



Ethnography, Reality, and Truth: The Vital Need for Studies of 'How Things Work' in Organizations and Management

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ABSTRACT There is considerable potential for ethnography to play a larger and more mainstream role in organization and management studies. Ethnography is not a research method. It is a way of writing about and analysing social life which has roots in both the sciences and the humanities. Whilst it prioritizes close and intensive observation in the gathering of information and insights, it may additionally and potentially use any of the full range of other research methods. A powerful rationale for 'good' ethnographic work is offered by Pragmatist Realist principles of truth, reality, and relevance-to-practice. Research based on these principles investigates the realities of 'how things work' in organizations. In doing this, it rigorously grounds and contextualizes the activities which the researcher observes and the accounts which they receive from organizational members. To do this well, researchers must avoid being diverted from the analysis of organizational patterns and managerial processes by researchers trying to 'get into the heads' of organizational members in order to capture their subjective experiences. Various moves can be identified which would encourage and enable more people to work ethnographically and to produce research which is inherently critical and is unfettered by attachment to any narrow specialist method, concept or 'perspective'.

PRELUDE

This *Point–Counterpoint* departs from the convention of presenting two voices speaking in opposition or debate. Although we cannot say that it is sung rather than spoken, the piece has been composed along two-part harmony lines. I first develop the main *theme* and *variations* of the piece and John Van Maanen adds the *Counterpoint* line. As in music, the development of the piece involves the two voices sometimes moving along in close consonance and sometimes with a touch of dissonance. The recurrent melody which gives the piece its unity is the expressed belief that ethnographic work can and should

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play a much more central role in the organization and management studies repertoire than it currently does. For this to happen, ethnographic work needs to be carried out with particular rigour. There also needs to be clarity about its essential nature and the truth claims that can be made for it. Some of the issues which are raised by consideration of these matters have relevance to everyone involved in the organization and management studies enterprise, regardless of their preference for qualitative rather than quantitative instrumentation, interpretive rather than positivist orchestrations, or empirical rather than theoretical tunings.*

Once this prelude closes, attention is turned the question of why we need more ethnographic work in organization and management studies, especially if we are interested in managerial and strategic practices or with the identities and emotions of the people who engage in such activities. This call is developed by clarifying just what doing 'good' ethnographic work entails. The virtues of Pragmatically-oriented ethnographic research are then identified, unashamedly utilizing notions of 'truth', 'reality', and relevance to practice. Following this, concerns are expressed about two dangerous diversions or distractions which need to be avoided; one with regard to the collection of research 'data', the other with regard to the fashionable interest in so-called 'lived experiences'. Having cleared the ground here, the question of 'what is to be done?' is tackled. The main theme of the piece is then reprised and the finale wraps up the work with a claim that the Pragmatist ethnographic enterprise is an inherently critical one which can help us move beyond the fragmentalizing tendencies currently prevalent in organization and management studies.

Although ethnography is a central concern of this *Point-Counterpoint*, the version of *realism* which is used here and the associated Pragmatist notion of truth claims, are tunes which might be rewardingly listened to by all organization and management researchers, writers, theorists, and reflective practitioners, regardless of how closely they wish to engage with ethnographic performances as such. We might note, for example, that in perhaps the most effective previous invitation to apply Pragmatism to organizational research (as a means to overcoming the positivism/anti-positivism divide in organization studies) there is no mention of ethnography whatsoever (Wicks and Freeman, 1998). All readers of *JMS* are therefore invited to this little concert party – apart perhaps from those who wish to close their ears to any notion that our shared task is to study 'how things work' in organizations and managerial work.

There is always a danger with ethnography enthusiasts that they end up more frequently writing about the activity than actually doing it (Bate, 1997) or adopting a lofty position where they pontificate about the wonders of ethnography without risking the 'contamination that might come from actually trying to construct a convincing account of ongoing organizational life based on first-hand information' (Van Maanen, 1989, p. 31). To keep myself as clear as possible from this danger, I shall ground my arguments in my own experiences and worries from my years of engagement with the craft of ethnography, as a researcher, a writer, a teacher, and a journal reviewer. The advocacy in which I engage is not presented out of any desire to argue for the superiority of one's

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own style of work or to take a swipe at other styles. My advocacy of ethnographic work is quite a reserved one in fact.

THE DISCOMFORTING NECESSITY FOR ETHNOGRAPHY

Insofar as I would claim to do ethnography, I would confess that I have too often been a reluctant ethnographer. As is the case with my ethnographic friend, John Van Maanen, this is not a reluctance to be inspired by the ‘humanities’ or by literary traditions when it comes to the writing part of the research job. The reluctance, on my part anyway, has resulted from a fear of having to face the enormous difficulties of gaining the very high grade of research access that is needed to prepare an ethnography, a fear of having to find the enormous amount of nervous energy and emotional resilience to be able to work for long hours ‘in the field’, and a fear of having to struggle, so often, to justify to reviewers and editors the reasons for one’s departure from the conventions of survey or interview-based and data-crunching research.

