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Not Another Case Study

A Middle-Range Interrogation of Ethnographic Case Studies in the Exploration of E-science

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This article addresses the need to problematize "cases" in science and technology studies (STS) work, as a middle-range theory issue. The focus is not on any one case study per se, but on why case studies exist and endure in STS. Case studies are part of a specific problematization in the field. We therefore explore relations between motivation for the use of cases (especially ethnographic ones), their constitution, and ways they can be invoked to make particular kinds of arguments in STS. We set out to examine the case as an object that links together research practices, intellectual debates, and programmatic concerns in our own work. Based on our experiences and on this reflection on the links between cases and questions in STS, we propose a number of case-making strategies that shift and enrich the deployment of ethnographic cases as an epistemic tool in STS.

Keywords: ethnography; modeling; theory; reflexivity; interdisciplinarity; epistemic cultures

Cases as a Middle-Range Issue

The lightweight ladyshave. Pasteur's network. Bicycles with big wheels. The visual power of bubble chambers. Dolly and her kin.[1] All these are well known in science and technology studies (STS), to the point of being emblematic of the field. They furthermore link STS to multiple audiences, and summarize much of what STS has to contribute. They also indicate the way in which these contributions are made—through careful examination of a particular episode or object, or in other words, as case studies. These case

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studies have borne, and are born of, important developments in STS. Cases would therefore seem to be important elements in the epistemic landscape of STS. On the other hand, the accusation that one's account is just a case study is a pretty damning one in these circles. In our research group, we have even been known to welcome each other's ideas for research projects with an eye roll and a muttering of "not another case study."[2]

This anecdotal beginning illustrates a deep tension around the roles cases can play, whether as ends in themselves, as illustrations, or as building blocks for theory. Case studies are good for deconstructing claims of universality (i.e., the scientific method). But how can they be used to inspire other work, without implicitly or explicitly falling into a universalizing fallacy? The obvious answer seems to be that everything is always, in the end, a case study. This move, however, makes the notion of case study so universal that it threatens to become meaningless. In this article we examine this tension, tracing its historical contours and implications for current and future research. This article therefore does not fit neatly in the genre of contribution that has come to be expected in STS journals, namely a report on a case study (see Wyatt and Balmer, 2007). Rather, we examine the entwinement of certain (kinds of) questions in STS, with the selection and the constitution of cases for working out these questions. We do so specifically in relation to our own work, to be as concrete as possible about the problematization for which cases are brought to bear. A focus on the "why" and "how" (and not just on the results) of our own use of cases enables us to reflexively sketch the institutional and cultural context in which the elaboration of cases is selected as a strategy for making knowledge, and to posit benefits of using case studies in new ways.

This article responds to an invitation to reflect on the "middle range in STS."[3] We use our ongoing research to illustrate why a "middle range" might be useful, and to demonstrate what it might look like in practice. The focus is on the use of ethnographic case studies, though the relation of this approach to our other methods is relevant. We use our experiences with these cases to constitute a middle range as the relations between methods, concepts, and empirical work. The middle range is neither a middle point between data and theory, nor a middle ground between the micro and the macro level of analysis. The middle range is explored through careful consideration of situations in which the deployment of ethnography either gives rise to dilemmas and seeming shortcomings in our work, or seems successful (or both). Middle ranging, we argue, enables us to make sense of these successes and failures.[4] By paying attention to the middle range, we are able to reflect on changing relations between methods, concepts and empirical work. These changes mark new opportunities for the use and constitutions of case studies in STS.

Working with Cases in STS

The use of cases can best be understood in relation to the research context and tradition in which they are pursued. Particular kinds of arguments can be made using case studies and, vice versa, case studies are also shaped by the aspirations of researchers to put them to use in certain ways. These associations between what needs to be investigated, how to investigate, and what counts as adequate evidence, constitute problematizations. This term is used here along the lines of Rabinow's reading of Foucault, as:

> the ensemble of discursive and nondiscursive practices that make something enter into the play of true and false and constitute it as an object of thought (whether in the form of moral reflection, scientific knowledge, political analysis, etc.). (Rabinow 2003, 18)

Problematizations are deeply social: what one can think about, query, and resolve are not merely cognitive puzzles. Rabinow discusses the uses of cases as a kind of problematization, one that is oriented to the practice of solving problems, and in which judgment of the practitioner in relation to local circumstances is highlighted.

The use of cases has been studied in STS as a feature of a science or professional practice. For example, the creation of cases in psychoanalytic literature shows the dynamics of power and canonization in an emerging field (Bos 2004). In other work, the use of cases has been argued to constitute a link between the biomedical and human sciences (medicine, psychology), where cases used in clinical settings are contrasted to the use of model organisms in biology (Ankeny, 2007). Morgan has also explored the discipline-specific ways in which facts can and cannot be extracted from particular cases (Morgan 2006). There are, it seems, discipline-specific ways of valuing and disregarding cases.

But what of the use of cases in STS itself? The notion of the disunity of science is one that has been intensely argued on the basis of contrasting cases in STS, for example in work that mobilizes tensions in controversies to emphasize the contingent nature of knowledge claims (Martin 1988; Hagendijk and Meeus 1993). There are undoubtedly other kinds of arguments for which case studies are deployed, but the pervasiveness of cases in works that address this topic is noteworthy, whether drawing on historical cases (Galison and Stump 1996), or contemporary ones (Traweek 1988; Knorr-Cetina 1981; Heintz, Merz, and Schumacher 2004).That the value of studying diversity in science is well accepted is further evidenced by the fact that scholars

debate the need for ordering cases (Geels, 2007) and the best level for performing comparisons (Knorr-Cetina 1999).[5]

Cases studies have also been used in STS to highlight the diversity of prototypes and forms of certain technologies (Bijker 1995), of users (Oudshoorn and Pinch 2003), and of uses and origin stories of technologies (Dumit 2004; De Laet and Mol 2000). Ethnographic cases have served to explore the singularity (or specificity) of a scientist deemed especially creative (in contrast to others; Mialet 1999). Diversity, as evidenced by contrasting case studies, is then used to demonstrate the nondeterministic character of technological development, since no single feature or logic can serve to demarcate it.

