CULTURAL ANTHROPOLOGY

FROM ANTHROPOLOGIST TO ACTANT (AND BACK TO ANTHROPOLOGY): Position, Impasse, and Observation in Sociotechnical Collaboration

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"Did you see that paper in *Nature* or *Science* about the glowing pig? They got the pig to express Green Fluorescent Protein." Sven was agitated. "Why did they do it?" We were sitting in the lounge on the top floor of the laboratory building positioned between several multinational pharmaceutical firms, other university research buildings, and a Swiss Telecom office on the Kleinbasel ("little" Basel) side of the Rhine. The light of early summer glowed through metal and glass, filling the room. Sven was testing me. I responded to his question with a question, asking him what he thought was the scientific significance of the glowing pig experiment. "No, no," he insisted, "in terms of your expertise, tell me why they did it." I stumbled a little, slightly taken aback, and suggested it was probably curiosity, that I would want to know what kind of problem it was addressing, and then what institutional affiliations the scientific team maintained. Before I could form another thought, Sven interjected, announcing the answer: "They did it because they could; because it was fun!" I asked him whether he thought there was in this instance, or in his work, a need for any further justification. His reply was immediate: "I don't need any justification." He took out a cigarette, picked up his coffee, and left the table.

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I was in Basel in order to collaborate. The fieldwork was made possible and framed by a three-year project funded by the National Science Foundation (NSF) based at Arizona State University (ASU): the Socio-Technical Integration Research project (STIR). The enterprise involved ten researchers working in laboratories around the world, including regular discussions and workshops among the participants about the shared endeavor. The STIR project itself was administered from within the Center for Nanotechnology in Society (CNS), one of a number of flagship endeavors of the Consortium for Science, Policy and Outcomes (CSPO) founded and chaired by ASU's president Michael Crow. The CSPO forms an integral part of ASU's endeavor to forge a "New American University," one appropriate to the twenty-first century, as well as to the position of the United States in global configurations of science, technology, capital, and values. The STIR project was part of efforts at ASU to design forms of collaboration between (among others) the social and natural sciences.

The STIR project was furthermore partly invented as a response to prior efforts to name social and ethical challenges in new scientific domains, especially genomics. An important precursor and counterpoint to STIR was the Ethical, Legal and Social Implications (ELSI) model of the Human Genome Project. Briefly stated, for the designers of STIR, the critical limitation of ELSI in the United States (Fisher 2005) was its position "downstream" and "external" to the practice of scientific inquiry. Research in this mode was advisory and limited to pointing out issues (Cook-Deegan 1994). These limitations produced several responses as to how social scientists might better design upstream and midstream engagement with natural science and engineering. *Upstream* means deliberation prior to the commencement of projects, while *midstream* refers to the effort to introduce and work on questions during ongoing inquiry. The STIR project was a response to this challenge and was built around a core methodology—a dialogical protocol that aimed "to modulate" the "midstream."

The method aimed to inflect laboratory scientists' reflexivity to augment their capacity to integrate sociotechnical issues into their practice through the redescription of scientific practice and the demonstration of contingency. Furthermore, the use of such a method aimed to increase (mutual) capacities for (ongoing) collaboration between natural and social scientists.

The *use* of such a method might be considered unanthropological. It was, however, intriguing as an *object* of participant observation insofar it drew not only from literature in the philosophy of technology (Mitcham 1994) and critical science policy (Lindblom and Woodhouse 1993) but also from a methodological and conceptual strain of work that has been crucial to burgeoning anthropologies of science and modernity in the past three decades: the material semiotics of actor-network theory. For Michel Callon (1980, 198), a core question of material-semiotic inquiry in the 1980s was how to *describe* socially and materially heterogeneous systems in their malleability as well as intransigence. The Arizona project sought to use such descriptions within collaborations to perturb the practice of science in one way or another: the anthropologist, in other words, was to be an engaged actor in the production of a self-reflexive socially and materially heterogeneous system. Such an endeavor appeared intriguing as both an object and a practice of anthropology.

The project made for a lively testing ground for ever-increasing promises of new forms of post-ELSI collaboration among social scientists and emerging technoscientific endeavors whose future directions are indeterminate. Their futures are indeterminate not only because of the endeavors' heterogeneity and contingency as material practices or artifacts, but also in their ramifications and significance. Precisely the question of significance with respect to material-semiotic heterogeneity justified such collaboration.

