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Who Allowed You To Observe? A Reflexive Overt Organizational Ethnography

Abstract

Observing people working within organizational contexts through time creates epistemological issues, more so when doing it overtly, with top management's official agreement. Power relations as well as hierarchical structures strongly influence the way people view the observer and interact with her in organizations. Those interactions also partly depend on his personal background – sex, age, professional position and so on. Following a reflexive approach, my objective is here to better grasp how top management's agreement to the ethnographer's entry on the field may influence both the way workers from differing hierarchical levels behave with her (and thus affect her observing conditions) and how he may analyse his ethnographic notes to develop scientific sociological results.

Keywords

Ethnography; Reflexivity; Organization; Work; Epistemology

The ways in which an ethnographer is allowed to observe people through time, as well as ways she may interpret those observations, are widely dependent on reciprocal social positions negotiated between the observer and observed. Observing young men in a popular area (Mauger 1991) differs from doing one's research among French grand bourgeois and aristocrats (Pinçon and Pinçon-Charlot 1997). Those interactions are locally co-produced by actors, partly influenced by their reciprocal positions in the social hierarchy. Rather than developing a clinical talent or an empathic capacity in order to inspire trust to reluctant observed people¹, ethnographers have developed a *reflexive* perspective (e.g. Burawoy 1998, 2003; Schwartz 1993; Taylor 2002). A *reflexive* approach insists that ethnographers become aware of locally co-constructed *frameworks* (Goffman 1974), so they may

¹ Isabelle Baszanger and Nicolas Dodier have beautifully shown that through the sixties and the seventies French ethnographers have progressively transformed their approach of participant observation from a "clinical talent linked to an empathic attitude" to a "reflexive process" based on successive interactions (Baszanger and Dodier 1997) (my translation).

either play with them to improve their observing conditions, or take them into account when analysing their *collected data* (Glaser and Strauss 1968).

Observing people at work is no different. One has to find a way between *involvement* and *detachment* (Elias 1956) when entering the field as well as when analysing ethnographic notes. However, observing people working within organizational contexts - enterprises, associations, administrations or political parties - creates some specific epistemological issues, particularly when doing so overtly with top management's official agreement. Power relations as well as hierarchical structures strongly influence the ways people view an observer and interact with them in organizations, partly depending on the observer's personal background - sex, age, professional position and so on. Organizational life is produced daily through constant games between workers from differing hierarchical levels. Being present with top management's help implies that observers become part of a specific interdependent relation developed by workers. My objective in this paper is to better grasp how top management's agreement to the ethnographer's entry in the field may influence the way workers from differing hierarchical levels behave towards the ethnographer and the implications this can have for the act of observing and analysis of data gathered. This issue will be mainly addressed through my own experience as an overt ethnographer in two large French private insurance companies, *Hermes* and *Mercure*². I will discuss how social relations I have co-produced with both operational and human resources top managers and first level workers have strongly shaped both the kind of observations I was allowed to make and some of the sociological interpretations that I ended up making³.

Getting access to a closed place: ways and constraints

Organizations are closed places to which access as an ethnographer is difficult to achieve. One must either be employed as a regular worker over a long period of time, or get top management's agreement to observe as openly as possible over time. Both methods have proven useful in studying organizations. Covert observation is specifically powerful in revealing workers' ability to resist management's rules (Roy, 1952), while overt observations help reveal wider organizational rules and regulations (Burawoy 1998; Strauss 1992; Rosen 1991).

I will not discuss epistemological advantages and limits of both covert and overt positions. I will focus instead on what it may mean to get access to this closed place and be allowed free access to people *in situ*, two of the major elements that define ethnography, as stated by Atkinson and Hammersley (1995/1983):

in its most characteristic forms it involves the ethnographer participating, overtly or covertly, in people's daily lives for an extended period of time, watching what happens, listening to what is said, asking questions – in fact, collecting whatever data are available to throw light on the issues that are the focus of the research” (p. 1).

² For confidential reasons, those companies' real names will not be mentioned. This 18 months ethnographic research was led as a PhD student, from October 1996 till March 1998, at *Hermes* and *Mercure*, two major French insurance companies. I observed work activity in two similar administrative-technical departments composed of about 100 workers each and led interviews with first-level employees, middle managers and functional and operational top managers. Based on this empirical work, I defended my PhD dissertation in 2000 and published several articles in French academic Journals (e.g. Buscatto 2001; 2002).

³ This article is based on a working paper first presented at the *American Sociological Association Congress*, San Francisco, August 13-17, 2004.

Getting such open access is indeed often difficult since most top managers experience this presence at best as an investment to be made profitable, at worse as a risk to be tightly controlled. How does one get top management's interest in one's work while being allowed to observe as freely as possible over time? Through my own experience at Hermes and Mercure, I will try to present some reflexive principles which may guide organizational ethnographers in such an attempt.

A fashionable topic, a concrete agreement

It gradually appeared that I had been accepted in this organization mainly because my research goal had been considered to be interesting by both Human Resources and operational managers. This had supposed that I transform my academic research interest – *“organizational socialisation in big private companies using a comparative approach”*⁴ - into acceptable and understable managerial terms. Following Callon's expression (1986) I had to *problematise* research goal in appropriate terms for the companies I wished to study. It was meant to both get top management's interest (and thus agreement) and construct a common frame analysis with them (and thus remain accepted in the long run).

When sending letters to big organisations, I had first translated my academic research question into operational terms: *“organizational learning conditions at work”*. I presented my research as *“easy to lead”* and as *“an experimental research”*. To my big surprise, three insurance companies showed interest, including Hermes and Mercure! Other letters got lost in recruiting services or were filed without even being answered.

As I found out later, once inside Hermes and Mercure, my problematisation work was successful in companies which were already sensitive to such a question: I was part of a fashion trend which had permeated Hermes and was developing within Mercure. *“Organizational learning”* had become a trendy topic in the French business literature: books were coming out; articles were published in the professional management press; consulting groups were offering organizational learning principles. And *“organizational learning”* had become a managerial issue within the insurance business. I later discovered that organizational groups had recently been created over this topic within Hermes and among insurance Human Resources specialists (such as the Mercure Training Manager) within the Insurance professional Union (*“Fédération Française des Sociétés d'Assurance”*). Managerial experiences had even started to be led in some of Hermes innovative departments – *“learning by distance”, “tutoring”, “quality groups”* - and were coordinated by the Hermes Human Resources Department. Those companies had also developed a heavy rhetoric on workers' *“participation”, “satisfaction”, “motivation”, “ability to change and learn”*... Translating my research objective into a management issue had caught their attention and I was first received by the Hermes Training Manager Head (the first to answer me) based on this potential reciprocal interest. I had then to be accepted as an ethnographer.