While there are important differences in the works mentioned above, it is striking that they all use cases to emphasize the diversity of science and technology, to de-essentialize them. Variation across cases is used to show that there is no single way of making knowledge or no universal meaning of a technology. As such, these case studies push for explanations of these differences, and explanations are sought in cultural, social, and institutional elements, rather than in ontological aspects.[6]

From this brief sketch, it is clear that there are significant epistemic advantages to using contrasting cases in STS. But what are the assumptions and limitations of such a problematization of the study of knowledge? In the course of planning and pursuing ethnographic case studies, we have reflected on these practices, and present here what we perceive to be their advantages and conundrums. The work below is a middle-range investigation that relates our aspirations to our methods, our methods to researching and writing, and our research activities to our theoretical and intellectual concerns. In the next section, we sketch our domain of work (the critical exploration of "e-science" and its possible implications for humanities and social sciences), and show how it is predicated on the diversity of knowledge-making practices. Our middle-range exploration therefore leads us, in the first instance, to make explicit our problematization of e-science.

Our Critique of E-science

Our current work at the Virtual Knowledge Studio for the Humanities and Social Sciences articulates a critical discourse about science and scholarship that relies to a great extent on the notion of diversity. One of the main aspects of our research program is a focus on practices of knowledge production.[7] While we willingly associate our work with cyber-infrastructure and e-science, we are keen to distinguish it from a technology-driven approach

by emphasizing the specificity of existing practices. Ethnography, together with historical scholarship, is often used as a method to establish this specificity, and we have set up a comparative structure in a number of our projects.

Maintaining sensitivity to diversity is something we believe to be crucial, but this stance also helps position our work as a critical discourse, one that contrasts with the one-size-fits-all steamroller of e-science. For example, we have recently written about the dominant stream in e-science development, which assumes that generic tools can fit all knowledge-making practices. Using ethnographic work performed in women's studies, we have been able to formulate two points (Wouters and Beaulieu 2006). First, women's studies is a field, unlike molecular biology or radiology, where computation and other manipulation of data are not core activities. Tools developed for computation would therefore have little appeal in this field. Second, through our conversations in the field, we were able to fantasize about different kinds of e-science tools that might well serve women's studies' ways of making knowledge. In other words, we asked, "How would e-science look, if it were to be elaborated from the epistemic culture of women's studies?" To briefly sketch an example: in women's studies, the principle of searching and extracting elements from texts is largely antithetical in this field. E-science tools that would be rethought to encompass context, rather than dismiss it, would be much better aligned to textual practices in women's studies. This case study enables us to identify a difference, one that is (so far) not recognized in the approaches, tools, and discourses of e-science. These contrasts, between a specific case and e-science as it is being given shape (in particular in the U.K.), shed light on the way e-science is oriented to particular cultures (and not others!). It enables a critique of its claims to offer generic tools for the benefit of all of science, and challenges its implication that there is something wrong with those who ignore or resist the self-evident benefits of such tools.

The critique of e-science sketched above may not seem extremely original, when placed in the context of this dominant problematization in STS. Yet, it is a valuable insight that builds on decades of STS research. While recognizing the usefulness of this problematization for our program, we also feel at times constrained by the notion of the case as usually configured. But first, we consider limitations of case studies in our work, given our current problematization, in which diversity is central. Noting the need to make cases comparable to demonstrate diversity, we then query the traditional importance of space in ethnographic studies, and the role of cases in interdisciplinary work.

Comparison and the Place of Ethnographic Case Studies

It seems that a comparative approach puts forward diversity, and that it is well suited to context-sensitive approaches that are important to STS. But using case studies does not only stress the uniqueness of instances, since that uniqueness relies on the use of what we might call a "comparison engine." In our work, we have used the framework of epistemic cultures (Knorr-Cetina 1999) with its strong comparative element, where cultures serve as templates against each other. In this framework, three features of epistemic cultures are important: the relation to the empirical (data), styles of experimentation, and social relations between units in a field (lab or research group structures; Knorr-Cetina 1999). Returning to the case discussed above, how are the specificities of women's studies made visible? If we take a notion such as "data" as an element of knowledge production that is important in many epistemic cultures, and compare these with a case such as a women's studies, we highlight the "lack" of an element such as "data" in women's studies, or examine the way this notion may or may not fit with this epistemic culture. We might pursue the comparison in the opposite direction (as we did) and show a lack in the epistemic culture of e-science. Whatever the direction of comparison, however, contrasts are constituted by the normative power of categories used for comparison. Comparison is therefore sustained by the proposition that there is an entity called "women's studies" that is comparable to an entity such as "molecular biology" or "high energy physics."

While this step may be backgrounded, the comparative approach only shows meaningful differences once cases are made comparable. Herein lies the unifying, perhaps even universalizing, tendency of case making and comparative approaches. A unit for comparison has to be constituted, and features for comparison have to be specified, if this approach is to yield interesting insights. We are therefore confronted with the middle-range issues of what might count as a proper comparison, of what it means for the case to be made comparable, and of how we might know this.

How then, do we make cases comparable? Space and place are important elements in constituting an ethnographic case, and as such, are a crucial underpinning of the way cases are turned into "entities" for comparison. If the village became the appropriate unit of study for anthropological fieldwork, the same can be said of the laboratory in STS ethnographies. Indeed, while other fields have also adopted ethnographic methods (for example, cultural and media studies), laboratory studies have tended to be "more unanimous on the question of 'ethnographic' locale" (Schlecker and Hirsch 2001, 76).[8]

The alignment of the place of knowledge production and its penetration by the ethnographer has become dominant in STS, with the study by Latour and Woolgar serving as the main exemplar (Latour and Woolgar 1986). Place has also played a role in other ethnographic inquiries in STS, such as user studies (Oudshoorn and Pinch 2003; Suchman 1987). More specifically, it is the face-to-face encounters in a specific space that gives ethnographic studies their edge. Fieldwork is done by following actors around, and these cases highlight the social activities involved in developing uses or knowledge in a particular setting. There is therefore an assumed triad between the space of the lab or context of use, the ethnographic account, and the constitution of a proper object of study for STS.

