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Social Science Research

From Field to Desk

⑤SAGE

5
Observation on the Move: Shadowing

The complexities of contemporary societies increase the difficulty of such traditional techniques of observation as participant observation and stationary direct observation. This chapter focuses on the technique of shadowing, as one way of avoiding those difficulties.

VARIETIES OF OBSERVATION

> Most of the knowledge which people have about social relations is derived from uncontrolled observation, whether participant or nonparticipant. (Goode and Hatt, 1952: 120)

There are indeed variations in observation techniques, although they tend to blur in the field. The distinctions drawn here are merely to assist a methodological reflec–tion by distilling traits that do not exist separately in research practice. The choice is always that of the researcher, and it is often an ethical as much as a methodological choice.

> The anthropologist actually moves from one role to the other while in the field. He may, for example, go on a fishing trip as a participant, but during the preparations for an important religious ceremony he will interview formally the important participant, or record the ritual chants during the ceremony. This shift is made easier by the fact that the patterns of the society are not likely to be changed in important ways by the presence of an outsider, if the role of the latter is properly defined. (Goode and Hatt, 1952: 122)

More than a half century after Goode and Hatt wrote these words, social scientists are all ethnologists or anthropologists of contemporary societies – men and women alike. Yet we have inherited many of the field techniques of our predecessors.

Indirect observation (one-way mirror, hidden camera) is used in social work, psychology, and criminology, but is considered unethical whenever it is happening without legal justification and/or without the knowledge and approval of people observed.

Then there is *direct observation* (including open videotaping), which can be divided into *participant* and *nonparticipant observation*. As discussed in Chapter 1, there has been a great deal of debate over what is and what is not participant observation, and my definition is an answer to my pragmatic needs rather than an attempt to bring a final word to the matter. I believe that it makes sense to call it 'participant observation' when observers are doing the same things as the people (or some of the people) they are observing. Self-observation (Brinkmann, 2012) can be also counted as a kind of participant observation.[1]

Nonparticipant observation can be further divided into *shadowing* and *stationary observation*. Photography and video recording, used early on by anthropologists (Collier and Collier, 1986), can aid both of them, and are increasingly present in all social studies.

In what follows, I focus on those types of observation techniques I consider helpful in studying the ways of work and life of mobile people living in contemporary societies. I begin with *shadowing* – following selected people for a time in their everyday occupations. This approach allows the researcher to move with them.

A BRIEF HISTORY OF SHADOWING

I first encountered the technique of shadowing in the work of Italian sociologist Marianella Sclavi (1989), who, during a prolonged visit to the USA, followed a neighbor's daughter to school every day.

Sclavi saw Truman Capote as her role model. In his short story, 'A day's work' (1975), Capote told readers how, for the whole working day, he followed a cleaning woman who was everything he was not: woman, Hispanic, large, working class, heterosexual. In Capote's story, Sclavi saw an excellent example of an idea suggested by Russian philosopher and literary theorist Mikhail Bakhtin. Bakhtin (1981) argued that good novels – and he saw them as deeply sociological – require an author to assume an attitude of *outsidedness* that would provide different grounds for communication than

does much-romanticized empathy. It aims at understanding not by identification (they are like us), but by the *recognition of differences* (we are different from them and they are different from us; by exploring these differences we will understand ourselves better).

As Bakhtin said in an interview, shortly before his death in 1975:

> In order to understand, it is immensely important for the person who under-stands to be *located outside* the object of his or her creative understanding – in time, in space, in culture. For one cannot ever really see one's own exterior and comprehend it as a whole, and no mirrors or photographs can help; our real exterior can be seen and understood only by other people, because they are located outside us in space, and because they are *others.* (Kelly, 1993: 61)

I found this stance attractive because it does not claim to represent the natives from an insider's perspective – a claim that has been rightly criticized for its colonial sedi-ments (see e.g. Prasad and Prasad, 2002). An observer can never know *better* than an actor; a stranger cannot say *more* about any culture than a native can, but observers and strangers can see *different* things than actors and natives can.[2] Bakhtin did not espouse the behaviorist idea of a complete separation between actors and observers, however; to the contrary, they may, and ought to, engage in a *dialogical relationship*.

