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HANDELSHØJSKOLEN
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Adapting in the Knowledge Economy

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Adapting in the Knowledge Economy

Lateral Strategies for Scientists and Those Who Study Them

Birgitte Gorm Hansen

Doctoral School of Organisation and Management Studies

PhD Series 22.2011

Adapting in the Knowledge Economy

- Lateral Strategies for Scientists and Those Who Study Them

PhD thesis for examination

Submitted June 2011 by Birgitte Gorm Hansen

Supervisor: Maja Horst, Copenhagen Business School

Co-supervisor: Casper Bruun Jensen, The IT University of Copenhagen

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The Doctoral School of Organisation and Management Studies (OMS) is an interdisciplinary research environment at Copenhagen Business School for PhD students working on theoretical and empirical themes related to the organisation and management of private, public and voluntary organizations.

Table of contents

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Welcome to paradox

There is a celebration in the canteen. Unlike the normal monthly "get-together", this one is important. Everyone in the Nano-Science Center[1] *has got it in their calendar. As I step into the elevator, I get updated on what happened at the department meeting this morning. Between 10 and 15 percent of the academic staff were laid off at the departments of physics and chemistry. In biology it was even worse. Some lost their jobs, some their whole research area. Some left the building in tears. None of the Nano-Science Center's physicists or chemists was affected though. "Still it feels kind of inappropriate to be throwing a party on the same afternoon", says the head of the secretariat. "At least it's a bit weird after a morning like that". She pauses for a moment as we exit the elevator and gives me a pale smile. "On the other hand it would be just as weird not to party, given the circumstances".*

As we enter the canteen Thomas Bjørnholm is already at the large meeting table opening the champagne bottles. He is dressed as on any normal working day, in jeans and a jacket. The bottles are all lined up on the big table, twenty or more of them, champagne glasses ready in neat rows. More glasses are waiting in the kitchen, wet, bottoms up on a dishcloth. The room slowly fills with people; I think pretty much everyone is here. I say hello to the plant biologist Birger Lindberg Møller who is standing next to some people I don't know. I´m assuming the new faces are all guests from the life and medical sciences. People are smiling; voices are low. No one touches the potato chips.

Bjørnholm takes a break from pouring champagne and raises his voice: "Everyone, please begin." Champagne glasses are passed around the room from hand to hand; people are flocking quietly around the tables as they continue their conversations. After a while, he gets up on a chair with a bottle in one hand, and a knife in the other, trying to get the attention of the room by beating a knife on the bottle, it

[1] The Nano-Science Center is a large interdisciplinary research center situated at the Copenhagen University. It was established in 2001 as a new research unit between the Niels Bohr Institute and the department of Chemistry, but also works closely with the Department of Biology and the Department of Neuroscience and Pharmacology. The center now works across the faculties of Natural Science, Health Science, Life Science and Pharmaceutical Science, hosting eight research groups and two basic science centers. The center was the first in Denmark to offer full bachelor and master's programs in nano science and houses 100 researchers, post docs and PhD students.

doesn't work. One of the physicists grabs two bottles and beat them aggressively against each other. This works, the room goes quiet; a light laughter goes through the group of people standing near to the physicist. Up on his chair Bjørnholm welcomes everyone in an enthusiastic, loud and clear tone of voice. His broad Danish accent makes him seem very down to earth. As he stands there on the chair with a knife in his hand, he looks like someone who just got up there to replace a light bulb and happened to come up with a speech on the way down. This is his usual style: improvised, relaxed, confident smile, enthusiastic gestures.

"It's a strange day today", he begins, "a very strange day to celebrate in. As we all know for some of us this morning was a somber one. But even though we probably all have mixed feelings about throwing a party on a day like this, we still need to remember that everyone in this room has good reason to celebrate". Bjørnholm is holds up a copy of the UNIK-application, a small white document. The cover is decorated with a colorful schematic drawing of a plant with 4 stems growing into a single flower. Each stem is named with a discipline: Molecular neurobiology, biophysics, molecular plant biology, chemistry & nano-science. As he waves the document in his hand he continues. "As our minister of science used to be employed in sports, he in his wisdom came up with the idea that research should work like a competition - a champion's league of Danish universities if you like." People laugh, as university academics usually do when someone makes a joke about the minister of science. "So, he decided that the principals of Danish universities should compete for a huge amount of money also known as the UNIK-research fund. We of course jumped in the game and wrote up this application on synthetic biology together with our partners". Bjørnholm points to Møller and a couple of other new faces in the room and explains how they were part of the application. Bjørnholm introduces the partners in the application by name, faculty and discipline before he continues. "It just so happens that our application was one out of four that got funded. The collaborators behind this application, as you may already know, have thus received 120 million Danish kroner to do a five-year project on synthetic biology in collaboration across three faculties and many institutes. Never before have I experienced receiving so much money in one go. This definitely calls for a celebration, no matter the circumstances. And as if this was not enough, we just got word that Susan got one million UDS a year for the next 10 years from BP to build up and support her project on extracting oil from the chalk-layers in the North Sea. Heads turn to Susan who is standing in the middle of the room, she responds by a quick nod and a smile. Bjørnholm continues: We will of course celebrate Susan's grant on a special occasion, but I thought that we should also share the good news today. Now that we are at it, I may as well add that we also have received word this week that the university administration has approved the building project for 2.5 billion kroner. This project will have top priority, possibly giving us a new building for interdisciplinary collaborations already by the year of 2012". The

physicist with the bottles interrupts: "And well, I won 100 kroner on a lottery ticket today".

Bjørnholm smiles and picks up on where he left: "All in all, this means that our area in general - and the Nano-Science Center in particular - is doing extremely well, even in these times of crisis". He pauses, then lights up with sudden, thoughtful smile, "It seems it's going so well in fact, that we don't even have time to celebrate each of our successes. We simply have to bundle them up like this and celebrate them all together on the last Friday of each month". As the laughter settles, Bjørnholm raises his glass: "I suggest we all make a toast to this good news, even though in many ways it feels odd to celebrate on a day like this. We still need to celebrate the good things, even in difficult times". The silence after the toast lasts a little longer than I expect, I shift the weight on my feet. Just before the moment becomes awkward, someone discretely grabs a potato chip, the party begins.

Introduction - Mixtures

> A mixture is not easily analyzed. Work, light, heat, a thousand pieces of information is necessary. If I wish to drink this water, I also have to drink the sugar, if I want the sugar, I must swallow the water, if I want one constituent, I have to pass via the result as well as all the other constituents. The continuous is unanalyzable at any given moment and so are mixtures. (Serres, 2008, p.79)

Most academic scholars have by now had more than one sip of *the knowledge economy*. We all know it is not just about knowledge. If we want to speak, think and write from inside a university, we also have to take words in our mouth that are invented by people outside it. If we want our research projects funded, we must swallow the interests of other parties. If we want to follow our academic dreams we have to pass via the dreams of ministries, research councils and corporations. Knowledge economy is a mixture, drinkable only to those who have the stomach for complexity.

In philosophy, Michel Serres has pointed to the problem of understanding mixtures by way of Henri Bergson's example of sugar water[2]. Bergson argues that a theory of knowledge must be based on duration and insists that we wait for a spoonful of sugar to dissolve in water before we can claim to know anything whatsoever about sugar water. It is not enough to recognize that the sugar, the water and the time of their dissolution are related, neither is it enough slice up the process and analyze each of the constituents. To Bergson, their relation is absolute, it forms or contracts into an indivisible whole (Bergson 1988). Looking at an isolated split second of the dissolution process will not help our understanding of it, we have to wait and understand it as duration. Serres is interested in founding a theory of knowledge on mixture and adds to Bergson's famous sugar water example the following comment: "He never required us

[2] 'Though our reasoning on isolated systems may imply that their history, past, present, and future, might be instantaneously unfurled like a fan, this history, in point of fact, unfolds itself gradually, as if it occupied a duration like our own. If I want to mix a glass of sugar and water, I must, willy nilly, wait until the sugar melts. This little fact is big with meaning' Bergson 1998 p. 9. See also Serres 2008 pp. 78-80.

to wait for the mixture thus formed to separate out again. Readers would have had to wait until the end of time" (2008, p.79).

In the social sciences and the humanities, some of us have waited for the knowledge economy to separate out again for quite some time now. At the risk of giving too much of the point of this thesis away I should warn the reader that according to the pages ahead, it looks as if we are going to have to give it a while longer. Knowledge *and* economy, science *and* industry, policy agendas *and* scientific practices are indeed contracting into an indivisible whole. This poses a serious challenge to analysis and even more so to critical inquiry.

The field participants represented in this thesis are all natural science scholars. Like us, they are busy people, they have little time. However, they do have the stomach for complexity. Consequently, they finished their drink long ago and went back to work. What all of the scientists we meet in the following pages have in common is that they have all adapted extremely well to the introduction of the concept of knowledge economy in their working lives. All of them are skilled in dealing with new and often conflicting performance measures and have learned to get the most out of an intensified demand to include government and industry interests in their work. They are remarkably skilled in attracting funding, they are part of extremely successful research programs, they are well connected with industry and they drink more champagne than I have ever seen anyone else do in a Danish university. I would not be unfair to claim that the people we meet in this thesis are doing extremely well in the knowledge economy. With citation indexes and budgets that exceed the wildest aspirations of most researchers in the humanities and social sciences, these people have beyond any doubt been extremely capable in making new agendas in research policy work to their advantage.

However, as will become clear in the following chapters, their success was not easily achieved and constitutes a rare privilege. Massive public investment in research and increase in private funding has also introduced fierce competition and new performance measures. Being a talented scientist is no longer enough to make a career, if it ever was. You need to be a top-performer in relation to a broad range of audiences in order to really become fundable. There are many parameters in which scientists need to perform with "excellence" and some of them are mutually exclusive. Not all Danish scientists celebrate new large research grants with champagne these days. Every celebration of a research grant rests on a mass of time consuming and exhausting failures at attracting funding and meeting new and ambiguous performance measures. The scientists who did not make it in the knowledge economy left the building before my fieldwork began. One of the successful scientists I interviewed even claims to be the last survivor in his research area, everyone else is gone, he says. Even among the winners, there is a sense of loss.

While we wait and patiently watch the dissolution of academia into the muddled waters of knowledge economy, we may as well think about what these successful scientists are up to. The aim of this thesis is thus to *trace the tactical maneuvers* of science through what Thomas Bjørnholm characterized as difficult times. It seems that an important part of their success is due to their skills in managing science by engaging and involving actors outside the university. I will especially be interested in finding out *what strategies these scientists have used when getting financial and political support for their projects*. How do they acquire their amazing fundability? Another and perhaps more important question is: *What are the costs of these strategies?* What is lost in the attempt to make it in the knowledge economy?

According to Serres, mixtures are not easily analyzed, at least not if analysis is understood in its etymological meaning as “to untie”. Dissecting and cutting up the qualities of a mixture into isolated constituents will not tell us very much about them. “To analyze is to destroy” Serres argues (2008, p.167). Separating out the mixtures of science in the knowledge economy into constituents like capital, politics, institutions, norms or social groups may thus not tell us very much about how these things work once they have contracted into an indivisible whole. As a consequence, this thesis will approach the task of analysis by way of mixing up things even further rather than trying to take them apart. A champagne toast may taste differently relative to what food is served with it and what events led up to the first sip. In a similar way, I am hoping to bring out – or rather bring in – new flavors in the academic commentary on science in the knowledge economy by adding seemingly incommensurable elements to facilitate description.

This is primarily an experimental endeavor. My analytical approach does not take the form of a hierarchical mode of representation where fieldwork experiences are built into the scale of a higher order theoretical framework. Rather, it poses a critique to the way such frameworks are used to represent, and sometimes also govern, the practice of science. Rather than putting things into context by elevating the conceptual to a higher ground above the empirical, my experiment allows the two to be in continuous variation (Jensen and Bowker 2011). I will thus be addressing field participants, not just as practitioners, but also as thinkers, as they are as conceptually informed in their work as I am in mine. The experiments will proceed by a series of *lateral* (Strathern 1999, Maurer 2005) moves between the practices and concepts of informants and those of science studies. I will be comparing scientists to the objects they study and the technologies they work with. More importantly, I will use their conceptual language to help me transform or rethink my own. Concepts invented by natural and life science scholars are then not taken to be an empirical reality or practice in need of conceptualization of from STS. Rather, natural and life science concepts are placed on the same plane of reality and invited to reflect back on concepts invented by science studies scholars. As a consequence, each chapter proceeds by mixing up things rather than taking them apart and is thus allowed to form its own peculiar mixtures.

In chapter one, I will give a historical account of how Danish research policy took up the concept of the *knowledge economy* and launched a series of new initiatives to promote increased *interaction* between the university and its surroundings – mainly industry. I will do this by mixing statements made in Danish research policy with some of the theoretical concepts in Science and Technology Studies (STS hereafter) that seem to have inspired an intensified focus on interaction. I will argue that the relation between university sector development in Denmark and conceptual development in STS is not one of external commentary to an empirical phenomenon "out there". Rather, conceptual resources invented in STS seem to be reiterated in Danish research policy. However, this does not mean that research policy concepts stands in a linear and unidirectional relationship to scientific practice, rather they too contract into mingled bodies.

Chapter two takes a look at two theoretical contributions to the problem of conceptualizing the mixtures of scientific practice. The first is selected from the constructivist strand of science studies and proposes that science was all a mixture to begin with. The other proposes that even if things were mixed up from the start they are now entering a danger zone of indescernibility and consists mainly of critical accounts of the convergences that seem to characterize the intensified focus on commercialization. These two positions in the literature are allowed to blend in with descriptions from my early fieldwork, thus presenting the field and the two conceptual contributions together. As it happens, this mixture often tastes quite odd. Some of the analytical distinctions made in the literature seem less suited to account for the complexity of science-industry mixtures as field participants conceive of them. The temptation to untie the mixtures of scientific practice to solid constituents like those of science and the market seems to get in the way of the inquiry.

In chapter three I will discuss the methodological problems entailed in analyzing mixtures. By assembling methodological discussions in STS, philosophy and social anthropology, I advocate for a *lateral* approach to analysis rather than a *hierarchical* one. Hierarchical analysis elevates the conceptual framework of the analyst to a higher abstract order from which the empirical can be made knowable. Lateral analysis, by contrast, places the conceptual and the empirical on the same plane of reality and proceeds by juxtaposition or analogy, thus allowing the two to "draw on" or "metastasize" into each other (Maurer 2005). I propose that just as STS concepts and policy concepts can be used to think about scientific practice, the concepts invented by scientists can be used to rethink policy vocabularies and the vocabularies of STS. I advocate for leaving behind hierarchical questions such as *what is this, an example of?* thus abstaining from putting fieldwork experiences into a higher order context in which they "fit". No undisputable conclusions will be made about the state of the Danish knowledge economy based on my analysis. Rather, I will proceed by lateral experimentation by asking questions such as *what is this comparable to?* Using analogies and comparisons as analytical devices invite analysis to move beyond preconceived ideas about scientific practice or pre-packaged critiques of how knowledge economy affects science. There is no knowing in advance whether I will succeed in doing so. As with all experimental approaches, *lateral analysis* takes risks and cannot make promises in advance.

Chapter four, five and six each give their version of a lateral analysis of the strategies deployed by scientists in response to the interaction-agenda promoted in Danish research policy. Each chapter proposes a different comparison to articulate scientists' strategies and think about their costs.

Chapter four will look at the self-representation of the Nano-Science Center as it is performed in press releases and by the manager Thomas Bjørnholm. I will compare the representation of science with the representation of nature, proposing that making a research project visible by way of power point slides is akin to making nature visible by way electron microscopy. The nano-scale world of high resolution microscopy is used as a conceptual framework to show how a specific research program is made fundable by acquiring specific types of visibility in which basic science research become the path to new and better futures. Fundability is a matter of fitting the research project to specific kinds of gazes in which it becomes visible as relevant, useful and necessary science. I will argue that the costs of "oversight" and "erasure" are necessary parts of bringing a research program into existence and not merely a distortion or pollution of interest-free basic science. Acquiring visibility rests on the complex task of producing specific kinds of visibility while leaving others in the dark.

In chapter five I turn to the world of plant biology to discuss the concept of the boundary as it has been used in STS studies of science-industry relations. Here I will draw on interviews and observations with Birger Lindberg Møller, who is the manager of another successful research center at the Copenhagen University: Pro-Active Plants. I will draw on Møller's biological vocabulary to depict the relationship between Pro-Active Plants and a small in-house biotech company. As an alternative to analyzing this relation in terms of boundary work or boundary blurring, I suggest the concept of symbiosis as derived from studies of plant/insect co-evolution. Comparing plant-insect relations with science-industry relations, I suggest that knowledge production and knowledge consumption are neither unidirectional nor linear processes. The survival of each party depends on specific kinds of divergence that make them depend on each other.

In chapter six I continue the biological entanglements to describe how a specific scientist succeeded in making his research program in plant GMO survive in spite of a rather hostile funding-habitat. Using plant-insect co-evolution as an analytical framework, I argue that the research program survived by adapting creatively to the developments in research policy rather than assimilating to them. It has been argued in the STS literature that research programs have a higher chance of survival if scientists assimilate to the needs of society and integrate non-academic interests in their work and produce "socially robust knowledge" (Nowotny 2003). Adding plant-insect co-evolution to the mixture, however, makes me suggest that this specific program acquired robustness not by being responsive and inclusive but rather by finding ways to hold on despite resistance. I suggest that the result is not socially robust *knowledge*, but rather robust *scientists* that gain support for their research despite lack of industrial interest, absence of public funding and massive political resistance.

In the concluding section I will reflect on the contributions of the thesis. First, I will summarize the methodological contribution. What is the value of taking a lateral approach to analysis and what kinds of descriptions are facilitated by this approach? Second, I will consider how the three lateral experiments in chapter four, five and six have contributed to science and technology studies. Third, I will discuss how the experimental contribution of the thesis reflects back on the interaction agenda of Danish research policy. The lateral experiments I conduct thus allow me to pose critical questions about the "runaway effects" of specific policy changes (Wright and Shore 2011). What points of critique can be derived from my inquiries and what kind of criticality could that be?

1
From insights to invoice

After two days of non-stop presentations in a stuffy concrete block of a conference center, we really needed to get out. Even an old tourist bus slowly crawling its way through the landscape, feels like a nice break. It has been a packed program and you can tell people did not have time to talk between sessions, they are using the drive as a nice opportunity to make up for it. The biologists seem to begin mingling with some of the physicists. Some are moving around in the bus, holding on to the backs of seats and swaying as they try to pick up on the conversation they left at lunch. I am sitting next to Robert Feidenhans´l, a rare privilege since his professorial and managerial duties normally keep me waiting outside his door along with a couple of his most patient PhD students. In the seat in front of me is associate professor Kim Lefmann, his head turned towards us. We are talking about research managers. Robert is telling me the story of a research manager he used to work for before he came to the Nano-Science Center. Even though this guy was probably a good manager and gave the institution a strong profile to the outside world, he was not respected by the scientists working in the department. He did not have the kind of scientific credibility that made him trustworthy, says Robert. He was just a "technician" who was fixed on the idea of turning the research group into a more business like unit. Taking them "*from insights to invoice*", so to speak. Rumor has it that this is the guy who coined this now infamous mantra associated with Danish research policy. Robert makes it clear to me that it was this manager and his insistent demand for patents and products that made him decide to quit his job there and move on. When I ask him what exactly made him move, he replies: *"they take your freedom away"*. For a while we discuss what this means: having your freedom taken away. Kim turns from his seat in front of us and looks at me. He says: "but even I had to leave, and I had a pretty sweet deal, I mean I was allowed to do my thing, but I just couldn't cope with this insistence on… you know: Where are your patents? Where are your products? I tried to explain to them that, as physicists, we are hardly the last link in the food-chain of science. We are probably more like the first link". Kim does not see his job as one of making patents and products. His job, he says, is to develop theory that other people may later take up and put to use. After failing to convince his former employer that his publication in Nature deserved at least a small press release, Kim did like pretty much everyone else had done before him and left. I ask Kim and Robert where "everyone else" had gone to. Kim replies: "Well, as it is, most of them are sitting in this bus you know". He turns to Robert, together they start pointing around the bus towards middle aged men in sandals and wrinkled shirts. They discuss among themselves who left when, in which order and why. Each and every one of them respected for their work, some of them walking legends in theoretical physics, and most of them approaching retirement. One by one they all gave up working for this institution and its "hopeless" management and dropped into the interdisciplinary umbrella of the Nano-Science Center. This is the point at which I realize that I have been hanging out in a refugee camp.

According to the scientists represented in this thesis, something has changed in Danish universities. To be sure, the storyline differs from scientist to scientist, but most of them will make it clear that the university is not what it was. Life as a scientist has gotten harder, they tell me. Not all of them will make the above reference to the *insights to invoice* agenda, launched by the Danish Government in 2003, when accounting for the changes they experience. It has almost been a decade since the Danish Government launched a new university reform and published a now infamous report under the headline "New Paths between Research and Industry – From insights to invoice" (Danish government 2003) [3]. A lot of water has flowed under the bridge since then. Never the less, references to the neoliberal turn in Danish research policy keeps popping up in jokes, in speeches, in interviews, in conversations; usually accompanied by a more or less subtle expression of loss in relation to something that went before.

Choosing a *context* for one's inquiry is more or less synonymous with setting a problem. However, the idea of contexts can be taken more or less lightly. As should become clear in chapter three, I am not imagining that refugee physicists above had their actions and decisions determined by developments in Danish research policy. However, it would equally be inadequate not to give some kind of account of the change that scientists like Feidenhans´l and Lefmann have a habit of making reference to when talking about their careers. Consequently, this chapter will focus on the way in which the concept of *knowledge economy* entered Danish research policy and motivated specific changes in the way research is governed, managed and funded. I will start by giving a brief overview of some concrete initiatives launched in Danish research policy, only focusing on a few key aspects that field participants have related to and reflected upon. I will then suggest a connection to the concept of *the knowledge economy.*[4] To do this, I draw on publications from the Danish Government and also briefly cite an interview with one of the leading figures in Danish research policy over the last decade[5]. This account does not constitute a study of policy as such and is mainly to be

[3] The literal translation of the subtitle of this report would be "from thought to invoice" (fra tanke til faktura). (Danish Government 2003)

[4] In Danish used interchangeably with "knowledge society" (Videnssamfundet).

[5] I have limited my account to the last decade (1999 to 2009) because the changes perceived by the field participants trace back to mainly this period. However, as in many other countries, the foundation for these changes was laid already in the 1980ies. Due to a strong social democratic influence on research

read as an overview. It is beyond the scope and agenda of this thesis to present in detail the many legal and organizational changes made in the Danish university sector[6]. I will nevertheless engage in detail with the *structure of the arguments* made for changes in research policy to set up a point of reference for the following chapters. I will argue that the overall motive for the changes and reforms in the Danish university system over the last decade has been to promote more "interaction" between the university and its surroundings[7]. The push for university scientists to - as it will be phrased later - "leave their study chambers" and deal with real world problems is the main agenda which all of the chapters to come will relate to in some way. Adding to this account I will be making the argument that the "interaction agenda" echoes or even reproduces knowledge claims made in science and technology studies (STS) a decade earlier. This last reflection sets up another problem that runs through this thesis, the problem of studying practices that do not map onto a clear distinction between the conceptual and the empirical.

The management reform

In march 2002 the then minister of science, technology and innovation, Helge Sander, announced that he was going to introduce the most profound reform of the Danish university sector since the opening of the Copenhagen university in 1479 (Andersen 2006). The main inspiration for the subsequent management reform seems to come from a proposal from the Danish Council for Research Policy[8] who advise the minster of science, technology and innovation. The proposal, drafted in 1999, which constituted one of the first concrete proposals for implementing corporate management and industrial interests directly into the organization of Danish universities (Andersen 2006). The idea was to make universities more flexible, efficient and responsive to the needs of society by giving them a less bureaucratic management structure, introducing people from corporate and cultural life in top management, and steering knowledge

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policy, the neoliberal commercialization-wave hit Denmark much later than it did in for instance the United States where entrepreneurial science began to pick up speed already in the mid 1970ies (Shapin, 2008).

[6] For a more detailed historical account of these changes (and the debate surrounding them) I refer to work done in the anthropology of policy, namely the work of Wright and Shore (2011) Wright and Ørberg (2011) and Andersen (2006) and Christensen and Pallesen (2003). Apart from the ministerial publications cited in the following my account will be drawing on the above sources and will be limited to changes that have been addressed by field participants. As a consequence, several aspects of the university reform will not be addressed here. One example is the many fusions of smaller universities, concentrating two-thirds of Denmark's research and education on three major universities as this was not addressed or referred to by field participants.

[7] The Danish term used in government and policy publications is "samspil"- literally meaning "interplay". However I have here used the word "interaction" instead of interplay to convey the urge for taking action and connecting two previously separate parts indicated in the use of the word "samspil". Whenever the word interaction is used in the following citations it refers to this Danish term "samspil".

[8] Danmarks Forskningspolitiske Råd

production more directly in relation to the interests of society (Ibid. Danish Government 2002). The 1999 proposal was not well received by the left wing political parties and was also debated in the academic community (Andersen 2006). Especially the push towards increased commercialization and corporate management of public universities were at first rejected by Jan Trøjborg, who was minister for research in 1999. However, the management reform that was implemented in 2003 ended up being almost identical to the original proposal made by the Danish Research Council in 1999 (Ibid.).

The reform was indeed a profound change and constituted a strong signal that Denmark was now going to make knowledge a strategic parameter and turn it into a competitive advantage. First of all, the Danish Government increased the economic investment in knowledge production. In 2006 investment in research was set by the government to reach one percent of the gross national product (Danish Government 2006). In the anthropology of policy, Rebecca Boden and Susan Wright (2010) have studied some of the economic impacts of the university reform. The authors estimate that the total income of Danish universities has risen by 42 per cent between 2005 and 2009 alone[9]. Accompanying this increased investment was a complete change of the way university research were managed, governed and funded.

Firstly, the relation between the university and the state changed in 2003 as universities began a reform towards more corporatized organizations. The university legislation was changed to make Danish universities self-owning institutions. Also universities were managed in terms of a new “aim and frame steering” – a contract and out-based payment system (Danish Government 2002). Universities were now governed by “development contracts” with the state. The goal of this change in university governance was to ensure high quality research and give the university the flexibility to respond to its environment in the most relevant way. This goal was still a major focus area in 2006, when the government launched its “globalization strategy”, which for a large part involved initiatives to boost knowledge production:

> “Danish universities are in many ways demonstrating that they are capable of world-class research. They have engaged students and skilled researchers. But there are weaknesses too. The distribution of the basic funding for the university does not reward high quality. Also, there is no systematic evaluation of the quality and relevance of education for society. There is a need for further development of the universities where quality and relevance form the touchstone” (Danish Government 2006, p. 20).

[9] However, the authors argue, this extra income has been channeled mainly into increased administration costs (expenditure equaling 746 associate professorships) or it has been accumulated as liquidity for the universities themselves. Wright and Boden sum up: “Universities have built up an extremely strong position financially (…) Wealth accumulation enhances the autonomy of corporate institutions, but an alternative would be to spend more of this public money on core functions of teaching and research” (Boden and Wright 2010, p.11).

The management structure of Danish universities underwent a similar change. Whereas university management was previously elected from within the academic community, the new university law introduced governing boards that had to have a majority of external members. Also, the board is responsible for electing the chairman of the board as well as the vice chancellor (rektor). The members of the governing board should have the ability to: "contribute to the promotion of the strategic work of the university with their experience and insights into education, research, knowledge dissemination and knowledge transfer" (Danish Government 2002 p.5). The importance of management experience is emphasized and boards are specifically to include managers from public and private organizations as well as from cultural institutions in addition to representatives from the academic world (Ibid.). The goal of the university reform as a whole was:

> "… to strengthen university management and open more for people from the outside so that we ensure a closer *interaction* between universities and the surrounding society. In this way the universities can participate in the development of the knowledge society – thereby bringing growth and welfare to the whole of society (Danish Government 2002, my emphasis).

Another feature of the reform was a shift from a management structure based on elected leaders to a management structure of appointed leaders in a top-down system similar to the way management is appointed in corporate life. Before the reform, managers answered to and represented their colleagues "below", by whom they had been appointed. After the reform, managers answer to the people "above"; heads of departments to deans and to the vice chancellor, who in turn is accountable to the governing board and the minister (Danish Government 2002, Wright and Ørberg 2011).

Competition

A guiding assumption behind changes in the funding of Danish universities has been that increased competition will result in higher quality research. University funding was not to be given out solely on the basis of the size of universities but on the basis of performance related measures such as number and rating of publications, number of finished PhDs and the universities' ability to attract external funding (Danish Government 2002, 2006).

Also, the universities were to compete for funding to ensure a high quality. In 2006 the government set the goal that 50% of all public funding was to be distributed in free competition. Proposals were to be evaluated on the basis of *quality* and *relevance*. Also, the government stressed that Danish "knowledge intensive corporations" should participate in the competition for public research funding, so that both for-profit and non-profit research institutions had equal opportunities to competing for public research funding (Danish Government 2006, p. 20-25).

Strategic research, flexible research

Adding to the top-down management of Danish universities was the increased focus on strategic and politically directed research. To make sure that universities were producing the relevant kind of knowledge, numerous new investments in research were offered in free competition and organized in relation to strategic themes or new political agendas that were seen as pivotal to the growth and welfare of the nation (Danish Government 2006). The Ministry of Science, Technology and Innovation launched the first catalogue of prioritized areas for strategic research in 2008. The themes were: “energy, climate and environment”, “production and technology”, “health and prevention”, “innovation and competitiveness”, “knowledge and education” and “people and societal design” (Ministry of Science, Technology and Innovation 2008). The themes mainly addressed the natural and technical sciences but did not exclude the social sciences and humanities. The themes were mapped and organized in accordance with prioritizations made on the basis of an analysis by the Danish Agency for Science, Technology and Innovation, which is placed under the Danish Ministry of Science, Technology and Innovation and performs tasks relating to research and innovation policy and supervises the Danish scientific research councils. Also, the focus on flexibility and relevance had made available more possibilities to establish research centers and large-scale projects that related to debates or perceived problems that expressed or might become major concerns in Danish society. One example is a center for research on the cold war, which was initiated by mainly two political parties.

Commercialization

At the time of the university reform in 2003, the idea of knowledge dissemination was very much conceived of as a market-based process. Science-industry collaboration was launched as the most important strategy for ensuring that the increased investment in knowledge production would in fact bring growth to the economy and strengthen the competitive advantage of Denmark. Under the now so well-known headline of taking Danish research *“from insight to invoice”* (Danish Government 2003) a series of initiatives were launched in order to strengthen science-industry collaboration and the commercialization of research from public universities. The government stressed that these changes were not made in order for the university to create their own revenue on the basis of commercializeable innovations[10], but rather to strengthen the dissemination and transfer of knowledge from the university sector to the world of business (Danish Government 2003).

In 2006, three years after the launching of the "insights to invoice-agenda" the Danish Council for Research Policy[11] published the report "Better Commercialization of Public Research for the Benefit of Society (Danmarks Forskningspolitiske Råd 2006). The report stated that Danish universities still had a large proportion of "unused potential" when it came to commercialization of research (Ibid.). Looking at the high volume and quality of research that characterized Danish universities, the council concluded that a much higher "output" could be expected. "Output" is here conceived as "patents, collaborations with private companies, and corporate leaders' assessment of research collaborations as a useful endeavor" (Danmarks Forskningspolitiske Råd 2006, p.6).

In the period from 1998 to present a range of initiatives were launched to facilitate an increase in precisely this type of output. This development continues today. One important feature was the change of the patent law so that it was no longer the individual researcher but the university who owned patents. Also, university academics were now obliged to report if they made an invention that had potential for commercial value. Accordingly the infrastructure for knowledge and technology transfer was to be strengthened by introducing technology transfer units into the university. "Patent workers" would help researchers identify and organize possibilities for commercialization and industrial application for their research (Danish Government 2003. p. 58) Also, technology transfer was given its own national web portal to help facilitate the conversion of insights to invoices (Danmarks Forskningspolitiske Råd 2006). Existing university legislation was viewed by the government as an impediment to the privatization of publicly funded research and science-industry partnerships. As a consequence, university legislation was changed to allow public universities to create spin-off companies to make a profit from their patented inventions and to develop new innovations in collaboration with private corporations (Danish Government 2003).

Adding to these broad legislative changes, was a series of changes made in research funding. Starting in 1998, the government had launched "innovation environments" which were to offer advice to knowledge entrepreneurs and help facilitate licensed agreements across private and public sectors and to finance early stages of potential innovations to "mature" them into commercializable projects and start-ups.

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[10] This strategy was first launched as a mission statement but was quickly abandoned as unrealistic (Danmarks Forskningspolitiske Råd 2006)
[11] Danmarks Forskningspolitiske Råd

An increased proportion of the extra funding allocated to knowledge production was targeted to projects, and proposals and research activities that had commercial potential. In 2003, the main potential was seen to lie in biotechnology and IT but these initiatives were launched as an equal opportunity across all faculties (Danish Government 2003). Research councils were increasingly emphasizing direct involvement of industry in their assessment of applications. In particular, strategic research funding was framed around the importance of industry-collaboration. In order to convince the strategic research council that a research proposal really does create value for industry it may still a beneficial approach to account for the way in which industry could be directly involved in the project. So called “network grants”[12] were allocated to aid the facilitation of networks and collaboration platforms in addition to the research project.

Other initiatives to promote industry collaboration included industrial PhD projects where PhDs were partly financed by private corporations. Furthermore new three-party-financing of PhDs and other research projects were launched where industry funding constituted a third of the funding for a project. Many other initiatives were taken to facilitate industry co-funding so that public funding would be channeled to feed projects that had a direct usefulness for business. According to many of the field participants, the relevance and contribution of research proposals were increasingly evaluated in terms of possible industry partnerships.

This very intense focus on commercialization that characterized policy changes up to 2006 seems to have been broadened somewhat in recent years to focus more generally on “societal relevance” as more than industrial development alone. For example research in obesity, prevention of life-style diseases and healthcare for the growing population of elderly citizens have been major focus points for strategic research in recent years (Ministry of Science, Technology and Innovation 2008).

Overall, the series of policy changes taking place from the end of the 1990s until 2009 emphasized the introduction of “outside” interests in all aspects of the Danish university sector. The perception was that the university should not stand outside society and should be infused with a sense of context. Changes and reforms were mainly introduced to ensure that university research was equipped to better accommodate and respond to the future needs of society. As I will argue below, this “opening up” of Danish universities to “people from the outside” was a specific effect of a policy context which was becoming increasingly focused on transforming Denmark into a *knowledge economy* or *knowledge-based society.* Interestingly, the introduction of the concept of *knowledge economy* into Danish research policy seems to be a consequence of putting the nation into a specific context, that of globalization.

[12] Netværksbevillinger

Denmark as a knowledge-based economy

The Danish mission to become a leading *knowledge economy* is often argued as a strategy of countering the threat of globalization. How will Denmark manage to keep up in the face of globalized competition? The answer, it seems, was knowledge. In 2006 the Danish Government published their strategy on how to make the best of globalization titled *Progress, Renewal and Comfort.* The mission statement clearly invokes knowledge as part of the solution to the problem of global competition*:*

> We need to ensure that Denmark has the power to compete so we will remain among the richest countries in the world (…). The knowledge, ideas and work of people are the key to use the possibilities given to us by globalization. Therefore Denmark needs to have a world-class educational system. We need to be a leading knowledge-society with research on the highest international level" (The Danish Government 2006, p. 4-5).

The concepts of knowledge society or knowledge-based economy have been used interchangeably in a European context and date back to the mid 1990s[13]. To begin with, this term was one of many buzzwords connected to what was called the *new economy* and evolved to its current use as a general term after 1995 (Godin 2006). The specific concepts of *knowledge-based economy*, or simply *knowledge economy* solidified into a general policy concept in no small part due to the OECD, who systematically developed indicators to measure the success rate of these kinds of economies in terms of how knowledge was produced, disseminated and integrated in national economies (Godin 2006, Wright and Ørberg 2011). In contemporary Europe, *knowledge economy* is thus a concept that to a great extent has been defined and promoted by the OECD (Ibid.). Like many other European countries, Denmark has taken up the OECD focus on knowledge production as a path to growth and value creation as a way to boost or strengthen a traditional industrial and agricultural production system.

When pointing to this increased focus on value creation, most field participants will mention Helge Sander, who was the Minister of Science between 2001 and 2010, as one of the driving forces behind this shift in rhetoric. Also, the Deputy Director General in the Danish Agency for Science, Technology and Innovation, Hans Müller Petersen has been mentioned by field participants. Müller Petersen has had a long career in policy and has been closely involved in the last decade of reforms. Before becoming Deputy Director General in 2006 he served as the head of administration in the department under the Ministry of Science, Technology and Innovation for many

[13] According to Godin (2006), the concept first emerged in the United States in the 1960s and 70s along with ideas of an information economy. However, as a more uniform buzzword used by policymakers, Godin investigates its resurrection in Europe in the 1990s and identifies the OECD as the driving force behind the now widely used spread of the term.

years. Field participants refer to him as one of the leading architects behind the "insights to invoice" agenda. However, Müller Petersen does not see himself as a driving force and points out that policy processes are not carried by key individuals like the minister or himself. However, he recognizes that the now infamous, catchphrase, originally conceived by the Helge Sander, has had large (perhaps too large) impact in the debate although it constitutes only a minor part of the overall motive for changing the way Danish universities are funded, governed and managed. Rather than a key figure "behind" policy changes, I am here drawing on the reflections of Müller Petersen because he is extremely well-informed about the series of changes to the Danish university sector over the last decade. To better understand the motivations and arguments behind the changes accounted for above, I will draw on examples given by Müller Petersen in an interview conducted in 2009[14]. His reflections are good examples of the kind of rhetoric and arguments used in Danish research policy. From there I will branch out to look at the conceptual resources that his arguments draw on.

According to Müller Petersen, the growing political recognition of the need to make a change culminated in 2001, when Denmark changed from a left wing to a right wing government. At the turn of the millennium it was becoming clear to both politicians and policy makers that Denmark needed to move "up" to a new level of production:

> "Yes, it was simply getting very clear that we lacked *interaction*[15]. There was a demand for making sure that knowledge was used and applied. (…) If you are to improve your competitive advantage (…) you either reduce costs like wages or you shift up to a different "gear" or level of production, that is, you place yourself differently in the value chain. And there is no doubt the growth is in the *end* of the value chain where knowledge is put into the product. The more advanced our high tech production gets, the larger the increase in value. So it's been about transforming Danish industries from an industrial production and getting it up on a level where you use knowledge and advanced technology as a strategic

[14] As I have conducted only a single interview with Hans Müller Petersen all citations refer to the recorded and transcribed interview done in the summer of 2009. As with all other interviews in this thesis, it was conducted in Danish and citations have been translated into English by me. I would like to stress that the data presented here are not considered sufficient for an in-depth account of the history of Danish research policy. This thesis is a study of scientists, not of research policy and as a consequence I have not taken the time to study the formation of the interaction agenda in detail through interviews and document studies. However, as I will argue later, science and research policy are co-existing species in a habitat and therefore affect each other in ways not all predictable or linear (see also Shore and Wright 1997 and Wright and Shore 2011). As will become clear in chapter three, studying Müller Petersen as a field participant would have required much more in-depth field work than what I have done here, this would definitely include a more elaborate account of conceptual and theoretical resources behind his statements, thus addressing him as a *thinker* as much as a *practitioner*. However, for the contextualizing purpose of this chapter, I will merely point to the structure of the argument made in policy documents, governmental reports and in this interview.

[15] In line with the governmental reports and policy documents, Müller Petersen is here using the Danish term "samspil" ("interplay").

parameter in production. People talk about transforming the Danish society from an industrial society to a knowledge society. There might not be a very clear idea of what that might exactly meant, but it definitely meant a much more well-considered and clear investment in knowledge as a productive factor."
(Interview 2009)

Setting the problem of knowledge economy seems to be an effect of the way in which policymakers, politicians and industry representatives perceived the global economic context. At a time where Denmark was actually doing rather well, not just financially but also in terms of international assessments of the quality of research and higher education there was nevertheless a growing concern that an increasingly globalized market would become a threat to nation. The solution, as Müller Petersen explains it, was to "put in" more knowledge in the existing production; to "shift to a higher gear" (Interview 2009). In the world of research policy it seemed evident that Denmark needed to make a leap from the beginning to the end of the value chain. In order to do this, knowledge was seen to be the most important resource. In 2003 the market was thought of as the most efficient disseminator of knowledge into society. The previously mentioned report published by the Danish Government in 2003, "New paths between research and industry – from insights to invoice" the Danish Government stated:

"The Danish conditions for growth need to be strengthened. A strengthened *interaction* between the industry and knowledge institutions will contribute to increased growth in Denmark. It is the goal of the government that Denmark be able to measure itself against the best in the world when it comes to *interaction* between industry and knowledge institutions. This goal is ambitious. There is no doubt that Denmark is facing great challenges, if this goal is to be achieved. Denmark is today in the middle range of the OECD countries when it comes to *interaction* between industry and knowledge institutions. We are not good enough at ensuring that industry knowledge and perspectives are mirrored in research and education. Denmark needs to establish new and better pathways between education, research and industry.
(Danish Government 2003 p.5, my emphasis)

Echoing the OECD

As shown in the citation above, the identification by the government in 2003 (and onwards) of a lack of *interaction* echoes the way the OECD set the problem of the knowledge-based economy. Benoît Godin, who has written extensively on science policy and its relationship to evaluations and science indicators, has traced the European trend of measuring and optimizing and focusing on the *use* of knowledge back to the formation of OECD indicators for measuring knowledge-based economies. Theories of national systems of innovation functioned as a predecessor for the more general concept of knowledge economy (Godin 2006). Godin argues that it was the development of these statistical indicators which in itself managed to solidify this concept as a tool for policy makers[16]. Both Müller Petersen and the Danish Government seem to repeat the way in which the OECD initially set the problem, putting the nation into a global context by way of comparing OECD statistics across member countries. A decade before the Danish Government launched the mission to increase *interaction* between the producers and the consumers of knowledge the author of the OECD Oslo manual of innovation stated:

> "the overall innovation performance of an economy depends not so much on how specific formal institutions (firms, research institutes, universities etc.) perform but on how they interact with each other"
> (Smith 1995 cited in Godin 2006, p.19).

According to Godin, OECD had worked for more than 10 years on identifying and developing indicators to measure the effectiveness of how economies made use of and integrated knowledge in their production system. The statistics launched by OECD created a ranking system of countries relative to their performance as knowledge economies. In this ranking, Denmark figured only as "middle range". Knowledge was consequently framed as a resource that was to be utilized much more efficiently, if Denmark was to have a chance in the global marketplace. Within statistical parameters measuring the effectiveness of knowledge economy it is no surprise that the conclusion Denmark arrives at is that the nation has to change into such a knowledge economy in order to retain its position and competitive advantage.

In the field of anthropology of policy, anthropologist Susan Wright and Jacob Williams Ørberg have pointed to the defining impacts that the OECD concept of knowledge economy has had not only on the Danish university reform but also in other OECD countries:

> "Some ministries, as in Denmark, generate much local publicity about their country's standing in OECD league tables, using them to name and shame and urge further reform so as to win the race to meet the OECD's vision of a success in the global knowledge economy. A combination of more pressure, shame and desire to beat the competition impels countries towards policy convergence. (Wright and Ørberg 2011 p. 3).

[16] Godin points to the writings of Lundvall and Johnson as leading figures behind the development of the "national system of innovation" approach which seems to have functioned as a predecessor for the concept of the *knowledge-based economy* in OECD. According to Lundvall and Johnson the problem was constituted not by a lack of knowledge but a lack of ability to make use of it (Godin 2006, p.19). The same argument is made throughout the interview with Müller Petersen.

The authors conceptualize the OECD as an "ambiguous policy zone" acting as both a forum for exchange of information among its members as well as a "policy actor" that frames policy in ways that make it conform to an ideology that is best described as neoliberal[17]. However, Denmark seems to have been a leading figure rather than merely a compliant member state:

> "Indeed, when we asked an official in the Ministry of Science, Technology and Innovation if the OECD had influenced the Danish university reform, he replied, no, we shaped OECD policy." (Wright and Ørberg 2011, p.2).

The problem of transforming Denmark into a knowledge society was thus set by two contextualizing moves, both closely related to the OECD concept of *knowledge-based economies*. Firstly, a globalized market was invoked as a driving force for the turn to knowledge as a resource and a route to saving the economy. Second, OECD statistics played an important role in setting the problem as one of use and dissemination of knowledge. Even though Denmark did well in terms of knowledge *production*, the new indicators developed by the OECD brought issues of knowledge *dissemination* to the forefront. Müller Petersen explains:

> In the period from 2001 the university legislation has been changed so that a third dimension was introduced in the mission statement of the universities. Whereas earlier they were to focus on research and teaching, they now have to do research, teach *and* disseminate[18] knowledge, actively contribute to disseminating the knowledge they possess out into society and to industry (Interview 2009).

At the beginning of the new millennium, the Danish Ministry of Science, Technology and Innovation saw knowledge dissemination mostly as a market-based process[19]. The government articulated the problem of making use of knowledge and integrating institutions in the form of a pressing need for increased interaction between university and industry. The general perception of politicians and policy makers seemed to be that the Danish university sector needed to become more efficient and adaptable to the changing needs of society and the increased global competition threatening industry.

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[17] The authors draw on the Henry et.al. for the concept of the "ambiguous policy zone" where the "report genre" allows normative agendas to transmute into factual description and from there into an inevitable process which in turn takes the form of a "new reality" which policy makers make decisions in relation to (Wright and Ørberg 2011).

[18] Muller Petersen uses the Danish term "sprede viden" og "vidensspredning" throughout the interview, literally meaning the "spreading of knowledge" or "knowledge spreading". Diffusion may be another translation; here I have chosen to stay with dissemination.

[19] See especially The Danish Government 2003. This agenda conformed to basic neoliberal tenets and was in line with the shift to a right wing government. According to Lave et al. (2010), one of the defining characteristics of neoliberal science policy is the idea of the market as the most efficient disseminator of knowledge. According to basic neoliberal tenets, "every successful economy is a knowledge economy" (Lave et al. 2002, p.40)

Both the volume and quality of knowledge production was seen as in need of a boost. Against this backdrop, a specific type of argumentative structure emerged: an argument in which transition from an old to a new system becomes the key figure.

The step1/step2 narrative

In many ways, the arguments for making changes in the university sector and the system of research funding have been articulated in form of a step1/step2 narrative[20]; juxtaposition between an old and a new way of producing knowledge.

Hans Müller Petersen argues that the shift that took place in Danish research policy from the turn of the millennium and onwards was necessary in order to accommodate the development that took place in the way in which society makes use of knowledge. This argument is made by reference to statistics showing how the distribution between public and private investment in science has been reversed. In the 1970s and 1980s public funding was predominant – now, private funding is twice as big as public funding. Müller Petersen points to solid statistical evidence when making this argument and says that this reversal happened as an effect of changes in society that expressed a growing need for knowledge as an intrinsic factor in production. Thus: ”The public sector no longer has a monopoly on the production of knowledge” (Interview 2009). In light of this transition, Müller Petersen finds it paradoxical that debates about research policy still revolve mainly around the distribution of public funding. If the private sector is now producing “twice as much knowledge” as the public sector we should realize that times have changed and draw the necessary conclusions. Feidenhans´l and the other runaway physicists we met in the bus are among the less radical voices in this debate. From the social sciences and humanities the focus on industry collaboration has been a topic of heated debate[21]. Yet, Müller Petersen sees this response as an inevitable part of a transition from step 1 to step 2:

> “I think that in the minds of many researchers this development has not really entered their heads yet. Some still seem to think that research is alone this very ceremonial thing of finding the objective knowledge, truth. That finding objective knowledge is an activity that has legitimacy in and of itself. And to some extent still is. But within the recent development in how societies make use of knowledge, more dimensions have been introduced. There is a higher demand for

[20] The identification of a “step1/step2” narrative as a dominant figure in debates about the knowledge economy is inspired by Mirowski and Sent, 2008. I will return to these authors again in chapter 2.

[21] Contemporary expressions of this debate can be found in a blog launched by academic researchers from several faculties in response to the changes in university management. The blog is named “freedom of research?” (forskningsfrihed?), contributors are mainly from the social sciences and humanities but also include names from health and life science faculties. See also Vedel and Gad (2011) for a presentation of the way Danish academics have posed critique against the intensified intermingling between university and industry.

knowledge throughout. Knowledge is not just produced today in order to find truth, it is also produced to be used. This means that as a researcher you have to get used to stepping out of your study chamber and enter some more dynamic relations with those who are consumers and providers of knowledge outside the university. This is a basic requirement for being a researcher today." (Interview 2009).

The structure of Müller Petersen's argument is one of transition: A move away from a past (step 1) where knowledge production took place in public universities to a present (step2) where knowledge production is distributed across the public and private sector and a broad array of actors take part in knowledge production. Consequently, the secluded offices of the public university are no longer the best vantage point for knowledge production.

When presented with the debate and occasional lamentation expressed by the present generation of university academics in response to these changes, Müller Petersen argues that this is a passing phenomenon, a fading symptom of the transition we have already been through:

> "In this process there is a loss of authority and perhaps also of autonomy. And I think that some may still have problems getting used to this (…) I think that in 5-10 years we will not have the same type of discussion, because the young, the PhDs today, they will not sit in the same way and isolate themselves and wait for a stroke of genius to carry them through their career. What will carry you through your career as a researcher today is the size and scope of your network and your ability to position yourself within this network. For this your knowledge has to be used and applied (…). I think that the discussions we are experiencing in relation to research policy now is the last paroxysm or a residue of the transition from what is called the *mode 1* of knowledge production to the *mode 2* of knowledge production. And that´s ok, it is after all exciting to be part of such times of transition." (Interview 2009).

Objections over loss of autonomy ("they take your freedom away") or authority ("where are your patents, where are your products") are here conceptualized as a sign of transition. In the knowledge-based society that Denmark is about to become the epistemic authority and independence of university scholars stand out as a thing of the past, the last paroxysms from a dying or at least obsolete system of knowledge production. However, the fact that we have entered a new *mode of knowledge production* does not mean that the old way of doing things is completely obsolete, says Müller Petersen:

> "So what is important in this context is to not just cast away the qualities in the traditional thinking of what research is and should be, but to build on it, to take

the best of this with us forward. Of course, there are qualities in the old mode of doing research too." (Interview 2009)

When asked what constitutes the new challenge of the Danish university system, Müller Petersen points to "balance" as the main problem. Recognizing that the "screaming and shouting" of industry representatives back at the end of the 1990s has now been accompanied with a "cry" from the other side – the academics. The symptom of imbalance is expressed in the fact that now:

> " .. it is the researchers that are screaming and shouting, that they are crying out: our space for maneuvering is getting more restricted (…) we cannot unfold our potential. That of course means that the right kind of balance has to be found (…). I think it will be one of the future challenges of the universities to find that balance." (Interview 2009).

According to Müller Petersen, the problem of balance is something that mainly the university has to solve because the Ministry and the Agency of Science, Technology and Innovation cannot legislate about this problem. "What we do in here is create the framework for things to happen" he says:

> "The question is, can we legislate about good and bad management? Should we write into the law that the management of universities should be good? You can't! It´s impossible. It´s about increasing the quality of the management we have in the university so that they can now find these balances where it is both possible for researchers to fully exercise their talent[22] and for knowledge to be disseminated". (Interview 2009)

Echoing *The New Production of Knowledge*

If the Danish Government's mission to transform the nation into a knowledge society resembles concepts and indicators appearing in OECD documents a decade earlier, the statements made by Müller Petersen resonate with yet another mid-1990s text. *"The New Production of Knowledge: The dynamics of science and research in contemporary societies"* was first published in 1994 by Michael Gibbons, Camille, Limoges, Helga Nowotny, Simon Schwartzman, Peter Scott and Michael Trow and is by now a well-cited work in STS (Gibbons, et al. 1994). In particular, the concept of *mode 2 knowledge* production has had much impact in science studies and even more so

[22] In Danish: "udfolde sig", literally meaning to "fold out" but is here used to signify a process where researchers get to flap their wings and fly as high as they like or are given the opportunity to freely unleash their potential. To not "fold out" yourself is analogous to be put in a box or have restrained room for maneuvering.

in the literature on science policy (Hessels and van Lente 2008, Godin 1998). Like the concept of the knowledge-based economy, the idea of a transition from mode 1 to mode 2 knowledge production became a well known conceptual distinction toward end of the 1990s[23]. Comparing the statements made by Müller Petersen to those contained in this 1994 publication, it seems that the conceptual repertoire of the book has now solidified into historical fact. In 1994 Gibbons et al. wrote:

> "The production of knowledge is advancing into a new phase. It operates according to new imperatives in tension with the traditional way of doing things with far-reaching implications. These changes are described in this book in terms of a shift in emphasis from a mode 1 to a mode 2."
> (Gibbons et. al 1994 p.19)

Müller Petersen has no need to cite the authors of this work as he readily reproduces its core arguments and conceptual vocabulary as common knowledge[24]. For

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[23] In 2007 the book had received over 1000 citations in academic journals and the number still increases (Hessels and Van Lente, 2008). Shinn (2002) investigates the impressive impacts of *The New Production of Knowledge* from its publication in 1994 and detects a marked increase in citations around the end 1990ies: "For the period of 1995 to June 1999 a total of 98 references were made to the book (…) The number of references for the first six months of 1999 already equals those for 1998; the total (not including self-citations) from 1995 to July 2002 is 266 citations" (Shinn 2002, p. 601). Especially education reviews are abundant in citations but citations are also found in the field of science policy, sociology of science and technology, psychology, sociology and social psychology. Citations are mainly found in Western Europe but also the United States and Canada. The concept of Mode 2 knowledge production has been found in policy documents in Europe, Canada and the United States (Hessels and van Lente, 2008; Godin,1998). Interestingly, a very steep increase in citations occurs around 1999 and rapidly climbing upwards from there to now. This coincides with the period in which the push for a radical reform of Danish university management and the need to embrace industrial interests began to pick up speed (Andersen, 2006). Today a Google search on the full title of the book will give 111,000 results while keywords like "mode 2 knowledge" will give 22,400,000 hits.

[24] This form of implicit citation is a classic within the constructivist strand of science and technology studies. Deleting, dropping or transforming the "modalities" attached to a knowledge claim removes contextual information that could undermine the *facticity* of a claim (Latour and Woolgar 1986, p. 75-86). It is important to notice that pointing to the literary process of transforming highly context-dependent and contestable claims into objective facts is not implying that such facts are then biased, false or should have been established by better means. Dropping modalities is how facts may come into being. In the constructionist world of symmetry (Callon 1986), objectivity is *made*, not found, a process that requires a breathtaking number of actions and literary inscription and fails more often than it succeeds (Latour 1987). In this particular example it is worth noticing the striking similarity between the way Müller Petersen grounds his perception of change in the fact that there has been an increase in private relative to public research funding over the last 30 years and the evidence for this same development presented in *The New Production of Knowledge*. The book cites colleagues in science and technology studies in order to make a similar argument (Gibbons et al. p. 50). The conclusion is strikingly similar: Knowledge production has changed and is now distributed on a wide range of actors; consequently the publicly funded "free" university is no longer the main site. It is not clear from the interview data whether Müller Petersen is in fact citing the 1994 book directly or whether he is referring to similar statistics provided elsewhere. The overall point here is not to trace the citation, but to point to a similarity in the structure of

readers who do are not (yet) familiar with the terminology of *The New Production of Knowledge,* I will here add that knowledge production *mode 1* is characterized by the authors as homogenous, “disciplinary”, “hierarchical”, “conservative” and carried out in the context of a purely academic community (Gibbons et al. p. 3). In contrast, *mode 2* of knowledge production is “transdisciplinary”, “heterogeneous”, “heterarchical”, “transient” and carried out in the context of application (Ibid.). The authors argue a) that knowledge production is a less and less self-contained activity, b) that both its theories and its techniques have spread beyond academia, c) that science is no longer a special type of institution which functions as a first link in the food chain “spilling over” knowledge to other sectors, d) that knowledge production is socially distributed, e) that it takes place in a global rather than a local context and f) in an expanding number of sites (Gibbons et al. 1994 chapter 7).

