Ethnography and interdisciplinary work: experiences from the US and Brazil

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ABSTRACT

This paper reflects on previous ethnographic research with multidisciplinary scientific teams in order to consider the place of STS-inspired ethnographies in thinking about and doing such larger scale, collaborative work. The practice of ethnography of/in multidisciplinary projects is both productive in terms of enabling knowledge about “interdisciplinary science in the making,” and disruptive, as it becomes a part of said knowledge processes in ways we have yet to fully consider. Through looking back at results from research conducted in the US (with a group involved in building a model of heat transfer in tissues) and in Brazil (with remote sensing scientists and work around deforestation monitoring), the paper explores two interrelated facets: (a) misunderstandings in interdisciplinary work are not reduceable to communication gaps, but pertain to differences in epistemic cultures, backgrounds, understandings of truth, and method; (b) participating in such interdisciplinary interactions places the ethnographer in an unstable but potentially productive position in terms of his own expertise. The article concludes by reflecting on how the exploration of these gaps and frictions can help construct a more active place for ethnography in such multidisciplinary efforts. By making explicit the disruptions involving such interactions (which include the ethnographer), they can be explored productively as tools for knowledge production.

KEYWORDS

Ethnography; STS; modeling; interdisciplinary scientific work; anthropology of science

1. Introduction

Ethnography has been central to STS research at least since the first empirical studies of “science in the making” or science in action (Knorr-Cetina 1981; Latour and Woolgar 1986) were published decades ago. These earlier studies included an interesting range of ethnographic work in different laboratory settings, composing a then emerging field of empirical discussion (Knorr-Cetina 1983) and ethnomethodologically inspired debates on science and scientific truth as a practical accomplishment (Lynch 1982, 1993). These studies were hailed as a turn (one of many to follow) from a philosophical and abstract debate around science and truth towards a more practical and empirical turn (Collins 1983), which would deeply mark STS in the decades to follow.
This empirical turn was also largely constructivist, which spurred many debates around the productivity of such ethnographic research for STS (Hess 1995, 2001), its relation to anthropology and to other disciplines (Franklin 1995; Reid and Traweek 2000), and also its limitations, including a lack of political bite (Winner 1993) and a difficulty in contributing to political and policy challenges (Collins and Evans 2002). Ethnographies of laboratories and other technoscience-related settings multiplied in the decades to follow (Knorr-Cetina 1992), expanding the scope of the questions asked and the mutual imbrication of ethnography and STS (Beaulieu 2004; Hine 2007).

This paper aims to contribute to the discussion by reflecting specifically on the ethnography of interdisciplinary scientific work. It reflects on my past research doing ethnographies of large scientific collaborative projects in order to consider the place of STS-inspired ethnographies in thinking about and doing such interdisciplinary work. I argue here that the practice of ethnography in multidisciplinary projects is both productive in terms of enabling knowledge about “science in the making,” and disruptive, as it becomes a part of said knowledge processes in ways we have yet to fully consider. My aim is thus to contribute both to debates around the problems and potentials of working in interdisciplinary settings, and to debates around ethnography of science and its potential to contribute to scientific practice in diverse areas where interdisciplinary teams are a common practice.

By looking back at results from research conducted in the US (with a group involved in building a model of heat transfer in tissues) and in Brazil (with remote sensing scientists and work around deforestation monitoring), the paper explores two interrelated facets: (a) misunderstandings in interdisciplinary work are not reducible to communication gaps, but pertain to differences in epistemic cultures, backgrounds, understandings of truth, and method; (b) participating in such interdisciplinary interactions place the ethnographer in an unstable but potentially productive position in terms of his/her own expertise. By communication gaps, I’m referring here to problems with the transmission of information between individuals. By epistemic differences, I’m referring to gaps associated with different understandings of truth, different tacit knowledges, and different conceptual backgrounds. The interactions enabled and sought by the ethnographers in their work can thus become more than a knowledge exercise or a potential disruption: ethnography can aspire to have a more active role in such projects by beginning to explore its potential to bring forth these challenges and working through them productively.

The advantages of doing a comparative discussion include the possibility of finding common themes among different ethnographic experiences in very different localities. Albeit done in very distinct socioeconomic and scientific contexts, the similarities in some of the challenges perceived were striking. Looking at some of those similarities can enable a richer conversation about the place of ethnography in studying but also in producing multidisciplinary knowledge. Far from dismissing cultural differences and situated aspects in terms of the analysis of scientific work and the challenges involved therein, the aim is to consolidate some common threads that tie together similar ethnographic observations, although done under very different circumstances.

The argument also does not deny the idea that scientific work and the knowledge produced through it is situated, partial and contingent, one crucial insight from many ethnographies of science (Latour 1995; Hess 2001; Fischer 2007). Comparing similarities in such
different contexts should not be seen here as an invitation to return to debates on what is unique to scientific knowledge in spite of cultural difference, which is of no interest to the argument. Stating that there are similarities goes hand in hand with reaffirming the situated and constructed character of knowledge production, in spite of the proximities analyzed.