In our current program, we examine the practices of scholars using new tools in doing research, like Web sites and databases. Because we focus on practices, rather than on technologies, ethnographic inquiry seems appropriate. We find, however, that the notion of place is not obviously transferable to settings that are of interest to us. In other words, how can we use a lab studies approach, when there doesn't seem to be a lab to go to? We are therefore pushing the problem of how to follow actors around, within and through infrastructures (Star 1999). This is different from arguing that e-science is totally spaceless, or that a humanities scholar needs no material setting. But the assumption of much ethnographic work that face-to-face interactions are necessary and sufficient to understand human activity is problematic (Hine, 2007).

In dealing with researchers who interact with digital tools or networked infrastructure, it is difficult to observe and participate in these activities, simply by virtue of physical copresence, since it may mean only witnessing a fairly passive activity (Lynch 1991). Presence in the space of work may need to be supplemented to include interactions with tools (Hine 2005). To be a participant observer in a largish infrastructure that is mediated seems to imply a "scaling up" or diversification of research activities, to encompass the multisitedness and multimedia settings of such ethnography, and to develop an understanding of the wealth of digital traces constituted through interactions with infrastructures.

The orientation of our research program to humanities scholars also challenges the traditional moves of lab studies. The activities of historians or philosophers tend to be much more solitary than those of lab-based researchers, an intimate kind of science. Debate, discussion, or consultation occur at very different points in the research process than they do in the course of lab work, and seem to be more discrete, rather than spread throughout the day or week. If the participant observer can enter a large room full of activity bustling

around lab benches in a reasonably unobtrusive way, inviting oneself into the study of a lone writer during their productive late night/early morning writing hours is a rather distinct proposition. The sociality of humanities scholarship has an altogether different rhythm (a point to which we return below).

In summary, while appreciative of the ethnographic tradition of lab studies, we feel the need to develop new understandings of field sites that do not over-invest in the notion of physical, face-to-face copresence, that can have variable scales, and that can include mediated traces of action as witness to sociality. To include these elements, we need to construct cases differently. This implies a shift in the traditional problematization for which case studies are used in STS.[9]

Shared Case, Different Method

A further point of reflection in our use of cases is their potential role in interdisciplinary inquiry. When some of us began working together a few years ago, the goal was both to find common ground, so as to form a group (rather than a collection of researchers), and to develop innovative approaches in STS. We were also well aware of the difficulties of communicating across different specialties in STS. One of our main efforts at interdisciplinary inquiry developed around the decision to pursue a common case study, in which we intended to observe and model problem solving and learning in research groups. In this project, nicknamed the Competence Project, we aimed to combine mathematical modeling approaches and ethnographic observations in the construction of a computer simulation.[10] The idea was that ethnographic observation could inform the model-building process, and modeling requirements could in turn influence the questions asked in ethnographic observations. We felt we were tackling the "hardest case," since simulations and ethnographic observations are usually posited as extremes in a continuum of social science methodologies. In the course of the project, we came to realize that our respective conceptions of the form and purpose of a case constrained our efforts at being interdisciplinary. We also realized that we had different ways of evaluating and understanding simulations in relation to fieldwork, since some of us distinguished between them at an ontological level, while others considered them to be contrasting epistemic approaches. This meant that the case took on different forms and functions in this project, and that much effort was put into articulating our respective expectations of "cases."

Intermezzo

As these reflections on the limitations of case studies indicate, the middle range is not a space of unification but the tracing of tensions and interrogations in our work. Three problems are present in our use of cases: the basis for comparison, the reliance on space to constitute cases, and cases as mediating objects in interdisciplinary research. While undoubtedly due in part to our own shortcomings, these frictions also point to an emerging problematization, in which the constitution and use of cases shifts. By illustrating our various attempts at using and adapting ethnography, we have tried to show what "must have happened to introduce uncertainty, a loss of familiarity; that loss, that uncertainty, is the result of difficulties in our previous ways of understanding, acting, relating" (Rabinow 2003, 18). In the rest of this article, we expand on the directions we want to explore, fueled by what we see as successful steps in rethinking whether and how to pursue case studies. We therefore revisit the conundrums thrown up by our aspirations for ethnographic case studies, and show how they might be fruitfully alleviated by our suggestions. These are: a focus on time rather than place or space, to constitute cases; the inclusion of researchers and infrastructures in fieldwork; and emphasis on interaction between methodological approaches and new representational forms.

Making the Field through Mediated Interaction

If a lab study relying on physical copresence is a limited strategy, which other approaches might be used to pursue case studies in the humanities or in e-science settings? In turn, if e-science is defined by the implication of large-scale infrastructure, how can the setting of a case study be conceptualized, and what might this mean for the pursuit of ethnographic fieldwork? If scientists are working with distributed infrastructure, it seems that an understanding of these research practices would require taking on multisitedness, and thinking about the field as distributed. Traditionally, ethnography has been associated with the micro level, with the local, and with situated knowledge. Yet, important work has been done using ethnographic sensibilities to show the links between the local and the global (Tsing 2005; Constable 2003), shedding light on the way various scales of human action interact, and recasting what are acceptable ambitions of ethnographic cases. Connective, multisited ethnography has been discussed (Fisher and Marcus 1986) and pursued (see Hine, 2007). These ethnographies move toward

a different problematization, a new version of what can be learned via an ethnographic case.

