Calling out for More: Comment on *The future of interpretive accounting research—a polyphonic debate*. Thomas Ahrens and Fourteen Others.

Peter Armstrong

Author's note

This article is a comment on an e-mail correspondence concerned with what its authors called 'interpretive accounting research'. In the apparent belief that the exchanges would prove illuminating, they were submitted for publication in *Critical Perspectives on Accounting* but were accepted only on condition that the same issue would carry comments by three academics of the editors' own choosing. The three chosen were Jesse Dillard of Portland State University, Hugh Willmott of Cardiff Business School and the present writer. The article is best read, therefore, in conjunction with the original exchange of e-mails, and perhaps the other commentaries too. These, together with additional contributions on the topic, can be accessed through the journal's website at:
http://www.elsevier.com/wps/find/journaldescription.cws_home/622813/description#description

I was feeling kinda seasick
But the crowd called out for more
Procul Harem

Abstract

This essay is a response to the 'polyphonic debate' on interpretive accounting research (Ahrens et al., this issue). The debate is considered first as a case study of the limitations of collaborative circles (Farrell, 2001) as a means of developing a particular approach to research. Whilst they may serve to reassure the individual that s(h)e is part of a consequential community of practice, they tend to do so at the expense of suppressing the mutual criticism on which the development of clear and robust ideas depends. Further, because such circles aim to promote their approach as well as define it, intellectual development tends to become confused with, and possibly subsumed by, the politics of academic career-making.

The paper then considers the case for interpretive accounting research as such. Because the polyphonic debate itself is uninformative in this respect, this consideration is based on a theoretical paper by Ahrens and Chapman and an ethnographic case-study of organizational control by Ahrens and Mollona (both forthcoming). The burden of both papers is that interpretive research offers a radical alternative to 'traditional' positivist approaches and that it possesses a unique potential to contribute to theory.

Neither of these contentions are borne out. The anti-positivism conventionally attributed to interpretive approaches is shown to be based on a misunderstanding whilst Ahrens and Mollona's illustrative case study turns out to be itself positivist and also to contribute very little to theories of organizational control.

This last fault may lie as much with the expectation as with the ethnography itself. The kind of theory developed in the course of ethnographic interpretation is short-range and situation-specific rather than abstract and general. The paper concludes with a brief consideration of the manner in which the two levels of theorisation might relate to one another.

Keywords: interpretive accounting research, ethnography, positivism, collaborative circles

1. Worries about identity

This collection of e-mails, two each from fifteen correspondents, might be regarded as a kind of post-completion audit of a series of EC sponsored conferences through which it was intended to set up a network of young (under 35) accounting researchers during the late 1990s. Nine years on, the fifteen participants, some of them now entering early middle age, reflect on the achievements of and prospects for what they call 'interpretive accounting research'. Quite what *folie à quinze* suggested that the case for the approach would be advanced by the publication of these musings is hard to imagine. Formally speaking they belong to a genre I might label, 'call-for', a lazy and largely redundant form of writing which has all-too-frequently substituted for a concrete demonstration of what the approached 'called for' can achieve.

We should, nevertheless be grateful. Whilst we do not learn much about interpretive accounting research, we learn rather a lot about the mid-life crisis as it impacts on the individual academic and, relatedly, on an intellectual niche. Viewed in this light, the e-mails make sombre reading and could well be taken as a warning.

The exchanges begin confidently enough. It is asserted at the outset that interpretive accounting research, like some of the researchers themselves, has come of age. As we read on, however, we discover that it is not a contented maturity, not one in which the search for meaning in intellectual labour has had a happy, or even an identifiable outcome. Despite the determinedly upbeat urgings of the head coach ('This is one of the most productive projects I have known! '), one of the contributors is actually moved to use the phrase 'existential angst'.

Part of the problem arises from the difficulty of explaining what interpretive accounting research actually *is*, and correlatively, of establishing some sort of identity for the network. It isn't that the authors themselves don't know, they reassure one another, it's more that they somehow can't find the words. So much for the linguistic preconditions of conceptual thought.

