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Subjectivity and Emotions as Sources of Insight in an Ethnographic Case Study: A Tale of the Field

Veronika Kisfalvi

HEC Montréal
Service de l'enseignement du management
eMail: veronika.kisfalvi@hec.ca

This article argues that case studies conducted within an ethnographic framework always contain an element of subjectivity and emotionality given the close relationships that researchers establish with participants in the field, and that while these elements can be a source of bias, they can also be transformed into valuable sources of insight as long as they are acknowledged and examined. Through the example of a lived field experience, this paper discusses how the subjective and emotional quality of the relationship established between researcher and participant, once examined, brought a deeper level of understanding and a greater degree of objectivity to findings obtained during an ethnographic case study carried out in an entrepreneurial firm. The methodological implications of the roles played by subjectivity and emotions in this type of research are also discussed.

INTRODUCTION

Organizational research using a case study design has enjoyed increasing popularity and legitimacy in the literature since the seminal work of Eisenhardt (1989) and Leonard-Barton (1990) in organizations and of Yin (1984) in qualitative methodology. A case study approach is often the method of choice when the researcher is interested in process issues, in exploring areas about which little is known, in theory building (Langley, 1999), and in ethnographic research. The results of such research, like those obtained through any other method, are reported in the form of articles published in scholarly journals. These articles usually follow a relatively fixed set of conventions in terms of structure and tone; the literature is reviewed, the theoretical groundwork is laid, the methodology outlined, and the findings presented and discussed; in most cases, this is done as impersonally and objectively as possible. Until recently, there have been few articles in which the emphasis is placed on exploring the lived experience of researchers involved in the actual process of gathering and treating the material which is ultimately used to identify the relevant data and to write up the results of such case studies for scholarly publication. The post-modern turn in the organizational literature at the end of the 1990s and into the new century has been a notable exception in that it has interjected

issues of perspective, voice, subjectivity, emotion and reflexivity into the methodological conversation (Calás and Smircich, 1999; Humphreys, Brown, and Hatch, 2003). This enlarged conversation, while quite recent and still somewhat marginal in the organizational literature, has had a longer and more prolific history and has moved closer to center stage in the sociological literature, where both the subjective nature of research and the necessity for reflexivity have been ongoing concerns starting as early as the 1970s (Irwin, 2006). In fact, according to Irwin, in the area of ethnographic studies in sociology, «To throw one's self into the field, body and soul, is now not only a valid stance, but marks investigatory excellence» (2006: 157).

Conducting research that brings —and is in fact intended to bring (Van Maanen, 1979, 2006; Lofland and Lofland, 1995; Irwin, 2006)— the researcher closely in contact with individuals in the field, can be an intensely personal experience and also one that can have important methodological implications. The aim of this article is to introduce these elements of ethnographic case study research into the context of organizational research, where they have been less reflected upon, and to explore what can happen around these issues in the field and how the researcher might wish to deal with them. To do so, it uses the author's own experience while carrying out empirical research in an entrepreneurial firm for her doctoral dissertation.

I argue that ethnographic case study research is an inherently subjective and emotionally charged method of inquiry given the sustained contact and the particular closeness that is developed between researchers and informants. While these elements can easily become a source of bias if they are defensively denied or otherwise unexamined, both subjectivity and emotions can also become valuable sources of insight if they are acknowledged and explored. This paper will discuss the potential role that the researcher's subjective, emotional reactions can play in bringing a deeper level of understanding and a greater degree of objectivity to findings obtained through such a research approach.

SUBJECTIVITY, ANXIETY, EMOTIONS: CONCEPTUAL UNDERPINNINGS

Some background of a conceptual nature is necessary here, dealing with the role of subjectivity and the seemingly inevitable presence of anxiety and emotions in research conducted in the behavioural sciences in general and when using ethnographic methods in particular. My primary source on subjectivity and anxiety (and to a lesser degree, emotions) is a work entitled *From Anxiety to Method in the Behavioural Sciences* by Georges Devereux (1967), anthropologist, psychoanalyst and ethno-psychoanalyst. Devereux (1967) attributes the source of subjectivity in part to cultural and sociological factors, but also in part to the researcher's personality and the past experiences that it reflects: «The scientist's personality is relevant to science, in that it accounts for that distortion of the material which is attributable to his intrapsychical-

ly determined lack of objectivity» (Devereux, 1967: 42-43). These subjective distortions are «especially marked where the observed material mobilizes anxiety» (1967: 43).

Devereux (1967) maintains that because the object of study is at once the other and the self (human beings are studying other human beings), the anxiety aroused by the inevitable confounding of self and other in this type of research is «the most significant and characteristic data of behavioural science research» (Devereux, 1967: xvii). Using examples from his own research, Devereux identifies some possible sources of anxiety in anthropological field work, and then extends these to the behavioural sciences in general:

On a more subjective level, anxiety is aroused by material which: 1/ threatens the basic vulnerability of any human being; 2/ revives idiosyncratic anxieties related to past experiences; 3/ threatens to undermine major defences or sublimations; 4/ exacerbates current problems, etc. (1967: 45).