To apprehend research practices that rely on infrastructure, the mediated character of these practices must be considered as part of fieldwork (Beaulieu 2004). This is all the more important because these elements are often considered to be "delocalizing" or "disembedding," and therefore antithetical to ethnography as a localized, face-to-face approach—an element further reinforced by the focus on a bounded space for the constitution of cases. How can traces of such interactions be constituted as important sources of insight for the ethnographer? The ethnographer seeking to consider and interrogate these traces is confronted with the dominance of other approaches that have embraced the study of such traces. Large-scale network analysis (Huberman 2001; Ingwersen 1998; Albert and Barabasi 2002; Park and Thelwall 2003) claims to account for social relations as expressed in Internet and Web-based networks. To speak to this body of work, an ethnographic approach has to posit how various traces might be made into a meaningful ethnographic object. For example, the hyperlink, assumed to be a trace of sociality, is a key "unit" to be captured and counted in many of these analyses, and is an important part of strategies for formalizing and scaling up case studies (Foot et al. 2003; Schneider and Foot 2005). Rather than see it as a predefined, countable unit that can be aggregated and considered as statistically (in)significant, hyperlinks can be constituted as a potentially meaningful element, a resource for culture (Beaulieu 2005; Beaulieu and Simakova 2006). These are ways of deploying ethnography to understand distributed, hybrid practices in e-science beyond a single space, without relying on formalization.

Making Cases through Temporality and Comparisons

Participant observation, witnessing of interaction in the space of the lab, and the study of social forms in STS ethnographies emphasize space. Some ethnographies, such as Traweek's study of high energy physics (Traweek 1988), have also considered the meanings given to other dimensions, such as time in the space of the laboratory. Traweek considers the reconfiguration of time as fundamental to the notion of laboratory for physicists. Yet, time is subsumed to notions of the laboratory, as organizing space of this culture, and of Traweek's account. The way time is organized in the space of the laboratory therefore provides insight into the knowledge produced, and into the boundaries between ordinary time and the time of scientists.

It may be important to disaggregate these various elements to rethink and redeploy ethnography. The space of the lab can also be known without entering it. It can be known in more functional rather than spatial terms, as the site where particular roles of actors are instantiated (Timmermans 2003; Sims 1999; Shapin and Schaffer 1985). When networks, rather than centers and peripheries, are the organizing principles, notions of flow and pace may be more resonant than those of place or of a micro-macro continuum. We might then consider posing questions about the function of stakeholders in various networks of exchange, rather than about the association of particular epistemologies and national cultures (King 2002, 780). The laboratories of e-science, largely constituted as distributed infrastructures, may be tantamount to an opportunity to question assumptions about ethnographic work in STS, and notions of the space of knowledge.

A problematization that does not focus so much on discovering what is going on in the laboratory can enable an ethnographic practice that constructs a field site by asking when, rather than where, is there something happening, and when is there sociality, creation, or discovery. This approach undoubtedly raises other issues, such as the difficulty in establishing what counts as synchronous action, or the fact that time frames may differ between actors (Hellsten, Leydesdorff, and Wouters 2006). Yet, it offers a departure from queries of much contemporary ethnography about "loss of the field." As to the issue of a comparison engine, it may be that a focus on time, rather than space or structure, is a good starting point. This entails shifting from locale as defining the ethnographic field to what we might term *focale* as the organizing principle. Instead of a boundary in space, we see a condensation in time as the way to know when one is "in the field." This is related to network ethnography (Howard 2002) but implies a more radical move away from space. *Network* is still a spatial metaphor, while *focus* points to sustained attention. Our notion also comes close to the methodology of "issue networks" (Rogers 2002), but is again different, in that this method takes snapshots of frozen hyperlink networks, whereas we want to embrace pace and patterns, rhythms and repetitions.

"Time" is particularly interesting when analyzing e-science, give the dominant expectation that it will speed up knowledge creation. Pels has shown that a core element of scientific and scholarly research is precisely the slowing down of discourse and interaction (Pels 2003), but it may not be the only modulation of time in scientific work. In different projects we have been confronted with the temporal instability of traces left by e-research online. This concerns search engines (Hellsten, Leydesdorff, and Wouters 2006) or Web indicators (Scharnhorst and Wouters, 2006). A focus on the modulation

of time as shaping scientific work would build on analyses where questions of timing and pace of work have been crucial (Lewenstein 1995; Ruhleder 1995).

We therefore call for an emphasis on processes in time to understand interactions between individuals and infrastructure. Whereas lab studies emphasized locality, the space of the lab, for the encounter of humans and things (Latour and Woolgar 1986; Knorr-Cetina 1999), recent work has emphasized the notion of time in these encounters (Gomart 2002), especially where software and networks are involved (Mackenzie 2003). Rather than manipulation and interrogation, other dynamics shape the encounter with objects: metaphors (Wyatt 2004), prospective accounts (Brown and Michael 2003), and distribution (Newman 1998).

Time as organizing principle is not free of assumptions and begs careful examination. If the lab has been characterized as the sacred space, there is a tendency to invoke sacred time, either the time "before" (before the separation of substances and effects; Gomart 2002; Berg and Mol 1998), or a new, layered psyche—"infrastructure is now the great inner space" (Bowker and Star 1999, 319)—or even imaginings (Mackenzie 2003), like a science studies dreamtime.

We suggest that processes, encounters, or flows may be good substantive bases for understanding and comparing e-science or humanities settings. An ethnography that engages with activities in terms of time and that concentrates on focalized regimes of metastability (Mackenzie 2002) may turn general claims about time regimes in science into thick descriptions of actual and potential time regimes, as they unfold in the creation of new research infrastructures. Ethnographic methods have been used to study the relation of space of work and place of knowledge to highlight the contingency of scientific work and the management of uncertainty (Voskuhl 2004; Waterton 2002). With a focus on time, issues of stabilization (another aspect of uncertainty) become interesting in their own right—rather than seen as obstacles to the proper pursuit of research. They might give insight into the meaning of new tools and the importance of certain aspects of infrastructures (search engines, persistence of Web pages; Hellsten, Leydesdorff, and Wouters 2006). We are encouraged in this endeavor by the works of Law and Mol (2003), who have developed the notion of the topology of fluids and fluid spaces as a style and conceptualization of objects, a move that highlights change.

How else can ethnography take the analyst beyond the here and now, beyond the specific locale, to enable multisitedness and comparison? Understanding time in relation to technology and knowledge can give insight into the way scientific work is structured. A focus on the ordering of time by scientists could help illuminate which technological mediations count as

tasks, or rather as part of an environment—a new version of the structure/ agency question. The organization of research could be explored in terms of time spent or time invested. If technology helps smooth over different contributions to a project (through its formalizing and standardizing elements; Thurk and Fine 2003), we can also ask how technologies smooth over the different stages of a project, and how they shift the amount of time spent on different tasks.

