Love, hate and indifference:
The impact of talk about relationships on the interpretation of talk in relationships

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Introduction

Methodologically, one of the things that make linguistic ethnography so interesting is its willingness to confront the challenges posed by different data collection methods within the context of its broader programme, and in this paper I'd like to focus on just one of the issues this raises: the relationship between linguistic data and other evidence from the field. I'd like to suggest that researchers may not have given sufficient attention the dangers of privileging linguistic data over other sources and will explore briefly what the implications of this might be for how we position ourselves as researchers.

While issues of trust, researcher identity and field relationships have received considerable attention in the literature (for recent examples see Russell 2005, Gubrium and Koro-Ljungberg 2005), less attention has been paid to issues of deliberate or unintentional deception, except where interviews are concerned (see, for example, Gardner 2001, Sikes 2000). In the absence of any consideration of direct linguistic evidence as problematic, we might easily assume that it occupies a privileged status, issues of selection (always limited by practical constraints) and transcription aside. However, this paper examines the effects of two cases where undisclosed relationships were discovered towards the end of the data collection process and explores the implications of this for interpretive positioning.

The first and more conventional of the two cases will show how relevant contextual information derived from fieldnotes led to a reinterpretation of a previously discrepant exchange involving teachers in a staffroom and the subsequent bracketing out of one participant's perspective on an important topic of staffroom talk. A second — and much more problematic — case reveals how interview data revealed a hitherto unsuspected antipathy that did not, as in the first case, manifest itself in professional interaction but prompted a re-interpretation of hitherto neglected personal interaction. First, though, I'd like to consider briefly some responses to the issue of deception in research.

Deception as a methodological issue

Sikes (2000: 257) offers an interesting characterisation of the different challenges facing qualitative and quantitative researchers in the matter of deception: while charges against the latter usually arise from dishonest manipulation of data, the failings of the former are more often than not associated with researcher gullibility or naïvity. As she points out, though, in both cases, the weakness lies in the researcher rather than the paradigm. From a qualitative perspective, the response would seem to lie in the development of a level of methodological sophistication that minimises the likelihood of such gullibility, and there is some evidence that researchers using interviews have responded to this challenge.

Responses have concentrated on the interview as a constructed event and the implications of this for interview design and analysis, particularly the latter. Issues of identity construction and category work have received particular attention and researchers such as Baker have pointed to how both interviewer and respondent 'are involved in the generation of versions of social reality built around categories and activities' (1997: 131), and how researchers use membership categories to 'collect' interviewees (2002: 783). By attending to this process of construction and the generation of accounts, the researcher is at least able to explore different dimensions of representation (see, for example, MacLure 1993 sacred and mundane texts), however elusive these may be in the context of an interview society (Atkinson and Silverman1997).

Sikes' own paper (2000) focuses on two cases where she discovered only after the relevant research project had been completed that a respondent had deliberately deceived both her and the other participants in the study. The paper is particularly interesting because of her rigorous interrogation of the methodological options available to her that might have exposed this deception. There was nothing, methodologically speaking, that might have guaranteed protection against these carefully crafted deceptions, and her conclusion reflects a situation that obtains however diligent the researcher:

> …it does seem to be important for researchers to acknowledge and actually celebrate that, even with the most rigorous procedures available, informants can work research situations to their own ends, and that they are human enough to be deceived.   (Sikes 2000: 268)

In the face of this, researchers drawing on fieldnotes and interviews have adopted interpretive positions that emphasise 'researcher reflexivity, openness and sensitivity to different ways of knowing (Gardner 2001: 186), a sensitivity to relationships with participants (Harrison, MacGibbon and Morton 2001) and to the researcher's own disciplinary frameworks (Gubrium and Koro-Ljungberg 2005). At the heart of the enterprise, the boundary of trust and rapport, as Russell (2005: 192) notes, need to be delicately balanced.