Taking STIR as an object of anthropological participant observation, my aim is twofold: to observe the project from within the terms of its own practice—the terms and practices of the project's director, other participants, as well as myself—to characterize *the problem of collaboration* experienced in the project. I then turn back to the project from an adjacent position to account for the problematic character of the experience of collaboration. I take up this second aim to pose a diagnostic question for anthropology: how, why, and to what end do anthropologists and other social scientists engage laboratory scientists in forms of collaboration (a question and a practice distinct from the stakes of ethnographic observation)? Such a diagnostic question, intimately connected to the first descriptive task of understanding the project of—and method for—collaboration on its own terms, offers a necessary antidote to a priori or ex post claims about either the necessity or the contingency of impasses and blockages experienced with efforts at collaboration. Such an antidote is furthermore necessary in light of an abundance of commentators willing to make epochal diagnoses about the relations between the sciences, humanities, and social sciences in the twentieth and twenty-first centuries (Snow 1993; Kagan 2009). Claims about the necessity of impasse, such as epistemic or ethical incommensurability, unduly put an accent on a determinative conception of socialization, while claims of the contingency of impasse frequently meander into the domain of personality as an explanatory factor (see Barthe et al. 2013, 190–93). Rather than seeking to diagnose problems in collaboration in terms of individuals and their socialization, I seek instead to show the second-order character of the indeterminations and discordances experienced by the director, other STIR researchers, and myself, which were produced, I suggest, by the mode and positions made available in this project. I show this to indicate specific characteristics of impasses to collaborative engagement between modes and forms of contemporary knowledge, so as to bring into relief a sharper problem of collaboration for anthropological inquiry.

I argue that an effect of STIR's dialogical technique, though aiming to modulate scientific reflexivity through purported collaboration, served instead to entrench scientific reflexivity within the norms of scientific practice that are external (and unpropitious) to collaboration with social scientists. The redescription of scientific reflexivity and the demonstration of the contingency of research practice, which may enable a scientist to "take more into account," left no place for the reflexivity of the anthropologist within the engagement. What was not in question in the STIR project was whether or not scientists are reflexive about what they do: they are, insofar as reflexivity can be understood in a pragmatic sense as the capacity to render describable how one goes about an activity. The dialogical protocol made such a capacity visible and sought to enable actors to act on the range of things rendered visible. The issue that motivates my argument is the effect of such a dialogical protocol for the collaboration meant to be fostered through the work of modulating reflexivity, and what such effect signifies for the practice of anthropological inquiry, as well as for the available positions for anthropologists within offers of collaboration.

PROBLEMS OF POSITION: To Be Affected and to Affect

"Problems of position" in anthropology have been captured and honed by Jeanne Favret-Saada (1977, 1992) in her anthropological inquiries into forms of accusation. When she went to rural northwest France, for example, and began to ask locals, in her capacity as "anthropological expert," about "witchcraft," her inquiry yielded little other than some stories about what happened in the old days, or vague claims about witchcraft accusations displaced onto other villages. By contrast, once she accepted to leave her given place as the anthropologist, something could happen in terms of anthropological knowledge: she allowed herself to be interpolated into the discourse of witchcraft. Interpolation, in this case, involved accepting the possibility that she herself could be subject to witchcraft, as well as accepting the invitation to occupy the place of the one who could un-witch (*désorceler*) the bewitched. Such a transformation allowed her to understand the character of the problematic experience of witchcraft accusation and how such a problematic experience is constituted by a system of relations between subject positions.

In terms of collaboration discourse and practice, the challenge of ever-increasing calls for anthropologist participation in collaborations is not only the capacity for the anthropologist to *be affected* (Favret-Saada 2009) but also the capacity to *affect* situations of participant observation. To affect a situation appears all the more problematic when the objective is set within systems at the heart of modernity, such as scientific institutions, and to affect such institutions through collaboration, in one's capacity as a collaborating anthropologist. Drawing on anthropological works treating the problem and practice of collaboration, and drawing on my work with Paul Rabinow and Gaymon Bennett in the Anthropological Research on the Contemporary (ARC) collaboratory, the constitutive concept of collaboration in this article, and the constitutive hypothesis orienting the fieldwork, is that a worthwhile collaboration is one in which two kinds of participants, in their engagement, are able to name a problem or do a practice that in their position as participants (prior to engagement) they would not have been able to do (Rabinow and Bennett 2012, 6–7; cf. Riles 2013, 563; Strathern 2000, 296). An anthropology of anthropologist–natural scientist collaboration would therefore be attentive to the character of the means of engagement, as well as to the character of the problems that could be named by such means; problems that should differ in kind from those that natural scientists or anthropologists could pose about the scientific activity in question outside of such engagement. Collaborative participation presupposes an endeavor of transformation (Rabinow and Stavrianakis 2013, 33).

To diagnose the significance of this particular case of STIR for an anthropology of collaborative practice, I briefly situate the case of STIR within a broader configuration and a comparative instance of social scientist collaboration with scientific expertise: Vololona Rabeharisoa and Michel Callon's engagement (1999) with the French Muscular Dystrophy Association (AFM). A turn to a comparative instance helps highlight the following conclusion: Though aiming to occupy the subject position of a collaborator, one in which to think with collaborative counterparts, I describe how the STIR method served to transform the anthropologist into occupying the position of an *actant*, in a strict sense—a mechanism of modifying people and things through the attempt to modify actions (Latour 2004, 75). The ethical and epistemic limits to such a position and mode of practice offer a negative and perturbing conclusion for anthropology: in situations in which anthropologists continue to pursue collaborations, there is a need to remediate how demands for practical intervention are connected to the possibilities of opening up shared problems (see Rappaport 2008). Such a diagnosis indicates the need for more reflection on the power relations at play in such forms of collaboration and what such relations both facilitate and block. Furthermore, in situations where such a binding of objectives seems to lead to impasse (demands for practical intervention and opening up shared problems), there exists a problem of the disaggregation and interconnection of distinct orders of intervention and observation. I conclude by characterizing some ethical and epistemic characteristics of such disaggregation, objectivation, and interconnection (cf. Friedman 2013; Rabinow and Stavrianakis 2013, 34–37).