In the beginning it seemed enough to define the interpretive paradigm in opposition to mainstream accounting research, the presumption being that this did not involve interpretation. As the years have gone by, however, a negative definition of this kind has proved less and less convincing as a means of identifying a distinctive approach. This is partly because some of the contributors have come round to the idea that some mainstream accounting research may not be so barren of interpretation as was first thought. An oppositional stance towards it, moreover, might rule out the possibility of

a mutually advantageous rapprochement. On reflection, too, the benefits of imprecision and diversity are not lightly to be put aside, even for those of existential security. As one contributor puts it, 'It is easy to agree . . . [when] . . . discussing issues in broad terms', whilst another fears that any attempt to spell out the nature of interpretive research might provoke disagreement. That disagreement can be so casually assumed to be a Bad Thing says much about the relative priorities of consensus and truth-to-experience in this, and perhaps in most, research networks. The danger of suppressing dissent in the interests of group cohesion, is that the habit may develop into a general atrophy of the critical intelligence.

The debate also speaks eloquently of the anxieties which haunt academic researchers in mid career, as they look back on the body of work which is accumulating behind them. For some, there is a sense of footprints disappearing into the sand; that the ambition to create an enduring and cumulative body of knowledge has not been securely realised. In the thirty e-mails there are no less than twenty-one expressions of anxiety over this issue, but in none of them is there a recognition of the tendential contradiction between the very concept of an interpretive epistemology and the expectation that its findings should be cumulative. For if interpretations are theory-laden and if, as is self-evidently the case, fashions in theory are subject to change, it follows that the interpretations of cultures which presently seem so persuasive will lose their resonance over time. Who now reads *The Raw and the Cooked* (Levy-Strauss, 1970)?

For others of the group, the fate of interpretive research is bound up with a more personal realization that hope is fragile and youth fleeting. Thus Fabrizio Panozzo:

Emotionally and socially, first, at least for me: our discipline is at stake, the one that enthused us, allowed us to share with others our intellectual passions and feel part of a community. This discipline is losing strength and appeal. We still practice it but we find it increasingly difficult to nominate, let alone predicate, what it is that we do.

Clearly this is spoken from the heart, but that does not mean that 'existential angst' is allowed to deflect attention from the more pressing matter of a career. Panozzo continues:

Also, this lack of enthusiasm, I think, leaves others, especially "the authorities", unimpressed.

In fact the unsympathetic attitude of 'the business school Deans' is mentioned on seven occasions in the thirty e-mails. At this level, that of the career prospects associated with interpretive research, the primary concern of the networkers is to create a 'positive identity', 'the fabrication of a proper disciplinary space' to quote Panozzo once more. Though the connotations of artificiality are, one hopes, unintended, this is the language of marketing frankly enough, an impression which is reinforced by the discussion of 'institutions' through which the interpretive paradigm might be propagated. The declared intention is to organize – more accurately, to re-organize – congenial research material under the interpretive brand image. This reorganization is to be given concrete expression in the boundary-maintenance practices of a journal and validated as a field of knowledge appropriate for teaching and examination on the basis of a textbook. Strategies of this kind may be appropriate

enough in a regime of commodified labour wherein competitive advantage is to be sought in the tactics of distinction, and perhaps the process is all too typical of how the university syllabus evolves. On the other hand, there may still be hope that the interstices of academic control systems allow for a less alienated articulation between the research and the social being of the researcher.

Another aspect of the networkers' search for meaning is a gnawing sense of irrelevance, irrelevance, that is, to 'practice'. The figure of the MBA student who says, 'OK that's all very interesting, what shall I do tomorrow when I go back to work?' makes a cameo appearance in a number of the contributions, indicating that for these academics, an important part of the validation of their intellectual output is in the hands of professional accountants and managers – a validation which has so far not been forthcoming. Acutely sensitive to this issue, one contributor goes so far as to suggest that interpretive research should take its agendas from the field of accounting practice, a move which would at least serve to clarify the distinction between interpretive and genuinely critical research.

There are, of course, elephants crashing unnoticed about the chat-room. Apart from one mention of Parmalat, the thirty e-mails convey no sense whatsoever that the practice of accounting has been engulfed in wave after wave of crisis and scandal, that it has been implicated world-wide in fraud, money-laundering, tax avoidance and the illicit trade in arms, that it has legitimated or concealed the mismanagement of public resources and private enterprise alike and that its vocabularies and metaphysics have insinuated themselves into every aspect of contemporary life, including that which still seeks to pass itself off as the disinterested pursuit of knowledge. Viewed from this perspective, one would have thought that the immediate problems confronting critical accounting research were ones of substance rather than method. Nor does it seem to have occurred to the networkers that there are areas of 'practice' other than that of professional accountancy to which their research might profitably speak. What is dispiriting about this debate – and reading these collected texts there can be no doubt that the unhappiness and sense of futility is entirely genuine – is that it is so intensely inward-looking and so preoccupied with the collective self-interest of the participants. Whatever the individual achievements of the individuals involved, and in some cases they are considerable, the group persona which comes across is self-absorbed and neurotic.