The 2009 interview with Hans Müller Petersen shows an almost perfect reproduction of the core argument in the *New Production of Knowledge*: it is a narrative of a radical break with past modes of knowledge production, a time of transition from step1 to step2. Of course, *The New Production of Knowledge* does go into more detail than Müller Petersen can allow himself in an interview situation. According to the book, the changes in the way societies make use of knowledge are an effect of, among other things, changes in the density of communication and the availability and advancement of technology into the everyday life of citizens. This increased density of communication between science and society is an indicator of the increased diffusion of knowledge[25]. Arguing that the sites in which knowledge production occurs are multiplying the authors claim:

> “deep seated structural changes are taking place in the relationship both within the scientific communities and society at large, with knowledge becoming socially distributed to ever wider segments of society” (Ibid, p.34).

Consequently:

> “The university must enlarge its view of its role in knowledge production from that of being a monopoly supplier to becoming a partner in both national and international contexts” (Gibbons et al. 1994, p.156).

If this was indeed the situation back in 1994, contemporary academics should now indeed find themselves in a situation where the quest for truth and objective knowledge

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the arguments made by the two sources: an increase in private funding means that knowledge production has changes site and entered into a new and more interactionist mode.

[25] See Gibbons et al. chapter 1, the same argument is made by the Danish Government’s strategy for Denmark in the global economy (2006) “globalization means that the economic, cultural and political connections across national borders are becoming more dense. We trade and communicate with almost all countries”. (Danish Government 2006, p.4).

is no longer "a legitimate activity in itself". The claim is that too many actors are involved to keep authority on the hands of academic scientists alone. Against this background it is no wonder that Müller Petersen identifies protests against the demand for direct societal (industrial) relevance of research from the academic community as a last paroxysm. Reproducing the argument from *The New Production of Knowledge* allows Müller Petersen to refer to the transition from mode 1 to mode 2 knowledge production as historical fact. This is done with the same ease and naturalness as one would refer to historical events like the French revolution or the fall of the Berlin wall. Both these accounts present the step1/step2 transition as an expression of progress. *The New Production of Knowledge*, though claiming to have a not normative agenda, identifies *mode 2* as a new way of doing science where the producers of knowledge work closely with the consumers of knowledge, thereby making it "more socially accountable and reflexive" than its predecessor (Gibbons et al. p. 3). *Mode 2* knowledge production is also identified as taking place in "those areas which currently define the frontier" of knowledge production and is to be found mainly among "intellectual leaders" (Gibbons et al. 1994 p. 1) [26]. In 2009 this narrative of progress seems to have hardened into a repertoire that allows Hans Müller Petersen to frame the collapse of institutional boundaries and scientific authority as a path to improvement. Both identify an increasing need for researchers who are ready to "step out of their study chamber" and transgress the traditional boundaries of the university; to become the *mode2* scientist.

The step 1/step2 narrative told by Müller Petersen thus bears strong resonances with the narrative of a shift between mode 1 and 2 knowledge production. It is a description of transition from the old to the new, although it is not to be understood *entirely* as an abandonment of the old for the new. Debate, according to Müller Petersen, is also a sign that the world of research has not yet found the right "balance" and this constitutes a challenge for research management. Reconciling or using the "best of both worlds" seems to be the way forward, although it is not clear how this should be done[27].

[26] Jensen 2010 similarly observes the progress-narrative embedded in the mode1/mode2 distinction (2010 p. 43 ff).

[27] See Wright and Ørberg for an interesting analysis how similar "best of both worlds" reconciliations are made in OECD reports. Using Hall´s notion of the "double shuffle", Wright and Ørberg argue that the contrast between an old and a new type of university as depicted in OECD documents (which bears striking resemblance but not direct reference to the mode1/mode2 distinction) is used as a political strategy. By making a "double-address" of both the old and the new way of managing and governing universities the transition narrative works to soothe and gain support from groups who favor the older/traditional value system while simultaneously pushing an ever more radical neoliberal agenda. The rhetorical strategy in OECD reports is characterized by a complete lack of description of or consideration for *how* a combination of the old and the new is to be achieved. If read through Wright and Ørberg, Müller Petersen's delegation of the "balance problem" to the university would exactly constitute such a double-shuffle move of arguing for the co-existence of traditional and future systems of knowledge production without ever suturing them together: "In the OECD documents we have seen a double shuffle being danced, where at each step the lead discourse takes in the direction of the market-driven university,

Performativity

In the previous sections, I have shown that concepts and arguments invented in the mid-1990is by the OECD and *The New Production of Knowledge* are echoed in the rhetoric and argumentative structure of Danish Government publications as well as in statements made by one of the leading architects behind Danish research policy. What makes the OECD concept of knowledge economy and *The New Production of Knowledge* mode1/mode2 terminology interesting as a part of the context for this thesis is not so much the claims that these texts make on reality or their status as theory in the field of science and technology studies, to which this thesis aims to contribute. Rather, what makes them interesting is the way these claims have been *iterated* or taken up in the Danish world of research policy providing the arguments for the most fundamental reform ever seen in the history of Danish university research.

Donald MacKenzie (2001, 2007) has summarized the last three decades of science and technology studies as being the discovery of the self-propelling or *performative* qualities of knowledge claims and factual accounts (MacKenzie 2001, p. 127). The performativity of a statement lies in its ability to bring into being the very thing that it names, creating its own practices and realities and making them real (Ibid.). Indeed science and technology studies (STS) has become a vast resource of tools for analyzing the self-referential loops of knowledge claims and tracing how theoretical constructs, categories and knowledge claims sometimes acquire the power to create the world in their own image[28]. At the face of it, the relation between theoretical concepts and

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it mobilizes support through an appeal to traditional aspects of the university. As the lead discourse advances, the supporting discourse is gradually transformed in the same direction, but always looks more traditional than its lead partner." (Wright and Ørberg 2011, p. 13). Clearly the step1/step2 narrative has strong argumentative powers not just for its solidifying and historicizing effects but also for its effectiveness as a political strategy.

[28] Often drawing on Austinian ideas and concepts, science and technology studies scholars have been preoccupied with the tendency for categories to bring about the phenomena they describe. An iconic example of a performative statement is "I now declare you husband and wife". In this instance the statement accomplishes the social act it describes through being uttered. Hence Austin's (1962) mantra 'doing things with words'. Performative statements are typically associated with what Nelson Goodman (1978) termed 'human kinds', that is, referents whose existence has particular kinds of social relations as their necessary conditions (e.g. 'marriage' requires particular kinds of kinship and gender relations in order to exist as an intelligible kind of thing in the world) as oppose to 'natural kinds' which are presumed not to require human social relations as necessary conditions for their existence. The relationship between human kinds and performative utterances is self-grounding such that each utterances effectively 'hardens' the putative existence of the referent human kinds, a process which Ian Hacking calls 'looping'. This "looping" effect of human categories has been documented in Hacking (2006) and Bowker and Star (1999). A key move in STS has been to extend the idea of looping to natural kinds. For example, Pickering (1995) draws heavily on the performative idiom as a basic ontological precondition in his study of physics. What we call a natural kind such as a 'quark', for instance, gains its status as such through performative acts embedded in scientific representational practices. The performativity of representation has also been discussed at length in the area of finance and economic theory by Michel Callon (1998) and Donald MacKenzie (2001, 2007). Another area which seems important in relation to the performativity of knowledge claims is the sociology of expectation (Brown, Rappert and Webster 2000, Horst 2007) where the generation of expectations in a social field has been documented to bring

concrete changes in Danish research policy indeed seems constitute such a performative loop. Thus, what is here interesting about the concept of *knowledge economy* and the idea of the transition from *mode1/mode2* is not their theoretical contribution to STS literature but their status as part of the empirical context of this thesis. In this particular context, their impressive ability for self-grounding is what constitutes their relevance.

Both the concept of the knowledge economy and the concept of mode 2 knowledge production started out more or less as manifestos or vague conceptual constructs which had “no world”. But in light of the changes taking place in Danish research policy these claims certainly seem to have acquired one. In other words, these texts seem to be *performative* statements; ideas that generate their own practices so as to create the world they describe (MacKenzie 2001, 2007). Benoît Godin has meticulously shown how this is indeed the case for the OECD concept of *knowledge-based economy*: “There are at least two kinds of relationships between statistics and concepts. In one, statistics give rise to and define a concept. (...). In the other kind (…) a concept gives rise to specific statistics. This was the case for the knowledge-based economy.” (Godin 2006, p.17).

In this way, both the idea of a *knowledge economy* and that of *Mode 2*are classic examples of a performative concept. The willingness of the Danish government to align the university sector with the OECD identification of a need to ensure that knowledge is *used* by creating interaction between the university and other sectors is striking. Also, this alignment of logics is identifiable in the series of changes in the Danish university sector outlined above. In all of the reforms and new funding initiatives, interaction and integration seem to be the main agenda.

Similarly, Godin (1998) has pointed to the performative abilities of the *mode 2* concept, showing how its adoption in policy has cemented the historical step1/step2 narrative and traced its diffusion into policy in the United States, Canada, France and England[29]. In light of the statements made by Müller Petersen in 2009 it seems obvious that the step1/step2 narrative of *the new production of knowledge* exhibits similar self-propelling or performative abilities. The mode1/mode2 distinction seems to have been a rather vague knowledge claim in 1994. Terry Shinn, who has done extensive research on science, education and science-industry relations, points to the almost complete lack of empirical data behind the conceptual framework of *The New Production of Knowledge:*

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about phenomena fitted to the expectation. For a nice review of the performativity literature in STS see Boll (2011). See Butler (2010) for a thorough examination of the Austinian notion of performativity and its relation to the writings of Callon and MacKenzie.

[29] See Jensen (2010) and Godin (1998) for similar performative readings of the mode 2 concept.

> "*The New Production of Knowledge* raises few questions about the evolution of science and technology, or about changes in the relations with enterprise and society. Instead, it offers a number of prefabricated indications about where science has putatively come from and where it is allegedly going. No questions, but lots of answers. On a parallel plane, almost no concrete evidence is given for the assertions advanced; and no provision for future is made for future empirical historical and sociological work. While the absence of data in the book is distressing, people interested in this approach and desiring to explore its possibilities might hope for precise information in subsequent works. However, this hope has not so far been realized (Shinn 2002, p. 603).

If empirical data is part of what constitutes the "world" of a statement in science and technology studies, *The New Production of Knowledge* clearly had none when it was first published. Furthermore, its knowledge claims have been questioned repeatedly by colleagues in the field[30]. Rather than constituting a body of research *The New Production of Knowledge* simply summarized a set of trends spotted by the authors. These trends are, of course, exemplified in the book by way of a selective set of cases drawn from science and technology studies and innovation studies but there is no historical evidence for the step1/step2 model. This feature gives the book a manifesto-like property that easily lends itself to normative interpretations (Shinn 2002, Godin 1998). Although the mode1/mode2 narrative is in no way composed to be the same kind of political and normative policy concept as that of the OECD *knowledge-based economy,* the mode1/mode2 transition narrative seems to have had a massive impact in research policy.

The last chapter of *The New Production of Knowledge* specifically addresses policy makers and points to future issues and likely evolutions of underlying trends. Especially interesting in the Danish context is the authors' identification of a need for a new management style "which can cope with permeable boundaries between institutions and other features of Mode 2 knowledge production" (Gibbons et al. 1994, p. 155). The authors identify the need of a change in the way we think about policy and address the need to change the structure of the university. One of the visions for the future university in 1994 was:

> "Large university-based institutes with tenured faculty, or government laboratories for fulfilling specific functions as well as permanent research units with tenured research staff set up for monocultural research will not be the policy

[30] See Hessels and van Lente (2008) for an elaborate analysis of the empirical basis, conceptual strength and political value of the mode 2 concept. Etzkowitz and Leydelsdorff (2000) similarly question the empirical base of the argument made by Gibbons et al., especially the assumptions about the existence of a mode 1 knowledge productions prior to the rise of mode 2 is criticized for being an unsubstantial claim.

> models of the future. Such organizations have become too expensive and inflexible to meet the needs of distributed knowledge production. An alternative model might involve the creation of lean "centres" employing a few administrators with a budget to stimulate networks of innovators in units attached to diverse institutions, agencies or firms. They would be periodically evaluated in terms of their effectiveness in process management. When their jobs were completed, or when decreasing returns became evident, they would be disbanded. These centers, like other institutions, created in the context of socially distributed knowledge production are likely to have many stakeholders and will need to be run and evaluated accordingly. Any policy that tended to entrench institutions, or encourage autarkic attitudes, is anachronistic." (Gibbons et al. 1994 p.161-162)

The rendering obsolete of a previous "entrenched" institutional order with "autarkic" tendencies has indeed been questioned within the field of science and technology studies. Etzkowitz and Leydelsdorff (2000), whom the authors quote extensively throughout the book, have contested the historical accuracy of the step1/step2 narrative and commented on the obvious lack of empirical data to accompany the statement. "Where have these ideas, of scientists as the isolated individual and science separated from the interests of society come from?" (Etzkowitz and Leydelsdorff 2000, p. 116). According to the authors, Mode 1 is a construct, a fictional background for an argument that has no basis in historical fact. When placed on such a "construct" or baseline, mode 2 indeed stands out as new and progressive. *The New Production of Knowledge* seems to acquire its strong appeal to the world of policy exactly by this rhetorical strategy of contrasting the two as developmental stages. Indeed, the "future" of research policy as speculatively painted in *The New Production of Knowledge* in 1994 seems to have been incarnated into changes made in the Danish university sector. Agendas of increasing interaction, opening up the university, avoiding the isolation of researchers, emphasizing industry collaborations and embedding all of this in new performance measures of quality and relevance is clearly in line with the idea of progress as presented in *The New Production of Knowledge.* Whether or not there ever was a real historical past of the *mode1* university, we are now facing a world in which there is, if not in fact then in practical reality. It is the past of the autonomous, isolated and purely academically driven university that policy changes are working to put behind us, and as we shall see in chapter two it is also this past which is defended by some of the commentators who are critical of *the knowledge economy*.

Reflections on contextualization

Returning to the tourist bus full of refugee physicists, I would like to add some reflections on the status the above account on policy changes in relation to the chapters ahead. Clearly, Robert Feidenhans´l and Kim Lefmann did not agree with the interaction agenda as it was interpreted by their previous management and did not see it as the future model of knowledge production. Ironically, the institution they fled from on account of its management's emphasis on patents and products was given special attention in the "insights to invoice" report published by the Danish Government in 2003. The institution was highlighted as one of the research environments that "lead the way" in the interaction agenda and engaged in knowledge production for the benefit of Danish society. The Government pointed to this specific setup as a future model for knowledge production (Danish Government 2003). Looking at these "refugee" physicists' success in attracting funding and developing their research area since that time, it is notable that Both Robert Feidenhans΄l and Kim Lefmann have done rather well in exile. According to several of Feidenhans'l's colleagues and PhD students, the name Feidenhans΄l is a door opener for physicists all over Europe. A PhD student told me that "if you have worked with Robert, you can get a job anywhere". He is part of many prestigious international collaborations and can hardly be said to have spent his career in the study chamber waiting for a "stroke of genius to carry him through his career". Similarly, Kim Lefmann has been intensely involved in lobbying for the placement of the new *European Spallation Source* in Lund and has worked through the last 10 years for Denmark to become a shareholder in this large-scale project. This dream of came true in 2010 when the Danish state decided to invest 1.4 billion DKK to become a shareholder in the project. Of course, both physicists joined Thomas Bjørnholm in the champagne toast for the celebration of the UNIK research fund and as Thomas noted in his speech, the Nano-Science Center is doing extremely well, even in times of acute crisis. Studying the strategic maneuvers of scientists in response to the "interaction" agenda that characterized the rise of the Danish *knowledge economy,* it is perhaps worth noticing that this agenda has had a mixed reception among the academic scientists represented in this thesis. It seems that their response is not a simple one of compliance and reiteration of performative statements. As the interaction agenda sets the "context", and thereby the problem, for this thesis it may be worth reflecting on what status to give such a context. A few words on how I frame the relation between ideas and practices, policies and their subjects may be relevant before we move on.

First, the "policy context" I have outlined above could not solely be described as a set of empirical practices "out there". As I have shown above, the interaction agenda of Danish research policy seems to include STS conceptual vocabularies. This makes the context of the Danish *knowledge economy* simultaneously a conceptual and an empirical phenomenon. The conceptual developments in science studies are clearly an important component of the empirical field of research policy and one wonders if this mixture of seemingly separate spheres of reality may diffuse into the practice of science too, as it adopts and adapts to the changes described above[31]. In

[31] Jensen (2010) has addressed a similar problem in his account of the electronic patient record as a *future-generating device*. Reflecting on the recent interest within STS of "acting with" science and technology (as opposed to merely describing the field from an outside perspective), Jensen draws

contrast to other studies of performative theory in STS, we have here not witnessed the reiteration of concepts invented *by scientists*, but rather the reiterations of concepts invented *by science studies*. The above account does not describe the performativity of economic theory, physics or new medical technologies (the "usual suspects" in STS) but rather, the looping of theoretical constructs and statistical representations from macro-sociological approaches within STS (what we normally take to belong to the realm of the conceptual or "theory"). Unsurprisingly, STS concepts which have for a large part been occupied with nailing and documenting the performative power of theory is in itself not "outside" the looping effects that it has meticulously mapped. Similar to the equations of physicists and the calculative instruments of economics, STS knowledge claims also seem to be fully capable of creating a world for their own images when taken up by policy makers such as Hans Müller Petersen. Setting the context for this thesis thus does not merely involve representing an empirical reality as events that took place in Denmark through the last decade. This reality is as much *conceptual* as it is empirical, including as it does the theoretical constructs that guided, inspired and redirected changes in the government and management of universities (Jensen and Bowker 2011). I will go into more detail with the spill-over effects between the conceptual and the empirical in chapter three in an attempt to find out how to study such a phenomenon. I will merely point to the observation that since STS conceptual vocabularies are an intrinsic part of the mixtures studied in this thesis, this context cannot acquire the status of an outside "background" or "scene" which determines and shape the practice of scientists.

Second, it would not be satisfactory to merely place the strategies and maneuvers deployed by Danish scientist in the above policy context and map them as examples of a performative loop between policy concepts and practice. Making the point that concepts have the potential to create practices should not reduce the inquiry to mapping impacts of overarching structural conditions. The refugee physicists in the bus are just a hint of the more elaborate observations of how scientists strategically responded to the interaction agenda in very creative ways. As argued in the anthropology of policy, the performative, productive and contestable nature of policy excludes the possibility of studying policy changes as external or general forces which constrain and steer practice through linear and predictable trajectories. (Wright and Shore 2011). According to anthropologist Susan Wright and Chris Shore, policy concepts have impressive "runaway effects" (Ibid.) in that they tend to migrate into new areas and shape practices in ways beyond their original purpose. These effects cannot be conceived of outside the activities of the subjects of policy as they actively engage with the changes in their habitat. The impacts of policy are then not to be viewed as a simple

attention to the interrelations between the success of the mode2-narrative and the preoccupation in STS with doing interventionist research (being useful to the field). Jensen calls for self- scrutiny within STS in relation to these topics and suggests fruitful couplings between the sociology of expectations and interventionist "action" approaches within STS. Perhaps the expectations generated within STS are becoming more important as a field of study in itself?

"trickle down" process where concepts unilaterally shape and determine practices. Less interested in policy as an instrument of rule than as something to be problematized, Shore and Wright inquire into "how people engage with policy and what they make of it" (Ibid. p. 10). Contextualizing the inquiry that I am about to begin by pointing to the above policy changes and the assumptions that seem to have guided them is thus not a matter of pointing to a force "behind" the actors studied in this thesis[32]. I am not suggesting that my study of successful scientists is merely documenting the "impacts" of these policy changes. Rather, I am studying how actors of the field respond proactively to changes in their environment to form new strategic and tactical responses.

The interaction agenda of Danish research policy is thus not a "thing out there" (Ibid. p.5) shaping practices. Rather, as I will argue in chapter three, the "interaction"-agenda is part of an *ecology* in which scientists, policy makers, industrial corporations and governmental agendas all have their immanent modes of existence (Stengers 2010). Scientists, their strategic maneuvers and the costs they may involve are thus not passive responses to environmental changes taking place outside or around them. Rather, they are active attempts to make the best of the options given to them and as in any other ecology the environment and the species living in it is not unilateral or deterministic. In articulating the strategic and tactical responses of the "winners" of the Danish knowledge economy, I am thus not mapping what it is, but rather experimenting with what it may become (Stengers 2011). Using mainly the practice of two scientists, Thomas Bjørnholm and Birger Lindberg Møller, as a point of entry into this it should be obvious that I am not presenting a general account on the broader impact of the interaction agenda in Danish university science. Generalization is not the purpose of my account; rather I am interested in specific events that may point to new ways of setting the problem of knowledge economy. As pointed out by philosopher Judith Butler, performative effects like those documented in STS studies of physics and finance are rare event and do not come about as if by magic. Rather "the risk of breakdown and disruption is constitutive to any and all performative operations" (Butler 2010, p. 152). Performativity, Butler argues is not a magic spell, performative utterances can be set in motion based on ongoing renegotiations. Consequently, the interesting thing about performative statements is not necessarily just their self-propelling abilities but just as much the intrinsic risk "misfire" and unexpected outcomes (Butler 2010)[33]. In tracing the tactics of scientists who did well within the last decade of university reforms and today occupy powerful positions in their habitat, I am also hoping to trace the "misfires" of policy; the many transformations and creative adaptations that are part of any attempt to make ideas bring about new practices.

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[32] See Latour (1986) for a more elaborate version of this argument.

[33] Butler proposes this to be a critique of mainly the work of Michel Callon. This can be said to be a bit of a misfire in itself as most of Callon's authorship consists of elaborate accounts on the way large scale science and technology projects tend to fail or become subject to transformative translation processes. See Callon 2010 for a quick reply to Butler.

2
Conceptualizing mixtures

> "And while external cultural commentators evidently see the industry-academia divide sitting astride a major institutional, intellectual and moral fault line, it would be massively inaccurate to imagine that pertinent practitioners necessarily do so." (Shapin 2008, p.232).

> "Knowing things requires one first of all to place oneself between them. Not only in front in order to see them, but in the midst of their mixture, on the paths that unite them." (Serres 2008, p. 80).

I introduced this thesis claiming that "knowledge economy is a mixture". As will be made clear in this chapter, I am not alone in this observation. In fact, one could argue that a large part of the last 40 years of science studies is a large reservoir of concepts and vocabularies designed to deal with the mixed and mingled phenomena. Concepts like Mode 2 knowledge production (Gibbons et al.1994), triple helix (Etzkowits and Leydelsdorff 2000), boundary work (Gieryn 1983, 1999), boundary objects (Star & Greismer 1989), hybrid firms (Tuunainen 2005; Tuunainen & Knuuttila 2009), imbroglio (Owen-Schmidt 2006), hybridization (Latour 1993), co-evolution (Etzkowitz 2002), co-production (Jasanoff 2004), Trading Zones (Galison 1997), asymmetrical convergence (Kleinmann and Vallas 2003) networks and translation (Latour 1987, 1999; Callon 1986a and b) are all attempts to conceptualize the knotted and mingled nature of scientific practice.

I am thus not alone in the difficult task of making the mixtures of scientific practice knowable. Analysis is a tricky task when the object of study is characterized by its ability to tie up or draw together a wide range of practices that we would otherwise make sense of as separate entities with conflicting codes. The rise of the knowledge economy has not made this "mixture problem" less pertinent. One sip of the knowledge economy drink and you have already swallowed technical detail, social groups, capital interests, government agendas, public debates and potential futures. The problem of mixture thus poses a challenge for the task of analysis, at least if analysis is taken for its etymological meaning of "untying" (Brown 2011). As discussed in the introduction, Michel Serres has little faith in untying, differentiating and dissecting mingled phenomena in order to make them knowable and suggests that analysis is inherently a destructive operation. (Serres 2008, p. 167). Knowing things, Serres argues, means placing oneself *between* them or *amidst* their mixture. The two types of STS literature I will present and discuss in this chapter are chosen for their attempt to do just that –to think about what happens when boundaries blur and seemingly disparate elements convergence or associate to form altogether new qualities. The authors I present all place themselves amidst the practitioners of science by close readings of scientific documents, interviews with scientists and long-term ethnographic field work conducted in scientific laboratories. Thus, they can best be described as micro-sociological. Allow me to summarize the two approaches I will be discussing before we move on.

The first "position" is a constructivist approach. Here scientific practice is seen as intrinsically a mixture to begin with; always part of a heterogeneous set of practices that merge into one another, always a network of transversal relations between actors. This first perspective is largely descriptive and very much based on studies of scientific fact-making. As some of these accounts date back to a period before the idea *knowledge economy* really started to take hold, they do not necessarily take a normative stance in relation to the way government and capital interests are becoming part of the way science is governed and managed. However, their attempt to describe science as an intermingled and heterogeneous landscape this literature seems like a good place to start the tricky business of analyzing mixtures.

The other "position" is more normative and refers to a heterogeneous group of texts which take a critical perspective on the knowledge economy. In contrast to the first perspective, these accounts depict the present mixtures between public and private science, between science, capital and government interests as effects of a historical *dis*continuity. They claim that the knowledge economy and neoliberal research policy have resulted in a dramatic shift in the way universities are governed and managed. Moreover, this shift is diffusing into the practice of science. The mixture-problem in this critical perspective is not an ontological presupposition but a symptom of recent historical developments. We have already seen such an account in the previous chapter where a perceived shift towards intensified interaction was celebrated as a new and better mode of knowledge production. The critical position outlined in this chapter, however, diagnoses this shift as a threat to academic science and sees mixture as a politically directed process in which academia and industry converge, a process where science is only a minor constituent in relation to much stronger forces of capital and government interest. I have chosen to group a range of different accounts under the notion of a critical perspective on the mixtures or convergences of knowledge economy as they make an important contribution relative to the first perspective. In light of the changes taking place in the way academic science is funded, managed and governed, it is perhaps no longer enough to restate that science was a mixture to begin with. Some kind of critical inquiry seems to be needed to account for the lamentation or sense of loss expressed even by highly successful scientists.

These two contributions form the conceptual "context" of the thesis. Again, contexts are not given; they are specific ways of setting a problem. In the last chapter I placed the problem of the thesis in relation to a "policy context", a historical series of changes in the way Danish universities were governed, managed and funded. However, in doing so, we already ran into the mixture-problem as the conceptual seemed to spill over into the empirical. Policy, it seems, is already conceptual and thereby concepts are also already empirical. The "outside" policy environment of scientific practice is very much part of an "inside" world of the analyst in that the conceptual languages of STS seem to have become an intrinsic part of the way scientific practice is steered towards specific types of goals.

This chapter will continue the inquiry by accounting for a "conceptual" context that follows the mixing up the conceptual and the empirical, literature review and ethnographic observations. I will thus account for the two approaches by placing them amidst or between experiences from my early fieldwork. The commentaries and reactions of field participants to my questions and research interests will thus be allowed to blend in with the presentation of constructivist and critical approaches to the study of science. Sometimes field participants will exemplify the theory, at other times they will contest it. In order to have a taste or a sense of the muddled practice that characterizes the work of these successful scientists, it seems one cannot simply take their statements apart and explain them by an existing conceptual framework. At least for my part, I had to drink up and become affected by the questions they posed to my research interests.

Mingling with Nano Science

My first conversation with a field participant went right into the problem of mixture. As it happens, this topic required more than one drink. I was visiting the Nano-Science Center, an interdisciplinary research center placed in the Niels Bohr Institute at the Copenhagen University. Thomas Bjørnholm, the manager of the center had invited me to join the monthly *get-together* – a Friday afternoon event involving beer, interdisciplinary mingling and music. As I arrived in the canteen, the room was already full of people. I approached Bjørnholm to say hello and was immediately supplied with a drink and introduced to Per Hedegård, a theoretical physicist. Bjørnholm opened the conversation with "Well Per, aeh.. Bettina here is an anthropologist of some sorts and she is going to write a PhD thesis about us… that means you!" Per Hedegård smiled and gave me a look before rolling his eyes: "Oh dear… God help anyone who tries to make sense of these people". Bjørnholm smiled, turned to me and said: "Really, you should definitely talk to Per if you want to know what we are all about he has been around from the start". Then he left.

Alone with my new acquaintance, I quickly realized I had been thrown into the deep end of what I was later to recognize as the loud and lively debate culture of the Nils Bohr Institute. “So you are an anthropologist…” Hedegård said without waiting for me to reply. Within the next minute I realized that to Hedegård, this meant I had to be one of those qualitative researchers who has a religious hatred of statistical generalization and is mostly interested in gender inequality and feminism. My attempt to present him to a more nuanced view on social science (and feminism) unfortunately triggered my new acquaintance’s *Sokal-affair* reflexes which engaged us in a 20 minute heated discussion where Hedegård did most of the talking. I had still not had the chance to introduce myself with my real name and disciplinary background when Hedegård followed up on the science wars with a series of attacks on social constructivism, a subject he was surprisingly well rehearsed in discussing. Forty minutes and a lot of words later, I finally suggested that we went and got a second drink.

This returned us in good order to the less controversial subject of my research question. Or so I thought. However, this topic proved to be no better than the former. As I started explaining that my purpose was to inquire into how research managers in basic science cope with neo liberal research policy and its increasing pressure towards commercialization my new acquaintance burst out in uncontrollable laughter. Per Hedegård has a very loud voice, a very impatient and telling body language, and a laughter that can be heard through thick walls. After laughing louder and longer than I was really comfortable with, he turned to me and said that that was probably the most ridiculous topic he ever heard of. “Why on earth would you want to write about something like that? There is nothing new in what you are talking about. Scientists have always been “industrial” he told me. “How do you think science started out in the first place? Science has established itself always and only by way of industrial interests. The public funding of science is a recent phenomenon”. People who were paranoid and upset about the “insights to invoice” agenda of Danish research policy should remember that science for the first many hundred years was funded and supported always and only by private funding, and that this funding was often industrial, he argued. Thinking that there is a gap to be bridged or a clear boundary to be protected between the two is just silly. According to Hedegård, I might as well change the subject of my thesis right away and find a more relevant question to answer. I attempted to explain that in my area (which I hastened to add, was not anthropology but science and technology studies) a lot of research had been done that showed how the boundaries between science, industry and government are historically contingent, permeable and blurry. Hedegård listened for a bit, made another attempt at telling me that this was old news and then the conversation slowly died. Exhausted, I created closure by telling him that despite our epistemological differences, we were probably more in agreement than we thought. Hedegård was probably tired too, at least he did not protest loudly. Relieved I said goodbye and retreated to find my coat.

Constructivist accounts

Ironically, the laughter of Per Hedegård does seem to resonate with the constructivist and micro-sociological approaches he so forcefully disagrees with. Possibly, constructivist science studies and laughing physicists really are more in agreement than they can imagine. At least they both depict science as a practice that was mixed to begin with and would not see present attempts towards integration of science, industry and government interests as the cure to a bridging a big gap between them. Rather, they would argue, any attempt to create more interaction between science and industry is an attempt to fix things that were never really broken. As we shall see in a minute, constructivist science studies similarly conceive of science as something that was always already infused with industrial interests and closely involved with actors outside the university.

The scientific life

One of the most recent examples of this argument is the historian Steven Shapin's account of science as vocation in the United States (Shapin 2008). Shapin describes the different ways in which scientists' complex relations with external actors have been rendered problematic over time. The anxiety concerning the pollution of university science with industrial and government interests is not just a recent phenomenon provoked by a neo liberal turn in science policy. Shapin's historical account articulates in great detail how science has always been a muddled territory where interests and practices flow into one another producing new forms of tension and fertilizing new opportunities for scientific fundability. Scientific life, Shapin argues, cannot be uniformly described in terms of coherent norms and practices. At least not if you are claiming to represent the viewpoint of scientists themselves who, according to Shapin, do not start out in assumptions about a moral fault line between academia and industry.

It seems that Shapin´s scientists would laugh along with Hedegård as they do not necessarily see a big gap separating science and industry. The two are quite closely related in practice and recently they seem to become more and more alike as norms and practices travel between them. Shapin even goes so far as to describe an almost complete reversal of roles between the academic and the corporate scientist. In Shapin´s account the academics who left the university to do for-profit science paradoxically report their corporate environment to be more academic than the university departments they left behind. Corporate science is thus depicted as much less restrained in comparison with a former university career. Teaching obligations, fierce competition over public grants and the constant pressure to find external funding add to the highly competitive space of the publicly funded university and makes it a lot more restrained in practice than often imagined. In some cases, Shapin´s informants depict corporate science as more curiosity-driven, free, creative, and academically stimulating than its university equivalent. Academic science, by contrast, is depicted as a restricted struggle for fundability which does not really live up to our common sense expectations of free research in the publicly funded university[34].

> The desire for a "free space" in which to conduct the inquiries that one wants to conduct, that one might feel oneself driven to conduct, is probably the major item in scientists' motivational lexicon. However, the institutions in which such free spaces may present themselves map only problematically onto the divide between academia and industry (Shapin 2008, p. 263).

When looking at its espoused values as they were stated on websites, press releases and in speeches, the Nano-Science Center certainly did not seem to have a problem with the "insights to invoice agenda". It only took a few weeks of field work there to understand why Hedegård was so amused about my focus on how scientists "cope" with an external pressure to bridge the gap from insights to invoice. The center as a whole did not *cope* – when looking at the funding given to the groups and projects based in the center it would be fair to say that it *excelled* at making itself relevant to society (often conceived as industry). The Nano-Science Center's webpage for "business relations" proudly states: "Many talk about the necessity of cooperation between universities and industry. At the Nano-Science Center we do something about it". This bold statement is followed by an account of the number and type of industry collaborations, industry funding and the kinds of initiatives and networks the center is part of in their effort to create: "more direct access for companies, businesses, NGO´s and trading environments"[35].

Corporate development really did seem to be much more embedded in the everyday life of natural scientists than I as a social science scholar had imagined. Coming from a business school, it was quite a surprise to realize that a basic science center placed at the Nils Bohr Institute – historically a home for theoretical physics - seemed at least as entrepreneurial as my own home department. Within the first weeks I was contacted several times by PhD students who wanted to know about courses in entrepreneurship and business administration. Contrary to my social science home, these people seemed extremely keen on thinking about marketable products alongside with epistemic problems. "Back home" in the social sciences I was accustomed to watching senior scholars gain respect from younger colleagues and students by boosting

[34] Although Shapin does give priority to this surprising reversal of roles, he does recognize that corporate science is not always perceived at an environment of pure fun and freedom. Heterogeneity seems to be the overall trope guiding Shapin's account. Similarly, Mark Peter Jones' account of the rise and fall of Hybridtech shows how innovation and creative academic spirit suffocates as a small, dynamic and entrepreneurial firm mergers with a larger and more formally managed biotech corporation. From being an attractive workplace more academic than the academy, Hybridtech is slowly transformed to a dull place to work, a transformation which has devastating effects on productiveness and bottom line numbers (Jones, 2011).

[35] http://nano.ku.dk/english/business/

their capacity for high theory, not by promoting their consultancy sideline. Out in the field, however, things were different. The people who were perceived as "star scientists" by the younger generation were often those who could span across both worlds without watering down either part of their professional life. Young ambitious scholars as well as senior management saw industry engagement as an extra - a thing to spice up your scientific profile and a demonstration of academic surplus capacity. Rather than seeing industry interests as an external distraction, academics at the Nano-Science Center seemed to feed their work and productivity by way of the presence of industrial actors. Besides formal industry collaboration, industry funding of basic science projects and a newly started university spin-off, there were plenty of other ways in which the Nano-Science Center was collaborating with industry. Industry could provide ground for training of master and PhD students, a network of old students, friends and colleagues or a market place for laboratory equipment and services. The university was anything but an institutional enclosure, entrenched in an ivory tower with no contact to the world of business.

The sociology of translation

The above observations resonate with another constructivist perspective in the field of science studies, namely the laboratory studies of Bruno Latour and the studies of innovation processes made by Michel Callon (Latour 1987, 1999b; Callon 1986a). These studies form an important part of the foundations for what has been labeled *the sociology of translation* or *actor-network theory* (Latour 1996, 2005; Callon 1980, 1986b). What this body of literature offers is an analytical vocabulary for grasping the transversal movements of scientific practice as it pays close attention to the details of scientific practice as fact-making. As argued by Latour, mixture and heterogeneity is an intrinsic property and characteristic to the construction of scientific facts:

> "the very difference between the "inside" and the "outside", and the difference between "micro" and "macro" levels, is precisely what laboratories are built to destabilize and undo" (Latour 1999, p.258).

Whether actors and interests can be located inside the university or outside it, whether the situation at hand revolves around micro level interactions in the laboratory or macro level interactions in media, politics or scientific controversies *the sociology of translation* insists on analyzing it in terms of the same vocabulary. Categories like *actor, actant, translation* and *network* are attempts to think without preconceived distinctions between nature, culture, micro, macro, external and internal. This idea was launched as a principle of "generalized symmetry", knowing that studying science means studying society and nature at the same time. Symmetry here means to describe events in the field in a single repertoire whether they be social, natural or political. The analyst, Callon argues, should not repeat the analytical repertoires and distinctions made by field participants but will need to come up with a different repertoire for describing events in the field which makes no a-priori assumptions of separate spheres of reality like the nature and culture, micro and macro, scientific and social (Callon 1986a). The analytical repertoire thus needs to be invented by the analyst in response to the task at hand and needs to be construed in a way that does not require the analyst to change registers when accounting for things that would otherwise be thought of as either "technical" or "social", belonging to "science" or belonging to "capital interest". In the sociology of translation these distinctions are an *outcome* rather than a starting point of analysis. Callon and Latour claim to simply "follow the actors" wherever they go and study the *formation* of social groups, natural, social, political and conceptual entities rather than labeling them in accordance with a preconceived vocabulary (Ibid.; Latour 1987, 2005). In this perspective it does not make sense to argue that science is either free from outside interest or polluted by it.

Ironically, resonating with the argument posed by Per Hedegård over the second drink, the sociology of translation claims that it was all a mixture to begin with, therefore we need to start analysis with a mixture-vocabulary and not one of solid institutional constituents, tensions between conflicting norms or separations between science and politics. Latour and Callon will take the mixture-argument much further than Hedegård in claiming that an analysis of scientific practice has to give as much privilege to the human actors as to the objects they study, the machines they work with and the technologies for keeping practices stable across time and geographical distance (Latour 1991, 1997a). *The sociology of translation* argues that if scientific practice is conceived of as an institutional enclosure which can be integrated with or protected from outside interests, we are not doing justice to the events taking place in the field. More controversially, Latour and Callon would argue that we will not understand scientific practice only from looking at the explanations given by the scientists as these tend to purify and separate out abstract knowledge claims from the heterogeneous and messy practices in which they gain *facticity* (Latour and Woolgar 1986). To Latour, the grandiose "front door" ready-made scientific fact is less interesting than the messy and often invisible back door of science in the making (Latour 1987). *The sociology of translation* attempts to follow the transversal lines and chains of association that make up the construction of scientific facts, and does so by inventing a new vocabulary designed to grasp heterogeneous complexity of scientific mixtures. The sociology of translation advocated the invention of a conceptual vocabulary that is broad enough to articulate the complexity of a practice which is simultaneously practical, political, conceptual, material and personal.

It is not difficult to hear an echo of Michel Serres' mixture-argument in this approach. As Serres stated in relation to Bergson's example of sugar water, we will have to wait *until the end of time* if we expect the mixture to separate out again. Serres argues that separating the constituents of mixtures may make them analyzable but will tell us nothing about their qualities or effects. The task of knowing about mixtures without destroying them by analytical dissection can indeed be traced back to the early beginning of this approach. Both Latour and Callon have been heavily influenced by the philosophy of Serres, especially the concept of *translation*, which has long been a touchstone of their argument[36].

Callon was the first to make use of Serres in his attempt to trace the way scientists in electrochemistry engage in *problematizations* as an intrinsic part of the attempt to construct fuel cells. Scientific practice is, according to Callon, a mixture or a "mysterious chemistry, the constant renewed fusions which permanently produce the social and the cognitive" (Callon 1980 p. 198). Callon argues than when scientists like '*Y'* create technical objects or knowledge, they simultaneously create social groups, interests and positions. If one wants to study a particular process then you have to take all the others into account, since everything hangs together:

> "Definitions of problems and the links between them cannot be distinguished from the work of organizing fields of interest to be aggregated (…) Definition of a problem implies definition of a group, even if no empirical unity can be named. Y gives shape to the social, he builds a field of positions. We can go no further. The list of problems as suggested by Y cannot be *deduced* from the state of scientific and technical knowledge. *It translates a determination to incorporate interests,* and to interest those who are still only potential partners. In fact Y´s programme represents an attempt to mobilize social groups. I propose to call this particular logic by which problems are directly associated with groups: The socio-logic of translation."
>
> (Callon 1980, p.210 emphasis in original)

Studying scientific practice as a process of translation means tracing the associations made by scientists as they mobilize other actors (as for example natural or technical objects and social groups) and make them comply with their research program. It means watching how scientists work to dissolve institutional boundaries and destabilize scales. Trying to make sense of this by separating the process into solid constituents like institution, social, technical and political will not help the analyst, Callon argues. The scientists described by Callon and Latour jump scales from science to society and back again, thereby contracting politics, technology and industrial interests into an indivisible whole, a mixture which cannot adequately be described by looking at its constituents. Science, Latour claims was never a pure breed:

> "Science is not politics. It is politics by other means. But people object that "science does not reduce to power". Precisely. It does not reduce to power. It offers *other means*. But it will be objected again that "by their

[36] For more on the relation between the sociology of translation and the philosophy of Michel Serres, see Serres and Latour 1995 and Brown 2011.

> nature these means cannot be foreseen". Precisely. If they were foreseeable, they would already be used by an opposing power. What could be better than a fresh form of power that no one knows how to use? Call up the reserves!" (Latour 1988a, p.229).

A fresh form of power requires fresh analytical vocabularies, Latour argues: "call up the reserves!". As we broaden the focus from the early studies of fact-making conducted by Latour and Callon and begin to include also the attempts to *manage* science, matters get even more complicated. Most of the field participants presented in this thesis have taken on the task of research management along with their academic work[37]. They did not cease to be scholars just because they became managers and/or entrepreneurs. Some are full time top-managers *and* star scientists. Thomas Bjørnholm who manages the Nano-Science Center spends most of his office hours in meetings but this has not kept him from publishing in Science and Nature. On his webpage we find 150 publications and a citation number of 3304. Other field participants are scientists fast on their way towards tenured positions who have taken on middle management along with the tasks of teaching publishing and hunting for resources. All of them have working lives that require them to mingle with students, natural objects, excel sheets, ministerial publications, venture capitalists, instruments, sales representatives, politicians, legislative rules, business plans, journalists and corporate scientists in order to do their job. Given the unquestionable success of the Nano-Science Center's attempt to get other actors interested (read: invested) in their research programs, they seem to do it well.

The project baron

If we are to conduct a Latourian reading of the actors of this field, Per Hedegård does have every reason to laugh. Not only is he himself a very capable scientist, he is also part of the network of the Nano-Science Center, a very extensive field of associations which spans the world of science, capital and government. If Bjørnholm, who manages the Nano-Science center, is indeed the same kind of actor as the scientists portrayed by Callon and Latour, he is a master of navigating through muddled waters and has no problem whatsoever with neoliberal science policy. As a natural scientist, Bjørnholm is capable of being nature's spokesperson; a very powerful position which allows him to tie together natural objects with the imaginations of industrialists and policy makers and form strong and extensive networks of associations

[37] Not a lot of attention has been given to research management in the sociology of translation. Early laboratory studies seemed to concentrate mainly on scientific practice. Law (1994) is one exception. However, Law's taxonomy of four modes of ordering in research management is hardly an attempt to take the challenge of the mixture problem in science studies.

(Latour 1991; Callon 1986a). Give Bjørnholm a laboratory and he will "raise you the world" (Latour 1999).

If we go back to the bus full of refugee physicists, Robert Feidenhans´l will tell us a story of science as a fresh form of power which traverses the distinctions by which we normally make sense of university management and governance. Feidenhans´l is a man who seems to know everyone in the Danish university system. He frequently makes use of a specific label to characterize the powerful scientist of today: The *project baron*:

> Feidenhans´l: Project barons, there are many of them around the system and mind you, they are *not* heads of departments but they never the less have an enormous influence. They have huge networks, big grants and full professorships. And really, what does a head of department or a dean really have to do good with? The dean can't do a thing if all the project barons are going somewhere else. So you have a very complex… for example the most powerful person in Danish research is clearly Professor X.
>
> BGH: yes, that´s what everyone tells me, why is that?
>
> Feidenhans´l: Well, X is enormously influential. I mean formally he is just a professor, but the dean does not make a single move without consulting him first. So here is one of those persons who can act outside the official managerial structures and go beyond.
>
> BGH: And that is what you call a project baron?
>
> Feidenhans´l: Yes, because all he lives by is getting money *all the time*.
>
> PhD Student: The second he does not get money he can't do anything.
>
> Feidenhans´l: Most of the free research funds we get here come from other sources and not by the official university route. So the people who get money are those in power.
>
> Lefmann: Oh you would definitely want to be a project baron rather than a dean. Otherwise you get too far away from research, it's better to do some research yourself and also get a lot of influence on what you do. (Recorded conversation 2009)

Project barons are people who are so skilled at getting external funding that they direct the way university research is going rather than comply with university management. Some field participants even say (but these are rumors) that project barons like the professor X referred to above have influence higher up in the ministries and are capable of dictating and influencing the twists and turns of strategic research funding to fit their interests. When university professors acquire the magical status of the project baron, they muster allies such as industrialists, politicians, ministries and research councils and thereby force their dean or head of department to "*make a detour"* that encompasses their research program or *"go their way"*(Latour 1987). The project baron does not shy away from the mixtures of knowledge economy, he (sic.) drinks up and orders another one. According to Feidenhans´l, these people are usually professors situated around the Danish university system. Later on we will meet a professor in plant biology whom Feidenhans´l has labeled as one among the league of Danish *project barons* and hear his views on how to navigate between academic, industrial and political interest.

According to Feidenhans´l, Thomas Bjørnholm would definitely qualify for the title of the *project baron*. Maybe that is why Per Hedegård can afford to laugh about my anxieties concerning the effects of neoliberal science policy on his professional life. The presence of industrial interests does not put restraints on those who are working under the protective shelter of project barons it seems. Rather, it is a steady stream of external funding that allows people like Hedegård to follow his research interests without constraints. Since the Nano-Science center has been immensely successful in getting big grants and topping those up with industry funding and venture capital, it has formed an extremely attractive workplace for refugee physicists as well as other scientists who want to pursue their basic science interests. If you know how to make associations, translate interests, create positions and form social groups, neoliberal science policy and knowledge economy is your playground. There is nothing qualitatively new in this, says Latour, the strategic network building of scientists was required from the beginning. If this is really the situation at the Nano-Science Center, I may have been right in assuming that Hedegård is much more in agreement with the constructivist strand of science studies than he would care to know. However, matters are a little more complicated than that.

"We are more in agreement than you think"

It took me months of fieldwork to discover that claiming to be more in agreement with your discussion partner than you initially thought carries some heavy historical baggage when stated in the context of the Niels Bohr institute. I may actually have offended Hedegård without knowing it by suggesting this. Allegedly, Niels Bohr himself had a habit of ending a public scientific debate by discretely approaching the speaker to engage with him personally. On some occasions, it is said that he would open this conversation with the words: *"I believe we are more in agreement than you* *think"*[38]. Whenever this sentence was uttered, it meant that Niels Bohr thought of you as a *complete idiot*. If you heard these words from Bohr, it was the knell summoning you to a painful session at the blackboard where your claims would be taken apart one by one with sharp, merciless meticulousness, using nothing but a piece of chalk. In fact the physicist, Kim Lefmann, later warned me to never use that phrase when talking with his colleagues at the Niels Bohr Institute as it could easily be read as an insulting attack rather than attempt at reconciliation.

Whether or not Hedegård was offended by what I now realize was a possibly insulting attempt at reconciling constructivism and theoretical physics has never been clear to me. He did actually speak to me again on several occasions after our first introduction. To be sure, I did not mean to tell him that he was a complete idiot. I genuinely meant we were probably on the same page, when it comes to thinking about science as always already a mixture, as an intrinsically industrial, entrepreneurial, scientific, material and political activity. However, even though the constructionist take on scientific practice delivered by the sociology of translation is a promising approach for thinking about mixtures, we may need more than that. As Feidenhans´l and Lefmann made it clear when accounting for how the Nano-Science Center houses a great deal of refugee physicists, there is more than laughter to be heard when talking to successful scientists. Even Per Hedegård will testify to the fact that the integration agenda has had some rather disastrous effects on scientific practice. He likes to argue, that if we had applied an *insights-to-invoice* agenda to the practice of science up to the industrial revolution where the most pertinent problem was transportation, we would today have an impressive array of extremely big horses and no combustion engine. Hedegård likes to state this as another example of how misguided it is to think of science as something that has to bridge a gap to the surrounding society and make itself useful to industry. To him, science is already industrial. Rather than being of service for the industrial revolution, Hedegård would claim that science paved the way and made it possible. Most of the field participants I met in the Nano-Science Center are an interesting blend of entrepreneurial spirit with very strong affinities towards free, basic research agendas. They all insist that science should be free to make its own mixtures and think out of the box regardless of what the perceived needs of society may look like at the time. Hedegård likes to joke, but even to him this is no laughing matter. The new knowledge economy may therefore be a different kind of mixture than the ones mapped by *the sociology of translation.*

[38] Danish: "*Vi er mere enige end De aner*". Bohr´s phrase is perhaps better translated as "I believe we are more in agreement than you have a sense of/can imagine".

Critical accounts

Times have clearly changed since Latour published "Science in Action". The image of the "world-raising" laboratory scientist that destabilizes scales and mobilizes allies into fresh forms of networked power does look a bit tired in light of the rise of neoliberal science policy. Trials of strength now take place *within* new and very powerful networks of which scientists rarely occupy a privileged Machiavellian position. As explained in chapter one, scientists feel that their freedom is shrinking and even the winners have a sense that something is lost. Feidenhans'l's concept of the project baron is not just a matter of pointing to the existence of a fresh form of power; it is also an expression of lamentation.

Feidenhans´l is not the only one who has identified the rise of a new aristocracy in academic science. In a 2009 article in *Universitetsavisen* - a newspaper distributed by the Copenhagen University - sociologists Heine Andersen and Inge Henningsen divide academic science up into "the barons of science[39]:" and the "ball fetchers". Drawing on a report published by the authors evaluating the Danish research councils' distribution of public funding, they criticize the idea that increased competition naturally generates better research. Andersen and Henningsen compare the distribution of funding to a tennis tournament where all players have to live exclusively by prize money. Needless to say, this system results in most of the players leaving the tournament after the first rounds to instead become ball fetchers, leaving only the previous winners to compete for the finals (Andersen and Henningsen 2009).

A similar critique of Latour's focus on "trials of strength"[40] between laboratories was launched by feminist scholars in science studies. "Surely you are joking, monsieur Latour" (Amsterdamska 1990) was one critical response to Latour's "Science in Action" which still remains one of the key works in constructionist science studies. Amsterdamska criticized the sociology of translation for focusing on war-metaphors and reducing science to game between gladiators. As also argued by other feminist scholars in science studies, too much attention has been given to crowning the

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[39] "Feidenhans´l makes no explicit reference to Andersen and Henningsen and tells me he picked up the concept of the *project baron* from a colleague. Heine Andersen also testifies to the expression of the "barons of science" to be a term that has circulated among academics in the Danish university world before it was taken up by him and Henningsen in Universitetsavisen when accounting for the main points in their report (Andersen, personal communication).

[40] A trial of strength is really a trial of network "length" between competing theories and laboratories. Chains of association between statements, machines, inscriptions and other human and non human actors compete by making associations and only one party wins. Latour goes into detail with this process by for example accounting for how the Watson and Crick "won" the trial of strength that has gave us the now uncontestable image of DNA as a double helix structure. The fact of the double helix has reduced competing models to artifact (Latour 1987)

kings of scientific practice while neglecting some of the critical potential in the sociology of translation (Star 1991, Haraway 1997).

Similarly, Shapin's descriptive account of the heterogeneous and unpredictable convergence between for-profit and non-profit science has been met with critique. Barbara Hernstein Smith has questioned the legitimacy of Shapins' claim to a reversal of positions between the corporate and academic science and asks for some reflection on whether or not the future of the scientific life he describes should not also be an *academic life*:

> "Shapin observes that the scientific life in academia and in industry are becoming increasingly convergent. That certainly seems to be the case and it's important to know about it. But, he might agree, some aspects of that development can also properly be lamented." (Smith 2009, p.12)

The refugee physicists and even Per Hedegård would agree here: a bit of lamentation is perhaps in order when accounting for how scientists experience the changes taking place in their habitat. If it ever was, it is perhaps no longer entirely unproblematic to diagnose science as a "fresh form of power" which mixes and destabilizes known distinctions and creates transversal networks of association. Just because Louis Pasteur came out on top in "the Pasteurization of France" (Latour 1988a), every other scientist will not necessarily be doing the same[41]. Science, industry, government and society may have been mixed from the beginning, but perhaps not in quite the same manner as they are now. Without the refugee camps sheltered by project barons, young researchers or new research areas are facing rough weather. And refugee camps are quite rare. Several of the scientists I have talked to tell me that not all project barons see it as their mission to gather and distribute resources and create larger environments around them to shelter researchers who happen to have shorter networks. Some simply take care of their own.

If we go into critical accounts of scientific practice in the knowledge economy, we find more than a few authors who would contest the constructionist accounts depicted above. Some things may have been mixed to begin with, but as argued by the authors presented below the intensity, scope and toxic effect of this infusion has changed dramatically within the last three decades.

Neoliberal science policy

As hinted in the beginning of the previous chapter by the refugee physicists Feidenhans´l and Kim Lefmann, the interaction-agenda of Danish research policy is not always well-received by scientists. The espoused "insights to invoice"

[41] To be fair, *the sociology of translation* has been at least as preoccupied with showing scientific and especially technological failures, even while telling the story of the winning gladiator. Pasteur only wins because competing laboratory-networks lose. Epic techno-science failures are depicted in Latour 1996b and Callon 1986, 1986b.

value statements found on the Nano Science center website do not necessarily map on to the values and incentives of the scientists working and managing it.

All of the scientists I have encountered in fieldwork see themselves as first and foremost academics. They do not imagine themselves to be a resource for industry and trading environments and are not primarily in the business of patenting and short-term value creation. These things are "extras" to be sprinkled on top of an impressive academic profile. Like Feidenhans´l and Lefmann, they often see themselves as the *first* link in the food chain of science, *not* the last. They want freedom to work with problems that are interesting to them and they want respect for their research. It is quite peculiar that a place like the Nano-Science Center whose success is very much based on its ability to attract external and private funding is also the refugee camp for scientists who do *not* thrive in the age of commercialization and *mode 2 knowledge production*. According to several of the field participants, the Center's impressive fundability and its broad weave of interdisciplinary relations gives them just the shelter they need to breathe freely. Vague enough in its interdisciplinary connections to make promises worth funding and strong enough in its scientific base to secure a steady output of publications, the center constitutes a rare playground. Here, under the networked protection of Thomas Bjørnholm - a project baron *par excellence* - physicists are free to meet up at the blackboard and fight over meter-long equations without having to fear for the economic survival of their research program. Insights may be turned into invoices, but from the appropriate end of the scientific food chain.

Per Hedegård is exactly such a scientist, working as he does mainly in theoretical physics. Based on a more critical inquiry, one might ask whether Hedegård would have the stomach for capital and government interests that life outside the refugee camp seems to require. Some of the more recent and also more critical accounts of science in the knowledge economy would claim that he may very well be facing a future where refugee camps are a thing of the past as crude capitalist agendas soak into the fine pores and associations which make up the practice of science.

The contributions presented in the following would not only question whether Hedegård can afford to laugh much longer, they would also pose a harsh critique of Danish research policy and its interaction agenda. According to these critical accounts, concepts like *knowledge economy* and *mode 2* knowledge production are highly problematic and the rapid diffusion of these ideas into the practice of science may have brought scientific inquiry to "the brink of defeat" (Stengers 2011). One thing the critical commentaries on commercialization of science have in common is a critique of the ease and naturalness with which *mode 2* anti-differentiation has been prophesied to progress. The mode1/mode2 narrative of Danish research policy seems to suggest that integration is a peaceful self-directed and organic process which will work itself out in time as the interaction-agenda leads the way. However, as will be argued below, this overlooks the resilience, tensions and dissonance connected to creating new hybrid organizational forms (Tuunainen & Knuuttila 2010). Another feature of the celebratory accounts which has been criticized by the following perspectives is the tendency to suffice with the observation that "something is happening to science" (Croissant & Restivo 2001). Indeed, several authors have argued that these transformations did not just happen *as if by magic*; neither did they just evolve naturally as a consequence of a change in the way societies make use of knowledge. Looking at how *the knowledge economy* came about, it is clear that several actors have actively sought to intervene in the way science is organized and practiced.

