

# From co-location to co-presence: Shifts in the use of ethnography for the study of knowledge

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## Abstract

Ethnography has been successfully deployed in science and technology studies, and more specifically in laboratory studies. By using co-presence rather than co-location as a starting point to conceptualize and articulate fieldwork, new aspects of knowledge production are foregrounded in ethnographic studies. This research note proposes and discusses co-presence as an epistemic strategy that pays close attention to non-lab based knowledge production that can embrace textuality, infrastructure and mediation, and that draws into relief the role of ethnographer as author, participant-observer and scholar. Furthermore, co-presence as an approach to doing fieldwork generates new prospects for the study of knowledge production. It enables STS to develop the ethnographic study of highly mediated, distributed or non-lab-based fields, such as the humanities, e-research and e-science.

## Keywords

database, ethnography, e-research, e-science, infrastructure, laboratory studies, methods, networks

Arrival stories in ethnographic accounts typically tell of the great difficulties and small successes of early stages of fieldwork, and I also begin this paper in that way. As a PhD student some 10 years ago, I spent 6 months doing fieldwork in two laboratories, with a pause of a few months in between, spent at my home department. Armed with what I considered to be a significant experience, I embarked on my second period of fieldwork. On my first day, I deployed what had been a hugely successful strategy to ‘get things going’ in my fieldwork in lab number one. ‘Could you give me a tour of the lab?’, I asked the lab director who was welcoming me. ‘Well, if you really want to find out what we’re

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doing, the best thing for you to do is to look at what we've got on the website', was the answer – an answer that sent me red-faced back to my cubicle, to type up notes of this exchange that focused on the difficulty of 'being taken seriously' by these bio-medical-physics types. This 'brush-off' served to steel my determination to get something out of these people. I had already grasped, it seems, that iteration and recovery from mistakes is 90% of fieldwork. But looking back on this episode, I now have a very different reading of my 'success' with this strategy in the first lab and my 'failure' in the second. I also think that, were this to happen today, I would respond very differently to this invitation to look at the website, and would not think of it as a dismissal.

Youthful arrogance probably had a part to play in this episode. I hadn't come all the way to North America to look at websites – which I'd already done from home anyway. But arguably, what also underlies the humiliation and frustration I felt when being told to look at the website are ideas about the proper way to do fieldwork in STS that I had internalized. This story illustrates how assumptions about what counts as proper ethnographic observation shape one's field. Because I had set out to do participant-observation in a particular place, I tended to reject this invitation as not being very relevant to what was worth observing, and therefore out of line with my goals to conduct an ethnographic lab study.

In this paper, I put forward the concept of co-presence as a way to shape fieldwork.<sup>1</sup> It is a preferable formulation to others, such as 'going into the field', which implies (the desirability of) physical co-location as a requisite for ethnographic investigation. A focus on achieving co-presence also emphasizes that interaction is a potentially rewarding but precarious achievement (Goffman, 1957), and that physical presence is not equivalent to availability for interaction (Goffman, 1971). While physical co-location can be a resource for participants, it is not in itself a sufficient criterion for co-presence. Co-presence decentralizes the notion of space without excluding it. It opens up the possibility that co-presence might be established through a variety of modes, physical co-location being one among others. Not only does it enable the researcher to take mediated settings very seriously (insofar as they are a means or resource for being co-present), but it also does not exclude face-to-face situations. Co-presence as a starting point enables a more symmetrical treatment of forms of interaction.

Co-presence can therefore be used to construct a field site and enables STS researchers to consider new aspects of knowledge production that may not be strongly tied to a physically defined space such as a lab. Rather than a report on a specific piece of ethnographic research, what follows is therefore a reflection on ethnographic methods that conjoins what is worth asking, how one might go about seeking answers, and what might count as an answer. This discussion focuses on the relation between the creation of the 'field' as the object of ethnography and the insights that can be obtained from doing 'fieldwork'. I address these questions here in terms of co-presence and show how particular problematizations (Rabinow, 2003) in STS can be tied to specific ways of doing ethnographic research.

