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Organizing: how to study it and how to write about it

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Abstract

Purpose – The purpose of this paper is to review emerging approaches to field studies of organizing that aim to avoid the problems of traditional methods and techniques.

Design/methodology/approach – The paper is based in pragmatist philosophy and constructionist perspective. Within this frame, the paper starts with a brief history of fieldwork methods in organization study, continues by diagnosing their shortcomings in the light of contemporary developments, and ends by reviewing promising approaches to studying contemporary organizations.

Findings – Young researchers are warned about possible risks and gains from experimenting with new methods.

Originality/value – The potential value of the paper is in its function of a guide for organization scholars looking for innovative approaches to their study object.

Keywords Organizational analysis, Organizations, Organizational processes, Research

Paper type Case study

Traditional ethnographies do not offer the best approach to studying contemporary organizations, where organizing happens simultaneously in many places. This paper reviews some alternatives, while acknowledging the debt of organization studies to the fields of anthropology and ethnology. The text ends with a discussion of various ways of reporting field studies.

A fruitful encounter with anthropology

When Jaques (1951) of the Tavistock Institute published his dissertation entitled *The Changing Culture of the Factory*, it was seen as a daring metaphor. True, several anthropologists started doing organization studies: Wolcott (2003) was shadowing a school principal, and Rohlen (1974) studied socialization in a Japanese company. But, it was the 1980s before anthropology made a serious entry into organization studies.

The first opening occurred under the flag of consulting, which probably explains a great deal of its success: if something can be put to practical use, it must be good. Thus, it was Deal and Kennedy (1982) who signaled the appearance of a new paradigm with their *Corporate Cultures: The Rites and Rituals of Corporate Life. Administrative Science Quarterly* dedicated a whole issue to organizational culture in 1983 (Volume 28, No. 3), in which Smircich (1983) was already pointing out that, although the sources of the new interpretative paradigm clearly lay in anthropology, the ways of applying it to organization studies were many and sometimes conflicting.

The rest is history, documented many a time, including my own version (Czarniawska, 1992). In what follows, I would like to focus on phenomena related to this triumphal entry of anthropology into organization studies that have received little acknowledgment up to the present.

Let there be no doubt about one thing: to me, the emergence of a cultural paradigm in organization studies was the best thing that happened in my nearly 40-year research career. Anthropology brought with it a dramatic change of perspective and a whole new set of metaphors, as well as a legitimacy to field methods that had barely been acknowledged in organization studies. There can be no talk of Trojan horses or of bewaring the Greeks even when they carry gifts. This was an enormous and much-appreciated gift (I could call it a loan but I am not sure we will be able to repay it). Nevertheless, each excursion into another field of knowledge brings with it things that were embedded in a different type of soil, and it is inevitable that particles of that soil are still clinging to the roots, practically invisible to the new owner. Additionally, a re-embedding into the new soil could produce deviations and hybrids that are sometimes superior to the original, and sometimes not. In what follows, I present two such unexpected consequences of the growing use of anthropological concepts and methods: the static view of organization and the colonial heritage of the identity paradigm. Having discussed these two, I move to suggesting ways of counteracting these undesirable loans when doing fieldwork and when writing up its results.

The dregs
Organization as a village, or, what is the "site"?
Latour and Callon (1981) pointed out that sociologists, even the most critical ones, become unwitting collaborators in power games by solidifying and stabilizing various macro actors, helping them to hide the fact that they consist of a network of heterogeneous actants. Latour and Callon spoke of "society," but the organization scholars were as busy doing the same to "organization." All of a sudden, the term that used to denote a state of affairs (organization in contrast to chaos), started to denote an entity, very much a super-person, a smaller leviathan.

One possible explanation is that it had to do with a visitor from another planet: systems theory (Czarniawska, 2006). As we know, discourses tend to create their objects (Foucault, 1972); a changing discourse accordingly changed its object. The application of systems theory, highly visible in the early 1960s (Waldo, 1961) required a creation of separate units divided by "boundaries" from their "environments" and related to them by "adaptation." The participle "organized" has been replaced by the adjective "organizational." This conceptual move must have been attractive, in that it provided a middle ground between mechanistic Taylorism and idealist administration theory, and also related organization theory to what was then the most fashionable branch of science – cybernetics.

Not everybody welcomed this redefinition. Weick (1979) pleaded convincingly for a return to studies of organizing, to the study of what people do when they act collectively in order to achieve something.

