Tobias Fridholm

Working Together
Exploring Relational Tensions in Swedish Academia
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Abstract

This study explores the basic social conditions for high-quality university research, and focuses on research in science and technology in Sweden. Swedish research policy has adopted more of a market perspective on academic research and its role in society. This has meant the promotion of competition between researchers, increased focus on efficiency at universities, and attempts to make academia harmonize more with industry and other actors. How do such policies affect the variety of perspectives within the academic system? How do they affect the positions and identities of individual academics? These issues are discussed through the concept of "relational tensions". Relational tensions refer to social strains arising when interacting actors have different perspectives. Relational tensions can stimulate creativity, but may also cause unproductive conflicts. The discussion is underpinned by interviews with university researchers and a case study of Uppsala BIO-X, a program to commercialize university research in biotechnology. Typical cases of relational tensions are identified. These concern both interpersonal relations and differences between organized science and industry. A notable observation concerns potential frustration of individual academics, as competition and efficiency tends to make their positions and identities more contested. Researchers cope with relational tensions in three identified ways: socialization, seclusion, and lateral authority. Socialization is natural and often necessary, but reduces the variety of perspectives. Seclusion serves to retain variety and independence, but reduces interaction with others. Lateral authority is to formally or informally lend a researcher more authority, which improves the chance of maintaining a variety of perspectives without reducing interaction. The sustained usefulness of academic research arguably depends on its ability to foster and communicate a variety of perspectives. Hence, (i) promoting lateral authority seems fruitful within academia and in relations between academia and industry, and (ii) encouraging competition and efficiency may to some extent be counterproductive.

Keywords: Economic geography, Relational tensions, Academic research, Academic entrepreneurship, Research commercialization, Creativity, Creative milieus, Innovation, Innovation systems, Trust, Authority, Research policy, Triple helix, Biotechnology, Swedish research, Universities, Expertise, Professionalism, Professions, Research collaboration, Collaborations, Research funding

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1. Introduction

This study concerns the basic social conditions for scientific research. The point of the departure is the observation that Swedish research policy during the last two decades has adopted more of a market perspective on science and its role in society. The value of Swedish academic research is largely defined by how it harmonizes with the perceived interests of industry and other actors in society. At the same time, performance in academic milieus and among researchers is now more than ever considered in terms of competitiveness.

The shifting research policy addresses important problems. The performance of the Swedish economy is dependent on the ability of Swedish industry to stay innovative, and that ability partly rests on the quality and relevance of inputs from academia. Following citation indicators, however, the scientific quality of Swedish academia has for a long time developed relatively poorly.

Against this background, Swedish governments have taken a number of measures to increase the quality and relevance of Swedish academic research in recent years. Examples include encouraging universities to coordinate their research activities; implementing ways to align academic research with the interests of industry and other actors in society; and promoting elite research by, for instance, encouraging competition between researchers and introducing so-called “Centres of Excellence”.

However, this might introduce certain problems. Individual academic researchers might become frustrated if their positions become more contested: they might feel squeezed between the interests of organized science and of industry, and increased competition tends to imply less room for divergent perspectives on an individual level. The latter is likely to lead to less variety in the research system, which might be negative for the economic system in the long run and which might adversely affect potentially ground-breaking elite research.

This study addresses these problems by exploring the basic social conditions for high-quality university research, both in academia and in the interface between academia and industry. The discussion is underpinned and illustrated by inputs from interviews with well-established researchers at Swedish universities and from a case study of a “triple helix” initiative to commercialize university research in biotechnology, Uppsala BIO-X.
1.1 Background

In September 2008 the Swedish government presented the research and innovation bill for the years 2009 to 2012. The allocations to universities would increase by 5 billion SEK, which according to the government was “by far the largest increase of resources for Swedish research that have ever been presented at one single occasion” (Prop. 2008/09:50:22, author’s translation). Around 1.8 of the 5 billion SEK were earmarked for research fields observed as strategic for, in particular, Swedish industry and health care. Although the content of the bill was criticized from several directions, the size of the allocation was applauded more or less in unison by professional experts and industrial representatives. Moreover, the increased investment in research was, according to Ministers Björklund and Leijonborg (2008, author’s translation), “the most expensive reform” their government intended to make. Clearly, this was an occasion to celebrate.

The story is notable in two ways. First, it is apparent that many actors find the allocation of significant financial resources to university research to be of great importance even when other publicly funded activities, such as health care and elderly care, are struggling financially. Second, this was the first time a bill for research policy was labeled “research and innovation bill” instead of “research bill” as earlier. In other words, it seems that research at Swedish universities, which not long ago was a fairly anonymous activity, is now viewed as being of significant societal interest. It also seems that the hope is not just that Swedish research will move the scientific frontier but that it will substantially bolster Swedish industry and health care.

This research policy goes back to the observation that the sustained competitiveness of high-cost economies – and hence their capacities to fund public welfare and keep down unemployment – largely depends on their capacities to innovate (Schumpeter 1968 [1934], Nelson and Winter 1982, Lundvall 1992, Cooke 2002, Fagerberg 2005). High-cost economies, such as Sweden, typically create their wealth on global markets characterized by monopolistic competition. Success on such markets relies on firms offering products that their customers find qualitatively better – and as a consequence, to some extent different – than the products of competitors. Thus, although the individual firm wants to stabilize its position in a market, the position is constantly destabilized by a changing environment. Each firm therefore has to look ahead, constantly preparing new or enhanced products (Porter 1997, Scott 1998, 2006). In addition, the uncertainty characterizing many sectors has made many large firms – for example, in biotechnology and ICT – unwilling to make long-term investments in in-house research (Lazonick and O’Sullivan 2000, Chesbrough 2003, Cooke 2005). Utilizing novel findings in academic research and harnessing creative friction in discussions with academic researchers has in that way become a potentially useful strategy for many firms, and as such of interest to governments.
At the same time, research policies are, of course, not straightforwardly dictated by voters and other actors in society; such policies reflect an ideological perspective among decision-makers regarding the utility of research and how the utility might be supported. The governmental views of what makes research useful for society has shifted over time, in similar patterns across the Western world. In the 1800s, educating knowledgeable individuals, who would then explore humanity and the world, was an important goal. After the Second World War, the role that academic research could play in pushing forward technological development was emphasized more. And during the last decades, research policies have shifted toward a market perspective, in which interaction, coordination, and performance have been emphasized (Elzinga and Jamison 1995, Delanty 2001). This is well reflected in Lars Leijonborg’s, then Minister for Higher Education and Research, comment in the press release for the above-mentioned research bill. The comment is here presented in its entirety:

“Knowledge is about to become the most important factor for competitiveness. A country that falls behind regarding knowledge and innovation might get considerable difficulties. That is the first reason why we are making the biggest investment ever in Swedish research. The second reason is that we want to contribute to solutions to great problems that humanity faces, such as the climate issue and cures or mitigations for severe diseases. … But more money is not enough. Swedish research has also been hampered by a number of system failures. By being consistent in upholding quality and competition, we provide the conditions needed for Swedish researchers to become the very best” (Ministry of Education and Research 2008, author’s translation).

However, a market view of academic research introduces two kinds of tensions. First, it cannot be assumed that problems interesting to organized academia and those identified by governments or industrial actors overlap. Second, this view generally puts the individual academic in a somewhat more contested position – she often gets less room to influence the direction of her research, and her performance is evaluated more frequently. A market perspective of academia implies that all research problems are not equally worth exploring – and as a consequence, that all research skills are not equally valued – and that what is valuable is largely defined by others than the labeled academic expert (cf Stehr 1994, Alvesson 2004, Sennett 2008).

The individual academic might thus sense that her role and identity is a bit challenged. This goes back to knowledge fundamentally being about embodied human capacities and conceptualizations of the world, including the self-identity (Lave and Wenger 1991). Anytime an individual has to adapt to the external world she has to negotiate her identity. If individuals are encouraged to focus on the same types of problems, or work in the same types of contexts, they have to develop shared ground for interaction. It is
thus likely that collaborating individuals over time will become more similar (Berger and Luckmann 1966).

As a consequence, contemporary strategies to make academia adapt to a “knowledge-based economy” introduce two potential problems. First, if researchers become more similar to one another, there is a risk that the variety of perspectives and projects in research decreases, which might reduce the chance for innovative research in the future (March 1991, cf Beunza and Stark 2004). Second, the individual researcher might find her contested identity and position frustrating, and for that reason create destructive conflicts, lose engagement, or withdraw from the public discussion (Sennett 2008).

This discussion is highly relevant to economic geography, which is much concerned with the knowledge-based economy. However, as has been indicated elsewhere (Allen 2002, Amin and Cohendet 2004, Ibert 2010), it is common among economic geographers who study knowledge creation to use the different perspectives of interacting actors – the relational distance between them – as a theoretical starting point and to focus on how to create more common ground for fruitful interaction, relational proximity. As a consequence, the issues of how to sustain variety and potential creative friction between different perspectives – the conditions for maintaining a productive relational tension over time – tend to be overlooked. In addition, the position and interests of the individual in the organization are often overlooked as well (Ettlinger 2003, Grabher and Ibert 2006, Faulconbridge and Muzio 2007). Thus, relatively little attention has been paid to how the issues of relational distance and proximity are handled by interacting individual academic researchers.

1.2 Aim and research questions

Against that background, the aim of this study is to explore basic social conditions for high-quality university research. To reduce complexity, the study is limited to academic research in science and technology in Sweden. Focus is on individual academic researchers. These are studied in relation to two organized social systems: academia and industry.

The study is guided by three sets of research questions, which all concern the position of the individual academic in a social context. The first set concerns the fundamentals of productive collaborations.

1. What makes research collaborations work? In what ways do research collaborations within academia differ from research collaborations between academics and industry?
The second set focuses on the position of the individual academic researcher within the social contexts of academia and the interface between academia and industry.

2. What makes a researcher a legitimate participant in academia? What makes a researcher a legitimate participant in the interface between academia and industry?

The third set of questions concerns a focus of the study: relational tensions, which refers to tensions caused by different perspectives in the interaction between an individual academic and another actor.

3. In which situations might academic researchers experience relational tensions? How are relational tensions handled by academic researchers?

1.3 Outline

The remainder of the study is presented in eight chapters. Chapter two presents the theoretical framework. The chapter is based on the observation that the study concerns relations on two dimensions: relations between individuals and relations between organized social systems – in this case, organized academia and industry. The chapter begins by observing that knowledge creation is fundamentally about the performance of knowledgeable individuals; it continues by discussing how the perspectives of individuals are shaped by social interaction. The following two sections present principles that structure organized science and science-based industries. The remaining part of the chapter concerns relational tensions in contexts of knowledge creation and how relational tension has been discussed within economic geography.

In chapter three, the context of Swedish academic research is discussed. This context is primarily the result of governmental research policies. The chapter begins by briefly introducing Swedish research policies up until the early 1990s, pointing to the differing conceptualizations of the utility of research. The main part of the chapter concerns the contemporary research system. During this period, policies on academia have promoted efficiency, competitive performance, and alignment with industry and other actors in society. New forms of governance – including a range of new funding bodies and new types of funding such as “Centres of Excellence” – have been introduced and are presented in the second part of the chapter.

Chapter four presents the method and empirical material as well as methodological considerations. The dissertation is based on two studies. The first concerns well-established university researchers in Sweden and is main-
ly based on interviews with individual researchers. The interviewees were structurally sampled from a population consisting of all researchers named as “principal investigators” in any of 110 “Centres of Excellence” funded by Swedish public research funders between 2003 and 2007. The second study concerns a specific so-called “triple-helix” program, Uppsala BIO-X, which is aimed at commercializing university research in biotechnology. That study is based on document studies and interviews with key individuals both in management and in the two main projects funded within the program.

Chapters five to eight comprise the empirical part of the study. Chapter five concerns the fundament of fruitful research collaborations within academia – depicted both in the ways interviewees depict “the good research environment” and fruitful cross-disciplinary basic research – as well as across the interface between academia and industry. Lateral authority is observed as important, as it keeps researchers motivated and supports productive creative friction. A potentially negative aspect concerns socialization, which is both natural and often necessary to create sufficient common ground, but which reduces relational distance between participants. Self-reflection, however, might diminish the negative effects of socialization. The main conclusion is that individual researchers might perform better if they have a relatively high degree of control over their personal time and space.

Chapters six and seven are similarly structured. Chapter six explores relational tensions within organized academia, while chapter seven concerns relational tensions in the interface between academia and industry. In both chapters, focus is on relational tensions that are largely unproductive.

Chapter six begins with observations about what makes individuals legitimate participants in academic spheres and continues by identifying relational tensions that interviewees have experienced within their scientific fields, their universities, and with regard to directed funding. The key discussion in the chapter concerns strategies to cope with relational tensions. Three approaches are observed: socialization, lateral authority, and seclusion. The former reduces relational distance, while the latter two provide better opportunities for a sustained relational distance – in the case of lateral authority, by maintaining interaction; in the case of seclusion, by decreasing interaction.

Chapter seven begins by identifying what makes academic researchers legitimate participants in the interface with industry. The remainder of the chapter concerns relational tensions in the interface, first by identifying typical cases. These include whether or not to communicate findings openly, situations in which the different perspectives of the academic and industry might create problems, and tensions that might arise as situations change during collaborations. As in the previous chapter, ways of handling relational tensions are discussed in terms of socialization, lateral authority, and seclusion.
Chapter eight comprises the study of Uppsala BIO-X. The development of Uppsala BIO-X from its inception in 2003 throughout 2007 is explored, both regarding the format of the organization as well as the relations between the management and the first two main projects funded by the program. A key source for relational tensions in Uppsala BIO-X has been the balance between science and industry. Tensions are also observed with regard to the role of the projects and its participants.

Chapter nine is the conclusion of the study. An important part of the conclusion concerns the role that lateral authority can play in increasing performance within the research system in the long run. This is partly at odds with the likely effects of a research system that includes increased expectations of efficiency and competition. The chapter also provides an outlook toward the potential contributions of this study to the field of economic geography, suggestions of future studies, and some potential ideas for practitioners.
2. Theory: Relational tension as a basis for knowledge creation

In this chapter the theoretical underpinnings for this study will be presented. This will be done through a theoretical discussion based on previous research. The chapter departs from the observation that this study principally concerns social relations on two dimensions. The first dimension concerns individuals, who relate to the world largely based on internalized response schemes. All individuals conceive of the world in somewhat different ways and have somewhat different interests. The second dimension concerns relations between two types of established social systems: science and industry. The systems are, in turn, represented by individuals and by structures (organizations, places, and so on). The ultimate goals for actors in the two systems differ. In science the goal is knowledge recognized by peers, while in industry the goal is success in the market. Most individuals in this study bridge the two systems by producing studies for specific scientific fields while also looking to the interests of specific firms in specific markets. In various ways, they therefore experience and produce relational tension between the two types of systems. The relational tension can be positively utilized for knowledge creation, but it can also create problems.

The outline of the chapter is as follows. In the first section the learning individual will be presented. The argument is that the individual learns by experiences, during which her conceptualizations of the world are changed. The second section concerns the individual in a social context. The argument in this section is that individuals become more similar to each other when they interact but that the incentive to conform depends on the norms that characterize the social environments in question. In the third section science is presented as a social system aimed at a better understanding of the world. In the fourth section science-based industry is presented, and it is argued that the industry is a social system organized around the market. The fifth section concerns relational tension, particularly in the science-industry interface, both with respect to organized knowledge creation and with respect to how it is handled in economic geography. The final section contains a conclusion.
2.1. The learning individual

The argument in this section is that knowledge is embodied, developed as we engage with the world around us. Knowledge is situated; it is intrinsically related to place. There can be no knowledge unless there is a human body, and the body is always located somewhere. All knowledge is therefore based on experiences acquired in interaction with the world. A theory of knowledge should thus be built on three fundamentals: an agent, an activity performed by the agent, and a context in which the activity is performed, see Figure 1 (Lave and Wenger 1991).

![Figure 1. The fundamentals of a theory of knowledge. Based on Lave and Wenger (1991).](image)

The knowledge an individual possesses is based on sensory impressions—from the eyes, ears, fingertips, or guts. The impressions are interpreted by the neural system. The tools used for interpretation have been shaped by previous experiences. Traces of those experiences have been memorized to be used as reference in the future. The reference system is quite statistical. It works a bit like paths in the forest—the more often a path is used, the more natural it becomes to use it again. The process of interpretation is largely unconscious. At least 95 percent of the activities of the human neural system are unconsciously undertaken. The figure is, however, approximate: there is

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1 This is not entirely true. Cognitive scientists have shown that humans are to some extent genetically programmed with “knowledge frames” that virtually all humans share. This helps, for example, to recognize some dangerous animals, to categorize objects (e.g. trees as a distinct group of phenomena above spruces and pines), and to support the development of a common language (Lakoff and Johnson 1999). However, in the context of this study, the relevant knowledge is much more intricate, and should therefore be understood as virtually exclusively dependent on an individual’s specific experiences.
no definite border between the conscious and the unconscious (Lakoff and Johnson 1999).

The sensory impressions of the material world are made intelligible with conceptual tools in the neural system. The world is only understandable through the tools “on hand”. It is therefore impossible to understand the material world as it is. Although all sensory impressions are material, the neural system produces representations of that which is experienced. From those representations concepts of the mind give the experiences meaning; all meaning is immaterial. Phenomena in the world – which include also the individual herself – are that way understood as if they are the same as the concepts of the mind. For each phenomenon encountered the mind takes a leap, whereby something (the phenomenon) is understood as something else but similar (a memory of another phenomenon). Human thought-and-action is crucially based on making this kind of leap, from the concepts of the mind to the physical reality (Lakoff and Johnson 1999, Olsson 2007).

Human actions could that way be seen as concepts of the mind – knowledge about the world – seeking their expression. But when expressed, the concept turns into a phenomenon – knowledge in the world – to a human body acting (Cook and Brown 1999, Olsson 2007). The expressed knowledge can be interpreted again, virtually in action, and improve future expressions (Sennett 2008). As such, human action mediates knowledge from one place to another and from one moment to the next in the “generative dance” between on the one hand the human body inscribed with knowledge and on the other hand the context of the situation (Cook and Brown 1999).

Thus, while knowledge about the world is crucial, it is yet “meaningless unless [it] can be made specific to the situation at hand” (Lave and Wenger 1991:33).

Moreover, the mind also has to understand how concepts relate to each other and if there a hierarchy between them (Bohm 1996 [1968]). Understanding the order of the concepts helps the individual to conceive of the world in a more coherent way. It also helps her to understand her own position, which perspective she should use. It is impossible to understand the world without ordered concepts; and since the individual observes herself as a phenomenon, the perceived order of the world also informs the individual of her own identity.

To change knowledge about the world – to learn – is consequently to both change one’s view of the world and to change one’s own identity. Since all

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2 Since knowledge in action is not a noun but a verb, many authors talk about it as knowing, and not as knowledge (e.g. Cook and Brown 1999, Orlikowski 2002, and Ibert 2007). Although I acknowledge “knowing” to be a more accurate term, I nevertheless stick to “knowledge” in this study, partly because the general terminology is more adapted to knowledge as a noun, partly because the empirical study was not participatory observation and “knowing” therefore could only be studied in the interview situation.
learning is based on experiences, it is therefore required that the individual has observed and consciously or unconsciously reflected on the difference between what she has experienced and her inner conceptions. The observation can be difficult since the brain has a tendency to use habitual paths, but repeated training helps to develop a reflexive attitude. Moreover, the learning process does not need to occur in the immediate presence of that which was experienced; reflexive work can be undertaken long afterward, as humans are able to recall memories. To the extent the individual actually learns, she changes her conceptualizations of the phenomenon in focus. As an effect, the individual’s ideas about the correct order – including her own position – changes as well (Bohm 1996 [1968], Wenger 1998, Sennett 2008).

As a consequence, there are two basic preconditions for advanced learning. First, that the individual is capable and enabled to focus on specific phenomena for a long time. Knowing how to focus is an essential skill (Sennett 2008), and the qualities of the immediate environment are important, too. With fewer disturbances, the individual can focus more on reflecting on the phenomena she is interested in and be more easily in control of her own thought-and-action. The individual can then better rearrange her conceptualizations and even available physical things – in a laboratory, for example – such that new and better orders can more easily be revealed (Amin and Cohendet 2004, Barnes 2004, Sennett 2008).

Second, the individual must find it valuable to change her conceptions. She has to believe that there exists an order of the world that is better than the one she is aware of, and that she is able to observe the other order. Since learning is based on experience, the new order must not be in conflict with other knowledge that to the individual must be true. As knowledge is largely unconscious, such conflicts might be experienced as a feeling that something is wrong rather than as a statement that can be put in words. For the most fundamental learning nothing can be “holy”. The ultimate aim for “the perfect learner” is perfect harmony between her knowledge and the world she experiences. This implies that knowledge is fundamentally about ethics and that, to maximize learning, the individual should treat the phenomenon as fixed and her own conceptions of it as unstable (Bohm 1996 [1968], Olsson 2007, Sennett 2008).

In conclusion, knowledge development – learning – is about comparing one’s conceptualizations of the world with one’s sensory impressions in a process that is both conscious and unconscious. Knowledge is in that way inescapably embodied. It is thus not correct to focus on knowledge as being “about” the world; it is just as much “in” the world. For the same reason, an individual can know only what she has personally experienced.

For the rest of this chapter three points should be noted in particular. First, in learning the fixed point is the experience. Learning requires the individual to open up herself for change. She cannot take her knowledge for granted.
The “perfect learner” therefore finds no need to change the observed phenomenon if it cannot be restored – the more she can allow the phenomenon to “speak” to her without interference from herself or from other phenomena, the greater the potential for learning about the phenomenon of interest. Second, learning is based on the (tacit) presumption that a better understanding of a phenomenon is possible. “Better” implies that the other understanding is more correct and therefore desirable. Third, learning implies a changed conceptualization of the world. As concepts change, so do the relations between them. One concept is how the individual conceives of herself, her identity. To learn is thus to both change the view on how the world is constituted and to become a different individual.

2.2 The individual and the collective

The fact that an individual can only know what she has personally experienced is a challenge in a social context. “Social” here refers to individuals interconnected and interrelated with each other in ways that might include face-to-face interaction as well as links mediated via cultural artifacts (Amin and Cohendet 2004, Latour 2005). In order to effectively interact with each other, individuals have to conceptualize phenomena in somewhat similar ways. The obvious paradox is that such socialization might decrease the innovative potential.

This section, divided into four parts, concerns the paradox of socialization. First, the paradox of the pros and cons of communication and socialization will be discussed. The second part concerns issues of legitimacy and institutionalization. In the third part the concept of power is introduced and unpacked, with the aim to better conceptualize characteristics of social relation. In the fourth part focus is on authority, which is observed to be the most important form of power in social environments. The section serves as a basis for the next two sections on the two types of social systems relevant to this study: science and industry.

Interaction: communication and socialization

There is a paradox regarding social interaction and knowledge creation. On the one hand, interaction offers a chance for individuals to experience difference and thereby get a chance to deepen their knowledge. One example would be to listen to somebody who tells about how to solve a specific research problem. On the other hand, since learning requires an individual to change her initial perspective, there is an increased chance that interaction makes individuals more similar to each other. It is thereby less likely that they will be able to express different perspectives and thereby perhaps solve the same research problem in other ways.
The issue gets most evident in face-to-face interaction. Since face-to-face interaction enables effective communication and coordination, it is very common in many organized human activities, including science and science-based industry (Storper and Venables 2004, Moodysson and Jonsson 2007, Mattsson 2007, Amin and Roberts 2008). Adapted from Storper and Venables (2004), two main ways can be identified in which face-to-face communication supports knowledge work.

First, it enables quick and flexible communication that includes not only an effective verbal exchange but also bodily expressions, as well as the use of other visual indicators such as illustrative drawings or rearrangement of the material at the site (Nohria and Eccles 1992, Amin and Cohendet 2004). In particular, the bodily expressions are worth noting. Humans are more sensitive to bodily expressions, not least facial ones, than they are aware of (Goffman 2005 [1967]). In fact, it seems that words are only a minor part of the message. The psychologist Mehrabian (1981:viii, quoted in Storper and Venables 2004:355-6) notes: “[when words] contradict the messages contained within them, others mistrust what we say – they rely almost completely on what we do”. As a consequence, face-to-face interaction makes it considerably easier to detect other individuals’ intentions, including their lies (Storper and Venables 2004, Olsson 2007).