It is a warning then – for this commentator that the impasse revealed in these exchanges is the logical end-point of a particular metatheory of academic research: one which assumes that its basic unit of output is properly the *contribution* to a relatively well-defined and well-ordered debate conducted with other academics.

2. Marking out a Territory

Since we do not learn much about interpretive research from the 'polyphonic debate' itself, it is reasonable to turn to the work of one of its prime movers. Fortunately, Ahrens and Chapman (forthcoming) offers an up-to-the-minute statement of what is meant. As its title implies, the paper is another 'call-for'.

First, a point of terminology: whereas the ‘polyphonic debate’ speaks throughout of interpretive research Ahrens and Chapman use the term ‘qualitative’. We can be reasonably sure that the same type of research is at issue, however, since they also tell us that the methodological literature treats the two terms as synonymous (and synonymous too with ‘naturalistic’, ‘holistic’ and ‘phenomenological’).

Second, a bouquet: it is refreshing (and unusual) to find methodology (a philosophical procedure) distinguished from methods (the means of accessing data). To say of a methodology that it is qualitative, however, still leaves much unsaid. Taken literally, it simply means that the treatment of the data makes no supposition that there are things in it (for example interview responses) which can be presumed to be identical in kind and so quantified. This being the case, it is not true to claim, as Ahrens and Chapman do, that qualitative methodologies, of themselves, ‘offer an alternative to positivism’ (p. 5).

Consider the following two statements:

Genealogy is gray, meticulous and patiently documentary . . . it rejects the metahistorical deployment of ideal significations and indefinite teleologies . . .

Foucault, 1984, p. 76-7

Our aim as researchers was to find things out, rather than mouth text-book phrases about capitalism and class.

Miller and O’Leary, 1998, p. 712

Both statements clearly concern the qualitative treatment of data and both, with varying degrees of clarity, are statements of the positivist belief that its treatment can eschew interpretation¹ (see Donnelly (1982) in the case of Foucault). The point is germane to the present case because I will later argue that a recent qualitative research by Ahrens and Mollona is, at its heart, positivist.

If it is possible for qualitative research to be positivist, it is conversely true that the quantitative treatment of data need not be. Few would argue, to take a prominent example, that Bourdieu’s recourse to quantitative data in *Distinction* (1984) makes it a positivist study. Positivism, to be clear about it, is the belief that propositions about the social world can be unambiguously verified against an objective social reality. It is *not* sufficiently defined, as Ahrens and Chapman seem to think, by the mere belief

¹ Having learnt to my cost that interpretations of Foucault are rather more contestable than those of the social world in its entirety, I offer the following. One of the themes in the essay from which the quotation is taken is that ‘genealogy’ offers a way of historicising the taken-for-granted interpretive schemes through which people understand their world. In order to do this comprehensively, it must offer a point of leverage independent of those schemes. In the quotation reproduced in the main text, Foucault is suggesting that a sufficiently scrupulous reading of the historical record itself can offer just such a point of reference. That is positivism. In practice, of course, Foucault was certainly not a positivist. Later in the same essay, he found his way out by grounding his genealogy in precisely the kind of grand trans-historical scheme of interpretation which he sought to subvert – that ‘only a single drama is ever played out in this “non-place” [i.e. that in which core meanings are contested], the endlessly repeated play of dominations’ (1984, p. 85). By thus falling short of his declared ambition to become a positivist, he was able to write histories which have at least the merit of originality. Miller and O’Leary’s lapse into positivism, also temporary, is at a lower level of sophistication. In their haste to contest Marxist reinterpretations of their data, they fall into the positivist trap of opposing them to an untheorised ‘what is found out’.

that such a reality exists (p. 5)². On this point I can only refer them to the critical realist writings of Bhaskar (1986) and the applications in Ackroyd and Fleetwood (2000, 2004).