One could imagine that anthropology would be an ideal partner in such an enterprise. After all, when the anthropologists studied the "organization" of peoples and tribes, they looked precisely at the ways of organizing, stabilized by repetition and other cultural forms. But, this is not how it worked, and one can trace at least two connections that show how the steps of the borrowing process went. Both have much more to do with the method than with the metaphors. The metaphor of a "tribe" was close to many definitions that see "organization" as a grouping of people, whether

related by a common goal, an employment contract, or some other social adhesive. Debating the usefulness of such a definition for the enterprise of studying organizing could be useful, and such debate can be fruitful. The problem lies, however, in that which is unsaid because it is taken for granted. Although many nomadic tribes exist, the classics of anthropology were studies made in villages. After all, a "site" is a place, a location, a position. To study an organization, a researcher needed to go there, and stay put for as long as possible (see Bate, 1997, complaining about "anthropological tourism" in organizational research).

The only problem is that while the researcher is there, the managers being studied may not be. As I have pointed out in other contexts (Czarniawska, 2004, 2007), modern management occurs in a net of fragmented, multiple contexts, through multitudes of kaleidoscopic movements. Organizing happens in many places at once, and organizers move around quickly and frequently. As Strannegård noted in the title of his study of an IT company, the people he studied were constantly "already elsewhere" (Strannegård and Friberg, 2001[1]). Additionally, not all interactions require a physical presence. Knorr Cetina and Bruegger (2002) spoke about embodied presence and response presence, the latter not necessarily visible to an observer, as when people "talk" to somebody via e-mail, or during teleconferences.

Also, the group of "temps" is growing; Barley and Kunda (2004) spoke of the "unraveling of permanent employment." As to "loyalty to organization," the finance workers studied by Renemark (2007) showed none: they moved from company to company without hesitation after having collected their bonuses, and often brought others along with them.

Brose (2004) suggested that contemporary western societies are characterized by three connected phenomena. The first is acceleration, the speeding up of social processes, a shorter product life cycle, and more highly paced innovation. The second phenomenon, related to the first and commented frequently upon by Bauman (1995) is the shortened time horizon of expectations and orientations, resulting in a shorter duration of social structures and personal commitments. These two are causes and effects of the third phenomenon, an increasing simultaneity of events in what Schütz and Luckmann (1983) called "the world at reach." And as our world at reach has widened, there is a problem in trying to record and interpret it. Zapping is one solution, a bird's-eye view is another; but they hardly solve the difficulty of contemporary fieldwork: how to study the same object in different places at the same time?

Brose (2004) concluded that the increasing simultaneity causes an increase of non-simultaneity, of the number of people who, while living at the same time, do not live in the same time. Therefore, Brose postulated that there is a need to study the emergence of new practices and structures that are supposed to facilitate living and working with different temporalities in a way that no longer relies on hierarchical or sequential ordering of activities. The question is: what would be the right approach to studying the emergence of such practices and structures?

Anthropology is trying to meet this challenge in various ways, but the obstacles of the methodological tradition are strong. As pointed out by Firth (1951), the approach used in traditional studies of pre-modern cultures was static. It was assumed that these cultures reproduced themselves with minor deviations, and that they could be transformed only by structural, exogenous forces (Czarniawska, 2001). In this sense,

organization studies were better equipped to capture change. In contrast, anthropologists were not looking for change; their task was to record what was repetitive and usual (Redfield, 1953). This tendency was reinforced by the favored usage of the "ethnographic present" in writing, a style that produced what Firth (1951) called "a literary embalming."

As can be seen from my references, the anthropologists themselves noticed the static character of their studies early on, and tried energetically to change it. Excursions into other disciplines, however, usually end up if not in a museum at least in the gallery of canonic works. As Prasad and Prasad (2002) observed, even Bate's (1997) critique was infused with a strong nostalgia for the "heroic" ethnographies of the older days. This tendency is mutual: anthropologists like Hutchins (1995) who visit the world of organization theory, end up with classics such as Cyert and March (1963).

It is therefore in the interest of both disciplines to complement traditional ethnographies with studies of people on the move, simultaneous multi-site organizing, and virtual management. In the later part of this paper, I review some of the possibilities. Before that, however, I need to inspect another accidental loan from anthropology – its colonial purposes.

The fixation on identity

The issues of identity and "identity work" dominate the present discourse of organization theory and consulting (Deetz, 1994; Whetten and Godfrey, 1998; Hatch and Schultz, 2003) as well as anthropology (see, e.g. Stewart and Strathern, 2000; also, "identity" produced 137,000 hits on Google). The practice of organizing, however, reveals the constant co-presence of alterity in organizational image construction (Czarniawska, 2002). The interplay of identity and alterity is continuous; new organizations become objects of desire and old objects of desire serve as negative examples. Also, "the Other" is not passive, but is doing symmetrical work at all times.