My first (and only) meeting with the Hermes Training Manager Head before entering the field consisted of demonstrating that a deep and open research could

⁴ All quotations have been translated from French to English. Unless stated otherwise, I quote expressions/sentences which were either written in documents (including mine), or heard during observations or interviews.

help them to analyse organizational realities which seemed to escape them. As explained later by this first interlocutor, the topic, even if fashionable, was perceived as difficult and complex and a long-term scientific research had then been viewed as one experimental tool in this managerial process. I was then allowed to observe over a long period of time (one year and a half) in my own way, i.e. with a relative freedom of action, once access to each field was obtained. In return for this authorization, I was expected to respect confidentiality within and outside the organisation. My freedom of action was compensated by my public silence: all published articles, such as this one, use pseudonyms, which allows me to present my sociological results with no restraints. A concrete agreement was also settled: deliver oral and written restitutions to both local workers and human resources management. The same problematisation and negotiation process was repeated each time I entered a new operational field: operational managers were chosen by the Human Resources Manager for their training investment; confidentiality was required from me; I was asked for a specific written and oral presentation of my results. Compared with Hermes consulting fees standard, money given to me was defined as expense reimbursement – which it indeed was since I was financed by an academic scholarship - rather than as fees: the cost of my 18 months stay was not even equivalent to a week's consulting fees!

Well-founded fears and interests

Top managers belong to a social organization and accepting a researcher doing a survey supposes that the latter may correspond to an organizational need or resource. If not, risks are considered too high to let an observer in. We follow here Everett Hughes' (1971) idea that:

the fears which lead people to make it difficult for investigators are often enough well-founded, more than that, they lie in the nature of social life. (p. 436)

For managers I met, risks were obvious (and often expressed as such with me when they felt more at ease). Being left alone, the ethnographer might discuss *taboo* issues, raise tricky questions or invite workers to raise unwanted problems. The ethnographer may also give a negative impression of the top manager's ability to manage when delivering sociological results or when discussing with other top managers. They may even give confidential information to competing top managers within the organization. As beautifully demonstrated by Melville Dalton (1959) in his ethnographic work on managers, management is not one unique homogeneous group but is constituted of several people defending differing interests, situated at different hierarchical levels, located in different buildings, holding several functions. They tend not to want to give other managers reasons to criticize them.

The advantage to a manager of allowing a researcher to work in his organization is often more difficult to grasp. In my case, I was allowed to do ethnographic work first because I was studying a political question – “*organizational learning*” – and in a novel way. As they sometimes told me, friendly operational top managers thought they might get some symbolic reward in looking for new ideas and enhance their image as innovative managers. Training Managers were already active in finding new ways to answer such a political issue. But the reasons one may be accepted are sometimes much less tangible. One top manager may be happy to discuss her organization with academic outsiders without necessarily paying much

interest to the researcher's final results. She may be interested in adding to her prestige due to the positive image of academics in her company. It may also happen that a top manager is an ethnographer's friend and is happy to help him in his job (Barley, 1990)! The reasons a researcher is allowed to enter the field will influence the way their position develops within the organization and the way in which they are able to negotiate their position over time. I will thus discuss how my concrete agreement with top management influenced my relationship with the managers that I have regularly met through my research.

Being trapped in difficult roles as a constant risk

As discussed by Paul Rabinow (1977) - whose difficult and forced entry in a Moroccan village highly shaped the way he interacted with villagers and the strategies he had to develop to get accepted - being allowed to lead an ethnographic project is only a first step in entering the field. Researchers have to evolve in a tricky environment since pressures, ambiguities and difficulties are always coming up in the way of the observer's goal to work as freely as possible (Schwartzman, 1993). How may one conceptualize such realities in an organizational context in order to maintain the requisite freedom of action over time and use it to develop original results?

Identifying and co-producing adapted frameworks

A very useful concept to analyse social positions co-constructed by top managers and first-level employees together with the researcher is what Goffman (1974) calls *frame analysis*: that is, the analysis of the way social experiences are organized through interaction. Any work situation implies that actors define, interpret, *frame* a situation, whatever its reality - e.g. dealing with a new file, talking to a customer or relating to an outsider. Far from being an open situation, framing is performed under certain constraints. Most experienced situations have already received shared and stable collective definitions. This is what Goffman referred to as *primary frameworks*, whether *natural* or *social*, and these cannot be easily ignored. Ignoring these frameworks risks negative consequences such as exclusion, being laughed at, or misunderstood. However, those *social frameworks* evolve through actors' daily activity. That is people ongoingly transform primary frameworks through interaction. One may also observe conflicts between frameworks when actors differ in their ways of interpreting the current situation.

Observers as well as observed workers define their relative positions through interaction, this definition being partly influenced by available organizational frameworks. Through all his behaviours, words, actions, a researcher may participate either in reproducing current frameworks or in transforming them over time. Analysing those games and experiences is what is called distancing or reflexive work. I will here discuss frameworks which were co-produced with top managers throughout my field work, during which my constant objective was to be as left alone and free as possible. These analyses were not ready-made when starting my work, but have been produced throughout my research when attempting to formalize many of my interactions with top managers.

Are you risky? Are you useful?

Even if my relative freedom of action seemed to be part of the initial agreement with top management, it was in fact constantly threatened and negotiated in my different interactions with operational and functional management. Two types of pressures repeatedly emerged in my daily experience.