By this standard I do not believe that most of the traditional studies of management control systems, based on the quantitative analysis of questionnaire data, have actually been positivist *in the performative sense*. It is true that studies such as Hopwood (1973) were not notable for extensive ratiocination on the theory-dependent nature of their take on socially-constructed and therefore multiple realities. Nor that the single narrative which they offered was but one amongst the many possible, contestable and ultimately fictive representations. Innocent of the thin ice on which they took their stance, their stoutly-declared aim was to 'find things out'. That their choices of ontology, epistemology and methodology were unconscious, however, does not necessarily mean that those choices added up to positivism.

Despite its rhetoric of hard-nosed facticity, the typical questionnaire study tacitly acknowledges the dependence of what is perceived on the research instruments through which it is perceived in the care taken over the operationalisation of its concepts. The normal procedure is to follow-up a pre-test questionnaire with interviews designed to check both the respondents' interpretation of the questions and the researchers' interpretation of the replies. There is interpretation too in the typical discussion of the data: it is far from exclusive to qualitative research.

Nor, whatever the style of the research, does it cease with the publication of a paper. The readers, one hopes, add further layers of interpretation (and evaluation) to Giddens' double hermeneutic (e.g. 1987, p. 30-1). To some extent Ahrens and Chapman are aware of this, as is evidenced by their quotation of a remark to the effect that plausibility is a matter of the reader's response (p. 19). It is not just a unidimensional plausibility which is at stake, however. The exposed mechanics of the 'positivist' text invite qualitative re-interpretation too. To some extent, this actually happened with Hopwood's study when certain of its interpretations were contested in Otley's replication (1978).

The consequence, supposing that the research is taken up by interested academics, is that positivist research becomes an argumentative resource for a debate in which the participants are free to raise all the questions of ontology, epistemology and methodology which are omitted from the actual text. It is a matter for wonderment that those social scientists who make so much of theory-dependence in reading the 'texts' of social reality, or indeed of the active consumption of the messages of the mass media, as in 'reception theory' (Hall, 1980), fail to realise that the same applies to the texts produced by their own kind.

I am *not* arguing that positivism is a philosophically defensible position. The point rather is that it is seldom actually practiced as advertised, and that even if it were, its products might still form the raw material of a more nuanced understanding of the social world. From this, it follows that the need for an alternative may not be as pressing as is argued by Ahrens and Chapman.

² All page references to these forthcoming papers are counted from the first page of the paper.

Nor do the conclusions of their paper tell us much about the nature of that alternative. One has to search through their main text to discover that its sub-title, 'positioning data to contribute to theory', refers to a process remarkably similar to the 'grounded theory' described forty years ago by Glaser and Strauss (1967). Theory, that is, is to be constructed through an iteration in which hypotheses and observation are adjusted, one in the light of the other, so as to achieve a mutual consistency. Ahrens and Chapman acknowledge Glaser and Strauss, it is true, but only in a footnote which also makes the claim that their own approach is more comprehensive inasmuch as it extends to a dialogue with other scholars. As if that of Glaser and Strauss did not.

Meanwhile, the divergence between the program and practice of positivist research suggests that we might examine interpretive research from the same angle. Does it, in other words, do what it says on the label? Obviously anything like an adequate answer to this question would require a literature review well beyond the scope of this brief comment. All I can do here is examine an example. Again, the obvious choice in the present context is a recent work by Ahrens and a co-worker.

3. Performative Contradictions in Interpretive Research

Ahrens and Mollona's ethnography of a Sheffield steel mill (forthcoming) begins with the first 10 pages restating the case for the qualitative approach, partly through a critique of the accounting literature on control and partly through a consideration of anthropological methodology. Densely argued and heavily armoured with citations, the general drift might strike the lay reader as a highly elaborated display of self-conscious diffidence, always supposing she could be persuaded to read it through. Excellent reasons are given why we should be wary of anything produced by interpretive research. Its readings, we are warned, are inevitably theory-laden, provisional, subjective and open to revision. It is as if the writer were trying to absorb all the processes of critical reading into the text itself, thence to claim the resulting tangle of reservations as a particular virtue of the qualitative approach.

In fact it could be counted as a vice. In effect the reader is imagined as a de-skilled epistemological dope, capable of nothing more than the most credulous and literal apprehension of a text. It is a style of address as redundant and ultimately irritating as its conversational counterpart: the compulsion to preface every observation with the defensive phrase 'it's only my opinion.' It is irritating because of its undertones of mendacity and unnecessary because it is true of any human utterance.