An example: as I was writing this text, Saab (now a part of General Motors) was reducing its operations in both Germany and Sweden. An image was being constructed with the help of identity/alterity relations for two plants, two unions, and the corporation. The workers' solidarity was the crucial point in this interplay: should the Swedish workplaces be saved at the expense of the German workplaces? Who is "we" and who is "the Other"? How do "we" differ from "them"?

How, then, can the fixation on identity – not only in organization theory but in most social sciences – be explained? I see it, following Brooks (2005), as sediment from the times of colonialism and urbanization. This claim can be seen as somewhat strange in the face of the fact that postcolonialism is clearly fashionable in organization theory (Prasad, 2003), and the authors interested in identity recognize the connection. The dominant claim is that the postcolonial identities are changing and undergoing transformations; the alterity of the other must be recognized. What most of the authors engaged in the identity project failed to notice is that these issues are already framed by colonialism and remain so (I was one of those authors).

Brooks (2005) called this excessive focus on identity an emergence of an identity paradigm and tied it back to the nineteenth century, when two phenomena were catching the attention of the young nation states. One was urbanization, the enormous movement from the countryside to the city. The bourgeois became frightened; criminality was on the rise, and it was taking new, sophisticated forms. As Brooks

pointed out, the picturesque figure of a "master criminal" in a variety of disguises was not only a figment of the vivid imaginations of novelists, but existed in reality, to the exasperation of police forces. Another nineteenth century problem had to do with the exigencies of running the colonies. How to tell the natives from one another if they all look alike to the eye of the colonialists? How to tell working class people from one another, if they not only wear the same clothes, but also imitate the bourgeois (or the other way around)? The problem, therefore, was too many differences and too few differences. A search for various technologies of identification was activated during this period: physiognomy, phrenology, and then photography and fingerprinting, were put at the service of the police and the colonial authorities.

Whereas, the issues of identity were first raised in relation to persons, they were transferred, by analogy, to the realm of abstract entities, such as legal persons (corporations; Lamoreaux, 2003) and nation states. Thus, the emergence of the identity paradigm in the nineteenth century was also connected to the rise of nationalism (Anderson, 1991). People grouped within the new borders desperately needed to know what they had in common, as the tradition was for them to look for differences.

This "identity free of alterity" has caused many political problems (Buruma, 2002) but also a significant semantic slide of the term. From denoting a relationship (identity, like an alterity, is a judgment resulting from a comparison), identity has become a property – something to have or to lack. The constructivist vs essentialist stances have nothing to do with it; both perspectives are possible when studying the identity/alterity interplay.

Cultural anthropologists, and organization theorists after them, use the term "alterity", but always in relation to other people – not to themselves. And yet, "identity" and "alterity" are twin terms, opposites. "Identity" denotes a relationship of similarity: in the case of a person, this relationship is with oneself at another time, with something else (for example, an identity card), or with some other people. Correspondingly, alterity denotes a relationship of difference: from oneself at another time, from something else, or from somebody else. One term requires the other to be understood; in everyday discourse, however, the term "alterity" has been forgotten.

I am not claiming that I am the first to notice this complex identity/alterity interplay; in fact, Latour (1993), under the influence of Gabriel Tarde and Gilles Deleuze, spoke of acts of differentiation and identification. I am merely suggesting that all this complexity was forced into one term that, inevitably, went into a glide. This gliding can be stopped by adopting a relational view of identity/alterity interplay in organizational image construction. So now, let me turn to possible ways of doing fieldwork and writing it up that will counteract the image of village-like organizations and identity-obsessed organizers.

Shadowing, and other techniques for doing fieldwork on the move

This section presents in brief my recent text on methodology, in which I collect various suggestions on ways of studying organizing that takes place simultaneously in different places, where times goes quickly, and people and objects travel constantly (Czarniawska, 2007). The book does not purport to have individuated the right approach to field studies in modern societies, but it has an ambition to consolidate and extend the search for such approaches. In the end, the attractiveness of all techniques

needs to be measured against the degree to which they permit the researcher to tackle the peculiarities of modern practices, such as organizing.

Before I move there, I quickly review the techniques used in the three works I mentioned as highlighting the mobile and temporary character of modern organizing: Strannegård and Friberg (2001), Barley and Kunda (2004) and Renemark (2007). They all used anthropological techniques in all their richness, which helped them to notice their limits.