On the one hand, questions and requests were put to me to provide usable data (on people, on other parts of the organisation, on research usefulness). Here is one example of many that I experienced during my research and which occurred on December 17th 1996:

After only a few days doing fieldwork in a new administrative department, and while I was observing a middle manager for a full day, I happened to attend a monthly department meeting which gathered all middle managers, the two operational top managers and a few functional specialists. Once the meeting was finished, a social gathering, *un pot*, was organized to celebrate the event. Seeing me in the crowd, the two operational managers very soon came to speak to me. The head of the department then asked me *“What do you think of our department? How was my talk, do you think it was effective?”* I tried to politely decline to answer, saying I was just starting my observations and that, anyway, I would wait till the end of my work to give some sociological results. He insisted, saying he just wanted to know my *“first impressions”*. I said *“I was not interested in people, but in functioning rules”*... He then asked me *“since you study training, why are you attending such a meeting?”* I did then remind him that *“my topic was not training, but learning at work, organizational learning”*... which soon ended our conversation and enabled me to resume my observation of the middle manager I had planned to observe that day!

On the other hand, my presence was regularly considered to be risky. I was often (and nicely) asked questions such as: *“Who did you speak to?”*, *“Who did you (or will you) inform of your results?”*, *“How is confidentiality ensured?”* These were some of the questions that signaled to me the threat I posed to the individuals that made up the observed organisations. In short, I was considered to be politically risky.

Both pressures could sometimes create some very paradoxical situations. I was supposed to both inspire their trust as a serious researcher (and thus be allowed to continue my work) and limit answers to their questions in order to ensure my confidentiality clause and my ethical position (and not to be kicked out of the field)! In other words, I had to navigate between two negative frameworks which might be attributed to me and prevent me both from observing in the long run and doing it as openly as possible: the *“immature trainee”*⁵, lacking seriousness and being impossible to trust; the *“professional consultant”*⁶ who may use all those observations in too efficient, and maybe dangerous ways. As will be shown now, what happens to the observer can also be highly dependent upon the resources that s/he starts up with and ways in which s/he uses them to evolve within a given organizational context.

⁵ My expression.

⁶ My expression.

A tendency to look like an “immature trainee”

When top managers were informed of my former experiences both as a Training Manager in a big industrial company and as a former researcher in a famous French automobile enterprise⁷, my observing conditions were quite favored. Indeed, when I had not been introduced as such, I seemed to appear as an “immature trainee” given my apparent youth, my position as a PhD student, my passive observing position and my informal look (these traits will be described more fully later on). And this role of immature trainee has often appeared as a constraint while doing observations and interviews among top managers – and only at that hierarchical level as will be shown later. I progressively found out that if top managers were perceiving me as a young and inexperienced researcher, they tended to either avoid spending time with me (which limited my ability to be informed), or to develop a strong wooden language throughout our discussions or interviews (regarding human resource management rules, their careers in the company or their appreciation of the company objectives and rules). Here is an example of such a phenomenon which happened on October 15th 1996:

When starting field work, I decided to interview Human resources (HR) and Training functional managers in order to better grasp both companies HR rules and projects. Only two weeks after having started my observations at the Hermes Company Headquarters, I did interview the woman in charge of Hermes “*management and human resources processes, rules and tools*” which were used to “*manage careers, competencies, jobs, evaluations*” in the company. When I first asked her technical, as well as political, questions regarding the new “*competencies management*” systems, the ways they were implemented and/or perceived in the company, I was given general and quite tautological answers such as: “*competency management is linked to people’s abilities*”, or, speaking of the links between the former and the new HR systems “*those two systems are not compatible because they do not answer to the same logic*”. Even when asked very specific questions such as “*why such an incompatibility?*” or “*how is the management of comptencies linked to the other HR tools?*”, I would not get any more specific answers. After thirty minutes of exchanges like these, I decided to share with the interviewee some of my own knowledge of such processes, referring to my former experience as a Training Manager in a company which had tried to implement similar projects. The interview then took a completely new direction. She started to be more technically specific and, interestingly, explain the human “*resistances*” that top management was confronted with within the company – “*resistances*” from Unions and from employees. Following the questions on training, salaries and career issues were then explored much more precisely and were politically contextualized. It was as if discovering that I was a (former) HR expert had led the interviewee to drop her wooden and somewhat closed language and answer my questions much more openly...

As shown with this example, when my interlocutors knew about my past experience, prior to our meeting and/or throughout our interactions, I found out that the level of discussions was quite changed. This experience had also highly motivated Hermes Training Manager to accept my offer in the first place. During the

⁷ I had led fieldwork in this French automobile company in 1994 and had published one article in a French academic Journal in 1996.

recruiting interview, I was asked specific questions on my former experiences (the same questions were asked when meeting operational top managers). And this experience was regularly mentioned as the basis of those top managers' trust (and examples of other academic requests they rejected for the same reason). My knowledge of management norms of presentation had obviously helped me to gain their trust. Whilst this was a blessing, engendering their trust based on my past roles was a mixed blessing as will be discussed now.

Being confused with a “*professional consultant*”

Indeed, given the way I used my personal background to enter the field and create meaningful interactions with top management, I had then to strongly fight against the potential confusion with a “*professional consultant*” which was also creating observing difficulties. As already mentioned, top managers repeatedly asked for my evaluation of their organizational efficiency, managerial abilities or workers' capacities. Once I had become aware of this potential confusion (and its potential effects on my observation strategy), I started to timidly answer that my role was to be as confidential as possible and that I was not able to give a serious opinion before the end of my research. Since it did not seem very convincing to them, I also developed a clearer strategy which might be stated as follows. I attempted to erase signs of business professionalism and build on academic resources to become labelled as a “*university expert*”⁸. My clear objective was then to avoid being characterised as a professional consultant, since it was creating strong limits to observing both top managers and workers who would have rightly felt judged and evaluated (and not simply observed) by me and may have used me in their ‘power games’ (which they did anyway, as will be discussed later, but without preventing me from remaining in the field).

In response to this situation I gradually developed different devices when interacting with top managers. I was often repeating the very limited cost of my research and explained it by the academic character of my work. I restated my neutrality through open behaviours: systematically refusing to comment on people or to give my opinion on organizational issues. I also provided my interlocutors with academic articles or references, always avoiding all requests to formalise a specific judgement or conclude on a given topic. Asking unusual questions to top managers while interviewing them was also a way to look more academic (as supported by comments on my “*weird questions*” at the end of some interviews). Last but not least, my clothes, ways of speaking and of presenting myself indicated, from their perspective, a lack of professionalism (too laid back, not efficient enough). This position was a perpetual construction, constantly threatened by new events, contacts or requests. And I, of course, never fulfilled this strategy and had to constantly redefine my social position in the expected way.

