Article

On the Track of C/overt Research: Lessons From Taking Ethnographic Ethics to the Extreme

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Tereza Virtová[1], Tereza Stöckelová[2], and Helena Krásná[3]

Abstract

Despite the growing body of literature that critically assesses the ambiguous impacts of institutional review boards (IRBs) on anthropological research, the key standards on which the IRB evaluations are based often remain unquestioned. By exposing the genealogy of an undercover research in which the authors participated as ethnographer, supervisor, and research participant, this article problematizes some of these standards and addresses the issues of power dynamics in research, informed consent, and anonymization in published work. It argues that rather than addressing genuine ethical dilemmas, IRB standards and the ethical fiction of informed consent mainly protect researchers from having to openly face the uncertainties of fieldwork. As an alternative, the authors put forth the notion of c/overt research, which perceives any research as processual and, in effect, becoming overt only during the research process itself. As such, it forces researchers to cultivate sensitivity to research ethics.

Keywords

collaborative ethnography, informed consent, institutional review board (IRB), research ethics, undercover research

Covert research is hard to defend in social science today. After projects such as the Tuskegee Syphilis Experiment on impoverished African Americans, which was a display of scientific arrogance, covert research became unacceptable within academic communities. A few known "classical" covert research projects led to heated critical debates and sometimes even to fistfights between researchers, their colleagues, and the research participants (e.g., Humphreys, 1970; Nathan, 2005b; Scheper-Hughes, 2004). Professional codes of research ethics (e.g., American Anthropological Association [AAA], 1971, 2012) have explicitly opposed covert research as a form of deception. Also institutional review boards (IRBs) that have been regulating research practice in Anglo-American contexts since late 1970s largely disapprove of any elements of covertness in research proposals as irreconcilable with the principle of informed consent (Christians, 2005).[1] Moreover, the critique of IRB, which has been increasingly vocal and has recently led to a decision in the United States to exempt most social science research from IRB approvals,[2] opposes any form of deception and covertness in even more principled terms. Covert research finds itself in fundamental conflict with collaborative and community-based research designs that these critiques advocate (Christians, 2005). If

> participants have a co-equal say in how research should be conducted, what should be studies, which methods should be used, which findings are valid and acceptable, how that findings are to be implanted and how the consequences of such actions are to be accessed (Denzin, 2003, p. 257, cited in Christians, 2005, p. 157),

it follows they have to be fully included in the research process at all stages.

At the same time, a number of researchers have noted that the distinction between covered and overt research is fluid rather than sharp and absolute (e.g., Herrera, 1999). Lugosi (2006) discusses the dynamics of concealment and disclosure in his ethnographic study of a suburban bar to argue that it is neither possible nor desirable to achieve complete overtness in ethnographic research and "the criticisms levelled against covert methods should not stop the fieldworker from engaging in research that involves covertness" (Lugosi, 2006, p. 541). Given their principled opposition to deception and covertness, standard codes of research ethics and IRB

[1]Institute of Philosophy, Czech Academy of Sciences, Prague, Czech Republic
[2]Institute of Sociology, Czech Academy of Sciences, Prague, Czech Republic
[3]Prague, Czech Republic

Corresponding Author:
Tereza Virtová, Institute of Philosophy, Czech Academy of Sciences, Jilska 1, 11000 Prague, Czech Republic.
Email: virtova@flu.cas.cz

procedures provide limited practical guidance in terms of how to deal with elements of concealment in a research study, and they tend to shortcut rather than unpack many ethically troubling issues. In their study on access in ethnographic research, Alcadipani and Hodgson (2009) argue that ethical codes and guidelines allow researchers to avoid addressing ethically tense situations in the field. Recounting a fieldwork carried out in a newspaper printing site, they emphasize that "in many situations ethical guidelines provided an excuse to withhold information in order to keep good field relations and maintain research access, and provided a rationale for disregarding both personal moral misgivings and critical research commitments" (Alcadipani & Hodgson, 2009, p. 138).

In this article, we offer for discussion a genealogy of a covert research we participated in as an ethnographer (Author 1), a supervisor (Author 2), and a research participant (Author 3). We do not propose any straightforward defense of covert research, not least because we are still troubled by our own experience of it. However, we wouldn't spurn it on the basis of being unethical, which is the case in ethical guidelines; on the contrary, we see it as hyper-ethical, unbearably ethical at some moments. The key motivation for writing this article was our assumption that we can take important lessons from covert research for regular research projects with the informed consent of participants. It is precisely due to the ethical precariousness of covert research that it poses salient questions that are not usually asked in supposedly overt research or that are only "solved" by reference to ethical guidelines and codes of ethics, not by thoroughly exploring such questions in a manner appropriate to a particular research situation. We must also stress from the outset that our research was not strictly covert. More accurately, it gradually shifted from being covert to overt as it was gradually revealed to some research participants—resulting, finally, in one of the participants becoming a coauthor of this article. In this text, we highlight and discuss this process and its ambiguities by proposing a notion of "c/overt research." Following Lugosi (2006), we also want to stress that all research is to an extent c/overt. As regular overt research evolves into its final form in the actual course of the research, neither researchers nor the researched know in advance what will and may happen. Consequently, overt research is regulated by what we call the ethical fiction of informed consent. In examining a case of ethnographic ethics carried to the extreme, in this text we discuss the following questions.

The first set of issues concerns what the AAA holds as the first principle of research ethics—doing no harm to participants. The first section of the Code of Ethics of the AAA states,

> [in] research, anthropologists' paramount responsibility is to those they study. When there is a conflict of interest, these individuals must come first. Anthropologists must do everything in their power to protect the physical, social, and psychological welfare and to honor the dignity and privacy of those studied. (AAA, 1971)

But to whom do we grant the status of a research participant in the first place, who is then a subject of our ethical consideration, norms and informed consent? How do we distinguish between full-fledged research participants with strictly defined rights and casual passersby who happen to enter our field rather tangentially and on a onetime basis? And what if the research situation and relations develop over time and these categories get blurred? The above-cited sentence from the AAA code of ethics also presupposes that conflicts only arise between researchers on the one side and research participants on the other. But what about conflicts of interest between various research participants? What good choice can an ethnographer make in such circumstances and what ethical guidelines can help?

