Stop making sense: the trials and tribulations of qualitative data analysis

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An account is given of a research project with particular attention to the place of data analysis and interpretation. It shines some light into what often remains the black box of qualitative research practice, the mysterious process of generating a research narrative based on the data. The paper provides other apprentice researchers with an insight into what the data analytical experience can be like, warts and all. It raises issues to do with qualitative data analysis, the use of CAQDAS, grounded theory, the relationship between the narratives of the researcher and the researched, and about the training of qualitative researchers.

Key words: UK, research practice, qualitative data analysis, CAQDAS, research training, grounded theory

Introduction

It's a horrible but inescapable fact that it takes more time to organise, write and present material well than it does to gather it . . . The sensible researcher will allow as much free time to write his [sic] report as he spent in the field. If he is really astute and can get away with it, he will allow more time. (Wax 1983, 193–4; in Cook and Crang 2007, 131)

The title of this paper refers to the 1984 Talking Heads album Stop Making Sense. *The opening lines of the song* Psycho Killer *give a good indication of the less euphoric moments of learning to interpret qualitative data.*

In recent years a lot of reflexive attention has been brought to bear on the production of social scientific knowledge (McDowell 1992; Katz 1994; Rose 1997; Crang 2003).[1] One aspect of the research process which has received little reflexive attention is the nitty gritty of qualitative data analysis and writing up. This remains a bit of a black box (Tesch 1990; Pickles 1992; Stroh 2000; Bergman 2003; Cook and Crang 2007).

Bell and Encel (1978, 8) note that few post graduate researchers tell their tales. Cook (2001; Cook *et al.* 2005) is an example of a geographer who has given an insight into the way in which the PhD is an 'organisational product' (Becker 2007), from the point of view of the postgraduate researcher. The consideration of data analysis joins the positionality debate in that the positionality of researchers and the social context in which they are undertaking research will influence what strikes them in the data, their decisions about what to include and what to omit, the kinds of stories to tell and not to tell. But it is not this wider context of data interpretation I discuss here. It has been extensively discussed elsewhere as the literature referred to above suggests. I focus not so much on what informs our interpretive acts as on what it is like to be in the middle of that messy process. It is like emphasising say light blue in a painting, rather than say yellow; both colours remain part of the painting, but for now we are most interested in light blue.

This paper contributes to the reflexive debate about social scientific research practice by providing

a frank account of what qualitative data analysis can be like, from the point of view of someone in the middle of learning to do qualitative data analysis. My intention is to contribute to a stock of representations of what qualitative data analysis is like at the experiential level. Such representations can allow other apprentice researchers more easily to situate their own evolving experience with reference to a shared imaginary.

It is tempting to focus on the most codified part of the data interpretation process as this is the easiest to talk about. I will try to capture both the moments of interpretation that are more easily talked about such as attributing labels to segments of field notes and transcripts (coding), as well as those moments which are more elusive. At the end of their book on ethnography, which includes chapters on data analysis and writing up, Cook and Crang are concerned that they 'probably haven't given enough attention to the process, i.e. the pains and pressures of writing' (2007, 202). I also try to give attention to some of that.

Bottomley (1978), reflecting on his postgraduate research (which he abandoned), shows very nicely how he struggled with issues along the way and then later discovered material that helped him (or could have helped him) resolve his conundrums. I seek to take a leaf out of Bottomley's book. The image of my own apprenticeship which springs to mind (in retrospect) is of being parachuted into a small forest in an unfamiliar landscape with very little sense of how to think about that landscape, what its characteristics are and on what basis to navigate it. One finds oneself exploring a small part of one of the major regions possibly referred to as the region of 'qualitative methods', or another region known as the region of 'quantitative methods'. Perhaps one is parachuted into a border region, the region of 'mixed' or 'multiple methods'. One leaves the country having perhaps bored down deeply in a fairly narrow area – digging in, perhaps even finding oneself in a bit of a hole at times. I did become much more familiar with the landscape of social scientific methods. However, I left with a rather more limited idea of its overall geography than I had expected, let alone its geology (ontology and epistemology) or a broader view of the kinds of theoretical winds which blow on that landscape and the ends to which those winds can be harnessed.

First, I offer a few reflections on the nature of data analysis, in particular how it appeared to me in the course of my training; second, I give a few necessary details of the project itself; third, I give an account of the main phases of the research and the place of data analysis within that; fourth, I discuss some of the issues which are raised by the account and offer some possible ways forward.