The attitude of outsidedness replaces sentimental idealization with mutual respect between strangers – a symmetry. Rather than taming 'them' to become like 'us', we expect differences. These differences, in turn, are seen as a source of knowledge, not least about ourselves. This requirement of outsidedness is difficult to achieve, how-ever, not only in premodern societies, but also in such fields as organization studies, in which researchers often behave condescendingly toward practitioners by giving advice, establishing the 'best practice', or 'emancipating the oppressed'. Thus, shad-owing is a technique – and an attitude.

As often happens with innovations, shadowing seems to have been invented in several places in parallel. Or perhaps, as Robert Merton (1965/1985) noted, all ideas are around all the time, in some form or another. In 2000, another Italian sociolo-gist, Giampietro Gobo, asked well-known qualitative methodologist Jay Gubrium if he knew the origins of the shadowing technique (Gobo, 2005). Gubrium sent the query to all the contributors to the handbook he edited with James Holstein – including me. I did not answer, certain that my sources – Sclavi and Capote – would emerge without my intervention. They did not. Gobo did not know of Sclavi's studies, just as I had not known about Giuseppe Bonazzi's (1998) study until I read an article by Seonaidh McDonald (2005). Bonazzi had quoted as *his* models the studies of Mintzberg (1973) and Charles N. Walker, Robert H. Guest, and Arthur N. Turner's (1956) studies of foremen. He distanced himself from his predecessors, however, calling theirs 'quantitative studies, which made no concession to interac-tion between the researcher and the subjects observed' (Bonazzi, 1998: 223). On the other hand, he said, he constantly shuttled 'between hard data gathering and inter-action with the subjects' (p. 223).

   This incomplete genealogy of shadowing is an illustration of the plurality of the social sciences, in which the Web is of great help in establishing connections, but makes us too certain of the completeness of our sources – a methodological point in its own right. Gobo's appeal located one main suspect, however: Oregon education scholar Harry F. Wolcott. In a letter to Jay Gubrium, Wolcott explained that during his study reported in *Man in the Principal's Office: An Ethnography* (1973/2003), he acquired a nickname 'the shadow', based on the old radio show[3] and as it well explained his role in the field, he used it himself in the book (Czarniawska, 2007: 24). Another discipline within the social sciences that uses shadowing – often without using the term – is consumer studies (see e.g. Miller, 1998). Shadowing is also used as an educational technique, particularly in teaching and nursing (Roan and Rooney, 2006; Lindberg and Czarniawska, 2006).[4]

   In this chapter I present several shadowing studies to illustrate their advantages and some of the difficulties that may arise. I begin with a management study, followed by a school study, and finally a consumer study. The order in which they are presented is roughly chronological, to show that the perception of advantages and disadvantages changes over time, following the contemporary preoccupations of the social science community.

THE USES OF SHADOWING

'Structured observation' and the worries of its time

Henry Mintzberg, one of the most famous management researchers, began his introduction of what he called 'a structured observation' with a criticism of the diary studies of management, prevalent at that time (1970):

> Not one of these studies provides substantial insight into the actual content of managerial activities. ...The reader is told where managers spend their time, with whom they spend their time, how they interact (telephone, face-to-face, etc.) and so on. But the reader is never told *what* is transacted. (Mintzberg, 1970: 88; italics in the original)

The diary method assumed that the researcher already knew *what* managers were doing, and needed only to learn *how much* of *which*. 'But of which what?' asked Mintzberg. Surely nobody could use Fayol's categories (planning, organizing, coordinating, and controlling) to describe actual behavior? Some categories were necessary, Mintzberg reasoned; otherwise the researcher would be lost in the minutiae of everyday work. He suggested what he saw as a compromise solution:

> I use the label 'structured observation' to refer to a methodology which couples the flexibility of open-ended observation with the discipline of seeking certain types of structured data. The researcher observes the manager as he [Mintzberg

shadowed five men] performs his work. Each observed event (a verbal contact or a piece of incoming or outgoing mail) is categorized by the researcher in a number of ways (e.g., duration, participants, purpose) as in the diary method but with one vital difference. *The categories are developed as the observation takes place.* (Mintzberg, 1970: 90; italics in the original)

Readers in the second decade of the 21st century may wonder about this obsession with structure and categories, but at the time Mintzberg wrote, even direct observation was supposed to be strictly structured (social psychologist Robert Bales, 1950, had created a widely used form for recording observed interaction). Indeed, some pages after this quote, Mintzberg apologized to the reader who 'may feel that some of the categories are not sufficiently "neat" ' (p. 94). Again, in the 1970s, formal logic was considered an essential part of research training, and the categories were expected, at the very least, not to be overlapping, a requirement often neglected in contemporary classifications. Between listing the categories, Mintzberg gave examples of his field notes, showing that he had, in fact, shadowed 'Mr. M'. He sat in M's office and walked with M to the plant; they returned to the office, and then went to a meeting with consultants.

Mintzberg then produced examples of his coding procedures, which could have been included as illustrations to a textbook on grounded theory, although Glaser and Strauss (1967) are never quoted. The reason is, most likely, that grounded theory summarizes the commonsense of fieldwork, and at that time such commonsense did not require referencing.

Commenting on his method, Mintzberg referred to Rosemary Stewart's (1967) critique of observation techniques. She identified three problems: a lack of understanding about what was taking place, an exclusion from all confidential activities, and the size of the sample. Sampling was an issue for Mintzberg, whose ambition it was to turn his field material into quantifiable data. Because it is not an issue in the present context, however, I focus on the first two problems.

Mintzberg reformulated the critique concerning the possible lack of understanding into a problem of (proper) classification. As the article was written before 'the linguistic turn'[5] reached management studies, the idea of 'right' (valid) categories was still strong. At present, categories are evaluated in terms of their 'sensitizing'[6] power, whereas their validity (in the sense of being supported by the material, not in the sense of corresponding to reality) can be easily checked by NUD★IST-type software. The issue of understanding remains, but it has been resolved by the experience of science and technology scholars, who claim that a dedicated fieldworker can, after a time, understand even the work of quantum physicists (although they would *not* be able to perform it).

An analogy with traditional anthropology is often evoked: Anthropologists, too, needed time to understand the language and customs of the tribes they studied. The main difference was that they remained in the same village, whereas the village is now global. Acquiring an understanding has become a signal of satiation for a field

researcher. The day when everything said at an observed meeting is fully understandable is the day to return to one's office – not only for reasons of efficient resource management, but because complete understanding means 'going native', at which point attention drops and outsidedness is at peril. When one understands everything, there is nothing left to explain.

The problem of exclusion from confidential activities was resolved by Mintzberg in conversations with shadowed managers, who reported to him what had taken place. Had they reported it all, and were their reports truthful? Yet if the confidential meetings were crucial, their outcome would soon become visible in future events. If not, it mattered little; nobody, not even the practitioners, could be everywhere, and many things were happening simultaneously. When shadowing a manager at a municipal company in Stockholm, I was excluded from appraisal interviews, which I thought was only fair. I do not allow shadowing of my advisory sessions, because I believe it is too trying for the doctoral student. Such encounters are deeply personal.

Mintzberg addressed other problematic aspects of his method, such as the possibility of a Hawthorne effect (in the sense of the possible impact of the researcher's presence and attention). He could see some of the effects of his presence, but he saw them as unimportant from the perspective of his research interests. The people being shadowed obviously explain to the researcher what they and others are doing, which increases the proportion of reflection in their daily work; but reflection can be triggered by other events and is usually considered beneficial. Other people contacting the shadowed person may watch their tongues, but they may also do the opposite, hoping that a witness can add weight to their utterances.