BOUNDARY WORK IN A BOUNDARY ORGANIZATION

Today, ASU constitutes a frontier of the so-called New American University. In the outskirts of the remediated desert of Tempe, the replication of a nineteenth-century German scholastic ideal has been jettisoned for the design of a new organizational form to pursue "knowledge and values." When we met in 2009, the director of the STIR project, Erik Fisher, had completed a transition from a PhD at the University of Colorado, where he had also been a humanities advisor to the College of Engineering, to a permanent position at ASU. At the University of Colorado Erik had worked with a thermal engineering laboratory to develop the "engagement protocol" we were to use in the STIR project. He had searched during his thesis work for a manner of participant observation that could attempt to maximize the effects of observation on observers (cf. Luhmann 1988; Langlitz and Helmreich 2005).

At ASU, the project was understood as part of the university's growing arsenal for conducting "real-time technology assessment" (Guston and Sarewitz 2002). Such techniques and the organizational form for their actualization have been designed to produce a specific kind of "boundary work." Whereas in some strains of science and technology studies boundary work as a concept functioned to lay out the ideological codes and power relations that produce distinctions between science and non-science (Gieryn 1983, 1999), some science policy scholars took the distinction of science and politics as an organizational one and then worked to show how a different organizational form might serve to rework this boundary (Guston 2001, 2014). Such organizations, their objects, and objectives reorder relations of science and politics through consent to what the CNS calls "a productive cooperation," a commonplace that indicate a confidence in capacities to manage the power relations traversing these boundary conditions. Boundary organizations facilitate collaboration between scientists and non-scientists, and they create the combined political and social order through the generation of boundary objects (Guston 2001, 401). A boundary organization generates its own authority by assembling agents in this frontier to stabilize the relation between principals to whom the organization is then accountable (in our case, the National Science Foundation as funders and bioscientists in laboratories as the object of intervention). At CNS, this authority and capacity is named specifically in relation to a "shared problem" with a political imperative: democracy.

If the diagnostic starting point for the CNS was a politically remediative one, set within the possibilities of boundary work, Erik was nevertheless realistic about the institutional constraints faced by scientists. In what Erik calls the constraint of autonomy (below), we can follow György Márkus (1987, 6) in calling the monologic character of science: that is, signification within scientific practice is determined by the domain of inquiry to which it refers. The parameters of this domain are no doubt open. The ease with which concern for patents entered molecular biology after the 1980s, along with molecular biologists trained with Juris Doctor qualifications, is exemplary of such malleability. Nevertheless, the degree of openness and the character of pertinence are not limitless and constitute empirical questions.

In early 2009, prior to leaving for Switzerland, I participated in an introductory workshop held at ASU. Erik explained the scope, mode, and aims of the enterprise. We sat in a halogen-lit, windowless room, somewhere in the vast expanse of the campus, sheltered from the January heat. "We've pitched this project as an alternative to viewing the scientific method as non-problematically applied." Erik was talking to the ten of us, all Ph.D. researchers, participating in his project. "We've hemmed ourselves into a highly constrained space, the laboratory, by highly constrained actors, and the question is: What is possible? What are we able to bring about?" Erik started by suggesting that the natural and technical sciences were caught between monologic self-assurance and the dialogical possibilities of boundary work.

His tone was both serious and upbeat. "This project is realistic in recognizing that scientific actors are highly autonomous and that's what we're dealing with." "So," he asked, "what does it take for self-governance to change on some practical or structurally significant level?" In the design of his project, an operator of reflexivity (the STIR researcher) would provide the medium and action for reflection by scientists within the milieu of their work.

Erik's design of a protocol furthermore constituted a response to "outside" manners of attempting to control science. As he indicated to us: "It is not that those outside and top-down approaches aren't good, or are bad, guided or misguided, it's just that they were ineffective." As such, the STIR project brought together two methodological postulates that were critical to the manner in which engagement could be afforded: on the one hand, STIR partook of the framing of the boundary organization. On the other hand, it is important to note how Erik's observation and diagnosis of the constraints of working within the space of the laboratory, along with his aim to perturb the quality and quantity of questions taken into account by scientists, precluded naming by right and in advance the character of the problem or the mode of justification for entering into such collaboration (as in democracy for the CNS). As such, the autonomy of the scientific actors, as well as the scope of their quotidian and medium-term concerns, constrained the starting point of the engagement. As I will show, this constraint had consequences for its open telos. As such, we could characterize STIR as a form for boundary work positioned internally to the activity of science. We could not presuppose a shared problem or goal other than what was pertinent to laboratory scientists vis-à-vis their own thought and work.

