

# 2

## ▷▷▷▷▷ Sensibility Two Questions of Knowledge

### Introduction

This sensibility introduces the tricky question of knowledge in ethnography. The discussion is designed to build on the previous strategic sensibility. I will, first, establish some initial definitions for the concepts to be used in the subsequent discussion. Second, I will look at three distinct approaches to knowledge in ethnography – the realist, narrative and reflexive approach. I will suggest in this section that each of these approaches is complex and can sometimes overlap, and that all are subject to many arguments regarding, for example, the most appropriate way to approach realism or reflexivity. Third, I will consider some traditional questions of research methodology and knowledge (questions of validity, usefulness and rigour) which are appropriate to forms of organizational ethnography.

### Questions of epistemology and ethnography

#### Definitions

Under the last sensibility I suggested that ethnographers can begin building an ethnographic strategy prior to entering the field and can use such a strategy to orient their activities in the field. The strategy can be useful for negotiating access, for figuring out appropriate first steps in the field and for prising the ethnographer away from full immersion in the field setting. However, I stressed under the last sensibility that an initial strategy should not be treated as a step-by-step guide to be slavishly followed in completing ethnographic research. Instead, the strategy should be constantly worked upon and kept at the forefront of considerations of what to do next. One important aspect for building an ethnographic strategy is

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entering into questions of knowledge: what kind of claims to knowledge can ethnography make? On what basis can ethnography demonstrate the validity, usefulness or rigour of its claims? Do assessments of ethnographic claims to knowledge represent further opportunities for ethnographic investigation?

Such questions of knowledge are intertwined with forms of ontology and epistemology. Prior to entering into an analysis of different claims to knowledge made through ethnography, I will propose some outline definitions of these areas. First, *ontology* relates to questions of the status of the world, including questions of how the world is made up and how aspects of the world hold together. Second, *epistemology* relates to the kinds of claims to knowledge we can make about the world. An epistemology establishes the basis for what can be known and how that knowledge can be assessed. As we shall see in the next section, forms of ontology and epistemology shift between different approaches to ethnography. That is, the kinds of approach to the world (what the world is made up from) and the kinds of claims to knowledge about the world (what it is we can say about the world) vary between ethnographic approaches.

Third, we should note that different forms of ontology and epistemology are frequently positioned in social science research in terms of oppositions. There are oppositions composed between *essentialism* (that things in the world, for example technologies, have certain properties) and *relativism* (that the properties of things are not fixed and need to be understood in their varied ways), between *positivism* (that the world exists to be analysed and that predictive, scientific laws can be developed) and *interpretivism* (that the world is open to multiple claims as to what is going on), and between *realism* (that the world exists to be explained, although not necessarily through predictive laws) and *constructivism* (that any version of a local aspect of the world is a local accomplishment, including ethnography itself). Such oppositions can be shifted around so that, for example, essentialism is understood as opposed to constructivism. These oppositions are also mapped on to distinctions made between *objective* (the world provides the material from which research extracts) and *subjective* (the research is the inevitable product of the researchers' engagement in the world) forms of research and between *inductive* (research which draws general conclusions from specific research) and *deductive* (research which involves the production of a hypothesis to be tested) approaches to research. While I only offer a very brief introduction to some possible ways of thinking about these topics here, the following section will provide more ethnographic detail on the ways and means through which these areas are made to make ethnographic sense.

The following section collates different ethnographic approaches under three headings. I have done this in order to provide a reasonably succinct and coherent description of the principal ontological and epistemological considerations entered into by ethnographers. However, readers should

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note that these three headings also incorporate a variety of subtle (and not so subtle) ontological and epistemological distinctions which I shall begin to discuss here and will pick up on under subsequent sensibilities.

### **Ontology, epistemology and ethnography**

#### **Realist ethnography**

In exemplar two, the work of Malinowski is used to establish one version of a science of ethnography. The claim to science invoked in this exemplar is that there exists a rigorous and coherent means of engaging with an ethnographic field-site. Malinowski aimed to establish ethnography as an academic discipline. Hence his appeal to rigour, to standards and to regular ways of doing ethnography need to be understood in this light. For our purposes, Malinowski's scientific approach to ethnography introduces a particular set of ontological and epistemological issues. Malinowski enters into the ethnography claiming that the world is there to be understood and that a rigorous, robust and routinized (ethnographic) way of engaging with the world can be developed. The ontological underpinnings of this approach are realist: the world is available for engagement by the ethnographer. The epistemological approach here is also realist: the data collected can be assessed for the extent to which it accurately reflects the field-site from which it has been collected. Although 'science' is proclaimed as the standard to be achieved, Malinowski's work need not be thought of as positivistic, at least in the sense that he is not seeking to establish universal laws that govern both Trobriand islanders and those living in other parts of the world, and he makes no attempt at predictive modelling. Realism and positivism do not necessarily need to be understood as one and the same (for more on this, see Blaikie, 1993).

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### **Exemplar Two**

B. Malinowski (1922/2002) *Argonauts of the Western Pacific* (Routledge, London)

The purpose of this exemplar is to provide three principle insights for those interested in organizational ethnography. First, this summary will offer an introduction to the work of Malinowski, and provide detail on how his work formed an important contribution to the historical development of ethnography. Second, Malinowski's approach to realist, objectivist, scientific ethnography will be introduced, providing insights into the epistemological underpinnings of his work. Third, ideas of exchange and the ways in which apparently economic relationships are heavily implicated in issues of magic, myth and religious belief in Malinowski's work will provide a starting point for considering the usefulness of early anthropology for studying the modern corporation.