‘Why on earth engage in such a painful way of doing research then?’, I am bound to be asked. My answer is a simple one. I have always believed, whether I have liked it or not, that we cannot really learn a lot about what ‘actually happens’ or about ‘how things work’ in organizations without doing the intensive type of close-observational or participative research that is central to ethnographic endeavour. Early thoughts along these lines encouraged me, after my first degree, to start a research career by entering an industrial relations management job and engaging in a participant observation study of the change project in which I was centrally involved (Watson, 1982). I felt that there was no real alternative to this if I wanted to contribute in a worthwhile way to the social scientific understanding of how managers manage, how organizational change comes about, how micropolitics operate, and how employment relationships are shaped and maintained.

Recently, I have come to feel that this is even more the case for researchers wishing to investigate matters of human identity and strategic practice in organizational and managerial settings. It is fairly clear that survey methods have only limited potential for examining such matters but, as I shall argue later, interviews carried out in the absence of close observation and workplace interaction with research ‘subjects’ may be little better. Lots of researchers are investigating and writing about ‘identities’ at present. This ‘ostensively new master signifier’ is applied to a wide range of substantive topics, and scholars ‘working from strikingly different philosophical frameworks’ have been drawn to its ‘theoretical promise’ (Alvesson et al., 2008b, p. 8). Well, are not identities something to do with the sort of person someone is, in their own minds and in the eyes of others? Do we seriously believe that we can throw much light on such matters by, for example, analysing what we extract from the digital tape recorders that we sit between ourselves and the ‘subjects’ to whom we say hello, interview, and bid fare thee well? To talk of someone’s identity surely requires that, to a reasonable extent, we *get to know* them and the context in which they live and work. We can rarely go all the way with this. We have to be realistic. Nevertheless, a degree of talking to people, watching them, and sharing tasks with them over a period of time in the varying settings or circumstances that are relevant to our investigation might be expected before we can convincingly claim that we know what we are talking about.

The argument that we can say little about organizations and identities unless we get close to human action and social interactions must surely apply even more to research which prioritizes the notion of 'practice'. One especially significant move in this direction has been the development of a 'strategy as practice' research agenda. This calls for examination of how practitioners act, what work they do, with whom they interact, and what practical reasoning they apply in their own localized experience of strategy' (Jarzabkowski, 2005, p. 9). Johnson et al. (2007, p. 3) write of a concern with what 'the people engaged in strategizing actually do and how do they influence strategic outcomes'. It is hard to imagine how any of these ambitions can be fulfilled without a great deal of close and intensive field research and, wherever possible, participant observation. If the strategy-as-practice agenda is to be fulfilled we can hope to read some exciting strategy ethnographies. Just what we can expect if this does not happen is hard to imagine.

It is inevitably discomfiting to argue that our students and colleagues are not going to find out much as researchers unless they get 'close to the action' that they intend to write and generalize about and get reasonably close to the people involved in organizational practices. Just how threatening it can be to talk in this way was brought home to me recently when, in reviewing a paper of mine which combined interview with participant observation material, the journal reviewers baulked at my utterly sincere comment that I felt that I would not have got anywhere near understanding the issues being investigated if I had not, at one stage, worked in the factory alongside the individuals whose identities and practices I was examining. The wording of this conclusion had to be considerably toned down before the article was accepted for publication. A friend commented, 'I know that in all honesty you felt you should say what you did. But you just had to soften the message; you were in danger of frightening the horses.'

I am afraid there are some more workhorse-frightening passages to come shortly, before the melody softens and the music becomes a little more soothing. First, though, it is necessary to clarify just what we are talking about when we speak of ethnography.

THE ESSENTIAL CHARACTER OF ETHNOGRAPHIC WORK

As will become clearer later, Pragmatism (in the philosophical sense of the term rather than the everyday 'being expedient' sense) is a *leitmotif* of the present composition. Pragmatism would require us to replace the question 'what is ethnography?' with the question 'how might we most helpfully *use* the concept of ethnography to enable us to do more worthwhile research in the organization and management studies field?'. To further that project it is helpful not to define ethnography, as many do, as a research method. A textbook on organizational ethnography, for example, defines ethnography as 'a research methodology . . . which involves the observation of and participation in particular groupings . . .' (Neyland, 2008, p. 1). In Pragmatist terms, we do not say that definitions like this are *wrong*. We are fully in line with Pragmatist principles, however, if we say that this is a less useful conception of the type of research practice with which we are concerned than would be a conception of ethnography as the *outcome* of research. Ethnography is the product and not the method of production. Ethnography is most usefully defined as a *style of social science writing which draws upon the writer's close observation of and involvement with people in a particular social setting and relates the words spoken and the practices*

observed or experienced to the overall cultural framework within which they occurred. This concept or 'working definition' of ethnography has two strengths.