He concludes that «[a]ny effective behavioural science methodology [... m]ust use the subjectivity inherent in all observation as the royal road to an authentic, rather than fictitious, objectivity» (Devereux, 1967: xvii). According to Devereux (1967), all research in the behavioural sciences (both qualitative and quantitative) has an element of subjectivity in it. In order to reduce the distortions caused by the researcher's own idiosyncratic subjectivity in this type of research, she or he must first and foremost acknowledge (rather than defensively deny) this subjectivity; then, particular attention must be paid to the moments of anxiety that arise during the research process (when interacting with others in the field, for example, or when analyzing data, or even when developing questions for a survey). Furthermore, Devereux links subjectivity, anxiety and emotions (or affect, as he terms it) and the importance of recognizing and taking them into account and in order to attain greater objectivity: «Objectivity results from the creative control of consciously recognized irrational reactions, *without loss of affect*» (Devereux, 1967: 100, emphasis in original).

Rather than advocating the removal of affect from research, Devereux criticizes the fact that «some behavioural scientists (...) dissociate themselves from their subjects and assume a more or less extra-human observer position [which] is a source of unconscious anxiety, giving rise to a variety of defences» and argues that «The resulting loss of feeling and the impairment of the (...) sense of one's own humanity would in themselves be sufficient reasons for avoiding aloofness, even if it were not obvious that the most productive way in which one can study man is through the medium of one's own humanity» (1967: 156).

Before addressing the question of subjectivity and emotions in my own research, I would like to now address the issue of terminology when discussing emotions, as well as the issue of the usefulness and positive contribution of emotions to research and particularly to ethnographic approaches that take place in social settings. I have thus far used the words emotions, feelings and affect almost interchangeably, and would now like to clarify their usage for the rest of this article. I will

use the work of Antonio Damasio (1994, 1999) —a clinical neurologist and neuroscientist researching the role of emotions and the body in rational decision-making and in consciousness— in order to address both the issue of terminology and the importance of paying attention to emotions when conducting ethnographic research.

Damasio maintains that while learning and culture can alter the expression of emotions and give them new meanings, emotions themselves are «biologically determined processes, depending on innately set brain devices, laid down by a long evolutionary history» (1994: 51). Furthermore, he proposes the following working definitions: «An emotion, be it happiness or sadness, embarrassment or pride, is a patterned collection of chemical and neural responses that is produced by the brain when it detects the presence of an emotionally competent stimulus —an object or situation, for example. The processing of the stimulus may be conscious but it need not be, as the responses are engendered automatically. A working definition of feelings is a different matter. Feelings are the mental representation of the physiological changes that characterize emotions. Unlike emotions, which are scientifically public, feelings are indeed private» (Damasio, 2001: 781-782).

Damasio links the feeling of emotions to the bodily sensations accompanying them and to the image of the body produced in the brain: «a feeling depends on the juxtaposition of an image of the body proper to an image of something else, such as the visual image of a face or the auditory image of a melody (...) To feel an emotion it is necessary (...) that neural signals from viscera, from muscles and joints, and from neurotransmitter nuclei —all of which are activated during the process of emotion— reach certain subcortical nuclei and the cerebral cortex» (1994: 145).

He proposes that «the term *feeling* should be reserved for the private, mental experience of an emotion, while the term *emotion* should be used to designate the collection of [bodily] responses, many of which are publicly observable» (Damasio, 1999: 42, italics in original). He also explicitly connects feelings to consciousness: «To the simple definition of emotion as a specifically caused transient change of the organism state corresponds a simple definition for feeling an emotion: It is the representation of that transient change in organism state in terms of neural patterns and ensuing images. When those images are accompanied, one instant later, by a sense of self in the act of knowing, and when they are enhanced, they become conscious. They are, in a true sense, feelings of feelings» (Damasio, 1999: 282).

According to Damasio «Feelings let us *mind the body* (...) They let us mind the body “live,” when they give us perceptual images of the body, or “by rebroadcast,” when they give us recalled images of the body state appropriate to certain circumstances [that we are currently experiencing]» (1994: 159, italics in original).

In addition, «Feelings offer us a glimpse of what goes on in our flesh, as a momentary image of that flesh is juxtaposed to the images of other objects and situations; in so doing, feelings modify our comprehensive notion of those other objects and situations. By dint of juxta-

position, body images give to other images a quality of goodness or badness, of pleasure and pain» (Damasio, 1994: 159). Furthermore, «feeling your emotional states, which is to say being conscious of emotions, offers you flexibility of response based on the particular history of your interactions with the environment» (1994: 133). For Damasio, then, emotions are automatic and visceral reactions that have the potential to become the conscious feelings that help us evaluate, make meaning of and navigate through our experiences. I will use his terminology in the rest of this article to refer to emotions as unconscious or preconscious mind/body reactions and to feelings as emotions made conscious and available for examination to the subject as he or she experiences situations.