What is data analysis anyway, when and where does it happen?

Tesch (1990) identifies 26 analytical strategies that can be applied to qualitative data (Coffey and Atkinson 1996). Ryan and Bernhard (2003) identify 12 approaches to identifying 'themes' in qualitative data (Cook and Crang 2007). Miles and Huberman (1994) have provided an encyclopaedic collection of approaches to qualitative analysis. From within geography Cook and Crang (2007) give realistic attention to different aspects of the interpretative process in the context of ethnography, including data analysis and writing up. In spite of reading some of the literature on data analysis (e.g. Strauss 1987; Strauss and Corbyn 1998; Silverman 2000), I found data analysis a bit of a black box during my PhD. It appeared to be something that happens, somehow, by mixing analytical approach (e.g. grounded theory) with data (e.g. field notes, transcripts) and technology (e.g. ATLAS/ti). Moreover, 'data analysis' was a phrase I found it difficult to get comfortable with. It seemed to be about cutting things up and examining the pieces to better understand the whole, a bloody and violent activity. What I was doing was more akin to a creative process of constructing and attributing meaning to phenomena that entered my experience in the course of a structured social process referred to as 'research'. 'Sense making' seemed to be a more satisfactory label, hence the title of the paper. The tension between 'analysis' and 'interpretation' is reflected in the literature:

> For some authors, analysis refers primarily to the tasks of coding, indexing, sorting, retrieving, or otherwise manipulating data . . . From such a perspective, the task of analysis can be conceived primarily in terms of data handling . . . For others in the field, analysis refers primarily to the imaginative work of interpretation, and the more procedural, categorizing tasks are relegated to the preliminary work of ordering and sorting the data. (Coffey and Atkinson 1996, 6–7)[2]

Data analysis, as interpretation of experiences encountered in the course of the research process, goes on throughout the research process and not

only in dedicated moments of focused data interpretation. Nor does it go on only while one is 'at work'. So, for example, one of the key interpretative moments in the research came in conversation about the research with a sociologist friend in a bar in London. Two other very important interpretative moments came during conversations in the field. We should therefore be on the lookout for moments of interpretation whenever they arise and be ready to capture them.[3] I experienced making sense of the data as a very intimate part of my being in the world, and as a process that made self-conscious (reflexive) use of the researcher's ordinary meaning-making processes. I was, albeit without knowing it, in sympathy with symbolic interactionist authors such as Mead (1934) who hold that society, reality and self are constructed through the dynamic interplay between action and language (Charmaz 2006).

A thumbnail sketch of the research project

The research was funded by an ESRC CASE studentship with the Forestry Commission of Great Britain (FC) as the CASE partner. It ran from September 2001 to November 2005, when the thesis was accepted (Schiellerup 2005). The project began life as an enquiry into social learning in forestry policy networks with particular reference to 'social forestry', a contemporary buzz-word in forestry policy. The phrase captures the idea of forestry being undertaken for societal ends other than timber production, such as health, educational or environmental outcomes. Data collection consisted of a combination of multi-site participant observation at about 20 mainly FC events in England, Wales and Scotland, and 40 long semi-structured interviews.

Grounded theory

My data analytical strategy was informed by an engagement with grounded theory. It is one of the most common approaches to identifying themes in qualitative data (Cook and Crang 2007). Charmaz provides a definition:

> a method . . . that focuses on creating conceptual frameworks or theories through building inductive analysis from the data. Hence, the analytic categories are directly 'grounded' in the data. The method favours analysis over description, fresh categories over preconceived ideas and extant theories, and systematically focused sequential data collection over large initial samples. The method is distinguished from others since it involves the researcher in data

analysis while collecting data – we use this data to inform and shape further data collection. Thus the sharp distinction between data collection and analysis phases of traditional research is intentionally blurred in grounded theory studies. (2006, 187–8)[4]

Within grounded theory, I worked with the ideas of identifying 'what is going on in the data' (as opposed to arriving with a preconceived opinion), 'open coding', 'axial coding', 'categories', 'properties', 'dimensions', 'conditions and consequences' in order to develop a grounded theory which could account for the data in a plausible way. The meaning of 'constant comparison', which is supposed to lie at the heart of the method, remained a mystery to me until I read Becker's long exposition of comparison (2007, 40 onwards).