First, this way of conceptualizing ethnography touches on the essence of the activity. This is an anthropological concern to understand the human as a 'cultured being' ('ethno') and to write about them ('graphy') in a manner which provides rich insights into aspects of humans' cultured lives. To talk of 'cultured lives' in this way means relating the details of the particular events and utterances observed and experienced 'in the field' to a *cultural whole*. As Baszanger and Dodier (2004, p. 13) put it, there is a 'global reference which encompasses these observations and within which the different data throw light on each other'. This means, in the case of ethnographic work in organizational settings, 'wrapping up' any specific concerns, say with the nature of managerial work or the identity work of strategy-makers, within broader attention to 'the construction of cultural norms, expressions of organizational values, and patterns of workplace behaviour' (Bryman and Bell, 2003, p. 317).

The second advantage of treating ethnography primarily as a written form is that, whilst pointing us towards certain vital or core investigative-procedure requirements, it allows – even encourages – the use of a wide range of methods. To write ethnography and meet the criterion of setting what has been seen and experienced into an overall cultural framework, there clearly has to be close observation and intensive involvement in the field. However, such participant observation can be considerably strengthened if, alongside it and possibly at the same time, interviews are carried out, documents are analysed, statistics are collected, and perhaps even small surveys undertaken. All the material gathered in this way is analysed, of course, within the process of cultural learning and reflection which only deep immersion in the field setting can make possible. Given that a whole range of methods might be used by any particular ethnographer, it is clearly unhelpful to define ethnography as a research method itself.

What cannot be avoided by any would-be ethnographer is the undertaking of some significant participant observation. Recognizing that a claim to having been a participant observer is vital, aspiring ethnographers are often tempted to exploit the fact that simply entering the same room as the research subjects and watching and listening to them for a brief period could reasonably be claimed to be participant observation. Bate (1997, p. 1150) has excoriated such moves with his notion of 'jet plane ethnography' in which fieldwork is a matter more of a series of flying visits to the research site rather than the sort of long-term stay that he regards as vital. Again in the Pragmatist spirit, it would be helpful (as opposed to 'correct') to characterize participative observation in a way consistent with the original 'impulse' of the Chicago school of sociology: to get closely involved with the people being studied in their 'natural' setting (as opposed to a laboratory or interview room) and actively interact and share experiences with them in a manner going beyond simple observation.

Participant observation, then, is a research practice in which the investigator *joins* the group, community, or organization being studied, as either a full or partial member, and both participates in and observes activities, asks questions, takes part in conversations, and reads relevant documents. The fieldwork which 'gives rise to ethnography', as Gellner and Hirsch (2001, p. 1) helpfully put it, is a practice in which 'the researcher engages with the people being studied, shares their life as far as possible, and converses

with them in their own terms'. For this practice to serve any significant ethnographic purpose, the observation has to occur over a period of time which is sufficient for the researcher to appreciate the range of norms, practices, and values, official and unofficial alike, which characterize that research setting. Classic organization and management texts include the *Boys in White* study of the medical school (Becker et al., 1961), the *Men Who Manage* study of managerial work (Dalton, 1959), the *Banana Time* study of work group behaviour (Roy, 1958), and the *On the Shopfloor* study of factory life (Lupton, 1963). Such classics provided the inspiration for more recent research such as Collinson's (1992) shopfloor study, Kunda's (1992) study of an American high-technology company, Delbridge's (1998) study of new manufacturing techniques and worker experience in two factories, my own account of managerial work in a UK telecommunications manufacturing company (Watson, 2001), Down's (2006) study of entrepreneurship in a small business, and Ho's (2009) ethnography of Wall Street.

THE PROMISE OF ETHNOGRAPHY: SEEKING TRUTHS, IDENTIFYING REALITIES, AND INFORMING PRACTICES

Studies like the ones mentioned above can tell us valuable truths about the realities of work, organizations, and management which, if born in mind by readers of these studies, will enable those readers to cope more effectively than they otherwise might should they become practically involved in the settings covered in the studies. Just a minute though. Could it not be argued that this sort of claim could be made for any good scientific research – within and beyond organizational social science? And would we not accept that some of the most highly regarded novels, poems, or films are open to similar evaluation? Yes indeed, is the answer to both of these questions. It might be difficult to do, but let us imagine a young person setting out into the world of courtship, sex, and potential marriage (or other long-term sexual partnership) asking our advice on 'helpful things that I might read'. We would surely think hard about just which novels, films, health pamphlets, or pieces of social science research we would recommend. Some of us, in our day jobs, are post-structuralists or social-constructivists who professionally scorn the notion of 'the real world'. Others are critical realists who think of reality in terms of beneath-the-surface 'causal powers' (Archer, 1995; Sayer, 2000). Yet would we not all seek out reading to recommend to this young person which we believed to be 'truthful' in quite a simple sense? Would we not want to help enlighten this youth about the *realities* of sex, 'going out' and living together? Surely we would look for sources of understanding and insight about the world which, compared to the books, films, and articles that we would avoid, would make it more likely that our young friend succeeds in whatever their sexual and 'mating' projects might be.