The second set of issues we want to tackle concerns feedback from research participants. Creating space for substantial feedback has lately become an important obligation for researchers. Such practice has ethical as well as epistemological dimensions. As Latour (2000) argues in reference to the verb "to object," objectivity can be achieved in the social sciences (like in the natural sciences) through the possibilities open to those (people, things, and materials) participating in our study to *object* to what we, as researchers and authors, are saying about them. What weight should we, however, grant to the research participants? Would anything, including a veto, be acceptable? With regard to the c/overt character of her research, which created a feeling of permanent ethical deficiency and guilt, the ethnographer in our case strove to meet wishes of the research participants as much as possible. In such circumstances, it was even more difficult to face the fact that the positions and views of two key research participants diverged significantly. How to take feedback from research participants seriously while not entirely giving in to it?

Finally, the third set of questions we are dealing with concerns anonymization. The anonymization of specific information that could harm participants is a standard publishing practice. Sometimes, anthropologists even anonymize research participants against their will, so to speak, to protect them from harm that they might not necessarily anticipate (AAA, 2006). But a standard anonymization of persons and locations does not necessarily ensure that research participants will not be recognized by each other and by the people who know them in person. Therefore, the ethnographer in our case decided to change also some characteristics of the informants to prevent them from recognizing themselves and each other. She used elements of what Humphreys and Watson (2009) call "semi-fictionalised ethnography,"

which, according to them, is especially useful in cases of "highly sensitive or confidential" data that "would not be publishable without very heavy disguising" (p. 43). But what to think in these cases about the possible decrease or shift in the informative value of the ethnographic accounts? And what if the author decides to go even further and disguise her own identity and use a pseudonym? In research practice, authorship is connected not only with recognition, but also with accountability and persuasiveness of an argument. So how is the author's anonymization justifiable in academic practice?

These questions and our answers to them have evolved significantly in the course of our research. And it is precisely this genealogy we want to trace in the first part of this article, drawing upon various notes and communications that arose during our collaboration: the ethnographer's field notes and the written exchanges (text messages, emails, and letters) between the ethnographer and research participant, and between the ethnographer and the supervisor. Like many others (e.g., Alcadipani & Hodgson, 2009; Barton, 2011; Ferdinand, Pearson, Rowe, & Worthington, 2007; Willmott, 1998), we want to emphasize that research ethics cannot be fully resolved in advance or after the research is finished. Instead, it needs continuous rethinking as the fieldwork and research relations develop. In this text, we offer for consideration our own ethical dilemmas and how we dealt with them. We believe that the original contribution of our approach is twofold (a) Helena, the researcher participant, has been involved in this reflection as an active research collaborator and a coauthor of the present article; (b) this text is not a simple recount of the research process: it is its reconstruction based on the views of the three of us captured in written form in different (and shifting) positions. In the second part of the article, we discuss the abovementioned issues of research ethics more generally and argue that we can learn important lessons from c/overt research that can be applied to overt studies with informed consent. Finally, we may well dare to defend the widely denigrated practice of undercover or, in our conceptualization, c/overt research.

Going Undercover

Transnational corporations in the Czech Republic have received significant state support in the last two decades in the form of central investment incentives (Czechinvest, 2015). However, these corporations are then frequently analyzed, discussed, and criticized in the light of their intense workforce flexibilization and precarious labor conditions (see Čaněk, 2016; *iDNES*, 2010). Andrijašević and Sacchetto (2013), who conducted a study of two branch factories in the Czech Republic owned by the Taiwanese electronics contract manufacturer Foxconn, analyze the company's organization of work and workforce and describe the extreme labor conditions of primarily migrant workers. The ethnographer was employed as a research assistant in the project of Andrijašević and Sacchetto and thereby had a chance to listen to the stories of a number of (former) employees, stories that were full of reports about the everyday pressures, sometimes using extreme language—for example, when one ex-worker of the company likened Foxconn to Auschwitz and the assembly line leaders to the gestapo.

Such disquieting accounts, and the fact that so far very limited empirical research has ever been carried out on these "assembly lines," led the ethnographer to come up with the idea of undertaking engaged ethnographic research for her master's thesis. She had had a long-term interest in the situation of manual workers and the changing forms of exploitation to which they are subjected. From the start, she was thus certain that her key responsibility was to the workers (not the factory managers and owners), to whom she would sooner or later reveal her research plans. Before starting her fieldwork, she thus understood her research ethics in structural terms, in reference to Marx (2015): "[T]he individual [can hardly be] responsible for relations whose creature he socially remains, however much he may subjectively raise himself above them" (p. 7).

However, getting access to a global factory is very difficult, which the ethnographer had experienced herself when she was involved in the previous study of Foxconn conducted by Andrijašević and Sacchetto. When the ethnographer contacted the factory managers on their behalf, her inquiries were usually met with silence. If the factory representatives refused to even talk to "Western" researchers, would they let in an MA student if she asked?

This was a field site where it was possible to see suffering that had so far remained undocumented, and there was the problem of the closed nature of the factory. This led the ethnographer to decide to enter the terrain in disguise. It was not difficult to become an employee of one of the electronics manufacturers in a regional town as she could easily join the mass of fluctuating seasonal workers who complement the core labor force of the factory. Given the fact that in Czech universities no IRB for anthropological research exists, the consent of the supervisor was all the ethnographer needed. They were both aware of the extreme nature of the fieldwork, which was going to be ethically perilous and highly demanding. However, they believed that the case could be justified.

