The Mundane Politics of 'Security Research:' Tailoring Research Problems

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Abstract

Since the late 20th century, Germany's federal science policy has shifted towards an emphasis on commercialization and/or applicability of academic research. University researchers working within such strategic funding schemes then have to balance commitments to their government commission, their research, and their academic careers, which can often be at odds with each other. Drawing on an ethnographic study of the development of a 'smart' video surveillance system, I analyze some of the strategies which have helped a government-funded, transdisciplinary group of researchers to navigate conflicting expectations from their government, academia, and the wider public in their everyday work. To varying degrees, they managed to align conflicting expectations from the government and their departments by tailoring research problems which were able to travel across different social worlds. By drawing attention to work practices on the ground', this article contributes ethnographic detail to the question of how researchers construct scientific problems under pressures to make their work relevant for societal and commercial purposes.

Keywords: directed funding, commercialization, tailoring, boundary work, algorithms, surveillance technology

'Neoliberal technoscience' and directed research funding

Since 2007, the German Ministry for Education and Research has funded projects which are supposed to develop security technologies and procedures with a funding scheme called the "Security Research Program." The program has heavily emphasized the development of new surveillance technologies, such as those used to monitor urban spaces. Funding requirements for university researchers include the commitment to finding solutions to security problems, collaboration with small and medium enterprises, and the inclusion of social scientists or legal scholars. The research program's goal is to increase citizens' security through transdisciplinary research, and to strengthen the position of German companies on national and international markets by transferring the research to security products and services.

Directed funding schemes like the Security Research Program can be situated in an ongoing debate on 'neoliberal technoscience' and the increasing commercialization and applicability of scientific research. As Lave, Mirowski, and Randalls (2010: 667) point out, cross-cutting features of 'neoliberal technoscience' include, among other

things, the "rollback of public funding for universities" and "the separation of research and teaching missions, leading to rising numbers of temporary faculty." Particularly the rollback of long-term funding makes scientists more dependent on short-term directed funding schemes sponsored by industry or governments, and thus more amenable to the latter's demands to make their research relevant for societal or commercial purposes.

However, it remains a subject of ongoing debate how and to what extent knowledge production is changing under conditions of 'neoliberal technoscience.' Although scientists working in directed research projects have to anticipate demands for commercialization and social relevance if they want to obtain funding, it seems unlikely that they will give up their commitment to their academic disciplines. Academic institutions and organizations, in turn, may not always reward the kinds of research that governments or industry fund scientists to carry out. Thus, scientists working in directed funding schemes may have to navigate multiple and conflicting disciplinary, political and economic demands.

This paper explores the ways in which scientists deal with such conflicting demands in their everyday work. Although we have a fairly good idea of how organizations manage tensions resulting from the changing institutional landscape on an administrative level (Guston, 1999; 2001; Miller, 2001; Parker and Crona, 2012; Tuunainen, 2005a, 2005b; Tuunainen and Knuuttila, 2009; Wehrens et al., 2013), knowledge production 'on the ground' is still relatively unexplored. The aim of this paper is thus to contribute empirical detail regarding knowledge production under conditions of directed research funding, and to further the understanding of how scientists construct scientific problems under pressures to make their work relevant for societal and commercial purposes.

Drawing on an ethnographic study involving a transdisciplinary research group commissioned by the Security Research Program to develop an automated closed-circuit television system (CCTV), I show how scientists navigated conflicting expectations in their work by tailoring research problems that were able to travel across different social worlds. By tailoring research problems that fell into their departments' previous lines of research, but could also be interpreted as practical problems pertinent to surveillance systems, the scientists in my study managed to "keep politics near enough" to secure their funding, but "not too close" to interfere with their research interests (Gieryn, 1995: 434–439). However, tailoring their work also meant continuous 'articulation work' (Fujimura, 1987, 1996; Star and Strauss, 1999). The varying extent of the articulation work necessary to cope with conflicting expectations was tied to the ways in which they positioned themselves with respect to the government's demands: The more work they had to put into adjusting their scientific problems to conflicting demands over the course of their project, the more problematic was their experience of the government's demands.

Tensions, misalignment, and articulation in scientific work

A number of scholars have raised the question whether political efforts to commercialize university research have led to significant changes in academic practices and institutions. Drawing attention to modes of knowledge production, terms such as 'mode 2' (Gibbons et al., 1994; Nowotny et al., 2001), 'post-normal science' (Ziman, 2000) and 'academic capitalism' (Slaughter & Rhoades, 2004) attempt to capture the increasing importance of political and economic considerations in academic research. These models claim that such considerations shift the purposes of scientific work from understanding the basic principles of the natural world to the development of applicable and marketable technologies. Others have framed the question in more institutional and organizational terms, claiming that changing notions regarding the purpose of science are reflected in increased interdependencies between universities, industry and governments, eventually resulting in 'entrepreneurial universities' (Etzkowitz, 2003; also see Kleinman and Vallas, 2001 on converging academic and corporate cultures).[1]

More recent work has provided plenty of evidence that changes are, by far, not as sweeping as earlier attempts to capture 'neoliberal technoscience' have suggested. This work has examined

in more empirical detail how university-based scientists and organizations perceive and deal with the complexities of their changing environments. For example, scientists display varying attitudes concerning engagement with corporate or policy actors, ranging from advocating engagement to outright resistance (Goldstein, 2010; Holloway, 2015; Lam, 2010; Owen-Smith and Powell, 2002). What seems to account for the variety of attitudes among scientists is the fact that the current ecology of academic knowledge production is one of multiplying contradictory regimes, logics, or social worlds (for different takes on the theme of multiplicity, see Miller, 2001; Owen-Smith and Powell, 2002; Tuunainen, 2005b; Vallas and Kleinman, 2008).[2] On the individual level, tensions resulting from conflicting social worlds may be experienced by scientists as considerable 'role-strain' (Boardman & Bozeman, 2007).

