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Ethnography and the Management of Organisations

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6.1 Introduction

Ethnography is not a new activity. However, there is currently in organisation studies a new interest in ethnography as scholars increasingly recognise the value of research writing that takes readers deeply inside organisations. Also new, I feel, is the realisation that such research may be the only way to increase our understanding of those people whose work is critical to every organisation: the managers. And how, I wondered, might I explain to readers of the present book the nature of ethnography and demonstrate its potential for getting close to the work and lives of managers. I could run through the few existing ethnographic writings on managerial work, picking out features which might inspire and inform people considering doing ethnography. But there exists so little ethnographic work on managerial work to serve this purpose. So, alternatively,

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I could operate in a traditional textbook manner and present in a magisterial voice lists of do's and don'ts that might help potential ethnographers. But this did not feel right. It would not help me to convey what I might call the essential spirit of ethnographic work. But, then again, how could one write about something as ethereal as an 'essential spirit'? I answered this question by deciding to write a chapter looking at some of my own ethnographic endeavours in what I take to be the style of a good ethnographer.

If I were to attempt to write in an 'ethnographic spirit', I said to myself, I could treat my years of experience of this broad style of research as an account of the 'field' of ethnographically inclined research in managerial settings. The chapter would thus be one of those 'tales of the field' that John Van Maanen (1988) writes about when looking at the nature of ethnography, but with the 'field' here being ethnographic work itself. And this is what I decided to do. I recognised that such a venture would not be as easy as it might at first seem. The main challenge was one of pulling off the trick of being simultaneously the writer of a chapter and the subject of it. What do I mean about being the subject of one's own writing? Well, I do not mean engaging in what some writers call 'autoethnography'. My own biography is not in itself likely to be of interest to readers. What might be of interest, however, is an account of the learning process involved in making ethnographies. A classic notion that ethnographers have used over the years, since its introduction in the writing of Geer et al. (1968), is that of the people being studied (medical students in this case) *learning the ropes* of the occupation or organisation in which they are involved. What this chapter does, then, is to share with readers 'the ropes' of ethnographic endeavour in managerial contexts, as I have learned them. To put this another way, everything that I have to say about ethnography comes from what I have learned about succeeding 'in the field' in my various attempts to produce effective and interesting accounts of 'how things work' in organisations, occupations and workplaces. Having said this, I feel it is important to establish just what I mean about a key characteristic of ethnography: the research aspiration to produce *truthful* accounts of 'how things work' in the social world.

6.2 Researching 'How Things Work' in Organisations

Research students are typically encouraged to be clear about their research questions when they embark on a study. Although it is often difficult to be explicit about the research questions behind ethnographic enterprises, it is helpful to try to be explicit about just what it is one is trying to discover or understand better than previously. Let me illustrate this from some early experiences. After graduating in sociology (with 'industrial sociology' as my special interest) I decided that I would like a career as an academic industrial sociologist. And I recognised that to do this I would need to acquire a research degree. There were several factors that discouraged me from becoming a full-time research student, but significant among these factors was the type of broad research questions I had in mind. These questions were shaped by the discussions I had had with my father over the years about the way relationships and activities were managed in the factory where he worked as a spray painter. Why, in particular, were there so many tensions and time-wasting conflicts between managers and workers and between some managers and other managers in that factory?

I learned from friends in other workplaces and from experiences in the organisations where I worked in vacations that this was not abnormal. In my degree work I inevitably came across plenty of studies offering concepts, insights and theories which were relevant to my type of question. But I found very little research which looked at the managerial processes that play a key role in all this. Outstanding, however, was an ethnography in which the researcher actually became an industrial manager and was able to analyse managerial work *from the inside*. This was Melville Dalton's ethnographic study *Men Who Manage* (1939). But I could see no way in which I, as a doctoral student based in a university sociology department, might tackle the sort of questions which Dalton's book had inspired in me—to put it simply, questions about how things generally tend 'to work' among managers and others in work organisations. My industrial sociology tutor then asked me one day when I was reflecting on my future

career, 'Why not seek a trainee managerial role in a large work organisation, register for a part-time research degree and, at the same time, earn a reasonable living whilst you investigate the issues which interest you. And you would end up with a qualification which can launch you into an academic job'.