A SCENE OF INSTRUCTION

At the heart of the STIR project lay a "decision protocol," (figure 1) that Erik had designed during his Ph.D. research. His thesis had posed the following question: How to put into practice U.S. legislation mandating the "integration of social considerations" into nanotechnology research? During the course of two and a half years he conducted multiple forms of participant observation, which resulted, in part, in the development of a mechanism for such integration.

The protocol meant to describe any given research decision in generic terms. Materially, it was a simple sheet of paper divided into quadrants. The "code" of the protocol was explained to us at the January training session in Arizona: *Opportunity* meant any situations characterized by the need to make a decision, where decision means commitment to a course of action; *Considerations* meant any parameters of a decision; *Alternatives* meant available courses of action; *Outcomes* meant the response to the opportunity, by means of selecting one or more alternatives, in light of one or more considerations. The purpose was to find moments with opportunities present, to map the process using the tool, and to see if

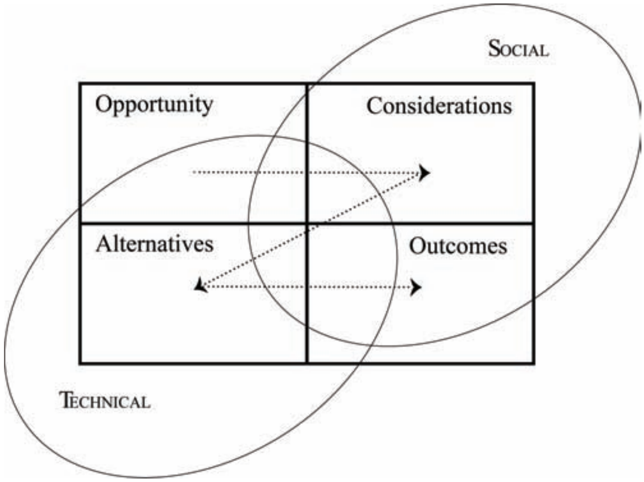

Figure 1. STIR decision protocol

recursive use of the protocol made any difference to research practice. The tool aimed to make visible latent concerns otherwise invisible.

We listened to Erik narrate an example, one I would hear subsequently repeated, which would function as a model for the use of the protocol. A researcher with whom Erik had been working had made a comment about a material he worked with being "messy." This observation became the starting point for discussions between them, about the inadequacy of an experimental material. This starting point generated further questions about possible alternatives. They began to name different considerations and possibilities, probing the status of the experimental material. After a few weeks of discussion, a better alternative was chosen, one experimentally and environmentally more sound.

Several things are important in this situation: Erik was working with a Ph.D. researcher who had received the overall experimental design from his laboratory director. This design specified using the first material and no other. The challenge for the dialogic pair was to take enough time and to lay out reasons, considerations, and alternatives at each step of the research. The work between Erik and the nanoscientist led to a change in the research setup, and their dialogue opened a new line of inquiry for the whole laboratory, based on the selection of this alternative experimental material. In our seminar room at ASU we were presented with an exemplar and proof of concept. The efficacy of the protocol was thus demonstrated in its capacity to redirect the technical and creative motion of scientific practice through an internal modulation. A normative parameter in the design of the STIR project and method was thus its capacity for any scientific system to withstand this form of environmental "irritation" (Luhmann 1996). As Erik would repeat, one of the collective goals of the endeavor was to "give the lie" to the idea that letting social scientists into laboratories slowed down or impeded research. Boundary work and a shift to a dialogic mode of engagement with social, ethical, and political questions, in other words, could be shown as productive for the scientific pole of the engagement.

In the exemplar offered to us, Erik's position in the midstream and the practice of modulation was parameterized by the (self-) creative movement, and goal, of the scientific side of the dialogical pairing. Such a position, and such a practice, presupposes a dialectical commitment to the becoming of the scientific domain (qua scientific domain)—a domain that, as Erik indicated to us from the outset, is nonisomorphic with a domain of collaboration that one might wish to create and occupy. Such a diagnosis shares in a mood of realism about what the scientific domain will and will not tolerate. Such a commitment to the ethical becoming of scientists in the domain of their work then further presupposes a weakly dialectical relation of the homeostatic reproduction of values and norms, with breakdowns and transformation in values and norms within that domain (Faubion 2011, 114).

Such a relation is, indeed, presupposed. There is, however, a problem.

Following James Faubion's systems theoretic articulation of an enlarged set of parameters for inquiry into ethical domains, and taking the starting point from Niklas Luhmann, we must recognize that systems are both structured and organized (Faubion 2011, 6–7). A structure is the combination of how institutions are arranged (their divisions of labor, material arrangements, etc.), the parameters and metrics relative to which decisions are made, and the habits and dispositions of individuals formed by the institution (their socialization). Structured systems are open (i.e., not closed systems of semiotic oppositions). Unlike Luhmann's systems theory, however, those of us in the STIR project had to assume the possibility of *organizational* openness in addition to such *structural* openness. The organization of systems, for Luhmann, has as a requirement the closed differentiation of a system relative to its environment. The problem probed by these STIR studies could be formulated in the following manner: in addition to the possibility of reformulating the practices of scientific systems, can such structured systems *reorganize themselves* (open themselves) relative to their changing environments, and can the structural changes be calibrated to this organizational openness?