ATLAS/ti

I used ATLAS/ti for all phases of the analytical process, including theory-building. I also trialled NVivo, but I found the relationship between codes too hierarchical (see also Stroh (2000); for a comparison between different packages, see Levins and Silver (2007)).

ATLAS/ti uses the idea of the 'hermeneutic unit' to refer to a collection of files that relate to a particular project. The emphasis is on interpretation. The intention of the creator of ATLAS/ti is that it should be more than a tool for organising and retrieving data; it should also help, in the terminology of ATLAS/ti, to build theory. This can be done through the aggregation of codes in different ways such as through the use of 'super codes', 'code families' and 'networks'. Code families are based on the idea of certain codes sharing a similarity. Super codes, however, refer to codes that have been generated through searches in the data based on the combination of ordinary codes. Networks are networks of codes where the relationship between different codes can be specified. These different tools are intended to serve as a means towards increasing levels of abstraction in the interpretation of the data. There are also other means of mapping the relationship between codes. In fact, ATLAS/ti has a multiplicity of overlapping tools which contributes to its flexibility, but also to a sense that it can be difficult to get an overview. Stroh (2000) notes that even when CAQDAS packages have theory-building functions, such as is the case with ATLAS/ti, it is often the code-and-retrieve functions that are most used and the theory-building functions never get used. ATLAS/ti is also supposed to be of help in writing. It

Table 1   Main phases of the research

Month	Phase
1–12	Early experiences in the field
9	Trialling CAQDAS using early interviews
13–14	Focused attempt to interpret these
15–18	Identification of key empirical themes and potentially useful theoretical literature
19–22	Focused engagement with theoretical literatures
23–27	Design and execution of more focused second round of field work exploring substantive grounded theory
28–44	Focused data interpretation and writing up
45–51	Submit, viva, final acceptance

contains a 'memo' function in which one can record 'aha!' moments in the analysis before they evaporate into the ether. It is also possible to write comments about the meaning of different codes attached to those codes in the software. In practice comments on the meaning of codes and recording of 'aha!' moments in the analysis can overlap, and it can be difficult to keep track of the different 'layers of textuality' (Cook and Crang 2007, 152) through which the interpretation is becoming embodied. All the different elements in ATLAS/ti can be displayed visually, and this can help thinking about relationships, although the graphics are neither particularly sophisticated, nor attractive.

The research trajectory

In this section I want to set out the main phases of the research and the place of data analysis within that overall trajectory. Although data collection, interpretation, and engagements with the substantive and theoretical literatures interpenetrate throughout the research process, it is possible (in retrospect) to identify phases in which a particular activity predominated (Table 1).

Having done a certain amount of fieldwork, I decided to try and bring this together by a burst of more focused data interpretation. During months 13 and 14, I identified a number of themes in the original research proposal and also used Strauss' idea of open and *in vivo* coding to generate more codes. Using ATLAS/ti allowed me to attribute the same code to lots of different bits of text, and easily retrieve that text. Because I was doing open coding, I quickly generated a large number of codes (nearly 200). The number of codes generated using ATLAS/ti will in part depend on how much of the data interpretation process one decides to undertake in

ATLAS/ti. The more one undertakes in ATLAS/ti, the greater number of codes, and the greater the risk of losing the overview of the codes.

I 'wrote through' the codes that had emerged as most important (social forestry, social learning, what is forestry for/about?, participation). The theme 'what is forestry for?' had emerged out of early fieldwork. An important moment in the interpretative process came when I was trying to write through the code 'social forestry' and think about what to do with text that indicated the code, but that did not explicitly refer to social forestry. I realised that both the data which explicitly made reference to social forestry, as well as the material which did not, indicated a bigger theme, the presence of a struggle about the ends and means of forestry as practised and regulated by the FC. Both types of material were best interpreted in this light. I therefore combined the codes 'social forestry' and 'what is forestry for?' in a third code, 'ideas about the ends and means of forestry'.

Further fieldwork during months 26 and 27 suggested to me that the FC was experiencing a crisis of identity:

> Hypothesis: Forestry in GB has been based on timber production at least since the creation of the FC. Over time, forestry for job creation, forestry for recreation, better design of forests have become part of forestry, but the raison d'être remained timber production. It is the core identity in GB forestry. This is what is being challenged. (Field notes 23–25 November 2002)

Drawing on Goffman (1959) and Giddens (1984 1991), I became interested in the way the FC was going about reinventing its identity in response to changes in its 'setting of action'.