Although ethical questions and troubles were expected, this proved to be even more complicated during the five months of fieldwork on the factory assembly line. These months were exhausting and this is evident in the ethnographer's field notes, which are full of doubts regarding the chosen approach and a creeping sense of paranoia about being "caught red-handed." In the second month of the field work, notes of the following kind frequently appeared:

When the line leader says that she has something important to tell us, I am terrified that she will single me out and publicly pillory me because she has found out about me being a researcher. I feel I would be able to explain to Helena what my intentions are in the factory, but it would be much more difficult with the other workers. I also ask too many questions. I fill my head with observations during the work shift and then during the break I sit up in the toilet jotting notes. But sometimes the manual work is so exhausting I'm unable to manage to do any anthropology at all. (2013 July)

Not only did the ethnographer have difficulty combining manual labor with ethnographic work, sometimes the enraged activist role entered the mix—for example, when she spoke out against employer's demand of excessive overtime hours during one dispute. In the covert research, all three aspects blurred and it was impossible to keep them neatly separated. She rather quickly abandoned the initial plan to keep her research identity a secret at the beginning and to relate to other workers solely as a fellow laborer. She realized she could not infinitely fabricate fake details from her life, so she decided just not to speak about some things and thus to be truthful about most of the others. As her relations with her colleagues deepened, the lies and deceit became more and more unacceptable. The strength of personal ties and closeness to some colleagues also started to determine who would be conceived as a "research participant" (and later given the chance to authorize the findings before publication) and who only "happened to be in the field." The ethnographer sometimes intentionally controlled whom she built relations with. The initial plan to live in the workers' dormitory and share a room with three other workers proved to be unsustainable. This is a quote from her field notes:

The girls are sleeping now and I'm writing field notes. Today was a breaking point—I'm sure I can't stay here with them. It's too intimate set-up here to feel ethically okay with writing anything about these people. I feel a big difference between working with someone and living with her. (2013 August)

In the final months of the fieldwork, the ethnographer pushed herself to disclose her research interest, which happened gradually and in different degrees. She told five coworkers about her plan to write an MA thesis focused on factory work; to most of the other coworkers, she said she would be writing "something for school" about the assembly line. She felt relieved. She was also able to see that her colleagues were finally able to make sense of the fact that a university student from the capital city was spending several months working on an assembly line in a regional town.

At the beginning of the research, the line leader fell in the category of those to whom the ethnographer felt no principal commitment, and this was further reinforced by mutual personal antipathy. However, the researcher learnt more in the course of the fieldwork—for example, about the line leader starting out in a low-ranking labor position and gradually working up to the lower management. She was around 50 years old and her fear of losing her job was no less than that of the other workers. On her last day at the factory, the ethnographer told the line leader about her intention to write something about the factory for school. The line leader said little in response.

The Research Participants Strike Back

Ethical problems and dilemmas did not, of course, end when the ethnographer left the field. The ethnographer sent a draft of her article to be submitted to a Czech-language journal to her coworkers at the factory for feedback. Shortly after that she received an angry email from one of them, whom we shall call Zuzana.

The ethnographer then wrote the following email to her supervisor, March 18:

(. . .) Zuzana thinks the paper will expose them and they will be sacked, she said I used them and she doesn't want me to continue writing the paper at all. Tomorrow, I'm going to visit Helena, who is more comfortable with the draft. I'm never ever going to do this sort of undercover research again, I really failed to anticipate all this.

The supervisor sent her an email reply shortly after:

(. . .) It's customary that people don't like what and how we write about them. Working undercover only amplifies this. I'm not saying you shouldn't be concerned by their reaction, but don't let it get to you. They have no monopoly on or veto over your text. And write all this down so that we have notes that can later be methodologically elaborated on or otherwise. This is also your responsibility. This is an experience that could not easily be foreseen. As you say, it's now time to take things seriously.

After meeting Helena, the ethnographer summarized her feelings in a text message to the supervisor dated March 19:

Helena is totally okay, so I'm becoming more relaxed. I'm travelling home writing field notes. I'm trying to take it academically, even if it hurts.

The supervisor replied back,

(. . .) Science does not stand in opposition to emotions and pain. You don't need to choose, in fact it's impossible.

In her field notes, the ethnographer noted the developments over the last days as follows (March 19):

Today we sat down with Helena and reworked stuff she considered risky. I am grateful for that, but I cannot stop

thinking about the email from Zuzana. I'm doing ethnography that tries to be engaged and now the informant I stood by reacts like this Why write it then? What is this knowledge for? I naively thought they would like it, that they might appreciate that I wrote about the working class instead of the usual stuff, etc. However, it's crystal clear now that they are concerned about losing their jobs. Even Helena suggested mainly deleting things that were not in line with rules, like that we were chatting on the line. I am very concerned about Zuzana though. She was surprised and offended that I used direct quotes. She doesn't want to talk about it with me. She told Helena she shouldn't even meet me in the pub as someone might be eavesdropping. She thinks she and Helena have to put an end to it all. Helena is relaxed and she's got ideas. She likes the character of Helena and does not want me to rework it into a male character. She told me to take Zuzana's criticism as valuable data and that this is what will happen in my profession, that I should be tougher. And that I definitely should not give up now. Another worker from the factory supposedly asked if I had already written something. Helena thinks we should keep quiet for now, she said she hadn't heard from me.

The ethnographer then left to take up a fellowship abroad and later sent a chapter of her MA thesis to Helena, who reacted as follows (June 25):

I have just finished reading what you sent to me. It's the first text on such a scientific level I ever read. You are really cool. I am in fact still surprised by the extent of your research and mainly by its complexity. In every letter you send me, you give me a piece of new information about yourself and your work. I would even say I don't know whom you'll finally turn out to be. I don't mean this badly, on the contrary. (. . .)

A few days later (July 1) another message arrived in the ethnographer's email inbox with the subject "a letter from Helena," which consisted of a scanned handwritten letter:

My encounter with Anthropology

Anthropology—I only knew that it existed and that it dealt with researching and discovering unknown tribes somewhere in Africa. A passionate anthropologist with a machete in his hand, clearing a path through a forest to meet a savage and his tribe. A desire for adventure drives him and if his journey is a success and he returns to civilisation, he describes the local people and their habits . . . And if he does not come back, it probably means that he also became an examined object and perhaps experienced personally how savages treat an unknown visitor.