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### ***Malinowski and the history of ethnography***

Malinowski's *Argonauts of the Western Pacific* is based on a two-year ethnographic study of Trobriand society in New Guinea. Malinowski's work was an important contribution to early anthropology, resisting the convention for dependence upon second-hand accounts of particular settings in favour of getting close to the action. This 'getting close', however, involved Malinowski living with the tribes being studied, actively participating in their rites and rituals, learning their language and ways of life. This was something of a departure from most anthropology of the time, which involved 'getting close' by living with missionaries or colonial dignitaries who were, to an extent, 'outsiders' to the local setting. For Malinowski, although such relations with missionaries or colonialists were an important starting point for a study, it was far more valuable to shed off the shadow of the colonial administrator in favour of attempting to get as close to the action as possible. However, this was not a straightforward methodological development. Malinowski opens his book by expressing concern that ethnographic research:

...is in the sadly ludicrous, not to say tragic, position, that at the very moment when it begins to put its workshop in order, to forge its proper tools, to start ready for work on its appointed task, the material of its study melts away with hopeless rapidity. Just now, when the methods and aims of scientific field ethnology have taken shape, when men fully trained for the work have begun to travel into savage countries and study their inhabitants – these die away under our very eyes. (2002/1922: xv)

Malinowski suggests that ethnography in the early part of the twentieth century occupied a tense position between the need to establish its methodological rigour (see next section) and the constant loss of suitable sites for study. The colonial domination and (what might be termed) 'civilization' (in the sense of transforming a tribe to live along the principles of western European moral, religious and cultural principles) of tribal cultures resulted in fewer locations to study. This was to be followed by the dissolution of colonial territories and the concomitant decline in the number of opportunities to study tribal settings through the auspices of colonial management. For Malinowski, however, this decline in field-sites was particularly painful as he felt the field of anthropology was only just setting about the business of establishing its methodological principles. To emphasize the importance of Malinowski's work lay in his attempts to establish rigorous anthropological principles, though living in the setting under study.

I therefore had constantly the daily life of the natives before my eyes, while accidental, dramatic occurrences, deaths, quarrels, village brawls, public and ceremonial events, could not escape my notice. (2002/1922: xvii)

This theme of ethnographic immersion in the field might appear similar to modern ethnographic concerns. However, two points require careful consideration. First, Malinowski was working hard to render this principle of immersion in the field as the proper approach to ethnography. It was by no means settled that this was the 'proper' way to go about ethnographic research. Second, he wanted to tie this immersion in to the development of ethnography as a scientific method of enquiry.

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### ***Realist, scientific ethnography***

Malinowski's work can be seen as part of an attempt to establish and secure a future for ethnographic research through paying particular attention to (Malinowski's own version of) methodological rigour. In this book he clearly sets out the principles by which he argues research should be done.

One of the first conditions of acceptable ethnographic work certainly is that it should deal with the totality of all social, cultural and psychological aspects of the community, for they are so interwoven that not one can be understood without taking into consideration all the others. (2002/1922: xvi)

For Malinowski, forms of exchange needed to be considered in relation to social relationships, myth, magic and rituals. However, this was part of a scientific, realist and objectivist study. Thus economic exchange alongside myth and magic were opened up for ethnographic analysis. Each of these areas was now accessible for ethnographic investigation and, furthermore, the methods and techniques of collection were to form an important aspect of writing.

The results of scientific research in any branch of learning ought to be presented in a manner absolutely candid and above board. (2002/1922: 2)

I consider that only such ethnographic sources are of unquestionable scientific value, in which we can clearly draw the line between, on the one hand, the results of direct observation and of native statements and interpretations, and on the other, the inferences of the author, based on his common sense and psychological insight. (2002/1922: 3)

Malinowski did not restrict his thoughts on the science of ethnography to declarations regarding the ways in which ethnographic writings should be presented. He argued that there were three principles which needed to be followed in completing a scientific ethnographic enquiry. First, the organization of the tribe and an anatomy of its culture needed to be recorded clearly. The ethnographer should provide a 'concrete, statistical, documentation' of the tribe (2002/1922: 24). Second, this documentation should provide the basics of an ethnography which could then be fleshed out through the provision of detail on what Malinowski termed 'the imponderabilia of actual life,' (2002/1922: 24). Third, these ethnographic details needed to be recorded alongside documentation of the 'native mentality' (2002/1922: 24). This mentality should be sought through statements from natives, characteristic narratives of the natives' concerns and items of folklore and magic. He argued that the final goal of such scientific ethnography should be to 'grasp the natives' point of view ... his vision of his world' (2002/1922: 25).

This talk of a scientific, objectivist, realist approach to ethnography appears to situate Malinowski's approach to ethnography within a different set of epistemological concerns from the narrative and reflexivist approaches. However, the need to get close to the action, to immerse oneself in the setting, and to attempt to take into consideration multiple 'things' which might be going on (the economic, social and religious, not just exchange relationships) each appear as relevant now as they did in the early part of the twentieth century. It is difficult to comprehensively assess from a contemporary perspective the need to establish ethnography as a field of research

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(given that anthropology and sociological forms of ethnography are now well established) and the necessity of invoking science as the basis for methodological rigour. It could be argued that this attempt to establish anthropological ethnography as useful and rigorous finds a close match in contemporary attempts to establish the usefulness of ethnography in organizational settings (see sensibilities one, four and the Conclusion).

### ***Exchange and the value of early anthropology***

Beyond this comparison between establishing ethnography in colonial and organizational settings, Malinowski's work provides a variety of further insights for organizational ethnographers to consider. First, his approach to exchange relationships among Trobriand islanders offers insights into the way exchange forms a focal point for social organization of the islanders. 'Kula' is the local term for a form of ceremonial exchange which maintains reciprocal relations between islanders. To give establishes and maintains the reciprocal obligation to receive something of similar status. However, Malinowski argues that the equality of such exchange is not held together by any particular law or sanction. Instead, the expectation and social order is subtle and held together by a variety of status, symbolic, mythic and ritualistic relations. These ideas might aid the organizational ethnographer in throwing into relief (thinking about things from a different perspective) the informal, non-sanctionable relations which pervade the modern workplace and are crucial for the maintenance of the workplace. The ways in which employees interact might be held together by similar complex social and psychological relationships as the Trobriand islanders.