Our experiences lead us to the following working assumptions: If e-science is characterized as distributed and networked, then cases will best rely on a type of ethnographic practice that emphasizes knowledge of the network, rather than embedding in a face-to-face situation, and widen what is considered a useful trace, besides paper inscriptions. If areas of knowledge production such as the humanities are to be explored, the constitution of field sites in terms of time cycles and rhythms may be especially useful.

Interdisciplinarity, Interactivity, and New Representations

In this aspect of our work, too, we have benefited from examining the middle range. Middle-range theories (or MRT) have been used to reflect on problems of interdisciplinarity, and the role of theory in understanding a social system. Merton's call for MRT in sociology has served to question the role of system theory as a grand theory (Weick 1974). These researchers explored the possibility for system theory to cease pinning social actions in equilibrium states, constant connections, and rigid frameworks, and for it to embrace unstable objects and coexisting ambiguous states, and seek alternative explanations for "the rest of the variance" (Weick 1974, 366). More recent approaches to formalization in research, such as self-organization theories (Krohn, Küppers, and Nowotny 1990) and complexity theory (Mainzer 1997) have shifted from universal to finer-grained sociologically rooted theorizing (Epstein and Axtell 1996) and their ambitions are critically addressed (Chattoe and Gilbert 2001). These approaches nevertheless imply a great degree of formalization.

In the Competence Project, formalizations of this kind dominated the early stages, so that simulations were elaborated from formal models. Ethnography was seen as a means to deliver, eventually, evidence to support existing models. Modeling work therefore proceeded with the idea that "data" would eventually be considered and that it enriches the project, but that ethnographic practice need not be part of the development of simulations from

the start. This shaped both the role of fieldwork and of simulations in the project, and this "head start" for the modeling work created a difficult position for ethnography. We do not mean that this is an absolute advantage, but in this instance, being "ahead" was shaped by the tradition of connections between simulation building and modeling.[11] We eventually came to acknowledge that the simulation would largely result in an exploration of the modeling work in the project, rather than of the ethnographic fieldwork. Yet, the ethnographic contribution did not evaporate. Rather, it was used to shape the simulation-building process. The repeated discussions sparked by the ethnographer about what it means to model, to simulate, and to do fieldwork resulted in a more explicit and situated simulation practice. We tried to avoid compromise in this later stage, and turned our differences into reflexive practices. Interactions in the course of the project pushed us toward interrogations of what it was that each of us felt unable to contribute, or why we felt unable to accept what others presented as contributions to making our case study. This experience has led us to seek a different conceptualization for interdisciplinary practice, and to explore new forms of representations for ethnographic work. This has in turn pushed us to interrogate assumptions about "closure." We carefully considered the importance of iteration in fieldwork, and the way fears of fixing and freezing the field in ethnographic research shaped our interactions. Our ambitions go beyond obtaining an "acknowledgement" that the soft sciences can contribute to the hard ones, or developing a translation of concepts to establish a baseline. Notions of "sharing views" or "commonly elaborating data" are far too thin to account for the more fruitful interactions in this project.

Having learned from this experience, we propose problematizations that focus on the possibility of dialogue, narrativity, and interaction. We offer these as starting points that may take research in directions other than "triangulation." The geometrical connotations and the possibilities of absolute determination associated with this approach raise the same objections for us as do issues of "zooming in" and the idea of a micro-macro continuum.[12] With "interactivity" or dialogue in mind, we might be able to relate ethnography and modeling as two representations engaged in knowledge making. As approaches that interact and interrogate each other, they are not necessarily complementary views on the same object. Some work in STS has begun to explore this approach. Maynard and Shaeffer (2000) argue for embracing the asymmetry between methods as an analytic stance. They turn ethnomethodological conversation analysis (ECA) loose on survey researchers' work, and argue that ECA has something to contribute to survey research (SR). Yet, they see this relation as strictly unidirectional:

Although survey researchers most certainly could, for example, report numbers, distributions and attitudes of ethnomethodologists and conversation analysts round the work, ECA does not need, and cannot use, SR to improve its knowledge base; SR cannot enter into the science of ethnomethodology. (Maynard and Schaeffer 2000, 355)

We would like to go one step further, and propose that relations to other methodologies can be asymmetric without being unidirectional. Similarly, the main "deliverable" of the Competence Project was a simulation that largely represented modeling activities, while other efforts, such as this article, were more bidirectional.

Interactivity might thus be an important theme to elaborate ethnographies of e-science for our purposes. In the case of the Competence Project, interaction around the construction of the simulation and the ethnography encouraged questioning of assumptions about human behavior and the elaboration of more reflexive formulations in both approaches. The simulation then becomes not only a text for deconstruction, but also a narrative of the interactions between the constituents of the project. We therefore rejoin Campbell's suggestion to develop performativity and a sensibility to narrative and discourse, not only as a way to deal with the issue of what is generalizable between areas of study (Campbell 2004), but also as a reflexive strategy.

Epilogue

What we signal as problematic is of course not entirely new, but by addressing these discomforts with case studies and how they arise, we have shown a need for a shift in the intersection of ethnography and STS inquiry. We may not yet have found a better problematization in relation to the comparative issue—a comparison in time is not so different from a comparison in space. It does still seem interesting to compare different ways of doing scholarly work in terms of flow, pace, and rhythms. At least it gives us a break from the issues that have already been around for a long time, in terms, as Nik Brown noted, of the "directional notion of building-up and scaling down" (Brown 2005, 2). Indeed, the undesirability of a geometrical notion of zooming in and out and encountering the "middle range" along the way was a much-emphasized point in the workshop (Wyatt and Balmer, 2007). A focus on time, while also having connotations of linearity, at least emphasizes narrativity and the contingency of unfolding social realities.