What comparison can mean here is that through it we can also find points of contact which may show that differences in science are conditioned by elements other than geographical location or cultural differences. This has direct relevance to how we understand comparative ethnographic work, in terms of how we interpret differences observed or how we pre-classify realities (as “global North” or “global South,” for example). It also has relevance to reflecting on the place of ethnography in such projects. As I have previously argued, there might be an unexplored place for ethnography in directly participating in collaborative multidisciplinary efforts (Monteiro 2017). I want to continue that reflection here, bringing in more elements to the discussion through a comparative look at previous work I have done.

2. Doing ethnography of interdisciplinary scientific work

Past research on interdisciplinary teams (Baird, Moore, and Jagodzinski 2000) has identified some common problems which are present in such collaborative settings. These include what we could classify as issues of “epistemic distance”: different epistemologies and concepts between members and their disciplines; different methodologies and different forms of formulating research questions can make working together and understanding each other difficult. Other kinds of problems could be related to communication styles, having to do with how people express what they mean to say in collaborative settings (Bracken and Oughton 2006). For the purposes of this paper, we will focus much more on those elements related to epistemic distance, as they are central to what interdisciplinary work is supposed to deliver: a gaze that goes beyond the limitations of discipline, and that enables one to look at complex, multilayered problems in novel ways.

Doing interdisciplinary research in practice is much more risky than the above definition may suggest: when engaging in interdisciplinary work, participants must discuss views about each other’s work style, procedures of validity and definitions, issues that go to the core of how one sees an object or scientific problem epistemically (McCallin 2006). Having to explain oneself in such a setting, as has been described and will be discussed below, can put one into an uneasy place of uncertainty, much different than when those definitions and procedures are shared or even tacit (Collins 2001). As we will see below, this risk applies also to ethnographers in such settings: having to explain oneself during ethnographic work can also challenge certainties and make one revisit research questions and approaches, in ways not anticipated before engaging with the research itself (Shah 2017).

In that sense, such collaborative settings are not free from ambivalence or dishonesty (Fitzgerald et al. 2014), configuring a social setting that is still poorly understood in its full complexity, even as it is increasingly called to answer bold scientific questions or becomes the preferred modus operandi in large research projects (Jeffrey 2003). Cooperating between disciplines can be marked at times by distrust, as in the cases where social
scientific insights are ignored or dismissed by natural science colleagues (Rabinow and Bennett 2012), which relates directly to the place of ethnographers: can our knowledge gain more legitimacy as another crucial expertise alongside natural scientists, in looking at phenomena which involve both human and non-human actors and variables? If so, how can that be accomplished in practice? How can we learn from ongoing ethnographic engagements with scientific teams? How to advance the potential of ethnographies in these settings?

Doing interdisciplinary work thus involves not only negotiating epistemic distance but also negotiating other aspects of the work relationships between scientists: a politics of disciplinary knowledge that usually values quantitative or experimental research over qualitative ones; and also negotiating around language and other communication barriers. Authors have also discussed the issue of the political economy of rewards in such work: the costs of doing interdisciplinary science, aside from having to invest in a knowledge base outside of one’s specialty, involve the risk of losing out on rewards easier to attain when one works in a single discipline. These include career progression, awards and peer recognition (Kaplan, Milde, and Cowan 2017). These costs make engaging in interdisciplinary research too risky for some. Some authors have pointed out that students may be able to bridge those divides more easily by being at the start of their careers, lacking both previous investment in specific knowledges and the awareness of the high risks of such endeavors (Kaplan, Milde, and Cowan 2017).

It has become common sense to say that real-world problems do not respect disciplinary boundaries (Carr, Loucks, and Blöschl 2018), yet little is still known on how to actually turn collaborative projects into practical research results as similar challenges are repeatedly reported in empirical research on such work. Career impediments and costs; cognitive and epistemic distance; problems in communication and mistrust between disciplines are not new to studies in interdisciplinary cooperation. Evaluation frameworks have been developed (Carr, Loucks, and Blöschl 2018), and an increasing number of ethnographers like myself have engaged with such teams in order to report on how knowledge is produced and how multidisciplinarity is collectively achieved (Monteiro and Keating 2009).

My goal here is not to review or reiterate such findings, but to look at them under a different light: through analyzing retrospectively some of my past empirical research, I want to propose a more active role for ethnography as expertise in participating in such projects. The ethnographies discussed below span many years of interacting with scientists, participating in complex and multinational cooperation efforts which include health, biology, computer modeling, and environmental sciences. This wide array of research topics could be seen as overwhelming to process, yet the constancy of the challenges encountered in fieldwork enables one to begin a reflection on a place for the ethnographer in such contexts.