Ethnographic approaches in STS have been used to explore a wide-range of questions about knowledge production.<sup>2</sup> Ethnographies have addressed 'knowledge-in-the-making', and researchers have focused on observing manipulations of the empirical, the hard core of science, in order to show that perfectly ordinary social and cultural processes such as negotiation, competition, trust, symbolic activity or accommodation were also taking

place in the laboratory (Fujimura and Fortun, 1996; Lynch, 1985; Traweek, 1988). Significant concepts in STS, such as the construction of facts, the role of inscriptions, and what counts as a contribution to scientific work and knowledge have been addressed and refined through ethnographic investigations (Latour and Woolgar, 1986 [1979]; Knorr, 1981, Knorr Cetina, 1999; Lynch, 1985).

In particular, lab studies have been hailed as one of the major contributions to constructivism because of the way they demonstrate how the universal claims of science are localized (Knorr Cetina, 1995). Ethnographic studies remain important for establishing some of the perennial claims of STS, namely the ontological and epistemological diversity of science and of the meaning of technology (Beaulieu et al., 2007; Oudshoorn and Pinch, 2003) and the persistence of variations in practices (Sommerlund, 2006; Voskuhl, 2004).

Recently, ethnographic studies have also been called for in order to study the constitution of facts as a not-yet-fully-tackled issue in STS (Doing, 2007); to develop a better understanding of what counts as labour and of how this labour is valued as knowledge production becomes 'informationalized' (Virtual Knowledge Studio for the Humanities and Social Sciences [VKS], 2007); as a way to understand the constitution of new objects of knowledge (Jensen, 2006; Knorr Cetina and Bruegger, 2000; Woolgar and Coopmans, 2006); as a way to develop better interventions from STS (Hess, 2001); and to counter new universalizing claims around ICT from policy-makers and funding agencies by developing new accounts of practice (Hine, 2006a; VKS, 2007).

A powerful way to argue the non-universality of scientific claims, and to study science as a practical accomplishment, is therefore to show how knowledge production is embedded in local environments (Merz, 2006). As Knorr Cetina (1995: 151) puts it: 'If construction is wrapped up in bounded locales, the ethnographer needs to "penetrate the spaces" and the stream of practices from which fact construction arises.' The place of knowledge production (and lab studies in particular) has therefore been important in developing ethnographic contributions for STS.

Labs undoubtedly are very interesting kinds of places, shaped and marked by specific kinds of institutional routines and demarcated from other spaces in order to function (Derksen, 1997; Henke and Gieryn, 2007; Timmermans, 2003). Labs are also changing entities, and as Hine notes, in some disciplines, the use of particular kinds of tools such as databases, may affect the spatial organization of labs so that:

[i]n Lynch's (1991) terms, the laboratory remains a place where different topical contextures are enacted, but it is cross-cut by the topical contexture of the database. ... The database and the laboratory can therefore co-exist as different frameworks for organizing action, without one necessarily threatening the other. (Hine, 2006b: 292–293)

Other work has also considered the challenges of studying more distributed settings of knowledge production (Star, 1999), or considered the need for a mobile and connective ethnography (Dirksen, 2007; Fay, 2007; Hine, 2007; Wakeford, 2000), which would connect nodes in networks or (a subset of) the varieties of spaces where knowledge is produced.

The evocation of the space of knowledge or an appeal for the diversification of spaces to consider for studying science ethnographically are recurrent elements across these

calls for ethnographic research and elaborations on ethnography of knowledge production.<sup>3</sup> As such, the articulation of fieldwork in terms of spaces links particular topologies and social theory: regions, networks and flows are figures that shape how ethnographic knowledge is conceptualized, which boundaries are drawn and what kinds of engagement are foregrounded (de Laet and Mol, 2000; Mol and Law, 1994). The present text makes a similar argument in putting forth co-presence as a focus of fieldwork that elaborates upon ‘the stream of practices’ and that engages with new forms of knowledge production. The conceptualization, elaboration and configuration of the field through the figure of ‘co-presence’ are presented and analyzed in terms of their consequences for doing fieldwork and for what can be learned from it.

### Co-location/co-presence

As STS scholars increasingly study forms of knowledge production where the space of the lab (or similar locale) is much less central, other ways of conceptualizing the field may be especially useful for ethnographic research. In particular, ethnographic approaches must loosen their grip on co-location as a necessary requirement for ‘being in the field’ if they are to consider important issues about knowledge production that arise in fields, such as those in the humanities or e-research. Key STS topics, like new forms of authoritative knowledge, the changing shape of scientific work, and dynamics of innovation can be explored through ethnography. But in order to do so, the ethnographic approach must adapt in order to study these fields in which research practices are not concentrated in lab-like spaces.