If there is no way out of the tension between universalizing elements and the elaboration of specificity in comparative work, our experiences speak to

a way to recast this debate. Cases are then made comparable not through the use of a formalized framework, but through interaction and conversation. This shift further emphasizes the role of the knowing subject (understood as both the analyst and actor), whose expertise evolves through conversations. It carries the additional implication of greater personal responsibility for knowledge, and emphasizes one's accumulating experiences as source of insight. Such a conclusion is also important for recent debates about interventionist STS (Zuiderent-Jerak 2007) and about third wave STS (Collins and Evans 2002). Paying attention to the importance of a scholar's developing pursuits and interrogation of cases would also especially enrich the latter debate, which tends to see expertise as body of knowledge, rather than practice.

Our experiences with case studies in the field of e-science studies encourage us to address the complexity in roles case studies can play. Our analysis goes beyond an easy allocation of case studies to the "specificity" end of a specific/universal continuum, and shows rather how the specific and universal are conjoined. We have noted and illustrated the way case studies can be seen as generic epistemic devices in their own right, given the need to establish a basis for comparison to make difference visible.

Finally, to return to the question at the beginning of the article: do we need to keep making this point about the diversity of science? In the face of the strength of the discourse about the modernization of science via e-science tools, where these ambitions about universal and generic tools for science are given (yet again) new life, then yes, the point still needs to be made. But we could also imagine working toward a situation in which every initiative about e-science begins with recognition of diversity and specificity. Infrastructures produced would therefore be "specific" or "situated" rather than "generic." Common elements, if present, would arise from practices—emergent, but not presupposed. Given an inversion of the current situation, contributions from ethnographic cases to the building of infrastructures would be in a privileged position—certainly one more powerful than the current call on ethnographers to help build good user interfaces for e-science tools.

Which other possibilities exist to meet our ambitions, to deal with multi-sitedness and distributed infrastructure, and to enable comparison? New forms, like simulations, may enable new kinds of communication. Traditional forms, such as narrative, may also be useful for making STS arguments (Turner 2001). Interactivity, as both a mode of presence and dialogue, may be another developing trope, one associated with accountability and intersubjectivity, while also being in line with narratives told by actors (Jensen and Markussen 2001) and with internet sociality (Mortensen and Walker 2002). Interactivity also connotes a developing relationship over time, further reinforcing our

hunch about its importance. Whether ethnography can be uncoupled from the tradition of laboratory studies and reinvented for the investigation of other forms seems to us a tantalizing, if risky, prospect. Indeed, if STS, as a field, is able to foster such critical projects around ethnography, its relevance to more general sociological work may increase (Wyatt and Balmer, 2007; Yearley 2005).

Thus far, our explorations and reflections are on the issue of middle-range theory. Rather than take on the body of work that addresses middle-range theory as a starting point, we have preferred to specify our "problem" in terms of our particular local aspirations. The "middle range" is therefore, for our purposes, the need to make connections across cases and across disciplines, and the possibility of doing this without appealing to fixed relations or essential qualities. Middle ranging, as we have addressed it here, becomes a practice of reflexive interrogation that is eminently practical.

Notes

1. See van Oost (2003), Latour (1988), Bijker (1995), Galison (1997), and Franklin, Lury, and Stacey (2000).

2. This trope was also invoked by Sally Wyatt, in a preparatory discussion to the workshop. See note 3.

3. This article is based on a paper prepared for *Middle Range Theories in Science and Technology Studies*, ESRC and ASCoR workshop, April 27-29, 2005, Amsterdam. The authors are grateful to Helen Kennedy, who acted as respondent and made us aware of the centrality of specificity as a motif in our argument; to the organizers and participants of the workshop; and to colleagues at the VKS for invaluable input and support. Sarah de Rijcke, Kalpana Shankar, Maarten Derksen, and Jessica Mesman generously provided feedback on an earlier draft.

4. With thanks to Nik Brown for verbing *middle range* at the workshop.

5. See Fisher and Marcus (1986) for a critique of "comparative cultures" as an approach to cultural diversity and attendant holism in anthropology.

6. These arguments have important political dimensions in STS because they deconstruct positivist claims and technological determinism (see Wyatt 2007).

7. A five-year program, funded by the Royal Netherlands Academy of Arts and Sciences (KNAW). See http://www.virtualknowledgestudio.nl/program/

8. Though see Hine (2007) and papers presented at the workshop on virtual ethnography, VKS, Amsterdam, September 27-29, 2006. http://www.virtualknowledgestudio.nl/conferences/virtual-ethnography/

9. The argument so far could be read as a plea to change our theories and tools because they are no longer functional in the world—this dysfunction being due to new forms of science (e-science) or objects that are new to STS (humanities). This seemingly realist line of argumentation is only one part of the work we are trying to do in this article, as we attempt to show the conditions under which our problematization shifts.

10. See http://www.virtualknowledgestudio.nl/projects/research-networks.php

11. Ethnography has similar advantages in relation to other representational forms, such as narratives. These were also experienced in the collaboration, when "modelers" attempted to write about the research process.

12. One possibility is to maintain the metaphor, but extend its connotation to that of the love triangle, where each member interrogates the other on its supposed attractiveness (what does he have that I don't?). While love triangles may not last, they are nevertheless moments of intense engagement.

References

Albert, R., and A. L. Barabasi. 2002. Statistical mechanics of complex networks. *Reviews of Modern Physics* 74 (1): 47-97.

Ankeny, R. 2007. Model organisms as case-based reasoning: Worms in contemporary biomedical science. In *Science without laws: Model systems, cases, and exemplary narratives*, ed. A. N. H. Creager, E. Lunbeck, and M. N. Wise. Durham, NC: Duke University Press.

Beaulieu, A. 2004. Mediating ethnography: Objectivity and the making of ethnographies of the Internet. *Social Epistemology* 18 (2/3): 139-64.

———. 2005. Sociable hyperlinks: An ethnographic approach to connectivity. In *Virtual methods: Issues in social research on the Internet*, ed. C. Hine, 183-98. Oxford: Berg Publishers.