Second, Malinowski's analysis of exchange focuses on the material artefacts involved in social organization. The canoe forms a focus for holding together these social exchange relations. The building of a canoe, in a similar vein to the introduction of a new technology in the workplace, is a site for superstition, suspicion and ritual. Treating objects as the centre for social interaction and observing the complex social relationships in which an identity for the object is made and maintained can be an important means through which to develop an ethnographic understanding of the local organization.

Third, Malinowski's approach to ceremony among those he studied reminds organizational ethnographers of the importance of staged interactions among corporate employees. Ensuring ceremonies are carried out in the correct manner, with members taking appropriate roles, responding in conventional ways, doing things at the right time, ensures the continuation of the tribe, its rituals and its members' identities. Corporate presentations, product launches, press conferences and boardroom meetings, among other organizational events, could be approached in much the same way, revealing something of the organization and its members.

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This form of realist ontology and epistemology has led to debates in ethnography about the quality, accuracy and reliability of some ethnographers' findings. For example, Margaret Mead's (1928) work claimed that teenage girls in Samoa experienced a less troublesome transition from teenagehood to adulthood than, for example, American teenage girls. Yet other

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ethnographers visiting the same islands argued that Mead's findings were flawed (Freeman, 1983). Critics such as Freeman argued that Mead did not know the language, had not immersed herself fully in the field (by staying with non-natives), had been duped by locals who were playing a joke on her and had misunderstood key aspects of Samoan life. These criticisms were established on further realist grounds: Freeman was closer to the action, lived with the islanders more closely, was more closely incorporated into their ways of life, learnt the language and was not so easily misled by locals' stories. In this criticism, the ontology remains the same: that there is a world out there from which observational data can be extracted. The criticism is also predicated on the same epistemology: that the version of the world presented is available for testing in terms of how accurately it represents the world out there.

However, responses to this criticism have shifted the ontological and epistemological grounds for debate. Critics have suggested that work such as that of Freeman failed to apply the same standards of criticism to their own work. Rather than finishing the argument here, Shore (1983) goes on to suggest that perhaps Mead and Freeman experienced different aspects of Samoan islanders' lives, that engagement at different times with the islanders might reveal different facets of their lives and that each ethnographer engaged with different aspects of teenage-hood. The ontological grounds for the argument are moved here from a realist perspective (that the world exists) to a relativist perspective (that there is not one single version of the world available). The epistemological basis for the ethnography also shifts. In place of questions regarding the accuracy with which the ethnography represents reality (including questions of who has the authoritative account of events) come suggestions that each of the ethnographies reports an aspect of the social life of the islanders. This provides a constructivist epistemology as a critique of a realist ethnography. Instead of a single, knowable reality providing the basis on which to assess an ethnography, there is a range of distinct views (for example, from different ethnographers, islanders and colonialists) that might provide a basis for considering the ethnography (see 'Reflexive ethnography' section below).

In this debate it could be argued that the islanders themselves provide an important means to assess the validity of the research. A similar point can be expressed in relation to organizational ethnography where members of the organization can form an important group who may express views on the ethnography. This can be treated in a more or less realist fashion. Members' views of the ethnography can be utilized to assess the (realist) accuracy of the ethnography (for example, do members feel that the ethnographer has made the same sense of the organization that they have, see exemplar seven) or members' views can be utilized as part of the (socially constructed aspects of the) research (for example, members' views of the ethnography expressed in an early phase of the research can be treated as data to be engaged with in later stages of the research, see exemplar fourteen).

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### Critical realism

Realism is not a singular entity. In order to introduce, briefly, an alternative to the realist suppositions of Mead and Malinowski's work, I will present some of the arguments of Dellbridge's (1998) factory floor analysis. In this ethnography of Japanese models of manufacturing exported to UK contexts, Dellbridge rejects positivist assertions that social science research can and should propose hypotheses to be tested, in order to produce predictive laws, based on assumptions that there is a world out there to be studied which is governed by logic (this critical, realist and non-positivist stance is mirrored in other ethnographic work, see for example, Jordan and Yeomans, 1995). Instead, Dellbridge approaches the world as a social product of the interactions which go into and maintain its production. However, unlike reflexive social constructivists (see 'Reflexive ethnography' section below), Dellbridge argues that his approach should be considered as a form of critical or theoretical realism.

This research is actually founded on a form of 'theoretical realism' ... this position regards social action as occurring within relatively enduring social-structural conditions which do not determine those actions but do constitute a form of 'objective reality' within which those actions take place. (Dellbridge, 1998: 17).

Ontologically, Dellbridge's work suggests that there is a real social world out there to be investigated and that this social world is the outcome of ongoing social interaction. Social reality further provides for particular structural relations that do not then necessarily fix or determine social outcomes, but social actions are outcomes through which we can see that particular structural relations may have played a role or may provide a tendency towards a particular outcome. Epistemologically, Dellbridge's work provides for a tentative means to assess the validity of ethnographic claims. He argues that forms of data collection are subject to bias and forms of interpretation entered into by the researcher, but this should be treated as a challenge rather than a problem which undermines research. Epistemologically, the challenge is to open up the ethnography for readers to assess the validity of the claims being made.

The way I see this challenge is thus: the researcher is responsible for persuading the reader that the work is worthy of their time and consideration, rather than rendered useless due to the biases inherent in collection, interpretation, and presentation of the data. (Dellbridge, 1998: 18)

In Dellbridge's work we can begin to see the complexities of titles such as 'realism'. The title can be used (just about) to cover an array of differing approaches based around a premise that a social reality is available for analysis. That premise, however, provides the grounds for the development

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of a distinct range of ontological and epistemological positions (Dellbridge's work only provides for one alternative version of realism; further versions can be found in exemplars seven and thirteen).