So, I was not interested in Anthropology, it became interested in me.

One day I came to work and saw a new, temporary worker. Since we were working closely together, we often talked. The young lady was explaining where she was from and that she was a student. She was interested in everything and I quite willingly responded to what she asked. And since I am such a naive extrovert, I like talking quite a lot and quite openly. Later, when a friendship started to develop between us (probably on purpose), I gradually learned what this girl was studying. She took this summer job because she needed to write a student thesis. Time passed and when she quit the factory, we agreed that when she writes something, we would comment on it. Later, when I really didn't want her to publish some things, my demands were met and the passages deleted from the text.

But this was not everything I had gradually learnt about the work of an anthropologist. After some time I again had an opportunity to look at another, more specialised text—still on the same topic [the draft first chapter of the thesis]. Only when I read that work did I begin to understand what anthropology is. The anthropologist in my view has a kind of dual personality. He has to disguise himself in order to establish friendship and trust in the researched subjects and get under their skin.

An anthropologist must have an informant (a horrible word), which I became without knowing it. At that moment, when this "clicked" for me, I completely froze. I realised that I should be glad that the secret police of the socialist regime didn't exist anymore, because to make me an informant would have been easy enough. In my naivety, I could be easily fooled.

I do not condemn the work of anthropologists or other scientists, although sometimes I do not believe in the significance of it. But I don't know if I would want to do something that is based on a lie, on deceiving the actors, on "intruding" in a community or a society. To observe actors like animals in a zoo, writing everything down in notes and pretending to be one of them.

According to what I've had the opportunity to read, a previously described reaction from another woman—also an informant, with whom the anthropologist had established a friendly relationship—applies precisely to me. The woman felt exploited and cheated by someone she trusted.

On the other hand, I think it can happen that the anthropologist himself may suffer from remorse and struggle with himself over whether what he is doing is right, because he is probably never able to assess whether his relationship with the informant and the actors will not be morally condemned.

Anyway, for me it's a big experience. And I know now that I will be more vigilant when a new temp joins us again and says he's a college student and is interested in our work.

The ethnographer had by this time returned to the Czech Republic and they met with Helena. After the meeting, the ethnographer noted down in her diary,

I sent Helena my first chapter where I discuss the research process and ethics. I didn't send any comments along with it, hoping that she would not take some of my hidden research

tactics as some kind of betrayal. Her reply was disastrous for me. Helena's role was absolutely fundamental to the justification for doing the research undercover. It was as though she awoke my biggest nightmare—I was so afraid of this happening and then it happened, it hunted me down. I thought of withdrawing my thesis and the article, writing about it to Helena and Zuzana . . . as though I could justify myself in front of them and by trampling on the whole research I could somehow redeem myself. Then I met Helena and she completely downplayed it, she said that she just wrote it because I asked her to write something, so that I and my supervisor would have something to discuss and that she could not even remember what exactly it was all about. But it is clear that she just won't say what pisses her off right to my face, she's not that type of a person. Well, I began to defend myself, that in my opinion she was not a deceived subject . . . I did not agree with her interpretation and did not want to accept it completely without explaining myself only because she was a research participant . . . but that made her maybe even more intimidated.

The ethnographer later wrote an email to Helena asking her if she was interested in contributing with her own experience of a manual worker to a popular street paper. Helena responded in an email (September 4),

A moment ago, I finished reading what you wrote to me and my heart started beating strongly. You write about the insignificant me as if I was some promising writer. Incredible. Thanks—you don't even know how you're helping to boost my self-esteem. Of course I'll be glad to assist you in your work. I can't wait.

Couple of months after the street paper was published (with Helena's own article in it), Helena at her own initiative sent the ethnographer an email with a scanned letter called "The other side of the barricade" (March 29):

The purpose of this writing is not to criticise a researcher, but an attempt to describe how I perceived the whole period of the research process, afterwards a little more informed.

It all started simply—a new temp came to work in our department. This girl "burst" into my rigid, grey life and in a very short time we managed to establish a connection. Later, she indicated that she might write something about the factory as schoolwork. I thought it would be something like a school essay, "what I did on holiday."

She awoke a desire for knowledge in me. In quite a simple way—a question—an answer, I learned a lot and, paradoxically, I also perhaps researched the one who was researching us. The "work for school," as she called it, went on and the closer we became to each other, the more interesting it was, but also the more complicated. After all, at work we are bound by confidentiality.

Suddenly, everything was on paper. Our "bragging" at work acquired a completely different dimension. My colleague, who was a little older than me, was suddenly so full of this feeling of being at risk that she withdrew from any participation in the project. I must admit I was also weighing up which side of the barricade I should stand on. Something was telling me: do the same thing your colleague did, why should you care that something goes wrong for someone else. Protect yourself—don't let yourself lose your job, if it all explodes, because of some student's schoolwork. And, on the other hand, I told myself that there must be some other solution than to cowardly run away and leave the girl in the lurch. It was also kind of a challenge for me to deal with the situation. When we met at my home, she was scared and disappointed by the reaction of my colleague. I tried to encourage and reassure her in some way, which made us even closer.

I was really happy that this work that had evolved so dramatically was eventually handed in. Even though I was the one who almost buried it. Unaware, I had written a short essay on the work of an anthropologist—as I saw it as a complete layperson. And that was right around the time of the deadline for submitting the thesis, so I caused more disruption in the psyche of my friend—the researcher. Fortunately, she found support in her supervisor and handed the thesis in. Because, as she told me later: the ethics of the research, which I weight on a bit in my letter, were more important to her than the degree. I was petrified, and felt like a fool and a traitor. And what a surprise when a while later Tereza asked me if I wanted to write something about my work and the conditions I work in.

