STUDYING INNOVATION PROCESSES IN REAL-TIME

The promises and challenges of ethnography in the study of industrial creativity

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INTRODUCTION: THE MISMATCH BETWEEN KNOWLEDGE AND METHOD

It has recently been argued that there is a mismatch between our knowledge of innovation processes and the methods commonly employed to study them (Sørensen et al., 2010). However, while Sørensen et al. suggest experimental methods as the way forward, we will in this paper explore and discuss the potential benefits and challenges of doing 'real-time ethnography' of innovation processes, particularly related to the interactive (networking, open innovation, user-involvement, etc), complex (heterogeneity) and situated nature of business innovation processes. There are two initial premises that have influenced our research aims, and our theoretical and empirical choices in this study. First, in the literature there has been a clear call for 'process studies[3]' and interactive studies of innovation and organisation (Pavitt, 2005; Tsoukas & Chia, 2002; Van de Ven & Poole, 2005; Hernes, 2007; Håkansson & Waluszewski 2007), which involves making process – or interaction – the point of departure. There is a need to improve our understanding of the mechanisms and dynamics of how organisation and innovations unfold in practice, or of how they come into being.

Our starting point is the recent PhD thesis of one of the authors (Hoholm, 2009); an actor-network theory (ANT) inspired ethnographic study of innovation processes in the food industry. The aim was to 'follow the actor' (Latour, 1987), wherever the action happened to unfold, to understand the processes and practices of industrial innovation in a broader sense; to avoid the managerialist and the technological determinist pitfalls. We will use this experience as a way to reflect on the problems and potential benefits of doing 'real-time ethnography[4]'. In this study, the innovation processes in question turned out to produce interaction across organizations, industrial networks and even across sectors, as well as

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[3] See also Olsen (forthcoming) for a constructive critique of such calls for 'process studies', which argues that *ontological* process metaphysics will be of limited value to the field, but that its *epistemology* will make interesting and potentially useful contributions at the level of analytical conceptualisation.
[4] We acknowledge that ethnography is, by definition, 'real-time'. However, we would by this term exclude ethnographic studies of more stable phenomena, such as 'culture' or 'practice' or 'coordination' within established settings. Instead we are interested in the actual process of innovating.

coming up with products previously unseen in the food domain: A high-end brand consisting of a salami of salmon, and hyper-fresh high quality salmon loins, embraced by some of the world's best chefs. Based on this experience we will problematise the injunction of the Latourian ideal to follow the actors, follow the artefacts, and to follow the controversies, in the sense that capturing these things in real time is an attractive but far from unproblematic option.

Since the late 1970s, ethnography has become a common approach to studying knowledge production within science and technology studies (Hess, 1992; Law, 2004). According to Law, this is because it reveals "the relative messiness of practice", helping us to "understand the often ragged ways in which knowledge is produced in research" (Law, 2004:18-19). Acknowledging the need for investigating *practices* of industrial innovation, including its situated and contingent character, makes a good case for ethnography. If a central issue, perhaps *the* central issue, in industrial innovation is knowledge (both when it is available and when it is lacking), then it is reasonable to think that similar methods could be fruitfully applied to understand how knowledge is produced in such settings. This is consistent with Hess' (1992:14) call for de-centring the laboratory in STS studies, as knowledge production is distributed across a number of different settings and practices. The main differences from most ethnographies of science and technology are perhaps, first of all, the distributedness and heterogeneity of industrial practices, often involving more heterogeneous constellations of actors. This certainly includes scientists and technologists, but also marketing and sales personnel, business managers, logistics and distribution actors, politicians, bureaucrats, investors and customers. This means that action is found in many places, often at the same time. It also means that the ethnographer is challenged to engage in observing and understanding very different kinds of practices, though s/he does not always have the privilege of focusing on just one of them, and is certainly not able to capture 'everything' relevant to the process. In this sense, our aim is not only to de-centre the laboratory, as in STS, but also business management and organization, which has been put at the centre of a great deal of business research. Corporate board and management in the organisations studied are just a few of the many places and practices one may have to visit; in fact, most of the time in the field is often spent with 'operational' project participants, such as middle and project managers, scientists, technologists and marketers.