Consequences on the quality of results

As shown in these different examples, my initial position in the organization and my strategies to co-produce an open interaction context with top managers were

⁸ My expression.

linked both to my perceived personal resources – former experiences, formal status, age or professional expertise –, to my strategies for shaping perceptions one way or the other, and to the organizational context in which I was positioned. Each ethnographer co- constructs their social position with top managers based on a constant analysis of what is going on in the ethnographic relationship. It allows the ethnographer to stay in the field as freely as possible and to be perceived in more distanced ways by workers who will then develop their own ways of managing the impression they make to the ethnographer.

More than reporting results, ensuring “likelihood”

As already stated, I was supposed to present oral and written reports. I will present my epistemological use of such a practice as it is rarely discussed in academic work (Michael Burawoy speaks of a *valedictory revisit* (Burawoy 2003: 674)). Once again, I was torn between two contradictory objectives. On the one hand, I was interested in receiving useful comments on my analysis through a constructive dialogue. On the other hand, I intended to present results without recommending any solutions, while recommendations were most valued by managers - and would have thus encouraged them to communicate further with me. Indeed, more than respecting a contractual agreement (and paying my observation debt), I intended to use oral reports in order to nourish my reflexive sociological work. Following François Dubet’s epistemological reflections, my hypothesis here was to build *likelihood* (*vraisemblance*) with observed people in order to found a debate and then feed my scientific results (Dubet 1994).

The reasoning works as follows. According to the Weberian paradigm, actors have *good reasons* to do what they do. A sociological analysis formalises actors’ points of view, ways of proceeding and practical choices in a very specific way, visualising interdependency relations between actors while they tend to view themselves as individual actors. In other words, through their analytical work, the researcher tries to situate individual actions in their collective reality, attempting to redefine a personal experience as an organizational phenomenon. Then, as Dubet clearly states “*sociological interpretation presents itself as a problem, between the internal sense of action and the one built up by the sociologist.*” (p. 94). *Likelihood* is then supposed to solve this difficulty in that it forces the researcher to write up results which, at the same time, remain as close as possible to people’s experience and present a collective analysis. People should then recognize their daily realities while acknowledging a new way to express and explain it.

But this does not mean that people agree on all aspects of the report. It implies more that even when disagreeing, their disagreement might not concern the report of their experience, but explanations developed by the researcher. And even when *likelihood* is ensured, the ways people interpret or discuss results indicate that there are many ways to experience such realities. Is this sociological analysis acceptable to them, in which terms and at which conditions? Such reactions may help support the analysis, but also bring new proofs, new perspectives. Is it unbearable, and if so why? Do actors disagree with each other and on which terms? Such a conflict or disagreement between observed people or with the sociologist, if discussed with actors, may lead either to the reformulation of results, or the development of new hypothesis. It is only through an open dialogue that researchers may use reporting back to enlighten their sociological analysis. If not used to convince others of one’s

analysis (membership validation), but to once more become aware of actors' ways of thinking, such an oral report may enrich the sociologist's analysis, supporting some points, transforming others, deepening some forgotten points. Once more, among several other possibilities, I will discuss one specific and tricky example which happened in June 1998 and may help understand this perspective:

The restitution of my other written reports mainly helped me to enrich my first organizational analysis. But one such report disappeared from the organization due to a conflict with operational top managers. Not only was this conflict never settled, but it also led me to develop a specific academic article! Conflict is indeed one of the most difficult situations to be experienced when one attempts to reach *likelihood*, *vraisemblance*, but it ended up being a very rich experience at least in terms of producing sociological results. Here is the situation. When providing those two observed operational top managers with my sociological report, they rejected it without radical transformation (the same report had been considered as very insightful by Hermes Training Manager). My main interlocutor, M. X, was very aggressive on the phone, accusing me of writing "*wrong conclusions*" and "*immature analysis*". Right after his phone call, I had a discussion with Hermes Training Manager who had already been informed and who told me to calm down (I was quite upset and not convinced at all to be in the right). She then called M. X and assured him that the report would be kept confidential, asking him to meet me to see how to transform it.

A meeting was then organized with the two main operational managers of the department. Through our discussion, it appeared then that they rejected the way I explained middle managers' marginalisation from the organization – even if it was only part of the complete report. They seemed to agree with my main analysis – middle managers' exclusion and marginalisation from the organization (they often said "*it's true*") – but wanted to explain it through "*historical*" and "*psychological*" explanations which would have legitimated their managerial choices. As explained by M. X: "*You have to put this analysis in its historical context. I have tried to work with those middle managers, to motivate them, to delegate tasks, to train them... But I soon understood they would say yes, but they would not do what was expected. (...) They wanted to change, but they could not, they were limited... (...) After two years, I bypassed them and worked with first-level employees. What you say is true, but I had no other solution. I had too many middle managers, but because of Union pressures, I could not discard them.*"

While he thought middle managers were marginalised from the organization due to their psychological inability to evolve, I presented collective organizational mechanisms which reduced those people to their denigrated position and legitimated it. While we could agree on the diagnosis, we would not reach an acceptable agreement on explanations. Not only was the *likelihood* of my report ensured, but our heated discussions also enriched my analysis of this phenomenon. It progressively led me to conceptualise this phenomenon as the expression of a *social psychologisation process* which produced and legitimated this social group position within Hermes without workers being aware of it. While operational managers could protect their interests - the report was never read outside this department, I never

discussed those results with other members of the organization -, I used this open conflict to nourish my academic work (Buscatto 2002)⁹.

Observing workers: a locally co-produced position

When first entering my operational fields, specific organizational frameworks were already available to workers to help them perceive me. It soon became clear that my position as a non-participant observer - never experienced before by those observed workers - was widely associated with that of an outside employee, sent by top management to evaluate organizational processes: internal or outside consultants. This held even when my reasons to observe appeared to them as different from those of *outsiders*.