In general, however, one should remember Becker's (1970: 43) comment quoted in the previous chapter: People being shadowed need to transact their business and cannot suspend their activities for the sake of a performance that is specifically addressed to a researcher. Impression management requires effort and concentration, dedicated to keeping job performance undisrupted (Goffman, 1959) – and job performance is, after all, the topic of the study. What is more, people who meet the shadowed person and the researcher cannot risk a special performance without danger of exposure by the coworkers. In other words, it is unlikely that the shadowed people and the encountered others collude in staging and maintaining a special performance merely for the sake of the researcher. It has been my experience that after the initial curiosity has died off (usually a matter of a few minutes) people began to ignore me, as they usually have more important agendas to attend to.

Mintzberg's results were reported in *The Nature of Managerial Work* (1973), which has become a classic. The contents of the book are well advertised by its title, and I would claim that it was precisely the shadowing that made this 'nature' visible.[7] His study was repeated 30 years later by Stefan Tengblad, who shadowed Swedish managers (Tengblad, 2003, 2006).

The shadow in the principal's office

Harry F. Wolcott was an anthropologist who studied the Kwakiutl for his dissertation, then turned his attention to the field of education. Like Henry Mintzberg, he noticed that diary-type studies suffered from many shortcomings, and that they would not allow him to answer his central question: 'What do school principals actually do?' He did not seem to be aware of Mintzberg's study, not only because the two studies were done practically in parallel, but probably also because management was not yet perceived as a general profession in the 1970s, with precepts considered applicable everywhere. Wolcott decided to put his anthropological skills to work, but he realized from the beginning that his study, with its focus on one school principal, would differ markedly from studies of tribes or kinship (Wolcott, 1973/2003).

Wolcott specified a whole set of criteria before choosing a person to follow, but in the end he was eager to admit that 'good fortune' also played a part in his choice. He was looking for a person who was:

- a full-time, career principal (i.e. without teaching duties and not seeing his job as a step to another job);

- responsible for only one elementary school;

- experienced in his job;

- male (most principals were men, although most teachers were women); and

- likely to survive a two-year close contact with the researcher without personal hostilities.

This last criterion is especially significant. Although, to my knowledge, no other researcher who used shadowing has mentioned it, its lack can produce many unexpected results – as Chapter 8 will show. Wolcott admitted that he decided against following a certain principal because that man wore white socks with a dark business suit and talked patronizingly to pupils.

Wolcott first secured the cooperation of the principal, and then applied for formal acceptance higher up the hierarchy. 'By using this approach I felt I could avoid the possibility of having an overzealous superintendent summarily assign some fair-haired principal to be my cooperating subject or an underzealous one reject the project because he doubted that any of "the boys" would be interested' (1973/2003: 3). It needs to be added that cooperation from higher levels was necessary for shadowing, not least because Wolcott had the opportunity to meet the superintendent while achieving it.

The superintendent looked quizzically at me as he stopped to chat with the members of the committee just before it convened. 'Say, you're not writing all this down, are you?' he asked. 'I write everything down,' I replied. I added that if

he was interested in the study I would welcome the chance to talk to him about it in detail. He was and I did. (1973/2003: 3)

The writing-down part of this exchange is familiar to me from my own shadowing experiences, but not a supervisor's interest in discussing the project – probably because my shadowing periods have been so much shorter: Two weeks in a row, after which I vanish, truly like a shadow. Neither is there any possibility of my becoming friends with the people I shadow – a realistic prospect in a two-year study – a situation that, as Wolcott pointed out, could be psychologically comforting, but may jeopardize contacts with coworkers, who may be unduly careful in their statements about the person shadowed.

Wolcott received the principal's permission to conduct virtually every activity that can be said to constitute shadowing: Recording in writing what was said and done, attending formal and informal meetings and conferences, interviewing him and other people who were encountered during the shadowing, and accessing various notes and documents. He also continued shadowing in the principal's private life.