Slaughter and Rhodes (2004, 2005) have traced the legislative basis for a more competitive research policy in the United States, a process which involved a coalition of agencies, manufacturers´ associations and councils (2005). According to the authors, this process did not occur by itself, but was an expression of conscious reprioritization based on a broad range of interests. As a consequence, public interest was framed in terms of private sector economic development (Ibid).

Another set of commentaries focuses on the impact of neoliberal science policy (Lave et al 2010, Frieckel & Moore 2006). One of the important features in the shift made in Danish research policy is the addition of knowledge *dissemination* to the traditional key missions of research and teaching. According to Lave et al. (2010) this focus on dissemination is the touchstone of neoliberal science policy as it introduces the *market* as the most efficient and adequate disseminator of knowledge. As a consequence, the commercialization of knowledge and the "rollback" of public funding constitute two important impacts of neoliberal ideology and its diffusion into policy practices and university legislation (Lave et al. 2010). A related effect is the rise of audit culture and systematic performance measurement of university knowledge production (Shore and Wright 2006, Strathern 1997).

An important point for this group of accounts is that the shift towards integration or interaction between science and industry is not an inevitable or self-directed evolution but a consciously and politically motivated strategic intervention. According to STS accounts of neoliberal science policy and the theory of academic capitalism, the impact of this intervention can and should be thoroughly examined, questioned and, if possible, resisted.

Convergence

A broad range of critical perspectives has been offered since the publication of the *New Production of Knowledge* to support the idea that things could have been otherwise. These perspectives come from multiple disciplines and provide a range of different empirical approaches to shed light on the impacts of policy-induced shifts in how universities are governed. An overall theme is a focus on *convergence*, the observation that universities and industry are becoming more alike and tend to absorb more and more features from each other within their management structure and the way tasks are organized and conducted[42].

A range of micro-sociological approaches spreading over a broad range of empirical settings have also made it clear that the impacts of changes in science policy extend far further than formal ties between university and industry and changes in patent systems (Lave et al. 2010; Kleinman 2003). Studying how social codes and practices are traded across the boundary between institutions, Kleinman and Vallas (2006) offer an elaborate account of how a policy-directed change in the logic informing university research has profound and far reaching impacts on the everyday practice and management of science. These effects pervade not only the way the university disseminates knowledge through technology transfer, industrial partnerships and patenting systems, but have also entered core academic missions of research and teaching. Looking at formal as well as informal relations between science and industry, several authors show that academia and industry are converging in a much more systematic and pervasive manner than the one mapped by celebratory macro-sociological accounts and that this convergence is in no way free of conflict or tension (Kleinman and Vallas 2006, Tuunainen 2005, Owen-Smith 2006).

Such convergence tendencies are not hard to miss when observing the working life of experimental scientists. But where the accounts of academic capitalism tend to focus on obvious and formal and quantifiable relations between institutions (patents, spin-offs, venture capital) more ethnographically oriented approaches shed light on the detailed way in which policy changes has had impacts on the everyday work in scientific practice. The story of science in the knowledge economy is not told by merely pointing to the fact that a rising curve of private industry funding is now about to cross a declining curve of public funding. In practice, matters are far more mingled than that.

In the following I will present some of the arguments in the critical literature on science/industry convergence and mix it up with a couple of examples from my fieldwork. I will focus on the traditional key missions of the modern university – publication and teaching – and outline the ways in which different contributions to the critical literature on science-industry relations have conceptualized the convergence between scientific and corporate codes and practices.

Publication mixtures

Publications are, along with teaching a core “product” of the modern university. But according to studies of corporate/academic science interfaces publication

[42] The convergence argument posed in this body of literature differs from Shapin’s account of a reversal of roles between corporate and academic science in that it is described as a uniform and pervasive tendency that threatens the integrity and independence of scientific practice. The accounts presented in this section do not portray modern science as a heterogeneous and unpredictable field of activity where free spaces for inquiry pop up in unexpected places.

practices are spreading to new areas and take on multiple purposes. Publication is not merely a matter of knowledge dissemination. Corporate science uses academic journal publications to boost credibility (Kleinman and Vallas 2003, Kleinman 2003) and in the medical sciences this convergence of practices is presently having some rather disturbing effects. Sismondo (2011) has traced the "ghost management" of publication in the medical sciences, a process where big pharmaceutical companies assign independent academic authors strategically to publications written by professional "ghost writers" employed by industry. Not surprisingly, these publications tend to show the benefits of the drug in question and thus help market new drugs to physicians. The credibility of independent, academic and disinterested science is thus acquiring an additional value as a powerful marketing tool. Sismondo estimates that within medical research around 40% of literature on recently approved drugs is more or less "ghost managed" by the pharmaceutical industry. He sarcastically adds: "despite our constant worries on this matter, academics still have high status" (2011, p.14). Given the rather shocking material, one can only wonder for how long. Disinterested science is clearly not just a valuable natural resource in terms of creating value for society by way of relevant and useful innovations. The toxic dissolution of pharmaceutical ghost writing and publication management into the authorships of academic scientists tells us that disinterested science is also a valuable resource for big corporations as it gives their products the *golden touch* of credibility.

The scary ghost world of big pharma publication is an extreme example that bears no resemblance to the practice of the scientists who are represented in this thesis. However, the tendency for corporate science to mirror academic science was still visible in parts of my fieldwork. According to several field participants, private corporations may be interested in academic science for other reasons than plugging in more advanced technology in their production. Companies that produce laboratory kits, high resolution visualization technologies, cell lines or other infrastructural products for the modern laboratory see academic scientists mainly as markets and also view their publications as potential marketing tools. If ground-breaking research is published with reference to the use of specific patented research methods or specialized equipment, the company producing the technology can benefit from an increase in citations. Conversely, having a renowned and respected scientist dismiss a commercial product can be a serious threat to corporate science. One field participant tried for months to get a specific laboratory kit to work without success. As the company providing the technology was not on the ball with getting back to him and did not show a lot of initiative in giving him appropriate economic compensation he got their undivided attention by exposing their inadequacy to colleagues. He added their manager to a CC on an email sent to his entire network of researchers across academic and corporate research that accounted for the many problems associated with this kit. Corporate "for-profit" science clearly feeds on academic "non-profit" science to strengthen credibility and market their products. Most websites selling laboratory kits will list academic publications ("technical references") to boost their credibility and the corporate scientists are as involved in academic journal publication as academic scientists. Corporate science indeed does produce masses of literature that is published in highly ranked journals and is steadily increasing in its volume (Shapin 2008).

The world of venture capital also has interests in academic publications. Talking to venture capitalists and business angels who were investing in a small spin-off company started at the Nano-Science Center, it was quite clear that academics are not the only ones who subscribe to "Science" and "Nature". Of course, venture capitalists are primarily looking for scientists that can provide a "game changing technology" and are able to convincingly "elevator pitch[43]" their idea to a room full of potential investors. Pitching scientific innovations to investors is a difficult task that takes preparation, rehearsal and lots of trial and error[44]. But that does not mean that A-list journal publications are solely a performance measure for ivory tower academics. One Danish business angel involved in spin-off companies sees prestigious publications mainly as marketing:

> "Well, he [the physicist] had his paper in Nano Science, you know, and in Nature. And this means something. And then sometime it is cited and you can feel that right away. It's not easy to say why it happens but all of a sudden someone cites it and then we suddenly have requests [from potential investors] from all over the world without really knowing why. And then it shows that it is because it has been mentioned in some journal. As long as you don't have a product, right, then such things are really important because they are "stamp of approval". A paper in Nature really means something.
>
> (Interview 2008)

According to the critical approaches in science studies, the entry of corporate or capital interests into academic journal publication is not a one way street.

[43] To those who do not hang out with scientists on an everyday basis and hence get to know words like "elevator pitch", a little clarification may be necessary. An elevator pitch means that you can pitch your idea to an investor in the time it takes for the elevator to reach the top of a New York sky scraper. If you can't sell your idea within this time frame, your idea is probably not fundable at all.

[44] It would lead too far to outline the details of how such skills are acquired. Suffice it to say that when talking to entrepreneurial scientists and venture capitalists that I met in the field the ability to pitch innovations to a room full of venture capitalists is described as an acquired skill. For most theoretical physicists such performances require training, hard work and a detailed knowledge of business plans, dress code, voice management, pace and other types of impression management not necessarily associated with the vocation of science. All of this of course has to be balanced out with the appropriate amount scientific "neediness" in order to still be authentic enough to convince investors. Basically, what the venture capitalists and business angels interviewed for this thesis say they are looking for are "game changing technologies" created by "nerds with communication skills" who can be trained to become trustworthy and charismatic performers. See Shapin 2008 (chapter 8) for a similar but much more elaborated account of the venture-pitch boot camp for North American scientists.

The above examples mainly testify to a growing convergence between academic and corporate practices, as the practice of publication planning spreads and acquires new functions. However, according to Sismondo, it also points to the reverse process. Ghost writing in the pharmaceutical industry includes the extensive use of "key opinion leaders", an alliance between big pharma companies and medical science which helps promote academic careers by ghost writing, professional publication planning, invitations to give keynotes and other "covert" ways of supporting scientists who lend credibility to medical products. One research manager at the Nano-Science Center told me that collaborating with big and well known electronics companies gave one's academic profile a boost and thereby added to his credentials. Students will be interested in you as a supervisor if you work with a brand name, and colleagues generally have respect for you if you are able to acquire a big grant from industry. Industry collaboration may then contribute to academic cycles of credit in new ways[45]. The critical convergence argument is not only concerned about publication though. Another concern for critical accounts of the commercialization of science is how convergence is reflected in university teaching and academic training.

Teaching mixtures

Slaughter and Rhodes (2004) have suggested that students are no longer the subjects of academic training when entering the university. Rather they are *markets*[46]. Not only are large universities imposing university brand culture on their students, they are also allowing for corporations to market themselves to students.

Others argue that students are turning into *products* and that higher education is turning into a production site rather than a training site. Croissant and Restivo (2001) have showed how university students become products in interactions between university and industrial corporations. According to the authors, the traffic of skilled students from the university to industry is not just a matter of disseminating fully matured candidates into professional lives in industry, they argue. The learning process itself is also a resource to industry. Having a "small but steady supply" of students trained to do specific routine tasks and with specific skills is particularly "well received" by industrial partners in science-industry collaborations. The authors argue that students and learning are becoming products and the use of academic students in

[45] Latour and Woolgar's (1986) notion of "cycles of credit" interprets the term credit as *reward* as well as *credibility*. The point is that both are part of the same cycle. Recognition, money, equipment, arguments and data enter into a cycle of credit which makes no sense if the analyst separates grant money/prestige/position from citations/arguments/inscription devices/citations. Sismondo's example suggests a further addition to the model in that corporate recognition now enters the cycle of credit and helps researchers accumulate "credit" both in form of economic rewards and credibility. This adds a ghostly halo around the credibility cycle which has not been taken into account by Latour and Woolgar, although their model was among the first to map associations of fact, value and credibility.

[46] Here it is important to note that Danish universities do not take tuition fees and are thus very different from the American universities researched by Slaughter and Rhodes.

industrial collaboration socializes them into a “patronage system of directed research” (Croissant & Restivo 2001, p.158). The new generation of academics thus learn specific interaction styles and norms of academic work by their exposure to industrial settings and this has influences on the way in which they frame scientific problems and also on their future career (Ibid; Hackett, Croissant & Schneider 2001). Croissant and Restivo draw on these observations about the socialization of students into being a resource and function as manual labor for industry to give the following rather grim predictions for the future of the academia:

> “This does not only mean that universities will be more and more likely to take on the values and organizational forms of government and industry (as those two institutions converge structurally and culturally), it also means that universities will be increasingly likely to be fully absorbed into the structures of industry. That is, they will be seen, and see themselves, as annexes, to the extent that they can sustain their material identities. They might also become obsolete as their structures and functions become increasingly redefined and molded to the interests and needs of government, the military and the worlds of industry and finance.” (Croissant & Restivo 2001)

Indeed, most of the scientists represented in this thesis will recognize that there is an ongoing traffic of students between academic and corporate science. Thomas Bjørnholm proudly claims that for the Nano-Science Center “the students are our main product”. He uses this argument repeatedly when accounting for how the center makes itself useful to society. The center *produces* a steady output of trained students that are popular in industry settings and are hired almost immediately after they leave the exam board.

The most widespread form of science-industry collaborations that I observed in the Nano-Science center was senior scientists and research managers who facilitated contact with industry partners for their bachelor and masters students in order to provide them with a field in which to investigate interesting problems for their thesis. Research managers all tell of bachelor and masters students who are sent out to companies to do work on a problem that may be relevant to industry. One research manager even phrased it “we have 13 people working in Novozymes” (a large biotech company). The “we” of the research center clearly does not stop at the gates of the university building. “We” have people placed everywhere and these people remain colleagues and do not transmute into “they”– the representatives of corporate life. Some students go on to a career in corporate science after they finish their degree; some continue their industry collaboration in their PhDs.

Feidenhans´l claims that this socialization process is a big part of his job, one that is often never talked about because the focus is always on academic merit and discoveries. Through his long career he has supervised a great number of PhD and masters students and jokingly tells me that his job is to socialize them into a bourgeois lifestyle. He tells me that some of his students will joke about him as a socialization-machine. Students enter his office with piercings, tattoos and dreadlocks and exit it with a steady job, house, mortgage and kids. One of his old students produced the below representation of this socialization process as a goodbye present after finishing her masters:

Figure 1: "The funnel of society" (Samfundstragten). Feidenhans'l is in the center right side pushing the buttons of a machine which converts ill-adapted students into a normalized, manageable workforce.

Adding to this is the presence of industry in teaching and training. Master and PhD training would entail close connections with companies producing scientific instruments, laboratory kits, cell lines and other equipment. A normal working day for both scientists and their more experienced students would naturally involve emailing with colleagues working in corporate science, answering emails from corporations interested in doing joint projects and going to corporate websites to purchase laboratory equipment or kits or look up references. In teaching settings, PhD students at the Nano-Science Center tell me that industry presentations are business as usual in academic PhD courses, sometimes talking up more than 15% of the lectures. Indeed during my fieldwork I did not attend a PhD course that did not have one or two corporate presentations.

Both Feidenhans'l's dedication to socializing students into workers and the presence of corporate presentations in PhD courses would, according to the critical convergence argument, be a sign of the transformation of what was traditionally a site for learning into a site for marketing and production. The *funnel of society* and the presence of industry in teaching settings would, according to the critical convergence argument, be just one more example of a subtle socialization processes through which undergraduate and graduate students are taught to function as both market and resource for corporate science and technology. However, this reading of the above examples would not sit well with Feidenhans'l, nor with the PhD students I met in the field. In fact, my attempt to pose critical questions about the science-industry convergence was seen as rather dull or just oddly misguided. As I initially tried to map the intertwinements between academic and corporate science by pointing to the many informal ties between them (corporate scientists are often friends, former colleagues or old students), I noticed that most field participants found it hard to understand why this is interesting or relevant at all.

Reactions in field work

Of course, the critical convergence argument is not foreign to Danish university academics in that it resembles general points made in the Danish debate on science-industry collaboration and the growing concern about freedom of research (Vedel and Gad 2011). The basic argument – science is being saturated and polluted by corporate agendas, practices and norms – is in no way unfamiliar or nonsensical to the scientists I met in the field. Some would tell me that they did indeed see a tendency for academic scientists to be pushed into more aggressive quests for funding and that the tendency to hype up one's research results is growing among colleagues. However, many are wary of "overselling" their results at conferences or to journalists and they have little respect for colleagues who do[47]. When I occasionally presented the critical convergence argument in conversations with scientists they would mostly dismiss it as simply inaccurate. One even called it a "left wing" drama that has not taken into account how science actually works and how industry plays a role in science. The mixtures of science and industry are a complex and cannot be understood by breaking them up into the solid constituents of preformed academic and industrial interests.

For example, Feidenhans'l thinks of student socialization in a much broader perspective than that of having a workforce fitted to industrial needs and to paying their taxes. He sees his job as one of fostering a creative, dedicated and interested cohort of new scientists who will engage in the new problems and find new ways of making sense of nature. It may very well be that this task entails inventing new

[47] Helmreich (2009) makes a similar observation based on extensive fieldwork in marine biology.

and useful things or participating in value creation. However, he insists that the most important thing to ensure innovative potential in science is *fun*. “If it is not fun to do research, it goes nowhere”, he says. What is interesting perhaps, is that going for fun and creativity does not necessarily exclude engaging in industry collaboration.

In line with Per Hedegård, many other field participants would claim that the presence of industry is absolutely necessary for the practice of science. Not merely as a source of funding but for all kinds of reasons. First, academic scientists have colleagues who are spread across academic and corporate science and this requires academic scientists to collaborate with both. As stated above, (and shown in great detail in Shapin 2008) scientists often address a “we” defined by research interests, not a “they” defined on institutional location. Second, corporate science often has access to equipment and techniques that academic scientists could never afford, and the academic scientists who were well connected did indeed make extensive use of industry contacts to get access to equipment and experimental infrastructure. Third, industry funding adds to your credentials but is not by any means a substitute for them. The scientists and research managers that I talked to made it very clear that industrial collaborations have to be fitted to their own long term academic interests. If not carefully managed and selected, industry funding or collaboration could potentially damage or derail academic careers.

As for the presence of corporate presentations in academic training, PhD students would make it very clear that corporate science clearly has its place in a PhD course in a natural science department. Natural scientists need infrastructures to a point that completely sets them apart from social science. The equipment used in laboratories *is* produced by corporations (often by colleagues). Academic science lives and breathes through industry in that it cannot go about its business without technology platforms that are for the most produced in corporate settings. Whilst more or less directly marketing a specific software to use in university research may have caused some trouble in my social science home, the corporate presentations and training settings were business as usual for natural science students. Whereas I would conduct my research equipped with a few software packages, a recording device and a laptop, the PhD students I met in the field needed access to equipment and infrastructure so expensive that they were often shared between institutions or even nations. Needless to say, research infrastructure is big business in itself, and it draws together actors from a great variety of institutional settings.

Getting into field work, it seemed that there was no clear or univocal separation between academic science and its outside and no single way of judging the consequences and effects of science-industry convergence. Making a critical argument about science-industry convergence is not a simple operation of mapping the ways in which capital corrupts academia. The entwinement between academic science and corporate development is not a straightforward matter of going from *insights to invoice*.

The mixtures of knowledge economy, at least as they blended into my fieldwork, are more heterogeneous and unpredictable than that.

Critique and analytical dissection

The two positions outlined in this chapter are expressions of a dilemma or a debate about how to study science in the knowledge economy. The constructivist argument posed by Shapin, Latour and Callon was predominantly descriptive and highlighted the transversal lines and boundary-blurring effects that science as a practice seems to create. The critical convergence-stories, by contrast, argued for questioning and resisting the new mixtures in science. Critical studies of commercialization do not identify many occasions to celebrate the scientific gladiators and hail the project baron. The work of project barons and champagne drinking scientists may be impressive when viewed from the point of view of the laboratory. However, critical convergence studies claim that it is precisely this work which is helping to slowly erode the boundaries of the university and brings academic science to its knees as university researchers become active initiators of academic capitalism. As for-profit and non-profit science converge and take up more and more of each other's features, critical commentaries become more and more wary of assuming that this process is random. Rather, as argued by Kleinman and Vallas, it may be a form of mixture where capital always comes out in top:

> "We call this process *asymmetrical convergence* because although the emerging hybrid regimes are constructed of codes and practices from both sides of the divide between industry and academia "in the last instance" it is the logic of profit that is shaping the process. Industry adopts attributes of academic culture in the interest of increasing profitability, and academia draws on private-sector codes and practices for either directly commercial purposes or indirectly because of the legitimacy universities gain by adopting elements of commercial culture ". (Kleinman and Vallas 2003, p. 37)

Unlike the descriptive account of Shapin and the initial observations I made in the field, the bottom line for the critical accounts of how corporate science and university science converge is that academic science is watered down, drowned or submerged by capitalist interests and industrial agendas. In this light, the Latourian image of the scientist-gladiator who wins the trial of strength looks somewhat bloated.

Kleinman (2003) does not see science as a new and unknown form of power that subverts political agendas, institutional structures, managerial hierarchies and the micro/macro divide. No need to call up the reserves. In fact, he argues, we need to hold on to some of the very same binary distinctions which Latour and Callon advocated rethinking. In the knowledge economy, scientists are not necessarily free to make transversal connections and form social groups at will, he claims. This process is structured by more overarching processes.

Kleinman's book "Impure Cultures" ends with an essay that directly responds to the stories of mixture and destabilization provided by Latour. "It takes more than a laboratory to raise a world", Kleinman argues in reply to Latour. Based on extensive field work in an American laboratory, Kleinman critiques Latour and Callon's faith in the scientific laboratory as a scale-destabilizing device and sees a danger in their attempt to make a mixture-argument. The sociology of translation insists that no prior distinction between the social, technical, political and institutional be assumed before the inquiry (Callon 1986, Latour 2005). However, Kleinman argues that the attempt to "raise the world" and create powerful networks by way of the laboratory may very well be carrying with it a much more subtle and corrosive erosion of university boundaries. This erosion, he claims, is not analyzable when the analyst blurs the distinctions between what is outside the laboratory bench (social) and what is inside (technical).

> "Callon never examines what it means for something to become "distinctly" technical or social. He does not entertain the possibility that while boundaries between the social and the technical may, at times, be indistinct, there might be good analytical reasons for highlighting the differences." (Kleinmann 2003, p.140)

If one wants to examine the costs and trade-offs associated with the mixtures of knowledge economy, Kleinman argues, a set of analytical separations will be necessary. "Social factors" (such as funding possibilities, the market, commercial potential, public opinion, and politics) are prior to "technical factors (such as making fuel cells or biotechnology work). Without *first* taking the social factors into account, scientists will never get to "inside" of technical factors (Ibid p. 140-141). According to Kleinman and Vallas' idea of asymmetrical convergence, science may be a mixture, but capital and government interest comes first and profit always comes out on top

The critical accounts outlined above thus seem to base their normative position in an analytical move of making a-priori distinctions between the university and its outside. Even if the practice of science shows that boundaries have always been blurred Kleinman argues for pragmatically analyzing them as such in order to map asymmetries and costs. This assumption seems to be shared among the critical commentators presented above. By talking about convergences, hybrid regimes, erosion of boundaries or academic capitalism, all of the above authors point to the same kind of process: something that was once a separate and autonomous space is now being infused or polluted with an external logic. However, as demonstrated above, this binary vocabulary does not seem to do sufficient analytical work when talking to the field participants represented above. The heterogeneity and complexity of scientific practice as field participants engaged with it poses an analytical challenge, especially if one wants to retain a critical perspective.

Separating out and giving analytical priority to social, economic, and political constituents in the knowledge economy mixture was therefore not a viable strategy in my fieldwork. Allow me to go into more detail with a short example from an interview I did with a scientist about his strategic engagement with capital and government interests. The description below shows how this particular field participant resisted my analytical separation between the social and the technical and questioned my tendency to give priority to the former.

The icing and the cake

In the fourth interview with plant biologist and research manager Birger Lindberg Møller, I was asking questions about managerial decision making and his relations to industry partners and corporate science. This took us straight into the complex business of patenting (or rather the politics and strategy of patenting). Knowing how to write a patent is very, very important in Møller's area of research. If you do not write a patent, others might decide to do so even though they have not conducted the experiments. If you do not have ownership to the patents, you may run the risk of losing the opportunity of commercial use of your results and reduce the interest of putative industrial partners. A patent holder may not even want to use the patent but will just want ownership of it to avoid competition with her/his own patented products. Your research may end up in a corporate drawer somewhere if you do not get into the game of patenting. Up until this point, the "social" part of managing plant biology has defined most of the interactions and interviews I have done with Møller. As a helpful informant, he willingly answered questions on how to stay afloat and maneuver through the muddled waters of the knowledge economy. However, while replying to one of my questions on the politics and strategies of patenting, Møller felt the need to contest my questioning:

> BLM: so there is a lot of politics in this … (pauses, looks up) we are talking a lot of politics in this conversation! Why are we not talking about research?
> BGH: Well, from the way you seem to work the two seem to be closely related.
> BLM: Yeah, but you always ask me about the political stuff instead of asking me about the stuff I do!
> BGH: Well at some point I would actually like to hear more about the stuff you *really* do

BLM: Yeah. Yes, this here is only the icing on the cake - The stuff around it.
BGH: Hmm are you sure about that?
BLM: I am!
BGH: And … because?
BLM: Well… it just so happens that it is a very important icing
(Interview 2009)

In the following interviews, Møller repeatedly insisted that what he does is *research*. Since this is his real job. All the political stuff, the networking, the public talks, the press, politics, the industry collaborations, the strategic maneuvers, the grant application activities are "icing on the cake", he says.

As any good STS student, I had of course read Latour's "Science in Action" and was very intrigued with the portrait of scientific practice as the two-faced god, Janus. When met with Møller's reluctance to talk about "politics", I immediately concluded for myself that this was to be interpreted as an act of *purification;* Møller was trying to guide me through the grandiose front door of ready-made scientific facts and denying access to the back door of *science in the making*. Of course, I insisted on being shown the back door; I wanted detailed knowledge of how Møller draws together laboratories, politics and capital in order to construct a successful research program. I readily attributed to Møller a strategy of dropping modalities deleting all the messy hybrids that make up science. However, as the interviews moved on, I realized that something different was going on. Møller was not trying to strip science of all its social, economic or political hybrids in order to reduce complexity and give me a polished picture. Rather than trying to reduce complexity and gain facticity, he was complaining that my questions were not complexity-sensitive enough.

According to the argument posed by Kleinman above, there is no science without someone first making sure that it is fundable. Kleinman argues for the necessity of making an analytical distinction between the social and the technical in that the social (fundability, mingling, politics) comes "before" the technical (biology, plants, laboratories). However, when translated to a strategy for conducting interviews with a scientist like Møller, this analytical separation ran into some difficulties. Møller kept complaining that we had not touched the heart of the matter at all.

Even though Møller's insistence that we started out in the laboratory and branched out from there seems to resonate well with the sociology of translation, I will argue that his resistance to my questions took my account much further into mixture than the sociology of translation had done before. True, Møller's practice simultaneously mixes plants, gene sequences, excel sheets, patent laws and policy documents and in many way this sounds like Latour's plea for studying scientists by "mixing hydrogen bonds with deadlines" (Latour 1987). However, to Møller, science is much more than "politics by other means".

If taken seriously, Møller's protest to my incessant questions about what he terms as "politics" is a plea for *increasing complexity* not an attempt to purify and reduce scientific practice to ready-made facts. Latour would claim that we need to trace the heterogeneous networks between the technical and the social. However, my attempt to do exactly that was heard by Møller as a one-sided focus on "politics", even if I was trying to map it through scientific, that is, *other means*. According to Møller, I would miss the point if stuck to stories about network formations, chains of associations and the formation of new interest groups around the construction of scientific facts. When making his icing-on-the-cake-argument, he is not telling me that the icing is irrelevant and the cake should be stripped of icing in order to be properly understood. On the contrary, he says "it happens to be a very important icing". What he says is that the interviewer has reduced complexity to the point of no point when starting out always and only with what he considers to be the political or "social" part of his work. By sticking to what is interesting to a social scientist, the interviewer is missing the opportunity to get into the details of plant biology. In other words his protest would mean something like: *Fair enough, icing is really important, but where's the cake*? According to Møller, we have not touched the heart of the matter.

A stomach for complexity?

The critical studies of science-industry convergence seem to assume the erosion of boundaries which were once well protected by institutional and moral order. Interestingly, the same analytical move is made by those who celebrate this erosion: the macro-sociological science studies whose concepts are being reiterated in Danish research policy. The celebration of the intensified interaction between university, industry and government is similarly based on an analytical strategy of contrasting it with a prior state of separateness: the *mode 1* university. Hence, in both the celebration and the critique of knowledge economy, the normative stance on mixture seems to be based on an analytical operation of separating a mixture into its solid constituents. In chapter two I showed how STS vocabularies made possible a view on boundary blurring as progress. In this chapter I have shown how a different branch of STS claims that boundary blurring is corrosive of scientific practice. Boundary blurring, and hence an assumption of a prior separation, nevertheless remain the basic assumption of both approaches.

As argued by Shapin, when portraying science in the knowledge economy as part of a shift towards erosion, integration and blurring of boundaries (critical or celebratory), we insert a normative "fault line" in our accounts and fail to recognize that natural scientists may not see things in terms of the same separations. Their work seems to depend on the *absence* of a gap between the university and its outside, rather than an attempt to either cross over it or keep things separate. Anthropologist, Marilyn Strathern, has pointed out how the focus on integration of science, industry and government paradoxically creates the very separation that it is trying to overcome. When focusing on bridging the gap from insights to invoice, the university is implicitly given a rather odd position "outside" society (Strathern 2005). The celebratory accounts of boundary blurring and the Danish interaction agenda can thus be said to be guided by an implicit assumption that science is an exterior to societal value creation and needs to be unified with society. The critical accounts, paradoxically, make a similar argument, although "in reverse": academia and scientific practice is in itself "outside society" and needs to be kept separate from polluting and corrosive influences in order to retain its freedom.

Mirowski and Sent have identified a tendency for STS accounts of the commercialization of science to take exactly this kind of "step 1/step 2 narrative" (Mirowski and Sent 2008, p 636). Step 1 implies a past where the university was more pure and isolated and step 2 is invoked as new era of boundary blurring or collapse (2008). Similar to the scientists represented above, the authors argue that whether "upbeat or downbeat" the step1/step 2 model is too crude (Ibid). It tends to oversimplify the subtlety and complexity of the problem of commercialization and to ignore the historically complex interrelations between science and industry. The authors attribute this tendency towards oversimplifications to the fact that micro sociological approaches within STS were rather late in turning their nuanced and complexity-sensitive gaze towards the problem of commercialization. STS scholars instead focused their attention on fact construction inside the laboratory and vastly ignored the fact that important structural reconfigurations were taking place in the way science was funded and governed[48]:

> "Consequently, many contemporary discussions of the commercialization of science have proved deeply unsatisfying, tethered as they are to totemic monolithic abstractions of Science and The Market pushing each other around in platonic hyperspace".
>
> (Mirowski and Sent 2008 p. 636)

This platonic hyperspace situates science as by nature outside the social, outside politics, outside society. The same monolithic abstractions can be identified in the integration agenda of Danish research policy as outlined in the previous chapter. The scientists I encountered in fieldwork, however, insisted on telling more mingled and complex stories than those outlined by policy makers, critical social scientists and in some cases even by the sociology of translation.

[48] See Kleinmann 2003 for a similar argument.

Admittedly, the sociology of translation does not start out in dualism and gaps. However, it may be fair to say that it never really became a sociology of mixtures either. In the work of Latour and Callon mixtures are still to some extent analyzed by redirecting the emphasis towards the *destabilization* of existing science/society or micro/macro scales. Claiming that such distinctions do not exist in the empirical reality of scientific practice seems no hindrance from putting great binary distinctions center stage in the argument as that which must be overcome analytically[49]. Looking at destabilizations and hybridizations between known analytical categories is equivalent to watching sugar dissolve into water. Does such an inquiry allow us to know what particular mixtures taste like and what they do? One could argue that Latour and Callon have perhaps not quite finished their drink yet.

In light of the two types of step1/step2 narratives outlined up to now, it seems we need to think again before we claim to know what is really happening to scientific practice in the knowledge economy. As pointed out by Shapin natural scientists are much better at dealing with mixtures than social scientists and humanists are:

> "Indeed, one of the causes of the undeniably poor understanding that academic humanists and social scientists still have of their natural scientific and engineering colleagues, and of their continuing attraction to a simplistic language of binary opposition, must have something to do with the intense organizational conservatism of the former compared with the vertiginous organizational innovation and heterogeneity of the latter." (Shapin 2008, p. 265).

Maybe the natural scientists of Shapin's study, like those I encountered in the field, do have more of a stomach for complexity than we in the social sciences. This of course poses a challenge to analysis if analysis is approached as a task of "untying" and thereby destroying the delicate entanglements that make up scientific practice by separating them out into solid constituents. Take policy vocabularies out of the study of research management and you will understand nothing. Take industry and research councils out of the equation and the same things happens. And if we are to take Møller's question seriously: delete natural objects, instruments and scientific conceptualizations from the account and we will miss the point altogether. It all hangs together like sugar dissolved in water; one sip and you have already consumed the whole lot. Donna Haraway sums up the mixture problem by arguing that we should not view scientific

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[49] See Latour 1988 for an example of this approach. Latour and Callon's focus on destabilization and hybridization across classical analytical distinctions is part of a larger argument in relation to the existing literature in science studies available at the time. As the sociology of translation was launched as an alternative approach to the *sociology of scientific knowledge* a lot of work was done to argue against prior distinctions between formal and informal relations, social and technical factors and most of all human and non-human agency. See Collins and Yearly (1992 and 1992a) and Callon & Latour (1992) for the famous "epistemological chicken" debate between the two approaches in science studies.

practice as a set of dissolving constituents but rather drink up and find out what kinds of mixtures are being made and see what they do:

> "Technoscience provokes an interest in zones of implosion, more than in boundaries, crossed or not. The most interesting question is: What forms of life survive and flourish in those dense, imploded zones?" (Haraway 1994, p.62)

The fact that mixtures are "unanalyzable" (read: impossible to untie or divide) does, however, not mean that they are unknowable (Brown 2011). In my attempt to map the maneuvers and strategies of scientists as they respond to the interaction-agenda of Danish research policy, Haraway's question seems more important to the task at hand for this thesis and indeed to the field participants than mapping the erosion of boundaries between academia and industry. Rather than looking at destabilization, convergence and boundary blurring, I will try to give an account of the mixtures that scientists engage in that can perhaps give an idea of what they taste like. What kinds of scientists survive and flourish when living on this particular mixture and how do they do it?

Starting out in mixtures, to be sure, does not exclude critique. On the contrary, I would claim that it is precisely *because* things are mixed to begin with that we need to keep track of what kinds of mixtures are being created, how they work, what they do, who comes out on top, when and why. As the few examples from fieldwork I have provided here have already hinted, this is in no way univocal, and academic scientists do not have a single strategy by which they maneuver. Sometimes industry funding gives prestige, sometimes it erodes credibility, sometimes science creates revolutions and guides industry, and sometimes "they take your freedom away". We cannot allow ourselves to be too sure about the outcomes of "interaction" prior to the inquiry. Analyzing the mixtures of knowledge economy is in every way an experimental endeavor whose outcome cannot be mapped in advance. The way forward, it seems, is to experiment. Rather than speculate endlessly on whether or not the sugar has now fully dissolved, we must instead simply drink up.

If taken as a starting point of analysis rather than as an end point, the idea of mixtures may offer an entry into posing a critique from within the practice of science in the knowledge economy, rather than assuming that we already know what the problem is in advance (either: "we need more science-industry interaction", or:"capital always comes out on top"). It may not be enough to lament the fact that boundaries are dissolving and practices are converging. Waiting for knowledge economy to separate out again could perhaps be more dangerous than to finish the drink. There is always the hope that we acquire the stomach for complexity along the way.

3
Lateral analysis

The wolf´s game

The reason of the strongest is always best.
We will show this shortly.
A lamb quenched his thirst
In the current of a pure stream,
A fasting wolf arrives, looking for adventure
And whom hunger draws to this place
“Who makes you so bold as to muddy my drink”?
Said the animal full of rage:
“You will be punished for your temerity”
“Sire”, answers the lamb, “may it please Your Majesty
Not to become angry;
But rather let him consider
That I am quenching my thirst
In the stream
More than *twenty* steps below Him;
And that, as a result, in no way
Can I muddy His drink.
“You muddy it”, responded this cruel beast;
“And I know that you slandered me last year”
“How could I have done so, if I had not yet been born?”
Responded the lamb; I am not yet weaned”
“If it is not you, then it is your brother”
“I do not have any”.
“Then it is one of your clan;
For you hardly spare me
You, your shepherds and your dogs
I have been told: I must avenge myself”
Upon which, deep into the woods
The wolf carries him off and then eats him,
Without any other form of *process*.
(La Fontaine, “the lamb and the wolf” as presented in Serres 1979).

A game of cat´s cradle

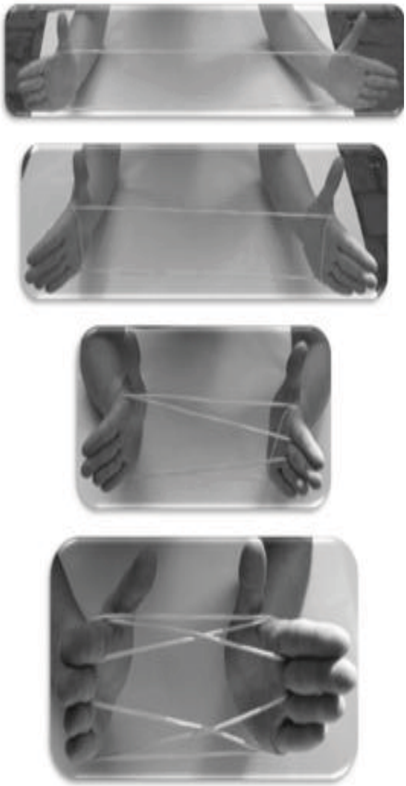

Figure 2: Cat’s Cradle

What could it mean analytically and methodologically to acquire a stomach for complexity? How does one analyze mixtures without breaking them down into solid constituents? With these questions in mind this chapter will argue that for a *lateral* rather than a *hierarchical* approach to analysis. I will argue that two of the accounts of science in the knowledge economy we have been presented with so far make their argument by approaching the task of analysis as one of superimposing a hierarchically ordered structure on the relation between the conceptual and the empirical. Inspired by Serres, I will, first, name this approach to doing analysis *The wolf´s game*. The wolf´s game is a hierarchical game of representation where the analyst always wins; a game that easily reduces the empirical to a resource for conceptualization. Second, I will suggest an alternative approach to analysis. Contrary to the hierarchical model of representation, this approach is characterized by a comparative mode of analysis that refrains from setting up a hierarchical order between the conceptual and the empirical. Two quite different sources of inspiration feed into this argument. The first is the philosophical work of Isabelle Stengers, which invites us to think of the relationship between the conceptual and the empirical in terms of an *ecology* of living and interdependent practices rather than an unrestrained diffusion of ideas into a silenced empirical world. The second source of inspiration comes from social anthropology and suggests that the relationship between the "empirical" practices and "conceptual" practices consists of lateral back and forth movements in which the two blend into and draw on each other (Strathern 1999, Maurer 2005). Inspired by Donna Haraway, I think of the ecological and lateral approach to analysis in terms of *a game of cat´s cradle* (1994). Doing a "lateral analysis" means establishing partial connections (Strathern 2004) with the actors of the field in such a way that the analyst and the field exchange properties and analytical distinctions and engage in a transformative encounter. Contrary to the wolf´s game, a game of cat´s cradle cannot be "won" by any of the participants; it is primarily an experimental endeavor.

Finally, I will show how the game of cat´s cradle was played in the context of doing fieldwork and writing up the lateral analysis that constitute the three following chapters. The collective work of the cat´s cradle is not thought of as a strategy to solve the problems of the field by presenting an accurate description. Rather than producing a correct and generalizable representation, lateral analysis is the elicited product of an *encounter* in which relations are articulated and crafted to embrace complexity. Rather than filtering things *out* and reducing mixtures to solid constituents, I will argue for a process of analysis that *adds* incommensurable elements to the existing mixtures in order to bring out new or unanticipated flavors.

The conceptual and the empirical

In chapter one, we saw concepts from macro-sociological science and technology being reiterated in Danish research policy. Following the performative turn in science studies one could claim that the changes taking place in the way university research is governed and managed is an expression of a "looping effect" where concepts that supposedly describe scientific practice are being reiterated in a way that makes them bring about the phenomenon that they named. A related problem arrives when we look at the critical, micro-sociological approaches in STS described in chapter two. Since these concepts make use of binary distinctions to make sense of the field, the use of these concepts in analysis will invite descriptions to reiterate their basic assumption of a divide between science and industry. The same binary distinctions between science and industry have been reiterated in the Danish debate on the university sector, and critique is often phrased as a fear that the academic university is becoming polluted or corrupted by capital or government interests (Vedel & Gad 2011). In this way, both celebratory and critical accounts of what the knowledge economy is doing to academic science, take off from an analytical vocabulary founded on binary distinctions.

My primary aim is not to establish whether or not this binary vocabulary has, in fact, had a performatively "looping" effect on scientific practice. Rather, I am pointing to the possible loops between description and practice to stress that the conceptual and the empirical are obviously not separate domains of reality; they may readily *spill over into* one another. They may also enter into peculiar mixtures. The introduction to the fieldwork and the STS commentary I gave in chapter two showed that ideas do not unproblematically map on to practices; much less do they determine them. However, neither do they hover around practices in an abstract heaven of ideas. It should be no surprise to find that scientists, too, *think*, and that they engage with the conceptual attempts of others to map, measure and represent their practice[50] The conceptual distinctions of policy makers are thus not only an external commentary but rather *part of the phenomenon* to be understood, because these distinctions are also known, used strategically, rejected, laughed at or simply ignored by field practitioners.

The same is of course true for my own conceptual vocabulary derived from the STS literature. Indeed, Hedegård felt the need to laugh and reject the basic analytical distinctions that guided the beginning of my inquiries. Likewise, most of the scientists I met through fieldwork had elaborate reflections and specific strategies in response to the changes they perceived in the way universities are funded, governed and managed. More importantly, my field participants also insisted on being approached as more than practitioners, namely as researchers – that is *thinkers*. STS has strong tradition for approaching science as *practice*, taking the Latourian back door of science

[50] Anthropologist Annelise Riles has pointed to this problem of collapse between field and commentary as her field practitioners are engaged in the same kind of activity as herself. Conceptualizing is part of the practice of field participants - they do what we do (Riles 2001).

in empirical action rather than the grandiose front door of science as conceptual abstraction. However, as pointed out by STS scholars Casper Bruun Jensen and Geoffrey Bowker, the habit of addressing scientists as practicing actors rather than as thinkers misses out that which makes science important and exciting to scientists (Jensen and Bowker 2011, see also Stengers 2011). Furthermore, Jensen and Bowker argue, the "practice turn" in STS cements a strict separation between the conceptual and the empirical. This is rather odd for a field that has branded itself on studying the creation of theories, concepts and scientific facts. Should science studies really make a strict division of labor where scientists are reduced to a mute reservoir of empirical "practices" and all the conceptual "thinking" is delegated to the STS scholar? Jensen and Bowker suggest that STS would do well to relieve itself of the burden to "think *representationally* about concepts, *realistically* about the empirical and *dualistically* about the relation between word and world" (Jensen and Bowker 2011 p. 4). Taking a fresh look at the conceptual/empirical divide may be a creative way out of a field that is increasingly becoming an eternal spawn of new case studies that merely exemplify or reiterate well-established theoretical perspectives, the authors argue (ibid)[51]. In invoking this critique of the conceptual/empirical divide I am thus *not* addressing their peculiar mixtures in Danish research policy in order to pave the way for another case study exemplifying, for instance, the brilliant points made by Mackenzie and Callon in their studies of performative economic theory. Rather, I am making the observation that concepts and practices tend to *blend into one another* to suggest that it may be worthwhile to experiment with alternatives to the above division of labor between the scientist and the STS scholars. In line with Jensen and Bowker I would like to propose that the relation between the conceptual and the empirical is fundamentally uncertain and should be "up for grabs", especially in fields where the actors' self-described job is to conceptualize (Ibid.).

Binary analytical vocabularies are thus not the main problem for the following discussion. The problem is rather the *manner* in which conceptual vocabularies are adopted and deployed in the process of analysis. Trying to avoid simply reiterating and exemplifying the conceptual distinctions by which we already make sense of the relationship between science, industry and government, requires thinking about how to set up the task of analysis. To return to the two binary accounts of knowledge economy it is worth pointing out that there are similarities in the analytical setup that guides the arguments made by Danish research policy and those

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[51] For a timely critique of the development of STS from a frontier of analytical inventiveness towards "normal science" see Beaulieu, Scharnhorst and Wouters´ amusing paper "Not Another Case Study" (2007) The authors reflect on the increasing tendency for the A-journals of the discipline to publish an ever growing number of case studies that merely reiterate or confirm ground-breaking analytical work previously done by leading theoretical figures of the field. *Yet another* case study mapping processes of translation, trading zones or human-nonhuman mingling may not really be what STS needs at this point. The argument of the paper is towards the development of "middle-range" theory; however this argument still does not fundamentally question the conceptual/empirical divide that I am addressing here.

made by critical social scientists. In the following, I show that the binary vocabulary of the *New Production of Knowledge* is deployed by Hans Müller Petersen *in the same manner*, as the binary vocabulary of the critical convergence argument are deployed by the authors presented in chapter two. Both follow a similar hierarchical model of analysis, or what Michel Serres would call *the wolf´s game*.

The wolf´s game – analytical predator/prey relations

In the very amusing paper: "The Algebra of Literature", Serres works his way through the La Fontaine fable "The wolf and the lamb" by way of a mathematical analysis. Doing so he shows that the tale of the wolf and the lamb situated along a stream, exhibits a specific kind of ordered structure (Serres 1979). *The wolf´s game* is Serres' name for this structure: in essence a game of having the last word. It is a hierarchical game with only one winner and the winner happens to be always a wolf. The wolf sets up his business so that he is in position to place final judgment and assign causes without having to take into account the arguments of the lamb. The upstream/ downstream order of the fable is what sets up this hierarchy.

In the fable, the wolf asserts its right to kill the lamb by accusing it of muddying his drink. The lamb, of course, protests. In the course of their conversation, the wolf nevertheless legitimizes killing his prey by taking over the lamb´s downstream position. At first, the lamb rightfully points to its position *twenty steps* below the wolf and claims that "as a result, in no way can I muddy his drink". But the wolf is not having it: "I know it was you who slandered me last year" he continues. The lamb protests by pointing to his downstream position in this new order of time: "How could I have done so, had I not yet been born" … "I am not yet weaned", he argues. However, contestation is useless once we play by wolf-rules.

The wolf wins the game by *switching analytical* position: despite his obvious upstream position he manages to argue that he is really downstream in relation to the lamb. He does this by introducing a *new conceptual order* in which the lamb is relocated upstream from the wolf. The new order is successfully set by introducing the concept of the clan. By referring to the clan as a more encompassing context for the actions of the lamb, the wolf reduces the lamb to an empirical *example of* a more general phenomenon ("you, your shepherds and your dogs"). The lamb is now placed upstream and is consequently the cause of the miseries of the fasting wolf ("for you hardly spare me"). From this downstream position the wolf has the privilege of final judgment and undisputable conclusion: "I *must* avenge myself". In short, the wolf´s game involves the invention of a system of reasoning in which the conceptually informed judgment of the analyst has more authority than the empirical practice of the analyzed; a system in which the reason of the strongest is always best. The wolf-analyst is consequently free to eat his empirical prey "without any other form of process".

In a sense, we have already seen the wolf game being played in chapter one and two. I propose, for example, that Hans Müller Petersen whom we met in the first chapter is quite a wolf![52] The Danish Agency of Science, Technology and Innovation can, by referring to *the New Production of Knowledge* and a larger set of OECD statistics, allow itself to disregard the arguments presented by academic scientists as a last paroxysm of a dying world. In the order introduced by Hans Müller Petersen, protesting scientists are merely empirical *examples* of the mode 1 system of knowledge production that has by now become anachronistic. The reason of the strongest here concludes, based on conceptual work done by Gibbons et al. 1994, that the world has moved on from mode 1 to mode 2 long ago and that this event took place "upstream" in time. Consequently, Danish research policy is free to proceed with its "interaction" agenda without any further form of process, regardless of fleeing or laughing physicists and wild protests from the academic community.

In a more sophisticated manner, the critical convergence argument can be said to be similarly engaged in a wolf-game. Critical narratives of science-industry convergence brings into analysis an order, in which the actors of the field become mere *examples of* a more general order conceptualized as for example "academic capitalism" or "asymmetrical convergence". This example is then given a historical position "upstream" from the point of the analyst. Critical STS scholars are consequently free to disregard the arguments posed by Per Heedegaard in chapter two and to misread Robert Feidenhans´l's function in the socialization of students. Authors like Croissant and Restivo or Kleinmann and Vallas would perhaps respond to the laughing physicist by pointing out that Hedegaard is unaccountable as a witness to account for the muddying of the waters of Danish knowledge economy as he is himself already swimming in them already. Hedegård is here approached as a practitioner, not a thinker. His viewpoints are empirical and muddled, while the viewpoint of the STS scholar is conceptually and critically clear. From there, it is straight off to the woods.

In short, both the policy maker and the critical social scientist's accounts of science take on the task of analysis as a wolf´s game, a hierarchical mode of analysis where the empirical and the conceptual are strictly separated and placed on different scales. Both accounts proceed by tactically jumping to a downstream position from where they can assign causes, insert a higher conceptual order and claim to have a more encompassing perspective. Per Hedegård may laugh as loud as he likes, the physicists may flee to new and better research environments but this makes no difference for the inquiry. Whether we take on the binary perspective of Danish research policy or the

[52] And by placing him in this conceptual order and labeling him as such of course, so am I! Bear with the example for now, my business in this thesis is not with Müller Petersen as an empirical object of study but with the scientists I have followed in the field.

binary perspective of the critical convergence argument, the statements of field participants will be placed on a hierarchical axis in which the conceptual form a higher context than that of the empirical practices. Consequently the complexity exhibited in the muddled mixtures of scientific practice will be superimposed with a binary vocabulary. Protests that do not map on to binary distinctions will have little consequences for a wolfish analysis.

Similar to Serres, philosopher Isabelle Stengers has characterized the tendency for representationalist practices to engage in wolf-games. Stengers makes sense of such tendencies in terms of a *predator/prey relationship* (Stengers 2011). In representing one´s object of study, a predator/prey relation can be said to be present when the objectivity of the analyst is strengthened by refraining from taking seriously the conceptualizations generated by informants in the field. This game is tempting for anyone who engages in the practice of representation.

> "Practices that maintain a stronger definition of objectivity will freely define others as potential prey; and all sciences will define as prey whatever is not scientific" (Stengers 2011, p. 60)

Predator/prey relations are unilateral, just like the relation between the wolf and the lamb. One captures and consumes the other and the story ends. According to Stengers, a unilateral relation between the analytical and the empirical is taking place when the analyst imposes categories on the object of study which "do not concern it" (Stengers 2011, 57). Predator/prey relationships are present whenever irrelevant questions are asked and conceptualizations are made only in the vocabulary of the analyst[53]. The wolf´s game is a predator/prey relation in that it subsumes the protests of the lamb under the order instantiated by the wolf. The lamb does not need to agree, understand or even relate to the terms by which it is being made an example of a more encompassing context.

An ecological approach to analysis

As an alternative to the predator/prey relationship, Stengers introduces the notion of *ecology*, to highlight both the *political* and *scientific* meaning of this term. Stengers finds analytical resources in the notion of ecologies since they imply a population of life forms that cannot be understood outside of their relation to other life forms in their habitat. Each species are part of a complex ecology and therefore exhibit an "immanent mode of existence" (Stengers 2010). It cannot be reduced to a cog in a

[53] See Latour 1997 who in the foreword to Stengers' book "Power and Invention" highlight the normative "shibboleth" proposed by Stengers to discriminate between good and bad research. Another exposition of Stengers' argument is found in Latour 2004 and in Stengers 2000 and 2000a.

machine or to a functional part because it rests on unstable and mutually dependent relational processes.

> "Ecology is not a science of functions. The populations whose entangled coexistence it describes are not fully defined by the respective roles they play in that entanglement, in such a way that we could deduce the identity of each on the basis of its role. (Stengers 2010 p.34)

This notion of an ecology as a mass of mutually dependent populations each of which has its own immanent mode of existence but nevertheless depends on the presence of the others. The ecological approach is not an invocation of a greater "system" or a more encompassing "whole", where all the individual parts have their respective function. Rather, ecological relationships are "metastable" and risky; they thrive on the constant engagements, spillovers, mixtures and making of relations between populations. Science, Stengers argues, is an "ecology of practices" (2005, 2010). This observation puts some restraints on how I can proceed: both in terms of analyzing the strategies and practices of the scientists we meet in this thesis and in terms of the kinds of conclusions I can allow myself to draw about their implications and consequences. When approaching science as an ecology of practices, I need to remain sensitive to the singularity of the specific practices and examine them as particular "species" in relation to a "habitat" or an "environment". As argued by Stengers (and by one of the scientists we meet in later chapters) environments do not exactly *determine* the behavior of the populations living in them. Thinking ecologically, rather, is thinking "*par le milieu*", that is, *thinking through* the middle of an environment rather than *thinking about* it from grounding normative definitions or an "ideal sky" of abstract ideas or prepackaged conceptual orders (Stengers 2005) [54]. Stengers' notion of ecologies of practice is thus a very particular "tool for thinking" (2005).

Drawing on the philosophy of Gilles Deleuze, Stengers argues for an approach to analysis that turns away from *recognizing* science in terms of an established conceptual or analytical order to instead engage in *thinking* with it or "in the presence of it" [55]. The ecological approach thus does not allow me to engage analytically with the

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[54] For an excellent anthropological equivalent of thinking *through* the things we encounter during field work rather than thinking *about* them, see Henare, Holbraad and Wastell 2007. The anthropological agenda of *thinking through things* shares many features with Stengers philosophical work. It is likely that their parallel lines of thought are due to their common inspiration from the philosophy of Deleuze and that of Latour.

[55] Here Stengers draws on philosopher Gilles Deleuze's distinction between *thinking* and *recognition* (2004, chapter 3) and the turn away from interpretation and towards experimentation that seems to follow it as a methodological requirement (Deleuze and Parnet 1987). In Deleuze, experiments with new concepts are preferred over interpretations guided by existing vocabularies. Similarly, Stengers seems to have little passion for questions like "what does it mean" and rather occupies herself with making proposals based on questions like "what can it become?". Deleuze and Guattari claims that the best way to follow a great philosopher is to "do what they did" rather than "repeat what they said", that it is never take for granted that we already know what constitutes the relevant problem of our field and always allow the empirical encounter to transform the categories by which we make sense of it (Deleuze and Guattari

practice of scientists as if they were passively affected by policy concepts. Placing scientists in the realm of practicing actors determined by a higher upstream conceptual order that unilaterally shapes their actions and strategies would be an analytical wolf-game, which fails to take into account the singularity of their practices and their immanent modes of existence. An ecological approach therefore does not allow me to assume that the practice of science is determined by the performative iteration of policy concepts or simply molded to fit the *insights to invoice agenda.* As Stengers says, we need tools for *thinking*, that is we need to invent new concepts that are part of - and therefore restrained by - the intimate interconnectedness of an ecology of practices. The relation between practices and concepts is uncertain and "metastable".

Another way of characterizing this aspect of the ecology of practices, is that it demands a mode of analysis where the object of study is rendered *active*[56]. An ecological approach would not allow me as an analyst to approach the words and actions of field participants as were they a passive raw material for analysis. In taking an ecological approach I need to be wary of any inscription of data into a pre-existing conceptual vocabulary or analytical category as it would fail to recognize the singularity of the specific practices studied. What Stengers extracts from scientific and political notions of ecology is a need to:

> "Abandon all temptations to think of nature as submissive, manipulable, assimilable to some "raw material" on which we could be free to impose whatever organization we chose". (Stengers, 2010, p.34)

This normative claim would, according to Stengers, apply whether or not the "we" are policy makers trying to change the practice of science or STS scholars trying to analyze or describe it. Ecologists, in Stengers' use of the concept, will have to risk extending to nature the power to tell how it should be described (2000a). Perhaps the same is true if one's object of study is not "nature" but the practice of specific "species" of very successful scientists? If so, we cannot allow an analysis that ignores the laughter of Per Hedegård. The ecological approach reminds us that researchers are not a passive "raw material" for analysis. Their practice is conceptual by nature. Rather than assuming that we already know the effects of Danish research policy on the practice studied, Stengers' ecological approach would have us risk our assumptions and allow the singularity of actors in the field to surprise us. The ecological approach would not view scientists' strategies as determined by overarching "environmental" orders that manipulate, regulate and determine them. (Stengers 2005). If scientists are parts of an

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1994 p.28.). The task, according to Stengers and Deleuze is to create new concepts that fit the ever changing problems encountered in our inquiries. Stengers' idea of *thinking par le milieu* (2005) and her quest for *surprises* (2000a) is an encouragement to be sensitive to the singularity of one´s object of study in the same way as Møller would need to be sensitive to the specific way the species he studies respond to the questions he poses to them.