My observation work thus consisted in transforming my original position as an outsider close to top management to a more neutral, but never fully reached, position as a young neutral researcher guaranteeing confidentiality. This work was constant, difficult to maintain, never fully controlled given the hierarchical nature of organizational life. I will show how I progressively tried to redefine my position through interaction while being conscious of the limits of this attempt when analysing my *collected data* in order not to be a victim of what was said or done in front of me.

My original observer position: “an outsider close to top management”¹⁰

As an observer, I was first viewed as a burden, as a disturbing element and sometimes even as a risk. Being unknown, of course, always creates some embarrassment when first observing. But mistrust, even fear were highly increased by the way I entered into the field: I was sent by the Human Resources department and operational management. I was then mainly perceived as an investigator interested in unveiling personal and collective strategies, daily hidden practices, supposed management weaknesses, *etc.* Workers perceived me first as a top management employee and likened me to a consultant who might use her observations to recommend changes which were not necessarily wanted (fewer workers, organizational reforms, increased productivity). This was reinforced by the fact that observed workers had never met a researcher before. Here is a clear example of fears one may create when being introduced in the field by HR and/or operational top managers:

I spent a full day observing Jeanine¹¹, a first-level employee. I also interviewed her for a full hour at the end of that same day. This specific observation was led at the beginning of my fieldwork in her department, on March 22nd 1997. Jeanine had seen me about once or twice before I observed her. In the days following her observation and interview, I felt Jeanine was uncomfortable seeing me there. I took the first opportunity I found to informally discuss with her, understand what was going on, make sure she was not worried because of this experience... Here is what she then told me: “*When you asked me questions, I was quite open. We all try*

⁹ I here discuss formal reports, but reports may also be informal, face-to-face, friendly or “accidental” as I have experienced throughout my ethnographic research in the French Jazz world (Buscatto, 2007a, 2007b).

¹⁰ My expression.

¹¹ A fake name.

to be. I told myself that we had to help you out in your work and that anyway things will change here. But afterwards, I got worried. I wondered what you would do with all this, if it was not going to create problems. I am worried... even if I know things will change..." In this case, fears have appeared afterwards and did not seem to have affected my observation conditions: Jeanine was indeed quite open during the observation day and the interview, I did have access to several clandestine games and private information -. But those fears were experienced fully afterwards, which gave me the opportunity to assure her once more that I would preserve confidentiality. However, this example does show that my observing position might create difficulties which needed to be dealt with. It also reveals that, as we will now see, my strategies to overcome such fears, at least while observing and leading interviews, may have been quite successful with Jeanine.

Being perceived as a “neutral young research trainee”¹²

Even if I had not yet been aware of this original position, workers would have helped me discover it through their regular questions about my research (“*who asks for it?*”, “*why?*”, “*what for?*”, “*what will be written?*”, “*what will they do with it?*”) and their first behaviours towards me: getting silent when I first arrived, escaping observation when possible, being uptight on first days... I then developed different strategies in order to dissociate myself from this original position. After several attempts, I tried to build what I might call today a “*neutral young research trainee*” position, partly trying to be assimilated with young trainees or temporary workers, partly constructing a new framework with observed workers. My ideal (and never reached) goal was to be perceived as a young researcher still learning at school and based in a different professional world, the academic world. Power relations and hierarchical stakes were not supposed to influence my behaviour since I did not belong here, being thus able to ensure confidentiality and neutrality to observed people – which anyway I had planned to do for obvious ethical reasons.

In order to get closer to this social position, I adopted a fitting physical and verbal appearance: wearing jeans, tee shirts and comfortable shoes; being underequipped (pens, paper and handpack at best); reinforcing the impression of relative youth by the lack of make-up, jewels or glasses (observed employees, mainly women, were over 40 years old, I was then about thirty and, according to them, looked about 25); expressing all my questions or requests as a “*personal favor*”; using a simple language; accepting personal conversations on my private life and interests... I also presented myself as a trainee, completing a PhD at the University, not mentioning (without hiding it) my former experiences, since, as the following experience shows, this generally limited my observing conditions:

Indeed, one of my first observations of a middle manager led beginning of November 1996 had been quite a failure apparently due to him knowing my former experience as a middle manager. Indeed, when I started the daily observation of Paul¹³, not only did I explain my research topic in broad terms (“*organizational learning conditions*”) and ensure him that confidentiality would be preserved, but I also informed him of my former experience as a middle manager. I then thought that this might create some

¹² My expression.

¹³ A fake name.

easiness in our relationship, as experienced with top managers before. But instead of working on his own, as I had asked him to do and as most observed workers did afterwards, this middle manager spent the day explaining what he was doing, reasons he was doing it, goals he was trying to achieve, ways he analysed his work situation... He sometimes asked me for my opinion, suggesting my former operational experience might be useful to him... And even if I repeatedly asked him not to explain anything while working, not to describe his actions, not to discuss his work until our formal interview, he never stopped doing it... After this experience, I decided never to mention this past experience again to middle managers, unless asked to do so, and I never had any middle manager explaining his work throughout the day again.

I then concluded that this position as a young trainee, even if incomplete, encouraged people to relax, to conduct their business as usual, and to open up, even to help me in my work. Most people were over 40 years old, and they were also sensitive to the problems of young people with regard to unemployment and getting a permanent job. Therefore, young trainees tend to inspire more pity than fear. The numerous questions I was asked about my future, and the remarks made on my difficult job (*"is it not too difficult to observe all day?"*), partly support the idea that I was partly accepted as a young trainee. I was also easily called by my surname or *"tutoyé"* without even being asked if I agreed to it. Workers were easily available to answer my questions, to help me and sometimes came to me to give me some work papers for what they called *"my file"*. They would accept an interview even if time was short or they were afraid to do it, telling me afterwards they wanted to *"help"* me. I also found that after a few hours or days, workers would speak about 'private' topics in front of me (family issues for example), would cheat on top management, would deal with their private (and forbidden) business in my presence (calling an administration office to settle private problems for example) with no embarrassment.