State-of-the-art knowledge on innovation processes

From the last 20-30 years of research, a number of characteristics of innovation processes have been identified. We know that innovation processes are highly situated and contingent (Van de Ven et al.1999; Pavitt, 2005), while maintaining that there are at least some room for action (agency) during the process (e.g. Garud & Karnøe, 2003). We further know that innovators rarely have all the resources and knowledge needed to realize their idea alone, thus having resource mobilization as one of the central tasks (Hoholm, 2009), which again means that innovation processes depends on –and are shaped by – interaction with others (Håkansson and Waluszewski, 2007). The complexity and uncertainty of innovation lead to ongoing learning processes in interaction with other actors as well as with the involved materials and technologies (Orlikowski, 2002; Ven de Ven et al.1999). Contingency means, of course, that innovation processes are likely to take surprising directions, sometimes failing to connect to crucial actors and resources, while (sometimes) succeeding to connect to other actors and resources than anticipated (Latour, 1996; Van de Ven et al.1999; Hoholm, 2009). This need of connecting the innovation process to other ongoing processes is fundamental: unless the innovation in some way or another can be adapted to the already established world out there,

it is not likely to realize (Håkansson & Waluszewski, 2007; Hoholm, 2009). Industrial innovation, of course, involves market practices (Araujo et al.2008) and users of various kinds (Von Hippel, 1988; Pinch & Oudshoorn, 2003; Håkansson & Waluszewski, 2007). Last but not least, we know that organizations and people tend to forget or hide most of the complexity and controversy involved in the making and implementation of an innovation (Latour, 1987; 1996), including politicized and contested issues (Brekke, 2009).

However, while knowing all this about innovation processes, there is still some work to be done in order to develop methods that take this knowledge into account, and that facilitate the systematic study of innovation processes so that we can understand these (and other) characteristics and mechanisms better.

The common methods in use

There are many problems with researching innovation. First, accounting for outcomes of social activity demands analytic tools that are able to include both humans and non-humans, as the social is socio-materially constituted. Second, accounting for interaction processes, including how things get stabilised and de-stabilised is a very different task from the more common social science methodology of measuring input and output factors to prove causality and significant relationships, or to map elements of a social realm as if they were static, stable and generalisable. Third, if only 'hard facts' are accounted for, it would be impossible to tell stories about the ordering of the social: We need to include the intentions, strategies and compromises that are made, and how actors inscribe meaning into their materials and activities. While Gupta et al. (2007) with good reason calls for more complex and comprehensive studies of innovation processes, we do not agree with them that such studies should be 'multi-level'. The argument against multi-level analysis is one of not "settling the question of scale in advance" (Hernes, 2007:74). When the innovation researcher starts following the action, or the "connections and associations made between heterogeneous actors" (Hernes, 2007:74), the term 'context', and the distinction between micro- and macro-levels are no longer relevant as analytical concepts. Context, if anything, becomes an empirical phenomenon of how the actors draw boundaries and 'frame' their activities.

Before going on to outline and give an example of what we mean by 'real-time ethnography' of innovation processes, we will have a quick glance of the most common methods in use for studying innovation process.

Quantitative methods has been used to study a number of issues related to innovation processes, such as R&D investments, IPR, firm growth, open innovation, user-driven innovation, etc *(insert references)*. However, with quantitative studies, we loose complexity and the possibility of letting time frames and process pathways be an outcome of the case/innovation process itself. *(needs further development)*

Business history has contributed with a large number of historical studies of businesses, industrial phenomena, and broader industrial processes within certain time periods *(insert references)*. Sometimes wide and institutional, other times local and entrepreneurial, often in the form of 'historical case studies'. While contributing with rich and longitudinal studies, with historical (case-)studies we also get the problem of retrospection. With retrospection we tend to loose complexity, as 'stabilized' history rarely reveal much about all the dead-ends, chance events, and controversies involved in most innovation processes. Hence, what we get

is somewhat truncated views, despite the researcher's efforts to critically examine and cross-check sources. We get stylized cases of the past, where it is difficult to break through the story as the collective would prefer remembering it. From this we may wonder whether theories of innovation process and historical studies are contradictory.