[56] See Haraway (1991), and Latour (2004) for similar points.

ecology, they are not functional parts that stand in a stable relationship with a larger whole.

An ecological approach thus requires us to *become affected* (Latour 2004, Despret 2004) by our object of study, to think *through* it rather than *about* or *on top* of it (Henare, Wastell and Holbraad 2007, Jensen and Bowker 2011). After all, it suggests, there is no way of being sure how changing environments will affect specific species until one has conducted the experiment in practice.

Similarly, there is no telling in advance what constitutes a viable strategy for the scientists we meet in this thesis when faced with fierce competition, strategic and politically directed research funding, intensified demands for demonstrating relevance to society and new demands to bridge a perceived gap between science and industry. If we take the ecological approach, these questions can only be examined by risky experimentation with living practices.

Reciprocal capture

In addition to the requirement to become affected and to take the singularity of practices into account, it is incumbent to note that the analyst cannot herself stand "outside" ecology. Thinking about science *par le milieu* means that the analyst cannot be disentangled from the ecology of practices that forms the object of study, precisely because she is disabled from claiming to be operating on a different conceptual plane. The analyst, according to Stengers must be seen as an intrinsic part of the ecology rather than as a downstream predator. An ecological relationship between the analyst and the object of study means therefore that neither can be fully defined by their respective functions (thinker and practioner); they are as much defined in their *mutual relationship*.

With Stengers, we could conceive of such a relationship in terms of a process of *reciprocal capture* (Stengers 2010). In contrast to the wolf′s game where the prey simply endures the predators attack with no further form of process, *reciprocal capture* entails a mutual relation that is characterized by uncertainty and risky relations. In relationships of reciprocal capture each species *retains its own agenda* but continues simultaneously to depend on the other(s) for its survival. Contrary to the rather peculiar situation depicted by La Fontaine's "the wolf and the lamb" where the wolf is allowed to completely ignore the recalcitrance of the lamb, a "real" ecological relationship between predator and prey would require the wolf to take into account, incorporate and become affected by the habits, needs and behavior of lambs. If the lamb had not already been integrated in the body of the wolf through millions of years of evolutionary trial and error there would be no capture. Real lambs rarely hang around long enough to debate whether or not they should be eaten. Their bodies have already integrated a reference to wolves, in the sense that they will recognize the potential danger and avoid such encounters[57]. As a more romantic example of an ecological relationship Stengers points to symbiotic relations between species (more on this later). The point here is that the ecological approach points to a reciprocal process in both examples:

> “We can speak of reciprocal capture when a *dual* process of identity construction is produced; regardless of the manner, and usually in ways that are completely different, identities that coinvent each other each integrate a reference to the other for their own benefit”
>
> (Stengers 2010 p. 36).

By this dual process each party *integrates a reference* to the other in that they take the other´s presence, behavior and habits into account. This works for Stengers whether the “species” are wasps and orchids or neutrinos and physicists[58]. I propose that the notion of double capture applies equally to the relation between STS scholars and the scientists studied by them. In order for an ecological relationship to be established between them, both engage in a process of reciprocal capture where they integrate mutual references. Of course, both “species” exist with their own autonomous goals and agendas but at the same time they depend on each other’s presence in order to survive.

Practice versus idea

When playing the wolf´s game, Stengers’ suggests, we necessarily fail to take into account the immanent mode of existence of both the analyst and the actors of the field. In other words we fail precisely to think of this relation ecologically. Stengers sees the predator/prey strategy as a danger to science (social or natural) in the same manner as the “DDT strategy” is dangerous for an ecology (Stengers 2010) because it fails to take into account the complex relations and mutual dependencies of ecology.

[57] For a brilliant description of how sheep incorporate predators in their social organization and flock behavior, see Despret (2005). Sheep, according to the primatologist Thelma Rowels studied by Despret do not organize themselves around competition for food. Rather than making sure they eat, their social organization revolves around not being eaten, a finely attuned and socially distributed process that make sheep a lot more intelligent than common sense would have it. Despret’s analysis indeed approaches both sheep and primatologists as thinkers *and* practicing actors .

[58] Stengers (2010) asks: Where would neutrino´s be without the goals and interests of physicists? Where would physicists be if neutrinos did not exhibit an agency independent of their agendas? For more on wasps and orchids see the notion of *double capture* as outlined in Deleuze & Guattari (1988). Double capture is at the base of Stengers argument as it points to the process by which wasp and the orchid engage in dual identity construction. The wasp needs to *become orchid¸* and vice versa in order for the relationship to work to the advantage of both. However, Stengers extends this notion to ecologies in general, claiming that all species relate to their environment even if their relationship is not a “romantic”, mutually beneficial one.

For Stengers, the contrast between the wolf´s game and an ecological relationship is best articulated in terms of the difference between *practices* and *ideas*:

> "The contrast between practice and idea is crucial in that it stands in opposition to their hierarchical articulation: the idea conceived as vision would precede, inspire and command practice, thus defined as application, a simple implementation. The idea so conceived is by definition limitless, capable of being freely extended, unaffected by the petty constraints of its particular "applications". It can encounter no resistance except that of other ideas. It´s job is to rule and it has no other challenge other than the elimination of whatever serves as an obstacle to this rule" (Stengers 2010, p. 39).

The leap to ideas is a wolf´s game, through which one's conceptual order is extended without consideration for the resistance it encounters or the effects it has. As suggested in chapter one, the looping effect of the diffusion of STS concepts suggests the possibility of concepts elevated to ideas that freely extend themselves in and as policy initiatives. STS concepts like mode 2 knowledge production, knowledge economy, asymmetrical convergence or academic capitalism – or, indeed, performative loops – risk turning into analytical equivalents of "DDT strategies", when conceived of as ideas rather than as practices.

To Stengers, however, it is not the conceptual *as such* that poses a problem. Rather, it is the elevation of conceptual language, crafted in relation to specific practices, into a set of abstract ideas. When, for example the concept of knowledge economy or the distinction between mode1/mode2 knowledge production is disseminated as ideas, they begin to act like predators, who do not need to take into account the behavior of their prey, because *they are capable* of capturing anything they like. Stengers contrasts abstract ideas with a notion of practices:

> "Practices cannot, any more than living beings, address a silent world, the docile substrate of convictions and interpretations; their mode of existence is relational and constrained, not hallucinatory or visionary; their avatars do not refer to a more general authority for whom they would be a local translation, but to a here and now they fabricate and which makes them possible."
>
> (Stengers 2010, p. 40)

When thinking ecologically about the complex relationship between the conceptual and the empirical, Stengers promotes a view of the two as parts of an ecology of *practices* rather than as an ecology of *ideas*. When viewed ecologically, rather than operating as 'downstream external commentaries', STS concepts must be understood as involved in a metastable and uncertain relationship to the empirical practices they attempt to describe. Accordingly, there is no *application* of abstract ideas, however relativised these may be, only the practice of *fabricating* authority in reciprocal processes of capture. The conceptual and the empirical form mixtures in these processes. By ordering the conceptualization of science in the knowledge economy through a set of binary distinctions, and by paying no heed to how this may create friction within these, analysts engage in abstraction rather than ecology.

This is all very interesting from a philosophical or abstract methodological view. However, there is still a long way from Stengers' philosophical account of normative criteria to the concrete challenge of doing fieldwork. The restrictions placed on the analyst by Stengers' ecological approach sets a high standard for what counts as a proper ecological relationship and hence for doing good research. It is surely much easier to engage in wolf-games than to establish reciprocal processes of capture between the STS scholar and the natural scientist. Indeed, my fieldwork involved constant temptation to leap to ideas and unleash the analytical wolf.

Wolfish appetites

First of all, life as a social science wolf is difficult if one´s prey happens to consist mainly of other, even larger, wolves. Natural scientists have as their main occupation to create objective scientific facts. Thinkers, as much as practitioners, are accustomed to winning hierarchical games of representation. They are not likely to agree to being treated as passive and mute empirical material.

This became painfully apparent in doing field work. The first many months of the field work mainly consisted in trying to convince people that I was really a researcher. Some would assume I was a journalist or, if they were more acquainted with social science like Hedegård, an anthropologist "of some sorts". Qualitative social science would, however, did not qualify as serious research and at no point was I approached as a fellow "scientist". One participant burst out in laughter when I talked about my field notes as "data". "Would you really call that data?" he said in disbelief and pointed to my scribbled notes. At a dinner in Paris, where I followed the collaboration between a group of biologist and representatives from European biotech firms, I was asked by a French biologist, whether I published my texts in newspapers or whether it was more like magazines? When I told her that my discipline had peer reviewed academic journals just like molecular biology, she was silent for a while. Then she asked if I was part of a laboratory and where it was placed. Oh, what sweet delight it would have been to put my sharp wolfish teeth in her. I wanted to locate myself at least twenty steps below the place where the waters of knowledge economy got muddied. I wanted to unflinchingly demonstrate how my conceptual vocabulary was based on a more encompassing and higher context than that of molecular biology. I wanted my definition of objectivity to be stronger than hers. In short, I wanted to avenge myself. I could not wait to get back to my desk where I was free to write up my Paris dinner notes with all the bite of poststructuralist criticality. There, safely behind my keyboard I would consume my data material "without any further form of process". Such are the desires of a fasting social science wolf, looking for adventure.

Ethnographic moments and lateral moves

Unsurprisingly, the philosophy of Stengers and Deleuze were not much help in finding out *how* to establish a way of actually doing fieldwork and analysis as a process of reciprocal capture. My informants were really not very interested in what I did and were difficult to engage with due to disciplinary differences, their busy schedules, concerns about confidentiality and the fact that my project simply made no sense to most of them. Here the reflections of social anthropology suggested itself as a less idealist and more practical source of inspiration. The work of Marilyn Strathern, who has given the problem of ethnographic writing much devoted attention, proved to be extremely useful (Strathern 1999, 2004).

According to Strathern, the relation between "the field" and "the desk" (what I may dare to call the empirical and the conceptual) is intrinsically a matter of *comparison* or of shifting positions (Strathern 1999). However, in Strathern, positions do not shift along with a hierarchical upstream/downstream axis as they do in the wolf´s game. Rather, Strathern argues that ethnographic writing proceeds by a series of *lateral* reflections, comparisons, juxtapositions and analogies between what is thinkable in the field and at the desk (Strathern 1999). The "immersion" demanded by ethnographic fieldwork is what affords this back and forth movement between these conceptualizations and experiences. Strathern argues that ethnographic insights are by nature lateral reflections in that they are created by way of a back and forth movement between perspectives of the fieldworker and those present in the field (Strathern 1999, p. 24-25).

The notion of ethnography as a lateral endeavor rather than a hierarchical one is further elaborated by anthropologist Bill Maurer. The empirical "data", Maurer argues, does not come "before" the conceptual as its raw material. Rather, the conceptual and the empirical sit *alongside* each other, thus suggesting that anthropology is "a practice of lateral reading and writing, neither descriptive nor explanatory but repetitive, multiplicative, and/or accelerative" (Maurer 2005 p.7). Seen thus, the job of the conceptual is not to *explain* or shed light on the empirical. Rathern, Maurer sees both as practices that "draw on" or "metastasize into" one another. Studying Islamic banking, alternative currencies and payment systems, Maurer takes the conceptual repertoires of his field participants to be as much "theory" as they are "data" (Maurer

2011, 2005). Concepts and practices are approached as belonging to the same plane of reality, as Maurer places them in lateral rather than hierarchical relationship. Maurer's bibliography, he suggests, is thus as much a list of "raw data" as it is a list of conceptual *resources for analysis*; as well, some of them belong to the world of anthropology and others to the world of Islamic banking.

In a similar vein, anthropologist Stefan Helmreich aims to analyze a variety of marine biology practices by going "*athwart theory"* (2009 p. 23-25). This implies "cross-wiring" anthropological theory and the theories of the marine biologists that Helmreich followed in the field. Common to these authors is their use of what we normally think of as empirical "native categories" as "concepts" to rethink known analytical distinctions. Helmreich, for example, constructs the notion of "blue-green capitalism", not just as a play on words, but as a way of articulating the way blue-green algae and other biological organisms are mobilized by marine biologists and venture capitalists, in their efforts to turn the microbial sea into a new frontier for resource extraction. Blue-sky promise, green futures, blue ocean strategies and blue-green algae are intermingled in analysis to convey what is going on in the intermingling between biotech and marine biology[59]. Maurer´s study of payment infrastructures likewise makes use of field participant's concept of "interchange" to make sense of the "plumbing" of modern economics and question whether the notion of capitalism has perhaps become inadequate as analytical resource when faced with the problem of payment. In both accounts, the comparative operation of ethnography thus becomes a transversal or sideways movement on a single plane of reality where conceptual and empirical are in continuous variation[60].

This mode of social anthropology offers an approach where fieldwork and deskwork laterally affect each other. Rather than a hierarchical division between field and an analysis where analysis takes place further downstream and from a more encompassing perspective, "lateral" ethnography can be said to engage fieldwork and deskwork together. Writing becomes a second field, not an external downstream commentary on events that already took place. Doing ethnography thus means inhabiting a double field, not just that of the actors studied but also that of the desk. The *ethnographic moment*, Strathern argues, is a lateral effect of "engaging these two fields

[59] The work of Maurer and Helmreich resonates clearly with the comparative turn taken by Strathern. Other examples of "lateral moves" in social anthropology are Riles (2001), Holbraad (2008) Pedersen (2007) and Pedersen & Høyer 2008.

[60] Deleuze is clearly a source of inspiration for this kind of lateral reasoning and is explicitly referred to by both Maurer and Helmreich. The monism of Deleuze places everything on a single plane of reality (the plane of immanence) and thus follows the conceptual and the empirical in continuous variations rather than assuming them to belong to separate and hierarchically organized spheres of reality (Deleuze and Guattari 1988, see also Jensen & Bowker 2011). In management philosophy, Bent Meier Sørensen has taken up a similar approach from the same Deleuzian inspirations. Sørensen (2010) suggests a *method of juxtaposition* to bring about new problematizations rather than solving already existing problems and juxtaposes religious art with organizational diagrams. The juxtapositions result in rather surprising readings of both art and organization.

together" (Strathern 1999, p 2). Some ethnographic moments simply extend into this double field and become impossible to "shake off". Strathern's Melanesian examples include, for example, the "witch child" killed by natives with reference to physical characteristics that to a British anthropologist seem completely arbitrary. Such moments cannot easily be shaken off, understood or digested; they pose basic questions to the analytical distinctions and frameworks of the anthropologist (Strathern 1999). For Strathern, then, the ethnographic moment is that which forces the fieldworker to become affected by the impossibility of understanding or reconciling the experience with known categories or images of thought. Such moments are at once viscerally "empirical"; yet they are as much conceptual: they mess up analytical distinctions and provoke the creation of new concepts and ideas.

Ethnographic writing as a process of reciprocal capture

This makes ethnographic writing an ecological problem rather than a matter of diffusing ideas. Both the notion of reciprocal capture and the idea of engagement within a double field are facilitated by comparative operations. One could say that both substitute the question "*what is this an example of*?" with the question "*what is this comparable to?"*[61] Both entail comparisons but whereas the former implies a hierarchy through generalization to reduce complexity, the latter refrains from doing so in order to increase it. Neither reciprocal processes of capture nor ethnographic moments occur independently from close engagements with the actors of the field. Deskwork and fieldwork integrates a reference to each other but for different - lateral – reasons (Maurer 2005).

Despite apparent differences between the philosophy of Stengers and the anthropology of Strathern[62] I would like to suggest that Strathern's aim to engage a

[61] This contrasts with Gideon Kunda´s inductive, interpretive approach to ethnographic writing (1992). Kunda writes that the most crucial question the ethnographer can ask is the question *what is this an example of?*: "To what larger as yet unspecified set, to what meta-narrative, does this unit of meaning belong? Into what larger framework can it be fit?" (Kunda 2010 p. 9) Recognizing that this question indeed plays a part in any analysis it may be worth contesting the hierarchical mode of representation it here is made to promote. Data are neatly delegated to the realm of the lower level empirical order and framed as functional pieces in a puzzle. Ethnography, in Kunda's version, becomes a science of functions and data is approached as cogs in a machine to which they make no difference and have no transformative power. Kunda´s data is not a tool for thinking. Also, even if this hierarchical mode of analysis is pursued one might wonder if Kunda´s question does enough analytical work on its own. Where would an analyst like for example Foucault be if he had merely asked the question: "what is this the example of" and refrained from ground breaking modes of analysis entailed in questions like "what is this the answer to"?
[62] Admittedly, Strathern does not seem to be as much an ecologist as Stengers would have it. For example she makes it quite apparent that her comparisons are not made in a reciprocal process of capture with her field participants thus responding to their objections and criteria for relevant comparisons. The comparison is "mine" she notes (Strathern 2004). Strathernian analogies between for example Melanesian and Euro-American notions of kinship or property are rather crafted in the process of ethnographic writing, and their evaluation takes place in the presence of fellow anthropologists and are in no way at the

double field sits well with Stengers' "ecological" approach. First of all, the ethnographic moment and the double field exclude the predator/prey relationship. Strathern argues that in order for there to be a double field and a lateral movement between its two parts, difference and divergences cannot be collapsed into a common ground. Learning from her Melanesian fieldwork, Strathern argues that engaging two fields together or creating an exchange between them does not happen by finding an all-encompassing context by which the experience can make sense. Neither does the working together of these two fields rest on the building of a common ground or shared understanding between fieldworker and informant. Fieldworker and informant have to remain *severed* for a productive exchange to take place (Strathern 2001, 2004). "Engaging the two fields together" in the Strathernian sense is not equivalent to the arrival of a common understanding; nor does it correspond to the notion of letting go of one´s own analytical agenda in order to faithfully give voice to the native viewpoint. Alterity and divergence remain central in this somewhat "ecological" form of social anthropology. The practices of the field and the practices of the writing fieldworker remain guided by their own goals and aims but are nevertheless deeply dependent on each other for their existence. They remain two fields, but a *partial connection* is established between them; they never reconcile into a unified whole but still feed on the creation of connections between them (Strathern 2004). This type of mutual engagement is not made to fit into a more encompassing context or a more general framework in which practices come together and find common ground.

Strathern has critically scrutinized anthropological debates on ethnographic writing and points to attempts to avoid the wolf-games like ethnocentrism that may however not be free of wolfish maneuvers altogether. She argues that postmodern approaches to ethnographic writing, has translated the "death of the subject" into the "death of the fieldworker" understood as a single locus of authorship or a final downstream representation. Fair enough but perhaps not far enough, she argues. Killing off the fieldworker is not synonomous to killing off the tendency to superimpose the field with the vocabulary or framework of the analyst. Strathern critiques a notion of the fieldworker introduced in some postmodern approaches to ethnographic writing as someone that integrates ethnographic experiences and collects an ever growing pastiche or collage of new perspectives. The fieldworker has been killed off perhaps, but has then been replaced by the tourist or the consumer who samples cultural experiences only to enhance the sense of his/her own (2004, p. 15). Strathern is wary of the consumerism she detects when anthropologists approach the field as a text with multiple interpretations.

> "Do we really turn aside from thinking about ourselves as the producers of particular texts only to encounter the voracious consumer of all of them?

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mercy of her field participants (Strathern 2002). Consequently there is a notable difference between the two authors´ view on how a proper inquiry is to proceed.

> (…) for the consumer's gut, alas, in the long run turns everything to flesh." (Strathern 2004 p.15).

Leaving aside the predator position of the authoritative text-producing author, does not immunize the analyst from the possibility of turning into a wolf or applying a "DDT strategy". In line with Stengers, Strathern argues that we have not gone far enough. The relativist tourist-fieldworker may surf through differences and note them down in a pastiche or collage but thereby also collapse them all in an all-encompassive relativist mode of analysis where ideas freely compete and all recalcitrance will turn into flesh[63]. Strathern´s *ethnographic moment* is precisely characterized by the *failure* of wolfish attempts to chew up alterity. Ethnography, according to Strathern takes place by *crafting* relationships between divergent practices, not by one practice consuming the other. In demanding that both fields are engaged together, Strathern insists that none of them are turned into flesh or prey.

> "The one component is of a different order from the other, and is not created by what creates that other. They are not built into each other´s scale (…) Switching perspectives (…) requires neither that a position left behind is obliterated nor that it is subsumed. In turn neither position offers an encompassing context or inclusive perspective." (Strathern 2004 p. 38-39).

Strathern seems to advocate a way of comparing elements which retains and even highlights their alterity. The switching of positions that took place along the upstream/downstream axis in the wolf´s game thus completely changes its character when turned over on its side into lateral analysis. What Stengers and Strathern both indicate is that comparisons are creative operations. Comparisons, when thought of as exchanges, engagements or ecological relations, make and create new relationships. They do not map already existing ones, they do not posit relationships in a unilateral way and they do not play wolf-games of inventing new and ever more encompassing contexts. In this sense neither ethnocentrism (the free diffusion ruling ideas) or empathy (common ground) will constitute ethnographic moments or facilitate reciprocal processes of capture[64].

In a much stronger tone of voice, Stengers proposes a normative criterion for distinguishing between good and bad comparisons. Wolf-games are simply *bad comparisons* as their way of juxtaposing the empirical and the conceptual allows for one

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[63] Strathern makes explicit reference to Haraway's notion of the cyborg as a trope for how to think of the ethnographic fieldworker. However, her reflections on the consumerism of postmodernist approaches to ethnographic writing also resonate with Haraway's reflections on the cynicism of postmodernist feminists who ended up approaching every empirical encounter as texts to be deconstructed or used as passive surfaces for inscription. The conclusion, according to Haraway was that of the bored tourist: "they´re just texts anyway, let the boys have them back" (Haraway 1991).

[64] See Latour 2004 and 1997 for a similar argument drawing on the work of Stengers and Despret.

vocabulary to weaken and consume the other. An ecology where one type of predator suddenly develop the capacity to prey indiscriminately on everything it encounters constitutes an ecological disaster, Stengers argue. Whenever the vocabulary of one party is superseded by the vocabulary of the other (and hence does not need to integrate a reference to it), Stengers argues, we simply have a case of foul play - bad research (Stengers 1997, Latour 1997, Stengers 2011). By contrast, comparativism should be a method of learning with only one general rule:

> "…that rapports be created between terms in their "full force", with no "foul play" weakening one and ensuring the position of the other (…) Those you address must be empowered to evaluate the relevance of your interest, to agree or refuse to answer, and even to spit in your human, too human, face. "Learning from" requires encountering, and encountering may indeed imply comparison, but there is no comparison if the encountered others are defined as unable to understand the point of comparison. We are returned here to the Latin etymology of "comparison": *compare* designates those who regard each other as equals – that is, as able to agree, which means also able to disagree, object, negotiate and contest." (Stengers 2011, p.62-63)

Where Strathern's work revolves around ethnographic moments that allow for the two fields to work together in partial connections, Stengers "general rule" of keeping retaining divergence and contestations take this a step further, demanding that both sides of the comparison are able to object and make themselves "indigestible" to the other. Stengers wants to think through the middle of her study object to make it a tool for thinking, Strathern looks for fieldwork experiences that lie *beyond* what the fieldworker can consume or make sense of in known analytical vocabularies. In both cases, the inquiry is an encounter which produces an *excess*, an alterity within relationships that cannot be ignored or reconciled. Stenger's advocates deliberately *maximizing the recalcitrance* (Latour 2004) of the object of study, allowing for the actors to object, refuse and negotiate our descriptions and the distinctions that guide them. Rather than consuming the perspectives of those we study, Stengers prefers the actors of the field spit the analyst in the face and continually contest the account[65]. This "full force" mode of encountering ones object of study constitute Stengers' "ethnographic moment" - that which forces her to think and cannot be shaken off.

[65] The use of words like relevance, interest or empowerment in Stengers should not, as I read them, be subjected to common sense understanding. Stengers notion of thinking *par le mileu* is not an approach designed for collective problem solving, giving voice to the repressed or accounting for the native point of view. Her business, as I read her, is not action science, nor accurate representation but rather philosophy - that is the creation of new modes of thinking based on encounters with an object of study that resists our existing categories.

Indigestible alterity

What seems in the philosophical approach of Stengers to be a problem of normative criteria or thinking ecologically becomes very much a practical task when doing fieldwork. At least my fieldwork seemed to inhibit predatory consumption. Despite the near proximity between my two fields, located only 15 minutes apart in two Danish universities, indigestible alterity was clearly a force to be reckoned with.

Starting out in the Nano-Science Center, I was set to do an ethnography of research management. The first manager who invited me to follow his work was the X ray physicist Robert Feidenhans´l. After a few weeks of asking him completely irrelevant questions and failing to convey clearly what my project was about, he finally gave me some friendly advice: what I needed to do, according to Robert, was learn some basic physics. “If you don’t understand the physics”, he said “you will not understand the management of it either. It all hangs together”. As my fieldwork was not planned as a laboratory study and was initially focused only on the management of science, I found this detour into physics a bit over the top. After all, I had set out to study the strategies and maneuvers of scientists in relation to the *insights to invoice* agenda, not the practice of X-ray physics. Also, the fieldwork was planned as to take up no more than 6 months (out of the total of 2.5 years) so I was not planning on staying for long [66]. At the onset of the project, I was interested in actor network theory and critical management studies; especially Deleuze-inspired critiques of capitalism and entrepreneurship. What “becomings” was I to witness as science and entrepreneurship entered into a zone of indiscernibility, I wondered? To my knowledge these issues had *nothing* to do with basic physics! In this situation, I found myself spending the first six months at the Nano-Science Center banging my head against a wall of technical and scientific indigestibility. The center was an interdisciplinary salad bowl, with most of the projects overlapping between at least three disciplines. This meant that observations and conversations would form a confusing cacophony of technical languages from nano-biology, chemistry, theoretical physics and electronic engineering. Hardly any of the field participants were fluent in more than two of these dialects, however, I was completely lost in all of them.

[66] In fact, the fieldwork ended up taking place over a period of 1,5 year although with several short and one longer interruption. The fieldwork started in the Nano-Science Center and ended in a prolonged series of interviews with scientists I had met in the center but who were working at a different faculty. However, what now feels like my “real fieldwork” material did not really begin to come together until I finally managed to “pull out” from spending days following meetings and managers and began writing. Deskwork materials collected initially as “background data” to help me write up descriptions grew to become a large part of the materials used in the following chapters. Also, the desk work eventually took me back into doing more interviews which extended into email correspondences, exchanges of my writing and new meetings. As it was my first-time fieldwork it was quite a surprising experience that it never really got “done”.

On top of this, it was absolutely impossible for me to understand why Feidenhans´l felt that the science and the management hung tightly together. To me, it seemed that there were two completely separate worlds: A comprehensible one made out of excel-sheet, meeting agendas, airports, phone calls, emails, calendars and budgets and a completely incomprehensible one haunting me with abstract high resolution images, coordinate systems and untranslatable technical language. Especially the images were interesting, as their presence seemed extremely important in both internal communication between scientists in a group, between scientists within a discipline and even more so in the representation for public media. However, interesting as they were, I still did not understand what they meant. First of all, it took a lot of training to decode these images and, second, their use was not just about technical content. I had no idea what their function really was; still, I was struck by their presence in a very broad range of the activities in which scientists would get themselves involved. I am not afraid to admit that I still do not understand most of what goes on in a normal lab meeting in the Nano-Science Center and that most images are unreadable to me without patient assistance from a relevant scientist.

Unsurprisingly, perhaps, my initial fieldwork felt as if it was going nowhere. Every day I would come home with notes that were either incomprehensible to myself or just a boring collection of general observations on management practices that might as well have been made in a consultancy firm. Things were either too specific and therefore incomprehensible, or too general and therefore insignificant. Also, negotiating access remained difficult. I was not free to explore what I felt were "my" research interests and was often politely led away from exactly the spaces I felt I had the analytical expertise to decode. Events that I felt were really important, such as meetings with venture capitalists, negotiations with lawyers about setting up industry collaborations, or encounters with new potential industry partners mostly took place behind closed doors – and for good reasons. Often the explanation was that it was too risky to bring along an "observer"; it would make new contacts anxious or confused about my presence and my agenda. Others were afraid of what I might write if I was allowed in. One manager rejected having me around her group and her work on account of the fact that I "would not understand the science" and would consequently "write all kinds of things". The burden of explaining or translating the scientific practice into a language that this scientist thought would make sense to someone like me was simply too heavy. "I don´t have that kind of time" she said. When I was allowed to join a research manager at work, it would usually be in their office, in the canteen or at meetings at the university. Sometimes encounters would take place at academic conferences where disciplinary language differences again made comprehension difficult. Consequently, the first few months of field work were completely beyond my consumerist appetites. Strathern´s observation that fieldwork always contains *more* than what can be consumed, and that fieldwork demands of fieldworker that she does something she *cannot do*, was spot on.

Adding to the indigestibility of the ethnographic experience was the fact that no downstream position *seemed possible* when studying the winners of the Danish knowledge economy. They were much *too big* for me to carry off into the woods. Even back home in the protected space behind the keyboard, there was no possibility to "wolf it up". Since my informants were highly respected and well connected scientists who tended to think of me as some kind of journalist, they demanded to read and comment on my work before it got published. The few scientists who did agree to my presence or to a series of interviews were not planning to accept the role of the prey. They were used to dealing with PhD students and were not prepared to grant me an analytical downstream position. Any hierarchical move was useless as I would simply be told that I had *misunderstood* or worse – it would mean that I would be completely ignored. Since the wolf game could not be played I had to find a different game.

A game of cat´s cradle

Donna Haraway has suggested an evocative metaphor for an ecological approach to doing ethnography: the cat´s cradle. To play cat´s cradle all you need is a piece of string and two sets of hands (or more). Strings are wrapped around fingers and participants pass between alternating hands and making new figures. Often there is an idea of the kind of figure the game is supposed to end up in (my personal favorite is the Eiffel tower). However, more often, the game ends up in entanglements or just goes on in loops of slightly different patterns without producing an actual end product. You can play the game with the purpose of making a predetermined figure but the fun part is really the continued alternation and avoiding getting permanently stuck in entanglements. Nobody wins a game of cat´s cradle and each participant needs the other's hands in order to proceed (Haraway 1994). Haraway invokes this game to highlight the need for science studies to continually become surprised by what is studied (1994). Cat´s cradle is not the work of single actor either; it is a "game about complex, collaborative practices for making and passing on culturally interesting patterns. Cat´s cradle belongs to no one, to no "one" culture or self, to no frozen subject or object" (Ibid. p. 70)

As a metaphor for establishing an ecological relationship between the desk and the field, I will argue that the comparisons of Marilyn Strathern exhibit features of a game of cat´s cradle; invariably two or more positions alternate in a way where figures are created without one position building the other one into its own scale. No one vocabulary provides an encompassing context for the other. Rather, their divergence is precisely what fertilizes analysis and moves the reader beyond the propagation of self-fulfilling claims.

Laterally, playing cat´s cradle instead of wolf-games changes what is meant by "context". In the chapters that follow, I will *not* be invoking research policy as a more encompassing context with which we can think about science. Nor will I suggest that STS analysis provides a higher conceptual order for explaining what goes on in the field. Instead, as in a game of cat´s cradle, I aim to stay open and attuned to reciprocal process of capture - between the conceptual and the empirical and between fieldworker and field participants. In asking what the vocabularies of scientists are *comparable* to, rather than asking what they are an *example* of, I aim to allow for the conceptual vocabulary of scientists to say something about policy as well as about STS concepts. What can their conceptualizations teach me (or us) about standard social science analytical distinctions? What kind of analytical work can they do for our understanding of science in the knowledge economy?

In my fieldwork, the shift from a hierarchical to a lateral mode of analysis was concretely initiated by persistent recalcitrance from Professor Birger Lindberg Møller, who was briefly introduced in chapter two. Møller is very passionate about his field of research; he soon became tired of my questions about his strategic behavior in relation to new performance measures and science and industry relations. He did not want to talk about "politics" - he wanted to talk about "research". Having learned the lesson with Robert with whom I never established an interesting relation, I responded to his protest by starting to ask questions about plant biology. This turned out to be fascinating; actually a lot more interesting to me than the previous fieldwork. As Møller talked through the power points, lectures and projects he was involved in, I got sucked into the details of plant/insect communication. Gradually I devoted more and more attention to his scientific work and ventured into reading journal articles. This development generated a lot more interviews than Møller had initially agreed to do and also extended desk research and extra fieldwork dangerously beyond the agreed time frame. In fact, it is fair to say that Møller and I still haven't really stopped field work. And, indeed, an interesting side-effect of this focus on Møller as a thinker rather than a practitioner was that it generated what Stengers would call *interest*. What happened as I turned my attention more to Møller's scientific vocabulary was, paradoxically, that my questions about "political stuff" began to become more interesting to him, too. Interviews and informal conversations in connection with events that we both participated in at the Nano-Science Center ended up as a central part of my empirical material[67]. Over a period of eight months, Møller would constantly tell me that he was too busy, but nevertheless agree to do another interview.

The biological entities studied in Møller's laboratory could not be left out or shaken off. They were nevertheless oddly irrelevant to stories of managerial and

[67] I did a total of twelve transcribed interviews conducted in Danish. Most were long (1.5 to 2 hours) and based in planned themes and interview guides, some were short and improvised based on conversations we had in connection with fieldwork in the Nano-Science center where Møller was part of several projects.

strategic decisions. At the time, I was still not sure whether the fact that I was becoming more interested in Møller's research on plant/insect relationships than I had ever been in research management was a sign of my fieldwork finally going in the right direction or simply a final surreal derailment. However, there seemed to be no other way since part of Møller's motivations for even doing an elaborate series of interviews on "my" research questions was that we would also talk about "his" research. Going by way of plants/insect communication, was my way of gaining access to stories of how to manage a field of research in response to changes in the way university science was managed, funded and governed. I doubt Møller would have cared for my descriptions in the way he ended up doing, if his scientific objects of interest had not been part of the conversation.

As I was simultaneously getting more interested in the work of Strathern and Maurer, I gradually became more confident that the odd mixtures in my fieldwork were not a detour but a lateral entry point for doing analysis. Insects and plants placed alongside strategic decisions and tactical management maneuvers began to seem like a less surreal juxtaposition. During the last interviews, plants and insects were beginning to *take over* the descriptions of strategic and managerial practices; they found their way into the interviews as analytical resources rather than merely as "data". Møller was quite amused by having his work returned to him as "theory" and did not mind being compared to a moth or a beetle. At this point, I began to write up descriptions that mixed the conceptual language of scientists with their managerial and strategic practices. That process sent me back to previous informants at the Nano-Science Center to ask new questions. For example, I found a way of giving expression to the extensive use of image materials in nano science in my writing. Going back to get detailed accounts on how an electron microscope works or how to tag a protein in fluorescence microscopy became a fruitful way for making use of the indigestible cascade of incomprehensible images that almost drowned me during the first couple of months. Yet, going back to do more "technical" inquiries was hardly ever successful. Some informants had moved on and most were too busy to provide me with enough detail to make sense of the process of imaging. One exception was Thomas Bjørnholm, the manager of the Nano-Science center, who had not previously been able to make himself available for close-up fieldwork due to time-pressure. Bjørnholm agreed to do two interviews. In these interviews he talked me through a specific power point presentation that I had seen him do before my field work formally started and that had stayed with me ever since. He also provided me with some of the background of the image material that he used in this slide show, and he pointed me to people who could explain how the images had been made. In the same manner that the plants and insects studied by Møller shaped the way I made sense of his managerial practice, the high-resolution imaging techniques which formed the basis of Bjørnholm's Power Point presentations began to structure my description of the way the Nano-Science center was presented in press releases and on their website.

I would like to think of my encounters with the extensive use of images in the Nano-Science Center and with Møller's insistence on focusing on starting out in plant/insect communication as the ethnographic moments that structured my descriptions and made me think about science in the knowledge economy. Passing my clumsily entangled strings of writing by way of the "native" scientific vocabulary rather than insisting on structuring my descriptions on well-rehearsed concepts like translation, networks or academic capitalism became my game of cat´s cradle. I started thinking about research management *in terms of* plant/insect communication and high-resolution imaging techniques. The idea of taking an "emic" perspective was of course not unfamiliar to me, but this was not just a matter of representing the field from the perspective of the "natives". Rather, I would suggest that it formed a process of reciprocal capture where their research and mine became partially engaged.

Paradoxically, this process really started to pick up speed when I started to "give away" my analytical points and share possible analogies with Møller and Bjørnholm Interviews continued for a while as I slowly started writing them up and I began to invite them to help out in making analogies between their scientific and managerial work. With Thomas this resulted in a few laughs and some generous sharing of technical details to help me out. With Møller, reciprocal capture was almost unavoidable as he himself tended to make use of biological and evolutionary metaphors when accounting for the managerial and "political" part of his work and required me to engage with his vocabulary. I was not able to bring the idea of comparing plants, larvae and beetles to research managers home to the "desk" or to claim that the comparisons were mine alone as Møller would insist on contesting or modifying whatever analogies or suggestions I brought to the table. Initially this engagement on Møller´s behalf was merely an attempt to help and make sure that I had my biological facts straight, but as it happens he did not stop there. He seemed to find the idea of having his "strategies" of managerial work compared to the "strategies" of tobacco plants, moths and beetles rather amusing, although not to weird to engage with. As his contestations and "corrections" of my clumsy attempts at acquiring a workable biological vocabulary became part of the way I made sense of his managerial and strategic practices, he also began to contest or support possible analogies.

For example, Møller would not accept any framing of his managerial practice in terms of parasites or parasitism as he claimed that this analogy rested on my poor understanding of the complex biological systems he was working with – not to mention the strategies he deployed when trying to get his research funded. My initial attempt to use Michel Serres' notion of the parasite to make sense of his managerial decisions was rejected flat out with reference to my failure to distinguish between a parasite and a symbiont. Møller was not afraid to send me back to the drawing table to come up with a better analogy. Getting my "facts straight" involved unfamiliar practices such as reading journals like "Phytochemistry" and "Current opinions in plant biology" as much as familiar procedures such as correct transcription, translation and citation of interview data.

The dependency entailed in an ecological relationship between researcher and field participant became very apparent in the final stages of writing. Assuming that fieldwork was "done" at the end of the last interview, I was quite surprised to find that the game of cat´s cradle is not easily terminated once it really got going. Thinking of Møller's strategic behaviors "par le milieu" made it very difficult to disentangle him from the writing process. Møller had, like everyone else, demanded to read my descriptions prior to publication and promised to comment and correct the biological facts. However, the first, second *and* third drafts of chapters five and six did not come back with comments, they came back with massive track changes! Møller saw no problem in tampering with the argument and altering citations from carefully transcribed interviews just as he unflinchingly restated the whole point of several versions of the descriptions. Møller clearly did not see himself as passive and mute material for analysis. Rather he acted as any professor would act when one of his PhD students sends him a draft paper. The example below is taken from an early version of the chapter five. Møller´s text is underscored or marked, the rest is mine.

> "The larva has developed a specialized procedure to sequester the cyanogenic glucosides and ~~is specialized into~~ storeing the cyanogenic glucosides inside its own body without it causing tissue damage or intoxication. In this way a deterrent designed to work *against* insects is *incorporated* in the insects own body and given a new function (Ibid). In those cases where the insect cannot obtain enough cyanogenic glucosides by feeding on its host plant, the insect has developed a procedure to *de novo* synthesize the plant defense compound itself. This demonstrates that the insect in course of evolution has become dependent on the ability to acquire and store these compounds. But *de novo* synthesis costs a lot of energy and larvae who are required to do a lot of *de novo* synthesis shows slower growth and poor biological fitness. BIRGITTE: you can use this later in relation to that plant biologists are less competitive if they have to develop technology platforms themselves

The example above is not at all extreme, in fact most of the two chapters discussing Møller's strategic response to the interaction-agenda of Danish research policy is a product of ongoing textual negotiations. I am still considering whether Møller should be assigned as co-author on a journal article based on chapter five. Some parts of the text took months' of e-mail exchange and went through several iterations with new track changes added by Møller at every turn. The last version was sent to me only two weeks before my thesis submission deadline and involved reorganizing chapter six altogether. In the above example, Møller stays on his own turf and corrects my use of biological terminology. However, as is also indicated in the above example he readily engaged in suggesting alternative analogies and did not shy away from claiming that I needed to change my argument. For example, this process involved a focus for the whole thesis on “strategies” rather than “tactics” as first suggested by me (with reference to De Certeau´s famous distinction between the two[68]). Møller, however was not keen on the military sound of “tactics” and furthermore insisted that his responses are strategic in that they involve long-term planning and looking ahead and are not just situational day-to-day maneuvers in a battle zone. Møller would also modify or simply “correct” my use of interview citations as he often felt the original transcripts did not properly convey the point he was trying to make at the time. I on my side would accept some changes and forcefully reject others, either in e-mail correspondences or in meetings where we would go through the document and negotiate about individual track changes one by one. (Reciprocal capture is a full-on job). The text is in no way a finished product as large parts of it is now in review as journal articles, thus adding new participants to the game of cat´s cradle. Also, I am counting on Møller to persistently contest my conclusions as we still disagree on several points in relation to issues such as strategic research councils and the benefits of competition in research environments. Also, we do not at all have the same critical approach to thinking about the knowledge economy. Møller sees most of the criticism coming from colleagues in the humanities and social sciences as mere conservative complaints and excuses for not getting in the game. I on my side will claim that his view on the current problems in getting funding for social sciences and humanities research sounds a bit like Marie Antoinette’s: “let them eat cake”. Last but not least, we still forcefully disagree in our views on plant GMO, a subject to which Møller has dedicated his entire professional life.

In relation to Bjørnholm, who plays an important role in chapter four the game of cat’s cradle was a lot shorter and with less surprises. The exchange of analogies via interviews and writing did not end up in entanglements so much as it slowly petered out, as Bjørnholm was promoted to pro vice chancellor at the Copenhagen University and got too busy elsewhere. More importantly, the fieldwork encounter did not last long enough and did not relate to Bjørnholm’s own research to the point of facilitating the level of interdependency and dual identity construction that the collaboration with Møller made available. Bjørnholm did not have as many stakes in the text and the collaboration around the description did not really support him in his own goals and agendas. A series of e-mails and a few comments to first and the last version of the chapter was all the engagement with my text that he could fit in. Thomas claims to find the descriptions “very interesting” and has apparently made use of some of my earlier presentations of my work (not part of this thesis) in other contexts but he left the writing process once he had seen the last draft of chapter four where the self-representational practices are placed alongside the practice of representation done by electron microscopy. Other field participants have the previous chapters and provided

[68] See De Certeau (1884).

comments. All field participants mentioned in this thesis have requested that they appear with their real names and institutional affiliations.

Lateral analysis

This chapter started out asking how one can analytically develop *a stomach for complexity*. I have proposed a lateral approach to address this question. As we have seen, it entails that the conceptual and the empirical are allowed to blend into, or draw on, each other in a sideways rather than hierarchical movement. The idea here is that to analyze does not have to entail an operation of reducing, cutting up and subtracting elements from a mixture. Rather I have suggested that analysis may be approached as a process of *adding* elements or allowing for new juxtapositions. This experiment may enable me to make sense of the complex mixtures of academic science without separating it out into solid institutional spheres of reality like those of university, industry and government Non-hierarchical and comparative exchanges between conceptual/empirical mixtures create an analytical effect that allows me to go beyond the diffusion of ideas or the reiteration of existing vocabularies. Neither the native's point of view nor that of the analyst is privileged in this operation. Rather the practices of *both* are approached *both* as "data" *and* as "theory". Literature published in *Social Studies of Science* is not of a higher conceptual order than that published in *Phytochemistry*, as is manifest in how both are listed in alphabetical order at the back of this thesis. Both *simultaneously* belong to the realm of the conceptual and the empirical.

A range of different inspirations guided me towards this "lateral" approach to analysis. I will sum them up briefly here. First of all Jensen and Bowker's reflections on the benefits of tampering with the conceptual/empirical distinction provided an analytical point of entry into a field characterized exactly by the blurring of the two. Seeing the conceptual and the empirical as "open sets" (Jensen 2010, Jensen and Bowker 2011) suggested a way of analyzing mixtures without taking them apart into solid constituents but rather to follow them in their continuous variation. Serres' and Stengers' observations on wolf-games' analytical predators, helped me identify the problems with the analytical procedure used in both celebratory and critical accounts of the knowledge economy. Stengers' notion of ecologies of practice and her preference for reciprocal processes of capture over predator/prey relationships helped me conceive of an alternative to the hierarchical diffusion of ideas into practices or the privileging of the conceptual over the empirical. Social anthropology in the particular form given to this endeavor by Marilyn Strathern and her colleagues took this a step further. Ethnography, to this way of seeing, is a comparative operation that may, with luck, include an "ethnographic moment" that forces the ethnographer to think rather than recognize. The terms that are compared through this operation, however, should not be *built into* the scale of one another. Rather, each needs to retain its alterity and the comparison *should* remain partly indigestible to both sides. The work of Bill Maurer, Stefan Helmreich and Annelise Riles also inspired the concrete mode of analysis that I will proceed with in the following chapters by allowing the conceptual language of field participants to structure the way I approach the problem of the knowledge economy. Using the conceptual and technical vocabulary of scientists to analyze their strategic responses to changes in their "habitat" is thus directly inspired by the lateral move in social anthropology. Lastly, I suggested Haraway's game of cat´s cradle as an alternative to the wolf´s game as a metaphor for how the lateral analysis presented in the three following chapters came into being. The basic analytical question posed in the three following chapters is thus not *"what is this an example of?"* thus making the practices of field participants an instance of a higher order phenomenon. Rather I ask *"what is this comparable to?"* thus experimenting with how new conceptual/empirical blends may bring out different flavors in the mixtures of knowledge economy. In a sense you could say that this mode of analysis uses the juxtaposition of two sources of "data sets", one coming from the research practices of field participants and another coming from the managerial and strategic practice of the same people. However, I really see both "sets" as *open* (Jensen, 2010, Jensen and Bowker, 2011) in the sense that they are simultaneously conceptual and empirical. Theories about plant/insect co-evolution or technical accounts of high-resolution visualization techniques function may be part of the "data material" collected through fieldwork, but they also form the conceptual framework for analyzing the mixtures between science and industry or between science and policy agendas. Rather than a method or a fixed analytical strategy, the inspirations and approaches that I have here loosely connected under the term "lateral analysis" is an experiment or *tool for thinking*. There is no telling whether it will work. That can only be found out by experimentation. Let´s play.

4
Feeling to see
– Oversight in Nano Science

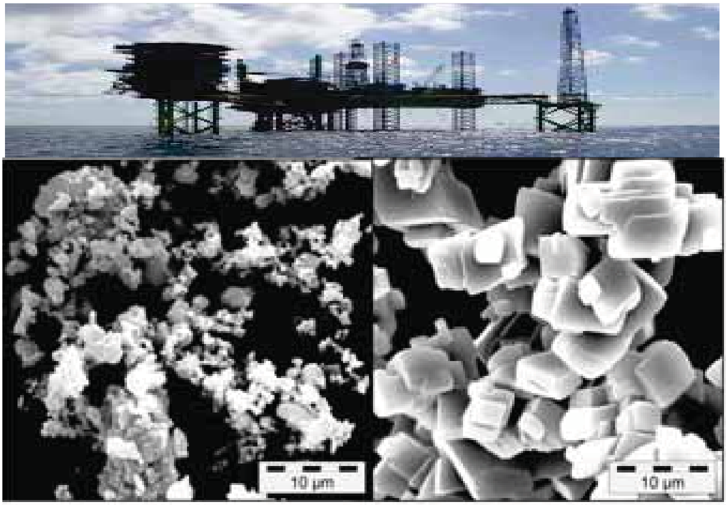

Figure 3: Power Point slide showing two chalk samples and an oil rig presented at the Copenhagen University, 2007.

There are around 200 people in the auditorium, most of them non-scientists[69]. The conference theme is "nano technology and risk" and is hosted by the Nano-Science Center. Thomas Bjørnholm is not wearing a tie. The microphone works perfectly; his friendly voice fills every corner of the room. To start his potpourri of what kind of research is done at the Nano-Science Center, Bjørnholm is showing images from the nano-geo science group's research project on chalk. On the large projector screen, two microscope images are aligned side by side. Bjørnholm explains that the left

[69] A previous version of this chapter is presently undergoing second review for publication in a special issue of *International Review of Management* focusing on vision and "looking" in management and organization. The comments of the special issue editors have been very helpful in finalizing the chapter.

side microscope-image shows crystals from a chalk sample taken from an oil field in the North Sea. The one on the right is taken from pure calcite crystals grown in a laboratory. The two crystal structures look very different. On the left, chalk crystals curl up in little irregular knots surrounded by lots of black space gaping between them. If one had a black and white photograph of an asteroid field somewhere in outer space, it would probably look something like this. On the right side of the screen large, regular rhomb-shaped crystals fill the image like big white teeth, leaving only a little black space around the edges. At first sight it seems like the right side is just a closer view of a similar structure to that on the left, but as is made clear at the bottom of the slide, both images are the same scale, picturing a structure only around 30 micrometers wide. North Sea chalk crystals, Bjørnholm explains, are for some reason smaller and more irregular than pure calcite crystals. Because of the unique shape of the natural crystals, the whole underground which makes up the North Sea oil fields have a sponge-like, porous structure. Pure calcite crystals like the ones on the right are much larger, more compact and make up a more homogenous, compressed structure. At the top centre of the screen, a smaller image of an oil rig is inserted.

Bjørnholm mentions how the chalk project is funded by Maersk Oil and the Danish Advanced Technology Fund and goes on to explain the difficulties in oil recovery. Getting the oil out of the North Sea lime structures is getting progressively more difficult. Technically, the oil fields are only half-empty but it will not be long before oil production will reach a crisis, since most of what is left has turned out to be hard if not impossible to recover. The sponge-like structure of the rock making up the oil field is a big challenge in North Sea oil production. Another challenge is the "stickiness" of North Sea chalk. The unique structure and property of the North Sea chalk make the oil bind to the surface of the lime structure despite the well established fact that chalk crystals normally repel oil. Bjørnholm says that it is estimated that 50-75% of North Sea oil seems to be stuck in little droplets clinging to the surface of the small cavities in the chalk and it will take a lot of work, a lot of money and a lot of CO2 to get it out. The chalk project has been looking at chalk surfaces for some time now to solve the mystery of why the North Sea chalk differs in structure and surface properties from pure calcite despite the fact that their chemical composition is almost 100 per cent identical. By analyzing the surface of crystal samples like those of the two images on the screen, the nano-geo science group hopes to find a way to get a more of the remaining oil out of the oil fields. Even a small increase in the current yield of the oil fields may make an enormous economic difference, Bjørnholm adds. If the total yield of the existing oil fields was to be improved by just one percent relative to the amount presently available in the fields, it would add roughly a whole year's production to the current total yield.

The chalk project presentation was my first introduction to Thomas Bjørnholm, the head of the Nano-Science Center at the University of Copenhagen[70]. In later conversations he has told me that at the time of this conference the slide with the two juxtaposed images was one of his most frequently used examples for showing the kind of work done at the center. The emphasis on collaborating with industry characterized the whole potpourri of research presented by Bjørnholm that afternoon. The presentation of the chalk project, however, offers something extra besides being yet another example of how nano science has become interesting to industrial corporations like Maersk. It offers an insight into specific strategies for *acquiring visibility* which help the the research projects in the Nano-Science center get funded. Bjørnholm' presentation demonstrates the simultaneous presence of *two types* of visibility which are both indispensible to scientific practice. On the one hand, the two images on the projector screen and the way they were created are an excellent example of how science makes *nature* visible through visualization technologies - a practice where scientists acquire visibility by placing themselves *behind* the microscope. On the other hand, the presentation as a whole also tells us something interesting about the way *science* itself acquires visibility through specific forms of representation: Bjørnholm and the scientists working on the project need to strategically place the project *in front* of different types of gazes or optics which make scientific practice visible, countable and comparable as a resource. Acquiring this *double visibility* is thus a necessary part of his strategic maneuvers to promote and support the Nano-Science Center.

I will argue that Bjørnholm' strategy for acquiring visibility take place by spanning both scales into this kind of double visibility. As argued by Kleinmann (2003) it takes more than a laboratory to raise a world; staying *behind* the microscope will not do. The chalk project could not have acquired its impressive fundability without specific maneuvers in the craft of scientific self-representation. Based on public representations of the chalk project, this chapter will describe the operations necessary for acquiring double visibility with specific attention to the costs involved in this accomplishment.

[70] The ethnographic material for this chapter draws on fieldwork done in the Nano-Science Center at the University of Copenhagen between 2007 and 2009. A series of observations and conversations with Thomas Bjørnholm as well as longer interviews and conversations with other scientists working at the Nano-Science Center forms the main part of the material. This is supplemented with documents such as press releases, popular science accounts and scientific publications by the scientists working in the chalk project. For the technical details on electron microscopy, I have relied on interviews with scientists working in the project and scientists dealing with the type of microscopes which have produced the images presented above. Popular science accounts of the workings of scanning electron microscopy have been used to supplement the technical information given by the field participants.

Destabilizing scales

Critical inquires into the two types of visibility outlined above have been attempted in several accounts, although they are rarely considered together. Constructivist and feminist approaches to the study of science and technology have provided a rich source of critical accounts on the effects of making nature visible through new visualization technologies, showing how representing cannot be thought separately from intervening (Hacking 1986, Rapp 1997, Fox-Keller 1996, Casper 1994, Franklin 1991, Haraway 1991, 1995, 1997, Latour 1986b, 1999b). Tsing (2007) and Helmreich (2009) have showed how new visualization techniques become important in a process of turning indonesian rainforests and the deep sea into new "socially "empty" frontiers for resource extraction. Although the critical studies of the commercialization of science which was presented in chapter two does give attention to the way scientific practice is becoming a "frontier" in itself, the coupling of the two types of "frontiering" via visualization techniques are rarely commented upon. However, other modes of vision than that of satellite images and microscopies are involved in making science visible as a site for resource extraction. Academic science is very much tied in with the instruments for measuring, auditing and performance-managing research in order to turn it into a countable, auditable and comparable resource (Strathern 1997, 1999, 2005, 2006). When it comes to instruments of auditing and performance measuring, peering does indeed equal interfering, there is no measuring of academic output that does not at the same time mold and rework academic research to fit the gazes and instruments by which it is measured (Shore & Wright 2000).

However, when the commercial and entrepreneurial aspects of science are under scrutiny, scientific practice and its marketization are oddly separated and often studied as two separate scales or practices defined by separate structural conditions and guided by conflicting normative assumptions (Jones 2009). Kleinman argues for an analytical separation between the "social" task of fundraising/gaining legitimacy and the "technical" task of constructing facts in the first is a precondition for the other. However, as apparent in Bjørnholm' power point the double visibility of microscope images and visions of future economic potential takes place in one gesture. The two kinds of visibility exhibited in Bjørnholm power point presentation (making nature visible as resource *while* making science visible as resource) do not take place as separate and distinct practices and does not seem to map neatly onto two neatly ordered scales of reality. Looking at people like Bjørnholm whose daily practice requires him to jump from venture pitch to academic lecture within the same working day, does not leave much basis for assuming that the two types of visibility are mutually exclusive, working against each other or distributed in prior or secondary activities. By contrast, I will argue that the simultaneity of these two types of visibility is vital to an academic endeavor like the chalk project. If any of these modes of vision are taken out there is no chalk project. The two practices draw on each other.

The sociology of translation seems a promising approach in its attempt to study the practice of science without drawing a-priori distinctions between different scales such as (science/society) or kinds of practices (science/communication). Studying science as a practice of translation, Latour and Callon have documented the ability of science to destabilize scales of micro and macro by dissolving the distinction between the "inside" the laboratory and "outside" in society (Callon 1986, Latour 1987, 1988, 1999). Latour argues that the idea of science as an enclosed epistemic realm immune to (or potentially corruptible by) the social fails to show how laboratories manage to transform society by extending their practices beyond the laboratory confinement and translate the interests of other actors to fit their own.[71]. "Give me a laboratory and I will raise the world" nicely sums up the Latourian depiction of science as a practice capable of destabilizing any scale between the local and the global, micro and macro or inside and outside (Latour 1999). The sociology of translation thus makes us zoom in on the power of the laboratory to jump or destabilize scales. Analyzing how Bjørnholm makes *science* visible would then become a matter of mapping the way he makes *nature* visible to enroll the consumers of knowledge production into giving legitimacy and funding to the Nano-Science Center, either by the formation of actor networks or by generating performative expectations (see Callon 1986 and Horst 2007 respectively). The double visibility of scientific practice is then a matter of translation which allows scientists to connect otherwise incommensurable scales. However, what is not often addressed in the sociology of translation are the *costs* of leaping between the social and the technical, making facts and making fundability. What are the expenditures of making science into a visible object of knowledge through various types of instruments, gazes and performance measures? One could argue that the increased pressure for commercialization of science and the general demand on scientists to demonstrate relevance and usefulness to society have transformed the task of scale-destabilization or network extension into a rather fragile and unmanageable performance measure in itself (Strathern 2005). As argued by Hans Müller Petersen in chapter one, scientists can no longer rely on a stroke of genius to carry them through their careers but should instead invest their energy in increasing the size and scope of their networks. The technical has to be made visible as relevant to the social.