The bulk of the literature has emphasized how organizations manage such tensions on an administrative level, emphasizing a struggle over resources. In the case of private companies using university resources ('hybrid firms'), tensions may be managed through geographical or physical separation and formal redistribution of academic and corporate roles and resources in an attempt to maintain what are perceived as traditional cultural boundaries (Tuunainen, 2005a, 2005b; Tuunainen and Knuuttila, 2009). In the case of specialized 'boundary organizations' dedicated to coordinating and facilitating research spanning multiple domains (i.e. academia, corporations, and policy), struggles may be managed through the provision of resources and legitimacy for 'hybrid research' and by negotiating multiple stakeholder demands (i.e. Guston, 1999, 2001; Miller, 2001; Parker and Crona, 2012; Wehrens et al., 2013). With its slightly more functionalist slant, the notion of boundary organizations has gained particular popularity, as it asks what conditions enable such 'hybrid spaces' to successfully coordinate and facilitate 'hybrid research.' Interestingly, the literature suggests that boundary organizations, despite their considerable efforts, are rarely successful in resolving occurring tensions in the long run.

We know less about the ways in which scientists deal with conflicting demands on the ground in their everyday work. Accounts of how scientists construct and go about their scientific problems under increasing pressures to make their work relevant for social or commercial purposes are also sometimes difficult to reconcile. For example, while Cooper (2009: 648) argues that "commercially engaged scientists […] are more likely to express the importance of market-oriented solutions," Calvert's (2006) work suggests that scientists might only do so strategically to secure funding, while they continue with their previous lines of work regardless of their funders' demands. On the other hand, Parker and Crona's (2012) study suggests that scientists choose their problems and approaches according to who the most powerful stakeholder is at a given time, perhaps slightly understating scientists' agency and perspectives. The picture painted here is one in which scientists either do what they want regardless of the conflicting demands posed on them, or simply obey the 'most powerful' stakeholder at any given time.[3] What is missing from these accounts is a deeper analysis of how scientists struggle through conflicting demands, how these struggles shape their work and, in turn, what kinds of working processes and objects make navigating conflicting demands more or less feasible. Paying attention to conflicts and processes might also enable us to better understand why scientists position themselves differently under similar conditions, and why this is easier for some more than others.

Social worlds/arenas theory is useful to analyze how scientists navigate what they experience as competing demands, because it focuses on conflict and process, and because it offers a range of sensitizing concepts for the analysis of scientific work (Clarke, 1991; Clarke and Star, 2003, 2008; Gerson, 1983; Strauss, 1991). From an interactionist perspective, academic disciplines and specialties can be viewed as social worlds, as groups which share commitments to common activities, as well as resources and ideologies stipulating how to go about their work (cf. Clarke, 1991: 131; Strauss, 1991). Social worlds lack clear boundaries and can be laced with conflict, but can more or less coincide with formal organizational structures such as university departments. This is a situation where university researchers have to

navigate demands both from their specialty fields and from their respective organizations.

Demands put forth by directed funding schemes, such as the German Security Research Program's demands for applicability and commercialization, can then be viewed as posing another set of constraints on participating university researchers. Since at least the 1990s, long-term funding and numbers of tenured faculty in Germany have declined in relation to student numbers, a development which has in turn increased the importance of third-party funding for faculty to conduct their research and to fund their doctoral candidates and postdoctoral researchers (cf. Kreckel, 2008). If 'soft money' from the government becomes increasingly important to conduct research and fund academic staff, but at the same time is increasingly tied to demands for applicability and commercialization, scientists in Germany are likely to be more amenable to these demands. Because scientific and practical problems are not necessarily congruent, however, current government discourses via directed funding programs turn university researchers' workplaces into an arena rife with potential conflict in which scientists have to balance commitments to their research, their academic careers and political demands for marketable technologies. I therefore understand the commercialization pressures scientists face as a need to simultaneous negotiate multiple commitments in misaligned or competing social worlds.

It is useful to remember that misalignment between scientific work and social worlds is not an unusual feature of scientific work. Scientists routinely have to coordinate their work with their departments, their disciplines, or their funders through a mundane process of continuous reorganization and tinkering (Fujimura, 1987, 1996; also see Knorr Cetina, 1981). This means that, in addition to their intellectual labor, scientists have to "articulate alignment" – "pulling together everything that is needed to carry out production tasks: planning, organizing, monitoring, evaluating, adjusting, coordinating and integrating activities" (Fujimura, 1987: 258). Articulation work feeds back into the construction of scientific problems, creating scientific problems which are 'do-able' (Fujimura, 1987) given available skills and resources, connect to concerns in wider fields of research or disciplines, and are interesting for funders.

Articulating alignment in scientific work is more likely to succeed if abundant resources are available. For example, in cases where demands cannot be reconciled and resources are available, scientists may split and package their work, and outsource undesirable tasks to subcontractors (see i.e. Baumeler, 2009; Fujimura, 1987, 1996). Such divisions of labor allow scientists to pursue their scientific interests while at the same time formally satisfying their funders' demands. However, if the resources to do this are lacking, as was the case in my study, scientists may tailor their research problems to fit the needs of what they see as conflicting demands from misaligned social worlds. Calvert (2006: 208–9) defines tailoring as researchers' efforts to "make their work appear more applied to gain funding and resources."

Extending Calvert's concept of 'tailoring,' I understand it as a specific instance of articulating alignment under conditions which pose strong constraints on articulation work. Tailoring can be generally understood as the mutual translation between researchers' scientific interests and practical problems. There are at least two kinds of tailoring, which are likely to transition into one another iteratively during the research process, but which can be distinguished by their purpose and process. *Forward tailoring* serves to obtain funding by translating practical problems articulated by funders into scientific problems. This is the original meaning of Calvert's definition stated above. The typical case for this kind of tailoring occurred in my study in the process of writing grant proposals for directed funding schemes. However, I also observed a second kind of tailoring, which I term *reverse tailoring*. This strategy reacts to existing research problems which were ill-fitted to the needs of the different social worlds involved in the research process. The typical case for this kind of tailoring occurred in my study if research problems fit the needs of the funders, but not what scientists see as the needs of their discipline. In such cases, scientists translate problems which are interesting to them and feasible with the available skills and resources into new problems which are close enough to what they anticipate

to be the practical problems funders want solved. Reverse tailoring serves to keep existing funding which would be risked if they were to diverge too much from funders' demands, while at the same time allowing scientists to pursue their research interests. Both kinds of tailoring serve to protect researchers' relative autonomy against what they perceive as increased pressures to produce commercial and/or applied research, and, in a reading more focused on power relationships, can thus be understood as a specific kind of 'boundary-work' (Gieryn, 1983, 1995, 1999).