If ethnography as participant observation not only describes but participates and helps shape the practices being studied; and as a specific expertise interacting in an already multidisciplinary context, could ethnography be more than just observation, and become more participation in these projects? If so, what could that begin to look like? Ethnography has been construed as “anti-social” (Mosse 2006) in the context of analyzing professional communities. Mosse’s reflection on the challenges of being both insider and analyst is relevant to this debate, as he shows the clear impact of an ethnographer as a co-participant (in this case, in development work) and the risks associated with his analytical and writing
process. Friction caused by his analysis included loss of mutual trust and questioning by his peers. Can we avoid such perils as active participants in multidisciplinary work?

Social scientists are increasingly being called upon to participate in scientific teams (Goulden et al. 2017), as technologies are becoming ever more inextricably intertwined into the fabric of society, and as funders push for more access to real-world problems and faster innovation, which requires understanding audiences, perceptions, and values involved in technology. Research has shown the great potential for ethnography to collaborate in building technological systems such as information systems in health (Forsythe 1998), highlighting the cultural dimensions of technology, and technical systems (Forsythe 1993). But what is at stake in bringing in social scientists to study and participate in such teams? Will we be instrumentalized to provide social data to become input into larger technological systems, or can we have a different, more active voice?

3. Comparing the US and Brazil

Previously, I have done ethnographic work in laboratory settings where interdisciplinary cooperation was a central characteristic. These were located in very different places: one in the US, among scientists involved in the modeling of heat transfer in tissues; another in Brazil, observing groups focused on the environmental and deforestation science. After looking at these experiences comparatively, I believe I can both look for similarities and differences between these settings and the ways through which interdisciplinary work is accomplished. This enables an analysis of common threads between these contexts that can give us clues to a more productive place for ethnography and ethnographers in producing knowledge with, and not only about such teams.

The US research was done between November 2006 and March 2008. In it, I observed a group of scientists based at a major public university in the southern part of the country. They were working on a model of heat transfer in tissues, based on data acquired at a research hospital located in a different city 160 miles away, also making use of a supercomputing facility belonging to their university to help process the data and render the models. The team was made up of professors, postdoctoral researchers, and graduate students. Their areas of expertise include computer science, biomedical and civil engineering, applied mathematics, computational mechanics, scientific visualization, biomedical engineering, and medicine.

The team’s scientific goal was to produce a system that would enable accurate computer predictions of heat-induced cell damage to be computed and made available to doctors performing surgery in real-time. This technology would hopefully result in a new paradigm for minimally invasive thermal therapy for prostate cancer using lasers. This tool would be able to predict the behavior of cancer and healthy cells when heated, thus enabling the surgeon to make better informed decisions on how to conduct the intervention, effectively automating parts of the surgical procedure.

The ethnographic research consisted of attending and videotaping weekly team meetings, interviewing all active members of the team and doing observations in the facilities where team members worked. Thirty-two meetings of the group were observed. Videotapes of these meetings were analyzed. All members of the team were interviewed at least once. Observations were made at two talks and one international conference attended by the team, and at the supercomputing facility. Two trips were also made to the research hospital.
While the team was headed by an American researcher, internationally recognized in his field and also very high up in the university hierarchy, the team was very international, which was also a feature of my experience in this university as a postdoctoral researcher. The team had members from the US, Iran, India, and China, among others. It had at its disposal a state-of-the-art facility to work, which included ample meeting rooms, a diversity of laboratories to develop work with visualizations (which were crucial for the objectives of the project) and also a very advanced supercomputing facility. The presence of the advanced computing power was one of the features that made the research question possible, as there was then a major push in the US towards work with cyberinfrastructures (Bement 2007). Thus it was clear that I was located at the “center” of the global scientific system for several reasons: access to funding and infrastructure not available in many places around the world; presence of a very international research team, both in terms of professors and students involved; location of the project in a dedicated facility which included office space and access to advanced equipment (advanced computing and visualization infrastructure) unmatched by other locations in the U.S. and around the world. All of this contextualizes the project as a unique opportunity to observe frontier science in action.

As a postdoctoral researcher myself, I was given full access to meetings, but my participation in their research was limited to observing and recording interactions, except in one meeting where I presented my own research project and explained to the team what I was doing and why I wanted to videotape their discussions. I was not a full member of the team and did not engage in exchanges of the central aims of the project, as it did not include humanities or a social science component. This positionality was then not a factor in my own research questions, as I was focused on studying interaction. But looking back on it now enables a reflection on the elements I did not consider then, including the potential participation of an ethnographer in the team and the potential for this to help both the interdisciplinary work and even the final goal of the project. I was then a “local,” meaning I was a postdoctoral researcher at the same university, with similar professional responsibilities as other researchers in the team. This probably helped me in terms of being accepted as an observer but might have contributed to me being positioned by the team as mostly that, and not as a social scientist who would engage in dialogue about the research questions.