Despite giving little attention to the commercial and communicative strategies which support laboratories in "raising the world", Latour is not blind to the fact that network formations entail costs. Pointing to the double meaning of the word

[71] The necessity of studying scientific and social issues in the same vocabulary is shared by a broader range of constructivist approaches within science studies. Ethnographic accounts of scientific practice have drawn attention to the problems entailed in assuming that science is a "special" enclosed practice immune to social influences (Knorr Cetina 1994,Latour and Woolgar 1986, Turnbull and Watson 1994, Jones 2009, Shapin 2008)

oversight Latour demonstrates how science only manages to produce clarity and overview by way of leaving out detail and failing to represent most of the characteristics of the object of study (Latour 1999). The notion of oversight as a necessary precondition to making nature visible becomes useful in a new way in relation to the chalk project. If oversight is a necessary precondition to representing nature, perhaps this also goes for representing science. Using this basic notion of oversight as an entry into looking at the way the Nano-Science Center makes both *chalk* and the chalk *project* visible generates an interesting set of questions for analysis. Peering into the strategies of visibility deployed by the Nano-Science Center, I will ask: *What operations are necessary for providing an image which makes the chalk project visible as useful and relevant to society?* And being attentive to the double meaning of oversight, I will also ask: *What needs to fall out of visibility in order for this operation to be successful?*

Given Bjørnholm' ability to destabilize scales by simultaneously representing nature and representing science, I will attempt to analyze the present example of doing so by way of the same scaling experiment[72]. I will thus experiment with creating analogies between the two types of visibility demonstrated in Bjørnholm' Power Point presentation, thereby juxtaposing data which we would otherwise see as taking place on different scales, one "social" and prior and one "technical" and secondary. I will argue that costs are visible on both scales and can be framed in the same vocabulary. Perhaps transporting the double notion of oversight from an analysis of representing nature to an analysis of scientific self-representation is worth a try. Rendering the two types of visibility comparable will allow me to use the practice and costs involved in acquiring one type of visibility as an analytical device for thinking about the practice and costs of acquiring the other type of visibility. I will thus be juxtaposing the way in which the *chalk samples* in Bjørnholm' presentation were made visible with the way in which the *chalk project* is made visible. For this experiment to work it is important to note that the two types of visibility are compared in order for the juxtaposition to work as an *analytical* device rather than an explanatory device. One practice does not represent or explain the other; rather, the two practices of acquiring visibility are made to *draw on* each other analytically (Maurer 2005). Understanding the costs involved in representing nature may do some interesting analytical work when used as a device to describe costs involved in making science visible.

[72] The work of Marilyn Strathern (2004, 1999, 2006) is the main inspiration for a methodology of scale experimentation. In many ways doing ethnography is always already a scaling experiment in the sense that knowledge of the field can only be produced on the basis of comparisons. The fieldworker shifts scales by nature by going "out" and back again and is therefore forced to juxtapose inherently incommensurable elements which would not come together outside a particular and partial assemblage created as an ethnographic effect. In the Strathernian version of social anthropology this takes place as a lateral move of transversal journeys in and out of different fields and on varying levels or scales. The juxtaposition of these "things" is what generates an analysis. The work of Riles (2001), Maurer (2005) Henare, Holbraad and Wastell (2007) and Pedersen & Høyer (2008) have demonstrated the power of the lateral move to produce interesting insights and surprising effects.

I will organize this description by dividing my account into two parts defined by a two-step procedure used in electron microscopy. First, I will describe the practice of *sample preparation*, showing how *chalk samples* need to be manufactured and enhanced in order to become capable of displaying "feelable forces" fitted to the gaze of the instrument. The notion of sample preparation is then put to work in an analysis of the way in which Bjørnholm and the Nano-Science Center enhance certain features of the chalk *project* by adding information to it in order to make it perceptible to the audience as relevant and useful for society. Second, I will go through the process of *imaging* chalk by way of electron microscopy, a process which converts "feelable forces" into a visual image at the necessary cost of disintegrating the sample and gradually erasing its structural features. I will compare this with the way Bjørnholm creates a promissory image of chalk research, paying attention to the erasures necessary to produce such an image. I will conclude by discussing the costs of the *necessary* process of manufacturing vision in order to produce objects of knowledge.

Before we begin, I will give a brief introduction to the status that visualization technology and the "need to see" enjoys at the Nano-Science Center and provide a few technical details on how the nano scale is rendered visible by high resolution techniques. Both introduce us to a whole new mode of vision:

Feeling to see - needing to see

It is the lunch break of the Nano-Science Center's conference on synthetic biology, a new interdisciplinary project which spans three faculties. Bjørnholm is at the table discussing the challenges of cross disciplinary collaboration with a guest speaker. Bjørnholm explains that one of the differences between nano science and life sciences lies in their respective conception of what constitutes *data*. "Biology is like this" he says to his guest, putting one hand over his eyes while he clumsily fumbles around the tablecloth, napkins and mobile phones on the dinner table. "They have very little idea about what the structure actually looks like because they work by just trying out a bunch of stuff. In nano science we are just the opposite you know, we are more like this": Bjørnholm lowers his head and puts his cheek down to the table and stares wildly at a small stain on the table cloth. While shielding his gaze from the activity of the room with his left hand, his right index fingernail is scratching the stain repeatedly. Bjørnholm and his colleague laugh at this comical self-portrait and Bjørnholm lifts his head. "That's a big challenge in this project, how to make those two worlds meet. In nano science, we really *need* to *see* things", he says, holding his hands as if he was clenching an imagined object in front of him.

Bjørnholm' demonstration of nano science's attention to and need for visual data is evident in this little performance, but can also be seen stated implicitly in his Power Points and other material released by the Nano-Science Center: Images are everywhere. They flood Bjørnholm' presentations, they are all over the Nano-Science Center's website and the aesthetics of high resolution microscopy decorate the covers of popular science journals every time "nano" is on the agenda. Explaining to audiences in lecture halls, conference rooms and popular science publications what images portray and how small the objects actually are is an almost obligatory move in nano science communication. Working in nano science means working with state of the art high resolution visualization technology. The vivid and self-reflexive body language of Bjørnholm provides a good point of entry to describe how visibility is acquired by nano scientists. Especially, it is no coincidence that Bjørnholm makes a "grasping" movement with his hands as he states his need to *see things*. This actually presents quite well how a lot of the visualization technology in the Nano-Science Center operates by "feeling" objects on order to see them. In high resolution techniques for visualizing nature *peering* often means *interfering (Hacking 1986)*[73].

Going back to the two Chalk-images in Bjørnholm' Power Point, it is important to note that these samples have not really been "seen" by a microscope in the common-sense understanding of vision. First of all, an electron microscope does not look like a normal optical microscope. The machine interface with the human eye is not an enhanced looking-glass type of seeing. The microscope itself has no place in which to fit a human eye, all the "seeing" is done inside a closed vacuum chamber and subsequently translated into an image on a computer screen. The need for such complexity in technologically mediated vision is due to the scale of the images. Nano-scale objects are so small that they disappear between the wavelengths of light. Consequently they do not reflect light at all. Nano scale is a dark place and it requires a whole other set of techniques to be made visible to the human eye. To capture a visual representation of what goes on at the nano scale you need to *feel your way forward.* The first peering into the nano scale was brought about in the early 1980s by the invention of scanning tunneling microscopes, machines for which "seeing" is literally touching. By moving a very small sensitive needle over the surface of the sample the minuscule movements of the needle are recorded and translated into an image. Electron microscopy similarly "touches" the sample in order to see it by bombarding the sample with electrons and registering the number and angle of the electrons which are in return shot off the surface. The point here is that in order to see anything in nano scale, you first have to "feel" it.

[73] The interventionist and transgressive nature of instrument gazes is by no means restricted to nano science alone. See Hacking 1986 for a thorough analysis of the history and philosophy of microscopes and Fox Keller 1996 for an account of the transgressive nature of the "biological gaze", a mode of vision which often destroys its object in order to render it visible. Franklin 1991, Rapp 1997 and Casper 1994 have provided rich accounts of the politic of new reproduction technologies in medicine with particular attention to the political, legal and ethical effects of visualizing the fetus.

Strictly speaking, the samples and specimens we find in a scientific laboratory are not really objects of nature, but rather products of a cultured environment (Knorr-Cetina 1994, Latour 1987, Latour and Woolgar 1986). In order to be useful as samples, objects have to be modified or *manufactured* (Knorr-Cetina 1999) to fit the technologies of the laboratory, the methodological requirements of the experiment and the research question. This process of manufacture means that samples taken directly from nature need extensive work before they can be made into objects of knowledge. Samples have to yield comparable results and often need further enhancement or protection in order to stay stable in the experimental setup. Let's go back to the example at hand: the two images of chalk crystals in Bjørnholm' Power Point slides. What does it take to convert North Sea chalk into an image on a projector screen? In order to generate the two scanning electron microscope images in Bjørnholm' presentations the two types of chalk have had to undergo several operations. The first is *sample preparation*.

Sample preparation: Seeing is enhancing

Preparing a chalk sample for electron microscopy is an intervention involving the addition of new elements to the sample in order to make it display "feelable" forces. This process of enhancement gives the sample new features which make it visible to a specific instruments' particular mode of seeing. I will first outline the operations involved in preparing the samples in Bjørnholm' presentation. Then I will compare these operations to the way in which Bjørnholm and the nano-geo science group manage to make the project display "feelable" forces which manage to "touch" the gaze of the audience.

Once a good chalk sample is found and cut down to size, the next step is to make it stable enough to withstand the imaging process. In order to stay stable during the 10 seconds it takes an electron microscope to generate an image, most samples will need extensive preparation. Biological samples or other samples containing water (such as the chalk samples from the North Sea) will have to be dried or frozen at extremely low temperatures in order not to evaporate and disintegrate in a vacuum chamber before an image can be generated. Another important component of sample preparation for electron microscopy is what I will here call *enhancement*. Due to the technicalities of some types of electron microscopes, the sample has to be turned into a conductive material in order for the microscope to generate an image. If the sample cannot conduct electricity, it will charge in the high vacuum chamber and as a consequence fail to produce a signal that the microscope sensor can "feel". Minerals like chalk are invisible to most types of electron microscopes if they are not first turned into conductive materials. In order to do this, the chalk samples we see in Bjørnholm' presentation have been supplied with a conductive metal coating i.e. the surface is covered with a thin film of gold prior to the microscopy. Instead of bombarding the chalk surface with electrons, the microscope reads electrons shot of from a gold-surface thin enough to retain the original topography of the sample in question. The bare unmediated surface of chalk is darker than the bottom of the North Sea and not available for visualization without sample preparation. As microscopes for imaging objects at the nano scale work by feeling the forces on the surface, *it is necessary to first manufacture the sample to display "feelable" forces*. Sample preparation can thus be summarized as a process of manufacturing the object of knowledge in a way which makes the sample perceptible to the instrument.

The preparation necessary to produce a visual representation of chalk surfaces at the nano scale shows, in a very literal way, how peering and interfering go hand in hand. The images we see of the chalk sample are thus not images of the material as it looks when placed at the bottom of the North Sea. The effect of sample preparation for scanning electron microscopy is a process of *adding something to the object*, thereby opening it up to a series of modifications and molding it into visual availability. The sample preparation of electron microscopy then *fills a gap* between the sensor and the sensed, by adding mediating objects that make the sample display the *feelable forces* required by the instrument in question. This process of filling the gap between the sample and the sensor is what makes the object acquire visibility. It is this additive operation of the laboratory which allows for visibility to develop and for objects of knowledge to gain objectivity (Latour 1999b).

Adding mediating objects

In what way do Bjørnholm and the Nano-Science Center *fill a gap* between the object and the gaze in order to make chalk research display feelable forces? As we shall see, a number of mediating objects have to be added to the chalk project in order to make it feelable to the audience. The first one is the history of chalk:

The history of chalk

As we learn in Bjørnholm' Power point presentation, the chemical composition of the two juxtaposed chalk samples is almost 100 per cent identical. Consequently, the obvious difference between their structure and property is a bit of a mystery. Something very subtle and small has to account for difference between asteroid fields and big white teeth. I will here cite an explanation which the Nano-Science Center has reproduced in several versions in press releases, popular science articles and geology news. The speaker is Tue Hassenkam, the scientist who created the two images used in Bjørnholm' presentation, explaining why North Sea chalk happens to be porous and sticky when really it ought to be compact and repellant.

"Ass. Prof. Hassenkam believes that the surprising behavior of the material in the surface of the chalk can be explained by studying how the chalk was formed. "Chalk is actually the casings of ancient algae. The algae gave their cases a type of "surface coating" to make them resistant to water. And it is probably this surface coating that we can see in action here, even 60 million years later" (News Release by the Copehagen University, May 2008. Published on www.geology.com).

The history of chalk and thereby the evidence for the presence of some form of tiny subtle surface coating is visible in Bjørnholm' Power Point slide. Bjørnholm demonstrates this by pointing to the left side image and shows that it contains small circular structures - so called coccolithes. Seeing coccolithes, however, is an acquired skill. The two images do not speak for themselves; pointing fingers have to be added in order for the relevant juxtaposition to become visible. The image on the right below shows a coccolithe in isolation, the left one shows coccolithes embedded in the North Sea chalk crystal structure (the most obvious one is in the upper left corner).

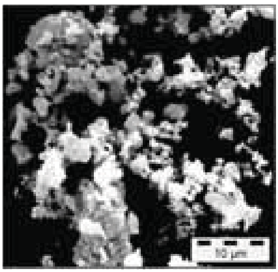

Figure 4a: Crystal structure containing residual coccolithes

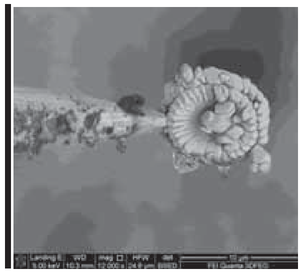

Figure 4b: Intact coccoholite close-up

Coccolithes, we learn, are the "diving suits" of ancient algae which managed to convert the minerals present in sea water into a calcite armor plate. North Sea chalk is thus made up of these ancient algae casings accumulated at the bottom of the ocean through millions of years. Bjørnholm points to the right hand image of pure calcite crystals and tells us that under normal conditions chalk is water soluble and will not retain its original topography when soaked in water for millions of years. What should have occurred on the left hand image in Bjørnholm' Power Point is what we see on the right hand image: A process of dissolution and re- crystallization which has gradually ripened the small irregular crystals into bigger ones until they look like big white teeth. This re-crystallization process happens with most crystalline structures as a function of pressure, temperature and time and results in a homogenous compact structure. North Sea chalk, however, is a different story altogether.

If North Sea chalk had been water soluble, we would not be seeing any coccolithes in the structure. The knotty and porous structure would have been re-crystallized into big white teeth, and the two samples would have looked identical. But, as the juxtaposition of the two SEM-images makes clear, this is not the case. No re-crystallization has taken place on the left hand image, which means that something has kept the structure stable for 60 million years despite the fact that it was soaked in seawater.

Bjørnholm tells me that the algae covered the calcite plates with a specific kind of surface coating in order to protect themselves from the seawater. According to Bjørnholm, the scientists in the chalk project have now discovered that this surface coating consists of a tiny layer of polysaccharides – sugar molecules. This surface "sugar" coating does not bind to the molecular structure of water and has the power of keeping the chalk rock in the North Sea stable.

What Bjørnholm is doing here is adding the history of chalk to the two juxtaposed images. As a mediating object, the stories of ancient algae highlights specific differences in a spatial juxtaposition between chalk *in the lab* and chalk out *in the North Sea*. But more addition is necessary in order to make the chalk project acquire the right kind of visibility. The history of the chalk sticks to another and more important addition: The problem of oil recovery.

The problem of oil recovery

The survival mechanisms of ancient algae are not very interesting in themselves. However, when added as an explanation to the porous stickiness of North Sea chalk, they mediate a connection between the chalk project and the problem of oil recovery. This connection is made by Bjørnholm in the following way:

Had the surface of the chalk sample been water soluble like it is in the pure mineral crystals on Bjørnholm' right-hand image, it would have happily made bindings with water molecules and conversely repelled oil. But as it is made evident in the electron microscope, North Sea chalk is different. Because of the polysaccharide coating, the crystals have remained small and irregular and the whole structure of the rock is porous and sponge-like. Bjørnholm explains that when a surface repels water (hydrophobic), its molecular composition will simultaneously make it bind to oil. The water-resistant sugar-coating thus explains why the surface of North Sea chalk is "sticky" and poses a problem in oil recovery.

As we recall, Bjørnholm' whole presentation revolves around the problem of recovering oil from porous, "sticky" rock structures, and draws the attention to the economic potential hidden in solving the mystery of chalk crystal surfaces. The survival mechanisms of ancient algae provide an important explanation to the mystery of why North Sea oil is becoming increasingly difficult and expensive to pump out. The larger holes and pools are now about to be depleted and the remaining oil is dispersed into millions of tiny little sticky cavities.

Consequently, Bjørnholm tells us, oil recovery today is not done like in the old days when the most difficult task was finding the oil[74]. Today as oil fields are getting older, the pump has to "work overtime" (www.science.ku.dk 24 March, 2009). In the North Sea oil fields oil production consequently takes place by making not one, but two holes in the underground rock. One hole is used to flush seawater into the structure whereby the oil is flushed out of the cavities and into the second hole - the bore hole- functioning as a well from which a mixture of oil and water can be pumped out of the oil field[75]. This process, however, still leaves between 50 and 75% of the oil at the bottom of the ocean. The problem of sticky chalk surfaces certainly adds to the problem, making the process of oil production even more difficult and energy-consuming.

The problem of oil recovery added to the juxtaposition of the two chalk samples by way of the stickiness of ancient algae casings. The polysaccharide-survival mechanisms of ancient algae are made to bind together not just the chalk and the oil but also the chalk project and problems of oil recovery. This process of addition, which Latour would call the making of associations, thus reconfigures the way we view the two juxtaposed images. The difference between the two juxtaposed samples now becomes the *explanation* for the problem in oil recovery. Having added the history of chalk and the problem of recovering oil from porous, sticky rocks to the two juxtaposed images, we are now presented with the following *spatial* juxtaposition:

[74] See Bowker (1994) for an account of the early intermingling of oil industry and information management.

[75] When talking me through his presentation, Bjørnholm describes oil recovery much in the same manner as it is done at Maersk Oil's website (www.maersk.dk). The following account is, however, mainly generated from Bjørnholm' original presentation as well as interviews and conversations with him on how he presents the Nano-Science Center and the chalk project in particular.

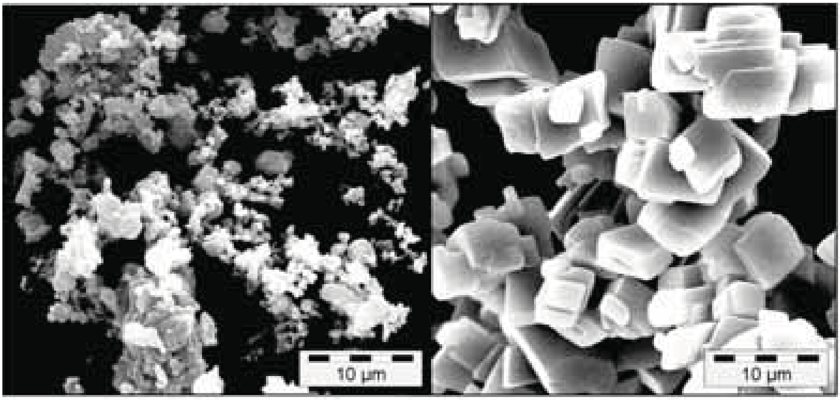

Figure 5a: Out in nature, water-proof, porous, oil-binding, fixed structure.

Figure 5b: Inside the lab, water soluble, homogenous, oil- repelling manipulable structure

Without the addition of pointing fingers, the history of chalk and the problems in oil recovery, the audience would still be comparing asteroid fields with big white teeth. Preparing the chalk project for representation thus adds mediating objects to images. In electron microscopy this task is done in order to fill a gap between the object and the instrument. How does the addition of ancient algae and problems in oil recovery *fill a gap* between the sensor and the sensed when Bjørnholm and the Nano-Science Center represent the chalk project? Looking at the way the project is represented in the media, it seems that the chalk project is required to fit a specific kind of gaze: That of the petrochemical-dependent consumer.

Filling the gap

With every account produced about the chalk project comes an account of a coming oil crisis. The fundability of the chalk project seems a direct product of this enhancement. Bjørnholm makes explicit reference to the involvement of Maersk Oil. A year after his presentation, BP followed up with a basic science grant whose size made headlines in the Danish world of research. The connection between chalk research and global oil crises is nicely demonstrated by the manager of the Nano geo science group when presenting the work of her group in a prestigious conference presentation titled: “Nano Geo Science: Cleaner water, more oil and taking out the garbage”. After listing the projects currently running in her group – most of them relating to problems of pollution and climate problems - she specifically addresses the rather controversial research interest in oil recovery. How does this agenda fit into a talk concerning “earth’s future”:

> "Now on the middle one there: "producing oil from existing reservoirs"… You can't make me say that it's not important to make sustainable energy forms now! We need to start now. We need to develop and bring some of the existing technologies to maturity and we can't wait with that. I really wonder why governments are not subsidizing alternate energy to a greater extent. But I'm not a politician. We need to be producing sustainable energy forms. But in the meantime we can't just stop using oil tomorrow. If we stopped using oil right now, this instant, half of you would be sitting there naked because the clothes you are wearing contain oil products, the computer I am working on now, the car you drove in or the train, lots and lots of plastics… petrochemicals, we need… we can't stop using oil… right now. So if we don't find a little bit more oil out of the existing reservoirs then - oil people tell me - we've got… we've used up all the easy oil, the only oil that's left is the hard oil: Hard to get out of the ground, hard to produce in a way that doesn't pollute - from coal for example. So, if we don't find some more we're going to have oil prices like we did last summer or worse. And we know what kind of instability that costs. So: Society's problems, that is what we are aiming at". (http://nano.ku.dk/english/research/nanogeo/susan_lecture_stream/)

The professor refers to as the view of "oil people" when telling us that we will soon be left with only the kind of oil which is hard, impossible or polluting to recover[76]. Oil is a finite resource and oil fields are about to be half-empty. Given what we know about cavities and stickiness, this might actually mean almost empty. The prospects of an inevitable decline in oil production definitely constitute a feelable force for any audience. Most of us will be naked, only a few of us will be driving or working on laptops, and discount airlines will be an odd parenthesis in the history of infrastructure. An interesting move is the transition from rendering the view of oil people equivalent to that of the consumer to that of rendering the needs of the consumer equivalent to those of "society". Adding the view of oil people to the representation of the chalk project directly touches the petrochemical-dependent consumer and in turn produces an almost unquestionable sense of necessity. Demonstrating a direct connection between the chalk project and the needs of a society consisting of oil dependent consumers makes it display the right kind of "feelable forces" to an audience who can immediately identify with a need for nylon, electronics, medicine, heating and fuel.

Filling the gap between the sample and the gaze that renders is visible demands sample preparation: adding the history of chalk and the view of oil people to the sample in order to make the chalk project display a feelable force to future

[76] The view of "oil people" is related to a larger research area, that of "peak oil" or "peak oil theory", which the professor does not address in this particular presentation.

consumers. When framing the chalk project in terms of its relevance to oil production and connecting this problem to the immediate needs of the consumer as the most immediate representatives of "society's problems", its visibility as relevant and useful research becomes crystal clear. In fact, adding mediating objects to the spatial juxtaposition between laboratory sample and North Sea sample makes chalk research visible as a societal necessity, saving us from a world of "instability". The steep increase in the funding displayed by the nano geo science group testifies to the efficiency of this enhancement. Chalk research definitely managed to acquire visibility to Maersk Oil, the Danish Advanced Technology Fund and BP. This strategy for acquiring visibility works by highlighting and enhancing particular details which make North Sea chalk different from other types of chalk. Fitting the sample to the gaze of the instrument here means manufacturing the chalk project as an object of knowledge, so that is now includes the assumptions of "oil people" and future consumers. Without making the chalk project visible in relation to this particular optic, the project will not acquire visibility. Preparing science for representation and enhancing it to display forces that are immediately "feelable" to future consumers and the oil industry prepares the chalk project for the production of a certain type of imaging: that which produces a promise.

Imaging: Seeing is erasing

The following account will first outline how the imaging process takes place in electron microscopy. I will show how the *conversion* of "feelable" forces into a "seeable" image involves a necessary cost of disintegrating the structure of the sample, thereby *erasing* details in the structure of the surface. Conversion and erasure are thus necessary trade-offs for imaging chalk crystals. I will then draw on the notion of conversion and erasure to look at the promissory mode in which Bjørnholm strategically represents the chalk project. Making a promissory image of the chalk project involves a conversion in which specific details in chalk research need to be blurred or erased. I will argue that these costs are constitutive to the production of a promissory image.

Conversion

As explained earlier, most microscopes designed to make the nano scale visible are really devices for converting the "feeling" of forces into the "seeing" of surfaces. Electron microscopes like the one used to create Bjørnholm' slides make nature visible in a rather violent manner: putting the carefully prepared sample inside a high vacuum chamber and bombarding it with an electron gun. This bombardment results in secondary electrons being "knocked off" from the surface of the sample. The electrons that leave the surface of the sample will fly out in the vacuum chamber. There they are registered by a receiver which "feels" the number of electrons shot off from different angles. A high number of electrons signify a steep angle on the surface and a low number a more flat slope. The receiver then amplifies this “signal” and sends it to a computer. Here the signal is converted into an image, showing in great detail the topography of the surface. The result of this conversion into a visual image is quite breathtaking and looks a bit like a normal black and white photograph only a very close close-up. When looking at the detailed surface topography of pollen, insect wings, mineral crystals or snowflakes, it feels like having a snapshot of the otherwise invisible nano scale. One easily forgets that what is “seen” is really a calculated conversion of secondary electrons hitting a sensor. But the conversion of feelable forces into visual images comes at a price, namely the disintegration of the sample itself.

Erasure

The scientists making the images in Bjørnholm’ presentation are very aware of the fact that samples are not just naturally given and fixed entities. Even with the most careful preparation, samples will change and often disintegrate during the imaging process. When one of the experienced scientists working with electron microscopes first showed me a series of coccolithe close-ups he made it very clear that the particular kind of electron microscopy that he had used to generate the images always involve working “against the clock”. The series of images showed a clear deterioration of the structure by each image produced in the electron microscope. The specific lines and structures were gradually erased for each image generated in the microscope until they eventually faded into white. The visibility of chalk crystal structure and composition will inevitably disintegrate under the electron bombardment. Even a robust mineral like chalk which tends to make good stable samples only last a couple of minutes inside some types of electron microscopes during which the images generated will lose their quality and fade. This makes imaging difficult and series of experiments on the same sample almost impossible. Before a proper image has been secured, the interesting features are likely to have been erased. All that remains is the image itself.

From feeling to seeing: Imaging promise

The image presented of the chalk project takes the form of a promissory statement. Comparing the process of creating this image with the process of electron microscopy, we are made to ask what kind of conversion and erasure is needed in order to pose a promise. Bjørnholm manages to create a promissory image by making a leap from the nature/laboratory juxtaposition of the two samples and into a juxtaposition of present and future North Sea chalk. I will here show how this is done and discuss what kind of erasure is involved when Bjørnholm converts feelable forces into a promissory image of the chalk project.

Discovering the layer of polysaccharides is only the beginning, Bjørnholm tells me. Polysaccharides may be water resistant, but they are not very hard to break down by other chemical compounds. When talking me through his presentation, Bjørnholm repeatedly mentions the possibility of adding a kind of detergent or "soap" to the sea water that is flushed through the sponge-like structure of the rock in oil recovery. If one could find something that is not too expensive and add it to the water infused into the porous rock, Bjørnholm says, the sticky surface coating may be washed away and consequently the chalk would repel oil rather than bind to it. This would mean that more oil could be recovered from the North Sea.

However, Bjørnholm' presentation of the chalk project goes much further than those of his colleagues. If dissolving the surface coating will really prove efficient in facilitating a process of dissolution and re-crystallization, the problem of the sponge-like structure of North Sea chalk might be solved along with the problem of stickiness. There may be a way to modify the structure itself*, if only* it could be made water soluble. Hypothetically, removing the surface coating and baring the surface of the rock would expose the surface of the chalk crystals directly to the sea water. Without the surface coating, the chalk crystals would be water soluble and one might imagine the possibility of dissolving the whole crystalline structure in the chalk of the North Sea, making it lose its specific properties seen on Bjørnholm' left hand "asteroid field"-image by turning it into a water soluble, moldable substance like the one on the right hand image. Once dissolved, Bjørnholm says, the chalk might re-crystallize and ripen as in the right hand picture, forming bigger and more homogenous crystals; big white teeth:

> "Well, you see, the crystals on the right side image are packed much closer together than those on the left. And as this all takes place two kilometers down and under an enormous pressure so all process that diminish the volume can be an advantage to oil recovery because the structure can be compressed further, leaving little or no cavities left. So the gravity of the earth would help pressing out the oil here. And if we combine this with a chemical process which will allow for a form that is more compressed then it would be even more… I mean the pressure added by gravity can be used to assist this process (...). This, of course, all depends on the possibility to chemically induce a process of re-crystallization otherwise the structure will stay like it is, regardless of the pressure (…). If this is successful then the structure is pressed together and the oil will be squeezed out of all the little cavities. That would make things a lot easier (Interview January 2010).

Bjørnholm is hoping that dissolving the layer of polysaccharides will make North Sea chalk on his left hand picture look and act exactly like the pure calcite crystals on his right hand picture. No longer being water resistant, sticky and solid but instead being water soluble, oil repellant and moldable.

> “When I get really warmed up to my theme in my talks, this is the point where I’ll say something like: “Future oil recovery might become analogous to milking a cow”. But that’s only when I’m really on a roll (“oppe og køre”). (Interview December 2010)

When Tomas is “on a roll” he uses the juxtaposition of the two samples to show not only what is on the picture and how it relates to the history of chalk but also the future prospects of knowing more about chalk crystals and their surfaces. Not only does knowing the molecular composition of the surface of North Sea chalk allow him to pose a solution to the stickiness-problem, he is also able to image a possible future, in which oil fields can be converted from hard porous rock and into soft sponges which can easily be squeezed or “milked” for their precious content. An important conversion is taking place here. The two images in Bjørnholm’ presentation are converted from a *spatial juxtaposition* between North Sea chalk and pure calcite into a *temporal juxtaposition* of present and future oil fields:

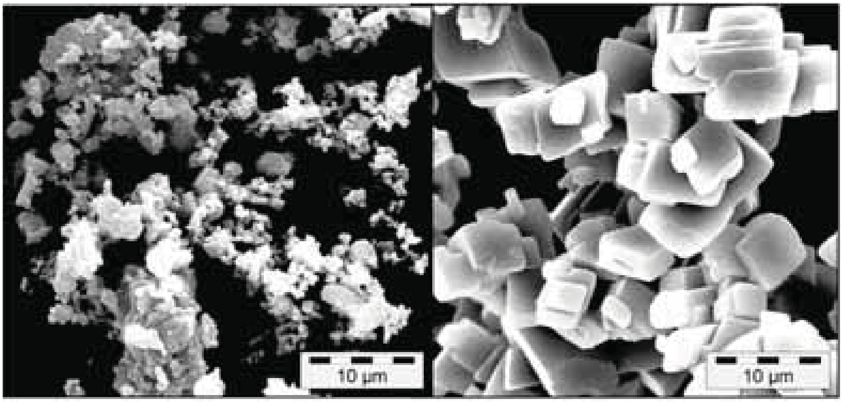

Figure 6a: Present oil field *Figure 6b: Future oil field*

In the future, the toolbox of nano science might make the chalk sample on the left side of Bjørnholm’ presentation look and act just like that on the right. Taking a spatial juxtaposition and converting it into a temporal one allows Bjørnholm able to convert the “feelings” produced by the spatial juxtaposition into a the “seeing” of a promissory image: The possibility of a future North Sea oil field that can be “milked” like cows.

Samples are indeed manufactured in the process of imaging as both chalk crystals and chalk projects need to undergo various processes of preparation and imaging in order to fit the gaze which renders them visible. In electron microscopy, this is an absolutely necessary process if one wants to bring invisible scales into visibility. I would like to suggest that the chalk project is no different from chalk crystals in the sense that it constitutes an invisible scale if not manufactured into an object of knowledge, visible and feelable as a potential resource. The chalk project is no longer the same once it has been reconfigured by Bjørnholm as a possible future solution to crises in oil production. In this conversion chalk crystal surfaces are converted from being a *barrier* to oil recovery into being a *possibility* for optimized resource extraction. Following Bjørnholm' presentation and the future of oil recovery he images in it, the only barrier holding the oil industry back from milking North Sea oil fields like a cow seem to be a microscopically thin layer of polysaccharides. The relations between chalk crystal surfaces and the oil industry are thus also converted in this imaging process; the former now becoming a site for intervention and resource extraction by the latter. By this conversion of the signal generated from the sample, a dark, intransigent rock structure is transformed into manipulable crystal surfaces full of yet unused potential. The imaging process of presenting the chalk project in terms of juxtaposition between present and future oil production is a process of manufacturing oil fields that are *half-full* rather than *half empty*. The chalk project now figures as a direct route to that specific future and is imaged as useful, relevant and necessary research.

When seen as promissory imaging, we become aware of the costs involved in this conversion. Bjørnholm is not blind to costs. When interviewing him about his use of the Power Point slides he makes it very clear that even though he would repeatedly use the punch line about milking oil fields like cows to end his power point presentation, statements like that are to be used with caution:

> "The important thing I need to stress here is that this is *the dream*", he says. "I will always be aware of how I say a remark like that and make sure to make say it tongue-in-cheek. ("med et glimt I øjet") (…) For example you would never hear the manager of the nano geo science group say something like that, and I would never talk of my own research in such terms. My job in this situation is to paint with the broad brush" (interview Bjørnholm 2010)

"Panting with the broad brush", means making nano science feelable enough to be perceived by a non-scientific gaze but also producing a promissory image vague enough to be uncontestable. This is a delicate balance. Using the "broad brush" necessarily involves quite a lot of erasure or bleaching in order to convert two chalk samples into a promissory image.

Painting with the broad brush thus means allowing some of the less promissory or more controversial details to be erased, blurred or simply fade into white. This becomes apparent when talking to the people collaborating with or working in the nano geo science group. Some would directly contest Bjørnholm' hypothesis of re-crystallization and the possibility of dissolving the lime structures of the North Sea in water. According to one of the scientist the idea of dissolving North Sea chalk in water is very unlikely. You would need acid to dissolve a chalk structure like the one in the North Sea oil fields, and removing the polysaccharide coating would not change the structure dramatically, although admittedly it might make it less water resistant. In fact, the scientists in the chalk project are not even sure that the surface coating really consists of polysaccharides at all. So far, nobody has been able to produce an actual image of what lies between the small plates making up a coccolithe, the sample deteriorates in the electron microscope before a good close-up can be obtained. Furthermore, some argue that re-crystallization is a process which takes hundreds of years and requires a much higher pressure and a temperature than that present under the North Sea.

Converting the spatial juxtaposition into a temporal one to image a promise is thus *inseparable* from erasing or bleaching information about the project which cannot be translated into a promissory image. The difference between Bjørnholm' broad brush image of the chalk project and more academically targeted presentations painted with a technical "narrow brush" is not one of more or less accurate. One image is not more true to the original sample than the other; they are both manufacturing objects of knowledge to make them visible to specific gazes. Creating a promissory image at the cost of erasing details which do not make themselves feelable to the audience seems to be a necessary operation to imaging what Bjørnholm calls "the dream". Being a talented broad-brush painter thus means knowing what to erase when confronted with specific audiences. This work of trading off technical details or "fading into white" seems pivotal to the work Bjørnholm does as a research manager and crucial to the success and fundability of the Nano-Science Center.

The promise of science

Analyzing the Nano-Science Center's representation of the chalk project in terms of a two-step model which shows the manufactured nature of visible objects show some of the costs involved in this process. It is, however, important to notice that I have *not* made the comparison in order to deflate the images produced in either types of visibility as mere fiction. In the same way we accept the *effect* of scanning electron microscopy to produce detailed topographical images of the calcite surface, making them visible and knowable, we must accept the *effect* of the image Bjørnholm and the

Nano-Science Center produces. Manufacturing objects of knowledge is precisely what makes them objective, real and uncontested (Latour 1999b, Knorr-Cetina 1999).

The unmediated, untouched reality of the chalk project is at least as dark as the nano scale. The chalk project would not have been so successful in attracting funding, researchers and media attention without the simultaneous task of representing nature and representing science to fit the optics of promise and value creation by which science is rendered visible to those who fund and govern academic research. It is the double visibility and promissory imagery that makes it interesting to Maersk Oil and later BP. This chapter has showed how acquiring visibility necessarily involves a manufactured enhancement and erasure, whether acquiring visibility takes place behind or in front of the microscope. The point of comparing the two types of visibility is thus *not* to export the crisis of representation into the field of entrepreneurial science to critique the close relation between peering and interfering. Rather, the comparison allows me to show how manufacturing of objects of knowledge *is a necessary precondition to knowledge production*. I have demonstrated the effects of such a process of manufacture as they play out in double visibility: Without paying the cost of manufactured enhancement and erasure, no image can be produced and no object of knowledge can emerge. This analysis clearly applies even when the object of knowledge happens to be science itself. In both cases *oversight comes at the cost of oversight*.

In order for the chalk project to be able to produce knowledge, its knowledge production must be fitted to the gaze that sees, approves and funds it by adding elements which fill the gap of relevance to an abstract future society. The question remains what this cost leaves out. Which forces are not "feelable" and which kinds of scientific practice are excluded from visibility in the type of optics which Bjørnholm and the Nano-Science Center try to fit their research to. The necessary operations of making the chalk project "feelable" in order for it to become visible to the gaze of specific audiences seems to come with a set of specific costs: 1) The enhancement of features which display relevance to future consumers, consumers who happen to think like "oil people" in terms of finite resources and an urgent need for more efficient value extraction 2) The erasure of non-promissory elements like technical facts and specificities which cannot be converted into a promissory image in the eyes of this type of consumer.

When asking what kinds of forces are not feelable to the audience and hence are not visible in scientific self representation the attempt to stabilize a vague performance measure by reaching out to the consumer is a key point. Performing visibility in the way Bjørnholm and the Nano-Science Center do have as a necessary cost. Elements that are not related to consumption are left in the darkness of the nano scale. Apparently, non consumer-friendly progress in science and scientific knowledge seem too tiny or irrelevant to reflect the gaze of the assumed audience.

Perhaps we need to see the scientific imaging of promissory futures as more than "world raising" extensions of the laboratory network. By making a promissory image fitted to the gaze of a future consumer science is not merely extending its network and translating interests. Rather, both nature and science are manufactured and tampered with in terms of being enacted as resources fitted for consumption. Chalk is transformed from recalcitrant solid rock into moldable, "milkable" crystal surfaces and thereby the chalk *project* is transformed from highly technical and theoretical basic science research to a possible solution to a coming energy crisis. What was a barrier to value-creation is converted into a resource: North Sea chalk crystals are a solution rather than a problem and basic science research is a site for value creation rather than a cost for society.

These two moves are made simultaneously, they draw on each other. Without the technicalities of chalk samples and electron microscopy there would be no images to show to investors, reviewers, research boards, or media. Without broad brush painting and scientific promises there would be no money, instruments, buildings or staff.

What is interesting here is that turning chalk and chalk research into an object of knowledge seems to take place as a conversion of sites which demands resources into sites for resource *extraction*. What fall out in this conversion are forces which cannot be felt by the potential consumer and hence cannot acquire visibility as an object of knowledge. The cost of this visibility seems to be a necessary erasure of the less promissory aspects of the project as painting with the broad brush entails collapsing differences and bleaching out scientific findings which are not directed towards consumption and resource extraction.

The question remains whether the "world raising" power of science could also be used to invent new forms of visibility rather than adapting to the ones presently available? New discoveries in nano science often happen on the basis of modifying or building *new types of microscopes* crafted to make the invisible visible. Taking this observation into the practice of scientific self representation, it could be interesting to inquire into the possibility of inventing modes of vision not based on an image of resource extraction and not necessarily fitted to a consumerist gaze. This, to be sure, would involve entering into a proactive modification of the gaze that funds and approves of scientific practice. In light of the analysis conducted in this chapter, the promise posed by science may not lie in its power to *assimilate* knowledge production to the present problems of society in order to extend the laboratory and raise the world. What makes scientific practice promising is rather its ability to *invent* new modes of vision by fitting samples to gazes but even more so by inventing new instruments and modes of vision. Needless to say this is a lot easier when standing behind the microscope and much more challenging when placed in front of one that is already constructed for a specific mode of vision. What is presently lost in the darkness between the wavelengths of consumerism and value creation could perhaps be made feelable and visible if scientific self-representation was expected to be less adaptive and more creative in its ways of acquiring visibility. This would of course only be possible if the gazes that render scientific fundability and legitimacy visible were available for the same creative modification as the one scientific instruments undergo when scientists work to extend vision into new scales. Perhaps the word for such a modification of optics would be something like *enlightenment*.

5
Beyond the Boundary

> "For a start, the concept of the boundary is one of the less subtle in the social science repertoire". (Strathern 2001, p.520)

> "The 'symbiotic agreement' is an event, the production of new, immanent modes of existence, and not the recognition of a more powerful interest before which divergent particular interests would have to bow down. Nor is it the consequence of a harmonization that would transcend the egoism of those interests" (Stengers 2010, p.35)

In chapter one I presented some of goals set by the Danish Government for the transformation of the university sector. This chapter will be looking specifically at the way one particular scientist related to the policy agenda of turning knowledge production into a resource for industry[77]. As previously discussed, several actors had identified academic science as an area with much "unused potential" and the problem of knowledge transfer and knowledge dissemination were becoming key areas of interest in research policy. This was mainly articulated as a problem of transgressing institutional boundaries between university and industry. The *insight to invoice* agenda as it was launched in 2003 consisted of a set of commercialization-initiatives targeted at bridging this gap between science and corporate development. The core idea was to promote more *interaction* between the two sectors, thus bridging the perceived gap between them. In the agenda set by Danish research policy, scientists were framed as producers of knowledge while industry was perceived as consumers of knowledge. The funding system followed this logic, with many new initiatives supporting science/industry partnerships. Having contacts in the world of corporate science and having an industry partner written into a research application has thus become an important strategy for scientists, especially if the division of labor can be articulated as a unidirectional flow from production to consumption of knowledge.

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[77] An early draft of this chapter is published as a journal article in a special issue of *Bulletin of Science, Technology and Society* focusing on "corporate science" (Gorm Hansen 2011 – in press). The chapter has benefitted greatly from the comments of the editors and two anonymous reviewers.

In this chapter I will inquire into the strategies deployed by one particular scientist in relation to the increased focus on bridging the gap between insight and invoice. I will focus on Birger Lindberg Møller, professor in plant biology at the Copenhagen University. How did he deal with the increased focus on science-industry collaboration? We already met professor Møller in chapter 2, where he resisted my focus on the strategic or "political" side of his work by impatiently asking me "when are we going to talk about research". Møller wanted to talk about plant biology. He did not accept that I started out in the "social" and "political" part of his work but required that we talked about the technical and scientific content of what he does. Otherwise, he felt I would miss the point and produce a purely "political" narrative. In response to this, this chapter will laterally align plants and insects with scientists and biotech corporations. "Biological strategies" and "scientific strategies" are allowed to draw on each other. In juxtaposing plant/insect relations with science/industry relations I will experiment with new ways of talking about the relationship between science and industry. What is generated in this lateralization is an alternative to the concept of the boundary as the main analytical framework for thinking about the commercialization of science.

The lateral exchange allows me to suggest the concept of *symbiosis* as a model for understanding the relationship between science and industry. Symbiosis is an interesting analytical tool as it denies us the habit of thinking in fixed and clearly bounded entities connected by a relation between them (Haraway 1995). A symbiotic relation consists of two species which form a mutually dependent whole by diverging from each other in ways that make each of them useful for the other's well-being (Stengers 2010). Thinking with the concept of symbiosis makes it possible to take into account the mutual dependencies between plant biology and biotech without engaging in a-priori analytical separations that divide science and industry into a set of isolated constituents with more blurry boundaries. It may, despite its somewhat romantic connotations, also prove helpful in thinking critically about the commercialization of science without retreating into binary a-priori divisions. Symbiosis, we learn from Møller, is not a stable harmonious relationship; rather it is a subtle form of ongoing "chemical warfare" in which interests never fully converge. I will be arguing that a focus on integrating boundaries and hybridizing sectors is akin to a "parasite logic" (Brown 2002, 2004) of unidirectional consumption with little or no value-production in the collaborative chain. I will also argue for a move beyond boundary blurring and hybridization into a "symbiont" logic, where mutual flows of production and consumption add value to each party while allowing for a vital divergence between them. First I will give a short presentation of ways in which the concept of the boundary has been used in STS to think about the strategic behavior of scientists in relation to commercialization. Then I will briefly introduce a critique of STS´s love of boundaries and hybrids as it has been posed by Strathern. From there I will move on to a lateral alternation between descriptions of Møller´s academic and managerial work. Developing analogies between the two do different analytical work than that of the conceptual language of boundary and hybrid. I will suggest that rather than protecting a pre existing boundary between academic science and the biotech industry, Møller is engaged in making a symbiont relationship manageable.

Boundary work

The problem of strategically managing the relation between science and industry has been greatly inspired by Thomas Gieryn´s notion of *boundary work* (1983, 1999). According to Gieryn's original tenet, the boundaries of science are flexible and ambiguous. Science is *"No single thing"* (Gieryn 1983, p. 792). Boundary work designates the literary and rhetorical strategy by which scientists construct the boundaries of their practice in relation to shifting audiences. By rhetorically defining a given practice as "other," scientists struggle for authority, autonomy and resources by constantly redefining what constitutes science.

In critical studies of the commercialization of science, Gieryn's original tenet has been taken up and reframed in terms of boundary *maintenance.* According to Tuunainen (2005) the celebration of the science/industry synergy and productive integration between science, industry and government is much too optimistic. Accounts of dynamic integration and productive merging of previously separate sectors overlook the fact that new hybrid organizational forms live dangerous lives in the zone between different conflicting institutional norms and regulations (ibid.). In a case study conducted in a traditional university department of plant biology, Tuunainen shows how a hybrid firm comes to be marginalized from the core academic department over a number of years by a process of organizational and material boundary work. In Tuunainen's study, the hybrid firm or the incubator, so often celebrated for its integrating power (Etzkowitz 2002) takes on the properties of a tool for upholding boundaries rather than transgressing them. Boundary work, in Tuunainen's definition of the concept, is a matter of boundary *maintenance* rather than a matter of erecting new boundaries around multiple and flexible practice. According to Tuunainen, the demarcation of science is by no means managed by scientists' rhetorical and literary strategies alone. Rather, the boundary work of academic scientists is embedded in existing organizational structures and institutional spheres that are not easily negotiated or transformed. The attempts of scientists at maintaining or negotiating institutional boundaries should not be seen as a solely rhetorical, literary ideological phenomenon where science is defined as if by magic in the performative statements made by its practitioners. Rather, the negotiation of institutional boundaries is depicted as an ongoing material and organizational practice building on existing institutional structures (Tuunainen 2005, Tuunainen & Knuuttila 2009).

Lam (2010) similarly draws on the notion of boundary work in an analysis of scientists' strategic roles in interpreting and shaping the intensified demand for science/industry collaboration. The study shows how scientists manage "role-tension" between scientific and entrepreneurial identities in response to the increased infusion of the marketplace into academia. The strategies deployed by scientists in negotiating the boundaries of their work and role identities vary from scientist to scientist and Lam lists a typology ranging from the "traditionalist" to the "entrepreneurial". Lam´s study shows that different strategies of managing role-tension are present for the different types of scientists. From this perspective it seems that the overall response to the commercialization of knowledge is the construction of new hybrid identities. Lam concludes that the boundary between science and business is "fuzzy but not dissolved" as the demarcation between academia and corporate development helps settle role-identity and decreases the complexity of having to follow both academic and entrepreneurial goals. By making demarcations between science and industry scientists manage to "adapt to external changes without undermining the core logic of academic science" (Lam 2010, p. 334).

Strangely, the concepts of boundary and hybrid thus seem to be important analytical resources for the policy agenda which promotes science/industry interaction as well as for the critical commentary which has rendered it problematic. Both are occupied with how previously separate spheres of activity and institutional orders mix. Whether the future of the entrepreneurial university is articulated as an optimistic narrative of interaction and synergy or as a pessimistic story of the conflicts involved in maintaining and protecting institutional boundaries and coherent identities, the concepts of boundaries and hybridization remain the basic vocabulary.

Beyond the boundary

Strathern has questioned the widespread use of concepts like boundary and hybrid in STS. Claiming that these concepts are not among the most subtle in the social science repertoire, she suspects they are not capable of doing the kind of analytical work necessary to articulate a complex and heterogeneous empirical field (Strathern 2001). Starting from the boundary invariably reduces complexity, Strathern argues. The problem with boundaries and hybrids is that it fixes the categories by which we think of the two terms in a relation[78]. Consequently, when analyzing the intertwinings of science

[78] In the vocabulary of Deleuze´s radical empiricism (Inspired by William James), the theoretical problem of this chapter amounts to thinking of relations as "external to their terms" (1991, p.99). Relations are thus conceived as primary rather than as something which connects pre-existing and fixed entities. In taking the relation as something that precceds the terms, relations are analyzed as something which transform what they connect. Deleuze's empiricism follows the transformation rather than drawing lines between pre-existing entities.

and industry as boundary blurring, boundary work or hybridization between conflicting roles, the categories of science and the market (science and industry) will be “left uninflected” (Strathern, 2001, p.520). Strathern argues that the analytical vocabulary may simply invite a reduction of complexity by reifying the two terms in a relationship. With Strathern´s critique in mind, we may suspect that the concept of the boundary lacks the subtlety to facilitate an articulate description of how scientists strategically relate to the increased focus on commercialization.

The above critiques suggest that starting an analysis of science in the knowledge economy by pointing to the erosion or blurring of boundaries between may be misleading. Perhaps we miss important subtleties when describing scientific practice in terms of tensions between pre-existing institutional spheres and the emergence of hybrid identities between them. Strathern insists that distinctions do not necessarily imply a preexisting purity. Let´s take these reflections with us into the office of Birger Lindberg Møller:

The many mustaches of plant biology

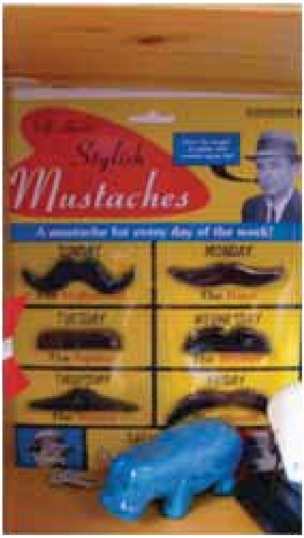


Figure 7: Stylish Mustaches

Professor Birger Lindberg Møller is the head of a large interdisciplinary research center called Pro-Active Plants, situated in a Danish university. The center is the continuation of a previous research center (Center for Molecular Plant Physiology (PlaCe) which was funded by two-5-year grants from 1998 to 2008. Pro-Active Plants was funded from 2008 and so far is set to run until 2013. Both centers were based on a series of successful collaborations spanning several universities and departments as well as Møller´s lifelong dedication to plant biology and GMO. One of the major objectives for the Pro-Active Plants is to find out how plants "talk" with their environment through the exchange of chemical information (Malmberg 2007). Things are going quite well for Pro-Active Plants; recently the institute which houses the center celebrated the simultaneous appointment of *six* new professors - three of them situated in the center and appointed to support its leading research programs. Things are also going well in terms of funding, despite the fact that money has been notoriously difficult to get in the area of Danish plant GMO due to political resistance and financial risk.

On the bookshelf in Møller's office is a package of fake "Stylish mustaches", one for each day of the week. He reveals that he bought the mustaches to be placed on his table when he once had a meeting with an important visitor. The mustaches were part of a joke he made to remind the visitor that "there is a lot of *show* here" (interview 2009). Professor Møller is is man of many mustaches and indeed his work cannot be said to consist of "*one single thing*" (Gieryn 1983).

Having made his career in the years where plant GMO was highly controversial, Møller is accustomed to spending a lot of time relating the research agenda of his center to broader societal and political questions. The center has been very successful in attracting funding, public as well as private. However, this success only came through hard work outside the confines of the laboratory, he tells me. Møller is a vivid performer and a talented presenter. He never fails to make the usefulness and relevance of Pro-Active Plants visible in relation to climate problems, the quest for sustainable energy or alleviating world hunger. It is evident that he is extremely well connected and conscious of the importance of networking with a broad spectrum of actors and sectors. In fact, he sees the ability to mingle with people from outside the university as fundamental to the success of Pro-Active Plants:

> "We are incredibly dependent on the efforts of other people. If you don´t realize that, you will not get very far. We don't have all the technologies up and running here, we need to work with people who think our projects are exciting and who are convinced they are also going to profit from working with us on the things that we are interested in". (Interview 2009)

According to Møller, Pro-Active plants is dependent on actors outside plant biology. Getting other people interested is pivotal if you want to go far in research.

A large part of Møller's managerial work is to mingle with people outside the university department and establish relations in order to keep a constant influx of money, staff, ideas, technologies and political good will. As a scientist and as a research manager he spends a considerable amount of his time travelling to conferences, going to debate meetings, giving interviews in the media, writing up patents in collaborations with biotech companies, working as an expert witness in patenting court cases, making evaluations of other university departments, being on the phone with representatives from research councils, talking to venture capitalists and meeting with politicians who take an interest in climate and food. All of these activities are woven into his workday as he makes the seamless transition from these activities to those of teaching, supervising, writing and checking up on the lab facilities.

However, despite the mustache joke, Møller does *not* see his core activity as one of mingling and maintaining relations. On the contrary, he tells me, there is much more to plant biology than the show. We have already heard Møller make his subtle protest in chapter two where he resisted my interview questions about "politics" and wanted to talk about his research. Here Møller made a demarcation between *the icing and the cake* – the social stuff around his research and the technical content of the research itself - and implied that I would not adequately describe his job by pointing his mustache-juggling social activities. Surely, there would be no science without these activities but according to Møller, we would be missing the point altogether if we focused first and only on this part of his job.

Møller sees himself as a scientist, not primarily a manager, an opinion maker[79] or an administrator. He is a plant biologist who happens to have to take on some managerial responsibility and communications work in order to do his job well. He *has* to be a man of many mustaches to make things work. I initially questioned this account by pointing out that from a quick look at his weekly schedule Møller seemed to spend more than 60% of his time on icing the cake rather than making the cake[80]. For example, this plant biologist has to allocate designated time slots twice a year in order to be sure he even makes it into the laboratory and conducts some actual experiments. If

[79] Møller labels himself an opinion maker when referring to the part of his job that requires him to play an active part in debates about plant GMO. Møller often conveys dissatisfaction with the Danish and European debate feels that the resistance against GMO is for a large part based on wrong information, oversimplification or dogma (Møller 2008). To Møller, GMO is an important step to counter world hunger, poverty due to the rise of food prices, climate problems, and even the gender inequality in developing countries (Møller 2000, 2010b).