Lars Strannegård, who was trained by Gideon Kunda, gained access to an IT company in Stockholm between October 1999 and April 2001. This period included the time when all IT companies were trying to deal with the "millennium bug," as well as stock market swings, e.g. this company's astronomic increase in value in 1999 to an equally astronomic fall in 2000. His ethnography is impressive, and it led to two crucial observations in tune with my previous argument. First, while Strannegård was "then and there," the people he observed, and especially the managers, were "already elsewhere": traveling, meeting clients, but also in the future, they were in another company, in another world. Second, when Strannegård describes their attempts to create an identity, it soon becomes clear that they were building an alterity, concentrating on acts of differentiation. They did not speak about what they were, or who they were like, but about what they were not, and how they were different, and from whom (almost everybody in "the old economy").

Barley and Kunda (2004, p. 26) deployed the whole arsenal of anthropological methods. Indeed, they were fully aware of the difficulties of studying the freelancers and contractors in "the new economy", where everybody seemed to be "already elsewhere":

> Doing fieldwork in an organization, a subculture, or a tribe requires entering a single, relatively well-defined social setting, which, as most ethnographers discover, is difficult enough. Getting inside the market for technical contractors meant understanding the story from three perspectives: the contractor's, the client's, and the staffing agency's. It also meant studying a sprawling, emergent, loosely coupled, and at least partly virtual social system.

Their solution was to do a multiethnography[2]: six months at a staffing agency, and then three months each at two other agencies with different business models. Next, they interviewed 71 contractors. It is important to stress at this point that they did not assume that the interview material provided them with a picture of "how it really worked." Interview material is always material for studying the dominant discourse (and deviations from it). Barley and Kunda (2001, p. 28) were well aware of this: "… we are reasonably confident that our data identify key themes, issues, and dilemmas that are of widespread concern to technical contractors." Finally, they interviewed employees of client firms: a thorough round of the interviews at three big firms in Silicon Valley complemented by interviews with managers of seven other firms. The result?

> Our goal is to provide a window into the day-to-day realities of contracting as a basis for developing a wider interpretation of contracting's relevance in a knowledge economy.
>
> History, however, does not wait for ethnographers to complete their work. About a year after we left the field, as we were pondering the significance of our data, the economic conditions of high-tech employment changed dramatically […] In short, our tale portrays contracting at a peak of demand. Whether it speaks only to a specific historical period, and

the extent to which it continues to be relevant, ultimately remains to be seen. This, of course, is the fate of all social science (2001, p. 30).

Indeed, it is, but it became more apparent with the advent of the phenomena described by Brose (2004), and has consequences for the issues of identity: while the contractors were, indeed, trying to construct a distinct identity at the peak of the demand, it crashed along with the stock market. Busy differentiating themselves, they may need to identify again with the previously rejected groups or professions.

I am far from suggesting that, had Barley and Kunda used other methods, they would have been able to catch the fleeting phenomena in a better way. To the contrary, their work shows that fieldwork knows no "method"; it relies on pragmatism, luck, and moral sensibility. The knowledge of a variety of techniques, and the will to innovate rather than follow static prescriptions of method books, remain central to the craft of fieldwork, as to all others. Barley and Kunda (2001) pointed out, for example, that traditional observation is usually inadequate to capture computer work, and they recommended a more sophisticated use of technical aids in such observations.

Renemark (2007) wanted to find out why, in spite of many media campaigns, there are so few women working in finance in Sweden – as traders, brokers and analysts, rather than as bank clerks. He focused his search on two obvious domains: recruitment methods and everyday work. The first he tackled through work-life interviews, being careful not to interpret them as the report of real life events, but as ways of narrating lives. The second he resolved by making workday observations at multiple sites. It needs to be added that the attempt to free the researcher from the trap of one site need not imply a neglect of the setting; to the contrary, the settings, technology-intensive as they tend to be nowadays, are of utmost importance. In this aspect, Renemark's study confirmed the results reported by Knorr Cetina and Bruegger (2002)[3]. Here again, he stated that "the organization" was of practically no importance for the finance workers; if anything, they lived in two "villages": their local community (the city) and their virtual community, which partly overlapped. Intent on differentiating themselves from the backstage workers employed by finance companies, they were unclear about differences and similarities between their own jobs and occupations. Once again, the alterity of themselves seemed to be more important than their identity.

In what follows, I report several techniques used by researchers trying to capture the specificity of present-day organizing, that is, its mobile, dispersed, heterogeneous and computer-mediated character.

Shadowing
Shadowing (following a person through his or her working day, and sometimes even afterwards) has been used in sociology (Sclavi, 1989, 2007), consumer studies (Miller, 1998), and organization studies, although not always under this designation (Mintzberg, 1970; Wolcott, 2003; Czarniawska, 1998).

