Managing Strategic Research

An Empirical Analysis of Science-industry Collaboration in a Pharmaceutical Company

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www.cbs.dk

ISSN 0906-6934

Print ISBN: 978-87-93155-08-4
Online ISBN: 978-87-93155-09-1

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PhD Series 04.2014

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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.

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1. SUMMARY

In recent years, the concept of 'strategic research' has played a prominent role in Danish public research policy. This thesis investigates how strategic research develops in a pharmaceutical company. Politicians and policy makers have tended to see science-industry collaboration as the main strategic tool for stimulating national growth and job creation. They have also anticipated that companies and politicians in the future will have an increasingly important role in specifying societal and industrial problems that can be solved through science-industry collaborations. Hence, strategic research is today closely associated with what is termed 'demand-driven innovation'.

Science-industry collaboration has also attracted interest in industry, for instance, in pharmaceutical companies. However, here we find quite different ideas about strategic research and science-industry collaboration. Rather than representing a tool for providing short-term solutions, pharmaceutical companies have seen science-industry collaboration as a device for building long-term platforms of innovation. Arising from a curiosity concerning the differences between policy and corporate practices of strategic research, this thesis asks the following questions: What characterizes strategic research in a private company? Through which practices does strategic research (and science-industry collaboration) develop? What characterizes the management of strategic research?

In the field of research policy studies, the main challenge of science-industry collaboration is often described as 'overcoming barriers' related to separate cultures. According to this portrayal, university and industry are seen as two separate domains that need to be aligned in order to collaborate. However, the field of Science and Technology Studies (STS) approaches this quite differently,

arguing that phenomena like science-industry interaction might be understood in terms of 'co-production' (Jasanoff 2004).

Drawing on concepts such as 'trading zones' (Galison 1997), 'doable problems' (Fujimura 1987, 1996), and 'boundary objects' (Star and Griesemer 1989), the thesis develops a conceptual framework that focuses in particular on the examination of 'screens'. The thesis suggests that the notion of screens is a suitable tool for investigating strategic co-productions that do not presume alignment. Analyzing such forms of co-production as dependent on several kinds of screens, which categorize, project or occlude relations, the thesis aims to offer further insights into the dynamics of science-industry collaboration.

The empirical focus of the thesis is the Danish pharmaceutical company Lundbeck, which specializes in drugs for the treatment of brain disorders. Based on a form of interventionist participant observation, the thesis investigates how, in recent years, research managers in Lundbeck have developed new strategies and approaches to research. Like other pharmaceutical companies, Lundbeck has been under pressure due to changes in the market structure and consequently the business model of the pharmaceutical industry. Despite increased pressure, Lundbeck's new research strategies have been based on a relatively open and in that sense 'risky' approach. This approach has implied intensified collaboration with external academic research groups. The purpose of these collaborations has been to develop deeper insight into the biology of diseases and to base drug discovery on more profound knowledge about biological mechanisms relevant to human diseases. This context of risk and uncertainty offers a rich case for studying practices of strategic research, science-industry collaboration and research management.

The thesis offers three main findings. First, in Lundbeck strategic research is not demand-driven but rather '*strategic-explorative*'. Rather than developing in a

highly calculated, predefined or predictable process, research is progressing in a quite experimental and open process. Second, science-industry collaboration does not merely develop from processes of the alignment of unlike, or even inherently incommensurable, cultures but also through what might be termed '*misaligned co-production'*, which takes place both in external collaboration and within the company itself. In Lundbeck, we encounter cases where industrial and academic engagements are completely entangled. Consequently, establishing science-industry collaboration requires not merging but rather making important differences explicit, both from the start and during the process of collaboration. Third, this means that managing strategic research is not about bridging diverse stable worlds but about managing changing conditions and emergent relations. This does not imply the absence of structures and strategies but rather an anticipation of change. The notion of *'adaptive frameworks'* (Vedel et al. 2013) offers an approach that tries to capture the practical implications of managing strategic research.

This thesis has implications for policy, academic research and practical research management. In contrast to the recent focus on demand-driven innovation, the thesis suggests that even within companies, demands develop in explorative processes. Such demands often emerge from rather loose but prospective ideas that also give rise to expectations of change. Accordingly the thesis suggests that the somewhat rigid categorizations of research that are currently developing in (for example) Danish research policy are not sufficiently nuanced to capture the significance of strategic research in companies. Hence, the thesis hopes to stimulate debate about policy ideas of strategic research, innovation and companies.

To feed into such a policy discussion based on a 'serviceable STS' approach (Webster 2007), the thesis suggests further research in continuation of the three

main findings. What is the nature of strategic-explorative research if we consider more cases, in diverse companies and industries? What are the broader implications of thinking about collaboration in terms of misaligned co-production? What does it take to manage research according to adaptive frameworks, both in policy and in industry?

The thesis is structured in two parts. The first part introduces the empirical case and the conceptual framework. The second part contains four empirical chapters. Each of these chapters explores a case of science-industry collaboration. The first two chapters investigate collaborations between Lundbeck and external academic research groups. Based on these chapters, the thesis suggests that external collaboration gains strategic value in Lundbeck by being explorative rather than solving a precisely defined problem. It also proposes that collaboration progresses in an intricate process of explicating differences between science and industry. The final two chapters investigate science-industry collaboration within Lundbeck. Specifically, they explore how strategic research not only emerges as an outcome of external collaboration but also as a result of increased collaboration between different internal parts of the company. Within Lundbeck, we thus also find examples of misaligned co-production that challenge the idea of seeing science-industry collaboration merely as a matter of 'bridge building'. The concluding chapter summarizes the main findings and discusses their implications for practitioners and future research.

2. DANSK RESUMÉ

'Strategisk forskning' har i de seneste år spillet en central rolle i dansk forskningspolitik. Politikere og embedsmænd har præsenteret universitets-industri samarbejde som det vigtigste strategiske redskab til at stimulere vækst og beskæftigelse i Danmark. Det har givet forventning om, at virksomheder og politikere i fremtiden vil få en fremtrædende rolle i at identificere essentielle samfunds- og erhvervsmæssige problemer, som kan løses via universitets-industri samarbejde. På denne måde er strategisk forskning i dag tæt forbundet med det, som kaldes 'efterspørgselsdrevet innovation'. Denne afhandling undersøger, hvordan strategisk forskning udspiller sig i medicinalvirksomheden Lundbeck.

Universitets-industri samarbejde har også tiltrukket sig interesse fra industrien for eksempel fra medicinalvirksomheder. Her finder vi imidlertid nogle meget anderledes ideer om, hvad strategisk forskning og universitets-industri samarbejde er. Medicinalvirksomheder har eksempelvis set universitets-industri samarbejde som et redskab til at opbygge langsigtede platforme for innovation snarere end til at løse kortsigtede problemer. Med baggrund i en interesse for at forstå disse forskelle mellem forskningspolitiske tilgange til strategisk forskning og virksomheders konkrete udvikling deraf undersøger afhandlingen følgende spørgsmål: Hvad karakteriserer strategisk forskning i en privat virksomhed? Gennem hvilke praksisser bliver strategisk forskning (og universitets-industri samarbejde) til? Hvad karakteriserer ledelse af strategisk forskning?

Inden for studier af forskningspolitik anses nødvendigheden af at 'overvinde barrierer' relateret til forskningskulturelle forskelle ofte som en af de vigtigste udfordringer ved universitets-industri samarbejde. I disse studier anses

universiteter og virksomheder således som to separate domæner, der skal 'alignes', for at kunne samarbejde. I forhold til denne analyseramme tilbyder forskningsfeltet Science and Technology Studies (STS) en væsentlig anden tilgang. I første omgang er præmissen for mange STS tilgange, at fænomener som universitets-industri samarbejde bør forstås som 'co-produced' (Jasanoff 2004). Co-production er også den analytiske præmis for herværende studie.

Baseret på begreber såsom 'trading zones' (Galison 1997), 'doable problems' (Fujimura 1987, 1996) og 'boundary objects' (Star og Griesemer 1989), som alle illustrerer 'co-production', udvikler afhandlingen et begrebsapparat, som i særlig grad fokuserer på en undersøgelse af begrebet 'screens'. Screens foreslås som et begrebsapparat, der er egnet til at undersøge strategiske former for co-production, som ikke er baseret på adskilte domæner, der skal sammenknyttes. Ved at analysere co-production som processer der både bygger på og genererer forskellige slags screens, tilbyder afhandlingen ny indsigt i dynamikken i universitets-industri samarbejde.

Det empiriske fokus i afhandlingen er den danske medicinalvirksomhed Lundbeck, som er specialiseret i at udvikle og producere lægemidler til behandling af neurologiske sygdomme. Baseret på intervenerende deltagende observation undersøger afhandlingen, hvordan forskningsledere i Lundbeck i de senere år har udviklet nye forskningsstrategier og tilgange til forskning. I lighed med andre medicinalvirksomheder har Lundbeck været under stort pres som følge af strukturelle og markedsmæssige ændringer i medicinalindustrien. Dette til trods har Lundbeck udviklet nye forskningsstrategier baseret på en relativt åben og risikobetonet tilgang. Denne tilgang har bl.a. medført intensiveret samarbejde med eksterne akademiske forskningsgrupper med det formål at basere udviklingen af nye lægemidler på mere grundlæggende viden om biologiske mekanismer. Denne kontekst af risiko og usikkerhed udgør en god case

for at studere ideer om strategisk forskning, universitets-industri samarbejde og forskningsledelse.

Afhandlingen når frem til tre overordnede konklusioner: For det første er strategisk forskning i Lundbeck ikke efterspørgselsdrevet men snarere *'strategisk-udforskende'*. Forskningen skrider frem i en ganske eksperimentel og åben proces frem for at udvikle sig lineært, kontrolleret og forudsigeligt. For det andet udvikler universitets-industri samarbejde sig ikke kun på baggrund af processer, som skaber forbindelser mellem basalt set forskellige domæner eller kulturer. Det udvikler sig også som følge af det som i afhandlingen karakteriseres som *'misaligned co-production'*, hvor eksplicitering af forskelle er en del af samarbejdets dynamik. Sådanne misaligned co-productions finder sted både i eksterne samarbejder og internt i virksomheden. I Lundbeck finder vi eksempelvis cases, hvor industrielle og akademiske aktiviteter er fuldstændig sammenfiltrede. At etablere universitets-industri samarbejde kræver således ikke nødvendigvis en sammenkobling af forskellige kulturer men snarere en løbende og gensidig præcisering af de vigtige forskelle, der også driver samarbejdet. For det tredje drejer ledelse af strategisk forskning sig ikke primært om 'brobygning' mellem stabile domæner men om at håndtere relationer, der udvikler sig under foranderlige betingelser. Det betyder ikke, at ledelse sker uden struktur eller strategier men snarere, at der ledes med en forventning om forandring. Begrebet *'adaptive frameworks'* (Vedel et al. 2013) foreslår en tilgang, som forsøger at indfange de praktiske aspekter af ledelse af strategisk forskning.

Afhandlingen har implikationer for forskningspolitik, akademisk forskning og praktisk forskningsledelse. Til forskel fra det nuværende forskningspolitiske fokus på efterspørgselsdrevet innovation viser denne afhandling, at selv *internt i* virksomheder udvikler efterspørgsel sig ofte i udforskende processer. Efterspørgsel udvikler sig ofte på baggrund af temmelig løse ideer som både har

et potentiale og en indbygget forventning om muligheden for forandring. Som følge heraf foreslår afhandlingen, at de noget rigide kategoriseringer af forskning, som i øjeblikket florerer i dansk forskningspolitik, ikke er tilstrækkeligt nuancerede til at indfange, hvad der faktisk er kendetegnende ved strategisk forskning, som det udfolder sig i praksis i virksomheder. Dermed håber afhandlingen at stimulere debat om forskningspolitiske ideer om strategisk forskning, innovation og virksomheder.

For at føde ind i en forskningspolitisk diskussion baseret på en 'brugbar STS' tilgang (Webster 2007) indikerer afhandlingen endvidere behovet for mere forskning i forlængelse af de tre hovedkonklusioner. Presserende spørgsmål inkluderer blandt andet: Hvad drejer strategisk-udforskende forskning sig om hvis vi ser på flere cases i forskellige virksomheder og industrier? Hvad er de bredere implikationer af at tænke samarbejde som misaligned co-production? Hvad kræver det at lede forskning ved hjælp af adaptive frameworks både i forskningspolitik og i industri?

Afhandlingen er struktureret i to dele. Den første del introducerer den empiriske case og begrebsapparatet. Den anden del indeholder fire empiriske kapitler. Hvert af disse kapitler udforsker et eksempel på universitets-industri samarbejde. De første to kapitler undersøger samarbejder mellem Lundbeck og eksterne akademiske forskningsgrupper. Baseret på disse kapitler foreslår afhandlingen, at eksternt samarbejde får strategisk værdi i Lundbeck ved at være udforskende snarere end ved at løse præcist definerede problemer. Disse to kapitler viser også, at samarbejde skrider frem i en kringlet proces, der både skaber ligheder og forskelle mellem forskning og industri. De sidste to kapitler undersøger forsknings-industri samarbejde internt i Lundbeck. Kapitlerne undersøger specifikt, hvordan strategisk forskning ikke kun udvikler sig på baggrund af eksterne samarbejder men også som et resultat af styrket samarbejde mellem

forskellige dele af Lundbeck. Inden for Lundbeck finder vi således også eksempler på misaligned co-production, hvilket atter udfordrer ideen om at se universitets-industri samarbejde udelukkende som 'brobygning' mellem domæner. I konklusionen opsamles hovedkonklusioner samt afhandlingens implikationer for videre forskning og praktikere.

3. ACKNOWLEDGEMENTS

This project has taken place from 2008 to 2013 and it has involved several institutions and people. I have, therefore, many people to thank.

I would like to thank Peter Høngaard Andersen for taking on this project and for inviting me into Lundbeck. Based on our first meeting in August 2008, Peter decided to engage uncompromisingly in the project and to make it part of his reflections on new strategies and approaches to research in Lundbeck. From beginning to end, Peter has set up favorable conditions for the project and made it easy for me to get access to data and people. He has taken a strong interest in my research process and given me valuable advice. I have found our continuous discussions about the nature of research, management and collaboration extremely stimulating and rewarding. Our conversations have always left me with new perspectives on research and I have enjoyed the experience of seeing my ideas circulate in the development of new strategies.

I also want to warmly thank my academic supervisor, Alan Irwin. Alan has been a great supervisor for this project, while also being the Dean of Research at Copenhagen Business School. From the beginning, Alan encouraged me to be open and to welcome surprises, both with regard to what my industrial PhD set-up might provide in terms of opportunities and challenges, and to what the empirical material would present. Resisting the impulse to make quick decisions, Alan supported me in thinking flexibly about the material without drowning it in pre-conceived theory. Alan has played a key role in this project and been an excellent help in pulling together the diverse parts of the projects, in terms of both its practical use and conceptual contributions.

I want to thank both Peter and Alan for their collaboration on our joint paper "Externalizing research through adaptive frameworks", published in *Nature Reviews Drug Discovery* (Vedel et al. 2013, see Appendix A). I see this paper as an important result of our collaboration: not only in terms of publishing together, but also in terms of the experience of developing these ideas collectively. It took place in lively, highly engaged discussions about research management in practice and what matters to this subject.

Other people have played important roles at different stages of the project. Thanks to Maja Horst for early discussions and for encouraging me to develop these ideas into a PhD project. Also thanks to Randi Markussen, my co-supervisor. Randi played an important role in setting up the PhD project and in helping me see how studies in Lundbeck might link to my practical experience with research policy and management. I thank Randi for very inspiring discussions and for always pointing to important and, for me, new perspectives.

A special thanks to Casper Bruun Jensen, both for his continuous support and inspiring conversations over the years, and for detailed comments on various drafts of the thesis. Thanks also to Signe Vikkelsø for providing valuable inputs to the conceptual and methodological framework of the thesis. Thanks to Christopher Gad, my co-author for the paper "A Public Trial de Novo. Rethinking Industrial Interest" (Vedel et al. 2011), for an energetic and fun collaboration based on a shared curiosity for the notion of 'industrial interest'.

A group of people helped me develop the notion of screens. The second empirical chapter is a further development of the paper "A first encounter. Framing research collaboration with screens," published in a special issue on screens in *STS Encounters* (Vedel 2011). Thanks to Anna Tsing for discussing the empirical case with me over a three-day stay at Klitgården in Skagen. Also thanks to Brit Ross Winthereik for valuable inputs on the notion screens. I thank the anonymous

reviewers of the paper for highly useful comments. Finally, thanks to Helen Verran for discussion and correspondence about the notion of screens.

At times this PhD has posed challenges in terms of keeping things together while moving between institutions and work places. I want to thank Thomas Basbøll for providing me with effective tools for planning and structuring academic writing. A special thanks to Tina Wilk for encouraging and valuable inputs on how to handle my different, and sometimes contradictory, ambitions as researcher and practitioner.

I am deeply indebted to people at Lundbeck for taking a strong interest in this project. Especially, I thank Marianne Dragsted who made life at Lundbeck very pleasant. With exceptional skill, Marianne solved every problem that we encountered on the way. Thanks also to Kim Andersen, Benny Bang-Andersen, Ann-Louise Bergström, Tina Bott, Lise Tøttrup Brennum, Klaus Bøgesø, Matthew Cascio, Michael Didriksen, Jan Egebjerg, Karina Fog, Ib Vestergaard Klewe, Stine Hove Marsling, Helle Northeved, Jan Thorleif Pedersen, Lise Abildgaard Ryberg, Helle Sickmann, Klaus Bæk Simonsen, Tina Bryan Stensbøl, and Stevin Zorn for inspiring discussions, lively lunch conversation, for teamwork and for inviting me into management group discussions.

I also want to thank my colleagues at Copenhagen Business School, Department of Organization, for creating informal and stimulating work conditions. A special thanks to my office colleagues Amalie Hauge-Helgestad, Didde Maria Humle, Mette Brehm Johansen and Marie Henriette Madsen for creating a highly attentive and encouraging office atmosphere. Also sincerely thanks to Karen Boll, Shannon Hessel, Marie Mathiesen, Elisabeth Naima Mikkelsen, Trine Pallesen and Soley Rasmussen who have all provided valuable inputs to the thesis and the process of writing it. Outside Copenhagen Business School, I want to thank Morten Andreasen, Mia Trolle Borup and Lotte Jansen for always thoughtful and high-

spirited discussions about the topic of research management that we have common interest in.

I want to thank a group of old friends whose continuous support and encouragement over the years has been invaluable for me. Thanks to Marlene Bernth Johansen, Johanne Luise Boesdal, Line Bjørg Jensen, Kristian Knak Nygaard, and Milena Torquati.

My interest in researchers, their passions and struggles, developed in my childhood home thanks to my parents, Kaj Vedel and Eva Bjørn Vedel, who are both scientists. Owing to them, I developed a strong interest for science and a curiosity towards the social aspects of science. I thank both for their caring and thoughtful encouragement. In particular, I want to thank my mother, who has been a research manager for many years, for introducing me to the challenges and joys of managing research, collaborating across fields and making research relevant in a broader societal context.

Finally, it almost goes without saying that making this thesis would have been impossible without the love and support of my husband, Simon Kiilerich Vedel, and my lovely children, Siri and Eske.

4. PREFACE

This thesis explores 'strategic research' in a Danish pharmaceutical company. Since this topic might obviously be assumed to concern public policy discussions related to strategic research or industrial strategies related to the current crisis in the pharmaceutical industry, it is perhaps worthwhile to say a little about what the thesis is *not*.

This thesis is not primarily concerned with policy, although it gives insights into this area. Certainly, the notion of strategic research gives strong connotations to public policy and, in fact, we rarely hear about strategic research outside the world of policy. In policy, however, strategic research has in recent years played a prominent role. We can see this in both Danish and European research strategies in which strategic research is seen as important for stimulating growth and job creation.[1] Today, Danish and European policy makers are deeply involved in defining strategic research and organizing it in the most optimal ways. It would be highly relevant to study these current policy interests in strategic research from *within* policy institutions but this is not the main focus of this thesis.

Likewise, this thesis is not mainly about the pharmaceutical industry, although it gives important insights into the strategies and change processes in one company. The empirical context of a pharmaceutical company gives connotations to the pharmaceutical industry in general. In recent years, the pharmaceutical industry has been under significant pressure and consequently new research and business models have emerged.[2] It would therefore be highly relevant to explore what

[1] See the Danish "Innovation Strategy" (Regeringen 2012) and the European research framework program "Horizon 2020".
[2] See Vedel et al. 2013 for further discussion. Also, see Munos 2009.

characterize these new models and investigate their implications for the pharmaceutical industry as such but this has not been the primary concern of this thesis.

The starting point of the thesis is rather the phenomenon that in these years, policy makers and industrial research managers share interests in exploring the potential of science-industry collaboration, in policy often referred to as university-industry collaboration. In a Danish research policy context, policymakers see university-industry collaboration almost as identical to strategic research. Among pharmaceutical companies, research managers have seen collaboration with external academic groups as an important tool for developing new insights and overcoming the crisis. But, although we might identify shared interests in science-industry collaboration, public policymakers and industrial research managers have quite *unlike* approaches to science-industry collaboration. Based on a curiosity concerning these dissimilarities, the thesis investigates the following questions: What characterizes strategic research in a private company? Through which practices does strategic research (and science-industry collaboration) develop? What characterizes the management of strategic research? I start out by describing how I came into this study since it has implications for how I define the field of research of this thesis.

From the beginning of 2006 to the end of 2008 I was employed at a Danish public foundation, the Danish National Advanced Technology Foundation (DNATF), established in 2006 to give public funds to research collaborations between public research institutions and private companies. The foundation had a board and staff, and its administration was independent of the existing research advising and funding system. During the first year, the foundation developed what it referred to as strategies and instruments. One of the questions raised at this time was how to set up the foundation in a way that would make it possible to

flexibly meet the dynamic market of research applicants. For instance, the board of the foundation was concerned that the biannual calls for research proposals with which the then current Danish research funding system operated were too rigid to support the dynamic development of new research ideas. The board was also concerned with how to make sure that the applicants would collaborate seriously rather than just split up upon receiving the grant. This question was related to the idea that participants from private companies and public research institutions came from different worlds and consequently had different interests in the projects they developed together. Consequently, it was assumed that public private research collaboration would need special facilitation and attention in order to succeed. Based on these concerns, the staff developed a close dialogue with potential applicants and they carefully followed up on projects that received grants. Furthermore, in order to receive funds from the foundation implied appointing a project leader that had a special task in mediating between the perceived worlds of the participants and in setting a joint direction of the project.

Working at the Danish National Advanced Technology Foundation (DNATF) made me wonder about the nature of science-industry collaboration, public policy approaches to strategic research and research management. First, I was concerned that the foundation's instruments and concepts were introduced with too little reflexivity concerning their implications or consequences. They were working, yes, but the conditions for receiving critical feedback from applicants were not good. For instance, the instruments were designed to facilitate a close dialogue with applicants allowing timely changes in the organization of project plans of the funded research projects when needed. However, in practice the applicants' incentives for dialoguing with the members of the foundation concerning specific failures or problems were not good since bringing details about failed projects or bad collaboration among the applicants out in the open

might give the foundation a good reason to withdraw funding from the project. In other words, even though instruments were designed to facilitate an open dialogue, the double role of the foundation as at once investor and discussion partner was not always optimal. Indeed, it sometimes led to the unintended situation of making the applicants and receivers of funding overly strategic in their relation to the foundation.

Second, my encounters with a large number of research collaborations within a broad range of research areas and industries raised a number of questions: What drives research collaboration forward? In what ways are university and corporate research interests different, or similar? What, indeed, does strategic research mean in a corporate context? What are the specific challenges of managing strategic research? In this way, my interest in research collaboration, strategic research and companies arose from working with research policies and managing collaborations in a Danish public policy context.

Along with these experiences from Danish public policy, I entered this PhD with a background in Science and Technology studies (STS) from Information and Media Studies at University of Aarhus. My interest in STS developed from the late 1990s until 2005, and in particular I became interested in what characterizes research as a collective practice (Stengers 1997, 2000; Strathern 2004). However, rather than being interested in laboratory studies (Cetina 1999; Latour and Woolgar 1979; Latour 1987; Pickering 1995), I was interested in how researchers managed and organized research and how they collaborated with researchers with different backgrounds. I continued to have this curiosity as I entered public policy, however I gradually became interested in the management of public funding. I was particularly interested in the implications of a more dialogue-based approach to individual cases of collaboration. At the foundation, managing and organizing collaborations implied both continuous discussion with individual

projects and general categorizing of the projects to present different trends vis-à-vis the board. While the dialogue-based approach generated an increasingly deep insight into the *differences* between collaborations the need to communicate both publicly and to the board required homogenizing the collaborations and to some extent ignoring differences. Of course, such categorization was an inevitable part of managing public research and making decisions. However, it was also constantly a discussion at the foundation how categorizing collaborations ignored or rendered invisible important nuances and differences among them (Bowker and Star 1999).

One of the questions that the board and staff asked was how to develop a way of granting money to research that implied more dialogue with applicants and more follow up on research projects than previously seen in a Danish research funding context. Part of the answer to these questions involved the development of 'an industrial approach' to managing public research grants. Gradually, research grants were seen as akin to investments in a growing portfolio. Thinking of grants as investments legitimized following them quite closely because, as investors, we were supposed to be interested in their progress and well-being. However, although I took part in developing it, this approach also struck me as generating a somewhat idealized image of industrial research management. According to this image, managing research implied effective management tools and follow up routines. These seemed slightly idealized comparing to the sometimes quite messy practices that we encountered in dialogues with individual projects. I consequently became interested in exploring the management of strategic research from within an actual company. I wanted to know what actually characterized research management practices in such a context.

I was particularly curious about research management within a research-based company with many research projects and with collaborations with external

academic research groups. Also, I was interested in a company whose research bore some resemblance to academic research. This mattered since I began to think of my project as potentially challenging the idea of university and industry as two separate cultures with separate norms and goals that was particularly prevalent in Danish research policy around that time. Based on these criteria, the pharmaceutical industry came to my attention. This industry is widely known for engaging in research that is very basic in nature, yet decided on with the long-term purpose of developing a marketable drug (Petryna et al. 2006). Also, the pharmaceutical industry is relatively strong in Denmark with global well-known companies such as Novo Nordisk and Lundbeck.

Hence, I contacted the then head of research of Lundbeck, a Danish pharmaceutical company specializing in drugs for the treatment of brain disorders. In August 2008, I presented him with a list of research questions concerning the management of science-industry collaboration in an industrial context. The proposal was well received and, indeed, seemed in some sense to fit with the then current strategies in Lundbeck. For many years, collaboration with external research groups had taken place in Lundbeck. However, due to changing conditions for research and innovation in the pharmaceutical industry, research collaboration with externals had become a *strategic* concern.

Based on this situation of good timing, I defined my field of research as managing strategic research with an empirical focus on the interface between Danish research policy and a Danish pharmaceutical company, Lundbeck. However, the notion of an 'interface' turned out to be a rather crude metaphor for the many interlocking processes that both tie together and separate public research policy and new collaborative practices in Lundbeck. In this thesis, I analyze these processes by developing the notion of different forms of collaborative screens.

5. INTRODUCTION TO THE THESIS

In recent years, strategic research has become a 'matter of concern' (Latour 2004) in Denmark. It has appeared in a number of settings. In research policy, strategic research has emerged as a new area with its own policies. It has twice provided an occasion for fundamentally reorganizing the Danish research funding and advisory system (Ministeriet for Videnskab 2003a; 2003b; 2010a; 2010b; FIVU 2009; Regeringen 2012; DSF 2013). First, in 2004 when strategic research emerged as a distinct domain with the making of two separate public research councils: The Danish Council for Strategic Research (DSF) and the Danish Council for Independent Research (DFF). Second, in 2012, when the Danish minister of Research, Innovation and Higher Education proposed an extensive rethinking of the field of strategic research in Denmark in a new national "Innovation Strategy" (Regeringen 2012).

Simultaneously, strategic research has been at the center of public debates. In these debates, a main (academic) critique of strategic research has been that it threatens to compromise classic scientific values and norms and leads to conflicts of interests in its efforts to combine the diverse fields of science, society and industry.[3] In addition, strategic research has emerged as a topic that research managers and strategists at universities and in companies have had to tackle (ATV 2012). In diverse professional forums, research managers from universities and industries have begun discussing various aspects of strategic research, for instance, whether the current perception of strategic research as a means to change the stagnant Danish economy is correctly perceived and organized.

[3] For an example and analysis of this debate, see Vedel and Gad 2011.

When investigating strategic research in the context of Danish research policy, it is immediately noticeable that strategic research is widely perceived to be closer to society and industry than independent research (DSF 2013). Often, in documents describing Danish research policy, research is arranged on a straight line extending from independent research on the left to the market on the right. In that sense, strategic research is imagined as more directly addressing societal problems than independent research, which, in contrast, is perceived to mainly address the academic communities. In fact, I would suggest that this linear perception of research has become increasingly prevalent in Danish research policy. See diagram below, which was recently presented in the Danish "Innovation Strategy" of 2012.

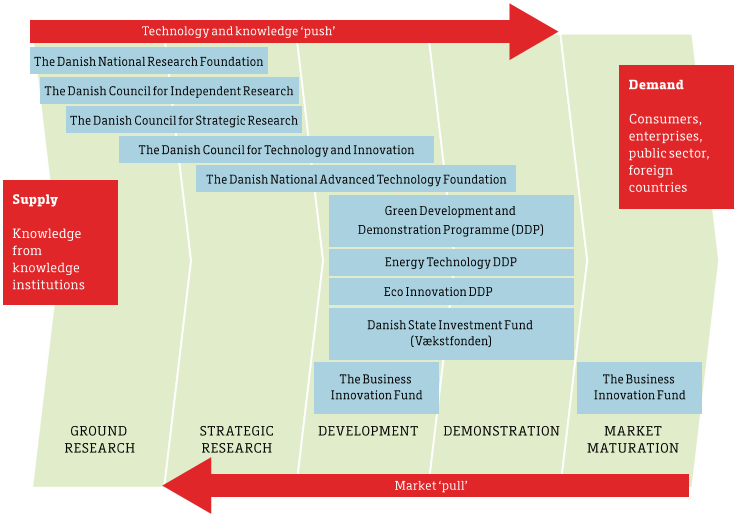

Illustration 1: The linear organization of the Danish public research advisory and funding system (Regeringen 2012).

In the context of Danish research policy, the linear view of public research funds is a mobilizing force. For instance, it is used to discuss the specific mandate of individual research funding institutions and to determine which kinds of research

projects ought to be supported by which institutions. As a consequence of being placed in the middle, strategic research is often presented as research that "connects" (Regeringen 2012) the diverse domains of science, society and industry in contrast to independent research that according to this image is slightly more isolated from society.

In the field of Science and Technology Studies (STS), strategic research is interesting not least at the level of practice. In its emphasis on practice, STS approaches differ from many other perspectives on strategic research (for instance, based on economics or political science) that focus on defining its conceptual meaning, resulting in general definitions of strategic research (Emmeche and Faye 2010). Working within a broadly 'co-productionist' perspective on STS (Jasanoff 2004), and rather than attempting to give an exact definition of what strategic research means, I am interested in how strategic research develops at the level of practical research engagements and in which diverse forms it takes there. How and through which processes, I ask, does strategic research develop in the context of Lundbeck's pharmaceutical research?

Accordingly, the present study explores the notion of strategic research in the context of Lundbeck. Lundbeck is a Danish global research-based company that specializes in drugs for the treatment of disorders in the central nervous system (CNS). Like many other pharmaceutical companies, Lundbeck has been affected by changing research and market conditions in the pharmaceutical industry. Although pharmaceutical companies today make larger investments in research and development than ever before, fewer research and development projects result in new molecular entities that reach the market in the form of drugs (Munos et al. 2009). Many factors contribute to this situation, including changes in the regulatory system and in public concerns over risks in various diseases. For Lundbeck, the expiration of a number of key patents between 2012 and 2014 has

increased the pressure. Research managers in Lundbeck have seen this crisis as an occasion for rethinking their research strategies and opening up to the surrounding world, for instance, by engaging in strategic collaborations with external research groups at universities and in companies. Lundbeck, therefore, offers an excellent case for examining the management of strategic research in a corporate context. Based on this situation, I am interested in examining the following research questions:

Research questions

What characterizes strategic research in a private company? Through which practices does strategic research (and science-industry collaboration) develop? What characterizes the management of strategic research?

Unsurprisingly, these are questions that research managers in Lundbeck also ask themselves. In addition, I consider what the fields of public research policy and STS might learn about strategic research by studying it empirically in industry. This is interesting insofar as industry is precisely what policymakers argue that strategic research should connect with.

The structure of the thesis

The thesis is organized in two parts. The first part introduces the empirical field, the methodology and the conceptual framework of the thesis. I first give an introduction to Lundbeck and describe how I have studied Lundbeck using an active form of participant observation. Then I take a step back to explore how the notion of strategic research has developed in Danish research policy with particular embedded ideas about companies. I use this understanding of the setting of Danish national discussions of strategic research as a springboard for

exploring prevalent approaches to university-industry interaction in research policy literature such as the Triple Helix and Mode 2. These approaches then become a platform for investigating alternative 'co-productionist' approaches (Jasanoff 2004) to research collaboration, in particular 'boundary objects' (Star and Griesemer 1989), 'doable problems' (Fujimura 1987, 1996) and 'trading zones' (Galison 1997). While these notions generally focus on processes of constructing alignment in heterogeneous scientific practices, I argue that it is equally interesting to explore the role of *difference* in research collaboration, and hence in strategic research. I show that difference can be elicited by paying attention to a number of 'screens' that I develop for the purpose of this thesis, which I refer to as projecting, categorizing, occluding screens. In various ways, these screens are useful for developing a deeper understanding of the dynamics of strategic research.

The second part of the thesis offers an empirical exploration of strategic research in Lundbeck based on four empirical chapters. The first two chapters explore strategic research as illustrated by science-industry collaboration between Lundbeck and external academic groups. The final two chapters look at in-house collaboration between different parts of Lundbeck as also illustrative of strategic research.

The first chapter "Managing emergent relations" presents a case in which Lundbeck research managers collaborate with academic researchers from a university. The participants are all interested in advancing research in the biological mechanism neurocell. However, they do this in a somewhat surprising way that challenges both the idea of strategic research as a restricted and predictable form of research and the general notions of what is academic and what is industrial.

The second chapter entitled “The first encounter” explores yet another case of collaboration between Lundbeck and an external academic group, this time the Mayo Clinic, an American not-for-profit research institution. The chapter investigates the phenomenon that a failed first meeting between the two groups did not prevent the collaboration from eventually becoming a success. I explore how this can be understood by describing the differentiating screens that characterized the collaboration.

The third chapter called “Making screens for future research” explores how strategic research also develops in-house Lundbeck, in the context of a strategy process called Synapse. This strategy seeks to connect diverse parts of Lundbeck and develop strategic research areas based on collaboration between preclinical and clinical research. The chapter investigates how different screens are involved in developing these strategic research areas.

The forth chapter is entitled “The project leader of the future” and it investigates how the notion of project leadership develops in relation to the development of new strategic research practices. The project leader is seen as connecting diverse parts of Lundbeck but also as particular to research practices in Lundbeck. The chapter investigates the project leader notion using an active interventionist approach and explores the screens that became visible.

In conclusion, I present the main findings and discuss the implications of these for practitioners and for future research.

But first, let me introduce the empirical context of Lundbeck.

6. INTRODUCTION TO LUNDBECK

Lundbeck is a Danish global pharmaceutical company that specializes in drugs for the treatment of disorders in the central nervous system (CNS), sometimes also referred to as brain diseases. The company was founded in 1915 and today it employs 6,000 people worldwide out of which 2,000 are employed in Denmark. This makes Lundbeck one of the largest companies in Denmark. Lundbeck presents itself as "fully integrated"[4], which means that it engages in research, development, production, marketing and sale of pharmaceuticals. CNS disorders include depression, anxiety, psychotic disorders, epilepsy, Huntington, Alzheimer's and Parkinson's diseases. However, as a consequence of a new research strategy, Lundbeck might expand the number of CNS diseases it engages in. Lundbeck is particularly known for Cipralex and Lexapro, both of which are based on escitalopram, an antidepressant of the selective serotonin reuptake inhibitor class known as SSRI, for the treatment of depression.

Today, Lundbeck's general management consists of the Executive Management that has three members. Ulf Wiinberg who is President and CEO of Lundbeck heads this group and is responsible for commercial operations. The Executive Management also includes the Head of Research and Development, which includes the area of Patents and Trademarks, and the Head of Finance, IT, Sourcing, Commercial and Investor Relations. In addition to the three areas that the Executive Management covers, the organization of Lundbeck is divided into six other areas. These are Corporate Business Development and Strategy; Corporate Human Resources; Corporate Legal; Corporate Secretariat and Project Office; Corporate Public Affairs; and Supply Operations and Engineering. A senior

[4] www.lundbeck.com.

vice president heads each of these areas. The headquarters of Lundbeck are located in Valby, part of the Copenhagen Municipality. This short introduction portrays how Lundbeck is organized as of 2013, but, as I will show, the formal organization of Lundbeck has changed several times in recent years. These changes have appeared as a consequence of new ways of perceiving the role of research in relation to business.

Today, research activities in Lundbeck are geographically distributed over three sites: the main site in Denmark, and two smaller sites in the US and China. Altogether, about 1,200 specialists are employed in Research and Development. Since 2011, Research and Development have been grouped as one area in Lundbeck. Until then, Research and Development were organized as two separate domains, each with their own head. Since this thesis is based on fieldwork in the period 2009-2013, it is relevant to note that between 2009 to 2011, Lundbeck Research included four quite diverse activities: Drug Discovery, including very early identification of unmet needs and definition of research projects; Non-Clinical Safety Research, covering early tests of toxicity and other safety issues; Business Development, involving systematic search for potential research partners and business opportunities; and finally Patents and Trademarks, including evaluation of patent and license opportunities. My research was primarily located in the area of Drug Discovery in the then department of Molecular Neurobiology, which, by the employees in that area, was perceived as "real research".

Drug Discovery was organized into three main divisions: Medicinal Chemistry, Neurobiology and Pharmacology. As in a matrix organization, research projects cut across these three divisions according to specific diseases referred to as 'indication areas'. During my research period, these indication areas were Alzheimer's disease, Parkinson's disease, Psychoses, and Depression and Anxiety.

Each of the indications had a strategy, for instance, an Alzheimer's disease strategy, describing how current research activities aimed at developing a drug for this specific disease. Besides belonging to specific departments such as the Molecular Neurobiology department, employees in the research division were organized in groups according to indication areas. The purpose was to build up expertise within each of the indications. Each indication group had a chair, usually a department manager or section head, responsible for an annually revised strategy plan.

In 2010, this organization of research was revised as an outcome of an extensive strategy process, Synapse, to which I return. One consequence of this strategy process was that indications were replaced by biological mechanisms as the main organizing principle, causing a considerable reorganization of research groups and expertise. Where research was previously driven by certainty about the specific indication towards which it was directed, the reorganization was based on the idea that the same biological mechanism might be relevant for *several* diseases. Accordingly, research into one biological mechanism might lead to innovations within a number of indication areas. This implied uncertainty about indications but the perceived benefit was that it might lead to potential innovations in the longer term. It also implied a strong focus on articulating biological hypotheses. This, too, meant a reversal of the research process, since previously research had typically been initiated based on newly discovered effects in known compounds. Consequently, *medicinal chemists* had played an important role in pointing out these compounds. In contrast, the reorganization put the focus on 'disease biology', privileging *biologists* and the development of scientific hypothesis based on mechanisms, structures and patterns. As a consequence of the strategy process, the departments in Lundbeck's research

division were renamed as Discovery Chemistry, Neurodegeneration, and Synaptic Transmission. I explore these changes in detail in Chapter 12.

The changes to the formal organization of Lundbeck in 2011 were the results of an extensive strategy process, named Synapse, which roughly took place between 2009 and 2011. In the nervous system, 'a synapse' is a structure that permits a neuron or nerve cell to pass an electrical or chemical signal to another cell, thus making a connection or communication. According to a dictionary definition, a synapse is "a junction between two nerve cells, consisting of a minute gap, across which impulses pass by diffusion of a neurotransmitter".[5] Metaphorically, the concept of the synapse suggests both the idea of a gap and a connection. Lundbeck's strategy process, Synapse, took inspiration from this idea by aiming at developing new strategies that were based on a sustained effort at making *connections*. In fact, the Lundbeck management already had several specific connections in mind.[6]

First, there were connections between Lundbeck and the outside world. In the research division, such connections implied relating actively to an increasingly complex regulatory system and to external research partners. At a general level, these connections also involved creating more societal awareness of brain diseases, an aim that later developed into a systematic effort to address societal agendas as part of Public Affairs.

Second, there were connections *within* Lundbeck; that is, between divisions and activities that had previously been seen as distinct. Specifically, this included the divisions of Research, Development and Clinical Research. By appealing to the

[5] www.oxforddictionaries.com.
[6] During 2005 to 2007, before Synapse was initiated, a strategy process took in Lundbeck Research only. It was called "Lundbeck 2020" and was based on similar ideas of connecting Lundbeck to an outside world.

concept of synapses in the overall vision, the management aimed at integrating these divisions and preventing unfortunate misaligned perspectives when transferring an activity from one division to another. Thus, by incorporating the perspectives of Development and Clinical Research in the prioritization and structure of new research projects, the aim was to avoid the potential situation where a project was rejected as relevant or useful in the transition from Research to Development. In this way, the Synapse strategy process depended on a rather intriguing notion of strategic research, which I explore in more detail in Chapters 12 and 13.

Before the organizational changes that followed the Synapse strategy process, three research groups had primarily managed research in Lundbeck. On a weekly basis, the members of these groups discussed a broad range of research related issues, including strategic initiatives related to collaboration with external research groups. The first was the Research Management Board (RMB), headed by the executive vice president of research. This board consisted of each of the heads of the research divisions, including the head of Drug Discovery in Denmark, the head of Drug Discovery in the United States, the head of Non-Clinical Safety Research, the head of Patents and Trademarks, and the head of External Affairs. Secondly, the Drug Discovery Management Team (DDMT) headed by the head of Drug Discovery in Denmark consisted of the three divisional directors, each representing a research area: the Divisional Director of Chemistry, the Divisional Director of Pharmacology, and the Divisional Director of Molecular Neurobiology. Finally, the Research and Development Management Board (RDMB) consisted of the Research Management Board and its equivalent in the Development division. This board had the task of coordinating activities between Research and Development at a general level, which included ensuring the transition of projects

from Research to Development, an effort that in the context of the Synapse strategy became a key concern.

The names of departments and the constitution of these management groups all bear witness to the great importance of research in Lundbeck. The shift from a focus on *diseases* to focus on *biology* also illustrates that a certain kind of research has a particularly high status in Lundbeck. Indeed, naming research units after biological mechanisms such as "synaptic transmission" suggests that it is important that research strategies are reflected in the organizational structure. When I first visited Lundbeck, I immediately noticed the strong emphasis on science. From looking at organizational diagrams, I also observed that the coordination between Research and Development was primarily seen as taking place in a joint coordination group at the level of top management. After Synapse, however, integration came to be seen as an effort that took place at all levels, as representatives of development division and clinical research were included even in the early prioritization and organization of research projects.

Again, by looking at the formal structure of Lundbeck, one might also notice an increased focus on certain activities in the years 2009-2013. Especially the areas of Alliance Management and Public Affairs were prioritized, resulting in the appointment of an Alliance Management Director around 2008-2009 and the establishment of a Global Public Affairs department in 2011. Both events illustrate an interest in linking to an external world represented by alliance partners, patients and regulative authorities, and society in general. They also suggest an acknowledgment of the fact that the invention and marketing of new drugs is not only a matter of initiating the right or most excellent research projects but also relied on preparing the ground for new drugs more generally. In Lundbeck, this involved interacting with Danish public research policy, activities

related to European research initiatives and agendas, and global perceptions and priorities related to the focus diseases.

The sense of research in Lundbeck

Entering the headquarters of Lundbeck, one is greeted by a large modern glass building. The building arches over a street that crosses through the Lundbeck site.

Illustration 2: The entrance to Lundbeck headquarters and the building housing general management (picture taken from the North Gate, Autumn 2013).

Inside, you find yourself in a large high-ceiling reception hall with modern furniture, screens and a large reception desk. The hall is connected with the lecture hall, the canteen and café area, and also with the first, second and third floors of the building where management is located. The lecture hall is large and, at first glance, it resembles a modern university lecture theatre. It is an

amphitheater with chairs in front of a large projection screen. At the stage in front of the screen is a stand for speakers. Once a year the lecture hall houses the annual Project Review where Lundbeck researchers give scientific presentations of their projects and managers evaluate the progress of the research portfolio. The reception hall is quiet, yet busy with visitors checking in and out. Going up the floors, you notice a change in atmosphere from busy to calm, professional and quiet. The hallway with manager offices appears modern with glass walls and wooden floors. In each of the vice presidents' offices there is a desk, a meeting table and a large screen on the wall for presentations. In order to get into a manager's office you pass the executive secretary that sits in a smaller adjacent office.

From the reception hall, a third passage takes you to the canteen. The canteen is situated in a large open building with high ceilings and light coming in from windows above and at the sides. Through the windows, there is a view to a pleasant outdoor area with tables and benches. At one end, there is a large buffet with food. At the center, there are tables in different shapes, round, long and single tables behind partition walls. At the other end, there is a café with high tables, soft chairs and a group of industrial espresso machines that people queue up behind after lunch. Above the café, there is an indoor terrace overlooking the canteen area. This is a more quiet and private place where people sit for meetings or department lunches. At the end of the reception hall, opposite to the reception desk, in front of the lecture hall entrance, there is a large open space. On the back wall there is a big poster of a smiling woman, a patient, who suffers from one the diseases that Lundbeck targets. This space is used for receptions and poster presentations at the annual Project Review.

Illustration 3: The main street crossing through Lundbeck headquarters. The yellow building at the center of the picture houses Molecular Neurobiology/Drug Discovery (picture taken from the management building in Summer 2013).

The street that crosses through the Lundbeck site divides a mixture of buildings. On the left side, the first building is a brown brick building that houses Finance, Legal and Business Development. Three yellow brick buildings housing the three main research activities follow: the building housing Chemistry, the building for Drug Discovery research, and the building for Non-Clinical Safety Research. On the right side, behind the large canteen building there are a number of red brick buildings that house the divisions of Patent and Trademarks, Development and Human Resources.

From the outside, the three buildings on the left side of the street that house Research look fairly similar. They are all yellow and have four floors. However, getting inside one quickly experiences different senses of research. The building

housing Chemistry has no main entrance facing the street that crosses through the site. Rather the middle of the building where you would expect to find an entrance is partly covered behind trees (see illustration 3). Instead you enter in the side of the building through a small door. This building is rather closed to the outside world. A staircase leads to the upper floors of the building. On the first floor, the hallways are narrow, the offices are small and doors are closed. In the middle of the hallway, there is an open area with coffee machines, tables and chairs. Further ahead on the left there is a crossing hallway with offices with glass walls. A laboratory facility is on the right. Here you meet technicians and researchers with white coats, reminding you that you are in a research facility.

The next building houses Drug Discovery and the department of Molecular Neurobiology. This building has a main entrance facing the street that leads directly into a hallway. This hallway is wider and more open than the hallways in the chemistry building. There are offices on both sides. In the middle there is an open space with a kitchenette, tables and chairs, an espresso machine and a bookshelf with magazines and journals such as *Nature, Science,* and *Nature Reviews Drug Discovery*. A sign over the entrance door says "The cortex café" indicating that this is where the brain researchers stop for coffee. Moving upstairs, on the first floor on the right there is another hallway with offices. On one wall there is a large Myers Briggs poster with about 20 small pictures of employees distributed in four quadrants. This tells you that you are in a place where researchers are not only evaluated by their scientific and technical skills but also seen as individuals with certain psychological preferences affecting their team performance. The divisional director for molecular neurobiology has his office here and so does the department manager. Entering into her office, you notice that it is rather small with a large desk and a small meeting table with chairs. Her desk is covered with papers, journals, diagrams, posters, meeting

minutes, and power point presentations. On the walls, there are more posters and a child's drawings. The meeting table is also covered with papers, which she pushes aside to make space for sitting. One of the sidewalls is covered with bookshelves with ring binders in various colors, books, cassettes with journals and piles of papers. The atmosphere is cozy and informal. This office could just as well be in a university; there are many signs of science – journals, posters and presentations with images and tables.

At the other end of the hallway is a lab facility. Behind the lab there is an open office space. On the right are a number of small desks where a group of PhD researchers sit, both those that are employed by Lundbeck and those employed at Danish universities but come to Lundbeck's advanced laboratories to conduct experiments. On the left, there is a larger area for technicians. Coming from the other end of the hallway, you immediately notice that this is a more lively part of the research building. The technicians move around and in and out of the laboratory, sit down at their desks, discuss with their colleagues and then move up again. In addition to this activity, you notice a significant traffic of researchers from the other end of the hallway to and from the espresso machine at the back corner of the room, often making this an occasion for making short discussion with the PhDs or technicians.

On the top floor, a large office belongs to the head of Drug Discovery. It overlooks a housing area in Valby. His desk is tidy, almost empty, with only a few small piles of paper. Opposite to his desk is a meeting table and above the table is a screen used for presenting material at meetings. In this office, you get the impression that research is to a significant degree about giving presentations on screens and discussing their content around a meeting table. The signs of research such as journals and posters are completely absent.

