Introduction: Under suspicious eyes – surveillance states, security zones and ethnographic fieldwork

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Introduction

Whilst doing fieldwork on a natural disaster in Gilgit-Baltistan, the high-mountainous area of northern Pakistan, Martin Sökefeld interviewed Mohammad Ali (name changed), a friend who worked with a major NGO engaged in rural development, in his office. For two hours they talked about damage, compensation and plans for rehabilitation. A few weeks later, Mohammad Ali stated that immediately after Martin's departure he had been visited by officers representing four different intelligence agencies, one after the other. They all wanted to know what the two had talked about. In recounting this story, Mohammad Ali joked about the paranoia of 'the agencies' in Gilgit-Baltistan. This, however, was not a one-off but rather a regular experience.

On another occasion, Martin interviewed Said Khan (name changed) in his rented room on the second floor of a building in Gilgit. When he left Said and walked down the stairs, two men wearing motorcycle helmets, an unmistakable sign of 'agents', passed him in the stairwell. Later, Said confirmed that these agents had indeed visited him and enquired about their meeting.

Sabine Strasser worked as an Associate Professor at the Middle East Technical University in Ankara between 2007 and 2011 and was quite familiar with the entanglement of politics and academia in the country. When she went back to Turkey in 2015, to lay down administrative ground for a study on young, unaccompanied refugees in the country, she was shown a 'secret letter' that YÖK (Yükseköğretim Kurulu, Council of Higher Education), the main institution of state control over academia, had sent to all universities. This letter, in the name of 'protecting privacy', prohibited any research among Syrian refugees. Experts in migration studies were convinced that the government was simply aiming at curbing the observation of refugee politics and asylum practices in Turkey. Foreigners, according to these experts' opinion at that time, would not

gain a permit in the field of migration studies anyway. Conducting research in Turkey from outside has become more difficult and hidden since then. For instance, tensions between the state and academics culminated in the persecution of academics who had signed a petition against curfews and for peace in Kurdish areas of the country. A short time later, they were depicted as "terrorists with a pencil" and categorised as a threat to the state, similar to ISIS. Since then, close monitoring and waves of dismissals and detentions, in particular after the failed coup d'état in July 2016, have become part of everyday life, and several foreign researchers have been expelled from the country or closely controlled. Yet, should anthropologists withdraw from fields which are difficult to access for political reasons? Should we leave when surveillance becomes visible, tighter and when international funding bodies respond to these tensions with stricter ethical requirements?

Our experiences indicate how individual provocations, institutional control and the overall experience of collective/state surveillance have a significant impact on our fieldwork, in that they create fear and uncertainties that force us to adapt our strategies and methodologies of research. We are living in an age of surveillance. In many cities we are observed by CCTV-cameras at every significant intersection, and when we use the internet, especially major search engines and social networking platforms, all of our steps are recorded. Furthermore, as a consequence of the disclosures made by Edward Snowden, we have learnt to what extent the world is being subjected to surveillance by intelligence agencies such as the NSA. While such operations target us as citizens, surveillance also affects us as anthropologists and interferes with our fieldwork. On the one hand, anthropology is increasingly interested in all kinds of 'security zones', such as border areas, laboratories, hospitals, refugee camps, prisons and industrial plants. On the other hand, surveillance triggered by more general efforts to assure 'security' in the public space (Maguire et al. 2014) does not stop short of our field sites. In the wake of real or perceived security threats, states, companies or other surveillance agencies have become highly distrustful, and in many contexts anthropological fieldworkers are regarded as very suspicious subjects and sometimes even as outright spies.

In recent years, surveillance studies have become a burgeoning sub-discipline of the social sciences. They examine data gathering by social networking sites (Fuchs 2009), analyse relations between surveillance practices and gender (Dubrofsky and Magnet 2015), research surveillance through CCTV (Frois 2014, Lauritsen and Feuerbach 2015) or describe surveillant assemblages (Haggerty and Ericson 2000). Our concern, of course, is more restricted and specific to anthropological practice.

In this special issue, which is the outcome of a workshop held at the EASA Conference 2014 in Tallinn, we discuss the impact of surveillance on 'ordinary fieldwork'. From conversations with colleagues we know that many fieldworkers can recount the experiences of being 'shadowed' or at least being scared of control in the field. Nonetheless, any discussion of such occurrences is normally limited to informal talk and the private exchange of anecdotes; they are rarely discussed openly or even in published form, for obvious reasons, perhaps. Katherine Verdery's recent books and articles,

which reflect on fieldwork in Romania in the 1970s and 1980s under the surveillance of the Securitate, are a notable exception and possible only due to a change in the regime and thus access to her files (Verdery 2012, 2014 and forthcoming). However, given the increasing problem of surveillance in the field, we think it is time for a broader debate and a more explicit approach to this topic in our discipline. Considering the grave methodological, ethical and political implications of surveillance in the field, we have to come to terms with the fact that not only do we observe, ask questions and collect data, but we are also simultaneously the objects of sometimes secret – and very often quite obvious – observation and surveillance.