Nevertheless, it is difficult in practice to avoid historical studies all together, as there will often be a need for supplements from historical materials to understand the ongoing process. Sometimes, as in the example used in this paper (Hoholm, 2009), it may become necessary to include a set of events that happened before the researcher entered the setting. In these cases one has to rely solely on documentation and interviews with participants in these events, cross-checking the information with different actors, thus being unable to rule out the possibility important aspects of these events could have been lost (as in historical case studies). Still, the aim may not be to find the ultimate truth about these events, but to re-construct and re-present the challenges the actors were facing in a trustworthy way. Thus, in terms of the process of gathering data, there are two different methodological challenges during the real-time and more historical parts of this; the distributedness of (inter-) organisational ethnography in real-time, and the danger of actors' post-hoc rationalisation of past events. One way to handle these challenges is by cross-checking historical materials, and through keeping in regular contact with key informants in order to catch up with recent events when they were still 'fresh' and their meaning had not yet been collectively stabilised in the organisation.

Cox and Hassard (2007) review and discuss retrospective methods in organisational research, and warn against positivist and interpretivist positions that assume the past may be controlled or distinguished from the present in retrospective research. Instead, they propose a position of re-presentation, in which the present is not understood as being independent of the past, but rather that the past, present and future are co-constituted both in the negotiation of meaning in organisations, and during the writing of the researcher. In this sense, the stabilisation of history is an interesting topic in itself – of how actors delete and/or re-interpret aspects of their past in their ongoing processes of realising their present projects and identities. One could argue that this acute sense of temporality is present in G.H.Mead's notion of temporality and indeed, in views of agency inspired by Mead. Emirbayer and Mische's (1998) paper on agency is one such example. The striking implication is, of course, that situated actors and the real-time ethnographer are very much in the same boat, even if the character and purpose of their narratives are rather different. The researcher also has to acknowledge that his story is just one of a number of potential versions, account transparently for how this came to be, and make an argument for its value. The resulting time and shape of the re-presented processes are therefore both products of the participants' negotiations, and of the researcher's purpose, questions and fieldwork (who one talked to, what documents one got access to, when and where one got to observe the ongoing processes, etc).

Action research and intervention research have been employed for understanding and facilitating innovation processes for a long time (e.g. Karlsen, 2006 on action research on organizational innovation, and Engeström, 1987 on intervention research/activity theory), although not being the most visible in terms of publishing theoretical contributions. Recently, Sørensen et al. (2010) have suggested that experimental methods – laboratory as well as natural experiments – should be used more for studying and influencing innovation processes, particularly related to open innovation. Action research and experimental methods can be explained as real-time studies of particular innovation processes while also either (a)

manipulating the process (AR) or (b) raising methodological awareness and systematism (EM) in order both to learn about and at the same to improve the process.

Case studies are perhaps the most common method after quantitative methods for studying innovation process. Often this means using combinations of interviews and written materials to capture the present (and/or previous) state of some phenomenon. Sometimes it also includes observations and repeated interviewing over time, with the ambition to follow processes as they evolve in real-time. In this article we aim for contributing to a more rigorous approach to real-time ethnography of (industrial) innovation processes. However, we find many case studies to be rather static; presenting 'cultures', 'practices', 'structures' and so forth. Below we first seek to explain what we mean by 'real-time' ethnography of innovation with actor-network theory as point of departure, before we provide an example.

What is it to follow innovation in the making?