However, I could not be simply assimilated as a young trainee since some of my behaviours did not fit this role: taking notes on people's behaviours, printing official papers, observing and meeting with management, and writing reports. Questions were often asked about those activities. And, that is why I also progressively constructed, through interaction, a second framework with organizational members: as a *"neutral researcher guaranteeing confidentiality"*¹⁴. When beginning an observation, I would explain the sociological method in metaphorical words (*"you are numbers"*, *"I am only interested in shared behaviours"*) and the way I could ensure confidentiality (*"I will use only anonymous quotations"*, *"only shared behaviours or discourses are quoted"*, *"I will quote only common situations, so you ca not be recognized"*)¹⁵. I also rapidly announced my parallel research at Hermes and at Mercure to be sure it would not be known behind my back: a merging between both companies was announced during my research and it became a threatening issue, specially at Mercure. I also tried to adopt some systematic behaviours to embody my role. Whenever asked, I would show my notes, so that people would know it only consisted in noting very specific behaviours (who says what to who, what is done, which timing...). Most people would then tell me they felt sorry for me for doing such a dull work. It even became a joke in one of the

¹⁴ My expression.

¹⁵ For obvious ethical reasons, I did respect those promises while writing written reports, even if it sometimes limited my ability to support my claims.

observed departments, one employee announcing aloud how I was to write their actions down (in quite an efficient way!).

I also always refused to comment on people's work and to answer questions on other observed groups, departments or companies. If people insisted, I would show them that this was the only way to protect their own confidentiality. I would also never leave my notes unattended, which meant carrying them everywhere. Over time, I became part of their organizational life as a silent and acceptable observer¹⁶.

Not being fooled by one's own acceptance

Even when partly reached, this neutral position is never completely shared with workers and one should not be fooled by a feeling of acceptance. I was always, at least partly, viewed as an outsider close to top management and several observations could not have been interpreted rightly without taking this reality into account. In other words, part of observed behaviours and discourses are constructed in answer to this perceived position and being aware of this possibility helps better analyse observations. What is hidden or overlooked indirectly indicates what is valued or forbidden by colleagues, middle or top managers from the studied world.

Use the ethnographer to pass messages on to top management

The researcher may be used to pass messages on to top management. This use, if systematic, may deserve to be analysed in sociological ways. This happened to me often with first level employees when observing and/or doing interviews at Mercure. Management was often criticized for not giving enough incentives, for giving too much work, and for being inconsistent, as in the following example recorded on April 14th 1997:

After having spent a full day observing Gina¹⁷ at work, I had a full hour interview with her to discuss work, relations with colleagues, training, promotion, etc. Gina had been working at Mercure for the last ten years as an insurance first-level employee. Half-way the interview, I asked Gina *"how do you perceive your work unit objectives?"* She answered quite strongly *"We have to satisfy the customer. That's what they keep telling us, every week. Work and work, fast and well, that's what they repeat endlessly. It's true. Every wednesday, they have a meeting, and every thursday, they ask us to work more."* I then asked her to explain what she meant, to which she then answered *"We already work a lot (...) The problem is that we have to work, but we should not ask anything. It is not very motivating. We are seventeen and out of seventeen people, only two people get a raise. Some people did not get a raise for more than ten years..."* After the interview, while discussing with her manager, I found out that Gina had recently received a promotion and a raise (I then checked the information with her and she confirmed it). But throughout the interview, Gina never mentioned it. She had mainly complained about work being too heavy and not being rewarded rightly at Mercure...

¹⁶ In a overt ethnographic survey led in 2001-2002 at La Poste, the French postal service public company, I also tried to co-construct a similar framework with observed workers. But since I had then become an academic public servant at La Sorbonne, the co-constructed framework I tried to implement was not focused on my youth any longer, but heavily oriented toward my academic supposed neutral position.

¹⁷ A fake name.

What may have appeared as a kind of manipulation (she did not mention her own promotion while complaining about this company poor promotion policy) was in fact the expression of an organizational rule. Most first level Mercure employees, evolving in an enriched Taylorian organization, were constantly producing and reinforcing strategies of resistance (Roy 1954) in order to work less, get better paid and work in better conditions – daily observations helped me reveal them of course. Very active union representatives were also regularly observed distributing leaflets expressing such issues, discussing with employees, helping them formalize their complaints in their work units. I even observed a Union representative choosing to discuss one employee's new situation with a middle manager in front of me, so that I would witness his helpful work in the organization. My role had been defined in relation to this context, and becoming aware of my role enabled me to produce one more proof on their strategies of resistance which I had become a part of.

Hide relational difficulties from the observer

It also appeared that it was difficult, or even impossible, to observe what employees and/or middle managers had defined as their personal organizational life or their hidden compromises - and which John Van Maanen calls "*collective secrets*" (1979). Relational difficulties were not easily discussed as such, enmity tended to be erased from discussions and conflicts concealed. Personal difficulties, even if collectively created, were mostly hidden. I will present here two examples of such hidden difficulties which I have unveiled through careful and specific observations, leading to the production of interesting sociological results.

The first example happened at Hermes. As already discussed, I have progressively identified a *social psychologisation process* which was producing and legitimating Hermes middle managers' marginalisation within this organization. But this finding had been quite difficult to achieve empirically, mainly because of middle managers' tendency to hide their difficulties from me and/or to express their situation in positive ways in order to *save their face*. During my first interviews with these middle managers, they were mainly stating their supposed new management responsibilities in order to describe their new roles and tasks in their work units. They described themselves as people leading their direct reports actively, leading meetings openly, creating innovative projects... While first observed, they also tried to focus on tasks which would illustrate such skills – handling a budget, preparing a direct report's evaluation, dealing with difficult insurance files... However, they could not maintain such games long. I progressively observed that their direct reports were often nagging them, were misbehaving during meetings or were bypassing them to handle tricky files... I also progressively observed them trying to escape their new role and/or failing in implementing it – incapacity to prepare a budget, to lead a meeting or to train a newcomer. I then decided to focus on their situation. When observing first-level employees, I collected as much data as possible on their difficulties. I also followed more middle managers than previously planned and decided to (softly) confront them with those observed facts during interviews. This last strategy often led them to express their difficulties openly while explaining them as the consequence of "*psychological*" deficiencies – their "*personality*" did not fit this new managing role... Most of them then used me more and more to complain (and pass on messages to top management), get advice to

handle their future meetings or careers or to try to understand what was going on at Hermes...