The main advantage of shadowing over stationary observation is, by definition, its mobility. But, it is not only the pure presence of movement that matters: after all, not even stationary observers remain in the same place, immobile. Shadowing creates a peculiar twosome – the person shadowed and the person doing the shadowing – in which the dynamics of cognition become complex and therefore interesting. There is a mutual observation, an establishing of similarities and differences; then there is a focus

created by the movements of the person shadowed, and the double perception of a kind – the researcher guesses (and asks about) perceptions of the events being perceived as well.

The price of the shadowing is an increase in psychic discomfort, but that is also a source of insight. Shadowing necessarily produces a rupture in the taken-for-granted on both sides. On the part of the researcher, this renders threats to the personal and professional identity of the researcher unavoidable, but that is a necessary price of learning.

As to the possible effects of shadowing for and on the person shadowed, they can vary from one study to another. At least, one of my shadowings has boosted the morale of the person shadowed, who fell victim to a hasty restructuring. The school principal shadowed by Wolcott (2003) assured him that it contributed to his professional growth. Shadowing usually enhances the status of the person who is shadowed in the eyes of co-workers.

It must be added that shadowing presents not a few practical challenges. Access must, to a certain degree, be continuously re-negotiated – not only with new persons, but even with the same person who can say the next day that being shadowed is beginning to be tiresome. Such a possibility exists in other types of observations, of course, but it is less likely, as people who find the presence of a researcher discomforting can literally hide in ways that are not accessible to the person who is being shadowed.

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).

How far shadowing can go is always negotiable and negotiated. The research or the company budget decides whether or not the researcher is allowed to participate in international meetings at distant places. The main point is not so much that the person who is shadowed moves, and the researcher with him or her; it is that their perception field moves as well, meeting constantly new landscapes, into which the researcher can peek. "I am a camera," wrote Isherwood (1998) in his *Goodbye to Berlin*; it can be paraphrased in the present context as "the Other's eye is a camera."

Diary studies, or how to be many places at the same time
Shadowing people does not resolve the issues of simultaneity and invisibility, so other techniques must be considered. In this section, I briefly comment on a field technique known as diary studies. The term diary, however, has at least two meanings: diary as a log of activities (planned or performed), and diary as a narrative reporting of past events. There can hardly be a sharp distinction between the two: some people write a lot in their Filofaxes, whereas others make only perfunctory notes of past events; the recent "blog" (web log) is an obvious hybrid.

The two variations have a long tradition in field studies, but have recently been updated, mainly in human-computer studies (for an overview of the use of diaries in social sciences, see Alaszewski, 2006).

It is likely that log studies originated in Fredrick Winslow Taylor's and Franck and Lillian Gilbreths' time and motion studies, developed in the 1910s. The Swede, Sune Carlson, inaugurated the use of diaries in managers' studies

(Carlson, 1951; Tengblad, 2003). The present tendency to use computer logs helped to eliminate or reduce what was seen as problems of the early diary studies and even other fieldwork techniques, such as obtrusiveness of observational techniques; additional workload of self-observational techniques such as diaries; and recall problems in interviews that aim at providing a list of past activities.

Real diaries (including letters) have been used in social sciences at least since Thomas and Znaniecki's (1927) work, *The Polish Peasant in Europe and America* (1918/1921). As not everybody can count on the existence of or access to unsolicited diaries and letters, researchers often solicit diary writing. Among the most fascinating cases are those of autoethnographies (when people describe their ways of living). One of the most striking examples is mass-observation, a UK social research organization founded in 1937 (Hubble, 2006; Stanley, 2007); its Archive is kept by the University of Sussex. Its aim was originally to record everyday life in Britain through a panel of 500 untrained volunteer observers who either maintained diaries or replied to open-ended questionnaires. Financed by the UK Government, the organization also paid investigators to record people's conversation and behavior at work, in the street, and on various public occasions. The original effort ended in the 1950s, after various criticisms pointed out the "subjectivity" and "lack of validity" of such material. The methodological revolution that took place in the 1970s re-established "subjectivity" as the only possible source of knowing, and challenged the meaning of validity as one-to-one correspondence with "reality." This led to the relaunching of mass-observation in 1981, and the collection continues.

Organization scholars in Finland came up with the idea of creating a diary archive, to be used by a team of researchers. Lean Site Management was a research program undertaken at The Laboratory of Industrial Management, Faculty of Technology, at Åbo Akademi University in Finland between 1998 and 2004. Its aim was an extensive investigation of organizational processes and structures, including intercultural aspects of organizing on international industrial project sites (Gustafsson *et al.*, 2003). A group of researchers from Åbo Akademi and the Royal Technological University in Stockholm calling themselves Project-Based Industry collaborated with the Finnish producer and importer of diesel power plants, Wärtsilä.