It is also clear from Damasio's work that feelings are ubiquitous in healthy humans and are essential for self-preservation and self-awareness; in this sense, having feelings is by itself obviously useful. But beyond their value in these processes, Damasio (1994: 245) also suggests that «feelings are a powerful influence on reason [... T]he brain systems required by the former are enmeshed in those needed by the latter, and (...) such specific systems are interwoven with those that regulate the body» ; furthermore, «emotions and feelings may not be the intruders in the bastion of reason at all: they may be enmeshed in its networks, for worse and for better» (1994: xii).

Echoing Devereux's (1967) contention about the importance of considering feelings (or affect in Devereux's terminology) as a way toward a more holistic and less biased science, Damasio (1994: 246) suggests that «taking stock of the pervasive role of feelings may give us a chance of enhancing their positive effects and reducing their potential harm». For him, far from being a luxury or an epiphenomenon, feelings and emotions «serve as internal guides, and they help us communicate to others signals that can also guide them (...) They are the result of a most curious physiological arrangement that has turned the brain into the body's captive audience» (1994: xv).

Calling on evidence gathered during his and others' clinical work with patients suffering damage to the brain regions involved in the feeling of emotions, Damasio (1994: 54) concludes that when there is damage to these regions, «[t]he powers of reason and the experience of emotion decline together». While it may be surprising given our long-held bias toward reason and the widespread belief that feelings hinder objectivity and hence rationality, «certain aspects of the process of emotion and feeling are indispensable for rationality» (Damasio, 1994: xiii). Damasio's clinical evidence reveals that «selective reduction of emotion is at least as prejudicial for rationality as excessive emotion. It certainly does not seem true that reason stands to gain from operating without the leverage of emotion. On the contrary, emotion probably assists reasoning, especially when it comes to personal and social matters involving risk and conflict» (1999: 41-42). This seems to be particularly the case when it comes to evaluating and appropriately responding to social situations (which characterize both organisations and the ethnographic research carried out in them). «The smooth integration of this process is what allows us to make social judgments

leading to the decisions that in turn make us into effective social beings» (Damasio, 1994: 38). Damasio's contention that «[w]ell-targeted and well-deployed emotion seems to be a support system without which the edifice of reason cannot operate properly» (1999: 42) is a major cornerstone of the current wave of research in the area of emotional intelligence, increasingly seen as an important and very desirable skill to possess in the workplace (Goleman, 1995).

Taken together, Devereux's and Damasio's positions provide strong support for the necessity to seriously reflect upon the subjective experiences, anxieties and other feelings that accompany any research with an ethnographic orientation (and perhaps all research in the behavioural sciences, as Devereux [1967] has suggested). Such a reflection holds out the possibility of making us into more aware, more transparent and less biased researchers. It offers a tool with which to enhance our understanding of our experiences in the field and of the data we thus obtain as well as of the relationships we form with our subjects, and to reflect upon the impact of all of this on our findings, analyses and ultimate theorizing.

To sum up, the ethnographic research process in particular seems inevitably fraught with issues of subjectivity, anxiety and emotions, although these disturbances can often be denied or obscured by a methodological pseudo-objectivity (Devereux, 1967). We have everything to gain from not denying but rather addressing these aspects consciously and from using them as yet more data (albeit of a different nature) that can inform our subsequent understandings of the phenomena we are studying. Devereux argued, as early as 1967, that «[t]he next objective in behavioural science research must therefore be the reintroduction of affect into research» (1967: 156). What follows, then, may be seen in part as a response to this call. In the rest of this article, I explore the subjectivity inherent in my research project as well as the anxiety and some of the other feelings that the project itself and the relationship I formed with the main subject of my inquiry aroused in me during the fieldwork and the subsequent data analysis and writing.

THE STUDY

The research study that is described in this paper, conducted during my doctoral studies, forms the basis of several subsequent articles (Kisfalvi, 2000, 2002) that follow the standard presentation protocol for journal articles such as the ones described in the introduction here. I present here what lay behind the scenes of these articles, the actual lived experience of one researcher in the process of conducting an ethnographically informed single-case study that primarily focused on one individual. In this respect, this paper is an example of what Van Maanen (1988) has referred to as a confessional tale of the field.

The study in question concerned strategic leadership and in particular the role of emotions and character in strategic decision-making. It focused on one entrepreneur and his character, the business he had

built up, and the interactions between this person and his top management team. The conceptual framework was psychoanalytic in nature. Psychoanalytic approaches can be distinguished from the more traditional and essentially cognitive approaches in the area of strategic leadership in that many of the latter consist of synchronic correlation studies. These cognitive studies have been heavily influenced by cybernetic models (Stubbart, 1987) that can address neither the feeling aspects of choice (Langley, Mintzberg, Pitcher, Posada, and Saint-Macary, 1995) nor the personal meaning with which a top manager might imbue a particular strategic option. While the sensemaking literature (spearheaded by Weick, 1995) comes closer, it too is dominated by a cognitive bias and leaves out considerations of personal history and emotions. As such, I considered these approaches poorly suited to the task of exploring the more subjective elements involved in strategic decision-making, which were the focus of my research study. A more holistic and ethnographic approach was called for, one that could look at how strategists' past histories might affect their present lives, and whose present actions likely resonate with deep personal meaning that is not always consciously apprehended by the strategist.