Second, face-to-face communication is an effective way to form a group. By interacting face-to-face, individuals extend their social control, as they abandon anonymity and are better able to evaluate each other. They become better positioned to judge each others’ competence (Allen 2003, Storper and Venables 2004). They are also better able to observe patterns of social interaction in groups, including how internal status is determined (Nohria and Eccles 1992). In addition, face-to-face communication can increase group members’ motivation to work hard. If one individual makes an effort to meet another one face to face – especially with geographically dispersed groups or busy individuals– or to help without formally having to do so, that tends to stimulate the commitment of others in the group and make them identify with each other (Wenger 1998, Orlíkowski 2002, Storper and Venables 2004). It is widely observed that development of trust is much enhanced by face-to-face interaction (Nohria and Eccles 1992, Murphy 2006, Faulconbridge 2007). Face-to-face work is thus very effective in inducing collective action (Nohria and Eccles 1992) and collective identities (Brown and Duguid 1991, Wenger 1998).

However, the efficiency of face-to-face interaction for smooth communication implies efficiency also in socialization. The relational distance between individuals thereby gets shorter. In order to find common ground to communicate, individuals have to open themselves up for change such that their respective expressions and conceptualizations become more alike. They begin, to some extent, to imitate each other (Berger and Luckmann 1966, Goffman 2005 [1967]). They might, for example, start to talk to each other
by using each other’s expressions, or conceive of a phenomenon through an abstract drawing the team produced. Long-term interaction tends to transform individuals’ identities more fundamentally, such that they acquire similar tastes, similar ways of expressing themselves in and outside the common arena, and identify as belonging to the same group (Bourdieu 1984, Wenger 1998).

The interacting individuals are not equally prone to adapt to the others. One reason is that individuals differ to the degree they interpret and respond to the situation (Yeung 2005). Another reason is that some individuals have roles, statuses, or positions that others want to attain; they are central in a perceived community (Lave and Wenger 1991) or unusually charismatic (Fligstein 2001, Allen 2003). To become recognized as part of the community – to be “legitimate” – the individual in one way or another has to express similarities with the others that the others recognize. If the newcomer has a desire to become central in the community, she is induced to imitate those with a higher status (Lave and Wenger 1991). This point regards practices in a very broad sense, often also general behavior and moral sentiments, and leads, if the common focus is on one relatively similar type of activity, to the development of a somewhat distinct “Community of Practice” in which some participants have more influence than others (Brown and Duguid 1991, Lave and Wenger 1991, Wenger 1998).

In a social context individuals are thus, to some extent, induced to become more alike. That is attained by learning from each other, by internalizing more similar conceptualizations of the world. Two points are particularly worth noting. First, if individuals don’t have the same perspectives, they define truth differently. In that case, it is not enough for an individual to express her truth; she also has to be believed – in a social context truth and trust are practically the same. The frustration might be solved either by adapting to the others and internalizing their view – in effect to trust them instead of oneself – or by accepting a more peripheral position (Lave and Wenger 1991, Olsson 2007).

Second, the closer individuals interact, the less room there is for divergent perspectives to be developed; there is less opportunity to play with thought-and-action (Olsson 2007). Some periods with less sensory stimulation from talk, texts, and other artifacts associated with others are beneficial in this respect (Amin and Roberts 2008, Sennett 2008). It might also include the arrangement of things in a place in a way that makes the place quite different from other places and that stimulates new perspectives. More radical creativity is associated with the existence of spaces and places where “other orders” are possible (Thrift 1999, Barnes 2004). For organized activities in which different perspectives are beneficial, close interaction might thus have negative implications in the long run (March 1991). It is also negative regarding individuals’ opportunities to be in control of their individual identities, to conceptualize themselves (Olsson 2007).
Legitimization and Institutionalization

The degree to which some perspectives come to dominate over others depends on the extent to which they are viewed as legitimate. The degree to which a perspective is legitimate depends on the degree to which it is socially acknowledged as being correct and appropriate (Berger and Luckmann 1966). The ground on which legitimacy rests differs between social spheres. In an ideal knowledge environment, the legitimacy of a perspective depends entirely on the extent to which it is deemed to mirror the world as it really is (cf. Bohm 1996 [1968]). In other contexts the legitimacy of a perspective also depends on the extent to which it is represented by specific, influential actors (Berger and Luckmann 1966, Lave and Wenger 1991, Colyvas and Powell 2006) Since any perspective is intrinsically tied to the actor who represents it, legitimate participation in a social system is therefore tied to the knowledge an individual is observed to embody (Lave and Wenger 1991, Wenger 1998).

Over time what is legitimate sometimes becomes taken for granted. That is, it becomes internalized as a natural template for thoughts-and-actions, whereby it reduces the cognitive effort and decreases the risk for problematic ambiguities in communication (Berger and Luckmann 1966, Colyvas and Powell 2006). If such a “template” gets naturalized within a broader social sphere, it turns into an institution. An institution is part of a “system of rules, procedures and conventions, both of a formal and informal nature” (Martin 2000:79) that includes attitudes and values as well as habits and manners (Bourdieu 1998, Gertler 2004). Over time frequent patterns of thought-and-action tend to be institutionalized and taken for granted, such that explicit rules become, in effect, unnecessary. Institutionalization is a prerequisite for social life; as Johnson (1992:26) notes, it would otherwise be virtually necessary to “start life from scratch every day”. It also means that individuals to some extent harmonize their actions with each other even when they do not physically interact.

The term “institution” is central in the social sciences, though used quite differently across the disciplines. Based on the observation that knowledge is situated, one remark should be made. It is particularly important to distinguish between, on the one hand, what an individual is theoretically able to do and, on the other hand, what she is capable of doing in any given situation (Gertler 2004). An institution should, in this light, be understood as an a priori understanding of the order of the world that to some extent constrains and directs the individual. Thus, the individual acts reflectively and there is certainly a degree of individual agency (Gertler 2004, Colyvas and Powell 2008, cf. Bourdieu 1998). This distinction is important to make, since to learn, as noted above, is indeed when an individual changes her conceptualizations of the world based on her observations. Such a change might be to internalize an institution, but also to transform or break one. Similarly, it
must not be taken for granted that institutions are stable, only that they are quite inert.³

Legitimacy in organized environments partly depends on the extent to which an individual expresses knowledge in accordance with the knowledge held by central actors, such as managers or key evaluators. These perspectives might be inscribed into rules and laws, but they may also be tacit and primarily indicated by the actions of the influential actors (cf Lave and Wenger 1991). Higher pressure for legitimacy means an increased likelihood for institutionalization to occur.

However, institutionalization has unfortunate consequences for environments in which novel knowledge creation is high on the agenda. Institutionalization makes individuals less able to critically reflect on what they think-and-do, and less capable of imagining something different. They become relationally proximate. If, in addition, individuals have, institutionally, got used to adapting to, or even internalizing, knowledge expressed by others rather than to rely on their own instincts – for example, because of legitimacy demands for participation – the chance that they will be able to present ground-breaking insights decreases as well (March 1991, Hollingsworth 2006). The knowledge expressed by an individual or an organization thus depends very much on the characteristics of the relations between individuals; how valuable an individual is who is a bit relationally distant from the other.

Power

What happens in relations between people and the world around them is often understood as an effect of power. As Yeung (2005) notes, it is difficult to have a relational understanding to explain change without bringing power into the discussion. Power is intrinsically related to space, and vice versa (Allen 2003, Yeung 2005). Allen (2003:3) defines power as a “relational effect of social interaction”, where “social” refers not only to direct, immediate interaction between individuals but to the ways in which socialized people interpret and respond to the impressions they make. In other words, people or institutions do not emit power as an intact effect of their capacities

³ As Gertler (2004) observes, this understanding of the term “institution” is thereby slightly different from how the term is defined in, for example, two of the leading works on innovation systems – Lundvall’s (ed.) National Systems of Innovation: Toward a Theory of Innovation and Interactive Learning from 1992 and Edquist’s (ed.) Systems of Innovation: Technologies, Institutions and Organizations from 1997. In those two works the boundary between, on the one hand, the institution as a structure informing the action before it is taken, and on the other hand, the action as such – which potentially breaks with the institutional structure – is not very well articulated. Johnson (1992:26) defines an institution as “sets of habits, routines, rules, norms and laws, which regulate the relations between people and shape human interaction”. A similar definition is used by Edquist and Johnson (1997).
power is an aspect of knowledge and as such translated across space. That makes any attempt to exercise power a bit ambiguous; the outcome cannot be calculated in advance (Allen 2003, Yeung 2005). It also means that power might be quite visible or virtually invisible. Examples of the former are when individuals are excluded from discussions or represented as less competent. An example of the latter is when an individual acts against her personal beliefs if she consciously or unconsciously suspects the “more proper” – true – action could lead to negative consequences (Allen 2003).

Table 1. Forms of power. Based on Allen (2003). Sorted on spatial implication.

<table>
<thead>
<tr>
<th>Form of power</th>
<th>Characteristics</th>
<th>Spatial implication</th>
</tr>
</thead>
<tbody>
<tr>
<td>Authority</td>
<td>An actor is approved by others.</td>
<td>Physical proximity beneficial. Depends on recognition, which is based on norms and enabled by shared spaces.</td>
</tr>
<tr>
<td>Domination</td>
<td>One actor rules over another.</td>
<td>Physical proximity beneficial. Depends much on supervision or similarity in contexts.</td>
</tr>
<tr>
<td>Negotiation</td>
<td>Discussion to identify common goals.</td>
<td>Physical proximity fairly beneficial. Depends on effective and honest communication.</td>
</tr>
<tr>
<td>Persuasion</td>
<td>Discussion to become approved.</td>
<td>Physical proximity fairly beneficial. Depends on effective and honest communication.</td>
</tr>
<tr>
<td>Manipulation</td>
<td>Restriction of information in order to change a relation.</td>
<td>Physical proximity not beneficial. Depends on possibility of being detected by others.</td>
</tr>
<tr>
<td>Seduction</td>
<td>Changing a relation by allurement.</td>
<td>Physical proximity not beneficial. Depends on possibility to communicate.</td>
</tr>
</tbody>
</table>

Allen (2003) observes several forms of power. These, including their spatial implications, are presented in Table 1. Many of these forms of power are elusive: a relation might easily shift from being of one kind to another, and several forms of power might be in play in the same moment. The various forms of power differ in several ways. For example, some (authority, negotiation, persuasion, and seduction) imply that subjection is voluntary to the extent the subject is conscious of what is going on. The other forms are less about voluntarism and more about straightforward discipline. Also, the effectiveness of many forms of power is intimately related to physical space.
Some (domination, authority, coercion) depend much on physical proximity, for example, when stricter control of actions and intentions are desired. Other forms (manipulation and seduction) often work better from a distance, when some senses cannot be utilized, and bodily behavior not interpreted – for instance, when communication primarily occurs through transmitted texts, images, or other artifacts.

The prevalence of various forms of power depends much on the degree to which social environments are hierarchical or diverse. If actors with different perspectives compete with each other for influential positions, then domination, authority, negotiation, manipulation, and seduction are probably common. If actors with similar perspectives compete with each other, negotiation and authority should be quite typical. However, if it is legitimate developing perspectives that are new to the environment, actors most likely do not compete as much with each other. In those cases authority dominates in social relations. Authority is thus relevant in all three cases.

Lateral authority

Authority and knowledge are closely connected to each other. That goes back to knowledge as embodied. The individual recognized for being competent is recognized for embodying socially valuable knowledge. It is not possible to be recognized as knowledgeable without being recognized as an authority worth observing and listening to. An individual’s authority is at any moment dependent on her legitimacy in the views of others. The competent individual is implicitly assumed to let her embodied knowledge direct the actions and statements she makes. The authority for being knowledgeable thus comes from “below”: it is given by others based on observed personal abilities (Sennett 2008).

However, it is also possible to have authority without the need of having recognition from below at every instance. Authority might also be derived from a central position in a formal or informal organization. Although individuals get such positions because of their embodied knowledge, the position as such is made durable partly by being institutionalized in rules, restrictions and expectations that circumscribe both the choices of the positioned individual as well as the possibilities of others to question their actions. In formal organizations, the implicit assumption is that the holder of a position uses her knowledge to make decisions for the good of the organization, regardless of divergent personal opinions (Sennett 2005).

To separate between the two forms of authority, the former is commonly referred to as lateral authority, and the latter as vertical authority (Powell 1990, Dahlander and Mahony 2008). The two forms overlap, of course. Regardless of position, all individuals are normally recognized as able to carry out tasks without relying entirely on a set of instructions. And in academia, a professor has, on the one hand, a high position in the university bureaucracy
and is, on the other hand, usually seen as very competent in her field. The basic point, however, is that to the extent vertical authority is in disharmony with lateral authority, knowledge-creating individuals are constrained, and increasingly so the lower in the hierarchy or more peripheral they are.

Lateral authority is in that way also connected to non-competitive relations between individuals—that is, to environments in which individuals do not have to prove their capacities such that they might lose or gain status on the expense of each other. Individuals are then better able to conceptualize their experiences and themselves in their own ways. That increases the likelihood of having an environment in which different perspectives coexist; that relational distance between interacting individuals can better be sustained. Non-competitive relations should be understood as a cultural characteristic, but it depends much on the conditions given by the organization (cf. Grabher 2002). The defining characteristic of lateral authority is, as Sennett (2008) points out, that if an individual finds another one authoritative, she does not assume that the other one is a copy of her, but tries to appreciate the difference. Learning is indeed about treating the observed phenomenon as fixed and one’s own conceptions as unstable.

2.3 Science

Organizations are largely shaped by prevailing institutions (DiMaggio and Powell 1983, Hollingsworth 2000). Some of these institutions are spread across a whole society, while others are delimited to a smaller sphere, such as a firm, a professional community, or to the members of a small group (Hollingsworth 2000). The landscape of varying conceptualizations of various phenomena is challenging to organizations. An organization only works if its members are somewhat coordinated. For that reason, some understandings often need to be made explicit in rules, laws, and configurations of places such that the participants conceptualize the situations in similar ways and on that basis act in acceptable orchestration. Thus, instruments for coordination “not only record history, but shape its future path” (Levitt and March 1988:326); they, in effect, encourage institutionalization as a means to help the organization survive. These observations are certainly relevant to both science, presented in this section, and industry, presented in the next.

Science is often noted to stick together and thrive because of a strong collective identity among researchers (Merton 1973 [1942], Ziman 2000, Collins 2007, Misheva 2008). That identity should be seen as an institutionalized outcome of strong socialization that primarily occurs during the PhD education. Newcomers who want to be legitimate observe the knowledge the senior researchers express and internalize much of it (Bourdieu 2004, Misheva 2008). The basic reason to why socialization is strong is because science is a very specific field. Over the years, conceptualizations and norms
have developed in close interplay between scientists and their context (other scientists locally and globally, studied phenomena, machines, and so on), which requires many hours of hard and engaged work from the newcomer in developing her ability to engage in legitimate and fruitful dialogue with other scientists (Bourdieu 2004). Science is thus a field characterized by substantial relational proximity, which is necessary for the internal communication – and thereby for the reproduction of the field.

Relational proximity in science is produced through physical proximity between the bodies of scientists and the phenomena they encounter. More precisely, scientists seek to create situations in which the conceptualizations and viewpoints are so shared that objectivity is possible (Knorr-Cetina 1999). Shared perspectives enable the most effective communication. Laboratory work is therefore very standardized: protocols on how to conduct specific parts of the work are widely spread, and laboratories across the world often use the same type of equipment from the same brands to avoid uncertainty. The more precisely scientists can replicate each other’s arrangements, the more able they are to experimentally and conceptually isolate various phenomena from each other, and that way make progress (Latour 1987, Knorr-Cetina 1999, Ibert 2010).

“High-quality” physical proximity also enhances the embodied skills of scientists. It is commonly observed that junior scientists who interact intensively with unusually skilled scientists often become successful themselves. Collins (2007) notes, for example, how great philosophers remarkably often have been the disciples of other great philosophers. Geography’s “quantitative revolution” was undertaken by a group of quite densely connected and elitist young scholars (Barnes 2004). Merton (1968) and Hollingsworth (2006) observe that a small number of academic milieus have produced a disproportionately large amount of ground-breaking research. Heinze et al (2009) find that many breakthroughs in human genetics and nanotechnology have been made by findings of junior researchers, but in environments where the senior research leader has been unusually closely involved in the practical research; and Törnqvist (2004, 2009) observes that many great scientists around the mid-20th century began their careers as stimulated pupils in a few Jewish elite schools in Hungary a few decades earlier. Strong socialization and proximity to unusually skilled teachers apparently supports the development of good scientists and good results.

However, as has been argued throughout this chapter, knowledge novel to a community is not a result of relational proximity alone but also by some degree of relational distanciation. The above-mentioned juniors, in addition to substantial interaction with brilliant peers, have also been able to change perspective a bit. That has been possible for several reasons. First, they have generally been able to utilize diversity in the local environments to rub their conceptualizations against those of skilled others. The diversity is found both in resident scientists and in visitors (Törnqvist 2004, 2009, Hollingsworth
Second, they have usually had autonomy and resources that have enabled them to try out quite unorthodox ideas without too much resistance from other scientists or external funders. Such resistance might be found both in formal organizations as vertical authority or lack of capital, as well as in informal social relations (Barnes 2004, Törnqvist 2004, 2009, Hollingsworth 2006, Heinze et al 2009). Third, they have had considerable integrity, energy, and other skills to survive intellectual fights. This trait, Merton (1968) and Collins (2007) argue, is one of the key learnings from close interaction with a successful senior scientist. Heinze et al (2009) further note that quite a few of their research subjects drew positive energy from rivalry with other scientists. Socialization is thus of great importance in learning what others have found and in developing the ability to communicate with other scientists, but scientific success ultimately depends much on the capacity and ability to develop new perspectives – and that at least the most qualified peers agree that they are correct (Bourdieu 2004).

The relational characteristics of various scientific disciplines sometimes differ significantly. The differences have largely developed because of features of the studied phenomena and due to constraints posed by costs and the complexity of the needed skills and machinery (Knorr-Cetina 1999). This is well illustrated in Knorr-Cetina’s (1999) comparison between a number of German labs in molecular biology and research in high-energy particle physics at CERN. Her observations are summarized in the next two paragraphs.

Molecular biology is shaped by two principal factors. First, it relies much on advanced embodied skills in trial-and-error work at the lab bench. According to Knorr-Cetina, this is largely because the studied phenomena present themselves with a variety and ambiguity that (so far) machines cannot handle. Second, due to the need for external grants and pressure to participate on the fast-moving international stage, the field is very structured by peer recognition, which is received through a steady production of good results. These two points make molecular biology individuated and competitive and are why work in the field is arranged in minor organizations, laboratories, in which a principal investigator (PI) directs a handful of junior researchers and PhD students. The complexity of the field makes it difficult for newcomers to overview; and the complexity of the required embodied skills enables each individual to master only one or two “techniques”, demanding that more extensive problems be solved through collaborative efforts. In the end, however, everyone is judged based on their individual performances. That makes cooperation a bit more difficult, but allows for risk-taking (Knorr-Cetina 1999).

The field of high-energy physics that Knorr-Cetina investigates differs from molecular biology in a number of ways. Most notably, it is much less individuated. High-energy physics at CERN is by necessity undertaken in huge and extremely expensive experiments that can involve more than a thousand scientists and run for a decade, throughout which the scientists use
and construct immensely complex equipment. This decreases the room for large-scale trial and error. There is not much hierarchy in the organization; the work is coordinated via a virtually countless number of overlapping and intersecting groups in which discussions on how to interpret ambiguous data take place and consensus decisions are made that guide the work forward. This makes the experiment an immense socio-material assemblage, which in effect, takes on a life of its own whereby the individual scientists are led by the experiment rather than the other way around. One consequence is that it is the experiment, not the individual researcher, that is viewed as the knowledge-producing actor (“The experiment needs this”, etc). The scientists thus become part of a community in which their individual identities are suppressed. That is great for cooperation and reduces the degree of risk-taking (Knorr-Cetina 1999).

To sum up the section on science, four points should be noted. First, *science* is greatly dependent upon relational proximity. Shared conceptualizations of the same phenomena support communication. Without substantial relational proximity science – or more so, its specific fields – would not be possible as a social system. Second, *scientific progress* depends on relational distance. Even if scientists perfectly share the same perspective outright, progress (the value) is by and large determined by the degree to which the study is somewhat different and somewhat better than previous studies. This valuation is made by other scientists. This is an opening for possible tensions between individual scientists. Third, relational characteristics of scientific fields not only refer to relations between individuals but also to the arrangement of places. As the examples of molecular biology and high-energy physics illustrate, the possibilities to rearrange places are often quite limited. That is because of characteristics of the studied phenomena, because of requirements and costs to empirically test theories, and because of resistance from peers and funders. Fourth, the fixed point in science is when knowledge is recognized by peers. If researchers share the same perspective, this is in effect the same as the fixed point in learning.

### 2.4 Science-based industry

Just like science, the science-based industry is a social system structured by the relations between its participants as well as between the participants and the institutions, organizations, and places that constitute the field in question. Just like in science, success in industry is about producing something that others want, and that others have not already produced. However, compared with science, industry is generally a more rigid system. For that reason, industry is characterized by promoting more relational proximity than science is.

In capitalist economies the central point, and thereby fundamental structuring principle, is the market. The capitalist market is, to put it a bit crudely,
about one actor (the customer) who is willing to trade capital for the right to another actor’s (the producer) goods or services (cf Swedberg and Granovetter 2001). The firm is the emblematic actor in capitalist markets (Kogut and Zander 1992, Maskell 2001). In science-based industries firms typically succeed because they offer products that are qualitatively better than products from competitors, rather than being “the same but cheaper”. Such markets are thus structured not as much by price as by product advantage (Porter 1998, Cooke 2002).

Another way to put it is that markets in science-based industries are characterized by Chamberlinian monopolistic competition. Although cost-reduction is important, what matters at least as much is the ability to recognize and control a market niche that is specific for the product. Such a niche is only possible if the product is understood by potential customers as being in one way or another different from other products on the market. In the niche the product is transferred to customers, who in turn channel a sufficient amount of capital into the producer organization (Scott 1998, 2006).

The need to control a product-specific niche has both upstream (supplier-side) and downstream (customer-side) implications. Most importantly, that involves recursive interaction with actors in both directions — the firm is better able to adapt its actions and products (Lundvall 1992, Grabher et al 2008) while the other actor possibly changes their conceptualizations of the product (Berndt and Boeckler 2009).

Downstream the firm has to make sure that there is a sufficiently large market. The market should not only exist at the time of observation but should continue for at least some time into the future. The firm thus has to target potential customers, find out what they want, and possibly try to shape the wants such that the products of the firm appear more desirable (Callon and Muniesa 2005, Berndt and Boeckler 2009). This is often achieved by developing products in close interaction with customers, often at the customer’s site, to adjust the product to fit better (Grabher et al 2008). Before a market transaction occurs, the product needs to be viewed as a delimited object that can be detached from the seller and inserted into the context of the buyer. The (distant) goal for the firm is to have their niche instituted such that the product or the firm becomes the “the natural choice” (Callon and Muniesa 2005). In other words, in downstream relations firms seek to increase the relational proximity to the customer.

Upstream the firm needs to make sure that its competitive advantage does not erode from underneath. The firm, for example, must ensure that it has intellectual property rights, reliable links to subcontractors and investors of satisfying quality, and sufficiently skilled and engaged employees (cf Fagerberg et al 2005 and the cluster-literature in economic geography, e.g. Maskell and Malmberg 1999, Cooke 2002, Wolfe and Gertler 2004). This, too, implies engineering relations such that the firm hopefully gets a situa-
tion that better suits its intentions (Akrich et al 2002a, 2002b), which are to increase relational proximity.

The depiction of markets for science-based industries has so far focused on what firms seek to achieve. However, the environment offers considerable constraints, which mean that firms are in effect often quite locked into old tracks. Three main reasons can be identified.