There are significant differences between the kinds of spaces used for natural or life science and for humanities research. These differences, as well as the very significance of space, hold implications for what makes up the *field* for ethnographic research. Entering a bustling lab is a very different proposition from installing oneself in the study of a lone scholar, as I experienced in a recent ethnographic project on a group of women’s studies scholars. In this humanities setting what happens is ‘intimate science’, a term I used to denote these more solitary practices, which are not structured around a public space for manipulation of empirical phenomena, as is the case in scientific labs.<sup>4</sup> This is different from arguing that women’s studies scholars lack physical presence, that they have no need of particular kinds of spaces, and that they are not attached to these. It is also far from the case that such scholarship is strictly an individual matter, because social interaction is no less crucial for rendering ‘public’ philosophical, historical or literary knowledge, than it is for a laboratory science. But where should one look for the *field*? How can one develop an ethnography that provides insight into the material and symbolic resources needed, the skills of researchers, and the mechanisms that validate research practices in a field like women’s studies? To look for a space of knowledge was not a promising avenue. Asking ‘Where is the field?’ led to largely metaphorical answers, which were not very satisfying to the fieldworker. How can these forms of research be investigated ethnographically?

A further challenge for STS ethnographers is to follow ‘streams of practices’ that are mediated by information and communication technology in order to study forms of knowledge production that fall under the labels of cyberinfrastructure, e-research or

e-science (Hine, 2006a, 2008; Olson et al., 2008). Indeed ethnography, especially in the cultural anthropology tradition, has had a difficult relationship to mediation, which is often experienced as a challenge to its core epistemic commitments to witness face-to-face (often oral) communications and interactions. Ethnography has thus been involved in border disputes with other disciplines that focus on ‘texts’, such as cultural studies or history (Amit, 2000; Gupta and Ferguson, 1997). The move from studying texts to observing interactions by examining laboratory practices and interactions has been especially valuable for STS. Going beyond published accounts and observing working scientists proved to be a successful strategy in the face of previous studies that treated formal representations (scientific papers) as adequate accounts of science (Knorr Cetina, 1995). At the same time, lab studies were quick to point to the wealth of other texts and traces (‘inscriptions’) that preceded and exceeded accounts written up in scientific paper (Latour and Woolgar, 1986 [1979]). Inscriptions are even more ubiquitous in e-research settings. E-research increasingly relies on the use of inscriptions not only as interactional resources, but also as modes of mediation. Such inscriptions assume highly malleable digital form, making it even more crucial to develop sophisticated understandings of their production, circulation and remediation (Bolter and Grusin, 2000). These streams of practices therefore exceed what can be witnessed in face-to-face interaction or in the space of a lab – itself increasingly problematized and denaturalized as a unit (Gieryn, 2006; Vinck and Zarma, 2007). The issue for ethnographers is therefore not to go beyond the texts and traces, nor simply to take notice of their presence, but to find a way to fully embrace them as part of the field. Because it does not assume the centrality of shared space, the notion of co-presence can be useful in ethnographies of e-research, as well as of other fields in the sciences and humanities that involve highly mediated forms of research or where the lab does not figure so prominently.

## Co-presence and conceptualization of the field

The challenge, as I have set it up so far, is to draw on the ethnographic tradition of STS, while developing new understandings of field sites that involve distributed work and mediated action, and do not depend on the centrality of laboratories and the notion of physical, face-to-face interaction. I propose co-presence as a motif or figure for the ethnographic object. Simply put, ‘establishing co-presence’ is a distinct epistemic strategy that leads the ethnographer to ask, ‘How can I establish co-presence?’ rather than ‘Where do I go?’ This difference has implications (and, I argue, significant advantages), which are detailed in the rest of this paper.

Conceptually, co-presence foregrounds the relationship between self and other and interaction that *achieves* presence in a setting. Co-presence is an interactive accomplishment by participants and ethnographers alike, and it does not share the unidirectional and oculo-centric connotations of witnessing (Woolgar and Coopmans, 2006). Most importantly, doing ethnographic research by focusing on co-presence highlights the centrality of shared meaning achieved in and through interaction. While giving a complete account of shared meaning is not an attainable goal, even a partial failure to encompass it can be productive of further insights (Metcalf, 2002).