### Narrative ethnography

I will use the term 'narrative ethnography' in this section to group together ethnographies which involve an account that is developed through relations between an ethnographer and one important research participant, sometimes called a key informant. I use the term 'narrative' in the absence of any more recognized term for grouping these studies together. Key informants can often also act as gatekeepers for an ethnographic study, enabling the ethnographer to access the field-site being studied. Such key informants and gatekeepers often proceed to constitute much of the narrative of the ethnography. They are sometimes the best story-teller in the tribe (see, for example, Smith, 1981), are someone who is particularly interested in the process of ethnographic research (see, for example, Harper, 1998; exemplar one) or hold a particular position in the group being studied which enables them to offer a particular kind of narrative of the group. The latter type of relationship can be seen in the work of Whyte (1955; exemplar three). Whyte developed a relationship in the field with a key informant Doc, whom Whyte suggests is the leader of the street corner gang he is studying. Whyte also acts as something of a gatekeeper, taking an interest in Whyte's work and introducing Doc to various areas of street corner life (from Saturday night bowling to competing for the attention of local girls).

This form of narrative ethnography and the relationships it involves, raise particular questions of ontology and epistemology. Frequently, narrative ethnographies are predicated upon relativist ontology. The claim is often put forward that the narrative is an account among many possible accounts of the nature of the particular world in focus. This need not be seen as problematic. If the aim of an ethnographic study is to study CEOs' views of the world of corporate branding, it may be quite conceivable that these worldviews would be distinct from other members of the same organization without such difference undermining the valuable insights provided into CEOs' worldviews. The same can be said of Whyte's (1955) study. The production of a detailed analysis of Doc's view of street corner life is very different ontologically from contemporary media representations of the area where Doc lived. In place of a media version of the street corner world based on numbers (for example, crime rates, population statistics, rates of poverty) and scandals, Whyte provides an ethnography based on the detail of everyday life. Whyte suggests that through ethnography, in place of numbers, come lives.

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### Exemplar Two

W. Whyte (1955) *Street Corner Society* (University of Chicago Press, Chicago)

Whyte's study of street corner society in 1930s' Boston provides an example of an early sociological foray into the field of ethnography. It is a product of the Chicago School of sociology and has its own particular brand of practical politics. The study of street corner life was designed to counter the then contemporary media and political accounts of life on a street corner:

Through sight-seeing or statistics one may discover that bathtubs are rare, that children overrun the narrow and neglected streets, that the juvenile delinquency rate is high, that crime is prevalent among adults, and that a large proportion of the population was on home relief or W.P.A. during the depression. In this view, Cornerville people appear as social work clients, as defendants in criminal cases, or as undifferentiated members of 'the masses.' There is one thing wrong with such a picture: no human beings are in it. (1955: xv)

Whyte's approach did not just advocate getting close to the street corner, he also sought to provide a picture of the mundane, ordinary, everyday aspects of street corner life. This ordinariness was an important feature of the study – to counter spectacular stories about crime, unemployment and so on, Whyte wanted to show how street corner life is mostly banal. The importance of this study for considering organizational ethnography is that it provides a form of narrative approach to the methodology, it offers important insights on field relations and becoming a member of the collective being studied, it provides detail on ways to represent complex social relations, and it says something of the fragmentary and changing features of social organization.

#### ***An introduction to narrative forms of ethnography***

'Narrative' can be used as a term to group together a particular epistemological approach to ethnography. By epistemological approach I mean the approach to knowledge which the author takes in observing and writing about observations. Narrative approaches to ethnography are based around a particular key informant who provides much of the narrative of the group or organization being studied. In traditional anthropological studies of tribes in far-flung places there was often a particularly good storyteller, willing participant or gatekeeper who provided much of the insight into what went on in the tribe. This is equally the case for ethnographies closer to home. In Whyte's study of street corner society, Doc is both the key informer and gatekeeper for the study. Doc enables Whyte to find a position within the men whose lives are based around various activities on the street corner and Doc provides a great deal of the narrative about their lives (from explanations of bowling and social status to the group's occasional interest in girls).

Narrative ethnographies involve epistemological questions regarding the account offered by the key informant. For Whyte, the account offered by Doc is available to be assessed in realist terms (that is, how accurately the account portrays street corner life). However, not all narrative ethnographies take this stance. Whyte uses Doc's narrative to build up a history of street corner life (through Doc's tales of his childhood) and to

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provide a context within which street corner life unfolds (through Doc's tales of what goes on elsewhere, what histories others bring to the street corner, where people's families come from and so on).

### **Access**

Accessing street corner life is made possible by Doc acting as a gatekeeper for Whyte. Through this relationship access is relatively straightforward. However, such an access story can still be investigated for what it tells us about the organizational details of street corner life. Whyte is brought into the street corner through Doc. Doc represents himself as the toughest guy on the street corner and suggests others defer to his judgement (or even seek his judgement at particular times). Doc suggests the consequences of not acting in line with Doc's expectations are nearly always a fight or at least the threat of violence. Doc represents himself as the best fighter on the street corner. Not listening to Doc is akin to asking to challenge his position and challenge him to a fight. Being introduced to the other members of street corner life by Doc is therefore like being introduced by the chief of a tribe. To question the introduction of Whyte would be to ask for a fight.

Although it might seem an unlikely comparison to make between the self-professed toughest guy on the street corner and what might happen in a modern corporate organization, there are parallels to be drawn. The organization can be investigated for the ways in which status might be made and maintained and differentiated between members. The ethnographer might want to reflect on their own status in relation to other members of the organization. If the ethnographer's access is predicated on one particular gatekeeper, their position within any notional organizational hierarchy might be considered. While for Whyte's study the hierarchy is made and maintained through fighting, a wish to avoid fighting and the need to emphasize 'respect', modern corporate organizations can also be studied to investigate the ways in which status is actively achieved. An ethnographer's status may relate in complex ways to the status of the member who introduces them into the organization (see sensibility three).