First, the firm has to handle very complex situations. Firms have to engage in a wide range of situations in which they have little capacity to influence the rules, but which demand much from them. Examples include matching products with customers, developing technologies in products, and taking care of financial matters – all these often in different parts of the world (Scott 1998, Bunnell and Coe 2001, Dicken 2007). This external complexity forces firms to compose a division of labor, which in turn is made up of people who all have somewhat different knowledges and motives, and thus also create internal complexity (Kogut and Zander 1996). Coordination of a heterogeneous organization in a complex environment, often across long physical and relational distances, is an important task for firms (Kogut and Zander 1996, Bathelt et al 2004, Faulconbridge 2007).

Second, past choices and institutionalized patterns lead firms in directions that cannot easily be changed. Products based on the types of science investigated in this study are typically the outcome of very large investments in research and development. In pharmaceutical biotechnology, for instance, such costs even tend to grow exponentially the closer the development process gets to a marketable product (Cooke 2007). Often there are also significant “sunk costs” in, for example, machinery – the paper and pulp industry being a typical example –in the embodied skills of employees, and in the symbolic representations of the firm and its products. In addition, the organizational arrangement of the firm has resulted in institutionalized patterns of thought-and-action that are difficult to change. These factors mean that firms are often strongly tied to the institutionalized and materialized world, which make it expensive and hazardous to make fundamental changes (Fagerberg 2005, Lam 2005).

Third, uncertainty is an all-pervasive structuring mechanism in economic activities. The future has become increasingly difficult to predict, which in turn induces firms to remain on the safe turf. One reason is that corporate governance – as supported by, for example, the influential Organisation for Economic Co-operation and Development (OECD) (2004) – has become focused on maximizing shareholder value, which has made corporate capital more fast-moving. The argument is that focus on shareholder interests will make the overall economy more efficient, both because capital can be allocated more effectively and because firms – especially managers – will be spurred to work towards larger profits. The downside is that firms, independent of size, often have limited resources to long-term investment (Lazonick and O’Sullivan 2000). This is widely observed as a great problem for eco-
onomic prosperity in the long-run for both firms and regions (Lazonick and O’Sullivan 2000, Shane 2004, VINNOVA 2007b). Another reason is that customer preferences in some markets change quite rapidly. An important reason is that the value of consumer products is much based on the symbolic aspect, a fact to which both firms and consumers pay increased attention (Sayer 1997, Thrift 2000, Power and Scott 2004).

The argument in this section has been that in comparison with scientists, industrial actors are typically more closely tied to the preferences of others, over whom they exert limited authority. Although, to some extent, a firm can change the way its products and intentions are valued by others, it is relatively forced to comply. The complex situations and relatively large-scale efforts add to the point. For those reasons science-based industry is typically characterized by promoting more relational proximity than is science. This section can be concluded in four points.

First, science-based industry induces relational proximity. At some point the industrial actors have to succeed in the market, and that is more likely to happen if the actor acts in accordance with customers and investors. Inside firms, employees are organized in relation to observed external contexts and organizational strategies. The rewards from adapting to others are that way quite high (March 1991). Another way to put it is: in industry the truth is to a great extent given by the market.

Second, industrial success depends on the extent to which the actor is able to attain and retain a central position. The position is, however, constantly challenged. The industrial context is that way about hierarchies. And as such, industrial success, as is in science, is connected to relational distance from competitors—to destabilizing the positions of others.

Third, in industry the position of the individual is normally more determined by the environment than it is in science. Firms are quite restricted in the choice of strategies by customers, investors, and subcontractors, as well as – like any social space – permeated by institutions. The individual employee therefore has a bit limited opportunities to define her own identity and develop new perspectives. For both the firm and the employee, the ties to established institutions and places are quite rigid, and divergence from the beaten track therefore often (too) costly.

Fourth, the fixed point in industry is the market. That is distinctly different from the fixed points in learning and science, fundamentally because it is not necessarily tied to the ambition to harmonize with the world as it “really is”. This opens the door for tensions in the science-industry interface.

2.5 Relational tensions in knowledge creation

The concept of “relational tension” refers to tensions that arise when different perspectives are contrasted against each other in social interaction. Rela-
tional tensions are fundamental in knowledge creation and can be harnessed to help industry develop new products and to vitalize science. They can also be a source of conflict, both because of the different identities of the interacting individuals and because of constraints imposed by their organizational environments. It is, by and large, an issue of balancing coordination and autonomy. This section is separated into two parts. In the first part two key difficulties in organizing knowledge creation are presented. In the second the discussion within economic geography on relational tension in knowledge creation is reviewed, with focus on the science-industry interface.

Relational tensions in organized knowledge creation

In attempts to organize knowledge creation, the tension between coordination and autonomy is evident. The former is emphasized by managers; the latter, by experts (Bailyn 1991, March 1991). As argued throughout this chapter, the tension goes back to relational distance between individuals in different positions. Two primary aspects and respective typical solutions are identified.

First, a manager often finds it difficult to evaluate the performance of an expert. This is due to the specificity of expert knowledge. As a consequence, there is, on the part of the expert, room for slack and non-compliance. The manager therefore has to balance between two alternatives: either to trust that the expert does what is expected from her and tells her the truth or to define quality criteria that laymen can also understand (Sennett 2008).

During the last decade the latter has become increasingly common in academia (Hellström 2004, Benner 2008, see chapter three) and elsewhere (Power 1997, cf Sennett 1998, 2008). While on the part of the expert, the former is more or less unproblematic – she gets authority – the latter might cause problems. The more the quality criteria deviate from the expert’s own definitions of quality – the truth, according to the expert – the less the expert will feel trusted (and valued), and the less she will trust (and value) the evaluator. In such a situation, the expert might, consciously or unconsciously, choose to change her perspective and comply: socialization. Another way of handling the tension is that the expert will be less engaged and less willing to publicly express knowledge she holds dear or is a bit unsure about (Sennett 2008); she chooses seclusion as a way to retain independence and the possibility to retain her perspective. Moreover, it often does not help to make managers out of experts – they either tend to be interested in exploration and autonomy rather than in coordination and control (Bailyn 1991) or to develop managerial perspectives over time on the expense of their expert ones (cf March 1991).

Second, if actors much concerned about end-users work with actors who are less so – for example, when industrialists work with scientists – the need to handle relational tensions gets a bit more acute (Lam 2007, Ibert 2010).
Productive communication and creative friction should be achieved, preferably without decreasing relational distance too much. Thus, they must not socialize too strongly with each other, nor develop too much interdependence (Nooteboom 2000, Hollingsworth 2006). At the same time, the individuals often face multiple incentives, from the involved organization(s) and themselves, for their individual careers (Grabher and Ibert 2006, Lam 2007). For example, until the expert has expressed her knowledge, the value for the project lies in her embodied knowledge; after the expression, that value lies in the knowledge expressed, while the expert is only valued to the extent she is held to express valuable knowledge also in the future (Grabher 2002, Sennett 2008).

To effectively handle the problem with relational distance in organizations, this type of work is therefore usually organized in projects (Grabher 2004, Amin and Roberts 2008) or spatially organized such that participants interact with each other only part of the time (Beunza and Stark 2004) – one might view it as “managed seclusion”. The tension can also be mediated via the objects with which they work: a productive friction can be better upheld if the same object is fascinating from several points of view, such as from different scientific fields or from both science and industry (Knorr-Cetina 1999, Ibert 2010). The problem of multiple incentives can also be handled by trust (Murphy 2006). Trust is usually gained from friendly relations between individuals (Knorr-Cetina 1999), from categorizing individuals in groups to which different degrees of trust are assigned (Grabher 2002), or from “safety nets” such as stable employment (Lam 2007) – all of which ensure lateral authority. The fundamental problem to deal with is relationally distant individuals being uncertain about the consequences their actions will have for their own situation (Grabher 2002, Sennett 2008).

Relational tensions and economic geography

Most leading economic geographers emphasize the weight of relational tension in innovation practices and use it (although mainly implicitly) as a point of departure in their discussions (Amin and Cohendet 1999, Cooke 2002, Ibert 2010). When explicitly discussed, commentators in or nearby the economic geography field have recognized relational tension in terms of rivalry (Grabher 2002, Malmberg and Maskell 2006), productive friction (Beunza and Stark 2005, cf Amin and Roberts 2008), and cognitive distance (Nooteboom 2000).

However, by using relational tension as a point of theoretical departure, and not as a key dimension, economic geographers have tended to emphasize relational proximity over distance (Amin 1999, Grabher 2006, Ibert 2010). Key works emphasize how co-location in specific places benefits relational proximity, which increases in importance in a globalizing world with growing competition (Storper 1997, Scott 1998, Maskell and Malm-
berg 1999, Sturgeon 2003), and how physical distance can be balanced by relational proximity (Bathelt et al 2004, Gertler 2004, Moodysson et al 2008). The discussion has generally concerned how relational proximity has enabled coordination, control, and governance via local or regional institutions (e.g. Gertler 2004, Depner and Bathelt 2009) or organizational cultures (e.g. Moodysson and Jonsson 2007, Faulconbridge 2008). Thus, “as relational proximity is regarded as enabling innovation, [relational distance] is most commonly addressed as being mainly obstructive to innovation” (Ibert 2010:190). The social fundamentals for innovation thereby tend to be partly “black-boxed”.

The same pattern is evident in economic geographic studies that concern the academic-industrial interface. Departing from the observation that relational tension in the interaction between academics and industry benefits economic dynamics (cf Cooke 2002), economic geographers have usually discussed research activities at universities in terms of how they catalyze regional clusters and innovation systems (Saxenian 1994, Chatterton and Goddard 2000, Wolfe and Gertler 2004, cf Cooke et al 2004, Lawton-Smith 2007). In that discussion a few lines of arguments can be identified.

First, geographers have argued how universities serve as “hubs” where knowledge networks are tied together and extended to firms in the region (Benneworth and Hopers 2007, Coenen 2007, Moodysson and Jonsson 2007, Bramwell and Wolfe 2008, Moodysson 2008, Benneworth et al 2009). Much of this discussion has concerned how university research benefits the local biotechnology industry (Waxell 2005, Cooke 2005, 2006, Lawton-Smith and Bagchi-sen 2006). A few commentators have noted how various types of universities – most notably, institutes of technology and traditional research universities – differ regarding their potential to boost regional economies (Boucher et al 2003, Feldman and Desrochers 2003).

Second, it has been argued that universities supply its region with an educated workforce, either directly through graduates (Florida 1999, Sörlin and Törnqvist 2000, Wikhall 2001, Lawton Smith and Bagchi-sen 2006, Benneworth et al 2009) or indirectly by attracting talented people (Cederlund 1999, Florida 2002, Gertler and Vinodrai 2004).

Third, a few geographers have also pointed out that universities can boost regional economic development by engaging in regional governance (Benneworth and Hopers 2007, Benneworth et al 2009). These contributions are how universities are linked into the broader economic system.

However, to show that relations between universities and firms benefit the economy is not the same as to argue that the relations would be more beneficial if they were tighter (cf Pavitt 2001, Dosi et al 2006). More relational proximity could occasionally be counterproductive – individuals in academia and industry could socialize “too much”, and the environmental constraints would most certainly increase, which might lead to lock-in effects and less inventive university researchers. On this point, most of the above commenta-
tors have either been silent or focused on examples in which distance is a problem. Only a few economic geographers have critically discussed the risks with relational proximity in the academia-industry interface.

Florida (1999:72) argues that the capacities of universities to “replenish the talent stock” get drained if they are pitted against (much stronger) corporations in a race for patenting and commercializing technologies; academic prestige and good educational programs are connected to unregulated basic research. Törnqvist (2004) observes that scientific creativity thrives in settings where talented and mobile individuals can work unaffected by economic incentives. Amin and Cohendet (2004:138) argue that releasing “lateral sources of knowledge” via long-term investments in less-regulated universities and other organizations for innovations would benefit the economy in the long run. Ibert (2010) has shown how relational distance was utilized in an innovation project. The discussion is, however, somewhat more visible in related economic disciplines. For example, leading scholars such as Pavitt (1998, 2001); Lundvall (2002); Dosi et al (2006); and Powell, Owen-Smith and Colyvas (2007) and all argue that universities should, besides a networking capacity, also have a solid base in basic research, since that is more likely to produce a skilled workforce and to lay more rigid fundaments for new products and firms.

2.6 Chapter conclusion

If individuals interact, the friction between their different perspectives – the relational tension – opens up a possibility for novel and interesting insights. That is the main motive for professional interaction between academics as well as between industry and academics. Utilizing relational tension are thus connected to great promises. But there are also risks: interaction tends to make the actors more alike – lead to socialization – which erodes the productive difference, and it might also lead to individuals representing undesired perspectives either being excluded or choosing seclusion. The argument in this chapter is that relational tension in this study is best studied across two dimensions: between individuals and as an effect of overlaps between two types of organized social systems, science and industry.

This has led to the identification of three different “fixed points” of focus for the different actors and around which the activities evolve. For learning individuals, the fixed point is the experienced phenomenon. In science, the fixed point is the individually acquired knowledge that is recognized by peers. To the extent scientists share perspectives – which they generally try to do – this is the same fixed point as for learning individuals. In industry, the fixed point is the market. That point differs from the previous since it need not include the ambition to keep the experienced phenomenon stable.
The differences between the three fixed points open the door for relational tensions within science and in the science-industry interface. Relational tensions can be productively harnessed to develop better knowledge and pave the way for innovations. They can also create problems, by opening up for conflicts and unhappiness. Between scientists tensions might arise if they interact and do not share the same perspective or if they compete over the same position. Between scientists and industrialists tensions can produce exciting new findings, but also create problems as the scientist is not necessarily interested in adapting to the market and the industrialist in learning more about details of interest to the scientist. Between science (to which the scientist responds) and industry tensions arise typically because the former demands relevant results and the latter marketable products.

The demands of social recognition in science (by peers) and industry (by markets) stabilize the fields – newcomers and new perspectives are legitimized by insiders. The degree to which insiders appreciate relational distance thus matters. The fields are made further rigid by non-reversible investments in, for example, instruments and infrastructure, as well as by institutionalized patterns of thought-and-action. As such, both science and the economy are made up of “assemblages”, to which individuals have to adapt their perspectives; too much divergence – relational distance – might mean exclusion. This constraint to individual knowledge is generally more pervasive in industry than in science.

Many economic geographers recognize the potential of tensions in social interaction to make economies more competitive. This is not least true regarding the interface between science and industry. However, focus – whether explicit or implicit – in those studies has generally been on how the tension should be a bit relieved, i.e. on relational proximity. Less interest has been paid to how relational distance, and as such the potential for relational tension, can be sustained over time, as well as to how organized spaces might constrain knowledge creation by individuals or groups. This will be discussed in this dissertation around the concepts of socialization, lateral authority, and seclusion.

The discussion in this chapter is particularly visible in chapters five to eight, which contain the discussion of the empirical material, and in chapter nine, the conclusion. But before that, the study is further introduced with first a chapter on the context of the study, the Swedish system for academic research, which is followed by a presentation of the method and material in chapter four.
3. Context: The changing Swedish research system

The aim of this chapter is to introduce the context of the study, the changing research system in Sweden. Since the early 1990s, the context of Swedish university research has changed significantly. New funding bodies, new types of funding – such as “Centres of Excellence” – and an increased pressure to rationalize and coordinate the activities have transformed many academic environments. The changes are an important motivation for this study and have to a great extent shaped the respondents and their situations. Since governmental decisions are the prime shapers of the system, the discussion is focused on changing perspectives in research policies. The main argument is that in the present research system utility is largely defined from a market perspective.

The chapter is divided into three sections. In the first section the historical perspective is provided. Research policies from the late 1800s to 1990 are introduced, and it is argued that this period is characterized by a linear perspective, in which governance was made by allocation of resources. From the 1940s, focus was on transferable results. Until the late 1960s the distance between academia and other spheres of society was relatively great, and the individual academic could, throughout the whole period, be fairly independent. In the second section the present paradigm is presented. During this period, quality has been emphasized, which has included the embodied skills of the academics. This section also includes a presentation of new funders, new types of funding, and reasons for the “crisis-discussion” that has characterized the period. While formally having significant independence, the individual researcher is more constrained in the present system by a lack of opportunities than she was before.

3.1 Research policies until 1990: Linearity and relevance

In this section the Swedish public research system will be put in a historical context. It is argued that the utility goals of research policy have shifted over time and, as will be further explicated in the following section, left traces that also shape the current debates. This section consists of three parts, separated because they represent somewhat distinct periods. The first part con-
cerns the formation of research policy and ranges from the late 1800s to the 1940s. The second part covers the 1940s throughout the 1970s, during which the Swedish and international research system got a shape that largely lives on still today. The third part concerns the 1980s, during which the paradigm of the 1990s emerged.

Late 1800s to 1940s: Formation

European academia and the state began to engage seriously with each other in the 1800s, as part of the institutionalization of the nation-states. Universities in many countries, such as Sweden, were modeled largely after Humboldt’s vision of a university that satisfied two aims: to understand humanity and the world around her (Bildung) and to support the creation of the nation-state by helping to establish a shared sense of the “national” and by educating knowledgeable clergymen for the state (Delanty 2001, Wittrock 2006). In this vision, individual learning was an important goal.

In the late 19th century, most European states also began to show interest in how universities could support industry (Delanty 2001). Many states, including Sweden, invested substantially in science and engineering, mainly in institutes of technology (Nybom 1997). At this time the so-called “development pairs” were formed in Sweden, as research environments at institutes of technology (primarily the Royal Institute of Technology) and single large corporations (for example, ASEA and L.M. Ericsson), with governmental agencies as customers (for example, for energy and telecommunication), developed close and stable links that were sustained for many decades (Fridlund 1999). Regarding universities, the Humboldtian ideas still seemed to remain relatively strong.

At the beginning of the First World War, scientists and engineers had strong positions in many European states, and dreams about a technocratic, progressive society were widespread (Elzinga and Jamison 1995). However, after the war governmental support to universities dropped, partly because of the economic downturn, and remained at modest levels until the early 1930s. In Sweden the 1920s was an era that laid the foundation for several Swedish Nobel Prizes; this basic research was, however, principally financed by private foundations (Nybom 1997).

State support increased again during the 1930s. Unlike the rest of Europe, the Soviet Union, which had invested heavily in large-scale planning and science during the 1920s, had developed more rapidly. Many European intellectuals – far from all who were socialists – took inspiration from the Soviet case, and Bernal, a British chemist, wrote a widely spread manifesto in which he argued that state support to science would improve society by advancing the knowledge frontier (Elzinga and Jamison 1995). Bernal’s strictly functionalist model is perhaps the purest account of the so-called “linear model”, a widely used concept in research policy that refers to the idea of ba-
sic research pushing applied research, which in turn pushes product development and informs political decisions (Stokes 1997). In this vision, transferable research results were a highlighted aspect of the utility of academic research.

The 1940s to 1980: Institutionalization

The real boost to basic science came with the Second World War. War efforts had indicated the potential of science to push societal and economic development. In an influential report to the U.S. government in 1945, Vannevar Bush argued that unrestricted science had the highest potential, and that society therefore should let the most qualified basic researchers work with plenty of resources and without conflicts of interest (Nybom 1997). His advice was followed, and this marks the beginning of the so-called “Big Science”-era in the Western world – which, just as the Cold War began, actually bore strong similarities with Bernal’s socialist suggestion (Elzinga and Jamison 1995). This should also be seen as the establishment of a “contract” between academia and government, whereby, as Vavakova (1998:210, cited in Cohendet et al 2006:237) puts it,

“government promises to fund the basic science that peer reviewers find most worthy of support, and scientists promise that the research will be performed well and honestly and will provide a steady stream of discoveries that can be translated into new products, medicines, or weapons”.

The model had three notable implications. First, the incorporation of intentional quality control meant that the idea of a “pure” linear model was abandoned. Second, the model includes a strategic dimension, whereby planning actors might allocate resources to fields of specific relevance. Third, it was also, as Elzinga and Jamison (1995:583) note, a “victory for elitism”, as state bureaucrats and corporate officials let scientists make the priorities while “socially conscious, populist” aspects were downplayed. The latter was not only an expression of technocracy, but also an effect of debates following the Second World War, in which some commentators, for example Robert Merton (1973 [1942]) and Michael Polanyi (1958), sought to protect science from totalitarian ideologies (Elzinga and Jamison 1995). The effect was, by today’s standards, a relatively long relational distance between science and industry: research quality was largely determined by academics, while relevance was primarily decided upon before the initial allocation to basic research fields. Thus, although utility was largely defined as usable results, interaction between academia and industry or the state could be quite limited.

The large investments in science throughout most of the Western world also meant the establishment of an organizational and institutional structure. In most countries this structure consisted of three “components”: basic re-
search was carried out with long-term, broad perspectives at publicly funded universities; adapted to a context of application in more short-term, specific projects at semi-public research institutes; and finally developed into products at private firms. However, in Sweden another organizational arrangement was preferred, with only a small institute sector that operated in fewer fields. The motivation, formulated first in the 1940s and reinforced in the early 1960s, was that leading firms were often capable of doing applied research on their own, partly due to the established development-pairs, partly because influential sectors were dominated by large firms. Creation of institutes, it was observed, might instead lead to unfortunate lock-in, while without institutes the links between university research – most notably in institutes of technology – and industry might be tighter and more flexible (Sörlin 2006).

During the 1940s, the Swedish state established a handful of research councils and a couple of committees, with the aim of providing support to specific research fields. In medicine, science, and social science, this primarily meant extra funding to risky research and leading basic research via peer-review. Others, such as the councils for agriculture and engineering, were controlled by committees dominated by representatives from the state and industry (Nybom 1997). Also, a few research institutes were initiated, primarily in sectors where the state was not a big customer, for example, in forest products and textiles (Sörlin 2006). As an effect, Sweden constructed an innovation system with relatively little distance between the actors: both universities and the research institutes came relatively close to the “customer” (industry or the state). Since few publicly funded organizations for applied research were initiated later on, this choice has considerably shaped the public research system in Sweden and caused much debate (see for example Persson 2001, Benner 2001, 2008).

During the 1960s and 1970s, the state and academia came closer to each other in three major ways. First the OECD was established as a forum for Western politicians and state bureaucrats. The OECD soon became an important arena to theorize and coordinate research policies, and to solve problems – often by help of panels with academics and industrialists (Elzinga and Jamison 1995, Stokes 1997). Second, research policy was established as a scientific field, for example, with the Science and Technology Policy Research Unit (SPRU) in Brighton and with specialized academic journals such as Minerva and, in 1971, Research Policy (Elzinga and Jamison 1995, Fagerberg 2005). Third, the role that university research could play to satisfy social goals got more attention, whereas the elitist dimension got less prominent (Elzinga and Jamison 1995). Underpinning all three points was a growing awareness of the limits of the linear model: what academia produced did not harmonize well enough with the interests of industry and the state (Stokes 1997). As a response, emphasis was put on the benefits of a closer relationship between academia and the latter.
The most notable development during the late 1960s and 1970s was the institutionalization of so-called “sectoral research”: applied research mainly directed by politicians and industrial representatives, with the aim to better satisfy their expressed needs. Sectoral research was partly an outcome of policy discussions nationally and in the OECD, partly a response to demands from the civil society following the criticized role of science in, for example, the Vietnam War and scientists’ partial neglect of social and environmental problems among other things (Elzinga and Jamison 1995, Delanty 2001).

In Sweden the idea of sectoral research had comparably strong political support. Swedish sectoral research was directed by committees modeled similar to the applied research councils. In the committees recognized stakeholders (industry, the state, and labor unions) via common agreements initiated and funded research projects, often by contracting university researchers (Nybom 1997, Persson 2001). The expansion of sectoral research implied both an organizational and cultural transformation of Swedish universities. The main reason was a growing amount of, in Nybom’s (1997, author’s translation) words, “researching bureaucrats or bureaucratic researchers, whose status and influence were not primarily due to their scientific eminence but to the allegedly high societal relevance of their activities”. There was, in other words, a confluence between “research quality” and “societal relevance” in which the scientific dimension got less attention than it previously did. Introducing sectoral research at universities was thus a distinct step towards relational proximity between academia and the rest of society.

