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THE TEXTUAL DISEMBODIMENT OF KNOWLEDGE IN RESEARCH ACCOUNT WRITING

JUDITH ALDRIDGE

Abstract   Recent writers on autobiography and biography have questioned the extent to which the text represents a set of 'facts' about 'a life'. The following arguments question the extent to which the research account can be seen to represent a set of facts about the research process, in a simple and straightforward manner. Concepts problematized by writers on autobiography and biography, including authorship, self, time and memory, are used here to re-embody what will be argued is the textually disembodied knowledge contained in most research account writing. How simplistic invocations of these concepts can be seen as hinging on actually very complex textual constructions will be explored. The arguments have their source in two pieces of research writing; the first is a survey research account of some research undertaken for a master's thesis; the second was produced as a re-appraisal of that thesis, the purpose of which was to explore the more personal, autobiographical aspects of that same research process.

Key words: autobiography, biography, survey research, research accounts, author-ship, self, time, memory, textual analysis.

Introduction

The current focus in social scientific literature on metatheorising – analysing and theorising theory – is the legacy of the mainstream tradition of social science. Social scientists have long puzzled over epistemological issues that challenge the validity and grounding of their theorising. More recently, however, epistemological questions have begun to take centre stage in serious intellectual debate. Social scientists seem unwilling to put forward their claims without earnest examination of the philosophical, rational and moral validity of making intellectual claims to truth. Thus, insights by many feminist con-tributors now have resonance for the whole of social science, where these writers raise the suspicion that transcendental claims to truth often reflect the experiences of white western males (or of late, white, middle-class, first world feminists). This epistemological concern is similarly reflected by critics who identify a postmodern condition, and object to the idea of a coherent and stable self with insight into societal mechanisms.

One consequence of the trend toward considering epistemological issues has been the scrutiny of the role played by the social scientist in the production of knowledge. Writers now often insist on the need to reveal, understand and analyse, not only the *product* of knowledge but its *production* and therefore, its

producer. Nancy Miller, for example, discusses how her personal identity and experiences form her academic argument within the field of literary criticism. She advocates a kind of writing she ~alls 'personal criticism', entailing 'an explicitly autobiographical performance within the act of criticism' (1990:1). Liz Stanley similarly urges sociologists to make central in analytic writing what she terms 'intellectual autobiography'. Such writing involves, not the issuing of personal data along with the writing up of research, but the 'teasing out of *how* research processes are understood [by research writers] so as to produce any particular product' (1990:120). As readers, we are then able to engage actively with a text whose author opens up to us how the research processes are interpreted and understood by the researcher. For example, as Stanley demon-strates, the issuing of an 'elderly statistic' requires a host of interpretive procedures by health professionals and researchers alike – interpretive pro-cedures normally masked by standard 'these are the results' approaches to research account writing.

Notwithstanding these autobiographical moves, many aspects of social scien-tific writing remain untouched by concern to make explicit the mechanisms by which academic research and writing are accomplished. Especially in quanti-tative research, all trace of the production and producer has traditionally been expected to be wiped from research account writing. In spite of historical social scientific precedent to attend to epistemological issues, writers of quantitative research accounts for the most part proceed as though epistemological concerns are either implicitly unproblematic, or irrelevant to their task.

It is my contention, however, that epistemological concerns about claims to truth in quantitative research account writing are neither unproblematic, nor irrelevant. On the contrary, a host of rhetorical procedures are required to disentangle in writing the producer and the production of knowledge from the product. The simplistic epistemological ideas assumed in such writing actually hinge on complex writing conventions that 'textually disembody' the know-ledge contained from its time, place and person of production. In other words, the production and the producer of knowledge are systematically *removed* from the rich and diverse experience of the research process, in quantitative research account writing.

The Auto/Biographical Connection

The connection between the argument to be made here and auto/biography[1] in sociology exists on three levels. Firstly, to explore the process of doing a certain type of sociological writing is to attend very closely to what *sociologists* do. Sociological method texts, I would argue, deal rather with what sociologists contend happens when research is carried out, and *not* with how sociologists go about the process of translating 'the research' – a multi-faceted experience in time – into a piece of writing. This latter experience is rarely written about in a

standard research account, but is instead systematically stripped from it. The writing, and the research of which the writing is an account, constitute two very different things, the writing not simply being a direct representation of the research. How sociologists accomplish this representation – the techniques, the rhetoric, what is emphasised, what is stripped away, what kinds of words are used and not used – is what Paul Atkinson (1990) refers to as the 'poetics' of sociological writing (though in connection with his work on ethnographic writing only). By poetics, Atkinson understands the study of 'the conventions whereby the texts themselves are constructed and interpreted' (1990:3).