To study innovations in the making, we need to get in the position to follow actors and resources through crucial and conclusive phases innovation cases. This is a challenge, because it is not easy to 'follow the actor' (or action) (Latour, 1987) when it is unclear who and what will move the process towards success or failure. Our conception of studying innovation processes is to study an emerging object or practice from the inception of an idea to its realisation (or failure). Further, it involves studying the interactional processes of the involved actors, whether they be scientists, engineers, managers, marketing and production staff or customers, governments and finance institutions, not to mention the non-human actors, such as technologies, texts and buildings. Studying innovation in real-time is about observing and accounting for an object or practice that is also a heterogeneous network: To investigate how ideas, knowledge and meaning gradually get transformed and embodied, and thereby making the innovation more real.

How does this inform empirical research? At best, to practice real-time innovation ethnography is to build research on careful empirical observations, and acknowledge some central principles. First, it is "important not to start out assuming whatever we wish to explain" (Law 1992:380); instead, Law suggests to "start out with interaction, and assume that interaction is all there is", rather than starting out with some abstract overarching concept, like class or structure. And from there ask questions about how some interactions manage to reproduce themselves into more stable orderings. How do they "overcome resistance and become macro-social?" (Law 1992:380). Latour (1987) outlines a set of methodological rules, and a set of principles for analysis emphasising the overarching ideas of symmetry, agnosticism and free association. These principles are also discussed by Callon (1986), who makes a rather sharp critique of sociologists for their asymmetric analyses. This becomes visible in the paradox that sociologists act as if the *agnosticism* they apply to natural science and technology, which allows for a plurality of descriptions, is not applicable towards society as well: Nature is uncertain but society is not. In this way they remove their own knowledge from public discussion (Callon, 1986:197), which means that society is always given the last word. Callon's argument is that both the social and the natural are equally uncertain and ambiguous, and hence disputable. He therefore generalises the principle of agnosticism, or of analytic impartiality, stating that one should not a priori privilege humans above non-humans. Thus, sociology has no solid foundations, and "is as debatable as the knowledge and objects which it accounts for" (ibid:199). The heritage from semiotics and post-structuralism become visible in Callon's description of scientific and technological innovations as 'dramatic stories'

in which both the identity and importance of actors are at issue (1986:199). The repertoire should not include pre-established analytical grids or categories; rather there should be an attempt to follow the actors to understand how they build and explain their world (ibid:201). Hence the principle of '*free association*' is introduced, emphasising idealist methods such as ethnomethodology, phenomenology, discourse analysis, etc.

However, industrial innovation processes involve sets of epistemic and economic practices which have not yet been studied extensively withinANT/STS[5]. These are practices with different aims, frames and evaluation criteria from scientific practices; the stabilisation and evaluation of knowledge is performed less according to scientific norms of knowledge production than economic norms of profitability, return on investments, etc. This claim has also been partly addressed by Mouritsen and Dechow (2001) and Mouritsen et al. (2009). The general point they make is that management technologies are used to make sense of the past, make room for innovations and generally help stabilise knowledge. Hence, transformations of scientific knowledge into industrial and economic practices are often uncertain processes demanding considerable time and resources (Håkansson & Waluszewski, 2007). The inclusion of practitioners of various kinds and in various places is beneficial towards the understanding more of such complex processes; arguably the main source of uncertainty is found in the interaction between various practices and practitioners – especially between those that are new to each other. Advantages of this methodology are the opportunities following social (and socio-material) processes and practices *as they evolve*. Ethnography may be said to be about participant observation, with the ethnographer "participating, overtly or covertly, in people's daily lives for an extended period of time, watching what happens, listening to what is said, asking questions – in fact, collecting whatever data are available to throw light on the issues that are the focus of the research" (Hammersley & Atkinson, 1995:1).