The ethnography in Brazil involved following a collaborative multidisciplinary and international project, which sought to produce data on the impact of global and regional climate change in forests and people, and how land-use and other activities also fed back into climate dynamics. The work was conducted between November 2013 and October 2014, including observations of joint discussion meetings (some of which were taped and transcribed), workshops and nine interviews with participants. I also attended one engagement workshop held in Brasília and a closing meeting and discussion held in the state of Pará, Brazil. The expertise of the participants included atmosphere and climate scientists, remote sensing scientists, environmental scientists, anthropologists, biologists, and ecologists.

The project sought to produce knowledge about possible tipping points1 (Nepstad et al. 2008) in the Amazon, as understood by current scientific debates on the biome.

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1Tipping points here mean, in deforestation and climate research (Nepstad et al. 2008), probable “points-of-no-return” whereby deforestation, if kept up unchecked, may produce changes in the relations between vegetation, climate, rainfall and other variables pushing the Amazon biome towards a drier, more fire-prone stable state. Thus, closer to savanna than to its current forest, according to some authors, meaning less biomass, less potential for incorporating carbon from the atmosphere and potentially causing other environmental and social problems.
The project wanted to produce “early alerts” of such possible points of no return, in order to provide policy makers reliable information about what to do in time to avoid irreversible damage to the forest. Publicity materials laid out project objectives in a way that suggested certainty (i.e. by stating that models would be improved by bringing in stakeholders to participate in creating scenarios for the future), but also made explicit how the project’s leaders were keenly aware of the relevant uncertainties involved. Modeling was a central aspect, as they hoped to develop a blueprint for an early alert system based on better predictions.

My position in this case was different than in the US. I was already an assistant professor, and thus my negotiations to enter the field were different: I was accepted as an observer by the PIs of the project, but I was also called upon in different moments to contribute to reflect on goals and research questions by some colleagues who worked on the project. I had met these people in previous ethnographic work and was already a more well-recognized person by the community which was doing research on deforestation, remote sensing, and climate change in Brazil. In some instances, I was also asked by some colleagues with which I had closer institutional and personal relationships to help informally with project questions and reflections, due to my background in social sciences and interest in environmental research (which was at the core of what this project wanted to achieve), even though I was not officially a member of the team. Being a professor, but also being from Brazil in a group with both Brazilians and Europeans, in a project concerned with social and natural aspects of environmental dynamics in the Amazon might have helped me to be seen in this case as a (sometimes) potential interlocutor and not merely a pure “observer.”

This was also a very different experience than my US ethnography in terms of the geopolitical issues at stake: the PIs were European, and much of the funding was international. Most of the project governance happened outside of Brazil. Also, the project questions were developed outside, pertaining to the issues of interest in international calls and the careers of these international researchers, who could access this funding. This hierarchy was not at issue during my ethnography, as I was then interested in different variables related to computer modeling and knowledge production. But looking back in a comparative fashion they are crucial to better contextualize how science is made in such drastically different conditions.

My experience in both projects was marked by the explicit nature of the myriad misunderstandings involved in the daily work and interactions between scientists. This was especially visible when scientists were presenting and explaining research to each other, a practice very common in work meetings in both settings. Such meetings were crucial moments of exchange, where data and perceptions about the problems at hand were interactively produced. It was precisely in those moments where the challenge of achieving understanding across disciplinary boundaries and different epistemic cultures (Knorr-Cetina 1999) became more visible, as will be discussed below. And in my interactions with them, the issue of my own scientific or epistemic points of view came to the fore.

The challenges in working across disciplines were very often treated and perceived as “problems in communication.” What I argue below is that these challenges cannot be reducible to problems in communicating ideas between different individuals but should be understood as misunderstandings related to epistemological premises, ideas of objectivity and forms of treating specific empirical material as evidence. Thus, these were problems of an epistemological nature (related to different epistemic cultures, which include shared
concepts and methodologies, tacit understandings and practices, shared conceptual backgrounds) and not communicational (meaning here issues related to the exchange of information; “information” meaning a stable quantity of concepts or data which in principle would be able to be shared transparently between individuals). Seeing the challenges in cooperating as epistemic and not communicational enables a broader understanding of the issues at stake in interdisciplinary work, which includes a potential place for ethnography to contribute epistemically in such collaborative efforts.

3.1. Problems in communication?

One common theme in both ethnographies, as already mentioned, was that scientists tended to see problems in working across epistemic boundaries as problems in communication, thus helping to make invisible the problem of how to effectively cross such boundaries or at least find a third common language which could be based in specialized instruments or in partly shared concepts (Kaplan, Milde, and Cowan 2017). They revealed in interviews and in informal conversations problems in understanding other peoples’ work during project meetings; they complained about time spent explaining what an experiment meant to an audience made up of scientists from very different fields; there were also complaints about the work it took to arrive at results when having to deal with the lack of common definitions and parameters of objectivity. This is far from a small problem: if being multidisciplinary is about looking at a complex problem from different angles and viewpoints, thus enabling a richer, fuller view of a whole, then the inability to do so points to important gaps in the potential of such cooperation to produce innovative answers.