I have deliberately used a non-work example to make the general epistemological point that truth claims can be made for both scientific and literary writing which stress the impact of that material upon human practices rather than depending entirely on representationalist 'correspondence' claims to accuracy. This basic point applies just as powerfully to organizations and management as to personal activities. I remember, as a student, reading *Boys in White* (Becker et al., 1961) and thinking how enormously helpful this reading would have been had I chosen to study in a medical school. The truths offered by these researchers seemed to me to be valuable counters to the information that

I had previously got from Hollywood accounts of the lives of trainee doctors. I thought along similar lines when I read some prison ethnographies! Perhaps more to the point here, is to say that the way that I behaved when seeking and entering a managerial job after leaving university was closely informed by Dalton's (1959) study of managerial 'realities' and it was directly and invaluable informed by my reading of another 'classic': Gouldner's (1954) *Patterns of Industrial Bureaucracy*. It would have taken me a lot longer to learn the ropes of factory life and management had I not learned from Gouldner about the importance of 'indulgency patterns' in worker-supervisor relationships, for example. What is really important here is that much of what I then came across in management textbooks immediately seemed to me to be utterly *unrealistic* and likely to hinder rather than help anyone becoming involved with managers, as a manager themselves or as a non-managerial worker.

We are here applying the epistemological principles of Pragmatism, a way of thinking which originated in late 19th century America with Peirce, James, and Dewey and which inspired the work of the 20th century Chicago ethnographers. As Joas puts it, the 'guiding idea' of this Pragmatism is that 'truth' is not to do with getting a correct 'representation of reality in cognition' but is an expression of an 'an increase of the power to act in relation to an environment' (Joas, 1993, p. 21). What we can clearly infer from this statement is that we are not dealing with any notion of absolute, final, or foundational truths. Pragmatism seeks relative rather than absolute truths. Note that I did not say above that what I read in management textbooks was 'wrong'. Yet Pragmatism allows me to say that it was nevertheless inferior to, less truthful than, a poorer informer-of-action than what I read in the ethnographic studies I have highlighted. Although I have inevitably been pleased with the astonishingly high sales and the frequent scholarly citing of my own *In Search of Management* (Watson, 2001) ethnography, I have been more thrilled by hearing of people working in industry who have not only keenly read the book but passed it onto to others to learn about 'how things work' in management.

The everyday concept of 'how the world works' moves us on from the epistemological to the ontological aspect of what can be called Pragmatic Realism. The 'realism' here is not the kind of naïve realism (Hammersley, 1992) in which the ethnographer directly reports a world which is taken to be unambiguously 'there'. It is not the realism implicit in 'realist tale' ethnographies (Van Maanen, 1988) where the research report tends to beg the question of the researcher's own role in interpreting and framing what is observed and experienced. Pragmatism would be rather uneasy with the rather mechanical or biological sounding realism of the critical realists' generative mechanisms ('structures, powers and capacities that, literally, "make things happen in the world" '; Reed, 2009, p. 432). There are strong continuities, nevertheless, between Pragmatism and critical realism (Johnson and Duberley, 2000). Like critical realism, Pragmatic realism accepts the importance of processes of social construction, researcher interpretation, narrative/discursive framing, and all the rest, without denying that there are realities which exist in the social world, independently of the way they are observed or interpreted. These are realities which, as Berger and Luckmann (1967, p. 15) say, have 'a being independent of our volition'.

To return to the example of the reading to be recommended to our young friend, there are realities, however they are presented, of, say, certain sexual practices leading to pregnancies and others leading to diseases. These are the 'brute force' realities which

Wright Mills – the most Pragmatist of sociologists (Watson, 2009) – tells us that Peirce, the original pragmatist, argued are ‘forced upon cognition’ (Mills, 1966, p. 158). In the same way that the young person has to ‘learn the ropes’ of sexual behaviour, those entering work organizations have to learn the ropes of the sort of roles, rules, norms, unofficial practices, politics, discourses, and cultures to be found in organizations. It is interesting and significant that the notion of ‘learning the ropes’ was a key one in the medical school ethnography mentioned earlier (see Geer et al., 1968). The ethnographer seeking to establish themselves in their research setting has to learn the ropes of that setting in the same way that sailors on sailing ships had to learn the ropes, in a more literal sense of the word ‘ropes’, to survive in their jobs. What a good ethnographer does, in effect, is to write about the understandings they acquire as they learn the ropes of a particular organizational or occupational setting (or type of setting) in such a way that, in principle, any reader would be able to cope and survive on board such organizational vessels – whether they board those vessels as sailors, passengers, or officers. The ethnography they write is more or less true to the extent to which its reader would, in principle, be informed to cope in settings like the one described and analysed.