The German Security Research Program

This paper is based on ethnographic fieldwork in which I accompanied a transdisciplinary group of researchers based in universities, research institutes, and companies who were commissioned to develop the software for an automated closed-circuit television system (CCTV) within the German Security Research Program.[4] The researchers tried to mechanize surveillance processes in order for the systems to identify 'dangerous' behavior and situations automatically and in real-time, and to alert the human security staff in such cases. The idea was that operators do not have to watch the screens at all times, but are alerted by the systems in an event of interest.

In its first round (2007–2012), the program has mainly funded the development of security and surveillance technologies. By investing in university and corporate research and development, the program's overall goal is to increase citizens' security, and to strengthen the competitiveness of German medium-sized technology companies on international markets. To ensure that the research meets these goals, the government has formalized its demands in the program's funding requirements and review criteria.

In terms of content, research projects have to clearly outline how they plan to contribute to the solution of national security problems. Mobilizing imageries of crime and terrorism, and referring to the limited capacities of human security staff, the government expects the researchers to develop technical fixes to social problems of crime and terrorism, as well as to increase the efficiency of surveillance processes by mechanizing them:

> Do operators always react instantly when seeing something conspicuous on the screens? Unfortunately not, because it would require a lot of people to monitor 1,700 camera screens. […] In order for the system to detect further – and very diverse – conspicuous events on its own, we need to turn to science. […] The software would have to analyze the passengers' movement in the footage and filter all movements of normal speed. What movements are typical for violent crime? It will be necessary to identify this. There is a lot of work ahead for the researchers.[5] (Bundesregierung, 2011; my translation)

Government expectations concerning crime, terrorism, and security work indicate a shifting political understanding of university researcher's professional 'jurisdictions' (Abbott, 1988). Implicit in expectations to contribute to the solution of security problems is the government's understanding that academic researchers can act as experts on crime and terrorism. Similarly, the government's expectation that new technology should render surveillance processes more efficient and effective assumes that engineers can act as experts in security work.

The government expects researchers not only to assume responsibility for solving security problems, but reframes their work explicitly as an economic activity:

> Through research and innovation, [the Security Research Program] offers the possibility of promoting the competitiveness of the companies involved, as well as their security technologies' marketability, to establish security as a national, locational and economic factor, and to open up possibilities on a European level. (Bundesministerium für Bildung und Forschung, 2007: 7; my translation)[6]

Pressures for commercialization are particularly pertinent to the technological projects funded by the Security Research Program. These expectations are formalized in an explicit obligation to transfer the research into products or patents ("*Verwertungspflicht*"), thus encouraging researchers to orient their work towards economic growth and international competitiveness.

In terms of organization, research projects are required to work in a transdisciplinary fashion,

collaborating not only across disciplines, but also with end users and small and medium enterprises. In order to shorten the duration of technology transfer from research to market, the government has formalized the involvement of small and medium enterprises in its funding requirements. By incorporating both end users and industry, the government hopes to ensure the development of useful technologies.

Finally, particularly with controversial technologies – surveillance technologies being a prime example – the government has incorporated additional reflexive mechanisms to account for potential undesirable consequences, perhaps also for reasons of legitimacy. Because the program puts heavy emphasis on applicability and commercialization, the government expects research projects to calculate the possible social consequences of the security technologies' use. In order to monitor the projects for possible undesirable implications, the government has made it mandatory for technological projects to work with social scientists or legal and ethics scholars.

The Security Research Program's criteria are put through an altered review and selection process which differs significantly from traditional peer review. Instead of recruiting reviewers from within academia, and selecting them according to their specialties, it outsources the review and supervision of projects to a spin-off organization of the Association of German Engineers (VDI). Employees of this organization are responsible for both reviewing grant proposals and monitoring projects. Although some of them have a doctoral degree in the natural or engineering sciences, they have left their academic career path to be employed full-time by this organization. Once these employees have made their initial selection of grant proposals, they forward the project proposals to the Federal Ministry of Education and Research for final approval. The way in which the Security Research Program structures its review process and project supervision thus shifts discretion from academic review panels ('traditional' peer review) to bureaucratic entities, and can be read as the German government's expansion of social control in order to protect its investments.

Developing a 'smart' CCTV system

The researchers in my study applied to the program by proposing to develop the software for an automated CCTV system. University researchers included computer scientists, geoscientists, electrical engineers and legal scholars. Furthermore, the project included members of two private research institutes who were mainly computer scientists by training. On the corporate side, the project comprised a consulting agency that carried out cost-benefit analyses and an IT company which was supposed to integrate the system for technology transfer. Finally, the project included two officers from regional police crime units, who were expected to share their expertise in detecting criminal behavior. The project was relatively large, and at different times involved between 25 and 30 members, about half of whom were university researchers. In my analysis, I have focused on the university researchers involved in the project. Thus, when in the remainder of this paper I refer to researchers, I mean the project's senior scientists on the faculty level, as well as their doctoral candidates, all based in different universities across Germany. I have substituted all names, places, and unique technical terms with pseudonyms.

The group's goal outlined in the grant proposal was to mechanize surveillance processes in order for the system to identify 'dangerous' situations automatically and in real-time. Their idea was that operators do not have to watch the screens at all times, but are alerted by the system to an event of interest. They argued that their surveillance system, in contrast to non-automated CCTV systems, would facilitate intervention before the fact, and would also reduce personnel cost through automation.

The Security Research Program, as outlined above, expected the group to develop technical fixes to social problems of crime and terrorism, and to increase the efficiency of surveillance processes. Furthermore, they expected the group to consider privacy regulations in the system's design. These expectations refer to two separate groups of actors: solving problems of crime and terrorism and considering privacy regulation both refer to monitored individuals, while increasing the efficiency of surveillance work refers to human operators and security staff. In what follows,

therefore, I show how the researchers navigated expectations from academia, the government, and the wider public in their work by analyzing how the researchers classified deviance and conformity of monitored groups, and how they mechanized the work of human operators.

The selective memory of 'smart' CCTV

The German government expected the research group to consider possible undesirable consequences of their surveillance system's use. As in most technological projects funded by the program, this meant reducing all possible social implications to data protection issues. Data protection guidelines are relatively well institutionalized in Germany's legal code. Video footage may usually be stored up to 24 hours; longer storage is only permitted in case of a reported criminal incident. To account for privacy rights, the Security Research Program has made it mandatory for developers and legal scholars to collaborate.