Commenting on the effects of his stay in the field, Wolcott said:

It is tempting to report that after a brief 'period of adjustment' the researcher blended perfectly into the school setting and everyone at school continued about his business totally oblivious to him. Although my presence at the school was not intended to require major adaptations by those being observed, it seems unrealistic to insist that things were just the same with or without me there. (1973/2003: 11)

In making this comment, Wolcott echoed the sarcastic observation made by British anthropologist Nigel Barley: 'Much nonsense has been written, by people who should know better, about the anthropologist "being accepted" ' (Barley, 1983: 56). When the object of study is not an exotic tribe but a modern school or a corporation, however, an illusion of acceptance is more likely to arise ('You, coming from a business school, will surely agree ...', or 'You, who taught in school yourself ...', as Wolcott did). These tender illusions do not remove the basic sense of estrangement that becomes obvious under prolonged contact.

So, what difference did Wolcott's presence make? Wolcott did not believe that it made a great deal of difference to the principal's habitual ways of acting and speaking, but Wolcott's questioning (necessary in order to understand the principal's actions) was most likely achieving a change of frame: From the principal's taken-for-grantedness to an inspection, or even to a self-critique of his actions. His 'natural attitude' (Schütz, 1953/1973) probably gave way to a questioning attitude, or even to change. For instance, Wolcott administered a simple questionnaire concerning contacts among staff members. Its results made the principal realize that the staff did not have many occasions to meet socially. This and other comments seem to suggest that the impact of the shadowing was largely positive. As in Mintzberg's study, the researcher's presence was bound to facilitate reflection, but reflection is rarely detrimental, or so we are taught to believe.

Wolcott has also attracted attention to the drudgery of fieldwork: Long days, boredom, and doubts about whether or not the project made sense. The last aspect

must have been the result of his long stay in the field. As he said, 'a lessening in note writing usually signaled the approaching finale to a productive observation period' (1973/2003: 16) and the advantages of a long period of shadowing the same person must be weighed against this disadvantage. As he noted, however, the principal he shadowed would fall asleep during some meetings, a liberty that his shadow could not afford to take.

Accompanying shoppers

Daniel Miller (1998) did not call his way of doing fieldwork shadowing, but I think that the following quote fully justifies my decision to include his work here:

> For a one-year period, 1994–1995, I attempted to conduct an ethnography of shopping on and around a street in North London. This was carried out in association with Alison Clarke [then Miller's doctoral student]. I say 'attempted' because, given the absence of community and the intensely private nature of London households, this could not be an ethnography in the conventional sense. Nevertheless through conversation, being present in the home and accompanying householders during their shopping, I tried to reach an understanding of the nature of shopping through greater or lesser exposure to seventy-six households. (1998: 9)

Miller, who, like Wolcott, is a trained anthropologist, sounds apologetic; but in fact he splendidly outlines the specificity of shadowing. His was not an ethnography for several reasons. His aim was not to describe 'the ways of life', but 'the nature of shopping', a phenomenon situated in time and place. And he did not study a tribe. He actually spoke of 'the absence of community', wishing to emphasize that some people are, or at least feel, outside of any community, isolated within a busy urban context. Shadowing, he claimed, is more suitable for describing the lives of such people than is standard ethnography (Miller, 2007).

The fact that Miller was interested in shopping meant that he undertook fascinating variations on straightforward shadowing. He followed a married couple, Sheila and Bob, while they shopped together, for example. In his reading, they both held conservative notions of gender differences, which provided grounds for a constant comic banter between the spouses as they shopped.

> A key element within this comic banter is her constant criticism of his lack of shopping skills. ... Taken in context, however, these criticisms are a mechanism she uses to affirm that as a man, although he may shop, he is not a natural shopper. He is thereby able to receive such 'criticisms' as praise for his natural manliness, something which he recognizes. (1998: 25)

A potential criticism of Miller's study is the type of critique from which Mintzberg defended himself: That the banter was produced for the benefit of the shadow. All the

better, I say, and I would imagine that Marianella Sclavi would agree. A performance confirming the importance of the traditional gender division of labor can be seen as a message to the researcher, much stronger and more convincing than could any answer to an interview question. Again, impression management is a methodological problem only under the assumption that deeds and utterances of people under study should correspond one-on-one to some reality hidden behind appearances, to be revealed in the course of research. If this assumption is replaced by the Goffmanian premise that life is a theater, however, then that which is played is of central importance. Impression management, yes; but as I emphasized in the previous chapter, *what impression* are the performers trying to produce?