### **Becoming a member**

Although Whyte uses Doc to gain access to the collective activity of street corner life, this is not the same as saying he has become a member. Becoming a member, attaining the status of accepted and regular, ordinary participant in the group requires more than access. In Whyte's study, ten pin bowling becomes an important activity for demonstrating and achieving status. Whenever Doc, Mike and Danny (those Doc perceived to be of highest status) were bowling against Alec and Joe (those Doc perceived to be lower in the social hierarchy), it was no longer just about bowling but establishing and maintaining social order. Doc suggested that when bowling, Alec or Joe could not win:

They wouldn't have known how to take it. That's why we were out to beat them. If they had won, there would have been a lot of noise. Plenty of arguments. We would have called it lucky – things like that. We would have tried to get them in another match and ruin them. We would have to put them in their places. (1955: 21)

Whyte uses the bowling as an example of the way street corner life is stratified and organized. What Whyte also reveals is the extent to which he became a member of

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the organization. It seems that he was always held somewhat at arm's length from the group. In the bowling competition Whyte was asked to participate after one of the regulars had to pull out. Whyte went on to win the competition, but received none of the pressure (noise, catcalls, pushes) that others received when trying to bowl. Doc explained this activity as follows:

We didn't want to make it tough for you, because we all like you, and the other fellows did too. If somebody had tried to make it tough for you, we would have protected you. ... If Joe Dodge or Alec had been out in front, it would have been different. We would have talked them out of it. We would have made plenty of noise. We would have been really vicious. (1955: 21)

What Doc's account of the bowling demonstrates is that although bowling was the key arbiter of social status, Whyte's bowling didn't count. He was effectively allowed to win (at least in not being put off each time he attempted to bowl), because his winning would not threaten anyone's group status. In this sense Whyte had access to the organization, but only partial membership. This partial membership, however, and close relationship with Doc enabled him to produce a detailed account of the organization of street corner life.

### **Organizing and representing data**

Whyte's study of street corner life, although depending a great deal for its narrative content on a key informant, introduces the reader to numerous characters, activities, locations and forms of social ordering. To make sense of this array of detail, Whyte deploys a diagram (1955: 13). This diagram represents the entangled relations of the group and the hierarchical social status relations developed through fighting, threats of fighting, bowling, associations with girls, and so on. This kind of diagram, as with any form of simplified representation in ethnography, is difficult to accomplish. Ethnography bases many of its claims to robustness and relevance on being close to the action and providing significant detail on that action; a visual representation of names in boxes with lines drawn between them risks reducing ethnographic closeness and detail to a (relatively) simple picture. Whyte overcomes this difficulty by not employing the representation as an end point. In no sense is the diagram the thing to take away from the study. Instead, the diagram provides the reader with a way of navigating through the ensuing text so that names, activities and relationships can be understood as related to positions.

### **Organizations**

Whyte's study of street corner society provides an opportunity to reflect on the orderly and disorderly within organizational forms. While the bowling provides a means to structure social order, it is predicated upon making sufficient noise and threatening violence to those deemed lower down the social order. When some members of the group begin seeing girls from a group called the Aphrodite Club, this again raises further forms of order and disorder. Doc proclaims himself to be the greatest lover (which the other members eventually accept), maintaining a sense of the stratified order of the groups' relations. However, some of the groups' members associate with girls who exclude or are rude towards other members of the group. This threat of re-stratification through non-members of the group leads to some disorder (and some members

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leaving the street corner life). These forms of order and disorder, stratification, re-stratification, membership and partial membership can provide a rich backdrop of ideas for organizational ethnographers seeking to understand the way corporate organizations work.

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What kinds of epistemological claims does such a narrative approach involve? The claims to knowledge that narrative ethnography can make are intricately involved with the relationships established between ethnographers, informants and gatekeepers. For Whyte, the relationship with Doc enables him to produce what he claims is a detailed and accurate account of street corner life. Whyte is quite happy for this account to be assessed epistemologically for its accuracy in realist terms (see Whyte's follow up to *Street Corner Society*, 1993). However, critics of Whyte have suggested that further exploration is required of the principle relationship between Whyte and Doc (see, for example, Boelen, 1992). These critics suggest that Whyte may have failed to adequately explore the ethics of his relations with Doc, particularly in terms of whether or not Doc gained as much from the (1955) study as Whyte. In place of realist assertions of accuracy, come ethical assertions of the correct way in which to carry out research (see sensibility four for more on field relations and sensibility nine for more on ethics). Further criticisms of Whyte's (1955) work have suggested that Whyte's realism is predicated upon an outdated mode of realist epistemology which ignores more recent ethnographic moves to clearly address forms of subjectivity (see 'Reflexive ethnography' section below, and for detailed criticisms see Jermier, 1991; Denzin, 1992).

### Reflexive ethnography

This section will introduce the principles of reflexive ethnography. Different versions of reflexivity can be understood as more or less radical. I will begin with what I perceive to be the less radical orientations of reflexivity before giving consideration to more radical, reflexive textual experimentation. I will argue that these forms of experimentation can be useful in helping us to think through some of the principles which might underpin our ethnographic research.

In a general sense, reflexivity suggests that members of the world are (reflexively) engaged in making sense of and producing (a version of) the world. The world is not independent of reflexive efforts to make it make sense. Further, ethnographies fit into this sense-making practice and ethnographers are reflexively engaged in the production of sense through their ethnographies. In its less radical orientation, this leads to reflexive questions of the author: how is the author involved in the production of the ethnographic text? These questions are given slightly different emphasis in confessional and auto-ethnographic modes.

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### Confessional ethnography

The principles of confessional ethnography (Van Maanen, 1988) are a detailed study of a particular ethnographic setting incorporating much reflection on the role the ethnographer has played in the setting. The way the ethnographer has entered the setting, become an adequate member of the setting and formed particular relations with members of the field become features of ethnographic analysis. This is not particularly different from other forms of ethnographic study. However, under the confessional mode, as the term suggests, greater space is devoted to potentially problematic issues involving the researcher. The epistemological approach focuses on making available information on the ways in which the ethnographer has been closely involved in the setting. The strength of this kind of knowledge claim lies in allowing readers to assess the reliability and rigour of the ethnography presented and the problems the ethnographer may have experienced.

