on *Discourse as Data* edited by Wetherell et al. (2001), and my sentence followed a direct quote from Taylor's chapter in this text. Whilst the responsibility for the paraphrased version of Taylor's words is all mine, in the context of the rest of my argument, the offending sentence was intended to differentiate what is usually referred to as 'naturally occurring talk' from talk that is generated within the specific context of research interview encounters, which can be viewed as 'conversations with a (specific) purpose'. My intention here was not to imply that the various important discursive psychology analyses of help lines, counselling and therapy sessions and offender treatment programmes are somehow conversations *without* a declared purpose, nor to gloss the subtlety of the question of what constitutes 'naturally occurring talk' or 'naturalistic records'. Jonathan Potter and others have been discussing this thorny question thoughtfully and in some depth over several years – as I acknowledge in BDBT. Potter and Hepburn's classification of the nuanced distinction between 'natural' and 'naturalistic' records is well-made and useful.

I feel I must take issue however with their characterization of my analysis of the interview extracts quoted in BDBT as setting out to identify 'the "views" or "meanings" of a social group such as "young people'" (p. 278). In BDBT, I do not assume that the young women's accounts produced in the research encounter constitute internal entities known as 'views' or 'meanings' that might be held by those young people about hand cream or the Brazil (or England) football teams. Rather, I would see these young women's accounts as produced in relation to my talk as well as to the institutional contexts of the school *and* the research process. However, I would also see their talk and other activities generated in and by the research encounter (such as the sharing of hand cream or waving the hand-made Brazil flag) as drawing on cultural discourses with resonances beyond the immediate context of the research encounter.

I do not assume that research interviews or ethnographic observation will give me a straightforward insight into the role of consumption in young people's lives – my concern is rather to explore how young people are positioned and position themselves in relation to this issue, not to seek to record their 'views' in any straightforward way as if research interviews record 'what people really think'. Nor do I see this interest in young people's relationship to consumption as mutually exclusive of an interest in the research interview as an interactional event in its own right. My aim in presenting a (partial) analysis of the two interview extracts was to try, however imperfectly, to illustrate this in practice whilst simultaneously engaging with the debates on the virtues and drawbacks of naturalistic records and research interviews.

In my analysis of my own research interview material, Potter and Hepburn note that I do not offer much in the way of analysis 'on the issues of gender, class, ideology and consumption' that have characterized some of my previous work (p. 280). This is somewhat disingenuous because in BDBT I explain that this is not my intention at the start of the 'analysis' section. However, in considering the wider implications of the flag-waving incident I do make a relatively brief mention of the role of 'race' as a trope in the young women's talk. Re-reading the section on the 'flag-waving episode' in the context of the pervasive waving of 'the England flag' in parts of the UK prior to the start of the 2006 World Cup, I am struck by the complexity of stated allegiances to different football teams. In the interview transcript and in my subsequent analysis, I highlight the way in which this process is racialized by some of the young women – but other processes are also at work in these accounts. Zaby's statement that 'most of the black girls in the school support Brazil' in contrast to Gemma's statement about the (presumably white) majority who support England appears to be counter-intuitive. After all, a substantial proportion of England players are of African, African Caribbean and Black British origin or descent: this is hardly the 'lilywhite' team that won the World Cup in 1966. Zaby and Karen's stated support for Brazil (along with 'most of the black girls in the school') can also be viewed as counterintuitive: why would they not ally themselves with pre-dominantly black teams such as Jamaica or Cameroon? Football at this level is a highly lucrative global enterprise, and support is increasingly constituted as a matter of (consumer) 'choice', so 'race', ethnicity and geographical location can

no longer be used to straightforwardly identify particular groups as supporters of specific teams.

Potter and Hepburn also object to the representational practices I use in BDBT, which hinder them in the type of analysis they would prefer to conduct. In writing the article I spent some time considering whether to represent the quoted interview material in the lighter than 'Jefferson-lite' form that I knew many discursive psychologists would find objectionable. In the end, despite some of the undoubted advantages (and limitations) of the Jeffersonian system, I decided to stick with my own representational practices, with all their flaws, because they suit the type of analysis I conduct and are common in the qualitative social research tradition in which I work. The transcription process is theory laden, and as Lapadat and Lindsay point out: 'the choices that researchers make about transcription enact the theories they hold and constrain the interpretations they can draw from their data' (1999: 64). I suspect that Potter, Hepburn, Henwood and myself would all endorse this statement, but perhaps from rather different epistemological perspectives.

My reason for refusing to send a sound recording of the interview to Potter and Hepburn as requested was a serious one. As I noted in my email response to their request: 'I'd prefer not to – partly space constraints, but also ethically, the research team on this project never asked participants' permission to circulate their talk. We specifically said that only members of the research team would listen to the tapes'. I also asked the research team (Ann Phoenix, Rosaleen Croghan and Janine Hunter) for their response to this request and they were unanimously unwilling to circulate the tape for the same ethical reasons.

I have no objection to the proposition that the talk generated in research interviews is, to some degree, produced for the benefit of the researcher – but my point is that this is unlikely to be *all* that is going on in any research encounter. And in my view *both* the talk (and other activities) that are generated for the benefit (or the hindrance) of the researcher *and* the talk that is produced for other audiences and in relation to wider ideological configurations can fruitfully be the focus of analysis. In practice, it may not be possible to make a sharp distinction between talk that is generated for different audiences and in relation to different discursive, rhetorical and ideological contexts. Looking again at the interview transcript that revolves around what I have termed 'the hand-cream moment', I would not want to make such a sharp distinction between 'emic' and 'etic' elements of the interaction. Zaby's offer of hand cream could be viewed as both 'emic' (i.e. as a shared cultural practice between the young women) and as 'etic' (as produced in part as a display of sophisticated feminine consumption in front of me as an educated adult visitor from the university). To some extent it is impossible to identify which is which. Karen Henwood's response to BDBT highlights this point well when she emphasizes the inevitable intermingling of the emic and etic elements of the research interview encounter.

Maybe the crux of the difference in perspective between myself and Potter and Hepburn can best be appreciated through an allusion to the various positions from which one can conduct research. Potter and Hepburn repeatedly refer to themselves as 'analysts', or refer to the researcher as 'managing the

interaction' (p. 280), whilst I tend to present myself as a 'researcher' who 'engages' with other research participants. I do not constitute myself as an analyst, even though I quite patently practise analysis (albeit not to the standard or in the manner that might be acceptable to Potter and Hepburn). So whilst Potter and Hepburn and I share a great deal of common ground in this debate, the points on which we differ are fundamental, revolving around different visions of the practice of research, what it might involve and what it *feels* like. For me, social science research encapsulates a broad, complex and contradictory set of epistemological frameworks, methodological processes and research techniques and analytic procedures. Above all, I would not want to reduce the diversity of these practices into one way of doing things or to silence debate between these various perspectives, any more no doubt than would Jonathan Potter, Alexa Hepburn or Karen Henwood.

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