The 1980s: Less planning, more market

During the late 1970s and the 1980s, the promises of planning were contested. Liberal market ideologies gained momentum, and advocates for deregulation and a small state strengthened their position both in national politics and in the OECD (Elzinga and Jamison 1995, Benner 2008). Across the Western world, research systems were increasingly being viewed as if they were structured like markets (Delanty 2001), and as part of a growing emphasis on performance, Swedish research was pressured to become more scientifically valid. Critics stressed the inappropriateness of the same actor both making quality judgments and coordinating the research, which led the state to partly withdraw from the centralized system of sectoral research and delegate some of the quality judgments to expert panels in research council-like bodies (Persson 2001, Benner 2008). The development could be seen as an increased emphasis on science as a distinct social system.

There was, at the same time, little interest from the state and industry in returning to a university system with the autonomy of the 1940s–60s. During the 1980s, societal interests became understood as being largely those of the economy; the role that science played to support the economy was thus
highlighted, and policies adjusted. That view also got strong support from the fact that academic research on biotechnology and computer science had turned out to lay the bases for new and promising industrial sectors. Research policies in Sweden and the OECD therefore shifted to the still dominating aim of finding a good balance between the goals of long-term potential and shorter term relevance, with emphasis on economic output (Benner 2008).

3.2 Research policies from the early 1990s: Quality and interaction

During the last two decades, the Swedish research system has changed quite thoroughly. Although usable research results are still an important aspect of utility, the education of knowledgeable individuals has gotten more attention. As such, and as part of a growing emphasis on performance, policies have shifted towards more stress on quality by the help of competition. Relevance should be secured through interaction across the university boundaries. In this section, the contemporary research system will be introduced. The section is separated into three parts. In the first part, recent theory that has influenced research policy will be introduced. The second part seeks to account for a notable aspect of the current research scene: the crisis debate. In the third part new forms of governance, including organizational arrangements, are introduced.

New perspectives on research

In the early 1990s the market perspective on academia got its breakthrough, and it has since then guided public research policy (Delanty 2001, Lundvall and Borrás 2005). For that reason, the discussion on research policy has mainly been influenced by economic theories. Also, these changed in the 1990s: the rationalist paradigm that had dominated many social scientific disciplines throughout the postwar period was challenged by approaches that, to various degrees, emphasized context. In the economic sciences, the new perspectives were most notably represented by evolutionary and institutional economics (Mytelka and Smith 2002, Fagerberg et al 2005).

The influence of these schools has led policy-makers to emphasize the potential gains to firms interacting with academia. Theoretically, the argument goes back to the recognition of the cognitive, perspectival aspect (Lundvall 1992, Dasgupta and David 1994, Rosenberg 1994, Edquist 1997). The focus on interaction is also indicated by the positive reception of the two originally academic concepts “innovation systems” and “clusters”, which have inspired the OECD as well as many national governments and agencies (Martin and
Sunley 2003, Lundvall and Borrás 2005), including Sweden (Eklund 2007). Overall, the communication between academia and research policy-makers on innovation matters has been fairly intense, and theoretical concepts have to some extent been co-produced (Mytelka and Smith 2002).

Three new concepts specifically have gotten much attention in academia and research policy during the late 1990s and early 2000s. First, and partly outside the economic discussion, the description of an ongoing shift from “Mode 1” to “Mode 2” has been much discussed. The framework of Mode 1/Mode 2 was proposed by a group of researchers fronted by Gibbons and Nowotny (Gibbons et al 1994, Nowotny et al 2001). In the first book they present the two modes as dichotomies on five points:

1. Mode 1 knowledge is produced in an academic context, while Mode 2 is generated in a context of application;
2. Mode 1 is traditional disciplinary science, while Mode 2 is characterized by transdisciplinarity;
3. Mode 1 knowledge is produced at universities, while Mode 2 knowledge is developed within a range of different types of organizations;
4. developers of Mode 1 knowledge are quite authoritative, while Mode 2 is generated through a reflexive process that is more socially accountable; and finally
5. the quality of Mode 1 knowledge is determined by academics through traditional peer-review, while peers in Mode 2 knowledge are more difficult to identify, as different interests are weighed against each other (Gibbons et al 1994).

In the more recent book they focus more on what they perceive as an expanding space of knowledge production where non-academics also have – although on unequal terms – the possibility to participate and thereby influence the scientific discussion (Nowotny et al 2001). Ziman (2000) similarly refers to the Mode 2 phenomenon as “post-academic science”.

The validity of the Mode1/Mode 2-framework has been contested. It has, for example, been argued that the political dimension is partly overlooked both in general (Pestre 2003) and in particular given that actions of universities were always regulated by the state and conditioned based on their usefulness (Etzkowitz and Leydesdorff 2000, Edqvist 2003). As such, the shift from Mode 1 to Mode 2 should not be viewed as a shift to a “new type of knowledge production” but as part of a general ideological shift towards a more market-like perspective on knowledge that emphasizes the role – or occasionally, and more normatively, the democratic right – of non-academics to participate and defend their stake in advanced knowledge production (Delanty 2001, Bleiklie and Byrkjeflot 2002). An indication of that
The point is that policy-makers have occasionally used Mode 2 as an argument to bring academia closer to industry (Wald 2007).

Second, closely related to the Mode 1/Mode 2-discussion, the so-called “triple-helix” framework has become quite widespread in research-policy spheres. Explicitly normative, the triple helix was developed by Etzkowitz and Leydesdorff (1997, 2000) and prescribes how a society can stimulate innovation. The fundamental idea is that innovation should be recognized as an ultimate means for prosperity – or in the terms of this study, as “the fixed point”. Thus, all societal actors should, in one way or another, support innovation. The triple helix-framework concerns the increased innovative potential that arises if three important types of actors – governments, industry, and universities – compete a bit less with each other than is usually the case and try instead to harmonize their activities. The idea of the triple helix supports interactivity between different actors at all points in the development process – from basic research to the last stages of product development (Etzkowitz and Leydesdorff 2000, Etzkowitz 2005). The triple-helix framework has got much attention in Swedish research policy, primarily by providing a rationale for the activities of VINNOVA, a Swedish governmental agency that will be returned to later.

Third, another influential novelty has been “strategic research”, the result of a reconceptualization of the taxonomy for research activities. Previously, research had usually been divided into two categories: “basic” and “applied”, whereby only the latter could be effectively directed and connected to context. Around 1990, a third term, “strategic research”, began to spread in poli-
cy circles – this time primarily promoted by British and U.S. officials rather than the OECD – referring to research in fields of perceived future importance to national economies (Stokes 1997). Stokes, observing that strategic research could not be readily incorporated into the linear figure used to illustrate basic and applied research, presented a four-field figure (see Figure 2) in which strategic research was referred to as “Pasteur’s quadrant”. He argues that focus on strategic research seems fruitful to renew the contract between science and society, and his thoughts have received significant attention in Swedish research policy spheres (cf Eklund 2007).

Swedish university research in crisis?

Partly interconnected with the discussion on how to conceptualize academic research in the broader societal context, Swedish academia has been subject to a debate on its quality, in which academics, policy-makers, and representatives from industry have participated. A commonly held observation has been that the Swedish public research system is, to some extent, in a crisis (cf Fridlund and Sandström 2000). In this section three aspects of the debate will be highlighted: quality, funding, and the role of the university.

Quality

Swedish researchers are, in international comparison, very productive. The Swedish production of scientific articles per capita is higher than in almost any other country (cf Swedish Research Council 2007), arguably partly because the performance of Swedish academics has largely been judged based on their number of published articles (Sandström 2002). However, looking at citation indicators, we see that Swedish academic research has developed poorly during, at least, the last 25 years. Although Sweden is still the 6th most-cited country in the world, Swedish academia displays a worse development over time than some typical countries of reference (see Figure 3). In addition, compared with 16 other European countries, in 2004 Sweden was cited slightly below the average of those countries (Swedish Research Council 2007).

The slump is largely explained by the lack of highly cited publications. Compared with academics in Switzerland, Denmark, and the Netherlands, Sweden-based researchers produce few highly cited publications, and that explains most of the difference between these countries (Swedish Research Council 2010). Another reason is that many of Sweden’s largest – and thus presumably most important – research areas are relatively weak. The two largest broad fields, biomedicine and clinical medicine, are both less cited than the global average, that is, weaker than Swedish research in general (Swedish Research Council 2007); and – as shown in a study with less aggregated fields – the three largest fields, molecular biology & biochemistry, neuroscience, and oncology, all produce fewer highly cited articles than
would be expected given the total amount of articles they produce (Swedish Research Council 2010).

**Funding**

Research funding to Swedish universities is usually divided into three categories. “Direct state funding” is funding the state allocates to universities in the state budget. With regard to content, this type of funding was between 1999 and 2009 specified to six broad areas of research – for example “medicine” and “human sciences” – but for the rest unregulated. The second category, “contributions for research” refers to funding coming from a broad range of funders, and that normally goes to research that is carried out according to established academic norms – the funder cannot expect exclusive right to the results. Finally, “commissioned research” refers to externally

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4 Direct state funding is from 2009 not assigned to specified areas of research. This allocation now takes place at the level of the university (Swedish National Agency for Higher Education (2010b)).
funded research over which the funder might have significant influence over how the research is planned, carried out, and the results are communicated (Swedish National Agency for Higher Education 2009). All three categories are relevant to the empirical investigation in this study. With regard to the funding structure, three main points can be observed.

**High dependence on external funding**

First, Swedish university research is, both from a historical and international perspective, relatively highly dependent on external funding. In the early 1980s less than a third of research in the laboratory sciences at Swedish universities relied on external funding, while today often two-thirds of the funding is external (Sandström et al 2005). The share of external funding has grown from just over 30 per cent in 1981/82 to a little more than 50 per cent in 2007, while at the same time the diversity among funding sources has increased (see Figure 4) Moreover, in European comparison, Swedish university research receives a relatively low share of its funding directly from the state. In 2004 universities in Sweden and Finland received about 50 per cent of their funding directly from the state, while in Germany, France, and Denmark, for example, it was about 60 percent; Norway about 70 percent; and the Netherlands about 80 percent. The United Kingdom and Ireland were lower, with about 40 percent of funding coming directly from the state (Swedish Research Council 2008a).
The arguably most important reason to the high dependence on external funding is that university research has expanded considerably during the last two decades. Figure 5 shows that the real allocations to university research, after the rather drastic downturn that followed a financial crisis in the mid-1990s (Melander 2006), grew by about 70 per cent between 1993 and 2003. In fact, in absolute figures (constant prices) the direct state funding grew throughout the 1980s, and has since the mid-1990s been stable. The exception is the significant cut during the financial crisis in the early 1990s. The expanded university research might partly explain some of the increase in external funding; one might, as for example Hollingsworth (2006), expect that governments find it difficult to motivate significantly growing research expenditures without increasing its abilities to get returns on the investments.

Figure 5 also shows that the total man-years in research grew relatively consistently between the early 1980s and 2003. The gap between the funding and the man-years are probably mainly explained by a growing share of research being carried out by junior researchers and PhD students, and by difficulties to estimate the man-years. The publication frequency, which grew significantly more than the funding and man-years, indicates the growing competition in research. Another reason to the increase in external funding, is that peer-review of applications is seen as a means to ensure quality, and is thus part of the market perspective and its emphasis on performance (Benner 2008). For example, as seen in Figure 6, also besides direct state funding most of the funding for university research comes from the public sector.
Figure 6. Direct state funding, Contributions for research, and Commissioned research 2008, per type of funder. Based on Swedish National Agency for Higher Education (2009).
One more reason is that the direct state funding has increasingly been channeled, or tied, to specific purposes, which means less resources to research problems defined by the individual researchers alone. Around 2000, at least 80 percent of direct state funding was tied to purposes such as premises, other infrastructure, PhD education, and wages of permanently employed faculty and staff (Sandström 2002). A range of reasons for the decreased funding or diminished opportunities for “free research” based on direct state funding can be observed.

(i) There is less “slack time” that might be used for research. The reasons include undergraduate education that has expanded more than its funding has; rationalization of hospitals that implies less time for research for researchers in medicine, who share their time between working as, for example, a physician and a professor (Swedish Research Council 2002); and that some external funders have raised demands on administration and reporting of projects (Benner 2008).

(ii) Resources shrink as costs increase or efficiency goals are not met. For example, the government adjusts its funding according to average productivity development in the private service sector – an increase that most likely is difficult to attain in research and education (Swedish Research Council 2008a); the costs for groundbreaking laboratory research have increased considerably over the last 10-20 years (Sandström 2002); universities have had to permanently employ many researchers on temporary contracts or external funding (Benner 2008); and the PhD education has got considerably more expensive after the universities began to employ the PhD students (Swedish Research Council 2002).

(iii) Resources have in a number of ways been more tied to specific purposes. The reasons include a substantially increased share of research funding to PhD education since the mid-1990s (Swedish Research Council 2008a); expectations to adjust research to fit with the requirements of the expanded undergraduate education; that some external funders have begun to demand co-funding from the university, which, in effect, has to come from the direct state funding (Sandström et al 2005); and that some funders award smaller grants than the researchers have applied for, which sometimes means that direct state funding has to cover up for the costs (Sandström 2002).

(iv) A number of new universities and university colleges were established during the late 1980s and the 1990s, which meant that the old and research-intensive (and generally better) universities got a smaller share of the direct state funding than before (Sandström et al 2005).

This development has to some extent been counterbalanced by larger fees for externally funded projects. Universities have argued that they subsidize external funding by carrying too large a share of the costs for infrastructure. The fees imposed on external funding have therefore been stepwise increased – although, according to universities, still not enough (The Association of Swedish Higher Education 2005, Swedish National Agency for Higher Education 2009).
Short-term grants dominate

The second main observation regarding the funding structure is that it is relatively dominated by short-term grants. Criticism on this point is commonly expressed by Swedish academics, not least in the natural sciences. Following Sandström (2002), the dominance of short-term grants easily becomes a problem when the share of direct state funding is low. In such situations groundbreaking research has to be funded by external grants. Such a system requires two kinds of funding: one that stepwise supports, with small and short-term grants, research projects that in effect have been going on for a long time, with the aim to ensure incremental progress; and one that supports potentially groundbreaking research with larger and more long-term grants. The former type is typical for research councils, which are run by committees that tend to make compromises, refute projects peripheral to the field, and be “fair” by distributing the resources evenly across fields and to many small projects. That tends to lead to lock-in effects. The second type has traditionally been virtually absent in Swedish governmental research funding and is today only really represented by the strategic research foundations and some Centres of Excellence. These will be presented below (Sandström 2000, 2002, Benner 2008).

Lack of organizations for applied research

Third, the lack of public organizations designated for applied research continues to create tension. From a perspective of the Swedish economy, the share of publicly funded research directed by non-academics is comparably low. Representatives of that perspective therefore want the share to increase (Benner 2008). From a perspective of Swedish academia, the share of (to some extent) directed research at universities is relatively high; in 2004 it was most probably higher than ever before (Sandström et al 2005). Representatives of that perspective therefore want that share to decrease. This debate, however, is complicated by a lack of precise statistics and by conflicting categorizations used by the debaters. A factor that probably also influences the debate is that most European countries today approach the “Swedish model” with more applied research at universities (Sandström 2002).

The seemingly insoluble problem is that even if the potential economic benefits of direct links between university research and industry are obvious, the institutionalized reward systems and cultures in the two spheres differ significantly. Academic status and careers are connected to progress in basic

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5 In an attempt to estimate the share of directed research – which is difficult due to the lack of exact data – Sandström et al (2005) concluded that after a considerable decrease during the 1980s, the share of directed research grew during the 1990s to, in 2004, be higher than it was at the peak of centralized sectoral research. At the same time the funding bodies in which academics were in majority increased their share of external funding.
research; few university researchers thus want to specialize in applied research. That makes it difficult to establish environments for applied research in academia. Moreover, since applied research environments in Swedish academia tend to be difficult to recognize, it is also difficult to estimate which ones are better than others (Sandström 2002). The situation is largely a result of the institutionalization of the “postwar contract” and the organization of publicly funded research, as well as the internationalization of academia. The increased emphasis on performance from the 1980s onwards has further pushed the development.

The role of the university
Beside quality and funding, a third stream of arguments for a crisis in Swedish university research concerns the role of the university more broadly and is part of an international discussion (Sörlin 1996, Delanty 2001). The expansion of the utilitarian market perspective onto universities is criticized basically with the argument that a “good” society is linked to the virtues encouraged by free exploration. Increased emphasis on academic adherence to the institutionalized, established social systems represented by “customers” in industry and the state is held to promote detrimental opportunism and to erode the scientific profession. The criticism mainly comes from researchers in the social sciences and the humanities, but it concerns all parts of academia. This discussion seems to occur in spaces that are somewhat different from the policy discussion on research for economic utility (cf Delanty 2001, Neave et al 2006, Hasselberg 2009).

New forms of governance
As part of the transformation of academia into a sphere that harmonizes more with other parts of society and yet produces research of sufficient scientific quality, the Swedish state has introduced a range of new forms of governance to academia. In this section, three major aspects of the new order will be presented. First, it is argued that universities are encouraged to become actors in their own right. In the second part it is shown how the new ideas have been introduced via a new constellation of public funding bodies. Finally, a new type of funding is presented: Centres of Excellence.

Universities as organizational actors
Universities have transformed from arenas for individual researchers into organizations acting in their own right. According to Krücken and Meier (2006) this transformation generally displays four features. First, universities establish their legitimacy by evaluating themselves with reference to quality. Regarding research, that is generally not the same as traditional peer-review: instead of using the most skilled researcher as reference (Bourdieu 2004), standardized and rougher bibliometric measurements are used. Second, universities define
their organizational goals. Third, universities aim to create standardized and more transparent boundaries between internal activities and the rest of society. Fourth, university management becomes standardized and, to an increasing degree, specialized. A notable outcome is that central and specialized administration of universities grows and that researchers do more administrative work (Krücken and Meier 2006). All four points are aspects of the strengthened market perspective that emphasizes performance and the benefits of coordination.

Since the early 1990s Swedish universities have been encouraged to become organizational actors (Benner 2001, Melander 2006). The response from university management has, however, been fairly hesitant; they have either been ideologically unwilling or been constrained by internal politics. There is, however, one exception: Karolinska Institute criticized the government for being too slow and in the mid-1990s had initiated its own internal transformation (Benner 2008).

Swedish governments have driven the organizational transformation of universities mostly by external incentives. Two main strategies can be observed. First, a range of regulatory changes have been imposed. A non-exhaustive list includes

- a legal change in 1993 that forced universities to evaluate and assess their activities on a regular basis;
- a new law in 1993 that allowed universities to develop self-owned non-profit firms responsible for commercializing, which has since been strongly encouraged;
- the introduction in 1996 of a legal requirement, “the third mission”, that universities must collaborate with the rest of society, a regulation that has since been sharpened (Swedish Research Council 2008b);
- demands on universities to identify strong research environments and develop strategies with these in focus; and
- demands of universities to co-finance special grants directed towards the best researchers (Benner 2008).

Second, policies have been directed towards individual researchers. Capable researchers have been identified and empowered. This has also worked “backwards”, as these researchers have put pressure on university management to respond with quite significant decisions, for example, to invest in infrastructure or co-funding (Benner 2008). The strategy has been carried out via a reformed funding structure. That includes new organizations as well as new types of funding.

**New funding bodies**

There has been an extensive reorganization of public funding bodies. Of all the major public bodies that provide means for externally funded research today, none existed before 1994. Five major changes in the field can be iden-
tified. First, the academically controlled research councils, most of them formed in the 1940s, were united in the Swedish Research Council (VR) in 2001. Just like the old councils, the VR would be dominated by academic researchers and fund basic research, but the hope was that the VR would counteract the perceived lock-in by breaking up undesired instituted networks by supporting strong cross-disciplinary environments and potential synergies between fields (Prop. 2000/01:3). However, thus far, Benner (2008) notes, the VR has largely continued in the tracks of the old councils.

Second, the sectoral research was reorganized again. The system with expert panels established in the 1980s was also criticized – most strongly from academia – for being too distant from basic research and scientific standards. A new compromise was made, as most sectoral research was in 2001 transferred to two research councils, the Swedish Council for Working Life and Social Research (FAS) and the Swedish Research Council for Environment, Agricultural Sciences and Spatial Planning (Formas), in which academics had a majority representation. These have developed into two quite traditional research councils and, Benner (2008) observes, are in most fields not very oriented towards interests expressed by practitioners. Support to engineering was, however, kept under governmental control and is today mainly administered by VINNOVA and the Swedish Energy Agency (Benner 2008).

Third, in the early 1990s a number of public research foundations were initiated. The foundations were arranged so that politicians could not take full control of them, and industry was well represented in the two largest foundations (Benner 2001). The primary idea was to challenge old structures by offering new forms of stable finance. An important task was to support “strategic research”, i.e. basic research in fields or for purposes of specific societal interest (cf Pasteur’s quadrant). The three largest foundations were, and still are, the Swedish Foundation for Strategic Research (SSF) aimed at supporting Sweden’s competitiveness in science, medicine, and engineering with later utilization in industry and other parts of society; The Foundation for Strategic Environmental Research (MISTRA); and The Knowledge Foundation (KK), which mainly supports research in collaboration with industry at the four universities established in the 1990s and at the university colleges. These foundations are still active (Benner 2005, Benner and Sörlin 2007).  

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6 In addition, as part of the establishment of these foundations a large sum was transferred to the already existing Bank of Sweden Tercentenary Foundation (RJ) to provide similar support in the social sciences and humanities.

7 The other, smaller, foundations are: the Swedish Foundation for International Cooperation in Research and Higher Education (STINT), the Vårdal Foundation, the Foundation for the International Institute for Industrial Environmental Economics at Lund University, the Foundation for Baltic and East European Studies (specific for Södertörn University College), the Innovationscentrum Foundation (managed by ALMI Företagspartner), and the Foundation for the Culture of the Future. The latter two direct much of their funding to other purposes than university research (for more information on the public research foundations, see Sörlin 2005).
Fourth, the Swedish Agency for Innovation Systems (VINNOVA) was established in 2001 as a response to the gap left when much of the sectoral research was allocated to the more academically oriented FAS and Formas (Eklund 2007). VINNOVA is intended to cover and inform the government on the innovation policy field, which stretches across several traditional policy spheres (most notably, research, industrial, and regional development policies), as well as to design and run programs to stimulate innovation in engineering, transport, communication, and working life sectors. An important aim of VINNOVA is to stimulate relations among the three spheres of industry, academia, and public administration, for which inspiration has been drawn from the triple-helix framework. Therefore, VINNOVA normally prescribes active participation, including co-funding, from all three spheres in its funded programs (Jacob 2006). VINNOVA has been relatively much debated, partly because it took a role no actor had before (thereby crossing traditional boundaries) and partly because its aim is to be quite actively involved in many of the activities it supports (Benner 2008).

Fifth, the European Union (EU) has become an increasingly important research funder. The EU has thus far preferred to fund large constellations of researchers, often with links to industry (Benner 2008). However, recently it has also instituted a funding structure, the European Research Council (ERC), which has the intention to give very large (by Swedish standards) grants to individual researchers regardless of disciplinary belonging. The ERC’s first call for proposals closed in April 2007 (European Research Council 2010).

Figure 7 displays funding from the major funding agencies mentioned above and from which the interviewed researchers get much of the funding.

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The Swedish Research Council is the by far largest funder, and has since 2002 increased its share of funding. Funding from the European Union has increased, while financial support from the public research foundations has decreased. Not included in the figure, a substantial share – about 40 per cent, similar to the funding from the Swedish Research Council – of the funding from the public sector comes from state authorities that are not primarily established to support research, and from municipalities and county councils (Swedish National Agency for Higher Education 2003, 2004, 2005, 2006, 2007a, 2008, 2009).

New types of funding: large programs

The new funding bodies initiated types of funding that had never or only rarely existed in Swedish research before. In some cases the main interest is to reform Swedish academia, and in other cases to ensure that an applied, sectoral perspective lives on. One example was funding based on the condition that also other partners contributed with resources, for example, VINNOVA’s demands on co-funding from industrial partners and the home university. Another example is SSF’s long-term grants in “strategic research” (Benner 2008).