Of course, there are different degrees of surveillance. As we constantly watch others and ourselves, in order to relate to one another, a certain amount of monitoring is a dimension of social life in general, and so in this sense any fieldworker is always 'under surveillance' in her or his field. That said, the surveillance we refer to herein is executed mostly by some specific body collecting data in order to exert control over actors in a social field. Such monitoring takes place in order to prevent the production of knowledge that is unwanted and regarded, for some reason, as detrimental.

In this introduction we will first look into different debates on surveillance within anthropology, focusing on collaboration with intelligence services. We then define our specific field of interest in this volume and describe the particularities of doing fieldwork under surveillance, as there are instable and shifting power relations between the observer and the observed in post-colonial contexts. Furthermore, we reconsider how the mantra of building trust with our research participants becomes questionable during ethnographic fieldwork that is pervaded with tension, contradictions and anxiety whilst under surveillance. Finally, we deal with questions of how surveillance affects methodologies and how issues of ethics have to be addressed differently in these circumstances.

Anthropology as an accomplice of intelligence

Anthropologists have been active on both sides of the 'surveillance divide': often we are objects of surveillance in the field – this is our concern here –, but we also need to acknowledge the fact that in many cases anthropologists have been accomplices in intelligence work. Anthropology's professional ethics is largely the consequence of the efforts to come to terms with such complicity, efforts which started quite late in the history of the discipline. Remember that Franz Boas was excoriated by his colleagues and censured by the American Anthropological Association for his 1919 disclosure and critique of fellow anthropologists' complicity with intelligence agencies during World War 1.[1] Only after more critical reflection, which emerged with the decolonisation of

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[1] For a detailed discussion of these events see Price 2008: 11ff.

our research fields and continued to question post-colonial global regimes of power, did the rejection of complicity with military and intelligence agencies become a kind of common understanding amongst the majority of anthropologists. The exposure of Project Camelot in 1964, the planned recruitment of anthropologists and other social scientists for data collection in the context of counter-insurgency interventions in Latin America by the US-Army which, interestingly, was not revealed by US anthropologists but by Norwegian sociologist Johan Galtung, became the turning point and ultimately resulted in the first draft of the AAA's Statement on Ethics. Because of the context of its genesis, this statement emphasised anthropologists' responsibility for the people they study and urged that fieldwork should never be done covertly. Nevertheless, especially in the United States, cooperation between anthropologists and the military apparatus has never fully ceased, and in the last decade, military interventions in Iraq and Afghanistan have revived such collaboration on an unprecedented scale (Price 2011); for instance, in so-called 'human terrain systems', anthropologists have been 'embedded' in military teams operating in both countries. Such kinds of engagement continue to dominate debates about the relationship between anthropologists and intelligence services, although we can assume that most professional anthropologists would reject this notion entirely.

In addition, our codes of ethics prove that in reflections about the politics and ethics of fieldwork, anthropologists are almost routinely put on the side of the powerful and therefore need to take care not to cause harm to their research participants. The figure of the powerful and potentially harmful anthropologist survives from the colonial past of our discipline. However, things have become much more ambivalent and complicated since that time, and in many contexts anthropologists in the field are much less powerful than is often assumed; thus, it is not only our interlocutors and research partners who might suffer harm as a consequence of fieldwork, because, today, anthropologists often work in settings that pose considerable danger to them, too. Fields pervaded by conflict and open or covert violence are not uncommon, and many of us do 'Fieldwork under Fire', as Nordstrom and Robben's edited volume (1995) is aptly titled. Furthermore, 'Surviving Field Research' (Sriram et al. 2009) is a major concern in such contexts, and while 'Dangerous Fields' (Kovats-Bernat 2002) often come with a heightened level of surveillance, such monitoring of fieldworkers is not limited to such circumstances. Thus, the experiences recounted and analysed in the articles that make up this collection took place in much less spectacular settings; however, they most certainly exemplify such contradictory and ambiguous power relations in the field.

Being suspicious

In the introduction to his path-breaking edited volume, *Anthropology and the Colonial Encounter*, Talal Asad remarked that in the olden days of colonial anthropology the

object of anthropological study was made "accessible and safe" by the colonial power structure:

> "[...] because of it, sustained physical proximity between the observing European and the living non-European became a practical possibility. It made possible the kind of human intimacy on which anthropological fieldwork is based, but ensured that intimacy should be one-sided and provisional" (Asad 1973:17).