When an ethnographer raises the question of ‘how to be co-present’, many elements besides space can be important. Instead of taking shared space as a primary or necessary condition for ‘being in the field’,<sup>5</sup> she can foreground social interaction as constitutive of co-presence. As an illustration, imagine a telephone conversation: when engaging in a phone call, we can determine when we are co-present with our intended recipient, and it is possible to analyse the texture and quality of how this co-presence is achieved and sustained. Neither face-to-face contact nor shared physical space is necessary. We rely on interactional routines, such as answering, turn taking, interruptions, and so on, to establish and sustain the situation. Some of these routines are conventional, and others are idiosyncratic, and some may be infrastructural (for example, electronic signals that mark particular connections). All help constitute the specific outlines of the field. Presence mediated via the telephone can also take on different temporal forms – leaving messages on a machine, ‘telephone tag’ – and indeed synchronous or time-shifted interactions can be associated with different forms of presence.

A mediated relation to informants is sometimes treated as an obstacle to ethnographic rapport that is supposedly most reliably achieved through face-to-face research. A broader conception of co-presence enables the ethnographer to examine mediation as a feature of social relations rather than a barrier to them. According to this conception of co-presence, mediation is not a shortcoming of networked sociality, as some fear (Urry, 2003). Instead, specific modes of mediation can be crucial for meaningful practices in which to engage as participant-observers (Boellstorff, 2008; Newman, 1998; Riles, 2001; Taylor, 1999). Electronic networks and infrastructures, for example, are both topics and resources for fieldwork (Hine, 2006b; Mackenzie, 2003). When interaction is a condition for co-presence, it does not necessarily take the form of physical proximity. ‘Where to go’ is but one question among many, rather than an absolute condition for fieldwork. Travelling to the field becomes one possible strategy, one resource or means for achieving co-presence, rather than an absolute condition to be fulfilled in order for research to count as ‘ethnographic’. It also problematizes imperatives such as: ‘You can’t possibly go to all the places where the participants are!’ or ‘You need to meet your respondent!’

Even when treating interaction as a key element, one must consider all the components that sustain it. To return to the example of the telephone call, technological elements such as automated responses, transmission mode, receiving apparatus, platform, quality of the connection and various other signals also become part of the ethnographic object, because they are also crucial for establishing co-presence. In other words, if infrastructure is necessary to establish the possibility of co-presence, it is not something extraneous to the field. It can be fully embraced.<sup>6</sup> Co-presence can also be achieved through a combination of mediated and face-to-face interaction. A common sight at a conference, music festival or other large event is to see someone talking on their mobile phone, describing what they’re watching or doing, and while the conversation continues they are joined by their interlocutor – at which point the mobile phone connection is turned off and the conversation continues face-to-face. There is no breach or disjuncture of co-presence for the interlocutors in this interaction, though the modalities of co-presence are different. Co-presence therefore draws attention to the performative aspect of doing ethnographic fieldwork. Space, texts and infrastructures become so many resources in establishing co-presence that can be embraced as constitutive of the field.

## Epistemic authority, reflexivity and ethics

Constituting the ethnographic field, pursuing fieldwork and writing are intertwined activities that make up the epistemic power of the ethnographer. Re-articulating 'the field' simultaneously changes the ethnographer's role, since the relation to space has traditionally been crucial for defining her authority and identity. Focusing on co-presence also can make other demands on the ethnographer, shifting her role as traveller or author. Co-presence involves not so much the ability to travel but rather emphasizes coordination, flexibility and availability. For example, during my fieldwork in women's studies, lectures and other events were so important for constituting my field that it was more important to determine 'when' the field would be available than 'where' it could be found. Rather than organizing travel, the challenge was to organize availability, as the rhythms of this group had to be coordinated with other schedules and commitments. Thus, the friction between kinds of time were more telling of the particularities of the field, than were the contrasts between the spaces of my academic 'home' and those of women's studies. The logistics of absence/presence were productive of insight, above and beyond travelling to enter a space.

Given that travel is less salient for access, and that access (for example, to a website) may be available for others, 'being there' is no longer such a strong claim for the ethnographer. In anthropology, Tyler (1986) and many others have critiqued the epistemic authority that is based on having gone into an exotic field, and proposed as an alternative that the ethnographer's authority should be based in authorship. In settings studied in STS, ethnographers are far from the only authors or authorities (about science) on the scene. This is all the more so when the field is highly mediated and generates many traces of interaction, the availability of which predates the ethnographer's fieldwork. Some scholars (in ethnomethodology and especially in internet studies) see this as a radical break in the epistemic strategies of ethnography, and have chosen to take as a starting point that 'the ethnography' already exists in the shape of these traces (Schaap, 2001). When much of this mediation produces textual forms, the ethnographer's writing (and any putative value of this activity) cannot simply be defined in contrast to the 'orality' of the field.