The psychoanalytic conceptual framework is particularly suited to exploring personal, emotional and biographical factors. The psychoanalytic literature suggests that the cognitive processes involved in strategic decision-making are intricately interwoven with the strategist's emotional experience and personal history (a position that Damasio's [1994, 1999] work in the area of neuroscience seems to support). It considers thinking, feeling and acting (which constitute behaviour [Moore and Fine, 1990]) as integrated and deeply rooted in past formative experiences. In addition, while much of the strategy literature treats strategic issues as if they were emotionally neutral and atemporal, certain issues are inevitably highly meaningful and emotionally charged hot issues for a particular strategist because of past formative experiences (Noël 1989; Kets de Vries, Miller, and Noël, 1993). The psychoanalytic literature, on the other hand, recognizes the importance and influence of past experiences and associated emotions on current behaviours. Further description of this somewhat unconventional conceptual approach in organizational research and the literature that it has inspired can be found in Kisfalvi (2000, 2002) and in Kisfalvi and Pitcher (2003). The reflections developed in the present article (like the published work mentioned above) are inspired by concepts drawn from the psychoanalytic literature; as such, the subjective, personal and emotional aspects of my research experience —reflecting also the idea that in ethnography «our past is present in us as a project» (Fabian, 1983, cited in Humphreys et al., 2003: 9)— are the ones emphasized.

Methodologically, the study I conducted can be described as ethnographic in nature; it consisted of a single-case design and relied primarily on interviews with the entrepreneur and with the members of his team, on non-participant observation and on consultation of archival material for data gathering. My fieldwork was carried out on site over

a nine-month period during which time I developed a relationship with my principal research subject that on examination turned out to be quite complex, and that was quite different from the objective relationships between researcher and research subject that tends to be reported in journal articles.

In what follows, and in keeping within the psychoanalytic paradigm of both this article and the research study itself, I will concentrate on two particular aspects of my subjective and emotional experience which accompanied the carrying out of the study: my anxieties and their causes and the sadness (associated with mourning) that was evoked at various points during the research experience. Although there was some overlap, feelings of anxiety were most prevalent during the data collection phase, that is, during the entry into the field and the fieldwork conducted on site, while the sadness was most prevalent upon leaving the field and during the data analysis and writing phases. Sources of anxiety included the difficulties of finding a site and a principal research subject given the nature of the research question, which had to do with business strategy and character; the particular history of the research subject, as it related to that of the researcher (he was a Holocaust survivor, while I am the daughter of survivors); and the particular dynamics of the interaction between the personalities of the research subject and the researcher. The sadness associated with the process of mourning, on the other hand, was related more to the transitions between different phases of the research, each with their particular characteristics.

SOURCES OF ANXIETY DURING DATA COLLECTION

Anxiety was never very far during the beginning phases of my study and during the entire data collection period, although its sources were often quite distinct. Unravelling and understanding the sources of this anxiety became crucial for my understanding of my research experience, and particularly when it came time to interpret the findings.

THE NATURE OF THE RESEARCH QUESTION

The first source of anxiety had to do with the difficulties of finding a research site, given the nature of my research question. Repeatedly, and perhaps quite understandably, when my potential subjects heard me link the words “the CEO’s character” and “business strategy”, they would immediately decline my request to use their firm as my research site. Ben Levitsky¹ himself initially responded to my request by pushing the desk chair in which he was sitting further and further away from me during our first meeting, communicating nonverbally but unmistakably the reticence that he was feeling. When he finally accepted to become my research subject, I was extremely relieved and grateful, but also extremely aware of how difficult it would be to find another site in case this one did not work out. As a result, I became inordinately

1. A pseudonym.

concerned about not making a false step, and hypersensitive to the quality of my relationship with Ben. This hypersensitivity, while difficult to live with day-to-day in the field, was ultimately helpful in my ability to subsequently reflect on the nature of this relationship and its impact on my findings.

My concerns about the relationship were intensified by the fact that even for me, my research question seemed somewhat indiscreet, and made me feel like I was prying, an aspect of case study research to which, by nature, I am particularly sensitive. The voyeuristic element of doing case research on an individual was therefore intensified for me, and further highlighted the tenuousness of my relationship with Ben. The importance that the seeming fragility of this relationship had assumed for me from the very beginning of the study would clearly have consequences for the kind of data that I would be able (and willing) to acquire. I believe that my feelings of tenuousness and my anxiety about the appropriateness of my study somewhat curtailed the types of questions that I felt free to ask, and therefore formed the first boundary or limitation of my research. This limitation was somewhat attenuated when about half-way into the study I decided to search for, and eventually found, a second site which could have served as back-up if needed.

THE NATURE OF THE RESEARCH SUBJECT

WHO BEN WAS FOR ME...