This focus on the *doing* of sociological writing, or on the 'poetics' of sociology to use Atkinson's phrase, is one way that the sociologist as creator of meaning (as opposed to apparent channeler of disembodied knowledge), can be made visible. The resulting shift in investigative focus from the knowledge product to its producer is a shift that opens to our investigation the socially situated individuals that produce sociology, and their interpretive experiences of doing so.[2] And it is this shift in focus that I identify as a sort of autobiographical move. The analysis that will follow represents an attempt to demonstrate this particular autobiographical move in an analysis of one particular account of survey research writing.

The second connection with auto/biography in sociology rests with the kinds of issues explored by some of the writers who deal explicitly with theoretical issues around auto/biographical texts (e.g. contributors to Benstock 1988; contributors to Brodzki and Schenck 1988; Conway 1990; Denzin 1989; and Stanley 1992). The issues explored by these writers are many and varied; some have resonance for me in thinking about research account writing. Many of these contributors question in particular the notion that auto/biographical writing represents a set of 'facts' about a 'life'. In research account writing, I want to question the assumption that the research account represents, in simple one-to-one correspondence, a set of 'facts' about an experience in time of 'research'.

Some of these writers on auto/biography also problematize the concepts that comprise the very currency of the auto/biographical endeavour: authorship/ self, time, and memory. I will argue that these concepts are implied, through their absence in much research account writing, to be unproblematic, and simply the currency of our thinking and writing about research. I will further argue that when we *re-embody* the textually disembodied knowledge that is much of research account writing, the simplistic invocations of authorship/self, time and memory can be seen to hinge on actually very complex textual conventions.

The third connection with auto/biography in sociology occurs on a much more direct and obvious level. The particular account of research account writing I will deal with is a survey research account that I produced for a master's thesis in Canada (Aldridge 1990). Shortly after beginning a Ph.D. in England some months later, I wrote a paper (Aldridge 1991) re-appraising the

thesis. The purpose was to 're-embody' (in some preliminary ways) the disembodied knowledge contained in the thesis, and the effect was a more personal, autobiographical version of the research events than that contained in the thesis. The arguments that follow, therefore, have as their source the experience of re-writing and re-thinking a particular research experience.

Thus far I have directed my comments to the writing of accounts of *quantitative* research. However, albeit in modified ways, exactly the same arguments pertain to *qualitative* research account writing. While, for example in ethnographic writing, there is no standard move to excise presentation of the researcher's presence within the research, there are still standard structural and rhetorical means of producing and presenting such accounts which 'scientize' them and the researcher who produces them. My case study, discussed in some detail below, concerns a piece of survey research, but it could equally well have concerned a set of interview data, or an ethnography, or some other qualitative approach.

The Research Account – A Set of 'Facts' About 'The Research'?

Various writers on auto/biography have challenged, to greater or lesser degrees, the extent to which an auto/biographical text can be referential of a life (Brodzki and Schenck 1988; Denzin 1989; Derrida 1976, 1987; Graham, Hinds, Hobby and Wilcox 1989; Jelinek 1980; Stanley 1992; Stanton 1987; Stein 1988), even though the standard auto/biographical text is generally presented as a set of 'facts' about 'a life'. By separating out for discussion the textual self from the referential self, these authors highlight the possibility that the self is *constructed* in auto/biographical writing, rather than being fully-formed, and then *represented* (either partially or in total) by the auto/biographer.

The challenge to the tenet of auto/biography as referential of a life takes various forms. The problematic nature of memory is cited by some authors, which at best is seen as selective, and at its most damaging, as socially and temporally constituted activity, and thus constructive – not simply selective – of a life (Olney 1980; Hankiss 1981). Time and chronology are also problematized by authors who point out, for example, that what is salient in the present is used to select and construct in the text relevant elements of the past (e.g. Kohli 1981). The inevitable intertwining of the lives of the writer and the written supplies still further challenge to the artifice of auto/biography as referential of a life. The life that is written becomes a product of the temporal and contingent location of its author, as well as of that author's individual relevances and concerns. Conversely, the experience of writing the life *itself* becomes part of the life of the author, and thus part of his or her interpretive and analytic 'autobiography' (Wise 1990).