There is, however, little evidence that similar attention has been paid to the challenges of using spoken data, though interest in methodological issues in conversation analysis (CA) is growing (e.g. ten Have 1990, 1999; Seedhouse 2004, 2005). In qualitative research generally, the lion's share of attention has fallen on problems associated with the process of transcription (e.g. Ochs 1979, Poland 1995, Green et al. 1997, Coates and Thornborrow 1999, Lapadat and Lindsay 1999, Tilley 2003), producing a somewhat imbalanced methodological picture which might even be taken to suggest that the challenges in using recorded interaction are located here and that the analytical process is less problematic. While this may not be a problem for experienced linguistic ethnographers, novice researchers might be encouraged by this distortion to privilege one data source over others. One way of redressing the balance of representation is to provide examples which present spoken interaction is a slightly more problematic light and it is in this spirit that the following examples are offered.

The deviant case
In a sense, my first example is fairly unproblematic but it seems to me to be interesting because of the way in which I was finally able to account for it. Briefly, the context of its occurrence is a research project exploring collaborative interaction in a small staffroom where the core staff had worked together for a considerable time. Participant observation over a period of 15 months, including extended periods, amounted to the equivalent of a term's full-time work during which I took extensive fieldnotes. These were supported by interviews and recordings, the latter including breaktime talk (every morning break for a term) and all meetings over the course of a term.

These teachers, in common with others, have ways of relating classroom anecdotes that follow fairly predictable patterns, and one of the features of such talk is that it is designed to involve colleagues in a shared assessment of the situation (Richards 1999) and of the student, who is often cast in a humorous light. The following is typical of such exchanges:

```
Extract 1(a)
01     Paul: Katsuko is still
02           (1.0)
03           a bit dreamy, (.) and I said (.) 'Are you a hundred
04           percent' (.) and she said ((non-verbal expression))
05    Keith: hhhhhe┌heheheh
06    Harry:      └Heheheheh=
07    Keith: =Heheheh
08  Annette: A hundred percent what?
09    Harry: She was late in.
10  Annette: °>Heheheh<°
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11      Paul: Yeah. An::d (.) sh- (.) 'Ye:s but
12            (0.6)
13      Paul: I:
14            (0.6)
15      Paul: it's difficult for me: to:'
16            (0.5)
17      Paul: and then she didn't ⌈finish the sentence.
18     Harry:                      ⌊Express myself. Heheheh
19   Annette: hhhh hehe⌈heh
20     Keith:          ⌊Hahah
21      Paul: An:d actually (.) she's working by herself.
22            °(You know)° and- because sh- I mean she- (.) she
23            looks as if- (.) >she looks as if< she's on drugs.
24            An- and I was worried because (.) sh- I said (.) she
25            was looking up a word and I said 'What are you
26            looking for?'  nd she said (.) 'rambling'.
27     Harry: hhhhhhheheh
28      Paul: t huh
```

What follows, however, is far from typical; in fact, Ed's defence of the student is the only one of its kind in the data (running to over 400 pages of transcript) and represents a deviant case:

Extract 1(b)
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29       Ed: Fair enough. ⌈Well why not.⌉
30      Paul:             ⌊And I thought⌋ may:be (.) maybe (.)
31            something her landlady has said to her.
32       Ed: We did ⌈e:::r
33   Annette:       ⌊Well see it wasn't anything (that had
34            come up ⌈in person ⌉
35       Ed:         ⌊No last er⌋
36      Paul: No no. ⌈Absolutely not.
37       Ed:        ⌊Last week we did e::m >(this you know)<
38            some work on types of er walking. That was what
39            it was. ⌈(xxxx)
40   Annette:         ⌊O:h right.
41      Paul: And she suddenly thought today (.)
42            'That's ⌈something I've forgotten to do.'⌉
43   Annette:         ⌊That she'd look it up,            ⌋ yes.
44      Paul: Yeah. 'I could ramble.'
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Qualitative researchers are familiar with the importance of the deviant case in the analytical process (e.g. Seale 1999, Erickson, 1986) and its function in the analytical process has been summed up by Peräkyla (1997) in terms of three options:

- Examples where participants orient to the unusualness of the instance, in which case they provide additional support for the analyst's position.
- Those that cannot be discounted and therefore require modification of the analyst's position.
- Cases that are demonstrably exceptional and can be accepted as such.