That said, it also true that opinions vary in their capacity to persuade. On this, Ahrens and Chapman quote Geertz (1995) to the effect that the field itself operates as a 'powerful disciplinary force' (p. 3). That is well said, but it makes nonsense of their declared disbelief in the existence of an objective reality (p. 5) and it also means that the writing of ethnography needs to show this disciplinary force in operation. The problem with expositions of methodology as dense and convoluted as those offered by Ahrens and Chapman and Ahrens and Mollona is that such comparatively straightforward issues tend to be overlooked. In this instance, epistemology and ontology seem to have lost contact with one another in the general murk of the writing.

It is with some relief, therefore, that we arrive at the steel mill and find ourselves on solid ground. In two terse pages on the history of the company, we learn that it has been the victim of (possibly inadvertent) predatory pricing by Corus, a much larger steel maker. The year before the study (2000), there had been 60 redundancies (which, strangely, fail to rate a mention in the ethnography). In the year following the study, the company went into administration, only to be purchased and re-opened by its original owners, minus the furnaces and rolling mills – and minus too, of course, the workers who once operated these parts of the plant.

Oddly, given this history, the five pages of the actual ethnography are written in the present tense. Also oddly, given the proclaimed methodological stance, they are written in straightforward declarative sentences. The fieldwork covered two of the departments, smelting and grinding. Smelting depended on the craft skills and the tacit knowledge of the workers – fact. Grinding was subject to management work and time standards, the workers were paid bonuses based on these standards, and this led them to endorse management policies which maximised the amount, rather than the quality, of production – also facts. There is no sense that both skill and management policies are social constructs, nor that the accounts of both are theory-dependent. Apart from the quotations from interviews, in which we are briefly invited to kibitz the interpretive process, we are back with the innocent realism of Lupton's 1963 study of incentive payment systems, a work gets straight into the business of participant observation, without burdening the reader with extensive reflections on methodology.

We are also back with Lupton's rational action interpretations. Finding the market for quality steels undercut by Corus, the management sought to concentrate on the production of low-grade steels. *Because* these called for little of their tacit skills, the smelters opposed this policy. *Because* low grade steels maximised their production-related bonus payments, on the other hand, the grinders were all in favour – once more, facts. Not only are the radical uncertainties of interpretation set to one side in this account, the operational theory is not the promised subtle interplay between culture and practice, but a straightforward inference of attitudes from interests. It so happens that I find both the observations and the reasoning wholly convincing, but that does not alter the point that they are flatly at odds with Ahrens and Mollona's declared methodology. If not actually positivist, the operational methodology of these ten pages of ethnographic description is at least as close to it as the typical quantitative study.

To me that would not matter if the account were adequate in other respects. Unfortunately it is not. Remember that it is supposed to be a study of control, not of cultural divisions and differences in attitude towards management policy. Control, however, does not figure all that prominently in the ethnography itself. There is control of the process by the smelters, in the course of which they also control the work of auxiliary labourers and, to a limited extent, challenge the control of their supervisor who is more interested in saving power than in delivering a quality product. The grinders, on the other hand largely acceded to management controls because, like Lockwood and Goldthorpe's (1969) affluent workers which they so

much resemble, they are interested only in the money. And as far as control is concerned, that's about it.

Does it need saying that this is a drastically myopic view of the total system of control within which these workers were enmeshed? Any remotely adequate account would begin with an outline of the company's system of accounting and human resource controls and preferably look for any influence on these of Corus' price cuts. And what about the redundancies which took place only months before the study began? Are we to believe that these were absent from the workers' repertoires of symbolic signification or merely that they were irrelevant to the question of control?

In its limitations, the study exemplifies the two problems of omission to which all purely ethnographic research is susceptible: that which is above notice and that which is beneath it. That which is above is a question of context, something which, by definition, cannot be directly apprehended in the course of participant observation. As far as control is concerned, I would call this context 'the capitalist social relations of production', whilst Tinker (2002) has argued for the more inclusive 'commodity form'. Both forms of words, I suspect, would invite accusations of importing 'a whiff of the gulag' into the discussion (Miller and O'Leary, 1998). Perhaps the term, 'the employment relationship', might just scrape past the censors.