Hermes middle managers' strategies told me much about their shame about being marginalised, the reasons they gave for their difficulties, their desire to get out of this difficult situation, their despair to ever change, and other workers' lack of compassion. Discovering individual actors' strategies to hide some facts helped me to identify a collective phenomenon, a *social psychologisation process*. This tendency to hide some personal or collective realities may even lead to a collective strategy developed to hide some events from the observer, as experienced at Mercure:

My arrival at Mercure corresponded with a conflict developing between employees and middle managers, but open and collective disputes never occurred in front of me. From time to time, I would perceive a tense atmosphere when arriving in a department (red cheeks, tense bodies, frowning or sulking employees, heavy silence). I also observed strong remarks between some employees and their chief. After a few unsuccessful attempts to understand what was going on, asking employees to explain those observations, I have progressively found out that a fight was just developing over the work load issue. What I then found could be summarized as follows: for two years, one or two temporary workers had been hired *per* department in order to replace employees sent on intensive training. Now that the training plan was over, temporary workers had been dismissed. But for two years, an implicit work load share had been agreed on by workers, giving temporary workers less interesting tasks (what Hughes might have called "*dirty work*") and asking them for higher productivity norms. Regular employees had now to share this dirty work and increase their productivity norms. They wanted to get something out of it, but did not seem to think it that legitimate. They did not want an outsider, partly sent by top management, to see them while exchanging bad words on this issue. Indeed, first-level employees never explained this conflict to me. People who finally informed me were middle managers and qualified employees who did then criticise those first-level employees who were using temporary workers to do their "*dirty work*". When I then asked first-level workers if this information was true, they would agree to it and explain it their own way, as a resisting and thus legitimate strategy.

Not being able to observe such conflicts was directly caused by collective behaviours which told me many things about the organizational dynamics and the strong separation between first level workers, middle managers and nearly promoted workers. I was then able to discover it and get to its collective dynamics only because some workers did not consider this conflict as legitimate and were ready to expose the colleagues whom they thought to be wrong to the relative 'outsider'.

Trying to look good?

Another kind of *face work* people engage in might consist in trying to work at best and to hide difficulties and resistance strategies. I decided to discuss it last since it is often presented as observations' main difficulty, while it appeared to me as a key source of knowledge.

First, when a worker tries to do their best, it tells much about what the "*best work attitude*" is supposed to be in a specific organization. Just as when reading official rules and norms, first days of observation may indicate what the official

behaviour is supposed to look like, some workers naturally succeeding in applying such norms, getting promoted and valued in the organization. Being identified as an outsider close to top management may then become an advantage as long as one is aware of it:

For instance, at Mercure, I twice observed the same middle manager, Jean-Luc¹⁸, with a six month time lag. During my second observation, on November 27th 1997, he did what he had not done during my first observation: counselling his business insurance interlocutors. He was then applying a new norm officially instated and given much importance during my absence - as I found out when reading new objectives and tasks given to middle managers at Mercure. When I asked him about this difference in his job, he seemed surprised and pretended he had always done so. He also explained at length how important it was to do so... I still do not know whether he had chosen to behave that way to look good in front of me or if he was trying to be positively valued by his hierarchy and, when confronted with it, did not want to look too eager to accept new rules – Mercure and Hermes were merging, and this new rule was clearly imported from Hermes... But thanks to this observation, I did find out that this norm was perceived by some middle managers as a new official and important norm to be implemented at work.

Secondly, and more importantly, as discussed by Becker in *Sociological Work: Method and Substance*, workers tend not to be able to play a new role over time for two main reasons. On the one hand, they relate to people who mainly continue to ask them what they are used to being asked to because work has to be done. Workers have to get their job completed, and even if they try to adopt a perfect attitude in the first hours, they have to forget it when confronted with real problems to be settled. I was often asked to help in producing clandestine behaviours, resisting or creative practices, just because this is what the work situation required observed workers to do in order to complete a normal day and not get into trouble with their hierarchy and with their colleagues. On the other hand, one observed worker may be able to practice such a game, but setting up a collective strategy is most of the time too difficult to handle. I usually became part of clandestine games which were shown to me at length (reading names, limiting work, helping settling problems)... I had no choice to return to my first passive answers because of the negative reaction they tended to provoke. I was integrated by first level employees in their games.

The observer's position is locally co-produced with workers from different hierarchical levels. It widely evolves throughout the observing time, depending on the ethnographer's personal resources and behavioural strategies, organizational context, individuals' positions within and outside the observed organization. Each daily observation is a new day in this fragile and fluid construction. Only a systematic and daily analysis, focusing on a reflexive analysis of the observer's social positions, enables the ethnographer either to work on transforming, reproducing or eliminating them, or to use collected data as sensible signs of more general rules (Glaser and Strauss, 1968). I tried to shift from my original position as an outsider close to top management to a young research trainee position through different strategies. But hopefully people resist in their own ways and their resisting strategies told me a lot about organizational dynamics.

¹⁸ A fake name.

Discussion and Conclusion

By observing people at work, one becomes part of organizational realities and participates in workers' interdependency relations since:

The outstanding peculiarity of this method is that the observer, in greater or lesser degree, is caught up in the very web of social interaction, which he observes, analyzes, reports. (Hughes 1971: 505).

In order to both build acceptable positions with observed people, and produce valid sociological conclusions, the ethnographer constantly works on co-producing adapted social positions in the organization and on becoming aware of negotiated roles over time. This is *reflexive* work. Even if based on the use of several positive techniques, it mainly requires constant questioning of the meaning of observations, depending on the social context in which they emerge and are developed.

This is tricky work, organizations being closed, hierarchical and sometimes conflictual places. The overt ethnographer first develops convincing strategies to get accepted by top managers. Once accepted, they must constantly negotiate their social position in order to be left relatively free as an ethnographer. They must then co-produce frameworks with observed workers, in order to distance themselves from initial tricky positions - such as an observer close to top management -. They also work on interpreting observed behaviours produced by observed people to hide, transform or distort discourses and practices.