Although there is some evidence in my data to support the first option (an analysis of the above extract reveals, for example, Paul's use of 'and', first in line 30 to pursue his narrative instead of responding to Ed's comment, then in line 41 to extend Ed's account in order to highlight the absurdity of picking up a thought from a week before), it does not seem to me to be conclusive. But more to the point in the context of this particular project, it does nothing to explain the occurrence and I was left with the third option, methodologically acceptably but interpretively frustrating. I searched in vain through fieldnotes, interview data (especially that relating to Ed) and transcripts but could find neither anything to explain Ed's response here nor any other response from him that deviated from the norm.

The fieldwork was technically complete when I came across an explanation of this episode. As part of the process of leaving the field, I would spend the odd morning or afternoon in the school, chatting with the staff and perhaps helping out with the odd minor tasks. It was on one

of these visits, after Ed had left the school, that Harry happened to mention that he'd seen Ed around the town with his new wife — Katsuko. Unbeknown to any of the teachers at the school, Ed had become engaged to her during his time there, so his response to Paul's story was not in support of a student but in defence of his fiancée.

The case is therefore a straightforward one, but it raises two interesting points. The first is the almost accidental discovery of the relationship that explained the deviant case and the second is the way in which field data, rather than analysis of the talk, accounted for it. This might seem unimportant were it not for the fact that some conversation analysts favourably contrast their focus on the members' own production of talk as the material for study with the ethnographer's reliance on interviews or fieldnotes, which leave the process of situated production unexamined (ten Have, 1990). My aim in presenting this example is to suggest that such complacency may be ill-advised.

The disturbing case

My second case is more disturbing and in some ways less easy to pin down. This again emerged at the end of a project, this time one involving research scientists in the private sector. In this case, although I was able to observe at random points in the data collection period, my exposure to the field was much briefer than at the Pen, amounting to only one week of participant observation during which detailed fieldnotes were taken. This was supported by more than 15 hours of recordings made over a period of about two years (including meetings, common room and office talk), and interviews with staff over a similar period.

The research unit, Dots, comprised three groups: research scientists who had overall responsibility for projects, technicians and scientific officers who followed the various procedures and routines that underpin these projects, and, linking these two groups, a pair of senior scientific officers, Sue and Penny, who were responsible for drawing up weekly plans and ensuring that these were implemented successfully. It would no be an exaggeration, in fact, to describe Sue and Penny as the linchpins of the operation.

In terms of practical outcomes and professional relationships, the pairing seemed to work very well. Analysis of the discourse in a variety of meetings revealed a well-oiled working relationship that had developed efficient procedures and economical interactional routines. Observations and interviews supported this reading of the situation and revealed no evidence of tensions or rifts between the two. However, towards the end of my stay at Dots I had the opportunity to go out with the group to a local pub to mark Sue's departure to another institution and at the end of this, almost by chance, I returned to the unit with her and decided that it might be interesting to invite her views as someone about to leave the group. The result was an interview in which she revealed a deep antipathy to the colleague closest to her in the organisation, someone with whom she had shared an office for many years:

> She drives me <u>mad</u>. … Penny and I get on <u>really</u> well with the (finance) but she drives me <u>potty.</u> …I find her disorganised, lazy, selfish, unapproachable. <u>Really</u> unapproachable.

In terms of my research project, this intense dislike was of merely peripheral interest since I was interested in the professional interaction of group members, not their personal relationships (Richards in press). However, it came as something of a blow to my personal esteem as a researcher, making a mockery of any comfortable assumptions about my sensitivity as an observer. Personal pride drove me to dig deeper despite the fact that the project itself did not call for this.

I set up further interviews with one the two secretaries and Penny herself in which I was able to invite comments on the relationship between Penny and Sue, and the picture that emerged merely confirmed my original assumptions: although colleagues did not see Penny and Sue as personally close, neither did they perceive any tensions between them. The interview with Penny also failed to turn up anything which indicated that she was aware of the depth of Sue's feelings.