This way of thinking about the truth claims of research is relevant to all types of organizational and managerial research. But it is especially relevant to ethnography because of ethnography’s character, as many see it, as ‘the most scientific of the humanities and the most humanistic of the sciences’ (Van Maanen, 2006, p. 13). As science, organizational ethnography needs to be concerned with creating systematic generalizations about ‘how the world works’. It needs to be theoretically informed and informing; it needs to contribute to the broader body of knowledge which constitutes organization and management studies. It enables theoretical, rather than empirical, generalizations to be made. Hammersley (1992) talks of labelling theory as an example of theoretical generalizations which have emerged from ethnographies and I have illustrated the principle of ‘theoretically generalizing at the level of process’ with some theorizing about the politics of managerial communication developed in my own ethnographic work (Watson, 2001, p. xiv). What about the humanities then? The generalizations and theoretical insights which ethnographers develop can be given extra force, immediacy, and credibility through the ethnographer’s deployment of skills seen more often in the humanities than in science. The researcher’s close involvement in the research setting provides the sort of rich material and depth of knowledge which makes it possible for the ethnographic writer to bring to bear on aspects of organizational life the sensitivity of the novelist or dramatist to the complex nuances of social life (Atkinson, 1990; Rose, 1990; Watson, 1995). Ethnographers use creative writing techniques and ‘fictionalize’ certain aspects of their narrative to various degrees (Humphreys and Watson, 2009). All ethnographic work must nevertheless be differentiated from the novel, play, or short story. Although it can most certainly be applied to them, literary creations do not *have to* pass the Pragmatist test of truthfulness. Many of them are quite rightly there simply to entertain, divert, delight, or shock. The truth claims of ethnography must, however, stand up to close Pragmatist scrutiny if it wishes to retain its social science credentials. Any ethnography worthy of the name, in its fieldwork accounts and in the theoretical generalizations interwoven with these, has the potential to inform the projects and practices – whatever these might be – of anyone entering the type of setting covered by the study.

MOVING BEYOND THE ‘SHE WOULD SAY THAT, WOULDN’T SHE?’ PROBLEM

I hope that, by now, it is clear what sort of research and writing it is being claimed we need a lot more of. It is suggested that bringing ethnographic work more to the fore is vitally necessary if people studying organizations and their management are going to meet more frequently the very basic scientific criterion of writing about how organization and management ‘works’. Such a development would also increase the amount of work which has pertinence for those who engage with organizational and managerial practices, in one respect or another. It might also help us move away from some of the naivety and lack of critical rigour of so much that we publish, whether we march under the banner of ‘quantitative’ research or sail under the ‘qualitative’ flag. I am referring to the assumption that is often made by researchers that either the numbers that they collect or the words that they note down can be made sense of without ‘situating’ them in the organizational/political context in which they came into being.

People in organizations recording numbers (into a bureaucratic ‘return’, into a database, or into the survey document sent to them by a researcher) or making verbal statements (written or oral) rarely do so without some consideration of their personal or group interests or preferences – within the bounds, of course, of what they will not be punished for. This, if I dare say so, is an unavoidable truth about how the social world works. As a participant observer in managerial settings I have rarely seen statements made or figures presented which were not contestable, sometimes very significantly. We know in our everyday lives that we need to contextualize, question, or ‘take with a pinch of salt’ anything said to us or any numbers proffered – whether these are evaluations of the value of our car, the number of lovers our best friend claims to have had, or the amount of beer our daughter said she drank last night. We say of the job applicant who tells us, ‘I have actually done a lot more research than my publications would suggest’, ‘Well, in this context, she would say that wouldn’t she?’. We then take this insight into account when we make our appointment decision. If we really care about rigour, we must ask why, when we constantly make judgments like this in our everyday lives, we so often put such considerations to one side when we operate as organizational and managerial researchers. Among most academic researchers there is surely some awareness that philosophers like Austin (1962) long ago established that speech is action (and never just ‘saying’), and social scientists like Goffman (1959) showed that all communication has a ‘presentation of self’ dimension. So why is this knowledge, like the everyday street-wisdom that we developed in infancy, frequently forgotten when we put on our researcher hats? The answer is simple. It is because we so rarely have the information to ‘situate’ the verbal utterances and number-based statements of the people we research. If we were to work ethnographically, however, we would have a great deal more information to enable us to apply this sort of rigour to our handling of ‘data’. Not only that, we would find that the people who supply us with information would be far more circumspect about what they tell us if they saw us as a person they knew and encountered everyday in the workplace rather than as ‘that researcher from the university up the road’.