Writers taking a postmodern approach question the idea of a stable or coherent self that can be known or written, and further object to the possibility

that a text can have genuine referent to anything other than the text itself (e.g. Derrida 1976, 1987; Stanley 1992). Some challenge the tenet of auto/biography as referential of a life by acknowledging the possibility, even inevitability, of the active reader, whose reading of the text is not limited by the interpretation of the autobiographer (Lury 1982; McHoul 1982; Derrida 1987; Stanley 1992). As these writers have challenged the tenet of autobiography as referential of a life, I will argue that to conceive of the research account as a set of 'facts' about 'the research' obscures much of the process involved in moving from the research event to our written accounts of it.

Most sociologists present research account writing as referential in a straightforward way of the research process – a particular set of events in time. My thesis was no exception. According to the chronology of events as outlined in its text, ostensibly I as researcher: studied a body of literature; discovered a 'gap' in that literature – some hypotheses as yet untested; chose a survey as the suitable method to test these hypotheses; assembled the relevant measures; collected data; analysed the data as an appropriate test of my hypotheses; and, finally, drew fitting conclusions.

But as I argued in the later alternative account of the research (Aldridge 1991), a case can be made that the entire account of the research in the thesis was in many respects an artifice or construction. The hypotheses made in the thesis were not, in fact, derived from the literature as the thesis account suggests, but from hunches and personal experiences. And, the data were gathered before I even read the literature. Faced then with a large data set, and lacking a clear set of questions with which to interrogate the data, I finally approached the literature in order to find hypotheses that could be derived from it, and that could be sensibly, realistically and legitimately be tested with the existing data.

I argued in this alternative account, therefore, that the conventional survey research account in the thesis was only one possible construction of the 'reality' of the research. I could account for the development of hypotheses with reference to my personal experiences. Or as another alternative, the research could be accounted for by exploring my own negotiation of the way that the politics and academic leanings of the department in which the thesis was written were understood by me in an attempt to write the kind of thesis that would be seen to demonstrate scholarly proficiency. This negotiation of the academic climate affected not only the way the initial project was conceived (in conjunction with the 'hunches'), but also the way that the research process was textually accounted for in the thesis. In the thesis, these personal aspects of research were played down, and the technical and impersonal aspects played up.

In my alternative account, the writing of the research account was therefore understood as a partial, personal, and limited construction and reconstruction of the research event, in many of the same ways as the auto/biographer constructs the life rather than records it in a straightforward manner. As

memory, time, and authorship/self, when problematized, reveal the inadequacies of conceiving auto/biography as referential of 'a life', the dangers of presuming research account writing to be referential of 'the research event' can be explored. In what follows it will be argued that the treatment of authorship/self, time and memory in research writing (a treatment that actually hinges on complex textual constructions) comprises the strategies or tactics through which research account writers rhetorically maintain that it is legitimate science that they do.

Authorship and Self

Rare is the sociological writing that does not contain reference to the ideas of other sociologists. In fact, situating our work through the use of the literature review is considered to be integral to participation in the scientific and sociological community. But more than this, it is only through reference to the work of other sociologists that we demonstrate ourselves to be researching and theorising in *scientifically* meaningful ways. Ideas and hypotheses to be researched must be shown to build upon or derive from (even if they transcend) existing research and theorising. Thus, making reference to other sociologists is a way of rhetorically demonstrating scientific validity for the research.

Going about the business of 'textually' deriving my hypotheses from existing research, therefore, was a way of textually accounting for what was in fact a very different research process in such a way as to lend scientific credibility to the text. By writing in this way, the actual research experience was stripped from the textual account of it. It is this process that I call the 'textual disembodiment of knowledge'.

One technique sociologists use to make reference to the work of other sociologists is particularly illustrative here. Sociologists do not simply summarise each of the texts of a literature in their review; the texts are instead clustered together according to some attribute identified by the writer of the review. Thus, the writer of the literature review might write, for example, that *all* researches into a particular area of sociology fall into three main types, or perhaps, draw on two different theoretical frameworks. The nature of the clusters identified by the sociologist is used, therefore, to impose a framework upon the body of literature as a whole and to place the original authors within it. And the clusters or categories into which the original contributors are placed will often represent a unique or 'trademark' contribution to a literature *in itself*.