When my article came out and Tereza called to tell me about the good response it received, I swelled with pride. Later, the colleague (who refused to participate in the research) asked about whether I still see Tereza. Well, I think she regrets her fierce reaction a bit. I told her that we still meet and that I even wrote an article for a magazine. She was surprised but didn't answer when I asked whether she wanted to read it. Just as she had refused to read the thesis when I offered it to her. Had anyone told me that I'd be writing an article for a magazine, I'd have thought him a fool. I'd say that all I'd write was a shopping list. Or a birthday card. (. . .)

As a collaborative ethnography (Lassiter, 2005), this article is another attempt to shatter the stiff conceptual and ethical barricade between the researcher and the researched, and also between covert and overt research.

Discussion

Drawing upon our empirical material, we will now in more detail discuss the power dynamics between the researcher and the researched, the pitfalls of informed consent, and the complex issue of anonymization in published work. As stated in the introduction, some of the motives, topics, and problems that arise and are particularly pronounced in undercover or c/overt research, also apply to overt research. As extreme as they are, they can shed light on a "regular"

research practice that sometimes takes such issues for granted or overlooks them.

Fieldwork Powers

The power that researchers have over the researched, who, in a typical top-down study, are overridden by the cultural, social, and economic capital of scientists, is a common topic in ethnographic literature (see Murphy & Dingwall, 2007; Wolf, 1996). The response to this situation in contemporary anthropology has been twofold. First, there have been attempts to pursue collaborative practices based on a research design that seeks to provide space and resources for a more active involvement of the researched and the articulation of their interests. Second, a variety of more or less formal ethical obligations have been devised to guide and direct ethnographic practice when asymmetrical situations arise. The ban on undercover research stipulated in the Code of Ethics of the AAA (AAA, 2012) is one of the measures designed to limit the power of the researcher over research subjects. In our study, however, we have seen that the undercover or more precisely c/overt nature of a research not only increases the power of the researchers over the research subjects, but also—and at first glance this might seem paradoxical—it increases the power of the research subjects over the ethnographer. It is not due to the covertness of the research but to the current way in which such research is perceived within the academic community, which deems it a highly problematic approach. The dynamics of the research described above clearly show that it was this aspect of the research that led the ethnographer to constantly reexamine her methods, and to her feelings of guilt and eventual readiness to even grant her key informants the right of veto over her research results and to abandon her planned publications. Although similar situations can and do occur in overt research settings (Plankey-Videla, 2012), they are greatly magnified in the covert context. Two details are worth examining in this context.

The first related to publishing an article in a Czech-language journal. The ethnographer gave five of her closest coworkers and key informants at the factory a draft of her article to read. A young subcultural couple, who worked at the factory for about the same length of time as the ethnographer, were not really interested in the article. After the ethnographer asked them repeatedly to read it, they went through it and emailed her a comment about her having done "a nice job." Just as they were indifferent about their temporary summer job, they were equally indifferent about the article on the factory. Another informant, a young worker, was not interested in commenting on the article at all. Two other coworkers and informants were among the older generation of workers; they had worked at the factory for many years and their plan was to stay there until retirement. They both responded to the article very carefully. Zuzana's reaction was clearly dismissive—She did not want to be in the article and did not agree with it being published at all. The ethnographer, who had come to the factory to defend the workers' interests, broke down and noted in her fieldwork diary that she [had] "become[s] a hostage to her informants." She realized that the informants had enormous power over what she could publish and how and whether it would be published at all. The role of her coworker Helena was of key importance in this. Unlike Zuzana, she liked the article and sent several pages of comments on it. This actually helped the ethnographer to cope with Zuzana's rejection. It was crucial in these moments that one informant had a favorable opinion. Had both key informants rejected the article, neither it nor the thesis would have probably been published.

The second situation arose later and in connection with the thesis. Helena's first reaction to the thesis chapter was to send an email praising the work, which was followed by the critical letter printed above. In her letter, Helena made remarkably skillful use of the "vocabulary" of the thesis, which allowed her to see anew and to articulate her own position in the research as an instrumentalized and exploited subject. The ethnographer broke down again and considered withdrawing the thesis. The supervisor did not support it, but at the same time did not express a firm rejection. She warned the ethnographer about making empty gestures: The ethnographer could afford to withdraw the thesis because she already has a master's degree from previous studies and could therefore start her planned doctoral studies anyway. The decisive factor was ultimately a personal meeting with Helena, who softened her position. The ethnographer was on one hand relieved about this turnaround—not only because of her thesis, but mainly because her relationship with Helena was restored. On the other hand, she realized that the symbolic dominance that an ethnographer (and at that moment someone who almost held two master's degrees) almost inevitably has over a factory worker in a situation of confrontation over a text, played a role in Helena's change in attitude.

While covert research may seem to amplify dramatically the asymmetries between the researcher and the researched subjects, these asymmetries came out as quite fluid and variable in our c/overt research. The power and self-confidence of Helena, a "mere" informant, strengthened when she was reading and commenting on the draft articles and writing her own letters and article, and the "privileged" ethnographer lost ground during difficult research situations and particularly when faced with "resistance" from the research subjects. Both the power of the ethnographer and the research participants intensified. The power charge of their *relationship* was increasing, as the unfolding research substantially affected both sides. We believe that the distress the ethnographer felt as the result of the concealment, this basal metabolism of guilt in the foundations of the

research design, sheds light on dynamics that may also often be present—though in a more moderate form—in overt research.

The Pitfalls of Informed Consent

As noted above, the ethnographer sent the first draft of her journal article to her five informants. In fact, she turned her five factory coworkers into research participants, to whom she fully uncovered her research intentions and whose feedback she actively elicited and was prepared to take seriously. Why these five?

Had she carried out her research at a university in the Anglo-American world, where unlike in the Czech Republic research projects have to be evaluated by IRBs, she would have had to submit a protocol with detailed information about the project, an assessment of risks and benefits, and a plan to obtain informed consent from human subjects in order for the research to be approved. Informed consent should guarantee that the research subject made the decision to participate in the research project voluntarily and is "informed" of its purpose, methods, risks, and sponsors. In addition, while obtaining and maintaining lawful and informed consent in anthropology, researchers should also "[p]rovide a long-term mechanism for study subjects to contact the researcher or the researcher's institution to express concerns at a later date and/or to withdraw their data from the research process" (AAA, 2000).