In ethnography, emphasis is put on the interaction between the ethnographer and actors in the field, hence the argument that ethnographic fieldwork has a dialogical nature (Hess, 1992). Such real time studies have more power to elucidate the uncertainties and contingencies the actors experience in the course of deciding and acting. This is not a matter of constructing an objective truth of what happened; rather, it is an attempt at re-constructing the actors' experiences, interpretations and actions in the face of the 'opportunities' and 'uncertainties' of innovation. An attempt to re-construct/re-present some of the difficulties, controversies and choices that the involved actors had to handle, and avoid post-hoc rationalisation. When situated actors and the ethnographer make sense of stories as they unfold, in some sense, there is a strong sense both to linearise and compact narratives as you go along. The point that Deuten and Rip (2000) raise is an interesting one in this context. The narratives of successful innovations tend to successively eliminate the tortuous traces of its own production in favor of simpler, straight line narratives. By contrast, failures tend to be more amenable to complex, detective story genres (who did it?) one finds in Latour's Aramis (1996), for example. Latour (1987:258) underscores the need for studying knowledge production *in action*, in order to "either arrive before the facts and machines are blackboxed or (...) follow the controversies that reopen them", thereby looking for the transformations the innovation go through during the process of its realisation. In addition to the advantage of 'real-time' studies of contingent processes, and the provision of 'thick descriptions' (Geertz, 1973) of the empirical field, ethnography may produce both deep insights and increased variety of interpretations. The ethnographer is not granted sovereignty over interpreting and theorising the case. If research

[5] See chapters one and six for the presentation and discussion of some recent contributions in an emerging field of economic and industrial sociology with roots in STS.

is viewed as being an ever-evolving discussion, this should be viewed as a great advantage, and if the analysis of social issues is the researcher's "constructions of other people's constructions of what they and their compatriots are up to" (Geertz, 1973:9), his/her discussion, re-interpretation and comparison with other cases should be viewed as a necessity.

However some authors have also posed critical questions of the Actor-Network theorists' approach to ethnography. In summing up some of these critiques, Vickers and Fox (2004) mention some arguments about representation. ANT has tended to focus on elites, a narrow set of actors: Despite its claim for symmetrical analysis it still tends to represent some humans more than others. ANT has also been accused of refusing to engage in ethical debate, taking a relativist standpoint. However, Vickers and Fox suggests that there is nothing innately relativist in ANT as such, though this still might be the case in some of the ANT studies that have been done. Some argue that ANT is marked by 'analytical decontextualism', that the "wider social context is often let out of the analysis", and even that ANT researchers only pick out the bits from their ethnographies which corroborate their ANT points. Vickers and Fox' solutions to this issue are to focus more on non-elite people, and on processes of counter-enrolment/resistance. They further accept that ANT offers a kind of de-contextualisation, like other ethnographic approaches, the context in sight is dependent on the viewpoint of the studied actors and those of the observer. However, in line with the de-contextualism-argument, we find that there is good reason to seek to 'stretch' actor-network ethnographies a bit beyond what has commonly been done in this field of research; tracing the network building activities beyond local sites and projects, and connecting the narrative to some of the other narratives with which it interacts in practice.

DOING INNOVATION PROCDESS ETHNOGRAPHY: AN EXAMPLE

Gathering materials

In line with ethnographic methods, the fieldwork of the mentioned thesis work (Hoholm, 2009) consisted of gathering a highly heterogeneous mix of research materials, using participant observation, informal conversations, interviews and document analysis. During in initial period of 6 months, the researcher spent a lot of time at the focal company's R&D department, both with some workgroups, and talking to various people around in the organisation. He also began conversations with what would come to be a small sett of key informants. After a few months, he went with two of them to a Salmon Farm partner for observing the first attempt at large scale production of a new salmon product there. He also joined the project manager on a new trip to the Fish Farm a second time three months later, and finally visited the Fish Farm in two years later for a last round of interviews with the management at the Fish Farm.