That which lies beneath the surface of face-to-face sociality is what Pierre Bourdieu called the 'Doxa' (e.g. 1990, p. 26), the elusive but socially consequential taken-for-granted which the ethnomethodologists of the 1960s sought to flush out with their techniques of disruptive questioning (Garfinkel, 1967). This 'beneath' connects with the 'above' inasmuch as it includes a particular image of the wider social order and it thereby connects the individual consciousness to what I would call 'social structure'. In their methodological reflections, Ahrens and Mollona show an awareness of these issues, but they do not, in practice, inform their ethnography. For example, the steel workers routinely used the word 'capitalist' as an insult when those in one department found themselves moved to say hard things of those in another (p. 22-3). Yet, beyond remarking on the 'curiosity' of the usage, Ahrens and Mollona make nothing of it, amazingly enough.

What, then, can be concluded, from Massimiliano Mollona's gruelling eleven months in Sheffield? It is not easy to make out. The paper's conclusion picks up the argument more-or-less where the introduction set it down, as if the empirics had been nothing more than a brief dream of solidity. We are back with the late manner of Henry James as applied to social science. Though not exactly circular, the sentences proceed in a series of self-referential loops which make it difficult to decide whether or not they have actually advanced beyond their starting-points:

cultural practices . . . were structured by their membership of particular shop floor groups. These groups were defined by distinctive subcultures that arose from the workers' tasks and shop floor practices, skills and occupational histories . . . (Ahrens and Mollona, forthcoming, p. 27)

The practical nature of organisational subcultures is revealed through the ways in which their members actively reconstitute their control practices by drawing on them as a shared resource. (ibid)

Quite apart from their enervating lack of specificity, the problem with such statements - and it is quite a serious one - is that 'practices' are thought of as somehow separable from culture, so that one can meaningfully speak of them as enacting culture on the one hand and as constituting it on the other.

It is possible that this gratuitous cloning of concepts is the product of an overly-cerebral conception of culture, one which excludes physical action. Alternatively, it may be an inheritance from those studies (some of them cited by Ahrens and Mollona) which treat culture as an independent variable which is then discovered to 'influence' particular attitudes or behaviours which are actually part of that same culture. Either way, it is necessary to insist that culture, in its normal anthropological usage, is a word which *encompasses* practices. Understanding the term in this manner makes nonsense of the idea that the mutual influence of culture and practices is something which needs to be demonstrated by empirical research.

A preoccupation with this non-issue, unfortunately, has worked to crowd out the real question, and one which ethnography is eminently suited to answer – that of the mode of articulation between the various aspects of a culture. On this matter there were conclusions to be drawn from the fieldwork, though not, for the most part, to do with control, however broadly defined.

4. Looking for A Way Out

These, I recognise, are harsh comments to make of a study which, at its ethnographic heart is interesting and, in its own modest terms, convincing. If interpretative accounting research is to make progress, however, this is the kind of criticism which needs to be circulating within the research community which seeks to promote it, though possibly expressed with a solicitude which I am unable to command. As Tinker has exemplified (2002, 2005), social science can only escape from its tendency to produce an ornamental version of the dominant world-view through the *agon* of forcefully-expressed dissent. It may seem perverse to 'call-for' more unpleasantness in academic life, but it is a well-attested feature of scientific revolutions that things get personal in ways that consensus-nodes of like-minded intellectuals are unlikely to tolerate (Kuhn, 1962).

This does not mean that I agree with Tinker (2005) that ethnographic research is inherently flawed as an approach management control systems. In fact I think it is only through the medium of such research that interested teachers and practitioners can be confronted with the human consequences of what they preach and practice. My own modest contribution in this vein argued that variance reporting systems may encourage managers to believe that their function has been adequately discharged once the individuals responsible for adverse variances have been located and punished (Armstrong, 1989). This was based on four months' full-time fieldwork originally carried out for another purpose. It differed from Mollona's in the crucial respect that it was non-participant and therefore allowed contact with managers as well as workers.

However difficult it may be to negotiate, this kind of access seems to be essential if systems of control are to be the object of study. This is because they operate in the first instance by cascading agency relationships down through the organization in ways which are simply inaccessible to a participant tied to the operative level.

More than my own study, however, I would recommend Hanlon(1994) as a demonstration of the potential of ethnographic research on accounting. There the uncommitted reader will find a vividly achieved ethnography of work practices in an audit team. These are theorised through an analysis of career trajectories and centre-periphery relationships within a firm competing in the globalised market for professional accounting services.