The third building houses Non-Clinical Safety Research. It is situated close to the south gate of the Lundbeck site. This building has a large open entrance facing the street with stairs leading up to it. The entrance has a glass front and is open and welcoming. Inside you find yourself in an entrance hall and stairs take you to the upper floors. On the second floor, where the head of Non-Clinical Safety Research sits, there is an open office space. The atmosphere is lively, employees discuss across the tables. At the top floor of this building, one finds a large bright conference room overlooking the Lundbeck site. The room is named after Vibeke Tøjner, a contemporary Danish painter that specializes in abstract paintings of landscapes. The room is used for special events as when the three main research management groups meet to coordinate conclusions from annual Project Review.

The different sense of research in the three buildings that house research in Lundbeck is noticeable. Especially distinct is the difference in how closed and open the buildings and the research activities are to outsiders. In the Chemistry building, the research seems mainly to be taking place behind closed doors though it is visible in and around the lab facilities. The building for Molecular Neurobiology is less architecturally closed and the long hallways with open and closed offices indicate that research takes place not only in labs, but also behind desks. In the research manager's offices you get the clear impression that research involves presentations on screens. The lab facility again signals laboratory research. This space opens up in an otherwise closed building. The technicians and students are crammed into a corner but nonetheless the open space here is welcoming and attracts researchers from other ends of the building. The Non-clinical Safety Research Building is welcoming and less messy. People are busy discussing things in open spaces. Here research is about coordinating activities and discussing findings and tests.

There are also some immediate signs of research that you encounter when visiting Lundbeck: the lecture hall in the reception building; the laboratory facilities in several of the research buildings some of which you can actually see from the street through the windows; physical objects like book shelves with journals indicating science; offices with papers and books and signs of activity in organizing and presenting knowledge, pictures of molecules on the walls. Isolating these observations, Lundbeck reminds you of an academic research institution. However, other signs lead your thoughts elsewhere. The large-scale canteen, the food that is served, the coffee machines, the quality of the seats in the lecture hall, the exclusivity of the reception area, the atmosphere at the management floors, and the fact that, except from around lunch time, there is very little physical activity on the Lundbeck site, no students walking to and from lectures. All of this suggests that you are in a company and that whatever research is conducted here has specific purposes, namely producing pharmaceutical drugs.

Conclusion

As we have seen, Lundbeck potentially offers a rich context for exploring how strategic research and science-industry collaboration develops in a company. Not only is it interesting to explore how research unfolds in the context of a research company, it is also particularly interesting to follow this process in situations, as in Lundbeck, where research strategies are being developed. How are particular research areas selected? How do research managers take the potential future of Lundbeck into account when initiating new research? How do different parts of Lundbeck, some research oriented and some more business oriented, work together in developing new strategies? These are relevant questions to explore in the empirical context of Lundbeck.

I now move to discuss *how* I investigated Lundbeck. This entails discussion of the particular set-up of my PhD and the methodological tools I have used to explore Lundbeck.

7. METHODOLOGY

This PhD project has occasioned many methodological questions. Some of these questions relates to its set-up as an Industrial PhD, a Danish scheme that I return to. Others relate to how I have chosen to explore strategic research in Lundbeck in a particularly active way. Over a period of five years, I spent on average 2-3 days in Lundbeck a week. This has amounted to many hours and, I hope, a deep insight in the concerns and activities of the research managers in Lundbeck. This involvement also raises a number of questions. How did I investigate strategic research in Lundbeck, using which methods and tools? How did particular research questions and findings develop? What characterized the role of my research in Lundbeck? In this chapter, I discuss these questions.

An Industrial PhD set-up

My scholarship is an industrial PhD, a particular Danish scheme that requires a short explanation. An industrial PhD is a collaboration between a company, a university and a PhD researcher. The company employs the PhD student who is simultaneously affiliated with a public research institution (in this case, Copenhagen Business School). The PhD project is acquired to have "industrial relevance"[7], which in practice can be interpreted in many ways. In my case, industrial relevance was understood as giving relevant input to an on-going process of developing best practices for external research collaboration. The Danish Ministry of Science, Innovation and Higher Education and the company co-fund the project and the company employs the PhD researcher. The industrial

[7] www.fivu.dk.

PhD scheme was introduced in 2002 to educate doctoral researchers with a particular industrial focus and to increase interaction between universities and companies.[8] Initiating an Industrial PhD project can be done by either a company, a university or by the potential PhD candidate. In the present case, I took the initiative by formulating initial research questions in collaboration with my academic supervisor. These questions concerned the implications for research managers of increased strategic research collaboration with universities. I presented my proposal to the then head of research in Lundbeck and we discussed how it might become interesting to the company. As described in the preface, there was, at that time, a more or less immediate match between my academic interests and the concerns among research managers at Lundbeck. Or at least, at this particular time, the proposed research problem was defined loosely enough to make such a match seem plausible.

Fieldwork in Lundbeck

Over a period of five years, from November 2008 to November 2013, interrupted by one year's maternity leave from July 2011 to August 2012, I studied research strategies in Lundbeck. From November 2008 to July 2011, I was actively engaged in fieldwork and I continued to spend time in Lundbeck after my return in August 2012. As a natural consequence of my employment, I was engaged in discussions about research strategies also after my return. Throughout the period, I had an office. From November 2008 to July 2009, my desk was in an open office space with other PhD students. From July 2009 to July 2011, I moved to an office in the hallway that housed the head of research and his staff. In 2011, the head of research became leader of a new area, Global Public Affairs. When I returned in

[8] www.fivu.dk.

August 2012, I joined this department and moved to an office in the building housing the reception and general management (c.f. the description of different workspaces in Lundbeck in the previous chapter).

From external collaboration to strategic research

My research focus and methods changed during the project. To begin with, my project was defined as a study of "the implications for research managers of increased external research collaboration". Approaching this quite openly, I explored what constituted research management in Lundbeck, who were seen as research managers, what external research meant and how collaboration was perceived among research managers. I explored these questions using different methods. First, I used informal conversations with employees in the Research division to get an overview of what kind of concerns and activities was related to external collaboration. This involved talking to scientists, research managers, technicians, students, and secretaries. In addition, I participated in meetings that concerned specific external collaborations and involved, for instance, planning and the making of contracts. In Chapters 10-13, I describe the activities that took place in these meetings, and below I return to how I negotiated access to them. I also conducted formal interviews with the research managers in Lundbeck who were particularly involved in making collaborations and developing strategies. Further, I eventually came to actively stimulate discussion by hosting seminars for project leaders and research managers. Thus I used a variety of methods to investigate what the implications for research managers were of the changing conditions for research.

Around midway, the emphasis of my research changed. This was in part a consequence of the initiation of the strategy program, Synapse that ran from

2009-2011 and created a new orientation to research. Because I had taken an active part in discussing research strategies among research managers, I was invited to participate in Synapse as a member of a working group. This working group had the task of considering the question of what kind of behavior might promote collaboration between Research and Development activities in Lundbeck. In Chapter 13, I discuss how the working group approached this question. At first, I saw participating in a formal working group as an opportunity to put my investigation of external collaboration into perspective. It gave me an occasion for learning about internal issues, including how employees from other parts of Lundbeck saw the role of research and perceived of external collaborations that were so central to research managers. I was also interested in the in-house implications of the new strategies that I had previously studied and of other new overall strategies. I wondered what characterized collaboration between researchers and their colleagues from other parts of Lundbeck, both in this strategy process and more generally. The purpose of exploring these questions was not to develop a comparative research design that would allow me to identify the similarities and dissimilarities between internal and external collaboration. Rather the purpose was to use this occasion actively as a background for interpreting the implications of external collaborations for Lundbeck more generally. I decided to take this opportunity, knowing that it would open up for a new world of data.

However, participating in the behavior-working group in fact became more than background information and slightly changed the overall focus of my research. Managing external research collaboration was clearly related to managing other forms of research and other relations than those purely Lundbeck external. By studying external collaboration, I had mainly focused on the interface between Lundbeck researchers and external collaborators. However, as it turned out, this

interface was difficult to isolate from in-house research practices in Lundbeck and from broader issues of managing research. I started to notice that when research managers in Lundbeck talked about external collaboration during Synapse they immediately drew parallels to in-house research, and to the Synapse ambition of connecting preclinical and clinical research, and Research and Development more effectively.[9] Thus, rather than study the specific implications for *research managers* of increased external research, I changed focus to exploring external collaboration *in the context of Lundbeck*. This meant accounting both for relations between Lundbeck research managers and external collaborators, and for relations between Lundbeck research managers and in-house colleagues. As the Synapse process highlighted, although people from Research and Development were part of the same company, they were not necessarily obviously, not to say optimally, linked.

Consequently, upon my return in 2012, I changed the general emphasis of the project from external collaboration to strategic research. Focusing on strategic research had the effect of relating and including several interesting research inquiries. First, I was interested in the relation between research managers in Lundbeck and external collaborators. This also implied an interest for how external collaboration was related to other research activities. Second, I was interested in the relation between the divisions of Research and Development in Lundbeck. Although they were part of the same company, employees in Research and Development clearly saw research in very different ways. Finally, I was interested in the relation between Danish public research policy and strategy making in Lundbeck. For obvious reasons, strategic research implied quite different things in these two contexts. Thus, it was interesting to see how, in

[9] I Chapter 12, I describe how Synapse developed a new type of concern for external collaboration.

policy, companies' interests were embedded in the notion of strategic research, whereas research managers in Lundbeck defined relevant and strategic research as, to some extent, more similar to independent research. Hence, defining my object of study as strategic research had the effect of pulling together a number of contexts and interfaces.

Based on this account of how my focus and object of study developed in the process of studying it, how might the general approach be characterized? As described, I used a number of methods associated with ethnography. But I was also involved in a very active sense, which requires reflection. I now describe how my methodology changed during the fieldwork.

Following the actor

To explore external research strategies in Lundbeck I initially used a method that, following Bruno Latour's early actor network theory, might loosely be referred to as 'following the actor' (Latour 1987). This method implies following an object or problem as it travels between the lab and the outside world. As I saw it at that time, I investigated the implications of increased external collaboration in 'the laboratory of Lundbeck'. Using this method of following implied a quite open approach to what constituted external collaboration in Lundbeck and to where to find it. In principal, although my problem was predefined as studying implications for research managers, I might find that external collaboration was an activity that rather occupied students and technicians. However, my PhD set-up to an extent restrained how open-ended I could be. As the PhD was sponsored by research managers and had, at least to some extent, to fit their concerns about strategies, making a lab study among students and technicians did not seem an obvious thing to do. Thus, rather than follow external collaboration into the labs

of Lundbeck I primarily followed it into a 'managerial lab'. This entailed following management discussions of new collaboration, strategies, and visions.

However, although the method of following the actor provided me with a usefully open-ended approach to what might constitute external collaboration, it also turned out to be difficult to use in my case. In the end, it did not very precisely characterize what I did. I was *following* to be sure, but I was also participating, hosting, leading, making drafts and outlines, participating in teamwork in workgroups and actively interfering in discussions. On the one hand, this was due to the research managers' expectations. They asked for my opinion in particular cases, for example querying how I saw external collaboration based on my experience with public policy and requesting that I drafted recommendations for how to govern external collaboration. There was a manifest difference in *following* what research managers did and *making recommendations* for what they should do. I did not have much of a problem with making presentations and recommendations. Based on my former experience and my growing familiarity with Lundbeck's management this was relatively easy. The problem was rather the methodological questions raised by this engagement. What was the nature of what I studied when I was involved to such an extent? Would I be able to distance myself from it to the extent that following suggested was necessary? Based on this, I continue to discuss my approach in relation to the method of participant observation.

Participant observation

As mentioned, the notion of following the actor was slightly problematic, since it suggested something more open and passive than what I actually did. To emphasize my role as a participant I started to characterize my main method as

participant observation. Participant observation is a qualitative research tool and the main ethnographic approach used in a number of social science fields such as, for instance, social anthropology. Using this method, the researcher aims at getting a close familiarity with a practice or culture over an extended period of time (Hammersley and Atkinson 1983; Ybema et al. 2009; Neyland 2008).

In many respects, describing my engagement as participant observation makes sense. Even so, this characterization also does not fully capture what I did. After all, I was not only participating in order to be able to *observe* action, I was also *stimulating* action. Consequently, if it this was indeed participant observation the strong emphasis was on participation. So how might I describe my approach? How did specific inquiries develop in the process of studying Lundbeck? What guided my selection of empirical material? In the following, I discuss three concepts that became important for my answers to these questions.

Ethno-epistemic assemblages

First, I drew on Alan Irwin and Mike Michael's notion of 'ethno-epistemic assemblages' to reflect on my methodology (Irwin and Michael 2003). The term requires some explanation. 'Ethno' refers to local and situated knowledge. 'Epistemic' highlights the nature of knowledge as truth claims about the world. Finally, 'assemblage' describes how diverse forms of local knowledge that are sort of *pulled together* constitute a phenomenon. Irwin and Michael introduce the notion as a pragmatic, empirical research tool. For them, studying something as an ethno-epistemic assemblage implies a focus on the empirical constitution of a phenomenon. In contrast, I have found this term useful for reflecting on methodological questions. Especially, I have found the term helpful in order to explain how my object of study developed in the process of studying it.

Thus, I would now suggest that rather than constitute an interface between two worlds, Danish public research policy and Lundbeck's research practices are part of the same ethno-epistemic assemblage concerning strategic research. Both have particular practices and ways of projecting research and university-industry collaboration. Rather than focus on one part of this assemblage, I have focused on the relations between several parts such as Danish research policy and Lundbeck. This also means that more practices might be seen as part of this assemblage. I have noticed how the different parts of the assemblage are embedded in different practices and activities related to strategic research but nonetheless also relates in quite different ways. For example, the research strategies that developed in Lundbeck's research division before Synapse, in the years 2005-2007, were not unaffected by Synapse that aimed at closely connecting different parts of Lundbeck, and potentially also different local strategies. Likewise, Danish research policy is not unaffected by ideas about research and innovation that emerge in the context of industry.

I have approached this assemblage of strategic research by looking at both the constitution and organization of strategic research in Danish research policy; prevalent academic discussions in the field of research policy and strategies and practices in Lundbeck. First, I focused on how Lundbeck research managers were related to external academic researchers. However, spending time in Lundbeck opened different kinds of in-house science-industry collaboration that also contributed to a description of the assemblage of strategic research.

How specifically has the notion of ethno-epistemic assemblages guided my reflections on my object and role as a researcher? First, I view my object as an assemblage, and as something continuously *being assembled*, rather than as a well-defined and clearly delineated object. This has opened up for connecting diverse empirical (situated) knowledge about strategic research rather than

focusing on one ideal type. While being an excellent tool for opening up and making connections, approaching something as an ethno-epistemic assemblage also, of course, requires making certain decisions about what to take analytically into account. In the next section, I return to how I made these decisions in collaboration with people from Lundbeck.

Second, the notion of ethno-epistemic assemblage has guided how I have seen my own role as a researcher. Rather than conceiving of myself as an observer of a strictly defined research object, I have adopted a pragmatic approach and made decisions according to what has been *interesting* and *feasible* within the set-up of my research. Being employed by Lundbeck made certain things possible and other things impossible. It would probably have been quite inappropriate, and also quite irrelevant, to come with a strong critique of the research management in Lundbeck since I was working with them. Thus, deciding what was interesting research was not a completely open matter but also related to what research managers in Lundbeck found interesting. Therefore, I have not seen my role as *enforcing* a particular research scheme into a set-up that was not geared for this. Rather, I have seen my role as exploring an ethno-epistemic assemblage consisting of many potentially interesting practices and relations. I have also brought a particular knowledge into this, while investigating it.

In the beginning of this chapter, I described how my research focus developed in the process of studying Lundbeck. I now see this development as the outcome of a particular methodological approach that is different from following the actor and participant observation. To describe this approach, I draw on Joan Fujimura's (Fujimura 1987, 1996) notion of constructing 'doable problems'. Fujimura originally developed this notion to describe how researchers with different backgrounds managed to work together. Besides its value for the analysis of scientific collaboration, which I return to in Chapter 9, I also see this notion as

relevant for reflecting on methodological issues. In particular, I have found it useful for describing how my research developed in a collaboration that took place between myself and the research managers at Lundbeck. Indeed, I think that this collaboration can be reasonably characterized in terms of developing doable problems within the company.

Constructing strategic research in Lundbeck as a doable problem

According to Fujimura, doable problems are socio-technical achievements (Fujimura 1996: 10). They are the outcome of particular events and conditions, some of which researchers control and others they do not. Fujimura highlights the often invisible 'articulation work' that is related to making research feasible. She describes this as the "the amorphous and ambiguous work of planning, organizing, monitoring, evaluating, adjusting, coordinating and integrating activities" (ibid. 11). We often see such activities as *administrative* rather than *scientific* and they are often not viewed as creative and important for research. However, in the process of constructing a doable problem *content* and *context* are not separate but begin to merge. So how does the notion of doable problems potentially shed light on methodological questions and on how the focus on my research developed? What characterized the way my research project developed as a doable problem in the context of Lundbeck? What kind of merging of context and content took place?

The way in which my research focus gradually changed illustrates the ongoing construction of the do-ability of my project. Particular events and circumstances were important for rendering my project feasible. My project was initiated based on an initial discussion of what would be relevant and interesting research questions. As mentioned, I presented my academic interests at a first meeting in

Lundbeck. Initiating the project became possible because my questions were quite open. For instance, I asked: what are the implications of increased external research for research managers? Because the questions were quite loosely defined they relatively easily connected with emerging interests among research managers in Lundbeck. In this phase, making my PhD project *doable* involved developing a research proposal that would be peer reviewed by social scientists and at the same time make sense to research managers in Lundbeck. Thus the project did not initially become doable as a consequence of posing an excellent research question but rather by posing a quite open question that could be interpreted in diverse ways.

Then the fact that I was employed in Lundbeck made certain inquiries and approaches possible that would have been very difficult to pursue under other circumstances. It became possible to ask questions that required entering the intricate processes of managing research in Lundbeck. In addition, it became possible to develop an open-ended approach in which specific inquiries developed in the process rather than up-front. Had I not been employed in Lundbeck, I would have had to negotiate access to Lundbeck based on a more precisely defined initial research interest and research design. Of course, access would also have been more restricted.

Although my employment in Lundbeck implied certain favorable conditions in terms of access, access was in practice a matter of negotiation. Often access was negotiated in relation to *meeting participation*. Since I was spending a lot of time in Lundbeck, it was possible at the last minute to decide whether I should participate in an upcoming meeting or not. A decision was often made based on a quick conversation with the head of research about whether participating in the meeting would benefit my research or benefit the participants because I could provide inputs on matters of concern. Sometimes we agreed on the relevance of

participating and at other times we simply disagreed. This could go both ways, I could resist participating in a meeting because it was not sufficiently relevant and took up too much time. Or they, usually the head of research, would argue that a meeting that I was otherwise interested in was not relevant, thus implicitly also making an argument for what my research mainly concerned. Thus although I was employed and this gave some favorable conditions negotiation continued to be necessary.

Developing the focus and content of my research was thus related to different kinds of articulation work. In particular, such work was important in relation to meetings, as which meetings I participated in and how, indicated what my research concerned. Also, articulation work concerned the nature of my research and how it contributed to Lundbeck.

Articulation work and access to meetings

As mentioned, much of my fieldwork consisted in attending meetings. In Chapters 10-13, I describe a number of meetings and particular events that took place during meetings. In the next section, I describe how these events became crucial to my analysis. The first meetings in which I participated concerned collaboration with external partners. What took place at these meetings might in fact be seen as a kind of articulation work that made particular collaborations doable. These meetings involved, for instance, discussion of plans, presentations of purpose and strategies, financial structure, and contracts. To begin with, I was invited to participate in meetings in the Research division that concerned particular ongoing external collaborations. These collaborations were all in different ways seen as challenging and so my research came to concern external collaborations that were viewed as particularly surprising or difficult to handle.

As Synapse was initiated, it was a little unclear how this strategy process related to my research. This again involved intensified articulation work on my part. I described how my research related to Synapse and how I planned to use participating in Synapse as important background information. My articulation work was particularly concerned with integrating my research and ongoing events at Lundbeck by making strong relations between my research interests and developing activities in Lundbeck.

The process of discussing my participation in Synapse also involved articulation work on research managers' part. One research manager suggested that my involvement in Synapse implied a significant change in research focus as he saw my research as primarily concerning external collaboration in Research. Another research manager argued that participating in Synapse would, in fact, strengthen my research, since any form of additional insights into Lundbeck would be valuable for my education. Another research manager suggested that participating in Synapse would be an opportunity to contribute directly to the strategy work, based on my insights from studying research in Lundbeck. In spite of these different ways of articulating my research, developing an interest for Synapse also involved new issues in terms of access. Getting access to meetings and people had not previously been an issue. However, since Synapse involved many different divisions of Lundbeck getting access was a little more complicated. It required having a quite clear contribution and a clear reason for being there, which in part was up to me to develop.

In the beginning of 2010, I made a number of interviews with research managers about the implications of Synapse for external collaboration in research. Towards the end of 2010, the discussions about my potential participation in Synapse resulted in an invitation to participate in a working group on project leader competencies. Chapter 13 describes this process in detail. Being a member of this

project leader-working group became a platform for subsequently participating as a member of a formal Synapse working group concerned with collaborative *behavior* of leaders. In this process, my research focus developed in a negotiation process in which external collaboration became increasingly linked with project leadership. For now I simply highlight that making my research project a doable problem in the context of Lundbeck involved articulation work that was particular intense in relation to meeting participation. Negotiating access to meetings required far more articulation work than making interviews. In these negotiations, it became clear that it was possible to see the purpose of my research in different ways.

Articulation work and the nature of my research

I also engaged in intense articulation work in relation to the role or impact of my research. In particular, there were two ways in which the role of my research was negotiated. As noted, I started out with the intention of simply exploring, describing, and analyzing how external collaboration developed in Lundbeck. However, at several occasions I was also asked to give specific recommendations. I illustrate this with a quick example and return to a more elaborate discussion of my role in Chapter 13. In May 2009, I hosted a seminar for the Research Management Board (RMB) on external collaboration. It was called "Future Models of Collaboration". Hosting this seminar involved stimulating discussion about the challenges of external collaboration. In this seminar, individual members of the RMB gave presentations about the challenges they saw, specifically in relation to the *governance* of external collaboration. After the seminar, the head of research invited me to make a white paper that outlined best practices for external collaboration in Lundbeck. Making this white paper involved integrating a number of things such as the RMB members' observations and discussions, my

observations from the seminar, my own ideas based on my research and my former experiences with governance and management of research from public policy, and transforming all this into recommendations. However, although the head of research had an interest in presenting this white paper as an outcome of my research, I wanted to play down my role because the white paper also contained a mixture of the RMB members' *experiences* that were not exactly a result of my *research*. However, downplaying my role was not uncomplicated. In the process, it became obvious that presenting this white paper as a result of my research had the important function of legitimizing my role as a researcher in Lundbeck. My research was seen as leading to different things, some of which were early operational recommendations. This was but one of several situations in which I produced lists of recommendations or described models that were seen as part of my research results. As I saw it, these were indeed results of my research; however, they were not the end results, but rather necessary intermediary points that would enable me to retain the *doability* of the project.

Towards the end of my PhD, I experienced a different form of articulation work about the outcomes of my research. This articulation work was related to the event of making a joint paper with my academic supervisor, Alan Irwin, and Peter Høngaard Andersen, a Lundbeck research manager (see appendix A). The paper was published in *Nature Reviews Drug Discovery*, a journal that is widely read among research managers in Lundbeck. In Lundbeck, the comment was received well. Within the first week after its publication, several research managers contacted me with congratulations. When I asked them what they thought about the comment, several of them said, "it *describes* very well how we work". This was of course a positive response; however, I was curious about this idea about *description* and how more precisely they viewed the specific impact of my research. I discussed this question in detail with one senior research manager. As

I had expected, he argued that it was difficult to pinpoint the exact *impact* of my research. This led to an interesting discussion of whether I had, in fact, merely *described* how they worked in Lundbeck and thus had acquired an industrial perspective. Or whether I had in fact helped to *shape or change* particular strategic discussions. Without being able to answer this question unequivocally, it is noteworthy that in some situations making my research *doable* required making it very operational and instructional, while at other times it was *doable* because it was merely descriptive and not in fact changing anything.[10] This topic also feeds into the potential role STS research, to which I return in conclusion.

The notion of constructing doable problems opens up important questions concerning the articulation work it takes to define and develop a research projects and research agendas. At times, it has indeed made more sense to understand my research in terms of research collaboration rather than a researcher/informant relationship. The content of my project has developed according to the present conditions and while I have been quite an active researcher, so too have research managers in Lundbeck also actively given input to my research and indeed to my role as a researcher. I end this chapter by introducing a third notion, 'disconcertment' that I have used as a data-selecting tool. This notion helps me to explain my way of focusing in the many meetings and discussions that make up my data material.

Disconcertment

The notion of 'disconcertment' relates to classic methodological problems of encountering and analyzing the field. In STS, Helen Verran has developed

[10] None of the research managers argued that my research had *no impact* but it was rather difficult, also for me, to pinpoint what exactly it had changed and produced.

disconcertment (Verran 1999; 2001) and recently others have taken it up. Thus, in "Cultivating Disconcertment", John Law and Wen-yuan Lin argue that disconcertment is a valuable methodological tool for exploring the *intersections* of different knowledge forms (Law and Lin 2011). According to dictionary definition, to disconcert means, "to upset the progress of".[11] It comes from *desconcerter*, which means *des-* (expressing reversal) + *concerter* "bring together", hence the connotations of disharmony or deconstruction.

Verran discusses the notion of disconcertment in relation to fieldwork in Africa, where she studied diverse ways of teaching mathematics. During fieldwork, she observed teaching based on completely different conceptions of number, which disconcerted her Western, mathematical understanding. According to Verran, disconcertment marks moments of "double seeing", such as seeing the "normal" notion of length, presented and performed in a strange way in an African math class (Verran 2001: 5). Thus disconcerting "double seeing" occurs in consequence of a comparative moment, where one experiences both sameness and difference.

Verran argues that such odd moments can be deployed strategically. "This disconcertment, source of both clear delight and confused misery, must be privileged and nurtured, valued and expanded upon." (Verran 2001: 5). Even if one's first intuition is to try to "explain" (away) these moments by searching for a sense of underlying coherence, Verran encourages sustained exploration of just what makes them disturbing. "It is easy to ignore and pass by these moments—part of the problem is their fleeting subtlety—yet it is possible to become acutely sensitized to them" (ibid.). Using a terminology, which I develop further on in this thesis, I propose that disconcertment is produced where different forms of

[11] www.oxforddictionaries.com.

screening collide, which will make much more sense when I introduce the notion of screens in Chapter 9.

Verran presents disconcertment as a tool that can be used by analysts to study somewhat awkward or embarrassing situations. As she argues, disconcertment is something that the analyst can gradually become sensitive to and become able to use analytically. Thus, Verran encourages the analyst to develop a sensitivity to one's own discomfort, using "bodily disconcertment" as an expression of what might be called metaphysical disjuncture (Law and Lin 2011: 137). That disconcertment is located in the body obviously troubles any easy aspiration to deploy it strategically or rationally. Its usefulness depends on articulating experiences that are per definition unforeseeable, caused by particular encounters in the field. But even so one might also think that different research conditions might create more or less optimal circumstances for both perceiving and experiencing disconcertment. Since disconcertment is produced not only by great differences but also by subtle dissimilarities of presumptions and expectations, we can also assume that it requires quite close relations with the people and things one wants to investigate.

In the following analyses, however, I attend to disconcertment in a somewhat different way than Verran recommends. Rather than only using my own disconcertment, I am also interested in *public* moments of disconcertment, sensed both by the analyst and people in the field. One might see this emphasis as focused on a collective, even epistemic form of disconcertment in which a group of people has a shared experience that something is out of order. Verran argues that a disconcerted laughter should be taken seriously as an indicator of tensions brought to light by particular situations, and she refers to her own "visceral laughter" (Verran 1999; 2001: 5). But one might also observe situations of collective laughter. Moments of awkward laughter, I suggest, offer an entry point

for understanding the collective sense of what is normal and straightforward in a situation, since it indicates a disruption of the normal. A focus on *collective* disconcertment also assumes that the analyst is not entirely or alone responsible for creating or registering these moments but that they might be present in situations where the unusual appears and is dealt with by a group.

It is in this sense that I use disconcertment as a methodological *selecting and organizing device*. The empirical situations that I discuss in the Chapters 10-13 all have in common that they elicit moments of disjuncture: at various times, managing external collaboration, collaborating with in-house colleagues, and defining new strategies presented generated such moments of disconcertment. Whereas STS analyses often focus on the mundane and normal (Woolgar and Neyland 2013), I suggest that focusing on instances of *misalignment* is also a way of opening up for an understanding of what constitutes the normal.

Conclusion

In this chapter, I have described the methods and tools that I used to explore strategic research in Lundbeck. The Industrial PhD set-up has given me an opportunity to get close to the thesis' matters of concern. To further take advantage of this opportunity, I have deployed an open-ended approach with a strong emphasis on participation. Rather than primarily observing, I have taken active part in discussions at Lundbeck. As a consequence of deploying such an approach, my object of study changed in the process from external collaboration in the research division of Lundbeck to strategic research in Lundbeck at large. I think of this object of study as an ethno-epistemic assemblage. By defining this assemblage broadly as strategic research I have been able to pull together a number of diverse practices of strategic research. Consequently, I have not only

looked at contrasts between Danish public research policy and research practices in Lundbeck, but also explored contrasting research practices in Lundbeck related to collaboration with external academic research groups and in-house colleagues.

Rather than implement a predefined research agenda, my main methodology is inspired by the idea of constructing a doable problem. I have participated in a research collaboration that developed pragmatically according to what was interesting and doable for diverse collaborators. Perhaps this sounds easy enough, but it involved significant articulation work related both to access and to the role of my research as simultaneously highly instructive and merely descriptive. I suggest further that part of what made the project doable was my willingness to let it adapt to such different outcomes and purposes. Finally, I use the notion of disconcertment as a selecting and organizing device. Because of the nature of my fieldwork, I not only use my own disconcertment but also draw on public and shared experiences of disconcertment. I want to take forms of disconcertment seriously as valuable sources for developing new understandings of strategic research.

I now take a step back to explore how strategic research and ideas about science-industry collaboration have developed in the context of Danish research policy. The purpose of this is to provide an understanding of the Danish national discussions of strategic research that both relate to concerns in Lundbeck and suggest quite different categorizations of research than what we have seen in Lundbeck. So how is strategic research defined and organized in Danish research policy?

8. STRATEGIC RESEARCH IN DENMARK

In Danish public research policy, 'strategic research' has a taken on a particular meaning. Not only is it seen as a particular form of research that can be distinguished from basic, independent research. Presently, strategic research also has a separate organization with its own councils, program committees and secretariats. So what characterizes strategic research in a Danish research policy context? Which practices and considerations are related to the current organization of strategic research as separate from basic, independent research? What is the background for this particular Danish construction in which strategic research has developed as a distinct domain? These are the questions explored in this chapter. Its aim is to clarify how I see the relation between the Danish national context of research policy and Lundbeck, a global pharmaceutical company.

From one perspective, Danish research policy and Lundbeck inhabit two separate worlds. Managing research is obviously a quite different task depending on whether it takes place in public institutions or in a private company. Emphasizing the different worlds of Danish research policy and Lundbeck is useful because it immediately requires that we make explicit what is particular about each of these worlds. Being able to shift back and forth between Danish research policy and Lundbeck is analytically helpful in terms of pinpointing particular traits and differences. Even so, my main emphasis is on the research management practices of Lundbeck. Thus, the world of Danish public policy serves as an important background for this exploration. Yet, the key notion of strategic research comes from public policy. It is not a widely used term in Lundbeck where managers rather talk about "research strategies" or specific "strategic research areas". The

structure of the thesis reflects this difference as policy is mainly described in this chapter and separated from discussions of Lundbeck practices introduced before this chapter and in the following empirical analyses.

Even so, as mentioned, I also see Danish public policy and Lundbeck as part of the same field, or ethno-epistemic assemblage (Irwin and Michael 2003). In Danish public research policy, we find very specific ideas about *companies* and what they want from the public sector, for example in terms of educated candidates and relevant research. In Lundbeck, too, we find research managers that are deeply engaged in public research policy. For instance, in Lundbeck I noticed that two members of the Research Management Board (RMB) were in fact also members of specific councils and thus also dealt with public policies in a quite direct sense.

Not least, I see Danish research policy and Lundbeck as connected in relation to one particular idea. This is the key idea that university-industry collaboration is a key tool for creating innovation and growth. In public policy, university-industry collaboration has been introduced as a main driver of national economic growth and as a tool for addressing particular societal or industrial problems. Research managers in Lundbeck agree that university-industry collaboration is an important part of research strategies but emphasize a different main purpose of research collaboration. They propose that academic collaboration is important for developing new platforms for innovation with long-term effects. Building platforms is quite different from solving problems. In this way, a shared interest in strategic use of university-industry collaboration paves the way for a potentially intriguing difference in how the main purpose of these collaborations is imagined. These similarities and differences are central to the exploration of this thesis.

To elucidate the current Danish research policy situation, I draw on official documents, law materials and background discussions that illustrate the current

Danish construction. In particular, I focus on the law of 2004 that established the Danish Council for Strategic Research (DSF) and on a set of comments to the 2004 law that describes the purpose of particular elements in the law. I also draw on website material from the councils and foundations that govern strategic research in Denmark. This material shows how strategic research is administered and which elements of strategic research are given special attentions. In addition, I draw on public debates about strategic research in media and at conferences. Finally, I draw on my knowledge as a former employee at the Danish National Advanced Technology Foundation (DNATF) established in 2005-2006 as part of a new organization of public research funds that gave strategic research a particular importance. As noted in the introduction, the DNATF was established to rethink and renew existing ways of managing publicly funded research. Being part of this process implied constant articulation of the differences between managing public research in the context of the DNATF and existing practices of administering research developed in the context of the Danish Agency for Science, Technology and Innovation (DASTI).

Drawing on this material, I explore the current organization of Danish public research funding. This organization is currently changing in important ways as the Danish political parties have just (this autumn 2013) agreed on a new reform of strategic research in Denmark. Although apparently radical, however, the new reform is based on some of the same principles that have characterized the Danish research advisory and funding system since 2004. Thus, it remains relevant to explore the emergence of the idea of introducing strategic research as a separate field in Danish research policy. I do so by analyzing the basic models of research that have characterized Danish research policy making in recent years. Finally, I explore how managing strategic research became a concern in Danish research policy. How, then, do current policy ideas about research management

characterize strategic research? And how is research management presented from the perspective of policy? But, first of all, what actually is meant by strategic research?

Strategic research and free research

In Denmark, the current public research advisory and funding system is based on a distinction between 'strategic' and what is often termed 'independent' research.[12] In fact, in Denmark, the term 'free' is often used interchangeably with independent, hence the Danish construction "Det Fri Forskningsråd".[13] The distinction between strategic and free research was introduced and formalized by law in 2004 (Ministeriet for Videnskab 2003a). Since then, the Danish research advisory and funding system has been organized according to this distinction (DSF 2013). I start out by considering the present system and its main councils and committees.

Today, the core of the Danish research advisory and funding system consists of the Danish Council for Strategic Research (DSF) and the Danish Council for Independent Research (DFF), both of which were introduced with the 2004 law. The DSF has seven program commissions, each of which defines a field of strategic concern: 'Education and Creativity'; 'Health, Food and Welfare'; 'Individuals, Disease and Society'; 'Peace and Conflict'; 'Strategic Growth Technologies'; 'Sustainable Energy and Environment'; and 'Transport and Infrastructure'. Similarly, the Danish Council for Independent Research (DFF) has five sub-councils covering 'The Humanities'; 'Natural Sciences'; 'Social Sciences';

[12] www.fivu.dk.

[13] The official English translation is "The Danish Council for Independent Research". But in Danish, the term "free" ("fri") is used instead of independent; hence a more precise translation would be "The Danish Council of *Free* Research".

'Medical Sciences', and 'Technology and Production Sciences'. In addition to the DSF and the DFF, there is the Danish Council for Technology and Innovation (RTI), which has an advisory function and administers a number of strategic initiatives, including the Industrial PhD scheme. In addition to DSF, DFF and RTI, the Danish system has two public foundations. The Danish National Research Foundation (DNRF), which was established in 1991 and funds centers of excellence, and the Danish National Advanced Technology Foundation (DNATF) that was established by a separate law in 2004 to fund research collaborations between companies and universities. It is also relevant to mention the Danish Council for Research Policy (DCRP) that advises the minister on research related topics and the Danish Committee on Scientific Dishonesty (DCSD), which makes decisions in cases of potential fraud. Finally, the 2004 law established a coordination committee with the task of coordinating the diverse research funding bodies, offering advice on activities and, in general, attempting to keep the entire system working together.

Before 2004, the organization of the Danish research advisory and funding system followed a traditional university faculty structure (Ministeriet for Videnskab 2003b: 13). Thus, there were five national research councils, representing the 'Humanities'; 'Natural Sciences'; 'Social Sciences'; 'Medical Sciences', and the 'Technology and Production Sciences'. The individual research councils were responsible for funding research *as well as* defining strategic initiatives. Thus, each council had a strategy function that identified areas of strategic interests and developed strategy plans (ibid. 10). Accordingly, strategic research was an integrated responsibility of the scientific councils, rather than a distinct unit.

Which concerns and notions of research then led to the reorganization of strategic research around 2004-2005? According to the notes and comments accompanying the 2004 law proposal, the reorganization of the Danish research system was based on a number of emerging concerns. One important issue was

that the existing system, in which strategic research was an integral part of a research field, did not support strategic research "on its own terms" (ibid. 11). This was a problem because strategic research was seen as unique form of research that *thrived* under particular conditions. To promote and nurture such conditions, the solution was to separate strategic research entirely from non-strategic research.

Another concern had to do with the question of how to coordinate the increasing number of councils and committees that had separate purposes and covered different types of research (ibid. 9). As a separate area of research, strategic research had emerged as something of an umbrella concept that held together "strategic, applied as well as industrial research" (ibid. 12-14). Danish policy makers suggested that to simplify the system, while still facilitating the making of relevant categories, the number of program committees should be kept as low as possible. For this reason, a coordination committee was established to deal with the question of how to align the individual councils. In particular, the issue was that within the areas of strategic research, several councils and funds had overlapping mandates so that the need for clarifying individual missions was growing. These two concerns – how to treat strategic research on its own terms and how to maintain alignment in an increasingly diversifying system – materialized into the invention of the Danish Council for Strategic Research and a coordination committee.

In December 2012, the new Danish government of 2011 announced a new innovation strategy called "Denmark – a nation of solutions" (Regeringen 2012), also referred to as the "Innovation Strategy". This strategy proposed an extensive reform of the Danish research advisory and funding system, focusing in particular on *strengthening* strategic research in various ways. It mainly did this by collapsing several councils and funding bodies dealing with strategic research

into a new entity named the Danish Innovation Foundation. The Innovation Strategy stated that international comparison showed the Danish research advisory and funding system to be functioning well. However, taking into account the small size of the country, the system was still too complicated with too many councils and foundations (ibid. 12). According to the Innovation Strategy, the existence of several overlapping research councils and foundations was the results of gradually introduced changes. Each council and foundation had been designed to solve a particular problem without considering the implications for the system as a whole. Although the existing councils and funding bodies had each played an important role in the system, it was presently difficult to the make coherent *strategic* prioritizations *across* the system due to its complexities. In particular, it had become difficult to coordinate and integrate activities within strategic research. Consequently, the Innovation Strategy suggested merging the councils and foundations supporting strategic research into one entity, the Danish Innovation Foundation (ibid. 13).[14]

Based on this short exploration of how strategic research has become a significant notion in Danish research policy, I would like to emphasize how the definition of strategic research developed from *contrasting* it with a particular idea of academic research. First, strategic research was associated with topics of current political interest: a certain research set-up that implied collaboration between universities and companies and a problem-oriented approach that aimed to solve particular problems (DSF 2013). In contrast, academic research was defined as taking place exclusively at universities (ibid.). As noted, the comments to the 2004 law emphasized that strategic research should be attended to "on its own terms", even as the requirements of free research should also be met (Ministeriet for Videnskab 2003b: 11). This formulation suggested a distinction between strategic

[14] Danish politicians are currently negotiating the specific terms of this new fund.

and free research in which the two forms of research were imagined to succeed under quite different conditions. The assumption was that these conditions were most effectively protected *if kept apart*.

Further, the then existing system of public research was based on an on-going effort to define strategic research by cutting it into smaller and smaller pieces. For instance, the comments to the 2004 law made use of a differentiation between strategic, applied and industrial research, while also emphasizing that these were, in some sense, similar forms of research. We might thus say that the new Innovation Strategy took a new approach to this by merging all former subcategories of strategic research into one.

Another notable tendency that has continued between 2004 and 2013 is a strong interest in coordinating strategic research by making the system flexible and coherent. Overall, the Innovation Strategy suggested that coordination improves the system as a whole and makes it easier for the government, researchers and companies to interact with the public funding system. However, based on both public debates and internal discussions among policy makers in Denmark, it also appears that coordination is not all that easy. Despite numerous attempts to get strategic research right, it remains rather unclear what strategic research implies. Illustrative of this, the current reorganizations of the public research system are mainly dealing with strategic research while it is somewhat taken for granted that we know what basic, independent research is. In spite of the ambivalences of strategic research, however, several important characteristics are recurrent, and I now turn to a description of these traits.

Relevance and strategic quality

Strategic research is generally associated with the criteria of *relevance* (DSF 2013). For instance, the Danish National Advanced Technology Foundation (DNATF), one of the councils and funds that cover strategic research in Denmark, has three main selection criteria for funding research projects. One is "obvious industrial potential" which means that the research is seen as appropriate to an industrial end goal.[15] The Danish Council for Strategic Research (DSF) defines relevance more broadly as "the relevance of the research project for the societal challenges that founds it" (DSF 2013: 3). In contrast, Danish independent research is associated with researchers' freedom to choose topic, methods and theories, hence the particular notion of free research. Independent research is associated with *excellence* in contrast to relevance. The notion of relevance sometimes occurs in descriptions of independent research but then it addresses the relevance of the researcher for carrying out a certain research project (DFF 2013).

In a Danish policy context, the distinction between relevance (to society or industry) and excellence of the research (and relevance of the applicant to the research) has raised the question of whether strategic research implies less scientific *quality* than basic independent research. Sometimes raising this question assumed that research *relating to* society or industry somehow compromises quality. The question of the quality of strategic research tells us a lot about the basic assumptions at play in Danish research policy. For instance, questioning the scientific quality of strategic research seems to suggest that quality is not a *relational* matter but rather an *inherent* quality that can be predicted, identified and measured. In recent years, these discussions have been

[15] www.hoejteknologifonden.dk/ansoeger/udvaelgelseskriterier.

reflected in the way councils and foundations covering strategic research in Denmark have defined their selection criteria. One of the criteria for selection suggested by the DNATF is defined as "research and innovation of high international standard",[16] suggesting that strategic problem solving requires high quality research. In a similar manner, and even more explicitly, the DSF has introduced a new concept in Danish research policy, which they call *'strategic quality'* (DSF 2013). According to the DSF, strategic quality is composed by "the relevance of the research", "the potential effect of the research" and "the research standard" (ibid. 3ff.). In this definition, as in the DNATF's definition of related criteria, quality is folded into the idea of strategic research as a component at the same level as societal and industrial relevance.

Concepts such as strategic quality illustrate what we might think of as the fundamental ambiguity of the concept of strategic research. On the one hand, the organization of Danish public research is based on a clear separation of strategic and basic research, and of relevant and excellent research. Public discourses, events and debates often refer to this separation as a natural given.[17] On the other hand, in recent years, we have initiatives and new definitions such as strategic quality that clearly questions the separation of strategic and essential qualities of science. These tensions inherent in the notion of strategic research raise a number of questions of key importance for this thesis. Can strategic research not be excellent? And can excellent research not be strategic? Further, what is the role of companies in such research? A recent report published by the European Science Foundation, "Science in society: caring for our futures in turbulent times" addresses this matter directly and argues for "linking excellence to relevance and responsibility" (Felt et al. 2013: 4). However, although there are these initiatives

[16] www.hoejteknologifonden.dk.
[17] See Vedel and Gad 2011.

that somewhat soften the terminology and link domains otherwise assumed to be distinct in new ways, it is characteristic of the Danish situation that excellence and relevance are generally seen as separated and descriptive of different research practices.

Linear models of research

In Danish research policy, strategic research is often presented using a linear model. An example of such a linear model was recently used in the recent Danish Innovation Strategy of December 2012 to explain the reorganization of the Danish research system. See diagram below that I also showed in the introduction.

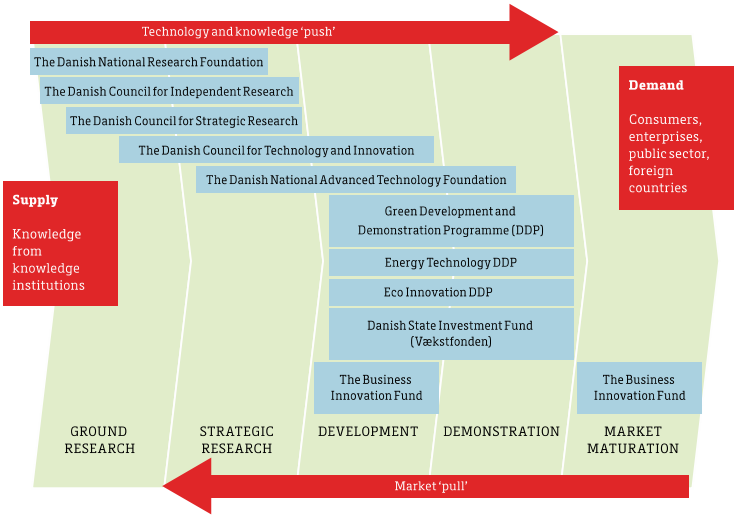

Illustration 4: The linear organization of the Danish public research advisory and funding system (Regeringen 2012)

What does this model illustrate? The model shows a spectrum of research types ranging from basic research on the left to market maturation on the right. The model then suggests that the market, exemplified by "consumers, companies, the

public sector and other nations", represents a demand and that "knowledge institutions and companies" supply knowledge to meet this demand. It proposes a dynamic that is constituted by a science and technology push and a market pull, illustrated by the red arrows. In the model, the main public councils and foundations are located from left to right. On the far left is the Danish National Research Foundation (DNRF) that funds centers of excellence. Here, we also find the Danish Council for Independent Research (DFF) that operates in the same field. The three councils and foundations that support strategic research follow: the Danish Council for Strategic Research (DSF), the Danish National Advanced Technology Foundation (DNATF) and the Danish Council for Technology and Innovation (RTI). At the far right closest to the market we find the Growth Foundation, a state investment fund[18]. Although the model differentiates between different forms of research using a scale from basic research to market it also illustrates potential overlaps between adjacent research councils and foundations. However, because the model is *linear* it does not facilitate discussion of overlaps or shared practices between forms of research that are situated far from each other on the scale. Certainly, the policy notion of quality potentially cuts *across* the basic categories of the model. But, as I will explore in this thesis, there are also other important research practices to which such linear policy models render us blind. For instance, in this model basic research is placed far from the market, suggesting both that basic research and markets are different worlds and that basic research has to transform or mature into something else in order to become market relevant. However, the specific constitution of both markets and basic research varies, and therefore it is obvious that this model only describes one type of relationship whereas we might think of others.

[18] Vækstfonden.

Although the linear model of research has been important in the Danish research policy context, its limitations have also been debated. Whereas the model has been useful for overall policy discussions of the prioritization of public funds, it usefulness as a tool for categorizing *individual* research projects has been considerably more ambiguous. Here, I draw on my experience from working in the Danish National Advanced Technology Foundation (DNATF), since talk of these ambiguities have rarely been part of the Danish public debate. As noted, the linear model facilitates broad differentiations between basic and strategic research without accounting for subtle differences or exceptions to the rule. However, this inability to account for subtleties becomes a problem within the individual councils where the boundaries between different research practices are often recognized to be much more blurry.

As the DNATF was established in 2006, we often discussed the basic characteristics of a project funded by us in comparison with projects funded by other councils and foundations. In the selection process, how could we identify a suitable project that obviously lived up to our funding criteria? While looking for such distinctive characteristics, we worked from the experience that projects funded by DNATF were quite diverse and distributed across different industries and research areas. They had different risk profiles. The possibility of succeeding seemed good for some projects and less good for others. In order to evaluate the developing funding profile of DNATF, we started to categorize projects that had received funding. However, categorizing individual projects was not easy. For instance, most of the time, evaluating whether a certain research activity exemplified basic research and high risk was impossible, even for the participants in the project. Consequently, identifying a clear profile within the area of strategic research proved to be highly challenging in practice. Indeed, it seemed that DNATF projects were primarily characterized by drawing on quite *different* forms

of research practices. Thus, even though the linear research policy model made sense as a tool for differentiating the mandates of public councils and foundation it was relatively useless as a tool for categorizing research proposals, not to mention describing the growing portfolio of projects that had received grants from DNATF.