> My observations of forestry so far point to the existence of a process of redefinition of what forestry

is about . . . a process of change towards goals which are . . . emerging as part of that process . . . What are the consequences . . . for people who work in forestry, for society and for the forest? What is forestry becoming through this process? (Research diary 19–20 February 2003)

Since it had also become clear that being a forester was an important part of the identity of staff working for the FC, I became particularly interested in the implications for the FC's organisational identity and the work-based identities of staff ('consequences' in the terminology of grounded theory).

Although I was drawing on grounded theory as an analytical approach, it was very hard to gain analytical purchase on the material and this only emerged as a result of struggling with the data. What, for example, was the epistemological status I was conferring on the data? Interpreting and representing the meanings of research subjects is standard practice in qualitative social science. There has been substantial discussion about the nature of such representations, sometimes referred to as a 'crisis of representation', although not everyone agrees that there is such a crisis (see, e.g., Flaherty *et al.* 2002 and Crang 2005 for discussion). Becker (2007) provides a helpful and enjoyable analysis of the different kinds of work involved in representation.

In the spring of the second year I had to prepare for my upgrading workshop and then during months 22 and 23 for the second round of fieldwork. These became important interpretative moments in the research. It was only as I was preparing the inter-view frame that the overall conceptual framework (or substantive grounded theory; Charmaz 2006) fell into place.

The second phase of fieldwork consisted mostly of 36 interviews. Coming back from fieldwork in month 27, I began coding the interviews according to my conceptual framework. In total there were more than 1000 pages of transcripts and field notes. I could not code much more than two interviews a day if I also wanted to be writing about the interpreta-tions that I was making by recording them in my research diary or by using the memoing function in ATLAS/ti. The interview frame had been constructed to gather more data on the conceptual framework. Therefore, the codes not surprisingly became very full as the coding progressed. I began to despair of the purpose of all this coding. I assumed I had to go back to reading them to develop the analysis further from there (constant comparison). This began to

look increasingly unrealistic the fuller the codes became. I developed additional codes to help me build theory and to decrease the size of individual codes. At final count there were close to 900 codes.

Organising, interpreting and writing up qualitative data is a lengthy and exhausting process, as the quote by Wax at the beginning of the paper indicates. It can also be emotionally demanding, and there are certainly moments when it can be difficult to 'face the facts'. Drawing on Malinowski (1948), Becker (1986) writes about teaching a class in writing and getting participants (graduate students, post-PhD researchers, as well as a few of his younger colleagues) to talk about the kinds of rituals they engaged in to deal with the anxiety-provoking aspects of writing. Discussion revealed that they were afraid of two things: first, '(t)hat they would not be able to organize their thoughts, that writing would be a big, confusing chaos that would drive them mad'; second, 'that what they wrote would be "wrong" and that (unspecified) people would laugh at them' (p. 4). This latter fear, says Becker, appeared to account for more of the ritual.

The theoretical and methodological chapters were relatively easy. However, writing up the empirical chapters was more difficult in spite of the ample fieldwork notes and research diary that I had kept along the way. In part I suffered from feeling that I did not know enough to say one thing or the other. A key challenge was to find a narrative structure that felt 'right'. I was able to lance the boil using the technique of 'free writing' (Wolcott 2001, 25–6; Becker 1986, 54).[5] The point is to keep writing without censure until a point of emptiness. I was only able to do this by conceiving of it as a letter to a close friend in which I was 'just telling him a little bit about this or that subject'. I dedicated a day or two to each empirical chapter and then left them for a while to get some distance from the material I had produced.

I had to take the emotional charge out of writing the thesis. It was helpful to imagine a sympathetic, non-judgemental recipient who had nothing to do with academia and to whom I had to explain all I had come to take for granted. The data collection process, as far as ethnographic methods are con-cerned, is a more or less controlled and closely observed socialisation process. Part of the difficulty of writing up is that, however attentive one has been throughout the fieldwork, what is learned in this process to some extent becomes part of one's taken-for-granted world. This is only brought out for

reflection with difficulty or through events that somehow shake up our routine (Giddens 1990).

Discussion

Seduced by Atlas

ATLAS/ti was probably a good enough choice in spite of my lack of perspective on the data analytical process when I was choosing between CAQDAS packages. It is important, however, to have a handle on the data analytical process before embarking on using CAQDAS, otherwise one may find oneself inappropriately socialised by the 'agenda' inscribed in the software. As Stroh puts it: 'It is easy to forget that using a computer immediately sets a particular relationship between researcher and their data' (2000, 235). Coffey and Atkinson (1996) make a similar point.