The principle of informed consent is considered to be the "gold standard" of professional ethics (Fluehr-Lobban, 2013, p. 56), despite being one of the most criticized aspects of the IRB (Zavisca, 2007). In ethnographic inductive research, the issues under study and the relevance of the questions transform and are clarified during the process, which means it is almost impossible to inform research participants comprehensively. Consent can thus hardly be viewed as an event before one starts the fieldwork, but as a process, a part of constant negotiation between research participants and the anthropologist (Church, Shopes, & Blanchard, 2002).

Van den Hoonaard (2002) highlights the ethnocentric and middle-class aspects of the requirement that an individual provides written consent to research. Current research ethics are grounded in the tradition of liberal democracy, emphasizing individualism. In more collectivist cultures, this requirement is problematic, as it is when a researcher wants to study elites or marginalized groups. Zavisca (2007) draws attention to the problem of "voluntariness"—for example, in the case of economic asymmetry, the ethnographer may represent a source of power or income and the informants therefore agree to participate regardless of the researcher's intentions. Similar issue arises in hierarchical settings where consent to research tends to be granted by superiors, for example, factory managers. Subordinates, for example, employees, may then have little chance to exert their free will with regard to their research participation (Plankey-Videla, 2012). Another problematic situation, and one typical for long-term field research, occurs when a researcher and an informant become friends. Such a configuration blurs the boundary between information shared with the researcher in an "informed" way and that shared with a friend. Lindbloom (2004) has questioned the alleged anonymity that is supposed to be guaranteed by providing informed consent. The researcher promises that the data obtained from an informant will be anonymized, but this often does not protect informants from being recognized by those around them. Furthermore, the informant is asked to state his name and signature in the form. According to Lindbloom, instead of guaranteeing anonymity, which is the original idea behind it, the signed form may destroy it.

Homan (1992) has pointed out that one of the functions of informed consent is to accentuate the distinction between overt and covert research methods. In reality, though, working with informed consent from all research participants is virtually impossible. Haggerty (2004 in Zavisca, 2007) cites the case of a Canadian student who was advised by an IRB to look away anytime she encountered anyone during her fieldwork who did not explicitly agree to participate in her research. The absurdity of this recommendation indicates that informing everyone and in all circumstances is usually not feasible and it is always necessary to draw a dividing line.

And that is what the ethnographer did here. Given the undercover nature of the research, it is clear that she could not ask the research participants for informed consent beforehand. However, she did ask five coworkers for retroactive informed consent and informed the majority of her colleagues in her working group about "writing something for school." Creating a boundary between these five "key informants" and the remaining "informants" was not easy and depended on several factors. Friendship and closeness determined how often certain persons appeared in her notes and were consequently key factors in designating whom the five key informants would be. Another factor determining that some coworkers would be informed while others would not was the position of Helena in the research: She is the person cited and involved most in the publications and would therefore be the one most at risk if the factory's management ever learned of the research. Hence, the ethnographer decided to comply with her wish to keep the circle of people who know about the research small. Finally, as the supervisor had pointed out, had the ethnographer informed all her coworkers about her research she would have made them complicit in it, dragged them into a forced alliance, and presented them with the dilemma of whether to report her to management. As a researcher and as a friend (who still suffers from remorse for not having informed all the participants about her research), she would have felt more

at ease and less in conflict with the rules of research had she asked participants at least for retrospective consent to research and authorization of her draft article. However, after having carefully weighed the possible risks for the participants, she decided not to inform most of them and to leave them with the opportunity to distance themselves from the research if it was revealed to the management—just as the already "informed" Zuzana had tried to do after reading the draft of the article. Zuzana did not want to be informed any more about the research development and tried to persuade Helena to prevent the article from being published.

Possibilities, Limits, and Consequences of Anonymization

If we do not obtain authorization from research participants for the publication of our writings, how can we be sure that our descriptions and interpretations reflect them and do not insult them or even put them at risk? An anthropologist writing under the pseudonym Rebekah Nathan (2005b), who secretly conducted research in the role of a student at the university where she taught, thought about her fellow students and imagined herself in their skin while writing:

> Would I be comfortable saying what I was writing that moment, in that chapter, if I were saying it to them? I tried projecting, too: Would I be comfortable if I were a student and recognized in a book, written by a professor at my own university, an informal conversation that I thought I was having privately with a classmate? (Nathan, 2005a)

In our case, this strategy could not be employed. The information about Zuzana in the article was not of a sensitive and personal nature; it related to factory work and the ethnographer could hardly imagine being bothered were such information to be published about her. Yet Zuzana's reaction was dismissive. The ethnographer thus rather chose to anonymize thoroughly. She changed the unauthorized stories so that even their protagonists could not recognize themselves (or one another). She mostly changed the sex, age, or family background of the main protagonists in the stories. These are characteristics that involve areas the ethnographer did not focus on and that did not directly relate to her research questions or analysis. We believe anonymizing the participants in this way does not detract from the validity of the arguments in the article, but it may have reinforced certain stereotypes. This is most apparent in the case of gender stereotypes. Because the ethnographer did not explicitly focus on gender, she frequently changed the sex of the participants, to correspond to the more prevalent, dominant, hierarchical gender structure: The line leader, though in reality a female, is presented in the thesis as male, as is a female laborer whose task happened to be one typically performed by men. A male line assistant, subordinate to the line leader, was turned into a woman in the thesis. Finally, all the female operators with whom the ethnographer worked on the assembly line were written about in the generic masculine language, which is the convention in the Czech language.