Although we imagine that access to colonial field sites may also often have involved considerable negotiation with the colonial administration, admission to many contemporary sites has become much more difficult because of "post-colonial instability" (Kovats-Bernat 2002:211) and a lack of the 'backup power' required to navigate bureaucracy which grants – or withholds – research permits. Consider the example of Pakistan again: legally, fieldwork by foreigners in the country requires a 'No Objection Certificate' (NOC). Such a 'research NOC' has to be applied for at the Ministry of Interior, based in Islamabad, following which the ministry routinely forwards any application for checking and approval to at least four different intelligence agencies. Thus, in Pakistan, the surveillance of research starts immediately, before one actually enters 'the field'. Such checking of applications for research NOCs is an expression of mistrust and hints at perhaps the most significant consequence of surveillance for fieldwork: it makes the building of trust, the most important social resource for anthropological research, very difficult. In Pakistan, though, distrust on the part of the government and its agencies against foreigners is not entirely unjustified, as there have been several critical incidents with foreign activities in the country. Two events have been especially significant in this regard. On January 27, 2011, the US citizen Raymond Davis shot two Pakistani men in Lahore because he felt threatened by them. Davis initially identified himself as a 'plumber' working at the US Consulate in Lahore, and US authorities admitted only four weeks later that he was in fact a CIA agent. Roughly three months later, on May 2, 2011, US Navy SEALs Special Forces killed Osama Bin Laden in a raid in Abbottabad, Northern Pakistan. After these incidents, suspicion against foreigners working in the country, foreign anthropologists included, grew noticeably. In Turkey distrust always increases in line with internal conflicts, international criticism and conspiracy theories. Turkey allows studies only in cooperation with Turkish universities, local authorities and with a research permit issued by the Turkish Consulate in the respective country. While, fortunately, not all countries are hotspots of surveillance comparable to Pakistan and Turkey, these examples show aptly that fieldwork continues to be affected by global inequalities of power and power struggles that sometimes translate quickly into obstacles to anthropological research.

Surveillance is often triggered by the assumption that the fieldworker might in fact be a spy, a form of suspicion from which Katherine Verdery suffered. In her article *Observers observed* she points out that our methods are indeed in some ways strikingly similar to the practices of spies and agents, since we often use pseudonyms for our informants, we code (and increasingly encrypt) our field notes, use tape recorders

and are involved in a comprehensive data gathering operation that goes way beyond our formally-stated research questions (Verdery 2012:17). Like spies, we should add, fieldworkers attempt to work inconspicuously and take pains not to attract too much attention, in order not to 'disturb' the setting in which they collect data (cf. Wolcott 1995:152). Interestingly, Mitchell (1993:46) bases his distinction between fieldworkers and spies not on what they do but on matters of ideology and institutional backing. The suspicion of spying quite often falls on anthropologists. In her study on fieldwork hazards, appropriately titled *Surviving Fieldwork*, Nancy Howell reports that 25 per cent of the social anthropologists included in her sample had to deal, at one time or another, with this suspicion (Howell 1990:97).

From the different experiences presented in this volume we can draw the conclusion that our efforts to be unobtrusive very often arouse unreserved suspicion. In many countries, social science is largely identified with conducting surveys, and as such a 'scientist' that does not work with questionnaires and spends most of her or his time simply hanging out with people does not appear to be scientific at all. In an environment deeply infused with mistrust, the conclusion is not far-fetched that the self-proclaimed scientist who apparently does not really do science is, probably, a spy. And, as Howell points out, spying is "a difficult charge to defend against when one is there in search of information" (1990:97).

Building trust in a field under surveillance

Almost every introduction to anthropological fieldwork emphasises the necessity of *trust* as a significant resource; it is, in fact, the social fundament required for conducting ethnographic research, particularly participant observation and in-depth interviewing. As our intention is to get 'close to people', because we want to understand 'their perspectives', we spend a considerable part of our research time developing what is mostly called 'rapport', and trust is a very significant element in this respect. LeCompte and Schensul (2010:14) even equate "building rapport" with "gaining trust," while for Dewalt and Dewalt rapport means the sharing of the goals – "at least to some extent" – of the researcher and the participant, so that:

> "[…] both come to the point when each is committed to help the other achieve his or her goal, when informants participate in providing information for 'the book' or the study, and when the researcher approaches the interaction in a respectful and thoughtful way that allows the informant to tell his or her story" (Dewalt and Dewalt 2002:40).