These conditions of work are increasingly common, for both ethnographers in STS and for the multiplying numbers of researchers working as in-house ethnographers at corporations such as Microsoft or Google, in the tradition of Xerox PARC. How have these been dealt with? Some ethnographers may choose to build their claims to authorship by seeking new (hypermedia) forms for 'writing culture' that go beyond the mediation that already exists in the field. Accordingly, ethnographers try to differentiate their roles from those of participants in terms of (kinds of) writing or of the manipulation of digital inscriptions. One strategy has been to produce sophisticated visualizations or meta-analyses based on existing traces. This strategy characterizes existing writing as 'empirical material', above which the ethnographer rises (Beaulieu, 2004; Gajjala, 2002). Another strategy consists in investing in kinds of writing, such as paper monographs (Hine, 2008; Miller and Slater, 2000; Taylor, 2006), which creates distance between the writing and inscriptions in the field and the setting in which the ethnographer is making knowledge claims. This distance is sometimes acknowledged as a concession to academic reward systems (Bruns, 2008) or even celebrated as the integration of the 'virtual' into the prosaic academic realm (Boelstorff, 2008).

An alternative response to these conditions is to base authority on situatedness (Haraway, 1988) or on heuristic explications of relations (Strathern, 2004). On a methodological level, this means paying close attention to how we value the writing that already exists in the field, and to how our mediation affects the material gathered and the value of the ethnographer's contribution.

In the course of my fieldwork on women's studies, a mailing list became an important part of my field. My postings on the mailing list were a way to achieve co-presence, both as a fieldworker and as an author. I used the list not only as a means of ethnographic investigation, but also as a way of signalling and linking my own research and writing practices to others on the list. For example, when debates erupted about the boundaries or purpose of the list, the posts I contributed detailed how my observations led me to make particular statements. I also posted to the list about my upcoming presentations at conferences, about publications, and about the way interactions on and around the list figured in these activities. In turn, I used reactions from the list to these posts in my presentations, to further situate the ethnographic insights I was presenting to colleagues at conferences. Throughout, I also considered how certain exchanges were pursued via the list, and others via emails directed to me personally or to a small subgroup.

Another important element in situating my activities and explicating these relations was the continuity of identity I performed throughout the research. By consistently using the same name across these settings, this diversity of writing activities was traceable by colleagues and participants alike. I further used my personal professional homepage, easily found by typing my not-too-common name in Google, as an interface that provided links to my writing, to other research activities and to my institutional affiliation. In short, I sought to configure myself as a consistent, traceable ethnographer and author across these various sites and moments in the research. As a result, I was able to enact and document my epistemic authority with regards to the field and my academic context as situated and relational, rather than by claiming exclusivity of access, or by distancing and exoticizing the field as Other. This decision has also meant choosing accountability rather than anonymity as an ethical strategy.<sup>7</sup> It also decreased both my own anxiety about possible reactions from the field regarding eventual publications and, I believe, some participants' suspicions about possible outcomes.

Co-presence as an epistemic move is therefore an opportunity to question the distinctions between fieldwork/analysis/dissemination that were constructed by relying on geographical distance or on the distance between the life world of the lab and that of the STS academic. When research practices and research relationship occur in an increasingly mediated setting, issues of *contiguity* increase in importance and complexity (Beaulieu and Estalella, 2009). What distinguishes these efforts from other reflexive projects (Behar, 1996) is the contiguity of the various roles and accompanying accountabilities. Not only do researchers spend time managing their presence on the web or in other digital settings, attending to their personal pages (Hine, 2000) or avatars (Boellstorff, 2008), they must also monitor how others (including non-human others, such as search engines) may be framing the ethnographer's research, and the potential effect this can have on their informants, funders and colleagues.