After having spent a few months in regular contact with the R&D department, he gradually increased the contact with people on the commercial side; the commercialization manager of the salmon project, managers, and people working on several issues, such as IPR, international marketing, etc. Shortly, it was time for the first international market tour, and the researcher joined the team to a huge food fair in Paris, hence getting a chance to get to know a number of involved people better. It still was not clear whether the salmon project was the best innovation process to study over time. Therefore another project (biomarine biotech) was investigated in parallel, involving e.g. a tour to the remote place where the plant was situated, together with two of the project managers from the Food Company.

The first months of the second year the researcher borrowed a desk in the open-plan offices of the Food Company. He spent several days a week there to observe their work practices, strengthen his informal dialogue with central actors in the relevant projects, conduct a first round of interviews, and go systematically through available project documentation in project and individual archives. Several of these people became key informants in his fieldwork, people who he met relatively often; they were in various ways involved in the processes he was studying, and willing to openly share their views and experiences.

With regard to his status and role during the fieldwork, as a newcomer to the organisations he was allowed the role of 'acceptable incompetent' (Hammersley and Atkinson 1995:103), thereby having the opportunity to ask 'silly' questions about things 'insiders' take for granted. Related to Junker's typology of social roles in the field, he was perhaps closest to the 'observer as participant' role, meaning that he did not take part in actually doing any of the activities he studied, and he did not have any tasks or responsibilities within the organisations. At the same time, everyone knew that he was a researcher, and he spent a great deal of time talking and interacting with the actors. This is 'participant observation' (Hess, 1992), not to just observe behaviour, but also to engage in dialogue. As with many other ethnographers, the informal conversations at the desk, by the coffee machine and over lunch provided him both with valuable information and with a deepened understanding of the practices of the organisation. He sought to keep in regular contact with people at the R&D department also during this period, although not as often as the year before. More of the action in the project had moved to the marketing department, to the production facilities, and to various customer locations.

The researcher did not use a structured interview guide during any of the interviews; instead he brought lists of topics that he wanted to cover during the conversation. This is, according to Hammersley and Atkinson (1995:154), perhaps the clearest difference between the way survey interviewers and ethnographers structure interviews: Between 'standardised' and 'reflexive' interviewing. This is not a matter of unstructured versus structured; rather, the ethnographer structures the interview – together with the interviewee – as conversations, where the order and mode of questions may shift as the conversations evolve; non-directive, directive and even confrontational. In addition to these interviews, his understanding of what was going on, including emerging innovation strategies, work practices and power relations, was to a large extent shaped by the informal interaction over time with the various participants in the studied processes. Meeting up with individuals and groups during their daily work activities, sitting at a desk in their open plan offices, and travelling with them to partner and customer meetings produced a large number of interesting observations and informal conversations.

For practical reasons, he did not have the opportunity to balance his time spent 'in the field' evenly throughout the investigated process or across the places where things happened. First, this was because parts of the relevant processes had already taken place. In the salmon project, the initial explorative and science based phase was finished, as well as the first phase of the product development phase. For these parts of the story, the researcher therefore had to rely on a combination of interviews, informal conversations and document analysis. Key personnel from these early phases who were no longer participants in the projects, still needed to be interviewed. Second, the real-time processes under investigation were unpredictable and complex. Sometimes meetings and discussions had taken place on short notice in times and places where the researcher was not present. Even if he experienced an open attitude to his

presence, it did not always mean that he was invited to business meetings or other events of potential impact. Other times things happened several places at the same time, like when the people at the production struggled to improve and stabilise their production routines together with people from the R&D department, the marketing people worked with adapting their strategy towards potential customers, and the management worked on renegotiating agreements between the involved parties at the same time.

On a few occasions the researcher had the opportunity to present his (preliminary) interpretations of the innovation process back to project participants and well-informed groups of people: In meetings with strategic staff, middle managers and project participants, in seminars with the company board members present and by getting project participants to read and give feedback on the different papers describing and analysing the case study.