Ben was from Eastern Europe, easily identifiable as an orthodox Jew through his dress and a survivor of the concentration camps; while the conditions he endured were terrible, he also told me that he had learned to become a businessman in the camps, as it was the only way to survive. I myself come from a Hungarian Jewish family, and both my father and grandfather had been interned in, and survived, Nazi labour camps during the war. Not only had I been personally affected by the Holocaust through my family's experiences, I had become intensely interested in this moment of history; in addition, at the time of the study, I was feeling it increasingly important to openly affirm my Jewishness. Ben was in his mid seventies when I met him, and because of his war experiences and his age, I could not help but see in him some of my grandfather, with whom I had been very close as a child (what, in psychoanalytic terms, may be described as a typical transference reaction: see Devereux [1967] and Hunt [1989]). I was therefore favourably disposed toward him even before starting the fieldwork, although as it turned out he was not an easily approachable person. Ruthless in business, emotionally volatile and prone to anger, stern, but also highly intelligent and perceptive when it came to people, he presented a daunting combination both as an individual and as a research subject. I wanted desperately to be able to like him nonetheless: «I want to like Ben,» says one of my journal entries, «all the Holocaust stuff, suffering (...) His overcoming victimization, asserting his "thereness" like I feel the need to». And I was terrified, in this case on a more personal, emotional and not yet fully conscious level, of being rejected by him,

as is evidenced by some of the dreams I had during the time I was conducting the research. (Devereux [1967: 54] himself suggested that researchers use their dreams as a way to make more conscious and to understand their anxiety reactions. Listening to the body constitutes another way, as is implied by Damasio's [1994, 1999] work, since such anxieties are experienced in the body [Damasio, 1994, 1999; LeDoux, 1996]; when we perpetrate what Damasio [1994] has called Descartes' error, the splitting of the body from the mind in the quest for rationality, we lose the insights to which our physical reactions to situations can lead us [Damasio, 1994]).

The following is an account of two of my dreams that occurred close together during the fieldwork and that I noted in my field journals.

Dream 1: «A dream sometime over the long weekend: I am at a meal with Ben and a number of other people. It is very relaxed. I feel a part of things. I suspect this is a Passover seder [ritual meal], since one of the fantasies I had was to be invited to his house or his son's for the first seder (...) the idea of breaking through, of belonging, of being a part of his life (...) and of feeling comfortable, relaxed, normal around this person. A wish that others share?»

Dream 2: «Again, the sense of well-being, of things going well, and then going a bit too far —too intimate— I touch Ben's hands in the dream, and he violently recoils and yells at me "I don't want to be touched! Don't touch me!" (...) Whose dream is this? (...) Getting too close, hurting, to be violently rejected. Breaking through the anxiety — how do we do it? How do I? How does my subject? Is there relief on the other side?»

This fear of rejection was transformed into a constant anxiety that I would do the wrong thing and that Ben would ask me to stop my research and leave, one that dovetailed neatly with the other anxieties I have described above. The anxiety got so intense that at times I would literally feel immense relief at not seeing Ben's car in the parking lot when I arrived at the research site in the mornings. The irony of this was not lost on me, as is evidenced by the following excerpt from my field notes: «Ben is in —I see his car in his spot. I get out of my car with a sinking feeling (...) I feel strongly that I'm happier and much more at ease, that my research goes "better" when he is out of the office —wonderful, since he's my research focus!»

To put it mildly, Ben simply terrified me. While I could have attempted to ignore this feeling as not objective and therefore as having no place in the research (after all, it was "just an emotional reaction"), I decided not to do so. My reasoning was that if after being exposed to him for only a short while I felt scared, fragile and tenuous around Ben, it was possible that his managers experienced similar reactions. So one day, when I was speaking to Ben's assistant, I told him that Ben terrified me. His response: «Oh yeah? You're not the only one!» This exchange subsequently led to a whole new line of inquiry dealing with Ben's emotional impact on his managers and how they coped with it, and ultimately provided me with a fuller understanding of the impact of Ben's personality and personal history on top management team dynamics and strategy making processes in the firm. Had I decided to

discount my own feelings because they interfered with my objectivity, the initial conversation with the manager could never have taken place.

It is clear that for me, Ben represented something that went beyond that of a mere research subject (if such a thing actually exists) with whom I could maintain a certain distance and scientific objectivity. But I was not the only one to be anxious; I, too, represented something for Ben that I believe in turn elicited a certain amount of anxiety on his part.

WHO I WAS FOR BEN...

On a number of occasions, Ben made reference to his lack of formal education and the fact that he came from a simple family, and to his pride in accomplishing as much as he had in his life despite his humble origins. For example, he underscored the fact that he had succeeded despite his lack of formal education, and that back in Europe, he had held his own «with doctor chemists (...) They came up with ideas, but how to actually carry them out in a practical way, how to put them to work, they were unable. This was my job». He spoke with disdain of these people who, unlike him, were highly educated but so impractical: «I went to a kind of technical school [and not to a college or university]. More in practice (...) because in practice you should be able to avoid accidents and things like that». His emphasis of these issues, coupled with certain references to his mother who had been one of the more educated women in his village, and to his sister who had married a highly-educated man, left the impression that Ben might be sensitive about the level of education he had been able to achieve.