Beaulieu, A., and E. Simakova. 2006. Textured connectivity: An ethnographic approach to understanding the timescape of hyperlinks. *Cybermetrics* 10 (1). Available from http://www.cindoc.csic.es/cybermetrics/vol10iss1.html (accessed December 1, 2006).

Berg, M., and A. Mol, eds. 1998. *Differences in medicine: Unraveling practices, techniques, and bodies*. Durham, NC: Duke University Press.

Bijker, W. E. 1995. *Of bicycles, bakelites, and bulbs: Toward a theory of sociotechnical change*. Cambridge, MA: MIT Press.

Bos, J. 2004. Everyday life objectivity. *Social Epistemology* 18 (2/3): 123-38.

Bowker, G., and S. L. Star. 1999. *Sorting things out: Classification and its consequences*. Cambridge, MA: MIT Press.

Brown, N. 2005. Home on the (mid-) range—Some notes on theory in STS. Paper presented at the workshop Middle Range Theories in Science and Technology Studies, Amsterdam, April 27-29.

Brown, N., and M. Michael. 2003. A sociology of expectations: Retrospecting prospects and prospecting retrospects. *Technology Analysis & Strategic Management* 15 (1): 3-18.

Campbell, N. D. 2004. Credible performances: The performativity of science studies. *Social Studies of Science* 34 (3): 433-42.

Chattoe, E., and N. Gilbert. 2001. Understanding consumption: What interviews with retired households can reveal about budgetary decisions. *Sociological Research Online* 6 (3). Available from http://www.socresonline.org.uk/6/3/chattoe.html (accessed December 1, 2006).

Collins, H. M., and R. Evans. 2002. The third wave of science studies: Studies of expertise and experience. *Social Studies of Science* 32 (2): 235-96.

Constable, N. 2003. *Romance on a global stage: Pen pals, virtual ethnography, and "mail-order" marriages*. Berkeley, CA: University of California Press.

De Laet, M. K., and A. Mol. 2000. The Zimbabwe bush pump: Mechanics of a fluid technology. *Social Studies of Science* 30 (2): 225-63.

Dumit, J. 2004. *Picturing personhood: Brain scans and biomedical identity*. Princeton, NJ: Princeton University Press.

Epstein, J. M., and R. Axtell. 1996. *Growing artificial societies: Social science from bottom up*. Cambridge, MA: MIT Press.

Fisher, M. M. J., and G. E. Marcus. 1986. *Anthropology as cultural critique: An experimental moment in the human sciences*. Chicago: University of Chicago Press.

Foot, K. A., S. M. Schneider, M. Dougherty, M. Xenos, and E. Larsen. 2003. Analyzing linking practices: Candidate sites in the 2002 U.S. electoral Web sphere. *Journal of Computer-Mediated Communication* 8 (4). Available from http://www.jcmc.indiana.edu/vol8/issue4/foot.html (accessed December 1, 2006).

Franklin, S., C. Lury, and J. Stacey. 2000. *Global nature, global culture*. New York: Sage.

Galison, P. 1997. *Image and logic: A material culture of microphysics*. Chicago, IL: University of Chicago Press.

Galison, P., and D. J. Stump, eds. 1996. *The disunity of science: Boundaries, contexts, and power*. Palo Alto, CA: Stanford University Press.

Geels, F. 2007. Feelings of discontent and the promise of middle range theory for STS: Examples from technology dynamics. *Science, Technology, & Human Values* 32(6).

Gomart, E. 2002. Methadone: Six effects in search of a substance. *Social Studies of Science* 32 (1): 93-135.

Hagendijk, R. and J. Meeus. 1993. Blind faith: Fact, fiction and fraud in public controversy over science. *Public Understanding of Science* 2(4): 391-415.

Heintz, B., M. Merz, and C. Schumacher. 2004. *Wissenschaft, die Grenzen schafft*. Bielefeld: transcript Verlag.

Hellsten, I., L. Leydesdorff, and P. Wouters. 2006. Multiple presents: How search engines re-write the past. *New Media & Society* 8 (6): 901-24.

Hine, C. 2007. Multi-sited ethnography as a middle range methodology for contemporary STS. *Science, Technology, & Human Values* 32(6).

Hine, C., ed. 2005. *Virtual methods: Issues in social research on the Internet*: Oxford: Berg Publishers.

Howard, P. 2002. Network ethnography and the hypermedia organization: New organizations, new media, new methods. *New Media & Society* 4 (4): 551-75.

Huberman, B. A. 2001. *The laws of the Web: Patterns in the ecology of information*. Cambridge, MA: MIT Press.

Ingwersen, P. 1998. The calculation of Web impact factors. *Journal of Documentation* 54 (2): 236-43.

Jensen, C., and R. Markussen. 2001. Marup church and the politics of hybridization: On complexities of choice. *Social Studies of Science* 31 (6): 795-819.

King, N. 2002. Security, disease, commerce: Ideologies of postcolonial global health. *Social Studies of Science* 32 (5/6): 763-89.

Knorr-Cetina, K. 1981. *The manufacture of knowledge: An essay on the constructivist and contextual nature of science*. Oxford: Pergamon.

———. 1999. *Epistemic cultures: How the sciences make knowledge*. Cambridge, MA: Harvard University Press.

Krohn, W., G. Küppers, and H. Nowotny. 1990. *Selforganization: Portrait of a scientific revolution*. Dordrecht: Kluwer Academic Publisher.

Latour, B. 1988. *The pasteurization of France*. Cambridge, MA: Harvard University Press.

Latour, B., and S. Woolgar. 1986. *Laboratory life: The construction of scientific facts*. 2nd ed. Princeton, NJ: Princeton University Press.

Law, J, and A. Mol. 2003. Situating technoscience: An inquiry into spatialities. Centre for Science Studies, Lancaster University, Lancaster. Available from http://www.lancs.ac.uk/fss/sociology/research/resalph.htm#law (accessed December 1, 2006).

Lewenstein, B. V. 1995. Do public electronic bulletin boards help create scientific knowledge? The cold-fusion case. *Science, Technology, & Human Values* 20 (2): 123-49.