Besides concealing the characteristics and identities of her colleagues, the ethnographer decided to use a less common type of anonymization. The academic article that had sparked Zuzana's and to some extent Helena's concerns was published under a fictional name, minimizing the clues with which to identify the factory and specific people. While Helena appreciated this gesture, the ethnographer's academic colleagues did not. In research practice, authorship is associated not only with credit, but also with conviction and responsibility. The ethnographer's name ties research and publications to a specific person, who exposes herself to possible criticism from colleagues, research participants, and the wider public. For one reviewer of the article, using a pseudonym was only a rhetorical trick on readers by which the ethnographer "obscured hardly acceptable conduct."

Although we take seriously that the original decision to do covert research remains controversial and it can never be justified universally, the researcher's decision to conceal herself on the contrary emerged out of the research process and as a result of situated ethical reflection and was intended to minimize harm to everyone involved. The ethnographer and her supervisor did not judge categorical transparency to be the strongest imperative in this particular case.

Remarkably, unlike the professional community, academic institutions responded to the researcher's anonymization in her article and her covert research practice in an unhesitatingly positive way. The university rector approved a written request from the ethnographer for an exemption that her defended thesis would not be made available online. Later, the school responded favorably to the published article. The ethnographer and the supervisor had feared that under a pseudonym the article would not officially count as scientific publication and the institution would thus lose "performance points" that contribute to its funding. It turned out that it was possible to link the pseudonym the ethnographer used to her official personal identification number, thereby attributing the "performance" to the institution. Moreover, the ethnographer managed to present a paper at an international academic conference organized by her school under her last name's abbreviation only. Although the absence of IRB in the Czech social sciences might look preferable in the context of our argument for c/overt research, it rather is a manifestation of a disinterest in research ethics issues, which is in stark contrast with our position.

Conclusion: In the Name of C/overt Research

In this article, we discussed the pitfalls and dilemmas of undercover research that gradually becomes overt to some of the research participants. As a result, we propose the term c/overt research as an alternative to the opposition of overt versus covert research. The radical version of a c/overt research assumes more moderate form in regular research practice. The circle of participants expands or contracts and participants only gradually learn what the research is really about and what it entails for them. Not because the researcher lacks ethics and decides to keep them uninformed, but because the research itself evolves and changes. The research is in a sense revealing itself even to the ethnographer herself. Despite the imperative principle of informed consent, "the gold standard" of research ethics, it is impossible to adhere to this principle in a full and absolute sense in the actual course of research. By examining our own experience with the extreme version of c/overt research, we tried to analyze the ethical fiction of informed consent and shed a critical light on the IRB procedures and established standards of professional research ethics. Moreover, we exemplify that elements of covertness in the research process do not necessarily preclude intense collaboration and partnership with research subjects, as this article testifies. As has been repeatedly argued in the literature (Alcadipani & Hodgson, 2009; Church, Shopes, & Blanchard, 2002; Lindbloom, 2004; Shopes, 2000; Van den Hoonaard, 2002; Zavisca, 2007), strict codes of ethics and IRB procedures tend to obscure rather than address the fundamental uncertainties that are embedded in the research. Similarly to Barton (2011) and Ferdinand et al. (2007), we believe that ethical reasoning and decisions of a researcher are "mediated by the specific context in which they arise" (Ferdinand et al., 2007, p. 535), and that one must "laboriously negotiate ethical issues on a case-by-case basis" (Barton, 2011, p. 443).

But if we admit this is the case, what is the reason for the categorical prohibition of covert research and the absolute rule of the principle of transparency? To conclude on a slightly provocative note, we could claim that the prohibition of covert research and the ethical fiction of informed consent mainly protect the researchers themselves from having to deal with serious ethical dilemmas and the stress of fieldwork and the uncertainties that accompany it. It is the notion of c/overt research that forces researchers to cultivate sensitivity to research ethics.

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Notes

1. Such negative attitude toward covertness led some anthropologists to withdraw their application for research approval and to request being viewed as a human rights investigative reporter (see Scheper-Hughes, 2004).
2. The Common Rule, a set of regulations defining protection of research participants in the United States, has been revised on January 19, 2017 (see https://www.gpo.gov/fdsys/pkg/FR-2017-01-19/pdf/2017-01058.pdf) and significant changes concerning social and behavioral sciences have been made. Human subject research that is considered low risk (including survey and interview procedures and observation of public behavior uninfluenced by the investigators) is exempt from the institutional review board (IRB) and the investigators themselves decide whether their research falls under the exempt category. The revised Common Rule will be implemented into regulatory frameworks of institutional bodies by January 2018.

References

Alcadipani, R., & Hodgson, D. (2009). By any means necessary? Ethnographic access, ethics and the critical researcher. *Tamara Journal*, *7*(4), 127-146.

American Anthropological Association. (1971). *AAA statements on ethics: Principles of professional responsibility*. Author. Retrieved from http://www.americananthro.org/ParticipateAndAdvocate/Content.aspx?ItemNumber=1656

American Anthropological Association. (2000). *Briefing paper on informed consent*. Author. Retrieved from http://www.americananthro.org/ParticipateAndAdvocate/Content.aspx?ItemNumber=13144

American Anthropological Association. (2006). *Case 5: Anonymity declined*. Author. Retrieved from http://www.aaanet.org/committees/ethics/case5.htm

American Anthropological Association. (2012). *Principles of Professional Responsibility*. American Anthropological Association. Retrieved August 20, 2014, from http://ethics.aaanet.org/category/statement/

Andrijašević, R., & Sacchetto, D. (2013). Odlišné režimy práce? Foxconn v České republice [Different labour regimes? Foxconn in the Czech Republic]. *Kolektivně proti kapitálu*. Retrieved from http://protikapitalu.org/down/Foxconn_CR-SR.pdf

Barton, B. (2011). My auto/ethnographic dilemma: Who owns the story? *Qualitative Sociology*, *34*, 431-445.

Čaněk, M. (2016). Building the European centre in Czechia: Foxconn's local integration in regional and global labour markets. In J. Drahokoupil, R. Andrijašević, & D. Sacchetto (Eds.), *Flexible workforces and low profit margins: Electronics assembly between Europe and China* (pp. 95-112). Brussels, Belgium: Etui.