Furthermore, when there is potential for the researcher's colleagues to be present in the field (and vice versa), the ethnographer must simultaneously attend to multiple kinds

of accountability. Contiguity of mediated settings leads not only to more reflexive use of language and to faster cycles of self-fulfilling prophecy of social research (Pollner, 2002), but it can also mean a kind of hyper-reflexivity that requires both skill and intensive work on the part of the researcher. There are a number of ways of managing these tensions. My own approach has been to acknowledge these multiple accountabilities, and to make explicit their various ethical implications as the research unfolds (for example, by writing in my blog that is read by both colleagues and participants). Co-presence as an approach to fieldwork can therefore involve novel kinds of writing and performance of academic selves – a project that may be more or less risky, in professional terms, depending on one's position.<sup>8</sup> But along with the anxieties and challenges that derive from these new conditions, these settings also put forth important question for STS about knowledge and its legitimacy in networked and multi-media contexts (Taylor and Kolko, 2003).

### **Co-presence beyond fieldwork**

The proposal not to worry about space so much, whether actual or metaphorical, and to take co-presence as a different starting point for ethnographies of knowledge practice also has implications for the organization of fieldwork as a form of academic work. Issues of collaborative or team ethnography may also become more pressing, as problems of scale are presented by fieldwork in networked settings. Given the wealth of traces in mediated settings and the role of infrastructure, how is the researcher to filter, organize and make sense of these traces, some of which look and feel like 'data' from engineering or physics? To deal with this, ethnographers may need to align themselves to particular actors in order to get access to the backstage of infrastructures and mine the log files. Researchers can also draw on tools and collaborators, though drawing other experts into one's fieldwork may require the production of particular kinds of fieldnotes and recordings (see Newman, 1998). Ethnographic research will then tend to take on the shape of teamwork, of small-scale social research. Obviously, whether such deployment of multiple kinds of expertise is necessary will vary according to the issues pursued in particular projects. The ethnographer may also find herself in need of infrastructural allies, who provide access to particular layers of the field. Today, they more often involve corporate partners or other agencies that modulate access to sites of knowledge production, such as university administrations or national service centres for research, but these alliances are no less in need of critique than earlier ones between ethnographers and the colonial powers that provided infrastructural privilege.

For the ethnographer, constituting the object through co-presence may also demand different analytical skills. It would be interesting to consider how ethnographers manage the element of meta-alternation, by which I mean the skill needed to be able to switch from one kind of presence to another. Switching roles is an integral part of everyday life and ethnographers have always taken part in multiple worlds. Fieldwork in mediated settings such as the web involves a particular version of this because these multiple worlds become contiguous, for example, existing as multiple windows on a single desktop. Ethnography of research in the humanities and social sciences are also much closer to home for STS scholars, so that scholars on campus can be both ethnographic subjects and colleagues in the course of a single day. Much has been written in anthropology on

the function of travel to the field, of leaving it, and returning periodically to it, and on the way this shapes ethnographic knowledge (Halstead et al., 2008). When the field is continuous with many aspects of our work, discrete boundaries may become less important, and other markers besides (accounts of) departure may serve similar purposes. The possibilities for ending one's co-presence and for modulating one's presence in a field that is always potentially present are being explored by ethnographers: for example, I found myself decreasing my presence in the field by visiting fewer events and by changing the level of public access to my blog (using privacy settings to restrict access to certain posts). I also gradually unsubscribed from mailing lists and other services, though I have found myself 'signing on' to lists or platforms again when wanting to signal to particular constituencies the publication of papers arising from fieldwork (Beaulieu and Høybye, 2010). I have also seen colleagues abandon the idea that a setting marks a kind of activity and embrace a performative view of when they are at work or play (Copier, 2007). Fieldwork turns out to be a more explicitly cyclical activity – one that is always more or less continuous.

The contiguity of the field and of the ethnographer's activities also implies an awareness of the circulation of one's own work, and attention to the way mediation changes the durability, and mutuality of field relations (Mortensen and Walker, 2002). Ethnographers require particular skills to modulate these relations. For example, in studying other scholars, a backstage is difficult to manage – why one is needed is of course the productive question here. This may point to a different kind of attachment (Jensen, 2007) and new forms of accountability for ethnographers, and for those who find themselves being studied, leading to research that is more cooperative rather than objectifying.