Thus, naming these problems as issues of communication worked to produce a neglect of some of the larger issues at hand, in spite of the best intentions of both teams and their leaders. If they were merely communication problems, then they could be solved by simply finding better ways of explaining something, for example. But if the problem is elsewhere, and thus if the other problems are not addressed, then the solutions to them are also beyond the scope of managing communication, which is a gap I want to argue could and should be better explored by ethnography.

In the US case, this was very clear throughout the research. The meetings observed often started with sometimes lengthy explanations about the definitions used by each researcher in their work. As one participant said: “Okay, we made our way through the definitions! (laughs) Let’s now look at actually what we’re using” (Monteiro 2010b). In this project, since the work was mainly related to the building of a computer model, many meetings dealt with specific images produced by different PhD students and professors: images of heat, MRI images, visualizations of cells, and cell death rates due to heat, or images from graphs, which caused many times confusion among participants. Invariably they needed to get all participants minimally up to speed on how each image was produced, what it actually was meant to represent, how each researcher defined noise, error and other parameters.

The empirical examples below help illustrate the many instances where misunderstanding refers to much more than issues in communication. In one case, a PhD student with a biology background talked to the team about her part in the project, related to modeling temperature change in tissues: “The whole point of our model right is to model the uh, changing, temperature throughout the prostate, this is the heat transfer, model and
we understand that, temperature comes as a change in energy” (Monteiro 2010a). While for the purposes of the model she had to consider tissue as homogeneous, and the variable temperature then could be looked at isolated from others (“as a change in energy,” a definition from physics), in interviews she commented how this was a reduction of the complexity of how temperature in fact behaves in living tissues, as there were different layers of skin, fat, and muscle, not to mention the blood flow in these tissues, all of which made it very hard to establish the exact dynamics of temperature through the entire living organism.

In this case, the misunderstanding was pointed out by one researcher in terms of what she perceived as reductionist understandings on the part of the rest of the team, especially those focused on modeling, which tended to reduce biological complexity. While this was voiced to me during a conversation, this was not made explicit in the meetings I observed, which could have implications on how the model was framed, the potential for accurate measures deriving from a model with embedded assumptions about tissues, and a host of other factors implicated in how different researchers understand the model and what kind of model is ultimately produced.

In another exchange, we see how scientists understand reliability and objectivity in very different ways. A PhD student with a medical background was presenting her results to the team during a meeting. Her research was related to developing cell cultures, and she was trying to establish how they reacted to an increase in temperature. She presented data from a flow cytometer showing cell death, and after she was done there were questions about how reliable her measurements were, how accurate they were and how the instrument actually worked (Monteiro and Keating 2009). These were all questions of no immediate concern to her, since she had come to accept (as she let me know in interviews) that particular instrument as a black-boxed reliable method of measuring live and dead cells. But to others, she needed to explain in much more detail than she was expecting every physical parameter involved in her experiment.

A question about the data acquisition process of the flow cytometer shows how, while one member (...) is fully confident in her instruments, the other member (...) is working to assess the experiment through the usual methods used in his field. The instrument in question is commonly used in the type of experiment Laura is doing, namely, in vitro cell cultures that will be used in validating the model through heating experiments. While she presented data concerning numbers of live or dead cells accepting the flow cytometer’s reliability, many members had questions concerning her methodology and the physics of how the instrument worked. (Monteiro and Keating 2009)

Thus, in a collaboration involving modeling, biology, physics, mathematics, and medical sciences, results deriving from a flow cytometer had very different implications: members unfamiliar with the instruments asked many questions concerning the internal physics of how dead or live cells could be counted; for the medical researcher, these were issues of little relevance, as she trusted the instruments and how they presented results. She was more concerned in interpreting the results emerging from the experiment. The frustration on both sides was not reflected on during meetings, but I was able to detect it through my interactions with the team. Yet, had they been openly debated, there could potentially be a discussion of the results which benefited from interdisciplinary views, maybe establishing better experimental methods, instead of naming these difficulties as problems in communication.
While problems in working across biology and modeling/mathematics were at play in the US case, the issues in the Brazilian example had more to do with how to deal with variables from social and environmental phenomena. While this is a common issue in most research of environmental issues, it was clear that incorporating social variables into an environmental model was a bigger challenge than anticipated even by the PIs of this project. One PI mentioned the problem of dealing with policy actors, something he felt could be a central contribution of the project. But he also mentioned problems in scale and in the “mismatch” between different sets of data:

And I’m not saying we are going to do very much better, but we are trying. Because we also see those problems [in communicating between scientific work and policy]. There […], there is a mismatch between the scale of modeling and the scale of, and the scope of the policy, and also other socio-economic research. If you need data on a municipality level for socio-economic research. For physical modeling they are on a pixel basis or something. How do you match one with the other? (interview with project PI, Brasilia, November 2013)

The challenge posed by matching socioeconomic data with modeling data and practice is similar to what was at stake in the US case. Also similar was how the modeling component tried to produce numerical data amenable to become input for a computer model. The socioeconomic component did not always deliver those types of data, and the objective of relating to policy created another complication which was not solved during my period of observation.