Analysing empirical data becomes, then, a central source of knowledge. Discussions on methodological issues are more than a simple exchange of technical recipes, they enable researchers to enrich the quality of their sociological results, and thus work on improving the quality of qualitative methods. But reflexivity is not specific to the use of qualitative methods in ethnographic sociology. It has been present in ethnology or history for decades (Bloch 1949)¹⁹. More surprisingly, it has also been observed in some "hard sciences" such as particle physics, where experiments as such are defined as objects of study in order to better interpret results and to develop further experiments (Knorr-Cetina 1992). One may thus hope that reflexivity expands to all sociological research, quantitative and qualitative, since all research processes produce *artefacts* (Silverman 2007) and *social biases* (Gaxie 1990; Le Noé 2002; Peneff 1988). Those may be reduced, but never quite avoided through the simple application of positive techniques. Why not 'use' reflexivity as a way to improve the quality of quantitative, as well as qualitative, methods then?

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¹⁹ « *Vieille sous la forme embryonnaire du récit, longtemps encombrée de fictions, plus longtemps encore attachée aux événements les plus immédiatement saisissables, elle reste, comme entreprise raisonnée d'analyse, toute jeune.* » (Bloch Marc 1997/1949: 43).

References

- Atkinson, Paul and Hammersley, Martyn (1995/1983) *Ethnography. Principles in Practice*. New York: Routledge.
- Bazsanger, Isabelle and Nicolas Dodier (1997) "Totalisation et altérité dans l'enquête ethnographique." *Revue française de sociologie* 38: 37-66.
- Barley, Stephen R. (1990) "Images of Imaging: Notes on Doing Longitudinal Field Work." *Organization Science* 1: 220-247.
- Bate, Paul S. (1997) "Whatever Happened to Organizational Anthropology? A Review of the Field of Organizational Ethnography and Anthropological Studies." *Human Relations* 50 (9): 1147-1175.
- Becker Howard S. (1970) *Sociological Work: Method and Substance*. Chicago: Aldine.
- Bloch, Marc (1997 (1949)) *Apologie pour l'histoire ou Métier d'historien*. Paris: Armand Colin.
- Burawoy, Michael B. (1998) "The Extended Case Method". *Sociological Theory* 16 (1): 4-33.
- (2003) "Revisits: An Outline of a Theory of Reflexive Ethnography." *American Sociological Review* 68 (5): 645-678.
- (2007a) *Femmes du jazz. Musicalités, féminités, marginalisations*. Paris: CNRS Editions.
- (2007b) "Contributions of Ethnography to Gendered Sociology: the French Jazz World." *Qualitative Sociology Review* III (3): 46-58. Special issue "Ethnographies of Artistic Work" edited by Becker Howard S. and Buscatto Marie
- (2002) "Des managers à la marge : la stigmatisation d'une hiérarchie intermédiaire." *Revue française de sociologie* 43 (1): 73-98.
- (2001) "La relation d'emploi à l'ombre du chômage. Le cas de deux sociétés privées de service." *Travail et Emploi* 87: 67-79.
- Callon, Michel (1986) "Eléments pour une sociologie de la traduction. La domestication des coquilles Saint Jacques et des marins-pêcheurs dans la baie de Saint-Brieuc." *L'Année Sociologique* 36: 169-208.
- Dalton Melville (1959) *Men who manage. Fusions of feeling and theory in administration*. New York: John Wiley and sons.
- Dubet, François (1994) "Vraisemblance : entre les sociologues et les acteurs." *L'Année Sociologique* 44: 83-108.
- Elias, Norbert (1956) "Problems of involvement and detachment." *The British Journal of Sociology* 7(3): 226-252.
- Gaxie Daniel (1990) "Au-delà des apparences... Sur quelques problèmes de mesure des opinions." *Actes de la Recherche en Sciences Sociales* 81-82: 97-112.
- Glaser, Barney G. and Strauss, Anselm L. (1968) *The Discovery of Grounded Theory. Strategies for Qualitative Research*. London: Weindenfeld and Nicolson.

- Goffman, Erving (1974) *Frame Analysis*. New York: Harper and Row.
- Hughes, Everett C. (1971) *The Sociological Eye: Selected Papers on Work, Self and the Study of Society*. Book 2, Chicago: Aldine.
- Knorr-Cetina, Karin (1996). "Le "souci de soi" ou les "tâtonnements" : ethnographie de l'empirie dans deux disciplines scientifiques." *Sociologie du travail* 38 (3): 311-330.
- Le Noé Olivier (2002) "Apports et apories des statistiques sur la lecture. Formes de connaissance ou de reconnaissance de la lecture." *Sociétés et Représentations* 14: 249-273.
- Mauger, Gérard (1991) "Enquêter en milieu populaire." *Genèses* 6:125-143.
- Peneff, Jean (1988) "The observers observed: French survey researchers at work." *Social Problems* 35: 520-535.
- Pinçon, Michel and Pinçon-Charlot, Monique (1997) *Voyage en grande bourgeoisie. Journal d'enquête*. Paris: PUF.
- Roy, Donald (1952) "Quota Restriction and Goldbricking in a Machine Shop." *The American Journal of Sociology* LVII (5): 427-442.
- Rabinow, Paul (1977) "Reflections on Feldwork in Morocco." Berkeley: University of California Press.
- Rosen, Michael (1991) "Coming to Terms with the Field: Understanding and Doing Organizational Ethnography." *Journal of Management Studies* 28 (1): 1-24.
- Schwartz, Olivier (1993) "L'empirisme irréductible. La fin de l'empirisme?." Pp. 265-305 in *Le Hobo. Sociologie du sans-abri*. N. Anderson (1923). Paris: Nathan.
- Schwartzman, Helen B. (1993) *Ethnography in Organizations*. London: Sage Publications.
- Silverman David (2007) "Art and Artefact in Qualitative Research." *Improving the Quality of Qualitative Research Conference*, The European Science Foundation, Kristiansand, Norway, June 25-27, 2007 (unpublished communication).
- Strauss, Anselm L. (1992) *La Trame de la négociation. Sociologie qualitative et interactionnisme*. Paris: L'Harmattan.
- Taylor, Stephanie (ed.) (2002) *Ethnographic Research. A Reader*. London: Sage Publications.
- Van Maanen, John (1979) "The Fact of Fiction in Organizational Ethnography." *Administrative Science Quarterly* 24: 539-550.

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