The research group started sending to the Wärtsilä sites students in their final year of studies who needed material for their Master's theses or students who had just graduated and wanted more practical experience. Each of them had a concrete practical task to fulfill, depending on their specialties, and an obligation to conduct continuous observation. In this way, it has been possible to follow 20-some projects over a period of three years. The material collected by the student-trainees was accessible to everybody in the research group; furthermore, it has been discussed and improved in the course of its collection by the whole group. Later, the researchers used their own research interest as a selection tool, but they also had an opportunity to complete the material during their visits to the field. The result was an impressive archive of field material documenting projects happening at the same time in many distant places in the world.

Last but not least is the diary-interview technique (Zimmerman and Wieder, 1977), which I called, in an act of parallel invention, "an observant participation" (Beksiak *et al.*, 1978; Czarniawska, 1998). For Zimmerman and Wieder, the problem was not so much being in all places at the same time, but being in places where it was not their

business to be. They asked 60 members of a counter-culture community in Southern California to keep diaries for seven days in a row. The analysis of the diaries was the basis for subsequent biographical interviews.

Weil (2006) has suggested that the use of diaries for social research is actually only beginning; she may be right, as digital technologies can be of enormous help in the use of such techniques.

Following objects
This technique is usually connected with the approach known as actor-network theory, increasingly popular in organization studies (Latour, 2005). It fulfills the demand for symmetric studies – studies that grant (or give back) artifacts an important role in the development of human affairs. From the perspective of the fieldwork, the technique removes some of the psychological discomforts of both shadowing and direct observation, but it multiplies the negotiations of access.

The study most often evoked in this context is Latour's (1999) description of the chain of transformations that changed the samples of soil taken in the Amazon forest into a scientific paper. Latour was there when the samples were taken, and followed them up to the point when they became a published article.

Adolfsson (2005) repeated Latour's procedure in the city of Stockholm, studying the process of air and water quality control for the city. She followed the samples of water and air, collected from the roofs and lakes of Stockholm, through their transformations, ending up in pollution reports, urban policies, and even objets d'art.

Frandsen (n.d.) followed an accounting report backwards: to the places and coordination systems that led to its production. This journey allowed her to reveal how accounting, this apparently neutral technical aid, frames ways of seeing and acting in the world.

Bruni (2005) studied a quasi-object: an electronic patient record system in an Italian hospital. Having to deal with a quasi-object, he did a quasi-following: he was present when and where the EPR system made its appearance. This technique may be one of the answers to Barley and Kunda's (2001) appeal for looking for new ways of studying computer work. As most people have now become "information workers," the ways of observing this work must be improved upon. Although "life in cyberspace" and "virtual reality" attract a great deal of attention (on virtual ethnographies, see, e.g. Hine, 2000), more attention should be devoted to the connections between the activities inside and outside cyberspace (Kociatkiewicz, 2004).

It is noteworthy that this relatively new technique calls for novel ways of presenting results; indeed, the studies of Latour, Adolfsson and Frandsen are all photo-reportages. Scientific reports are in great need of a renewal of forms and contents; the last section of this chapter is dedicated to these issues.

How to dramatize without sentimentalizing
The topic of writing up ethnographies, especially in the form of scientific articles, was always a problematic one. Ethnography's "natural" form is a monograph; structuring it along the lines of an experimental study makes it look defective, if not outright ridiculous. Rosen (2002) successfully attempted to present several of his studies in journal articles, but even he could best present his findings and his devices (including literary devices) in book form. Although anthropologists experimented with a variety

of forms (Geertz, 1988), organization scholars mostly limited themselves to imitating either the laboratory genre or the classic ethnography genre. More experimenting could do us good – with the provision that many experimental forms must, by definition, fail. Formulated in fashionable market terms, we need to look for forms of writing to be able to compete with other genres that focus organizing – economic sociology and consultants' texts being the most obvious examples.

Literary theory, which I evoke often in this section, has an old recipe for achieving the goal of being read: to dramatize. This, however, can be accomplished in several different ways, and now I will discuss three of those.