As I have shown, the Danish policy notions of strategic and independent research draw on a linear model of research. Now, simultaneously with the emergence of strategic research institutions in Danish research policy another model gained influence, which spoke to the question of how to conceive strategic research. This was Donald Stokes' model of Pasteur's quadrant (Stokes 1997). In contrast to the linear model, Stokes' model suggested a potential alignment of research ambitions that was usually seen as apart. Let us therefore consider the basic idea of this model and how it affected the Danish policy construction of strategic research.

Strategic research as Pasteur's quadrant

Donald Stokes originally proposed the model of Pasteur's quadrant to challenge Vannevar Bush's distinction between basic research and applied research, suggested in his book *Science: The Endless Frontier* (Stokes 1997; Bush 1945). Instead of existing in separate categories, Stokes suggests that research develops from the interplay between different motivations. He identifies two main ambitions for research, which are 'a drive towards fundamental understanding' and 'a drive towards applied use' (Stokes 1997). By focusing on drives rather than stable domains, Stokes emphasizes the dynamic of different research forms. He goes on to propose that understanding the interplay between the different motivating forces of research has important implications for research policy and

the organization of research funding. Rather than think only in basic or applied science policy should consider other mixed forms. In Denmark, Stokes' model has not been used in official strategies although it has often been used in public discussions of policy.

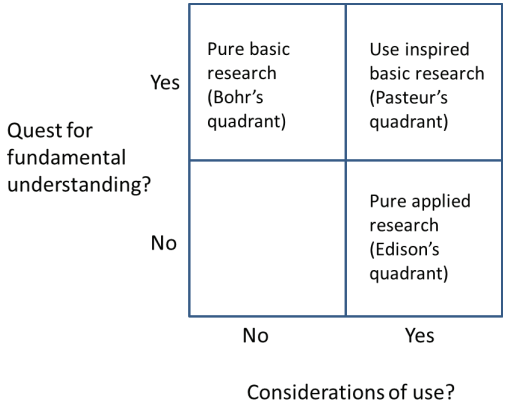

Illustration 5: Donald Stokes' model of Pasteur's Quadrant (Stokes 1997).

In Stokes' model, the y-axis represents an increasing drive towards fundamental understanding and the x-axis an increasing drive towards applied use. In the lower quadrant on the left side, according to Stokes, one finds the knowledge of "common man".[19] On the lower right, we find research in which the drive towards applied use is significant. Edison's light bulb offers an example. At the top left, Bohr's quadrant covers research with a drive towards fundamental understanding. Finally, at the top right, Pasteur's quadrant combines a drive towards fundamental understanding with considerations of use. Based on this

[19] STS approaches this very differently. See Irwin 1995 and Irwin and Wynne 1996.

model, Stokes argued that some research is capable of integrating more than one drive or direction.

In discussions of Danish research policy, Pasteur's quadrant model has been used to challenge a linear model of research. In particular, the quadrant enables policy makers to go beyond the limited notion that areas of research are always clearly delineated and either applied or basic. Thus, the main attraction of this model is its potential for creating a new form of policy conceptualization in which it is possible for basic research and commercialization to meet and overlap. In a Danish context, the model has allowed councils and foundations to argue that strategic research is not a narrow field but one that expands in many directions. In the DNATF, in particular, the model was also used in a number of ways. The secretariat used it to stimulate discussion in the board about the main field and scope of the foundation. Eventually it was agreed that a majority of the foundation's funded research should fall within Pasteur's quadrant. Further, it was used in the secretariat as a tool for categorizing research projects. Here, it influenced discussion of incoming applications, for instance by using Pasteur's quadrant as a yardstick with which to measure their strategic qualities. In addition, it was used for public communication about the purpose of the foundation, which was described as a connecting high quality research with perspectives of commercialization. Finally, the model was used to think about the challenges of strategic research for research managers. As noted, the model focused on bringing together potentially different motivations for research. Although Pasteur's quadrant was seen as a distinct research form, the construction of the model made it clear that it was composed by different drives. In the context of DNATF, the specific challenges for research managers were consequently seen as related to bringing together and managing these drives. The model was thus used as a tool for discussing research management. This leads me

to ask: how has research management developed as a particular concern, closely related to the overall ambition of strategic research?

The role of research management in Danish policy

Around 2005, with the new organization of the Danish research advisory and funding system, the role of research managers began to emerge as an important question in Danish research policy. Since then it has had a prominent role in Danish research policy. Several examples can be used to illustrate this trend. In 2011, the Danish Academy of Technical Sciences (ATV) initiated a project called 'The Value Creating University'.[20] This project aimed to stimulate a debate about the role of universities in Danish society. One of the main themes was research management. Although the debate illustrated a wide spectrum of ideas about ideal university research management, there seemed to be a general agreement about the value of an increased focus on research management. In the debate, some argued that good research management should always be based on advanced technical skills, implying that ideal research managers are scientists that have advanced to become leaders. Others argued that research managers should rather be professional managers with specific training in leadership as scientists often make bad leaders (ATV 2012: 37), which I return to below.[21] Similarly, in the autumn of 2012, research management was a key topic at yearly networking event of the Danish National Advanced Technology Foundation (DNATF).[22] Here, the Danish Minister for Science, Innovation and Higher Education presented the Pasteur Award, given annually to a project leader that had excelled in 'bridge building' between universities and companies in a

[20] www.atv.dk.
[21] Amanda Goodall: "Expert Knowledge and university leadership" at www.atv.dk.
[22] www.hoejteknologifonden.dk/netvaerksdag.

research project funded by DNATF. As a third example, consider that the Danish Council for Strategic Research (DSF) gives special attention to research management in its 2013 strategy document. In this document, we read that "strategic research activities are to be performed with due emphasis on skilled research management" (DSF 2013: 10). In this way, research management has indeed become a criterion for receiving research funds.

Thus, research management ideas crop up in public seminars, conferences, in relation to discussion of the future of universities and as a basic requirement for receiving funds. Talk of research management reflects diverse ideas about research and *vice versa*; ideas about the dynamics of research management also illustrate what is seen as constituting research practices. We might then ask what is it about the nature of strategic research in particular that makes research management appear so crucial for successful outcomes.

As noted, the Danish National Advanced Technology Foundation (DNATF) and the Danish Council for Strategic Research (DSF) have included specific demands for research management in their criteria for selecting and funding research projects. Similarly, as noted above, in 2008 DNATF established an award for exemplary research project leadership.[23] In both contexts, the specific nature of strategic research as *connecting* several aims and perspectives (Stokes 1997) has been used to explain an increased need for research management. The DNATF has specifically attempted to specify the kinds of problems that good research management is meant to solve. According to a DNATF project guide, companies and universities bring different values and norms to a project. In order for successful collaboration to take place, these norms and values do not need to

[23] www.hoejteknologifonden.dk (opslag).

merge but an initial alignment of expectations is required.[24] This alignment is important because otherwise the different interests and goals of the participants will start to diverge and eventually create chaos. The task of the project leader is thus to ensure a continuous alignment of expectations and to prevent chaotic divergence. According to DNATF, it is thus the *inherently diverging nature* of strategic research that makes research management crucial and thus the main accomplishment of the project leader is to set a joint direction. Here, project managers are not seen as *administrative* managers but rather as *leaders* that are capable of connecting different perspectives. In Chapter 13, I return to a discussion of how this is seen in Lundbeck.

As mentioned, there is an interesting tension in the idea of research management that recent policy initiatives such as awards and renewed selection criteria give attention to. On the one hand, such initiatives advance the idea of a *scientific* research manager who has a background in research. In Denmark, institutions such as the Danish Academy of Technical Sciences (ATV) highlight this idea of the technically skilled research manager. Also key contributors to debates on Danish research policy have advanced this idea of a science-manager based on their backgrounds in science and corporate research management.[25] According to these proponents of *scientific* research management, the ideal manager is a scientist with deep insight into the scientific content of the project. On the other hand, others cultivate just the opposite idea, namely, that ideal research management requires *professional* leadership. This form of research management has been promoted in Denmark with the institution of professional university management. Where university management used to be constituted by university

[24] See www.hoejteknologifonden.dk.

[25] Examples of contributors are Professor and Chairman Flemming Besenbacher from The Carlsberg Foundation and Jens Rostrup-Nielsen, former senior research manager at Haldor Topsøe.

researchers it is increasingly also composed by managers with professional leadership training. To some degree, we see these two trends combine in the context of DNATF, as project leaders that are rewarded with the Pasteur' prize is given professional management training at Harvard Business School.[26] All of this suggests that the notion of research management is rather ambiguous, and contain within itself multiple conflicting tendencies. However, it is obvious that ideas like strategic research, research collaboration and research management are deeply entangled in the context of Danish policy.

Conclusion

In this chapter, I have offered an introduction to how strategic research has developed in the context of Danish public research policy. About ten years ago, strategic research emerged through a particular structure of several separate councils and foundations. Presently, strategic research is developing as an even more separate domain in Danish policy with its own organization. In particular, strategic research has merged into one more or less coherent area to be governed by the Danish Innovation Foundation. The ideas behind this recent reorganization illustrate certain key concerns related to strategic research. Strategic research is seen as a main national driver for economic growth in Denmark, one that thrives best in a separate yet coherent system for strategic research. Since strategic research combines different ambitions and inputs defined by universities, politicians and companies, the question of research management becomes a particular important theme. The main role of research managers in relation to strategic research is to align expectations and set a direction given the potential risk of divergence. In the views of strategic research we have encountered, we

[26] http://hoejteknologifonden.dk/pasteurprogram.

also get the sense that *basic* research is, in fact, a much more uniform area of research than strategic research because it is somehow governed by coherent disciplines and scientific interests. In comparison, in these policy accounts, strategic research emerges as a much more *heterogeneous* practice that would develop chaotically if it was not properly governed.

In this overview, I have touched upon the practical and conceptual limitations of what I referred to as a linear model. In spite of this criticism, it is also clear that bureaucracies have an urgent need for relevant categories and structures. Rather than see efforts of categorization as misguided and reductive, we might use them as entry-points for coming to terms with the creative processes of administration (Jensen 2011). Since the main focus of my thesis is a detailed investigation of Lundbeck's research management practices, the world of public policy inevitably looks less vibrant and lively compared to what will follow. However, this is an artifact of the thesis structure and does not at all correspond to my own experiences of having worked with creating these bureaucratic structures. Thus, I share Paul du Gay's view that bureaucracies and the arrangements they produce are *fascinating* as well as immensely important (du Gay 2000). With this proviso in mind, I now move to explore how university-industry collaboration has been conceptualized in academic research in the field of research policy and also in Science and Technology Studies. As we shall see, there are quite significant overlaps between the models that we have seen in Danish research policy and the academic descriptions of science-industry interaction. So what characterizes some of the main ideas of university-industry collaboration in research policy studies?

9. CONCEPTUAL FRAMEWORK

This thesis is concerned with university-industry relations, research collaboration, research management, and categorizations of research such as strategic research. Both in Danish public policy and in Lundbeck, these topics are closely related. In both of these worlds, we are witness to a particular promotion of university-industry interaction, which raises certain questions: What makes university-industry collaboration work? And what is the role of research managers in connecting universities and industries? Unsurprisingly, these questions have also been widely discussed in the academic field of research policy. Here I explore the arguments that emerge in these discussions about the significance of university-industry interaction, research collaboration and research management. As we will see, certain ways of presenting these arguments recur, each of which rely on particular metaphors and assumptions concerning the domains of science and industry. I proceed to discuss some prevalent ways of presenting university-industry interaction in research policy literature including The Triple Helix model (Leydesdorff and Etzkowitz 1996) and Mode 2 research (Gibbons et al. 1994; Nowotny et al. 2001). I use this initial investigation as a starting point for developing a conceptual framework drawing on analytical resources from STS.

Bridging gaps and breaking barriers

Perhaps the most prevalent way of presenting university-industry interaction is by using the metaphor of a *'gap'*. This metaphor is widely used in policy talk and

documents.[27] It is also widespread in academic studies that look at research collaboration and science-industry interaction in the context of research policy (Jones-Evans et al. 1999; Luna and Velasco 2003; Sapsed et al. 2007; Kotha et al. 2013; Garud et al. 2013). A recent example is "Bridging the mutual knowledge gap: Coordination and the commercialization of university science" (Kotha et al. 2013). In this paper, Reddi Kotha et al. look at gaps that hinder effective commercialization of university research. They argue that there are several knowledge gaps worth considering when studying processes of commercialization. First, there is a 'gap' between the involved research disciplines that constitute "inventor teams" (ibid. 499). In these teams, there might be different "science distances" (bid. 506) between the involved researchers. Such distances can be shorter or wider, depending on which disciplines are involved and whether they share methods and theories. Second, the authors argue that there is a gap between university research and users involved in commercialization, corresponding to a gap between "inventor teams" and "licensee teams" (ibid. 517). In this case, rather than a disciplinary difference, the difference between participants with or without know-how of commercialization constitutes the 'gap'.

Both kinds of knowledge gap cause "coordination problems" (ibid. 500). In order to collaborate effectively, the participants need to align and coordinate their different forms of knowledge. As participants collaborate based on different backgrounds, collaboration, according to the authors, invariably has certain "costs" (ibid.). Thus, they take an interest in what might reduce these collaboration costs. They conclude that the smaller the "science distance" among the collaborators the lower the costs of collaborating. They also claim that "prior

[27] See the Danish "Innovation Strategy" (Regeringen 2012) and the European research framework "Horizon 2020".

collaboration experience" both related to interdisciplinary collaboration and to commercialization also effectively reduce collaboration problems (ibid. 505).

As the title of Kotha et al.'s paper illustrates, the terminology of gaps is closely related to language of *'bridges'*. Usually, the expression 'bridging a gap' implies overcoming a difference, especially an unfortunate one. In Kotha et al. this difference is seen as both a disciplinary difference and a difference between inventors and users of research. However, both in policy and in research policy studies, the idea of 'bridging a gap' is also often used to describe interaction between the different domains of science and society. An example of using this terminology to describe interaction between scientists and non-scientists is found in Garud et al.'s "Boundaries, breaches, and bridges: The case of Climategate" (Garud et al. 2013). In this study, the authors focus on the boundaries of scientific enterprise in relation to a case in which climate scientists' work was illegitimately posted on the Internet. The authors develop the notion of "boundary bridging work" to describe "efforts required by scientists to connect with downstream stakeholders, especially when the science/non-science boundary has been breached" (ibid.).[28] I return to the notion of the notion of "boundary work" (Star and Griesemer 1989; Gieryn 1999). Here, I simply pause to note that the idea of 'bridging a gap' is a prevalent way of framing studies of university-industry interaction.

Investigating *'barriers'* that hinder university-industry interaction is another widespread way of presenting research in this field. Johan Bruneel et al.'s paper "Investigating the factors that diminish the barriers to university-industry collaboration" (Bruneel et al. 2010) offers an illustration. These authors argue, "although the literature on university-industry links has begun to uncover the

[28] Forthcoming paper in *Research Policy* 2013.

reasons for, and types of, collaboration between universities and businesses, it offers relatively little explanation of ways to reduce the *barriers* to these collaborations" (ibid. 858 my italics). Hence, they propose to "unpack the nature of the *obstacles* to collaboration between universities and industries" (ibid. my italics). In particular, they suggest that there are two different types of barriers: "orientation-related barriers" related to the specific interests of the involved university and industry representatives in the collaboration, and "transaction-related barriers" (ibid.) related to conflicts over immaterial property (IP) and university administration. Consequently, the authors identify different factors that *reduce* these various barriers (ibid. 860). They find three main mechanisms that diminish them: "experience of collaboration", "breadth of interaction channels", and "inter-organizational trust" (ibid.).

It is interesting that although the notions of 'bridges' and 'barriers' often appear in studies that describe university-industry interaction in quite *concrete* terms using notions such as 'cost', 'distance' and 'transaction', their conclusions suggest something much more *intangible*. Kotha et al. and Bruneel et al., for instance, both conclude that 'experience' and 'trust' reduce 'distance' and 'costs', which seems like quite abstract answers to offer to quite specific questions (Kotha et al. 2013: 509-510; Bruneel et al. 2010: 860-861). For one thing, it raises questions about the specific, empirical relation between collaboration 'cost' and 'trust' in the cases under discussion. More generally, it illustrates the difficulty of pinpointing precisely what are the mechanisms of university-industry interaction.

I would further point to another interesting tendency and tension: although the terms of gaps and barriers present university and industry as distinct domains, they imply remarkably different notions of *how* these domains are separated. According to a dictionary definition, a gap is "a break or hole in an object or between two objects, an unfilled space or interval" and "a break in continuity, a

difference, especially an undesirable one, between two views or situations".[29] In comparison, a bridge is "a structure carrying a road, path, railroad, or canal across a river, ravine, road, railroad, or other obstacle".[30] Set side by side, the idea of 'bridging a gap' suggests that the domains of university and industry are disconnected and that it is the *disconnection* that is undesirable. Accordingly, a bridge is constructed to allow traffic from one side of the bridge to the other. But the bridge is a construction and does not become an integrated part of either of the two domains.

In contrast, a barrier is, again by dictionary definition, "a fence or other obstacle that prevents movement or access".[31] Here we do not have an unfilled space between two objects but rather a situation of adjacent objects separated by something like a wall that can be *removed* to allow interaction. Although the metaphors of bridges and barriers are not proposed as coherent theories or models, they significantly influence the kind of research questions that are pursued in the literature on university-industry interaction. In particular, many research policy studies develop questions based on a framework of separated domains, which lead them to address the misalignment or disconnection between these domains by investigating factors and mechanisms that make interaction smooth and effective. If barriers and gaps were not taken for granted, research questions would have to be posed differently. If the starting point were not separate domains research question would not automatically address activities that connect and align. Rather they would have to ask more broadly, who or what interacts and in what ways?

[29] www.oxforddictionaries.com.
[30] www.oxforddictionaries.com.
[31] www.oxforddictionaries.com.

Finding the right partners and means of interaction

Yet another prevalent way of presenting university-industry interaction is related to identifying an ideal match of universities and companies, or some optimal means of interaction. For instance, in "Finding the right partners: Institutional and personal modes of governance of university-industry relations" (Freitas et al. 2013) Isabel Maria Bodas Freitas et al. investigate different ways of approaching university-industry interaction, which they refer to as distinct "governance modes" (ibid. 51). The authors focus particularly on the *effectiveness* of different ways of governing university-industry relations. They suggest that there are two general modes of governance. First, an "institutional mode of governance" in which interaction is mediated by the administrative structures at the university such as technology transfer offices. Second, a "personal contractual mode of governance" in which interaction involves formal and binding contractual agreements between companies and academics researchers without the direct involvement of a university. The authors argue that, often, the personal contractual mode is overlooked in the literature on university-industry interaction, resulting in a too strong focus on the role of technology transfer offices. They advocate more detailed investigation of other governance forms, and they argue that the *right* choice of governance model depends on a number of factors, including, the size of a company and its tendency to adapt to "an open approach to technology and innovation development" (ibid. 60). Accordingly, they find that "personal modes" are more effective than "institutional modes" for making the best arrangements with external research partners (ibid.). As the title "Finding the right partners," indicates, the authors also believe that there is such a thing as 'a right partner' that can be identified *in advance* of collaboration. Successfully identifying this partner is primarily a matter of choosing the correct

governance model and acknowledging that personal relationships are sometimes more effective than institutional governance.

Other studies are more concerned with finding the *best* means of interaction, often referred to as channels. As an example of this type of study, consider Claudia De Fuentes et al. "Best channels of academia-industry interaction for long-term benefit" (Fuentes and Dutrénit 2012). According to theses authors, university-industry connections can be conceptualized as a three-stage process relating to "drivers of interaction", "channels of interaction" and "perceived benefits of collaboration". They proposes a process where successful interaction initially depends on the collaborators' motivation to collaboration, subsequently on how their interaction is mediated, and finally on how they benefit from collaborating (ibid. 1669).

These authors further argue that there is a particular pattern between initial motivation, interaction channels and benefits: "different drivers to collaborate determine specific types of knowledge flows through certain channels, and these channels also have an impact on the specific benefits that agents perceive from interaction" (ibid.). Accordingly, the authors define best channels as the means that give companies long-term benefits of interacting with public research institutions. Interpreted this way, they argue that there are generally speaking three kinds of best channels: joint or contract R&D, intellectual property rights (IPR), and human resources. Rather than interpreting channels in a narrow sense as means of communication, the authors consider the overall definition of joint projects, IPR and the engagement of people with different backgrounds as the most promising ways of ensuring long-term results. As in previous examples, we find an interesting relation between the research questions in this study and the findings. While the title "Best channels of academic-industry interaction for long-term benefit" produces the expectation of being presented with very specific and

concrete mechanisms, the authors find that the best channels are in fact composed by a number of things. These include defining a joint project, developing intellectual property rights and experience, which are activities that together seem to cover practically all forms of interaction rather than point to one optimal channel.

Studying the governance models that lead to finding optimal collaboration partners and exploring the best channels of interaction for long-term results illustrate a similar concern for optimizing strategies and forms of organizing university-industry interaction. This concern entails an interesting embedded assumption about collaboration. The purpose, drivers and expected outcome of collaboration are assumed, at least to some extent, to be known *in advance* of its initiation. From this follows the idea that optimizing is possible according to a predefined (stable) purpose and expected outcome. As a consequence, we also find a strong focus on the organization of university-industry interaction that in various ways support the interaction. Shahid Yusuf's study "Intermediating knowledge exchange between universities and businesses" (Yusuf 2008) illustrates this concern for supporting organizations. Yusuf argues that "achieving effective knowledge exchange requires the midwifery of different kinds of intermediaries often working in concert" (ibid. 1167) and he identifies four types of intermediaries: "the general purpose intermediary", which is the research institution, "the specialized intermediary" exemplified by the university technology licensing office (TLO), "the financial intermediary" for instance, a venture capitalist and, finally, "the institutional intermediary" often a public agency (ibid. 1170). Using this typology, the author argues that the different intermediating parties play different, sometimes overlapping, roles, but together they aim to make the knowledge exchange smooth and effective. One might notice the distinction in studies such as this between the main parties involved in

collaboration, referred to in general terms as universities and businesses, and parties that act to merely support the main interaction. To talk about intermediary organizations as separate from the main parties requires certain assumptions about research activities as being quite separate from administrative and funding activates. This involves an implicit categorization of research, to which I will now attend.

Pasteur scientists and Mode 2 activities

According to research policy studies university-industry interactions thus come in multiple different forms. Such studies thus generally offer various ways of categorizing these forms and types. For an example, consider the article "How do collaborations with universities affect firms' innovative performance? The role of "Pasteur scientists" in the advanced materials field" (Baba et al. 2009). Here, Yasunori Baba et al. draw on Donald Stokes' categorization of research with different drives (see Chapter 8). Drawing on Stokes' model of Pasteur's quadrants, Baba et al. define two different researcher *types*, arguing that "Pasteur scientists" are people "who never lose sight of the desire to advance scientific understanding, but whose research has potential real-world utility" (ibid. 757). In comparison, they define "star scientists" as "scientists who conduct pure basic research, oriented to the scientific discovery, having little interest in the potential uses of the research findings for the real world (such as Niels Bohr)" (ibid.).

Based on this classification, the authors explore the roles that researchers with different experiences typically play in collaborations and they examine how researchers' experiences affect the innovative performance of the companies they collaborate with. They conclude that ""Pasteur scientists" increase firms' R&D productivity, measured as number of registered patents", whereas "star

scientists" exert little effect on their [companies'] innovative output" (ibid. 756). The endeavor to distinguish between different types of *researchers* rather than different *research practices* has some particular effects. In particular, we can observe a shift from considering orientations at an overall level to characterizing the *behavior* of individuals with the purpose of evaluating their effect on industrial innovation. This focus requires the stabilization of a number of factors related to both research collaboration and industrial innovation. For example, it requires stabilizing the type of researcher and the impact that one researcher has on an innovation process. In the particular case of Baba et al., it also requires the assumption that patents can be seen isolated as illustrations of innovation. Using Stokes' model to discuss different research *activities* rather than researcher *types*, however, suggests that the same researcher *might* in fact be involved in various practices, which would not lead to a uniform set of researcher behaviors.

For another example of the endeavor to classify researcher types, we can consult Carole Estabrooks et al.'s "Knowledge translation and research careers: Mode l and Mode ll activity among health researchers" (Estabrooks et al. 2008). This paper draws on the conceptual framework of Mode 2 research, introduced by Michael Gibbons, Helga Nowotny et al., in order to characterize the activities of individual researchers. I return to a more detailed discussion of the Mode 2 framework below. Here I note that the authors put particular emphasis on characterizing the individual researcher by distinguishing between "applied researchers" and "basic researchers", which they render equivalent to Mode 2 and Mode 1 knowledge production (Estabrooks et al. 2008: 1066). Using this terminology, the authors compare the general characteristics of health researchers with the specific "knowledge translation activities" of these researchers. They propose, for instance, that 'applied researchers' who engage in more 'Mode 2 activities' also report more impact of their work and "higher

relational capital" than basic researchers (ibid.). The authors then use these findings to discuss potential tensions between Mode 1 and Mode 2 activities, proposing that Mode 2 might take place at the *cost* of promotion, tenure, grants and awards.

Other analyses are premised not on the introduction of novel schemes of classification but on more traditional categories. Thus, Markus Perkmann et al.'s "Engaging excellence? Effects of faculty quality on university engagement with industry" (Perkmann et al. 2011) offers the notion of 'excellence' as significant of classic university research and proceeds to explore how excellent research engages with industry. Accordingly, the emphasis is on *how* the quality of university research shapes engagements with industry. They ask, for instance, whether it is always the case that there is a positive relationship between the research quality produced by individual academics and the subsequent commercialization of that research. They also insist that the relationship between research quality and commercial activity varies from discipline to discipline: in technology oriented disciplines such as the medical and biological sciences faculty, quality is positively related to industrial involvement, whereas in the social sciences they find this not to be the case (ibid. 756).

This study thus illustrates a concern with links between excellent basic research and industrial innovation based on the idea that basic research and industrial innovation constitute two extremes in a linear spectrum akin to the one we have previously encountered in the context of Danish policy. Because of the imagined distance between these extremes, the idea of connecting them again becomes a matter of concern. Again, we encounter an idea of a 'gap' and a notion of 'disciplinary distance' to be 'bridged'. In this argument, however, the excellence of technology-oriented disciplines is more easily used or applied in industry than excellent research from social science. In other words, aside from the distinction

between basic research and industrial innovation, it also introduces a classification of scientific disciplines according to how well their basic research activities match the demands of industry.

While Perkmann et al. discuss the impact of scientific disciplines on industry, other studies address the interface between public and private research more directly. An example of this is Andrew Toole's study "The impact of public basic research on industrial innovation: Evidence from the pharmaceutical industry" (Toole 2012). Toole is concerned with the complex relationship between public research and pharmaceutical innovation and, in particular, with the relation between public clinical research and innovation of new molecular entities (NME). Toole argues that industries are often assumed to benefit from publicly supported university research. Based on his study of pharmaceutical industry, he questions whether this is the case. He argues that factors that are usually seen as external to research, such as market size and the regulatory structure imposed by for instance, the U.S. Food and Drug Administration (FDA), play a much more significant role than the involvement of university research. This study suggests that public university research does not have direct impact on industrial innovation and it thus questions prevalent ideas about innovation as being merely a matter of a disconnection of public and private research spheres. Perhaps even more intriguingly, Toole discusses the broader context for university-industry interaction. Rather than focusing exclusively on universities, industries and their interactions, he concludes that the innovation of companies in the pharmaceutical industry is mainly affected by mundane and traditional factors such as market size. Thus, even if this analysis is based on a traditional separation of public and private activities, it ends up insisting on the need to take other factors into consideration.

The various studies of Pasteur scientists, Mode 2 activities, excellent research, and industrial innovation that I have examined exemplify different categorizations of research that structure the analysis of university-industry interaction within the field of research policy studies. According to Perkmann et al. we are presently witness to a strong focus on the researcher *as an individual*, which has opened up for new insights in what drives collaboration. In "Academic engagement and commercialization: A review of the literature on university–industry relations" (Perkmann et al. 2013), Perkmann et al. first note the increasing prevalence of studies focusing on academic scientists' involvement in research collaboration. As they argue, academic researchers are involved in various forms of collaboration, including "collaborative research, contract research, consulting and informal relationships for university-industry knowledge transfer" (ibid. 423). Subsequently, the authors argue that these activities should be seen as "academic engagements" rather than "commercialization" activities. These "academic engagements" they argue are not in contrast to traditional academic activities but rather *"closely aligned"* with them and "pursued by academics to access resources supporting their research agendas" (ibid.). So we see here a form categorization of research and researchers that potentially questions the relevance of initiating analysis of university-industry interaction based on only considering the alignment of fundamentally misaligned interests and activities. Instead, they suggest that individual researchers are *already* engaging in diverse activities with companies *without* compromising their own research agenda. Thus, although many prevalent categorizations of research support the idea of universities and industries as separate domains, there are also categorizations, such as academic engagements, that seem to allow for a different perspective on interaction.

A stable image of interaction

So what characterizes some of these prevalent ways of presenting and studying university-industry interaction? First, I have identified a recurrent tendency to analyze university-industry interaction in terms of 'bridging gaps' and 'breaking barriers'. Although gaps and barriers take on different meanings in the literature, both terms assume that university and industry operate as distinct domains, either separated by a space or by an obstacle. Consequently, using these metaphors generates studies of the multitude of mechanisms, organizations and strategies that might be imagined to connect these severed domains.

Second, I have observed a widely shared interest in what we might call optimization, focused on how to make the best and most effective use of university-industry interaction, for instance, in the form of industrial innovation. As we have seen, authors suggest that interaction can be improved through focus on diverse forms of governance, means of collaborating and communicating, and support mechanisms in the form of intermediary organizations. Finally, I have commented on both traditional and novel ways of classifying research and researchers. Among the traditional categorization schemes, we find those that juxtapose excellent research and industrial innovation. Newer schemes separate Star scientist from Pasteur scientists or Mode 1 from Mode 2 activities. Like the metaphors of gaps and barriers these categorizations of research have implications for how university-industry interaction is approached. Rather than investigate categorizations as they develop in practice, drawing on theoretical models these categorizations are applied to explore particular assumed forms of behavior and activities among researchers.

Considering these findings together, which kind of image do we get of university-industry interaction, the nature of research management, research collaboration, and different forms of research? I suggest we get an image of relatively stable

interaction, which is a contradiction in terms. Certain institutions such as universities and companies appear as figures of stability and endurance. Even when we begin exploring interaction between these institutions, we get the impression that the institutions remain relatively unaffected of this. Largely, they continue to have the same characteristics that make them readily recognizable as companies and universities. Thus, universities remain focused on scientific quality and publications, and companies on markets and commercial outcomes. We will continue to easily identify a university and a company regardless of its new collaborations and relations. The widespread notion of *alignment* illustrates this, as it presumes that arranging universities and industries on a straight line can be done without changing their internal components or considering their possible transformations. In this sense, the image of university and industry stays rather *two-dimensional*. It is this image I aim to complicate through my empirical studies.

But what are the available alternatives to this image? One model that has been highly influential is the model of the Triple Helix of university-industry-government relations that investigates university-industry interaction in what seems to be more dynamic and fluid terms. In the next section, I look more closely at this alternative and its assumptions.

The dynamics of interaction: The Triple Helix

For almost two decades, the Triple Helix of university-industry-government relations has been a prevalent model for analyzing university-industry interaction in a societal context. It was introduced in 1996 by the sociologists Loet Leydesdorff and Henry Etzkowitz in "Emergence of a Triple Helix of university-industry-governance relations" (Leydesdorff and Etzkowitz 1996). Today, the

Triple Helix model has its own research field with associated discussions, institutes and conferences.[32] Here I examine what characterizes this analytical model and what kind of research has it generated.

The model takes visual inspiration from the image of triple stranded DNA, characterized by a repeated structure in which three oligonucleotides wind around each other to form a triple helix. Adopting this idea, Etzkowitz and Leydesdorff want to show that university, industry and government are interrelated rather than separate worlds. Furthermore, they use this metaphor to argue that not only are university, industry and government interrelated; they *become* related in a dynamic process. In this view, the world is thus changeable and neither science nor society has any absolutely fixed characteristics. The Triple Helix model is sophisticated, not only because it enables a direct and clear identification of policy as a component in innovation, which is often more indirectly present in the above-mentioned studies. It also facilitates a rather fluid and dynamic idea of interaction. As Etzkowitz and Leydesdorff write: "In contrast to a double helix (or a co-evolution of two dynamics), a Triple Helix is not expected to be stable" (Etzkowitz and Leydesdorff 2000: 112). Hence, innovation depends on continuous hybridization of university, industry and government elements, which suggests something quite different than the metaphors of 'gaps' and 'bridges'. Let us therefore look more closely at the premises of this model.

First, the Triple Helix model assumes that the university plays a key role in innovation in the knowledge society. By emphasizing the role of the university, the authors challenge traditional models of innovation that consider either

[32] See www.triplehelixassociation.org and the Triple Helix International Conference at tha2013.org.

industry or the state as the main drivers of innovation processes[33] (ibid. 109). Today, proponents of the Triple Helix suggest that the potential for innovation and societal, economic development lies in a more (rather than less) prominent role for the university. This idea sometimes referred to as the emergence of 'the entrepreneurial university' "envisions an academic structure and function that is revised through the *alignment* of economic development with research and teaching as academic missions" (Etzkowitz et al. 2000: 314, my italics). Rather than radically changing its fundamental tasks and purposes, the entrepreneurial university embraces a third mission by contributing directly to developing the national economy. The university does this by both providing human capital in the form of educated researchers and by acting as an incubator for new firms (Ibid. 315). The Triple Helix thus opens for a rather fundamental reconsideration of the role of the university that used to be seen as more isolated from society. Hence, the kinds of questions that take on urgency include whether academia can "encompass a third mission of economic development in addition to research and teaching?" and how "each of these various tasks" can "contribute to the mission of the university?" (Etzkowitz and Leydesdorff 2000: 110).

Etzkowitz and Leydesdorff's ambition in advocating the Triple Helix model goes considerably beyond an academic clarification. Indeed, they want to use the model to stimulate a number of important policy discussions. As they argue, the Triple Helix is useful for studying how "different possible resolutions of the relations among the institutional spheres of university, industry and government can help to generate alternative strategies for economic growth and social transformation" (ibid.). To clarify this claim, the authors discuss different

[33] Etzkowitz and Leydesdorff contrast their theory to models such as "the national system of innovation" (NSI) that privileges companies and the "triangle model" that privileges the state in innovation processes (Etzkowitz and Leydesdorff 2000: 109).

generations of the Triple Helix model, both historically and across nations. They suggest that one might identify a Triple Helix l model at the time when the State was in charge and directed the relations between academia and industry. They associate this model with the former Soviet Union and socialist Eastern European countries. In contrast, strong institutional borders that separated universities from both industry and government characterized a Triple Helix ll model. This version, they suggest, can be identified in public policy debates, for instance, in Sweden in the *Research 2000 Report*, which recommended that universities should *withdraw* from direct contributions to industry (ibid. 109-110). Finally, they argue for a Triple helix lll model in which there are overlaps between the three institutions, where they even have shifting roles and where hybrid organizations emerge at the interfaces between them (ibid.). Thus the Triple Helix model is potentially useful for addressing quite different situations of interaction.

The Triple Helix model has generated a broad range of research concerned with different issues, contexts and using diverse research method, mainly quantitative survey studies but also qualitative research. Within this tradition, we find studies of science policy, industrial innovation, and university norms alongside meta-studies concerned with the validation of the Triple Helix model itself. In spite of this variety, the application of the model is noticeable for its *comparative* use. For instance, we find a large number of studies that investigate the status of Triple Helix interaction in different nations. Casas et al.'s study "The building of knowledge spaces in Mexico: a regional approach to networking" (Casas et al. 2000: 225-241) and Judith Sutz's "The university-industry-government relations in Latin America (Sutz 2000: 279-290) exemplify such comparative use. In other studies, comparison has led to reflections on the usefulness of the model when transported from its origin in developed countries to developing countries. However, in many instances the model is applied more or less directly, which

seems to generate less interesting studies than the ones that reflect on implication of the model itself.

Aside from national comparisons, other studies investigate innovation within different *industries*. Examples of these are Michael Nowak and Charles Grantham study "The virtual incubator: managing human capital in the software industry" (Nowak et. al. 2000) and Susanne Giesecke's study "The contrasting roles of government in the development of biotechnology industry in the US and Germany" (Giesecke 2000). In these two cases, the authors focus on the specificity of a particular industry and how it affects the overall dynamic of Triple Helix interaction. Like studies that apply the model to compare the status of interaction in nations, these studies of interaction within different industries mainly operate at a macro-level of analysis. It seems that the potential of the model to describe interaction in fluid terms is challenged when the model is more or less applied in these macro studies. The categories of university, industry and government are used to *identify* the interacting parties rather than more openly *analyze* the nature of the interaction. The result is a somewhat *stable* impression of what collaboration and interrelatedness implies.

In addition to these empirical studies, the Triple Helix model has given rise to an ongoing discussion about the extent to which it facilitates a deeper and more complex understanding of the dynamics of university-industry-government relations. In this vein, Etzkowitz et al. propose that the model opens up for exploring the "sub-processes" of university-industry-government interaction (Etzkowitz et al. 2000). They point attention to four such sub-processes: first, the "internal transformation in each of the helices" that covers studies of how companies work together or how universities develop missions within their own institutional borders (ibid. 315). Second, the "influence of one institutional sphere upon another in bringing about transformation" including studies of the way the

revision of rules and regulation by governments affects researchers and institutions (ibid.). Third, "the creation of a new overlay of trilateral linkages, networks and organizations among the three helices", that have the purpose of serving to maintain the interface and also to stimulate creativity (ibid.). Finally, the authors point to "the recursive effect of these inter-institutional networks representing academia, industry and government on their originating spheres and the larger society" (ibid.). In this way, the Triple Helix model continues to generate investigations that both explore something new (a sub process) and draw on the original framework of the Triple Helix.

Although the Triple Helix model has provided important and interesting studies of the dynamics of university-industry-government relations, it also takes certain institutional categories for granted. To some extent, this has to do with the metaphor of a triple helix and the image of three strings entwined in a structure. This image is important for illustrating the *interrelatedness* of universities, companies and government, both as an empirical focus and a theoretical concern. However, while describing a relationship, this image also provides an idea of three clearly distinct strings that are related in the same way in the same repeated distance and structure. It offers a picture of complex yet also quite determinate components and relations. This conceptualization raises certain empirical and methodological questions. Are we meant to assume that the three helices or institutions are always present *in the same way* and *to the same degree*? Are the institutions always identically entwined? It seems clear that the Triple Helix must be understood in rather loose terms in other to assist studies of dynamic and fluid relations. When understood and used in a too literal sense the dynamics it describes becomes less clear. While it highlights the flexible and complex relations between university, government and industry it provides less help in terms of pinpointing the specific qualities of their changing relations.

Shortly after the emergence of the Triple Helix, however, another model appeared, which has gained prominence and which seems to provide a slightly more nuanced framework for dealing with these issues. Thus, I continue to consider the framework of Mode 2 or 'the new production of knowledge' as it is also called. This framework is developed by Michael Gibbons et al. in *The New Production of Knowledge. The Dynamics of Science and Research in Contemporary Societies* (Gibbons et al. 1994) and by Helga Nowotny et al. in *Re-Thinking Science. Knowledge and the Public in an Age of Uncertainty* (Nowotny et al. 2001). Rather than look at university-industry-government relations specifically, this model considers the relationship between science and society more broadly. Like the Triple Helix, the Mode 2 model argues that the institution of science is presently undergoing rapid change and that it is strongly affected by a variety of societal agendas. Considering the extent to which national and European research strategies emphasize, for instance, research as "societal partnerships" (Regeringen 2012) this, to say the least, seems to be a relevant perspective today. So what are the elements of this model?

Science and society in partnership

In *The New production of Knowledge*, Gibbons et al. argued that we have experienced a shift in the way knowledge is produced and a changing relationship between science and society (Gibbons et al. 1994). Nowotny et al. further developed the framework in 2001 with the book *Re-thinking Science. Knowledge and the Public in an Age of Uncertainty* (Nowotny et al. 2001). Like Etzkowitz and Leydesdorff, Gibbons and Nowotny et al. propose an open dynamic framework for re-thinking science and its relation to society; one that differs from previous sociological models in which science and society are seen as separate domains (Merton 1973). However, rather than arguing that previous models were *wrong*,

the authors argue that today we are witness to a change in science-society relations so that the Mertonian model requires adaptation.

The Mode 2 framework is characterized by much closer interactions between science and society than those previous generations of sociologists of science encountered. Specifically, in Mode 2 science has become "context-sensitive" and even started to merge with society (Nowotny et al. 2001). Consequently, we are also witness to a new form of society, referred to as Mode 2 society that has started to "speak back" to science (ibid.). In a gestalt switch, society has become an *agent* influencing science, rather than a stable *background* supporting it. It is readily apparent that this framework differs radically from some of the studies previously discussed that look at universities and industry as separate domains connected mainly by 'bridges' (Kotha et al. 2013; Bruneel et al. 2010; Freitas et al. 2013; Yusuf 2008). For Gibbons and Nowotny et al., the relevant change that is occurring between science and society concerns not only the 'bridge' between science and society but also the very constitution of science and society. Science and society are equally affected and transformed by their mutual interactions. Gibbons and Nowotny et al. argue that the context-sensitivity of science is a strength rather than a weakness. For example, today, good science is not only evaluated by standards only developed *within* science but also defined by societal interests, needs and practices. We might see the idea of research based on demand-driven innovation as illustrating context-sensitivity.

The Mode 2 model has been widely adopted, both in academic discussions and among policy makers. For instance, today, it is widely assumed among policy makers that we need to know how knowledge is produced before we can distribute public funds optimally. One might even see this framework as self-exemplifying, developing at the *intersection* of academic discussion and policy making, since one of its main developers, Helga Nowotny, is both president of the

European Research Council and Professor emeritus of Social Studies of Science, ETH Zurich. However, as we see in Danish research policy (see Chapter 8), more linear models also constantly challenge this dynamic framework. It seems that while the overall public research *strategies* draw on Mode 2 thinking,[34] the specific *organization* of public funds turn to more linear models. This suggests that while it is easy to account for science and society relations at an overall strategic level, it is much harder to organize and manage according to them.

Asymmetrical descriptions of science and society

Although the Mode 2 framework has been widely adopted in research policy, it has also been criticized. One important criticism comes from the anthropologist Marilyn Strathern whose article "Re-describing Society" discusses the implications of the metaphor of a "partnership" between science and society (Strathern 2003a). Strathern suggests that while the Mode 2 model generally argues against a separation of science and society, the metaphors of "co-evolution" and "co-mingling" that it draws on nonetheless implies just such a separation. After all, the pre-fix "co-" means 'jointly', 'mutually' or 'together with another'. Even if this terminology is used to describe closely intermingled, even potentially merging, relations it also continues to maintain an image of two components. Consequently, "every attempt to show how science and society are implicated in one another also renews each as distinct objects" (ibid. 268). In the framework of Mode 2, science and society appears as each other's rhetorical reference point, which according to Strathern has problematic analytical consequences: society is consequently always constituted *in relation to* science

[34] See the Danish "Innovation Strategy" (Regeringen 2012) and the European research framework program "Horizon 2020".

and never considered on its own terms. Strathern does not argue for a separation of domains but rather for a more flexible approach what science and society might mean. The fact that the idea of separate domains continues to haunt the Mode 2 framework becomes clearer when we consider that the notion of a partnership. According to Strathern, the notion of a partnership is closely related to the idea of a 'transaction space' between science and society.

Nowotny et al. borrow the idea of "transaction" from the historian of science Peter Galison whose notion of the trading zone (itself adopted from an anthropological literature) illustrates how scientists representing "different cultures" work together (Galison 1997). Later in this chapter, I discuss the notion of the trading zone. Nowotny et al. uses the notion of a transaction space to argue that when interacting, science and society might have different interests: "each targets what it wants" (Strathern 2003a: 268). However, Strathern suggests, even though the Mode 2 model sees science and society as having potentially *different* interests, the very idea that they have entered a transaction space creates the expectation that they are nonetheless part of a *mutual* communication. In this way, Strathern argues that the Mode 2 framework entails not only a normatively imposed *necessity* of interaction but also a drive towards *consensus*: "The epistemology can be varied, but communication has to take place" (ibid.). Again, as we have seen before, interaction implies some kind of alignment because science and society "*appear to be consonant with one another*" (ibid. 274-275). What alternative does Strathern propose?

According to Strathern, we do not need to imagine science and society as interacting partners. Drawing on ethnography from Papua New Guinea, she describes a controversy between a group of local miners and governmental representatives. In this situation, no appeal was made to a common language or to any necessary relationship between science and society. Science and society were

not named as distinct spheres and therefore there was no need to facilitate "co-mingling". Instead the miners and governmental representatives made what Strathern terms *a settlement* in which they each subscribed to an accountability that did not require agreement or consensus. Each side simply had its own agenda. Contrary to Nowotny et al.'s framework, the interacting parties in Strathern's example did not see *alignment* as a basic condition of their interaction but wanted instead to make their lack of agreement explicit: "In short, they were ready to enter into a social arrangement on the basis of *difference*, not consensus." (ibid. 272)

Strathern's critique of the Mode 2 framework is partly driven by the idea that any general categorization of the components of science-society interaction misses the point: it never becomes completely clear what constitutes it and what it produces. Even so, something akin to the notion of a 'transaction space' can be extremely relevant when used to explore *particular* cases. For instance, according to Strathern, the Papua New Guinean example illustrates a situation in which two groups enter a social arrangement on the basis of difference and not consensus and transacts based on that recognition. What can transact however, are not abstractions, like science or government, but rather *specific* people with *particular* problems, issues and agendas (Strathern 2003a: 272). In this way, Strathern argues for an analytical sensitivity to the contexts of transactions and for substantiating what Gibbons, Nowotny et al. abstractly call Mode 2. Below, I return to what such an empirically oriented framework might look like.

The models of Triple Helix and Mode 2 describe dynamic relations between the institutions of universities, industries and governments and science and society. These models both highlight the complex and dynamic relationships between science, society and business. From the perspective of these models, we clearly see entangled relations rather than separated domains. According to the models

of Triple Helix and Mode 2 interaction is a *condition* rather than an *achievement* created by 'bridging gaps' or 'breaking barriers'. The main question they raises therefore concern the qualities of the interaction.

But as Strathern notes, the image of interaction remain rather harmonious. What if science, government and business continuously interact but without any premise of consensus? In fact, other models argue specifically for such more antagonistic forms of *convergence* and for the prevalence of asymmetrical relations. For instance, the sociologists Steven Peter Vallas and Daniel Lee Kleinman propose that the outcome of increasingly closer relations between universities and industries is what they term *"asymmetrical convergence"* (Kleinman and Vallas 2006: 37; Vallas and Kleinman 2008). They note that universities and companies are certainly converging as a result of increasingly tight collaborative relations and develop increasingly similar characteristics, practices and norms. Thus, universities become focused on commercial outcomes and companies become interested in basic knowledge production. But this convergence is somewhat imbalanced, they suggest. For by focusing on commercialization, the overall purpose of universities is, in fact, *changing* while industry only focuses on basic knowledge production as a means to *maintain* their overall purpose of commercialization. The authors conclude that this interaction often results in some kind of industrial domination, a possibility that neither the Triple Helix nor Mode 2 affords.[35]

[35] In Denmark, the discussion of "imbalance" in university-industry collaboration has been prevalent in both academic and public debates (Emmeche and Faye 2010). See also Vedel and Gad 2011.

From interaction to co-production

Until now, I have explored some of the prevalent ways in which university-industry relations are conceptualized in academic literature related to research policy matters. As I have indicated, these studies open up for different perspectives on interaction between science, society and industry. But even so, we also see certain terminology, embedding particular metaphors and assumptions, shared by studies of university-industry relations. Implicitly or explicitly, university and industry are seen as *separate domains* that are somehow misaligned or disconnected. Accordingly, collaboration requires alignment and reconnection, which takes place as concern for 'bridge building'.

I have also identified a particular language of *optimization*. In order to collaborate, there are certain things that can and ought to be done such as finding an ideal partner or identifying the ideal means of communication. All of this presupposes that in advance of the actual interactions certain kinds of relations can be identified as particularly promising. In contrast, the perspective of optimization shows rather little attention to the actual dynamics of particular instances of university-industry interaction. There are, however, some models, such as the Triple Helix and Mode 2, which specifically address such dynamics. These models view the interrelations between universities, industry and government, and science and society, as an already existing condition. However, they imply rather *harmonious* relationships among the interrelating parties. Correspondingly, they are less adequate for analyzing the kinds of *asymmetries* such interrelations can imply, in the form of divergences in interests and agendas. As Strathern argues, it is therefore important to be empirically attentive to the different forms of interrelations that actually occur, and to their specific implications. Strathern develops what we might see as a 'third' position that involves neither *separate domains* nor *harmonious consensus* but approaches science-society interaction as

an empirical question. This approach implies describing actual empirical relations, as the title "Re-*describing* Society" indicates.

Inspired by Strathern, I am interested in developing a framework suitable for studying strategic research in Lundbeck from such as third position. Rather than claiming to know in advance the general features of science-industry collaborations, I am interested in investigating them empirically. But although I insist on an empirical investigation this does not diminish my need for a conceptual framework to support, guide and focus my investigation. To develop this, I now look at how in the field of STS science-society interaction has also been conceptualized in terms of 'co-production' (Jasanoff 2004). First, I look at co-production more generally and subsequently I discuss co-productionist concepts that have been developed specifically to analyze research collaboration.