Over the course of the project, the researchers never openly questioned whether the expectation of "privacy-friendly security solutions" (Bundesministerium für Bildung und Forschung, 2012: 7) was a legitimate one, but, on the contrary, situated themselves as researchers sensitive to the risks of privacy violations. However, they did struggle intensely with the negative public responses to their work. All researchers were acutely aware that privacy in relation to surveillance technology is a highly controversial issue of public debate in the German media landscape. They actively monitored the criticisms of their work in the wider public sphere, which framed their work as a violation of privacy rights, and public responses to their work were a frequent topic of conversation throughout the project. Furthermore, many, particularly the junior researchers, struggled with the deeply political nature of their project. As Martin, the project's principal investigator explained:

> Personally, my assessment is that in Germany, people are very critical towards new technologies. That isn't only true for video surveillance [...] you can observe very critical attitudes in many areas which, to be sure, in many cases are justified. And I don't want to say that you have to accept everything uncritically, but the range is relatively broad […] I don't want to say it's better in other countries where it's perceived less critically, but it's a broad area – let's not discuss this too politically now. (Interview with principal investigator Martin, January 2011)

We can see that Martin is pulled in different directions by what he perceives as conflicting demands from the government and the wider public: While the government expects the group to contribute to public and private surveillance, he assumes that part of the public condemns the development of new surveillance technology. On the one hand, he recognizes that critical engagement with surveillance technology is necessary while, on the other hand, he cannot delegitimize his own work. Even though the researchers decided to build privacy-by-design measures into their system, the fact remained that ostensibly they were developing surveillance technology and thus contributing to public and private surveillance. His struggles were rooted in his personal political stances, as well as his commitment to his work.

Such tensions between conflicting expectations from the government and the wider public, as well as researchers' own ambivalence about surveillance resulted in ambivalence about whether or not they should include social issues as a legitimate part of their work. This is exemplified in how the researchers tried to explain their consideration of privacy regulation in the project:

> I already mentioned our colleagues in the data protection area. I mean, potentially, [the system] produces a large amount of personally identifiable data. Someone has to explain that to us engineers, because if you're not an expert you won't know if these are personally identifiable data or not […] so we're frequently discussing and thinking about how we can design [the system] technically in a way that data protection problems don't occur in the first place. (Interview with principal investigator Martin, January 2011)

> At this point, I can already reveal [that] we have a special legal division here with us in the project. [...] I mean, they're specifically here to advise us, well, in our scientific ambition, not to do stuff that legislation explicitly prohibits. So we have to see that we somehow don't gather – what do you call that? – personally identifiable data. That means we

> have to, at the point where we collect data that in the end points to one specific person or thing – because certain regularities are saved too exactly – we want to try to make it so that the data base we create can't be used with abusive intentions, I dunno, to somehow discriminate against people. (Interview with doctoral candidate Robin, January 2011)

These quotes show that, on the one hand, the researchers tried to position themselves as sensitive towards possible undesirable consequences of their work by demonstrating that the group built privacy regulations into the surveillance system's design. To some extent, they broke down distinctions between 'technical' and 'social' problems, thus creating overlaps between the worlds of law and engineering. On the other hand, they point out that their work is controlled by 'external,' competent authorities. This is particularly clear in Robin's statement: Although the legal scholars were formal members of the research project, Robin situated them as external to the project, because he did not understand them as part of the "scientific, ambitious" collective identity which developed the system. By underlining external authorities, he also drew a line between the researchers who follow their 'scientific curiosity' in a sheltered university environment, and the legal advisors as experts for the real world 'out there.'

The researchers resolved conflicting expectations from the government and the wider public by assuming partial responsibility for possible undesirable consequences of the surveillance system's use. In collaboration with the legal scholars in the project, they decided to 'inscribe' (Akrich, 1992) privacy regulations into the surveillance system by minimizing the personally identifiable data – the actual video footage. This means that they discarded any actual video footage immediately after analyzing it, which would only take a few seconds. While there would be a live feed from the video cameras, surveillance staff would not be able to go back and sift through the footage to look for specific people and events. The researchers thus excluded information about single individuals from the database, and embedded 'memory practices' (Bowker, 2008) into the surveillance system that prescribed individuals' identities as irrelevant to surveillance processes. This is how the system's memory is "selective": As a consequence of the researchers' negotiation of conflicting expectations from the government and the wider public, only the temporal and spatial qualities of monitored individuals' movement remained. Thus, boundaries between legitimate and illegitimate tasks could only be drawn rhetorically, while in their work on the system there was no other option than to give way to pressures to consider possible undesirable consequences of their work. Following Latour (1993), the way in which they dealt with what they perceived as the critical wider public can be described as rhetorical 'purification,' which could not be maintained in their work on the ground.

Classifying 'dangerous' behavior

Because the government expected the research group to develop a technological fix to problems of crime and terrorism, the group had to classify 'dangerous' behavior in order to code it into the surveillance system (cf. Bowker and Star, 2000). The embedding of privacy regulations was consequential for how the researchers built concepts of deviance and conformity into the surveillance system. Because they only kept computer-generated trajectories of movement, they needed to come up with a theory of how to read dangerous behavior from nothing more than a movement pattern.

For the researchers, defining crime for the purposes of their surveillance system was highly problematic for different reasons. Robin, who was primarily responsible for the behavioral analysis component of the software, told me about the problems that emerged when he tried to obtain knowledge about 'dangerous' behavior from the police officers. He told me that the officers had handed him a list of 43 different dangerous situations that they would have liked detected by the surveillance system. This list included situations as diverse as people running into train tracks, drug trafficking, suitcase bombs, and assault and battery. Robin was not very happy about the officers' insights, and strongly problematized the indexicality (Garfinkel, 1967) of social behavior, which can only be meaningfully understood in context and specific situations:

> So the guy who drops a suitcase bomb, right? He'll be damned if he danced around before planting his bomb somewhere, he'll just walk past and discreetly leave the suitcase […] so I have problems with the very interpretation of behavior, because how can we project this merely visually detectable behavior onto some concrete intention? For instance, this here's a culprit and this is a normal passer-by. Well that's simply not quite possible without problems. […] We can't say every time someone zigzags that's a bomber or something. That means some things we're simply not allowed to do and certain things we're just not capable of doing. (Interview with doctoral candidate Robin, January 2011)

For Robin, crime and terrorism were not so much social problems to which he wanted to contribute a solution. Rather, crime or criminal behavior presented itself as a practical problem for his work. Particularly, and Robin repeated this throughout the following months, he did not see himself as *professionally competent* to define and code dangerous behavior:

> Drug dealing? Well, I have to admit with drug dealing we don't stand a chance except if people act particularly stupid somehow. The only thing that happens with drug dealing, so first [there] is the typical exchange: Two people meet physically, well they're at the same place at the same time. We can detect things like that, the problem is just that [with this procedure] we automatically suspect everyone else in the scene whose paths cross for whatever reason, right? […] we can't just say here, the typical drug deal has the duration of ten seconds [and] all other interactions take much, much longer, right? Then we'd stand a chance but, *who's supposed to decide this?* (Interview with doctoral candidate Robin, January 2011; my emphasis)

Robin did not perceive himself as professionally qualified to decide what might still count as 'normal' and what might already count as 'deviant' behavior; more importantly, he did not want to assume responsibility for such decisions, either. According to Gerson (1983: 367), questioning whether or not specific problems are a part of one's work is a typical indicator for problems of legitimacy: "The emergence of a new segment or intersection […] always raises the question: 'Is this new way really part of our work? Is it really X-ology?' Such questions are the essence of issues of problem legitimacy." Robin decided that defining dangerous behavior was not a legitimate part of his work, and forwent the original proposal's plan to classify different types of dangerous behavior. In contrast to the researchers' negotiations of privacy, there is little ambivalence about whether or not defining deviance and conformity was part of his job: Robin clearly rejected government expectations to act as an expert on crime and terrorism.

A couple of months later, I had the opportunity to learn more about how dangerous behavior fit into the project. I was invited to a meeting where all project partners presented the state of their research to the funding institution's representatives and discussed further steps. After all partners were finished with their presentations, the principal investigator of the legal unit pointed out that the researchers had not explained how they wanted to achieve the detection of dangerous behavior. He noted that this posed a problem to his work, as he needed to know the CCTV system's specific procedures in order to evaluate whether they were legal according to current legislation. Robin and Max, another geosciences doctoral candidate, sat next to me, disgruntled. Robin moved closer and whispered that he was scared of being forced to integrate even more problematic system functions into his already problematic work. As a result of the legal professor's request, and after some perplexity among the rest of the university researchers, the principal investigator decided to split up all participants into groups to discuss different dangerous situations.

Since I was particularly interested in the interaction between the researchers and the police, I followed the group which included Mr. Weber, one of the crime unit officers. The group hesitantly began to discuss the "storyline" of a situation in which the system might be used – note that, at this point, the project had already been running for almost a year. The group did not get much further than deciding the scenario's location (a train station), and the discussion was frequently interrupted by awkward silences. While the principal investigator tried to keep up the discussion, I

noticed that Mr. Weber remained silent. I found this strange because I expected this scenario to be his area of expertise, so I was surprised that he did not provide the researchers with more information about what it is like to survey a crowded train station. I was not the only one to notice this, and as the conversation came to a halt, the project coordinator turned to Mr. Weber and asked: "Mr. Weber, why don't you tell us how you in your work know when someone's up to something? You have the practical experience…"The group looked at Mr. Weber with undivided attention. Mr. Weber shrugged uncomfortably and responded: "Well, yeah, that would be great if you could deduce certain behavior from movement patterns..." This surprised one researcher named Jonas, who moved abruptly toward the officer and cried out: "Oh, so you don't know either!?" The officer said nothing and the group mumbled through the awkward moment (field notes, May 2011).

After one year into the research project, 'dangerous behavior' – the very linchpin of the project – turned out to be an empty signifier. On one hand, the police officer could not turn his implicit police knowledge into knowledge explicit enough to translate into machine-readable code (Collins, 2010: 138). The researchers, on the other hand, did not see themselves as professionally competent to define dangerous behavior. But what struck me was not that they both were not able to create a workable classification system of dangerous behavior, but that they left this issue unresolved, and that the university researchers did not seem to care too much about it. To the university researchers, defining dangerous behavior simply seemed not to be the most important or interesting part of their work. This shows how the government's expectations that they act as experts on crime and terrorism did not align with what the university researchers viewed as interesting research problems.

However, Robin still had to code a concept of deviance and conformity into the surveillance system, because this was what he committed to do when he signed up for the project. How did Robin achieve this? He translated the problems formulated in the grant proposal into problems that he felt actually able to solve by using techniques from his discipline with which he was already familiar. This means that he constructed 'do-able problems' (Fujimura, 1987) by modifying existing algorithms he had already worked with at his department. By using these algorithms, Robin created his own theory of dangerous behavior. More precisely, he borrowed from a project that developed GPS technology in order for biologists to track seagulls and map their flying routes. These seagull data indicated the individual seagulls' coordinates at any given moment – hence their movement trajectories were stripped of everything but their spatial and temporal qualities. Biologists could, for instance, see where the majority of the flock was, and where some seagulls strayed from it. As he explained to me later, the seagull movement became, per analogy, his theory of deviant behavior:

> This isn't about dangerous behavior. I can't say anything about that. I can only make statements about what's significantly different. So what I ask is: What does everyone do in this situation? Everything other than that is significantly different. (Doctoral candidate Robin; field notes, May 2011)

Robin redefined the surveillance system's objectives from detecting "dangerous" behavior to detecting "significantly different" behavior, which might also be dangerous. His modified algorithm detected patterns of aggregated movements across the monitored space, thus analyzing "what most people do." He assumed that when people behave significantly differently than others, then there is an increased chance that these people are exhibiting the kind of behavior the system was supposed to detect. His theory was thus that 'conformity' means 'what most people do' and deviance is everything else, which means that the software detected not dangerous behavior, but risky behavior. Thus, he inscribed a binary classification of deviance and conformity which was based on statistical normalcy. The question of margins – what should still count as normal and what should count as deviant – was displaced by Robin to a hypothetical end user in an unknown future. As he told me later: "We are engineers, we don't want to assume responsibility for definitive decisions over dangerous behavior" (field notes, April 2012).