Throughout his introduction to participant observation, Jorgensen stresses the necessity to develop trust and cooperation with one's research participants, emphasising particularly that "The quality of data is improved when the participant observer estab-

lishes and sustains trusting and cooperative relationships with the people in the field" (Jorgensen1989:69). For Okely, participant observation in the anthropological sense requires a "chronology of understanding and trust" (2012:84), and Wolcott (1995:91) emphasises the ambivalence of rapport, underlining that the term often masks a great deal of anxiety in fieldwork, i.e. what happens if we fail to build such rapport? Field-work textbooks routinely stress that *we* have to act in such a way that our interlocutors are able to trust *us*. Yet, under surveillance, trust becomes highly problematic in the reverse way: whom can *we* trust? With whom can we share the goals of our fieldwork? Who amongst our *informants* might also be an *informer*? Who might perhaps betray our trust and take information about our research or data generated with our partners to actors or institutions that may use them for illegitimate and harmful purposes?

Surveillance often operates in a way that it makes itself known, at least to some extent. Also, in many cases, secret services are not *that* secretive, and so we often know or at least sense that we are being watched, and this necessarily changes our way of acting in the field and the manner in which we view social relations. Most importantly, through this panoptic experience, we become suspicious ourselves, in that the changes that are effected by us, knowing that we are under surveillance, are probably intended. Writing on surveillance in the early history of the Soviet Union, Peter Holquist notes:

> "Surveillance, then, was not designed to uncover popular sentiments and moods, nor was it intended merely to keep people under control; its whole purpose was to act on people, to change them. So the surveillance project encompasses both the attempt to gather information on popular moods and the measures intended to transform them" (Holquist 1997:417f).

Comparable with the practices of colonial governance that Thomas (1994) recorded, the quite obvious practice of surveillance is not simply meant to collect information; much more importantly, it has the intended effect of creating the idea and experience of a powerful state and of constituting the "ambit of state control" (Thomas 1994:124).

Surveillance has strong disciplining effects on fieldworkers, because, knowing that we are watched, we become very careful about where we go, whom we meet, which topics we address and what to ask. The contribution of Anna Zadrożna in this volume shows that these effects may be embodied by fieldworkers and have long-term harmful consequences. Such disciplining effects extend to the whole social field in which we work, our interlocutors included, because even though they may not harbour the suspicion that we might be spies, knowing that we are under observation forces them to be equally careful and reserved. Not all of them have an outlook on life that enables them to joke about the 'paranoia' of state agencies, as did Mohammad Ali in our introductory example; sometimes, a surveillance agency interferes much more directly than by just 'making itself known', by prohibiting access to particular places, institutions or persons, spreading malignant rumours or issuing direct threats to fieldworkers as well as to her or his interlocutors. Occasionally, fields are pervaded by surveillance to the extent that mistrust becomes a dominant social attitude, as in the case analysed

by Aurora Massa in this issue, and even if the fieldworker is not directly or perceptibly targeted herself, the building of rapport becomes very difficult whereby relationships of trust are perhaps outright prevented or at best limited to a very few persons in the field. With such trusted interlocutors, however, working under surveillance may produce a special bond of solidarity and create complicity among the victims of scrutiny, as shown in this issue by Angela Stienen's example from Colombia. This has a decisive and negative impact on fieldwork, though, as research is then probably limited to a specific and probably rather narrow network of 'conspiring' research partners, while others remain difficult to access or are left out altogether.

The actions of surveillance agencies are often not predictable, though it should be noted that such unpredictability is a significant strategy of power, as it generates a diffuse yet pervasive atmosphere of insecurity and vulnerability through which a hierarchy of power and vulnerability in relation to the researcher and participant is re-established, albeit in a more complicated and twisted way than the 'linear hierarchy' of colonial anthropology. Nowadays, the decisive relationship of power exists not between the researcher and the participant but in the relationship each of them has with the agents of surveillance. Both may be subject to interventions and threats, but while in most cases the threat to the researcher will be limited to the danger of being expelled from the field, thereby rendering his or her career vulnerable, the participant may suffer much more existential consequences that include threats to his or her employment, freedom or even life. Sometimes, the researcher is only indirectly affected by surveillance and threats that directly target her or his partners.

Like all social (and power) relations, surveillance dealings are strongly gendered, and diverging and perhaps contradicting gender norms and expectations of local participants and researchers often complicate fieldwork. Moreover, transgression of local norms may evoke particular suspicion, and surveillance provoked this way may include sexualised threats and assaults. As Anna Grieser and Anna Zadrożna discuss in their contributions to this volume, female fieldworkers that are subject to the surveillance of (often male-dominated) agencies may suffer a particular vulnerability that includes the fear of sexual violence, thus resulting in heightened mistrust and anxiety. In this vein, unwanted social relations or misbehaviour in gender-segregated contexts may create mistrust that can break off rapport and result in expulsion from the field site. Reports or rumours about sexual indiscretion, issued by powerful local actors, can trigger a gendered version of surveillance which is as harmful as control by any other agency. Hence, practices of surveillance and gender norms are often intertwined and mutually amplify the female researcher's, as well as the female research partner's, vulnerability. This encounter with gendered surveillance may therefore even strengthen sanctions of gendered norms.