5. Conclusion

By way of a tentative conclusion, I begin with the thought that it may be a disfavoured to the methodology to think of interpretive research primarily in terms of its contribution to theory, if theory is understood in the sense of abstract propositions which will generalise across settings. The kind of theories thrown up in the course of the ethnographic process differ from this model in that they 'hover low over the interpretations they govern' (Geertz, 1993, p. 25). It is theory produced in the process of trying to answer the question of what particular people are doing in a particular situation. Looked at in this light, Ahrens and Mollona's ethnography of the steelworks tells us a reasonable amount about how and why the Sheffield smelters went about controlling their process, and perhaps we should be content with that. Why is it so important to extract a 'theory of organizational control' from the study particularly when the results are so disappointing?

That said, situated theory of the kind produced by ethnography is certainly informed by prior and broader theories of what people in a particular situation *might* be doing, and it is not unreasonable to expect that situated theory should itself speak to these broader theorisations. The conditions under which it might do so, however, could have done with a more consideration by the polyphonic debaters.

For my own part. I would say that it depends on the extent that ethnographic interpretations are under-determined by prior theory. This does not mean that those parts of interpretation undetermined by theory might then be determined by raw data – that would indeed be positivism. What it does mean is that the prior theory with which the field is approached needs to contain substantial elements of contradiction, indecision and incoherence.

Very roughly, and for the purposes of this exposition only, this prior theory might be thought of as consisting of two parts. On the one hand, there is articulate, sophisticated and internally consistent 'big theory' – the kind one finds in libraries under Dewey classification number 301.01. This is the kind which we are made to include in essays and papers which will be assessed by people who expect that sort of thing. On the other hand, there is the kind of theory which is produced by people who have not read these books. It is incoherent, internally inconsistent and often not fully articulated. It is the kind of theory which we acquire and use in the course of everyday

life, the kind which the adepts of big theory treat as an object of study under the rubric 'folk theory'. The implicit disparagement is unwarranted because, even for the most sophisticated theorist, there are aspects of human experience – some of them likely to be encountered in the course of fieldwork- where big theory gives out and folk theory takes over. In fact the articulation between the two forms of theory is not as neat and complementary as this makes it sound. Particularly under the stress of unfamiliar experience, they overlap, contradict and spill into one another in ways which continually create and disrupt alternative framings of that experience.

The resulting disorientation and indecision, uncomfortable as it sounds, is precisely what is meant by open-mindedness, and it is also, (the point at last!) what makes it possible for ethnographic research to speak to big theory. Were it otherwise, the ethnographer would approach the field as nothing more than the avatar of a particular big theory, and the consequent determination to force the material into a pre-conceived pattern would convince no-one. Viewed in this light, the initial 'general bewilderment' experienced by all genuine ethnographers (Geertz, 1993, p. 27) is actually a period of trial-and-error during which disjointed and disparate elements of theory, mostly from the folk repertoire (because these come easily to hand) but sometimes too from the big books, are assembled into a jury-rigged means of understanding what is going on. Whilst this improvised device is certainly constructed from the material of prior theory, it is far from determined by it. Crucially, it is not determined by – though it is possible for it to end up consonant with - the prior formal theory with which ethnographic work began. In other words – again approximate – it is the failures of big theory in its encounter with the field which create the conditions under which the ethnographer must draw on folk theory.

The persuasive quality of accounts theorised in this ad hoc manner - for those willing to be persuaded – lies in what might be called the paradox of ethnography: that in order to convince it must offer the reader the means with which to remain unconvinced. This means does not consist of raw data as is misleadingly implied by Geertz's too-often-quoted 'thick *description*' (ibid p. 6, italics added). Rather it consists of folk-theoretical readings, any one of which might potentially be at variance with the writer's main line of interpretation but which, if that main line is robust, turns out as a matter of *contingent* fact, to fit with it quite nicely. That, I think, is how ethnography in particular, and interpretive research in general, might contribute to (big) theory.

It will probably now be obvious that I think that interpretive research has a great deal to contribute to our understanding of how accounting is actually performed. Despite three decades of critical accounting research, we still know remarkably little about how accountants go about the production of accountings, how the performance of audit is carried off, how standards and reporting conventions are used in practice, and so on. How, otherwise, can Enron have come as a surprise? Answers to questions of this kind – not perhaps in the sense of formal theory but in the sense of grasping through indicative cases how the culture of professional accounting operates – are, I suspect, intimately related to the recurrent crises and scandals mentioned in the first part of this essay. Because of the economic power wielded through the medium of

accounting, understandings of this kind may turn out to be the most practically consequential knowledge of accounting we can obtain.
That, I suppose, amounts to a 'call-for' of my own.