A co-production framework

In discussions of science and policy, Sheila Jasanoff has introduced the concept of co-production to describe relations between science and society (Jasanoff 2004). One of the merits of co-production is that it offers an alternative to the analytical models surveyed above, which focus either on partnerships or maintaining a disconnection between science and industry. According to Jasanoff, "the dominant discourses of economics, sociology and political science lack vocabularies to make sense of the untidy, uneven processes through which the production of science and technology becomes entangled with social norms and hierarchies" (ibid. 2). Thus, co-production is premised on a critique of models that *separate* science from society, or the domain of "science, nature, facts, objectivity, reason, and policy" from the domain of "culture, values, subjectivity, emotion and politics" (ibid. 3). Co-production is in line with Strathern's proposal to remain committed

to investigating and accounting for complex phenomena *empirically,* while avoiding the strategic deletions of particular contexts that facilitate a clear picture.

In STS, analyses in the idiom of co-production have covered a number of themes that Jasanoff refers to as "the emergence of new phenomena" (Daston 2000; Pickering 1995: Latour 1993), "the resolution of controversy" (Richards and Martin 1995; Shapin and Shaffer 1985; Collins 1985), "the standardization of knowledge and technology" (Bowker and Star 1999; Jasanoff 1995; Shapin 1994; Porter 1992; Latour 1987; Kuhn 1962), and "the enculturation of scientific practices" (Knorr Cetina 1999; Rabinow 1996; Traweek 1988) (Jasanoff 2004: 5-6). Viewing co-production as something of an umbrella approach rather than an integrated theoretical framework, I move on to consider a number of concepts each of which might be seen to exemplify co-production. All of these concepts have been developed to analyze research collaboration but, as we shall see, they engage in this task from different angles and with somewhat different results.

One main question that has been asked in STS is what characterizes the *boundaries* of science (Galison and Stump 1996; Gieryn 1999). According to the sociology of science affiliated with Robert Merton (Merton 1973), the institution of science has a number of unique features, which make its norms and practices distinct from other social domains. Not least, Merton argued, the institution of science is characterized by the obligation to set aside private interest and address scientific questions in a disinterested manner (ibid.). This idea has been widely contested. For instance, the Belgian philosopher of science Isabelle Stengers has argued that far from being "dis-interested" scientists are linked through *interest* (Stengers 1997). New findings, technologies and hypotheses attract interest, while the very process of "becoming interested" can be life changing for scientists

required to rethink their theories, projects, methods and technologies (ibid. 83-84).

Among STS scholars, interest has also been a key notion in understanding how laboratory findings become scientific facts (Latour 1983; 1988). In *Laboratory Life* (Latour and Woolgar 1979), Latour and Woolgar showed how facts gain credibility and gradually become harder to contest as they are taken up in societal practices outside of laboratory. One famous example is Louis Pasteur's invention of microbes that gradually was adopted for antibiotic vaccination across France (Latour 1988). Here, there is no strong boundary between the scientific lab and the outside world. Instead, the inside and the outside were, in Jasanoff's term, co-produced. The sociologist Thomas Gieryn summarizes this view with his observation that the boundaries of science are cultural and "permeable" (Gieryn 1999: 27).

If the boundaries between science and society are permeable and there are usually many interests at stake then how to understand the phenomenon of research collaboration? Several studies of scientific collaboration, which broadly align with the notion of co-production, have dealt specifically with the question of how scientists with different backgrounds and viewpoints work together and come to define shared objects of collaboration. I now look at some of these concepts that have been developed specifically to describe research collaboration. First I present them and then I comment on the relevance of these concepts for my case and research questions.

Boundary objects

Most famous among these concepts is Star and Griesemer's notion of the boundary object (Star and Griesemer 1989). Star and Griesemer defined their

interest in terms of the following dilemma: "Scientific work is heterogeneous, requiring many different actors and viewpoints. It also requires cooperation. The two create tension between divergent viewpoints and the need for generalized findings" (ibid. 387). Actors can manage this tension, they suggest, by collaborating via boundary objects (ibid.). Boundary objects are produced when actors (both scientists and non-scientists) work together to make representations of nature such as "specimens, field notes, museums and maps of particular territories" (ibid. 408). These objects have a boundary nature as they are at the same time "concrete and abstract, specific and general, conventionalized and customized" (ibid.).

In practice, boundary objects mean that participants in a collaboration can agree on an "overall purpose" or idea, in Star and Griesemer's case, the collection of animals in California and their delivery to the Zoological Museum. But for different social groups this overall purpose or idea may imply entirely different specific things. It might mean *income* for hunters and trappers, *devoted hobby activities* for others, and *professional science* for the curators. Because actors belong to different social worlds, boundary objects thus help to ensure that "shared goals are *lined up* in such a way that everybody has satisfying work to perform in each world" (ibid. 409 my italic). When collaborators work together via boundary objects they are thus enabled to have different relations to the objects that brought them together, yet this implies only a provisional alignment of goals.

As we see, the notion of boundary objects addresses the question of how participants in a collaboration work together. It suggests that it is possible to have a temporary, local alignment without thinking in terms general alignment between domains. Boundary objects have relevance to my case as we might think of strategic research collaboration in terms of co-production via boundary

objects. Boundary objects propose something quite different from both 'bridge building' and Mode 2 interaction that, in different ways, propose alignment in general terms, either as an accomplishment or a condition. With boundary objects we can study specifically how participants in a collaboration connect despite the different backgrounds they have.

The concept of boundary objects is excellent for exploring what comes to be shared among the parties. In contrast, the concept focuses less on how collaborations might develop from the gradual *explication and elicitation of differences* among the participants. As we shall see, however, such attentiveness to difference is important. For it is by no means always the case that the starting point for collaboration is radical difference to be gradually surmounted. In some situations, it is rather the case that collaborators are guided and shaped precisely by the elicitation, rather than erasure, of relevant differences. This leads to an interest in how we can study settlements without assuming the priority of alignment processes. While boundary objects offer an important framework for analyzing research collaboration it needs to be supplemented with other concepts that make it possible to address asymmetries and difference in collaboration.

Doable problems

In a series of analyses, the sociologist of science Joan Fujimura has looked closely at what producing a shared problem requires. The empirical focus of her early research was the *crafting* of cancer research, and in particular Fujimura argues that to craft science, researchers construct *doable problems* (Fujimura 1987; 1996). The construction of a doable problem entails not just defining an interesting research question but solving a problem from beginning to end. This entails, for instance, defining a problem, receiving funding, making plans, doing

experiments, and potentially restating the problem. To characterize a problem as "doable" can thus not be done in *advance* of the research. Rather, as mentioned in Chapter 7, it is a *socio-technical achievement* than can only be evaluated post-hoc, that is, after the research has been done (Fujimura 1996).

Fujimura centers her analytical attention on how scientists craft doable problems by *articulating* various work activities, gradually aligning them and rendering them increasingly doable. Articulation here means the "amorphous and ambiguous work of planning, organizing, monitoring, evaluating, adjusting, coordinating and integrating activities usually considered *administrative* rather than *scientific* (ibid. 11). In Fujimura's study of oncogene research, for example, doable problems emerged as oncogene protocols involving various standards of methods and theory were adopted and recreated across laboratories. Gradually, a network solidified around oncogene research, involving "laboratories, funding agencies, materials and their suppliers, journals, and scientists working in and around oncogenes" (ibid.). Thus, 'doability' was not achieved inside the walls of the laboratory but rather in the process of stabilizing a larger network around the research.

As already illustrated in Chapter 7, I find describing the processes of research collaboration in terms of constructing doable problems highly relevant. The concept focuses the attention on all the *practical* things that matter in order to make a collaboration work. Successful collaboration does not automatically develop from posing excellent research questions but involves continuous articulation work in order to become doable. In particular, the notion of doable problems addresses the question of how scientists collaborate together *despite* quite different backgrounds. Fujimura proposes that this is possible due to 'standardized packages' of theory and methods. Consequently, the work that explicates *differences* among the participants automatically becomes more

invisible and uninteresting from the perspective of doable problems. Since I am developing an interest for the role of *difference* (as well as *sameness*) in collaboration, doable problems, too, needs supplementation with other concepts, which I introduce below.

Trading zones

As a final exemplification of what collaboration might entail in a co-production idiom, I turn to the historian of science Peter Galison who introduces the metaphor of the *trading zone* in his analysis of physicists' collaborations (Galison 1997). As mentioned, Galison's trading zone is itself imported from anthropological studies of arenas where different cultures meet and exchange goods (ibid.). As one might recall, it is also the concept that inspired Nowotny et al. to develop their notion of a transaction space between Mode 2 science and society. Galison, however, develops the notion of trading zones in a quite different context, arguing that the extensive and difficult collaborations between *sub-disciplines* of physics can be understood in terms of a *trade*. Like traders that arrive at a foreign city, scientists enter a trading zone in which they define local and highly specific "rules of exchange" that govern their interaction (ibid.). Similar to Star and Griesemer's insistence on collaboration across heterogeneous practices, Galison writes that: "Two groups can agree on rules of exchange even if they ascribe utterly different significance to the objects being exchanged; they may even disagree on the meaning of the exchange process itself." (ibid. 783). Like anthropological studies of creole languages, scientists can communicate by means of languages that they invent for the purpose of temporary collaboration.

Although Galison paints a picture of collaboration as akin to the meeting of different cultures, he also insists that trading zones do not simply homogenize

differences. “Far from melting into a homogeneous entity”, he writes, “the different groups often maintain their distinctness, whether they are electrical engineers and mechanical engineers, or theorists and engineers, or theorists and experimenters” (ibid. 805-806).

Galison’s trading zone concept is highly relevant for studying research collaboration, in particular for addressing the question of what collaboration requires. Where Fujimura’s answer to this question is ongoing articulation work and the development of standardized packages, Galison’s answer is trading zones constituted by rules of exchange and creole/pidgin languages. Although Galison looks at collaborations among academics, we might use his terminology for considering collaboration between academic and industrial participants more generally. However, transferring the concept to my material the metaphor of trade becomes less useful. Contrary to Galison’s case, actual trade is an element of the collaborations I study. In these collaborations, Lundbeck participates in a trade in the form of an exchange of money for research that is inscribed in contractual arrangements. However, even if actual trade is what takes place according to the contract, one might wonder about the “non-trade” aspects of the collaboration. In fact, one might hypothesize that if collaboration between industrial and academic researchers were only trade, based on clearly defined roles and accountabilities, it would not today be such a prevalent matter of concern.

Again, like boundary objects and doable problems, trading zones are particularly relevant for addressing the question of what makes collaboration work despite the unlike participants it often involves. For that purpose, trading zones highlights how participants connect, not only by handling shared material objects but also at a discursive level. They develop common languages for addressing shared concerns. Again, the implication of posing a question that concerns the making of

connections in collaboration is that the notion of trading zones does not directly address the role of *difference* in collaboration. Differences between the collaborators are addressed indirectly and as the starting point of the analysis rather than as what collaboration produces in order to work. As already indicated I have a slightly different interest and want to consider collaboration as a form of co-production that might be constituted by both constructions of difference and sameness. I now explore how such an approach to co-production might look.

Constructing alignment and misaligned co-production

The concepts boundary objects, doable problems and trading zones are quite different and have emerged from somewhat different investigations. Thus, Star and Griesemer investigated collaboration between scientists and non-scientists in the context of a Zoological Museum; Galison explored collaboration between different kinds of physicists in large scientific experiments; and Fujimura studied collaboration between researchers with different backgrounds, theories and methods in cancer research. In spite of these different circumstances, all three concepts draw attention to how collaboration might take place *in spite of differences* between the participants. In each of the cases, collaboration takes some kind of alignment, even if it is only provisional and temporary. For instance, Fujimura specifically argues that "*doability* can be conceptualized as the *alignment of the three levels of work organization: experiment, laboratory, and the social world*" (Fujimura 1987: 261 original italic).

This alignment, however, takes quite a different form from the one we have encountered in Danish research policy documents and previously described academic discussions of university-industry collaboration. For these STS scholars do not presume a general *disconnection* of the domains of science and society that

is preventing effective research. Rather, they argue that scientific practice is *heterogeneous* and, therefore, the fact that nonetheless researchers *do* collaborate is considered an interesting research problem. Against all odds, the participants somehow manage to collaborate; so how is this practically possible? Addressing this question, these scholars show how co-production takes place in terms of provisional alignment via boundary objects, doable problems and trading zones.

Even if, within a co-productionist imaginary, this alignment is only a *temporary* construction, it is still worthwhile to look further into what is presumed by this mode of analysis concerning collaborative *sameness* and *difference*. In general, the starting point for all three concepts is that something needs to be done, whether it is constructing *shared* goals, *common* languages, *standardized* packages, or *rules* of exchange. Each of these terms directs the attention to construction of sameness rather than difference. Analyzing university-industry collaboration in terms of constructing trading zone entails focusing on how the collaborating parties connect by establishing a kind of mutual understanding through a shared language. Focusing on how such trading zones become constructed has the implication that differences between the participants become less distinct. The awareness of the analyst is on how alignment takes place. But perhaps, as Strathern suggests, some arrangements might be based on *explicating* and keeping in view these differences?

Whereas the notions of boundary objects, trading zones and doable problems might see the construction of temporary homogeneity as the main driver of collaboration, I am interested in a conceptual framework that is also suitable for analyzing collaboration as a process of explicating difference. It seems possible to think of situations where the participants in a science-industry collaboration do not *merge* but rather develop relations based on differentiating constructions of their relation. I am wondering if it might not be relevant and useful to develop a

co-production framework that gives attention to both the construction of sameness *and difference*. Not predictable, stable or general differences between science and industry but subtle and ongoing differentiations that emerge and dissolve in the course of a collaboration. Might we even think of the doability of a research project as the result of *explicating* such differences? What kind of framework would make it possible to explore such explication? Drawing on Fujimura's terminology, might we not expect to see articulation work related to developing differences?

Thus drawing on Star and Griesemer, Fujimura, and Galison, I raise a slightly different question. Are there collaborations that work in spite of very little alignment? What is the role of explicating difference in collaborations? How are the actions of the collaborating participants related, or not related, to institutional affiliations? In order to explore these questions, I introduce and develop the concept of the screen in the remaining parts of this chapter. The notion is to be seen as a further development of co-productionist studies of research collaboration. I think of my research interest in terms of what I call 'misaligned co-production', suggesting that co-production sometimes also takes the form of misalignment. In Chapters 10-13, it will be clearer what this means in practice when I explore science-industry collaboration in the context of Lundbeck.

Introducing screens

When I started to explore strategic research and science-industry collaboration in Lundbeck, I noticed that research managers spoke a lot about something they called *screening*. Screening appeared in many forms. In the laboratory, screening seemed to involve the design of assays to test various hypotheses. In scientific journals and at conferences, screening for new ideas involved evaluating the state

of the art of research fields. Then there were screening activities related to evaluating the quality and suitability of potential external partners. Such screening involved search tours and meetings with external researchers and companies. Finally, I noticed a form of screening related to identifying new markets or defining unmet needs in the landscape of patients, competitors and regulatory institutions. It seemed obvious that these forms of screening were quite *different*. Where some forms, such as screening in the lab, seemed a very purposeful activity, other forms, such as screening of new partners, involved more experimentation. But although they entailed different activities, forms of screening also seemed to share something important. They implied ways of categorizing and ordering research in Lundbeck.

Upon this realization, I noticed that the notion of *screens* has also been discussed in the field of STS. In STS, screens are used to analyze forms of categorizing and ordering. Of particular relevance is the work of the philosopher and ethnographer Helen Verran who has argued that analytical attentiveness to screens holds the promise of facilitating novel accounts of science and technology (Winthereik et al. 2011). As Verran notes, and in line with my empirical observations, screens can be many things and they can take many forms. Different screens, she suggests, surround us. There are the obvious physical screens of computers, cameras or phones. There are also entities that we might not immediately think of as screens but nonetheless imply screening, in the sense of categorizing, like diagrams, lists, and maps. Finally, we might think of screens in a more analytical sense as specific conceptual, discursive or material framings of what *matters* for a practice (Latour 2004).

Due to this empirical and conceptual convergence, I gradually became curious about the potential value of using screens as an analytical device for describing research collaboration and management. In particular, I wondered whether the

notion of screens could be used as a resource for addressing how strategic research unfolded in the empirical context of Lundbeck. The potential value of this would be to introduce a concept that had an empirical bearing among my informants while, at the same time, being very different from the usual metaphors outlined above describing strategic research in terms of 'bridging', connecting or generally aligning. In that sense, the notion of screens implies a particular kind of undecided or *lateral* position (Maurer 2005), neither quite empirical and nor quite conceptual, or both at once, but with varying valences depending on context.

Below I flesh out this idea, by specifying notion of screens that allow me to take into account some of the multiple screens encountered empirically. To strengthen this analytical device, I also discuss a number of other concepts from STS, which resemble screening in the sense that I work with the concept. This framework, I suggest, allow me to shed further light on issues of science-industry collaboration and strategic research practices in the following chapters.

Screens in STS

The notion of screens is not a mainstream concept in STS. However, Helen Verran has recently discussed screens in a way that I find valuable for the purpose of this project. Verran draws a parallel between screens and *indices*, and she argues that screens are primarily interesting to study for their *interventionist* capacities.

Verran develops her concept of screens by way of semiotics. In particular, inspired by Charles Sanders Peirce, Verran proposes that screens are like 'indices' (Winthereik et al. 2011; Verran 2013). In Peirce's terminology, an index is defined by having a direct relation to its referent. The statement "no smoke without fire" exemplifies an index. Here, smoke indices fire, since fire is a precondition for smoke, to which it directly gives rise. For another example, one might think of a

medical symptom as an index of a disease. Here, a disease produces the symptom, and the symptom, like fever, indexes the disease. By understanding screens as indices, Verran argues that screens have a direct relation to a practice.[36]

Though semiotics is often taken as a purely linguistic endeavor, Verran suggests that screen-indices go beyond language: they intervene in the world, organize it and shape action. Like a book index, for example, a screen implies a particular organization that facilitates certain forms of interaction (finding the passages with the key words listed in the index) but precludes others (finding passages with words not indexed). The keywords thus enable a particular and highly structured kind of interaction with the book. It is not simply a representation of the book's content; it *intervenes* in the readers' worlds by arranging things in a particular way and creating specific options for interacting with them.

Verran's screen-indices are relevant starting points for my investigation of science-industry collaboration and aim of developing a framework capable of describing misaligned co-production. Attentiveness to the multiple forms of screening that occur in research collaboration and management facilitates analysis of the implied orders and forms of ordering that these practices draw upon. In recent work, Verran has engaged in a similar analysis of organizational risk communication, which followed the relation between screens and practices of risk (Verran and de Weydenthal *forthcoming*). Here, she demonstrates how the notion of screens might be used in analyses of organizational dynamics. But even so my problems are also in some ways different.[37]

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[36] Verran also bases her concept on Gilles Deleuze's notion of screens. Deleuze argues that screens are like membranes that filter things in particular ways (Deleuze 1993).
[37] See Vedel 2011 for a discussion of screens. I became interested in screens in 2010 during a PhD course on the topic with Helen Verran and Lucy Suchman.

Where Verran uses screens to look at somewhat established orders and the ordering they entail I am interested in using screens to study practices that imply more dynamic constructions of order. My material implies developing strategies and collaborations rather than consist of fairly instituted constructions. Therefore, I develop the concept to fit my research interests in misaligned co-production and my empirical context of emergent strategies and collaborations. In order to further develop the notion of screens, I draw on two additional concepts that I use to emphasize and explore important aspects of my empirical material.

Screens as emergent relations and multiple justifications

The first notion that I draw on to further develop a notion of screens is Strathern's idea of "emergent relations" (Strathern 2003b). Strathern suggests that we should see scientific practice as a continuous process of emergent relations. In this way, the notion directly addresses the dynamic nature of collaboration. In her discussion of emergent relations, Strathern draws on the historian of science Mario Biagioli's idea of "multi-authorship" (Biagioli 2003). Multiple authors constitute scientific practice to the extent that it may even be difficult to clearly determine who and what has been involved in scientific inventions (Strathern 2003b: 167). Drawing a parallel between "kinship relations" and "authorship", Strathern further argues that in present scientific practices notions such as "conception", "origin" and "ownership" are not given but rather continuously reconfigured (Strathern 2003b: 168-169). Instead of seeing science-industry collaboration as a two-sided affair in which university-based researchers form one coherent part and industrial researchers constitute another part, this encourages a view of collaboration as produced by multiple contributions. It becomes less predictable which contributions will be made by whom and from

where. In addition, an interest in emerging relations give attention to ways in which objects and researchers *change* over the course of a scientific project. Rather than assuming well-established relations between participants, we can thus think of collaborations as constituted by developing connections between different "authors" and their various forms of collaborative "work".

I also take inspiration from the work of French sociologists Luc Boltanski and Laurent Thévenot on different "logics of justification" (Boltanski and Thévenot 2006).[38] Rather than viewing collaborative misunderstandings as due to cultural differences we might see them as exemplifying how participants appeal, more or less consciously, to different ways of justifying action. Boltanski and Thévenot's notion helps to expand the spectrum of explanations for collaborators' actions by offering a nuanced alternative to the notion that collaborators must be either academic or industrial and that each relies on a single mode of justification. I do not directly apply Boltanski and Thévenot's framework but rather take inspiration from the general idea that the same situation might be seen and interpreted based on different repertoires of explanation. Thus, I use this approach to explore the different justificatory logics that might be used even by the same researcher or group of researchers in various situations. As described earlier, we often encounter the idea that academic researchers apply one consistent line of thinking caused by their professional and institutional affiliation.

By developing the notion of *multiple* screens to take into account different forms of ordering, emergent relations and the varied use of justificatory logics in research collaboration, I hope to offer a rich picture of such collaborations, premised on difference and divergence rather than on consensus and harmony or

[38] Boltanski and Thévenot categorize justification into six "logics": civic, market, industrial, domestic, inspiration and fame (Boltanski and Thévenot 2006).

on inherently incommensurable differences. I now continue to specify the conceptual and empirical problems that my use of screens should be able to cover.

The multiple screens of collaboration

In this section, I begin to develop a notion of screens based on the particular empirical and conceptual problems that my research topic raises. So first, what kind of problems should my notion of screens address? Answering this question requires reflection on both the nature of my material and the research interests of this thesis, which are different from Verran's.

First, I study how strategic research unfolds in Lundbeck. These are not strategies that are already settled. My material concerns the making of new strategies and approaches, which takes place in dynamic, continuous processes of experimentation. Verran's notion of screens as indices that imply orders and are ordering is not completely adequate for studying practices and strategies that are unsettled and in process. Rather than *drawing* on strategies, diagrams and lists, these things emerge in an experimental process and display different expected futures for Lundbeck. I would like to address this dynamic, experimenting and "future-generating" aspect of strategy making (Jensen 2005; Rheinberger 1997) in my screens. Also, strategic research seems to involve attaching particular importance to something, which is also an aspect of this experimenting process that I would like to address. So I am conceptually interested in something more dynamic than studying orders and ordering imply.

Second, I study science-industry collaboration and this is an arena of dynamic and developing entities rather than stable ones. In Lundbeck, the above-mentioned strategy making involves experimentation not only with strategies but also with

actual collaboration and conditions for these. As mentioned, the notion of the screen should facilitate analysis of how collaboration develops not only in alignment processes but also in activities that misalign the participants in different ways. Verran's orders could potentially cover alignment and misalignment as involving different orders with implications for practice. However, orders seem to imply something slightly more permanent than what I have in mind. Like Fujimura, I want to consider collaborations from beginning to end and see how the participants construct their relation in different ways and with changing implications for the collaboration.

Third, my material concerns different forms of differentiation. Both science-industry collaboration and strategy-making seem not only to be about showing and making visible, but also about hiding and ignoring. In science-industry collaboration, some things are shared while others are rendered invisible, or hidden. Collaborative strategies in the pharmaceutical industry are famous for their confidentiality. In practice, the concerns with confidentiality involve not only sharing knowledge but also strategically hiding knowledge. Similarly, the making of strategies implies that some opportunities are chosen while others are strategically ignored. The analytical screens that I develop should be able to cover this aspect of co-production, which concerns leaving something out of sight or simply unshared.

Rather than use one screen to cover these requirements, I will thus develop a framework of *multiple* screens.

In everyday language and dictionary definitions screens have many meanings. Some appear rather more straightforward than Verran's view of screens as indices. For example, to screen means:

- To conceal, protect, or shelter (someone or something) with a screen or something forming a screen
- To show (a film or video) or broadcast (a television program)
- To test (a person or substance) for the presence or absence of a disease
- To pass (a substance such as grain or coal) through a large sieve or screen, especially so as to sort it into different sizes[39]

According to dictionary definitions, "to screen" also means "to keep something safe and shielded from the outside" and, in fact, also the opposite, "to show and put forward". To screen can also have a scientific meaning, namely to test something in a systematic way. Finally, screening can be used to emphasize the activity of categorizing and sorting according to a particular purpose or aim.

Considering these diverse definitions and comparing them with the kind of processes and problems I wish to address in my material has allowed me to identify three forms of screens that I view as particularly important for the present analysis. Together, I propose they enable the analysis of a series of important aspects of how strategic research and university-industry relations evolve in Lundbeck. I refer to these screens as projecting screens, categorizing screens and occluding screens.

Projecting screens

Several things might be understood as projecting screens. Research strategy documents illustrate one very material form of screens that display an expectation to research and the future it generates. As projecting screens, strategy documents describe a relation between present research activities and future

[39] www.oxforddictionaries.com.

achievements. If we do *this* research with *this* outcome then we will be able to do *this*. In a company like Lundbeck projections of research are often related to a produced drug. Strategy document *projects* research into a future in which a disease can be treated with a drug developed on the basis of the research. Research strategy documents are examples of quite material projecting screens. In addition to these, I also suggest that more discursive things can be understood in terms of projecting screens. A conception of research management like "the project leader of the future", which I return to in Chapter 13, is a projecting screen. It generates ideas about a future organization of Lundbeck and the role that research managers play.

Coming from the Latin word *projectum,* projecting has diverse connotations, including to "estimate or forecast", "to throw or cause to move forward" and "to present or promote".[40] In spite of their diversity, these meanings capture important aspects of strategic research such as developing future scenarios, setting a direction for research and making something visible, explicit and open for interrogation. In addition, projecting also comes in the form of "projecting something on to" and it has the distinct Freudian sense of "attributing or transferring an emotion or desire to (another person), especially unconsciously".[41] Although this meaning is quite different from the above-mentioned ideas of projection, it also potentially relates to aspects of strategic research. This meaning can be used to address how the making of strategies involves giving something a particular prominence. Making it strategic implies making it stand out from something else that is not considered strategic. Taking inspiration from these various meanings, I suggest that projecting screens are

[40] www.oxforddictionaries.com.
[41] www.oxforddictionaries.com.

analytically useful in highlighting the future-generating aspect of strategic research.

Categorizing screens

Many things might constitute a categorizing screen. We can think of quite material things such as collaboration contracts that describe a relation by distinguishing between different collaboration participants. Again we might also think of research strategy documents that highlight certain research activities by distinguishing them from others. Thus, the same thing might be seen as a categorizing and a projecting screen according to what we want to highlight and explore. More discursive things might also be seen as categorizing screens. An example might be an expression such as 'bridge building' that clearly implies a categorization. It involves the categorization of universities and industries as separated worlds in order to make sense of a bridge. I would also suggest, and test, whether arrangements and events can be seen as categorizing screens. It seems plausible that meetings and interaction between collaborators might both draw on and also produce categorizing of the participants with implications for how the collaboration can take place.

Both strategic research and science-industry collaboration clearly emerge from processes of categorization. Strategic research can be seen as the *outcome* of a categorization process in which some things are seen as strategic and important, while others are left behind. In addition, strategic research is a category in itself that is distinguished from other forms of research, as we saw in Danish research policy. Equally clearly, science-industry collaboration involves categorizing. One obvious categorizing is the distinction between academic and industrial. We

might also think about categorization more broadly as related to aligning and misaligning practices.

By deploying categorizing screens, I aim to analyze how the participants in research collaborations and strategic research classify themselves as similar and dissimilar. This screen can also be used to explore how different participants in a collaboration associate the nature of collaboration with different things and generate different expectations on that ground. Drawing on Strathern's idea of emergent relations, I do not see categorizing screens as *established orders*, as Verran's conception of screens as indices proposes. Rather I consider categorizing as an activity that has implications for collaboration because it also *generates* new relations rather than merely imply established ones.

Occluding screens

Many things might also compose an occluding screen. Occluding is about obstructing, as I will explain below. Again, a collaboration contract might serve as an example of an occluding screen. As mentioned, it involves a description of a relation that draws a line between two collaborating parties. But doing this, it also occludes other potential relations between these parties. The specific categorization does not necessarily account for how the collaboration unfolds in practice and in this sense it might occlude practical nuances. There are also more discursive forms of occluding screens. They can be conceptions such as the aforementioned "project leader of the future". Such conceptions project a clear image of something (in this case research management), while simultaneously occluding messy or present aspects of research management practices.

The term occluding comes from the Latin word *occludere*, which means, "to stop, close up or obstruct".[42] As suggested, strategic research and science-industry collaboration involves processes that might be analyzed as projecting and categorizing screens. However, strategic research and science-industry relations also involve the construction of invisibility. In science-industry collaboration, participants might aim to hide certain things from other participants. In strategic research, on purpose strategy makers hide what is not considered within a strategic focus. When I focus on occluding screens it is to emphasize that screens are not only devices from which something *emerges* but also something that potentially prevent relations.

Where the categorizing screen might be visualized as a dividing wall that cuts up a space into compartments and the projecting screen as a giant display for conceptions of future practices, the occluding screen might be seen as a dressing screen that one can *hide* behind. Accordingly, an occluding screen is somewhat like a barrier that can be placed strategically to prevent sight. We might also consider occluding screens as related to *protection*. In this sense, we can even think of Sigmund Freud's notion of "screen memories" in which unacceptable childhood memories are repressed as a defense mechanism, emphasizing a protecting aspect of screens (Freud 1899).

One might also think of ways in which managing research involves a number of activities that are not coherent but involve different perspectives, forms of engagement or, in Boltanski and Thévenot's terms, different logics of justification. Rather than observing how these diverse forms of engagement, in spite of everything, manage to hold together, the occluding screen helps to explore their mutual exclusions and forms of blindness. An interest in occluding screens

[42] www.oxforddictionaries.com.

highlights that managing research involves taking different things into account at different times, and literally, at times, to see a matter in only one way. In that sense, one might also see occluding screens as involved in processes of simplification and as acknowledging that sometimes following one "narrative trail" implies a "frugality" (Thompson 2002: 184).[43]

Further, occluding screens enable analysis both of what participants in a collaboration bring to it and what they strategically choose to hide. The term "collaborator" has the interesting double meaning of being both "a person who works jointly on an activity or project; an associate" and "a person who cooperates traitorously with an enemy; a defector" (Tsing 2005: 245).[44] As we have seen, informal and academic discussions of collaboration often privilege the idea that collaborating parties somehow seek harmony as part of being in a partnership. However, collaboration also implies strategic processes of finding out what the other parties want and developing strategies for achieving separate goals. Hence, an exploration of the occluding screens of collaboration pays attention to what we might call 'purposeful hiding' as well as to the deliberate making of barriers and obstructions within collaborative frames.

From this description of the three screens, we see that there are overlaps between the screens. A contract, for instance, might be analyzed in terms of all three screens and choosing among the screens is a matter of what one wants to analyze and highlight. For instance, we might see the contract as a categorizing screen if we want to explore the basic categorizations and relations that it draws on and produces. But if we rather want to explore how descriptions of research produce futures or involve projections of hopes and desires onto research activities, we

[43] "There is a frugality, however, to following selective, reductionist narrative trails through an episode and resisting multivocality" (Thompson 2002: 184).
[44] www.oxforddictionaries.com.

might see the contract as a projecting screen. Finally, we might also see a contract as an occluding screen and emphasize the things and practices that a contract leaves out or make invisible. Occluding might thus be seen as an implicit effect of categorizing and projecting, as both of these activities involve rendering certain aspects of a collaborative research invisible. For the sake of clarity I have chosen to define screens in terms of these three types. However, it is also possible to think of projecting, categorizing and occluding as diverse aspects of a screen.

Conclusion

In this chapter, I have presented a co-production framework as an alternative to prevalent ways of describing university-industry interaction and to the models of the Triple Helix and Mode 2. I have suggested that a co-production approach allows for empirical investigations of science-industry relations that are not based on an idea of absolute differences between science and industry domains or absolute alignment in form of a partnership. Then I have explored three concepts – boundary objects, doable problems and trading zones – that try to pinpoint what makes collaboration between different participants possible. These three concepts address various relevant aspects of how participants, at the level of practice, align in different ways. I have then suggested that it would be interesting to also explore the role of *differences* in collaboration. Not in any absolute way, but specifically and related to how collaboration develops from misalignment, for instance, as the participants explicate how they are differently engaged in the research they jointly work on.

To develop a framework that can describe misaligned co-production, I draw on Helen Verran's notion of screens. Verran uses the screen to explore the embedded and interventionist orders that things imply. However, since my material and my

research interests are different – I explore experimentation, strategies and collaboration – I develop the notion of screens further. With inspiration from Strathern and Boltanski and Thévenot, I have defined three specific forms of screens – projecting, categorizing and occluding screens. These screens assist me develop nuanced empirically oriented accounts of strategic research and science-industry collaboration in Lundbeck.

With this conceptual framework of screens in mind, I now turn to Lundbeck.

10. MANAGING EMERGENT RELATIONS

Early in my fieldwork, I took an interest in a particular collaboration that Lundbeck had initiated with a group of university-based researchers. Lundbeck research managers referred to this collaboration as "the neurocell collaboration".[45] It concerned the effect of a biological mechanism, neurocell, for drug discovery. This chapter investigates how strategic research developed in Lundbeck in relation to a specific collaboration. My inquiry focuses on the following questions: How did Lundbeck research managers come to see this collaboration as strategic research? What characterized the research as science-industry collaboration? What did managing this collaboration imply? I analyze these questions using the concept of screens that I defined in Chapter 9 as an analytical tool. I look at how strategic research developed in this collaboration in processes of projecting, categorizing and occluding screenings.

Discussion in this chapter is based on participant observation in approximately 20 meetings that occurred during the years from 2008 to 2010. In addition, this discussion draws on interviews with several Lundbeck research managers that were particularly involved in this collaboration. I also base this chapter on informal discussions with research managers, since a part of the activity related

[45] Neurocell is a pseudonym. In the following four empirical chapters, I use pseudonyms for research managers in Lundbeck and their key collaborators. See Appendix D for an overview of key people. I mention the research managers' actual title in Lundbeck to give the reader insight into their position in the company and into how they, in terms of their position, are related to other research managers. I have chosen to use pseudonyms to be able to *differentiate* the research managers. For instance, using pseudonyms, I illustrate how the research managers saw particular events differently, how the same research manager sometimes reflected on the same event in different ways, and how the research managers were involved in quite different activities. I sometimes group the research managers as "research managers in Lundbeck" as they also at times described themselves in general terms, typically in relation to collaborators inside or outside Lundbeck.

to this collaboration took place outside of formal meetings, in hallways and during lunch. Hallway discussion often ended with reflections such as "by the way, I thought about the neurocell collaboration, and this is what I think we should do." Taking informal discussions into consideration, formal meetings were often summarizing reflections that the research managers continuously had about on-going collaborations.

First, I offer a short introduction to what characterized Lundbeck research managers' activities concerning science-industry collaborations in general. Then, I describe what characterized the neurocell collaboration and how the collaboration changed and developed during key events. These events bear witness to the emergent nature of the collaboration. Subsequently, I analyze disconcertment in two episodes. I propose that investigating the disconcertment that emerged in the process of managing this collaboration highlights important dimensions of the nature of strategic research and science-industry collaboration. Finally, I summarize how the concept of screens is useful for making sense of this case.

Becoming interested in science-industry collaboration

I began to investigate external collaboration in Lundbeck by observing specific collaborations and activities related to managing them. Around 2009 to 2010, a group of Lundbeck research managers regularly discussed a handful of research collaborations. These research managers were Jens, who was the head of research; Lars, who was head of drug discovery; and Hans, the divisional director of molecular neurobiology. Occasionally, this group was expanded to include members of the two main management groups, the Research Management Board (RMB) and the Drug Discovery Management Team (DDMT). On other occasions,

the group expanded to include members from other parts of Lundbeck, such as Finance, Business Development, Patents and Trademarks, and Legal. Besides the neurocell collaboration, the collaborations that the research managers regularly discussed were a collaboration with an American non-profit research institution, a potential research collaboration with a Dutch biotech company, and a collaboration within a large European research initiative that involved both universities and companies.

One of my immediate observations was that each of these collaborations was very different from one another. Each of them even appeared to represent a different model of collaboration. Lundbeck's collaborators varied from biotech companies to academic research groups, and the size of the collaborations also changed from small groups to extensive networks of participants. Likewise, it differed who had taken the initiative to collaborate. However, in spite of these apparent differences, it was equally apparent that the group of research managers strategically categorized these four collaborations together and drew parallels between them. Indeed, they often described them as alike, simply because the collaborations shared an external element. They also often referred to these collaborations as similar because they all focused on a particular biological mechanism that was expected to have an important role in diseases in the central nervous system. Researching these biological mechanisms required external collaboration since building up the expertise in-house was seen as too long-term and inflexible. The biological mechanisms were thus both indicative in terms of a new strategy and had the potential to profoundly change the future pipeline of Lundbeck. Discussing collaborations and making specific decisions about them were therefore at once a matter of dealing with them individually and in relation to overall strategy.

For this investigation, I focused on those activities that could be observed when attempting to make sense of these strategic considerations about collaborations. First of all, I observed that the research managers spent a lot of time in meetings in which individual collaborations were discussed and plans were made. Collaboration implied tasks such as planning up-coming meetings with external collaborators; discussing scenarios for collaboration contracts; debating governance structures, both in-house and in relation to the external groups; and reflecting on incentive structures and future scenarios for these collaborations. Dealing with external collaboration also included preparing presentations for Lundbeck's general management in the form of the Board of Directors[46] and the Corporate Management Group[47] that were to approve large investments in new research. The research managers raised a number of questions during the treatment of each collaboration: What are the primary incentives that motivate and encourage this research, for us and for the external collaborators? How do we manage and influence the development of the collaboration? How does the potential outcome of this collaboration relate to in-house activities? How do we present this opportunity to the general management as valuable and worth investing in?

Although these questions were far from trivial and were given serious thought, the research managers approached them with an interesting mix of ease and optimism. Though the developing collaborations represented something new and in some sense unfamiliar, this group of research managers had a familiar way of dealing with new and uncertain things, on which they were clearly drawing. Thus,

[46] Lundbeck's Board of Directors consists of six external directors that are elected by Lundbeck's shareholders and three members elected by Lundbeck's Danish employees.
[47] The Corporate Management Group (until 2011) consisted of the heads of Lundbeck's main divisions. Today this larger group is replaced with the Executive Management that consists of the CEO, the head of R&D, and the head of Finance & IT.

despite the apparent risks of engaging in these collaborations, I noticed both laughter and confidence among this group of managers.

The neurocell collaboration particularly caught my attention. I observed that the group of Lundbeck research managers spent a lot of time on this collaboration, and that they were especially attentive to how this collaboration was taking shape. They had previously interacted with members of the university-based research group with whom they were collaborating, but it appeared that the research had only recently started to look particularly interesting and promising. Thus, deciding what to do about the collaboration was particularly crucial.

The neurocell collaboration

In 2003, a group of researchers made a discovery about the behavior of the receptor, neurocell, a discovery that soon after was published in a leading scientific journal. The authors had found that neurocell, a biological mechanism in the brain, which was usually seen as playing an active role in cell growth, was sometimes, under certain conditions, actively involved in the opposite process of cell death. The authors noted the fascinating phenomenon that in certain situations, for instance, in the case of stroke, the body's own protein immediately starts to kill brain cells, within minutes making the damage of a stroke worse. This discovery had broad relevance, not only for future research but also for drug discovery. For one thing, it became immediately interesting to understand how one might prevent the activation of the neurocell in situations of cell degeneration through drug-based control of molecule interaction.

The discovery of the surprising effects of neurocell created a lot of attention, not only in international research circles, but also in the Danish national context. Thus, in 2005, the Lundbeck Foundation, an industrial foundation granting money

for biomedical and natural science, and the major shareholder of Lundbeck A/S, funded a Centre of Excellence with the two leading Danish authors of the discovery, Martin and Søren. The center, known as BRAIN, was established at a university in conjunction with a department of biomedicine. BRAIN focused on the molecular, cellular, as well as physiological, functions of neurocell. Hence, the center's activities were inherently interdisciplinary, spanning studies of receptor crystal-structure and studies of physiological functions in animal models. It also drew on a large number of methodologies, including neurophysiology, protein structure, genetics and stereology. At the university, the center connected a number of departments across faculties and across specialties within the faculty of health, such as the departments of biomedicine and clinical medicine. The center also had a number of official international collaboration partners, among these the co-author of the 2004 publication, Henrik, who was employed at a different university.

However, the discovery of neurocell's lethal behavior did not merely result in strengthening academic research at BRAIN. Simultaneously, Martin and Henrik founded a private biotech company called PsychoIndex, through which they filed a number of patents involving neurocell technologies. As owners of PsychoIndex, Martin and Henrik contacted research managers at Lundbeck to discuss a collaboration agreement. According to my interviews with Lundbeck research managers, Martin and Henrik were excited about the potential commercial aspects of their research and particularly interested in the opportunity to develop a drug for treating Alzheimer's disease. Their initial contact led to a number of meetings, including visits at the university and at Lundbeck. Finally, it led to a small research agreement in which Lundbeck would test the technologies of PsychoIndex.

In the years 2006-2008, the research field of neurocell grew stronger. At BRAIN and at the university that the center collaborated with, intensified research activities resulted in several new discoveries that were published in high-profile journals including *Nature Neuroscience*, *Nature Reviews*, and *Journal of Neuroscience*. As more specific ideas for how to use the findings in drug discovery developed, the collaboration between Lundbeck and the founders of PsychoIndex grew increasingly interesting. The group of research managers at Lundbeck spent much time contemplating how to develop the collaboration. They were especially curious about the value of the patent filings from PsychoIndex and about the risk of another pharmaceutical company purchasing these patents, thus blocking the way for Lundbeck's ideas concerning developing a drug to treat Alzheimer's disease. Worrying about how to respond to this risk involved consideration of a number of questions. For example, would it be better to acquire the patent applications or rather to let them mature in the context of PsychoIndex and in relation to BRAIN's research activities? Considered in isolation, the patent files were not of obvious value. The files proposed a very broadly defined patent that would only rarely be approved. Even if they were approved, it was not obvious how they would lead to a new drug. Nonetheless, there was something potentially valuable and interesting about the patent files and their prospects of commercial use.

As the result of a thorough due diligence process in 2009, the research managers at Lundbeck decided to buy PsychoIndex, thereby making PsychoIndex a subsidiary to Lundbeck. Martin remained in charge of PsychoIndex as the key executive. This generated a number of new relations between Lundbeck and *the researcher* Martin. From the perspective of Lundbeck, the collaboration changed. Originally, the collaboration had relied on one main relation between Lundbeck

and the owners of PsychoIndex. Now, it relied on three separate relations between Lundbeck and external collaborators.

These new relations were described in three contracts. The first contract concerned the acquisition in which Lundbeck purchased the assets of PsychoIndex; this was an exchange of money for patents. The second contract was a consultancy agreement with the owners of PsychoIndex in which the owners advised Lundbeck on how to use the patents and technologies in their further investigations into the use of neurocell as a target for drugs; this was an exchange of money for advice. Finally, a third contract related to Martin implied a new relation between Lundbeck and the university research group. This contract stipulated that Lundbeck would give an annual grant to the group in return of a 'first right of refusal' to discoveries made by Martin. This right implied that, before publishing, Martin would share new discoveries with Lundbeck and give the company the chance to patent these discoveries before any other companies was given the option.

Both the university and Lundbeck viewed these agreements as a success. At the university, it was presented as a case of how funds for "free" basic research turned into knowledge that could subsequently be taken advantage of by industry. In Lundbeck, it was seen as a success to have acquired the patents and established a strong collaboration with the university-based research group.

With this background in mind, I now turn to a more careful discussion of the neurocell collaboration. Just from lining up the key events, it is clear that the collaboration evolved around a biological mechanism and a number of key people, among these three Lundbeck research managers, Jens, Lars, and Hans, and three researchers employed at universities, Martin, Søren, and Henrik. What I want to highlight, however, is the way key people emerged in different roles and relations, both as *users* and *producers* of knowledge, through the collaboration (Woolgar

1991). In the university context, for instance, Martin is a producer of new knowledge about neurocell behavior. However, he is also a user of the same knowledge in the context of PsychoIndex, where he files patents to sell to Lundbeck. This already suggests that he is in fact related to both Lundbeck and to the university research group in multiple ways. Martin, we might say, single-handedly creates *numerous* science-industry relations across traditional institutional boundaries.

The fact that we can observe this merely by describing the main events of the collaboration suggests that understandings of research collaboration indeed are in need of more complex and dynamic characterizations than the black and white alternatives with which we are too often presented. Using Strathern's terms, we are witness to a situation in which the relations between author and work are neither straightforward nor stable, but rather emergent and negotiated in the process (Strathern 2003b). In fact, exploring the notion of the origin of ideas and discoveries became important for managing the collaboration. So, how, more specifically, can we characterize the emergent relations that constitute this collaboration? To explore this, I now attend to two moments of disconcertment that I observed while participating in discussions of the neurocell collaboration that took place in Lundbeck. As described in Chapter 7, I use disconcertment as a tool for selecting and exploring instances where different screens appear and collide, creating a fundamental unsettlement.

The transfer document

Early in 2009, the research managers' considerations in relation to taking over PsychoIndex involved a process of due diligence. Due diligence is a detailed investigation of everything related to a business that takes place prior to signing a

contract. The purpose of due diligence in this example was to investigate the company PsychoIndex, and to get an overview of the company's assets and obligations, as well as its financial and organizational construction. I participated in five meetings that related directly to this due diligence. These meetings provided occasions for observing interaction between the Lundbeck research managers and employees from other parts of Lundbeck that also participated. The due diligence process involved not only an investigation of the research of PsychoIndex, but also a financial and legal investigation. Consequently, the meetings also included specialists from Patents and Trademarks, an expert from Legal, a specialist from Finance, and a tax specialist. An employee from Business Development hosted the process. At this point, the decision to buy PsychoIndex was already made. However, the research managers were still not certain about what kind of contractual set-up would be the best in terms of continuing collaboration with the owners. Deciding on a set-up required a thorough investigation of the company.

The due diligence meetings took place in the building housing Business Development and Finance. The purpose of these joint meetings was to discuss the progress of the due diligence process, which implied going through key documents such as the patent applications and the university logbooks accounting for the research activities that had led to the patents. In this process, two major concerns had emerged. One concern had to do with the ownership of the research that led to the patents that Lundbeck were now acquiring. The due diligence group at Lundbeck wanted to make sure that they knew exactly who had been involved in prior research activities and that all potential inventors were registered correctly in the patent files. In particular, they wanted to counter the risk that they might end up being accountable to other researchers than the owners of PsychoIndex. Another concern related to the potential obligations that

Lundbeck would take over when buying PsychoIndex. For instance, it was noticed that PsychoIndex participated in an EU collaboration. The group of investigators at Lundbeck wanted to make sure they understood what this relation exactly implied, so this was discussed at the meetings. Both concerns required sorting out complex business and research relations, a process that disclosed that the relevant research had not only been produced by the owners of PsychoIndex but also by students and colleagues at the university.

At one meeting, a document was brought up that immediately attracted both curiosity and attention. Hans was going through a pile of PsychoIndex documents, talking over each of them with the patent specialist. With a slightly despairing voice, yet not devoid of humor, Hans asked, "What about this?" pointing to a document. The document to which he referred showed a transfer of money from a university professor to a consultant. There was a slight pause in discussion in the room, interrupted by a quiet laughter among the research managers. The curious thing about the document was that Martin was both the professor and the consultant. In short, Martin, in his function as company owner and technology expert, had given advice to a research group of which he was also director.

The document generated disconcertment and commotion. There was laughter around the table, shuffling in the chairs, and eye contact between the research managers who knew Martin in person. However, there was also a serious element in this encounter with the document. After all, the group was in the middle of an investigation with the purpose of sorting and categorizing relations. The document seemed to question the exercise itself, since it blurred these relations, rather than supporting a distinction between them. At the same time, the document was unsettling because it raised suspicion of a questionable practice. If this was the case, the document was not obviously hidden.

These concerns came to an end as Hans, holding up the document, addressed the legal expert by asking, “Is this document a problem for us?” The legal expert, who had clearly not found the document as entertaining as the group of research managers, replied, “No. From a legal point of view, there is nothing wrong with this document”. The group then moved on and the intense moment of disconcertment passed. I pause here, however, to ask what we might learn about science-industry collaboration from exploring this moment in more detail. In the following section, I offer an analysis of the episode, suggesting that the transfer document can be understood in terms of *a categorizing screen*.

On the one hand, the document simply illustrates that researchers today often have multiple roles: they act as consultants, company owners, and research directors. On the other hand, looking at the document as a categorizing screen reveals something potentially interesting about how these multiple roles relate to one another in practice. Understood as a categorizing screen the transfer document *does* something; it classifies. The question is then, which categories does the transfer document present? And why does the document as a categorizing screen produce disconcertment? I suggest that the document and the unsettlement it created can tell us something important about the dynamics of science-industry collaboration and the challenges of managing strategic research.