The arguably most radical new type of support is the so-called “Centres of Excellence” (CoEs): projects unusually extensive (in time and in size) and awarded to groups instead of individuals (Benner 2008, Power and Malmberg 2008, Lundequist and Waxell forthcoming 2010). More or less all large public funders and large research foundations have initiated CoEs. Since this study is part of a research project partly focused on this specific type of funding, it should be introduced in some detail.

Although it is difficult to attain a clear definition of what a CoE is and what it is not, it can be argued to constitute a distinctive phenomenon – mainly because several characteristics either tend to be present at the same time or not at all. As developed from Lundequist and Waxell (forthcoming 2010), CoEs typically are:

- outcomes of programs run by large Swedish public research bodies aimed at enhancing the conditions for high-quality university research that proves valuable in the future;
- selected in competitive calls and comparably generously funded on comparably long terms;
- organized constellations of about five to 15 university researchers (including a manager) that are scientifically well established and that together represent several disciplines;
- marketed within as well as outside academia; and
- linked to industry. In most cases, that is partly because it was a prerequisite to get funding in that specific CoE program. In some programs partners in industry co-finance the CoE.
From a policy perspective, CoEs are aimed at solving a whole range of problems: under-financed elite researchers, a lack of cross-disciplinarity, lock-in to old disciplinary structures and loyalties, too small-scale research programs and research groups, a too low degree of strategic thinking at universities (in some cases the applications had to be ranked and submitted by the top management), and often also too vague a connection to potential applications. In addition, a consequence of the size of CoEs is that the funder becomes a pronounced stakeholder, which in combination with the managerial position of the research leader – in practice a new administrative level – makes CoEs easier to coordinate and challenges the university management (Benner 2008).

Although the Swedish Agency for Economic and Regional Growth (NUTEK) had initiated “Competence Centers” as meeting places for academics and industry in the mid-1990s, the first real CoEs were launched by SSF in 2003. The other public funding bodies followed. Between 2003 and 2007, twelve calls were made that resulted in the initiation of a total of 110 CoEs that involved a total of 593 different university researchers. The time range was between five and ten years – often with evaluations along the way, after which funding could be changed – and the yearly funding to each CoE usually ranged between five and 20 MSEK. VINNOVA’s CoEs were the most extensively funded, partly because of extensive co-funding from universities and industrial partners. The total funding for CoEs amounts to about ten billion SEK (Lundequist and Waxell forthcoming 2010). Since these funding bodies between 2003 and 2007 distributed in average about 3.8 billion SEK per year to universities (Swedish National Agency for Higher Education 2010), that means that a little more than twelve percent of that amount was distributed as CoE-funding. There are substantial differences between funders, though: VINNOVA and SSF used about 40 percent of their allocations to such programs, while most of the others only used a few percent of their resources to CoEs.

The CoE-concept has been subject to a rather hefty debate. The critical voices come to some extent from industry and the funding bodies, but mainly from individual academics and include (cf Rothstein 2005, Benner 2008, Bengtsson and Carlsson 2008, Eriksson 2009a, 2009b, Eriksson et al 2009) criticism on the format (“the research group around a single principal inves-

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8 The departments of NUTEK that created the Competence Centers became part of VINNOVA in 2001. VINNOVA thus “inherited” these centers.
9 CoEs included are VINNVÄXT; Institute Excellence Centers; Vinn Excellence Centers; Berzelii Centers; SSF Strategic Research Centers; SSF Strategic Research Centers, Life Science; SSF Strategic Research Centers, Microelectronics; Formas Strong research environments; FAS centers; VR Strong research environments; and Linnaeus Grants. CoEs from MISTRA are not included.
10 Between 2003 and 2010, the value of the SEK usually varied between 7–8 SEK/USD and 9–10 SEK/Euro (Oanda 2010).
tigator is the most appropriate unit to fund”, “only the most established will get funding”, “mediocre researchers get unfair benefits if they team up with stars”) and on the processes (“poor and unclear evaluations”, “incentives to manipulate funders”, “large waste of resources when proposals are rejected”). The type and amount of criticism varies between CoE-programs and funders. In addition, Benner (2008) indicates that not all funders ever really believed in the idea of funding larger constellations and not individual groups, but that the solution was pragmatically chosen to pave the way for a reorganized Swedish academia.

3.3 Chapter conclusion

Since the early 1990s, the performance of the national economy has been the dominant “fixed point” in Swedish research policy. For that reason, new funders, new types of funding, and changed regulations have been instituted – primarily with the hope to have academia better meet the needs of industry both in the short and long run. The market perspective is also visible in the increased emphases on performance, which means that academics compete more with each other, and coordination, visible, for example, in the expanded research management and rationalization of the organizations.

With the market perspective, the always dominant utilitarian perspective in research policy has taken a new shape. The definition of utility, and as an effect, the views on the roles and capacities of the individual academic, has shifted over the years. In the Humboldt university of the 1800s, emphasis was on individual learning, which implied great freedom for the researcher. During the postwar period, the weight of useful results was highlighted. The belief was that academics were most able to make the priorities and so were given considerable freedom. In the 1960s it was observed that academic research too poorly matched with societal needs, which led to the establishment of sectoral research in which priorities were primarily set by representatives of the state and industry. The average academic thus got less freedom.

In the present paradigm, the individual researcher is less openly directed than before. Instead, directives are imposed by the prioritization of specific fields or categories of researchers. Moreover, the utility has got more visible as the research system has been rationalized, which has decreased the room for research on “slack time”. While formally individual academics are more independent now than in the 1970s, some are therefore constrained by a lack of opportunities. Overall, an important aspect of the policy perspective on academia since the 1960s has been that academia and other spheres of society are too relationally distant from each other.

The more recent changes have challenged the academic system and spurred discussions about a “crisis” for Swedish academia. Some focus on the poorly developing scientific quality, which seems to relate to a lack of
long-term funding to elite researchers. Others focus on the applied research, of which they argue either that Swedish universities do too much or too little. Still others are afraid that the utility focus will seriously damage academia’s capacity to represent humanist virtues. The magnitude of the discussion indicates significant relational tensions in the field.

This study will seek to contribute to the discussion on how to stimulate academic research systems – in Sweden and elsewhere – by addressing some core issues that are sometimes overlooked: the basic social conditions that stimulate high-quality academic research. The discussion is supported by empirical material consisting of interviews with well-established university researchers and of a case study on a program to commercialize university research in biotechnology. The next chapter serves to introduce the empirical material as well as to describe how it was selected and handled.
4. Method and material

This chapter concerns the method and empirical material used in this study. As such, it contains considerations about methodology, methods, and material as well as descriptions of how the methods were applied and the material attained. The dissertation is based on two studies. The first concerns well-established university researchers in Sweden and is mainly based on interviews with individual researchers. The second study concerns a “triple-helix” project, Uppsala BIO-X, aimed at commercializing university research in biotechnology. That study is based on document studies and interviews.

The chapter consists of seven sections. The first section concerns methodology and methods in social science. In the second section this study is introduced with regard to the choice of approaches and the project of which the study is a part. Section three and four introduce the two studies that make up this dissertation: in section three the study of university researchers is presented, and section four concerns the case study of Uppsala BIO-X. In section five interviews as a scientific method are discussed. Section six is a discussion on the limitations of the study and the degree to which arguments made in this study can be transferred to other contexts. The final section is a conclusion.

4.1 Methodology and methods in social science

Some of the conclusions in chapter two have important bearings also for this study. These will be developed in this section, which consists of two parts. The first part focuses on learning as based on the conscious or unconscious belief that a better understanding is possible. The second part concerns the argument that maximal learning is achieved by treating the observed phenomenon as “fixed” and the conceptualizations of the learner as “unstable”.

Unfinished theory

The social sciences, including economic geography, have for some decades contained a vigorous and yet unsettled debate on what counts as proper science. The debaters range from those who defend an empiricist position in which science is delimited to “objective facts” and the rest is mere storytell-
ling to some postmodernists who claim that any account of the world is merely “text” and thereby no more scientific than any other account (Scott 2000, 2004, Barnes 2001, Alvesson and Sköldberg 2009).

Following the arguments in chapter two, both extremes should be avoided. Research in cognitive and behavioral sciences has convincingly shown that concepts of the mind are to a great extent, but not entirely, an outcome of socialization. As such, any allegedly objective scientific fact is not the result of “a view from nowhere” (Shapin 1995:5) but rather a view from somewhere – in science from the experienced, socialized bodies of researchers that generally try to express reality as precisely and truthfully as possible (Bohm 1996 [1971], Barnes 2001, Bourdieu 2004).

In addition, since learning is about distancing oneself from a priori understandings, it could be counterproductive to treat some of those understandings as objective and thereby unquestionable. Science as objective is not possible due to different, embodied knowledge. Science as “text”, or “anything”, is not possible because socialization does not explain everything, and because science rests on the assumption that as honest and truthful accounts as possible are sought. Real learning depends on the belief that a better understanding, more in harmony with the world, is possible. Therefore, it is, as Alvesson and Sköldberg (2009:3) put it,

“...pragmatically fruitful to assume the existence of a reality beyond the researcher’s egocentricity and the ethnocentricity of the research community (paradigms, consciousness, text, rhetorical maneuvering), and that we as researchers should be able to say something insightful about this reality.”

The problem in focus

In chapter two it was also observed that in learning, the experienced, observed world is “fixed”; the learner is supposed to change her perspective depending on which conceptualizations she consciously or unconsciously finds most appropriate to capture the world she experiences. As such, research invariably begins in reason, with the identification of a problem. Research revolves around the research problem. This has implications for the design of the study as well as for the relation between the researcher and her field – basically because each individual defines her own truth, while in a social context, each individual has to balance between her own truth and truths that are common or privileged among the other individuals.

Reflection is vital; that the researcher engages in dialogue with herself and others to try out her interpretations. It is just as important to look for differences as for regularities. The researcher builds on existing theories to provide opportunities for better understandings. That road goes via the observation of inconsistencies and incomplete expressions in the nexus of the researcher’s own theories, the theories of others, and in the sense she makes
of the phenomena under study (Alvesson and Sköldberg 2009). Theory is indeed unfinished. As Barnes (2001:551) observes with regard to the production of science in economic geography, the good researcher

“...always tries to negotiate a knife-edge between what Rorty (1982:191) calls ‘hope’ and what Ricoeur (1970:27) calls ‘suspicion’: that is, between the hope that there can be full agreement about a vocabulary and the suspicion that a better alternative is available. There is no final resolution to this tension, no single answer, but it does not mean that anything goes. For, while it is initially important to suspend one’s suspicion in order to give the new vocabulary [...] a chance, critical scrutiny is necessary to establish its usefulness.”

In the same vein, researchers should look for approaches that fit the problems, and not primarily look for how the problems fit ready-made approaches. Otherwise, there is a risk that the reports will tell how the studied phenomenon is represented in research, rather than how it is represented in real life (Alvesson and Deetz 2000). As Alvesson and Sköldberg (2009:304, emphasis excluded) comment with regard to social science:

“Empirical material should be seen as an argument in efforts to make a case for a particular way of understanding social reality, in the context of a never-ending debate.”

With the problem as the fixed point in research, some consequences follow. It is important to look beyond i) the alleged dichotomy between qualitative and quantitative methods; ii) possessions of specific datasets; iii) hopes of attaining practically useful results; iv) the research process as having starting points and end points, since reflections about the problem definition do not halt at those points; and v) it is important to be critical towards, and if needed, to go against, dominant perspectives in the scientific discipline. When a scientific study is designed and undertaken, these points should be taken into consideration. The remainder of this chapter illustrates how that has been done in this study.

4.2 The present study

In this section, the study is introduced with regard to the choice of approach and the context – a larger research project – in which the study was undertaken. The discussion continues in the following two sections by introducing the two studies that make up this dissertation. The first concerns well-established university researchers in Sweden and is primarily based on interviews. The second concerns Uppsala BIO-X, which is part of a triple-helix program in biotechnology and is aimed at commercializing academic research. The empirical material in the second study consists of documents and interviews.
Choosing an approach

There were several reasons to choose an approach largely based on personal interaction with the research subjects. First, I had no personal experience with research environments in science and technology. A study with limited interaction with the studied environments could easily become flawed and dogmatic if perspectives originating in too different of contexts were unknowingly transferred to the analysis. Second, the defined problem could barely be illuminated without conversations with the research subjects. Third, a relational theoretical perspective emphasizes contexts and heterogeneity, which also points towards interviews and case studies.

However, the study could have been combined with more quantitative approaches. Silverman (2006) observes three main ways in which qualitative and quantitative approaches – or, in effect, approaches balancing between interests in particularities or generalizations – can be combined. The researcher can i) do a qualitative study to enable a good structure of a subsequent quantitative analysis; or ii) make a quantitative analysis to identify a sample group for qualitative studies; or iii) let results from a quantitative study inform the qualitative study in order to locate it in a broader context. In this case, however, the broader approaches were rejected, primarily because that probably would have meant a less thorough discussion on the interview material and on the theoretical perspective.

The study could also have been conducted with other approaches than primarily interviews with individuals. There are three established major ways to conduct studies that inform the researcher on the level sought in this study. First, as chosen in this case, the researcher can perform interviews with individuals. Interviews enable direct interaction and can thereby be led as much by the respondent as by the interviewer. This open character considerably increases the likelihood of getting new insights. The researcher is also able to conduct quite a few interviews in a fairly short period of time, and thereby ensure diversity in the material. The main negative aspect is that it is easy to overlook the fact that interviews are situated – the actions of the researcher influences to a considerable degree what the respondent says – and that accounts therefore are not “genuine” in the sense that they reflect actual experiences or can be discursively analyzed to find out about actual meanings (Alvesson and Deetz 2000, Silverman 2006).

Second, the researcher can arrange focus group discussions. A focus group works in much the same ways as an interview. The main difference is that the interviewer becomes less central. The positive sides are that the respondents get more room, which enables looser and potentially more eye-opening discussions, and that the researcher can observe patterns in the social interaction between the respondents. The drawbacks are that it can be difficult to direct the discussion; that some perspectives might be suppressed in the
discussion (Kvale and Brinkmann 2009); and supposedly, that it can be difficult for the interviewer to gather all individuals she wants to participate.

Third, researchers can conduct an ethnography: observe ongoing events, typically over a considerable period of time as “embedded” in, and thereby part of, a local organizational setting. There are several good aspects to such an approach. Most fundamentally, the researcher gets a deep personal insight into processes and is able to document activities as they take place. The researcher also gets the chance to document, for example, symbolic aspects of actions and the uses of texts and material artifacts and to ask questions to the key actors about e.g. the reasons for certain actions more or less as they take place. Moreover, it is possible that the trust between the researcher and the research subjects deepens, which increases the likelihood of “truthful” accounts in interviews. The downsides are that it is a very time-consuming approach, that the rich documentation is difficult to handle and represent, that the researcher might get stuck in details and lose the broad picture, and that the researcher might have problems dealing with the organizational culture and actions either because they are too familiar or too incomprehensible (Alvesson and Deetz 2000).

Interviews were chosen as the main approach. Both in the case of university researchers and Uppsala BIO-X it would have been very difficult to gather a focus group that comprised the very busy individuals in question. In addition, there would also have been an obvious risk that some perspectives would have been silenced. Regarding ethnographies, it was not possible for Uppsala BIO-X, since that study concerned events that had already occurred. For the study of university researchers, two points spoke against ethnographies. First, that would have implied focus on a very limited number of cases. As new in the field, broad insights were preferred at the expense of depth. Second, it would most likely have been difficult to arrange observations. It would not have been possible to accompany a single researcher for a very long time in the specific contexts of interest. Another, more feasible, option would have been to observe strategic meetings in universities and between academics and industry. However, since focus was on individual researchers and not on projects, such an approach would have been a bit misleading. For those reasons, interviews were chosen as the main approach, combined with document studies primarily in the case of Uppsala BIO-X. The discussion on interviewing, with a focus on this study, is deepened in part five of section four.

The context: “Centres of Excellence”

This study is part of a project, “Research Excellence and Science-Based Industrial Systems”, funded by VINNOVA and administrated by the Centre for Research on Innovation and Industrial Dynamics (CIND) at Uppsala University. The aim of the project is to “analyse institutional and spatial factors that give rise to excellence (e.g. renewal, innovation and value crea-
tion) in internationally leading research milieus embedded in science-based industrial systems” (CIND 2010). The project is empirically directed towards 110 so-called Centres of Excellences (CoEs) that were funded by Swedish public research bodies\(^\text{11}\) between 2003 and 2007. The CoE-concept was further developed in chapter three.

The sampling of university researchers to interview was made from the 593 individuals mentioned as principal investigators in any of the CoEs. Also Uppsala BIO-X is, as part of Uppsala BIO, part of a CoE. However, Uppsala BIO-X does not have a great interest in university research per se, but focuses more on transferring that research from university to industry. Moreover, the projects funded by Uppsala BIO-X are short – only two to three years – and do not relate to each other. The study of Uppsala BIO-X should therefore not be seen as a study of a CoE.

### 4.3 The study of university researchers

The empirics on university researchers are based on 24 interviews. Twenty-two of these were made between 31 October 2007 and 1 April 2008, and two additional interviews were combined with the study of Uppsala BIO-X and made in January 2009. The interviews lasted between 45 and 100 minutes, with an average of 68 minutes. All interviews were tape-recorded and fully transcribed. The preferred place for the interviews was at the workplaces of the researchers, which was possible in all cases; typically in their offices in immediate proximity to the laboratories. In some cases a short guided tour around the laboratory was offered as well.

In addition, eight informant interviews were made. The first five were made before selecting the main interviewees, to guide the sampling. These informant interviews were made with three PhD students, one post-doc, and one professor at five different research environments in medicine, natural science, engineering, and materials science. These were informal and recorded only through written notes. The other three informant interviews were made with present or recent research managers at three large corporations when most of the main interviews had been conducted. These three interviews were formal, tape-recorded, and fully transcribed. They lasted 71, 82, and 103 minutes, respectively. The aim of these interviews was to get insight into collaborations between academics and firms from the perspective of the

\(^{11}\) These are the Swedish Research Council (VR), the Swedish Research Council Formas, the Swedish Council for Working Life and Social Research (FAS), the Swedish Governmental Agency for Innovation Systems (VINNOVA), the Swedish Foundation for Strategic Research (SSF), and the Knowledge Foundation (KK).
firms. All informant interviews were used only for background information and are not explicitly discussed in the study.

Sampling main interviewees

The main interviewees were selected because they had personally engaged seriously in both science and in the issues of applying the results. However, they did not have to appreciate both aspects. The CoE-material seemed to be a good pool for the interviewee sample. Many researchers were scientifically very well merited, and many had experience of work with industry. To get a broader perspective, individuals from several fields would be included.

The sampling was relatively formally made. Regarding the selection of researchers with sufficient scientific success, two established approaches were initially considered but rejected. Simonton (2004) made a similar sample by selecting by publication frequency and citations. However, identification of fields for comparisons of citations between individual researchers is a very difficult task (cf Sandström and Sandström 2009) and was deemed too time-consuming given the objectives of the study. Hollingsworth (2006) chose another alternative and sampled his research subjects based on prestigious scientific awards. Unfortunately, very few researchers in the CoE-material had received the most well-recognized awards. In addition, they had together received about 160 different awards, which would be very difficult to weigh against each other in order to establish a good sample criterion.12

Instead, a more modest approach was chosen. Inclusion as principal investigator (PI) in any of the funded CoEs was the criterion for selection. It was also decided to focus on the two largest scientific fields, in order to not have too much variance in the material. Since a CoE is extensive regarding both the amount of funding and time, it can be expected that almost all leading Swedish academics in science and technology applied for funding in any of the calls. Many of them should thus be included in the material. In addition, the funding was rewarded after a relatively detailed peer-review process. However, drawbacks relate to the collective and “strategic” funding: merited researchers can make up for less merited colleagues in the same project, and some fields were more favored than others. Yet, it can be expected that a sample based on researchers funded by CoEs implies a sample that on average contains more established and better-performing researchers than a random sample of Swedish university researchers would. As such, the quality of the sample was deemed sufficient given the objectives of this study.

12 After the sample was made for this study, Heinze and colleagues (Heinze and Bauer 2007, Heinze et al 2009) published a study where they let peers recognize outstanding achievements. Although that alternative was not considered in this study, it most probably would have been a too laborious task to conduct in a structured manner.
Regarding the criteria for work with applications, PIs in CoEs were expected to be aware of industry interest in general. Many of them had seriously considered commercial potential since funders required detailed expression of such a perspective in the applications, and in some cases required industry to take part. From the CVs included in the applications, it was also evident that most PIs had worked with industry at some point. Moreover, it was considered useful to have some variety in the material; preferably to find two equally large groups and allow for comparisons. Others had made similar selections based on engagement in start-ups (Shane 2004), collaborative projects between university researchers and firms (Broström 2009), and contractual relationships (Owen-Smith and Powell 2004, Powell et al 2005).

After investigating CVs and researchers’ homepages, two of those criteria appeared inappropriate: a large majority of researchers claimed relations with firms, and patenting differed considerably between scientific fields. Engagement in start-ups however appeared as a suitable criteria. About one in four PIs had been involved in start-ups. Engagement in start-ups is treated as a proxy to locate researchers with generally more experience of exploiting research findings than others; it is not a variable studied in its own right.

Two basic sample criteria were thus established. First, PIs in the two largest scientific fields in the CoE-material would be identified. Second, the selection would be separated between researchers who had or had not engaged in start-ups. The sample was constructed through a number of steps.

1. Any of the PIs mentioned in the 110 CoEs and the 593 individuals mentioned were listed. This work was done together with two colleagues.
2. CoEs who were not in science or technology were omitted.
3. CoEs who did not have research excellence at universities as a main criterion for funding were omitted. Left were 87 CoEs and a total of 439 individual researchers.\(^\text{13}\)
4. CoEs in the two research fields in which most researchers had been funded were identified and selected. Those fields were materials science, with 50 researchers, and neuroscience, with 47 researchers. Another nine researchers were mentioned in CoEs in both these fields. In total, the sample thus contained 106 researchers at this stage. As part of the informant interviews, PhD students active in the fields helped out with the identification.\(^\text{14}\)

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\(^\text{13}\) These were funded in either of the following CoE-programs: Berzelii Centers, Linnaeus Grants, SSF Strategic Research Centers Life Science, SSF Strategic Research Centers Microelectronics, SSF Strategic Research Centers (call in 2006), VR Strong research environments, VINN Excellence Center, or Formas Strong research environments.

\(^\text{14}\) A third identified field, microelectronics, also contained 50 researchers. However, the PhD student who assisted argued that although the field is recognized as such, it is internally so
The pool of researchers was separated between those who mentioned in their CVs that they had been engaged in start-ups (31 researchers) and those who did not mention engagement in start-ups (76 researchers). The aim was that each of the two groups would provide half of the interviewees.

Males and females were separated from each other, since that divide often influences how individuals in academia are perceived and selected (Clark and Corcoran 1986). Due to the skewed distribution in the material – about one in five were female – the aim was that about a third of the interviewees would be female researchers. If possible the 50 percent ratio of start-up researchers would be kept also among female researchers.

The decision was made to interview at least two researchers from the same CoE, to allow basic cross-check of accounts on how the research was organized in the same milieu.

The sample would be adjusted if it turned out to lean too much towards either institutes of technology (e.g. Chalmers University of Technology and Royal Institute of Technology) or traditional research universities (e.g. Lund University and Uppsala University). That is because these two, today formally almost indistinguishable, types of organizations have different historical trajectories: the former have always had industry support as an important goal, while the latter have been more oriented towards academia. Studies on U.S. counterparts had revealed how the different trajectories had considerably shaped the organizational cultures (Feldman and Desrochers 2003). Such adjustment of the sample turned out not to be necessary, however.