I did indeed enter a junior management position in one of the world's biggest and most successful aerospace companies. And I not only obtained a research degree but also achieved a professional management qualification—this combination of qualifications being extremely helpful in obtaining an industrial sociology lectureship in the fast-growing academic world of business and management studies. This is all very well. I've told a nice story of career success have I not? And I would be very pleased if I were able to say that I had become an ethnographer through my participant observation investigation. This was indeed true in a *de facto* way. I felt I really 'knew the ropes' about managerial careers and how 'things work' in managerial circles. However, at that time, writing in a fully ethnographic style, recounting events as they occurred and reporting in a reflexive manner on the day-to-day politicking which goes on in managerial ranks, was not seen as a good way forward for an aspiring academic. When I published a journal article on the foundry research (Watson 1982) I got away with simply referring to my 'case study'. I did not even mention my participant observation work, let alone use the term 'ethnography'. And in a subsequent research study on the personnel management occupation, emphasis was placed in the doctoral thesis, articles and book on the interview-based material that I had gathered from 100 personnel managers. Little attention was paid to the day-to-day insider experiences and acquired insights from my participant insider experience (in the first place as a junior manager and, in the second place, as a more senior 'industrial relations manager').

So, what sort of thing had I learned in all of this with regard to 'how things work' in the managerial world? To answer this question, I'll look back to my earliest research venture. Here, considerable conflicts were surfacing over the prospective opening of a very large foundry and it became apparent that a failure of the foundry's senior managers to explain, consult and negotiate over aspects of what was to be an enormous change in everyone's life was leading to powerful opposition by practically the

whole workforce to a move which was to occur only six months later. Having gained the sponsorship of the corporate Personnel Director, I took on an advisory role with the senior foundry-management team. The team was told that I was 'a qualified industrial sociologist'. And, on the basis of my extensive informal 'networking' across the foundry and a formal workforce survey which I designed and carried out (more to give my arguments credibility with the largely engineering-trained managers than to tell me things I had not discovered as a participant observer), I predicted that there would be a foundry strike well before anyone moved into the new 'casting facility'. There followed hours of argument and debate and one or two angry attacks on 'graduate know-alls'. The head of the foundry even called me in to sack me from the company one day, only to be told that I was on the Personnel HQ payroll rather than his. I was clearly learning very quickly 'the ropes' of participant observation research in management settings.

Essential to understanding these 'ropes' was the recognition that engagement in managerial politics, whether one likes it or not, is vital if the researcher is going to learn anything significant about the running of an organisation. There is no avoiding the necessity of researchers having to manage both 'friends' and enemies'. It is an element of 'how things work' in organisational ethnographic investigations. But what about bigger questions of 'how things work' in managerially led organisational change processes more broadly? Here theory and concepts have to come into play. I made central use of a pair of concepts, *orientations to work* and *implicit contracts*, to make sense of the situation in which the senior managers, with two exceptions, were highly committed to personal upward mobility in the company. Very clearly, these men understood that the company, for whom the new foundry was of considerable strategic significance, would reward them well in career terms if they were to succeed with this massive venture. The managers were proud of the scale, design and 'leading edge' nature of what was going to be the world's largest and most advanced 'precision casting facility'. That, at the time of my intervention in the management team, that the bulk of the workforce (middle managers to yard staff, skilled men and women to clerical workers and production engineers) were complaining with increasing bitterness about such matters as the lack of windows in the building, the banning of tea-

making on the shop-floor and, very significantly, the attitude of the senior managers that they 'knew best' when it came to the operation of steel foundries. This, I argued in my research writing, reflects the very different class or life-chance situation of non-senior-manager employees in industrial organisations, compared to the situation of senior managers. The former's implicit contract with the company was not centred on rapid upward career mobility. Succeeding with the world-class 'casting facility' would be recognised and rewarded as a great achievement by the senior managers. But it would, for most staff, involve uncertainty, disruption, paying higher bus fares to get to work and, of great symbolic significance, 'drinking management tea from management vending machines'. And, to focus on just one group of workers, the furnace men (a highly skilled group who 'get very thirsty after a shift') would have no public house to go to after work. There was thus a major gap between the priorities, the implicit contracts and the career expectations of the managerial 'dominant coalition' and the rest of the workforce.