First, what does the document do? There is a particular dynamic related to the document as a screen. On the one hand, it clearly juxtaposes researchers and consultants. It describes collaboration between two entities that are completely *distinct*, a consultant and a professor. It also describes the nature of their interaction. A professor receives advice from a consultant that in return receives payment. This is what you would expect to see when looking at a transfer document. On the other hand, the document as a screen also describes a *collapse* of categories. It turns out that the exchange takes place between Martin-the

researcher and Martin-the consultant. Although something is still juxtaposed and split up, it is also clear that something is the same. The fact that the same person appears in two different roles alters the immediate image of two distinct entities. In other words, according to this document seen as screen, Martin is both two different persons and the same person. The screen is dynamic. In one moment it is possible to see two distinct entities in collaboration, a consultant and a researcher, and in the next moment we only see one entity, Martin.

As a categorizing screen, the document also describes a relation that, in practice, is both impossible and possible. In practice it is impossible to separate Martin-the consultant from Martin-the researcher, and therefore the idea of interaction taking place between these two parties becomes grotesque. The transfer document describes an impossible relation that only makes sense within this document. However, in practice, Martin is neither consultant nor researcher, but deeply engaged in both consultancy and research. Because he is both, the document not only describes an impossible relation but addresses an important fact. Martin has generated a lot of activity in the field of neurocell research, and, in practice, some of the things he has initiated come back to him in different ways. His activities in the context of a company relates to his work in a research center.

The transfer document as categorizing screen generates disconcertment. Why? Some disconcertment clearly arises from the way the document categorizes different collaborative engagements and thereby suggests something grotesque. It suggests that it is possible to distinguish between Martin's diverse engagements in neurocell research to the extent that these engagements in fact reconnect, as if they did not cohere in the first place. The document ignores the fact that Martin is one person. The document suggests a sort of categorical inconsistency or rupture that is disconcerting because it does not relate to the Lundbeck research managers' experience of collaborating with Martin. But there is also

disconcertment related to encountering something that is getting very close to the boundary of what is acceptable activity. It raises concerns that a researcher *single-handedly* is making a transaction occur. However, at the meeting, this disconcertment was being taken care of. It is interesting to observe that the lawyers did not find the document disconcerting. In legal terms, the construction was conceptually possible, and the categories were not as ambiguous as they were to the research managers.

What do we learn about managing science-industry relations based on this example? The example suggests that, *in practice,* it is impossible to separate interests that are assumed to be distinct. In this example of science-industry collaboration, alignment was very strong from the start. The engagements of the distinct participants were even collapsed. Both Martin and Lundbeck did things that were commercial and excellent. Given this condition, what became important was to *separate* interests, as the document did. Thus, we might see the dynamics of science-industry collaboration as characterized by both making alignment and *misaligned co-production*. The case clearly illustrates co-production, not only in terms of merging, but also in terms of explicating differences between various researchers' different engagements. It is interesting that the misalignment in the case of the transfer document was not between two institutions but rather between different engagements that the same participant has. This raises questions not only about how, but also where, misalignment takes place in science-industry collaboration—a point to which I return.

In this example, I analyzed a document as a categorizing screen. I now look at an example of a more discursive categorizing screen, an expressed idea of a "double payment". In the example of the transfer document, disconcertment developed from a sense of *too* strong alignment or even categorical collapse. In the following

example, disconcertment developed from the opposite situation of too little alignment.

The double payment

During the due diligence process, I observed that the research managers had different ways of arguing for the importance of buying the assets of PsychoIndex. Drawing on Boltanski and Thévenot (2006), I see their diverse arguments as different logics of justification. As mentioned, the most common argument for buying PsychoIndex was that the purchase would prevent other companies from acquiring its patents. Such an event would potentially stop Lundbeck from using neurocell commercially. However, another argument suggested that the value of the patents depended heavily on future research on neurocell behavior, since there was not yet sufficient knowledge to know what the possibilities really were.[48] The research managers often used this argument to suggest that the purchase needed supplementation with continued collaboration and research.

I also observed a third argument for the PsychoIndex purchase, formulated by Jens, the head of research, who suggested that the acquisition had an altogether different purpose. According to Jens, the acquisition made an important statement in Lundbeck about changing research strategies. The primary purpose of the acquisition, he argued, did not have to do with external partners, but rather had to do with how research was understood within Lundbeck's organization. Jens suggested that the acquisition of PsychoIndex entailed a kind of 'double payment'. I found this statement interesting, as it seemed to suggest relations that I had not observed previously. As I inquired into this idea of a double payment, Jens

[48] I return to a discussion of the notion of "knowing enough" in Chapter 11.

explained that Lundbeck had *already* paid for this research once by funding a Centre of Excellence, and now they were paying for it again.

The proposed acquisition clearly troubled Jens, and the reason for his disconcertment opened up another perspective on the collaboration. According to him, the first payment for neurocell research took place as early as 2005, when the Lundbeck Foundation funded the BRAIN Centre of Excellence. This grant allowed a group of researchers, among these Martin, to develop ideas and work towards scientific breakthrough. This breakthrough then resulted in important publications and in patents filed by PsychoIndex. Now, however, Lundbeck was purchasing the patents and granting additional funds to the research environment at the university. In that sense, they were paying twice for the research. How did this statement categorize and describe the collaboration? I suggest that if we want to understand the idea of double payment it entails investigating this statement as a categorizing screen. Although the screen in this case is discursive, as it emerges in conversation, it is not only discursive, because it relates strongly to material organizational structures in Lundbeck that separate Lundbeck A/S from the Lundbeck Foundation. Understood as a categorizing screen, the statement of a double payment describes the collaboration in a new way.

Contrary to the transfer document that separated Martin's different involvements in neurocell, the double payment notion categorizes certain things that in practice are formally distinct as the same. This notion has an effect opposite to that of the transfer document, which rendered certain things formally distinct that in practice were the same. First, the idea of a double payment depends on interpreting neurocell research in the context of the BRAIN center and patent files in the context of PsychoIndex *as the same thing*. According to a traditional understanding, university research produces one type of object (neurocell as a

fact) and companies produce another type of object (neurocell as a *product*). However, Jens sees facts and products as closely related. He sees research results as comparable with patents and spin out companies, since all of these objects are the results of the initial research grant.

Second, the idea of a double payment describes a categorical collapse. It depends on a construction according to which Lundbeck A/S and the Lundbeck Foundation are merged entities, although they are formally two different legal entities with different missions and organizations. The Lundbeck Foundation owns the majority of Lundbeck and gives grants to biomedical and natural science in areas of research directly relevant to Lundbeck. Lundbeck A/S, on the contrary, is a company that engages in research with the main purpose of developing drugs. Consequently, although both are "Lundbeck", we would expect these two entities to see the potential in research activities in different ways. However, the notion of the double payment suggests that Lundbeck and the Lundbeck Foundation are related to neurocell *in the same way,* and that, in practice, their interests amount to the same thing.

From what perspective does this statement make sense? As head of research, Jens was authoritatively knowledgeable about the formal organization of Lundbeck A/S, the different Lundbeck institutions and their relations. Thus, the statement was not coming from a naïve outsider who would not immediately see the different parts of Lundbeck. However, as Jens explained his point of view, the acquisition of PsychoIndex was a double payment in the light of the changing strategies in Lundbeck A/S. According to Jens, Lundbeck A/S's interests in neurocell had grown out of a new strategy according to which neurocell research was no longer outside the scope of Lundbeck A/S's interests. Indeed, the new strategy had a strong focus on basic biological mechanisms and their potential for drug discovery. However, Jens worried that the Lundbeck Foundation did not

recognize the implications of this strategic change that would bring the interests of Lundbeck A/S and the Lundbeck Foundation closer. Consequently, he saw the situation as a matter of an unfortunate misalignment between Lundbeck A/S and the Lundbeck Foundation, although, formally, coordination was allowed to take place.

By this categorizing screen, excellent research and commercial activities become recognizable as the same, both in terms of the institutions that engage in these activities and in terms of the activities themselves. I suggest that Jens' disconcertment is caused by a collapse of interests in excellence and commercialization. I observed this collapse of categories during the PsychoIndex due diligence process, since it led to a kind of involuntary explication of the specific nature of the collaboration. The interaction among the participants in the neurocell collaboration brought to light the instability of categories of excellent and industrial research and elicited different kinds of relations between science and commercial activity. Seen from within the collaboration, the participants were at once engaged in both emergent excellent research and emergent commercial activities.

The stories of the transfer document and the double payment both contain disconcerting constructions that can be analyzed in terms of a categorizing screen. I suggest that these constructions are important to explore in order to increase our understanding of the dynamics of science-industry collaboration and the potential challenges of managing it. Both of the stories allow for the articulation of categorizing screens relevant to science-industry collaboration. The collaboration, which might also be seen as a case of university-industry collaboration, is brought to light as a complex assemblage of highly heterogeneous practices. In these practices, categories of science and industry collapse and differentiate in intriguing ways. Understanding and exploring this

dynamic is important for generating relevant accounts of what science-industry collaboration entails.

Strategic-explorative research

I now proceed to explore through which screens strategic research develops in Lundbeck. I begin by investigating how the neurocell collaboration gradually became strategic in Lundbeck. Subsequently, I look at how that collaboration was managed, paying particular attention to the screens this process involved. Not least, I explore the kind of management issues that arose in the context of this collaboration. This is interesting, insofar as the collaboration challenged the idea of stable science-industry collaboration, and thus presumably would also challenge the standard ways in which we imagine that such collaborations ought to be managed. Because the collaboration was constituted by emergent, rather than stable, relations, the problem of managing it ceased to have to do with solving the issue of how to bridge a default gap or break down a barrier. As we have already seen, if there are gaps and barriers in this case that separate different participants, they do not usually appear between the collaborating institutions, but rather within them (as within Lundbeck).

First, I look at how neurocell emerged as an instance of strategic research in Lundbeck. As my chronology of the main events suggests, the collaboration was not initiated based on a predefined strategy. Rather, it was initiated in discussions that happened between research managers from Lundbeck and the owners of PsychoIndex, and which dealt with the commercial prospects of neurocell behavior in brain diseases. As we saw, the collaboration gradually developed and transformed from this initial contact into a complex arrangement including both companies and research centers. One might then say that the collaboration

eventually *became* strategic research due to developing events rather than to preliminary predictions and plans. Making this research collaboration a matter of strategic attention was not so much a matter of making a decision as an outcome that related to the timing and maturation of research. For this reason, the collaboration is illustrative of what we might call *'strategic-explorative'* research. Engaging in neurocell research entailed a form of strategic experimentation rather than the execution of a ready-made strategy. For instance, Hans reflected on the experimental nature of the collaboration:

> We knew at that time that if we thought it through all the way to the clinic we would run into some difficulties. So it was hard and there were some weak indications but we also knew that this was very early biology. So either it would turn out to be a bad investment, and we would do some discoveries that wouldn't go anywhere, or we would in fact learn something over the next period of time.

I now consider through which categorizing screens this strategic-explorative research took place. This involves exploring how the Lundbeck research managers characterized the collaboration and the work in which they were involved. It also entails looking at the categorizations that emerge from describing how the collaboration was strategic and explorative, and how the Lundbeck participants related to external collaborators. In this section, I do not pinpoint one thing that can be understood in terms of a screen, but test what happens if we understand the research managers' descriptions and discussions of collaborations in terms of categorizing screens.

First, we have Hans, who described the interaction between the participants as if it made no difference where the participants come from. They all engaged in a strategic-explorative practice of considering different scenarios:

> We discuss both on a very detailed level sometimes, but also at a very general level: 'this could be interesting' or 'we have seen something'. Or we come back and say 'That which we talked

about is clearly impossible', right. Where he then says, 'Why don't we just do this and develop it in this disease?' and I say, 'It just isn't possible. We can't make those studies'.

Hans described how collaboration involves 'discussing at different levels', both in details and in general, and exploring different scenarios. It includes asking questions, exploring them and coming back with answers. Hans described the collaboration as uncomplicated. There is interaction about ideas in which some ideas are seen as impossible, while others seem to have a potential. We notice two things. First, that the industrial research managers do not have a ready-made research scheme, but rather that they engage in a dialogue. The process is explorative rather than based on a ready-made strategy. Second, there are no immediate indicators in this research managers' description of what the industrial contributions are and what the academic contributions are. There is discussion at several levels, and ideas and decisions emerge from this interaction. We might make sense of the description as a categorizing screen in which distinctions between strategic and explorative, and industrial and academic are blurred.

Then, the same research manager, Hans, also suggested something quite different about strategic-explorative research. According to him, the neurocell collaboration was strategic and important for Lundbeck, but would not have been possible to initiate *within* Lundbeck. He suggested that because the collaboration was a complex, partly external arrangement, it made certain things possible that would otherwise not have taken place in Lundbeck.

> They are doing so many new things that we would never do, because we are kind of more streamlined and controlled in what we do, whereas they are more active, right. They make a PhD student do some kind of crazy experiment, I mean, it's not crazy, but you [wonder], 'What will come out of all this?' right. And then nothing comes out of it. Or a nice scientific understanding comes out, but you can't really use it. But then suddenly against all odds someone *does* discover an extremely important thing.

If we comprehend this research managers' description in terms of a categorizing screen, we can observe that the account has more *distinct* categorizations, compared to the previous description. There is a clear distinction between the academic world and the world of Lundbeck that was otherwise blurred in the first description. As a screen, his description distinguishes between the *"crazy"* research activities that are allowed to take place at the university and the *"more streamlined and controlled"* organization of Lundbeck. However, rather than only distance himself from these crazy ideas and ask, "What will come out of all this?" Hans also argued that it is important to relate to them because this experimentation is also a source for developing *"an extremely important thing"*.

By comparing these two quotes and exploring them as screens that categorize, we see that Lundbeck strategic research develops in a strategic-explorative process. Sometimes engaging in this research is based on a strong alignment with external collaborators, as the first quote suggests. At other times, defining it as strategic involves differentiating Lundbeck from external research environments, as the second quote suggests. In this example, external collaboration becomes strategic because it is possible to experiment more freely in a semi-external research situation. This is fascinating, since it seems that what is strategic is the opportunity to relate to a relatively free form of research. Consequently, we see another instance of a categorical collapse, according to which strategic and free/explorative research merge.

I now move on to explore what managing this form of strategic-explorative research implied. As suggested earlier, there was a strong alignment of research interests in the neurocell collaboration. This condition had implications for how management developed.

Collaboration as "many different entrances"

One might imagine that managing this collaboration was challenging since it implied governing emergent relations, developing strategies and relating to researchers with multiple roles and interests. However, rather than conceive of the situation as a problem that needed a solution, the research managers in Lundbeck argued that there was an advantage to these emergent relations. Hans described the numerous engagements with Martin this way:

> In fact, it's an advantage to us, because it gives us an entrance, or many different entrances, and in this way it's only an advantage that he appears many places, right, because our contact interface through him actually becomes much larger.

Understood as a screen, the idea of Martin providing *"many different entrances"* implies a separation between Lundbeck and Martin, categorically. But rather than see Martin as part of one external world, Hans suggests that Martin links Lundbeck to a number of commercial and academic worlds. Interacting with Martin is subsequently the same as interacting with many different collaborators and research environment. Rather than control these relations, they are valuable because they are multiple and allows for different ways of collaborating. One might think that managing this multiplicity is mainly difficult. But it seems that, according to this idea of "entrances", it is rather a condition that potentially makes strategic-explorative research possible and valuable.

Rather than try to control the many entrances, Hans rather suggested that this condition allowed them to enter many different worlds in which they also came to have different roles. As he described it, entering a world sometimes required becoming similar to researchers. Again, we might understand this quote as a categorizing screen in which assumed distinctions between industrial and academic become blurred.

> I put on a creased shirt, or something like that, and then I become part of the environment. I know how it looks, and then it appears to him [the university researcher, Søren] that we are equally interested in this science when we first get to the table.

In this quote, Hans described how he "put on creased shirt" and this gave him access to an academic environment that he was not normally part of. However, he was very familiar with specific cultural codes that applied to this world and how using them potentially gave him access to particular discussions. Putting on a creased shirt made him appear as *"equally interested"* in the science and gave him access to *"get to the table"*. Understood as a screen, this description has an interesting categorizing dynamics. On the one hand, it makes a separation between the world of academic research and the world of industry. In order to access the academic world, industrial researchers must align themselves with certain cultural norms, here suggested as being a particular dress code. On the other hand, this description as a screen proposes that, in reality, the participants are equally interested in the research, but that it takes persuasion to make the academic collaborators see that the industrial research managers have similar interests.

Above, I have suggested that managing strategic research in Lundbeck is about managing emergent relations. Contrary to what one might expect, the emergent aspect of this research does not make it less strategic. We have also seen that managing strategic research is, at least to some extent, related to developing many different entrance points to research. Rather than control these entrance points, the research managers argue for the strength of having many opportunities.

However, even though managing strategic research was characterized by handling emergent relations with a certain reflexivity about entering different worlds, I also encountered a different form of managing. Managing in the form of

building relationships was most apparent in the early stages of the collaboration. Around the time of the due diligence process, however, the emphasis shifted to sorting out the emergent relations and even dealing with them separately. This form of management was more goal-directed and indeed more focused on gaining control. I now look at three collaboration contracts that emerged after the due diligence investigation and suggests that these contracts might be understood in terms of *occluding screens,* in that they made it possible to understand and govern the collaboration in three distinct relationships.

Managing research through contracts

As mentioned, the due diligence process resulted in a contractual framework consisting of three separate contracts. Each of these contracts defined a distinct relationship between Lundbeck and Martin. I suggest that the three contracts and the relationships they described can be understood in terms of an occluding screen, as they mutually occluded each other. As described in Chapter 9, the conceptual framework, occluding is about obstacles that are set up to block free access or vision of something. The first contract defined Lundbeck as the buyer of patents and Martin and his partner Henrik as the sellers. In the second contract, Lundbeck was the buyer of expert advice and Martin and Henrik consultants who offered this advice. Finally, in the third contract, Lundbeck offered money for "free" basic research activities related to neurocell at the university in exchange for access, obtained through Martin, to potential future discoveries.

Thinking in terms of this metaphor, we observe that the three contracts manage the mixed up and emergent relations of the neurocell collaboration by separating them, then dealing with them one at a time. Consequently, from the perspective of each of these contracts, the relations that the other contracts accounted for were

invisible. The occluding screen thus does something quite different than the categorizing screen. The categorizing screen also separates, but it is very clear what it is that is separated. If academic and industrial are categorized as separate domains, it is still clear that there is both an academic and industrial domain in play. In contrast, the occluding screen renders something *invisible*. Thus, although the neurocell collaboration was constituted by many different relationships, according to the consultancy contract, it was only about consultancy and not about basic research. From within each of these contracts, we only see consultancy, basic exploration or purchase. Instead of *multiple* entrance points, we now have *singular* entrance points that are regulated and controlled in different ways.

I suggest that this contractual arrangement implied a radically different way of managing the neurocell collaboration than what we saw in the former descriptions that emphasized multiple roles and relationships. As consisting of three occluding screens, the contractual arrangement implied important separations between the different roles and relationships that constituted the collaboration. As a result, from the perspective of each screen, the collaboration emerged as one-dimensional rather than multi-faceted. However, at times, this one-dimensional screening of the collaboration was challenged in practice. I now describe how the occluding aspect of the contracts became a practical problem.

Dissolving screens

In the contract that described a future collaboration between Lundbeck and the university research environment, it was stated that Lundbeck, in exchange for annual grants, would get access to new research findings, specifically those relating to Martin's research. The university management preferred this

formulation, since making a framework agreement in which Lundbeck was given access to all research related to neurocell at the university was seen as risky and not feasible. However, although the contract formulation had aimed at a limited access, the fact that Martin, in practice, was part of every publication became a problem. As a consequence of this contractual formulation, a large group of researchers were indirectly drawn into the collaboration between Lundbeck and Martin. According to Hans at Lundbeck, an emerging conflict within the university group threatened to jeopardize the long-term value of collaborating with the group as a whole:

> All of a sudden there are some young associate professors that are asked if they are part of Martin's group or not. They are clearly still working with Martin, so from our perspective we can say, "Yes, they are part of it." They are also independent and have their own funding, but they are dependent on a close collaboration with Martin, and his name is on their publications.

As Hans argued, making a formal contract between Lundbeck and Martin created new boundaries. Although these boundaries were part of a legal construction that, in some sense, seemed quite far from the collaborative activities, it still affected the group. Making these contracts implied strongly articulating some relations while ignoring others, and prioritizing research coming from some researchers rather than others. Within the contracts, this was not a problem because, from a legal point of view, this occluding construction was conceptually possible. However, in practice it was quite difficult, and also unfortunate, to occlude the larger network of researchers that was involved in neurocell research. It was unfortunate because the value of the collaboration lies in exactly the way in which it preceded without clear distinctions between groups and activities. The occluding effect of the contracts eventually became such a significant threat to the collaboration that lawyers at Lundbeck and at the university reopened the

contracts and defined the relation between Lundbeck and Martin in a way that took into account the activities and people that related to Martin.

I suggest that this example, even though it is only briefly presented here, is important for understanding the science-industry collaboration and what managing it entails in practice. It is particularly important to notice that the collaboration is managed in a quite open way that allows things and relations to emerge. As we see, as things develop, it becomes more pressing to control relations that are potentially commercially interesting. However, enforcing this control, for instance, through contracts, is not unproblematic, since the openness of the collaboration is also its potential value. Openness simply allows things to emerge that are impossible to imagine before they are suddenly there, as one research manager stated it.

I now move to a discussion of what using screens as analytical tools has so far resulted in.

Screens and doable problems

Working with this case of neurocell collaboration in Lundbeck raised a number of questions concerning science-industry relations and the management of strategic research. What characterizes science-industry collaboration in Lundbeck? What kind of management issues does it raise? How are individual collaborations such as the neurocell case related to overall strategies? As I have suggested, attentiveness to various forms of screens and modes of screening is helpful in addressing such questions. Here I suggest that this mode of analysis can be enhanced also by taking into account Joan Fujimura's notion of doable problems (Fujimura 1987, 1996).

Looking at the collaboration in terms of constructing a doable problem highlights the fact that the collaboration was the result of a number of interactive events, which together produced the doability of neurocell research. The collaboration was not the outcome of rational choice among research managers in Lundbeck but rather the consequence of experimentation in which research managers co-produced neurocell with many other people. It took hard work to figure out how new ideas about the behavior of neurocell might relate to Lundbeck's strategies and its business. To say that the collaboration was simply a matter of good timing would be to significantly underestimate the effort that went into getting it into place. The notion of doable problems focuses attention exactly on the articulation work that the group of research managers had to engage in to make the collaboration work.

To Lundbeck research managers, the process of considering neurocell as a strategic research area involved quite diverse activities and different phases. As mentioned, primarily three Lundbeck research managers worked on making the collaboration doable. They did this in quite diverse ways, which together illustrate a wide spectrum of articulation work activities related to making neurocell research strategic to Lundbeck.

Hans, the divisional director, was familiar with the university researchers, Martin and Søren, since he used to be employed at the university before coming to Lundbeck. In Lundbeck, Hans was known for having an extensive network and for being up to date on the relevant research issues. In order to access the neurocell collaboration, Hans would be the one who had browsed through journals to read about neurocell and who discussed specific data and new opportunities with the university research group. He also had insight into the governance of research at the university and detailed knowledge of the collaboration agreements.

Lars, the head of drug discovery, was concerned with quite different issues. His main worry was how the collaboration activities relating to neurocell would fit into the portfolio of research projects in Lundbeck. Working on this problem involved drawing numerous organizational diagrams on how new research entered Lundbeck and how external projects were resourced.

Finally, Jens, the head of research, again had completely different concerns. Jens was primarily interested in how this collaboration exemplified a new strategy focus on biological mechanisms and thus spearheaded a completely new way of doing research in Lundbeck. To Jens, working on the collaboration involved making convincing presentations about the relevance of this new approach to the general management of Lundbeck, discussing the collaboration with the other members of the Research Management Board, and emphasizing the unique contractual framework in which new incentive structures were defined.

At the managerial level, the doability of the collaboration was premised on this particular division of labor among the three research managers. Making it doable required attending to multiple issues at once: dealing with the science and emergent data; aligning with the in-house organization and contemplating resources, budgets and governance; and fitting into the broader organization of Lundbeck.

In this regard, it is noticeable that it seemed to take *more articulation work* to make the collaboration doable vis-à-vis Lundbeck than in relation to the external researchers. There are many reasons for this. For instance, there was an overlap in scientific backgrounds between some of the university researchers and some of the Lundbeck research managers; the research managers were *as interested* in this research opportunity as the university researchers; it was a matter of experimentation and as a new phenomenon in Lundbeck, it was necessary to explicate what the collaboration opportunity was about and afforded in terms of

business opening. I would like to emphasize this point that the problem of doability was not *mainly* an external achievement but rather an internal one. For what this suggests is that the worlds that had to be aligned to make the collaboration doable were not mainly a Lundbeck world in contrast to a university world. Rather, a number of things needed alignment in-house whereas it seemed fairly easy to construct a contractual arrangement between the involved parties. It was even possible to adjust this external framework when it showed to cause certain unwanted problems. It was certainly the case that there were distinct interests and engagements at play in the collaboration, but the main problem had little to do with creating *alignment* between academic and commercial interests, for in fact academic and commercial did not emerge as clearly distinct to begin with. To understand this situation, I return to what it might mean to think of the collaboration in terms of screens.

I have already mentioned several situations in which events can be understood in terms of screens that do something such as *categorize* or *occlude*. The transfer document operated as a categorizing screen that separated Martin-the consultant from Martin-the researcher, and the three contracts each described one relation, while occluding others. What if we consider the feasibility of the collaboration as constituted by various screens that make particular categorizations, occlusions and projections?

As noted, there were many shifts in the collaboration; relations changed continuously and neurocell transformed from being potentially interesting to being greatly important for Lundbeck's future business. Corresponding changes could be observed in how research managers described their partners: they changed from being seen as very academic and mainly interesting for scientific matters to highly interesting as business partners with specific commercial assets. Rather than see these changes as indicative of inconsistencies and

misalignments, I suggest that we might see them as produced by different screens operating in the course of the collaboration. So rather than say that the collaboration was made doable through alignment, I propose that different screens produced and governed the dynamics of the collaboration. In that sense, forms of screening and doability are inseparable. For one thing, screens that differentiated the collaborating parties, or misaligned them, like the contracts were as important as screens linked the collaborating partners and made them cohere like the idea of a double payment. In some situations, it was important to explicate differences among the participants while at other times, it was important to explicate an equal interest in the science.

Rather than notice efforts to align between diverse parties, in this chapter we have seen different screens that emphasize efforts to *differentiate* diverse participants. Specifically, we can think about screens in relation to the different phases of the collaboration. In the beginning, it was important to screen the collaboration as research involving parties with scientific-commercial interests in neurocell. The different backgrounds for being interested in neurocell became fused into one collaboration. There were efforts to align, even at the level of cultural codes as the creased shirt illustrated. In a second phase, differentiation became increasingly necessary. A different screening, taking the form of contracts, produced collaborators that were clearly distinct. At this stage, the research managers no longer support the previously valorized sense of indistinguishable activity. Like this, I suggest that the notion of screens is helpful in terms of describing misaligned co-production. This is relevant since producing misalignment turns out to be as important as, and sometimes even more important than, producing alignment. Thus, the main benefit of considering these changing relationships in terms of screens that give rise to them is that they make

visible a form of articulation work that both create sameness but *also* seek to differentiate between different worlds and forms of engagements.

Conclusion

In this chapter, we have seen how a group of research managers from Lundbeck deals with an emergent research opportunity. A close study of this collaboration has suggested that managing strategic research is neither straightforward nor premised on stable categories of industrial and academic interests. Instead, such managing requires dealing with a number of complex relations and trajectories and it depends on the ability to categorize and occlude these relations in diverse way, thus transforming the collaboration, moving it forward and making it doable.

Using the notion of screens has also raised important questions for the following chapters. What constitutes a screen and how does one recognize it? Can screens be both intentional and even management tools that are deliberately used or do they just coincidentally occur? In this chapter, I have suggested that both material things such as documents and discourse such as talk can be made sense of as screens. Rather than restrict my analysis to either material objects or discourse I have explored screens that produce disconcertment or disconcerting situations. I have found these screens the most interesting ones, regardless of what produces them, since they can be used to say something new about the *challenges* of managing science-industry collaboration and the different ways this is done, for instance, through contracts or with an open approach. I will reflect on this question of what produces a screen as I proceed from here.

11. THE FIRST ENCOUNTER

Simultaneous with exploring the neurocell collaboration, another collaboration was under preparation. It concerned plans for joint research with the Mayo Clinic in Florida, an American nonprofit research institution engaged in medical research, research education and treatment.[49] Although the Mayo Clinic was not a public university, research managers from Lundbeck considered researchers from Mayo Clinic as "academics". As a divisional director in Lundbeck, Hans, said "they are definitely academics, you see it in their way of working".

Like the neurocell collaboration, "the Mayo collaboration" (as it was called in Lundbeck) also explored biological mechanisms involving proteins such as LRRK and tau. This took place with a particular interest in the perspectives that these mechanisms might hold in terms of developing a drug for Alzheimer and Parkinson's diseases. Unlike the neurocell collaboration, the Mayo collaboration did not evolve out of a specific scientific breakthrough that held a commercial promise. Rather, it arose due to the personal relation between the head of research in Lundbeck, Jens, and the department manager of neuroscience at the Mayo Clinic, Adam, who had communicated about what seemed to be converging strategies. For years, Jens and Adam had discussed the possibilities of a collaboration between Lundbeck and Mayo Clinic but had not yet managed to define one. Presently, they were interested in exploring the benefits of working together simply by making the attempt. So unlike the neurocell collaboration, in which a formal collaboration emerged from a clear sense of urgency, this

[49] www.mayoclinic.org. I agreed with staff at the Mayo Clinic to use pseudonyms for key people but otherwise not conceal that it was the Mayo Clinic.

collaboration developed from the strategic decision to try to define a framework for research collaboration with Mayo Clinic.

According to several research managers, defining such as framework involved a rather new and open-ended approach to working hypotheses, specification of subprojects, scientific approaches, and particular experiments. Rather than focusing on a single target this collaboration was strategic in the sense of exploring particular areas of interests, centered on tau and LRRK, which it was then hoped would gradually develop into entities of relevance for drug discovery.

Recurrent recalling of disconcerting events

What mainly caught my attention, however, was not this new and open way of defining collaboration. It was rather the *recurrence* of an intriguing story, concerning a first meeting between Lundbeck and the Mayo Clinic. The story that was repeated by Lundbeck research managers (and eventually also Mayo Clinic staff), who reveled in detailing the "disastrous" first meeting with scientists from Mayo Clinic. The two groups had completely misunderstood each other, both with regard to their expectations for collaboration and respective roles. Retelling the story, research managers in Lundbeck emphasized the sense of creeping embarrassment and surprise they felt when confronted with a completely unrecognizable interpretation of their own intents and interests. In Lundbeck, this case was often used to illustrate the challenges of research collaboration with academics, and in particular how such collaboration involved being confronted with external researchers in new ways. It was also noteworthy that the story about the first meeting was not only told in the involved management team but also seemed to hold importance across teams. In 2009, for example, it was used as the main case at a Research Management Board (RMB) seminar called "Future

models of external collaboration".[50] It also appeared as a key case at the workshop series "Optimize External Research Collaboration" that took place in the context of the Drug Discovery Management Team (DDMT).[51]

In this chapter, I explore the Mayo collaboration to find out what kind of screening processes took place in this collaboration. The case is interesting, not least because in spite of the failed first meeting, in hindsight the collaboration was described as a success, both by Mayo Clinic staff and Lundbeck research managers. Using the notion of screens, I investigate how particular material-discursive circumstances in this first meeting produced what I see as a categorizing screen that separated Lundbeck and Mayo Clinic participants. I then explore how this initial screen was dissolved and how other things in the process of collaboration eventually produced other categorizing screens. In this chapter, I look at the dynamics of science-industry relations as related to the making and negotiation of diverse screens, not only screens that produce sameness but also screens that make the participants, their interests and engagements, *different*.

This chapter is based on participant observation in meetings and seminars in Lundbeck in which the Mayo collaboration was discussed and planned. In addition, it draws on interviews with the four Lundbeck research managers and one business developer who were all deeply involved in the collaboration. These people were Jens, the head of research; Hans, the divisional director; Jesper, a chief scientist and section head; Trine, the department manager for neurobiology and Will, a business developer from the American research site in Lundbeck.

[50] As described in Chapter 7, I hosted the seminar "Future models of external collaboration" for the RMB. The members of RMB repeatedly used the Mayo collaboration as an example of a challenging as well successful collaboration with external academic researchers.
[51] As I return to in Chapter 13, I also hosted the workshops named "Optimize External Research Collaboration" in which the Mayo collaboration emerged as an important example of collaboration.

Since I continued to hear about the mutual misunderstandings between the group from Lundbeck and the group from Mayo Clinic, I became increasingly curious to know how the event was seen from the perspective of Mayo Clinic staff. Hence I also interviewed Jerry and Dave who were both principal investigators from the Mayo Clinic and each responsible for one of the three main research projects of the collaboration; Nancy, a licensing manager at the Mayo Clinic; and Margaret, a technology development liaison officer at the Mayo Clinic. Nancy and Margaret were both involved in developing the collaboration contract with Lundbeck. Except for Nancy, all interviewees participated in the first meeting. The purpose of interviewing Mayo Clinic employees was not to ensure an equal representation of Lundbeck and Mayo Clinic's respective perspectives on the collaboration. Rather, it was to put into perspective the Lundbeck research managers' perceptions of the event.[52]

The opportunity to interview Mayo Clinic employees itself developed in an interesting way. After having studied the collaboration from within Lundbeck, I was keen on interviewing the department manager from the Mayo Clinic, Adam, and the three principal investigators Dave, Jerry and Laura. However, I soon realized that getting access to Mayo Clinic staff was not unproblematic. The department manager, Adam, never responded to my email requests and Jerry and Laura's willingness to be interviewed was conditioned on approval from the licensing manager at Mayo Clinic. However, this manager, Nancy, argued that getting such an approval would require a separate meeting in which lawyers from Lundbeck and from the Mayo Clinic participated and discussed the terms of the interviews. In her view, simply communicating about the collaboration was a potential violation of the agreement between Lundbeck and the Mayo Clinic.

[52] See Appendix D for a list of key participants and their positions.

However, this situation changed as a consequence of two events. First, Jens wrote directly to Nancy that "bringing in the lawyers" would not be necessary, as my investigation of the different experiences of collaboration would benefit both sides of the collaboration. Second, Nancy herself queried me several times about the purpose of my research. During these interviews, it became clear that the main problem was not in fact my research as such, but rather that I, as a Lundbeck *employee*, took an interest in investigating a collaboration partner. When Nancy learned that interviewing Mayo Clinic employees was part of a PhD project she therefore allowed the interviews and allowed that the Mayo Clinic could appear in my thesis. She even suggested that I also interviewed "administrative people" like herself and Margaret who again had "quite different perspectives" on the collaboration compared with Mayo Clinic researchers. Eventually, I was even invited to visit two of the Mayo Clinic offices, in Jacksonville, Florida and in Rochester, Minnesota.[53]

I continue to describe the collaboration as research managers in Lundbeck accounted for it. I then explore the disconcertment to which the first meeting gave rise, with particular reference to the screens that emerged and collided in this meeting. I then examine how and why these screens are constructed and how they changed in the process of defining a collaboration. Subsequently, I change perspective and look at the same event from the perspective of Mayo Clinic staff. Finally, I discuss how we might see these different and subtle screenings as significant for the practices of science-industry collaboration and strategic research management in Lundbeck.

[53] A visit to the Mayo Clinic was planned to take place in spring 2011 but cancelled due to unforeseen circumstances (pregnancy complications). Instead, I conducted the interviews using phone, Skype and videoconference. In one case, I was able to supplement a phone interview with a face-to-face interview, as Jerry from the Mayo Clinic was visiting Lundbeck headquarters in May 2011.

The Mayo collaboration

In 2007, a group of research managers from Lundbeck traveled to the Mayo Clinic in Jacksonville, Florida. The group consisted of Jens, Hans, Jesper, Trine and Will. The purpose of the trip was to visit the department of neuroscience at the Mayo Clinic. According to interviews, the trip was imbued with several expectations, among these to find out if the "personal chemistry" with the Mayo Clinic researchers was good and if it would be possible to define a framework agreement with the Mayo Clinic concerning research on tau and LRRK.

In advance, Jens had discussed the purpose of the meeting with Adam and they had decided that it would involve a presentation of the department of neuroscience and a tour around the Mayo Clinic site, including visiting labs and a hospital facility. The trip would also include a joint meeting. On the second day, smaller workshop meetings were planned between Lundbeck representatives and Mayo Clinic researchers.

As mentioned, Jens and Adam had discussed the opportunity to collaborate for years. However, Jens in particular argued that it was not until now that there was a good reason to collaborate. Things had changed, both at Lundbeck and at the Mayo Clinic. At Lundbeck, new strategies were emerging that put a strong emphasis on understanding the biology of diseases. At the Mayo Clinic, things were also changing. The general management of the Mayo Clinic had announced that the future strategy of the Mayo Clinic would focus on more basic research. Thus, there was a joint interest in exploring biological mechanisms at a basic level. I addition, both Lundbeck and the Mayo Clinic were under financial pressure. In Lundbeck, this related to expiring patents and changes in the regulatory system, which pushed the company towards new research models. At the Mayo Clinic, basic funds were decreasing and department managers were now searching for external funding. One might say that the strategies of the Mayo

Clinic and Lundbeck were *converging*, making joint research sensible from many points of view.

Adam hosted the meeting at the Mayo Clinic, which took place in a large conference room. During the day, researchers from the Mayo Clinic came into the room, gave presentations, participated in brief discussion with the Lundbeck research managers, and left. Consequently, there was a lot of traffic in and out of the room. Some Mayo Clinic researchers lingered for one and two presentations to hear a colleague's presentation but only Adam and the group from Lundbeck stayed throughout the day. At the end of the day, Adam turned towards the visitors and said "Okay, this is what we got, what do you want?" The question took the group of Lundbeck research managers by total surprise. A sneaking suspicion that had gradually developed during the day was now completely clear: this meeting was a complete misunderstanding. But what was the misunderstanding about?

One Lundbeck research manager, Trine, suggested that the meeting was a failure because of a mutual misunderstanding of the roles and expertise each of the potential collaborators brought to the table. "We were just bombarded with data", she said. According to her interpretation, the Mayo Clinic researchers had constructed the Lundbeck group as *industrialists* that knew exactly what they were looking for and therefore would be able to give Adam a clear response to his blunt question "what do you want?" However, as she had anticipated something quite different, she was now utterly confused.

> We came to them because we were not experts in this. We did not know what we wanted within tau. They were the ones that were supposed to give us that input. If it was their money and bid and they had to think about how one should approach a disease like Alzheimer's and had decided to focus on tau, what would they do?

Trine thus argued that the failure was due a misinterpretation of roles, not necessarily the Mayo Clinic scientists' *inability*, or *unwillingness*, to advice the Lundbeck research managers on how to explore LRRK and tau with the interest of developing a drug. Yet another research manager, Jesper, suggested that the failure was in fact due to the *inexperience* of the Mayo Clinic with industrial research. The Mayo Clinic researchers, Jesper suggested, did not have the background for presenting their research in a manner relevant to the pharmaceutical industry.

> If you are an academic and have spent all your time in an academic world, then you don't have [pause], you live a world where you maybe have a quite untrained perception of how [pause], of what kind of research that takes place in the pharmaceutical industry.

There was a particularly interesting twist to Jesper's account of the misunderstanding. From this argument, it certainly seemed that he believed in a separation between academic and industrial research, suggesting, as it did, that the Mayo Clinic researchers, being academics, were not able to relate to commercial research. However, when I inquired further into Jesper's notion of an "untrained" perception of Lundbeck, it became obvious that he was in fact mainly offended by the separation that the Mayo Clinic researchers had made between academic research and industrial research, thus failing to acknowledge the creativity and openness of commercial, pharmaceutical research.

> I mean untrained because they generally understood what takes place in the pharmaceutical industry as much more structured [pause] and so they might have imagined that we would be thinking only inside the box and have a much more narrow mind-set that we really had.

Using the categorizing screen, I now explore what constituted a screen in this situation, and what the screen *produced* in terms of particular categorizations of science and industry. As illustrated above, the group of research managers from Lundbeck had all observed a particular developing understanding of them that

they did not agree with. One research manager argued that the Mayo Clinic scientists had misunderstood what Lundbeck wanted and suggested that they could just pick and choose. Another research manager suggested that the Mayo Clinic scientists were not capable of understanding industrial interests and therefore only gave an academic presentation of the science. However, although they gave different explanations to what exactly was misunderstood in the situation they all saw that a distinction was made. I suggest that the categorizing screen can be used to explore what this distinction was about, how it was produced in the situation and what implications it had.

Emergent ideas of collaboration

In spite of their different accounts, the Lundbeck research managers seemed to agree that the meeting was a failure insofar as the Mayo Clinic researchers had premised it on a misconstrued notion of the potential collaboration and of Lundbeck's role in it. In which sense was the implied form of collaboration wrong? As Jens described it, Lundbeck research managers were looking for a kind of "equal collaboration", while the Mayo Clinic researchers were unexpectedly acting as if they were entering into what in Lundbeck was termed traditional "cash and carry" collaboration. This notion of "cash and carry" was used to illustrate collaborations with a clear distribution of roles: a company provided a question and "cash" and a research institution provided an answer that the company could eventually "carry" home. By "equal collaboration", Jens meant a form of collaboration in which all parties gave *scientific* input, even as Lundbeck would still also be paying for the collaboration. In his mind, this differed from "cash and carry". Although the intention of Lundbeck research managers was to establish an "equal collaboration" the arrangement of the first meeting with the Mayo Clinic researchers worked as a categorizing screen that made a sharp

distinction between companies' and research institutions' different engagements. The arrangement of the meeting can be seen as a screen that produced a different idea about collaboration, more similar to "cash and carry".

Even though the concept of an "equal collaboration" was also rather new to Lundbeck, the research managers had clearly not expected to be misunderstood on this point. After all, there had been a phase of preparation for the meeting. At first, Adam had in fact suggested the model of "cash and carry" collaboration to Jens, but he had received the response that this was not what Lundbeck wanted. Instead, Lundbeck wanted a framework, which would allow for change within a number of topics over a period of years. As Jens argued, putting all eggs in one basket by betting on just one biological mechanism would be too risky. His strategy was rather to broaden the collaboration and initiate a number of potentially valuable investigations in which Lundbeck researchers engaged deeply in external collaboration. I suggest that we can see these ideas of an equal and flexible collaboration framework that had developed in Lundbeck as a screen that categorized researchers as more or less the same whether they came from the Mayo Clinic or from Lundbeck.

Jens presented his idea of a flexible framework as quite *challenging* since a contract would often bind the parties for a limited number of years, making the research inadaptable to beneficial strategic changes. However, he insisted that, in advance of the meeting, he had thoroughly discussed his ideas for such a framework with Adam and, so he thought, convinced him that this was an optimal way of working together. Accordingly, the Lundbeck research managers shared the general expectation that they had flown to the US to explore ideas for such a framework agreement. This is why the scenery of the meeting in which current research projects at the neuroscience department was pitched to them conflicted with their anticipations and produced disconcertment.

In fact, it was surprising to Jens that the Mayo Clinic researchers would themselves even be interested in a contract research arrangement, given that they now had the opportunity to try something different. Why was Lundbeck's open-handed invitation to a joint scientific collaboration not greeted with excitement? This seemed completely strange, since such collaboration would appear to fit so well with the academic work style that Lundbeck research managers had observed at the Mayo Clinic. Thus the notion of collaboration itself seemed to hold quite different meanings for the participants and quite different ideas about science-industry interaction. I propose that we might understand these ideas about science-industry collaboration in terms of screens that categorize academic and corporate researches as different or similar. In the meeting, different ideas about collaboration produced different ideas about roles and interests. The appearance of these different categorizing screens produced disconcertment among the participants.

Thus, I see diverse assumptions about science-industry collaboration as screens that categorize the participants in different ways. Assumptions suggest that this screen is mainly constituted by something discursive. But, as we see in the accounts of the meeting, assumptions are also manifested in more material ways. There is a particular arrangement of the participants that suggests a separation. The Lundbeck participants are sat in front of a screen in a position suitable for watching scientific presentations. In comparison, an arrangement of the participants at conference table would have suggested that they took part in an equal round table discussion.

The question "this is what we got, what do you want?" that was repeated in interviews by several of meeting participants also had the effect of producing a separation of the participants and might consequently also be understood in terms of producing a categorizing screen. The question indicates that Mayo Clinic

offers research that can be purchased or invested in. The question also suggests that this research is not going to change as a consequence of purchase. This emerging idea of collaboration is offending to the Lundbeck research managers in that it presumes that they are not researchers capable of creatively engaging with the research but have only a financial or business interest. What we see in this example is that assumptions about collaboration, material arrangements of meetings and talk all add up to a screen that *does* something that eventually creates a strong reaction among the participants.

I continue to explore Lundbeck research managers' self-understanding as researchers more specifically in relation to the other major explanation offered by them for why this first meeting failed.

A science/industry screen

How did the Lundbeck research managers feel that their role had been misunderstood? First of all, the Lundbeck group was clearly seen as industrialists rather than researchers. The Lundbeck research managers were literally placed in front of a screen where the Mayo Clinic researchers gave presentations. However, they were not invited to join a discussion of these ideas. Further, with the exception of Adam, only the Lundbeck group stayed in the room, which made dialogue about and between the presentations impossible. At the end, the question posed by Adam, simply handed over the initiative to the Lundbeck group. In conjunction, a set of discursive and material arrangements generated a screen that differentiated between Mayo Clinic *scientists* and Lundbeck *industrialists*.

There were important nuances in the research managers' description of a science-industry collaboration. According to Hans, because Lundbeck was defined as

industrialists, the research managers were also ascribed the limited role of being potential "providers of money":

> Their perception of us was a checkbook. We provided money without demanding something particular in return. We were presumed to just say 'this is a good research group so let's see what comes out of supporting it'.

Although disappointing, this idea was not unfamiliar, as Jesper suggested, but something that he had encountered before:

> We have to move beyond the notion that industry is the 'cash cow' in these projects. Apparently there is a communication task in this because I often meet this idea that we are just supposed to give them money and then they can just proceed with what they are already doing.

According to Trine, Lundbeck was neither research experts nor investors and thus she felt that the screening of her as investor-expert was highly unsettling. As she argued, her role was neither to simply fund free research at the Mayo Clinic, which she argued that the department managers' statement "this is what we got" had suggested. Nor was she an investor capable of "picking a winner" among the suggested research areas. Considering the kind of collaborative relation she would be interested in, she said that she hoped the two groups would enter a dialogue in order to explore what might be an interesting outcome of the research. This might not imply agreement but it would be predicated on a certain level of awareness about what the other part wanted:

> But in my mind they were not necessarily supposed to do what we said. They needed to know what we wanted and have an incentive for giving us that.

Hence, a screen that several material and discursive elements in the meeting had generated caused her disconcertment. This screen had given her a particular role that she did not agree with. The Lundbeck researchers felt mis-categorized as industrialists that would provide for the Mayo researchers but in no way creatively engage in the research. However, there was something inescapable

about the screen; it was not just a matter of expressing disagreement. But how then did they see themselves? This becomes clearer as we examine how they began to actively change the screens that shaped the encounter.

Changing the screen

During the meeting, the research managers felt unsettled by the fact that they were restricted to a narrowly defined role as industrialists. Hans especially reacted against this. At the end of each presentation, Adam turned to the Lundbeck delegation to ask if they had any "clarifying" questions. According to Hans, the event was planned as a "scientific meeting", however, the presentations were quite generic and somewhat popularized. Hans interpreted this as a polite gesture to the visitors. However, it was insulting since it assumed that the visitors were unable to *understand* specific scientific content. During the meeting he became increasingly annoyed by being talked down to and he started to engage in a scientific discussion. As he explained, he deliberately asked sophisticated scientific question to reveal his knowledge. Nonchalantly, he referred to state-of-the-art publications and mentioned that he used to be an associate professor at a university. Primarily, he offered suggestions to show that he related to the research as a researcher. To begin with, the presenters ignored Hans' serious involvement as a scientist but eventually something happened.

> It takes a while before they respond really scientifically to your questions […] it was becoming really exciting and we became scientifically engaged in it and started to ask questions, and was first ignored. But then, all of a sudden, and it was actually Golde who interrupted and said 'but listen this is actually a really good suggestion'. […] If it really was important I'm not sure, but it was a mindset, a different way of interpreting the data.

According to Hans, the screen that was initially created by specific circumstances in the meeting (for instance, ideas about collaboration, the presentations and

gesture to choose among them) gradually dissolved and eventually changed. The Lundbeck research managers acted in a particular way by asking questions and initiating debate that was observed by the Mayo Clinic researchers. It created a moment of clarification of what the Lundbeck research managers were expecting, both in terms of how they saw collaboration and how they saw themselves as researchers.

> During the meeting I think they actually get the impression that we think it's genuinely interesting what they do because they see that we are responding to it like other researchers do and not just making notes. […] Because of this the atmosphere became good.