It must be emphasized that what has been said here does not weaken the arguments expressed earlier that working ethnographically (i.e. closely engaging with people ‘in the

field' with a view to writing ethnographically) is not an alternative to quantitative or qualitative research. Rather, it has an enormous potential to strengthen the effectiveness of a whole range of investigative techniques. This applies powerfully to that mainstay of so much social science research: the interview. For the sake of an easy and peaceful life I would prefer this not to be the case. But I have come to doubt whether there is much point at all, most of the time, in our going out into organizations, and especially into managers' offices, to interview people that we only really encounter in that interview event. All the general reasons about the researcher's inability to situate the statements made by the interviewee apply here. A number of experiences as a participant observer hearing organizational colleagues report what they have told a visiting researcher have forcefully reinforced my doubts here. Leaving aside some instances of unambiguous lying, I would say that what these research subjects told the researchers would rarely have given any kind of helpful insight whatsoever to the researchers about how things 'actually worked' in that organization. There are many reasons for this, ranging from a desire to 'look good', a desire to tell the researcher what the interviewee felt they wanted to hear, to a wish to get the event over quickly by over-simplifying matters. The respondents I have in mind here were often, but not exclusively, managers. But why did I suggest, above, that the problems which arise here are especially significant in the case of managers? The reason is this, and I speak as someone who has been interviewed on several occasions in my role as 'the manager': when you are being interviewed in such a context you are speaking as an official of the corporation. It is your job to put organizational issues in a good light. However much the interviewer may promise you that they can be trusted if you speak 'off the record', there is still an enormous pressure on you to speak largely positively of what is going on in *your* organization, whether this pressure comes from a sense of corporate commitment or a sense of fear.

In addition to the 'spokesperson' element of interviews with managers, there is an important occupational identity issue. To play out the 'social identity' (Watson, 2008) of 'manager', the individual is likely to feel it necessary to present themselves as a rational and strategic person who 'knows what they are doing' and who is 'in control' in their jobs. As one manager said to me after an interview with a visiting researcher, 'You know and I know that most of us here are flying by the seats of our pants. But I think that this is something we ought to keep to ourselves'. I understood this man's point. Because I worked alongside him in the day-to-day rough and tumble of that managerial context, I believe I gained far more worthwhile insights about managerial work from him than the visiting interviewer could possibly have done.

There is a view that the researcher should not create ambiguity by undertaking interviews, as opposed to having conversations, when carrying out ethnographic studies. (Silverman, 2007, p. 9) extols the virtues of 'naturally occurring data', regarding the 'manufactured data' of interviews and focus groups as 'quick fix' material. Nevertheless, in line with my earlier welcoming of multi-method research, I suggest that there is great virtue in incorporating interviews within ethnographic work. By carrying out relatively structured interviews, one can discuss a variety of issues in a single 'big' conversation with an individual, and produce material which allows comparisons with the accounts of others. There is also the side-benefit of electronic devices enabling greater accuracy in the recording of verbatim statements than is possible in one's daily field-note writing.

Spradley (1979) made famous the notion of the ethnographic interview, and Heyl (2001, p. 369) builds on his thinking to argue that what distinguishes the ethnographic from other types of interview is the duration and quality of the contact between researcher and researched and the 'quality of the emerging relationship'. This is a relationship, as I would put it, in which the subject feels confident to challenge the researcher and contribute to the shaping of the conversation, as opposed to falling into line with the interviewer's priorities and preconceptions.

The advantages of interviewing in an ethnographic context are strikingly demonstrated in Whittle's (2005) report of her study of consultants. What Whittle calls the 'richness' of her 'data-set' enabled her to 'reveal a very different picture' from what previous researchers had constructed on the basis of conventional interviews (Whittle, 2005, p. 1307). The conclusions about 'how things work' in the consultancy world produced by this research are counter-intuitive ones. This, I suggest, boldly underscores the power of ethnographic investigation. The importance of counter-intuitive learning occurred to me long ago when starting to present to fellow (non-ethnographic) researchers my conclusions about personnel and employment relations managers (drawing on three years of participant observation experience and an interviewing programme across a range of organizational types). An experienced industrial relations researcher glanced at one of my overhead projection slides and warned me that I had typed some of my conclusions 'the wrong way round'. He pointed to my conclusion that managerial employment specialists find themselves far more often engaged in conflict with other managers than they do with employee groups or trade unions. I believed, and still believe, that this is a *reality* of HR work, on the basis of participant observation, interviews, and the theoretical analysis of my material. It is part of 'how things work' in employment relations management – however unlikely or counter-intuitive it might appear to those who do not have first-hand experience of that field.

MOVING BEYOND THE DIVERSION AND CONCEIT OF CAPTURING PEOPLE'S 'LIVED EXPERIENCE'

There is clearly virtue in researchers having first-hand experience of the aspect of life they are studying. However, we must take great care here not to over-privilege the experiential aspect of our investigative work and insist that 'I know better than you because I was there and you were not'. As Hammersley points out, it is false to believe that researchers can have 'direct access to the truth, even to the truth about [their] own perceptions and feelings'; 'what we see is always a product of physiology and culture, as well as of what is there' (Hammersley, 1992, p. 192). The researcher is as much an 'ethno' – a 'cultured being' as I put it earlier – as the people they study. This necessitates the researcher writing *reflexively* so that the readership, or social scientific community (to speak more formally), can situate or 'appreciate in context' the content of the ethnographic account. As Alvesson et al. (2008a, p. 480) note, reflexive writing has increasingly concerned itself with 'the situated nature of knowledge'. Reflexivity is necessary because the researcher can never be 'free' of culture, discourse, or existing theory. This is true when they are making sense of the research experience as it happens. It is equally true when they subsequently write about it.