[80] Diane Forsythe's study of AI knowledge engineers (2001) makes a similar observation in the way AI practitioners define their job identity and disciplinary boundaries. AI knowledge engineers rely on knowledge, theory and technology borrowed or copied from other disciplines. This "building on older foundations" and the need to stray from the discipline is perceived by the practitioners as a potential threat to their identity and to the science. Consequently the argument "that´s not AI" is sounded frequently as an "alarm" signifying the danger in crossing boundaries. Forsythe also observes a tendency to separate between "real work" and extracurricular networking activities despite the fact that AI practitioners seem to devote more of their time and efforts to the latter than the former.

not, he will get "rusty" and will lose touch with the lab facilities and fail to make sure whether they live up to standards. Møller, however, insists that the activities we have discussed so far in the interviews constitute only 10% of his job. Indulging his wishes, the next interview focused on finding out what constitutes Møller´s *real job*:

> BGH: So, the last time we talked you said that what takes up most of your time is "reading and speaking to people". Could you expand on that a bit more? What sort of thing are you … doing? What is the central…
> BLM: I think that plants are incredibly interesting. A lot more interesting than most people! Plants are smart people. Haha
> BGH: Plants?
> BLM: Yes how plants work and think (...). They cannot run away when it is too cold, hot or dry or when there is not food available (light). They constantly need to change their metabolism to adapt to environmental changes. When an insect starts chewing on one of its leaves, it has to start to produce a defense compound that the particular insect does not like. So, plant/insect communication, the growth and development of plants, that's a great and understudied research area (Interview 2009).

Møller sees himself first and foremost as a plant biologist. Unless specifically asked about administrative details, policy agendas or strategic moves he will talk nonstop about plants; nothing but plants. He will use any excuse to shift the subject of an interview to his core research interests and to get out a Power Point presentation, a popular science account or a journal article to take home. Again and again he will make it clear that missing out on his research is missing the point of what he is all about. Being a plant biologist is what matters, all the other activities are "the stuff around it" the things that make plant biology work.

How should we read such a plea? If we adopt the concept of boundary work as used by Tuunainen and Lam, we may interpret the above interview extracts as text book examples of how scientists make demarcations between science and non-science in order to reconcile role-tension and reduce complexity within a practice full of paradoxes and conflicting interests. Møller´s protest to my one-sided focus on network formations and building relations with outside actors could easily be articulated as a need for protecting boundaries between what counts as inside to science and what counts as outside. If analyzed as an example of boundary work or boundary maintenance we can read Møller's statements as an attempt to separate his "real job" – pure science – from the management, mingling, entrepreneurship and politics that comes with it. Such an analysis would lead us to claim that Møller is engaged in purifying plant biology by stripping it of all the activities that keep it alive. From this perspective, the next step in an analysis of his strategic maneuvers through the knowledge economy would be to map the tensions between Møller's many "mustaches" and analyze the ways in which he constructs a hybrid but manageable identity as a scientist by excluding the lobbyist, the entrepreneur and the opinion maker whenever this suits his strategic purpose.

However, if we put Møller´s insistence on starting out in plant biology alongside Strathern's problematization of the vocabulary of boundary and hybrid we get a slightly different picture. Perhaps we have not yet been attentive to the subtleties of his practice and the complexity of the argument he is making in his protest to my questions about "strategies". By complaining that we forgot to include plant biology in the description of his work, we may argue that Møller is not primarily engaged in erecting a boundary between icing and cake, between real research and fake mustaches. Rather, he claims there is much *more* to his job than the *show*. His work includes more than leaping between science, industry, government and public. If taken seriously, Møller´s protest to a one-sided focus on the "show" part of his work may actually be read as a plea for *increasing complexity*, not an attempt to reduce it by stripping pure science of its strategic and political mustaches. In other words, a better-articulated analysis would be to understand his protest as: *Fair enough, mustaches and mingling are important activities but where is biology in this account?*

Rather than a plea for reducing complexity, separating the icing from the cake and reconciling role-tension, I will argue that Møller is suggesting an analytical route that goes in the opposite direction. If our analytical vocabulary merely reduces science to a mustache-juggling hybrid trying to protect a boundary between two pre-existing practices, we will be forced to ignore the question he posed in chapter two: *When are we going to talk about research?* Møller insists that we start out in plant biology and move outwards from there. For this purpose, we will now indulge the field participant and devote some attention to his "real job", that is plant biology.

Biological analogies

Although dear to the heart of STS, I will argue that hybridization and boundary blurring are not necessarily the best ways to conceptualize Møller´s managerial practice. I will make this argument by placing Møller´s own academic work on plant-insect co-evolution alongside the stories he tells about science-industry relations. The symbiotic relationship between a particular insect and its feeding plant may do some of the analytical work for thinking about science-industry relations that the concepts of boundary and hybrid are not cut out for.

Making analogies between biological systems and human societies is surely not a very original idea. Some may even argue that it is an extremely bad one. The attempt to compare the development of organic life with the evolution of human societies has produced more than a few wince-inducing moments in the history of the social and economic sciences[81]. Previous use of biological analogies to legitimize racism, eugenics and laissez-faire economic regulation rightly leaves a bad taste in the mouth of the contemporary thinker. Some would even argue that biological metaphors may best be left behind with the rest of nineteenth century evolutionism (McGee and Warms 1996).

However, Møller happens to be a biologist. Moreover, he is one of those biologists who like to think and speak in evolutionary and biological metaphors and tends to compare them to his own academic organization. He will not allow us to cut biology out of the equation when describing his managerial practices. Latour and Stengers argue that a good analysis takes the risk of allowing the concepts of the actors of the field to "do the talking" rather than sticking to a pre-established analytical vocabulary or set of ideas (Latour 2005, Latour 2004, Stengers 2000, 2010)[82]. Consequently, it may be worth the risk to experiment with making analogies between the biological and the social. The analogy is an experiment, not an attempt to generalize the workings of biological systems to the work of scientists in the knowledge economy. Such a generalization would be a wolf-game, weakening one term in the comparison at the expense of the other. I will not claim that scientific practice is an *example* of a more overarching biological "logic"; rather I will make it *comparable* to a biological system by juxtaposing the two parts of Møller´s account. In this lateral exchange I seek to approach Møller´s practice as a scientist and a research manager without posing what Stengers would call irrelevant or impolite questions to him. Stengers argues that when approaching a practice in order to study it we should be:

> … approaching it as it diverges, that is feeling its borders, experimenting with the questions which practitioners may accept as relevant, even if they are not their own questions, rather than postulating insulting questions that would lead them to mobilize and transform the border into a defense against their outside (Stengers 2005, p.184).

Taking up this advice of Stengers and Latour to experimenting with the concepts and the questions that Møller finds relevant would definitely require us to start out in plant biology and find an analytical vocabulary that increases complexity rather

[81] For a classic example see Spencer 1996.

[82] Although the Latourian ethos of "following the actors" (1987) and "becoming affected" (2004) by one´s field of study resonates well with the anthropological reflections of methodology, a different source for this line of thinking enters his work by way of philosophy. As made explicit in Latour 1997 and 2004, the normative criterion of "risky" analysis and "becoming affected" by one´s fieldwork draws heavily on the work of Stengers' notion of risk, surprise, thinking *par le milieu* and abstinence of ruling definitions (2000, 2005, 2007, 2010, 2011). The philosophical ethos of becoming affected by ones object of study is nicely summarized in Deleuze: "Something in the world forces us to think, this something is an object not of recognition but of a fundamental encounter". (Deleuze 2004, p. 176). Another philosophical inspiration for Latour´s normative turn is the work of Vincianne Despret (2004, 2005).

than reduces it. Despite good historical reasons to leave biological analogies alone, I will risk experimenting with them in order to make sense of the way Møller deals with the mixtures of knowledge economy. Let us therefore proceed to the heart of the matter: *plant-insect relationships.*

Co-evolution


8a


8b


8c

Figure 8a: Lotus corniculatus

Figure 8b: mature six-spot burnet moth

Figure 8c: Larva of six-spot burnet moth

Møller's main area of research is bio-active natural products that enable plants to defend themselves against insects and microbial pathogens. A plant produces a specific subset of these poisons in response to environmental challenges in order to fend off herbivores (plant-eaters) and disease-producing microorganisms. Plants are thus proactive living organisms, acting on their environment and entering complex chemical exchanges with other species in order to survive. Møller and his colleagues have done extensive research on one particular type of poisonous plant: *Lotus corniculatus.* The reason why this plant is interesting to Pro-Active Plant is that it is *cyanogenic.* The leaves of the plant contain *cyanogenic glucosides* – compounds that are able to release toxic cyanide (Møller, 2010, Zagrobenly et-al. 2008).

The presence of cyanogenic glucosides in the leaves of the plant makes it poisonous to anyone who tries to eat it. When the plant tissue is destroyed by a chewing insect, cyanogenic glucosides are brought into contact with degrading enzymes stored in other parts of the plant cell. These enzymes may also be present in the mouth and digestive system of the chewing insect. The enzymes break off the sugar residue of the cyanogenic glucoside, thereby releasing the toxic chemical hydrogen cyanide. Hydrogen cyanide is toxic because it inhibits respiration and therefore may be lethal when ingested in high concentrations. Cyanogenic glucosides are thus designed to function as *deterrents* by their bitter taste and toxicity. However if, like Møller, you want to know how plant poisons actually function, biochemistry will not get you very far.

Reviewing the literature on cyanogenic glucosides, he finds that matters are much more complex and that the effects of cyanogenic glucosides vary with circumstances and are not as well established as the textbooks may have it (Møller 2010). According to the argument posed by Møller and his colleagues, nobody really knows what a plant is capable of just from looking at it as an isolated species. The role of the poisonous compounds it contains must be studied in a way that takes the presence of other species and their entire environment into account, he argues. A plant that is harmless to one type of insect could be lethal when placed in a different habitat. (Zagrobenly et al. 2008). To illustrate this, Møller introduces the insect *Zygaena filipendulae* or the *six-spot burnet moth.*

The six-spot burnet moth is a day-flying moth which can be found in most of Europe. Due to 430 million years of genetic attunement, it has become a highly specialized herbivore possessing some rather impressive survival mechanisms. For example, it prefers to feed on *Lotus corniculatus* and seems unaffected by the toxic cyanogenic glucosides present in the plant (Zagrobelny et al. 2008). In fact, these insects have developed to survive in an ecological niche, a "cyanide society" – a plant-insect system built up around the presence of cyanogenic glucosides in their habitat. Food, metabolism, growth patterns, mating rituals, partner selection and rejection, reproduction, competitive advantage and protection against predators all have cyanogenic glucosides as a vital part of their functioning. This has been made possible through millions of years of genetic adaptation and mutations by which the plant has tried to defend itself from the insect and the insect has adapted to the defense system of the plant. *Co-evolution* is the name of this process through which organic life develops in mutual relationships with fine-tuned systems of mutual response to each other´s presence. It is not only plants that are proactive organisms; insects have their own way of acting and responding to their environment as well. Møller claims that the co-evolution of plants and insects is really a slow and prolonged process of "chemical warfare" (Møller 2010, p.337, Zagrobenly et al. 2008, p. 1457)

Symbionts

A symbiosis is characterized by a mutual dependency between two species. At first glance the above plant/insect relation does not seem symbiotic at all. Rather than a mutual relationship, the apparent producer/consumer relation between *Lotus corniculatus* and the six-spot burnet moth seems quite unidirectional. The plant produces food; the insect consumes it, the plant suffers attacks despite its best attempts to defend itself and the clever insect parasitizes the plant and reaps the benefits. In many ways this would be the direct opposite of a symbiosis, a "parasite logic" or *taking without giving*[83]. When suggesting the logic of the parasite to Møller, however, he will dismiss this model of analysis. Møller's research suggests a more subtle dynamic than that of the unidirectional logic of the parasite. In fact, he claims, this specific plants-insect relation is better conceived of as a *symbiosis:*

> The knowledge we have now allows us to conclude that the larvae and the grown insect take advantage of the plant. But when we start digging deeper, I'll be very surprised if we don´t find that the plant in some way also takes advantage of the presence of the insect. For example, the enzymes in the insect's saliva disrupt the plant cell walls in such a way that a signal emerges which introduces a relevant defense mechanism in the plant, for example against fungi, bacteria or virus which the larvae are carrying. You must mean symbiont rather than parasite (Email correspondence 2010).

The co-evolutionary framework demands that Møller study plant poisons as parts of a *symbiosis* between plant and insect. It is not enough to assume that poisons are always poisonous or that they work in the same way throughout all inter-species relationships. Neither is it certain that the order of production (plants) and consumption (insects) follows a linear, unidirectional path. The presence of cyanogenic glucosides in an ecosystem often has multiple complex effects in relation to other species, effects that are not necessarily related to deterring enemies and cannot be inferred from the plant's biochemical composition alone (Møller 2010). Cyanogenic glucosides may function as deterrents to most insects but to the six-spot burnet moth they have multiple functions, like nourishment and aphrodisiac. According to Møller, nobody knows what a given species is capable of without first taking its ecology or habitat into account. This is why Møller´s research takes place as live experiments with plants and insects rather than isolated studies of plant biochemistry.

Starting with plant biology and moving outwards now seems to suggest an analytical vocabulary quite different from that of mapping boundary work and identifying hybrid identities. Thinking about Møller's work in terms of co-evolution

[83] Parasite logic is a term developed by the Psychologist Steve Brown (2004) in his reading of Michel Serres "The parasite" (Serres 1992). Parasite logic describes a unidirectional flow from production to consumption where the last parasite in the chain enjoys the privilege of *taking* resources *without* having to give anything in return. Serres' understanding of the parasite is somewhat broader than that of our plant biologist as it includes other forms of unidirectional relationships like for example the relationship between a breast-feeding mother and her child (Serres 1992). Thus "parasite logic" not only signifies a host/guest relationship where the guest lives inside the body of the host but is a broader term to characterize a unidirectional chain of consumption. See also Brown & Stenner 2009 for commentary.

means engaging in empirical experimentation with mingled bodies and symbiotic relations that pull the rug from under the distinctions by which we would otherwise make sense of scientific practice. As argued by Stengers, symbiosis is not about common agendas, convergence of interests (Stengers 2010, 2007). According to Møller´s research we should rather analyze it as an ongoing and subtle chemical warfare.

In the following I will conduct such an analytical experiment by presenting two "strategies" deployed by the larvae of the six-spot burnet moth when feeding on the toxic lotus corniculatus plant. These "insect strategies" will be used as an analytical vocabulary for articulating the relationship between Pro-Active Plant and an in-house biotech company. This first is *circumvention*, a mechanism that allows the insect to turn a toxic substance around to make it work to its own advantage. This will be compared with Møller's reflections of how Pro-Active Plants takes advantage of an in-house biotech company. The second "insect strategy" is *sequestration*, a particular way in which the larva incorporates and stores toxins in its body, to be used against predators. I will draw an analogy to the way Pro-Active Plants has incorporated biotech industry to support research in plant biology and give it more competitive advantage.

Circumvention

The mechanism which allows the six-spot burnet moth to feed on *Lotus corniculatus* without being poisoned is an intelligent form of *chemical warfare*. The plant poison is supposed to harm the larvae once they start chewing on the toxic plant but the larvae of the six-spot burnet moth have found a way to *circumvent* the defense system of the plant (Zagrobelny et al. 2008, Møller 2010). Circumvention here means that the poison in the plant is turned around and made to work to the insect's own advantage. By feeding on the Lotus Corniculatus the insect itself becomes *poisonous* rather than being poisoned. In this way a deterrent designed to work *against* insects is *incorporated* in the insect's own body and given a new function: the ability to deter predators (Ibid).

Danish research policy in many ways views science and industry in terms of a unidirectional logic of production and consumption. A logic of the parasite, we may add. Science is articulated as a resource for value creation which needs to be tapped more efficiently by society. As the catchphrase *from insights to invoice* suggests, the relation between science and industry is thought to be a unidirectional flow: science produces knowledge and industry consumes it.

In response to this agenda, Møller and two of his colleagues decided to go into business and start a spin-off company[84]. To begin with, the company was based on investments from private investors, Danish venture capital funds and some major companies. The spin-off has now been bought up by a larger international biotech company and all its activities are now integrated as a subsidiary of this larger company, with headquarters in Central Europe. The subsidiary revolves around a technology platform for "high throughput glycosylation" – isolation of a multitude of enzymes that are able to link sugar residues to a diverse array of chemical compounds to identify possible uses in the medical industry. However, when talking to Møller, the notion of use or usefulness is not a unidirectional flow *from* science *to* industry:

> BLM: There were a number of reasons to establish the company. We thought it would be useful and able to make a business because these enzymes are able to link sugars on all kinds of drugs and thereby improve the solubility or stability of the drugs or permit the use of a lower dose. But it simultaneously creates a platform that enables business development *and* one that serves to advance our basic research. We are able to make use of it for ourselves. I mean we have people *here* that go down *there* to use the screening platform and often also take advantage of equipment that they have money to buy but which we can´t afford. On the other hand, we contribute with our research experiences in other areas that may be relevant to the company, in which they lack expertise. So you shouldn't create a company just for the business. It is … it's best if you can combine it in as many ways as possible (Interview 2009).

Starting up a spin-off that develops technologies for the medical industry is a way of making plant biology available as a resource for value creation, a way of taking the leap from insights to invoice, so to speak. But listening to the above description, it is clear that the relations and exchanges between Pro-Active Plants and the biotech company do not follow a linear logic where the research center *produces* knowledge that is subsequently utilized by a knowledge *consumer* – the biotech company. Rather, a different form of exchange is taking place. We already know that the success of Pro-Active Plants depends on the interest and engagement of "other people". Having technologies up and running requires getting other people interested – in this case an international biotech firm with headquarters in Central Europe. In the above description of the relation it seems the linear flow *from* science *to* industry is *circumvented,* to work to the advantage of the scientific laboratory, not just the company. The two directions are not mutually exclusive. To Pro-Active Plants, there are many advantages to having an in-house technology platform managed and maintained by a private company. First of all, the company makes technologies available that would

[84] The motives for creating a company and the function it has had for Møller and his colleagues will also be further explored in chapter six.

be extremely expensive for the university to acquire and maintain and which also require the recruitment of specialized technical staff (since machines rarely work alone). Circumventing the science-industry food chain means that Møller and his colleagues use the biotech company to their own advantage by delegating routine tasks to professional lab technicians paid by the biotech company. The ground floor biotech company thus seems to constitute a producer as well as a consumer in the food chain of knowledge production and Møller naturally refers to it as a resource to be utilized rather than a customer to be serviced:

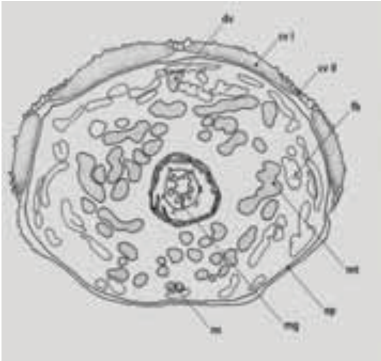
> BLM: …yes, tasks which were not suited to be done by us because they are cumbersome and time-consuming repetitive experiments if performed without access to a high throughput technology platform. But a company who has technicians hired to do stuff like that, that's just beautiful. We get access to a tool box and a platform that we would otherwise not have been able to make on our own. Also, the collaboration with the biotech company has enabled us to obtain funding that we would otherwise not have qualified to apply for (Interview 2009).

Consumption seems to be a mutual and symbiotic affair rather than a parasite logic of one party consuming the raw material constituted by another. Pro-Active Plants feeds on the biotech industry just as much as the reverse. Without the in-house company, the trivial or routine tasks necessary to make it in the highly competitive space of plant biology could potentially form a toxic ecology for scientific practice by taking up a lot of the time and demand routine tasks to be done by the PhD students working in the center.

> BLM: yeah, it is important to avoid people spending their time on the wrong things – e.g. to set up screening and platforms that may not end up being of any use for the research. For example that industry collaboration thing there ... we had an employee who set up a huge technology platform, right? And that took him 4 years, right, before any biological results started coming out – of course! And as a consequence he is being assessed as less qualified than his colleagues when he applies for jobs because he doesn't have the biology results, even though he is actually a better researcher (...) But then I tell him: "But really, you have to take a look at the kind of *environment* you have around you, right? I mean *why* have you accepted to spend so much time doing this? *Why* you and your supervisor did not discuss how to set this up right? (Interview 2009)

The fact that routine tasks like setting up a technology platform can do tissue damage to scientific practice does not imply that science is a pure practice which should be kept free of toxic industrial influences. Møller is not encouraging the researcher in question to back out of industry collaboration. In fact, he is advocating for the opposite - to set it up in a way that allows an industry partner to spend 4 years on setting up a platform so the researcher can concentrate on getting some publishable results. There is no plant biology without biotech, there is no experimental science without technology platforms. Pro-Active Plants needs biotech as its feeding plant no matter how toxic it may potentially be. Rather than trying to avoid the poisonous, distracting interfaces with industry partners and technology development, Møller argues that scientists have to "take a look at the kind of *environment"* their practice is embedded in. Setting up a technology platform "in the right way" as Møller suggests, means getting the biotech industry to work to your own advantage. Scientists should not separate themselves from industry; they should *incorporate* its tools, technicians and interests into their work and the material infrastructure of the university. By having an in-house biotech company, the academic scientists will not have to go through the laborious process of high throughput screening, enzyme production or other routine tasks. These tasks are not external to science, they are as necessary as breathing. At the same time this incorporation gives Møller and his colleagues a competitive advantage in relation to other scientists: an expensive and smoothly running toolbox as well as access to new types of public funding which require an industry partnership or collaboration. This constitutes a *circumvention* of the logic of production and consumption so that it now works both ways. However, the relation is symbiotic, in that this mutual flow of production and consumption takes place without disturbing or homogenizing the goals and agendas of plant biology to fit those of the biotech company. The biotech company does not need to diverge from its goals in order to be a resource for Pro-Active Plants, neither does the collaboration with the larger biotech corporation by way of the in-house company require Møller and his colleagues to adapt their research program to the interests of the biotech corporation. As in any symbiotic relationship, each species retains its own goals and agendas and does rest on common ground. Taking the notion of symbiosis further, let us return to the larvae of the six-spot burnet moth and see what analytical work can be done by looking at another one of its strategies for survival.

Sequestration



9a

9b

Figure 9a: Cross-section of a Zygaena filipendulae larva showing sequestered pockets where poison is stored.

Figure 9b: Droplets of cyanogenic glucosides are released from under the black spots in the larvae's skin upon attack.

When feeding on *Lotus corniculatus*, cyanogenic glucosides will inevitably enter the body of the insect. However, only the six-spot burnet moth can deal with this incorporation in a way that avoids intoxication. Cyanogenic glucosides are not allowed to diffuse into the tissue of the larvae and do damage to its body. The larva's strategy is to *sequester* the toxic cyanogenic glucosides in small pockets on the side of its body right behind the black spots. When an animal attacks a larva and squeezes its body, the larva will contract its body and thereby provoke a release of droplets of poison from behind the black spots on its sides, exposing the toxin directly to the digestive system of the predator (fig. 2). The direct result is pain and vomiting, leading the enemy to never feed on the larvae again. As a result, the six-spot burnet moth has few or no enemies in its natural habitat (Møller 2010, Interview 2009).

Symbiosis is not a stable equilibrium; it works by way of an ongoing *chemical warfare* (Møller 2010). As we will see, the strategies of the larvae studied in Møller´s laboratory resonate with the strategies of Pro-Active Plants. The center has been able to incorporate and *sequester* biotech inside its organization to give it a competitive advantage. The specific way that this incorporation has been made to work is due to the highly specialized relation between the research center and the particular biotech company. Not just any industry-incorporation will work to the advantage of Pro-Active Plants. Some kinds of science-industry relations are potentially toxic or dangerous in that they take up time and resources which could otherwise be dedicated to research. However, this particular constellation seems to work. The scientists get access to equipment, enzymes, technology platforms and the opportunity to outsource routine tasks while the company bases its product on knowledge developed by the scientists. According to Møller, the reason why it works is that biotech is incorporated in a way that allows plant researchers to do *biology* rather *technology*:

> BLM: In a research group, you need to establish a sufficient number of technology platforms in order to progress in your biological research in a competitive manner and do this properly (…) it's something like… you have to be a certain number of people in your research team. If every single group has to maintain these technology platforms, then they spend too much time on *doing technology* rather than *doing biology*. So (...) these kinds of digressions are costly. When a collaborating industry - like for example when starting up project X - if it provides access to a technology platform within your core competence area then you win at the end of the day (Interview 2009).

Incorporating biotech to your own advantage may at first glance seem similar to doing boundary work in that it separates the commercializable task of "doing technology" from the academic task of "doing biology". However, Møller seems to be arguing for something slightly different. The advantage of having an in-house biotech company is that it allows scientists to produce knowledge in a scientific research environment free of too many time- consuming digressions and distractions, such as developing technology platforms. Pro-Active Plants is doing biology *not* technology. It would be inaccurate to claim that Møller is not pointing to a *divergence* between the two, he is indeed making some form of demarcation. But is he drawing or maintaining a boundary between two independently existing and separable entities? I will argue that he is not.

Rather, it seems that the purpose of pointing to the divergences between biology and technology development and thereby between Pro-Active Plant and the biotech company is not to protect an academic core department from what is perceived as a toxic influence of corporate interest. Erecting a boundary or making a separation between the two would be like separating the six-spot burnet moth from the cyanogenic clucosides of *Lotus Corniculatus*. Such an analytical separation makes no sense since the two need each other to stay alive and well. Pro-Active Plants *needs* technology platforms to do plant biology and making them on your own takes time and ressources.

This logic of the symbiosis contrasts with the way the notion of boundary work has been deployed in studies of the commercialization of science. Where Tuunainen's study (2005) showed how a hybrid firm was marginalized from the academic department in order to maintain a traditional or pre-established boundary between science and industry, Møller's story shows something different. The example presented above illustrates that divergences and demarcations can have multiple functions and are not restricted to boundary maintenance. It seems that *nobody knows what divergences and demarcations will do until they have been studied within their particular ecology* and in relation to the practices that constitute them.

Møller's particular demarcation between "doing biology" and "doing technology" is not maintaining a boundary between two pre-existing institutional spheres. He is not protecting the purity of science when circumventing the logic of production and consumption. Rather, he is engaged in subtle "organizational warfare" in order to make sure that the symbiosis between plant biology and biotech stays manageable and productive. This symbiotic relationship is not characterized by a stable equilibrium or the lack of divergence between the species. In this particular case, divergence and demarcation are not acts of protection and purification; they are ways of *making symbiosis manageable and viable*.

In order to function as technology and *not* biology, biotech is *not* expelled from academic science. Rather, it is incorporated and *sequestered* under the skin of the university department. However, this practice of *sequestration* needs to be specialized to fit the singularity of this particular science/industry relationship in order to work without intoxication. Biotech can only be incorporated in a very particular way – alternative divisions of labor may become poisonous "digressions" that can potentially side-track plant biology from its research agenda. Sequestration is a strategy which makes this possible. In the same way that the larvae of the six-spot burnet moth sequester cyanogenic glucosides in little droplets under their skin, biotech components are sequestered in the periphery of the university building. They become part of the "body" of Pro-Active Plants but without doing "tissue damage" to the research center or the scientists working there. Sequestration is thus not the same as exclusion. Rather, it is a manageable form of incorporation. Symbionts are not hybrids; it is not an integration or fusion of two things. No clear and final separation can be made between the body of Pro-Active Plants and the body of the biotech company, but paradoxically the divergence between them is what makes their relationship mutually supporting. Drawing a clear boundary between them to allow either core academic values or corporate development to set the common agenda would not be possible without weakening both practices. The symbiotic nature of experimental science and technological innovation implies that they need each other in order to achieve each of their divergent goals. Take away the biotech company, and PhD students would suddenly find themselves *having to* do technology *in order to do* biology. There is no plant biology without technological infrastructure and staff that is paid to do routine tasks. There is no plant biology without the money to buy instruments and build technology platforms. In order to conduct their research, Pro-Active Plants need proper technology platforms up and running. Conversely, the biotech company needs the new biological results produced by pro-Active Plants in order to stay in the game. Pro-Active Plants may thus be a resource for the biotech industry, but only because it already lives and breathes through it. Like the plant and the insect, the two parties leave imprints inside each other. Møller´s strategy of sequestration is a way of preventing the laborious work of "doing technology" from taking over or do "tissue damage" to the body of plant biology.

Advantages of symbiotic relations

The case presented here suggests that the problem of science/industry relations is not one of interaction, synergy and the bridging of gaps between science and industry but rather one of managing productive and mutually beneficial divergences between them. Contrary to the argument that the knowledge economy is about convergence, I have shown that in this particular case it is a matter of skillfully managing *divergences* within a symbiont order or logic. It is worth noting here that in circumventing the logic of production and consumption, Møller's strategies form a lateral side-track to the "insights to invoice" agenda of Danish research policy. He stays in relation to this agenda but never assimilates into it in the sense imagined by Danish government. In a way, Pro-Active Plants fully complies with Danish research policy and in a way it turns it around to make it work to its own advantage. As I will show in chapter six, showing off droplets of industry collaboration to research councils and policy makers is an efficient survival strategy that gives Pro-Active Plants access to funding that would otherwise not be available. As very much a basic science endavour, Pro-Active Plants need to show that they are useful and relevant in order to gain fundability in the present policy habitat. The presence of an in-house industry collaboration can potentially deter critics who may accuse Pro-Active Plants of pursuing basic science without being of use to "society". Pro-Active Plants is already full of technologies and biotech products, incorporated under the skin of the university building and the infrastructure used in the production of knowledge. What is interesting here is the symbiotic relation guiding the relationship. In the vocabulary of Stengers, divergent particular and "egoist" interests are not made to "bow down" to more powerful ones (Stengers 2010, p.35). Neither the biotech company, nor the research center have changed their program and path to fit the agenda of the other. Rather, each party seems strengthened by this particular balance of divergence and dependency on the other. Circumvention (the strategy of reversing the logic of production and consumption) and sequestration (the strategy of skillful incorporation without intoxication) are both strategies of the symbiont. They allow Møller and his colleagues to diverge and pursue their own goals (*do biology)* while at the same time being part of an environment and making the necessary incorporations of other actors in this environment.

The material presented here thus proposes a different analytical starting point than that of boundary or hybrid. Using the biological vocabulary of Møller as a route to studying plant biology as "no single thing", Pro-Active Plants is articulated as part of a symbiotic relationship between that could be characterized as a subtle "organizational warfare" carefully circumventing and sequestering corporate interests that are vital to achieve the goals of plant biology. Without the mutual exchanges between the two, Pro-Active Plants would have to spend time and resources on developing the necessary tools and applications of their research. In this particular case, then, managing plant biology does *not* take place as an integration of different institutional spheres, the balancing of different identities, or the construction of hybrid organizations across a science/industry gap. Neither is it a straightforward matter of boundary work and defending the purity of science. Møller's scientific practice goes beyond the boundary. Likewise, the symbiotic relationship between Pro-Active Plants and the ground floor biotech company cannot be adequately described by referring to a process of hybridization between the institutions of science and industry. The concept of hybridization is too broad to articulate the particularity of what makes this relationship productive. The singularity of mutuality and divergence is what makes the difference between toxic tissue damage and viability.

Reflecting back on Danish research policy agendas, we may be conscious not to simplify the idea of symbiosis to make it sound more harmonious and stable than it is. The fact that science and industry in this case seem to engage in a symbiotic relationship should not lead us to suggest that a symbiont logic equals natural integration, science-industry synergy, adherence to common goals and knowledge-based value creation. Assuming the co-evolution of science and industry to be an unproblematic natural process towards *more* integration and *more* hybridization poses a danger, given that symbiotic relationships require space for blatant divergence, intelligent warfare and pragmatic demarcations.

Construing knowledge production solely as a resource that should be utilized by "outside" actors in order to ensure that science creates value is inherently a "parasite logic" of unidirectional flows that goes against the symbiotic nature of the biology/biotech relationship depicted in this particular case. Co-evolution is a delicate process and requires skillful and experimental incorporations that do not force diverging practices to assimilate to a common goal. Without this process the mingled bodies of science and industry become one dangerous and toxic habitat rather than a productive symbiosis. The strategies of circumvention and sequestration are ways of making symbiotic relationships manageable and ensuring the survival of both parties.

The particular symbiosis between Pro-Active Plants and the biotech company is thus not highlighted here to figure as a "best practice" that can be used as an argument to support the agenda for more integration and more hybridization between science and industry. Møller´s strategies work because they manage to retain divergence while engaging in mutual flows of production and consumption. They work only because circumvention and sequestration have been possible and thereby have been given the space for a productive divergence to breathe. Industry collaboration is nurturing for plant biology in this particular case because it has been carefully sequestered in the buildings of Pro-Active Plants but it may be lethal if incorporations are organized slightly differently or if exported to seemingly similar disciplinary habitats.

Not all mingled bodies form a symbiosis. Some forms of mingling are simply toxic, parasitic and dangerous to a research environment like that of Pro-Active Plants. Making symbiosis manageable means that scientists have to specialize in fine-tuned incorporations involving an ongoing sensitivity to how and when to diverge. If this delicate process of skillfully sequestering other practices in the body of science is not nurtured, the productivity of the symbiosis fails and one species takes over the other like a parasite. In the case of Pro-Active Plants we may suspect that the focus on further integrating and pushing a unidirectional "insight to invoice"- agenda on such delicate processes of attunement may endanger both species.

6
Robust scientists

The previous chapter explored the symbiotic relation between science and industry and argued for a framework of analysis that goes beyond the analytical preoccupation with boundaries. Continuing the lateralizations between the conceptual vocabulary of STS and that of plant biology this chapter will engage further with the *"ecological approach"* to thinking about science in the knowledge economy, thus making sense of Møller and his colleagues as a scientific "species" in relation to a habitat. As Møller´s own research revolves around the proactive and mutually dependent attunements between species in a habitat, this chapter experiments with articulating the practice of Møller and his colleagues as a proactive response to new demands from Danish research policy. What did an increased demand for a more responsive, flexible, competitive and collaborative knowledge production do for/with/through Møller and his colleagues? Møller has already shown us that nobody knows what a species is capable of before it has been examined in relationship to a broader habitat. *Lotus corniculatus* is a toxic plant in one relationship and a vital source of nourishment in another

I will thus look at the strategic maneuvers of Møller and his colleagues as part of an ecology where scientists live and develop in proactive relationships with specific political and economical habitats. I will allow policy agendas and the ideas that they draw on to run parallel with ecological and biological vocabularies to show some unexpected ways in which scientists have adapted to the interaction agenda. The "insights to invoice" initiatives designed to promote more interaction between science, industry and government has been met with a rather interesting response from Møller and his colleagues as they form part of an ecology inhabited by both policies and scientific species adapting to them in creative ways. Letting plants and insects loose in an analysis of the strategies deployed by Møller and his colleagues is an experiment which perhaps allows us to look for some of the unanticipated performances of policy. As will be shown, the ideas of policymakers and their conceptual heritages do not necessarily generate their own world freely and according to plan. Rather they enter into an ecology of practices in which they will be met with a variety of responses from the inhabiting species present in local habitats. I will thus not be assuming a linear process of policy implementation and I will not claim to have given a representation of the success or failure of specific policy initiatives. The lateralizations of policy vocabularies, STS concepts, strategic maneuvers, scientists, insects and plants is an *experiment* rather than an authoritative account of how Danish scientists generally adapted to changes in research policy. The experiment allows me to use scientists' vocabularies and concepts to think about policy, rather than the other way around. The conceptual vocabulary of plant biology will help me articulate the strategies of Møller and his colleagues and think about their costs. I will show that Møller is part of a specific species of scientists who have worked proactively to survive in a rather unfriendly habitat. His rather interesting responses to environmental changes point to unanticipated effects of demanding more social responsiveness, interaction and industry collaboration from academic science.

First, I will draw up a "conceptual environment" for the strategies deployed by Møller. I will do this by presenting science studies scholar Helga Nowotny's concept of *socially robust knowledge* (Nowotny 2003, Nowony et al. 2001) which resonates well with the "interaction" agenda of Danish research policy. Robust knowledge is one of many ways in which the need for scientists to "step out of the study chamber" or "create networks" have been conceptualized. The concepts shows a specific type of expectation surrounding the idea of making academic scientists relate to a broader social, cultural and political context. Second, I will outline Møller's perception of his political and economic habitat with specific attention to the way this habitat changes over the course of his career. Third, I will engage in a lateral analysis of three types of strategies deployed by Møller to adapt and survive within this habitat by putting his research on insect alongside his managerial practices and reflections. Finally, I will return to the concept of robust knowledge and engage in an ecological reflection on the adaptation of Møller to his policy-habitat. The chapter concludes that in this particular case Denmark did not get socially *robust knowledge* but rather politically and economically *robust scientists* and research programs capable of standing their ground despite shifting political and industrial interests. The ecological approach allows me to finally discuss some of the costs that this strategic and tactical response may have for science as a practice.

Robust knowledge

As it was outlined in the beginning of the thesis, the changes that took place in Danish research policy were informed by an idea of university research as something which was traditionally separate from society (the mode 1 university). *The Knowledge Economy*, by contrast, creates a condition for research which demands more interaction between science and industry as well as between academic interests and politically directed challenges and agendas. As shown in chapter 2, the overall idea behind policy changes to promote increased "interaction" was primarily one of getting researchers out of the "ivory tower"; that is, guide them towards a higher level of performance and make their knowledge production relevant to the future problems of society. As explained in chapter 1, Danish Research policy has been guided by the idea that knowledge production needs to be responsive to the political, economic, technical and social challenges of Danish society, not to mention the threat of globalization. Three changes were made to counter this problem: 1) an increased focus on competition between researchers (academic and/or corporate) to increase the quality of research 2) an increased focus on strategic and politically directed steering of research programs to ensure their relevance for society 3) increased focus on science/industry collaborations as a way to make knowledge production increase its potential for value-creation. This focus on steering research agendas towards more responsiveness to the needs of society and knowledge consumers is echoed in Helga Nowotny´s concept of *robust knowledge* (Nowotny 2003, Nowony et al. 2001). Following up on the *The New Production of Knowledge,* Helga Nowotny, Peter Scott and Michael Gibbons pointed to an intensified *"contextualization"* of science and fields of research (Nowotny et al. 2001 p. 166). Knowledge production increasingly needs to take into account a broader context than that of the laboratory or the university as it is increasingly being framed as a resource for value creation and expected to relate to political agendas and societal problems (ethical, environmental, economic, technical or social) (ibid.). Of course the degree of contextualization varies across different fields of research as some fields are more easily connected to "external" demands and requirements than others. However, the authors argue that intensified contextualization has beneficial effects on science in the form of an increase in the *social robustness* of knowledge:

> "Under contemporary conditions, the more strongly contextualized a scientific field or research domain is, the more socially robust is the knowledge it is likely to produce" (Nowotny et al. 2001, p.167)

Socially robust knowledge is an effect of the extent to which science is able to engage in relational work with a broader context of stakeholders and political agendas. Robustness, according to the authors is an effect generated over time as scientific knowledge is "infiltrated" and improved by "social knowledge". Robust, knowledge, the authors argue, is thus empirically based, always open ended and relational. Three aspects characterize socially robust knowledge: 1) Validity is tested not inside the laboratory but outside it in a world full of social, economic, cultural and political factors. 2) Robustness is achieved by a process of *extension* where a larger group of experts than that of the academic scientist is involved in mixing different types of knowledge. 3) Society is not the addressee but an active partner in knowledge production and repeatedly puts it to the test and modifies the knowledge produced. (Nowotny 2003)

Robustness is then a criterion which requires something *more* (not something other) than truth or accuracy of the facts produced in scientific laboratories. As knowledge emerges in close relationship between research fields and the social, political, environmental, ethical environments they are embedded in, these environments come to shape the agenda and focus of knowledge production.

The problem, according to Nowotny et al. is that scientists fail to understand that these factors are *not* “external” to science but are rather an intrinsic part of the way knowledge production needs to operate today. Nowotny et al. argue that scientists see the work of social robustness as a burden or something they have to work around and claim that this is due to their traditional taste for purity, isolation and a fear of contamination of their results (Ibid. 167 -169). However, the authors argue, it is precisely contextualization - the more open and direct engagement of the social world - of the research agenda of university scientists that will enhance the status and validity of a field of knowledge (Ibid. 178). Clearly, Nowotny et al. see robust knowledge as the knowledge of the future, the way for scientific fields to gain legitimacy and accountability. Without social robustness, scientific fields are endangered species, the authors argue, threatened by the multiple sources of authority which compete with traditional criterions of truth and objectivity. Truth and objectivity is no longer enough to keep a field of research alive and well.

In some ways the changes in Danish research policy exhibit a similar faith in *contextualization*. Governmental rhetoric and concrete reforms have, as explained in chapter one, clearly signaled a perceived need to get scientists out of the ivory tower and demanded that they become more responsive and flexible to meet the changing needs of the Danish society. Looking at Møllers reflections about the development of academic careers in plant biology, I will identify the strategies he deployed in response to this overall interaction-agenda. The analytical vocabulary which helps to identify these strategies is once again drawn from plant biology. Using metaphors from plant biology I will think of scientific and policy practices as part of an ecology in which the former provides a habitat for the latter. Nowotny et al. may have rightly observed that science is increasingly mixed and relationally engaged with the interests and agendas of its stakeholders. However, I will argue that not all forms of mixtures add to the robustness of knowledge production and not all kinds of “externalizations” give status and legitimacy to research programs and scientific careers.

Before we turn to the three strategies deployed by Møller, I will give some background on how he perceived the changes in his political and economic habitat in the course of his career. Sadly, this is also the story of how Møller became one of the “last men standing” in Danish plant GMO.

Survival of the fittest

Among the many scientists and research managers I have talked to in the course of my field work, Møller seems to be one of the more content and happy ones. Unlike so many other scientists, he almost never complains about the changes in performance measurement and possibilities for funding brought about by the neoliberal turn in Danish research policy. I first assumed that the contentment and success of Møller as a scientist was a symptom of his location in a very generous disciplinary environment. Making a career in the area of plant GMO means feeding on the abundant resources of the biotech industry and drawing on public support for a "green" future. At first glance it may seem that Møller's success is simply an effect of having made his career in the land of milk and honey. His biotech profile perfectly aligns with commercial biotech interests and thereby to the *insights to invoice* agenda of Danish research policy. However, when asking Møller if he would have complained more about new performance measures and policy changes if he had been in a less *hyped* field of research, Møller sighs, crosses his arms and gives me an overbearing look from behind the rim of his glasses:

> BLM: My DEAR Birgitte. If you had asked me about these things 10 years ago - and remember I have been working with GMO all along - do you *think* you could find a research area under as much pressure as mine from politicians as well as media?
>
> BGH: ha ha ha, fair enough!
>
> BLM: Could you FIND a single one under more pressure? Just asking. (both laugh)
>
> BGH: OK I guess, but if someone had asked you these questions 10 years ago, would you have answered in the same way as you do now? Wouldn't you have complained more? Wouldn't you have said:*"it's unfair the way they performance-measure our work, the competition is too fierce" give us a break"*?
>
> BLM: Yes! And you will still hear me do this from time to time. But we have actually survived by continuing to do excellent basic research. That's where we make a high score, not by getting involved in politics.
>
> BGH: Ok, because it seems what I have heard you say so far is that things are basically fair and ok.
>
> BLM: Well, I did not have much influence on them really so…But it *is* hard. I haven´t s… Look, there has been a really powerful *mechanism of selection* these past years, right. OK, but we have really managed to hold on to the same three projects throughout that whole period where there

> was hardly any money for us to get. I really think that's a fair statement. Having said that, admittedly, it is pretty cool when people are now suddenly *screaming* for the things we can do and the expertise we have developed. (…) Suddenly there is a positive aura around it and not a negative one.
>
> (Interview 2009)

In no way did Møller grow up in the land of milk and honey, he claims. When looking back at his career and the historical foundations for Proactive Plants and its predecessor *Center for Molecular Plant Physiology* (PlaCe), he will make it very apparent how his whole research program experienced strong marginalization due to the controversies and criticisms surrounding GMO in Denmark. According to Møller, he is one of the few Danish plant biologists who have survived a decade of public and political resistance to GMO. Møller gives three reasons why GMO resistance posed a serious problem.

Firstly, plant GMO was extremely unpopular in the media and under-prioritized in Danish research councils for decades and still is, to some extent. Secondly, as Danish research policy underwent a change towards intensified performance measurement and increased focus on commercialization of science, it managed to wipe out a whole cohort of plant biologists. Møller tells me that around 2001, scientists were increasingly expected to demonstrate the relevance and usefulness of their research by way of collaboration with industry partners. The problem for plant biologists like Møller working with crop plants was that no industry existed in the national context and the few potential industry partners that were actually available often deemed Møller´s research too basic and risky for investment. Investing in GMO meant investing in an area where the legislation was highly complex and kept changing, something that may or may not become approved or impossible to market in the future. Making the leap from *insight to invoice* rests on the assumption that an industry already exists. This change in policy was devastating to scientists working in fields of research where no industrial consumer of knowledge could be readily identified. The third reason why Møller claims that his career was made within a hostile habitat is the problem of working with crop plants. What Møller is doing and involved in as a researcher and an advisor is to create new plants. Plants which are more robust when grown under adverse conditions, plants which change color when they grow on top of a landmine, plants with improved yield and nutritional value that can grow in spite of radical climate changes, plants which can convert sunlight into new types of valuable compounds. But plants are slow. It takes many years to elucidate how to make plants better and finally to generate enough seeds for actually starting a production; the seasonal pace of nature is not fitted to the impatience of investors. Also, working with crop science and food was not very popular in the early days of Møller's career. Back in the 1970s and early 1980s, the EU had a surplus production of food and had to sell out "mountains of butter" at prices lower than production costs. Nobody was interested in investing in crop science. The profits in agriculture and food sciences do not reach those obtained in the pharma industries.

Møller knew that he would never be the kind of scientist to make a "blockbuster" product for an already existing market. Plants produce too little turnover. As a consequence, he maintains that PlaCe and Pro-Active plants have been doing basic science research all along. This, however is not synonomous to doing isolated ivory tower knowledge production. On the contrary, Møller is very engaged in the future needs of society. One of his most frequently used headlines for presentations and research proposals is *"planting the future"* (Møller 2008, 2010b) This, according to Møller is not done by inventing products for an already existing market but is rather a matter of inventing a new bio-based society.

The crossfire between the above three threats in the habitat of plant biology resulted in a long period of time where there was "hardly any money" to get for Møller and his colleagues. As a result he initiated parallel basic research programs of interest to the major biotech companies. The focus on basic research enabled him to obtain what, according to Møller turned out to be "a 10 year major block of grants from the Danish National Research Foundation" (Møller 2011, email correspondence). These dangers of industry-based collaboration for researchers who focus on long term basic research are still very much present Møller says. Industry partners may withdraw their engagement at the first sight of controversy in fear of media scandals or changes in legislation. Møller may get many offers, but once things really get going he is just as often turned down again due to the controversial nature of his research making it a risky investment. Møller indeed experienced some very unfriendly changes in his habitat and the "mechanism of selection" provided by Danish research policy did not make things easy for him. In the early days of Danish plant GMO, enterprise and politics was pure poison. How to make insights out of invoices in the absence of an existing industry? How to acquire an industry partner or prove one's usefulness when nobody else seems to think that one's research program should be allowed to grow? In short, the problem of Møller and his colleagues summed up to how to feed their research program in a rather toxic habitat. However, Møller tells me their research program stood its ground:

> BLM: Well, there is a kind of adaptation to this (…). The fact that it was … the fact that it's been so difficult have also made it necessary that to survive working on plants you had to be *super*! Damnit, it's just like they sometimes say about art, that you shouldn't give too much support to artists because the more money they get the more dull they become. And this is a terrible attitude, a TERRIBLE attitude. Really. But the problem is that there is some truth to it.
>
> BGH: ha ha, so you are advocating a sort of a *survival of the fittest*-logic?

> BLM: Well it's not because I wish for anyone else that they should experience what we have been through or have things the way we had them, but I can also see that we somehow managed to extract positive things from the negative debate, and that served to streamline some things and forced us to think ahead. It really has.
>
> BGH: And you are one of the survivors you could say?
>
> BLM: Yes! And one of the few (…) Yes, yeah. But it was also what I talked about with researcher X the other day. He asked me: How do you create a niche for yourself. And that's when I said: You can do that by being original or by holding on to your ideas for long enough until everyone else "dies" (Møller holds up two fingers and makes quotation marks in the air while giving me a bleak smile). In many ways our success is that the others have given up.
>
> (Interview 2009)

Møller is plant biologist to the core. To explain how changes in the way research was managed and funded left a hostile habitat for researchers he makes use of the idea of adaptation. Adaptation is the concept we need to use if we want to understand the strategies deployed by Møller, which allowed him to hold on while many other plant scientists suffered or gave up. How did Møller´s research centers PlaCe and Pro-Active Plants manage to survive and attract resources when so many others apparently did not? Which adaptations are necessary in order "stand your ground" as a scientist?

Surviving, as we shall see, is not really about strength and stamina. Survival of the fittest when interpreted in its cheapest macho-imagery will neglect to take into account the relational skills necessary for adaptation. As we shall see, Møller is not a macho-scientist who survived by brute force and stubbornness. Rather, his research program survived and evolved on the basis of its relations to the environment surrounding it. However the relational nature of his knowledge production is not like that imagined by Nowotny et al. PlaCe and Pro-Active plants have been immensely successful in relating to policy agendas and industrial interests, not by allowing them to become part of the research agenda or by extending the test of validity or relevance to political agendas or industrial needs. Rather, the relational strategies deployed by Møller is a set of fine-tuned specializations which allowed him and his colleagues to take advantage of an otherwise hostile habitat. I will show that PlaCe and Pro-Active plants did not isolate themselves from the neo-liberal habitat of Danish research policy, neither did the center assimilate itself to the demands of political agendas and commercial interests. Adaptation does not equal assimilation.

In order to stay true to the biologism inherent in Møller's way of making sense of his own practice, the following analysis is organized around the idea of adaptation. Adaptation produces robustness, Møller argues, assimilation erodes it. Being a highly specialized scientific species may be the way to survive an unfriendly habitat, but specialization also means fragility and danger. Inspired by the insects studied in Møller´s laboratory, I will first describe the kind of dangers he identifies in the changes that took place in the way research was funded. The overall danger is *dependency*. Then I will go on to articulate three strategies for adaptation which have been crucial for Møller and his research centers in response to this danger. The first one is *tasting*: Møller is constantly engaged in discriminating between good and bad resources or good and bad collaborations. Second I will look the strategy of *self-supplementation* an approach where scientists develop new functions to produce the resources they need. Third, I will outline the strategy of *countering imbalances* within the "body" of plant biology by storing and distributing resources independently of changes in the political or economic habitat. Each of these sections will draw on concepts and vocabularies from Møller´s research on plant-insects interactions and the ability of insects to deal with the toxicity of feeding plants. To begin this experiment, we will have to revisit the *six-spot burnet moth*.

Dependency - dangerous resources

Figure 10: Caterpillar of the six-spot burnet moth on its preferred feeding plant.

As explained earlier, the larva of the six-spot burnet moth circumvents the defense system of the plant *Lotus corniculatus* by sequestering the poison inside its own body rather than being poisoned. The larva thus gains an advantage because the larva's own predators then tend to or learn to avoid it. However, this strategy has a downside - *dependency* on the feeding plant. The larva may have overcome the problem of the toxicity of their food plant, but only at the expense of being completely dependent on the presence of very specific bio-active natural products (cyanogenic glucosides). The larva needs this plant in its nearby habitat if it is to survive.

The line of *Lotus corniculatus* used in the experiments conducted by Møller and his colleagues produces two types of cyanogenic glucosides: *Linamarin* and *lotaustralin*. The larvae depend on the presence of a precise combination of these two compounds in their body in order to grow and develop. This combination is only available in Lotus corniculatus, rendering this plant the only acceptable host plant for the insect larvae. Experiments conducted in Møller's laboratory show that larvae reared on acyanogenic plants exhibited decelerated development (Møller, 2010, p.6). In another experiment the larvae were reared on *Lotus japonicus*, a plant that produces similar defense compounds but with a lower level of linamarin. In this case the larvae similarly showed reduced growth. The "cyanide society" developed by the six-spot burnet moth through 450 million years of co-evolution depends on a specific balance of cyanogenic clucosides. Not only will an absence or imbalance of these compounds result in reduced growth, mating and reproduction will also be disturbed as females select males on the basis of the cyanogenic content in their bodies. During mating the male transfers cyanogenic glucosides and other compounds to the female. Cyanogenic glucosides may also be involved in protecting the eggs of the insect (Møller 2010, Zagrobenly et al. 2008). Adding to this is the problem of mobility. A larva only has a 3 meters' radius in its lifetime, meaning that if the right kind of food plant is unavailable within 3 meters, it will have to feed on non-cyanogenic plants.

To sum up: the fragility of the young larvae does not lie in their vulnerable physique, but rather in their *dependency* on a specific feeding plant. Given their limited mobility, the larvae depend on the presence of suitable feeding plants in their nearby habitat. Without this availability the larvae will not develop properly and will not reproduce (Møller 2010). A similar threat of dependency is present for young scientists, Møller argues.

Møller's research area is no longer under pressure as it was, he says. Møller tells me that to most Danish politicians it is now an "obvious fact" that the future society cannot be based on fossil fuels and that plants and plant biomass is going to be key future sustainable resources for food, bio energy and entirely new materials. This turn towards "green futures" has now made his research of obvious relevance and given it the legitimacy it so lacked for decades. According to Møller, the Danish biotech industry and politicians agree on this point and are very interested in exploiting the opportunities to develop new market areas. This has made funding more abundant for him and his colleagues. However, even now, where GMO has become a less contested area of research, fragility and dependency is still a problem. Møller proceeds with caution. He is wary of jumping on the first and the best possibility to collaborate with industry and does not apply for any grant he can find. An example is his story of a

GMO-meeting he went to in the Danish House of Industry[85]. At some point a debate broke out about the problems with science-industry collaboration and the dilemmas of scientists who get involved with commercial interests. A scientist was complaining about the limits put on his right to publish caused by co-funding his research activities with industry partners:

> BLM: Yes, then some people got up and said that they are not allowed to publish their results on some genetically modified crop because Monsanto have refused them the right to publish on it. And that´s when I say: What on earth is this? And that's when he explains: *"But it's Monsanto that pays for our work, they have provided the seeds"*. I had to tell him that this is not a very smart arrangement, it is just asking for trouble and you cannot build a career in science on such strategies … (pauses)... Yeah it was something like that: "You are a terrible fool (en værrre klovn) (…) You are supposed to come here and teach us something (about soy beans) and then you get yourself dependent of this Monsanto money, that's not very smart is it?
>
> BGH: I Guess not
>
> BLM: You need to define the basis for collaboration with industry from the very beginning, so you know that you can publish freely. Otherwise you can easily run into problems … I mean, he is outlining the problem as if you can´t make a neutral risk assessment of a Monsanto product. And yeah! If you're *paid* by Monsanto and have not set up the collaboration in a proper way, it can't be any surprise that you can't. Or at least there will be situations in which you can't.
>
> (Interview 2009)

The example shows that dependency is a real danger for scientists who want to feed their GMO research programs. Not all kinds of funding will facilitate growth. Interaction between science and industry is fine, but getting yourself dependent on "Monsanto money", according to Møller, is clearly a case of feeding on the wrong kind of resources. Some types of interactions and some kinds of funding are simply corrosive and inhibit scientists from unfolding and developing their research area. No new knowledge is produced, funding resources have been consumed in vain, and researchers have been feeding on the wrong kind of resources. Some types of funding – some types of scientific habitats – will invariably result in inhibited growth in knowledge production. This is clearly a completely untenable development for Møller. Science and the biotech industry may be closely intertwined and many excellent relationships with industry have been established, but not all kinds of interaction result

[85] Industriens Hus.

in a productive symbiosis. There are clearly types of industry mingling which are bad for the development of the discipline. Stated in a biological vocabulary, one could argue that the Monsanto-paid researcher is not part of a healthy symbiosis. Rather, his relation with Monsanto has required him to assimilate scientific practice to the needs and interests of Monsanto. Assimilation will not allow plant biology to grow. Assimilation is not adaptation.

Short sightedness and dependency on inappropriate resources is a repeated theme in conversations with Møller. In the above extract he sounds particularly harsh, but as he addresses this theme over and over again in interviews, it is clear that this is a big concern for him as one of the senior people in the field. He is deeply concerned about some of the adverse effects produced by the present climate in which scientists make their careers. Not only is the present system starving the younger generation with scarce resources and fierce competition, but it is also offering up the wrong type of funding that might inhibit growth and publication within the discipline. Møller is especially worried about the lack of ability to discriminate between good and bad resources exhibited in the younger generation of scientists and colleagues. He will tell me of the many pitfalls involved when scientists have to get funding from industry and work closely with commercial interests. The world of research, according to Møller, is a place full of dead ends that researchers can easily walk into in their quest for money. He tells me that when people are hungry for money because competition is fierce or because resources are scarce, they often end up going for the "kind of money you should stay away from". The consequence is malnutrition and decelerated growth patterns.