Thus there was a change of screen taking place from a science/industry screen to a science/science screen. The science/science screen that developed during the discussion of scientific content was not merging the participants into one type but rather contained the idea of different but equally scientific or academic engagements that connected.

Changing the Mayo Clinic researchers' perceptions of the Lundbeck research managers was important, according the Hans, not because the Lundbeck researchers were vain but because it was important for making collaboration possible. Rather than simply chose among presented research, Hans asked the Mayo Clinic researchers to make a "research proposal." The purpose of this was to present how research at Mayo Clinic might be relevant for drug discovery. He argued that the situation "was not different from if they were writing up an application for a research grant". Writing an application entailed "figuring out how to find a target within an area". According to the Lundbeck managers, making such a proposal for new research rather than pitching already existing research was an important first step towards the collaboration. Among the proposals eventually received, the Lundbeck research managers selected three projects that became the core of the collaboration.

Now, with the notion of screens we might see the making of a research proposal as part of the construction of a new categorizing screen that structured the collaboration. Where the first screen that was eventually dissolved categorized the participants as very differently engaged in the collaboration, this new screen brought them together in a different way. Rather than separate parties with separate interests and activities, we now observe a joint group of scientists that are working on "a joint research proposal".

However, from a slightly different angle, we might also see this collaboration as developing from some rather subtle differentiations of science. Not industrial science versus academic science but rather particular situated notions of science developing in the specific contexts of Lundbeck and the Mayo Clinic. These notions are not stable but exactly developing in the encounter of other ideas about what science is. The idea of a research proposal suggests that Mayo Clinic researchers are still offering research to Lundbeck research managers. In this sense, Mayo Clinic researchers are still seen as being the originators of research. However, the fact that Lundbeck research managers are then capable of looking at these proposals and selecting among them suggests that they are also scientists that in principle have no problems relating to the research that takes place at the Mayo Clinic. This is to say, the science/industry screen is not replaced by a screen that makes Lundbeck and Mayo Clinic the same but rather with a more dynamic screen that categorize more precise ideas about what describes the participants as researchers.

To begin, the collaboration developed from two main screens both differentiated academic and industrial researchers and made them alike. I now move on to discuss a number of later key events that shaped the development of the collaboration from the initial meeting. As mentioned, I first became interested in the recurrent story about a misunderstanding, which nevertheless did not seem

to have made collaboration impossible. Studying the further course of the collaboration, I discovered not only more kinds of categorizing screens related to science-industry relations but also screens that distinguished between scientists and managers.

Managing screens between the management and scientists

Shortly after the establishment of the research collaboration, some important changes occurred in the organization of the Mayo Clinic. All grants from the National Institute of Health were decreased by 10%. Consequently, the Mayo Clinic management decided to decrease basic research activities at the Jacksonville site and increase its efforts to attract external funds, primarily by collaborating with companies. In the transition from one strategy to another, many of the principal investigators at the Mayo Clinic was advised to look for other jobs, since they might not be able to continue their employment at Mayo Clinic. Thus, three of four principal investigators involved in the collaboration with Lundbeck soon left the Mayo Clinic for other positions. Dave found a position at the. As Department of Medical Genetics at the University of British Columbia, Canada, and Adam and Laura at the Center for Translational Medicine in Neurodegenerative Diseases, University of Florida the only principal investigator in the collaboration, Jerry stayed at the Mayo Clinic. From the perspective of the Lundbeck research managers, this meant that collaborating with Mayo Clinic now implied interacting not only with researchers at the Mayo Clinic in Jacksonville but also with researchers that were in the process of establishing themselves in new academic environments. In spite of this important change the collaboration was not cancelled.

When the Lundbeck research managers accounted for the Mayo collaboration they often laughed at the slightly absurd situation of collaborating with the Mayo Clinic by interacting with researchers at universities in Vancouver and Florida. Even so, the situation did not seem overly frustrating to them: the research activities generally continued as if nothing had happened. This illustrates the important point that moving between the neuroscience research environments at the Mayo Clinic, universities in British Columbia and Florida and at Lundbeck was not particularly difficult. The environments were, in some sense, very much alike, and people working in these places had a very similar academic training. So even though these environments were initially viewed as academic, industrial or semi-private, the collaboration did not rely on these categories. After the frustrations of the first meeting, the Lundbeck research managers mentioned no important difficulties in working together with former Mayo Clinic scientists, even as they moved into new academic environments. Cultural institutional differences seemed to be no issue. They did, however, point to another problem that had emerged as consequence of the changes at the Mayo Clinic.

The moving of the Mayo Clinic scientists separated them from the Mayo Clinic management and administration. The physical moving of these researchers created a new screen that categorized Mayo Clinic managers and scientists as very distinct. This screen that was actually in part constituted by an actual physical separation had a number of implications for the collaboration. First, the relation between the former Mayo Clinic scientists and the Mayo Clinic as an institution became ambiguous. Some of them had left in frustration over strategic and organizational changes at the Mayo Clinic. Nonetheless, to Lundbeck, and in the light of the collaboration, they were still Mayo Clinic scientists. When I first contacted the main Mayo Clinic researchers in the collaboration, Laura, Jerry, Dave and Adam, I noticed that they were very cautious about talking to me, which

in part had to do with the fact that it was complicated to sort out their relations to the Mayo Clinic. Getting access to these former Mayo Clinic researchers was not only, as suggested earlier, related to my relation to Lundbeck but also with their relation to the Mayo Clinic. Although the Mayo Clinic, in this collaboration, still employed these researchers they were also external to the Mayo Clinic and had new jobs. The three former Mayo Clinic scientists had very different ways of handling this. As mentioned, Adam never responded to my email request, whereas Laura said that she would be happy to participate but only if the Mayo Clinic allowed her to do so. Dave was very keen to participate in an interview but also extremely critical of the Mayo Clinic, which made it difficult to get him to talk about the collaboration. In addition, he was interested in building a strong relationship between his new research environment and Lundbeck but completely dissociated himself from the Mayo Clinic.

These complex relations between former Mayo Clinic scientists and Mayo Clinic staff resulted in a number of absurd situations, which the Lundbeck research managers described as distinctly frustrating and problematic. The administration at the Mayo Clinic was responsible for evaluating the exchange between Lundbeck and the Mayo Clinic, which included analyzing whether Lundbeck had received the agreed deliverables and whether the Mayo Clinic had received the agreed funds. However, this was not an easy task because the research activities and collaboration was taking place outside the Mayo Clinic between Lundbeck and former Mayo Clinic scientists. As one Lundbeck research manager suggested, things worked fine as long as no one was being overly strict about who did what from which location. However, towards the end of the collaboration when the Mayo Clinic administration started to evaluate the collaboration according to the contract who was doing what and was related to whom became very important questions to clarify.

Using the notion of screens, we can make sense of these events in the following way. According to the contract, this was a collaboration between Lundbeck and the Mayo Clinic about particular research projects. However, in practice, as a consequence of various events such as the moving of the Mayo Clinic scientists it became something else: a collaboration between researchers at Lundbeck and researchers at the Department of Medical Genetics at the University of British Columbia and at the Center for Translational Medicine in Neurodegenerative Diseases, University of Florida. In practice, among the researchers from Lundbeck and these universities this was not so important. They were still part of the same networks and worked on the same problems so therefore it was less important which institutions they were affiliated with. However, towards the end it became important to make the situation more comprehensible in order to make sure that according to the contract the exchange was completed. Sorting out these relations created a screen that categorized Mayo Clinic staff and managers as different from former Mayo Clinic researchers. In fact, these ending activities also made former Mayo Clinic researchers more the same as Lundbeck research managers because they all approached the collaboration in what they described as a more pragmatic way compared to the Mayo Clinic administration. According to this pragmatic approach it was less important whether all deliverables were achieved. It was more important that something interesting had come out and this was in fact the general perception, which I return to.

In the final phase, the Mayo Clinic decided to hold back payments from Lundbeck to the former and now external Mayo Clinic researchers because it was not clear to what extent these researchers had accomplished the agreed tasks. This was seen as a tremendous barrier to the collaboration and as an unfortunate example of how research and business administration had become separate practices in the course of the collaboration.

Here we have a screen that is highly important for understanding science-industry collaboration although it does not address relations between academics and industrialists but instead relations between researchers and administrators. That certain things in the collaboration (such as the moving of the Mayo Clinic scientists) produced this categorizing screen between scientists and business administrators says something significant about how dissolved the initial screen between Mayo Clinic scientists and Lundbeck investors had become. The collaborative actions had shifted from producing a science/industry screen to producing a science/management screens. Here I have used the notion of screens to explore other types of categorizations that emerged in the collaboration that were important although they did not concern the science/industry nexus.

"Knowing enough"

Before I discuss how the collaboration was seen from the point of view of scientists and managers at the Mayo Clinic, I now turn to explore the role of making difference in collaboration. Until now, I have described how Lundbeck research managers attempted to dissolve what we might see as a screen that made them industrialists and not scientists. I have shown how Lundbeck research managers *became* scientists in the process of interacting with Mayo Clinic researchers in a particular way. They did various things to make themselves slightly more similar to the researchers from the Mayo Clinic. However, although it was important for Lundbeck research managers to become scientists to make the collaboration possible, I also noticed situations in which it was highly important to explicate differences among the researchers, which I now explore. I argue that divergence and the explication of difference, as much as convergence, move collaboration forward. However, there are also situations in which differences are of the wrong kind, or so serious, that they inhibit collaboration.

When I interviewed Trine about how the Mayo collaboration ended, she told me a story about a discussion she had had with the former Mayo Clinic researcher, Dave. These people got along well, and throughout the collaboration they had had in-depth conversations about the process. The collaboration was now coming to an end simply because the contract was running out. However, Dave suggested that they should continue to work together to explore the LRRK mechanism. In response, Trine explained to me, she argued that, *as an industrialist*, there simply comes a time where one "knows enough". This strong emphasis on being an industrial researcher was slightly surprising because Trine had earlier explained how being equal researchers was highly important. As she saw it, this point had been reached, and thus there was no further reason to collaborate. I found the idea of "knowing enough" intriguing. When I inquired further into it Trine offered the following narrative.

> And then I tell him [Dave] that is very exciting but if you see it from our point of view we already have decided to make a LRRK inhibitor. A kinase inhibitor so now we also need a cell based assay that can show that we are really dealing with a LRRK inhibitor and an in vivo model that shows that we can interfere with the LRRK enzyme, the kinase activity. So we do not need more research on this, that's how we are. We don't need to know everything about LRRK to do this and now we have made our decision and now we are doing it.

What kind of screens does Trine's argument produce? We are dealing here with a screen that is constituted by talk but that nonetheless is also deeply rooted in practices and material organizational culture. Trine describes a situation where Dave suggests that more research could be continued in a particular way but that this is not interesting to her because she already "knows enough" to accomplish what she wants to do next based on the collaboration. Where a former screen categorized Trine and Dave as researchers equally interested in these biological mechanisms then the expression of "knowing enough" suggests that they are no longer equally interested. Rather there is an asymmetry. Dave wants to know

more and Trine knows enough. I now suggest that "knowing enough" produces a screen that differentiates industrialists and academic researchers. Here Trine suggests that industrialists and academics have different accountabilities.

First, notice the structure of Trine's argument. It relies on the assertion that "this is how we are" and "this is what we do", and because of that there is no point in continuing collaborative research. What she is describing is a specific aspect of the identity of the industrial researcher. For the industrialist, there comes a time where further research simply ceases to be worthwhile. Making this argument, Trine brings into play the organizational context of Lundbeck. From the internal point of Lundbeck, external collaboration serves the purpose of collecting enough data to make a strong case for a biology. When this data is available, completely different machinery starts up in which the biology is tested in cell based assays and in vivo models *in Lundbeck*. The research interest changes as the biology moves in-house. In external collaboration, the potential value of the biology is explored in an open-ended process, but once inside Lundbeck, it is evaluated for relevance and importance based on much more specific and severe criteria.

Trine talks about the industrialists' as having a particular relation to knowledge: it is a matter of "knowing enough". However, the issue of knowledge is itself intimately related to organizational process, and especially to the different phases of the collaboration. The beginning is characterized by a screening process geared towards opening up perspectives and be receptive to new inputs, whereas towards the end the issues is how to narrow the exploratory process to ensure that the production of industrially relevant knowledge can be made at Lundbeck. Thus, we encounter efforts to create convergence in the early phases of the collaboration and we see the reassertion of difference towards the end. Another way of saying this is that there is no clear-cut and general distinction between academic and industrial research in the context of this collaboration. At different

times, industrial research is characterized by both openness and narrowness. Different phases correspond with changes in the attitude to knowledge taken by Lundbeck research managers, and this illustrates the often-overlooked fact that strategic research is a dynamic process that involves both decision-making and exploration.

Trine suggests that the emphasis on "knowing enough" is distinctly industrial. As she argues, "this is how we are". Even so, we might inquire further into who the "we" concerns in this context. Speaking as an industrial researcher, Trine emphasizes her difference from her academic collaborator who always wants to know more. However, Trine is also a research manager, the department manager of neurobiology at Lundbeck. Yet, managing research, no matter if it is at a university or in a company, requires making decisions on what research to prioritize and develop, resource and promote. In fact, we might easily imagine a situation where a research manager at a university argues for discontinuing a project because it does not fit with strategic priorities and interests. In this sense, too, the distinction between academic and industrial settings blurs.

Mayo Clinic screens: Science/industry

As mentioned, I became increasingly interested to know how the Mayo Clinic collaborators had interpreted the first encounter with Lundbeck. At the Mayo Clinic, I interviewed Nancy, a licensing manager; Margaret, a technology development liaison officer; Jerry, one of the principal scientists, and Dave, a former Mayo Clinic scientist. All had been involved in the Lundbeck collaboration. With the exception of Nancy, they had all participated in the first meeting with Lundbeck, and they all agreed it had been surprising and more or less a failure. The accounts of Nancy and Margaret were particularly interesting because, in

their role as administrators, they were partly external to the collaboration. As Nancy saw it, the collaboration with Lundbeck "looked like a very good deal" since it implied "quite a generous upfront payment" and "the potential for a number of milestones that would be related to intellectual property". However, in her account, establishing a collaboration was proved challenging, especially because the aim, scope and content of the research project was "a bit of a moving target" for both parties. Neither really knew "what they were looking for". The target continued to move even after the collaboration was established as "expectations changed in the process". Where the group of research managers from Lundbeck saw the open-ended framework structure as strategically valuable, Nancy viewed it as slightly unsettling, as indicative of not knowing what to look for.

Margaret agreed that Lundbeck's approach was peculiar. Retelling the events of the first meeting, she emphasized that they had not expected the meeting to fail. In fact, the Mayo Clinic had had meetings like this with other companies and research groups with little trouble. According to Margaret it came as a great surprise that the scientific presentations did not generate much excitement. She described her moment of disconcertment moment in this way:

> The last thing he [Adam] said in his summary was 'and now we'd like to know what you think?' And it was like 'oh my God!' and I was like 'no this is not good, this is not good at all'. But when one of the men from Lundbeck spoke it was clear to me, I don't remember exactly what he said, but it became crystal clear to me that they didn't understand the science.

According to the Lundbeck research managers, the meeting failed because the Mayo Clinic researchers did not connect their current research to drug discovery. The threat of breakdown was prevented, as the managers from Lundbeck forced themselves into the scientific discussion, thus redefining themselves as scientists debating with other scientists about a research proposal. However, according to Margaret, the problem was that the Lundbeck managers could not understand the

science. In her account, the Lundbeck visitors *started out* as scientists who gradually turned into industrialists who did not have the same scientific training and interests as Mayo Clinic investigators. She used the disconcertment experienced at the meeting to reflect on the differences between scientists and companies:

> I see that the scientists think that they are talking to a scientist, but they are not. They are talking to a company, the company's goal is to make money not to develop a whole knowledge base, and they are not as interested in the knowledge as in the end result.

From the perspective of Lundbeck participants, a joint research proposal solved (their version of) this problem. Using the terminology of a research proposal made it possible to transform the Mayo Clinic presentations of on-going research into a collaborative project. This new research project defined a small number of focus areas and explored the potential of specific biological mechanisms for drug discovery. The idea of a research proposal that was familiar to all scientists made the Mayo Clinic investigators start to think in terms of specific goals, timelines and milestones. However, Margaret had quite a different story. At the end of the first day of Lundbeck's visit there was a joint dinner. Although Margaret described the dinner as a casual get-together, the atmosphere was rather gloomy. The disconcertment from the meeting had not disappeared and the participants were still struggling to interpret what had taken place. Feeling that something had to be done, Margaret began talking to the visitor sitting next to her: "you know, I don't think our people have a clue as to what your people want from us", she said. This led to a conversation about "the basic structure of the relationship", which took its point of departure in the kind of collaborative arrangements that Lundbeck had had with other academic institutions. Margaret concluded that Lundbeck was "looking for a very broad type of goal yet with a specific milestone". "Then I saw the light go on at the people from Lundbeck", Margaret said. Asked how she would

describe the change in mutual understanding between the morning and the evening, she explained:

> Well, the difference was that it was a much broader. In the morning, it was like a dissertation with full details and scientific, but in the evening it was more general. It was generalized 'okay, so we have this target and we'd like to spend some more time identifying it and it's going to be a major marker for Parkinson's disease and we would like to develop some mouse models for it, what we could do if we worked with you, we could develop cell based assay to do drug screening' because that's what Lundbeck was interested in.

Clearly, the forms of screening applied by the collaborators to one another are different. Indeed, we are witness to a radical reversal of accounts. Lundbeck managers viewed themselves as mistakenly identified as investors looking for a contract research arrangement. In contrast, they saw themselves as scientists entering a collaboration on an equal footing with Mayo Clinic scientists. They complained that the presentations were too popular whereas they wanted to engage real science. According to people from the Mayo Clinic, however, the problem was that they initially mistook the Lundbeck collaborators for scientists capable of understanding the details of research. However, during the meeting, this perception changed and instead Mayo Clinic participants came to see the Lundbeck representatives as people from a company, primarily interested in making money. Hence, they gradually popularized their discussions to make them understandable by their visitors.

The extent to which *both* of these opposed versions rely on standard categorizations of science and companies is noticeable, not least because the situation was also clearly hybrid. Yet, in spite of the incommensurable interpretations of events and even of mutual identities, the involved parties were clearly *also* able to communicate and connect. Gradually, the mutually exclusive interpretive screens that occluded the potentials of collaboration were replaced with others that produced sufficient similarities for the groups to fruitfully

collaborate. As noted, difference and divergence was reasserted at the very end. I now take a final look at the kinds of screens that emerged during the collaboration, from the point of view of Mayo Clinic employees.

Mayo Clinic screens: Science/business

Although there was a strong focus on science as the key factor that either connected or differentiated Mayo Clinic from Lundbeck, both parties also had a focus on business and intellectual property rights. It took more than a year to negotiate the contract. In Lundbeck it was often said that the aggressive IP and licensing strategy of the Mayo Clinic caused this delay. Indeed, although the Mayo Clinic appeared as the academic partner of the collaboration, the research managers in Lundbeck often joked that the Mayo Clinic was much more business oriented and eager to settle things in advance than Lundbeck. However, as the collaboration developed, the categorization that separated the Mayo Clinic and Lundbeck along the axis of science/business, transformed into a screen that separated science and business *within* the Mayo Clinic.

According to Nancy, it was profoundly disconcerting to realize that the scientists working with Lundbeck were leaving the Mayo Clinic, in part because she became aware of this very late in the process.

> I must admit I have never seen this happen before. The scientists that were working with Lundbeck left Mayo within a three-month period, [...] it was really appalling. I mean quite honestly from our end, we didn't, my office didn't find out about it until really close to when they left. And I called, I think it was Will at Lundbeck as soon as I knew, and I think they actually knew before I did.

The Mayo Clinic researchers were leaving the Mayo Clinic due to changes in the research strategies that meant that basic research would not be as highly prioritized as in used to be. Since these scientists saw themselves as scientists

involved in basic research they all sought other job opportunities. Both Lundbeck research managers and the former Mayo Clinic researchers gave this explanation.

At the time of the scientists' leaving from Mayo Clinic, there was a close interaction between these scientists and Lundbeck researchers. When the scientists left, the contract became immediately relevant, since it was necessary to re-evaluate the obligations and funding agreement. According to Nancy, it was a scramble to figure out what had taken place between Lundbeck and the researchers at the Mayo Clinic, as she realized that her office had effectively been sidetracked from the collaboration, making it very hard to get an overview. Based on this experience, she reflected on the gradual separation between business and scientists at the Mayo Clinic. During this phase, she saw no collaborative problems between Mayo Clinic scientists and Lundbeck scientists who seemed to be "talking back and forth" and shared the news about the moving researchers as if they were colleagues. However, she felt that her office had been separated from both the collaborative scientific activities and business discussions:

> What we lacked very much on the Mayo end was a coordination between the business people, you know, myself and Bernadette, and our Mayo scientists and also a lack of communication with Lundbeck's business people. So the scientists were talking back and forth, one assumes that they were happy with how things were progressing, and then, you know, things are starting to go a little wrong and they called me up saying 'Lundbeck is asking for more than we originally had agreed upon and we can't do it', we begin to hear about the problem after it has occurred rather than being in the discussion so that we could have helped before there was a problem.

What started as a distinction *between* Lundbeck and the Mayo Clinic ended as a distinction *within* the Mayo Clinic. What do we learn from this observation? It draws attention to constructions that are not directly related to distinguishing scientists from industrialists but nonetheless have importance for science-industry collaboration. It also suggests that we should not only look for barriers

and gaps *between* collaborating institutions but also think more openly about what might prevent and advance collaboration.

Conclusion

Whereas the previous chapter analyzed the management of emergent relations, this chapter has illustrated how external collaboration in Lundbeck involves experimenting with new frameworks for such emergent relations. As we have seen, developing such frameworks is challenging. Among other things, it involves a series of subtle and dramatic shifts in constructions of difference and similarity, convergence and divergence.

It is interesting to notice how far this case is from illustrating asymmetries in the sense of industrial dominance (as envisioned by, for instance, Vallas and Kleinman 2006). Indeed, we are rather witnessing to the *co-production* of science and industry. Co-production is not a result of collaboration between two institutions, representing science and industry, but the result of being deeply involved and implicated in the same thing, in diverse ways. Thus sometimes Lundbeck research managers will argue that the value and interest of the collaboration, as far as Lundbeck is concerned, is in the open format and the opportunity for making a broad search. At other times, we hear them say that, of course, they are searching for results that can be developed into a drug. Indeed, in the case of the Mayo collaboration, the Mayo Clinic seemed to expect more industrial determinacy than they actually encountered. The initial disconcertment produced by this surprise opened up for a process in which the object of study could be mutually defined. While I would not argue that industrial dominance nowhere takes place, this leads me to suggest that strategic research, at least as

developed in Lundbeck, does not in a general sense conflict with academic research agendas.[54]

External collaboration with academic groups clearly emerged as a new form of strategic research in Lundbeck and to a remarkable extent this happened detached from the rest of the Lundbeck organization. To get a better sense of the Lundbeck organization, I now turn to an analysis of how external collaboration related to this broader organizational context and to other emerging forms of strategic research.

[54] This might be seen as similar to Perkmann et al.'s notion of "academic engagements" (Perkmann et al. 2013). However, Perkmann et al. suggest that collaboration with industry does not (at all) alter academic agendas. What I suggest here is co-production, which implies change but not dominance.

12. MAKING SCREENS FOR FUTURE RESEARCH

While I studied external collaboration, Synapse was launched and managers began discussing the implications of this strategy for external collaboration. As mentioned in Chapter 6, Synapse was the name for a new strategy that emphasized making connections (hence the idea of *synapses*) between areas that were imagined to be separate. This included connections between Lundbeck and an outside world, in part constituted by external research collaborators. In addition, it involved connections *within* Lundbeck between separate divisions of Lundbeck such as Research and Development. For example, Hans reflected on the implications of Synapse. He suggested that some existing external collaborations would not have been possible to initiate under Synapse since it introduced the idea of thinking things through all the way to the Clinic. Synapse, it seemed, initiated some new ways of legitimizing research that somehow made it more complicated to start research without knowing the ending.

The Synapse strategy process thus introduced the idea of creating more integration between separate parts in Lundbeck. Doing so, it drew on the idea of initiating research on a rational basis. Whereas external collaborations had previously been initiated based on the initiative of a small group of research managers, new research was now going to be initiated as a joint decision among a broader group of managers. It would also be based on a systematic investigation of opportunities.

This chapter is based on interviews and conversation with Lundbeck research managers who participated in developing strategies in the process of Synapse. It is also based on interviews and conversation with (senior) research managers who were members of an established Synapse management and made final

decisions on which strategies, reorganizations and approaches to develop and implement.

After shortly introducing Synapse, I explore a number of disconcerting topics that came up in interviews with research managers, which reflected on the first phase of this strategy process. Contrary to the previous chapters, the forms of disconcertment I engage in this chapter were related to *processes* that stretched over a longer time period rather than an isolated *event*. I am particularly intrigued by the fact that it seemed to be *more difficult* for research managers to collaborate with people from Lundbeck than with people from the outside. Indeed, it seemed to imply *more risk* for research managers to open up their decision processes to other perspectives than to let external collaborators influence priorities. I continue to use the notion of screens to explore how activities in relation to Synapse produced various specific categorizations of research and new projections of what strategic research entails.

Introducing Synapse

Synapse was introduced in 2009 and continued until 2011. The process involved an extensive reorganization in the Research division and it defined a new approach to initiating research. Synapse had several phases. Here I focus on two most important phases as far as the research managers were concerned. The first was an initial phase in which new research areas and approaches were identified. The second phase aimed at integrating Research and Development processes. Both were, at least to start with, based on the idea that creating more integration between preclinical and clinical research and between Research and Development would improve the chances of becoming more innovative. Including clinical perspectives in the prioritization of very early research projects would

potentially increase the future value of these projects. Clinical perspectives could, for instance, make sure that the early research projects addressed a clear unmet need and that it would be possible to make clinical tests of the aims. Similarly, the inclusion of people from development in the prioritization of research projects was meant to make it more likely that the transition from research to development would be smooth.

This first phase, which took place in 2009-2010, focused on developing a new strategy for research. Management wanted to determine whether Lundbeck operated in the right areas of research and whether there were research areas, diseases and market opportunities that had been previously overlooked. Before Synapse, Lundbeck operated in a limited number of disease areas, which covered Alzheimer's disease, Parkinson's disease, psychosis, depression and anxiety. The intent of Synapse was to investigate whether Lundbeck's future business could include other disease areas. Thus, the process was initiated based on a strategic question phrased thus by the CEO: "are we working within the right areas?"

Phase one of Synapse introduced the idea of what Jens in particular referred to as a "biology focus". This was not completely new to research managers in the research division, where the idea of focusing on biology instead of disease had circulated among research managers for some years. In fact, an interest in biological mechanisms was exactly what had guided the making of both the neurocell and the Mayo collaboration. However, in both cases, the research managers' interest in biological mechanisms such as tau, LRRK and neurocell was based on their potential relation to specific diseases such as Alzheimer and Parkinson's, and the search for links between biology and diseases had therefore not been completely open. Compared to this, Synapse, introduced a more radical biology focus. Rather than look at links between biology and a particular disease, the intent was to "take a step back" and open up for more broad questions such

as: what biological mechanisms do we find interesting and which range of diseases might they link to? From now on, new research projects thus had to be qualified by strong hypothesis about the biology involved in various diseases.

This first phase engaged researchers and research managers at several levels. However, it also immediately introduced a new division of research managers, as a group of senior research managers became part of a Synapse management. The Synapse management consisted of managers from Research, Development and Clinical Research. It had the role of making final decisions about new strategic focus areas based on material and presentations developed in Synapse working groups. The members of these working groups also counted some of the research managers that usually took part in decision-making. Where research managers from different levels of the Research division usually worked together they were now separated. Thus, from the beginning, the initial organization of work in Synapse created a screen that separated Lundbeck research managers who were usually working together, a screen that I return to.

The consultancy firm Boston Consulting Group (BCG) managed the Synapse process. Its role was to keep the process on track and to facilitate the dialogue between members of the Lundbeck organization that did not usually work closely together. BCG worked closely with the Synapse management, and the general management including the CEO, and it generated questions for the working groups. It also developed various tools that were to be tested in the groups.

Early in the Synapse process, the notion of *product concepts* was proposed as a means to develop new research proposals that included clinical and business perspectives. I now examine this notion and explore the disconcertment to which it gave rise. I suggest that we can understand product concepts as a categorizing screen that classified and related various perspectives on research in Lundbeck.

Product concepts

The first phase of Synapse focused on identifying new strategic research and took place in the existing Disease Teams. As mentioned, in Lundbeck researchers and research managers were organized in groups that were concerned with a particular disease, hence Disease Teams. The Synapse management and BCG had initiated this phase by simply asking the Disease Teams to list all possible indications within the central nervous system that they had come across in their work and which did not fall within the categories of Alzheimer's and Parkinson's. For example, this initial phase allowed the Disease Teams to explore dementia, not simply as an aspect of Alzheimer's disease but also as an indication of other diseases such as HIV and stroke. To some research managers, this first phase of Synapse was extremely satisfactory. It allowed time to go into details with what they described as "intriguing" aspects of diseases that there was not usually time to explore. This was, for instance, the case for Jesper.

> The interesting about this process was that it was some of the things that we have looked at in the Disease Team but then we ended up by saying 'let's look at Alzheimer's because we don't have the resources or time to explore the other indications in depth'. But all of a sudden we had the time.

It was during this phase that the notion of product concepts came up. Members of the Synapse management suggested that the notion of a product concept could be used as a tool for organizing the proposals that the Disease Teams came up with. The notion of product concepts was meant to illustrate that a future product should be present in the initial articulation of research proposals. The quality and relevance of the research should consequently be evaluated based on the potential product that it would result in. Product concepts were designed to take a number of perspectives into account at the same time. They were visualized

using the shape of a chemical compound, a coronene shape consisting of seven adjacent hexagons, one in the center and six surrounding hexagons.

Illustration 6: Lundbeck's Product concept model.

The point of this visualization was to emphasize that a product concept ideally consisted of a number of components that besides "Disease Biology Understanding" also covered "Clear unmet needs" and "Commercial Attractiveness". A product concept thus combined several points, making it a concept rather than a loose idea, and it depended on formulating an explicit idea of the relation between biology and market perspectives.

To make product concepts into practical tools not only for generating ideas and describing relations but also for getting an overview of indications, they were to be filled into an excel sheet, thus eventually creating a database of indications. For each idea, the Disease Team members had to fill out a form containing questions for each dimension. Thus, these excel sheets catalogued the different possible indications that Lundbeck might choose to focus on. To some research managers, making product concepts was seen as a new way of making research proposals based on what Hans described as coherent "stories" about a potential relation between biology and disease:

> The idea in product concepts was to try to build up a story that said now we have some combinations, some new discoveries in the biology and then we are trying to make a document where we are trying to get as far as possible within the time that we have.

However, some research managers also found product concepts problematic. They argued that product concepts put strong emphasis on mainly *symptoms*, which resulted in a vast material on diverse groups and subgroups of indications. Even if each indication specified a clinical perspective and a business case, the approach failed to observe fundamental connections and similarities between diseases at the molecular level. Early in the process, some research managers, like Jesper, thus felt that the new approach was strangely "old fashioned".

> Disease was defined in relation to something quite old fashioned, which is a pattern of symptoms. Such a pattern of symptoms does not have anything to do with what is wrong with you at a molecular level.

Gradually the Disease Team members came to think that there was something wrong with the product concept approach. As they started to upload files in the product concept database, it became clear that there were actually several problems. First, it was possible to create a new file by making only a minor and potentially insignificant change in the indication. Two indications that in practice would be *the same* might thus emerge in the database as completely *different* projects. Rather than group the material in new and useful ways, the product concept approach threatened to result in chaos. Indeed, within a very short time, the developing database was overloaded with thousands of proposals.

Second, there was a problem with the way the different perspectives, illustrated by the different compartments of a product concept, were taken into account in the process of finishing a product concept. In some cases, the Disease Team members completed a concept by filling out all dimensions, including clinical perspectives. But in many cases, research managers simply single-handedly made

a file and completed only the descriptions of indications and the biology link. Thus, many concepts were not in reality the result of a *discussion* between different perspectives but rather of listing them that took place independently. The result was, according to Hans, an enormous amount of data that was only halfway completed.

> That process completely sidetracked because promptly there were 500 product concepts that were only more and less filled out by the clinicians.

In consequence, some of the Disease Team members that worked with product concepts did not share the enthusiasm for this tool with the Synapse management. Members of the Synapse management emphasized that this was simply suggested as a tool for "experimentation", as suggested by Lars, the head of drug discovery. It might reveal something new or it might not. However, it was suddenly difficult for the Disease Team members to communicate the specific weaknesses of the concept to the Synapse management. Research managers explained this as an artifact of the Synapse organization that had separated research managers who usually worked together. Rather than explicating their criticism to the Synapse management, it seemed that team members made the weaknesses of the product concept model visible by purposely overloading the database. Some research managers, like Thomas, laughed when they explained how the system was completely overwhelmed:

> We totally got the better of the system because it was completely unable to handle [laughing] I think we made around 5-600 product concepts in each Disease Team!

Eventually, the Disease Team members presented the product concept catalogue to the Synapse management group. As usual, the presentations provided an occasion for discussing the results. During the presentations, it became clear to the Synapse management that the product concept model had not worked according to expectations. As one of the research managers who presented on

behalf of his Disease Team explained, to some members of the Synapse management the limitations of product concepts was a genuine surprise. For others, the presentation of a final catalogue was an opportunity to state what they had thought was obvious from the start. In this vein, Jens, who had been quite skeptical about the tool from the start had burst out during a presentation of a long list of indications: "but, really, there is nothing new in this!" I now explore in more detail what exactly was slightly wrong with the idea of product concepts, according to research managers in Lundbeck.

According to some of the Disease Team members, like Thomas, who had worked intensely with product concepts it seemed that although product concepts was a new term the approach of taking various perspectives into account was in fact not new. Rather, as he suggested, "biology was already taken into account as part of considering diverse symptoms". Consequently, some Disease Team members argued that product concepts failed because they were believed to provide something vitally new where they were, in fact, blind to how research was already a creative process of making connections and comparing perspectives.

Another research manager, Hans, argued that the main problem with product concepts was the idea of having produced a list of available projects that had a lasting relevance:

> I think there was an expectation that product concepts would make up a *menu* of projects, enough to feed the organization for the next 2-3-5 years but it wasn't a menu, it was just pure chaos!

In his interpretation, the hope was to have a final record of proposals that the management could return to and pick from, hence the metaphor of a "menu" with options to choose from.

Yet another manager, Andreas, argued that the problem was related to an idea of having "turned every stone". The image of "turning every stone," suggested that

research was a process of discovering what was already there and available for scrutiny, hence the image of uncovering something hidden. This image was wrong, according to Andreas, because it described research in Lundbeck as a matter of simply mapping or listing things. Rather he argued that research in Lundbeck required interacting and creation of opportunities in a continuous process. At the time of the presentation to the Synapse management, the product concept catalogue might have presented the most recent ideas about indications and opportunities for making drugs. But from that moment and onwards the catalogue would be gradually more and more outdated. The catalogue, he argued, was "dead" in terms of setting a future direction. This seemed to be a key worry among research managers.

Even though the product concept model was questioned as tool for generating future products,[55] it remains analytically interesting because they articulated different changing notions of strategic research in Lundbeck. From the perspective of product concepts, strategic research was simply that which emerged through *systematic mapping* rather than what emerged through *experimentation* in external research collaboration. Although the notion of product concept was originally introduced as a tool for opening up new areas it came to constitute a rather restricting screen for defining new strategic research areas. It was restricting in two senses. First, it operated with a fixed set of categories that the articulation of new research proposals should relate to. These categories were promoted as equally relevant and interrelated, although in practice some research proposals might be specifically interesting seen from one or two perspectives. Thus the product concept model rendered heterogeneous research proposals more or less *the same*, making it difficult to effectively

[55] Although the model was criticized in Lundbeck Research, it was subsequently modified and renamed the "Value Proposition" model or "the flower", and is today used in Lundbeck.

evaluate the potential in each proposal. Second, the product concepts produced a restricting screen because they effectively suggested that strategic research would be the outcome of a controlled mapping process, which *occluded* important dynamics about how research took place in Lundbeck. Rather, than seeing strategic research as emergent, product concepts thus assumed it to be a much more controlled process, which included systematic mapping, rational choice, and harmonious interaction between diverse aspects of the research.

As noted, the disconcertment to which product concepts gave rise had to do with the fact that this tool failed to take into account the dynamic and creative relation making processes connecting disease, biology and patients. In other words, rather than a tool that supported the making of such relations, product concepts functioned as a reductive categorizing screen that delimited research opportunities in advance.

At the same time, however, Synapse also facilitated the construction of another form of screening. This form of screening relied on a distinction between research management conceived of as itself a creative process based on intuitions and 'gut feelings' and the quite different notion that management has to do with making rational choice between well-defined options. I now look into the emergence and qualities of this screen, and to its consequences for the reorganization at Lundbeck.

Transparency and 'gut feeling'

> There wasn't any roadmap for how this process was going to be executed. One of the great ideas was that we were trying to invent the process as we were going along.

As Jesper suggests in this quote, Synapse was premised on the idea that the very *process* of making new strategies in Lundbeck could not be defined in advance.

Rather, the Synapse management and BCG intended to find out in the process what was the best way of developing new strategies. However, although the research managers who participated in working groups in Synapse were all familiar with open and uncertain processes (they all had a research background), some of them found the particular openness of Synapse rather unsettling. This was in contrast to the open and "risk-taking" approach that the very same research managers had developed in relation to the neurocell and Mayo collaboration. The unsettlement was in particular related to the role of the Synapse management. Several research managers said they were unsure why the Synapse management did not just set direction for future research rather than involve all researchers in the process of finding it. The process seemed contradictory, as if it was at once open-ended with regard to how strategies and approaches were identified and closed with regard to what the Synapse management's exact plans and intentions were. As Thomas, one research manager, argued, it was slightly "mysteries" what the management had in mind:

> Why do they [the management] not come and say 'you need to work on this and this'?

At the same time, some of the research managers clearly enjoyed the openness of the process since it gave them the opportunity to affect the overall strategy:

> It gave us a freedom to explore what kind of direction we wanted to go in.

But this direct involvement also created uncertainty. During the initial phase, the process of identifying new strategic targets had become extremely transparent. Proposals for future research were not just discussed at meetings in Disease Teams or presented for the Research Management Group that usually evaluated upcoming research ideas. They were out in the open in a database, described in detail in lists, and compared. In contrast, it was unclear how the Synapse

management used the material that the working groups produced to make specific decisions, and in this sense the process was also strangely closed.

Research managers expressed disconcertment related to the collision of two styles of management, represented by the Synapse management and the working groups respectively. Where the working groups were asked to consider research opportunities using diverse rational tools that made their decisions clear and transparent, it was in contrast completely unclear how the Synapse management used this material to make final decisions. To some research managers from the working groups, it therefore seemed that the Synapse management continued to make decisions based on 'gut feeling', whereas the working groups were forced to feed into these discussion by making material based on rational considerations, sometimes against what they found most sensible thing to do.

In discussions of research opportunities 'gut feeling' had indeed been an accepted basis for making decisions. Especially Jens was known for a style of management where decisions were not the outcome of long systematic investigations but rather based on a strong intuition of what was right to do in diverse situations. I now explore the disconcertment that I noticed as a result of a collision between these two forms of research management.

After the initial listing of product concepts, the Synapse management decided to change focus from indications to biology. A number of biological processes now came to work as a sorting mechanism and led to the establishment of new groups. Described with the notion of screens, a new categorizing screen emerged in the process that related strategic research to biological mechanisms. The members of these groups were a mixture of research managers and clinical researchers. Each group had a name such as "Neurodegeneration", indicating the particular biological mechanism that was explored. In addition, the Synapse management provided the groups with a number of sub-headings describing biological

processes such as “mitochondrial dysfunction”, and a list of specific targets. The Synapse management now asked the new groups to prioritize biological targets. Rather than producing what was now seen as “arbitrary lists”, the groups were asked to reach an *agreement* about their priorities. Thus whereas the product concept model had allow collaboration between preclinical and clinical researchers to be indirect and uncoordinated, this part of the process forced these researchers to work together in a direct sense. To assist this process, BCG provided the groups with a rating tool. Thus the groups were to rate each target on a scale from one to six based on a number of parameters. For instance, one parameter was “drugability”, that is, ability to find a compound that could be used as a drug. Another parameter was the likelihood of finding “patients and populations”, corresponding to the target.

The working groups produced detailed presentations of the prioritized targets, explaining why each target was selected and why they saw it as a promising opportunity. These presentations were given to the Synapse management. During presentations, the working group members discussed their finding with the Synapse management and the management asked the groups to go back and provide more detail on particular targets and ideas. Meanwhile, the Synapse management began making lists of priorities based on the input from groups. Group members saw glimpses of these lists as they were given requests for more information. As one research manager, Trine, argued, it was slightly ambiguous what the lists that the Synapse management produced at this time illustrated. She started to wonder if topics that were on a list were prioritized highly or rather on the way out.

> I did not know how they had reached those conclusions, that is, we laughed a lot about this, right, because it was like ‘well does it then mean that the six or seven things, are they on the top of their list or the things that are going out, and why do they need more information on this?’

How to characterize this sense of disconcertment? Here I consider it in relation to how activities related to Synapse produced a categorizing screen that differentiated groups of research managers that usually worked together across management levels. This screen categorized research managers into two groups, one providing information and the other making decisions. This screen not only differentiated between different groups of research managers, it also made some processes extremely transparent while *occluding* others. It made the work of producing lists visible while the work related to making decisions using these lists and using 'gut feeling' invisible, at least to the research managers from the working groups.

Thus, whereas a new form of rationality and transparency was imposed on some managers, the decisions of the Synapse management were shielded and occluded from view, with the result that these decisions seemed ambiguous and generated uncertainty. In some situations, like in relation to the aforementioned lists of targets, the contrast between these forms of management became very visible. In itself this list suggested a process in which decisions were made according to rational criteria. However, the ambiguity of the content of the list suggested that it had not in fact been created by rational criteria (c.f. Jensen 2011).

It is worth repeating that the disconcertment, about lists in particular, and the first phase of Synapse in general, was not produced by any general dislike for intuitive decisions. On the contrary, it related to the way in which Synapse claimed to rationalize research by *occluding* 'gut feeling', which was otherwise widely accepted, but only for the lower level of management. As Trine argued this meant that it became increasingly difficult, among the working group members, to use experience as a valid basis for making decisions:

> I found it very frustrating because I tried to get through with the idea that a lot of what we do is somehow based on a 'gut feeling' and the experience that you have achieved so why? The

> managers, why couldn't they just use 'gut feeling' to guide us through this process? They usually don't have a problem with telling us what they think?

After having focused on the relation between the Synapse management and the working groups, I now consider yet a form of disconcertment that some research managers in the Synapse working groups gave accounts of. This disconcertment was related to working closely with clinical researchers in Synapse working groups. Of particular interest is the recurrent observation that many research managers seemed to find collaborating with their Lundbeck colleagues from other divisions much more challenging and difficult than working with external academic researchers. On the one hand, they consistently referred to university groups as "external" and to activities in Lundbeck as "in-house". On the other hand, many research managers were much more concerned with their own role as researchers in in-house contexts compared to in relation to external researchers. What does this tell us about the potential hurdles of strategic research and science-industry collaboration?

A research/clinic screen

As mentioned, one of the initial aims of Synapse was to make stronger connections between preclinical research and clinical research. Research managers conceptualized this task in different ways. Some suggested that it was a process of aligning preclinical and clinical research. Some proposed that it was about making a good story that would hold all the way to the clinic. Yet others argued that the process was about "broadening to see if a biological concept would make up an investment area". Others emphasized that a new strategic area could emerge either from "a biology push" or from an identified "unmet need" for a drug, which in Lundbeck was termed "the clinical gold".

As Thomas described the work process, the group members were perfectly aware of what their joint task was about:

> There need to be a strong biological rationale that kind of argues that this is something we would like to do. But there also has to be a need from the perspective of the clinic that says, 'if we could do this then it is because there is an unmet need that legitimizes that we do this'. And the clinicians might also say 'we have a symptom, symptoms, or a patient population that is not treated at all and we think that there is something wrong. Can we find a biology that might address this?' That is clinical pull.

Nonetheless, in practice *agreeing* on what was interesting and valuable was not always easy. It was my impression that the research managers had not previously worked this closely with clinical researchers. They had not previously been asked to sit down and discuss proposals for future research. Of course they had discussed strategies before, as members of Disease Teams, but then their roles had been clearer because they were representatives of different ends of a value chain. Now, from the perspectives of research managers, the clinical researchers were moving in on their territory and interfering in their affairs. This was interesting to observe because, among the research managers, there was a pronounced awareness that some boundaries in Lundbeck, such as the ones between Research and Development and between preclinical and clinical research, were not convenient and practical. However, when it actually came to inviting clinical researchers into what they understood as their area of experience this was immediately experienced as troublesome and difficult. In fact, talking to the research managers about their experiences with working with clinical researchers elicited detailed descriptions of the differences between clinical perspectives and what they saw as "real research".

Here we might fruitfully compare the relationship between preclinical research and clinical research in Lundbeck with that between Lundbeck research and external academic research. As we saw in the previous chapter, Lundbeck

research managers not only differentiated themselves from external academics from the Mayo Clinic. They also sometimes drew strong parallels, or insisted on similarities, even identity. Thus specific collaborations generate emergent forms of screening. At best, these forms of screening facilitate a broad spectrum of research forms *because* they do not assume a watertight distinction between industrial and academic research. As different screens take priority, external academic researchers may indeed emerge as more business-oriented than industrial researchers. We saw this in Chapter 10 where academic researchers from a university in fact approached research managers from Lundbeck because they saw commercial prospects in their research. Hence I concluded that in these collaborations, research emerged through a dynamic of changing screens that generate emergent relations, rather than emerged as a result of interaction between clearly delimited and stable forms of research. In this light, it was surprising to see how collaboration between research managers and clinical researchers primarily emerged through screens that consistently differentiated them and fixed their respective roles. There were of course variations. But although the overall intent of Synapse was to integrate and align, I did not in practice encounter any forms of screening that supported the merging or reconfiguration of these roles. Let us now look closer at how the research managers described their collaboration with clinical researchers.

Rather than bringing value to early research in Lundbeck, the research managers generally suggested that working with clinical researchers restricted their ideas. As one research manager put the matter, the problem was that the clinical researchers held on to "traditional norms" and were unwilling to explore new ideas. However, rather than try to understand the reservations clinical researchers had to their ideas, some research managers accused the clinical

researchers of rejecting interesting proposals based on "personal opinion" or due to being "overly cautious":

> We are confronted with a form of conservatism where we, where it becomes very difficult to sort out what is fair and what is just conservatism, right. 'We have never done that so we can't do it'.

Thus, the research managers presented themselves as adventurously thinking out of the box and challenging decisions about what can be done. What I find interesting here is not the presentation of clinical research as restricted but rather the way research in Lundbeck emerges in heterogeneous relational ways. I did not interview clinical researchers in Lundbeck and have no basis for describing collaboration seen from their point of view, although this would have been valuable. Notice how Trine above describes her interaction with in-house collaborators. She specifically argues that her research ideas are restricted with reference to "what can't be done". In comparison, consider the following description made by Hans of the role of a Lundbeck research manager in relation to an external academic collaborator.

> We come back and say 'that just isn't possible' right where he then says 'why don't we just do this and develop in that or this disease?' and I say ' that just isn't possible' right 'we can't make those studies'.

In this quote, Hans is specifically reminding the external researcher, Martin, who is thinking out of the box, "that just isn't possible". These two descriptions describe an identical relationship. They both stage a dialogue between two researchers and they both describe how one researcher has generated an idea that the other researcher rejects. In both examples, the researcher who proposes an idea emerges as an academic whereas the researcher restricting this by saying 'no, it is not relevant' represents an industrial perspective where not everything is valuable to do. It is thus more than a bit intriguing that the academic in the first quote is a Lundbeck research manager confronted with an in-house clinical

researcher colleague, whereas the second quote concerns an external collaborator, Martin, faced with a Lundbeck research manager, Hans. I do not think that the two situations illustrate inconsistency or contradiction. Rather, they offer a vivid illustration of how science and industry roles and perspectives are defined by very particular contexts rather than by institutional location. Thus, what is categorized as academic and industrial emerges from the interaction and is, at the same time, organizing that interaction.

Before moving on to address the new boundaries, which emerged within Lundbeck as a consequence of the Synapse process, I pause to reflect on one interesting phenomenon that was brought to light in one of my interviews. Synapse seemed to suggest that in order to become more innovative the different divisions of Lundbeck would have to learn more about each other's perspectives. Consequently, research managers in Lundbeck had to learn more about development and about clinical research in order to take part in making the right long-term decisions about research. Yet, research managers did not see this as a matter of expanding their expertise or converging with other divisions. As Hans said:

> It doesn't mean that my people have to become experts in clinical studies. They just need some kind of idea that that is where we are heading. They should be experts in the basic research and that is what they are good at.

As this illustrates, although there was a strong idea of alignment in the process of Synapse there was also the notion that too much alignment would destroy a *valuable tension* between different forms of expertise and research practice in Lundbeck.

> You have to make sure that you have alignment in this process. However, you also have to make sure that you don't have too much alignment because then you will kill everything.

I now move from considering particular disconcertments related to Synapse to look into the implications of Synapse.