Re-organising and reconstructing materials: Writing ethnography

To be sure, in ethnographic research, observation does not precede analysis as they are better depicted as constituting an intertwined process, a 'dance' between observing, talking, reading, thinking and writing. However, as many anthropologists have noted, perhaps the most demanding task of ethnography is *writing* (Geertz, 1973), so methodological resources are also needed for the textual treatment of the often massive amount of field materials (notes, interviews, documents, artefacts, pictures, videos, etc). Writing is a process of ordering these materials into a meaningful text, that is, a text that provides new insights into the particulars of the investigated setting, as well as what can be learned from this in dialogue with other studies of similar phenomena. Thus, how can we account for real-time studies of innovation processes, noting also that at the stage of writing, the case is already *past*? An ethnographic research strategy in inter-organisational settings tends to produce an enormous amount of detail, which is incomprehensible without some framework through which the story can be reformulated and analysed. An 'ordering strategy' was needed for handling the complex data (or 'capta' as suggested by Hernes, 2007).

All interviews and field notes were analysed, identifying and sorting the materials in themes and events, both in looking for themes 'growing out of' the texts, and themes related to research questions and theory. In this circular process of observing, analysing and reading theory, the materials had to be re-analysed and re-organised several times before he was confident that he had a set of themes and stories that would help him answer the research questions in a constructive way. This was also related to his attempts at structuring a narrative and theorising from the empirical materials. Many ideas about how the story should be told, and how theoretical discussions and contributions should be framed, were tested in writing and discussion with supervisors and other colleagues, before his thesis took shape. Should the chronology of the investigated events, the themes he wanted to emphasis, or even his own process of investigation be the underlying principle for presenting (re-constructing) the empirical stories in text? He ended up deciding on a relatively chronological and detailed description of the case preceded by an introductory chapter, which situates the case study in a network of interconnected processes.

In line with Geertz' (1973) concept of 'thick description', and Hess' (1992) suggestion to include more of the field materials in the account to avoid finite interpretations from a 'superior' ethnographer, he used field materials extensively. However, it was a lot easier to be explicit about the use of interview materials and documents than using field notes of

observations and experiences in the field. This has partly to do with the danger of ending up with a researcher-centric 'confessionalist' tale, and partly with the sense of certainty: This is really what the informants said, whether the reader agrees with his interpretation of it or not. Nevertheless, although the aim here is to make the case study both more transparent and convincing, he still have to carry the responsibility for the questions he asked, what quotes he included in this text and how he combined quotes and his own thinking.

With regard to the outcome of the study, he aimed at contributing to the field of innovation studies, first, by providing rich insights into an under-researched phenomenon, and second, by engaging in dialogue with related literature on conceptualising key characteristics of innovation processes, and discussing the relationships between them (see e.g. Weick, 1989; Walsham, 1995; Hammersley & Atkinson, 1995; Law, 1994 for discussions of generalisations from interpretive research). The theorising of this thesis is therefore best understood within the category of conceptual generalisation. He used his empirical account and the following analytic scheme to suggest the theoretical implications for innovation processes, while challenging and complementing previous studies. Still, the degree to which the insights from a study of a particular set of innovation processes within the food industry can be fruitfully applied to innovation processes in other settings remains a question for future research.

THE CHALLENGES OF INNOVATION PROCESS ETHNOGRAPHY

One of the reasons, however, that we to date have seen too few in-depth and longitudinal ethnographic studies of innovation processes is that it can be very hard to do. First, and similar to other longitudinal studies, one easily gets problems related to time constraints. It is, for example, doubtful that the mentioned food study could have been done within the ordinary timeframe of a PhD. The timeframe of the empirical study had to be kept open for quite some time, simply because it was impossible to know how the innovation process would develop. Thus, we may wonder whether our theories of innovation are constrained by our methods and time frames? Second, there is the problem of choosing what processes to follow. In the heat of action, there is not *one* innovation process, there are many ongoing processes at the same time, sometimes interacting and sometimes moving in different directions. Sometimes totally peripheral processes suddenly become very central, while later perhaps again moving to the periphery. Innovation studies (e.g. Van de Ven et al.,1999) tell of false starts, projects that fail, and sometimes failures that are picked up and successfully transformed into something else. The researcher, observing in such complex situations, faces the same problems as the manager: lack of information about how things will turn out, lack of tools for judging what parts of the process to observe, and little orientation as to what kind of data that will turn out the most useful. One cannot be everywhere at the same time, and organizations and networks are as we know spatially distributed, and many things that later will turn out to be important to the innovation process happened in some of the places where the researcher was not.