Robin's problems show how he struggled with conflicting understandings of his work: On the one hand, the government expected him to act as an expert on crime and terrorism, while on the other hand, he viewed defining deviance and conformity neither as a legitimate part of his work, nor as an interesting research problem. But, because he was committed to both the research project and his field of research, he had to find a way to satisfy the requirements of both worlds. He did so by adapting his theory to existing research, which offered him a sufficiently explicit conceptual foundation to solve two separate problems. First, his seagull theory allowed him to continue his work – which was primarily his doctoral dissertation, while secondly being close enough to the original plan to be interpreted by the funding institution as the execution of his commissioned research. Following Star and Griesemer (1989: 393), the seagull theory of deviant behavior can thus be described as a boundary object. Boundary objects have "different meanings in different social worlds, but their structure is common enough to more than one world to make them recognizable, a means of translation" (Star and Griesemer, 1989: 393). His seagull theory allowed Robin to balance government expectations of developing security technology and disciplinary expectations of developing a legitimate topic for his doctoral dissertation. But, following Clarke (1998: 7–8), we could also say that Robin's seagull theory *disciplines* his work in two ways: On the one hand, it aligns his work with the wider research in his department and, on the other hand, this alignment indicates that his discipline tends to bear greater control over his work than the government's demands.

Configuring surveillance operators

The Security Research Program expected the group to increase the efficiency of surveillance processes by partially mechanizing them. This means that the researchers configured how surveillance operators and security personnel would use the CCTV system (cf. Woolgar 1991; Hanseth and Monteiro, 1997), including the ways in which they would observe people and move through the monitored space. The system architecture played a major role in configuring these surveillance practices. It did so by ordering the relationships between the infrastructural components into a hierarchy – cameras, servers, storage, mobile devices, security staff, and communication protocols, among other factors.

One example of this hierarchical ordering is the way in which the researchers conceptualized the cameras as a self-organizing, decentralized, and autonomously communicating network. The idea was that the network would automatically compute the maximum coverage of the monitored space with a given number of cameras. Delegating parts of the observation to the CCTV system was supposed to compensate the limited attention span of surveillance operators: The users had only to act on their own discretion when the CCTV system detected something out of the ordinary and sent an alert to the user's screen. The user's job as defined by the group was to qualify the alert by deciding whether there was a reason to intervene. It was not the system's users who were supposed to control the cameras, but the camera network itself. Thus, the researchers distributed surveillance processes between technology and users by assigning significant parts of the observational work to the surveillance system, leaving the human operators with the task of decision making.

However, as Kai – a computer science doctoral candidate – explained to me, his preference for self-organizing networks over a manually controlled network was the mathematical problem at the core of it. The autonomous network was a modification of a geometrical problem known as the "art gallery problem." What Kai found exciting about this problem, as he explained to me, was that the problem was not unambiguously solvable, but that its solution could only be approximated with algorithms. If the maximum coverage could only be approximated, it meant that Kai also accepted the risks of potential instability. What seemed to be more important to Kai was the question whether the underlying problem was interesting against the backdrop of his department's line of research, while he never really talked about what the self-organizing network would do to render surveillance processes more efficient. Although the government expected the group to make surveillance processes more efficient, we can

see how questions of applicability faded into the background in their day-to-day work.

The preference for 'admirable problems' is even clearer in how the researchers from the department responsible for the system architecture dealt with questions of system stability. They originally designed the CCTV system as a (more or less) decentralized network to secure its stability. The idea was that if one part was damaged for whatever reason, the remaining components of the CCTV system would continue working and avoid a total breakdown. However, Kai explained that this architecture was by no means a guarantee of stability, and acknowledged that there were much more practical and applicable solutions. For instance, they could have built a centralized system and physically secured the center. This would not only have been just as effective, but also much more economical than the solution they had proposed. However,

> Securing the center would have been much cheaper, but not as interesting as a research topic. But, you know, it's not that important to me that people use it anyway. I actually wouldn't like it very much if the system worked, I mean, if the state monitored us. I just have an interest in it as a researcher. If I owned a house I'd set up a [CCTV] system right away, but if the state did it I'd be against it. (Doctoral candidate Kai; field notes, April 2012)

Kai knew that there were more practical and cheaper solutions to problems of stability. However, he was writing his doctoral dissertation for one of the participating computer science departments, which focuses on self-organizing, decentralized system architectures. Designing the CCTV system as a decentralized network aligned with the department's work and was considered a recognized research topic for an academic audience. For Kai and the other project members from his department, the recognition of their work by an academic audience thus seemed to be more relevant than that of the funding institution.

Kai's view of working at a university differed starkly from that of the government: While the government within the Security Research funding scheme framed university research explicitly as an economic activity, Kai drew a sharp distinction between what he viewed as academic and industrial research:

> In science, you can basically do what you want. In the industry, you won't be able to follow your interests; they'd never build the kind of system we're developing. Here, we're able to experiment, which wouldn't be possible in the industry – they'd bite your head off if you'd propose a concept like ours. (Doctoral candidate Kai, field notes April 2012)

While Kai surely plays down the structural constraints of research at universities, his statement shows that he, too, rejected the government's expectation that he act as an expert in surveillance work, a perspective which was shared by all of the doctoral candidates in the project.

The way in which the researchers configured surveillance operators again shows conflicting understandings of their work: While the government expected the researchers to make surveillance processes more efficient, for the scientists responsible for this task, this was not interesting enough as a research problem. But, because they were committed to both the research project and their field of research, they had to satisfy the requirements of both the government and their disciplines. They did so by translating between a practical problem (a functioning and stable CCTV system) and their own research interests (distributed algorithms for decentralized system architectures). However, Kai's case shows a much more pragmatic approach than Robin's: While Robin had to translate the grant proposal into doable problems when he realized that they were ill-fitted to satisfy the requirements of his discipline, Kai's supervisor had already created a problem while writing the grant proposal already which was both recognizable as a relevant practical problem to the funding institution, as well as an as an interesting research topic to them and their department colleagues.

Tailoring is invisible work

The Security Research Program expanded social control into university researchers' work by stipulating the purpose and social organization of their work: They were to contribute to the solution of

security problems and collaborate in a transdisciplinary fashion. The university researchers in my study then had to balance commitments to their government commission, their disciplines, and the wider public, which were often at odds with each other. What allowed them to navigate these conflicting expectations was their ability to create research problems that fell into their departments' previous lines of research, but could also be interpreted as practical problems pertinent to surveillance systems. This practice is nicely captured by Calvert (2006: 208–209) as research tailoring, which she defines as making one's work "appear more applied to gain funding and resources."