This vitally important point about researcher experience derives from recognizing that ethnography is an enterprise which is essentially concerned with *cultured* lives and with the way human utterances and field events relate to *cultural* wholes (above, p. 204). If we accept the point being made here about researchers' experiences, however, it must apply with even greater force to the experiences of the people whose 'cultured lives' are being researched. Yet it has become almost a *sine qua non* for 'qualitative' researchers to claim that they can reveal the *lived experiences* of their research subjects. The legitimacy of the much-used notion of lived experience (a quick internet search will reveal its enormous popularity) seems to be so taken for granted that it is rarely graced by any scholarly citation. It appears to have spread mimetically and without reflection on whether a term from phenomenology (Burch, 1990) is consistent with the primarily culture-analysis nature of ethnography. In saying this, however, it has to be acknowledged that an aspiration to get 'inside' people's heads among fieldworkers does exist. Brewer, for example, in his ethnography textbook definition of ethnography, refers to methods of studying people which 'capture their social meanings' (Brewer, 2000, p. 6). He supports this by quoting Goffman's declared object in *Asylums* 'to learn about the social world of the hospital inmate as this world is subjectively experienced by him' (Goffman, 1969, p. 7).

Goffman's declared interest in the *social world* of the asylum patients is wholly consistent with the spirit of ethnography. The same cannot be said about his apparent belief that one can penetrate people's 'subjective experience'. To clarify the issue here, Silverman reminds us that Max Weber was very clear that when he talked about *social action* (the basic subject matter of social science) he was 'not referring to individual states of mind but to the way in which action is defined in relationship to other actors and their intentions' (Silverman, 1994, p. 3). If Weber (1949) was right to say that 'when people engage in action they are more or less unconscious of its subjective meaning', we must ask, says Silverman (1994, p. 6), 'how on earth do you understand its subjective meaning?'. Ethnography is an anthropological or sociological activity rather than a psychological one. Out of modesty, it should drop the conceit of being able to research people's lived experiences. To be true to its anthropological origins, it must also avoid being diverted by such matters away from what we might call its 'proper' mission: the study of the connection between the actions and utterances of people in social settings with the cultures, discourses, narratives, and social, economic, and political structures within which those actions and utterances occur. It is ironic perhaps to say it, but if we focus in this way on 'how things work' in field settings rather than trying to get 'inside' people's experiences or poke about inside their heads and hearts, we might produce work which will be much more relevant to human *experience* and, indeed, to practice. Our research will have greater potential for informing the choices that our readers make about how they relate to the social setting we have studied. In this way, we might help in some small way with the choices that people make in their social lives and, in so doing, shape their experiences of the world.

SO WHAT IS TO BE DONE?

It is vital to increase the general awareness in the organization and management studies field of just what ethnographic work is and how much it can contribute both to the

scholarly 'body of knowledge' and to the practical thinking of citizens, workers, managers, students, and policy-makers. All of these people can benefit, by being enabled to make better-informed choices and decisions, from reading accessible, contextualized, and 'grounded' accounts of 'how things work' in organizations and management. For this to happen, ethnographic work – as long as it is rigorously conceived and practised – needs to be brought into the mainstream of organization and management studies. It has too much potential to be confined to the ghettos of specialist 'qualitative research' journals or to series of heavily priced hardback monographs that few (including many librarians) can afford to buy. Ethnographic materials have enormous potential in the teaching part of our work. I have found it invaluable, for example, to work in a 'negotiated narrative' style of teaching. This sets alongside each other, for critical and theoretical scrutiny and debate, student 'tales from experience' and 'ethnographic episodes' from research (Watson, 2006, pp. xxiii–xvi).

To bring ethnography into the mainstream, new researchers need to be persuaded that they will not be risking their careers if they engage in intensive fieldwork and participative investigations which, by their nature, cannot offer their examiners and article-reviewers large samples or tight and formalized hypotheses. One hopes that the situation has improved in this respect, but when I wrote my own doctoral thesis and first book (Watson, 1977), I emphasized the interviewing programme which I undertook and marginalized the three years of participant observation fieldwork which framed every aspect of my empirical and theoretical endeavours. Newby did something similar in the same period when writing his study of agricultural workers (Newby, 1977a). He later reflected on the pressures of 'academic convention' and how these meant that 'the final monograph . . . contains little of the material gathered through participant observation, despite my voluminous fieldwork notes which I faithfully wrote up every evening' (Newby, 1977b, p. 127).