Modifications

Møller's research is about modifying and making improved crop plants to change their functions for specific purposes. Of particular interest is making plants that can repel insects without having to use a chemical deterrent. When it comes to poisonous plants like *Lotus corniculatus* and cassava (a potato-like plant growing in large parts of Africa) the insects that attack it have adapted to the plant's defense. Consequently, one can make the plants resistant to these insects by *switching off* the gene that produced cyanogenic glucosides[86].

The lack of toxins in the plant will, paradoxically, make the plant useless or "poisonous" to the insects that have now developed a dependency on cyanogenic glucosides. Acyanogenic "non-toxic" feeding plants are thus useless or even dangerous

[86] Conversely plants like *Arabidopis* which does not contain cyanogenic glucosides could be made resistant to insects that normally feed on it if modified to produce these compounds (Haldrup & Bruun 2004)

for the larvae to feed on. By modifying the plant, and leaving it as the only feeding plant for the insects, a whole generation of new larvae fail to grow and reproduce. The associated risk is that other generalist insects may now start to feed on the plants which they could not before. (Interview 2009, Haldrup & Bruun 2004)

Danish science policy has also been involved in its own modification experiments. Møller is rather critical of the consequences of micro management of research funding by politicians and the consequences this has had for the new generation of scientists. One policy agenda which he is particularly critical of is the modification of research funding towards strategic research (although with the exception of large programs which incorporate strong elements of basic research). Small research programs initiated by politicians as a result of specific issues that have been promoted in the media and where the politician wants to demonstrate the ability to act are normally useless, Møller argues. Such short-sighted initiatives constitute a repellant rather than nourishment for science. This kind of money should be approached with great caution, he says. Scientists have to be "fit for fight" not think with their stomachs before they engage with this kind of "modified" funding.

> BLM: Well, it really demands that you are fit for fight right, because if you are not either a very young scientist with a Science or Nature paper or a very well established scientist you have a hard time these days to get your research funded. Then it is very tempting to go for the kind of money that you shouldn't go after and work on issues which are only started up because of some political agenda, you know, something about food or whatever. Typically the raised problem lacks a solid foundation meaning that an informed politician or researchers should stay away from it. Really, that is never going to … that's the kind of thing you just *should not touch!* (…) It all ends up in trouble for these kinds of researchers.
>
> BGH: by *the kind you should not touch* of money do you mean short sighted… grants that…
>
> BLM: Yeah, projects where you start something and don't realize that when this grant is finished then you are out of money, right? No continuation is possible. Then you just start something new, right? (…) so you know, it's not like I can't see who suffer under this system, but the problem is that the money could have been spent a lot better.
>
> (Interview 2009)

According to Møller, research is a highly competitive field where only the fittest survive. One does not become fit by feeding on empty calories or short-term money based on a narrow agenda. Clearly, the modification of Danish research funding which has tied more and more resources up with politically selected strategic agendas has given plant biology a seductively dangerous habitat which, according to Møller, favors only the young shooting stars or the older, established and elite. Those who are in between these two categories easily end up "in trouble". According to Møller this trouble could not just be remedied by new and clever strategic maneuvers on behalf of this kind of researchers. It is part of the way in which new and modified resources have been introduced, such as small strategic research grants restricted to applied research and short-term programs. In the following exposition of the adverse effects of this tendency, we get a harsh critique of government and politics from Møller. By modifying the system of funding so that it favors short-term results and forces universities to think in terms of the present perceptions of society's problems, the Danish government is effectively *switching off* sources of funding that nourish the younger generations of scientists and makes them grow. Individual scientists may consequently be dragged into dead-end research:

> BLM: As a researcher you should be able to think ahead. Someone who just walks from one thing to the next never becomes an established researcher. Sometimes a political debate comes up and then the members of Parliament get all active and then they make some new research program. (…) just to close down a debate. Then when two or maximum 3 years pass by and the debate is all over no one has an interest in continuing this program. That's a bad arrangement for everyone. I mean, you can't accomplish a lot in 3 years, nothing that goes deeper.

Clearly, interaction and contextualization is not always productive. Contrary to the claim made by Nowotny et al. that the taste for purity make scientists shy away from taking into account external demands, Møller is addressing a different problem. He has no problem with engaging in strategic research or responding to politically directed agendas *if* they provide him with the right kind of resources: basic science funding. The kind of resources described above is dangerous not because Møller perceives them as "external" but rather because their short-sightedness preclude the necessary attunement to the needs of his research area. Going for short-sighted grants that will not nourish the research program as a whole over time does not accomplish a lot and renders the researchers dangerously dependent. Fragility is still a problem in that science depends on the availability of the right kind of "feeding plant". Like the larvae of the six-spot burnet plant biologists have restricted mobility. If they allow for an intensified "contextualization" to direct them they will shift to a new research area every three years. Wandering back and forth between different research topics and entering each topic without detailed prior knowledge entails high costs. It takes energy to crawl out of one's own research area to hunt for more appropriate resources.

> BLM: The problem is that younger or single researchers easily get caught in this system... they get a 3-year grant and they run some kind of project and then they think they can get another 3 years all on their own. But the

> politicians typically lost interest and want to start up something new, so you have reached a dead end. That means you are going strong for three years and then full stop. And then it runs out and then you get these frustrated researchers. (Interview 2009)

By "walking from one thing to the next", consuming whatever funding you encounter on the way, young or single (unattached to a larger group or research program) researchers will not create a strong academic profile, Møller argues. They will not attract resources for new PhDs and they will not form larger bodies or gain "critical mass" to establish new areas of research. The consequence of not doing so is stagnated growth and eroded academic profiles.

By contrast, researchers who are parts of a larger group can grow to form a larger body and can by way of their stage of development take advantage of a larger pool of resources and more improved functions. One might claim that Møller is precisely such a researcher, the kind Robert Feidenhans´l previously introduced to as a "project baron". As a scientist, Møller has already created a niche for himself and a lot of people are part of the work of developing this niche. Møller works constantly to apply for funding, that is after all what keeps a project baron in business, but basically he does not have to worry whether his research center will be closed down in a year. The centers he leads have grown strong and robust enough to pay the price of mobility. As one of the partners in the 120 million kroner UNIK research grant, Møller and Pro-Active Plants joined the champagne toast in the Nano-Science Center to celebrate a new venture into synthetic biology. In this interdisciplinary collaboration he needs to take the leap from plant biology to nano-biology – without of course leaving his core research interests. Clearly, he could not have taken this leap 25 years ago when he was a new researcher in an area that was marginalized and under much pressure to gain legitimacy. However, years of development of his academic profile, the building of a larger group and the establishment of a respected research program now allows him to fly much further than the young and hungry generation who can cover no more than three academic "square meters" and are left to find whatever resources they can within their specific research areas.

As we can see, Møller is quite critical of Danish research policy and conscious of how he deals with the demands it puts on him as a research manager. The modifications towards increased focus on short term strategic research creates fragile growth patterns for a cohort of researchers that are never the less expected to do well in the face of fierce international competition. Møller's research area has been a tough one to work in, there has been a powerful *"mechanism of selection"* he says. But Møller survived as one of the few. Let´s have a look at the strategies which allowed Møller to come this far. Again, the relation between the insects and feeding plants studied in Møller's laboratory will help us elucidate such strategic maneuvers. Inspired by Møller's work on the *flee beetle* and the *six-spot burnet moth,* I will outline three strategies which seem to have helped him build a niche for his research by "holding on until everyone else dies". Again, standing your ground and holding on is not just about brute force and stubbornness. Maneuvering within the environment provided by the Danish *knowledge economy* takes relational skill and the ability to orient towards the surroundings. In outlining the three strategies of *tasting*, *self-supplementing* and *countering imbalances,* it should be noted that all of the strategies presented here are specific to the mature researcher who has left the first fragile and dependent larvae stage. They are not necessarily applicable to young, hungry, isolated researchers that are not part of a larger body. Møller often stresses that this puts a lot of responsibility on mature researchers to foster the new generation of young independent researchers.

Tasting


11a


11b

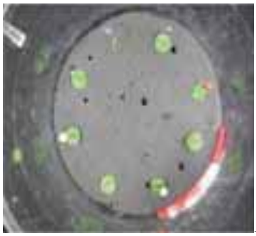

11c

Figure 11a: Plant attacked by flea beetles

Figure 11b: Adult Phyllotreta nemorum

Figure 11c: Free choice selection experiment set-up

Flea beetles are small, jumping beetles of the leaf beetle family. Most farmers will know them as pests as they attack agricultural plants like rape and can do a lot of damage especially in the seeding period (se figure 2 a). Flea beetles have sensors in their feet and antennas telling them whether the plant they walk on constitutes proper food or not. Møller and his colleagues have primarily conducted experiments with the flea beetle *Phyllotreta nemorum* (figure 2 b), the beetles are presented with a selection of wild type *brassicas e. g.* alongside the a genetically modified type. The modified plant produces cyanogenic glucosides, the wild type does not. The *Phyllotreta nemorum* clearly discriminates between the two plants. They will monitor, or as Møller jokingly calls it, *taste* with their feet and antennas and only feed on the wild type (see figure 2 c). Only if the beetles are starved for days and subsequently presented with just the modified plant will they reluctantly feed on it. However, this will make them go into a temporal paralysis after which they do not feed on the modified plant again.

With the six-spot burnet moth, a similar ability to discriminate and detect a suitable feeding plant is available to the grown insect. The female moth has no problems selecting a suitable plant when laying her eggs. However, in early developmental stages this ability to discriminate is not so apparent. Møller and his colleagues write:

> "*L. japonicus* and cyanogenic *L. corniculatus* contain about the same amount of CNglcs, but larvae develop faster and to a larger size on the latter food plant (Zagrobelny et al., 2007a). In spite of this, the *Z. filipendulae* larvae prefer *L. japonicus* over *L. corniculatus* in laboratory free choice feeding trials but the cue was diluted to insignificant levels in field trials (Zagrobelny et al., in press). The nature of the feeding cue detected in the laboratory experiments is not known" (Zagrobelny et al. 2007, p. 1463).

When modifying cyanogenic plants, young larvae of the six-spot burnet moth may not be as sharp as the female adult in detecting their feeding plant. However, the survival of the species depends on the clear detection of specific feeding cues and hence the selection of appropriate resources to facilitate growth and reproduction. It seems that the ability to detect the appropriate feeding plant is more developed in the mature insect than in the young larvae.

If the policy experiment conducted by the Danish ministry for Science, Technology and Innovation has modified research funding, you could say that Møller´s strategy has been to act like the *Phyllotreta nemorum* or the grown female of the six-spot burnet moth. According to his statement, the important part is to think before you apply for funding, or in the biological vocabulary: to monitor and *taste* before you eat. Møller argues that in times when resources are scarce, desperation should not lead scientists to stray too much away from their research interests in order to assimilate themselves to the most readily available resources. It will send researchers into paralysis or "full-speed for three years and then full stop". However, if scientists use their feet and antennas to monitor and thereby discriminate between safe and dangerous resources they may encounter situations where strategic research grants will constitute proper resources:

> BLM: So if you get dependent on this kind of grants, if that's the kind of money that drives your research, then there is no coherence in your research (…). But if some kind of program pops up and you already have a lot of stuff going in that particular area, then that's when I say that it's ok. (Interview 2009)

Every once in a while industry or strategic funding will be offered in areas which can easily be related to projects that are already part of the research program of Pro-Active plants. Looking at Møller's research profile over the last 30 years, it may look as if he has covered a lot of ground. PlaCe and Pro-Active plants have been involved in projects related to improved nutrition, hunger, crop science, green energy and nano medicine, but if you look closely you will see that whatever problem the research is trying to solve, the science involved stays close to the three specific research programs of PlaCe and Pro-Active Plants: *Photosynthesis*, *cell walls* and *bio-active natural products*. The strategy for choosing these the research topics was quite straight forward, Møller says:

> BLM: These three research programs address the key processes that enable plants to grow and develop and defend themselves from a fixed growth site. Plants cannot run away when it is too cold, hot or dry or when someone wants to eat them. Photosynthesis enables plants to make all organic compounds they need based on the use of sun light, carbon dioxide in the atmosphere and inorganic minerals. The ability to form a rigid cell wall enables plants to stand even though they do not have a skeleton. The ability of plants to produce an array of bio-active natural products enable plants to respond to and communicate with the environment e.g. to produce defense compounds towards an attacking insect.(E-mail correspondence 2011)

These three research programs have remained the focus over the last 30 years, Møller says. This rather basic science focus has, however, been related to a wide array of different problems and industries. For example photosynthesis projects have related to food and hunger problems and crop science, cell walls to biomass, bio fuel, climate and energy, and bioactive natural products to insect resistant plants, higher yields, drugs and the medical industry. Despite its basic science profile, the research center is able to relate to a wide range of partners and attract funding from many sources by way of these parallel basic research programs.

According to Møller, the habitat of plant biology requires an ability to discriminate between a wide array of resources. Going after the first funding opportunity that comes along is a dangerous strategy, especially when you are young and fragile and do not yet have your career on track. Having a long career behind him, Møller has a very well-developed ability to taste and discriminate between resources. Accordingly, he is not open to any kind of collaboration or ready to adapt his research to what he perceives as the goals of government and industry. An important part of his job is to be selective. As previously stated, Møller would never enter into a collaboration where an industry partner could stop him from publishing results. However, more fine-tuned abilities to "taste" and discriminate between options for future collaborations are also needed. Møller's maneuver is a simple one: *"Talking to people"*. Over the years he has developed a whole strategy of what to listen for and what kinds of questions to ask in order to monitor whether a future collaboration will work or not.

BGH: So if you are in a meeting with a prospective partner, maybe someone with a different disciplinary background from yours or someone from industry, and you are to find out if there is basis for some kind of collaboration (…) what do you do to find out if they are...

BLM: First of all: who is coming to the meeting, who is going to show up, is it the guy I'm going to work with or is it some … I mean it's *him* I want to talk to! And then you can also just pick up the phone and ask, and then they will try to explain what kind of problem they have, and that´s it. I mean you will ask things like: How would they try to set up this experiment? And if they then come with some completely hopeless plan then… I mean, if you can feel that they have actually given this some thought, right? If it makes me start thinking about things that I should worry about and remember to tell because it could be a typical pitfall that is easily overlooked, then I know they are *on track*.

(Interview 2009)

Asking simple questions, sometimes even questions that Møller already knows the answer to, is an important strategy. ”Even a stupid question demands a reasonable answer”, he says, and not any partner can give such an answer. To detect whether an academic or industry partner is "on track" means to check if they make sense of the problem, and if they have their basic facts straight.

The struggle for fundability is not a simple matter of mingling with as many different types of resource providers and collaborators as possible. As we saw in chapter 4, things are already mixed and mingled for Møller and his colleagues. The trick is to *mingle with care*. Forgetting to taste and discriminate and thereby ending up feeding on the wrong kind of resources is not an option, if you want your research program to survive. Mingling with industry and getting external funding can definitely be an advantage. However, as Møller makes it clear above, collaboration requires the prospective partner to "have given this some thought". This goes for corporate/academic science collaborations or for interdisciplinary collaborations alike. When meeting up with a prospective industry partner, Møller will not be interested if the person that shows up is just the CEO of a company or a marketing person. He needs to see the "person he will be working with". This person clearly has to be someone who would have an opinion on how to set up an experiment, someone who knows the science. Thus, selecting between resources is a process of detecting the presence of serious academic content in a possible collaboration and also checking whether the partner´s understanding of the research problem is compatible with that if Møller and his colleagues. The science has to be serious and fitted to the specialized needs of the research program. If not, the collaboration will be unproductive. Møller is often contacted by companies who want to talk about collaboration, so often indeed that he does not have time to go into all of them. Currently, there are more offers than he can cope with, he says.

Møller is neither young nor starved. Not anymore. He knows which resources to feed on and which to leave alone. Over the years and through trial, error and temporary paralysis, he sensitized himself to discriminate between resources just by making hundreds of phone calls, going to meetings, using information from the large network, and by experiencing the costly consequences of failed collaborations. For now he seems to have plenty of the right kind of funding within his radius of mobility, but there is no way to be sure that this situation will last in the future. As a scientist, he needs to remain highly sensitive and use his antennas to monitor possible resources on an everyday basis.

But what about researchers who are not in this privileged position of the mature scientist or the "project baron"? What about researchers who are too young to have sensitized themselves to discriminate between nourishing and weakening resources? What about researchers who are simply too starved to taste before they eat? According to Møller, not all scientists have survived the experiments conducted by Danish research policy with increased focus on directing research along political and strategic agendas. The lesson learned from Møller is that when researchers are presented with an abundance of more or less adequate resources and starved until they comply and eat up, they may end up weakened rather than fed. In order to be a successful researcher, Møller concludes, you have to keep your eye on the long-term benefits and protect the coherence in your research. Surviving means carefully tasting and discriminating and standing on your ground even in the face of starvation. Having experimented with new sources of funding and the paralysis they may cause in scientific productivity, he has learned to think – or rather to taste - before he eats. However, in order to stand his ground, another strategy is also needed. Møller has had to develop the ability to produce new functions in the body of the research group, allowing them to be less dependent on a specific kind of ressources. For outlining this next strategy, we will have to revisit the developmental stages of the six-spot burnet moth.

Self-supplementing

The larva stage in the development of the six-spot burnet moth can be divided into several sub-stages. Once a larva is past the first two stages, something interesting happens. The larvae become less dependent of the presence of cyanogenic plants. As we saw earlier, the larvae need a very specific balance between two kinds of cyanogenic glucosides: *linamarin and lotustralin*. In the absence of a feeding plant that provides them with this particular combination, young larvae are fragile and cannot survive. At the third stage of larva development, however, the larvae have grown bigger and more robust. At this stage, the larvae somehow manage to produce the relevant compounds inside their own body independently of their presence in the feeding plant. Møller writes:

> "A few species of arthropods are able to sequester cyanogenic glucosides from their feeding plant and to *supplement* the supply by *de novo* biosynthesis of the very same cyanogenic glucosides. This applies to larvae of Zygaena (Zygaenidae) (Møller, 2010 p.6)".

The larvae thus *supplement* the existing reservoirs by producing cyanogenic glucosides independently of the feeding plant. This process - *de novo bio synthesis* - is a function developed in the insect through what Møller calls "genetic neo-functionalization" or recruitment of new gene functions and natural selection. *De novo* biosynthesis allows the larva to produce within its own body the very compounds it would otherwise need from external sources. Møller Writes:

> "In those cases where the insect cannot obtain enough cyanogenic glucosides by feeding on its host plant, the insect has developed a procedure to *de nov*o synthesize the plant defense compound itself. This demonstrates that the insect in the course of evolution has become dependent on the ability to acquire and store these compounds. But *de novo* synthesis costs a lot of energy and the larvae who are required to do a lot of *de novo* synthesis show slower growth and poor biological fitness." (E-mail correspondence 2011)

According to Møller's research, it seems the larvae strive to maintain a certain threshold content and specific ratios between the two cyanogenic kinds of glucosides, "regardless of the content in their food plants" (Møller, 2010). The larvae seem to have created a mechanism for self-supply in case they need to top up their content of cyanogenic glucosides to retain the threshold balance. Through this mechanism of self-supply they maintain an adequate level of compounds in their bodies even in the absence of an appropriate feeding plant. This means that as larvae grow bigger and need more food, they can now choose between broader selections of plants when facing a habitat of scarce resources, but not without paying the costs of poor biological fitness and slower growth.

As we already know, Møller´s development as a scientist took place in a cross fire of scarce resources and what he perceives as a quite unfriendly policy habitat. Despite an increase in the overall investment in research by the Danish Government, some of the new types of funding resources remained unavailable to PlaCe and Pro-Active Plants. When Danish research policy began to create incentives for science and industry to find new pathways for collaboration, Møller remembers that plant GMO was cut off from a lot of new funding opportunities. Even though there was a significant increase in the overall funding as the Danish Government increased its investment in research and innovation, a large part of the money was ear-marked for research projects that involved collaboration with an industry partner. New industrial PhD stipends had to be co-financed with industry, and large-scale projects that collaborated directly with industry were preferred to those with a purely academic agenda. It was not so much a difference between basic and applied research, but rather a matter of whether or not industry could be directly involved as a partner in the research application, Møller says. However, in Møller's field no industry was available at the time, and GMO was still too controversial and risky for investment. The lack of private investment plant GMO posed a serious problem for Møller and his colleagues. There was no way of gaining access to the new types of funding, and even if there had been, the funding may not have been adequate in relation to their specific research program. This situation produced a new response on behalf of the scientist. Since they could not go down the dangerous path of assimilating their research program to short-sighted industrial or political interests, they saw no other way than to invent a new function in their group. Their strategy was simply to begin producing the very thing they lacked - an industry partner. This is how they came up with the idea of starting the small spin-off company, which was discussed in chapter 4. Before this small biotech company was acquired by a larger biotech company, of which it is now a subsidiary, the company was a key component in Møller's and three of his colleagues' survival strategy to secure sufficient and "healthy" funding by producing industrial interests that were not present in their surrounding habitat:

> BGH: How did you come up with the idea of making a company?
>
> BLM: Well, it was researcher Z, who was the manager of the cell wall group here, and researcher Y, who was in charge of photosynthesis, and a good friend with experience from employment in the biotech industry and me. There was so little research within the area of plant GMO in Denmark, and nobody dared do it, so we thought that we should try and show that we have got something going here.
>
> BGH: Nobody dared doing plant GMO?
>
> BLM: Nobody *ever* dared except for company X, and they have now stopped their activities in Denmark. Not in the plant area. GMO is too risky, the rules are too unclear. We thought we had some ideas and then there were just more and more research grants which you could only get if you had an industry partner. This means, when no industry was working within our field then there was a lot of research grants that we could *not even apply for*! So by creating a company which had our research program as its core interest, then we had the ideal industry partner! (…) Making a

company was a way of getting access to that money (Interview 2009).

Normally, the type of research grants that require scientists to fit their research program to the needs of an existing industry would be characterized by Møller as dangerous funding or "money you shouldn´t touch" (Unless of course you already happen to have "stuff going on" in their area of interest). The fact that no Danish GMO industry existed at the time because "no one dared" meant that Møller and his colleagues would have been forced to starve or to accept stop-go-paralysis and erosion of their academic profiles.

By creating a spin-off company which could represent corporate and societal interests on research applications, industrial interest is produced *de-novo* within the body of plant biology. This *self-supply* makes Møller and his colleagues able to feed on resources that would otherwise have been inaccessible or dangerously growth-inhibiting. By creating an industry partner that had their own research program as its mission statement, they were able to supplement their resources with grants that were earmarked for science-industry collaboration. Grants that would otherwise be either impossible to feed on or which would gradually erode the academic profiles and consume the surplus energy which makes a research program grow stronger. Not only did this strategy result in sending the right kind of signal to the ministry and the research councils by demonstrating a willingness to leave the study chamber and engage with commercial interests. It also gave access to new sources of funding in that it makes Møller and his colleagues able to feed on resources that would otherwise have been dangerous because of their lack of relevant scientific content. By developing the very thing they needed – an industry partner attuned to their specific research interests – Møller and his colleagues adapted to a hostile habitat without having to engage in dangerous assimilation processes or short-term funding based on applied research. However, to larvae and to scientists alike, the strategy of de *novo* production of the specific functions that trigger growth cannot replace an influx of adequate resources. Let´s start with the larvae:

> "Larvae are in part able to compensate for the level and composition of CNglcs (Cyanogenic glucosides) in their food plants by *de novo* biosynthesis of the lacking components. However, larval sequestration of CNglcs from the food plant does play the dominant role in the overall acquisition of CNglcs and *de novo* biosynthesis does not allow the larvae to fully adjust. When Zygaena larvae are feeding on acyanogenic plants and therefore forced to acquire all of their CNglcs by *de novo* biosynthesis, the energy spent on CNglc synthesis reduces the amount of energy left for growth and development" (Zagrobelny et al., 2008).

Producing what you need *de novo* is a good tactic when resources are scarce or of the wrong kind. However, it costs extra. This goes for the larva studied by

Møller as well as for Møller himself. As touched upon in chapter 4, engaging industrial collaboration and developing technology platforms take time that should have been spent "doing biology". Indeed, setting up industry collaboration can be rather time-consuming. Møller is not the only scientist who has developed a habit of heaving a deep sigh and making long pause when the conversations turns to technology transfer offices, lawyers, patent negotiations and problems with mixing public/private interest. Building technology platforms and starting a spin-off may be necessary in order to supplement the flow of resources and make sure that the right kind of funding becomes available, but to Møller it is mostly a supplement. In fact, as I was concluding the series of interviews with Møller, one of his major concerns was how to *get rid of* his personally owned patents and disconnect himself from his former spin-off. Having a spin-off company and owning patents may be a good idea in relation to one research council or as a strategy at one point in time as well as a hindrance at another. A patent which was at one point a gateway to fundability has now become a potential problem in that Møller needs to document that he is not applying for public funding in order to promote private gain. Consequently, he ended up selling his patents to rid himself of the complications they constituted when applying for large-scale public research funding. Becoming more self-sufficient by making your own spin off or setting up your own technology platform is a good supplement and scientists who have this new function built into their careers will gain a competitive advantage. However, there is no doubt that the dominant mode of resource acquisition remains basic science funding (public or private). Producing the resources you need *de* novo will eventually drain researchers of energy and constitute a costly digression. Supplementing resources by creative workarounds or carefully balanced collaborations are strategies which can give Møller the extra 10 per cent but it is not the main means of ensuring that his research area grows bigger, more legitimate and further strengthens his international standing. If he was to nourish his group and make things grow and thrive exclusively from self-supplemented resources it would quickly exhaust the whole endeavor. For this, we will need to turn to the third and last tactic. Apart from *tasting* resources before they are consumed and developing a *self-supply* of industrial interests, Møller and his colleagues rely on the larger body of their group to sequester, store and distribute resources in a way that ensures growth and development in order to *counteract imbalances* in funding. This strategy is not unlike that of six-spot burnet moth at its third larva-stage:

Countering imbalances – storing resources

Apart from working as a deterrent, cyanogenic glucosides are also made to work to the advantage of the larvae in the form of storage compounds. The energy present in these defense droplets on the side of the larvae´s body can be reabsorbed and further converted and end up in the primary metabolism of the larvae after an attack or irritation has stopped. In this way the droplets are not just functioning as a defense against predators but also as energy storage. Møller and his colleagues write:

> "Compounds that initially served defense purposes may now acquire new functions in host-insect recognition or be recruited as storage compounds that are mobilized when needed to counteract imbalances in primary metabolism. Thus as we learn more about the intricate functions of secondary metabolites in plants and arthropods, the distinction between primary and secondary metabolites is vanishing and becoming meaningless." (Zagrobenly et al. 2008).

Even though the cyanogenic glucosides are "secondary metabolites" – compounds that are normally not considered to be involved in growth, development and reproduction – they have significant meaning to the primary metabolism of the larvae. Reabsorbing and reconverting these defense compounds back into their primary metabolism allows them to feed on the energy present in the droplets and also to counter any chemical imbalance in their bodies in the absence of their preferred feeding plant.

Møller and his colleagues are similarly engaged in acquiring new functions from their existing resources, faced as they are with a habitat where the flow of resources is unpredictable. PlaCe and Pro-Active plants have grown into larger bodies and are no longer at the first fragile stage. Rather, it is now a large and well-developed body of researchers that have the energy to produce new functions, such as a spin-off and several technology platforms and to adapt creatively to changes in its habitat. The two centers managed by Møller throughout his career have enjoyed a steady growth pattern. If people leave to pursue their careers elsewhere, they usually return with new experience and new collaborators in their baggage. Møller describes the great responsibility that goes together with managing major research centers such as PlaCe and Pro-Active Plants, which is very unlike that of managing a new and fragile scientific career. As a manager he is not only caring for his own career but rather for a larger body of research involving several sub-groups.

> BLM: It's a huge responsibility to make things flourish and thrive (…) If I had spent the money only on my own project then all these projects on photosynthesis and plant cell walls had not been started here. You see?
>
> BGH: I guess...
>
> BLM: Today it is very difficult to predict which grant applications you get funded and which are going to be unsuccessful. This makes it very difficult to get the right amount of funding to the different projects. Some years a project was completely devoid of funding. However, when you get a grant, it typically takes time to get staff recruited and to buy instruments… So the very little resources we had available in these two

> areas of research they… we have always had a common box of money from which the group leaders could borrow to keep their research running in the years where their grant applications were turned down. From our total budget we could precisely calculate the costs of having a researcher and a technician working in the lab per year. So we kept track of the number of man months a project had been spending each year. That money then had to be paid back to the common money box as soon as grants started to come in again. This system only works when you have high trust in your colleagues and know they are performing as good as they can. When this is the case and a project was not getting any funding, the other project leaders of course thought that this was unfair that a colleague was not funded, right. So we have had this system where we help each other, where you can borrow from the common box of money when times are hard and then you will not get that stop-go thing. So it does take a lot of management to do this. But in all those years, we have never ended up in a situation where a project was not able to pay back exactly what was borrowed as things started to work for them. This is important because that secures that the grant money is used as specified in the conditions for the grant. So some projects will indeed end up having less money compared to other projects, because they are not able over long periods to get so much external funding compared to other groups. But this procedure made it possible to avoid wasteful stop-go research management where the resources end up being poorly used. (Interview, 2009)

Møller is talking about center grants and other external grants as part of one larger body thus blurring the distinction between external and internal funding, between primary and secondary metabolism we could add. The center has a "common box of money" - resources sequestered inside the collective body of the research group to ensure that the scientists in the group who start up new research programs or who, in a specific year, had a hard time funding ongoing research will not end up in a stop-go rhythm and be forced to run for the dangerous resources. The management problem is one of securing a stabile growth pattern; making sure that projects running in the center keep a steady progress and are not be interrupted by changes in political interest or availability of suitable industry partners. Pro-active Plants have long ago included research in photosynthesis, cell walls and bio-active natural products, like the cyanogenic glucosides Møller is working with, as core research areas. However, as the center grew it began to have enough resources to start up related side projects and the group is now part of several larger collaborations with the possibility of future commercial applications and extra industrial funding. However, the research environment has always remained focused on basic science projects throughout its existence.

In the first conference held by the UNIK research project on synthetic biology where Møller was a point organizer along with Thomas Bjørnholm, I attended a lecture given by a professor who was formerly employed in Møller´s group and now heads a large project on plant cell walls at a prestigious university in the United States. Plant cell walls are the key source of bio-energy production but due to their highly complex structures they pose a major problem in the development of bio fuels from plant materials, so they have recently become a hot and highly fundable research area. According to the presenter, raising 600 million USD for basic science research on plant cell walls was done by telling investors that if they wanted to build a multi-billion dollar industry based on plant cell walls they would have to take into account that we still know virtually nothing about them. There is still a long way to go, he argued while presenting slide after slide with gene sequences until almost all the non-biologists in the room had gone to sleep. Investors and consumers will have to be patient. Nothing really interesting will happen within the next 15 years, he said, but we need the long-term investments to make serious progress. The presenter is still one of Møller's close colleagues and the cell-wall project includes researchers from Pro-Active Plants. In light of Møller's above statement it is perhaps worth noticing that before cell walls could even become interesting to investors, someone had to nourish the early stages of this research by redirecting resources from a sequestered "common money box" into this complex research area to ensure a steady growth of the activities. In order to make sure these kinds of projects did not "die" at the early fragile stage, Møller and his colleagues could not rely on and render themselves dependent on the agenda controlled by interest groups, politicians, research councils and industry. At one point in time, biofuel is pure fantasy to the minds of investors and politicians, and two decades later it is thought of as the way to save the planet. For Møller and his colleagues it is not acceptable that its research agenda becomes *contextualized*, if this means that it fails to support and nourish new ideas completely independent of what goes on outside the laboratory. Here, Møller argues, scientists have to stand their ground.

Imbalances in the group must be counteracted by drawing on stored resources in order to maintain a threshold balance. If the right kind of funding is not available for the growth and development of Pro-Active Plants, the size and developmental stage of the group now allows them to draw stored resources back into the "primary metabolism" to be consumed when no primary metabolites are available for a specific part of the collective. This countering of "metabolic imbalances" for the group as a whole makes it necessary to view resources as potentially available for any function that may be required and which has been given high research priority. To make things flourish means to store and distribute resources in response to the changes in one´s habitat. By drawing on a "common money box" which can be used temporarily to counter imbalances in funding starved research project, Møller and his colleagues ensure the survival of new and fragile projects until they have grown bigger and more robust. Møller says: "we trust that it´s unfair they didn´t get funding". Colleagues who take risks and venture into new areas that may be too controversial or too new to attract resources are thus partly protected from having to feed on dangerous, inadequate or short-term funding. In this manner, ProActive Plants have managed to counteract imbalances and ensure steady growth patterns. In this way, starvation of individual projects and erosion of academic profiles is prevented.

Summary

The three different strategies I have outlined here all relate to the threat of dependency and the fragility that follows – dangers which, according to Møller, result in poor cost efficiency and constitute a key factor for stagnation in or erosion of research environments. To avoid becoming dependent on short-term grants or industrial interests that prevent research from growing bigger and/or from publishing their results, Møller and his colleagues engaged in three types of strategies, maneuvers which I have articulated here by allowing the analysis to draw on biological vocabularies. First, there is the strategic monitoring and *tasting* in order to discriminate between resources. This ability seems to be more expressed in Møller as a mature scientist than it has been in younger and more fragile individuals who can easily go into stop-go paralysis or dead ends by feeding on the wrong kind of resources. Second, there is the strategy of *self-supplementing*, a very unique function expressed in Møller and his colleagues that made them able to produce *de novo* the industrial interest they needed to qualify as applicants for grants that provide them with basic science funding. Third, there was the strategy of countering imbalances by allowing combined or stored resources to acquire new functions as they were distributed to smaller projects to support them in their fragile beginning. I showed how one of these projects has now grown to a much more impressive scale and forms part of a large international collaboration where investors can be told to stay put, be patient and pay the costs while we wait for science to solve the mystery of plant cell walls. All of these strategic approaches have as their fundamental characteristic that they provided new necessary functions as the research area grew and became gradually more – well yes – robust.

Robust scientists

"Interaction" between academic science and its corporate, political and social environment was a key parameter of success for the Danish Government in 2003. Based on the above accounts, it is however interesting to see that this interaction has many functions other than making the leap from insights to invoice. Hans Müller Petersen whom we met in chapter one would like to see scientists leave their study chamber, develop networks and engage in agile, flexible responses to the needs of society. In many ways it is fair to say that Møller and his colleagues have complied with this agenda. They have related to a wide range of partners, raised substantial external funding and they have, without a doubt, contributed to corporate development through several industry-collaborations. However, at the same time, they never left their three basic research projects. In fact, had they really “left the study chamber” to engage in contextualized and externally validated knowledge production, they would have needed to change their program to something completely different long ago. However, Møller and his colleagues did not engage in the production of socially robust knowledge. They did not turn to the outside of the laboratory to have their knowledge claims validated, evaluated and modified by political resistance, bad press and lack of industrial interest. They did not turn around to fit the needs of society as they were perceived in the early 1970’íes when nobody was interested in crop science and GMO was new and fragile. And according to Møller, doing so would have been a complete disaster:

> BLM: We kept our own research priorities because we thought they were well thought out. But we adapted. Lots of other researchers working with plants who tried to fully accommodate fashionable political ideas are no longer here anymore. There used to be little or no money in it, right, and there has been no interest you see. (..) I mean the whole EU policy… it´s been a boomerang, right. Now we are really short in EU of the qualified university researchers required to provide the basic knowledge on how to best change into a bio-based society not based on fossil fuels. The research policy of EU has been a complete failure in the plant area. (Interview 2009)

Contrary to the ideas of Nowotny et al., *socially robust knowledge* may not be an adequate explanation for this particular case where legitimacy and status were acquired *despite* rather than *as a part of* social, economic, cultural and political contexts. “External” tests of validity that mingle non-scientific types of knowledge with academic knowledge was *not* the way forward for Møller and his colleagues when trying to make their research programs gain legitimacy and grow. Had they allowed the “test of validity” to take place *outside* rather than inside the laboratory, Møller would have gone out of business in the middle of the 1990s. Møller claims that, despite the many types of collaborations and the many kinds of interests his research has related to, he has stuck to the same three basic research programs for the last 30 years no matter which way the political winds were blowing and what public opinion said. This was done by strategically tasting, monitoring, discriminating, supplementing, countering imbalances and distributing resources. He did not isolate himself from the surrounding habitat; neither did he assimilate to it. He *adapted* - an evolutionary activity that requires the species to never give up its own interests and survival and never bow down to a greater common agenda set by larger actors in the ecology. His knowledge production is relational, but not in the way Nowotny et al. conceives of the term. Danish plant biology did not gain its success by turning itself into a context-sensitive, moldable and responsive research program developed in a "process of extension" (Nowotny 2003, p.155) between laboratory, politics and social world where the former assimilates and fits itself to the changes in the latter. What Møller tells us is rather that it developed by a form of adaptation which requires scientists to *remain* "in their study chambers" and do the research they feel is necessary, even in the face of massive political resistance. Instead of socially *robust knowledge*, we have here a case of politically and economically *robust scientists*. Møller, his colleagues and their three research programs have developed resistance to the toxic changes that may take place in their habitat. As a consequence, Møller's story allows us to turn Nowotny´s concept on its head:

> BML: You have to plan things and you have to think about the long term prospects. I like to learn and listen but I don´t like to be pushed around in my thinking, I like that I get to set some agendas here… yeah I personally like that, and frankly I think that's a good ability in this business… to have this ability
>
> BGH: But you can say that because you are a "big caterpillar", can´t you? (both laugh)
>
> BLM: Yes! And that means I have to have a lot of money in my "common box". A lot of food has to be consumed every hour of the day all days a week
>
> BGH: Yes evidently! But on the other side if you enter a difficult period you can produce the compounds you need on your own (both laugh)
>
> BLM: Yeah, we have become pretty robust now, I would have liked us to be this robust 15 years ago.
>
> (Interview 2009)

Nowotny claims that reliable knowledge is no longer "self-sufficient" or "self-referential". I would like to argue that, in light of the present example, self-sufficiency is precisely what has allowed Møller and his colleagues to survive when everyone else were forced to "give up". Self-sufficiency has been achieved by deploying strategies of tasting resources, self-supplementing industrial interest and countering imbalances in their available resources by allowing them to acquire new functions when needed. This is what has made Møller capable of growing to his present size and developmental stage, a stage where he is able to "set some agendas" rather than assimilate to those set by politicians, media, industry and funding agencies. Where Nowotny claims that knowledge production needs to be guided by a process of contextualization in order to become socially robust (Nowotny 2003, p.155), Møller's success as a scientist was conversely achieved by circumventing this process and doing the exact opposite: to hold on until everyone else gave up. Pro-Active Plants have grown to a different level of functioning where resources can be carefully examined before being consumed, where self-supply or *de novo* production of fundability can be obtained and where imbalances caused by external demands can be countered by using, sequestering, and storing resources for later distribution. The criteria remain those of the academic scientist, although of course supported by a change in the climate surrounding "green" futures and the promises they now seem to hold. In this way, the research program becomes not more but *less* dependent of politically directed agendas.

Invasive species

One of the more charismatic guest speakers in the conference launching the UNIK research project on synthetic biology was an American professor managing a department in bioengineering from Berkeley, California. The conference was attended by most of the researchers in the Nano-Science Center and Pro-Active Plants along with a number of researchers from several other Danish research environments[87]*. The professor, a large man with big gestures, was invited specifically for this occasion and gave his presentation: "Towards Scalable Synthetic Biology & Engineering Beyond the Bioreactor" right after dinner. Unlike most of the evening presentations that week, this particular lecture kept the crowd wide awake.*

The presentation itself was fast-paced and highly entertaining. Besides giving a lot of technical detail concerning the potential benefits of engineering biological systems, the professor spent a considerable amount of time deflating popular horror fantasies about synthetic biology. As examples of the irrational fears flourishing in the debate, he showed images from the science fiction movie "I Am Legend" (2007). In the movie a new vaccine against cancer based on a genetically modified rabies virus mutates into a contagious disease that modifies the human species into aggressive, werewolf-like monsters that threaten to wipe out the human race and take over the earth. The professor juxtaposed this account with a list of examples of how human interference with inter-species relationships and complex biological systems has been around long before the rise of advanced biotech. Old-school "Non-synthetic biology", he argued, was actually a much more risky endeavor than modern biotech, and there was no reason to romanticize a pre-synbio era of pure and uncontaminated relationships with nature. To illustrate the dangers of "non-synthetic biology" the professor introduced the example of the Cane Toad.

The cane toad is a very large and extremely adaptable toad that was deliberately imported to Australia for pest control in sugar cane fields. It was set free in the Northern territory in the middle of the 1930s. Unlike most other amphibians in Australia that live mainly of insects, the Cane Toad is much larger, tougher and eats

[87] The following account is put together from field notes taken in a conference taking place in Bornholm in the spring of 2009, and a copy of the American professors Power Point slides. As I have not been able to get into contact with the professor I have refrained from giving him a name in this description. Adding to this material is descriptions of the cane toad, its habitat, life cycle, distribution and impact in the Australian ecology found on the internet search.

pretty much anything it can swallow. Considering that it is the biggest exemplar of its species that´s quite a mouthful. Apart from a broad range of insects, including bees and termites, the species can live off household scraps, garbage, dried pet food, snails, eggs (even its own), small snakes and mammals, aquatic plants, and, in some cases, algae. It survives in temperatures ranging from 5-40 degrees Celsius, can manage a loss of up to 50% of its body water and is a prolific breeder. The toad has thick skin that is highly toxic in its entire life cycle and helps it ward off predators. As a noticeable side effect, the toad's skin has also shown to be capable of killing off a broad range of insects in the Australian ecosystem. It lives in a variety of habitats from forests and mangroves to sand dunes and thrives also in urban areas. Needless to say the cane toad has now been labeled an invasive species as it threatens not only competing amphibians but a mass of other life forms in the Australian ecosystem. The next slide was a graphic representation of the rapid distribution of the Cane Toad in Australia over the past 65 years. The professor is probably not the first to label the decision to import the species "the largest ecological disaster on the continent to date".

The rest of the lecture focused on showing how synthetic biology could help to design more intelligent and much safer ways to interfere with nature. The professor´s proposal was to make a large research center that brought together scientists from various disciplines to create a "true bioengineering science". This, he proposed would facilitate a more well-designed, and well-considered engagement with the human modifications of biological systems. He stressed that the engineering task in synthetic biology is already among the most complex known and highlighted the importance of "uncertainty management". The need to manage uncertainty in the engineering of new life forms and biological systems results in complex "design requirements" that scientists will have to invest much effort in getting right. Synthetic biology, he said, will need to manage "not only uncertainty within the designed system, but also its ecology". According to the professor, part of the complexity of synthetic biology is that the field will need to take such things as biodiversity and ecological complexity into account. From there, the lecture took off into a long array of examples of how a true bioengineering science would be able to create new and ground breaking solutions to problems in medicine, energy, climate, agriculture and marine environment protection by way of new and carefully designed biological systems and technologies.

The presentation stirred many of the attending European scientists. After the lecture as the participants gathered in the bar, several expressed that they found the rhetoric too flashy and the results unnecessarily hyped. One researcher even suggested that the concept of synthetic biology was turned by the professor into a mere buzzword that could mean almost anything. His focus on promoting synthetic biology and deflating misguided or misinformed fears towards the bioengineering area was much too crude, people argued. Most comments started by pointing to nationality and suggested that this kind of talk might work in the U.S., but in Europe scientists know *better. The professor's idea of "uncertainty-management" clearly did not include his own public appearance. His European conspecifics were not quite sure how to respond. Some were concerned about the rather invasive tone in which the presentation was given. One was even quite upset and said that this kind of public presentation could potentially damage the work of other scientists in the field, as it could upset a lot of people who are already not too happy with the idea of synthetic biology. Others argued that a scientist like that would not last a year in Europe because nobody would take him seriously. The professor himself seemed to have a good time though. However, he had important engagements elsewhere and had moved on the next day.*

Conclusion
- Biodiversity

I began this thesis by considering "the problem of mixture". The problem had to do with the question of how to analyze, or even make knowable, a practice that tends to contract a wide range of actors, sectors, norms, techniques and social groups into an apparently "indivisible whole". In this situation, as I argued, it is not enough to show *that* things are mixed and mingled. Mixtures must be turned into the starting point of inquiry, rather than the end point. I return to the mixture problem at this point to draw up the main points of the thesis and articulate what I see as the central contributions.

First, a word on the methodological contribution of the thesis. Taking an *ecological* approach to the study of science took me into the problem of conceptual-empirical mixtures. While being heavily influenced by the constructionist agenda of taking the "back door" to science as a practice I argued, by way of Jensen and Bowker (2011), that this "practice turn" in STS has as one of its side effects a cementation of the conceptual-empirical division. However, in the field concepts and practices blend seamlessly into one another. This became very clear from the beginning when Hans Müller Petersen suggested that what Denmark needs is a generation of *mode 2 scientists* and even suggested that science is about network formations rather than waiting for a stroke of genius. Arguing for a lateral, rather than a hierarchical approach to analysis I did *not* argue that concepts such as asymmetrical convergence, networks, trials of strength, boundary work or robust knowledge are wrong or inaccurate. Rather, I argued for a different *use* of the conceptual repertoires of STS. Rather than granting analytical or theoretical frameworks hierarchical position "above" the field, I placed theoretical and conceptual contributions of STS "alongside" concepts and practices of field participants. In this way, the experiences from fieldwork were not "put into context" or turned into examples of a higher order.

Michel Callon has pointed out, that science mixes up the cognitive and the social when engaging with particular problems and that this activity does not take place "in" a given social context but rather works to *produce* new contexts (1980 p.216). More importantly, and quite in line with the lateral approach to analysis, he also suggests that "the sociologist is caught up in the same situation as the scientist" (1980, p. 217). The account given here by me is thus, according to Callon, yet another translation, yet another attempt to *produce* a context for oneself and others and thereby engage in specific problematizations. However, for scientists as well as for those who study them these are not undisputable conclusions. Callon argues that they will be parasitized or translated by new attempts to problematize science. The descriptions given here are clearly not suggested to be downstream external commentaries and are by their experimental nature parts of a debate. By taking a lateral, rather than a hierarchical approach to analysis, contextualization becomes more obviously a problematizing and contestable endeavor. Lateral analysis is less a matter of finding the final downstream position of the predator/parasite and more about encounters that make us think again about the way we tell stories about what is happening to science.

By allowing the conceptual and the empirical to blend into one another, I suggested that lateral exchanges aimed at establishing a *reciprocal process of capture* between the field and its commentary. This is not suggested to be a way of "empowering" field participants (it should be clear by now that they are not in need of help here) or "giving voice" to scientists (they have plenty) without polluting their reflections with non-native analytical concepts. I am not claiming to have made a more accurate, faithful or correct description, and I am certainly not claiming to have taken the native's point of view. Møller has not suggested that I compare him to an insect and Bjørnholm does not think of his Power Point presentations as high resolution images made by scanning electron microscopy. Approaching scientists as *thinkers* and going by way of their conceptual vocabulary is not about accuracy or faithful representation. Rather, it is a route to produce articulate and *interesting* descriptions. I am then suggesting that there are such things as good and bad comparisons. Good comparisons invite the inquiry to go beyond both parties' capacity to categorize or recognize knowledge economy mixtures into well-rehearsed conceptual distinctions. Lateral experimentation is a way to make me think through knowledge economy, not categorize, recognize or anticipate the outcome of my inquiry in order to offer yet another exemplification of well known analytical distinctions; *yet another case study* (Berlieu et al. 2007). Laterally comparing insects, microscopes, power points, interview transcripts, and academic publications across the natural, social and life sciences is an experiment that has invited me to think *through the middle* of, rather than around, about or above my object of study. In short, it allowed a transformation of the analytical distinctions I proceeded by.

Some may argue that critical distance and taking a normative position is obscured by this ethos of thinking ecologically as it requires me to abstain from an outside position or a higher context from which to pose a normative judgement of the events of the field. However, I will argue, lateral approaches to analysis does in no way exclude critical inquiry. There are other routes to criticality than generalizing the binary vocabulary that has characterized both critical and celebratory accounts of science in the knowledge economy. By my lateral engagements with the field I inquired into the possibility of identifying new points of critique specific to the case at hand. Rather than reproducing a critique of boundary blurring, erosion and dissolution by drawing on a binary vocabulary I have looked for alternative problematizations. In the descriptions presented here the main problem is not one of facilitating integration or protecting a boundary between the university and its "outside".

The three lateral experiments in chapter four, five and six are thus contributions to science and technology studies that have hopefully stirred a bit in the binary divisions and step1/step2 narratives of STS and Danish research policy. As should be clear by now, these experiments did not merely claim that knowledge economy is a mixture and showed some strategic attempts to navigate within them. More importantly, each chapter articulated very specific descriptions of *how* things mix and *with what effect*. What kinds of indivisible wholes have I depicted here, then? And what costs and risks are involved in the specific kinds of mixture I have identified?

Searching for a vocabulary by which to articulate mixtures without dissecting and destroying them, I have drawn on Stengers' notion of *ecology* as a trope that can account for the *immanent mode of existence* of scientific practice in relation to the actors it mingles with. By starting out *in* mixtures and taking an ecological approach to the study of science I have experimented with looking at scientists as "species" in relation to "habitats" or "samples" in relation to "microscopes". Both kinds of comparisons are "ecological" (although only one is biological) in the sense that they approach university science as intrinsically mixed up with and inseparable from its environment. The strategic maneuvers of scientists were thus depicted as ecological relationships that had multiple and unpredictable effects. Through the three lateral experiments with tracing academic scientists' strategic attempts to adapt to new habitats of the knowledge economy, I arrived at the following three propositions:

1) Making academic science visible and fundable entails fitting it to specific gazes by highlighting promissory elements that display *feelable forces* in relation to existing perceptions of "society's problems". The problems perceived by other actors in the ecology of a scientific field are thus intrinsically bound up with the way research projects are fitted or shaped. In the particular case of Thomas Bjørnholm and the Nano-Science Center these perceptions were mainly guided by a consumerist gaze and concerns of a coming energy crisis, fitting research in chalk crystal surfaces to the view of oil-people in order to bring the chalk project into being. However, other environments may produce other gazes and thereby other modes of scientific visibility. I stressed that this process of acquiring visibility, goes hand in hand with a necessary *erasure* of details that do not make up promissory images. The *oversight* entailed in this strategy for acquiring visibility is not to be seen as a pollution of pure science or a distortion of the true representation of academic research. Rather, I suggested, oversight, enhancement and erasure are constitutive parts of the production of knowledge.

A pure and untouched field of research that is *not manufactured* to fit the gazes that fund and govern it cannot come into being; it is bound to be left in untouchable darkness. Thus, without modifications, manufacture and incorporations of a multiplicity of views and gazes, there would have been no chalk project. To use Stengers' term, the project had an *immanent mode* of existence and cannot be conceived in separation from its political and industrial environment.

2) This rather general observation: that scientists integrate a reference to other actors in their environment was explored in more detail in the case of science-industry relations in Pro-Active Plants. Inspired by my main informant's own biological vocabulary, I used the concept of *circumvention* to argue that the relation between academic science and industry does not necessarily take the form of a unidirectional flow from production to consumption of knowledge. I also showed how the skillful incorporation and *sequestration* of biotech industry within the organization of the research center was not an example of boundary work or boundary maintenance but of mutual dependencies and forms of mixtures that work to the benefit of the biologists. Rather than *converging* with industrial interests, Møller seemed to be engaged in managing productive *divergences* between Pro-Active Plants and the biotech company that help both reach their different goals. This, I argued was not an example of boundary work or hybridization of institutional orders. Symbiotic agreements are not about boundaries and hybrids but about mutual incorporations that allow each species to retain conflicting agendas, not converge into common ground.

3) The specific skills required in order to engage safely in such incorporations was explored in chapter six. Here, I showed how Møller and his colleagues used three strategies to deal with the fragility and dependency that characterize their ecological relationships with other actors in their scientific ecology. Here I focused mainly on the political resistance and governmental agendas that Møller felt had formed a toxic habitat for the research centers he managed through his career. I suggested that the strategy of *tasting*, *self-supplementing* and *countering imbalances* allowed Møller to discriminate between resources, produce *de novo* the industrial interests he lacked, and redistribute existing resources for new purposes within the collective body of his growing research center. Contrary to the idea of *socially robust knowledge,* the deployment of these strategies resulted in the development of politically and economically robust scientists who can allow them self to diverge from industrial and political agendas by creating their own resources.

The strategies for acquiring visibility, managing symbiotic divergences and becoming robust all highlight the ecological relationships between the university, industry and government. All of the lateral experiments have described relational strategies in that they all incorporate goals, requirements, codes and practices from the actors around them. Scientific practice, as I have described it, is not a functional part of a larger system whose goals it must adhere to but rather has its own immanent mode of existence intimately bound up with other actors in a dynamic, living ecology.

However, these skillful incorporations and transformations in relation to fundability-optics, commercial interest or political agendas form quite specific kinds of mixtures. A common thread running through all of the inter-minglings I have articulated in the previous chapters is that their constituents, once mixed, do not form a homogenous or uniform mixture. Also, no particular constituent or "ingredient" is able to drown out the others. The ingredients, in other words, do not converge into sameness. Rather they engage in unstable mixtures and continue to *simultaneously* merge and diverge. I can briefly illustrate this point with reference to some of the strategic maneuvers of Bjørnholm and Møller.

By "painting with the broad brush", Bjørnholm manages to make chalk research fit with agendas of the oil industry, while leaving the parts of nano-science that are not interesting to industry or government in the dark. Yet, a major part of the work done in the chalk project is about studying calcite surfaces, which is very much a basic science endeavor. Thomas suspects that the surfaces may be a new frontier for resource extraction but he needs to leave the technical details that could make such statements risky in the dark. By erasing or bleaching technical content that can produce uncertainty and distort a promissory image, Thomas is not merely hyping up basic science to comply with the insight-invoice trajectory. Leaving vital technical content and unpredictable basic science questions outside representation means leaving them outside direct intervention. Not everything about the chalk project is fitted to a consumerist gaze, only the part that acquires visibility. However, when receiving one of the largest private research grants in Denmark to date, it is the project as a whole that benefits, not just its visible parts.

Similarly, Møller and Pro-Active Plants are engaged in a symbiotic relationship with the in-house biotech company. The symbiotic relationship, as well, is characterized by the simultaneous presence of divergent agendas and mutual dependency. Without the company, biologists have to engage in time consuming development of technology platforms. Paradoxically, this symbiotic agreement allows for divergence between biology and industry in allowing biologists to "do biology, not technology". This is enabled by *incorporating* industry, not separating itself from it.

Lastly, becoming a "robust" scientist is not a matter of standing your ground in face of resistance. Møller had not survived had he neglected to integrate a reference to GMO resistance and proceeded without sensitivity to his environment. He had to find new ways of getting access to resources. His strategies for doing so were not about externalization or assimilation of the research program to corporate or governmental agendas. Møller never left his research interests and proceeded according to his own goals. Robustness is an ecological relationship, developed as fine-tuned and skillful incorporations of political or industrial agendas and managed by careful discriminatory strategies that paradoxically allow scientists to go their own way. Møller and the research center have adapted in a way that make the seemingly hostile habitat work to the benefit of their scientific species. By opening his own company when he needed an industry partner, Møller simultaneously adapts to the insights to invoice agenda by being entrepreneurial and diverged from it by refusing to stray from his own basic research interests even when society, politics and industry urged him to change trajectory.

Whereas the mixtures of knowledge economy have been conceptualized as *either* a cause for univocal celebration (we need more interaction, synergy and collaboration across sectors) *or* condemnation (we need to keep science free from corrosive interests) my work here allows me to conclude that these stark choices will make us miss the point. By showing how scientific practice is not an ivory tower and cannot be conceived of as separate from society, I am also arguing that it needs *neither* enforced integration *nor* stalwart protection from outside interests. In the same way that synthetic biology will need to take not just the object of study into account but its whole ecology, I am arguing here that STS scholars could benefit from studying ecologies. The analytical focus is better directed at mixtures than or constituent parts or functional units in a system. Accordingly, no general answer can be given to the question of whether knowledge-economy mixtures are "toxic" or not. As in any ecology, the answer depends on the evolving responses from the populations that inhabit it.