Lynch, M. 1991. Laboratory space and the technological complex: An investigation of topical contextures. *Science in Context* 4(1): 51-78.

Mackenzie, A. 2002. *Transductions: Bodies and machines at speed*. New York: Continuum Press.

———. 2003. These things called systems: Collective imaginings and infrastructural software. *Social Studies of Science* 33 (3): 365-87.

Mainzer, K. 1997. *Thinking in complexity*. 3rd ed. Berlin: Springer.

Martin, B. 1988. Analyzing the fluoridation controversy: Resources and structures. *Social Studies of Science* 18 (2): 331-63.

Maynard, D. W, and N. C. Schaeffer. 2000. Towards a sociology of social scientific knowledge: Survey research and ethnomethodology's asymmetric alternates. *Social Studies of Science* 30 (3): 323-70.

Mialet, H. 1999. Do angels have bodies? Two stories about subjectivity in science: The cases of William X and Mister H. *Social Studies of Science* 29 (4): 551-81.

Morgan, M. 2006. When "facts" travel free? Paper presented at the annual meeting of the Society for Social Studies of Science, Vancouver, Canada, November 1-5.

Mortensen, T., and J. Walker. 2002. Blogging thoughts: Personal publication as an online research tool. In *Researching ICTs in context*. Available from http://www.intermedia.uio .no/konferanser/skikt-02/docs/Researching_ICTs_in_context-Ch11-Mortensen-Walker.pdf (accessed December 1, 2006).

Newman, S. 1998. Here, there and nowhere at all: Distribution, negotiation and virtuality in postmodern ethnography and engineering. *Knowledge and Society* 11:235-67.

Oudshoorn, N., and T. Pinch, eds. 2003. *How users matter: The co-construction of users and technology*. Cambridge, MA: MIT Press.

Park, H., and M. Thelwall. 2003. Hyperlink analyses of the World Wide Web: A review. *Journal of Computer Mediated Communication* 8 (4). Available from http://www.jcmc.indiana.edu/ vol8/issue4/park.html (accessed December 1, 2006).

Pels, D. 2003. *Unhastening science: Autonomy and reflexivity in the social theory of knowledge*. Liverpool, UK: Liverpool University Press.

Rabinow, P. 2003. *Anthropos today: Reflections on modern equipment*. Princeton, NJ: Princeton University Press.

Rogers, R. 2002. Operating issue networks. *Science as Culture* 11 (2): 191-213.

Ruhleder, K. 1995. Reconstructing artifacts, reconstructing work: From textual edition to online data-bank. *Science, Technology, & Human Values* 20 (1): 39-64.

Scharnhorst, A., and P. Wouters. 2006. Web indications: A new generation of S&T indicators? *Cybermetrics* 10 (1). Available from http://www.cindoc.csic.es/cybermetrics/vol10iss1 .html (accessed December 1, 2006).

Schlecker, M., and E. Hirsch. 2001. Incomplete knowledge: Ethnography and the crisis of context in studies of media, science and technology. *History of the Human Sciences* 14 (1): 69-87.

Schneider, S., and K. Foot. 2005. Web sphere analysis: An approach to studying online action. In *Virtual methods: Issues in social research on the Internet*, ed. C. Hine, 157-170. Oxford: Berg Publishers.

Shapin, S., and S. Schaffer. 1985. *Leviathan and the air pump: Hobbes, Boyle and the experimental life*. Princeton, NJ: Princeton University Press.

Sims, B. 1999. Concrete practices: Testing in an earthquake-engineering laboratory. *Social Studies of Science* 29 (4): 483-518.

Star, S. L. 1999. The ethnography of infrastructure. *American Behavioral Scientist* 43 (3): 377-91.

Suchman, L. A. 1987. *Plans and situated actions: The problem of human machine interaction*. Cambridge: Cambridge University Press.

Thurk, J., and G. A. Fine. 2003. The problem of tools: Technology and the sharing of knowledge. *Acta Sociologica* 46 (2): 107-17.

Timmermans, S. 2003. A black technician and blue babies. *Social Studies of Science* 33 (2): 197-229.

Traweek, S. 1988. *Beamtimes and lifetimes: The world of high energy physics*. London: Harvard University Press.

Tsing, A. L. 2005. *Friction: An ethnography of global connection*. Princeton, NJ: Princeton University Press.

Turner, R. S. 2001. On telling regulatory tales: rBST comes to Canada. *Social Studies of Science* 31 (4): 475-506.

Van Oost, E. 2003. Materialized gender: How shavers configure the users' femininity and masculinity. In *How users matter: The co-construction of users and technology*, ed. N. Oudshoorn and T. Pinch, 193-208. Cambridge, MA: MIT Press.

Voskuhl, A. 2004. Humans, machines, and conversations: An ethnographic study of the making of automatic speech recognition technologies. *Social Studies of Science* 34 (3): 393-421.

Waterton, C. 2002. From field to fantasy: Classifying nature, constructing Europe. *Social Studies of Science* 32 (2): 177-204.

Weick, K. E. 1974. Middle-range theories of social systems. *Behavioral Science* 19 (6): 357-67.

Wouters, P., and A. Beaulieu. 2006. Imagining e-science beyond computation. In *New infrastructures for knowledge production: Understanding e-science*, ed. C. Hine, 48-70. Hershey, PA: Idea Group.

Wyatt, S., and B. Balmer. 2007. Home on the range: What and where is the middle in science and technology studies? *Science, Technology, & Human Values* 32(6).

Wyatt, S. 2004. Danger! Metaphors at work in economics, geophysiology, and the Internet. *Science, Technology, & Human Values* 29 (2): 242-61.

———. 2007. Technological determinism is dead; long live technological determinism. In *New handbook of science, technology and society*, ed. E. Hackett, O. Amsterdamska, M. Lynch, and J. Wajcman, 165-80. Cambridge, MA: MIT Press.

Yearley, S. 2005. *Making sense of science: Understanding the social study of science*. London: Sage.

Zuiderent-Jerak, T. 2007. Intervening in health care. PhD diss., Institute of Health Policy and Management, Erasmus University Rotterdam, The Netherlands.

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