A close analysis of recorded exchanges revealed some evidence — slender but nevertheless demonstrably present — that their relationship was far from close. The exchanges in their office for example, avoided personal topics, instead relying almost exclusively on talk about the organisation, about the results of (work-related) web-surfing, or about their scores on computer games they played at home and in their breaks. On its own this merely revealed a lack of personal intimacy, but one that was atypical of the group as a whole. Only in the context of wider group talk did they find themselves engaging in talk on more personal topics, and here Sue's reception of teasing from Sue was not typical of the normal response to this in the informal setting of the common room. The following example occurs in a discussion of shopping habits and the use of lists. Sue's response to Julie that she occasionally buys things not on her list prompts a mild tease from Penny about her having a 'rebel streak':

```
Extract 2
01    Julie:  Do you ever buy something that's not
02            on the list
03            (0.6)
04      Sue:  Mm!?
05    Julie:  Do you ever buy things that aren't
06            on your list that you just
07            (happen ⌐to see).
08    Penny:          └Oo⌐:::h (.) real⌐
09      Sue:             └Yeah occasi⌐on┘ally.=
10    Penny:  =re⌐al rebel┐strea:k!=HEH⌐EHHeh┐eheheheh
11     Joe?:    └·ssssss!┘              |     |
12    Julie:                           |Heheh|ehehehheh
13      Sue:                           └Yeah.┘
```

Sue's response to this is interesting. While the others present laugh, she does not join in, merely reaffirming her response (line 13). As a po-faced receipt of a tease (Drew 1987), this is unexceptional, but Sue's failure to join in with the laughter, however briefly, is detectable in the data only when she is on the receiving end of Penny's teases. As an indication of her feelings about Penny, this barely registers, but her revelation in the interview had prompted a more careful comparison than I had originally undertaken.

Of course, there is nothing contradictory in this state of affairs: it is perfectly possible to maintain an equitable professional relationship in the face of an uncongenial personal one. In the Pen example, Ed's response in the extract is explained by the fact that although he is membershipping himself in the talk as 'Katsuko's teacher' rather than 'Katsuko's lover', it is the latter category which informs his response. In the case of Sue it is the category 'colleague of Penny' that accounts for her talk within the immediate working environment and one which she maintains, even though there are signs that at the professional edges (e.g. early in the morning or in breaks in the common room) the relationship is not an entirely comfortable one.

Methodologically, however, the almost accidental discovery of Sue's feelings raises questions about the status of interaction as an analytical resource. In the Pen case, the analysis itself was adequate, though the explanation had to be sought outside this; but here subtleties in the talk have been missed. Had my interest been in the relationship between the personal and the professional, it is possible (though I think unlikely) that I would have picked these up, but this merely adds to the suspicion that however carefully we analyse a text, without an equally careful analysis of the co-texts there may be aspects that we neglect. As Garfinkel demonstrated (1984), the analysis of spoken exchanges is capable of almost infinite expansion, but infinity is beyond our grasp and directionality is blinkered.

Responses
There are a number of responses that might be made to the above cases, and the only one that seems to me to be unacceptable is the assumption that if we take the right steps we can somehow insulate ourselves from the problems I have described. To show why I believe this to be the case, I'd like to address what seem to me to be the two possible types of response, focusing on the second and more problematic of my examples.

Methodological refinement

It might not be accidental that the more problematic case arose in a situation where data collection was less than ideal. The extended exposure to the field available at the Pen school was not possible here and participant observation was limited to a single week. Although interviews and recordings were made over nearly two years, it could be argued that this can never provide an adequate substitute for the process of shifting towards an emic perspective that extended field exposure provides. Perhaps, then, I missed the tiny signs that might have at least suggested a strained personal relationship between Sue and Penny, something that was hidden from their colleagues but perhaps accessible to the researcher. Alerted by Sue, I was able to detect tiny signs of distance between her and Penny in their daily interaction that I had missed in my initial analysis, and had I videotaped their talk there might well have been non-verbal signs pointing to a different line of analysis (I am grateful to Janet Maybin for this observation). Unfortunately, videotaping was not an option in this particular research context and is often very difficult to negotiate.