Given the value of leaving the study open-ended to allow some reconsideration, it was decided not to make all interviews in one intensive campaign, but to begin with about 20 and evaluate the indicative results. Based on the above procedure, that meant that ten of these would come from neuroscience and ten from materials science; that five in each field would have experience from starting a firm; and that at least three in each field would be women. The interviews were carried out between 31 October 2007 and 1 April 2008. In January 2009, two interviews in the neuroscience category were added. That resulted in the final sample (Table 2).
Table 2. *Interviews with university researchers, per category.*

<table>
<thead>
<tr>
<th>Category</th>
<th>Neuroscience</th>
<th>Materials science</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>Women</td>
<td>3</td>
<td>4</td>
<td>7</td>
</tr>
<tr>
<td>Men</td>
<td>10</td>
<td>7</td>
<td>17</td>
</tr>
<tr>
<td>Has been involved in start-up</td>
<td>8</td>
<td>5</td>
<td>13</td>
</tr>
<tr>
<td>Has not been involved in start-up</td>
<td>5</td>
<td>6</td>
<td>11</td>
</tr>
<tr>
<td>Total number of interviewees</td>
<td>13</td>
<td>11</td>
<td>24</td>
</tr>
</tbody>
</table>

After the first 20 interviews, it was, a bit unexpectedly, concluded that no category in the sample was particularly distinct. That ruled out structured comparisons across the sample. As a consequence, although a structured sample was created, the material should better be discussed from the perspective of each individual researcher. It also meant that the sample did not have to be expanded.

### 4.4 The study of Uppsala BIO-X

The study of Uppsala BIO-X concerned an organization rather than individuals. Uppsala BIO-X is a good case to include in this dissertation. A new type of triple-helix organization, Uppsala BIO-X could be expected to contain relational tensions to an unusual degree: university researchers were supposed to be unusually managed by representatives from industry, under the auspices of managers with no personal experience of that particular type of organization.

The study of Uppsala BIO-X was carried out step-wise between January 2008 and October 2009. It is based on analyses of documents from or about Uppsala BIO-X and on twelve interviews with ten key individuals (researchers, managers, and representatives from the board of directors). Three of these interviews, all with the “program manager”, were made by three other researchers at the end of January 2008. These interviews lasted in total almost 6 hours, and their overall quality and richness made me decide not to interview the manager on my own. The nine interviews that I conducted myself lasted between 32 and 77 minutes, with an average of 50 minutes. All were tape-recorded and fully transcribed. Four interviewees were at the same time interviewed as “well-established university researchers”. All interviews were carried out at the workplaces of the interviewees. In addition, some

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15 These interviews were made by my assistant supervisor Per Lundequist together with Robin Teigland, at Stockholm School of Economics, and Katarina Pettersson, then at Nordregio. I am grateful for their generosity to let me use these interviews in this study.
issues were clarified through discussions with Per Lundequist, who “real-time monitored” Uppsala BIO between 2005 and 2008 by, for example, observing meetings and interacting with some key individuals in Uppsala BIO. These discussions primarily took place during the co-writing of a conference-paper in August 2008 (see Fridholm and Lundequist 2008).

The opportunity to study Uppsala BIO-X opened up in January 2008, when the organization changed “program manager”. Uppsala BIO asked Per Lundequist to support the transfer. As part of that work, Per got access to most key documents from Uppsala BIO-X from its inauguration in 2003 until December 2007. Some of these documents were not public. By signing contracts that gave Uppsala BIO the right to accept the empirical content before publication, Per and I were allowed to use the documents in research. In addition to that possible constraint, the study was also constrained by the fact that it had to be retrospective.

The study of Uppsala BIO-X did not have as open-ended a design as did the study of university researchers. One reason was that it was initiated at a later stage, when I knew more about the field and which questions were important to ask. Another reason was that it was easier to foresee what material would be available and useful: constant access to documents, to which I could thus return to adjust the analysis, and a limited and well-defined pool of potential interviewees. However, it was still decided to let interviews inform each other and point out the relevance of which interviewees to contact next.

The case was approached in two ways. First, the development of Uppsala BIO-X and its funded research projects were mapped based on the available documents: protocols, internal and external reports, etc. Such documentation is good since it is a product of the time it was written. However, it usually lacks information about debates and ambiguities. They may also be formulated partly to establish a specific perspective, and are that way to some extent “political”; and they may lack important information because the writer of the document forgot about it. Second, and partly to compensate for those shortcomings, interviews were made with several key individuals in Uppsala BIO-X. These included researchers, managers, and “stake-holders” in the board of the triple-helix organization. The interviews were mainly made after the mapping, to enable questions about observed key events or patterns.

4.5 Interviews: Quality and technique

In this section interviews for this particular study will be discussed with regard to quality and technique. The section is based on the observation that interviews are situated in time and space. As such, interview accounts are not accurate representations of what really happened; they are fragmented and distorted by the work of time and self-identification, and are limited to what can be expressed in the interview situation. Moreover, interviews are
also products of the interview situations: actions, including verbal expres-
sions, of the interviewer affect the responses of the interviewed. Although
the interviewer is supposed to mainly be a listener, she coproduces the story
by nods, hums, silences, and follow-up questions (Alvesson and Deetz 2000,
Kvale and Brinkmann 2009).

This section is separated into four parts. In the first part focuses on what
makes material from interviews valid and reliable. In the second part the
confidentiality granted to some respondents is discussed. The third part con-
cerns how interviews should be performed, and how the interviews in this
study were performed. In the fourth part describes how the interviews in this
study were analyzed and reported.

Validity and reliability
First, interview studies are often criticized for having problems with validity,
the degree to which the represented material really gives a fair picture of the
studied phenomenon. It is held that anecdotes, too, often are presented as
evidence (Silverman 2006). This is often the result of unfortunate traces
from the empiricist tradition, evoking the idea that producing findings in
interview studies is similar to collecting data from a statistical yearbook.
However, since interviews are situated, co-produced by the researcher and
respondent, validity is primarily a question of honest reflexivity, in which
the researcher tries to understand the possibilities and limitations to her study
and seeks to represent the material as openly as possible (Alvesson and
Deetz 2000, Kvale and Brinkmann 2009).

Second, interview studies are sometimes also criticized for problems of
reliability. That is, they are inconsistent in that the same concepts and cate-
gories are attributed to phenomena that are not identical, and the studies can
therefore not be replicated (Silverman 2006, Kvale and Brinkmann 2009).
But this point represents too empiricist ideals. The reason why an interview
approach is chosen is often in order to allow the researcher to be open to the
particular, rather than to construct a framework for generalizations (Alves-
son and Deetz 2000). For that reason also reliability is best promoted by
transparency in the research process and the empirical material (Kvale and
Brinkmann 2009).

Confidentiality
The interest in relational tensions implies that parts of the interviews con-
cerned potentially sensitive issues. The sensitivity is indicated also by the
fairly heated debate about research policy in the media and internally in aca-
demia (see chapter three). For that reason the interviewed university re-
searchers got the chance, in most cases before the interviews, to be anonym-
ous. About half of them chose to be anonymous in the text, but most of them
agreed to be named at the end of the study. In the interviews with representatives for Uppsala BIO-X anonymity in the text could not be guaranteed; the number of people involved was too low, and the tasks too specific. It was therefore decided to keep low the number of quotes and to, as much as possible without obstructing the message, keep the individual respondents “hidden” in the broader story. In addition, the researchers in the projects were given the opportunity to see beforehand which of their quotes would be published, and in which context (preceding and succeeding paragraph of the text). However, no quote from a researcher was used.

The option to be anonymous has affected the interview accounts and the way they have been presented. Anonymity was primarily chosen by respondents who were unhappy about important issues and suspected that making their opinions public could negatively affect their situations. By being anonymous, it seems that those respondents expressed more honest and detailed accounts than they would have if they could be identified. Similarly, those very few who were obviously strategic in their responses were not anonymous. For anonymity reasons, a few of the interview excerpts have been slightly altered. In two cases the gender of the respondent has been changed. In a couple of cases information about the research interests of the respondent has been changed. No change is believed to have affected the message of the excerpt.

Performing the interview
This part is separated into three components: preparations, the actual interview, and the specific sample in this study: interviews with elite respondents.

Preparations
The interviews with university researchers were all prepared by gathering basic biographic information about the respondent, for example, by reading their CVs and personal web pages and by observing their roles in their research environments. It was decided that the interviews would be rather loosely structured. The questions would be fairly general about their academic histories, with particular attention to environmental conditions, as well as on a few broad themes regarding activities relating to the academy-industry interface. This strategy was applied partly because it would enable use of the material even if my perspective on the problem were to later change, and partly because one interview per individual gives rather thin

16 The choice was made by signing a document. However, according to Swedish law and academic tradition, all documentation of the study is publicly available upon request. See Appendix A for the document and more information.
information, thereby making structured comparisons between interviews difficult beyond very basic points.

Each interview was prepared by compiling a questionnaire for a semi-structured interview. A similar questionnaire was used for all interviews with university researchers. Occasionally, questions that previously had worked poorly or that seemed inadequate were changed. Following the strategy for the interviews, that was judged as a minor disadvantage. But at no occasion was a full theme omitted.

Interviews with representatives of Uppsala BIO-X were more tightly structured. The background information was thicker, the case more limited, and the problem definition fairly clear at an early stage. For those reasons, more personalized questionnaires were prepared for each interview.

**During the interview**

What makes an interview good or bad is largely determined by the performance of the interviewer during the interview: the degree to which she is able to get the respondent to talk honestly about the issues of interest and her ability to reflect and respond to the development of the interview as it occurs. Afterward, the content is “fixed” (Silverman 2006, Kvale and Brinkmann 2009). Kvale and Brinkmann (2009:164) list a number of “quality criteria” for an interview, which correspond well to the purpose with the interviews in this study. The criteria include

- the extent of spontaneous, rich, specific, and relevant answers;
- the extent of short questions and long responses;
- the degree of relevant follow-up questions;
- the degree to which the interviewer analyzes the interview during its course; and
- the degree to which the interviewer lets the respondent clarify the interviewer’s interpretations.

Moreover, the aim was to pose many open questions. Since an interview is a co-produced, situated event, posing open questions was however not a rigid principle. Also this point corresponds well to Kvale and Brinkmann’s (2009:173) observations when they argue that “the decisive issue is not whether to lead or not to lead, but where the interview questions lead, whether they lead to new, trustworthy, and worthwhile knowledge”. As such, open questions are to some extent also leading questions. Moreover, less open questions can be good to check reliability and might, partly by being fairly directing, invite for protests and thereby new perspectives. In

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17 See Appendix B for a representative example of a questionnaire used for interviews with university researchers.
addition, silence was successfully used as a tactic. If no follow-up question was posed, the interviewer often began to explicate the issue further, with details they supposed – not that the researcher had indicated – were important. That is valuable for the narrative approach chosen in this case and also seems to give the interviewer a sense that they control the interview. With the exception of the first four informant interviews, all interviews were tape-recorded. As soon as possible after all interviews, I sat down and noted specific observations and thoughts.

**Elite respondents**

Almost all interviews were made with “elite” respondents – either well-established researchers or managers. Such respondents are typically characterized by being unusually secure in interview situations, partly due to self-confidence in general, partly because they are used to expressing themselves in similar situations. They are therefore often good at avoiding or playing down sensitive issues, or at promoting their own agenda. On the one hand, these features are challenging to the interviewer, who should be unusually well-prepared with information about the respondent and her context, and alert to direct the interview. On the other hand, elite respondents are generally less sensitive than others to confrontational or challenging questions, which is often positive for the interviewer (Kvale and Brinkmann 2009).

These issues play into this study in three main ways. First, I occasionally found it a bit difficult, especially during the initial interviews, to handle the strong confidence and charismatic authority that characterized some interviewees. Although progress was made and all interviews were of acceptable quality, in a few cases it nevertheless had the unfortunate effect of my omitting or reformulating questions – mainly follow-up questions on somewhat sensitive details – that ought to have been asked.

Second, there were generally few signs that the respondents tried to direct or manipulate the interviews. To the contrary, given the sensitivity of some of the issues, many interviewees were surprisingly openhearted. This particularly regards the interviews with university researchers. Many of those seemed happy to “finally” talk about these issues, which they all found important, with an academic researcher instead of a representative of a governmental agency. The ability to be anonymous probably helped. In the case of Uppsala BIO-X, anonymity was not possible, which probably made some interviewees a bit more careful.

Third, I entered the study on university researchers with unfortunately little and too biased an idea about academia and research commercialization. That perspective was largely shaped by ongoing and quite heated and allegedly insightful discussions in the media, internally in academia, and in policy circles. I thus entered the field with a bit unfounded skepticism regarding the appropriateness of organized research and industrial relations as well as about how research in the sciences is undertaken and regulated. However, to
the extent they noticed, virtually all interviewees seemed not to have let that affect their responses more than marginally. My perspective changed to the better as more interviews were made.

In retrospect, a few more informant interviews, a more careful review of research on research policy, and a short ethnography at one or two laboratories would probably have been helpful before conducting the interviews. However, this observation also points to the benefits of qualitative research: engaging with the subjects is a good way to challenge one’s perspectives. Being uninformed is not only to lack insight; it also implies the risk of overstating the quality of what one actually knows.

Analysis and report
This part is divided into three components: interaction with respondents after the interview, transcription and ordering of the empirical material, and consideration about how to report the material in the study.

Post-interview interaction
There was in principle no interaction with any respondents after the interviews, for three reasons. First, regarding the study of university researchers, there was no need for perfectly correct representations of events; the personal views of the respondents were sought and used as illustrations rather than as “hard facts”. In the case of Uppsala BIO-X a stricter representation was necessary. Clarifications were needed in one case and were undertaken by e-mail. Second, letting the respondent read through and adjust interview transcripts does not always improve the quality, partly because interviewees get the chance to regret honest expressions, partly because the actual formulations are not insignificant (although translations to English deprive the quotes of some of that quality). Third, in the case of university researchers, all respondents could opt for anonymity, and therefore be less afraid to express their actual beliefs.

Transcription and ordering of the interview material
All tape-recorded interviews were fully transcribed. The transcriptions were made in ordinary language. Pauses and unfinished sentences were marked. Laughter was marked, as well as – in the margin – tones that indicated, for example, irony or sarcasm. The transcription process is a good opportunity for reflection. During transcription, notes were therefore made in the margins about possible connections to other interviews and to theoretical discussions.

The transcripts were read on numerous occasions, during which further comments were made in the margins. Comments were primarily used to identify themes and to remember reflections on specific statements. Software was not used, for two reasons. First, the number of interviews was managea-
ble on paper and as electronic documents. Second, I feared that structuring interviews in a software program could unintentionally make me lose sense of the context if I were to skip between small excerpts instead of flipping and scrolling through full interviews in a well-known format.

At a later stage in the process of analyzing the interviews with university researchers, thematic lists were created. These were organized on three levels, for example “industry relations”, with the subcategory “investors to personal start-ups”, which in turn might have the subcategory “located investor through university holding company”. On the lowest level all respondents were listed who fit with the statement, or who had provided a quotable statement. That was an effective way to order the material, and useful when writing the report.

**Reporting the interviews**

The interviews with university researchers have primarily been used as illustrations to the ongoing theoretical discussion, rather than as findings in their own right. The choice was made as the material gained in those interviews was, as noted above, too thin and too unique to allow strong conclusions.

The aim has been to use many quotes to show the empirical content as openly as possible. In some cases questions from the interviewer are also included to further show the context of the quote. These strategies are to help increase the validity and reliability of the interviews. In the case of Uppsala BIO-X, interview excerpts were less used than they were for university researchers, mainly because those interviews were more about specific events and less narrative in style. Since the study was reported in English, the interview excerpts had to be translated by the author. That unfortunately deprives them of some of their quality and makes “discursive” interpretations impossible.

Each individual interviewee has been assigned a randomly selected code number between 1 and 30 that is written next to their quotes. If a sentence is left out to make the quote clearer, that is marked by “…” . If several sentences are left out, that is marked by “/…/” and an empty line. In the few cases the interviewer is included in the quotes, that is marked by an empty line and “T:”.

**4.6 Limitations**

Regarding the representativeness, the study might be biased in four main ways. First, it is possible that CoEs attract specific types of researchers. For example, they might, more than others, enjoy working in teams or managing research, or be unusually good at attaining central positions in organizations (Benner 2008). It is also possible that they are more interested in commercialization of research findings than others. Following the arguments made in
chapter two, such traits probably do not correlate strongly with scientific skills. Thus, if CoEs contain more researchers with such traits than does academia in general, the sample might be biased toward researchers possessing more of a management perspective than the average Swedish academic.

Second, although several CoEs were cross-disciplinary to the extent that the categories became blurred, it is possible that the sample is biased as the respondents belonged to CoEs in neuroscience, materials science, or biotechnology (Uppsala BIO-X). That means that the discussion in the study is biased towards the distinctiveness of those fields. Most notably, that should concern the attitude toward the commercialization of research. The basic compilation of the individuals in the 110 CoEs reveals that researchers in the medical sciences and materials science are overrepresented with regard to start-ups. This probably colors their research environments.

Third, all interviewed university researchers are unusually well established in Swedish academia with regard to merits and resources. Moreover, many of them are at a relatively late stage in their careers. For that reason, the study should not be read as if it concerns the average Swedish academic. Fourth, Uppsala BIO-X, as a case in biotechnology with a novel and unique organizational set-up, is neither necessarily representative of “triple-helix programs” nor of commercialization programs in biotechnology. The arrangement has, however, been positive evaluated by the funder, VINNOVA (2007a), and might as such be emulated by other programs in the future. The study has been reported with these four points in mind.

Generalizations from the study are somewhat limited. The limited representativeness indicates a number of reasons to be careful in transferring conclusions based on empirical content from the study to other contexts. Moreover, since the sample does not meet the standards of statistical significance, no secure conclusions can be drawn based on the cited examples. However, some of the reasoning and conclusions connected to the theoretical discussion, which runs through the study, can probably be reasonably well utilized in other contexts: similar situations are likely also in other disciplines and countries.

4.7 Chapter conclusion

The empirical part of this study is based on 40 semi-structured interviews and on investigations of documents. Of the interviews, 24 were interviews with well-established university researchers in Sweden, funded by Centres of Excellences in neuroscience or materials science; an additional eight were interviews with representatives of Uppsala BIO-X; and eight were informant interviews for background information prior to the interviews with university researchers. The informant interviewees are not quoted in the study. In four cases university researchers were interviewed both as well-established uni-
versity researchers and as researchers in Uppsala BIO-X, increasing the total number of interviews for the study of Uppsala BIO-X to twelve. Three of the interviews with representatives from Uppsala BIO-X were made by other researchers than the author. Document studies were particularly used for the study of Uppsala BIO-X.

The discussion in this chapter has been based on the implications of the situatedness of research in time and space. On that ground, it has been argued that theory must be seen as unfinished and that learning is about focusing on the problem. Other aspects should thus be structured based on the “needs of the problem”. The situatedness of interviews implies that accounts are not accurate representations of what really happened; they are products of the interview situation, in which the interviewer and the respondent co-produce the story. Validity and reliability of interview studies are thus primarily attained by the researcher by reflecting on the possibilities and limitations of the study and seeking to represent the material as openly as possible. In this case, the interviews were generally too thin to allow for “strong conclusions”, and were therefore often used only to illustrate the theoretical discussion.

The study should be read with specific attention to its somewhat limited representativeness. It might be biased towards academics favoring research management and the commercialization of research findings, and towards neuroscience and materials science. Moreover, most of the respondents were also well-established, relatively old researchers with plenty of resources. Finally, Uppsala BIO-X is a special case among triple-helix programs and limited to biotechnology. This limits the opportunities for generalizations based on the empirical material. However, the theoretical discussion can in many cases probably be relatively well transferred to other contexts.

The remainder of this study is mainly consisting of discussions underpinned by the empirical material. The next four chapters contain the empirical investigations. Chapter five lays the ground by presenting findings on the characteristics of social relations that are fruitful for research collaborations. In other words, chapter five is largely “the good story”. These findings will in chapter six, seven and eight be coupled also with examples of more unproductive aspects of interaction and collaborations.
5. The social fundamentals of fruitful research collaborations

The aim of this chapter is to find out, with help of the empirical material, what characterizes fruitful relations between collaborating researchers. Lateral authority is observed as the most important characteristic of fruitful relations within academia as well as with representatives from industry. Lateral authority lets the individual determine on which phenomena focus should be. As such, she can determine on which fixed points her identity should be defined.

Obstacles to lateral authority include lack of resources and conflicts with other goals of the organizations. They also include (too) different perspectives between collaborators, which make efficient communication and working procedures more difficult to establish. For that reason, socialization is an important – though partly unconsciously undertaken – part of collaborative work. Face-to-face interaction provides the best opportunities for observation and adaptation. However, to internalize conceptualizations similar to those of others is to reduce relational distance. That decreases the chance of future breakthroughs.

The chapter consists of four sections. In the first section, characteristics of a good research environment in academia are discussed. The discussion continues in the second section, where the fundaments for successful cross-disciplinary research are identified. The third section concerns collaborations across the university-industry interface. The final section is a conclusion.

5.1 “The good academic research environment”

What characterizes a good research environment in academia? This question was addressed by asking university researchers about the contexts in which they believed they made their most important research. A fairly unambiguous pattern emerged; the interviewees pointed to the environments in which they had been the least constrained by lack of resources or by their colleagues. The environments are to an important degree local, but often stretched across long distances as many interviewees collaborate with researchers abroad. The section is separated into two parts. In the first part, key aspects of the desired independence are identified. The second part concerns lateral authority.
Independence

Academic research is in most fields very individuated. That includes all fields relevant to this study. Individuated refers to the fact that researchers are ultimately judged individually, even if the actual research efforts have been undertaken in groups. Many respondents indicated individuality by talking about work that is actually collective in terms of “I” instead of “we”. Some also repeatedly positioned themselves in relation to others, thus representing science as a field of inherent difference on the individual level.

Individuality is visible also in the two-tier structure of the research groups. This structure is widely institutionalized across contemporary academia and was more or less characteristic of interviewees’ present work environments, and in many cases also so for their PhD studies. The groups are divided between the principal investigator (PI) and the rest of the group, which in this study normally consists of two to ten PhD students, post-docs, or research assistants. All interviewed university researchers were PIs. In some cases the groups work together in labs somewhat secluded from other groups, and in other cases they share space with other groups in a broader research environment. The PI is the only “full scientist” and the one who represents the group to outsiders. The PI decides what projects to run, and consequently which skills group members should hold. In most cases – at least nowadays – PIs do not do much laboratory work; they mostly administer the group and engage in writing articles etc. These differences can create tensions between the PI and the others. The PI ultimately works for the good of the group while the others ultimately work for their future careers in other groups; and to the PI each group member is to some extent a strategic resource employed for a specific end, while the others might prefer to work against other ends (Knorr-Cetina 1999). Several respondents reflected on the tensions inherent in the organization, but – partly because it was not asked about in most interviews – relatively few made references to the others in their groups.

Throughout the material, the desire for independence in research appears relatively strong. This was indicated by frequent references to constraints and was visible in the ways “good environments” were portrayed and related to less desirable situations. Based on the interviews, three aspects of independence were identified. Since these specific points were not explicitly asked about, it is not possible to determine the extent to which they apply across the material, but it is likely that they to a relatively high degree apply to almost all interviewees.

First, most interviewees express a desire to define their own research problems. This interest often can be seen early in their careers. Illustrative of this is when three respondents mentioned how their supervisors had let them radically change topics for their PhD projects when they expressed discontent and lack of inspiration and how important that allowance was for them.
One of them grew tired of his supervisor’s project and defined, 1.5 years into the PhD program, a new and very different topic:

“For me that was a breaking point. Had he said ‘No, that won’t work, I need someone to finish these projects’ – his projects – I would have left. And then I would probably do something completely different today” (24).

The issue seems to get more important as careers progress. Most notably, several respondents tell about how at early stages in their careers they had already chosen to move to milieus that did not contain any established senior researcher, in order to get independence. Had the problem not existed, the most natural move for them should have been to a leading senior professor with plenty of knowledge, resources, and contacts. The interest in having the opportunity to define one’s own research problems is probably linked to the feeling of being a legitimate knowledge producer. As such, it enhances motivation.

Second, quite a few interviewees claim that freedom of choosing how to approach problems is important to them. This point is mentioned almost only with regard to their PhD studies, perhaps because it is generally less relevant later on. Several interviewees claim to have spent a lot of time trying to find out about methods or how to handle specific instruments. Some talk about it as a desire to “play in the lab”, while others present it as a “forced independence” as their supervisors did not instruct them a lot – occasionally because the supervisor lacked interest, but mostly because he or she found that to be a superior way of instruction. In retrospect, most interviewees found it a good strategy, even if it was hard at times. For example:

“[My supervisor was a] person who was supportive and inspiring in a very open way, but [who] really left a lot, I should say, to PhD students and post-docs to do something themselves in order to succeed. It is clear that a lot of internationally successful researchers were shaped in his group … But there were also many who did not cope with his lab.”