### Auto-ethnography

Auto-ethnography and confessional ethnography fulfil much of the same function. However, the emphasis in auto-ethnography can be a little different. In place of an ethnographic study which incorporates an analysis (or confession) of the moves made by the ethnographer, auto-ethnography is based around the story told by or through the ethnographer. In a similar manner to an auto-biography, an auto-ethnography (more explicitly than a confessional ethnography) is focused on the life experiences of the ethnographer in doing ethnography. The emphasis in auto-ethnography is often on the 'cultural study of one's own people' (Rosen, 1991: 4), rather than on the exotic cultures of foreign tribes. The reflexive engagement in auto-ethnography can be as much about the ways through which the ethnographer makes sense of themselves and their role in the world as it is about focusing on a particular group, location or organizational form. Richardson (2000: 923) suggests that writing in this mode of research acts as a way of 'finding out about yourself and your topic'. The epistemological grounds for making knowledge claims in this kind of ethnography depend on emphasizing the value of subjectivity. The ethnographer situates themselves as having a standpoint from which to express valuable insights into their own experiences. This can lead to jokes, such as that retold by Marcus (1994: 393), in which the native says to the ethnographer 'that's enough about you, let's talk about me'. Readers can find more detail on auto-ethnography from the work of Ellis and Bochner (1996, 2000; Ellis, 2004) and Reed-Dehaney (1997).

In the next section I will argue that confessional and auto-ethnographic modes of study are not as radical as other orientations of reflexivity. I suggest this is primarily because they do not ask as many questions of ontology. Although both modes of ethnographic engagement suggest that the ethnographer is reflexively engaged in the production of the ethnography, this is not always taken further into questions of the status of the setting in which

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the ethnographer is engaged. For example, while confessional ethnography might devote an amount of space to the author's reflexive position, participants in the study might be treated in conventional realist terms. The author's role might end up constituting more of the ethnography than the interaction between members of the ethnographic setting or might be given more consideration than the interaction between the ethnographic text and reader (see next section). In this sense, reflexive epistemology can be combined with realist ontology or questions of ontology can remain absent.

### More radical orientations of reflexivity

For more radically reflexive ethnographers, such auto- and confessional approaches are more about reflection (on, for example, the role of the researcher) than about reflexivity. To some extent the questions posed in confessional and auto-ethnographic modes are not very different from standard research questions regarding researcher influence. To restate, reflexivity suggests members of the world are reflexively engaged in the production and maintenance of the world as a more or less ordered phenomenon. Ethnography is itself involved in this ongoing reflexive production. Latour and Woolgar (1979) offer a more radical orientation of these questions of reflexivity in relation to ethnography. In studying scientists, Latour and Woolgar (exemplar four) reflexively engage with what it means for scientists (and the objects of science) to be reflexively engaged in the ongoing production of a more or less ordered science. The ethnography therefore involves investigation of what they as ethnographers are doing in attempting to ethnographically reproduce science. A central point of contention (like confessional and auto-ethnography) remains the role of the researcher in this ongoing reflexive production. However, scientists are being studied reflexively reproducing science and readers are invoked as engaging in a reflexive production process in reading the text. Reflexivity (in this sense) is not merely focused on reflection on what the ethnographers do, but involves an analysis of a series of relationships, some of which are in principle unknowable (for example, what the reader will make of the text). This can provide for an unsettling experience. Rosen (1991) suggests that being analytical about the reflexive constitution of exotic cultures (that is, the way natives go about making sense of going about) is more comfortable than being reflexive about our own activities (that is, questioning the taken-for-granted basis for the way we go about making sense of going about).

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### Exemplar Three

B. Latour and S. Woolgar (1979, 2nd edn, 1986) *Laboratory Life: The Construction of Scientific Facts* (Princeton University Press, Princeton, NJ)

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Latour and Woolgar's (1979) work is noted as being among the first in-depth ethnographic studies of the work scientists do to produce science. This ethnographic work shifted emphasis away from philosophers' concerns with the nature of scientific knowledge. Philosophers had focused on, for example, scientific knowledge as distinct from social scientific knowledge, or on the means by which scientists' advance knowledge (through, for example, Popperian falsification or Kuhnian paradigms). The ethnographic approach to the work of science and scientists also moved away from contemporary emphasis of social science approaches to natural science. In place of a focus on scientific error or mistakes, Latour and Woolgar pursued a symmetrical approach to the study of scientific knowledge. Normal, ordinary science was as much a focus for study as fraudulent, mistaken or incorrect science. This study was central to the development of the newly emerging field of science and technology studies, providing one means to raise questions regarding prevalent views on the nature of scientific knowledge and how scientists went about producing that knowledge. However, what is the significance of such a study for those doing organizational ethnography? I will argue in this brief summary that reflexive ethnography, ideas of social construction, social construction entangled with ideas of materialization and circumstances, and a focus on producing order from disorder can all be thought-provoking challenges for organizational ethnographers.

### ***Reflexive ethnography***

Unlike realist ethnography (where the world is assumed to exist as a knowable entity, from which an ethnography can abstract observational material, which can then be judged according to how accurately it represents the world out there), reflexive ethnography engages in a thorough and detailed analysis of the ethnographer's attempts to make sense of the world while those being studied are making sense of the world.

By reflexivity we mean to refer to the realisation that observers of scientific activity are engaged in methods which are essentially similar to those of the practitioners which they study. (1986: 30)

The ethnographer is reflexively engaged in making sense of the world and pays attention to their own methods of making sense, while also comparing ethnographic means of making sense with the sense-making methods of those studied. In this sense, it is more difficult to say that the world straightforwardly exists independently of the ethnographer and that observations can be collected from the world and judged according to how well they represent the world. In place of such an approach, the reflexive ethnographer enters into an analysis of the means of producing the ethnographic study. Thus instead of an assumption that the world exists outside the ethnography, the world is made apparent and rendered available through the ethnography. However, rendering the world available through ethnography is not straightforward – the reader of ethnography enters into a crucial relationship with the text, making sense of the world represented through the ethnography.