The PIs of the Brazilian project (who were European, leading a very diverse and multinational project) were more aware of how the issue was much more than a communication problem than in the US example. As the excerpt above makes clear, he perceived the problems in dealing with policy as issues in scope and in scale between natural scientists and those dealing with social issues (policy and social scientists). The problems involved in working between social and natural sciences were also explicitly stated in interviews and interactions in this project, in ways that were not present in the US case. However, like in the US, there was no clear answer as to how this could be dealt with, and there were no instruments in place within the project to both manage and possibly harness other understandings from such moments of friction and difficulty.

As an ethnographer talking to most people in both teams, I had a specific, even privileged view on this problem. As is typical of ethnography, I could perceive that the explicit aims of working well across disciplines often verbalized by scientists did not always translate into effective work in practice. The elements that were not so explicit were thus very revealing of how that was a constant issue throughout both projects. In the US case, the problem was how to bridge biology and medical understandings of tissues, cell death, and cell growth with mathematical modeling and physics. In the Brazilian case, the problem was how to produce social data that would be usable for environmental modelers, as well as how to effectively interact with policy actors in order to have real-world impact.

In both cases the objectives of working together were clear, yet there were no explicit ways to manage the problems detected by the ethnography. There was no explicit debate of those issues, but they were rather discussed privately or not mentioned at all. The ethnographic expertise of being able to analyze this mismatch between discourse and practice was in this case useful to detect the issue, and could potentially be harnessed if used to make explicit how this mismatch affected their work. By making open such problems,
and by working together with specialists from different disciplines in understanding this as a larger and deeper problem of epistemic misunderstandings, an ethnographer might effectively help make such frictions and silences into a productive dialogue and help produce knowledge in ways not being practiced thus far.

3.2. The place of the ethnographer

Negotiating my presence in the field through explaining and ultimately negotiating an epistemic positionality is another thread that runs through both experiences. Interacting with other scientists meant explaining “what I was doing” to them, which involved very often me having to explain my epistemic points of view, explaining what anthropology or ethnography was or meant, and what kind of data I was hoping to gather while interacting with them. As fellow scientists, the people I interacted with both understood what my own research practice was, but not always viewed it as having the same epistemic authority as their own science. This could be argued to be a feature of ethnography in any setting. But in an ethnography among scientists, this led me to reflect on how this everyday process of ethnography might be productive for knowledge production in teams: by promoting such reflexive engagement with epistemic framings, research questions, and theoretical frameworks, ethnography could with its specific expertise help such teams produce knowledge in ways not yet being practiced.

In both ethnographies I was in a sometimes uneasy position: both an outside researcher looking at the projects from a distance, and a sometimes-direct participant, engaging directly with part of the activities of the project. This uneasy position, on one hand, forced me to negotiate often what my place was, both in dealing with participants, and in reflecting through my experience, as other anthropologists have reported (Mosse 2005). While I had no fixed tasks, no fixed office space or desk in any laboratory, I was called upon to participate in specific moments: to present my own project to the group (in the US case); to present a paper at a conference as part of the group (in the US and the Brazil case); to help mediate activities organized by the project (in the Brazil case), or to help interpret results (in the Brazil case, albeit informally). Such moments put me in a position to question how deep I was embedded in the project (was I becoming part of the project, and thus losing my “objectivity?”), and also to reflect on the potentials of engaging more directly with such projects as co-participant.

In one instance I presented a paper along with the scientists I was studying, during the US research. The conference, held in 2007, was a major gathering of researchers working on computational mechanics, and the team presented their work on modeling biological processes. I presented my own ethnographic work, which generated some curiosity among audience members. Being perhaps the only ethnography at such a conference, this was to me then a unique opportunity to establish dialogue with other computation scientists about my qualitative work, also presenting a challenge of how to communicate my ideas to such an audience.

At the end of the panel I was surprised to see that many scientists were very interested in what I was presenting, but for reasons different than what I expected. Rather than being engaged with the problems of interdisciplinary work or modeling, they became very curious about the potential for the patients in experimental treatments to refuse or resist being operated by machines, something I mentioned in passing during my talk. As the group I was studying was presenting such a proposed treatment for prostate
cancer, they maybe realized that one possible overlooked aspect of the work was the patient: would they accept being shot with a laser, even though in theory this was “minimally invasive?” More importantly, would they trust the model, a machine, over the expertise of the doctor? Even though the idea was that a computer model would predict and be able to calculate the optimal intensity for the laser as well as the location of a tumor in the prostate, some in the audience were worried and came to me after the talk to ask about such a problem. I did not follow up on this group to see if they incorporated social science in later stages of the process, but this is a case where the potential for an ethnographer to participate actively in helping shape the technology is very clear.