At this level of conclusion, I would like to suggest that the effects of new interrelations between the species that make up scientific ecologies are *not predictable.* Thus, also, they fail to conform to frameworks that base their normative position on the invocation of "higher order" explanatory concepts including academic capitalism, asymmetrical convergence, robust knowledge or mode2 knowledge production. Pointing to such higher order contexts anticipates a future which we cannot possible know and assumes a "pre-mixtured" past of pure science separated from society that seems less and less convincing. Adapting to the knowledge economy is an experimental endeavor which can have multiple outcomes and multiple "runaway" effects - for scientists as well as for those who study them.

Following this approach to analysis allows me to conclude that STS may benefit from proceeding with more subtlety and dare to engage in risky experimentation when inquiring into the effects of knowledge economy on academic research. In some cases, industry collaboration may indeed be poisonous, yet in other cases it such collaboration can provide nourishment for science. Invariably, this depends on ecological factors, including the central question of whether the relationship is organized as a symbiont or a parasite logic, whether the habitat affords adaptation or demands assimilation.

Is science "selling out" when painting with the broad brush and engaging in promissory imagery? The answer depends on whether or not suitable relationship between enhancement and erasure has been established. In the case of Thomas Bjørnholm and the Nano-Science Center this relationship seems to have worked well, feeding abundant resources into basic science research while making the Nano- Science Center visible as a place that contributes to solving society's problems. *Will the intensified focus on science-industry collaboration result in science being engulfed by capital interests and reduce it to an annex of corporate development?* It depends. In Møller's case industry collaboration was seen to have multiple purposes and was set up in a way that seemed to reverse the roles between consumers and producers. However, for the young and fragile, industrial funding may be dangerous resources and straying from basic science interests may damage their career. *Is a more flexible and externalized mode of knowledge production the key to develop of academic science in the future?* Not necessarily. This all depends on the habitat and the survival strategies it affords to academics. *Is the interaction agenda of Danish research policy good or bad for academic science?* It depends on which species you examine, their immediate scientific habitat and their specie-specific ability to mutate and adapt. For successful and highly adaptable scientists like Bjørnholm and Møller, the interaction agenda has afforded an environment in which they could work to make their research area visible, fundable and extremely robust. Conducted in large interdisciplinary research centers, their research programs now form collective bodies that can swallow and digest a broad variety of corporate and governmental agendas without being intoxicated. This collective body allows them to continuously expand their capacity to ingest new forms of resources: Møller can now attract, capture and consume grant money targeted for a wide variety of agendas, public, political and corporate, and still manage make things flourish.

By looking at the strategies deployed by these successful scientists and research centers, I have further argued that a policy agenda designed to harvest the resources of knowledge production by steering scientist towards more "interaction" and contextualization of their research interests has not merely produced the desired *insights to invoice* effect. Rather, I have shown, several "runaway effects" or performative "misfires" (Wright and Shore 2011, Butler 2010) where policy initiatives and the concepts they draw on create inversions and surprising responses. I would like to suggest that the interaction agenda of Danish research policy has produced much *more* than it can control.

Møller knows all about this kind of dynamic from crop science. Every time scientists make a new modified plant or a new insect repellant, insects and pathogens adapt within a period of ten years. I have suggested that, seen as part of an ecology of practices, plant GMO research as a scientific "species" may relate to research policy analogous to the way specific insects relate to changes in their habitat or feeding plants. New defense compounds, repellants or modification of their resources designed to, as Møller says, "push them around in their thinking" and guide their behavior have possible misfires, mutations and runaway effects at their base. Research policy, in this particular case, does not seem to proceed by performative extension of ideas into scientific practice, rather they become parts of ecologies where they are overgrown, cross-talked, made useful, attacked and transformed by proactive responses from a broad range of actors[88].

As we have seen, the scientists I have studied are part of highly adaptable scientific species capable of mutating and forming larger collective bodies in order to turn changes in their habitat around and make them work to their advantage. They are proactive agents and cannot be counted upon to follow the will of the ministerial experimenter or the performative power of policy documents. Whether the "laboratory" is placed in a Danish university or in the Danish Ministry of Science, Technology and Innovation, this relational proactivity is a force to be reckoned with.

However, the ecological approach requires more than hailing the project baron and showing the success of the strategies deployed by scientific survivor-gladiators. As Stengers notes, ecology is not just a scientific concept, it is also a political one. Both Møller and Müller Petersen have no problem with manipulating biological and academic systems and their ecologies; they strongly believe that the systems they experiment with, biological or academic, need improvement and intervention. Their business is not ecology but rather the optimization of resource extraction. The policy makers who designed the changes in Møller's habitat did so without paying much attention to the existing complex interrelations that were already working in his scientific ecology. Shifting Danish Governments and policy makers have had no problem with manipulating the conditions for the survival of a broad range of research areas. Just as synthetic biology is not afraid of modifying species or experimenting with life forms and biological systems, the Ministry of Science, Technology and Innovation sees no problem in modifying the resources and habitats that make up scientific ecologies. However, there is one very apparent difference between the scientific "design" of biological systems and the policy "design" of university research. Namely their respective relationship to what people in the field of synthetic biology would call "uncertainty management".

Møller and Bjørnholm's scientific practice is restricted by massive political resistance, changing regulations, and a large number of legal and practical restrictions on their work. Laboratory safety rules, prohibitions for planting modified

[88] And once again, Callon 1980 would agree here.

crops, changing legislations, nano-toxicology, bad press, religious beliefs, skeptical investors are all recalcitrant actors that their experiments integrate a reference to. By contrast, the experimenters of Danish research policy do not seem bound by much formal responsibilities towards the scientific "species" whose habitats they have so radically reformed.

When policy is guided by the unrestrained diffusion of ideas like those of *knowledge economy* or *mode 2 knowledge production,* it deploys dualist thinking as the basis for university reforms. This binary vocabulary was explicitly set free around 2001 in the territory of Danish universities by way of a series of reforms and thus entered an ecology consisting of a complex mixture of many immanent modes of existence. As shown over many years of science studies as well as in this thesis, the practice of science is ill-fitted to such monolithic abstractions and dualistic thinking. According to the descriptions I have presented here, the mutual dependencies between, for example, science and industry rest on fine tuned processes of adaptation and divergence. These processes and symbiotic connections are, however, completely overlooked by the crude attempt to facilitate interaction between the two and create unidirectional flows from insights to invoice. An ecological perspective would require that we leave behind the idea that academic science is primarily or solely the raw material for wealth creation or an empty frontier "outside" society.

As a scientist, Møller would never be allowed to make such radical intervention in the ecology making up Danish fields or forests. He would need to go through a long array of security checks and risk assessments before he gets a chance to see his modified cassava-plants grow in the wild. He would need to take seriously the task of "uncertainty management"; ceaselessly documenting possible risks and anticipating any unforeseen effects of his research. He would need to take not just the immediate biological system but its whole ecology into account. And despite the current success of his research program, he continues to be in ongoing "warfare" with a mass of institutions, social groups and citizens who would like to see even more restraints on his experiments, if not shut them down completely. If Møller and his research program proceeds without integrating a reference to the ecologies of biological systems *as well as* those of scientific practice, he would probably not last long. In the UNIK conference lecture on synthetic biology, the American professor made an attempt to integrate a reference to the fears and resistance surrounding synthetic biology. However, according to the professor´s European colleagues, this attempt was not fine-tuned enough and failed to establish a proper ecological relationship to the environment it was given in. At best, they claimed, it would put him out of business and at worst it would damage the rest of the ecology making up the field. Møller's research center has worked its way through evolutionary trial and error and had adapted well. It is by now highly specialized in surviving in exactly this hostile habitat and seems to have mutated into an impressingly large and robust exemplat of its scientific species, capable of turning resistance into advantages.

The changing Danish governments and policy makers are bound by no similar constraints. Policy changes can be planned and executed *from the ivory tower of the Ministry*, although nobody knows what the proposed modifications will do once they enter into scientific ecologies. As research policy becomes occupied with finding new ways of treating knowledge production as a resource to be exploited, it seems crucial to find ways of articulating the ecological implications and discuss the need to responsibility required in this kind of experimentation.

Thomas Bjørnholm made a champagne toast in the absence of colleagues who left the building on the same morning after having their entire research area shut down. Møller's case made clear that, unfortunately, the success of Pro-Active Plants rests partly on the fact that a large number of competitors were forced to give up. The fact that Møller and Bjørnholm are part of scientific species that survived and succeeded does not give this thesis a happy ending.

What of the species of researchers who, as Møller says, "are no longer there" and cannot tell the story of how a whole generation "died" as the result of changes in the habitat? What of the young and fragile research areas that do not enjoy the privilege of being part of collective bodies – areas that have not gained "critical mass". What of the researchers that do not have the option of fleeing to the protecting shelter of a powerful "*project baron"*? And – I have to add – what of the social scientists and humanities scholars who by the nature of their work and the way it is organized are not able to deploy the same strategies? In the social sciences and humanities research has traditionally not been organized in large scale projects, tied together by expensive instrument-infrastructures or published in journal articles with up to 20 authors that all benefit from citations. Not all academic species can rapidly adapt to form large, robust bodies that gradually adapt to consume a broad variety of resources without ever changing their core research interests. Not all researchers can gain the kind of robustness acquired by Møller and his colleagues, simply by the nature of their work and their disciplinary habitat. In fact, the strategies outlined here constitute a rare exception expressed in a highly specialized and impressive example of adaptation.

So, to put it bluntly, the ecological approach to studying science in the knowledge economy makes me suspect that the interaction agenda of Danish research policy may have as a side-effect the evolution of a scientific "cane toad". Scientists like Møller and Bjørnholm could be the first symptoms that ten years of reforms and modifications towards more interaction is now resulting in adaptations whose effects we are yet to learn about. From the descriptions presented here it seems that the interaction agenda has modified the ecologies of university science to now include a few impressingly robust scientists who can consume a greater variety of resources and who are much larger than their competitors. The previously mentioned observation that Danish university research is being divided up into project barons and ball-fetchers may point to the rise of "invasive species" in scientific ecologies. The project barons of today may become the scientific "cane toads" of tomorrow. As a science and technology studies researcher with aspirations to become a political ecologist, I feel the need to ask what kind of uncertainty management would be required of Danish research policy in order to protect the biodiversity of academic science.

English summary

The thesis *Adapting in the Knowledge Economy* investigates the strategies deployed by academic scientists when trying to adapt and maneuver within an increasingly complex mixture of scientific, industrial and governmental agendas. Chapter one “From insights to invoice” summarizes the last decade of Danish research policy as a tendency towards intensified focus on *interaction* between the university and “outside” actors. Looking at Danish policy documents and interview data the chapter shows how policy changes responded to an idea of “ivory tower” researchers isolating themselves in Danish universities. Furthermore, the interaction agenda was motivated by the perception that knowledge was produced but not sufficiently *used*. Strongly influenced by the concept of *the knowledge economy* and that of *mode 2 knowledge production,* policy changes were directed at bridging a gap between the producers and the consumers of knowledge. A series of reforms and initiatives were launched to facilitate more interaction between science and industry as well as more responsiveness towards societies’ problems on behalf of the universities. This interaction agenda was coupled with an increase in the economic investment in research and an increased focus on competition between researchers in order to ensure high quality in knowledge production.

Chapter two “Conceptualizing mixtures” reviews two positions in Science and Technology Studies (STS) to look for an entry into analyzing the complex mixtures of scientific practice. A broad range of science studies have shown how the study of science poses a *mixture problem*. Scientific practice is characterized by its ability to draw together institutions, norms and practices that are normally considered separate spheres of reality. The first position is constructivist and argues that science was always a mixture. Constructivist science studies have shown how scientific practice destabilizes scales between science and industry, laboratory and society, by way of networked practices. This position promotes a vision of science as a heterogeneous field of practices that does not conform to major moral, institutional or normative fault lines. The second position is more critical and argues that the rise of neoliberal science policy has introduced a tendency towards convergence between industry and academia, thus corporatizing scientific practice and eroding the freedom of research. Juxtaposing the literature with examples form fieldwork in a Danish research center, the chapter shows that the practice of field participants is far more heterogeneous and mingled than assumed in the literature. The chapter identifies a need to develop a mode of analysis that takes mixtures as its *starting point* rather than an *outcome* of historical changes or network formation.

Chapter three “Lateral analysis” develops an analytical approach to cope with the mixture problem in science studies. Drawing on the philosophy of Isabelle Stengers it suggests an *ecological* approach to the study of science; studying scientists as a “species” that exhibit an immanent mode of existence in complex mutual relationships with a larger ecology or habitat. Ecology is suggested as a way to allow analysis to start out in mixtures and mutuality rather than in binary vocabularies. However, the ecological approach also tampers with the relationship between the researcher and the field in that the former must be viewed as an intrinsic part of the ecology and not an external commentary. As the study of academic scientists is as much a study of *thinkers* as it is a study of practitioners, the chapter argues for rethinking the distinction between the conceptual and the empirical (Jensen and Bowker 2011). The chapter contrasts a *hierarchical* approach to analysis with a *lateral* approach and advocates for the latter. The hierarchical approach assumes the analytical framework of science studies to posses a higher level of abstraction than that of the scientists studied. This hierarchical approach is made sense of as a “Wolf´s game” devouring data without being transformed by it (Serres 1979). As an alternative, the chapter suggests a lateral approach to analysis (Strathern 1999, Maurer 2005). Lateral analysis draws together inspirations from STS, philosophy and social anthropology to suggest a mode of analysis where concepts and data are placed alongside each other in lateral juxtapositions, rather than as hierarchical scales or *contexts* which order their relationships. Substituting the question: *What is this an example of?* with the question: *What is this comparable to*? allows for such an inquiry. Adding the conceptual repertoires of scientists to those of science studies, and allowing for analogies and comparisons to emerge between them without hierarchical imposition, is suggested as a way to bring out new flavors and analytical distinctions in science studies. Consequently, the conceptual language of field participants is considered capable of reflecting back on the conceptual language of science studies rather than the other way around. The chapter makes sense of the lateral back and forth movement between the conceptual and the empirical as a “game of cat´s cradle” (Haraway 1994). This experimental approach proceeds by comparing conceptual and empirical practices of field participants, using the academic work of natural scientists as an analytical framework by which to analyze their managerial strategies. This is suggested to allow science studies to think again about the way STS and research policy conceptualize and order scientific practice.

Chapter four, five and six all constitute such lateral experiments as they articulate the strategies deployed by two very successful scientists and the research centers they manage. Chapter four “Feeling to see – oversight in nano science” maps the strategies of visibility deployed by a Danish research center in the field of nano science. By comparing scientific self-representation with the workings of an electron microscope, the chapter demonstrates how the practice of making science visible and the practice of making nature visible entail similar costs. Jumping scales between laboratory and society is shown to involve costs similar to those of shifting from a human mode of vision to a nano scale mode of vision. Similar to making a sample visible to an electron microscope, the chapter argues that in order to make a specific research project visible, specific elements have to be enhanced, while others need to be erased. Drawing on the work of Bruno Latour, the notion of *oversight* guides the inquiry. Oversight shows that specific types of promissory visibility depend on a process of manufacturing the representation of scientific research in which highly technical or non-promissory details need to be erased. The chapter stresses that this is not a matter of corrupting or distorting an unmediated object but rather suggests that the process of manufacturing promissory self-representations is constitutive to scientific practice.

Chapter five "Beyond the boundary" takes the inquiry into the area of plant biology, studying the strategic maneuvers involved in managing science-industry relationships. In the STS literature these relationships are often analyzed by concepts such as *boundary* and *hybrid*. However, drawing on a biological vocabulary, the chapter argues that scientific practice exhibits more subtle dynamics beyond the analytical capabilities of these concepts. Using a field participant's research on plant-insect relationships, the chapter suggests the concept of *symbiosis* as a way to analyze the mixtures and connections between a university research center and an in-house biotech company. The symbiotic agreement entails a simultaneous process of mutual dependency and blatant divergence. Just as plants and insects make themselves useful to each other by engaging in a co-evolutionary "chemical warfare", the research center and the biotech company are engaged in an "organizational warfare" where divergent interests prove to be beneficial for both parties. The chapter suggests that a one-sided focus on bridging gaps or facilitating *interaction* misses the fine tuned symbiotic agreements between science and industry. When such productive divergences are demanded by policy to transmute into a unidirectional flow of resources from the producers to the consumers of knowledge this symbiont logic is jeopardized. By *circumventing* the flow from production to consumption of knowledge, the research center makes as much use of the biotech company as the other way around. Also, managing relationships to industry does not take place by maintaining a boundary between science and entrepreneurship. Rather, science-industry relations are made productive by *incorporating* and *sequestering* the biotech industry into the organization of academic science, facilitating divergences and divisions of labor that work to the benefit of both parties.

Chapter six "Robust scientists" takes the biological analogy further as it makes use of plant-insect relations to discuss the concept of "socially robust knowledge" (Nowotny 2003). In STS, Helga Nowotny has argued that by leaving the *ivory tower* and externalizing their research, scientists will gain more support and recognition and will succeed far better. Drawing on interviews with a plant biologist working in the area of GMO, matters seem a bit more complex than that. The chapter argues that although relational skills and adaptability seems to be at the heart of scientific survival, it is precisely by being self-sufficient and *not* dependent on political or industrial interest that this specific research group survived decades of political resistance and scarce resources. Comparing the development of a research group to the life cycle of a toxic moth studied by the scientists, the chapter argues that rather than socially *robust knowledge*, the response to new policy habitats have resulted in politically and economically *robust scientists*, capable of surviving in hostile habitats and of feeding on "toxic" resources. Scientific robustness is acquired by way of three strategies: 1) *tasting* and discriminating between resources so as to avoid funding that erodes academic profiles and push scientists away from their core interests, 2) developing a *self-supply* of industry interests by opening a company, thus getting access to funding that requires an industry partner without having to assimilate to industrial or political agendas, and 3) *balancing resources* within a larger collective body of researchers, thus finding multiple purposes within the acquired pool of resources to counter the inconsistencies and imbalances in resource influx that come from policy changes or shifting political and industrial interests.

The thesis concludes that the mixtures of knowledge economy as they play out in the practice of these highly successful scientists are not mainly a matter of convergence, corruption or erosion of academic research by capital and government interests. Rather than a problem of either bridging a gap between science and society, or maintaining a boundary between academy and capital, the problem seems to be how to safely manage scientific ecologies where actors coexist in fine-tuned processes of co-evolution, symbiotic relationships, and productive divergences. The thesis argues that by deploying a crude binary vocabulary on scientific practice, STS scholars and policy makers overlook vital subtleties that make these relationships work. Furthermore, the thesis concludes that the greatest concern is not necessarily the erosion of boundaries but rather the unanticipated mutations that scientific species undergo when adapting in the knowledge economy. The interaction agenda of Danish research policy is thus criticized for being a rather irresponsible experiment with scientific ecologies, threatening a vast majority of academic researchers with extinction by introducing hostile and competitive habitats not necessarily suited to their species. The unexpected consequence is that a few highly adaptable scientists survive these habitats by mutating into robust, large and successful exemplars of their species, capable of consuming a broad range of resources. These new and potentially invasive species could potentially threaten the *biodiversity* in academic environments and hence undermine the potential resource offered by knowledge production as it depends on a board variety of creative potential. The thesis suggests that policy makers step out of the *ministerial ivory tower* in order to dedicate more attention to the intricate workings of scientific practice so that further experiments with the university sector are guided towards protecting academic biodiversity.

Danish Summary

Afhandlingen *Tilpasning i Vidensøkonomien* undersøger de strategier, som anlægges af forskere i deres forsøg på, at opbygge et forskningsområde i en tid, hvor videnskabelig praksis indgår i nye blandingsforhold med industrielle interesser og politiske dagsordner. Kapitel et "From insights to invoice" opsummerer de sidste 10 års Dansk forskningspolitiks fokus på "samspil" mellem universiteterne og aktører "udefra". Kapitlet viser hvordan forskningspolitiske forandringer var motiveret af en betragtning om, at universitetsforskere isolerede sig selv i et akademisk "elfenbenstårn". Forskningspolitikken var ydermere drevet af en stærk motivation for at sikre at viden bliver anvendt kommercielt. Under indflydelse af begreber som "vidensøkonomi" og "*mode 2 vidensproduktion*" forsøgte forskningspolitikken at bygge bro mellem producenter og forbrugere af viden. En serie af reformer og nye initiativer blev iværksat for at finde nye veje mellem forskning og erhverv, samt mere samspil mellem sektorer. Denne "samspils-dagsorden" blev koblet med en øget satsning på viden i from af flere bevillinger til forskning og et øget fokus på konkurrence mellem forskere. Begge dele var tænkt som en mulighed for at sikre en høj kvalitet i forskningen og dermed styrke den nationale konkurrenceevne på det globale marked.

Kapitel to "Conceptualizing mixtures" gennemgår to positioner i videnskabs- og teknologistudier for at finde en indgang til at analysere vidensøkonomiens komplekse blandingsforhold. En bred vifte af teorier har vist hvordan videnskabelig praksis rummer et "blandings-problem" idet den sammentrækker institutioner, normer og arbejdsgange som normalt begrebssættes som tilhørende forskellige verdener eller sektorer. Den første position er konstruktivistisk og fremfører, at videnskaben altid har været en kompleks og heterogen blanding. Konstruktivistiske videnskabsstudier har vist hvordan videnskabelig praksis destabiliserer skalaer eller grænser mellem f.eks. videnskab og industri eller laboratorium og samfund, gennem etableringen af heterogene, sociotekniske netværk. Denne position ser således videnskaben som et heterogent praksisfelt, der ikke kan kortlægges ud fra prædefinerede moralske, institutionelle eller politiske skel. Den anden position er mere kritisk anlagt og argumenterer for, at universitetsforskning i stigende grad konvergerer med industrielle praksisser så den bliver gradvist mere "corporate". Dette ses som et resultat af en neoliberal drejning i forskningspolitikken. Ved at blande litteraturreviewet med eksempler fra et etnografisk feltarbejde i et dansk forskningscenter, viser kapitlet, at de involverede forskeres praksis i mange henseender er mere heterogen og kompleks end som så. Kapitlet identificerer et behov for, at udvikle en analytisk tilgang, der tager udgangspunkt i komplekse blandingsforhold. Disse må ses som en præmis for videnskabsstudier, snarere end et resultat af historiske processer eller netværksformationer.

Kapitel tre “Lateral analysis” udvikler en analytisk tilgang til håndtering af “blandingsproblemet” i STS. Inspireret af Isabelle Stengers’ filosofi foreslås en *økologisk* tilgang til studiet af forskere som “arter”, der hver har deres eget “immanente eksistensmodus” og indgår i et komplekst forhold med en større økologi eller habitat. At tænke i økologier er ensbetydende med, at tage udgangspunkt i blandinger og gensidige forhold, snarere end i dikotomiske eller binære distinktioner. Den økologiske tilgang rokker dog også ved relationen mellem forskeren og feltet idet videnskabsstudier ikke kan ses som en ekstern kommentar til feltets praksis, men må anskues som en integreret del af den videnskabelige økologi den studerer. Studiet af universitetsforskere som praktikere er i høj grad også et studie af *tænkere.* Derfor argumenterer kapitlet for en nytænkning af sondringen mellem det konceptuelle og empiriske (Jensen and Bowker 2011). Kapitlet modstiller en *hierarkisk* tilgang til analyse med en *lateral* tilgang og argumenterer for sidstnævnte. En hierarkisk tilgang til analyse antager at videnskabsstudier besidder et højere abstraktionsniveau end det der findes i feltet. Dette forstås som et “Wolf’s game” (Serres 1979), hvor forskeren fortærer sine data uden at lade sig transformere af dem. Som et alternativ foreslår kapitlet en lateral tilgang til analyse (Strathern 1999, Maurer 2005). Lateral analyse samler inspirationer fra STS, filosofi og social antropologi til en analytisk tilgang, hvor begreber og data er placeret ved siden af hinanden i laterale sammenstillinger, snarere end som hierarkiske skalaer eller “sammenhænge”. En sådan undersøgelse faciliteres ved at erstatte spørgsmålet: *Hvad er dette et eksempel på?* Med spørgsmålet: *Hvad er dette sammenligneligt med*? Ved at sætte informanternes videnskabelige begreber sammen med videnskabsstudiernes konceptuelle sprog, uden at foretage en hierarkisk prioritering mellem disse, frembringes nye smagsoplevelser og analytiske sondringer i studiet af videnskabelig praksis. Informantens begreber antages at være på samme niveau af realitet som forskerens og derfor i stand til at transformere og gentænke dennes begrebsmæssige distinktioner. De laterale udvekslinger mellem det konceptuelle og det empiriske forstås som et “game of cat’s cradle” (Haraway 1994). Denne eksperimenterende tilgang har som sin procedure at sammenligne informanternes konceptuelle og empiriske praksisser. Informanternes akademiske arbejde anvendes som et analytisk værktøj til at analysere deres strategiske og ledelsesmæssige manøvrer. Denne tilgang tillader videnskabsstudier at gentænke den måde, STS og forskningspolitik konceptualiserer og organiserer videnskabelig praksis.

Kapitel fire, fem og seks udgør alle sådanne laterale eksperimenter idet de artikulerer to meget succesfulde forskeres strategier i forbindelse med ledelsen af større forskningscentre. Kapitel fire "Feeling to see – oversight in nano science” viser hvordan et dansk forskningscenter for nanoscience gør sig selv synlig gennem specifikke strategier. Ved at sammenligne videnskabelige selv-repræsentationer med hvordan et elektronmikroskop virker, viser kapitlet, hvordan det, at gøre videnskab synlig, og det, at gøre naturen synlig indebær samme slags omkostninger. At destabilisere skalaer mellem laboratorium og samfund foreslås at involvere samme slags omkostninger som dem der karakteriserer et visuelt zoom til nano-skala. På samme måde som et elektron mikroskop fremhæver visse elementer og udvisker andre, argumenter kapitlet for, at repræsentationen af et forskningsprojekt involverer både fremhævelse og udeladelse. Her anvendes Latour's begreb *oversight* til at foreslå, at synlighed er noget der produceres simultant gennem fremhævelse og udeladelse. Dette er ikke et spørgsmål om at korrumpere eller forvrænge den videnskabelige virkelighed, snarere er kunsten, at fremstille lovende selv-repræsentationer en grundlæggende præmis for videnskabelig praksis.

I kapitel fem "Beyond the boundary" begiver afhandlingen sig ind i plantebiologien for at studere de strategiske manøvrer, der er involveret i at lede samarbejdet mellem videnskab og industri. I videnskabs- og teknologistudier bliver disse relationer oftest analyseret ved hjælp af begreber som *grænse* og *hybrid*. Det viser sig dog, at et biologisk ordforråd rummer begrebslige alternativer der er langt mere subtile og bedre i stand til at rumme kompleksitet. Ved hjælp af en informants forskning i kommunikationen mellem planter og insekter foreslås *symbiose* begrebet som en måde, at analysere blandinger og forbindelser mellem et universitets forskningscenter og et biotek firma placeret på universitetet. Symbiose er karakteriseret af såvel gensidig afhængighed som åbenlyse interessemodsætninger. Ligesom planter og insekter gør sig selv nyttige for hinanden gennem "kemisk krigsførelse" gør forskningscenteret og biotek firmaet sig nyttige for hinanden gennem "organisatorisk krigsførelse". I begge tilfælde gavner interessemodsætninger begge parter. Et ensidigt fokus på at bygge bro mellem videnskab og industri overser den subtile symbiotiske overenskomst mellem videnskab og industri. Når interessemodsætninger forsøges ophævet eller omgjort til et ensrettet flow af ressourcer fra videnskaben til industrien bringes den symbiotiske produktivitet i fare. Ved at omgå dette ensrettede flow får forskningscenteret lige så meget nytte at firmaet som den anden vej rundt. Ydermere er ledelsen af industrisamarbejde ikke et spørgsmål om at opretholde en grænse mellem videnskab og entreprenørskab. Samarbejde mellem videnskab og industri bliver netop produktivt ved at *inkorporere* og opdæmme biotek industri i den akademiske organisation. I den præsenterede case faciliterer disse strategier arbejdsdelinger og modsætninger der er gavnlige for begge parter.

Kapitel seks "Robust scientists" fortsætter den biologiske analogi ved at gøre brug af plante-insekt relationer til, at reflektere over begrebet "social robust viden" (Nowotny 2003). I STS, har Helga Nowotny argumenteret for, at forskere ville få mere medvind, anerkendelse og succes ved at forlade elfenbenstårnet og eksternalisere deres interessefelt. I en række interviews med en plantebiolog der arbejder med GMO, bliver dette billede dog mere komplekst. Selvom relationelle færdigheder og tilpasningsdygtighed er en af grundstenene i videnskabelig overlevelse, viser det sig netop at være selvtilstrækkelighed og uafhængighed af politiske og industrielle interesser, som har gjort dette forskningscenter i stand til at overleve, trods politisk modstand og knappe ressourcer. I kapitlet sammenlignes et forskningscenters udvikling med udviklingsstadierne hos et giftigt insekt, som studeres i informanternes laboratorium. Der argumenteres for, at forskernes tilpasning til ny forskningspolitik har resulteret i politisk og økonomiske robuste forskere snarere end social robust viden. Forskningscenteret lader til at være i stand til at overleve i fjendtlige habitater og leve af giftige ressourcer. Videnskabelig robusthed opnås gennem tre strategier: 1) at ”smage” på, og skelne mellem ressourcer, for at undgå forskningsmidler der eroderer forskernes akademiske profil og leder forskningscenteret væk fra dets interesseområde, 2) at blive selvforsynende med industrielle interesser ved at åbne et spin-off firma og således få adgang til forskningsmidler, der forudsætter en industripartner, uden at være tvunget til at assimilere sig til industrielle eller politiske dagsordner, og 3) at ophobe, og distribuere, ressourcer indenfor forskerkollektivet for at modvirke inkonsistens og ubalance i flowet af forskningsmidler, som forsages af skiftende industrielle eller politiske interesser.

Afhandlingen konkluderer, at problemet ved vidensøkonomiens blandingsforhold sådan, som det former sig for disse meget succesfulde forskere, *ikke* handler om hvordan kapitalinteresser og politiske dagsordner korrumperer den akademiske forskning. Problemet er ikke at finde nye veje mellem forskning og erhverv. Ej heller er problemet at opretholde en grænse mellem universitetet og industrien. Problemet er snarere, hvordan man sikkert forvalter videnskabelige økologier, hvor et væld af aktører sameksisterer i fintunede co-evolutionære processer, symbiotiske relationer og produktive modsætninger. Ved at gøre brug af et grovmotorisk binært analyseapparat overser videnskabsstudier og forskningspolitiske aktører de vitale og subtile dynamikker der får videnskabelige miljøer til at blomstre. Snarere end at gå i panik over den tilsigtede udviskning af grænserne mellem universitetet og udefrakommende interesser, bør man nøje overveje de utilsigtede konsekvenser af samspils-dagsordnen. Her tænkes især på de kreative mutationer som videnskabelige arter gennemgår når de tilpasser sig vidensøkonomien.

Samspils-dagsorden i Dansk forskningspolitik kritiseres således for at være et temmelig uansvarligt eksperiment med den videnskabelige økologi. Ved at introducere ufremkommelige og kompetitive miljøer gøres en bred vifte af forskere til truede arter. Den utilsigtede konsekvens lader til at være, at et fåtal af yderst tilpasningsdygtige forskere overlever i disse miljøer ved at udvikle sig til robuste, store og succesfulde eksemplarer af arten. Denne nye type forskerkollektiv er i stand til at konsumere et bredt udvalg, og en stor mængde, af de tilgængelige forskningsmidler og kan potentielt true biodiversiteten i dansk universitetsforskning. Det antages at en konkurrencedygtig vidensproduktion beror på en bred variation af kreativt potentiale. Afhandlingen foreslår at forskningspolitikken træder ud af sit *ministerielle elfenbenstårn* og bliver mere opmærksom på den videnskabelige praksis’ komplekse sammenhænge. På denne måde kunne yderligere eksperimenter med universitetssektoren i højere grad sigte mod at beskytte den akademiske biodiversitet.

References

Amsterdamska, O. (1990): “Surely you are Joking Monsieur Latour”. *Science, technology and human values,* vol. 15 no. 4, pp. 495-504.

Andersen, P. B. (2006): *An Insight into Ideas Surrounding the 2003 University Law – Development contracts and management reforms*, Working Paper 4 on the University Reform. Published online by the Danish University of Education.

Andersen, H, Henningsen, I. (2009): Videnskabens Baroner og Boldhentere. *Universitetsavisen,* July 16th.

Austin, J.L. (1962): *How to do things with words*, Oxford University Press, Oxford.

Beaulieu. A., Scharnhorst A., Wouters, P. (2007): “Not another Case Study – A Middle-Range Interrogation of Ethnographic in the Exploration of E-science” *Science, Technology and Human Values* 32 (6): pp. 672-93.

Bergson, H. (1998): *Creative Evolution,* Dover Publications, New York.

Boden, R., Wright, S. (2010): *Follow the Money*, Working paper on the university reform, no 16. Danish University of Education.

Boll, K. (2011): “The Segmentation Model. *Representing and performing the taxpayers. An analysis of the segmentation model with inspiration from Ian Hacking and Donald MacKenzie”, In: The Construction of Tax Compliance Practices, unpublished PhD Thesis.*

Bowker, G.C. (1994): *Science on the run: Information management and industrial geophysics at Schlumberger*, 1920-1940, MIT press.

Bowker, J., Star, S.L. (2000): *Sorting Things Out – Classification and its Consequences,* MIT Press.

Brown, S.D. (2002): Michel Serres, Science, Translation and the Logic of the Parasite. *Theory, Culture and Society,* 19(3), pp. 1-27.

Brown, S.D. (2004): Parasite Logic, *Journal of Organizational Change Management* 17(4), pp. 383-395.

Brown, S. D., Stenner, P. (2009): *Psychology Without Foundations – History, Philosophy and Psychosocial Theory,* Sage London.

Brown, S.D (2011): A Topology of the Sensible – Michel Serres Five Senses. Forthcoming in *New Formations*.

Brown, N., Rappert B., Webster, A. (2000): *Contested Futures – A sociology of prospective techno-science*, Ashgate Publishing, Aldershot.

Butler, J.D. (2010): Performative Agency, *Journal of Cultural Economy,* 3(2), pp. 147-161.

Callon, M. (1980): Struggles and negotiations to define what is problematic and what is not – the socio-logic transformation. In Knorr, K.D. Krohn, R and Whitley, R. (eds): "*The social process of scientific investigation.* Sociology of the Sciences, Vol. 4, pp.197-219, D. Reidel Publishing Company.

Callon, M. (1986a): "Some Elements of a Sociology of Translation: Domestication of the Scallops and the Fishermen at St Brieuc Bay" in Law, J. (ed.), *Power, action and belief: a new sociology of knowledge?* London, Routledge, pp.196-223.

Callon, M. (1986b). 'The Sociology of an Actor-Network: The Case of the Electric Vehicle'. Mapping the Dynamics of Science and Technology. Callon, M., Law, J. and Rip, A. (Eds). Macmillan Press, London: 19-34.

Callon, M. (Ed.). (1998). *The Laws of the Markets. Sociological Review Monograph*: Blackwell Publishers.

Callon, M. (2010): Performativity, Misfires and Politics, *Journal of Cultural Economy*, 3(2) pp. 163-169.

Callon, M, Latour B (1992): "Don´t Throw the Baby Out with the Bath School – a reply to Collins and Yearley", In: Pickering, A (ed.): *Science as Practice and Culture*, The University of Chicago Press, London.

Casper, M.J. 1994, "At the Margins of Humanity: Fetal Positions in Science and Medicine", *Science, Technology and Human Values,* vol. 19, no. 3, pp. 307-323.

Christensen, J. G. Pallesen, T. (2003): "Ledelse og administration på offentlige universiteter og forskningsinstitutioner", *Nye Nordisk Administrativt Tidsskrift,* 84, (2), pp. 115-140.

Collins, H, M, Yearley, S. (1992): “Epistemological Chicken”. In: Pickering, A. (ed.): *Science as Practice and Culture*, The University of Chicago Press, London.

Collins, H, M, Yearley, S. (1992a): “Journey into Space”. In: Pickering, A. (ed.): *Science as Practice and Culture*, The University of Chicago Press, London.

Cooper, M. H. (2009), "Commercialization of the University and Problem Choice by Academic Biological Scientists", *Science, Technology and Human Values,* vol. 34, no. 5, pp. 629-653.

Croissant, J. l. & Restivo, S. P. (2001): *Degrees of compromise: industrial interests and academic virtue*. State University of New York Press, Albany.

Danish Government (2002): *Tid til Forandring for Danmarks Universiteter – Styrket ledelse, Øget frihed, Stabil Økonomi,* Statsministeriet, København.

Danish Govenment (2003): *Nye veje mellem forskning og erhverv – fra tanke til faktura.* Online publication. http://vtu.dk/publikationer/2003/nye-veje-mellem-forskning-og-erhverv-fra-tanke-til-faktura/

Danish Government (2006): *Fremgang, Fornyelse og Tryghed – strategi for Danmark i den globale økonomi*. Statsministeriet, Schultz Information, Albertslund.

Danmarks Forskningspolitiske Råd (2006): *Bedre Kommercialisering af den Offentlige Forskning til Gavn for Samfundet*. Ministeriet for Videnskab, Teknologi og Udvikling, København.

de Certeau, M. (1984): *The Practice of Everyday Life*, University of California Press, Los Angeles.

Deleuze, G. (1991): *Empiricism and Subjectivity – an essay on Hume’s theory of human nature*, Columbia University Press, New York.

Deleuze, G. (2004): *Difference and Repetition*. Continnum Books, London.

Deleuze, G., Guattari, F. (1994): *What is philosophy?* Columbia University Press, New York.

Deleuze, G., Guattari, F. (1988): *A Thousand Plateaus – Capitalism & Schizophrenia*. Continuum, London.

Deleuze, G., Parnet, C. (1987): *Dialogues.* Athlone, London.

Despret: V. (2004): "The Body We Care for*:* Figures of Anthropo-zoo-genesis". *Body & Society*, Vol. 10, No. 2-3, pp. 111-134.

Despret, V. (2005): "Sheep do Have Opinions" in *Making Things Public: Atmospheres of Democracy*, B. Latour & P. Weibel, (eds.). MIT Press; ZKM, Center for Art and Media, Cambridge, Mass.; Karlsruhe, pp. 360-368.

Etzkowitz, H. & Leydesdorff, L. (2000): "The Dynamics of Innovation: From National Systems and "Mode2" to a Triple Helix of University-Industry-Government Relations". *Research Policy* 29, pp. 109-12.

Etzkowitz, H. (2002): "Incubation of Incubators: innovation as a triple helix of university-industry-government networks". *Science and Public Policy,* 29 (2), pp. 115-128.

Frickel, S., Moore, K. (2006): "Prospects and Challenges for a new Politica Sociology of Science". In: Frickel, S., Moore, K. (2006): *The New Political Sociology of Science – Institutions, networks and power*. University of Wisconsin Press, Madison, Wisconsin.

Forsythe, D. (2001): *Studying those who study us: An anthropologist in the world of artificial intelligence.* Stanford university press, Stanford.

Fox Keller, E. 1996, "The biological gaze" in *FutureNatural: nature, science, culture*, eds. G. Robertson, M. Mash, L. Tickner, J. Bird, B. Curtis & T. Putnam, Routledge, London.

Franklin, S. 1991, "Fetal Fascinations: new dimensions to the medical-scientific construction of fetal personhood" in *Off-centre : feminism and cultural studies*, eds. S. Franklin, C. Lury & J. Stacey, HarperCollins Academic, London, pp. 190-204.

Galison, P (1997): *Image and Logic – a material culture of microphysics*, University of Chicago Press.

Gibbons, M., Limoges, C., Nowotny, H., Schwarzman, S., Scott, P., Trow, M. (1994): *The New Production of Knowledge: The dynamics of science and research in contemporary societies,* Sage.

Gieryn, T. (1983): "Boundary-work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists". *American Sociological Review*, Vol.48 (6), pp. 781-795.

Gieryn, T. (1999): *Cultural Boundaries in Science – credibility on the line*, The University of Chicago Press, Chicago.

Godin, B. (2006): "The Knowledge-Based Economy – Conceptual Framework or Buzzword?" *Journal of Technology Transfer* 31(1), pp. 17-30.

Godin, B. (1998): "Writing Performative History: The New Atlantis?" *Social Studies of Science,* 28(3), pp. 465-483.

Goodman, N. (1978): *Ways of Worldmaking*, Hackett, New York.

Hackett, I, Croissant, S. P. & Schneider, B (2001): "Industry Academe and the Values of Undergraduate Engineers". *Research in Higher Education,* 33, pp. 273-295.

Hacking, I. (1986): *Representing and Intervening. Introductory Topics in the Philosophy of Natural Sciences.* Cambridge University Press, Cambridge.

Hacking, I. (2006): "Making Up People", *London review of Books*, 28(16) pp. 23-26.

Haldrup, A., Bruun, L. (2004): *Tjek på Biotek*. Publication by PLAce, Sangrill Grafisk Produktion.

Haraway, D. J. (1991) *Simians Cyborgs and Women – The Reinvention of Nature.* Free Association Books, London.

Haraway, D. J. (1994): "A Game of Cat´s Cradle - Science Studies, Feminist Theory, Cultural Studies", *Configurations,* 2. (1), pp. 59-71.

Haraway, D.J. (1995): "Cyborgs and Symbionts: Living Together in the New World Order" in *The Cyborg Handbook*, C.H. Gray, S. Mentor & H.J. Figueroa-Sarriera (eds), Routledge, New York; London.

Haraway, D. J. (1997): *Modest_Witness@ Second_Millennium: FemaleMan_meets_OncoMouse*, Routledge.

Helmreich, S. (2009): *Alien Ocean – anthropological voyages in microbial seas.* University of California Press, Los Angeles.

Henare, A., Holbraad, M., Wastell, S. (2007): "Introduction: Thinking Through Things". In Henare, A., Holbraad, M., Wastell, S. (Eds.), *Thinking Through Things – Theorising Artefacts Ethnographically.* Routledge, New York.

Hessels, L., van Lente, H. (2008): "Re-thinking New Knowledge Production: A Literature Review and a Research Agenda", *Research Policy*, vol. 37, pp. 740–760.

Holbraad, M., (2007): "The Power of Powder: Multiplicity and motion in the divinatory cosmology of Cuban Ifá (or *mana* again)". In: Henare, A., Holbraad, M., Wastell, S. (Eds.), *Thinking Through Things – Theorising Artefacts Ethnographically.* Routledge, New York.

Horst, M. (2007): "Public Expectations of Gene Therapy: Scientific Futures and Their Performative Effects on Scientific Citizenship": *Science, technology and human values*; vol. 32 (2).

Jasanoff, S (2004): *States of Knowledge- The co-production of science and social order*, Routledge

Jensen, C. B. (2010): *Ontologies for developing things: making health care futures through technology.* Sense, Rotterdam.

Jensen, C.B., Bowker, G. (2011): "The Conceptual and the Empirical in Continuous Variation", submitted, *Science, Technology and Human Values.*

Jones, M.P. (2009): "Entrepreneurial science: the rules of the game", *Social studies of science*; 39; 821.

Jones, M.P. (2011): "Eli lilly and the routinization of charisma". Paper submitted to *Bulletin of Science, Technology and Society*. Forthcoming.

Kleinman, D.L., Vallas, S. P. (2006): "Contradiction in Convergence: Universities and Industry in the Biotechnology Field". In: Frickel, S. , Moore, K. (2006): *The New Political Sociology of Science – Institutions, networks and power*. University of Wisconsin Press, Madison, Wisconsin.

Kleinman, D.L. (2003): *Impure cultures*, University of Wisconsin press, Madison.

Knorr Cetina, K. 1995, "Laboratory studies: The cultural approach to the study of science" in *Handbook of Science and Technology Studies,* Sage.

Knorr Cetina, K. (1999): *Epistemic cultures: How the sciences make knowledge*, Harvard College.

Kunda, G. (1992): *Engineering Culture. Control and Commitment in a High-Tech Corporation*. Temple University Press, Philadelphia.

Kunda, G. (2009): *Reflections on Ethnography – Science, inspiration and everyday life*, paper presented at the PhD course: Doing Ethnography, Copenhagen Business School.

Lam, A., (2010): From "Ivory Tower Traditionalists" to "Entrepreneurial Scientists"? *Social Studies of Science*, 40 (2), pp. 307-340.

Latour, B. (1986): 'The Powers of Association'. In Law, J. (Ed.): *Power, Action and Belief. A new sociology of knowledge?* Sociological Review monograph 32. Routledge & Kegan Paul, London: pp. 264-280.

Latour, B (1986b): "Visualization and Cognition: Thinking with Eyes and Hands" *Knowledge and society: Studies in sociology of culture past and present,* Vol. 6, pp. 1-40.

Latour, B. (1987): *Science in Action – how to Follow Scientists and Engineers through Society*. Harvard University press, Cambridge MA.

Latour, B (1988): "Unscrewing the Big Leviathan: How Actors Macro-structure Reality and How Sociologists Help Them to do so". In Knorr, Cetina, K & Cicourel, A.v. (eds): *Advances in social theory and methodology: toward an integration of micro and macro sociologies,* Boston, Routledge and Kegan.

Latour (1988a): *The Pasteurization of France*, Harvard College.

Latour, B. 1990, "Drawing things together" in *Representation in scientific practice*, (eds.) M. Lynch & S. Woolgar, Mass., MIT Press, Cambridge, pp. 19-68.

Latour, B (1991): "Technology is Society Made Durable", in Law, j. (ed.): *A sociology of monsters – Essays on Power, Technology and Domination,* Routldge, London.

Latour, B (1993): *We Have Never Been Modern*, Harvard College.

Latour, B (1996): "Om Aktør-netværksteori. Nogle få afklaringer og mere end nogle få forviklinger. *Philosophia,* 25 (3-4), pp. 47-64.

Latour, B (1996b) "*Aramis – or the love of technology*, Harvard University Press,

Latour, B., (1997): "Stenger´s Shibboleth". In I. Stengers: *Power and invention - situating science*. Minneapolis: University of Minnesota Press.

Latour, B (1997a): "Where Are the Missing Masses? The Sociology of a Few Mundane Artifacts" in Bjiker, W.e. & Law, J (eds.) "*Shaping Technology/ Building society: Studies in sociotechnical change"* MIT press.

Latour (1999): "Give me a laboratory and I will raise the world". In: Biagoli, M (ed.): *"The science studies reader"*, Routledge.

Latour (1999b): *Pandoras hope – essays on the reality of science studies*, Harvard University Press.

Latour, B. (2004): "How to talk about the body, the normative dimensions of science studies". *Body and Society, 10* (2-3), 20.

Latour, B. (2005): *Reassembling the social – an introduction to Actor-Network-Theory.* Oxford University Press, New York.

Latour, B., Woolgar, S. (1986): *Laboratory life – The construction of scientific facts*, Princeton University Press.

Lave, R., Mirowsky, P., Randalls, S. (2010): "Introduction: STS and Neoliberal Science", *Social Studies of Science*, 40(5), pp. 669-675.

Law, J (1994): *Organizing Modernity,* Blackwell, Oxford.

Leydesdorff, L (2000): "The Triple Helix: An evolutionary model of innovations". *Research Policy* 29, pp. 243-255.

MacKenzie, D. (2001): "Physics and Finance: S-Terms and modern Finance as a Topic for Science-Studies, *Science, Technology and Human values,* 24 (2), pp. 115-144.

Mackenzie, D. (2007): "Is Economics Performative? – Option Theory and the Construction of Derivative Markets", in MacKenzie, D., Muniesa, F., and Siu, L. (eds.) *"Do Economists Make Markets? – On the Peformativitity of Economics"* Princeton University Press, Oxford.

McGee, R.J. and Warms, R. L. (1996): *Anthropological Theory – An introductory history.* Mayfield Publishing Company. Mountain View.

Malmberg, A., (2007): Årets Modtager af VKR Legatet: Ingen forandring uden forankring. Dansk Kemi, 88/3 p 6-8.

Maurer, B. (2005): *Mutual Life, Limited – Isamic Banking Alternative Currencies Lateral Reason.* Princeton University Press, Oxfordshire.

Maurer, B. (2011): *Interchange, or Encountering the Markets of Payments,* Paper presented at the conference "Encountering Markets: consumers, credit and devices" held at the Copenhagen Business School, March 2011.

Ministry of Science, Technology and Innovation (2008): *Research 2015 – a basis for prioritization of strategic research*. Schultz Distribution, Albertslund.

Mirowski, P., Sent E.M. (2008). "The Commercialization of Science and the Response of STS." in Hackett, E. J., Amsterdamska, o. Lynch, M, Wajcman J. (eds.) *The Handbook of Science and Technology Studies*, Cambridge, MA: MIT Press.

Møller, B. L. (2000): "Cassanova-Projektet" *Naturens Verden,* (4) pp. 25-48

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Nowotny, H., Scott, P., & Gibbons, M. (2001): *Re-thinking science – knowledge and the public in an age of uncertainty*. Blackwell Publishers Ltd, Malden.

Nowotny, H. (2003): Dilemma of expertise – Democratising expertise and socially robust knowledge. *Science and Public Policy* 30 (3), pp.151-156.

Owen-Smith, J. (2006): Commercial Imbroglios: Propriety Science and the Contemporary University. In: Frickel, S., Moore, K. (2006): *The New Political Sociology of Science – Institutions, networks and power.*University of Wisconsin Press, Madison, Wisconsin.

Pedersen, M. A. (2007): "Talismans of Thought: Shamanist ontologies and extended cognition in northern Mongolia. In: Henare, A., Holbraad, M., Wastell, S. (Eds.), *Thinking Through Things – Theorising Artefacts Ethnographically.* Routledge, New York.

Pedersen, M. A., Høyer, L., (2008): "Lost in transition, fuzzy property and leaky selves in Ulaanbaatar". *Ethnos*, vol.23, no.1.

Pickering, A. (1995): *The Mangle of Practice – Time, Agency and Science.* University of Chicago Press, London.

Power, M (2004): "Counting, control and calculation: reflections on measuring and management" *Human Relations* 2004; 57; 765.

Rapp, R. 1997, "Real-Time Fetus: the role of the sonogram in the age of monitored reproduction" in *Cyborgs & citadels : anthropological interventions in emerging sciences and technologies*, eds. G.L. Downey & J. Dumit, 1st edn, School of American Research Press; Distributed by the University of Washington Press, Santa Fe, N.M.; Seattle, WA, pp. 312.

Riles, A. (2001): *The Network Inside Out*. The University of Michigan Press.

Serres: M. (1979): The Algebra of Literature: The Wolf´s Game. In: Harari, J.V. (ed.): *Textual Strategies: Perspectives in post-structuralist criticism*. Cornell University Press, Ithaca, NY.

Serres, M., (1982): *The Parasite*. University of Minnesota Press, Minneapolis.

Serres, M.,(1998): *Genese.* Gyldendal, København.

Serres, M., (2008): *The five senses – a philosophy of mingled bodies*. Continuum, London.

Serres, M., Latour, B., (1995): *Conversations on Science, Culture and Time,* The University of Michigan press, Ann Arbor.

Shapin, S., (2008): *The Scientific Life – a moral history of a late-modern vocation*. University of Chicago Press, London.

Shinn, T (2002): The Triple Helix and the New Production of Knowledge: Prepackaged Thinking on Science and Technology. *Social Studies of Science, 32 (4).*

Shore, C., and Wright, S., (1997): *Anthropology of Policy – critical perspectives on governance and power.* Routledge, London.

Shore, C., Wright, S. (2000): "Coercive Accountability: The rise of audit culture in higher education". In: Strathern, M (ed) *Audit Cultures: Anthropological studies in accountability, ethics and the academy*, Routledge.

Sismondo, S (2011): Corporate Disguises in Medical Science: Dodging the Interest Repertoire, Submitted to *Bulletin of Science, Technology and Society*.

Slaughter, S & Rhodes, G (2004): *Academic capitalism and the New Economy – Markets, State and Higher Education,* John Hopkins University press, London.

Slaughter, S & Rhodes, G (2005): "From "endless frontier" to "basic science for use" – Social contracts between science and society. *Science, Technology and Human Values,* 30 (4), pp. 536-572.

Smith, B.H: (2009): It´s like getting married -review of Shapins, s. 2008 "The scientific life", London review of Books, pp. 10-12.

Spencer, H. (1996):"The Social Organism". In: McGee, R.J. and Warms, R. L. (Eds.) *Anthropological Theory – An introductory history,* Mayfield Publishing Company. Mountain View.

Star, S.L. (1991): Power Technologies and the Phenomenology of Conventions: On being allergic to onions". In: Law, J. (ed): *A Sociology of Monsters – essays on power technology and domination,* Routledge.

Star SL & Griesemer J.R. (1989). "Institutional Ecology, 'Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907-39". *Social Studies of Science* 19 (4), pp. 387–420.

Stengers, I. (1997): *Power and invention: situating science,* University of Minnesota Press, Minneapolis; London.

Stengers, I. (2000): *The invention of modern science,* University of Minnesota Press, Minneapolis; London.

Stengers, I. (2000a): "God's Heart and the Stuff of Life", *Pli,* vol. 9, pp. 86-118.

Stengers, I (2005): "Introductory notes on an ecology of practices". *Cultural Studies Review* 11/1 (183-195).

Stengers, I. (2007): "Civilizing Modern Practices". Conference paper. *The American Anthropological Associations, in Washington DC*.

Stengers, I. (2010): *Cosmopolitics1 – Book I. The Science Wars,* University of Minnesota Press, Minneapolis.

Stengers, I. (2011): "Comparison as a Matter of Concern", *Common Knowledge* vol. 17/1 p. 48-64

Stipp S.L.S. (1999): “Toward a Conceptual Model of the Calcite Surface: Hydration, Hydrolysis and Surface Potential”. *Geochimica et Cosmochimica Acta* 63, p. 3121-3131

Strathern, M. (1997): “Improving ratings: Audit in the British university system” *Eur. Review,* 5, 305-321 (John Wiley & sons ltd).

Strathern, M. (1999): *Property, Substance and Effect – Anthropological Essays on Persons and Things*, The Athlone Press, New Jersey.

Strathern, M. (2001): “Cutting the Network”. *Journal of the Royal Anthropology Institute, 2, pp. 517-535.*

Strathern, M. (2004): *Partial Connections*, Alta Mira Press, Walnut Creek.

Strathern, M. (2005): “Robust Knowledge and Fragile Futures” in: Ong, a., Collier, s.j *Global Assemblages: Technology, Politics, ethics and anthropological problems,* Blackwell

Strathern, M. (2006): “Bullet Proofing: A Tale from the United Kingdom” *in* Annelise Riles ed. *Documents: Artifacts of Modern Knowledge.* Ann Arbor, MI: The University of Michigan Press, pp. 181-205.

Sørensen, Bent M (2010): *St Paul's Conversion: The Aesthetic Organization of Labour*, Organization Studies, 31(3), 307-326.

Tsing, A. L. (2007): *Friction – An Ethnography of Global Connection*, Princeton University press, New Jersey

Tuunainen, J (2005): “Contesting a Hybrid Firm at a Traditional University”, *Social Studies of Science*, 35/2 173-210.

Tuunainen, J & Knuuttila, A (2009): “Intermingling Science and Business Activities: A new Direction for Science and Universities?” *Science, Technology and Human Values* 34/6 p. 684-704.

Watson, Verran, H & Trunbull, D. (1994): “Science and other indigenous knowledge systems” In: Jasanoff, s. Markle, g.e., Petersen, J.C., Pinch, T. (eds.): *Handbook of science and technology studies*, Sage

Vedel, J. B., Gad, C. (2011): “A public trial de Novo - Rethinking ‘industrial interests’ ” Submitted to *Bulletin of Science, Technology & Society.*

Wright, S., Ørberg, J. S (2011): 'The double shuffle of university reform – the OECD/Denmark policy interface'. Forthcoming in: Nyhagen A., and Halvorsen, T. (eds), *Academic identities – academic challenges? American and European experience of the transformation of higher education and research.* Newcastle upon Tyne: Cambridge Scholar Press.

Wright, S., Shore, C. (2011): "Conceptualising Policy: Technologies of Governance and the Politics of Visibility". In: Shore, C, Wright, S and Peró, D. (eds) *Policy Worlds: Anthropology and the Anatomy of Contemporary Power*, EASA Series. Oxford: Berghahn, pp. 1-25.

Zagrobelny, M., Bak, S., Møller, B.L (2008): "Cyanogenisis in Plants and Anthrophods". *Phytochemistry¸* 69 (7), pp. 1457-1468.

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