Effects of surveillance on field methodology

If we do not want to run the risk that our research is quickly stopped, the experience and even the mere suspicion of being under surveillance require the adaptation of our methodology and the adoption of particular strategies in the field. In a way, this is already a self-disciplining effect of surveillance, because often we know or suspect in advance that a particular research site will be surveilled, and so we try to prepare accordingly for this situation. Under surveillance, fields and topics have to be approached more 'laterally', by cautiously attempting to assess possible traps and threats, because a head-on approach might alert the agents of surveillance and create unsurmountable obstacles for research. We need to identify and circumvent, or find ways to deal with, critical issues and potentially adverse actors. While it is true that every field of research should be entered cautiously and in a non-confrontational way, that careful exploration is a necessary phase of all ethnographic fieldwork and that every fieldworker has to resort to some kind of impression management (Goffman 1959), all of this is much more delicate in a field of surveillance and has potentially far-reaching consequences. While sometimes avoiding watching eyes is the advised strategy, other cases require continuous direct negotiation with them – it is part of the uncertainty generated by surveillance that we probably do not know outright how to handle the situation. Furthermore, these watchers may act in very diverse ways: while some issue explicit rules and restrictions, as in the case analysed by Anna Grieser in this volume, others, as shown in the example of Agnieszka Joniak-Lüthi, may require constant guesswork about what is somehow possible and what might be a transgression. However, explicit restrictions also never allow us any sense of certainty about what we can safely do, as Grieser's experience shows. Furthermore, the strategies of surveillance may change during fieldwork.

In spite of the critique of anthropological constructions of "the field" and the fixing of particular topics to certain sites (Appadurai 1988; Gupta/Ferguson 1997), the fact remains that there is often a strong relationship between particular research questions and specific locations. Some topics can be researched better in particular places, especially with the usual anthropological toolkit that requires proximity and a certain intimacy with the people engaged in that field. What we are able to learn has a very close relationship with our spatial location(s). Knowledge often has a strong spatial and temporal dimension. Time frames and locations are often contested themselves and are therefore subject to particular surveillance, perhaps because of ongoing resistance against disputed constructions, industries or mining or their 'strategic' positioning. After initial exploration we might come to the conclusion that we have to change our topic and/or site of research, as our original research questions, the places and moments in which we wanted to ask them and any associated 'local knowledge' are clearly off limits and would endanger not only our project, but also our interlocutors. Thus, considerable flexibility and readiness to alter timings, sites and questions are often significant elements for a methodology under surveillance. We may decide to work more at a distance, but the

networks of surveillance are often as far-reaching as are networks of unwanted activism against some controversial project, and such distant research may still have problematic effects on local actors. In addition, sometimes, the field itself has to become mobile, because movement in some contexts does help escape watchful eyes and ears.

Surveillance restricts our range of gathering knowledge, because certain sites, interlocutors and questions remain inaccessible, which is a specific aspect of the general condition of the incompleteness of knowledge. Our 'truths' always remain partial, as James Clifford (1986) has reminded us. An important way of making sense of the 'part' we somehow manage to know is to reflect upon the conditions that enable us to access this particular chunk of knowledge, shape (and limit) our perspective and obscure and preclude others. Surveillance actually imposes a specific topic on our research: the analysis of power, because doing fieldwork in this environment means doing (mostly unintended and unwanted) participant observation within a specific yet pervasive set of power relations. All of the examples herein show that a way to make surveillance productive for the anthropological project is to take the challenge and analyse the power relations involved in and unveiled by practices of surveillance.

Although 'covert research' is generally rejected by anthropological codes of fieldwork ethics – we come to this in the next section –, research under surveillance will almost necessarily not be fully open. It should not (and cannot) be secret – we are not intelligence agents, after all –, but we have to consider *how* open research can take place and to whom we can disclose what. Formal methods, especially those that involve some kinds of instruments or artefacts, are often problematic, as they make it difficult to turn a potentially precarious research situation instantaneously into a casual and unsuspicious everyday conversation – a strategy that may be required if suddenly the 'wrong people' appear on the spot. The use of a voice recorder is probably not advisable in this situation, and even taking copious notes may be problematic. Consequently, monitored fieldwork will rely to a great extent on memory, which brings its own practical and epistemological difficulties. Perhaps, then, formal interviews become impracticable and research has to be based much more on informal talk. However, as we said before, efforts to be unobtrusive may also be considered particularly suspicious. Thus, in some cases, formal methods and in particular surveys are perhaps particularly useful to show the harmlessness of research, i.e. that the questions being asked are actually unobjectionable.