More specifically, however, what happens when sociologists use this technique is that they explicate the unexplicated – the perhaps unstated intentions, meanings or implications of the author of the text – and in doing so deal not just with 'texts' but with 'authors'. Stanley and Wise (1990:45) illustrate this process in their borrowing of the idea of 'reading generously' or ungenerously the work of other theorists. Implied here is the notion that presumptions from

outside the text regarding the author's meanings and intentions are brought to any reading and interpretation of that text. Authors, therefore, are constructed, not referentially from their texts in a straightforward manner, but *by the sociologist who invokes them*. In writing the review of the literature, therefore, the sociologist constructs 'the author' from the author's text.

In part we demonstrate, again rhetorically, scientific credibility through the textual construction of authors. We show how we can stand outside the text, view it objectively, and understand what is presupposed or implied by the authors, even if those authors remain unaware (in fact, our credibility can be seen as even greater when we establish that the authors we have constructed *are* unaware of their own pre-suppositions or the implications of their arguments). We thus establish, textually, that our vision is wider than that of the author we have constructed.

The impulse to treat other texts in this way – to construct authors from them and then to see beyond them – became apparent in the writing of both the original, thesis version of the research, and the later, alternative auto-biographical version. This construction of authors occurred on two occasions. First, by attempting in the thesis account of the research to situate, textually, the research within the scientific literature, I constructed a 'reality' that had not existed prior to writing it in order to demonstrate adherence to scientific procedure. More specifically, I constructed a textual scenario that implied that my hypotheses were derived from a series of generalisations and propositions provided by other researchers – other authors – though in fact the hypotheses were not derived in this way. The second point at which the construction of authors occurred was when writing the more autobiographical account of the research. In that paper, I was required to construct *myself* as author – in this case, author of the original thesis.

To lend rhetorical credibility to this autobiographical account, I was required to construct myself as author of the text of the first account of the research, the thesis. Re-reading the autobiography paper, I can see a tension between, on the one hand, myself-as-author standing behind the second account of the research, and arguing for its broader vision; and myself-as-author needing to construct an 'author of straw' of the first account, having only a narrow vision, but constructed as such only to be torn down. This tension stood alongside the desire to argue that each version of the research – thesis and autobiography – was valid and meaningful.

At the same time that we construct other authors, other selves, from texts in research account writing, we also construct for public consumption a 'self' in the form of our *own* authorship. The autobiographical account of my research contained statements like 'I thought then that . . .', and 'I now would be reluctant to draw such sharp distinctions . . .'. This account constantly invokes, therefore, a 'self' in the form of an author to be understood by the reader. This process, moreover, stands in variance to the usual move in quantitative research account writing to excise one's 'self' from the text to provide rhetorical

scientific credibility (in qualitative research account writing a similar result is produced often without excising the self altogether, but using different rhetorical means to construct, typically, a 'scientific self').

In standard textual accounts, we are taught to say not 'I thought' but 'it was thought' and not 'I used a random sample', but 'a random sampling procedure was employed'. It is by excising the 'I' from the account – from our accounts of our personal experiences of the research – that we are seen, rhetorically, to be doing science. But does 'a random sampling procedure was employed' really accomplish the task of excising the writer from the text? Or does it rather construct a certain kind of writer? Perhaps as readers we are provided, not with less information about the author of such a text, but with more information of a different kind; at the least this includes what the writer holds to be standards of appropriate scientific writing. We are, after all, nonetheless aware that if a random sampling procedure was chosen, usually it was chosen by the person whose name appears as author of the text, whether 'I' is used or not.

The importance of active readership is underlined here, because how particular rhetorical devices are understood will depend at least in part on who is reading the text. Many research account writers feel strongly about *not* using the first person in their writing (except perhaps in specially sanctioned places like forewords or acknowledgements), and others feel especially strongly that the first person be used where appropriate. Subsequent use then provides the reader with extra information about the writer. And the preferences of readers will in turn affect their judgements, and, perhaps, subsequent constructions of those authors (as, possibly, 'a sociologist with a standard or conventional approach to research', as opposed to 'a sociologist working on the periphery of the discipline', or even 'a sociologist pushing back boundaries of the discipline'). A further example of this can be seen in the use of male nouns and pronouns to refer to people of both sexes, versus gender neutral or inclusive language. Here it has become less possible to use either strategy without a conscious awareness of placing oneself in one particular camp. And again, a reader's reading of the text is active, and not limited by the interpretation of the writer. We thus manage to pack in a great deal of auxiliary information regarding authors and selves, both as writers of texts and as readers of them.