Such calibration is supposed in the boundary work of the STIR mode and its capacity to modulate the form and practice of scientific reflexivity.

A MORE PROSAIC THING

Six months after our first collective meeting at ASU, I traveled to Switzerland to conduct a study using the STIR protocol. I had contacted the head of a bioprocess laboratory based in Europe's pharmaceutical capital, Basel. The group specialized in engineering diverse biological pathways and systems within cells, as well as in cell-free biosystems. The team comprised a range of specialists who worked together on the identification of problems and solutions for which their individual techniques were insufficient. Computer modelers, electrical engineers, protein engineers, biochemists, and molecular biologists labored to invent means for producing "high-value-added chemicals."

Members of the laboratory assured me that they recognized the importance of social and ethical questions in science, coded under the banner of stem cells. Since they worked on bacteria, however, or else on cell pathways reengineered in cell-free artificial environments, in their judgment no ethical issues weighed on their research. As one of my interlocutors ironized one morning, "Am I violating nature when I mutate these proteins?"

The laboratory director was more forthcoming in articulating a narrative of justification for the kind of application-oriented and still basic research science in which his lab engaged. He explained to me that in Germany especially, and to a degree in Switzerland, a delicate relationship existed between theoretical knowledge and the practical application of methods:

> This is tricky when it comes to connecting them, what can seem a somehow more prosaic thing, earning money with it . . . I think that people simply lack the connection, also the motivation. So, if you go through a typical German degree, unless it's economics, then the one thing that you will never encounter is a monetary drive to do something. After all, this is Humboldt's idea of the University. Humboldt did not have the idea to make university a money-generating institution, right, so the concept that the application of something is an important piece in your overall chain of value generation which in the end also keeps our society going, this is something that simply has to grow and it can best grow in a company. We can do our part here in making people realize that there is a second angle to what we do here, but we cannot really, or I don't want to create this idea of money

> as all-important. So I think then this is also why I see that Ph.D. students, I guess, are very easily motivated by things like working on antibiotics, because it's a very clear problem, it's not necessarily related to money, but it's related to saving lives.

He was referring to a then ongoing project that he had started a few years earlier with a Ph.D. student named Giovanni. Their goal had been to design a bioprocess in which to produce the precursor molecule for a type of antibiotic that has the capacity to treat infections from gram-positive bacteria. Currently this class of antibiotic is not in clinical use due to the absence of a platform for the cheap, efficient, and scalar production of a bioprocess. The problem was clear: multidrug resistant pathogens indicate a serious need for new drugs; therefore developing a platform for synthesis is a logical and worthwhile health intervention.

Giovanni had returned to the lab for a postdoc to see whether he could improve the final stage of the bioprocess. On a particular occasion, Giovanni and I met to discuss the progress of his work. It was clear from preceding observations and conversations that he was approaching a critical juncture in refining the production process for the molecule of interest. We met one afternoon and I took out a blank protocol sheet and turned on my recorder, a gesture that I had repeated more than a hundred times during my time in Basel. He was preoccupied with the interconnection of the processing problem and the movement of the drug from lab to market. Under "opportunity" I noted, "How to bring the drug to market?" "Which markets?" "What are the problems?" He named an important parameter, which I noted under "considerations" in our protocol: "Right now not many pharmaceutical companies invest in antibiotics, while future antibiotic resistance is both a looming health challenge, and when it gets serious, it will promote investment." I heard Giovanni narrate this statement in a double diagnostic register: the fact that pharmaceutical companies are not investing in this serious global health problem constitutes both a crisis and an opportunity. The double diagnostic register was synthesized into a calm statement of fact: "when it gets serious"—in the estimation of an unnamed actor—"it will promote investment." Logically, when that moment arrives, those who moved first would have an advantage to advance the technology and the health benefit it might provide.

Giovanni's narrative mood was Whiggish to the degree that scientific progress and the vagaries of industrialization were synthesized in a voice between the active and the passive and projected into a simple future: "it" will promote investment and will be promoted; the health crisis; the laboratory success; the relations that could be created between the lab and the pharmaceutical companies only a stone's throw away.

"We will need synergies and not just competition to find a good solution to the problem," he explained. Giovanni was demonstrating for me the distance between the norms and forms practically enacted in his bioprocess lab and the (naive) figure of the nineteenth-century German Romantic narrated for me by the director, a figure who would never use money as a justification for a life's work. "Part of the challenge in biotechnology," Giovanni continued, is "the formation of synergies to overcome resistances, both biological and institutional." He was pragmatic to the degree that any outcome, or solution, would be preferable to no outcome. Thus, moving from the top right quadrant of our protocol, "considerations," to the bottom left, "alternatives," I asked him what he was going to do. Given the institutional blockages, what could he do?

His reply was swift: "We could produce an amount of product necessary for clinical trials, so as to be able to look for partners from the pharmaceutical industry. We can also involve cosmetic companies, as the drug has also as anti-acne function." Relative to goals of health and wealth, and given the drug's utility, the possible outcome of involving a cosmetic company was just as plausible as involving a pharmaceutical company under considerations of lacking investment.