References

- Ackroyd, Stephen and Fleetwood, Steve (Eds.) (2000) Realist Perspectives on Management and Organization. London. Routledge.
- Ahrens, Thomas and Chapman, Christopher (forthcoming) Doing Qualitative Field Research in Management Accounting: Positioning Data to Contribute to Theory. Accounting, Organizations and Society
- Ahrens, Thomas and Mollona, Massimiliano (Forthcoming) Organisational Control as Cultural Practice—A Shop Floor Ethnography of a Sheffield Steel Mill. Accounting, Organisations and Society
- Armstrong, Peter (1989) Variance Reporting and the Delegation of Blame: a case study. Accounting, Auditing and Accountability Journal Vol. 2, No. 2, pp 29-46.
- Bhaskar, Roy (1986) Scientific Realism and Human Emancipation. London. Verso.
- Bourdieu, Pierre (1984) *Distinction: a Social Critique of the Judgement of Taste* London. Routledge
- Bourdieu, Pierre (1990) The Logic of Practice. Cambridge. Polity Press.
- Donnelly, M. (1986) Foucault's Genealogy of the Human Sciences. Ch. 2 in Gane, M. (ed) Towards a Critique of Foucault. London. Routledge and Kegan Paul. pp. 15-32.
- Farrell, Michael P. (2001) Collaborative Circles: Friendship Dynamics and Creative Work. Chicago. University of Chicago Press.
- Fleetwood, Steve and Ackroyd, Stephen (Eds.) (2004) Critical Realist Applications in Organisation and Management Studies. London. Routledge.
- Foucault, Michel (1984) Nietzsche, Genealogy, History. in Rabinow, P (ed) (1984) The Foucault Reader. London. Penguin. pp. 76-100.
- Garfinkel, Harold (1967) Studies in Ethnomethodology. Englewood Cliffs. Prentice-Hall.
- Geertz, Clifford (1995) After the fact: Two countries, Four decades, one Anthropologist, Cambridge, Ma. Harvard University Press.
- Geertz, Clifford (1993) The Interpretation of Cultures. London. Fontana.
- Giddens, Anthony (1987) Social Theory and Modern Sociology. Cambridge. Polity Press.
- Glaser, Barney G. and Strauss, Anselm L. (1967) The Discovery of Grounded Theory: Strategies for Qualitative Research. New York: Aldine de Gruyter.
- Goldthorpe, John, H., Lockwood, D., Bechhofer, F. and Platt, J. (1969) The Affluent Worker in the Class Structure. Cambridge. Cambridge University Press.
- Hall, Stuart (1980) Encoding/Decoding Culture, Media, Language in S. Hall, D. Hobson, A. Lowe and P. Willis (eds) *Culture, media, language : working papers in cultural studies, 1972-79*. London: Hutchinson
- Hanlon, G. (1994) *The Commercialisation of Accountancy: Flexible Accumulation and the Transformation of the Service Class*. Basingstoke. St Martins Press.
- Hopwood, Anthony (1973) An Accounting System and Managerial Behaviour. Farnborough. Saxon House.
- Kuhn, Thomas S. (1962) The Structure of Scientific Revolutions. Chicago. University of Chicago Press
- Lévi-Strauss, Claude (1970) The Raw and the Cooked. London. Cape.

- Lupton, Thomas (1963) *On the Shop Floor: Two Studies of Workshop Organization and Output*. Oxford. Pergamon Press
- Miller, Peter and O'Leary, T. (1998) Finding things out. Accounting Organizations and Society, Vol.23, No.7, pp.709-714.
- Otley, David T. (1978) Budget Use and Managerial Performance. Journal of Accounting Research. Vol. 16. No. 1. pp. 122-149
- Tinker, Tony (2002) Spectres of Marx and Braverman in the Twilight of Postmodernist Labour Process Research. Work Employment and Society, Vol. 16, No. 2, pp. 251-281
- Tinker, Tony (2005) The Withering of Criticism: a review of professional, Foucauldian, ethnographic, and epistemic studies in accounting. Accounting, Auditing and Accountability Journal. Vol.18, No. 1, pp. 100 – 135