This introduction to reflexive ethnography suggests there are three principle elements to take into account (although these are not easily separable and are by no means a step-by-step guide to reflexivity). First, the world does not straightforwardly exist independent of efforts to make sense of the world. Second, ethnographers are as caught up in this sense-making as are those being studied through the ethnography. Third,

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ethnographies thereby make available a description of participants' ways of making sense of the world, ethnographers' ways of making sense of the members' methods for making sense of the world, and make these available for readers to make their sense of the ethnography. For organizational ethnographers this is something of a challenge. How and in what ways does an organization (and an organization's membership) make sense of itself? How does the ethnographer make sense of the organization's attempts to make sense of the organization? Should the ethnographer be concerned about the ways in which readers might make sense of the text?

For Latour and Woolgar, such questions are treated as principles of research to be engaged with the process of researching and writing. They are not deemed relativist problems which result in ethnography (or any other social science pursuit) being unable to say anything about anything. Treating such issues as principles rather than problems entails incorporating them into the research project. This incorporation is on anthropological terms. A scientific laboratory is approached in much the same way as a tribe, with an in-depth, detailed, observational account of the setting made available through the ethnography. Making observations in particular settings is pursued in order to avoid making general statements about the nature of science or relying solely on scientists' own accounts of science. Latour and Woolgar employ the anthropological principle of strangeness in engaging with the field (in order to render everything available for analysis, emphasizing that much scientific work depends on a great deal of mundane, routine activity, not moments of dramatic revelation). In place of concern with what readers might make of the text, they suggest 'It is the reader who writes the text' (1986: 273).

For Latour and Woolgar, reflexivity involves applying ethnographic scepticism or strangeness to the ethnographic text itself, making available an analysis of the ways in which the ethnography has been put together. Or, as Latour and Woolgar have it in their slogan: '...reflexivity is the ethnographer of the text' (1986: 284).

### **'Social' construction**

This reflexive approach to ethnography introduces several further challenges for those undertaking organizational ethnography. The first of these is the notion of 'social' construction. This term was originally used by Latour and Woolgar to convey the idea that 'facts' were not straightforwardly available in the natural world to be collected and reported on by scientists. Instead, facts were constructed through multiple activities. They were 'social' in the sense that they were constructed through multiple, ongoing processes. However, the 'social' aspect of social construction was deemed problematic by Latour and Woolgar. Social was used by other researchers of science as a binary opposition to natural or technical. Such an approach does not fit comfortably with the symmetrical approach to science, treating everything with equal scepticism/interest. Introducing researchers' own caveats and binary oppositions effectively dismantles any possibility of symmetry as some things are now to be treated as belonging to the category of social, and some technical, according to decisions made by the ethnographers. Science, it is argued by Latour and Woolgar, is not made up of social features on the one hand and technical or natural features on the other. Instead, science is made and maintained through a series of actions (see next section). Latour and Woolgar summarize this as follows:

'So what does it mean to talk about "social" construction? There is no shame in admitting that the term no longer has any meaning' (1986: 281)

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What is the relevance for this loss of meaning regarding the 'social' for organizational ethnographers? Often organizational research can get caught up in elaborating the importance of social or cultural variables or factors. Much time can be given over to defining the social or cultural, to distinguishing it from other factors, to measuring, observing, understanding or analysing the social or cultural factors which might play a role in shaping an organization. What Latour and Woolgar argue is that the social (and indeed the cultural) should not have a category of its own. Instead, the organization (of the laboratory in their case) is the product of a range of activities. Through focusing on these activities, ethnographers can provide a nuanced and reflexive account of the organization and the myriad means which make and maintain the organization. Definitions, sub-categories, factors and variables relating to the social and cultural are somewhat of a distraction in this approach. So what does make and maintain the laboratory as an organization?

### ***Construction through persuasion, materialization and circumstances***

Although Latour and Woolgar are keen to shift attention away from social and cultural variables, by what means do they argue the laboratory is constructed? They suggest 'it is through practical operations that a statement can be transformed into an object' (1986: 236). These practical operations, through which a fact is constructed, involve convincing others that they have not been persuaded, that materialization provides evidence of the fact and that circumstances have never played any role in the apprehension of the fact.

The result of the construction of a fact is that it appears unconstructed by anyone; the result of rhetorical persuasion ... is that participants are convinced that they have not been convinced; the result of materialisation is that people can swear that material considerations are only minor components of the 'thought process'; the result of investments of credibility is that participants can claim that economics and beliefs are in no way related to the solidity of science; as to the circumstances, they simply vanish from accounts, being better left to political analysis than to the appreciation of the hard and solid world of facts! (1986: 240).

### ***Order from disorder***

These multiple processes of construction translate disorder into order. Although reflexive ethnography has been criticized for taking a stance which, it is claimed, is overly relativist (suggesting that things can be understood in multiple ways and therefore that anything goes in a postmodernist sense), Latour and Woolgar emphasize that the 'transformation of a set of equally probable statements into a set of unequally probable statements amounts to the creation of order' (1986: 244). Through processes of construction facts are produced and refined, other possibilities are eliminated, outcomes are reduced down to (often) a single point, and science, the laboratory and a particular fact are achieved. The value of this approach for organizational ethnographers lies in its avowed and determined scepticism. Nothing is left as assumed or taken for granted. Everything (including the ethnography itself) is made available as a point for further consideration. If applied to organizational settings, such an approach to construction would introduce the following questions: By what means is the organization produced and maintained? Through what processes are organizational facts

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constructed and talked about? What analytical utility do ideas of persuasion, materialization and circumstance have for organizational analyses? (These questions will be dealt with in more detail under sensibilities four and eight).