There are, at least, two points that are definitely worth reflecting on: a) is there such a thing as a real-time study?; b) how much can we theorise about process? The answer to the first one might be that capturing events in real time involves the same problems historians face in terms of parsing events to fit into a narrative that is necessarily partial and selective. But, trying to write history on the fly has a marked disadvantage compared to the historian's distance to a set of events. The second question raises interesting issues regarding the relation between history and the social sciences. On one corner, you have the Van de Ven and Poole (1995) position that you can study process in general and talk about ideal process types. On

the other corner, you have those who emphasise contingency and conjunction that is sensitive to path dependence and path creation to put the title of Garud and Karnøe (2001) to good use. In this sense, there is no such thing as process in general, only very specific processes that we can artificially delimit in space-time.

We need to get rid of the romantic view of ethnography. Instead we suggest that the innovation ethnographer should see research as a process of tracing, in fairly detailed ways, bits and pieces of processes that could not have been recording via other methods. Further, there is a need for competent balancing of method and theory: First, of keeping method both strong and open enough to enable systematic observation while being sensitive to the things that will turn out differently than expected. Second, of keeping theory both strong and open enough to enable ambitious theorizing based on messy and situated observations.

THEORISING IN ETHNOGRAPHIC STUDIES

According to Geertz, interpretive approaches to social research "tend to resist (...) conceptual articulation and thus to escape systematic modes of assessment" (1973:24). But, as "social actions are comments on more than themselves", there is "no reason why the conceptual structure of a cultural interpretation should be any less formulable (...) than that of, say, a biological observation" (ibid:23-24). There are several ways of dealing with this; from dividing strictly between empirical representation and theorizing, to integrating theory and practice to an extent that the result should be considered a 'web of materials', or a continuous dialogue between empirical materials, researcher interpretations and theory (see also Van Maanen (1988) on different styles of writing ethnography). Of course, during the entire process of choosing case and questions, of observing and talking to actors in the field, and especially choosing what to include and how, a written account will be highly influenced both by theory and the researcher's own reflection and interpretation along the way.

The mentioned study of innovation in the food industry above has some similarities with Latour's Aramis-study (1996), in the sense that they both seek to trace the contingent, and often controversial and hidden, processes leading up to technological and commercial innovation. However, there are also a couple of major differences. While Latour (1996) investigated a failed project, and thus could write it like a detective novel (or 'scientifiction' as he called it himself), with the question of "who killed it?" as the hub for the story. Second, Latour, studying an already finished project, had to employ historical methods (strengthening the detective metaphor). Despite the problems of historical methods, and Latour's own preferences for ethnography, in the investigation of failure it is probably easier to trace the controversies and challenges than in the case of success, as success-stories have a tendency to hide and delete every trace of controversy. In our case, which turned out to become a success-story for the participants, the researcher had the opportunity to observe a lot of the process when it happened, thus being able to describe choices, controversies and compromises as they appeared when they still were unsettled and nothing yet are taken as given. Law (2004) describes such situations as 'messy', which we think is to the point. This enabled the systematic development of analytical frameworks and theorising discussions taking controversy and uncertainty of how to relate the innovation to other networks and processes as the starting point. Hence, new and complementary insights could be produced about such innovation processes that could not have been done from historical studies, or that could have ended up as rather speculative if the researcher had not been there while it happened.

So, obviously, choice of method determines what you see, and what kind of data you get. And this is not a trivial point; if we want to understand more precisely – from the 'inside' – the mechanisms and challenges of innovating, we need to be serious in designing methodological strategies that enable this to happen.

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