I was pleasantly surprised about this curiosity regarding practical applications of my results, which emerged from qualitative data and social scientific framings of the model. I was also intrigued at this point about the potential for researchers like me to integrate such teams with “full citizenship,” participating in the idealization and the construction of such medical technologies. Aside from cooperating with scientists in realizing issues with interdisciplinary work, this experience engaging in dialogue at the conference showed me potentials of collaborating with teams beyond what I had initially set out to do. And although such collaborations are becoming more and more common in different areas of research, there is still a lack of realization of the productivity of such potentials and of the collaboration of ethnography beyond revealing unseen dynamics: ethnographers could aspire to produce interdisciplinary knowledge and even technologies alongside such teams, if given the opportunity and if their contribution was better understood by other scientists and even themselves.

This could be accomplished, for example, in terms of incorporating findings from ethnographic engagement with patients suffering from prostate cancer at the very start of a project attempting to construct a model to be used in treatment of the disease. These data would help to frame (and possibly reframe) some of the objectives of such a model, in terms of how it would function (depending on potential fears detected, as in the example mentioned). The ethnographer could also become relevant in terms of navigating the uneasiness and frustrations inside interdisciplinary teams, helping process difficulties encountered in dealing with numerical and biological data. The example below helps unpack these potentials based on interactions observed in the Brazil case.

This instance occurred during a workshop for the Brazilian project. During the workshop in the city of Brasília, where scientists discussed with policy actors different scenarios for environmental degradation in the Amazon, the PIs divided the group into smaller subgroups which would focus on specific aspects. I was promptly placed in one subgroup, and not only participated in the debates but also was named speaker during the broader discussion after our separate debates. Each subgroup included team members and also representatives from invited government and policy institutions (Ministry of Defense; scientific institutions such as Embrapa and INPE; Ministry of Fisheries, Ministry of the Environment, Ministry of Agriculture, among others).

As a participant observer, I helped the debates and thus took part in the activity as a team member of sorts. This was not planned, and I was enthusiastic about helping in the exercise as a way to become closer with the research team. But this experience also made me reflect on the untapped potentials of integrating ethnographers into such activities. As a de facto participant, I was able to help not only discussing the “social variables” which were so much of the focus in the project, but also in being attentive to the dynamics of the exercise itself.
(as this was valuable data for my own research). One element which stood out was the size of the subgroups: one of them focused on natural resources was much larger and had a more central spatial setting in the venue. The other, which was debating social issues, was very small and was also spatially marginal. When the larger group reconvened, and all of the subgroups presented their results, it became clear that the natural resources group had a more quantitative focus than the social issues group: they presented numbers relating to variables such as prices of raw materials, employment rates and other such indicators which could readily be transformed into numerical data.

Meanwhile, the social issues group debated topics such as violence and corruption and how they could be directly related to environmental degradation. Violence related to uncertainty in land ownership and pressures from illegal logging were mentioned as factors which contributed to deforestation and environmental degradation. Corruption in cities of the Amazon region, which would be a factor in promoting violence (as political leaders would align with or even promote illegal logging, for example) was also mentioned as a variable. The difficulty in quantifying such variables in order to integrate them into models was discussed by an anthropologist from the project, but the debate was not followed through in the exercise.

In this example, an ethnographer embedded in such a project could be a disruptive element, by questioning some of the assumptions that orient the project as a whole (e.g. how the modeling was being conceptualized) and not letting the debate over some variables be ignored. Such a disruption could have a very productive effect if it had a legitimate place in the debates: by making explicit some of the less visible assumptions and premises of the project, they could help rethink the early alert as it was being envisioned by pointing to limitations and inadequacies not considered beforehand. They could bring to the table elements not reflected upon by PIs when the project was imagined, such as violence or corruption, but which could be more crucial to the success of preventing environmental degradation than a computer model by itself or the easily quantifiable variables more amenable to become inputs for the proposed model.

In both cases, the potential contribution of the ethnographer’s presence was not under consideration by the team under study and was also not a central question of the ethnography itself. Therefore, the issues presented, and the potential contributions mentioned above are analyses constructed after the fact, having been through both experiences and having reflected on them in articles, talks, and conversations with fellow ethnographers. They were not the results of my ethnographies before this paper, although they began to be issues to me as a result of my own engagement with both studying interdisciplinary work and reflecting on ethnography of scientific teams.