Thus we see here an ordinary and characteristic situation. The original justification and goal was to remedy a significant global health problem. Given real structural limitations in the institutional relationship between molecular biology and global health, and given the demand to produce value and utility, switching justification from the amelioration of global health to one of individual well-being through cosmetic applications produced no tension or conflict. In line with the demands of the method, I dutifully typed up our protocol and gave Giovanni a copy in the hope that it would provoke or perturb further reflections. A week or so later, he told me that he had been recruited by a large local pharmaceutical company.

ENLIGHTENMENT?

The aim of the STIR project fits squarely within what Luhmann has called "second-order observation" (Luhmann 1995, xxxiv; Rabinow 2008, 64–65). If the work of observing a particular object is what he called observation of the first order, then observation of that work of observation is of a second order, that is, *how* an observer is observing his or her object. The STIR method was particular insofar as it used a tool to attempt to capture observations of the second order. The results of the protocol could be conceived as a boundary object constituted from a threshold between first- and second-order observations: a threshold between a narrative of what happened and a reflection on how it happened, as well as how else it could happen. To some degree this would be a process of "enlightenment" in a strict Luhmannian sense: the making manifest of latent structures and functions (Luhmann 1995, 343). Such alternatives may be as "liberating" as they are "confusing" or "risky" (Langlitz and Helmreich 2005, 20).

In Erik's framing of the project, our formal goal was to render manifest the "latent content" in the system of laboratory work and decision making to reconfigure and integrate sociotechnical elements into the scientific work. In his formal schema, the use of the protocol over time could render latent norms observable, thus making them available for reflection, and then possibly producing goal-directed modifications. What was striking was the extent to which naming alternatives, as well as naming the complex environments in which particular scientific work is practiced, proved to be neither liberating nor risky. To take the example above, Giovanni was well aware of the constraints of his position, of the available possibilities, and of the different temporal horizons of the work, as well as of the practical horizons he faced, such as needing a stable job and wanting to produce a result (any result) from his work. He was adept at performing a functional description and synthesis, simplifying and thus protecting his position from that complex environment.

Here, then, lies the trouble: a possible goal and use of the protocol is to render more complex the field and action under observation, to reflect on that complexity within tolerable limits. In practice, however, the tool served observers to both describe and then manage the complexity of the relation of first to second orders of observation.

I will now show how this problem of the relation of first to second orders of observation within the scope and aims of collaboration was raised as a problem among STIR researchers.

CONTEST

In the summer of 2010, the ten STIR researchers met for a workshop to discuss our preliminary results. It offered me a chance to compare my experience of using the protocol with other researchers. There were a number of presentations as well as small group exchanges, of which one in particular struck me: a Dutch philosopher, Daan, presented examples from his work in which he mobilized a distinction between "first-order and second-order reflective learning" to parse out the responses he had gathered through his STIR studies. Our discussion of his work was a catalyst for me subsequently to probe the problem of our position and practice as STIR researchers with respect to our fields of engagement. Daan explained the distinction he was using:

> First order is within boundaries of a value system and background theories, so in science and technology this would be improved achievements of a scientist's own interests in a network. Whereas second-order reflective learning involves taking the background theories and values as the object of learning, so second-order reflective learning is a form of reflection on the research system itself. What is important is that this is symmetrical, and in that sense reflective learning is not therapy for the scientist because it can also happen to me. So . . . I don't want to go into my examples of first order, which are examples broadly speaking of health and safety and responsible conduct of research.

His first example of second-order reflective learning was one in which a researcher discussed integrating a human gene into a mouse. The student decided against cloning a human gene and opted for the alternative of using a mouse gene, because "where it comes from is a bit ethical." A second example was of Daan following Erik's rhetorical injunction to shift to a subjunctive mood when conversation seemed blocked. During a discussion of the considerations around the development of gene synthesis technologies, Daan tried to create a hypothetical decision space in which a researcher could map out what the future considerations might be, if it were the case that construction of whole organisms became routine using gene synthesis. He described his interlocutors' earnest profession of considered reflection in a nonironic mood.

Erik's response to the presentation was immediate, vigorous, and important relative to where we had come in our collective difficulties in thinking together about the practice we had engaged in and the ends toward which we wished to work. To capture it succinctly, he asked Daan to discount the second example, "because all that shows is that there is some utility perceived in doing this activity. It begs the question, well, what are you doing?" The first example, of the human and mouse genes, was "potentially interesting," in Erik's evaluation, because within the narrative one could see how "latent concerns that were not otherwise expressed were stirred up." Instead of labeling them as "ethical" and then moving on, he asked Daan to dwell on the example: "So then the next question is, so what? Did this stick with someone? Did it get taken up later? We're waiting for the other shoe to fall, and I was thinking that, I was thinking, do we have to go back to first order so as to see a change in practice? That might be the interesting thing about why, you know, we care about first order."

Agitated, Daan responded: "I've got hunches; I mean, I've been following them around, but that's all highly speculative."