With hindsight, I can see that the fact that my codes were getting so full was an indication that my conceptual model *held*, my 'categories' were, to put it in Strauss and Corbyn's words, getting 'saturated':

> A category is considered saturated when no new information seems to emerge during coding, that is, when no new properties, dimensions, conditions, actions/interactions, or consequences are seen in the data. However, this statement is a matter of degree. In reality, if one looked long and hard enough, one always would find additional properties or dimensions. There always is that potential for the 'new' to emerge. Saturation is more a matter of reaching the point in the research where collecting additional data seems counterproductive; the 'new' that is uncovered does not add that much more to the explanation at this time. Or, as is sometimes the situation, the researcher runs out of time, money, or both. (Strauss and Corbyn 1998, 136)

Since starting the coding in month 27 I had been adding 'properties' and 'dimensions' to my categories and, as the large number of codes demonstrated, this was a potentially endless task. I could have drawn the conclusion that the conceptual framework held without coding up all 40 interviews. This was taking 'rigour' too far (Baxter and Eyles 1997). So despite my best intentions, I did end up getting seduced by CAQDAS inasmuch as the muscular effort of coding came to overshadow a reflected (and difficult) choice about how to turn the conceptual model into a thesis. I fell into precisely the trap against which Welsh warns:

> Because the electronic coding process is quick (compared to cutting and pasting pieces of text manually) it is possible that more coding will take place in a study which makes use of software than one that uses only manual methods, and it is not necessarily the case that this additional coding contributes much to an understanding of the data. (Welsh 2002, 4)

This would have been less likely to happen had I begun learning about ATLAS/ti on the basis of a firmer grasp of the qualitative data analytical process, both in terms of the analytical strategy I was employing and where I was situated in the trajectory of the overall interpretative process. We might even speculate that using ATLAS/ti became part of a kind of ritualistic appeasement of my anxieties about 'getting it wrong' (Becker 2007).

So to CAQDAS or not to CAQDAS? I remain ambivalent about ATLAS/ti. However, I have little to compare it with. After all, I have not tried to analyse data using a manually based system and it strikes me that one of the real benefits of ATLAS/ti is in re-coding different bits of data. The relative ease of coding may give more room for creativity. However, there are aspects to do with mastering (or just learning to live with) ATLAS/ti that require substantial attention and that therefore reduce the energy available for creative work (Cook and Crang 2007).

Running aground

I never did become truly comfortable with grounded theory. It appealed because of its explicitly iterative and emergent (Dick undated) approach to the research process, but I found the details of the approach as described in Strauss (1987) and Strauss and Corbyn (1998) unwieldy and too prescriptive. I did not engage with the Glaser school of grounded theory (e.g. Glaser 1998), although Glaser has criticised Strauss for being too prescriptive (Coffey and Atkinson 1996, 50). Subsequently, I have found Charmaz (2006) a more pedagogical way into grounded theory. The codification of the data analytical process that can be found in the grounded theory literature seeks to articulate something about the cognitive functions of the mind that we do spontaneously: attributing meaning to experience. We cannot help ourselves. Part of the difficulty is that the apparatus of grounded theory sometimes gets in the way of the very process it is meant to facilitate.

I continued looking at my 'picture' (my data) and its dimensions. Like Becker's (2007) photographs, it supported a large number of interpretations and I

could continue to attribute more and more meaning to it. This, says Becker, in the context of the interpretation of a photographic sequence, is why 'it is difficult – in fact, impossible – to settle on a definitive interpretation of such work' (Becker 2007, 53). If we have a fairly narrowly defined research topic perhaps knowing when to stop is easier. The grounded theory approach poses particular challenges for the novice researcher precisely because of its emphasis on openness and emergence. It is not surprising that the Grounded Theory Institute website includes the strap line 'Trust in emergence' (http://www.grounded-theory.com/index1.html). The opening up of the creative faculties of the mind as far as making sense (the attribution of meaning) is concerned is an important part of the apprenticeship to become a (social) scientist. However, it is also important to learn to gain closure, and this is perhaps more difficult. Research training could do well to address more explicitly (the pleasures of) creative meaning-making as well as (the pains of) closure.