At one Sunday morning 'steering group' meeting (where, interestingly from an anthropological point of view, tweed jackets and flannels were worn instead of the weekday 'senior manager' dark business suits) there was an attempt to put 'workforce complaints' down to the company's mistake of allowing 'a bloody sociologist' into the foundry. My response was to argue that my analysis was 'true' and I nervously suggested that it 'fitted with existing sociological research on change programmes'. Most boldly, I suggested to them that I would be willing to return to work in the Personnel HQ as long as they would promise to invite me back to the foundry to help them out when strike action 'starts to bite just before Christmas'. With this, the meeting was adjourned ('the ladies at home will have Sunday lunch ready'). The next day I was invited to remain in the foundry to devise a formal programme to 'involve' the workforce in the final stages of preparation for the move. I am pleased to report that this did happen and that the foundry did not have a strike. And, although I was encouraged not to write up my research in a fully ethnographic style (a notion I shall I explain later), I had learned the ropes of doing participant observation research among managers. And part of the learning that I have passed on to others over the years is that, if you want to gain research access to managerial goings-on, gain insights into strategic

activity and, it has to be said, get 'close to power', then there is no option but to deploy the highest level of social, political and rhetorical skills that one can manage. It cannot succeed without this. And this is something I was highly aware of when, 20 years later, I negotiated a one-year secondment from my business school with the senior management of another large company. This was in order to write the book which became *In Search of Management* (Watson 2001), a study I shall come back to shortly.

6.3 Ethnography and Truth-Telling

You may have noticed that, earlier, I mentioned the research aspiration 'to produce truthful accounts of "how things work" in the social world'. But the notion of truth is an exceedingly difficult one to use. I would have struggled at the time of my early research to fully articulate a philosophically and sociologically sound explanation of what I take 'truth' to be. Yes, I needed to persuade both my career sponsor and the foundry managers that my analysis of the problems in the foundry was a true one. One rhetorical move, offered by me and taken up by my Personnel Director sponsor, was to invoke the notion of professionalism and (social) scientific knowledge. The implication was what I said should be accepted and my advice acted upon because I was a trained and qualified industrial sociologist. Probably much more significant was the threat 'on your heads let it be if you ignore Tony's analysis'. In reality (I nearly said 'in truth'), one cannot predict events like strikes any more than one can simplistically present analyses of workforce attitudes as 'facts'. However, I was very happy to argue that the definition of the situation in the foundry that I was putting forward was a much wiser one to work with than the definition adopted by the majority of the senior managers. This was, to put it simply, 'all these negative statements made by the workforce are the result of foolish rumours; once people actually get to this superb new building they will recognise how much their working lives have improved'. This might, of course, be true. But I believed, and strongly argued, that my analysis was the truer one in the sense that, if one were to act on the basis of *my* definition of the situation, then the new foundry venture was more likely to be successful than if one acted upon the foundry senior manager one.

Although I did not clearly articulate this notion of 'relative truths' at the time, it was implicit in the foundry senior managers' eventual acceptance of such a position. And, of course, to adopt my view (supported, remember, by the politically influential Personnel Director from Company senior management) was to avoid any risk to their ambitious career plans. This point has to be made in order to remind us that different 'points of view' or definitions of the situation in organisations must always be understood in the context of organisational politics and career interests. And, of course, this did not just apply to the managers and the workers. I, as the 'expert researcher', was no disinterested party to what occurred; indeed, my career in the company was 'made' by my role in what became the successful and strike-free move to the new 'precision casting facility'.

Twenty years after these events, I found myself embedded for a year in a large telecoms development and manufacturing organisation as a senior manager and (overt) participant observer. My intention was to write what I hoped to be an important book about managerial work 'from the inside'. And it became clear that it would be academically necessary to deal more directly with the question of the 'truthfulness' of ethnographic writing than it had been back in my aerospace days. But it was not just a matter of what I would say to an academic audience. The managers I worked with as a colleague all knew that I was going to write a book at the end of my secondment to the company. And, one day, several of them (all with engineering and science backgrounds as in the previous business) challenged me on the validity of the book that I would write: 'You promised us at the start that you would change names, job titles and various other things so that no individual quoted or their action described in the book would be recognised by readers of the book. So how can you possibly claim that you are going to tell the truth about the managerial work we do?' It so happened that one of those managers had that very afternoon given me a copy of a management magazine that included a feature on the company. This was a glowing account of the brilliant success in change management that the company was achieving. It was clear who the journalist had interviewed for the article, not just because of the terminology, but because the account provided was one which was impressively career enhancing for two particular managers. And the picture

painted was one of harmony across the business, both within management itself and between the company and its employees.