Time and Memory

When we read a research account, we do not generally doubt that the events referred to in the account actually took place; nor are we likely to wonder if events took place in a different order. Though we may feel the critical imperative to be sceptical on many other levels, we rarely feel the need to challenge an account at this most basic level. And yet, it is at this most basic level that an account must always be a fabrication or construction of events. The reshuffling in the textual account of the parcels of time that comprise the

research process is a further tactic used by sociologists to disembody knowledge from its time, place and person of production. As I have argued earlier, the writing of the thesis account of my master's research involved fabricating a textual construction of the research process. The textual reshuffling of time occurred on two levels.

The first, somewhat superficial level, on which time is re-shuffled is the order in which research events are *presented* in the account. The text to be presented first is the introduction of the research question. This is followed by a description of the method chosen as appropriate to answer the question, an account of how the data were collected, an analysis of the data, and finally, a discussion of the meaning and implications of the findings. Virtually all method texts that advise on writing research accounts recommend this ordering for presentation (see for example Moser and Kalton 1971). But in spite of the fact that this order will rarely if ever vary, writers of research accounts almost never actually *write* the text in this order.

Although the text of the research account implies, by its order of present-ation, that it was also written in that order, rarely will this actually happen. The writing of later sections will require the revision of earlier text, and the converse will also be true. Of course, this is neither surprising nor particularly trouble-some – it simply points to the convention that research accounts will virtually always follow a particular sequence of presentation, even though researchers are unlikely to have written in this sequence. It is even expected that we will write the account in any order we please; we may not, however, present the account in any but one specific order.

The second way that sociologists reshuffle time in research account writing is perhaps more significant. The research account implies not only that the account was written in a particular order, but also that the research *event* of which it is an account actually *occurred* in that order. In the thesis account of my research, it is implied that a review of the literature occurred first, followed by the development of the hypotheses, followed by the choice of method. In fact, however, I began with the method.

The most usual demonstration of academic and sociological proficiency leading to a passable thesis in the department in which I did the master's research involved using highly quantitative survey methodology and carrying out sophisticated statistical analyses. Faced with the task of making a written account of the research, and armed with some hunches based on personal experience, I devised research questions that could be approached using the survey technique. After the data had been gathered, it became a matter of 'playing both ends against the middle': devising a hypothesis that might have resulted in the collection of data that I had already gathered; evaluating how appropriately that hypothesis could be tested using sophisticated statistical techniques; and finally, examining the literature to see if the hypothesis could sensibly, and appropriately, have been derived from it. Aside from the obvious inadequacies of this approach to research, why was it inconceivable to have

textually accounted for the research in the way that it actually occurred? Why do we make sure that our textual accounts of the research process imply that the process occurred in a particular order, even when for most of us, to a greater or lesser degree, the process will not have occurred in that order?

One answer is that this temporal ordering accomplishes the task of textually disembodying the knowledge from its production by excising, by cutting away, the researcher from the process of carrying out the research. The standard ordering in which research must be textually seen to be done takes creativity, insight and intelligence from the hands of the researcher, and places the guiding force of scientific inquiry squarely in the hands of scientific method and technique. It is by excising the sociologist and his or her personal experience of the research process from the textual account of it that we are seen to be doing science *best*; indeed, that we are seen to be doing science *at all*. Thus, if I textually accounted for the process described earlier (i.e. deriving hypotheses based on personal experience, and on ideas about negotiating the academic climate in order to produce a passable thesis – but then ignoring this and 'textually' deriving them from the literature), I would not have been demonstrating suitable scientific procedure.

The argument here should not be seen to suggest that all researchers textually account for their research in a way that implies hypotheses were derived using deductive reasoning, and that in fact, researchers actually proceed in an inductive manner. Even researchers working in a consciously and deliberately inductive way are unlikely to have actually proceeded in a straightforward inductive manner in the research process itself, though their textual account of the process will imply that they had. The point here is rather that whether we textually account for hypotheses inductively or deductively, there will have been a complex and mutually influencing interplay between theory and ideas on the one hand, and the development of testable hypotheses on the other. In my research, the ideas and hunches that preceded the research were among the factors that guided my choice of measures on which data were gathered. Theory and generalisations provided by other researchers refined and extended what became, increasingly, formally stated hypotheses. Neither one process nor the other came 'first'; though in the textual account of the research, it appears as though hypotheses were derived in a uni-directional manner, from only the literature. It is the complexity of the interplay between inductive and deductive reasoning, coupled with the removal of 'illegitimate' events and sources of inspiration from that reasoning process (such as personal experience, or the political negotiation of the academic environment in which the research is carried out, and so on) that is excised from the textual account.