Tailoring was crucial to "keep politics near enough" (Gieryn, 1995) to secure the researchers' funding, but "not too close" to interfere with their research interests. Their tailoring practices can thus be described as 'boundary-work' (Gieryn, 1983, 1995, 1999), because it served to protect their relative autonomy against the expansion of government control. However, unlike other research on multiple commitments in academic research, they did not protect their work from government oversight by quarreling with the funding institution about the legitimate boundaries of their work (cf. Gieryn, 1999; Jasanoff, 1990; Wehrens et al., 2013).[7] On the contrary, this type of boundary-work was reliant on the avoidance of conflict. It was thus not open boundary disputes which allowed them to manage their proximity to politics, but their carefully tailored research objects.

Based on my study, we can add a few points to Calvert's definition of tailoring. First, the purpose of tailoring is not only to gain funding, but also to secure existing funding. This is exemplified in the differences between Kai's and Robin's cases. In Kai's case, the tailoring could be termed 'forward tailoring,' because the translation was done in the grant proposal to attract funding, and then carried on throughout the entirety of the research process. This was a common and surprisingly open practice, as indicated in my field notes:

> The group is discussing possible ideas for a successive grant proposal within the Security Research Program. That is, the professors are talking while the doctoral candidates listen or work on their laptops. […] Martin [the principal investigator] jumps up and draws a table on the whiteboard. "We have to distinguish this – one is the paper perspective, the presentation perspective is another thing," and he fills out one column with application scenarios, and the other column with their corresponding research areas. "The story has to start with the user," he explains. On Martin's suggestion, the group decides that the consulting agency use their contacts in public transportation to find out whether they have "shopping lists" in order to develop the grant proposal from there. (Field notes, May 2012)

By contrast, Robin's case could be termed 'reverse tailoring.' He realized during the research process that the problem outlined in the grant proposal and his research interests were ill-fitted. But, because the government monitored the project's progress in intervals of six months and reserved the right to terminate funding if it evaluated the project as failing its goals, he needed to construct a new problem close enough to the original commission to satisfy the funding requirements. He did so in reverse, by defining the new problem in terms of its available solutions. Reverse tailoring was a strategy which drew significantly more resources than forward tailoring, because it necessitated continual adjusting, both rhetorically and in practice.

Second, the varying amount of work which went into tailoring their research also accounted for the varying degree to which the researchers experienced role conflict: Researchers who could work with problems which were well-fitted from the beginning moved with much more ease between social worlds. These researchers experienced their multiple commitments to the project, their departments, and the government's demands as less problematic than did researchers who had to work with ill-fitted research problems. This is again clear in contrasting Robin's and Kai's cases: While Kai could more or less straightforwardly carry out his part of the project, Robin struggled greatly throughout the project. Whether or not scientists' balancing acts become stabilized thus seems to be strongly linked to the ways in which research problems are structured: Although in both Robin's and Kai's cases demands were misaligned, it was certainly easier for Kai to navigate them than for Robin.

Third, in contrast to Calvert's (2006) assessment, tailoring was neither a single event during the research process, nor mere 'window dressing' which just portrayed their work as security research in order to obtain funding. Rather, it was a *continuous* negotiation to align their commitments to both their fields of research and the government program, and in some cases it required a tremendous amount of work.

The work that this tailoring required was 'invisible work' (Star and Strauss, 1999). This means that it was illegitimate work from the perspective of the funding institution and needed to be hidden (Möllers, 2016). If working within the framework of the Security Research Program indeed meant this amount of invisible work, why did they then apply to the program in the first place? The reasons the university researchers gave me in response to this question were strongly related to structural working conditions at German universities, rather than to the content of their work. Again, from my field notes:

> I'm outside with Martin [the principal investigator] and Robin [a doctoral candidate] for a smoke. I ask them why they applied to the Security Research Program, and how they designed this sort of huge, transdisciplinary project. Martin responds: "You need a lot of imagination to apply for a grant. This is a sort of top-down process; while you're working on one problem, new problems occur, which gives you reason to apply for another grant." Robin adds: "Well, and the grant proposals are mainly written to secure funding for the doctoral candidates." (Field notes, May 2011)

The rollback of long-term funding and the decline in tenured positions in relation to student numbers at German universities have opened way to an increasing number of short-term positions and precarious working conditions (Kreckel, 2008). For the senior scientists in my study, continuously producing grant applications was an acceptable and common remedy to the problem of securing funding for their doctoral candidates and post-docs. This arrangement is also evidenced by the high fluctuation of doctoral candidates and post-docs throughout the project duration: The researchers who had worked on the original project proposals usually left the project once they completed their dissertations. New doctoral candidates took their place, using the project to write their own dissertations.

Conclusion

The group never ended up transferring their work into a functioning and marketable surveillance system, despite the German government's significant expansion of control over the group's work. Neither its requirements in terms of content and organization of the group's research, nor the regular monitoring of the project's progress, nor even provisions to terminate funding in the case of negative evaluations at all led to commercialization. While this shows that scientists seem to have some leeway in finding creative workarounds, this does not mean that they do not, occasionally, struggle greatly with the constraints posed on them by directed funding schemes. Rather, the ways in which scientists struggle through conflicting demands shape their scientific work, just as the ways in which scientific problems are constructed shape the extent of their struggles.

To be sure, this was not simply a case of 'bad science.' The senior university researchers involved in the project were all respected scholars in their fields. Their reputation is also indicated by the fact that, during the project, they published several peer-refereed articles in international journals, and regularly presented peer-refereed papers at international conferences. Furthermore, all participating senior scholars, either during or after the project, were able to obtain the prestigious grants from the German Research Foundation (DFG), which have to undergo a rigorous peer-review process. However, *saying* "development" and *doing* "papers" and "grants" was viewed as the better long-term strategy for those who worked within an academic reward system.

My study reaffirms the need to remain attentive to the potentially multiplying lines of conflict researchers face in the midst of changing relationships between universities, governments, and industry. There was more at stake for the researchers than 'just' balancing their research and academic careers with the government's requirements. All of the university researchers were acutely aware of the deeply political nature

of their work, as it related to highly controversial issues such as surveillance, discrimination, and privacy. Furthermore, personal struggles with surveillance technology were a shared issue among some of the doctoral candidates, and were importantly rooted in their personal political stances and commitments to the general public. It is thus important to pay attention to the multiplying demands (cf. Vallas and Kleinman, 2008; Tuunainen, 2005b; Owen-Smith and Powell, 2002) scientists have to deal with in their day-to-day work in order to gain a richer understanding of scientific work under increasing commercialization pressures. However, this should not only include scientists' attitudes towards commercialization pressures, but, importantly, also the practices by which they 'make it work' despite the potential for conflict. We need more analyses of the way in which scientists struggle through conflicting demands, how these struggles shape their work, and, in turn, what kinds of working processes and objects make navigating conflicting demands more or less feasible.