Heroic stories

One type of text that is almost a genre of its own is biographies and autobiographies of great leaders, managers, and entrepreneurs. The recipe here is simple: the protagonist of the biography is presented as a unique individual. Variations occur: the leaders are often portrayed along the Moses' storyline, with unnatural qualities (see, e.g. Wildavsky, 1984; for a critique, see Czarniawska and Rhodes, 2006); the entrepreneurs as self-made people, usually with hard-working mothers; and the managers as a combination of the two. Typical episodes include epiphanies and challenges, the latter successfully met. The main problem of such a genre is, of course, that it ignores the collective character of all organizing. No wonder Mintzberg (1999, p. 12) became exceedingly irritated at having read yet another such heroic story:

> There is, believe it or not, some academic literature that suggests that leadership doesn't matter," we are told by the astonished *Fortune* writer. Well, this academic is no less astonished: there are, believe it or not, some business magazines so mesmerized with leadership that nothing else matters. "In four years Gestner has added more than \$40 billion to IBM's share value," this magazine wrote on April 14, 1997. Every penny of it! Nothing from the hundreds of thousands of other IBM employees. No role for the complex web of skills and relationships these people form. No contribution from luck. No help from growing economy. Just Gestner.

Mintzberg suggested that "managing quietly" might be an ideal worth pursuing. In terms of writing about management, however, would not that mean "managing boringly" as well? One of Renemark's (2007) findings was that most of the time the work of traders, brokers, and analysts resembles normal office work; in fact, it even more closely resembles university work. But, the public image portrays them as gamblers in a world full of challenges (this is one of the reasons that few women apply for these jobs), and it is an image they, themselves, cultivate with gusto. I doubt that any number of fascinating research reports could push the heroic genre out of circulation; there has always been a demand for heroic stories, from ancient Greece onward. The question is, then, how to present the heroism of everyday work, done collectively and routinely?

Fictionalization: Aramis

One solution tried with varying success is a fictionalization of the research report. Latour's (1996) *Aramis or the Love of Technology* may be seen as a successful example, although probably hard for beginners to follow.

The study is that of a failed technical project: the introduction of an automated train system that did not work (although simpler versions of it are often used as

airport shuttles). Latour presented is as a mystery: who killed Aramis? His conclusion was that nobody did: simply, nobody loved Aramis enough[4].

The mystery-cum-love story is accompanied by a Bildungsroman-type plot: a young apprentice (a doctoral student, we may presume) learns – theory and practice – from studying the case but also from his conversations with the master (his advisor). Here, is Latour (1996, pp. viii-ix) speaking about the genre he designed:

> What genre could I choose to bring about the fusion of two so clearly separated universes, that of culture and that of technology, as well as the fusion of three entirely distinct literary genres – the novel, the bureaucratic dossier, and sociological commentary? Science fiction is inadequate, since [it] usually draws upon technology for setting rather than plot. Even fiction is superfluous, for the engineers who dream up unheard-of systems always go further [...] than the best-woven plots. Realism would be misleading, for it would construct plausible settings for its narratives on the basis of specific states of science and technology, whereas what I want to show is how those states are generated [...] A journalistic approach might have sufficed, but journalism itself is split by the great divide [...] between popularizing technology and denouncing its politics. Adopting the discourse of the human sciences [...] was not an option, [...] for it would scarcely be fitting to call the hard science into question only in order to start taking the soft ones as dogma.

Before I continue the quote to arrive to its positive part – what Latour did rather than what he did not do – I would like to attract attention to the last sentence in the previous quote. Latour alludes to the writers who solidify the nature-culture divide by taking the culture side, in a mode originated by Wilhelm Dilthey (1833-1911). This device often leads to a sentimentalization of management and organizing, in which emotions and passions are as strictly divided from cognition and interests as in the "naturalist" perspective, only observed from the opposite side. It seems necessary to choose between the heart and the head or between the mind and the body, although life, if not social sciences, teaches us that they do not exist in separation. Therefore, Latour (1996, p. ix, italics in original) opted for a combination, but not at the expense of the requirements of the scientific genre:

> Was I obliged to leave reality behind in order to inject a bit of emotion and poetry into austere subjects? On the contrary, I wanted to come close enough to reality so that scientific worlds could become once again what they had been: possible worlds in conflict that move and shape one another. Did I have to take certain liberties with reality? None whatsoever. But I had to restore freedom to all the realities involved before any one of them could succeed in unifying the others. The hybrid genre I have devised for a hybrid task is what I call *scientifiction*.

How does one "restore freedom to realities"? This expression is another formulation of the symmetry principle that Latour promulgates. In his view, when studying conflicts and controversies one must withhold judgment until the end; otherwise the report becomes a pamphlet for a cause. The final judgments rarely concern the rights or wrongs of the parties involved; they usually indicate the type of social mechanism involved, and permit us to understand why the differing parties held to their opinions. Thus, the great issue of involvement versus non-involvement becomes resolved in a way that would have pleased Weber, who never pleaded with researchers to be indifferent to the issues they studied, but would rather not have his professor colleagues issuing political propaganda from their lecterns (it happened to be Nazi propaganda in his time).