Yet, in addition to the 'methodology of talking' that dominates typical fieldwork, ethnography under surveillance also requires a methodology of silence, in that we need to learn *not* to ask (Massa, this volume) and know which topics should be avoided (Joniak-Lüthi, this volume). In these cases, silence is not less dialogic or communicative than speaking, as it is a significant response in our relationships with our interlocutors. Perhaps relationships are even built through jointly keeping silent. Silence is not a void, then, a simple absence of words, but it does need to be practiced, interpreted and understood. Silence and silencing produce a significant proportion of our data.

While surveillance has decisive effects on how we can do fieldwork, it is impossible to formulate a clear-cut methodology of fieldwork under surveillance that would be

able to counter these effects. Nonetheless, the unpredictability of surveillance measures and their consequences has to be mirrored in our methodologies: we need to deal with precarious ambiguities, to juggle with what we don't know and to take both our research interests and the safety of our research partners into account. What Kovats-Bernat writes about 'dangerous' fields applies equally to fields under surveillance:

> "[...] we must begin with a fundamental shift in how *methodology* is defined – not as a rigid or fixed framework for the research but, rather, as an elastic, incorporative, integrative, and malleable practice. It should be informed by the shifting social complexities unique to unstable field sites and should depend on a level of investigative flexibility on the part of the ethnographer, who cannot always be expected to work in safety and security" (Kovats-Bernat 2002:210).

Ethical dilemmas and uncertainties

These rather cursory thoughts about fieldwork methodologies under surveillance make clear that working in such contexts is almost inherently problematic from an ethical viewpoint. Fieldwork ethics require us to ensure that our partners will suffer no harm as a consequence of their participation in our research. Furthermore, as the ethical codes of anthropology were originally drafted in order to denounce clearly covert investigation, the openness of research and in particular the informed consent of the people in the field is emphasised. Thus, the Code of Ethics of the American Anthropological Association demands that:

> "In both proposing and carrying out research, anthropological researchers must be open about the purpose(s), potential impacts, and source(s) of support for research projects with funders, colleagues, persons studied or providing information, and with relevant parties affected by the research" (AAA 2012:360).

Furthermore, it requires that:

> "[…] anthropological researchers should obtain in advance the informed consent of persons being studied, providing information, owning or controlling access to material being studied, or otherwise identified as having interests which might be impacted by the research" (AAA 2012:360f).

Most anthropologists would agree that in the context of anthropological fieldwork informed consent which is formalised through paperwork and signatures, needed to placate institutional review boards, is absurd.[2] In a surveillance situation, signed consent documents can even endanger our partners, as these papers are proof of their

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[2] It seems that sometimes review boards are another agency of surveillance with which anthropological research has to deal (Shea 2000). In a way, informed consent in strict terms is fundamentally problem-

probably unwanted participation. In essence, formalised informed consent would have precluded all of the research projects discussed in this issue, but also the less formalised form of informed consent that is advocated by the AAA Code of Ethics is problematic in fields under surveillance and would often preclude such research.[3] A crucial point is that consent often cannot be obtained *in advance*, as the AAA demands. As we wrote in the previous section, when entering the field we have to be very cautious not to alert surveillance agencies and to largely pass under the radar. As we do not know who is linked with these agencies and who is not, we cannot disclose the purpose of our presence in the field from the outset openly and indiscriminately to all actors; rather, we have to work our entry slowly, finding out who is who and to whom we can disclose what, without putting not only our research in jeopardy, but also the whole field in a state of turmoil. Additionally, surveillance agencies "own or control access to material" and "have interests which might be impacted by the research," as the AAA's specification of those from whom informed consent is required reads. But probably the AAA does not propose to inform a 'concerned' intelligence agency, to take this example, about a planned research project. The general norm of informed consent departs from the assumption that the anthropologist is in an unambiguous position of power vis-à-vis her or his counterparts,[4] though things are more complicated, as we have seen – and not only in fields of surveillance. Practices of informed consent need to reflect power relations in the field, then, in line with the question "Is everyone equally deserving of informed consent?" (Thorne 1980:293).