The supervisors and trainers of new researchers have a large responsibility here. So do the editors and reviewers of academic journals. I am sure that others writing ethnographically have suffered some of the same slings and arrows that I have: being required to state one's initial research questions in studies which did not actually start with any; being told, in effect, to 'bulk up' the discussion of 'the literature' with material from a quite different theoretical tradition from the one relevant to the field studied; being required to explain the techniques used to 'analyse the data' when one had not even thought of the totality of fieldwork insights and notes as 'data'; having to withdraw an article because an editor refused to accept the 'fictionalizing' of the identities of organizations necessary to fulfil promises of strict anonymity. So, yes, we need a greater sensitivity to the ethnographic tradition on the part of academic gatekeepers. But this must not be exaggerated. I say this as one of these gatekeepers myself; someone who too often finds himself sadly closing the gate on pieces of would-be ethnographic writing which fall badly short of the high level of skills of writing, analysis, and persuasion that the genre requires (Bate, 1997; Golden-Biddle and Locke, 1993; Watson, 1995).

An obvious and practical suggestion here is that we ensure we give as much attention to the technicalities and aesthetics of research writing, when we train researchers, as we do to techniques of data collection and analysis. But saying this raises the question of how many research trainees (and, might I suggest, research *re*-trainees) are going to seek this

help with a view to doing ethnography. Well, it should not be too difficult to get more people into fieldwork at what we might call the 'lower' end of organizational hierarchies. One model would be to encourage graduates to enter full-time organizational jobs and to register for a part-time research degree (as I did). Another would be to get more research postgraduates to exploit as research opportunities the part-time or temporary jobs that students often take to boost their incomes (being careful, though, that we do not overdo the production of call-centre ethnographies!). Just what can be achieved here is demonstrated in Bone's (2006) direct selling industry ethnography and Hawkins' (2008) study of recruitment consultants. Established academics might take sabbaticals to enter routine jobs (where, being novices, they would be very well placed to understand processes of 'learning the ropes'). Alternatively, they might exploit the occupational skills learned in previous careers or student vacation work (IT work, nursing, lorry-driving, bar managing, selling, for example).

Variants of these measures might be helpful when we come to the biggest challenge of all for ethnographic management and organization studies. This is the challenge of getting more people to work ethnographically at the managerial and strategy-making levels of organizations. Within organizations, ethnographers have a reasonable track record at the coal-face, workshop, and office level. More broadly, they have told us quite a lot about the dispossessed and powerless of the earth. What few ethnographers have managed to do is to study the rich and powerful. I have been fortunate enough to have written one of the rare participant observation studies of managerial work (as Linstead, 1997, observes) but not only did the top-level corporate managers in the business I studied make sure that they kept me at arms-length, I was told that they also investigated the possibility of legally blocking the publication of the study (it was only possible to disguise thinly the identity of the corporation). However, I got as far as I did because I had skills and knowledge which I was able to 'trade' for high quality research access. There must be a reasonable number of business school academics who might do something similar. There might also be potential high-level corporate career-switchers who possess tradable skills and knowledge. Such people could bring to the party retrospective ethnographic insights on which they could build with formal research work 'back out in the field'. However, none of this is a real possibility unless we can increase the appreciation of and raise the status of ethnographic research in the business school mainstream.

REPRISE AND FINALE

The above statement of a mission to improve the appreciation of ethnographic work and to raise its status reiterates the main theme and echoes several of the variations of the present piece. Let us now move towards the final cadence with a reprise of the claim that well-written ethnographies combine accessibility and relevance-to-practice with methodological and theoretical rigour which potentially puts them at heart of the organization and management studies repertoire.

With the orchestra now at full stretch, brass, strings, woodwind, and percussion come together to make a statement that has been building up beneath the surface all along: the ethnographic study of organization and management, conceived as a Pragmatist enterprise, is inherently critical. True to its commitment to examining the *realities* of and the

truths about how the organizational and managerial world ‘works’, it continually tests intuitive understandings about social life, it continually challenges conventional wisdoms, and it continually questions taken-for-granted or ideologically-grounded assumptions about the world. Critical management and organization studies are thus brought to the centre of our enterprise. Although it is helpful to have a degree of pluralism within our field of study, perhaps we should now concentrate our efforts on rigorously theorized empirical research which gets as ‘close to the action’ as is possible, rather than offering the world of practice *specialized* ‘critical management studies’, ‘critical realist interpretations’, ‘social constructionist perspectives’, ‘discursive analyses’, and all the rest. These various frames of reference have a role to play as *means* towards understanding the real world in which we all have to live. They must not become *ends* in themselves, however. This, of course, applies to ethnography as well. Also, and in spite of what I said earlier, I accept that it is not realistic to look for full-blown ethnographies from large numbers of researchers. Nevertheless, the higher the proportion of researchers that work within the spirit and basic principles of ethnography – adopting an ‘ethnographic orientation’ we might say – the more credible, worthwhile, and accessible will be our contributions to the societies that give us our privileged livings.

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