Multimedia

There are indeed attempts to present ethnographies on film, but here I present a more traditional effort, which retains the form of a monograph, although differing from traditional monographs in many respects: Strannegård's study of an IT company that I mentioned previously. It has two authors, Strannegård and Friberg (2001), but their contributions differ widely.

Friberg is an artist and a photographer, and she contributed to the book her own fieldwork and her own theory, expressed in the medium that is hers. She did not "illustrate" Strannegård's text; simply, her fieldwork and her theory corroborated Strannegård's fieldwork and theory.

This is the first and perhaps the greatest deviation from a standard scientific text, but there are more. One of the problems of ethnographic reports is the inclusion of quotes and fieldnotes: they are either too long or as often happens, contradict the text of the analysis below or above the quotation, especially if they are quoted literally. Strannegård's quotes are not in the text of his analysis, but parallel to it, in the literal sense of illustrations. A typical chapter contains a text; endnotes (more about them to follow); a photo by Friberg; and a text that is stylized as handwritten, and contains quotes from the interviews or fieldnotes that corroborate the text of the chapter. Although they look like photocopies of the actual note, they are not; ethnographers with illegible penmanship need not worry. This produces a much stronger "reality effect" than the famous "ethnographic present" that may have worked for the first 50 years of its use, but currently tends to confuse the reader.

Another deviation can be discerned in the text itself. It is a hybrid again, in the sense of mixing the description of the field with its analysis and interpretation, thus making obvious the fact that there are no interpretation-free descriptions. Put differently, the theory serves as the plot and the frame for presentation of the field material, which is indeed woven into and around it. I would call this way of writing essayistic, but in the sense of a text with literary qualities rather than in the sense of a first draft written from a personal viewpoint. To a reader like myself, it is well argued and well written.

This last effect is aided by the way of referencing, which follows the Oxford system, that is, it delegates all references to endnotes. This makes an incredible difference in the ease of reading, uninterrupted by parentheses, names and numbers (each parenthesis interrupts the process of reading, as you can experience right now). It seems that the publishing industry has not yet fully understood the advantages of computerization. The Oxford system was replaced by the Harvard system because the setting of footnotes was an exceedingly complex process in the pre-computer era, but there is no longer any reason to adhere to the Harvard system. One possible reason for its lingering may be the idea occasionally expressed that footnotes and endnotes are digressions to be avoided in a proper scientific text. The opposite school of thought, probably composed of avid readers of *Tristram Shandy*, appreciates all kinds of digressions. One example is a work of the famous Swedish sociologist, Johan Asplund (who refuses to be translated into English), consisting of 60 pages of text and 60 pages of endnotes (Asplund, 1987).

Instead of conclusions: will it work?

A young reader may want to ask if the previous section can be treated as a recipe for success. The answer is no, and the reasons are again to be found in literary theory.

One of the myths of academic writing is that it should address a specific audience. Nothing could be more wrong. As Flaubert pointed out, all writers of consequence wish to create their public, and at the outset they want to be read by everybody. It is possible to address a specific audience if one exists, which means that writers must imitate – either other writers, or themselves – as is the case with successful writers who after becoming successful begin writing series. What if one is not (yet) a writer of consequence? It does not matter, as the wish is the same. As the word "consequence" suggests, the result will only be known later.

Another misleading assumption behind this myth is that the public is unintelligent and can be manipulated. It is true that rhetoric is an art of persuasion and that people now and then like to be lured, but this does not mean that they will remain so. The reader response is unpredictable; it may repeat itself for years, and then change into its opposite. It is better to focus on literary devices than on outguessing readers' responses.

At this point, it must be obvious that tastes differ, even in the case of scientific texts. I am deeply impressed by Strannegård and Friberg's text, and have enumerated my reasons for it, although this is no guarantee that other readers will like it as much as I do. The general advice, then, would be to write as you like to read, imitating authors that you like. Painters, too, begin their training by imitating their masters, then rebel against them in order to find their own style. As the connoisseur of ethnographic writing, van Maanen (2006) pointed out, traditional ethnography needs be blended, stretched and combined – all in the name of a necessary textual sophistication, which is needed now much more than before.

Notes

1. It becomes obvious later in this chapter why the pronoun "he" is being used to describe a study with two authors.

2. Nadai and Maeder (2005) coined the expression "multi-sited ethnography."

3. Because Bruegger worked in finance, their study was based on self-observation. His was not an "autoethnography"; this term can be reserved for different kind of projects, described in the section on diary studies.

4. For a more detailed discussion of this and other works that use the mystery format, see Czarniawska, 1999.

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