However, it is not sufficient, and also not possible, to maintain the norm of informed consent by simply reserving it for the powerless and excluding bodies such as intelligence agencies. Power is distributed social relations, as Foucault taught us, and often it is difficult to tell the powerless from the powerful. Surveillance agencies particularly have often widely infiltrated society so that it is difficult or even impossible to know which person in the field has what kind of relation with an agency of surveillance. Individuals are often ambivalent and at times even forced to pass on certain information, and beyond easy moral judgements, informants and informers are sometimes the same persons. Also, bodies that are less threatening than intelligence agencies often try to obstruct access, in order to keep certain things out of view. Such bodies may include international NGOs or even humanitarian organisations, and so in such cases asking for informed consent might amount to inviting a kind of censorship of our research, which in turn would conflict with another postulate of anthropological ethics, namely

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atic in anthropology because of the openness of ethnographic fieldwork. Strictly speaking, even the researcher is not informed from the outset about where the research process will take her.

[3] There is a longstanding, voluminous critical discussion on informed consent in even much less problematic and difficult fields. Being honest, we have to admit that social research – like any social interaction – almost necessarily involves a certain degree of secrecy and deception. Openness is a matter of degree. See e.g. Mitchell 1993 and Thorne 1980.

[4] See Wax 1977 for a critical discussion of this issue.

the pursuit of knowledge and the integrity of science. If we do not outright refuse to do research in such contexts because we cannot comply easily with the norm of informed consent, we need to consider carefully whom in the field we can tell what and how much. Furthermore, it could even sometimes be regarded as an ethical imperative to do research in such fields, because this may be the only chance to generate knowledge which should not be kept local; on occasion, research has to be subversive. Perhaps, also, some of our interlocutors might be interested in us doing research even if it puts them in certain danger. Fieldwork under surveillance is therefore much more an issue of balancing contradictory ethical demands of "ethics in practice" (Guillemin and Gillam 2004), which probably does not result in a satisfactory solution, than of complying with pre-established rules. In his discussion on the complexities of informed consent, Marco Marzano argues that we should regard ethical problems during fieldwork not as accidents or as false moral steps but as stages of a "moral career" in which we have to come to terms with our own and others' moral identity (Marzano 2007:431).[5]

These considerations are certainly not intended as a plea for covert research, but fieldwork under surveillance requires a heightened degree of impression management and working "behind many masks" (Berreman 1962). A certain degree of deception is almost necessarily also required in order to protect one's partners and oneself in such contexts, and masquerade and deception are the 'weapons of the weak' in fieldwork facing powerful institutions. In the same vein, the practice of research needs to take particular care to ensure anonymity and protect the identity of our partners. In addition, measures for the strict protection of data are required.

A final ethical issue that has to be considered by researchers and their supervisors is the question of when to abort research. When do surveillance and the related practices of power reach a level that we have to abandon the field, in order to protect our interlocutors and ourselves? Akin to the 'boiling frog experiment', we perhaps would refuse even to start fieldwork if from the outset we were confronted with the same practices of surveillance that we might tolerate if they develop gradually in the course of our research. The termination of research becomes more difficult with the passing of time, as we have already invested considerable time and effort, are in the process of collecting data and hope that somehow we may manage to conclude our project successfully. Again, there is no easy and universal gauge, but while some researchers might be ready to take more risks than others, research should certainly not continue where the researcher and his or her partners run the danger of severe physical and/or psychological harm.

Reflecting upon 'fieldwork under suspicious eyes' we come to the conclusion that it is not fundamentally different from 'normal' fieldwork. The points discussed so far –

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[5] In order to emphasize that fieldwork ethics is not an issue of following fixed norms but of constant reflection and negotiation, the German Anthropological Association has formulated its Frankfurt Declaration about ethics in anthropology in the form of questions that require continuous consideration during all stages of fieldwork (German Anthropological Association 2008).

rapport, suspicion and trust, the possible necessity to change methods and topics, limitations of knowledge, power relations in the field, informed consent and impression management – are significant issues involving "inherent dilemmas" (Wolcott 1995: 123) for all kinds and contexts of anthropological fieldwork. Instead of being a totally different affair, fieldwork under surveillance is confronted with particular conditions and constraints that amplify the predicaments and difficulties of all ethnographic research. The discussion of surveillance thereby serves as a looking-glass that helps identify and discuss issues that are critical to anthropological fieldwork in general.

Contributions to this issue

Anna-Grieser takes us into the high mountains of Gilgit-Baltistan in northern Pakistan, where she did fieldwork on water systems under the intimidating surveillance of intelligence agencies. Her experiences actually triggered the workshop from which this special issue derived. Grieser analyses the surveillance to which she was subjected within a complex intersection of the political condition of a strategically important yet simultaneously marginalised area and of local gender norms which render a single female researcher particularly suspicious. The agency's interference forced her to change her topic and to limit her methodology, both of which left her with fragmented sets of data which, following Marcus' (2009) argument about the necessary incompleteness of ethnography, the auto-ethnography of surveillance enables to contextualise.