Faced with someone like the researcher, a doctoral candidate at the time, these anxieties about his level of education may well have been brought to the forefront. For example, as we were setting up our first interview, he asked «How long will you want to talk to me?» When I told him that it would be between one and two hours, and that «actually, it's you who will be doing most of the talking», he seemed surprised and slightly taken aback: «Oh, you want me to talk to you?» One incident in particular made me aware that my level of education could be troubling to him. It is reproduced below from my field notes.

«Ben: Are you doing work about us or for yourself now?»

«Veronika: About you!»

«Ben (pointing to the computer): No, you are working for yourself on your computer.

«Veronika: I'm working on my notes from here. Do you want me to do some work for you? Give me some work to do, and I'll happily do it.

«Ben (smiling): You want work? I will ask you to cook for me!»

«Order clerk: She's a good cook!»

«Veronika (thinking of the daunting task of cooking kosher food): I don't know...

«Ben (to no-one in particular, walking away): She doesn't even know if she's a good cook...»

Here I was, a highly educated doctoral student, yet like those «doctor chemists» back in Europe, unable to do practical things like cooking!

Had this been an isolated incident, I would have attributed my own anxiety about my inability to cook kosher food to my insecurities in the field-work situation ("I'm not good enough..."). But if we look at this incident in light of the satisfaction he seems to have gotten out of being more practical than those impractical «doctor chemists», a pattern seems to emerge, centered around his insecurities about his level of education, and a need to show up those who are better educated than he is.

Thus, we were both feeling anxious, insecure and a little exposed, but for quite different reasons. And in fact, we were both observing each other. For example, after my first formal interview with Ben, as I was driving home, I suddenly realized with a sinking feeling that he had actually found out as much, if not more, about my personal life and my life history than I had about his. Incidents such as these led me to conclude in my field notes that «Ben must be somewhat uncomfortable and anxious to know that I am doing what he also does (...) watching minutely the workings of his company, the movements and actions of his people, and of course, himself». These incidents also confirmed what I had been told about him —that he was extremely good at ferreting out information from people. In this sense, some aspects of the relationship I had formed with him were prototypic of the relationships he formed with his managers, and I thus treated them as data.

What Ben and I were feeling, I believe, was a sort of double or mirror anxiety, one that created a certain level of confusion in me, causing me to ask myself whose anxiety it was that I was feeling (or as I put it in my field notes, whose dream I was dreaming), his or my own. From my field notes: «After a while it is hard to tell if all of this is a major projection —transference on the researcher's [my] part, or really based on "observation"». Working within a psychoanalytic framework, I knew that in field research it was not at all unusual to confound one's own feelings for those of one's subjects and vice versa (Devereux, 1967; Hunt, 1989), and that I personally, even in day-to-day life, had a marked tendency to feel other people's feelings —a facility to fall into what psychoanalytic authors such as Devereux (1967) and Heimann (1950) would describe as counter-transference reactions. I also knew I had to make a special effort to try to unravel the sources of this anxiety (to own my own, but only my own). As social psychologist Alain Giami (2001: 6) in a recent reworking of Devereux's theories states: «A researcher's counter-transference can be defined as the sum of unconscious and emotional reactions, including anxiety, affecting his/her relation with the observed subject and situation. These reactions produce distortions in the process of knowledge construction that remain hidden from the researcher. Notions of "inappropriateness" and "resistance" (...) become central in understanding the cognitive processes affecting the researcher, because they highlight the researcher's reactions to aspects of reality emerging in fieldwork. Counter-transference points to the researcher's difficulty in clearly distinguishing material that comes from outside (the subject, the field) and from inside (his/her own emotional reactions). The researcher has to struggle with these emotional reactions and anxieties».

The attempts to tease apart my own anxieties and emotions from those of my research subject allowed me to become more objective, and to achieve a better understanding of my subject based on the feelings that he aroused in me (in fact, counter-transference reactions are often used by therapists in clinical psychoanalytic settings for just this purpose: Heimann, 1950; Giarni, 2001). In the end, I gained a greater understanding of my subject and myself and the consequences of our interaction for the research process, and confirmed what I had jotted down in my field notes about halfway through the study: «Everything is data».

SADNESS AND MOURNING DURING DATA ANALYSIS

One might think that given the emotionally challenging nature of the fieldwork, I would have felt great relief at the prospect of leaving the field, and this was what in fact did happen, but only in part. At the same time, it was difficult to leave the richness of the field for the more austere and emotionally neutral work of data coding. In addition, the anxiety that surrounded the data gathering during this study compounded the qualitative researcher's common anxiety about leaving the field too soon, before enough data had been gathered. While textbooks on qualitative research attempt to deal with this concern by advising that the researcher continue until a point of saturation is reached, it often remains unclear to the researcher what this saturation would actually look (or feel) like. I know that in my case, not only was I sure that I could have gone farther and been more probing with my questions to Ben had I felt somewhat less inhibited, I also could not rid myself of the thought that just one more day, one more interview, one more good contact with Ben or with another informant would give me the definitive piece of data that I felt I must still be missing to complete my study.