Not accounting for the multiplicity of constraints that university researchers face might also too easily obscure the social and structural conditions of their work. The amount of invisible work which went into their tailoring practices shows just how strongly they were being pushed and pulled in different directions by the government, academia, and the wider public. The researchers' reasons for applying to the Security Research Program despite these problems were, in turn, strongly tied to structural working conditions at German universities. Consequently recognizing that tailoring practices are to a certain extent a product of powerful misaligned or competing social worlds has implications for science policy. There are good reasons for governments to ask universities to contribute their expertise to the solution of societal problems, and good reasons to ask scientists to be accountable to citizens. However, my study indicates that this might be difficult to accomplish in a meaningful way if academic institutions do not reward the solution of practical problems, or if directed funding schemes ask scientists to engage in highly controversial activities.

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Notes

1 Theories of 'radical changes' in knowledge production share conceptually problematic assumptions which render them theoretically inadequate for the analysis of knowledge production. The models' claims of change rest on distinctions between 'old' and 'new' forms of knowledge production, which they tend to equate with 'basic' and 'applied' research. Despite occasional cautionary warnings, these dichotomies also persist in some of the empirical work on boundary organizations (although now couched in a different language which implies that 'hybrid' research and spaces were not hybrid 'before'). Both distinctions, as well as their equation, are quite problematic. The major pitfall is that they frame science in essentialist terms, a conceptualization which has received much scrutiny within previous STS work. For example, Gieryn's (1983, 1995, 1999) seminal work has shown that boundaries between science and non-science, or science and politics, are culturally and historically variable and relative to institutional and organizational contexts. Gieryn concludes that only little can be said about an essential core of science, which leaves us to understand it simply as consisting of scientists' practical accomplishments. In fact, science seems to have always been an 'impure' hybrid (Latour, 1993), which suggests that, historically, there might be more continuities than the models assume (see also Etzkowitz & Leydesdorff, 2000; Fuller, 2000). If no stable core of science exists, then distinctions between 'old' and 'new', 'basic' and 'applied', or 'pure' and 'hybrid' research seem fairly inappropriate in the attempt to understand the consequences of current pressures towards commercialization.

2 For a comment on their varying degrees of structuralism, see Jones (2009).

3 Quite a few of the empirical studies discussed above talk about scientists' "strategies" or "practices," but I feel that we end up not knowing very much about what scientists actually do in their work. This is perhaps also due to the fact that many studies are solely grounded in interview data. Had I merely analyzed the interviews I conducted at the beginning of my fieldwork, I too would be telling a different story. What the literature on both management and scientists' perspectives seems to routinely miss is that people often do things that are different from what they say they do (i.e. saying "system development" but doing "academic papers." See Khan and Jerolmack, 2013; Jerolmack & Khan, 2014 for an insightful discussion of what they call the problem of "attitudinal fallacy").

4 I gained access to the group as an embedded researcher. My task was to analyze automated surveillance systems for 'social implications' which included, for example, controlling for bias, or questions of public perceptions of video surveillance. My role in the field shaped the focus of my observations, in that I followed the work (cf. Marcus, 1995), and the work was mainly carried out by the junior researchers in the project. The group had at different times between 25 and 30 members, about half of them university researchers. My material is ethnographic, which means that it includes (1) observations, (2) in-depth interviews with the scientists, (3) documents which the scientists produced in the research process (i.e. grant proposals, papers, presentation slides), and (4) government documents for the funding scheme. After initial in-depth interviews, I carried out fieldwork with the research group over the course of two years. Interviews were unstructured and lasted between 60 and 120 minutes. I then observed project meetings which were held on average every two months for 2–5 days in different places in Germany. These included (1) meetings where (mostly the junior) researchers assembled the work of the past months ("hackathons"); (2) meetings where all project members presented the state of their research to the funding institution; (3) and two public demonstrations of prototypes. My analysis was guided by strategies developed in grounded theory and situational analysis (Clarke, 2005; Strauss & Corbin, 2008), using concepts developed in interactionist STS as sensitizing heuristics. All material is in German, and was translated by me for the purpose of this paper. I have substituted all names, places, and unique technical terms with a pseudonym.

5 "Reagieren Sicherheitskräfte immer sofort, wenn sie etwas Auffälliges auf dem Monitor sehen? Leider nicht, denn um 1.700 Kamerabilder ständig zu überwachen, brauchte es viele Menschen. […] Damit das System von selbst weitere – und sehr verschiedene – Auffälligkeiten erkennen kann, ist die Wissenschaft gefragt. […]Also wozu dann die Kameras? Zunächst dazu, um den Täter zu identifizieren und zu fassen.

Dem Opfer hilft dies allerdings wenig. Auch hier wäre Bilderkennung erforderlich. Und daran wird tatsächlich gearbeitet. Das Programm müsste die Bewegungen des Videobildes auswerten und alle normal schnellen Bewegungen der Fahrgäste herausfiltern. Welche Bewegungen sind typisch für eine Gewalttat? Diese wären zu identifizieren. Viel Arbeit für die Forscherinnen und Forscher" (Bundesregierung, 2011).

6 "[Das Sicherheitsforschungsprogramm] bietet die Möglichkeit, durch Forschung und Innovation die Wettbewerbsfähigkeit der beteiligten Unternehmen und die Marktfähigkeit der von ihnen erarbeiteten sicherheitstechnischen Lösungen zu fördern, Sicherheit als nationalen Standort und Wirtschaftsfaktor zu etablieren und Gestaltungsspielräume auf europäischer Ebene zu eröffnen" (Bundesministerium für Bildung und Forschung, 2007: 7).

7 Boundary-work can mean both drawing and blurring boundaries. In Gieryn's (1999) study, scientists draw boundaries to protect their autonomy against what they view as 'outside threats'; and they blur boundaries in order to claim authority over new subjects of research ('expansion'). This does not quite apply to my case: Although the researchers in my study rhetorically blurred the boundary when communicating with the funders, they did so to protect their autonomy, not to expand their authority into surveillance technology markets – after all, they never pursued technology transfer.