Christians, C. (2005). Ethics and politics in qualitative research. In N. K. Denzin & Y. S. Lincoln (Eds.), *The SAGE handbook of qualitative research* (3rd ed., pp. 139-164). Thousand Oaks, CA: SAGE.

Church, J. T., Shopes, L., & Blanchard, M. A. (2002). Should all disciplines be subject to the common rule? *Academe*, *88*(3), 62-69.

Czechinvest. (2015). *Investiční pobídky* [Investment incentives]. *Author*. Retrieved from http://www.czechinvest.org/investicni-pobidky-nove

Denzin, N. K. (2003). *Performance ethnography: Critical pedagogy and the politics of culture*. Thousand Oaks, CA: SAGE.

Ferdinand, J., Pearson, G., Rowe, M., & Worthington, F. (2007). A different kind of ethics. *Ethnography*, *8*, 519-543.

Fluehr-Lobban, C. (2013). *Ethics and anthropology: Ideas and practice*. Lanham, MD: AltaMira Press.

Haggerty, K. D. (2004). Ethics Creep: Governing Social Science Research in the Name of Ethics. *Qualitative Sociology*, *27*, 391-414.

Herrera, C. D. (1999). Two arguments for "covert methods" in social research. *The British Journal of Sociology*, *50*, 331-343.

Homan, R. (1992). The ethics of open methods. *The British Journal of Sociology*, *43*, 321-332.

Humphreys, L. (1970). *Tearoom trade: Impersonal sex in public places*. London, England: Duckworth.

Humphreys, M., & Watson, T. (2009). Ethnographic practices: From "writing-up ethnographic research" to "writing ethnography." In S. Ybema, D. Yanow, H. Wels, & F. H. Kamsteeg (Eds.), *Organizational ethnography: Studying the complexities of everyday life* (pp. 40-55). London, England: SAGE.

iDNES. (2010, June 14). Nošovická Hyundai přijala trest. Zaplatí 700 tisíc za špatné pracovní podmínky [Hyundai in Nosovice accepted the penalty. It will pay 700 000 CZK for poor working conditions]. Retrieved from http://ekonomika.idnes.cz/nosovicka-hyundai-prijala-trest-zaplati-700-tisic-za-spatne-pracovni-podminky-1w5-/ekoakcie.aspx?c=A100614_172739_ekoakcie_fih

Lassiter, L. E. (2005). Collaborative ethnography and public anthropology. *Current Anthropology*, *46*, 83-106.

Latour, B. (2000). When things strike back: A possible contribution of "science studies" to the social sciences. *The British Journal of Sociology*, *51*, 107-123.

Lindbloom, J. (2004). Preklopenie asymetrie po získaní údajov: Pomáha informovaný súhlas alebo škodí? [Reversing the asymmetry after getting data: Does the informed consent help or harm?] *Biograf*, *35*, 85-95.

Lugosi, P. (2006). Between overt and covert research: Concealment and disclosure in an ethnographic study of commercial hospitality. *Qualitative Inquiry*, *12*, 541-561.

Marx, K. (2015). *Capital: A critique of political economy* (Vol. I). Moscow, Russia: Progress Publishers. Retrieved from http://www.marxists.org/archive/marx/works/download/pdf/Capital-Volume-I.pdf

Murphy, E., & Dingwall, R. (2007). The ethics of ethnography. In P. Atkinson, A. Coffey, S. Delamont, J. Lofland, & L. Lofland (Eds.), *Handbook of ethnography* (pp. 339-351). London, England: SAGE.

Nathan, R. (2005a, July 29). An anthropologist goes under cover. *The Chronicle of Higher Education*. Retrieved from http://chronicle.com/article/An-Anthropologist-Goes-Under/26796

Nathan, R. (2005b). *My freshman year: What a professor learned by becoming a student*. Ithaca, NY: Cornell University Press.

Plankey-Videla, N. (2012). Informed consent as process: Problematizing informed consent in organizational ethnographies. *Qualitative Sociology*, *35*, 1-21.

Scheper-Hughes, N. (2004). Parts unknown: Undercover ethnography of the organs trafficking underworld. *Ethnography*, *5*, 29-73.

Shopes, L. (2000). Institutional review boards have a chilling effect on oral history. *Perspectives on History*, *38*(6), 34-37.

Van den Hoonaard, W. C. (2002). Ethical norming and qualitative research. In W. C. Van den Hoonaard (Ed.), *Walking the tightrope: Ethical issues for qualitative research* (pp. 3-16). Toronto, Ontario, Canada: University of Toronto Press.

Willmott, H. (1998). Towards a new ethics? The contribution of poststructuralism and posthumanism. In M. Parker (Ed.), *Ethics and organizations* (pp. 76-121). London, England: SAGE.

Wolf, D. L. (1996). Situating feminist dilemmas in fieldwork. In D. L. Wolf (Ed.), *Feminist dilemmas in fieldwork* (pp. 1-55). Oxford, UK: Westview Press.

Zavisca, J. (2007). Ethics in ethnographic fieldwork. *Forum for Anthropology and Culture*, *4*, 127-146.

Author Biographies

Tereza Virtová is a researcher at the Institute of Philosophy of the Czech Academy of Sciences and a doctoral candidate in general anthropology at Charles University. She has conducted research in the fields of science and education and is interested in the study of work and technology.

Tereza Stöckelová is a researcher in the Institute of Sociology of the Czech Academy of Sciences and associate professor in the Department of General Anthropology at Charles University. Her work is situated in between sociology, social anthropology, and science and technology studies, and draws upon actor network theory and related material semiotic methodologies. She has investigated academic practices in the context of current policy changes, relations between science and society, and environmental controversies, and most recently, the interfaces between biomedical and alternative therapeutic practices in the Czech Republic.

Helena Krásná is the pseudonym of a lifelong manual laborer and a novice writer.