No mention was made in the magazine article of either the enormous tensions between different groups of management in the company or the current trade union veto on key elements of the change programme. My colleagues looked at the document and I commented, 'I know from your faces that you don't think much of the article. But we cannot see it as it altogether untrue'. Where I would question the value of the article, I suggested, would be in terms of how helpful it can be as a guide to what is happening in the business if it were given to someone who was coming to work here. The managers agreed that it would be helpful in some ways, but most definitely not in others. It implied that, as one woman put it, 'life here is all sweetness and light. What could be more misleading than that'. And, I said, when it came to reading my book, 'you will probably say that some parts of it are truer than others. However, what I promise you now is that you will find my book to be a lot truer about how things work in the industrial world than what is in this magazine article'.

What I did not go on to say to the managers I was talking to was that I had now found what I referred to earlier as an epistemologically sound justification for this notion of relative truth. I had always had at the back my mind first-year degree-course learning about Popper's view that science can never lead to the discovery of final and irrefutable truths—all it could do was to improve on the existing knowledge current at any given time (Popper 1959). But towards the end of my year in the telecoms factory, my academic reading focused on (American) Pragmatic Philosophy and the distinctive notion of truth claims that it offered. This, like Popper's writing, suggests that there are no final or conclusive truths to be discovered. Any one piece of knowledge, research writing or teaching may, however, be more 'truthful' than another—in the sense that the knowledge in the 'truer' case could act as a better guide to action in the aspect of the world to which it related than the 'less true' one. At the simplest level of the new ethnographer learning the ropes of their trade, this 'test' of relevance to what people might potentially do in light of the knowledge they are creating, this Pragmatist notion of truth is immensely helpful. And, in the broader context of academic research on organisations and management, it suggests an enormously helpful role for

ethnographic style research reports. Insofar as the ethnographer's writing is about what they learned by 'getting close to the action', so we will have alternative learning material for management and business students who are currently so dependent on over-rational and prescriptive management textbooks.

6.4 What Do We Mean by 'Ethnography'?

Readers might have noticed that in this chapter so far, 'ethnography' has not been formally defined. And further, nowhere has the research work described and discussed been presented as 'ethnographic research' or the investigative work been portrayed as 'doing ethnography'. Terms like 'ethnographic work', the 'ethnographic enterprise' and 'ethnographic writing' have been used, however. So, what is going on here? Well, strange as it may seem in a chapter written in a research methods book, I want to argue that to get at the essential qualities of ethnography, it is helpful not to treat it as a research method at all. All the research looked at in preceding paragraphs is centred upon the broad research method of intensive field research and, more particularly, on participant observation. Intensive observation, with varying degrees of active participation in organisational processes, is a *necessary* condition for the production of ethnography. But it is not a *sufficient* condition. This is because ethnography is better understood as a form of writing, rather than as an investigative method. My formal definition of ethnography is *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* (Watson 2011, p. 205).

The two main clues to finding the essential qualities of ethnography lie in its origins in anthropology and in the word 'ethnography' itself. Thus, we can say that ethnography serves an anthropological interest in understanding the human as a *cultured being* ('ethno') through writing about them ('graphy') in a manner which provides deep insights into humans' *cultured lives.* To talk of 'cultured lives' in this way means relating the

details of the particular events and utterances observed, heard and experienced in the field to a *cultural whole* (Baszanger and Dodier 2004; Watson 2012). Thus, my foundry research account set the particularities of events in the aerospace company in the context of the differing social class imagery of people working there and in the emphasis in the organisational culture on managerial status aspiration. And the account of managerial life in the telecoms business contextualised the orientations of the managers and the events which unfolded in terms of competing managerial discourses which exist across the culture of contemporary work organisations and their management. Similarly, a study of a pub and brewing business located the organisation in the context of the role of pubs in the lives of English people and gave particular attention to the social phenomenon of the 'real ale' movement. And ethnographic work in an English village was set in the context of broader processes of urban–rural shifts over previous decades.