Thus, in the context of Synapse we can observe a form of science-industry collaboration that is different from collaboration between Lundbeck research managers and external researchers. In this science-industry collaboration, Lundbeck research managers become academics and clinical researchers become (from the perspective of the research managers) industrialists imposing certain criteria of relevance on the research. Although collaboration between Lundbeck research managers and external researchers, for instance, from the Mayo Clinic, is, in many ways, *different* from collaboration between Lundbeck research managers and Lundbeck clinicians, it seems to produce and draw on a *similar* screen that distinguish academic from industrial. There is a similarity in the way what emerges as academic is associated with lively, unrestricted thought processes whereas what is associated with industrial is the opposite activity of rejecting and restricting according to criteria of relevance. However, by comparing these two forms of science-industry collaboration we must also notice that research managers from Lundbeck emerge as both academic and industrial, depending on context.

In the context of Synapse, we also find ideas about science-industry *collaboration* that are worth comparing with ideas about collaboration emerging in external collaborations. First, we observe that Synapse is based on the idea that innovation develops from close interaction between different practices, some academic and some closer to market. This idea is recognizable from policy discussions, as described in Chapter 8. Thus we see in Synapse a process of convergence between assumed separate domains. However, in practice, among research managers in Lundbeck, we find the opposite notion, that in fact too much integration comes at the risk of important expertise. They suggest instead that collaboration develops

from a *"valuable tension"* between diverse domains. While the idea of complete convergence seems quite far from what we saw in Lundbeck's external collaborations the idea of valuable tensions seems much descriptive of this practice. Using the notion of screens, we might understand these tensions as created by changing screens that generate different categorizations of the participants. With this in mind, I now move from considering particular disconcertments related to Synapse to look into the implications of Synapse.

The emergence of new boundaries

In this chapter, I have explored disconcertment related to the specification of new strategic research areas and using new approaches. As we saw, the introduction of product concepts generated prioritized lists of targets to the Synapse management and entailed collaboration with clinicians, both of which produced forms of disconcertment. It took the shape not of a drastic shock or a big surprise but rather as a subtle form of discontinuity and interruption from the everyday norms of work. Exploring these moments is interesting because they make explicit what those norms are, and thus of the forms of screening through which strategic research in Lundbeck normally unfolded. I now consider in more details the actual outcomes of the Synapse process in terms of strategic research.

Synapse led to a remarkable change of the formal organization of research in Lundbeck and, also, to a significant round of layoffs among researchers. The layoffs came as a surprise to many of the involved research managers and many connected them with the identification of new research areas that they had been involved in. Defining new approaches to research, it seemed, had made certain existing forms of expertise unnecessary. Indeed, for several of the research managers this serious outcome also generated a sense of retrospective

disconcertment about the first phase of Synapse. In particular there seemed to them a tension between the rather casual way of making new strategies in which the process developed along the way, and the very tangible outcome in the form of layoffs.

Before Synapse, research was organized in three lines, Biology, Pharmacology and Chemistry, and in four Disease Teams for Alzheimer's disease, Parkinson's disease, Psychosis, and Depression and Anxiety. After Synapse, research was organized in separate Biology Units that each had its own technologies and resources. These Biology Units were named after what was broadly considered biological mechanisms: Neurodegeneration, Synaptic Transmission and Discovery Chemistry. In this sense, the first phase of Synapse resulted in the making of new boundaries, approaches and orientations to research. We can also see this reorganization as related to the emergence of new screens that classified research according to *(disease) biology* rather than *disease*. Aside from the layoffs, what were the implications of this new classification?

According to one research manager, working within Biology Units made daily research activities remarkably *easier*, since the reorganization comprised "everything that one needed in one unit". Consequently, some processes immediately seemed to accelerate because there was no preceding coordination with the line function. However, according to other research managers, the need to coordinate across research projects had in fact been an advantage of the old structure. As one manager, Hans, argued, trying to break down 'silos' between preclinical and clinical research had only produced new 'silos' in the research division. Biology Units had become what he described as "autonomous entities" that could easily operate without coordination. Consequently, instead of working together, the Biology Units had become in-house rivals.

> The risk of Synapse is that you're making some silos that become competitive units and we are too small for that because we do not have enough shared resources.

In this view, establishing autonomous entities posed a *strategic* problem. The initial phase of Synapse had made it clear that there were many possible roads to take. Consequently, management had come up with a number of very broad strategic areas, which provided them with a flexibility to define and initiate the best projects within these larger groupings. However, making the large areas autonomous units with their own technologies and staff also made it very difficult to prioritize between the units. What would the research management do, he wondered, if they found out the most promising research projects were *all* within one unit? And who would have been given the task to determine the potential of projects across the Biology Units?

Another research manager, Thomas, argued that the Biology Unit structure made it easier to collaborate *within* the unit but made the surface of contact with the surrounding organization smaller. He argued that having many potential contacts in the organization was important when moving a research project forward. Decreasing the number of potential contacts made the structure vulnerable to future change.

> The way forward for a project is shorter but people's surface of interaction within the organization has become much smaller.

Notice again the similarity between this argument for a broad "surface of interaction" and Hans' comment about the neurocell collaborator Martin's many roles as constituting a broad range of "entrance points" (in Chapter 10). Both suggest that engaging in complex emergent relations is not a *problem* that should be handled through control. Rather such relations are advantageous because they generate more *opportunities*. But what, in fact, were the implications of Synapse for external research collaboration?

Synapse and external collaboration

The first phase of Synapse did not directly address external collaboration but it was nonetheless often drawn into discussions. It was widely believed among research managers that the Biology Units would interact closely with external research groups, as the focus on biology would imply an expertise that the units neither had nor could establish. However, something about the rationale of Synapse made it more difficult rather than easier to initiate external collaborations. As one research manager argued, large framework collaborations like the Mayo collaboration would be difficult to initiate after Synapse, for in such collaborations the scientific rationale was the *outcome* of the collaboration rather than the starting point. The problem was that in Synapse, the initiation of research required not only a rationale but also a clear link to future clinical research. Such a link had not been defined in advance of some of the main cases of external collaboration, and had indeed even been actively *avoided*, as was the case with the Mayo collaboration. Whereas external collaboration had certainly been legitimized in various ways, using different repertoires of justification in different contexts, Synapse had produced a new screen in which research promise was directly related to clinical outcomes. Consequently, external collaboration ceased to offer something of a free space for experimentation and became subject to novel forms of screens that relied on much narrower kinds of legitimating.

In addition to these changed conditions for external research, some research managers also argued that the new boundaries of research in Lundbeck had consequences for the expertise that Lundbeck researchers could invest in external collaborations. As one research manager argued, the old organization had given Lundbeck researchers a focused expertise in particular diseases like Alzheimer's. Consequently, they were able to have in-depth discussions with external academics in which they contributed with cutting edge knowledge of

diseases. With the new organization, each unit would also develop detailed knowledge about biology but they would no longer automatically be up to date on diseases. This was an important problem since, as we saw in the Mayo collaboration, close interaction with external researchers was seen as constituting collaboration:

> We actually knew a lot about both Alzheimer's disease and Parkinson's before we started to make those collaborations. Now we risk going into collaborations where we are going into some diseases where we have no expertise at all. That's a concern I have.

Thus although external collaboration was considered an important and integral part of future strategies, certain strong ideas in Synapse slightly prevented external collaboration. The idea of "thinking things through" from start to end and the rule of only initiating research projects that clearly *anticipated* a future drug both conflicted with the way external collaboration had previously developed. It had often not been possible to think projects through, and external collaboration had provided a context for exploring things and gaining more certainty about different opportunities. In addition, the organization of research in new Biology Units had introduced a new form of expertise that was related to biology rather than disease. Contrary to what one would have believed this shift from disease to biology expertise had made it more difficult to interact with external researchers. With a precise competence in certain diseases research managers from Lundbeck had previously been able to challenge the ideas of external collaborators. In contrast, a broader knowledge about biological mechanisms did not give them the same ability to enter discussions about the *direction* of a joint research project.

A subtle emphasis on biology

As we have seen, one of the central ideas of Synapse was that a shift in focus from disease to biology potentially opened up a wide range of new diseases.

Theoretically, it implied thinking biology *before* indication, rather than the other way around. In practice, however, some research managers argued that this reversal was impossible. For in fact focusing on biology did not mean that a decision about indications and disease was postponed. Rather, biology, indications and diseases were considered together. Synapse relied on an idea of *connecting* biology, indications and diseases, however, as some research managers suggested, this implied enforcing an imagined *disconnection* between them. Accordingly, one research manager, Trine, argued that the focus on biology did not actually imply a shift to biology but rather introduced what she described as *"a subtle emphasis"*. In practice, a subtle emphasis did not imply a dramatically new research routine. As she argued,

> You have to find an indication quickly. Now you just can choose freely among all, but that doesn't mean that you shouldn't think about it very early in the process.

Indeed, this research manager suggested that making decisions about indications had always been related to biology. The limited number of indications that they had previously worked with might have constituted a constraint but thinking creatively about biology and diseases had nonetheless always taken place. As she argued,

> But really, people who worked in Alzheimer's disease units before they really tried all they could to understand the biology in Alzheimer's otherwise they couldn't say that there was something that they thought was a better idea than something else.

Consequently, Trine argued, "the shift to biology was oversold". Rather than a shift from one perspective to another, she argued that Synapse simply implied taking more perspectives into account when defining research projects.

> People are thinking harder about how the patient population looks and how the disease looks in a clinical perspective and that this is not just something that people will fix when it gets over to development.

So what at the level of strategies had seemed to introduce radically new research practices was at the level of practice rather a matter of describing research in a new ways, giving slightly more emphasis to biology and clinical perspectives.

Synapse obviously implied new emergent boundaries and relations *within* Lundbeck. Several research managers suggested that the new organization of research was particularly demanding for research managers and project leaders. What, in particular, did they have in mind?

Implications for research managers

Research managers argued that Synapse had particular implications for their work, which I suggest might be made sense of using the notion of occluding screens. First, establishing separate Biology Units meant that scientific discussions moved out of the coordinating DDMT and into the individual units. Since the units did not share resources and technologies, overall coordination was not needed. In principle, the units were only considered together by Lars, the head of drug discovery, who was responsible for all research activities. The research managers in the units no longer had any incentives to discuss progress outside the units. We might think of this implication in terms of screen in the following way: Activities and ideas that developed in the context of Synapse produced new categorizing screens that differentiated between different main biological mechanisms. Simultaneously, the separations emerged as occluding screens that removed a previous transparency between groups of researchers and research activities.

Some research managers saw this consequence of the reorganization (that we might understand in terms of an occluding screen) as problematic because the interaction between different divisions had previously been a valuable *source* for

generating ideas. As a result, the research management took various initiatives to strengthen interaction between the units. At the annual project review, for example, some researchers were appointed reviewers with the task of discussing the progress of projects in another unit. However, it was clear that the new structure had made it more difficult for research managers to consider the project portfolio across units. I propose that Synapse eventually organized research activities by generating new occluding screens between sections in the Research division, which made it harder for research managers to continue a valuable comparison of research projects across the research organization in Lundbeck.

Research managers also suggested that Synapse posed new challenges for the research project leaders. To lead a project post-Synapse entailed working across a number of boundaries. Research managers said that the new Biology Unit structure made some parts of managing research easier as decisions were made locally, in the unit. However, to an increasing extent, the project leader was also expected to be particularly proactive in making connections between activities that took place both within Lundbeck and outside Lundbeck. As Jesper argued:

> It [the focus on biology] demands a lot from the individual project leaders in terms of being able to have things done outside Lundbeck and at the same time make sure that the data gets in.

I return to the discussion the emergent role of research project leaders in Lundbeck in the following chapter. To wrap up this chapter, I discuss how the notion of screens helps shed light on the way strategic research emerged in relation to the Synapse strategy process in a quite different way than what we saw in collaboration with Mayo Clinic and with research groups around neurocell.

Synapse and misaligned co-production

How does strategic research and research collaboration emerge in the context of Synapse in Lundbeck? In some obvious ways, the situation in-house is very different from the contexts of external collaboration discussed in previous chapters. In the context of Synapse, strategic research was rendered as the outcome of a rational approach where strategic research areas are chosen after a process of mapping available options. This seems to be a more restricted sense of research than what we found in the neurocell collaboration. Here, value was seen as an emergent quality rather than a feature that was predictable in advance of engaging in various research relationships.

Second, it also seems quite clear that Synapse put an emphasis on collaboration between distinct parts of Lundbeck, such as between research managers and clinical researchers. Thus, Synapse introduced a new format for collaboration where different types of researchers work together in groups to prioritize research projects. Again, this is quite a different framework for collaboration that the emergent framework of the neurocell collaboration.

Although in Synapse research managers worked within a framework that highlighted processes of integration and alignment it is noteworthy that their accounts of relations with other parts of Lundbeck nevertheless continue to be based on *differentiation*. For example, collaborating with clinicians implies screens that strongly distinguish between what it implies to be research manager and clinical researcher in Lundbeck. So in this in-house context, collaboration, even when premised on ambitions of alignment, we also see misaligned co-production taking place. There is co-production but it develops from ideas about how the participants are different and produce different things.

However, although collaboration between preclinical and clinical research in Lundbeck seems to be premised on divergence there are also moments of alignment. Thus, one research manager told me that Synapse has made her think very hard about how a disease might look from a clinical perspective. We can see these aligning perspectives as examples of some sort of emergent relations. However, they are still quite different from the relations that are developed in external collaborations such as the Mayo collaboration.

In the Mayo collaboration, Lundbeck research managers emphasized their own credentials as academics. In a sense they *became* academics or, at least, similar to their collaborators in order to collaborate. In collaboration with in-house clinicians, the research managers are also emphasizing the importance of clinical perspectives. However, the difference is that while they find *becoming academic* important for making collaboration with the Mayo Clinic possible, they do not seem to find *becoming clinician* a constitutive factor of in-house collaboration. Drawing in a clinical perspective is a matter of developing a valuable tension rather than becoming the same. In this sense, the notion of misaligned co-production seems to describe an important dynamics of in-house science-industry collaboration. The implication of Synapse is that research managers can no longer refrain from thinking about what happens to a research project once it develops beyond their control (co-production) but they still demarcate their own research practices from other in-house practices.

Although the terminology of external collaboration and in-house research is prevalent in Lundbeck, the cases I have discussed also demonstrate the ambiguity of this distinction. In fact, different forms of screening continuously generate multiple externals and internals, even within Lundbeck. For example, within the Department of molecular neuroscience, clinical research was external to research. As a consequence of Synapse, the unit of Neurodegeneration became external to

the unit of Synaptic Transmission. In some cases, external collaborators became *internal* to researchers in Lundbeck simply because they had more related interests, backgrounds or approaches.

This chapter draws attention to the question of whether it is, in fact, in some situations, easier for industrial research managers to collaborate with external academic groups than with in-house colleagues. The implications of this question suggest a new direction for studies of science-industry collaboration. Often science-industry collaboration is seen as mainly challenging due to the cultural differences between academics and industrialists. As we have seen, the idea of such essential difference even emerges in practice in Lundbeck's external collaborations. Remember that the Mayo Clinic researchers took for granted that there would be a difference in culture and interests between the Mayo Clinic and Lundbeck and that collaboration would develop from quite distinct roles and engagements. However, although this idea emerged it was challenged by a different idea of science-industry collaboration in which there was an initial strong alignment between industrial and academic research interests. At various points, it then became important to differentiate, especially towards the end (c.f. the idea of "knowing enough"), but this did not change that interaction was not hard due to different scientific practices.

In comparison, in Synapse we see collaboration between research managers and clinical researchers. They are part of the same company and they participate in a strategy process that even emphasizes further integration. Nonetheless, they are far from essentially the same and we can observe activities and talk that strongly differentiate research managers and clinicians. For an outsider, these collaborating parties might seem the same because they are all part of the same company. But what we find is collaboration that develops as misaligned co-

production for instance, co-production that not mainly seeks to merge but also to make explicit important differences.

Conclusion

In this chapter, I have explored how research managers in Lundbeck have defined new strategic research areas using a new rational systematic approach. This approach focused on the mapping of opportunities, rational choices and the making of connections between separate parts of Lundbeck. Synapse resulted in a reorganization that strengthened the emphasis on biology and clinical research. I also discussed how collaboration within Lundbeck relied on forms of screening that differentiated starkly between kinds of researchers, despite the explicit focus on making connections. Finally, I have indicated that research managers in Lundbeck take on multiple roles and analyzed how these roles changes depending on particular contexts.

I now turn to consider what, more specifically, characterized the emergent role of research project leaders in Lundbeck. In particular, I examine how this role produced particular new screens between research and development. To examine these questions, I turn to the final phase of Synapse. Here, the focus was on the relationship between Research and Development and on the concept of project leaders as employees with a particular responsibility for establishing strong connections across boundaries.

13. THE PROJECT LEADER OF THE FUTURE

During Synapse, research management was viewed as central to the integration of different divisions of Lundbeck. I found this intriguing, since this conception of research management resonated with trends in Danish public research policy, while it also seemed to have some rather different entailments. In these novel collaborations between parts of Lundbeck, conceptions of research management were made explicit. These collaborations also brought some of the key assumptions about the nature of research in Lundbeck and about collaboration between research and non-research out in the open. In this chapter, I explore the kinds of screening that emerged in-house in the process of strengthening connections between the divisions of research and development. Specifically, I investigate the basic forms of *categorizations* on which the emerging notion of research management drew. I also explore various collaboration activities in Lundbeck as screens that entail various projections about the future organization of Lundbeck and consider how these activities and projections attach value to certain forms of research management rather than others.

This chapter presents a series of events in which I took active part. While previous chapters were based on participant observation and interviews, this chapter is based on formal participation. For instance, I was the host and organizer of a series of workshops, I presented findings and gave recommendations to the Research Management Board (RMB), I was a member of strategy working groups, and I took part in making a handbook that described a new operational model for Research and Development in Lundbeck. This direct participation gradually developed during my employment in Lundbeck. It gave me the advantage of close access to ongoing discussions and to different screenings of

research, collaboration and management. Yet such participation also meant that my research became more directly "attached" to ongoing activities in Lundbeck's research management (Jensen 2007).

Being this directly attached to ongoing activities has implications for how I use the notion of disconcertment in the present chapter. Whereas previously I explored Lundbeck research managers' disconcertment in situations that I had either observed or encountered in interviews, in this chapter I explore events that I participated in, and in which I myself experienced disconcerting moments. In some situations, the disconcertment was mine alone, but in other situations, it had a public, collective dimension, being shared either among the entire group of participants or among research managers.

Using different forms of disconcertment enables me to explore different aspects of research management. I use my own disconcertment to explore what is specific about research management practices in Lundbeck. When I experienced surprise in various situations, I used this to investigate further what generated my surprise, and which assumptions about industrial research management were challenged by what I saw. Thus, I used my own disconcertment to investigate what was normal in Lundbeck practices. In addition, I used a shared, public form of disconcertment to explore what emerged as surprising for Lundbeck research managers. This form of disconcertment can be used to explore what research managers in Lundbeck found challenging about research management, in particular when research management practices in Lundbeck met other practices.

Optimize External Research Collaboration

During 2008 and 2009, new ideas about external research collaboration started to develop among the research managers in Lundbeck. Research collaboration had,

of course, always taken place, but now research managers thought explicitly about how external collaboration fit with new research strategies and how it could best create value. Different concerns were at stake in these considerations. For example, Hans, was deeply engaged in initiating collaborations and developing specific contracts. Another research manager, Jens, was involved in formulating the overall research strategies of Lundbeck, in which external collaboration was one focus area among others. Yet another, Lars, was concerned with the interface between external collaboration and the in-house organization of research. To Lars, reflecting on external collaboration mattered in relation to the question of how to *optimize* the outcome of external collaboration and how to ensure a strong link between individual collaborations and overall strategies.

During this phase, I became involved in organizing a series of workshops about external research collaboration. One of the concerns among the research managers was that external research collaboration was not given separate attention. Instead, it was lumped together with in-house research in Lundbeck's portfolio of research projects. At the same time, there was continuous discussion about certain issues that specifically related to working with external collaborators. These issues concerned, for instance, how Lundbeck researchers could influence and motivate external collaborators, how they could maintain a form of scientific expertise that made them capable of challenging external collaborators' ideas and suggestions, and how the scope of external collaborations related to Lundbeck's strategic visions. In particular, Lars was interested in the formal organization of external collaboration and in finding ways to optimize their outcomes. Consequently, he proposed a series of workshops that would explore these issues. The workshops were called "Optimize External Research Collaboration". I hosted these workshops.

In total, three workshops were organized in the spring of 2009. In preparation, we—Lars and I—made a list of all ongoing external research collaborations. The list contained 18 collaborations, different in many ways, but similar in that they shared a formal external element, i.e. they all had a collaboration agreement. In some cases, the external collaborator was an academic research institution like the Mayo Clinic; in others, it was a network of collaborators working on a number of projects. In yet others, a contract research organization were the collaborator. The workshop participants were project leaders and scientists, as well as managers, working with these collaborations. In addition, the Drug Discovery Management Team (DDMT) participated. DDMT consisted of Lars (the head of drug discovery) and Hans, Niels and Anne who were all divisional directors.

In advance of the workshops, the project leaders were asked to prepare a presentation of the collaboration they led. Specifically, they were asked to describe how the collaboration was organized and managed, and how they believed it fit within the present developing research strategies. In addition, the project leaders were asked to address issues of significance and issues that had generated surprise or caused problems.

The workshops gave rise to lively discussion among the participants. One research manager, Trine, emphasized the coincidental development of many of the collaborations and argued that the idea of optimizing them, or relating them post-hoc to developing strategies in Lundbeck, was rather complicated. Another research manager, Jesper, described the difficulties of managing such collaborations, since it required leaders to work not only within the constraints of Lundbeck, but also to develop an understanding of how things worked at the collaborators' organization. One of the divisional directors, Niels, argued that working with contract research organizations was just as challenging as working with academic institutions, since it involved cultural differences equivalent to the

differences between academics and researchers at Lundbeck. Towards the end of these workshops, it was clear that each case presented a different story, and it would be difficult to compile a list of generic issues. Lars found this rather unsettling and hard to accept. After all, the purpose of bringing the leaders of external collaborations together was to discover and develop a way of optimizing the management of these collaborations. Instead, it primarily resulted in exhibiting diversity, which did not seem like a step forward.

Eventually, it was concluded that *optimizing* indeed meant different things from case to case, and, therefore, the strategy for managing collaboration had to be made "case by case". Even though the DDMT agreed that this conclusion felt right, they insisted that something more tangible had to come out of the workshops. To meet this demand, Lars and I developed a checklist for project leaders. According to this list, it was important to develop a contractual set-up with a clear distribution of immaterial rights and responsibilities. In most cases, this should be supplemented with an "adaptive framework" that allowed the collaborative content to develop and change (See Appendix A).

We can make sense of what happened in these workshops in terms of the screens that the meeting preparation, presentations and discussion produced. First, the planning of the workshops involved a screen that separated in-house research projects from research that had a formal external element. This categorizing screen separated research areas that were normally not considered to be distinct and grouped projects that were usually not considered to be alike. For instance, by categorizing external collaborations as similar, the screen constructed "cash and carry" projects as equal to the investigation of biological mechanisms. In the planning of the meetings, particular screens also developed that emphasized organization and management rather than discussion of scientific progress. One might say that there was an attempted separation of context (how the research

was conditioned and organized) and content (what the research particularly concerned). So activities in the planning process produced a screen that separated external research from in-house research, and management and organization from research progress.

At the workshops, the participating project leaders gave presentations according to the initial screening of these meetings. They gave accounts of various management and organizational topics. In these presentations, figures and illustrations of molecules were replaced with organizational diagrams. However, the discussions that emerged from presenting research in this way challenged the initial screening of research according to what was external and relevant for organizational/management. Several things became clear. It became clear that the external collaborations were not alike, and that categorizing them in this way displayed a diversity of different, rather than similar, projects. The screen that separated external from in-house also became problematic, because it was clear that external activities were closely related to internal activities, which was why they were not organized separately. In addition, during the discussions of organizational structures, issues about scientific content emerged. For instance, one research manager, Thomas, described how a controversy between two laboratories with which Lundbeck collaborated was not merely an *organizational* issue, but had implications for the progress of the research. Consequently, he suggested that organizational issues were deeply entangled with matters of a "strictly scientific nature".

In this way, the planning of the workshops had produced a new screen for sorting out research in Lundbeck. But using this screen turned out to be difficult because it not only categorized but also occluded important relations from sight. Distinguishing external from internal, and organizational/management issues from scientific progress, occluded the understanding that, in practice, external

collaboration was closely related to internal activities and organizational/management issues to scientific activities. Thus we might say that the discussions during the workshops challenged an initial categorizing, and in part also occluding, screen. The workshop discussions also produced a new screen with somewhat broader categories. This screen classified research in Lundbeck as consisting of interrelated research projects with various internal and external activities. This emerging screen arranged matters of research governance as closely related to matters of science. In the stories that the project leaders gave, these issues were not separate considerations.

The outcome of the workshops was a "case by case" approach, according to which differences and links rather than similarities and neat separations between research projects had developed. However, as mentioned, although this approach seemed right it was also unsatisfactory, according to Lars. Optimization and "case by case" were somewhat contradictory approaches, since optimization involved a desire to do something similar to a category of projects, and thus strategically move them in a particular direction. Managing these projects "case by case" left strategy more open and made it difficult to distinguish how these projects were illustrating a new strategic approach to research. In the following, I explore this contradiction further. Using the notion of screens helps me explore the development of strategic research in Lundbeck as a heterogeneous process in which various assumptions and sorting mechanisms are used and negotiated. In this case, it was not one document or one verbal expression that made a screen. Rather a number of activities produced screens that categorized research in particular ways, with implications. We also saw how screens gradually dissolved because it did not capture important aspects of research in Lundbeck.

The workshops "Optimize External Research Collaboration" initiated a discussion among research managers about how to make models based on the diversified

and changing nature of research collaborations. Simultaneous with these workshops, a parallel discussion about took place at the Research Management Board (RMB).[56] I was invited to present the conclusions from the workshops on optimization to the RMB—a presentation that, as we shall see, had a surprising outcome.

Project leader competencies

My presentation to the RMB took place in spring 2010. According to the RMB's secretary, the members were curious about the results of "Optimize External Research Collaboration" workshops that had taken place in the context of the DDMT. Hence, I was expected to present these results as well as stimulate a discussion about organizing and managing external collaboration. Unsurprisingly, the RMB was a rather different audience than the DDMT. Although the members of the DDMT represented different divisions of Lundbeck's drug discovery research, they all operated within discovery research. In contrast, the members of the RMB represented a much broader field of preclinical research, including drug discovery, non-clinical safety research (toxicity issues), and patents and trademarks. Presenting results from the workshops, therefore, meant presenting specific collaboration and organizational issues to a broader audience, not all of whom were equally familiar with these issues.

Aside from presenting the purpose and main issues of the "Optimize External Research Collaboration" workshops, I also aimed to explicate the difference I saw between the RMB's ambition to make models for external collaboration and the DDMT's decision to develop a "case by case" approach. The purpose was to

[56] This RMB discussion related to the aforementioned seminar "Future Models of Collaboration" that took place in spring 2009. See Chapter 6 for a description.

encourage a discussion about how overall models would be able to anticipate and take into account the complex dynamics of actual collaborations. On my concluding slide, I contrasted a "case by case" approach with a "model" approach, which produced yet another screen that separated two prevalent ways of managing research in Lundbeck. I did not argue that one approach was inherently better than the other, but I emphasized that the two were only vaguely connected, and in some sense even opposed, to one another.

As I described the outcomes and conclusions of the workshops, members of the RMB nodded and gave indications of appreciation and recognition. However, my final slide, which screened an adaptive approach as different from strategic models, immediately created a confused and unsettled atmosphere. It was not my impression that the members of RMB disagreed with the slide content. Yet, somehow, there was something slightly unacceptable about making this screen and describing the two approaches as being in opposition.

Breaking the awkward silence, Jens, the head of research, said, "I'm missing a slide on the competencies of the project leader of the future". This created immediate, disconcerted laughter in the room, and I also felt slightly confused by the question. After another brief silence, I argued that the diversity of research collaborations in Lundbeck suggested that there would not be *one* set of competencies but rather *various* competencies following from the specific nature of each collaboration. This explanation, however, did not change Jens' mind in regards to what was missing. "Yes, well, I still need a list of key competencies" he argued. Jens, and the rest of the RMB, had clearly understood, even appreciated, my presentation of collaboration as entailing emerging relations rather than ready-made strategic tools. So what was this request for a list of key competencies about?

Jens then said that he did not disagree with an adaptive approach. However, he was concerned with the apparent "gap" between Lundbeck's future research model and the skills of current project leaders. In this sense, he made the slide expressing opposition between the two approaches into a screen that *projected* a particular image of the future in which practice and models were connected. In fact, analyzing gaps was part of Synapse and a task that ran in parallel with discussions of new research models. Jens further said that, in the near future, human resource managers in Lundbeck would start to look for new research "profiles". At this point he would have to decide on the kinds of competencies they should be looking for. It was one thing, he said, to agree on a "case by case" approach *within* the Research division. Who would disagree? Yet it was quite another thing to be able to communicate to other parts of Lundbeck what characterized the specific challenges of research (which the slide suggested) and to specify the kinds of researchers and managers needed. Vis-à-vis Human Resources, presenting a list of issues that research managers in Lundbeck were currently dealing with would simply not "do the trick". So, he asked, what would a list of skills look like that reflected the complex issues that research managers were presently dealing with?

In the following weeks, I made a list of such competencies. I drew on my previous experience with management of research in a policy context and on input from Lundbeck research managers to do this. For example, the list emphasized the need for "a high tolerance level with regards to risk and change", and the need for "experience working with people with different scientific backgrounds". As he had suggested, Jens used the list in following discussions with human resource managers. As far as I know, the list was also used at other occasions. For instance, two years later a senior research manager, Jørgen, brought the list to a meeting in

the advisory board of a Danish university president. Here it was used in a discussion of the kind of university candidates companies like Lundbeck need.

In hindsight, the RMB meeting marked a turning point, at which time Lundbeck began shifting from a primary focus on models for the optimal organization of external collaboration to discussions of research project leadership. Both the research managers and I held on to the notion of the project leader of the future. It became a key concern both in the organization, as well as in my own research, which began exploring what this notion implied.

So, how were screens involved in generating this notion of a project leader of the future? We see how various presentations of research make categorizations that again have implications for what emerges as strategic concerns. As I have emphasized throughout this thesis, such presentations can be seen as particular screenings of research. In Lundbeck's annual Project Review, for instance, research projects were presented according to progress and expectations described in goals and milestones. As I have just shown, my presentation to the RMB also produced a screen that separated different approaches to research collaboration. Arguing that one approach to research collaboration accepts that such collaboration is diverse and messy, while another supposes that it is possible to model, my final slide operated as a categorizing screen that created a disconnection between two ways of dealing with research collaborations. Subsequently, Jens challenged this screening, first, as it seemed, by changing the topic of discussion, but, in reality, by claiming that the separation was a false one. In relation to parts of Lundbeck that did not share the research managers' experiences, the crucial issue, he suggested, became how to make a model that took its starting point in complexity but ended with a list of competencies. Subsequently, this very list of competencies that emphasized, for instance, "tolerance of change" became a screen on its own with, at least in principle,

implications for the recruiting of new project leaders. The list as a screen projected a view of future Lundbeck research as a heterogeneous practice that nevertheless must be managed by a research manager with particular core skills and experiences.

As we have seen, the notion of the project leader of the future emerged in discussions among research mangers in Lundbeck and eventually it became the center of attention in the final phase of Synapse. I now move on to discuss how the project leader of the future was first developed.

The multi-faceted research manager

Six months after the discussion of the project leader of the future at the RMB, a working group was established with the purpose of further developing the notion of future project leaders. I was invited to join the group. The group was asked to give a presentation of project leader skills at the annual goal coordination meeting in December 2010, once again in front of the RMB. The project leader-working group had four members: Richard, the head of drug discovery at the US research site; Andreas, a chief scientist from the chemistry department; Morten, a human resource manager affiliated with the research division; and me. We met three times in Andreas' office in the chemistry building. Participating in these meetings gave me valuable insight into how research was approached outside the RMB and DDMT. However, it was also disconcerting, as our discussions developed into a screen that categorized project leaders as superhuman, which I had great difficulties accepting.

First, I learned that the discussion of the project leader's role was not new in Lundbeck. In fact, Andreas and Morten had both been involved in several former groups dealing with this issue. These groups had focused in particular on

developing project leader courses and on professionalizing project leaders. In the research division, all project leaders were scientists and only a few had actual leadership training. For this reason, in-house project leader courses had been developed. However, our project leader-working group had a different task. We were meant to describe new skills that would potentially be required of project leaders in order to deal with research in the future of Lundbeck, post-Synapse. We were thus expected to use our different insights into Synapse to predict the kinds of skills that would be essential to acquire or nurture. In a sense, we were producing a projecting screen for project leaders.

Based on the first meeting, however, it was clear that we had very different ideas about both the implications of Synapse and the role of project leaders. Andreas and I argued that one main skill would be the ability to adapt to changes and to collaborate with external parties. Morten maintained that the outcome of our discussion should be the design of new training modules. Richard argued that the project leader of the future would be a multi-faceted artist. Not only would he or she have to be an excellent scientist, he or she would also be highly skilled in strategy and finance, and, finally, be an excellent leader. The list of imagined competencies was overwhelming and grew longer during each meeting.

There was an ambiguity to the list that made it disconcerting. In particular, Andreas and I argued that the list described a broad range of competencies, while, in reality, individual project leaders would have combinations of competencies, but not all. Thinking that one project leader could possess all competencies at once made us laugh. We gave examples of current project leaders who had excellent skills in some respects, but were less skilled in other respects. However, this did not matter, because in practice they were able to compensate for lacking skills.

However, Richard, argued that the elements on the list were not optional, but parts in a coherent description of the project leader of the future. He argued that since the list of competencies addressed a future organization, it was not relevant to argue that these competencies could not be achieved on the basis that, today, Lundbeck project leaders did not possess them. As Andreas and I challenged this view, arguing that the list hardly produced a realistic image, Richard insisted that Synapse in fact introduced completely new circumstances for research that would require completely new skills. This concerned us all, especially Morten, as the human resource manager. Would *any* of the current Lundbeck research project leaders be able to live up to these demands? Were the competencies something to strive for and gradually develop, or were we part of a process that would entail the replacement of most project leaders?

The outcome of the meeting was a presentation of the condensed list. We agreed to present the list while also describing our reservations about it. However, in practice this was made difficult by the fact that Richard gave the presentation and was also part of the RMB. So, in the presentation, the project leader of the future was described as a strong scientist, a strong leader, who was financially and strategically savvy, who was an expert in regulation, and who was good at influencing agendas and achieving results. Interestingly, we found it difficult to communicate the point that the list did not reflect the present project leader practices in Lundbeck. There was something about the way the task of describing future skills created an opportunity to think in more abstract terms, unattached from the present situation in Lundbeck. The list of project leader competencies became a projecting screen that envisioned a completely different Lundbeck. As screen the list constructed project leaders as key people in Lundbeck who pulled together a number of practices and activities.

While our task was to stimulate discussion about project leaders, the list seemed at once universal and disconnected from the current situation in Lundbeck. It failed to give a qualified estimate of the future that Synapse was generating, and it also failed to make a strong connection between that potential future and the specific implications for project leaders. Nonetheless, to my considerable surprise, the description of the project leader of the future did not at all disconcert the RMB, which I return to. Before describing the reception of the description, I briefly introduce the context of the goal coordination meeting. As the name suggests, at this time in Lundbeck coordination was at the center of the research managers' attention.

The goal coordination meeting

The annual goal coordination meeting had the purpose of organizing and aligning the specific goals for each area within research in Lundbeck. These areas were Drug Discovery Research, Non-Clinical Safety Research, and Patents and Trademarks. Within the area of Drug Discovery, two research sites needed to be aligned: the site in Denmark and the site in the US. Coordinating ideas of research between the Danish and the US site was a constant matter of concern for research managers. Even though the US site also involved drug discovery, it was seen as just as external to Danish drug discovery as other divisions of Lundbeck.[57] The coordination of goals then implied that the head of each research area within the RMB would give a presentation about the coming year's goals, and they would lead to a joint discussion about coherence. At this particular meeting two

[57] The coordination between the Lundbeck DK and US research sites is a story in itself that I have chosen not to include in this thesis. I visited the US site in New Jersey in May 2010. Here, I conducted approximately 20 interviews with US research managers. In addition, I held two workshops similar to the "Optimize External Research Collaboration" and "Future models of collaboration" seminars.

additional items were on the agenda. The first was a new set of operational principles introduced by the general management of Lundbeck. The second was the project leader of the future.

The goals coordination meeting took place at Munkebo Kro, a luxurious restaurant and conference venue in Northern Funen, Denmark. The participants were the RMB members and each of their management teams. Thus, the DDMT participated along with the management teams of Non-Clinical Safety Research, Patents and Trademarks, and the US management teams. In addition to these ordinary participants, a number of guests were invited in relation to the additional agenda items. Morten, the human resource manager, and Elena, a human resource manager from the US site, led the discussion about the new operating principles, and Andreas and I participated in the discussion of project leaders. The meeting took place over three days. The participants arrived in the evening on the first day to have dinner and socialize. On the second day, the two additional agenda items were discussed. On the third day, the guests parted and the research managers had their goal coordination discussion. Let us then consider the screening of research that emerged during these discussions, and the subsequent screening of project leaders and the future organization of Lundbeck.

The culture of research

On the second day of the meeting, Jens began by giving a presentation that related the task of coordinating goals and developing joint operating principles to Lundbeck's current situation. This involved depicting the broader purpose of Synapse and the way in which Lundbeck, in recent years, had developed as a company. To do this, he used a metaphor of trees.

Illustration 7: The tree metaphor of Lundbeck's development.

Jens' slide showed two trees. One was withered, in brown and grey colors; it was lifeless with almost no branches. The other was large and strong, with lots of branches and thick with green leaves. After a sluggish period, Lundbeck had recently experienced very considerable growth. The thriving green tree illustrated this. However, as also illustrated by this tree, the growth was rather uncontrolled. In his imagery, each branch represented a research project that had been initiated without prior coordination with other projects. Because there had been little coordination in recent years, the result had been that research had developed rather chaotically. The task of Synapse was consequently to "prune the tree", as Jens described. This illustrated a new approach to research that entailed much more communication and coordination between different research practices, as the metaphor of the synapse itself indicated.

Although "pruning the tree" immediately seemed to suggest something radically new and different compared with the image of a green leafy tree, it did not, to my surprise, evoke any strong reactions from the group of research managers. The

use of images was fascinating, since the green tree was contrasted with the withered tree that represented the past. However, the slide did not have an image of a tree after it had been pruned, and thus the green tree seemed to illustrate something healthy and desirable. But the green tree did not represent the new direction; rather the new direction was evoked with the expression of "pruning the tree". The idea of "pruning the tree" seemed to suggest *reducing* research projects in number or *controlling* them to a much larger extent than previously. Thus, it surprised me that the group of research managers did not react more explicitly to what the images were suggesting, and that they seemed to accept that research needed to be subjected to more control. I see the image of the trees, combined with the expression of "pruning the tree," as a screen that projects an idea of future research practices in Lundbeck as increasingly controlled.

In contrast to Jens' presentation, the following presentation on the new operating principles deeply disturbed some of the participating research managers. Elena, the human resource manager from the US research site, opened the discussion of Lundbeck's future operating principles. With the aim of stimulating a more coherent culture in Lundbeck, the general management had introduced four principles that were, according to research managers, somewhat enforced on them. Indeed, it was quite unclear to many of the research managers where the principles came from and what they meant. They suggested that the main idea was that if employees in Lundbeck followed these principles in whatever practices in which they were involved, Lundbeck would operate better as a business. First, Elena introduced the four new principles. They were: "Own the future", "Be ambitious and take action", "Better for less" and "Create results – together". As she explained, these principles were meant to be included as soon as possible, both in the annual goals and in the individual goals of all employees. But

what might they mean in the practical context of research? For instance, what would exemplify a "Better for less" approach?

To inspire a discussion about their adaptation, Elena, offered an introduction to the notion of "culture". "Each organization has a unique culture", she argued, "and so does Lundbeck". "But what is culture?" she asked. There were no responses from the audience. The situation started to grow uncomfortable. "Well, I have brought some examples of culture," she said, and put on a slide show. The first slide had the title "What is Culture?" and showed a cartoon.

Illustration 8: The cartoon illustration of corporate culture.

Two men sat behind desks wearing oversized harlequin hats. One of them says to the other: "I don't know how it started, either. All I know is that it is part of our corporate culture." Her second example concerned a company in which the CEO was always hostile towards his employees. He would often throw pens at his employees if they did not do as told. Elena argued, "This is also culture." Then Elena said to the crowd of managers, "So, you see, culture is important," and she asked, "The question is how do we *motivate* employees to adapt to these

operating principles, for example, how do we motivate them to 'think better with less'?"

Anne, one of the members of DDMT, was clearly affected by the presentation. With manifest irritation she exclaimed: "But this has nothing to do with us!" This abrupt confrontation clearly unsettled the participants in the room. Some started to laugh since the confrontation itself was slightly embarrassing. But Anne continued, "The way you talk about culture has nothing to do with us." Elena looked completely surprised by the sudden attack and asked why. "Researchers are clearly different," Anne argued, pointing to the slide with the two men wearing harlequin hats. "They are not motivated by external factors like you're suggesting. My employees are driven by curiosity, not some overall principles."

What seemed to take place in the situation was a discussion between two different perspectives. Where Elena gave an image of culture by drawing on a somewhat popular idea of culture, Anne presented research as a unique practice. Elena suggested that culture is what makes a group of people *cohere*. Then she addressed the question of what motivates people to work more efficiently and coherently in a company, thus making research a practice quite similar to other non-research practices. Anne's image of research as a *unique* practice generated a different *screen*, categorizing researchers from non-researchers. Drawing on this screen, Anne suggested that researchers are not motivated by external factors but rather work from a profound interest. The screen that Anne's account of researchers produced has the implication of presenting researchers in Lundbeck almost as academics, to some extent detached from the business of Lundbeck. This was somewhat surprising, considering that this screening of research that I read as suggesting something academic took place in the top research management in Lundbeck. However, it was a significant screening that was not left unchallenged.

Another research manager, Irene, who was Danish but working as a divisional director in the US site, interrupted the discussion between Anne and Elena with repressed laughter to say, "Well, researchers *think* they are different but they are really not." This created some amusement, but the affected research manager, Anne, did not laugh. Clearly annoyed, she responded, "Tell me *specifically* how this applies to us and what we are confronted with?" She added that she saw no point in discussing operational principle "in loose terms". Then Anne returned to Elena's introduction to operating principles, which suggested that although the four operating principles were shared, they were supposed to be adapted to particular work tasks in the different divisions. Adapting them meant prioritizing a joint discussion of them, as was what took place at the goal coordination meeting, and, as soon as possible, integrating the principles in both annual goals for each division and employees' individual goals. Anne asked, "How does discussing culture relate to the task of integrating the operating principles in the goals of my employees?" Again, this question made the atmosphere in the room grow tense. Elena did not answer this direct question.

At this point, Jens, who had kept in the background during this heated discussion, interrupted. He argued that the introduction of operating principles in the Research division was a little "enforced" but it was nonetheless the common perception in general management that introducing the principles had a high priority. However, he also said, "Let's see how far we get with this", which seemed to calm the divisional director, Anne. Lars supported this suggestion to take it easy by suggesting that using the operating principles in practice could be part of the evaluation of goals in the spring.

How can we make sense of this interaction as producing screens with implications for research management in Lundbeck? This is an example of a public moment of disconcertment. Although it was mainly the research manager,

Anne, and eventually the human resource manager, Elena, that felt disconcerted, the unsettlement that they felt spread to the other participants. I interpret the disconcertment as stemming from a collision of two screens of research that were produced by Anne and Elena. Elena's descriptions of culture, using images of men wearing funny hats without knowing why they do so, created a screen that categorized all employees in Lundbeck as the same. The analogy she made to the cartoon provoked Anne, who experienced it as condescending. Anne, the research manager, described research as a unique practice hardly adaptable to overall ideas about culture and operating principles. Thus, her account generated a completely different screen in which research is separated from non-research.

In order to understand the *relation* between the two contradictory screens we might backtrack a little. The head of research presents the image of trees and proposes that in order to become a healthy company, Lundbeck research activities must become more *coordinated* with other parts of the organization and also become more controlled. Following from this image, or *screen*, Elena's introduction of operating principle makes good sense, as the operating principles are meant, and presented as, devices for creating more *coordination*. However, there is something about the screen that Elena produces that is highly provoking. I suggest that the image of the two men wearing hats presents an idea of coordination in which the coordination is *incomprehensible*. The core of the story is that the two men do not see the immediate purpose of adapting to this culture of wearing hats. This is provoking because, according to Anne, the strategy on intensifying coordination in Lundbeck does have a *comprehensible* purpose of stimulating better research and products, and is not only a matter of culture. Also, as we shall see, many research managers did not associate collaboration with full integration and merging into one homogenous culture. Rather they saw

coordination as a matter of connecting with a specific purpose, while keeping their distinct research practices intact.

Thus, I used the notion of screen to explore emergent ideas about collaboration in-house. In this case, we have an *encounter* between researchers and non-researchers (represented by the human resource manager). It is similar to the encounter between Lundbeck research managers and Mayo Clinic researchers because it also generates screenings of researchers and industrialists. Yet it is also quite different from that encounter since it took place in Lundbeck as an in-house discussion about strategies and operating principles. Nonetheless, in this in-house context we also find situations in which misaligned co-production takes place, a point I return to. But how did the idea of research as a unique culture relate to the emergent notion of project leaders?

"The spider in the web"

After this commotion, Richard gave our presentation on the project leader of the future. This involved presenting a range of implications of Synapse and the aforementioned list of skills, which future project leaders were expected to have. Surprisingly, the (to my mind) excessive list of skills did not at all perturb the group of research managers. Indeed, when asked to prioritize the listed qualities, the response was as follows: "Well, they are all important." While I had anticipated disconcertment about an ideal image of a project leader presented on a slide that seemed dense with unsorted information, the managers did not perceive this as a problem. In fact, it seemed that to have a list was a priority that surpassed the relevance of the listed elements.

In the context of coordinating goals, however, the discussion of the future project leader served a general purpose that went beyond the quality of the list. After the

meeting, Jens explained that putting project leaders on the agenda signified that the RMB was already developing ideas about project leaders before entering into a discussion of leadership in the broader forum of the Synapse management. By having made this specification, it was possible to enter into discussions about future requirements for project leaders with managers from the Development division that saw project management in quite different ways, as I return to. At this time, it was anticipated that Synapse would focus precisely on the role of management for improving collaboration between parts of Lundbeck.

While the list of project leader competencies did not generate disconcertment as an unsorted list, it did raise questions that suggested a different screening of the role of project leaders than the multi-faceted project leader. During his presentation, Richard emphasized that, in the future, project leaders would have a crucial role in Lundbeck. Project leaders would be initiating new research projects, developing them and paving the way within the organization, and they would be coordinating with both in-house and external collaborators. He favored an image depicting the project leader as, as he said, “a spider in a web”. In this account, the spider was the project leader and the web illustrated the many relations, internal and external in which the project leader was involved. The image of a spider in a web produced a quite different idea about collaboration than the emergent relations that Hans had described in the neurocell collaboration. According to the spider in the web metaphor, a collaboration consisted of fairly stable relations (the pattern of the web), and the project leader of the collaboration had a key role (sat in the middle) in controlling these relations. In fact, Richard suggested that in external collaborations, the spider was a Lundbeck project leader that had full control of relations to the other participants in the project. All communication would go through this project leader. The counter argument, promoted by myself amongst others, was that

Lundbeck might well miss out on potentially emergent relations and interactions among the other collaborators if the organization sought too much control.

At the goal coordination meeting, the idea of the project leader as a spider in a web raised disconcerted questions. It was this image of a spider rather than the list in itself that produced a disconcerting screen. Towards the end, Hans interrupted Richard's presentation. Hans suggested, "The main problem here is that you're proposing a *completely* different organization." Rather than reacting to the list, Hans reacted to the *projecting* screen that the spider-image had produced. According to this screen, strong project leaders characterized the future organization of Lundbeck. Hans's objection indicated that something had to change radically to make this kind of project leadership possible in Lundbeck. As far as Hans was concerned, the project leader as spider was certainly not, as he said, "born out of the current Lundbeck". As he explained, the existing structure of the research organization was a matrix structure. It implied three divisional lines (Biology, Pharmacology and Chemistry) and a number of disease areas. Today, initiating a research project depended on a decision in the DDMT. The DDMT members would discuss the relevance of a project and consider how it might be resourced. Although Synapse implied a new emphasis on biology and a potential change of the matrix structure (which it did by introducing the mentioned Biology Units), the existing matrix structure did not leave the main initiative of new research up to project leaders.

Thus, the spider image gave rise to questions such as: What would be the implications for divisional directors in charge of coordinating resources and strategies? Would the project leader be able to ignore them to pursue his or her "autonomous" goals? The questioner, Hans, was extremely concerned with ensuring that project leaders would be able to follow their own inclinations without seeking careful coordination with the surrounding organization. The

main problem with our presentation was thus not the vast amount of skills it assumed necessary for project leadership in the future. Rather, our presentation came to work as a projecting screen that presented a future organization with considerable, but yet unclear, implications for project leaders. This screen was disconcerting because it failed to describe the connection between the existing organization and the projected organization.