 The complications of impression management became even more obvious when Miller shadowed a couple-to-be: a young divorced woman and her boyfriend.

> At this stage the crucial factor in shopping was my [Miller's] presence. This was an occasion to learn about each other's taste and forge a relationship in terms of shopping compatibility. But there was also a question as to how they appeared as a couple to an outsider. The sheer effort that I felt they were putting into showing me how happy they were together should not be seen as thereby false. It reflected their own question as to whether, when revealed in the reflected gaze of the anthropologist, they would find themselves to be in love. (1998: 29)

This was an intriguing situation, because this young couple, unlike Sheila and Bob, had not rehearsed their common performance many times. Theirs was a double trial – to perform together an act of acting together. One could venture a guess that the anthropologist's presence was beneficial to the couple, setting this double test for them. The anthropologist eventually managed to see more in their performance than they themselves knew. Although this was, by their own declaration, a couple that aimed at equality, the woman was trying to learn as much as possible about her boyfriend's habits and desires, while he was establishing his right to have the last word on everything. She could accept that, as long as he did not force her to acknowledge the fact. They did become engaged, however.

 On the basis of his study, Miller constructed a theory of shopping, in which he claims that commodities are used primarily in shaping social relationships. The shoppers constantly buy things for others, or for themselves with others in mind. Buying goods for others expresses the hope 'to influence these others into becoming the kind of people who would be the appropriate recipients for that which is being bought' (1998: 8). Routine provisioning, on the other hand, can be seen as a devotional rite usually performed by women – a rite confirming a gender role.

 One fascinating aspect of Miller's theorizing is his full awareness that the shoppers he shadowed did not share his theory. Most of them espoused a theory of shopping according to which shopping was an expression of deplorable hedonism and materialism; they also excluded provisioning from the definition of shopping. Miller noted the paradox implicit in the ethical requirement of fieldwork: respect

for the 'informants'' opinions. It is assumed that, short of accusing them of suffering from false consciousness, respectful fieldworkers must faithfully render the views of the natives. But there is another way: Fieldworkers can try to advance their own views, neither surrendering them to the views received nor asserting their supremacy, but simply adding to the views from the field. This is, in fact, the core of the dialogical relationship recommended by Bakhtin (1981), for whom it was obvious that the views of the observer and the views of the actors may clash. A dialogue need not be a duet.

The reader may think that it was still relatively easy for Miller to maintain his outsidedness, considering that shopping turned out to be strongly gendered. In a later study, Alison Clarke and Daniel Miller (2002) followed women when they shopped for clothes, and Clarke characterized their study as one that included participant observation. Miller sees it differently. He believes that fieldworkers must often:

> … transform themselves into something quite distinct from people's initial assumptions, often occupying many different persona in order to work with many different kinds of people. I assume it is my job to try and become the kind of person that the other individual prefers to spend time with, if I want them to spend a considerable time with me, so I will shift from being young, old, male, female, comic, serious, etc all the time. Similarly when working with a colleague, Alison or another, we try to exaggerate differences to give people an opportunity to respond to the kind of personality they prefer out of this choice based on our distinction. I don't see this as manipulative, I see it as part of our responsibility to make the experience comfortable for the people who are giving us this time and information. (Miller, 2007)

This stance corresponds to one suggested by Rosalie Wax:

> Perhaps good fieldwork is more like play-acting than most of us are willing to admit. Respondents rarely resent a fieldworker's 'acting like them' or 'learning their ways' as long as the fieldworker makes it clear that he knows he is only playing a part and that his newly acquired skills do not entitle him to any privileges which they are not willing to offer him. (1971/1985: 197)