Mourning the fieldwork thus consisted in dealing not only with the sense of sadness and loss that I felt saying goodbye to the people with whom I had established sometimes intense connections, as well as to the day-to-day concrete organizational reality; I had also, and perhaps more importantly, to deal with the feelings of anxiety associated with the sense that I was leaving the field without having all of the data I would have hoped to have. Mourning at this phase of the research therefore consisted of coming to accept that I had conducted a less-than-perfect study, and its logical corollary, that I was a less-than-perfect researcher. The realization that although not complete, my data was adequate, and that although not perfect, I was a good enough (to use Winnicott's [1965] expression) researcher, came later during the coding and analysis phases and represented the resolution of this particular phase of mourning.

Once I began to actually code my data, I entered a phase that, on an emotional level, was almost the diametrical opposite of being in the field. Whereas data gathering had often been accompanied by intense anxiety, the neutral, even cold, nature of data coding was its very

antithesis. This inherent characteristic of the coding process is particularly enhanced by the computerized tools now available to the qualitative researcher, in the form of qualitative data analysis software such as NUD*IST, the software package that I used for my data coding, or NVivo, a more recent version. The relative facility with which coding can be carried out with the help of such software encourages the researcher to develop a very extensive database containing a large number of codes and categories. In fact, it can be hard to stop.

I myself compulsively coded data for several months, deciding on and reworking the code or category to which a particular word or a phrase best belonged, or whether a new code needed to be created for it. This compulsive, seemingly more objective and quite emotionless activity was not only a welcome refuge from the difficult emotions and feelings encountered in the field, it was also in a very real sense a mask. First, because the software allowed me to generate so many codes and categories, it served to reassure me of the richness of my data and to allay my anxiety about not having enough. Then, precisely because it was such an emotionally neutral refuge, it effectively cut me off from my own feelings (self-estrangement masking itself as scientific objectivity, as both Devereux [1967] and Kleinman and Copp [1993] would put it), and consequently from the meaning of my data, fragmenting it into hundreds of disconnected chunks of information that remained meaningless on their own, waiting to be reshaped and recombined into the story I would later tell.

Only I found that I could not tell it. After I had coded data ad nauseam for months and felt that I had reached the point of diminishing returns, I literally could not write a sentence for three full weeks. In allowing me to submerge (but thankfully, as it turned out, not entirely drown) my emotions about my subject in a sea of codes and categories, the data coding had, ironically, cut me off from Ben's reality and from access to the story my data could tell, the meaning that it held. I could not write about Ben until I could feel his reality once again. In effect, the impression of being more in control and more professional that electronically coding and categorizing my data had given me now had to be abandoned, at least to a degree, another aspect of the research process that had to be mourned.

I vividly remember sitting in my office one afternoon in front of my computer, frustrated to no end by my inability to write, and I found myself thinking about Ben and his life. As I did so, he began to emerge for me once again, not as data, but as a real person with a past, someone with whom I had forged a real, albeit complex and often problematic and difficult relationship; I began once again to feel for him the mixed feelings I had felt in the field: the empathy for his hard life and terrible experiences, the admiration for his accomplishments, the fear of his moods, the anxiety that given the context, his power over me had generated, etc. But mainly, I felt a deep sadness for this man whose life experiences had made him into the complex and difficult individual he had become, a person with whom I (like his managers) had found it so hard to establish a caring relationship, and I began to cry. I believe that in that moment, in which I had once again gained access to the emo-

tions that the fieldwork and Ben had aroused in me and in which rather than suppressing them, I allowed these emotions to become conscious feelings, I began to truly mourn all that could have been, both in Ben's life and in the personal and professional relationship that I had developed with him during the research project. The next day I began to write, and once I started, there was no stopping it. I wrote 200 pages of what largely amounted to gibberish that needed to be severely edited for an academic audience, but a text that nonetheless contained the germ of my thesis as well as the essential elements of my subsequent articles.

SOME FINAL REFLEXIONS

Traditional research orthodoxy would have it that feelings and emotions have no place in research, and that as researchers we should be very wary of our emotional reactions lest they make us stray from the path of objectivity. Such advice can easily incite us to suppress our emotional reactions to our research subjects entirely, or in more psychoanalytic terms, to repress these reactions or to deny their existence. Such repression and denial, however, may not make us more objective but perhaps just the reverse; although we can drive these reactions underground, the risk is that they might emerge in more indirect ways that are perhaps harder to pinpoint and therefore even more likely to distort our perceptions and so pose an even greater threat to the very objectivity for which we are striving.