Another objection was that the presentation took for granted that project leaders in Lundbeck already had basic project leader skills. Lars wondered, "I'm not sure I agree with you that today project leaders in Lundbeck have basic project leader skills in place?" He continued, "And are they really missing these advanced political skills that your mention?" He added, "Is the point not that they are quite good at building networks but pay too little attention to basic things?" With his questions and examples, Lars produced a different screen that forecasted an alternative future in which sorting out practical basic things was in fact more important than building and controlling networks. By "basic" project leader skills, Lars referred to making plans, facilitating meetings and coordinating activities. He was known for his strong interest in processes. As he saw it, one of the greatest challenges was aligning expectations in-house, for instance, by anticipating specific needs in the process of research. Often, he argued, things did not move forward as fast as they could because project leaders were not thinking ahead. Thus, like Hans, Lars' comments suggested that our future scenario was not sufficiently rooted in Lundbeck's present situation.

In these discussions, we are dealing with screens that *categorize* research and *project* various futures, while *occluding* present conditions in Lundbeck. I suggest that we are witness to the confluence of a set of organizational dynamics related to research, collaboration, and expectations about coordination. As noted, a strong undercurrent in the discussion is the notion of alignment. Thus, Synapse is

about “pruning the tree” and aligning decisions and priorities when initiating research projects. We also encounter operating principles that are introduced with the purpose of creating a more homogenous work culture across Lundbeck’s divisions. Yet, these forms of screening, which project a sense of unity, are confronted by an alternative screen that presents research and researchers as unique and differentiated.

The emergence of project leadership as a significant theme also makes explicit a series of screening processes. On one hand, the project leader is presented as a researcher capable of *adapting* to changing conditions. This screening emerged at the RMB meeting in the initial discussion of a “case-by-case” approach to project leadership that had been developed in the DDMT workshops on “Optimize External Research Collaboration”. This adaptive leader screening collides with an alternative projection that emerged during the project leader-working group, according to which the competence of the project leader is based on a number of *predefined* skills. The latter projecting screen displayed the autonomy of the project leader, and thus focused on what he or she does rather than how he or she relates to the broader organizational context. It therefore collided with the adapting screen that emphasized the role of the project leader in relation to other practices and functions in Lundbeck.

Let us now consider how these different screens continued to develop and interact in the context of Synapse. During dinner on the second evening of meetings, I discussed my observations with Jørgen, a senior research manager and member of the RMB. I was curious about the apparent disagreement among research managers about the potential future of project leaders. Were they autonomous scientists or process experts? How did these different ideas relate to different views about the status of the current organization and the future implications of Synapse? But according to Jørgen these disagreements were

insignificant. Even if they seemed fundamental, they reflected only minor differences of opinion within the same overarching interpretation of research. "The real battlefield", as he argued, would be the final phase of Synapse. Here managers from Research and Development were to work together on developing a new operational model for Research and Development. This collaboration was referred to as "Point 5.2." I now explore how the notion of project leaders developed in the context of 5.2.

Point 5.2

Work stream 5.2, the final part of Synapse, focused on strengthening interactions between the divisions of Research and Development and was described by BCG and the Synapse management as "Defining updated R&D operating model". It had four work streams, which were "Underlying process", "Know-how exchange along the value chain", "Behavior" and "Governance". For each work stream, the Synapse management established a working group, consisting of managers from both the research and development divisions. The idea was to create a new operational model that achieved maximum buy-in from both of these divisions in Lundbeck by letting the model develop from a joint discussion of what was needed. Each working group had a senior manager from Research or Development as chair.

As a member of the former project leader-working group, I was invited to become a member of the working group on "Behavior". This group would discuss which kinds of behavior would promote collaboration across the divisions of research and development. Besides the members of the former project leader-working group, the behavior work group consisted of managers from the divisions of development, research and human resources. The work was structured in one-

day meetings and shorter meetings in subgroups and took place over a period of two months in spring 2011. A Boston Consulting Group consultant was employed as facilitator. The Synapse management had developed a set of key questions for each working group, and the groups were tasked with answering these questions by developing presentations containing models and plans.

To begin, our working group had to delineate its task, since "Behavior" was too broad a subject. Several participants suggested that the main problem was actually impossible to address, since it was constituted by the different kinds of behavior and work styles of the head of research and the head of development. However, since these behaviors were too controversial to be turned into a topic for conversation, the group decided to concentrate on behavior at the level of the management teams. This focus immediately stirred a discussion of the expected behaviors of board and management team members, and of people giving presentations to these boards. During the discussions of appropriate management behavior, the role of project leaders reemerged.

The working group activities produced somewhat different screenings of project leaders than we saw in the research managers' discussions. The notion of project leaders that had been formulated by the research managers and discussed at the goal coordination meeting was confronted by a completely different notion of management produced in this working group. To account for this discrepancy, I turn to a particularly disconcerting moment that illustrates how employees from the Development division challenged the notion of project leadership developed in the Research division. I take this moment to be of particular importance for understanding science-industry collaboration within Lundbeck and for accounting for the projected role of research managers in making collaboration across boundaries work. Not only does the situation explicate important aspects of different perceptions of management in Lundbeck, it also illustrates, in a quite

surprising manner, how research managers view the central differences between collaboration with in-house non-research practitioners and collaboration with external academic collaborators.

The behavior work group

In the behavior work group, we evaluated various existing material on project leaders in order to develop new material that could serve as input to the Synapse management. Outside of the group meetings, research managers had suggested that one of the collaborative challenges concerned the different views on management and leadership held by the Research and Development divisions. Research managers emphasized leadership, and they were strongly encouraged by Jens, and RMB, to do so. Leadership was associated with the ability to think independently of others, to set a direction and to think out of the box. In contrast, as research managers claimed, managers from Development emphasized management. Management was associated with the opposite of leadership. Rather than independent, the management was perceived (by both research managers and people from the development division) as teamwork, and as an activity, was about assisting to develop processes in a given direction.

Research managers saw this difference between leadership and management emphases as a problem, and sometimes referred to it as the main reason why collaboration between research and development was difficult. However, as a problem, it was difficult to effectively address for several reasons. It was unclear to the research managers whether their colleagues from Development shared their perception that this was indeed a key difference that needed alignment. It was also difficult to simply ask their colleagues, because research managers attributed great value to the notion of leadership. It was awkward to suggest that

people in Development had to change their perception in order to become more aligned with research managers.

In the same way that research managers saw their form of nearly academic research as a more desirable form of research than clinical research, they also saw their comparatively unrestrained form of leadership as more attractive than the controlled form of management. Since they associated leadership with their own form of research, it was difficult to discuss these differences out in the open. One episode, however, presented an opening in which to address and discuss just this difference.

During one workshop in the behavior work group, a human resource manager, Hanne, presented a number of slides that described how the divisions of Research and Development had previously described the skills of different managers and leaders. These discussions had taken part separately in the respective divisions. But Hanne had produced a slide in which these role descriptions were combined. In the workshop, the slide emerged as a categorizing screen that made the differences between behaviors and leadership roles explicit, and thus made it possible to specifically address the differences between Research project leaders and Development project managers.

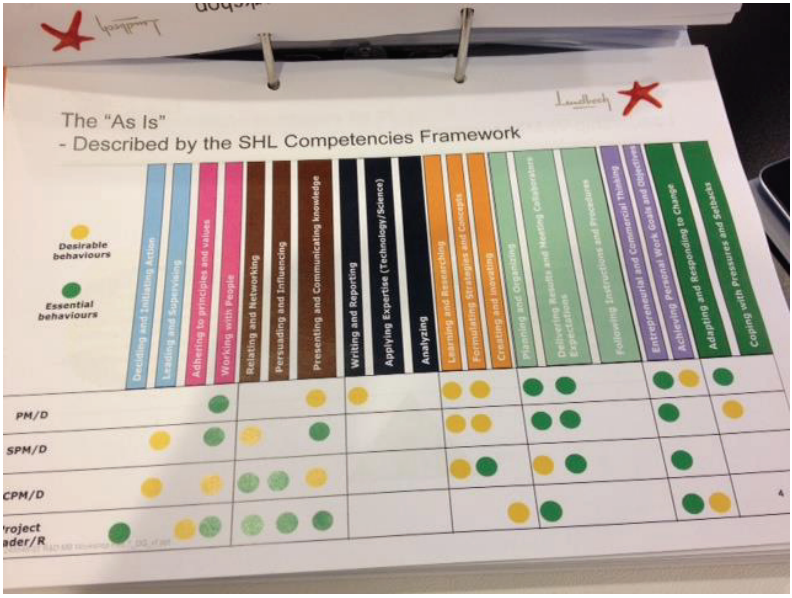

Illustration 9: The "As Is" slide illustrating the differences between Research leadership and Development management.

Hanne had named the diagram "The 'As is'", referring to how this slide represented the way in which the two divisions presently described various roles. The diagram combined a number of project manager and leader roles – for instance, the "PM/D" referred to project managers in the development division and the "Project Leader/R" pointed to the project leaders in the research division. These roles were then combined with "essential" and "desirable" behaviors for each role, drawing on a framework for describing competencies, developed by a company. These behaviors were, for instance, "Deciding and Initiating Actions", "Learning and Researching" and "Planning and Organizing". There were 20 behaviors listed. For each role, research and developed managers had marked which behaviors they found either "essential" (marked by a green dot), "desirable" (marked by yellow) or "not relevant" (marked by no dot).

Hanne did not make much out of the slide and was already preparing to move on when Jørgen who chaired the behavior work group, asked her to wait a minute. "Aha! This is interesting!" he said. This increased the attention of the working group members who were now staring intently at the slides. Jon, a chief scientist who had been occupied with his iPad, looked up and immediately said, "Isn't it interesting that creating and innovating isn't seen as essential for Development managers? It's not even marked as a desirable behavior." The group looked at the slide. Jørgen pointed to the slide and suggested: "This might be the problem. We have quite different ideas about leadership and management." The participating research managers, including the BCG consultant, all seemed to be uncomfortable about the slide that so obviously spelled out a difference that otherwise tended to remain tacit. However, there was also some relief that the discussion of the matter, without coming to the point, seemed to have come to an end.

Rather than showing any sign of disconcertment, Tanja, a development manager, responded: "No, I disagree. I don't see that there is a problem here". In her view the slide illustrated perfectly both how things were and how they should be. "I do not want project *leaders* running around and disturbing things," she explained. She calmly argued that in development processes "creating and innovating" was in fact not appropriate behavior. She explained that she saw research and development as two quite different things, and the slide accurately displayed this. She then argued that in development processes, management was the proper solution, since development was about "keeping things on track" and not diverging in all directions. This intervention took place during the Hanne's presentation. Jørgen, who had initiated a discussion about the slide, seemed to accept that Tanja did not see the difference as a problem. After a moment of quietly contemplating the slide, he suggested moving on.

In order to interpret this discrepancy, we may note that the slide introduces a screen that defines specific relations between Research and Development, and also between leadership and management. According to the slide, research project leadership is essentially about "deciding and initiating actions", "working with people", "relating and networking", "persuading and influencing", "presenting and communicating knowledge", "planning and organizing", and "entrepreneurial and commercial thinking". In contrast, according to the slide development project management is essentially about "Working with people", "planning and organizing", "delivering results and meeting collaborators expectations", "entrepreneurial and commercial thinking" and "adapting and responding to change". Chairing research manager, Jørgen, suggests that the slide visualizes a problem of collaboration because research and development projects are not managed according to the same ideas about what management should involve. To him, collaboration should evolve from a more aligned form of management style, based on the essential qualities of project leadership in research. In contrast, the development manager, Tanja, sees a different screen that categorizes Research and Development as too quite different practices that require different management. She suggests that the difference that the screen makes visual is not a problem. In her account, the diagram as screen only displays that collaboration should be based on an understanding of the important differences between research and development. Collaboration is not a matter of aligning management styles but about appreciating difference.

In Lundbeck, in the context of Synapse, ideas about collaboration and leadership developed from ideas about what research implies in Lundbeck, and how it potentially connects with other (non-research) practices. As we saw at the goal coordination meeting, the increasing integration and alignment that Synapse and the new operating principles introduced made it more crucial for research

managers to *explicate* what was distinct about research. As the direct collaboration between Research and Development managers in the behavior work group illustrated, in-house collaboration was fuelled by misalignment as well as alignment. Seen as illustrative of a form of science-industry collaboration, we might notice a difference between research managers' collaboration with external academic researchers and with in-house development managers. It seemed that where external collaboration meant becoming academic at some crucial points, in-house collaboration did not imply that research managers became development managers at some point.

I propose that comparing this case of collaboration with Lundbeck's external collaborations shows something important about science-industry collaboration. In both cases, we see instances of misaligned co-production in which collaboration develops from ideas about being different. In external collaborations the need to misalign emerges from conditions in which Lundbeck research managers are in fact very similar to their academic collaborators. However, in Lundbeck misalignment develops from being part of the same company but nonetheless *resisting* that this condition implies coherence and sameness. Lundbeck research is consequently screened as different in both cases, however for quite different reasons and with different outcomes.

Before concluding, I briefly consider the specific outcomes of Synapse in the years 2011 to 2013. Synapse officially came to an end in 2011 and was replaced by a phase of implementation. This phase was referred to as "Synergy" in order to emphasize continued efforts to strengthen interaction between different parts of Lundbeck. But what were the outcomes of Synapse? And what ideas about integration, collaboration and research management did these outcomes reflect? I address these questions in the following section.

Synaptic outcomes

One of the most important and tangible outcomes of Synapse Point 5.2 was an "R&D operating model". The model was constituted by the individual recommendations that the working groups had developed, and on which the Synapse management had signed off. The model thus emerged directly from the Synapse work streams. It included a new governance model for R&D replete with new boards, management teams and guidelines for knowledge sharing. It also included guidelines for the expected behavior of board members and project leaders.

In the behavior work group it became clear that defining a universal model for project leadership in Lundbeck was hardly possible or even relevant. Rather, the group's work suggested that there were quite significant differences between project leadership and project management, corresponding to the different work tasks of the two divisions. In the Research division, project leaders were multi-faceted scientists that had a key role in moving research projects forward. This involved a particular kind of reflexivity, known as "doing the right thing", even if doing so went against normal practice and involved doing something unexpected. In the Development division, in contrast, project managers were professional managers that were organized in a Project Manager unit. Development projects were resourced with project managers from this unit, not invented by them. Rather than reinvent processes, development project managers were to keep projects "on track" by stabilizing as many factors as possible. This difference between leaders and managers was not coincidental, nor did it reflect a more advanced culture in Research. Rather, it illustrated a difference in the objectives these project leaders or managers were working with and towards.

Instead of developing one general concept for managers/leaders in Lundbeck, it was eventually decided to accept the differences. The research managers accepted

that they should not aim to turn development managers into leaders by referring to the distinct nature of research. Focusing on the differences meant creating a joint understanding of the diverse processes of the value chain. In practice, a new project leader training program was developed and initiated. The program brought project leaders from Research and project managers from Development together in one-day meetings, during which they focused on the particular characteristics and aims of each part of the process. The intent was to create a mutual understanding based on the idea that collaboration would become much easier once the participants had a thorough understanding of the specific challenges and concerns of others.

Synapse 5.2 was framed as an exercise that had the purpose of developing a closer connection between the divisions of Research and Development, and different ideas of collaboration were shaped in this context of joint project leader training. In the context of the behavior work group, some research managers proposed that creating a close connection would require developing *similar* approaches with an emphasis on project leadership. This similarity was not imagined to take the form of a "third" approach to management/leadership. Rather it implied that project managers from the Development division would simply adapt to the culture of research and leadership. The idea of adaptation was intriguing in this context. Research managers suggested that research was a distinct practice that did not easily adapt to general organizational principles. Nonetheless, some of the participants in the behavior work group suggested that other parts of Lundbeck might adapt to the culture of research. Collaboration, from the perspective of research managers, became equivalent to a kind of imposed alignment. In contrast, after Synapse, in the context of the new R&D project leader program, collaboration became a matter of "understanding differences" without

anticipating strong alignment or the merging of distinct cultures and management forms.

In the context of the post-Synapse project leader program, an idea of collaboration as "understanding differences" emerged. Collaboration was defined as resulting from a process of creating increased understanding for the particular phases of developing a drug. Project leaders and managers were enrolled in a joint course in which a considerate part of the training concerned the pharmaceutical value chain, from drug discovery to sales and marketing. A divisional director from the Development division and two members of the project leader working group (Andreas, the chief scientists and Morten, the human resource manager) developed the project leader training program. It was based on the assumption that the more information the participants gained about each other's work practices, the better they would understand each other. Eventually coordination would also become easier. I suggest that we might see this project leader training program as gradually developing a new screen for categorizing collaboration. Rather than merge and align different practices, the new screen drew on subtle classification of different work tasks and how they related to each other. The main concern was not to smooth out differences between work practices, but rather to develop mutual appreciation of those differences. I propose that, in this phase, different parts of Lundbeck were made accountable to each other while also acknowledging that collaboration did not require fusing work into one practice. We might consider this a form of intentional misaligned co-production. The project leader training program was based on an idea of co-production between Research and Development. But it was also developed from an acknowledgement of misalignment, which was different from the general operating principles and shared notions of management/leadership. In the context of the project leader training program,

misaligned co-production was intended and seen as a constructive approach to collaboration between research and development in Lundbeck.

Conclusion

This chapter has shown how various activities in Lundbeck generated screenings with implications for research, management and collaboration. First, in the context of the "Optimize External Research Collaboration" workshops, we saw a screen that differentiated external collaboration from in-house research. Then, at the coordination meeting, and in particular in the discussion of adapting to general operating principles, we saw a screen that differentiated research from non-research. Subsequently, in the behavior work group, we saw a screen that distinguished project leaders from project managers, developing a hierarchy between leaders and managers by attributing more value to leaders, and to research. Finally, in the context of the project leader training program, activities produced a screen that separated drug discovery from drug development, without simultaneously *projecting* this difference as a problem for future innovation. Thus we saw a number of screens. Many of these screens started out as rather crude screens that then gradually dissolved, as they were challenged by certain things often related to present practices. We see strategic research developing through screens that categorize, occlude and project.

This chapter also illustrated that collaboration *within* Lundbeck takes place as a form of misaligned co-production in which the research manager is seen as having a particular role. This role partly resembles the notion of project leaders seen as 'bridge builders', as developed in both Danish research policy and policy research (See Chapters 8 and 9). In policy, project leaders are expected to connect the diverse cultures of universities and companies, and in Lundbeck project

leaders are in part anticipated to play an important role in connecting the domains of Research and Development. But the idea of project leaders as bridge builders in Lundbeck also differs from the notion found in policy. As we saw, there are different types of leaders and managers in Lundbeck, not all of which are seen as connecting separate practices. Despite the Synapse projection of making connections, parts of Lundbeck are not seen as completely disconnected. There is also a sense of coherence that comes from being part of one company that we do not find in policy descriptions of science-industry relations.

Misaligned co-production was particularly visible in three cases. First, in the situation where research managers encountered general operating principles. In this situation, both Synapse and the introduced operating principles described co-production. However, the research managers challenged the idea of too strong an alignment, with reference to the distinct nature of research, which produced a misalignment within co-production. Second, in the context of the behavior work group, again Synapse and point 5.2 created an anticipation of collaboration and merging. However, the development manager resisted this idea and suggested misalignment by referring to the differences between Research and Development practices. Finally, in the context of the project leader training program, co-production was no longer seen as a matter of merging cultures, but was from the start framed as misaligned.

The finding of misaligned co-production raises questions about what the main challenges of science-industry collaboration are, both between academic and industrial researchers, and within research-based companies. 'Bridge building' might not be the best way of understanding such collaboration since the participants might not be separate in any stable or predictable way. Thus understanding these relations, and how they are managed, remains an empirical question. I now move to the conclusion of the thesis.

14. CONCLUSION

In this final chapter, I first summarize some of the main findings and conclusions of the thesis. I also discuss what I view as the main implications for academic research and practitioners such as research managers and policy makers. As part of the conclusion, I also reflect on the concepts I have used. In particular, I return to the issue of what has been the purpose and value of using screens to analyze strategic research and science-industry collaboration. What have I accomplished by focusing on disconcertment? Finally, I reflect on this PhD as illustrative of science-industry collaboration.

Main findings

The thesis "Managing Strategic Research. An empirical analysis of science-industry collaboration in a pharmaceutical company" has offered four main empirical analyses, each of which bears a different relation to the title and addresses different research questions. First, I have aimed to articulate what strategic research means in the context of science-industry collaboration. Second, I have attempted to characterize such forms of collaboration. Finally, I have tried to carefully specify some of the particular qualities and characteristics of the management of such research. The thesis gives answers to these questions that are both particular to the empirical context of Lundbeck and have more general implications.

Strategic research as an strategic-explorative activity

The thesis has shown that research is not necessarily exclusively strategic or explorative. Rather, research can *become* strategic through an explorative process. As we have seen, both in policy and academic forums, distinguishing between exploratory (basic research) and strategic (applied research) is an important implicit or explicit discussion. Therefore, identifying different research forms that do not evolve from a separation between strategic and explorative can contribute to such discussions. In Lundbeck, I have thus identified a third form of research, which we might call *strategic-explorative* research. Strategic-explorative research is characterized by projects that anticipate solutions and end goals (such as a drug) but develop in a quite open process. In strategic-explorative research, strategic implies a direction whereas explorative involves an openness that potentially changes this direction.

Science-industry collaboration as misaligned co-production

The thesis has also advanced the argument that science-industry collaboration might be seen as *misaligned co-production*. Rather than presenting the worlds of academic and industrial research as essentially different, I have shown that in practice researchers from industry and academia share networks, training, methods, and theories. This implies that collaboration, in some situations, takes place based on a strong initial alignment between the participants' research interests. Consequently, rather than seeking to align diverse cultures, co-production in these situations becomes a matter of differentiating (or misaligning) various interests and engagements. In some situations, it is important to create forms of sameness (such as that all participants are scientists) whereas in other situations, it is important to amplify difference in order to

explicate the diverse purposes for being engaged. These purposes might be developing a drug or exploring a research area with the aim of publishing new findings.

That science-industry collaboration develops through misaligned co-production has important implications for how such collaboration can be understood. Contrary to the assumptions of much policy debate and policy research, the main challenge of science-industry collaboration might not only be a matter of overcoming difference but also, at important points, of overcoming sameness. Yet, the idea of misaligned co-production does not only offer novel insight into the dynamics of science-industry collaboration *between* institutions. For as we have seen, science-industry collaboration can also be found even *within* companies and research institutions (such as Lundbeck and the Mayo Clinic). Within such institutions, collaboration also sometimes takes the form of misaligned co-production. Although, science and industry perspectives co-exist in the same institution, it can still be crucial for collaboration to articulate differences between them. Thus misaligned co-production is a notion that raises fundamental questions about what science-industry collaboration means, where it might be studied and how it might be supported by policies or managed practically.

Research management as managing emergent relations

The thesis also argues that managing strategic research is to a significant extent about the management of *emergent relations*. To manage emergent relations implies taking seriously that in strategic-explorative research, ideas and relations develop in a process, rather than being defined formally in advance. Thus, managing strategic research implies openness and timely reactions to emergent things. That these processes are open does not mean that they are without aim or

purpose. Managing emergent relations does not demand that one refrains from setting a direction or specifying an approach. Rather, it means that being strategic has to do with developing an ongoing attention to the question of how to demarcate focus areas by deciding what to ignore, while simultaneously allowing for openness, flexibility and emergence. Appendix A contains a paper which offers the notion of *adaptive frameworks* to describe an approach suitable for managing emergent relations. This notion of adaptive frameworks requires some further explanation in light of the analyses found in the thesis.

Adaptive frameworks

In an attempt to synthesize some of the main discussions and findings that developed in the process of this PhD project, Peter Høngaard Andersen (Senior Vice President at Lundbeck), Alan Irwin (Professor and Research Dean at Copenhagen Business School) and I, wrote a paper together (Vedel et al. 2013, see Appendix A). We gave ourselves the challenge of formulating ideas that had developed in our joint discussions for a general audience. The paper was eventually published by *Nature Reviews Drug Discovery*, a journal that suited this purpose as it had a pharmaceutical research audience, was open to cross-disciplinary collaboration, and published short pieces. In the paper, we defined 'adaptive frameworks' as an approach to research management that implies structure and direction setting but at the same time a sensitivity and responsiveness to what might develop in the research process. This idea, we thought, captured some of the main issues of managing strategic research, primarily for an industry and policy audience.

Although this paper was an extension from the thesis rather than an integral part of the data collection it now seems to me to have further potential. 'Adaptive

frameworks' is not only a concept for practitioners but also potentially a fruitful way of describing the management of strategic research to a social science audience. It focuses attention on the basic condition of change in research while also insisting on the need to make strategies that attempt to give research activities a particular scope. Below I return to the notion of adaptive frameworks as I consider both implications for future academic research and for practitioners of the thesis.

Screens

Throughout the thesis, I have worked with an analytical framework based on the concept of screens. Thus I have used screens as conceptual tools for describing empirical socio-technical constructions of strategic research, science-industry collaboration and research management. In particular, I have identified three forms of screens and processes of screening: a projecting screen, a categorizing screen and an occluding screen. Using this framework, throughout the thesis I have identified different things *as* screens.

Some of these screens have been quite obviously material. For instance, I have suggested that a transfer document might be understood in terms of a screen that *categorizes* research consultants and researchers as two distinct parties in a collaboration, although in practice these categories collapse. I have also described collaboration contracts as screens. I suggested that contracts present collaboration by categorizing the participants in a particular way. For instance, 'consultants collaborating with scientists' is a categorization that addresses one dimension of the collaboration, while *occluding* others. These are just two examples of the material screens I have examined.

Other screens, however, have a more discursive form. For instance, I have suggested that the idea of a double payment that emerged during a somewhat disconcerting conversation produced a particular form of screen. Viewed as a screen the notion of a double payment *categorizes* Lundbeck and the Lundbeck Foundation as collapsed entities, while *occluding* a formal separation of the two institutions. This is also to say that though this screen is apparently discursive, it is still not *merely* discursive, since it also refers to formal organizational structures. It is material-semiotic, in the sense of Donna Haraway (1997).

But I have also argued that more complex arrangements, events and activities might be analyzed in terms of screens and screenings. For example, I argued that a meeting arrangement could operate as a screening device. This is how I approached the first encounter between Lundbeck and the Mayo Clinic. In this setting, the conjunction of many details together produced a screen that categorized investors and scientists as inherently different. This meeting generated both discursive and material screens (though all in the end are both at once). For example, statements such as "this is what we got, what do you want?" separated investors from scientists. Likewise, the material arrangement and temporal organization of the meeting worked to screen and differentiate between investor-guests and scientist-hosts. Likewise, within Lundbeck we have seen arrangements such as the organization of temporary working and management groups that have also categorized Lundbeck employees and Lundbeck practices in particular ways with implications for interaction and strategy making. In this way, I have identified documents, talk and organization/arrangements as screens.

It is my experience with these somewhat experimental analyses that it is relatively more easy to deal with obviously material things such as documents and contracts as screens. This is because it is easier to delimit and pinpoint just *what* it is that can be seen as a screen in these cases. Analytically, it is also

somewhat easier to be specific about what these objects *do* as screens. In addition to observing what these documents do in practice situations, the materiality of them makes it possible to take them home with you, sit with them and reflect on how and what they categorize, occlude or project.

In contrast, more discursive things are relatively hard to handle analytically as screens. It is more difficult to be specific about their screening qualities and often they appear interesting only as part of a particular composition of things. Again the first encounter with the Mayo Clinic serves as a good example. Here, the screen was not a document or a single expression but rather a set of circumstances. The screen was comprised by a mixture of the participants' attitudes, by how they were arranged in the room, and by what they said and how they acted. This situation is more difficult to analyze as a screen *but not less interesting.*

In fact, I would suggest that although the notion of screens might appear to be somewhat stretched when it is used to look at obviously discourse focused events like meetings, such events are potentially what it is *most interesting* to study. Indeed, looking at meetings and encounters opens up a lot of explicit articulation work related not only to describing and planning research but also to defining the participants' engagements and roles.

It is particularly interesting to examine the processes whereby screens change. For instance, I have offered examples of how researchers in Lundbeck encountered specific categorizations of them as investors or industrialists. At certain times, these categorizations were welcomed; indeed they were important for making what I referred to as misaligned co-production. But at other times, these categorizations prevented deep engagement in new research areas. Thus, I have also discussed how Lundbeck researchers attempted to change categorizing screens by acting or talking in a way that stimulated different forms of

classification. Indeed, we have come across some very active forms of screen change such as the adoption of cultural codes, like putting on a creased shirt or asking technical questions when in the company of academics. This goes to show that screens are not always passively accepted but that people engage in activities to change them.

Strengths and weaknesses of the notion of screens

The main strength of the concept of screens is its conceptual flexibility. This has allowed me to conceive science-industry collaboration in a new way. Thus, focusing on screens has allowed me to consider the simultaneous co-production of sameness and difference in science-industry collaboration. In particular, screens offered a framework for exploring constructions of relations, without in advance setting up expectations of finding either sameness or difference. My intent was thus to avoid both the prevalent idea that science and industry are separate domains and the opposite, that such collaborations either presume or create similarity. The analytical tool of screens thus offered a flexible way of rethinking the dynamics of science-industry collaboration by characterizing how *various* constructions make collaboration possible.

This flexibility made the analytical framework of screens adaptable to my empirical context but potentially it also makes it transferable to other contexts. Although I have emphasized categorizing, occluding and projecting screens, in other contexts different screens may be relevant, and the ones I have identified less so. My main point is simply that the framework of screens is itself potentially flexible and adaptable to diverse contexts.

However, this flexibility is also, as I see it, the central potential weakness of this analytical tool. Thus it might be objected that the very flexibility of screens is a

consequence of not using the concept with sufficient clarity, precision and consistency. In future research, I therefore believe it will be both interesting and valuable to explore more specific aspects of screens. This might, for instance, entail looking at *specific* objects in terms of *particular* screens. This could be done by studying collaboration contracts only, and in detail, as categorizing screens. It might also entail studying the analytical phenomenon of intentional screen change, as described above. In this sense, narrowing and specifying its analytical and empirical areas of application may enhance the concept of screens. In this thesis, however, my main purpose was to develop and test the framework of screens and to see what new insights this might give in terms of understanding strategic research, science-industry collaboration and research management.

Disconcertment

Disconcertment was one of the central methodological tools with which I have explored strategic research in Lundbeck. Disconcertment was used as a device for selecting particular cases and situations for analysis. It has also worked as an organizing tool that has given certain events and accounts a prominent role in the thesis. Hence, the thesis has offered more accounts of surprising or unusual situations than descriptions of what researchers do when they normally do research in Lundbeck.

As part of this process, I have identified different forms of disconcertment. First, there is the researchers' disconcertment, which is what Verran encourages us to take seriously (Verran 2001). The researcher encounters surprising things in the field though they are not necessarily surprising to people in the field. Second, I have pointed to public forms of disconcertment in which the researcher *and* people in the field share a moment of disconcertment. After the event, there might

be different interpretations of what *caused* the disconcertment but this form of disconcertment is characterized by a shared sense that something was wrong. Finally, I have identified a third form of disconcertment, which took the form of people in the field retelling stories about past disconcerting events. This is neither the researcher's disconcertment nor a shared form of disconcertment. The critical moment of disconcertment has passed (it might even be years ago) but it is still vividly recalled because it illustrated something important about what went on in a particular situation.

Disconcertment is an interesting methodological tool because it addresses situations where things are out of order. Exploring these moments thus gives us a key to understanding what that order is assumed to be. For instance, in the case of the Mayo Clinic, I would not have been able to see that there was something wrong with the meeting situation unless the participants that I later interviewed had described their disconcertment. Indeed, it seemed perfectly reasonable to expect that research managers from Lundbeck would be interested in presentations of ongoing research at the Mayo Clinic. Yet, exploring this recalled disconcertment revealed that the Lundbeck research managers at this particular point in the collaboration did not see themselves as investors but in fact as researchers. To make the collaboration possible, it was crucial to change this perception. Thus, disconcertment works as a tool for exploring practices and embedded assumptions *through* the unusual.

As a tool, disconcertment can be used in the three ways identified in the thesis: developing sensitivity to one's own disconcertment, to public shared disconcertment and to informants' reflections on past disconcerting moments. For future research it appears to me particularly promising to further explore the following question: What makes 'a good case' for exploring disconcertment? First, it seems that using disconcertment requires getting quite close to what one

studies. Being invited to explore situations where things are in the making, uncertain, or already gone wrong requires establishing some kind of trust that it takes time to build. In my case, being employed at Lundbeck made this easier but still it required engaging actively in the field over a long period of time. Second, there are potentially many disconcerting things to analyze. Sorting out which disconcerting things say something specifically important about the research topic requires familiarity with the field. Thus disconcertment seems to require longitudinal ethnographic studies.

It seems to me that the public shared forms are highly interesting and strong cases of disconcertment. Again the meeting between Lundbeck and the Mayo Clinic researchers illustrates this. Here a large group of participants argued that something was wrong in this meeting. Though they gave quite different explanations of *what* was wrong the fact that they all pointed to the event as unusual made it seem of obvious interest. In contrast, it seemed less useful to explore in depth the disconcertment that a single research manager expressed about the Synapse process. Rather than suggesting something generally significant about science-industry collaboration, this disconcertment could, in hindsight, just as well say something particular about this research manager's uncertainty about particular processes of change. Thus, I propose that studying disconcertment is particularly relevant when the empirical material allows exploring more public and collective forms of disconcertment. These public forms potentially reveal more important aspects of the phenomenon of strategic research as a collective practice than more private forms of disconcertment.

Implications for future academic work

The thesis contributes to studies of science-industry collaboration, both in STS and in the field of research policy. Centrally, I have argued that since many analytical frameworks are based on the presumption that the main challenge of such collaboration is to overcome difference, careful analysis of what such difference actually consists in, is very important. The thesis has given some inputs to what characterizes such collaborative difference. It has shown that in many situations there are *already* overlaps and coinciding interests, so that making difference between the aims and roles of the participants is in fact what organizes and constitutes collaboration. This insight offers a further set of opportunities for future research.

The thesis has drawn on a number of concepts developed in studies of scientific collaboration within universities among academic researchers from diverse disciplines (for instance, Galison 1996). These studies describe not so much science-industry differences as differences *between* disciplines and *between* scientific cultures. An interesting question for future research would consequently be: What might such studies of scientific collaboration learn from studies of science-industry collaboration? Do the participants in scientific collaborations also actively differentiate themselves from each other? Might we even find situations where disciplinary affiliation does not matter much and where other forms of sameness and differentiation become more important?

The thesis also offers insight into research management. As we have seen, what characterizes research management is related to how research and collaboration are screened. If strategic research develops from emergent relations, and if science and industry are not clearly separate domains, the management of research is not mainly a matter of 'bridge building'. Rather, managing research becomes a question of dealing with risk, uncertainty and unstable relationships,

and about making important links and differentiations. For future research, it would be interesting to explore the management of research in terms of such 'adaptive frameworks' and their requirements (Vedel et al. 2013). Although we developed this term in the context of dissemination to practitioners, it might also be interesting for the academic study of research management, not simply, that is, as an *administrative* management practice but as a *creative* practice for integrating research content and context.

Implications for practitioners

From the outset this thesis had the ambition not only of speaking to academic concerns but also to those of practitioners. In this endeavor, I think of this thesis as a form of interventionist and "serviceable STS" (Webster 2007: 459) that aims at not only "deconstructing" but also "shaping" policies (ibid. 462-463). Andrew Webster suggests that there are several "intervention spaces" for STS research, among these 'the characterization and anticipation of emerging technoscience fields', 'the exploitation of (future) technoscience', and 'the context of use of technoscience applications' (ibid. 462). I would add to these a new intervention space that might be called: 'basic assumptions about strategic research and science-industry collaboration'. Within this intervention space, the thesis has practical implications for both policy makers and research managers.

Implications for research managers

For practitioners, one of the main outcomes of this thesis is the paper "Externalizing research through adaptive frameworks". The paper addresses a practitioner audience (readers of *Nature Review Drug Discovery*), it was co-authored with a practitioner and it offers recommendations for how to work with

adaptive frameworks. These recommendations draw attention to what an adaptive framework approach implies. It implies developing more flexible ideas about what collaboration is (beyond "cash and carry") and specific contractual frameworks based on these.

The notion of adaptive frameworks addresses the practical challenges of managing strategic research and science-industry collaborations. As we have seen, these relate to an inbuilt tension in managing strategic research. On the one hand, managing strategic research implies stabilizing certain research by arguing for its potential relevance and value. On the other hand, it implies openness and allowing the research to destabilize these initial conceptions of relevance and value. Thus in practice managing strategic research means arguing for the value and potential of research one minute and allowing plans and frameworks to change in the next.

In relation to developing contractual frameworks deploying an adaptive framework approach implies careful interrogation of what needs to be settled in advance and what might be left open in science-industry collaboration. For instance, while intellectual property rights and financial arrangements might have to be settled up-front, scientific content might be allowed for to develop in the course of the collaboration. Of course, this does not leave everything open, since content develops in relation to certain defined ambitions and questions. But how these ambitions and questions are left rather open and settled in the process.

We also draw attention to the fact that although developing an adaptive framework approach might seem easy it nonetheless entails considerable organizational challenge. It requires that research managers have both technical skills and experience with research. In addition, communicating an adaptive framework approach to the organization might be quite hard, since it is premised on an in-built and productive tension between stabilizing things and keeping

them open. It is particularly challenging when this approach meets other organizational frameworks that aim at settling things. In Lundbeck, this was illustrated not least by the research managers' general skepticism about "bringing in the lawyers" to solve collaboration problems. In any case, it would be interesting to develop the notion of adaptive frameworks further, especially in terms of generating even more specific recommendations for an audience of practitioners.

Implications for policy makers

Finally, let me consider the implications of my findings for policy. In recent years, a particular form of strategic research has emerged as increasingly important in Danish research policy. New strategies and an ongoing reorganization of the Danish research advisory and funding system have put increasing emphasis on treating strategic research on its own terms and developing it in terms of demand-driven innovation. This suggests that strategic research continues to be seen as separate from basic ("free") research and based on quite specific requests articulated by politicians and companies. It is based on the assumption that it is possible to predict future societal and industrial needs for research and shape public research funding processes accordingly.

This thesis has developed quite a different account of strategic research. Where strategic research in contemporary Danish policy debates is seen as a predictable, valuable, and goal-oriented form of research, this thesis has offered a description of strategic research in which direction and value, at least to some extent, develop in the process of collaboration. These findings might stimulate a more careful discussion about what strategic research is, what makes research strategic valuable, and how companies in fact identify their needs and strategies.

In relation to this, it is central to emphasize that the outcomes of strategic-explorative research are not potentially *less* useful or *less* relevant than outcomes of a more restricted form of strategic research. Indeed, the opposite might well be the case, as strategic-explorative research (built on adaptive frameworks) implies an inbuilt alertness to potentially *useful* and *relevant* opportunities that are not predictable in advance. Thus we might not think of strategic-explorative research as potentially less capable of addressing societal or industrial problems but in fact as a more reflexive and relevant approach to both identifying and working with such problems.

In this regard, one particular concern relates to the kind of research projects that might receive funding within a public policy framework of strategic research based on demand-driven innovation. Compared to a private corporate context, where research projects can be initiated and ended with flexibility there is a different accountability related to public research funding. There is a demand for receiving a coherent case for research rather than mainly elucidated opportunities. However, even if we take this difference into account, we might imagine the emphasis on demand-driven innovation would imply prioritizing projects that clearly address a demand, anticipate a solution and define crucial steps to reach this end. Consequently, we might also imagine that research projects that describe a direction but otherwise leave the research process more open will have more difficulty in receiving funding. This development of strategic research is problematic because it overlooks a great strategic potential in research in the attempt of controlling it.

Another issue is how funded public funding agencies will manage and approach funded strategic research projects. To what extent are the receivers of research funds accountable within predefined research agendas? To stimulate discussion on these questions, adaptive frameworks might again be useful, also in this policy

context. If one considers the societal problems and the demands or needs for research defined by politicians and companies in terms of adaptive frameworks, a relevant policy approach to managing public research would need to be sufficiently flexible to allow strategic research projects to change direction, or even reconsider the overall problem or demand.

Possibly, it is more crucial *how* publicly funded strategic research projects are managed than the overall terminology that is used to describe them, for example, strategic research, advanced technology or demand-driven innovation. With this concern in mind, the main challenge is to introduce a conception of strategic research that acknowledges strategic-explorative research. This is challenging because in a policy context that relies on crude categorizations of research this interpretation of strategic research might be understood as vague or even lacking firm policy.

The thesis proposes a rethink of strategic research. In terms of policymaking it implies a request for *flexibility* in public research. It might eventually be relevant to think in terms of a re-categorization of basic and strategic research in Danish research policy. But in the short term it is much more important to carefully consider the ways in which strategic research is conceptualized and managed.

Final reflections

This research project was funded as part of the Danish scheme of industrial PhDs. It is thus interesting to reflexively consider this scheme of PhD as an example of science-industry collaboration, which is also the topic of the thesis. As a policy instrument, the industrial PhD was introduced to stimulate interaction between universities and companies and to educate researchers at the doctoral level who would also gain particular insight in industrial research. A recurrent criticism of

this scheme is that the industrial relevance necessarily comes at the expense of academic depth. This relates to a broader debate in Denmark in which it is often suggested that researchers who work with industry have a more applied perspective, which comes at the cost of intellectual sophistication.

However, I would suggest that this does not necessarily have to be the case, and I hope the thesis proves this point. As I have shown throughout the thesis, it is possible for researchers employed by a company to engage in highly academic research. Likewise, I have not experienced my collaboration with research managers in Lundbeck as limiting my academic development. Though I shared with research managers an interest in practical questions about science-industry collaboration and research management, in no way was I forced to explore particularly practical questions in particularly applied ways. Rather different outcomes developed, some of which took the form of recommendations and practical input, and others the form of academic publications. On a personal note, I have found this form of science-industry collaboration extremely rewarding.

15. APPENDIX A: VEDEL ET AL. 2013

COMMENT

Externalizing research through adaptive frameworks

Jane Bjørn Vedel, Alan Irwin and Peter Høngaard Andersen

Adaptive approaches to collaborations between industry and academic research institutions can enable both parties to achieve their goals more effectively. Here, we discuss our experience with such approaches and suggest recommendations for addressing the associated management challenges.

Collaborations with academic research institutions are an important component of the research and development (R&D) strategies of many pharmaceutical and biotechnology companies. The potential value of gaining access to a wider range of innovative research than would be achievable through internal R&D alone is a strong factor in favour of such collaborations. However, widespread assumptions about the differing organizational cultures in industry and academia mean that these collaborations are often perceived as being more risky than internal research. Here, drawing on our experience over the past decade and on research focusing on the specific challenges of externalizing research, we provide our recommendations for external research collaborations based on an adaptive framework model that is characterized by an open attitude and a willingness to embrace perceived risk.

Traditional collaboration models

There are numerous approaches to externalizing research, the most basic being known generally as the 'cash and carry' model. In this model, the academic partner receives funding in return for providing an answer to a question defined by the industry partner. However, this model ignores the fact that, in many cases, collaboration is based on a mutual generation of research ideas. Attempts to introduce a more collaborative aspect to externalized research have led to the adoption of various approaches that can broadly be categorized into two types: the alignment model and the partnership model.

The alignment model sees the differing cultures of academia and industry as the main challenge to collaboration. Consequently, it assumes that collaboration requires the alignment of these differences and seeks to mediate them through a focus on 'bridging the gap'[1]. This model is limited by the fact that it is blind both to potential overlaps between the collaborating parties and also to situations in which differences are sources of creativity.

By contrast, the partnership model assumes that academic institutions and industry are already in a 'partnership' and working towards consensus and common goals[2]. The partnership model has the opposite problem to the alignment model, however, in that it focuses all its attention on a presumed consensus and so pays little attention to emergent and varying differences in the collaborators' interests and incentives.

A key problem with both of these traditional models is that they draw an over-generalized picture of university–industry collaboration and its related challenges. In different ways, both models try to settle the interests of the collaborating partners upfront, rather than treating each collaboration as unique and evolving. The parties in a collaboration, whether from academia or industry, may indeed maintain very different backgrounds and incentives for entering a collaboration without seeking to align these[1].

The 'adaptive framework' model

Born out of a desire to understand the underlying biological mechanisms of brain diseases through basic research, the adaptive framework model practised at Lundbeck aims to more effectively accommodate environmental changes — such as scientific discoveries, changes in the commercial environment or competitor news — that happen during the course of a collaboration.

An adaptive framework approach has the following key elements: a relatively open relationship between the parties that is responsive to new developments; an acceptance that there will be considerable uncertainty in the development of relevant research; and a commitment to the co-production of research where both parties contribute to the development of innovative ideas. It is challenging in that it requires a substantial shift in management style to move beyond the definition of clear tasks and the fixed employer–employee relationship of the traditional 'cash and carry' model. Consequently, managing

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doi: 10.1038/nrd4138

research involves both continuously revising the focus and content of the collaboration, and also accommodating potentially diverging and shifting perspectives and interests. Importantly, the adaptive framework model sees continuous change and evolving interests as opportunities rather than problems to be overcome.

In our experience, the ability to react to research breakthroughs as well as organizational and market changes makes it possible to capitalize more effectively on the commercial potential of collaborations. Based on this experience, the following key points are recommended as a starting point for developing external research collaborations:

- Define the individual collaborations within a broader strategic context — collaborations are not isolated research projects but part of a larger long-term vision
- Delegate power — externalizing research implies a loss of direct control and therefore requires alternative ways of managing, motivating and setting directions while allowing change
- Define a plan for the implementation of collaboration outputs
- Define intellectual property rights upfront but allow specific research content to develop in the process
- Define plans and governance structure in a way that allows change and timely decision-making
- Establish contractual frameworks that take into account both research and business perspectives
- Externalizing research is about relationship building; therefore, successful planning and implementation of governance structures depends on continuous attention to personal relationships
- Adaptive frameworks require constant, close attention to the research and how it develops

Reflections

Externalizing research using an adaptive framework model raises challenges for research managers. It implies a form of research management that operates creatively and flexibly with a high degree of tolerance for uncertainty and change. Implementing an adaptive approach entails a 'double risk', as it responds to a situation of increased risk (externalized research) with an open approach rather than increasing control. As this approach is demanding for research managers and their organizations, it may not succeed in all companies but will depend on various internal and external factors: for example, the specific experiences of the managers and the scientific expertise of the parties involved in the external collaborations. Nonetheless, we consider that developing adaptive frameworks is a promising way for companies to approach and benefit from external research collaborations.

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Acknowledgements
This article was developed from an innovative collaboration between P.H.A. of Lundbeck and J.B.V. and A.I. of Copenhagen Business School. The authors would like to acknowledge M. Loughlin of Inspired Science who provided medical writing support.

Competing financial interests
The authors declare competing financial interests: see Web version for details.

16. APPENDIX B: LIST OF ACRONYMS

Acronyms	Full form
In Danish research policy	
ATV	Danish Academy of Technical Sciences In Danish: Akademiet for de Tekniske Videnskaber (hence ATV)
DASTI	The Danish Agency for Science, Technology and Higher Education (DASTI) In Danish: Styrelsen for Forskning og Innovation
DCRP	The Danish Council for Research Policy
DCSD	Danish Committee on Scientific Dishonesty
DFF	The Danish Council for Free Research In Danish: Det Fri Forskningsråd (hence DFF)
DNATF	The Danish National Advanced Technology Foundation In Danish: Højteknologifonden
DNRF	The Danish National Research Foundation
DSF	The Danish Council for Strategic Research In Danish: Det Strategiske Forskningsråd (hence DSF)
RTI	The Danish Council for Technology and Innovation In Danish: Rådet for Teknologi og Innovation (hence RTI)
In Lundbeck	
BCG	Boston Consulting Group
CNS	The central nervous system

DDMT	The Drug Discovery Management Team
R&DMB	The Research and Development Management Board
RMB	The Research Management Board

17. APPENDIX C: LIST OF ILLUSTRATIONS

18. APPENDIX D: LIST OF KEY PEOPLE/PSEUDONYMS

Pseudonym	Age	Position
Lundbeck		
Jens	55	Head of research/senior research manager
Lars	50	Head of drug discovery (DK)
Jørgen	65	Head of the behavior working group
Hans	50	Divisional director
Anne	45	Divisional director
Niels	45	Divisional director
Trine	40	Department manager
Jesper	45	Head of section/research manager
Thomas	45	Head of section/research manager
Andreas	45	Chief scientist/chemist
Morten	40	Human resource manager
Tanja	45	Development manager
Hanne	55	Human resource manager
Birgitte	45	Divisional director, Development
Richard	50	Head of drug discovery (US)
Jonathan – Jon	45	Chief scientist/chemist (US)
Elena	50	Human resource manager (US)
William – Will	40	Business developer (US)
Irene	55	Divisional director (US)
The neurocell collaboration		
Martin	50	Researcher at a university/co-founder of PsychoIndex
Søren	55	Researcher at a university
Henrik	50	Co-founder of PsychoIndex

The Mayo Clinic		
Adam	55	Department manager
Geraldo – Jerry	45	Principal investigator
Laura	40	Principal investigator
David - Dave	45	Principal investigator
Nancy	50	Licensing manager
Margaret	50	Technology development liaison officer

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