> Erik: "Unless you're going to speculate, you're leaving me hanging, all you're doing is saying, well, anyone could go into the lab and say [adopting an ironic tone] 'what do you think about this?' and the response would be 'well, I don't know,' and I say, 'well, think more about it,' and they say, 'err, OK, I guess it could scare me?' That's not in itself enough to make me think, you know, you're using a method, you're systematically applying it, you're studying it carefully, you have these questions that are both practical and theoretical, where practical might mean political or it might mean ethical, and now you're giving me some information, some data, to analyze with respect to these things. I'm not completing the loop; I'm not seeing what the point is or why I should care.
>
> "If I happen to be critical I could say, you haven't told me anything. I'll be critical now: I'm not convinced in any of these cases we have second-order reflective learning. In your [written] narrative, I was convinced, now I'm not."
>
> Author: "Is that insofar as you don't think that there is a learning component?"
>
> Erik: "I don't even see that there is second-order reflection."
>
> Daan: "Second-order reflection was defined as reflection on the research system. Now you're asking, *and that needs to have consequences*."
>
> Erik: "No, I'm not asking that. I'm saying, first, I need to be convinced. You could show me a sign that says the research system exists, and you could prove that I just reflected on the research system and therefore it's second order, and I might have to agree with you logically, but somehow it doesn't seem very relevant. So that's my first question.
>
> The second question is: Just proving that reflective learning occurred, I'm wondering, why should I care? If there are consequences, I care, if there is a problem, I care. Consequences would be a surefire way to do it. There has to be something more."

THE ACTOR, THE ACTANT, AND THE PROBLEM

Erik's intervention indicated a problem at the heart of the protocol regarding the relation between the use of the protocol as a means for modulating scientific practice and the intellectual work and stakes of the one enacting the protocol. There is an indetermination shown in this experience that is significant for a broader question of anthropological practice within collaborations: How to produce a mode of intervention that can systematically move observers, social and technoscientific, from a first-order engagement to a second-order engagement with the objects and objectives of their epistemic practice? As Daan indicated, second-order reflection is characterized as second order precisely insofar as it is also something that could happen to him (or us.) And yet, this is precisely the location of discordance and impasse: the protocol did not prove to be a medium for the creation of a shared problem of a second order.

Erik was well aware of this problem. His solution: a justification of work and worth through first-order changes. That is to say: a justification of social scientific participation in terms of the amelioration (by one measure or another) of the scientific system's capacity to function in achieving its practical aims; a binding of second- to first-order observations. The critique of ELSI research and researchers as being overly parameterized by their position *outside* scientific systems is mirrored here: our position on the inside of the first-order norms and practices of the scientific system was ultimately restrictive with respect to the hoped-for aims of collaboration at a second order of observation and reflection.

During the last two decades (at least), there has been growing attention to this problem of position, mode, and form of engagement in which diverse actors might participate in expert domains, including social scientists. An ever-growing literature examines how boundaries of authority, as well as the objects and objectives of such authority, are delegated from political to scientific domains (and back) (Callon, Lascoumes, and Barthe 2001). Many of the people at the institution that housed the STIR project are principal voices in just such a (growing) literature. Such reconfigurations of authority, knowledge, and (other) power relations are often parameterized by a specific issue or object. To take the example of sociological engagement with the French Muscular Dystrophy Association (AFM), the object in question is a disease around which knowledge claims, power moves, and translations are arranged. A "shared problem" emerged when "framings" and "overflowings" (Callon and Rabeharisoa 2003, 250) *incited* responses from those affected in one way or another, or who wished to affect a field of relations.

A recent paper by Michiel van Oudheusden and Brice Laurent (2013) is important on this point. The authors outline a typology of normative commitments and practices for social scientists in terms of how they engage in "public participation" between publics and scientists. The authors explicitly take up the work of Rabeharisoa and Callon with AFM to explicate a modality of engagement and position for sociological-anthropological participation in collaboration found in "articulating social identities not previously considered or clearly formulated" (van Oudheusden and Laurent 2013, 13). In this modality, the sociologist functions to help configure identities for patients, doctors, and scientists in which boundaries and forms of knowledge are moveable and shareable. Such a mode takes up the specific ethical and political first-order stakes of the disease as a parameter in shaping their manner of engagement and the form given to overflows and the responses incited.

By contrast, the practice of a project such as STIR took a different form and position. Given its scope, scale, and mode, the project was also more ambivalent about the order at which observation operated. In practice, the protocol was a tool for *managing* frames and overflows *within* the scientific system. Its range of applicability was vaster than the specificity of the politics and ethics of specific problematic experiences and knowledge (such as of a disease). The protocol is itself entirely formal; its object domain could be anything. Its designer boldly pushed a method of dialogue to its logical conclusion—that anyone, anywhere, in any domain, through a technique of questioning, methodically tracked, could have consequences for the system in which the tool is used. The ethics of the method resided precisely in the conviction its designer placed in the separation of efficacy and subjectivity. Such a separation in STIR is what distinguishes it from the ethical stakes of a project such as Callon and Rabeharisoa's engagement with AFM. For the latter, their first-order engagement was precisely, if only partly, a question of the ethical subjectivation of the engaged actors, that is, becoming an ethically marked subject by virtue of collaboration, which involved clarity about *the stakes of the problem*—a point of ambivalence Erik himself came to recognize in our discussions of Daan's case.