Questions of ontology and epistemology become more complex in this approach to reflexivity. For Latour and Woolgar, the appropriate question to ask is not does the world exist independently of the ethnography? Instead, given that the subjects of research and researchers are reflexively engaged in the ongoing production of a more or less ordered version of what's going on, the question becomes how can this be captured within a written text? Furthermore, what is the reflexive relation that readers will then enter into in reading the text? In this approach to reflexivity, reading is an active process of making sense of the text. Ontologically speaking, the world (in text) does not exist independently of readers' work to make it make sense. Epistemologically, the forms of knowledge claims made in such radically reflexive ethnographies involve forms of constructivism (I resist here using the term 'social constructivism' as Latour and Woolgar argue that it is difficult to draw a boundary around a specifically 'social' set of factors to set them apart from some other set of factors to be accorded a different category). Epistemologically, constructivists argue that the world does not exist independently of efforts which make and maintain (construct) the world and ethnographies are one such way of making sense of the world.

Other radically reflexive texts propose experimentation with the form of ethnographic writing itself. The PhD thesis of Ashmore (1989), for example, is a thesis focused on doing a thesis. In place of a single 'author' there are multiple voices incorporated into the text and the reader is invited to take an active part in making sense of Ashmore's thesis construction. I think the value of such textual experimentation is to shake up the ways we take for granted the formal expectations, structures and conventions for writing, for authorship and what normally constitutes an ethnography. What Ashmore's reflexive thesis accomplishes is a destabilizing of where and who the ethnographic author is and what the relationship should be between text, author and reader.

Although I cannot lay claim to such radical underpinnings for my own work, I find particular value in these reflexive orientations in my organizational research. First, these more radically reflexive texts do not leave anything settled. They demonstrate how far ethnographers can go in questioning what gets taken for granted. This is a primary principle in my own ethnographic work. Second questioning the position of authorship opens up further possibilities. In place of a conventional ethnographic relationship, where the ethnographer does the research and writes the research, we can think of alternative sets of relations which might help produce a text. In exemplar fourteen I set

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out the ways in which my own work has involved handing over some of the authorship of the ethnography to members of the organization. Third, reflexive texts open up questions of readership. In place of assuming that readers will make the same sense of the text that ethnographers have when writing the text, reading becomes an active process of sense-making. It is possible to ask these questions and loosen the imagination of readers and writers in a variety of different ways (see Watson, 1994). In organizational settings I have found it useful to give research participants my ethnography to read as a way of generating further ethnographic conversations. Thus, although I have presented these reflexive experimentations as radical, their detasbilizing of conventional ontological and epistemological premises can have utility for organizational ethnography.

### Organizational ethnography and knowledge claims

The preceding discussion has argued that questions of knowledge are an important feature of ethnographers' attempts to engage in research. It has suggested that particular ontological and epistemological approaches come with particular commitments which might shape the direction the ethnographer is to take in doing their research. In this sense, giving consideration to questions of knowledge can help ethnographers to further develop their ethnographic strategy (see sensibility one).

An implicit feature of this discussion has been that claims to knowledge are likely to be assessed quite differently by different audiences. For example, what might be considered academically rigorous, might not be considered to have utility for a particular organization taking part in an ethnographic study. Claims regarding the validity of ethnography depend on who is assessing the ethnography.

### Rigour and academic assessment

Each of the approaches to ethnography presented, in carrying particular epistemological commitments, also have implications for the ways in which the research should be regarded as academically rigorous (or not). Thus realist ethnographies can be assessed on the grounds of whether or not they accurately portray the reality of the setting studied, and reflexive ethnographies can be assessed for the lengths to which the ethnographer has gone in being reflexive (is there a constructed, reflexive stone that they have left unturned?). It follows that each of these approaches to ethnography can be interrogated from alternative epistemological standpoints. Rigour is perhaps best seen as an accomplishment. A recent development which alleviates some of these issues is modest ethnography (see exemplar ten). 'Rigour' in modest ethnography involves considering the partiality of research – ethnographers are partial (rather than impartial) and ethnographies are partial

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(rather than complete). Such modesty involves inviting readers to assess the strengths of the arguments presented and inviting ethnographers to pursue questions left unanswered by the study (as recommended by Malinowski in exemplar two).

### Utility and organizational assessment

What counts as useful information in an organizational context can be complex (see, for example, the Conclusion of this book). However, epistemological claims that knowledge is useful, valuable and has integrity (through appropriate ethnographic methodology) can be as much about process as about the content of the claims. Ethnographers can utilize the time frame for ethnographies (see sensibility five) to make an early presentation of ethnographic data in the field setting. Such presentations can be occasions for organizational members to be invited to make something of the data. Responses from organizational members can lead to the development of new ethnographic avenues for the research. Discussions can then form a part of the ethnographic process, with follow-up presentations forming further occasions for the development of the knowledge claims being made (these issues will be taken further in sensibility four). The epistemological responsibility for knowledge claims is then shifted in these discussions from being the sole preserve of the ethnographer to being shared among contributing participants to the study. Within organizational settings I have found this kind of approach useful for enhancing the status of the ethnography, the number of participants willing to take part and the extent to which ethnographic findings are considered valuable. Such ethnographic manoeuvres should also keep in focus that observations which counter what members of organizations want to say can have utility (see sensibility four).

### Combining utility and rigour

This separation of the grounds on which the knowledge claims of an ethnography can be assessed appears to rule out the possibility of combining academic rigour with organizational utility. However, this need not be the case. In subsequent discussions (see particularly sensibilities seven and eight), we will go through the grounds on which rigour and utility can be combined. We will also go through managing the different demands which organizational utility and academic rigour can make (in terms of access in sensibility three and in terms of time in sensibility five).

### Recommended reading

- Ashmore, M. (1989) *The Reflexive Thesis* (University of Chicago Press, Chicago)  
Blaikie, N. (1993) *Approaches to Social Enquiry* (Polity Press, Cambridge)  
Dellbridge, R. (1998) *Life on the Line in Contemporary Manufacturing* (Oxford University Press, Oxford)