The personal experiences of doing scientific research should not be textually seen to have existed because according to scientific rhetoric it should not matter who does scientific research. Who we are, why we did the research, and how we may have broken rules along the way, are aspects of the research process that make it other than conventional science. The class, race, or gender, or any other

aspect of the biography or social identity of the researcher, is seen to be made irrelevant, at least in part, through strict adherence to the scientific canon of temporality.

Another example of temporal regulation is seen in the determination of causality (for a discussion of social causality, see Hage and Meeker 1988). The event that is considered to be causal must precede in time the effect in order to be considered causally related to it (though there are other considerations as well in determining causality). But time is a wholly socially constructed convention, and thus a human creation (see Adam 1990 for a discussion of time and social science). Science, which allegedly transcends the limitations of the social, relies heavily on this social construction. Ironically, then, the scientific method, ostensibly providing us with the techniques to transcend our social selves as scientists and sociologists, in part does so through an insistence on the temporal regulation of scientific endeavour, while the fact that temporality itself is a social construction remains unproblematized.

As recent writers have convincingly argued (see, for example, Middleton and Edwards 1990), memory is not a simple, psychological, mental event, but rather a collective, social and interpretatively and communicatively located pheno-menon. If remembering depends on the social location of the researcher, then memory must be a process that evolves as time and the relevances of the researcher evolve; and of course, this will happen constantly even during the research process itself. Memory here takes on a multi-layered quality, where the context of the construction and re-construction of the memory plays a role in its invocation, and re-invocation.

The making of meticulous notes during the research process does nothing to displace the problem of memory. Notes are simply accounts of the research process, and, as such, partial constructions of it. The textual *absence* of memory as a device used in research from our accounts of it, however, points again to the stripping of the individual and the complex experience of producing research from our accounts of it. By excising such complexities, our texts are shown, though only rhetorically, to be evidence of science, and not some other activity, being done.

Conclusions

I have used my own experience of research (perhaps as a 'failed' researcher, since I did not follow standard scientific advice in carrying out the research, even though the textual account of it was by no means a failure) to explore the textual and rhetorical conventions used in writing survey research accounts. But I hope it has also been made clear that *all* such accounts (even those by researchers who followed standard scientific advice) necessarily rely on rhetorical devices and textual conventions; some variant of these is fundamental to all writing.

I have attempted to show that the excision of our experience of research from our textual accounts of it occurs only rhetorically. We must be seen to derive hypotheses appropriately from existing literature and thus from intellectual rather than from personal experience, where the intellectual and the personal are seen as very different planes of experience. We construct authors from their texts in order to lend breadth of vision to our accounts. And we construct for public consumption 'selves' as authors from rhetorical devices that include what we put into the text, as well as what we leave out. We imply that time and memory are only the non-problematic currency of rationality and thought, but strip our complex experiences of time and memory in the research from the accounts we give.

Perhaps research account writers need not excise their experiences from the research, but should instead use these experiences analytically. This article should be seen as one such attempt to use analytically my experiences of writing both a standard, conventional survey research account (my thesis) as well as a less conventional autobiographically-based account of that same process. But of what sociological use is this autobiographical move, as I have termed it? Being sensitized to complex research experiences will not secure any more faithful or accurate a recording of the 'truth' of the research process, nor provide a more conventionally complete account. It will, however, highlight aspects of the research process normally hidden from view. And moreover, it brings into focus some of the rhetorical techniques that are (most often unwittingly) used by research account writers.

I would not want to argue that alternative accounts should be free of rhetoric and textual contrivance, or that they even could be. Such is the nature of any text. The point to be made here is that, whatever its appearance, no text is free of rhetoric – even in much highly technical writing, as I have attempted to show for the survey research account. Though perhaps we can never strip the rhetoric from our writing, we can analyse how it is used, the possible reasons for its use, and the effects on both writer and reader.

Notes

1. The word auto/biography appears in this form following the conference 'Auto/Biography in Sociology' (Manchester, 2 January 1992) where the paper on which this article is based was given. It reflects the ontological and epistemological overlap between the two kinds of writing.
2. Survey research account writing that speaks explicitly to the personal experiences of doing the research writing is virtually absent from the quantitative survey research literature. Notable exceptions include Farran (1990), Pugh (1990) and Stanley (1990).

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