Similarly, Agnieszka Joniak-Lüthi had to cope with state surveillance during her fieldwork on infrastructure in the Chinese province of Xinjiang. Nevertheless, she experienced less direct interference but more constant anticipation of surveillance, which in turn created self-imposed censorship on herself and on her interlocutors. Joniak-Lüthi had to deal with multiple modes of silencing and lacuna as strategies of protection against saying things that could be controversial, which made the pervasiveness of state power and control palpable. The fieldworker's "participatory training in muting," as she calls it, enables her to "hear" silences that pervade the field and which cannot be elicited through verbal techniques. Long-term participant observation is the only possible way to grasp the power relations that entangle research participants in multiple ways.

In the course of several research projects, Anna Zadrożna kept returning to "The Village" in Macedonia, which she presents in her contribution as "a difficult field". She experienced how the interplay of gendered norms, suspicious mutual observations of villagers and the presence of specialised police bodies created an extended field of tension, fear and uncertainty. Perceived as a spy or an otherwise suspicious outsider, she could not rely on relations of trust but had to deal with blurred boundaries of surveillance and counter-surveillance, well-meant warnings and rarely open threads. She adopts the notion of "protective surveillance" to describe the repeated assumptions

of surveillance that are presented as well-meant advice. Surveillance and strategies used to demystify the assumption of being a spy have multiple effects in Zadrożna's case, in that they create not only anxiety and paranoia, but also creative data collection, since recording and writing are difficult in her "dangerous field".

Angela Stienen provides a different example of how the generalised condition of surveillance shapes people's everyday life and the anthropologist's experience. Her study on urban transformation programmes in Medellín, Columbia, back in the 1990s was monitored by armed groups of teenagers and children who exercised territorial control over the urban space. This "forced protection" was a result of the private security industry that was challenging the ideal of the state as the only authority with a legitimate right to violence and triggered competition over the right to protect. Nevertheless, in Medellín, the shifting alliances between the state and these groups of minors created a period of precarious peace for the neighbourhood in and with which Stienen worked. Instead of showing why she could not gain trust, and thus had to develop alternative methodologies, she discusses how under these obvious conditions of surveillance trustful relations were possible, not *instead* but *because* of forced protection.

During her study among Eritrean refugees in Ethiopia, Aurora Massa was asked to remain silent and not to ask questions. Both Eritrea and Ethiopia are characterised by pervasive state surveillance, and the refugees are subject to intimidating control by both states. Massa was not allowed to work in refugee camps and thus did research among Eritrean university students. Yet, due to pervasive mistrust, fieldwork among the students was not easy, either. Mistrust characterised not only the relationships between the students and the state(s), but also intimacy among themselves. Massa became implicated in webs of silence and lies that were intended to cover compromising issues. She concludes that lies and mistrust are not obstacles of fieldwork that need to be overcome in order to reveal 'the truth' but as significant aspects of students' social context that need to be analysed.

While the previous contributions focused mainly on fieldwork under surveillance in state contexts, i.e. on suspiciousness 'from above', Tomaso Trevisani's article analyses suspicion 'from below' in the context of industrial production. In many industries, suspiciousness and the fear of being spied on play a very significant role. Having done fieldwork in a foreign-owned former Soviet steel plant in Kazakhstan, Trevisani articulates the difficulties in gaining access to the closed world of the plant and overcoming the suspiciousness of the workers on a particular shop floor where he was allowed to do research. Focusing on the serious mistrust of the steel workers, not only towards the fieldworker, but also among themselves, Trevisani analyses the "industrial production of suspiciousness" and comes to the conclusion that in times of restructuring, mistrust helps management to promote discipline and gain control over the labour process. Finanlly, in her afterword, Katherine Verdery offers her engaged reflections on the articles of this issue.

In the contemporary world, zones of danger seem to multiply and expand (Andersson, in press), and security has become a concern of utmost relevance that pervades

lifeworlds around the globe. Furthermore, surveillance has become one of the most important instruments purportedly meant to ensure this security. Thus, conditions of surveillance expand and multiply. We venture the prediction that anthropologists and anthropology will increasingly be required to deal with issues of surveillance, both as a topic and subject matter and as a concomitant circumstance of fieldwork. Calls for an "anthropology of security" (Goldstein 2010, Maguire et al. 2014) therefore need to be accompanied by a heightened debate on the methodological implications of surveillance. With this special issue we hope to make a significant contribution to advancing this debate.

Acknowledgement

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