Although Wax and Miller agree on the main point, the small differences between their utterances illustrate well the difference between studying a strange culture and studying one's own. Miller's domestic skills did not have to be acquired for the purpose of the study (he declared that he had a life-long hobby of cake decoration), but they do not *entitle* him to the privilege of sharing people's time and attention – he has to earn it. Prasad and Prasad have quoted the same passage from Wax, but they concluded that, for Wax, 'the most effective form of going native takes place when it is performed as a *masquerade*, played out within clearly delineated rules and limits' (2002: 194). To my reading, Wax and Miller are saying that in fieldwork as in everyday

life, exotic cultures or not, strangers would do well to play likeable personae if they want people's time and attention. Sociologists from Goffman to Garfinkel have made it known that rules and limits are never clearly delineated, but known through transgressions and continuously renegotiated. This time it is Prasad and Prasad who are guilty of romanticizing; there is a visible trace of the myth of 'an authentic presence' behind their critique.

THE SHADOWY SIDE OF SHADOWING

Shadowing is easier than participant observation because shadowing does not require simultaneous action and observation and because participation in complex, professional activities would be impossible for most researchers. In terms of methodological gains, it permits one to preserve an attitude of outsidedness, whereas participant observation creates many opportunities for 'going native'. Yet shadowing does not prohibit the feeling or expressing of emotions, making them, as Sclavi (2007) rightly said, the main instrument of cognition. The point is never to behave like a fly on the wall (what a peculiar metaphor, considering what happens to the flies on the wall, once they have been noticed), but to behave like a responsible adult, showing respect and sympathy for others.

The main advantage of shadowing over stationary observation is, by definition, its mobility. The matter is, however, more complicated than is the sheer act of movement. After all, not even observers whom I call 'stationary' remain immobile in the same place during their study. Shadowing creates a peculiar duo: the person shadowed and the person doing the shadowing. This is where the dynamics of cognition become complex indeed, as I have tried to illustrate in this chapter. There is mutual observation, an establishing of similarities and differences; then there is a focus produced by the movements of the person shadowed, creating the double perception, as it were. The researcher guesses (and asks about) perceptions of the events being perceived as well: a camera with a mirror lens, to use a technical analogy.

I return to the issues of psychic discomfort and its role as a source of insight in Chapter 8. Here it is enough to say that perhaps shadowing meddles with the taken-for-granted, making threats to the personal and professional identity of the researcher unavoidable. But the psychological discomfort seems to be a necessary price of learning, as ethnomethodologists noticed long ago. The bonus lies in the extra self-knowledge that researchers can gain. The main compensation is a problematized picture of social reality that carries the possibility of liberation for those observed – if they happen to suffer from the reality they were led to construct – and the promise of a nontrivial story for the researcher.

Then there is the issue of blending in, or 'passing' (Hammersley and Atkinson, 1983/2007), not least in terms of clothing. Although the relationship between the person shadowed and the person shadowing can be resolved in several ways, blending in is necessary in order not to attract attention to the activity of shadowing. In

organization studies, it appears that male researchers have fewer problems blending in than female researchers do, as the dress code for men is much more limited than it is for women (McDowell, 1997, called men's clothing 'unmarked', in contrast to women's 'marked' outfits). In Warsaw, I did not know how to blend in, other than not dressing in any way that could attract attention. In Stockholm there was a clear dress code for professional women in public administration: jeans, shirt, and a jacket (the shirt has now been replaced by a low-cut top, with an obligatory necklace). This camouflage worked well, apart from the fact that when my identity was revealed, I was told that I 'did not look like a professor'. Alas, I was not able to establish how a professor should look, apart from a serious suspicion that I should have been a man. I chose to interpret it as confirmation of the right choice of camouflage, as the norm seems to be that it is doctoral students who do fieldwork. Of course, these are minor worries compared to those of the fieldworkers who studied outlaw bikers, the police, or the homeless (Hammersley and Atkinson, 1983/2007).

Yet another practical difficulty is the need for note taking, while being constantly on the move. All solutions are welcome: Taking notes whenever seated; dictating reflections whenever alone; and finally, writing up as much as possible at the end of each day (the most difficult of all, as shadowing is extremely tiring). Perhaps Reporter-type digital tools (Chapter 7) will become more accessible and easy to use.