It could be argued that I'm making a mountain out of a molehill: after all, triangulation worked in the end, producing insights into the nature of a relationship that had never been overtly expressed and that had remained hidden from colleagues. The problem with this is that my discovery in both cases was really a matter of luck: I might easily have been elsewhere during the Pen break where I learnt of Ed's relationship (it wasn't part of research plan) and my data collection was technically over when I decided almost on a whim to follow up a farewell pub lunch with a last interview. Even particularly rigorous approaches to integrated methods of the sort described by Moran-Ellis et al. (2006) would not necessarily have discovered what fell into my lap by chance through additional and unpredicted exposure to the field. I suspect that luck like this plays a bigger part in data collection than we either recognise or admit, and the best way of encouraging it is to adopt a 'the-harder-I-work-the-luckier-I-get' philosophy, accepting the limitations of all data collection methods but recognising that the more aspects we can keep in play the greater our chance of picking up connections that we might otherwise miss.

Perspectival realignment

Responses under this head lay the responsibility for the problem at the door of the researcher-as-analyst. Limitations of data collection aside, and assuming that my analytical procedures were not seriously deficient, was I blinkered? Might a shift in perspective during the ongoing analytical process have revealed a more complex situation than I had assumed? In the case of the Pen it is hard to see any force in this argument, but my focus on professional relationships and professional interaction in Dots could easily have distracted my attention from the personal dimension. It's hard to say how far this was true, but there is an important methodological point here, and one which Eisenhart has noted:

> Conventional ethnography is not known for its attention to divisions, struggles or inconsistencies within groups. In conventional ethnography the analytic focus is on identifying what is patterned (Wolcott, 1999) or typical (Erickson, 1979, 1986).
>
> (Eisenhart 2001: 23)

Perhaps we do need to pay more attention to such strains and fractures, but to carry this further and assume that attending to contradictions or focusing on potential disjunctions will somehow serve as a methodological prophylactic seems to me to be dangerous. In directing our attention too confidently in any particular direction in order to counter the risks associated with a different perspective, we are in danger of simply substituting one distorted perspective for another. In this respect it also needs to be noted that my discovery of Sue's — apparently unreciprocated — feelings came very late in the day and was not integrated into ongoing data collection and analysis, therefore raising questions about the cogency of my analysis to that point rather than feeding into a more richly articulated account. My subsequent re-analysis merely exposes a lack of analytical penetration, rather than providing an adequate basis for interpretive repositioning.

My final point is perhaps more contentious. There is nothing contradictory about sustaining a harmonious professional relationship at the same time as maintaining a deep personal antipathy, and my discovery of the latter (belatedly and perhaps lamentably) offers no more than a window onto a more complex relationship than I had originally identified. It is perhaps helpful to remind ourselves that the social world allows such contradictions, that the associations through which knowledges are formed are, in Gardner's terms (2001: 194), 'thick, heterogeneous and complex', but the recognition of this is the beginning of understanding, not an analytical end-point. What the analyst pursues is not an appreciation of the 'messiness' of the social world, but some understanding of its infinite complexity and surprising subtlety (I am grateful to Ben Rampton for this observation). If analysis exposes order, the problem is not that this is a chimera, it is rather that it is but one of many possibilities:

> To research at all is to place and create order(s) on the world. It is never a question of whether or not to have order: the question is which order to choose.
>
> (Blunt and Rose 1994: 5-8)

This is a far cry from the original aim of this paper, which was merely to provide a couple of examples underlining the dangers of privileging any single method and highlighting the incidental nature of discovery in the field. However, it points to the essential incompleteness of research into the social world. Perhaps in addition to treating participant evidence in the data as the product of this-here-now, we need to recognise that our own perspective is also the product of this-here-now in the context of ongoing discovery. All research, and all its products, are therefore no more than works in progress.

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