Losing it

Writing up involves leaving all the theses that could have been written behind in favour of 'this one'. One has to accept that 'some, perhaps a great deal, of your hard-won knowledge and material will end up, as film people used to say, on the cutting-room floor' (Becker 2007, 31). The act of closure therefore touches on the part of the psyche concerned with loss (Holmes 1993; Davidsen-Nielsen and Leick 2003). I had gone through different levels of enthusiasm. At different moments I had imagined the thesis as this or as that. It felt like my great expectations, often shared with research subjects, had come to rather little compared to these imagined theses. Moreover, research requires a certain amount of personal investment, of sacrificing other things in life. This means that the more one has put into the thesis (or given up for it), the more one is likely to expect from it and the more difficult it may be to get it to completion because somehow what appears to be under production does not seem worth the investment one has put in (or the losses incurred). Using a wider angled lens to see the thesis itself as part of a broader process labelled 'doing a PhD' may help, since while the thesis itself may at times not seem worth it, perhaps the whole adventure of the PhD is. It is worth remembering, in the heat of the extended moment of writing up, that the thesis itself is not the only outcome. The emphasis now put on the transferable skills acquired during a PhD may help.

Keeping hold of Ariadne's thread: shifting between your narrative and their narrative

Bulmer (1979) has commented that, rather than theory being 'discovered' (Glaser and Strauss 1967) or emerging as a purely inductive process, grounded theory involves a constant two-way dialectical process or 'flip-flop' between data and the researcher's conceptualisations (Pidgeon and Henwood 2004). In writing up I was often caught between the fear of losing the stories of the interviewees and the fear of losing my own story, the narrative structure I was attempting to create in the empirical parts of the thesis. The essential problem was to have enough empathy/sympathy to understand the narratives of the research subjects, but not so much that I would get lost in their perspectives. This was a challenge even when I *had established* my narrative thread. In telling a research narrative I still had to dip into the narratives (or perspectives) of the research subjects, and this is when it could be difficult to get out again. This is the really difficult move to make: between trying to understand from the inside and describing from the outside in terms of the categories that the researcher has 'discovered' or as Pidgeon and Henwood (2004) put it more appropriately, 'generated'. In this context, referring to individuals as 'interviewees' or 'research subjects' or 'informants' may actually be a useful distancing technique, although such language objectifies the individuals one encounters in the field. Such challenges are inherent in interpretative social science concerned with understanding the life-worlds of research subjects.

Conclusion

I was supervised by an experienced academic who has successfully nursed many a student to completion and my department had a more than respectable research rating. Moreover the department was implementing the ESRC's new research training agenda for the social sciences. Like Bottomley (1978), nor do I not want to suggest that my previous studies, and indeed professional experience, had left me entirely unequipped to do social scientific research. Given this, I can only think that the trials and tribulations recounted here may at least have some resonance with other PhD students. Even if this is not the case, what of those of us who don't make it to the end? Perhaps in the above reflections there are some clues.

In this paper I have tried to give a frank account of what the data analytical process was like for me. In so doing, I hope to contribute to the reflexive

debate about social scientific research practice in an area that has received relatively little attention: the creative process of attributing meaning to data and the transformation of such meanings to written-up work. Although there are texts available that describe how to do data analysis, there are fewer texts that seek to describe what it can be like at the experiential level. This is an important missing part in the collective conversation about (qualitative) knowledge production. It would be helpful for those of us in training to be able to draw on multiple representations of the research process and the place and shape of data analysis in this. This will enable us to more easily situate our own experience in relation to a shared imaginary research and data analytical trajectories. One way of doing this would be to commission a piece of research that explicitly investigated representations of the (qualitative) research process and the (qualitative) data analytical process.

Acknowledgements

Notes

1 See also earlier work, such as Bell and Encel (1978).
2 Coffey and Atkinson provide a useful discussion of the definitions of data analysis in Huberman and Miles (1994), Dey (1993) and Wolcott (1994).
3 Inspiration is not without risk as the story of Archimedes suggests. Having found the answer to his problem, he exclaimed 'eureka!', leapt out of his bath, and ran naked through the streets of Syracuse [http://en.wikipedia.org/wiki/Eureka_(word)].
4 Arguably the iterative approach to research that Charmaz claims for grounded theory actually characterises a lot of qualitative research. It is beyond the scope of this paper to determine whether this is as a result of the influence of grounded theory on research practice.
5 For more on different ways of getting going, see Wolcott (2001, 25).

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