Another very good reason not to treat ethnography as a method is in order to keep open the possibility of using a variety of other research methods to complement the essential intensive fieldwork necessary for an ethnography. Earlier, I mentioned the survey carried out in the foundry project. On reflection, I do not think I would have spent the time on this were it not for its 'political' value in my arguments with senior managers. However, in retrospect, I certainly would have done the set of interviews I carried out with each one of the foundry's senior managers. This was enormously helpful in making sense of many of the events in which I had seen them participating. But most fruitfully, it gave me an opportunity to discuss at length, in private and confidential terms, the reservations about the new foundry which I had begun to infer had developed with two of the managers. Initially, each of these men expressed in technical or business terms their reservations about the change. But very soon, as the conversation developed, each of them turned to their own current work orientation. For very different reasons, neither man wanted further promotion in the company. This meant that the enormous disruption in their working lives that was beginning to occur would not be compensated for by the sort of future career 'beyond the foundry' which excited and motivated their colleagues. Coming to understand these two 'deviant' cases provided an analytically powerful *comparative* boost to the

understanding of the orientations of the rest of their colleagues. It is possible that a skilled researcher visiting the foundry to interview managers might have elicited the sort of information that came out of my conversations with people whom I had got to know very well as colleagues. But I very much doubt it.

Interviews (in the sense of formally structured and recorded conversations) carried out by 'embedded' researchers are rather different from standard interviews. In my later major ethnographic study I waited until I had spent six months working alongside managers before I set up formal tape-recorded and structured interviews with 60 of them. The 'added value' of interviewers carried out by participant observers is more than a matter of their creating a higher level of trust between the parties (vital though this is) as Spradley explains in his book *The Ethnographic Interview* (1979). It enabled shared experiences and events to be examined and jointly considered to illustrate and 'fill out' day-to-day conversations. And, time and again, it valuably threw light on events and arguments that I had recorded in my day-to-day field notes. It was quite common for interviewed managers to comment to me, in the words of just one of these people, 'You'd never have got all that stuff out of me if it wasn't that I know you well. And I know you've seen enough of me in action for it to be impossible to bullshit you about what a great manager I am'.

In the same way that there can be a process of mutual reinforcement between the outcomes of interviews and the observations made through organisational participation, it is possible that small surveys, quantitative data analysis and documentary discourse analysis can all be brought into service in the process of preparing for an ethnography. The material produced by the use of these methods does not function as additional 'evidence', so to speak. It has to be woven into the fabric of the piece of ethnographic writing as a whole.

6.5 And Finally: Writing One's Ethnography

The phrase 'preparing for an ethnography' was used above to cover all the investigative work carried out by the researcher in the field (as well, often, in the library and in relevant archives). This utterly is not to play

down the significance of research work in the field. Ethnography only comes about, however, when this material is pulled together into a clear and coherent narrative together with appropriate concepts and theories from the social sciences, all of this relating detailed and specific matters to broader social and cultural 'wholes'. To give the degree of clarity and coherence that this requires, it is invaluable for the writer to use writing techniques found in 'creative' writing, and in novels particularly. Given its anthropological roots, ethnography draws on both the humanities and the social sciences. If we see both social science writing and high-quality novels as being concerned to identify and reflect upon truths about how the social world works, then it seems wise to develop a form of writing that brings together the strengths of both of these forms.

In ethnography, science provides research questions, concepts, theories and research techniques while creative writing such as novels provides techniques of narrative-shaping, engaging descriptions of people, places and events, and the presentation of research subjects' own words, thoughts and contributions to dialogues and conversations—conversations with each other and with the researcher. In addition to all of this rather challenging set of requirements, the ethnographic writer needs to build a trusting relationship with readers through taking them along with them in their engagement with the particular social setting that they have researched. This is most effectively done by the researcher writing in a reflexive manner: including themselves in the story, so to speak. This enables the reader to take into account whatever biases, interests, purposes, social skills and general human frailties that the investigator was throwing into the fieldwork mix. A wholly objective research account is never possible but one can get closer to it if the researcher/writer has revealed their hand throughout. I hope that I have done this effectively in the present chapter and that, taking into account my clear research preferences and beliefs, readers decide for themselves whether they wish to engage in ethnographic work—with all its tensions, frustrations, joys and opportunities to say something worthwhile and convincing about how organisational management actually works.

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