Co-presence can serve as the starting point for ethnographers in STS who study forms of research that usually do not involve large investments in physical settings such as labs. In such cases, co-presence opens new directions and new fields for STS inquiry, while drawing on the rich traditions of lab studies and of studies of infrastructures. Besides opening up new empirical directions, this approach also raises new analytical issues. Co-presence highlights unfolding interaction, which means that cycles of work become interesting, such as 'when' researchers are social as opposed to 'where' such sociality takes place. New terms may be needed to describe such fieldwork, such as 'focale', a point of sustained interest in contrast to 'locale' (Beaulieu et al., 2007). Focusing on co-presence is also an opportunity to rethink the appropriate way to communicate such work. As a concept that emphasizes interaction, co-presence shapes accounts of the field so that time and process are emphasized over space and place (Beaulieu et al., 2007). Co-presence draws attention to the temporal cycles of research, and enables the ethnographer to focus on the ways particular objects flow through phases of research. If time and infrastructures do turn out to be important elements in these accounts, how will this shape the way we write and communicate our research? We may see new forms arising, not only at different points all along the research process (as discussed in relation to authorship and contiguity), but also to convey descriptions of action in time and to provide insight into the infrastructural context of fieldwork. Hypermedia can be a good way to convey highly networked ways of working, and sequences, cycles and pace might well be rendered in digital media, perhaps as a form of unfolding hypertext or dedicated visualizations.

Co-presence also has the potential to shift the contributions of ethnography to STS. Focusing on new kinds of fields reveals roles and practices of knowledge production that differ from those observed in labs. For example, researchers may come to rely on particular kinds of people with special affinities with computers and software in order to sustain their research efforts, such as in the case of the ‘wizards’ described by Voskhul (2004: 405), and to develop new skills that enable them to become sensitive to the particularities of forms of documents (Riles, 2006). These and other studies that analyse inscriptions and writing devices as effective ways of articulating the relationship between individual and collective actions (Callon, 2002; Stanley, 2001) can be further explored through this kind of fieldwork that embraces mediation.

If studying research by being present in the lab showed the importance of having good hands, of embodied knowledge needed for making experiments work, ethnographies of mediated settings can show that other kinds of skills are turning out to be valuable in non-lab based knowledge production – sensitivities to textual and other mediated forms of expression, being able to sort, mash up, draw boundaries and otherwise navigate flows of data. Ethnographic inquiry based on co-presence can shed light on these different conceptions of scientific work. It can help to understand how researchers further elaborate and extend the particular material culture of digitization (Hine, 2008), and the new cognitive and embodied skills required by these settings (Alac, 2008; Myers, 2008). It can also further illuminate how modes of collecting (van House, 2002) and laboratory practices are transferred to web-based settings (Hine, 2002).

The effort needed to sustain co-presence should not be underestimated. Co-presence is a very active form of ‘field-making’. The field is constituted *in* the interaction. The field is not a container or background in which interaction takes place, and a certain lack of stability of the ‘field’ could be considered a potential loss of adopting this approach. This may make for ‘busy’ ethnographies. One of the implications of the assumption that interaction is a condition of co-presence may be to make ethnographies increasingly challenging to convey, at least until new genres and conventions emerge.

Finally, co-presence, as a way of deploying ethnography, can enrich our understanding of key concerns for STS: what counts as knowledge, and why, because this approach to fieldwork may also renew interest in the question of what one is studying in STS. If the ethnographer enters into what is recognized as a space of science, then he or she can (and often does) appeal to the setting to claim that at some point, the activities encountered will constitute science.<sup>9</sup> While this is questioned and deconstructed in interesting ways, the ethnographer can rely on the setting as a resource that contributes to constituting his or her object. However, if one is not taking the physical space of the lab as a necessary and sufficient condition for constituting the field, then one needs to articulate the criteria that will mark the activities as belonging to research. As fieldwork focuses on the connections that are part of these activities, this can be an occasion to further explore assumptions about boundaries, and the ways they impart certain status to knowledge in STS.

## Epilogue

In retrospect, I can now see why asking for a visit to the first lab had been such a good strategy. Its success was probably due more to the specific context of my research than to the fact

that I had hit on *the* ideal way to start fieldwork. My first site was located in new purpose-built facilities, in a prime location in the historical heart of the territory of the fields of neurology and neuroscience. The spatial organization of the lab (open plan) had also been specifically designed based on ‘new knowledge economy’ modes of management. The place and space of the lab were themselves important achievements for the lab director.