Although I did send some preliminary writing to the PIs in both cases (and a video produced as a result of the work in the US case), I did not receive much feedback in terms of my ethnographic points of view.\(^2\) I do not have data on how this writing could have affected the projects, as I am not sure how much of it was accessed during or after my

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\(^2\)This could be because the writing was related to my own research questions, and not to the teams’ projects or objectives. It is unclear in both cases if PIs had access or interest in ethnographies of science and technology which could inform the goals of these teams. But as such work becomes more available, and their value becomes more recognized, there is a potentially broader impact to be had by strictu sensu ethnographic work as it becomes a reference to work done by interdisciplinary teams. The arguments presented here, however, point to impact related to the presence and interactions of the ethnographer during scientific work; other discussions would require other kinds of data in order to be properly developed.
presence. Also, this was not an issue I discussed with them during the ethnographies, aside from conversations about my methods. Thus, the potential for ethnographic writing, which is the main product from my work in both cases, to become important in such interactions is unclear without further research or engagement with the scientists. The issues discussed in this paper were not debated with the PIs of both teams, and thus were not a factor in defining their objectives. Any such impact would have depended on conversations during the stages where problems are being defined and research groups are being negotiated. The complex dynamics of how this process works and how problems are defined in science is a research topic in itself and surpasses the scope of this particular paper. But it is worth mentioning that it is at that stage that the more active presence of ethnographers and social scientists should be negotiated: when project ideas are developed and submitted for funding.

4. Conclusions

As discussed above, ethnographic engagement with interdisciplinary work has enabled rich debates on the reach and limitations of such work. Empirical research has demonstrated a series of challenges which are involved in doing interdisciplinary team work: understanding each other’s theoretical premises, ways of working, concepts and conceptions of reality can be daunting and present difficulties that go beyond problems of communication but relate to epistemic misunderstandings. Aside from this, collaborating across disciplines involves risks not often discussed explicitly: career risks, as scientists may miss out on opportunities seized by colleagues which remain disciplinary; and risks in dedicating oneself to new knowledge, which requires questioning one’s own knowledge in ways not always well understood or explicitly debated in such initiatives.

My own ethnographic experience involved research in two different settings, but with similar challenges. One project involved a team of researchers building a model of heat transfer in tissues. One of the main scientific challenges was to work across computational modeling, mathematics, medicine, and biology. The other was a project developed in Brazil, involving a multinational effort to understand and model environmental dynamics in the Amazon. The scientific challenges here were more related to how to incorporate the social part of environmental dynamics into coupled models, involving thus discussions of both what those variables are in environmental science, and how they could become input for a computational model.

Looking back at these experiences, I argue here that there are two aspects of the results we can analyze in order to find common threads between the projects, using them to help rethink the place of ethnography in interdisciplinary teams: (1) how naming difficulties in understandings each other’s work as “communication problems” reduces them to something less than issues of epistemic distance and thus makes them more difficult to address and (2) the uneasy and uncertain place of the ethnographer in these groups, as they become participant observers and also take part in scientific activities and discussions along with the team members, becoming thus subject to constant reevaluation of their own contribution and premises like other participants.

I argue that these common aspects can help us rethink a more active place for ethnography in such projects, where it could be another expertise among the many involved, bringing in its own specific concepts and practices in order to produce innovative knowledge.
Ethnography, inasmuch as it allows for a close embeddedness in an interdisciplinary setting, enabling the social scientist to become more fluent in the myriad epistemologies at play, can produce unique understandings of how teams work in practice. Ethnographies can point out important mismatches in how this practice differs from explicit discourse, helping these to become objects of reflection in team dynamics. Ethnographies of scientific work have been very important in describing how knowledge practices are constructed and how evidence and “facts” emerge through interactive processes. This specific expertise can be part of a reflexive exercise within scientific teams which could promote new knowledge through different framings of knowledge and technology-in-the-making. Just as ethnographers have described how science and technologies are practical and interactive accomplishments, embedded in historical and cultural contexts, they can help re-orient such processes as active participants in the interactions they embed themselves in.

By dealing explicitly with issues of epistemic distance, ethnographers can help create common ground between disciplines and help address problems made less visible by being ignored or seen as issues of communication. Ethnographers can potentially be invaluable collaborators to interdisciplinary work in areas such as health and the environment, where variables related to biology, society, and mathematics are usually intertwined. Not only problems involving a social science component, but also projects where building new technologies are at play, or where interface with policy is a crucial aspect.

The objective here was not to propose a finished model of how this active participation can be achieved: doing ethnography, as others have discussed, is a praxis (Shah 2017), potentially revolutionary, where some of the more productive aspects are many times unforeseen before the engagement itself with reality happens. With that in mind, we should systematically look at how ethnography is being incorporated into large collaborative projects, in order to assess common gains and challenges being faced by these attempts. Also, we should strive to construct more normative and descriptive reports on how such collaborations with ethnographers can help in large projects, where building new technologies or discovering new knowledge is at play. That could help consolidate and legitimize our participation both in uncovering the dynamics of knowledge production and in producing new science and technology in novel ways.

Disclosure statement
No potential conflict of interest was reported by the author.

Funding
This work was supported by Fundação de Amparo à Pesquisa do Estado de São Paulo/São Paulo Research Foundation: [grant number 2013/11592-6]; the University of Texas at Austin.

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