T: “Why?”

“He was really inspiring, really helpful, and he really was able to get the best out of other people. But at the same time you had to do it on your own. You were really independent at the same time. The inspiration and support was there, but in the end he never told you exactly what to do. You had to figure that out on your own. And you had to identify… He gave a frame, sort of, said ‘Something like that is interesting’ and pointed in the right direction. But then you had to go there on your own and locate the problem. Not everyone was able to do that.”

T: “To handle the uncertainty? Or to simply…”
(interrupts) “No, nothing gets done. Some people... There’s a really big change when you begin as a PhD student. If you study at university and follow courses, then you are told what you are supposed to know. ... Throughout the whole education system you learn like that. The demands and expectations are well articulated, what you need to do to get a certain grade. But if you enroll as a PhD student, there’s nobody telling you ‘do this and that, and you’ll become a doctor’. That’s not possible. Ok, you can supervise in a very detailed way, say ‘Now you cleave that plasmid with that enzyme and you run it on a gel and you cut out that band...’ ... He did not at all supervise like that. He said, ‘This is an interesting question. Now it would be exciting to find an answer’ (laughs)” (13).

Following the interviewee, this freedom-to-choose approach is thus not only about a motivating sense of independence but also about the fact that the uncertainty makes the researchers learn better to reflect on the quality of their actions.

Third, many interviewees also express quite a strong desire to influence future strategies for the research groups they are in. That includes which instruments to invest in, which other research groups to contact for common projects, and which research proposals to write. To make a strategy is to impose some restrictions on future actions. The difficulties of influencing such strategic maneuvers had caused several interviewees to move to other universities, even if the research groups they left were comparably successful and offered a nice everyday atmosphere. One respondent also claimed that it had deterred him from moving his relatively weakly funded group to his long-time and well-funded collaborator in another department at the university.

Lateral authority
Implicit throughout the discussion on independence was that the interviewees prefer a high degree of lateral authority – that is, regardless of whether the position is treated as legitimate or whether it offers the chance to develop the knowledge they personally prefer, that they are trusted as capable individuals rather than treated as strategic assets. Three aspects of lateral authority are observed and considered in this section: that the participants have friendly relations with each other, that they are recognized as competent researchers, and the important role of the senior researcher.

Friendly relations
Almost every interviewee attends to the importance of a good social atmosphere, both in the research group and in collaborations across distance. At the minimum, there has to be some degree of “social harmony” between collaborating individuals, preferably genuine fondness of each other. Another way of putting it is that the interaction should be such that they could
work closely together for years, which occasionally happens. Many respondents indicate that their relations with close collaborators have been going on for decades, whereby each limited project has been followed by another one immediately or after a break of several years. Sometimes the relations have also developed into private friendship. Mutual respect for each other as individuals is one way to communicate lateral authority. Moreover, Knorr-Cetina (1999) observes, seeking to create friendly relations is also frequently used by PIs as a strategy to prevent competition, and thereby a poor climate for discussions within the group.

Two aspects of friendly relations should be highlighted. First, it gives the researcher social confirmation. One interviewee tells about his PhD studies:

“We were mechanical engineers, chemical engineers, and physicists. We were in the same group. We had long coffee breaks (laughs). We learned a lot from each other. And we worked all the time. … It was great fun, it really was. A really nice crew” (17).

He thus connects social confirmation to the motivation to work harder. Another respondent emphasized how these relations affect self-confidence: as a PhD student she felt insecure because she was unsure of how peers would judge the quality of her work; she found support from friends in the research group. A similar example comes from a female researcher who together with some other female colleagues felt sidestepped by the men in the department, which led the women – who initially did not have much more in common than being women in the same research milieu – to support each other by common coffee breaks and research collaboration. Although she is the only interviewee who claimed discrimination, a couple of the other women also noted that academic networks tend to be gendered, which negatively impacts their chance to get social confirmation at work. As one respondent puts it:

“Guys, men – after all they have a way, a quite natural way, to interact with men. Men select men. Now I sound like a woman on the barricades, which I am not. But you need comradeship. It might be that you want a female colleague to just have lunch with. You don’t always have that” (7).

Second, friendly relations can buttress research discussions. In such discussions important components of a research project might be questioned, thereby threatening the perceived competence of someone. If a researcher feels that her social value in the group is not at risk in the discussion, she is more likely to open up and potentially expose her lack of knowledge. One respondent was part of a large group of PhD students, and linked their propensity to engage in unusually confronting discussions, and their subsequent professional success, to the deep personal friendship among the members of the group.
**Competence**

If researchers can choose freely with whom to work, mutually recognized skills are a precondition for engaged interaction. As such, researchers need to recognize each other in two ways. First, it is an absolute prerequisite that the partners recognize each other’s abilities to support the development of satisfactory scientific results. Although categorizations of people are certainly commonly used as a rough indicator in this respect, it is evident that few researchers engage in collaborations without first having screened each other through personal interaction. One consequence – underlined by many respondents – is that interesting interaction is not necessarily confined to university researchers; anyone knowledgeable is interesting. For example, there are many skilled researchers in industry. In addition, researchers with exclusive access to unusual instruments or material are more interesting partners than others.\(^\text{18}\)

Second, for all collaborations the possibility of establishing agreeable work procedures must be recognized. What is “agreeable” appears to differ considerably among individuals and different fields. One interviewee noted, for example, that medical researchers are often more “functional” in their project setups than are others and that they therefore tolerate more open conflicts, while a physicist said with a laugh that “within physics we are not that concerned with etiquette” (6). However, unnecessary friction should, of course, be avoided, and several respondents indicated how interaction over time – during which face-to-face meetings at some point are almost always necessary – enables the parties to observe each other and mutually develop procedures that make collaborations run smoother and reduce the administrative effort.

Friction is not only a matter of personalities. It is also related to the research problem. If the motivation to work with a specific problem is very high, otherwise disagreeable collaborators might be tolerated or even barely noticed. Two women indicated, for example, how that makes the gender aspect relatively insignificant. One of them says:

“It is probably so that when you are in the middle of something, then you are so engaged that you don’t think of it. That I am the only woman among lots of men. You don’t think like that. But afterwards, when you sit down and look back a bit, you might think ‘It would perhaps have been different if I had been a man’. But I anyway think that when the final result is incredibly exciting research and the collaborations run well, then that aspect is somewhere in the background. It’s nothing you need to think about” (2).

\(^\text{18}\) The interviewees seemed able to meet new potential collaborators at virtually any occasion. Examples in the interviews included: local networks, events organized specifically to facilitate research discussions, research centers, administrative committees, courses for e.g. leadership, more loosely held social events, visibility in popular media, and reading articles. Two respondents had fruitful contacts they had never met face-to-face, but found in the literature.
Thus, to the extent that the focus is on the experienced, interesting phenomenon other aspects get peripheral. When individuals learn, they are immersed in the world they are experiencing. However, in the long run relatively subtle aspects still matter, summarized by one interviewee as

“There has to be some combination not only of good ideas – which there has to be – but also … of research interests that … not only fit intellectually, but also on a more social, emotional level” (4).

**Seniors as role models**

The third aspect of lateral authority concerns the role of senior researchers. The interview accounts of positive research environments are permeated with observations of the behavior of senior researchers. Generally, the more lateral authority the seniors show the interviewees, the better. Sometimes the seniors are talked about as “role models”. This aspect is particularly notable since it was not explicitly asked about in the interviews. Role models refers to seniors valued because they gave, through positive examples, fundamental insights about how to behave as a researcher – a professional identity.

All role models are depicted as being able to couple excellent research skills with generosity, professional integrity, or the ability to be unusually stimulating. Sometimes the role model comes from the local milieu; in other cases he or she has shown interest in the – at the time often young – respondent’s research at a conference or seminar and devoted time to discussions and support also later on. Thinking of – undoubtedly with a romantic flavor – the role model seems to have motivated the respondents to carry on challenging tasks. Some respondents also say that they to some extent have tried to imitate the role models once they themselves have become established researchers. In one of the most explicit accounts of role models, one respondent described the supervisor she had as a post-doc abroad:

"[W]hen I came to [my post-doc environment] he said on the first day 'You know, I work really hard, and I expect that you also work really hard.’ He put all the cards on the table, and that’s it. Yes, he really had time for everyone, always. No matter how stressed he was he always had time to talk to those who wanted to talk to him – well, perhaps not immediately, but at least on the same day. That is something I really have tried to hang on to, because that is important. And he was incredibly perceptive and good at observing the social atmosphere and trying to get it all together so that the whole machinery ran well. And then – an incredibly skilled researcher. The method. The scientific method. Yes, he has really supported my academic career; I’ve got to say that” (7).

Another respondent discussed role models in terms of ideal types he had formulated for himself. He then mentioned the names of three older researchers he had worked with, and continued:
“I think these individuals represent the whole facet of the ideal scientist. That is, science as an idea. It is a vocation, perspective, a way of being.”

“Objectivism. Observing matters impartially. I mean, with great skepticism, questioning, until you know, and then not take your knowing for granted but question that as well.”

“...you can be a researcher and live in society even when you are not a professional researcher. You can do it without resources. Observing matters is a lifestyle. It is something deeply human that is not talked about in the discussion, in the debate. Sometimes it is expressed like ‘Humanism is yet alive’ and there’s a lot... In the contemporary public debate the scientist is perhaps included and hidden in the humanist” (30).

He is the only one who explicitly connects his role models to a humanist ideal, though it seems to be reflected in the others’ role models as well. The role models have much authority, and are as such very influential. It seems that they are valued largely because they have refrained from drawing on their positions to use others for their own purposes. Instead, the respondents have been encouraged to explore their personal interests, which has given them more confidence to continue their work.

5.2 Teamwork in cross-disciplinary basic research

As indicated in the previous section, limited constraints and lateral authority is not only an ethical aspect; it is also a precondition for successful research. This is clearly evident in accounts about collaborations in basic research that cross traditional – and often manifest – disciplinary boundaries. Two aspects are identified in this section: the often arduous task of locating points of connection and establishing common frames of reference and the need for self-reflection and faith in partners’ abilities.

Developing common ground

The researchers have to sufficiently understand each other’s perspectives. That learning process requires much reflexivity, and therefore tends to demand much time and energy. The participants work with phenomena to which some of their partners often cannot relate at all, and use concepts that are sometimes unknown and that used differently between fields. For those reasons cross-disciplinary projects are typically preceded by much reading and many hours of informal and formal discussions during a long period of time – often several years. One engineer told about his struggles to communicate with his biologist collaborators:
“It was an interesting experience which was a bit difficult in the beginning, but I soon made up my mind: ‘Now I’ll try to learn their language. I have to understand what they’re saying.’ And I got myself a textbook called ‘The Cell’ which is about this thick [shows about ten cm] and contains, I think, 1200 pages. They read it in medical training and elsewhere for several years (laughs). And I made my way through it. So now I start to get an idea about how they think and… It’s terribly laborious, it really is” (20).

Another researcher, a chemist, told about his first encounters with a subfield in biology:

“…We really were quite far from each other in the beginning. Actually I did not understand anything at all. They could show me any picture on [what they were doing] and I understood nothing. It is so incredibly complex.”

“I will never forget the first meeting I had with them – we were just as puzzled both of us (laughs). Just the language, to begin with – I talked about [my substances], they talked about [theirs]. And we did not know what [their cells] are called, and they did not know what I call [my substances]. So we were really confused after the first meeting” (27).

The differences often also make it difficult to establish common goals and working procedures. Clear goals and established procedures tend to increase participants’ motivation and make it easier to get through upcoming obstacles. According to the respondents, success on this point largely depends on skills in one’s field of expertise – probably because that implies better abilities to project obstacles and see common connection points (cf. Sennett 2008). The difficulties are partly because participants do not want to leave their own field; they want to bridge it with other fields. Effective procedures also depend on handling the differing characteristics of the studied phenomena. For instance, an experiment based on living material has to be organized much differently than experiments on metal alloys. In addition, dispersed expertise easily increases the number of participants, which puts more demands on coordination.

The difficulty to find common ground is probably the main reason why most cross-disciplinary collaborations in this study seem to have started by discussing potential applications. However, over time the collaborations deepened. It seems that the ability to address basic research problems often takes several years to develop. This is partly due to dependence on external funding, which is difficult to get for such uncertain projects. As a consequence, the researchers can spend only a limited amount of their time – of which a large share often is spare time – on the cross-disciplinary work. For example, for the team of the chemist above it took almost ten years of continual interaction before a substantial grant could be secured for basic research. Socialization is thus a fundamental aspect in cross-disciplinary work, even if some phenomena take very long time to conceptualize well enough.
Self-reflection and trust

Cross-disciplinary work requires that participants not only reflect on the knowledge of others but reflect on their own as well. Several respondents pointed out that for such collaborations to work, participants must be willing to leave their secure home turf and be ready to give up also relatively fundamental principles. They also need to be willing and able to express themselves well, especially verbally it seems. That requires self-confidence and a good deal of empathy. For self-confidence, a solid base in one’s field of expertise appears important.

One respondent looked back with satisfaction on how he and his colleagues arranged their first substantial cross-disciplinary project. Afraid that many researchers might virtually kill the project at an early stage because of frustration in the communication, they selected the participants carefully:

“The first group of PhD students we had [in this cross-disciplinary project] consisted of students who had two years behind them when we got the project, not to have fledglings. That was really smart; they were people who had ‘Can I really make it to a PhD?’ behind them. … We also made sure to have people who were really humble when it comes to communication. Had respect for all people. Who didn’t say ‘Uh, he can’t get it!’ You can’t have that attitude. There is no one who… You don’t misunderstand out of pure ill will! (common laughter). If the other one doesn’t understand, you have to react like ‘Can I do something to make him understand?’ And eventually, when you create this common magic, then you are unbeatable” (11).

Following the interviewee, success in cross-disciplinary work thus concerns not only trust in one’s own capacities but also in the capacities of others. Because skills and perspectives differ more than they do in usual collaborations, the participants cannot very well judge the quality of the work the others do. That can be difficult to some, especially since competition is an institutionalized aspect of many academic environments. In the words of one respondent:

’[A] really important success factor [in cross-disciplinary work, author’s comment] is that you dare to realize that you are not the world’s number one in everything. Go to any presentation, and the professor who talks will flex his muscles and boast as much as he can to show off how damn good he is. That’s how it works. It’s a bit about selling – to show how unbelievably skilled and advanced and good you are. But [in the cross-disciplinary project] we had no possibility to understand each other’s terminology, since we came from two completely different environments. So it took us a while to, sort of, over and over again lower the level” (8).

Moreover, each researcher also has to trust that the partner keeps faith in the researcher’s own competence, even if the researcher has the knowledge of a beginner in parts of the common project. Most respondents with experience
of cross-disciplinary work pointed out that “to ask the stupid questions” is necessary to find common ground and indicated that some researchers are not capable of doing that well enough. Overall, a common observation was that frustration in cross-disciplinary work is effectively avoided if the participants – at least the PIs – enjoy each other as persons. Some respondents argued that it is a necessity.

5.3 Teamwork across the university-industry interface

By and large, the characteristics of “good academic environments” are reflected also in “good collaborations with industry”. The first part of the section concerns the development of connection points and trust in relations with industry. In the second part, two specific ingredients for satisfactory relations with industry are presented: that the academic researcher is interested in applications and that the collaboration fits within the frames given by the academic and industrial organizations. In the third part, attention is paid to a pattern that arises in the material – an identity difference between researchers who define themselves as “applied” and those who see themselves as “basic researchers”.

Common ground and trust

Just like in common projects within academia, collaborations across the academia-industry interface need to be based on socialization, whereby the participating individuals reflect about their own knowledge as well as their partners in order to establish connecting points and common working procedures. And just like within academia, this process should be driven by the participating individuals.

As such, harmony in the relations with industry depends more on the dispositions of the individuals involved rather than on their organizational belongings. One respondent talked about that at length and, similar to a few other interviewees, observed that some university researchers are practically oriented and prefer to work with bullet points to tick when predefined tasks have been carried out, while others who are more theoretical often are less structured and less interested in anchoring their ideas in a specific context. The same, he said, goes for industry. What thus matters, he argued, is what characterizes the problem. As manager in a CoE he therefore arranges interaction between specifically selected individuals depending on personality rather than on organizational belonging. He told about brainstorming sessions:

“It is about people with open senses. They have to be humble, a bit naïve, gullible, believe that things are possible to do. And then one of course has to
enter a phase where one looks critically at what has been said. Those who are really naïve then have to become a bit more cynical and think about… There has to this whole sequence and at the end of the day there has to come out something to try out in the lab. At these early stages there are often 50 ideas one wants to try, that just keep popping up during the whole meeting. What matters at that point is just to come up with a smart way to take notes. Write a bit, and just keep the discussion going. After two to three hours, we have often got really, really far” (11).

The prerequisite is that the participants are comfortable with each other. In this case that is ensured by relatively frequent interaction across the university-industry interface and further facilitated by co-location.

For researchers who have never before worked with industry, it takes time to find common ground. Several researchers told about long periods of communication with industrial representatives before being able to begin any serious research together. One interviewee who did not interact much with industry before becoming part of a CoE said:

”…it takes quite a long time before there is an established relation with industry. And we do not understand each other’s languages. We in academia might have difficulties with languages and so on, but it takes even longer time for industry. Their tunnel visions are even more rigid. The role of industry [in the CoE, author’s comment] has been important, and I feel that it is getting more important now when they know what this stuff is about. They know better what we can do, and we understand better what they want us to do. You see? We have reached some kind of synergy now.”

T: “Yes. After how much time?”

“I would say four to five years.”

T: “Okay. After how much interaction? And what kind of interaction?”

“Well, there was not much interaction in [a previous center]. There we had biannual meetings and seminars where we met industry. Now we have far better possibilities to work closer with industry. I then talk about [the CoE], in which we have focused project groups. That is better I think. We will have better interaction. In the end it boils down to people who have to understand each other and talk to each other.”

/.../

“The meetings. One must not downplay the meetings. There one really can sit down, calmly, and talk with each other. Just to be able to bring in new stuff. Otherwise there will be business-as-usual, and that is perhaps not very exciting.”

/.../

“I thought that it would go much faster to develop relations with industry than it does. I mean, it takes time in academia, but it also takes time for the firms. Also in those cases you learn – sometimes the hard way – what has future potential. Those firms are the ones with whom to develop long-term relations. And I say, over and over again, that one must not underestimate the
time it takes to establish something like this. There is, somehow, so much trust involved” (7).

As such, the interviewee argued that although reflecting about each other’s knowledge and situations is important for a fruitful relation, it is also important to show in action that each other’s perspectives and interests are recognized. A similar view was expressed by a basic researcher:

“I think the best is to have continuous contact for a longer period of time and really make friends first. … It is not possible to just tie together people who do not have sufficient personal chemistry and say ‘Now you are supposed to produce!’ It has to be that you really feel that you understand each other and sympathize on a lot of fronts, and that you have the same interest in the particular problem so you make phone calls and talk to each other if you have found something exciting, so that there really will be reciprocity. Then it becomes fun … Due to my extrovert personality, I have had enjoyable contacts with industry, people who have enjoyed it even if it has not ended in a patent or a product. We have quite simply sat down and discussed problems together” (1).

In contrast to the previous interviewee, this respondent emphasized more the weight of sharing perspective. While the previous interviewee focused on finding connection points whereby phenomena can be interesting from both academic and industrial perspectives, the second respondent expressed a preference for the development of science. The difference might, however, partly be explained by differences between their fields; the field of the second researcher is relatively close to applications.

Preconditions
Satisfactory collaboration with industry rests on two preconditions: first, that the university researcher is interested in applications and second, that the academic and industrial organizations enable room for a suitable project. These points are considered in turn below.

Interest in applications
On the part of the academic, success in the collaboration depends on her interest in applications. This point was commonly observed by the interviewees, including by two who had worked with industry primarily to get research funding. In this study, most respondents were interested in applications. Some indicated that the interest was a “moral obligation” to the other parts of society, and rationalized it by claiming responsibility to, for instance, a public funding body or the Swedish taxpayers. Others pointed to their employment at an institute of technology or applied department – employment that some of them chose just because of an interest in applications. Yet others pointed to a general interest in seeing their research materialize,
most clearly expressed by a respondent who defines himself as an applied researcher:

“[A]n article in a good journal is satisfying. It gives me satisfaction in my job. But there is nothing which gives as much satisfaction as when someone puts my research into use. That speaks to my vanity. That’s how it works” (17).

Most interviewees rationalize their interest in applied research by indicating several of those reasons.

In addition, two respondents claimed a more strategic reason for their interest in applications: that it often enhances the scientific quality. One of them said:

“I find research for its own sake completely uninteresting. … There has to be, also in our group, research purely driven by curiosity – ‘What is this? We want to learn more!’ – that kind of question. But I find it meaningless to have only that kind of research in the group. It usually does not lead anywhere. It is also that we have got many of our best research questions in discussion with industry. I mean, they come with pretty tough questions” (11).

He continued by telling about a project he once had with a large firm, which told the researchers what kind of applied technology it was interested in and specified the very tight cost-frames estimated for the technology to be commercially viable:

“That really was a great lift for us. It lifted us to a level which we otherwise would have reached. It is about either push oneself or pull oneself into the unknown, all the time” (11).

However, probably no respondent would like to sacrifice academic participation to focus more on applied research than they presently do. Many of them would probably agree with this statement from a researcher in materials science:

“[O]ne does not conduct research to find [commercial, author’s comment] potential. One conducts research on a topic one finds really interesting. But it is very important to identify the commercial potential in that research and preferably create those firms that can be created based on the academic environments we have. I think that is very important to take part in and try to engage especially younger colleagues in those ideas and realize them” (29).

Opportunities

Besides an interest in applications on part of the academic, there should also be an opportunity for both a firm and the researcher. An opportunity requires sufficient understanding of each other’s perspectives, intentions, and preferred working procedures. It also requires that the firm and the university
agree on the format – for example, that it is possible to finance a PhD student for four years on a specific project that satisfies requirements for both science and the firm. For those reasons, it often takes time before a collaboration is possible.

One interviewee told, for example, how his interest in problems faced by firms in a specific sector – for long periods primarily satisfied via discussions without any specific applications in mind – eventually resulted in a quite radical new technology:

“Suddenly one just sees ‘Hey, there is a solution to…’ but that moment really is preceded by quite a long process. It actually took me 15 years to get to this invention by means of different kinds of research. There was applied research on [x], there was very basic work on [y] and then some demands from industry that this was a problem they were interested in solving. And so every piece fell into place. We had a possible solution” (27).

Similarly, some other respondents also emphasized the need to understand the often long periods required before opportunities arise to link research with industrial needs. To that end, several respondents claimed to regularly devote time – which often is unfinanced – to discussions with industrial contacts in order to be updated about problems and conditions in industry.

Differences between applied and basic researchers

There are indications of differences on the points above between respondents who define themselves as “basic” and “applied” researchers. Basic researchers seem more focused than applied researchers on their scientific research interests and less on the interests of the firms. On the one hand, the differences are natural, given that the problems posed by applications and basic science require different perspectives, and as such, different identities. On the other hand, it is a notable aspect, given that also most of the interviewed basic researchers had, for various reasons, engaged in relations with industry. Some indications of differences are presented below.

Typically, applied researchers are relatively prone to express success in collaborations in terms of what it brings to the firm, such as “If the firm looks positively at what we do, and they get value for their money, it might develop to a far longer relation than just a PhD project” (22). That perspective was similarly expressed by another respondent, who usually develops technologies for partner firms:

“If I do not succeed in commercializing something I do, then I feel quite failed. I’ve got to say that. But this, what I do, it has at the same time been really lucrative: I have been able to publish a lot. A lot of papers. Both I and my PhD students have presented a lot at conferences, and we have really done well since we are viewed as one of the best groups in the world in [the
particular applied field]. And if we then do not succeed in commercialization, I tend to think ‘Ok, but what was then the meaning with this?’ ” (5).