Armed with these insights about the constitution of the field, and with the possibility of thinking about constituting the field through co-presence, rather than by inhabiting a shared physical site, I also look back on that episode at the second lab in a very different way. The suggestion to take a look at the website was not a brush-off: the achievements of this lab consisted of contributions to data pipelines, of special data reconstruction programmes and of sophisticated visualization tools – all elements best seen from the website. Looking at the website, and stopping there, would not have constituted anything approaching a rich field site. But looking back, it is clear to me that I would have gained something by treating the apparent brush-off as a resource for developing my fieldwork and my relation to the researchers.<sup>10</sup> At the very least, I could have used the website as an object through which to establish relations with the researchers, by setting up occasions to examine and explicate the site together. I could also have considered how this site functioned as an interface with the lab with newcomers such as myself, as well as other researchers. And I could also have explored its interactive possibilities (various downloads, ‘demos’ and tutorials were on offer) and the extent to which I could use this interface to appropriate the tools and findings of the lab for my own use. Rather than feeling that it was a move to alienate me from heart of the action, it would have been better to consider what this suggestion from the director conveyed about the lab’s objects of knowledge. By letting go of space as primary reference and necessary condition, a broader treatment of co-presence sheds a different light on how to find out about research practices. Although they may not involve walking around the halls together, these connections are essential to the work of researchers and need to be embraced by ethnographers.

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## Notes

1. I am not referring here to the body of work called ‘presence research’, which focuses on the design and use of computer-mediated communication applications (Zhao and Elesh, 2008), nor to co-presence as an unexpected aspect of the use of certain communication technologies (Ito and Okabe, 2005), which have different scope and aims.

2. Ethnography has been a hugely successful approach in STS. A series of ethnographic investigations became canonical to the field, with the origins of lab studies (Knorr Cetina, 1995) usually attributed to the appearance of publications by Latour and Woolgar (1986 [1979]), Knorr (1981) and Lynch (1985), and sometimes Traweek (1988) – though the francophone tradition traces lab studies to studies a full decade earlier (Vinck, 2007). As a tradition, it has been described as tending to be ‘more unanimous on the question of “ethnographic locale”’ (Schlecker and Hirsch, 2001: 76), while others see an increasing diversity of sites of inquiry in more recent years (Doing, 2007; Hess, 2001). A full discussion of these contrasting views on lab studies and of variations in the use of ethnography in STS would require more space than is available here.
3. The lab, as a special kind of place, continues to be the subject of publications (Gieryn, 2006; Mody, 2005), and is characterized as a significant space analogous to other sites in the knowledge society (Knorr Cetina, 1999; Merz, 2006; Vinck, 2007). Ethnographers have considered other spaces of knowledge outside the lab, such as interactions of researchers with lay experts (Waterton, 2002), various sites of science–society interaction (Kerr et al., 2007) and conferences or exhibitions (Heath et al., 1999).
4. Solitary research can also be found outside the humanities, for example in some areas of theoretical science and theoretical social science, or in mathematics.
5. Investments in the idea of the field as a (necessarily) spatial construct can sometimes lead to pushing the metaphor of ‘online space’ or ‘cyberspace’ to unproductive and rather constrained ends, a danger that was signalled early on by Hine (2000).
6. The role of objects in establishing presence is an important element to which I cannot fully do justice here, but see Knorr Cetina and Bruegger (2000), and Mackenzie (2003).
7. The extent to which this is really a choice is questionable. For one thing, the setting itself may not allow it. Years after completing fieldwork at one of the labs that I had always been careful to anonymize in presentations and papers, Google would find traces of my presence as a user registered to the lab’s computer system. Furthermore, the ethnographer is not the only one who makes connections or cares about them: participants increasingly demand full citation or else make explicit the identity of the researcher in their own writing. A less positive side of establishing co-presence in this way is that it regularly creates friction with some editors and reviewers who feel that the moral valence of anonymizing is such that no circumstances warrant its suspension.
8. The implications are of course different for tenured staff members and for PhD candidates. What is at stake can best be seen on the blogs of Adolfo Estalella (<http://estalella.wordpress.com/>) and Lilia Efimova (<http://blog.mathemagenic.com/>), who are experimenting with complex forms of co-presence with various aspects of their field over the course of their dissertation projects (both sites accessed 29 November 2009).
9. See for example the call by Doing (2007) to reinstate the lab as a bounded site for the analysis of the constitution of facts.
10. An obvious drawback to following up on this would have been to make my work less recognizable as an ethnographic lab study to my readers.

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