It is true that my emotional reactions and subsequent feelings affected my findings. But it was by confronting and trying to understand these and by attempting to examine precisely in which ways they affected my perceptions and reactions that I could arrive at a greater objectivity, as well as a better understanding of both my subject and my chosen methods. Had I not examined the sources of my own anxiety —had I not asked myself who Ben was for me— would I have dismissed the unlikeable and difficult aspects of Ben's personality that not only I but also his managers found hard to deal with? Had I not examined Ben's anxiety about my presence —had I not asked who I was for him— would I have unearthed his intense need for recognition, rooted in his desire to prove to himself and to others that he was at least the equal of those much better educated than he? Had I considered my emotionless data coding and analysis to be a triumph of objectivity, would my paralysis have lasted longer? And had I not allowed myself to feel Ben's reality again in an attempt to maintain this supposed objectivity, could I have written anything at all? And if I did, might I have missed the flavour of Ben's personality entirely?

In addition, it was precisely because of who I was, with its consequences for the relationship that formed with Ben —what he represented for me, and I for him— that I was able to access certain types of data about my subject. Another researcher, one who was perhaps more self-confident and less worried about being intrusive, someone who was not the child of Holocaust survivors —a researcher with a dif-

ferent personality, a different set of past experiences and subjective issues— would have in all likelihood developed a different kind of relationship with the subject, would have had access to somewhat different data as a consequence, and would perhaps have produced quite a different—and differently partial— study. In either case, generalizing would have been problematic.

This last observation raises interesting ontological questions concerning the ability of ethnographic research (and qualitative research in general) to objectively depict the world, and underscores the importance of a serious reflection on the consequence of our chosen methodological approach. It implies that wherever possible, such research should be carried out using multiple informants who can corroborate and enlarge each other's views and by at least two researchers working closely together to compare and enrich their perceptions (for an example of using more than one researcher as a way to work through counter-transference reactions, see Giami, 2001). It does not imply, however, that an individual researcher working with an individual subject is inevitably too subjective or wrong. Even when conducted by an individual alone, such research holds a mirror to a particular facet of reality, and will always yield a partial truth based on the particularities of the relationship developed between these two partners in the research. Methodological rigor might mean in this case a commitment on the part of the researcher to fully account for how this partial truth has been arrived at, and to communicate the process to the scientific community. As Devereux (1967) proposed, the royal road to such rigor might very well consist of a thorough recording and examination of the subjective and emotional aspects of the fieldwork. Such reflexivity can also keep ethnographic researchers honest by helping them avoid confounding the boundary that separates their experience from their subjects' or establishing a potentially damaging intimacy with them (Irwin, 2006). As Kleinman and Copp (1993: 13) have said, «we must consider who we are and what we believe [and I would add feel] when we do fieldwork. Otherwise we might not see how we shape the story».

CONCLUSION

This article has underscored the need to unravel and to understand the subjectivity, anxiety, emotional reactions and feelings that are experienced by researchers who conduct ethnographic case study research. I do not claim for a moment that my own particular idiosyncratic reactions can be generalized to other researchers and other field relationships; the difficulty of generalizing from this type of research is in fact one of the points I have tried to raise in this article. Nor do I feel a great need to generalize; as Van Maanen (2006: 18) has recently pointed out, «ethnography maintains an almost obsessive focus on the “empirical”. The witnessing ideal with its intense reliance on personalized seeing, hearing, experiencing in specific social settings continues to generate something of a hostility to generalizations and abstractions

not connected to immersion in situated detail»; and it is a stance he considers legitimate in this type of research. But if pushed to do so, I would maintain that ethnographic research in general, which often focuses on cultural and social settings, and ethnographic case study research in particular, which almost always aims to get up close to individuals and social situations, seems to be inherently subjective and emotional in nature, although as organizational researchers we may be hesitant to admit this for fear of damaging our credibility (Kleinman and Copp, 1993). It is not a question of choice, as some of the more controversial field practices (such as sexual intimacy with informants, drug use and the like) described by Irwin (2006) seem to be, but rather it is the nature of the beast — and perhaps the nature of research in the behavioural sciences in general, as Devereux (1967) proposed.

Whatever our reticence, as researchers we must consciously acknowledge and examine these aspects of ethnographic case study research. If we do not do so but simply repress them or sweep them under the rug, we risk unconsciously compromising our objectivity and separating ourselves from the meaning of our data. We also risk missing occasions to arrive at a deeper understanding not only of our data, our subjects and ourselves as researchers, but of the impact of our chosen methods on the types of data that we will be able to gather. It has been said that when doing ethnography, the researcher becomes the primary research instrument (Humphreys et al., 2003). As such, adjusting for our own disturbing reactions (Devereux, 1967) as researchers forms a part of the calibration of that instrument; an acknowledgement and examination of these reactions can only improve the ability of the instrument to do its job, and by extension, improve the quality of our research results.

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Veronika Kisfalvi (Ph.D., McGill University) is an associate professor at HEC Montréal, where she has been teaching courses on leadership, management skills and decision making since 1986. Her research focuses on the relationships between personality, emotions and strategic decision making in top management teams. She has given executive seminars in Switzerland, Holland, Tunisia, West Africa, Bulgaria, Trinidad, Romania and the USA. Prof. Kisfalvi currently holds the position of Director of the Graduate Diploma Program at HEC.

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