The last point I would like to raise here is the possible effects of shadowing for and on the person shadowed. Truman Capote's shadowing ended with the cleaning woman losing her job – but, I hasten to add, this was because Capote changed into a participant observer, and they both smoked hash at her workplace. At least one of my shadowings has boosted the morale of the person shadowed, who fell victim to a hasty restructuring. The principal shadowed by Harry Wolcott assured him that it contributed to his professional growth, and although Wolcott read it as due to the principal's tendency to create something positive from every situation, he accepted the statement with grati-tude. More striking was a comment from the superintendent, who told Wolcott, 'We're thinking of having you fellows start paying for information. You never help us with our problems anyway – you just study what interests you' (1973/2003: 15). Wolcott said that that was a comment the anthropologists of the day were often prone to hear – upon returning to their homelands from exotic sites, I assume – but I have never met with that reaction. One reason is that within my discipline it is only recently that researchers stopped playing 'company doctors', and began studying that which interests us, rather than what the company wants us to study. Another reason is that I and the people I study have been deeply indoctrinated into the belief that research ultimately helps practice, no matter what twisted routes it may take to get there. Van Maanen (1991: 34) was probably right when he said that fieldworkers must recognize that they cannot offer much of obvious value to those who are studied.

But shadowing isn't restricted to following humans. The idea has recently been extended to include nonhumans – more exactly, objects and quasi–objects. Following objects rather than people is an innovation introduced in studies of science and tech-nology, an innovation that is the topic of the next chapter.

Shadowing

Shadow a person you know who agrees to be shadowed; someone as different as possible from you in as many ways as possible. Shadow that person for some hours, reflect over everything that struck you as strange, and consider what it tells you about yourself.[8]

FURTHER READING

Brinkmann, Svend (2012) *Qualitative inquiry in everyday life.* London: Sage.

Collier, John Jr. and Collier, Malcolm (1967/1986) *Visual anthropology: Photography as a research method*. Albuquerque, NM: University of New Mexico Press.

Czarniawska, Barbara (2007) *Shadowing, and other techniques for doing fieldwork in modern societies*. Malmö/Copenhagen: Liber/CBS Press.

McDonald, Seonaidh and Simpson, Barbara (eds.) (2014) Special issue on shadowing research in organizations. *Qualitative Research in Organizations and Management.*

Wolcott, Harry F. (2008) *Ethnography: A way of seeing*. Lanham, MD: AltaMira Press.

Notes

1. I avoid the term 'autoethnography' because, as literally understood, it would assume a multiple personality disorder (*ethnos* means 'people'). Also, it is not clear if by 'autoethnography' the researchers signal the use of self-observation, a study of his or her own group, or both.

2. Bakhtin's stance, similar to Bruyn's (1966) 'detached involvement', finds corroboration in Niklas Luhmann's theory (Luhmann, 1998; Seidl and Becker, 2005).

3. The radio program *The Shadow* (there were many versions in other media) started on 31 July 1930. Each episode began with the narrator saying, 'Who knows what evil lurks in the hearts of men? The Shadow knows!' and ended with 'Crime does not pay … The Shadow knows!' (en.wikipedia.org/wiki/The_Shadow, accessed 21 April 2013).

4. For a thorough and detailed review of the uses of shadowing, see McDonald (2005). She has used the technique in the study of team leaders in a hi-tech organization.

5. It is generally agreed that the expression, 'the linguistic turn', originated with *The Linguistic Turn: Essays in Philosophical Method*, a 1967 anthology edited by Richard Rorty. It was at least another decade before this perspective trickled sideways from philosophy to the social sciences.

6. The notion of 'sensitizing concepts' was launched in 1954 by Chicago sociologist Herbert Blumer, in his critique of contemporary social theory. Again, it took several decades to make the idea popular.

7. Joyce Fletcher (1999) used Mintzberg's study as a model for her shadowing of female design engineers.

8. This exercise was inspired by one that Edgar Schein (1999) recommended for his students.