Researchers with the STIR project were not representatives of another domain, such as the law or a biosocial group. We were not bringing society, an identity, law, or anything else into the lab. As Erik described our role, we functioned to reflect back what is already there latently in the scientific system, to assist in the operation of self-observation of scientists by scientists. Reflecting on how I practiced this operation, I observe that I produced a mirror function, which is precisely the condition of the mere *actant*. The question for the anthropologist becomes: Why do this? Our justification in STIR was to modify research in some way—but relative to what purpose?

As Erik previously described it, his diagnosis of the current historical conjuncture of institutionalized science, one that oriented what would be actualized in the STIR endeavor, was of a means-ends displacement, in which the means of doing science became the end. Erik responded by accepting this situation as fact and then working from within it for the purpose of transformation. His diagnosis was one of goal displacement due to the fact that the scientific urge to conduct curiosity-driven inquiry requires resources; the means to conduct inquiry becomes the goal. Reflecting on his diagnosis of science and putting the lens back on STIR, he asked a question that had had a spectral presence during my fieldwork: "So why then bother to do what we've been doing? Why collaborate? This is the tragedy; it may come back to this: that we're simply doing the modern project better."

EXITING IMPASSE?

In trying to do the modern project better through supposed collaboration, the contours of a configuration of impasses were made visible, impasses that left the social scientific collaborators little room for maneuver.

1. Positioning the form of boundary work within the scope and scale of laboratory practice ultimately constituted an excessive constraint for the possibility of *collaboration*—even if it may have been an appropriate position for other forms of observation. A critical parameter of the problem of collaboration is thus how and where such boundaries are positioned, how actors are assembled around such boundaries, as well as (crucially) reflection on the different ends to which such work is oriented, including ends that may be justified in terms other than first-order (scientific or political) intervention. This impasse indexes power relations at play and showed limits to how a pluralism of forms of expertise can be given an organizational arrangement, one in which social scientists could form part of the transformation of the scientific field.
2. Collaboration—and its impasses—indicated the need for capacities and means to interconnect first- and second-order observations. A critical parameter of the problem of collaboration is whether the position and mode of the anthropological inquirer permits collective work at a second

order of observation, analysis, and diagnosis. The STIR case provided a negative result, creating a resource for further reflection on future anthropological inquiries as part of collaborations.

3. Collaboration as a practice entails occupying a position marked by a heterogeneity of demands; this subject position is an ethical one insofar as it takes work on oneself to occupy it and to take on the demand of collaboration. The occupation of such a position may, however, be at odds with the other epistemic and ethical demands made of subjects—as for example in the Dutch philosopher's indignation that second-order observation could count only as such if it had first-order effects. It may also be at odds with the uses scientific actors make of the opportunities that so-called collaboration affords—as in my experience with Giovanni.

Experiments in collaboration, perhaps especially the impasses and breakdowns in such experiments, offer a testing ground for anthropological participation in the transformation of modernity's well-documented separations (politics/science, nature/culture). Collaborative projects index both a demand for and the logical possibility of reconfigurations of the structure and organization of institutions and practices, as in the work of boundary organizations. Showing the heterogeneity or contingency of boundary objects, however, does little by itself. One may well be able to redescribe science. Such redescriptions, however, may either prove inefficacious, or else, even if "efficacious" by one consequentialist measure or another, dissatisfying as an anthropological engagement with *problems* of science and modernity. How then to take up collaboration as an anthropological problem, and how to take up the ethical stakes of this anthropological work (cf. Rabinow and Stavrianakis 2014)? At a minimum and with due diagnostic caution, the self-cancelling experience of collaboration in STIR, buttressed by an example from a different domain (AFM), indicates the importance of attention to anthropological mode through which problems of collaborative engagement can be identified, problems in which power relations and ethical stakes of the anthropological position and practice must be taken seriously.

ABSTRACT

Anthropologists are increasingly invited to participate in collaborations with natural scientists, among other experts, in their capacity as anthropologists. Such invitations give pause for thought about the character of the positions and practices that an anthropologist can occupy and perform. This article draws on participant observation in the Socio-Technical Integration Research (STIR) project, an endeavor based at

Arizona State University, which aimed to modulate scientific practice. I observe and analyze the disquiet of participating social scientists by questioning the epistemic, ethical, and affective parameters of such modulation, in which social scientists were ultimately positioned and framed as actants—and not engaged as thinking subjects—for the reflexivity of natural scientists toward natural scientific work. I describe how such a method for increasing and extending the scope of scientific reflexivity was ultimately bound to the dominant instrumental norms and values of contemporary technoscience. The article suggests that reflection on problems of collaboration through questions of position and mode of engagement opens the scope and parameters for contemporary anthropological inquiry into anthropological collaborations within domains of science and technology. [collaboration; ethics; modulation; participant-observation; position; reflexivity; science]

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