These two quotes might be contrasted with the quote in the end of the first part of this section (5.3), in which a basic researcher explained success in industrial collaborations by focusing on the possibilities to address scientific problems.

The emphasis on supporting industry is further visible in the arrangements of collaborations. Most applied researchers seem to arrange their research in relatively specific projects, and although organizational aspects sometimes makes matching difficult, they seem to spend less effort on finding common ground for collaborations compared with basic researchers. One applied researcher told about how collaborations are developed:

“[O]ne example, which is very typical – I have for a long time had really good contact with [Firm x]. I have contacted them and asked if they wanted a research project with me. They have been visiting me, I have given ideas to them, I have been invited to them to talk, I have sent project proposals to them, they have replied ‘That was fun and interesting; we will take it into consideration…’ And then after 2.5 years – this goes up and down, sometimes it is intensive, sometimes not – we came up with an idea that fit with [this CoE]. So now I have a PhD student from them. … This stuff – you need to be outreaching. There has to be good personal contact. You have to have ideas. And patience” (17).

Somewhat contrasting, basic researchers tended to put more emphasis on the weight and challenge of finding connection points that enable scientific exploration. That indicates an experienced longer relational distance between them and industry. Moreover, in the interviews with basic researchers research collaborations were generally expressed less in terms of single projects and more as long processes of which projects were the natural organization format. One of the clearest articulations of such a perspective comes from a basic researcher in materials science, who argued that collaborations concern mutual development of basic science, and typically have to be preceded by five to ten years of interaction, which is not actual collaboration:

“The intention is that [relations with firms] are supposed to go on for a long time. We enter the relation with these intentions reciprocally. Sometimes firms come to us and we do not feel that harmony. Sometimes we come to firms and they do not feel the harmony. Then there will be no collaboration. But we are really careful, and that is sort of my role, to be a policeman and say ‘This does not feel good. We should end this relation because it takes too much energy from us’. We then use that energy for our own purposes, for that we want to do”...
“[Over those five to ten years] we have had a few rounds. Typically they have employed two people from us. We might have borrowed one of their employees, sometimes even employed . . . . Typically we have made a few patents together. Typically those innovations have become incremental, partial improvements of their products. I cannot say that there has ever been a new product. In some case there has been a shift in technology” (30).

To what extent the differences are part of a strong pattern in academia is, however, not possible to estimate given the small sample and exploratory approach in this study. Moreover, any pattern would also be influenced by characteristics of the phenomena studied. Collaborations between academics and industry are further discussed in chapter seven.

5.4 Chapter conclusion

The discussion in this chapter indicates the importance of lateral authority. It enables researchers to control time and space. In research, that includes emphasis on the right to define problems and methods, and to influence plans for the future. It also includes possibilities to interact with skilled others, near and far. Moreover, it also includes assistants, which in most cases, and somewhat contradictory to the idea of lateral authority, largely refers to PhD students, partly because of lack of resources for administrators and lab assistants. In relations with industry, it concerns the abilities of the researchers to orient themselves about applications and find opportunities for collaborations. The more control of their actions and contexts, the more room for their personal knowledge, and the higher the motivation. Lateral authority enables the researchers to use as a fixed point the phenomenon they themselves find most valuable to focus on.

Control over time and space also explains the common observation that at all the organized events and activities the unplanned time – the “slack time” – is as important as the formal work time. At work, that means coffee breaks and other gaps. At conferences, the time between the scheduled sessions seems at least as important as the sessions: interested listeners might approach the speaker afterwards, and many researchers find the time offered at a dinner or over a beer long enough to sow a seed that might grow into future collaboration. In two cases long travels – once by car, once an intercontinental flight – were mentioned as gaps during which path-breaking discussions took place. If researchers can control their time and space, they can, both in the lab and at conferences, display their interests as precisely as they like and create the specific situations they prefer.

A researcher’s identity is developed in reflexive relation to her context. An important aspect is that it is shaped in relation to the studied phenomena. Focus on the specific problem informs the researcher about which position
she should have. The relevance of that point is indicated by some differences between basic and applied researchers. The context also includes interaction with other researchers. Much of that discussion concerns face-to-face interaction. Socialization is most effective in face-to-face interaction. Throughout the chapter, emphasis has also been put on the importance of finding common ground, connection points, and working procedures. That increase participants’ trust in each other and strengthen their abilities to sense the future. In teamwork, it reduces relational distance between the participants.

In the local research environment interaction enables the researcher to observe skilled others and find out in detail about what constitutes those skills. Senior researchers seem especially important in this respect. At conferences it is not only important for effective discussions on research – it enables researchers to find out about each other’s potential to do well in a common project, including how well they fit together socially. Just “to get a name and a face” of someone seems to be enough for many researchers to ask the other for favors.

This chapter was mainly about “the shiny story”; about characteristics of social interaction that is largely positive for research. The observations will in the next two chapters be coupled with, and partly contrasted against, examples that often might be a bit unproductive – either as a relatively immediate consequence of the interaction or for the academic research system in the long run. Chapter six concerns relational tensions in academia. Chapter seven, which is similarly structured, presents relational tensions in the interface between academia and industry.
6. Relational tensions in academia

This chapter concerns relational tensions experienced within the academic sphere. That includes within the scientific fields, local universities, and funding structures. Relational tensions occur when actors with different perspectives interact with each other. The most visible cases of tensions arise in contexts where only one perspective is acceptable.

Research policy represents, as noted above, a market perspective, partly because of its interest to satisfy industry, partly because it is influenced by economists’ ways of defining and ensuring quality. Competition and expert evaluations are thus emphasized. Competition is also fuelled by a general shortage of resources in relation to the number of researchers. Competition pits participants against each other, and thus increases the likelihood of relational tensions.

This chapter consists of four sections. The first section concerns what makes a researcher a legitimate participant in academia; it is argued that legitimacy comes with having one’s capabilities positively valued by insiders. In the second section examples of relational tensions in academia are identified. The third section concerns how relational tension is coped with. Socialization, lateral authority, and seclusion are identified as the three kinds of strategies that are used; while the first decreases relational distance, the latter two are strategies to continue with a sustained distance. The fourth section is the conclusion.

6.1 Legitimate participation in academia

To be part of academic networks an individual must be recognized as a legitimate participant. Legitimacy is connected to the possibility to conduct the research the researcher prefers. The issue of legitimacy increases in importance if the competition for positions in a social space increases. In academia competition for positions has increased since the early 1990s. In this section, two aspects of legitimacy are identified as particularly notable. In the first part the need to be recognized as capable is introduced. The second part concerns the issue of presence. Legitimate participation is the fixed point in science.
Capability

Participation in academia is fundamentally most connected to peer recognition – being recognized by others as being capable of doing good research. On this point, two aspects can be observed. First, insiders in academic networks must find the other individuals in the network potentially capable of fruitfully stimulating discussion in the network. It is thus not possible to enter advanced scientific circles – which often comprise only a small amount of individuals – unless those within that circle find the newcomer advanced too. A respondent in medical science told about how his time as a post-doc at one of the world’s finest labs made him realize how it is only a few researchers that compose the top in his field and realize what it takes to get there – and what being part of the top brings back in the form of insights into groundbreaking ongoing research and refereeing of pioneering studies that are not yet published. Since then, he claimed, one of his strongest desires has been to get back into these circles.

Second, the researcher must control a sufficient degree of resources. The most important resources are funding, instruments, and junior researchers – the latter since in most fields it appears impossible to be internationally competitive in the long run without being a PI. The competition for such positions is often strong; the available resources are relatively limited compared with the number of potential PIs. As a consequence, researchers also compete with peers or researchers from fields that are very different from their own. The competition occurs within their own local milieus as well as in the broader research system, for example, within universities or in relations with potential funders.

Resources are available in different ways. To some extent, they are linked to formally instituted positions, such as professorships. As expressed by one interviewee, who left a world-leading, well-equipped group for a professorship in a worse environment because of a desire to be “free”:

“[By free I mean] to be able to influence in which research projects you are involved. And have a possibility to influence how that research will be conducted. In our case, with lots of experimental work, the question concerns having a strong voice regarding ‘What equipment do we need?’ And then the question is what applications for funding you have to write, which contacts you have to make, in what place you have to be. To be sure things go the way you want, that you need to get…”

T: Then it’s good to be a professor?

“Yes. That helps” (2).

Access to resources is also tied to one’s internal status within the university. Several interviewees claimed that that kind of competition was rather intense at their universities and portrayed it only in negative terms, as creating an
unfriendly atmosphere. A couple of respondents claimed, for example, that in their local environments individuals without good publications might be ignored by some colleagues, sometimes even in informal social settings. Examples of how such status hierarchies negatively affect the research environment were observed to include not inviting others to take part in one’s funding or research contacts and an unwillingness to inform others about ongoing early-stage research. The degree of internal competition seems, however, to differ considerably between universities and different environments.

Presence

Competence concerns the connection between one’s person and the social value of one’s knowledge. The link makes the researcher “present” in the field also when she is physically absent and means that she can, in effect, make her voice heard over large areas and in extensive networks. It also means that the researcher does not need to start from scratch at every instance, but can rely on previous performances. The strong competition and often rapid progress in the field, however, induces academic researchers to prove their competence relatively frequently. The link between one’s person and one’s knowledge is typically established in two ways.

First, the link can be established via one’s name on a published paper. On this point, one of the older interviewees mentioned how he had received very little recognition for his most important work, a method which greatly improved the core technology of the field. In the early 1980s he presented the finding verbally at an international conference without ever publishing any paper or taking any patent. The finding rapidly spread among researchers and firms and soon became international standard. However, without publication there was no link to the interviewee. He told about his recently received honorary award for his achievements in the field:

“They said it was for my work on [x]. And [I replied] ‘This is interesting. It’s a method of minor importance, not much spread. But [my work on y], that’s in every [one of the world’s most spread appliances]’ (laughs). About half the group, the older researchers who were active at the time, they laughed. But the younger researchers had no clue about it (laughs)” (20).

Second, the link between the person and the knowledge can also be manifested by an embodied presence at specific events. In the previous chapter it was argued that face-to-face interaction is often necessary for efficient learning. In addition, such interaction also increases the chances of the specific individual getting to represent specific knowledge and being included in academic networks. This implies lots of travelling; many interviewees travel internationally at least once per month. Most of the important places (such as
labs or conferences) are in the U.S. Although the most frequent travelers pointed to the need to catch up with the knowledge frontier, a couple of interviewees who travel less – for example, for family reasons – noted how that makes them a bit peripheral. Moreover, embodied presence is also a key to attaining influential positions in research bureaucracies. Presence and participation can positively influence the chance of more resources in the future. Many respondents claimed that was part of the reason they participated in evaluations or distribution of funding, in expert panels, or in faculty boards.

The social value of a researcher’s embodied knowledge might also decrease. On this point, several interviewees pointed to a need for formal positions in the research environment to harmonize with their informally recognized competences. Moreover, many interviewees also observed how they or others judge the competence of others to a significant extent based on their publications. This link between quality and publications differed a bit, however; a few emphasized high frequency, while a couple of others argued that deep thoughts should be indicated by ups and downs in the number of publications.

Another example of the risk of decreased status is provided by a story about a faculty meeting. One participant argued against increasing the number of female professors so as not to risk status deflation of the profession. Nobody opposed the comment, most probably because nobody was willing to take personal responsibility for a protest. The interviewee commented that silence is typical for such meetings: “Why don’t they say anything? Because you are lurking and skulking to defend what is best for [your interests].” The interviewee later added:

“I have to realize, ‘No, I should be quiet, so that I won’t become the problem instead of us focusing on the actual problem’. I then sit down, back off and perhaps I won’t say anything at [the meeting] but let the question pass” (8).

In other words, just as competitive environments with flexible structures increase the potential benefits of associating one’s person with “the good”, they also make it more important to disassociate oneself from “the bad” and encourage actions with shorter-term consequences and social relations with less lateral authority. At the same time, pitting participants against each other also increases the likelihood of relational tensions.

6.2 Relational tensions in academic spaces

Relational tensions within academia are visible on three levels. This section is separated into three parts, which deal with one level each. The first part concerns relational tensions within the scientific fields, including its funding
structures. In the second part tensions between researchers within the same universities are discussed. The third part concerns tensions between academics and funders, which also represent non-academic – for example, industrial – interests.

Scientific fields

Within scientific fields, relational tensions often become visible when different perspectives are not tolerated. This lack of tolerance is due to two main reasons: because individual researchers conceive differently of the same phenomena and because organized academia represents history. The specific fields have been recognized, shaped, and instituted over a long period of time. Along the way formalized structures have been erected, such as journals, academic societies, and committees that distribute funding. This development has also been supported by policy-makers. Sometimes, also, industries have developed. All these make scientific fields more rigid and organized, and led to the establishment of a core and a periphery in the fields. For researchers in the field that means – for better or worse – less uncertainty about the future. It also means that the ones who belong to the core are more influential than others. For that reason, breakthroughs typically occur in the periphery. This implies a power imbalance; if peripheral researchers are to progress to the center, they generally have to adapt to the central researchers more than the central researchers adapt to them. If a central and a peripheral researcher conceive differently of the same phenomena, and both insist they are right, it is more likely that the central researcher “wins”.

Relational tensions in scientific fields are well illustrated with the help of two stories from the empirical material. The first individual is referred to as “the biologist”, and the second as “the physicist”. The biologist has faced problems because he has been doubly peripheral. For long periods he has problems getting funding, since in the Swedish funding system his specific and quite small field was not recognized in its own right, but as a border zone between two other fields. For several years he was also, he says, in effect left outside what he saw as his own field, since he did not believe in the dominant fundamental theory. However, after a few years there was a substantial shift; most researchers in the field gave up the dominant theory and turned to the theory in which the biologist had all the time believed:

“[A]ll of a sudden there were 2000 researchers interested in… and talked about how to do [what I and my two to three colleagues were doing]… So then, all of a sudden, this, which we were doing, was an existing field (laughs)” (14).

In that way he became established internationally. However, he still had problems getting funding. Until then he had made his living first by working
in an office and later by teaching, while doing most of his research during unpaid hours. Following the breakthrough, he continued to have very limited funding, but nevertheless was able to intensify his research by, for some time, living on the wage of his wife. It took about five years until he found it easier to get funding. The reason, he says, was that funders eventually began to recognize the border zone as a distinct field.

About ten years after the paradigmatic shift, his theoretical approach was again challenged, he observed. Neighboring fields had grown stronger and begun to address the same problems. The result has been that his field and the approaches it represents have become a bit more marginalized. This time, he argued, it is not a question of good or bad approaches but rather a question of fashion:

“If people have been doing something in a certain way for ten years, then people start to think, ‘is it really possible to do this?’ And so the paradigm is changed, so to speak. That’s at least how things have been within this field” (14).

However, regarding funding, the downturn seems not to have affected him negatively to any significant extent. For many years he has collaborated closely with leading researchers in adjacent and well-established fields, and the funders now contact suitable referees:

“The review process, at least nationally, was problematic for a long time, even if we occasionally received grants, which is positive, of course. But many … got review statements indicating that… [the funding bodies] did not use reviewers who knew anything about our field, … They have used reviewers who have been central in [neighboring fields]. Now things have changed. They know there is a border zone. They use reviewers from there. And all of a sudden: an application that previously would have been viewed as mediocre or vague is suddenly world leading, and so on. It is pretty much the same application and it is the same people who wrote it. That makes… [y]ou start to think – what is going on? What kinds of dynamics direct this?” (14).

The second researcher, the physicist, also struggled to get recognition within the academic field and among the researchers who evaluated his funding. The fundamental reason for that, he said, was that he was squeezed between two large groups of researchers. He presented his specific field as largely split in two halves, one dominated by U.S. researchers and one by Europeans – and each community as dominated, to some extent, by different views of what is “good” and as organized into separate academic networks that run their own conferences. Since the physicist quite radically changed the direction of his research about ten years after his PhD, he happened to enter the field with the perspective of the U.S. community and with contacts mainly based in the U.S. The European community of researchers largely ignored him, he said,
“There were no connections. No one who... I got no invitations, it is as simple as that. ... My first large international lecture on this stuff was in Spain 1995. I was then invited by [researcher x]. He was a rebel; he wanted to reform [the European network]. So he invited me to give a lecture. I got the finest lecture of them all and in [the European network] that’s like... Then I had [everything finished]. Everything was new and came from nowhere. It went well. But that was thanks to that researcher. Had he been the traditional type I would never have been there” (26).

His different perspective also gave him problems with funding. The peer reviewers in his research council did not believe in his approach, and perhaps not in the rather uncompromising way he pursued it. Two years after he received his second grant for that line of research, the research council terminated his grant:

“They said, ‘This is not good enough’. And then we were actually the first ones in the world doing this kind of thing. ... You ask about breakthroughs. For me, the breakthrough was in 1995 when we had produced [that which was presented on the conference in Spain]. But the [research council], they said, ‘No, we won’t have this’ and withdrew the grant. But on my part, [a private foundation] stepped in ... and provided the missing money, a few hundred thousand SEK. I got my grant again when we started to publish this stuff” (26).

An important reason, he thinks, for why the grant was withdrawn was his lack of publications. It took several years before he and his group attained publishable results. A couple of his PhD students even graduated with monographs. Today he partly regrets his strategy:

“I made a tactical mistake. I should have done some minor stuff to the side, and got some of what we call ‘bread papers’: simple articles which might not have any real scientific value but which at least are on the list of publications. But I didn’t do that. I said ‘Now let’s put all resources into this’, because it was pretty hard and challenging. And, at the same time, that’s what I wanted to do” (26).

However, he does not regret the choice not to establish strategic contact with disagreeing peers, who could have paved his way a bit: “But then I might have been influenced. Then I might have had to step into the line. Now I did what I wanted to do myself” (26).

What the stories of the biologist and the physicist show is that scientific truths are not unequivocal, and that in such cases there is an apparent risk that the more peripheral researcher gets more marginalized or perhaps even excluded.
Universities

Relational tensions that easily may take form of conflicts between researchers often arise also in the local environments, primarily when the available resources will be distributed between researchers or groups. Inevitably, such decisions have to involve a comparative valuation of those researchers and groups. Several interviewees engage in management of the department or the university, partly because they hope that will give better opportunities for the research they prefer.

Similar situations are evident also in several CoEs, as the resources at some point have to be distributed among individual researchers. Several interviewees noted that a few senior researchers in their CoEs had much influence in those situations. Some CoEs have been organized into, in effect, another – but more informal – funding body, in which the PIs or the junior researchers send project proposals to a board consisting of all or the most senior PIs. One respondent argued that in turn the potential in highly motivated and relatively risk-averse junior researchers tends to be neglected, and another – comparably young – respondent noted how he, by teaming up in a CoE with a well-established senior professor, had gotten great career opportunities but at the price of never getting the chance to pursue his own line of research. These comments could be linked to the observation above that several interviewees had turned down opportunities to work with resource-rich, well-established seniors because they wanted independence.

Engagement in research management also implies less time for actual research. While this trade-off has – as described in the previous chapter – often resulted in a division of labor in the research environments such that PIs often do not do hands-on work, several respondents found that situation unfortunate. A few of them had therefore deliberately chosen to keep their groups smaller than most of their peers, even if it meant that they were to become less internationally competitive.

Directed funding

Finally, relational tensions in academia also arise when funders direct funding to specific fields or distribute it with requirements such as expected utility for firms or public healthcare. In those cases funding is awarded based on incommensurable aims: scientific goals have to be weighed against applied relevance. In addition, the review assignment is typically split between evaluators with dispersed expertise, with inevitable arbitrariness when the accounts are weighed together. This is typical for CoEs, which are often based on complex proposals with multiple goals.

Thus, academics have to negotiate between scientific and other interests, and it might also be difficult to predict on what grounds research proposals will be evaluated. One interviewee told about a case that rendered him and
about ten colleagues much frustration. For more than a year they had been negotiating with each other and a funding agency about the possibility to set up a large research center. Eventually, they were invited to submit an extensive proposal that was – to their surprise at that point – rejected. It turned out, he says, that it was rejected largely because an influential representative from industry found the suggested application uninteresting. A few years, with very similar scientific content but with a new suggested application, they received funding. At that time, the researcher says, the previously suggested application was being developed abroad. In being declined funding in the first place, they lost several years in a fast-moving field and wasted much time and energy.

Common to all three cases of relational tension in academic spaces – within scientific fields, within universities, and in relation to directed funding – is that legitimacy is partly dependent on approval in formally instituted structures. The effect is that some actors function as gatekeepers. This differs a bit from research collaborations. Typically, in a research project all partners mutually adjust to each other, and if not, another partner might be found. Regarding funding, it is practically necessary to get approval from those in gate-keeping positions. As indicated by several of the examples in this section, conflicting perspectives between researchers and others in gate-keeping positions are sometimes settled not with reference to a scientific truth or to good intentions but by compromises or by a unilateral decision by the stronger member(s).

6.3 Coping with relational tensions in academia

Relational tensions can be dealt with in several ways. The most fundamental dividing line between the various approaches concerns whether or not to adopt the perspectives of others – whether or not to handle the tension by decreasing the relational distance. On that ground, three ways to handle relational tension have been identified. The section is divided according to these three. In the first part socialization – to decrease relational distance – is discussed. Four socialization strategies are identified. The second part concerns lateral authority, with a focus on how the central actors can cope with relational tensions by granting more legitimacy to differing perspectives amongst more peripheral researchers. In the third part seclusion – strategies to avoid interaction – is discussed as another means to handle (potential) relational tensions. It should be noted that socialization, lateral authority, and seclusion are to various degrees relevant in virtually all cases of relational tensions.
Socialization

One way to handle relational tension is to try to change one’s conceptualizations by internalizing the perspectives of others. This is to some extent a natural consequence of long-term interaction, and part of the legitimization of scientists. However, it reduces relational distance and might result in a researcher changing perspective not because the other is closer to reality but because she trusts other individuals more than herself. Socialization can be observed in several examples in this chapter. Identified aspects include, in the order they are presented below, scientific gate-keeping, developing organizational knowledge, compromising, and informing others.

Scientific gate-keeping

In science almost all interviewees seemed sensitive to situations in which they were expected to conform by socializing. This is indicated, for example, by the high value put on independence. One of the more extreme examples is the physicist mentioned above who did not even want to establish links with those he considered to be wrong.

Throughout this chapter there have been examples of holders of formally or informally instituted central positions who have exerted their vertical authority in disharmony with the lateral authority represented by others. While such actions are important as part of the quality assurance of science, they might also be applied in ways that can appear too strict. Such examples include the biologist and the physicist in relation to gate-keeping peers in the fields and in funding bodies, and several interviewees in relation to senior researchers in their universities and CoEs. In such cases the less well-positioned individuals had strong incentives to comply, even if it was against their conviction of truth, or they would risk exclusion. In some cases the well-positioned individuals made scientific judgments that later were widely recognized as ill-informed.

Organizational knowledge

Many interviewees were less sensitive to socialization when it came to positions in the university organization than they were with scientific issues. Quite a few recognized how good knowledge of the organization and of research management could help them to attain a more stable position, which in turn could support their scientific efforts. Organizational knowledge largely concerns relatively standardized procedures. For example, this interviewee perceives a relative distance between research and management, but engaged in the latter for strategic reasons and because of encouragement from the senior researcher in the research environment:

“Today, being a really good researcher is not enough. It is a bit absurd, but you have to be much more than that. You have to be good at selling your projects, you have to be a good business administrator to keep control on the
economy in the projects, you have to be able to communicate the projects with firms and all the others, and then you of course have to have a platform in the organization where you work.”

“I saw a bit like investing in creating an infrastructure that will enhance research in the long run.”

“The inner academic career, you know. That was great, it made me take part in how the university is run, how things work, made me learn how decisions are being made, and so on. And that has really been a fantastic asset. I mean, I look at colleagues in my own age who not at all had the same support to engage in how the university works” (7).

Some other interviewees expressed similar views that knowledge about the organization and management is a good way to cope with relational tensions in the organization. In several cases seniors had encouraged them and opened doors in the organization.

Another aspect of “organizational knowledge” is when researchers orient themselves about funders. One respondent argued that success with both applications and research largely depends on the researcher reflecting carefully both on her own intentions and the goals of the funder. If the researcher reads instructions cautiously and makes sure to learn about the funder, perhaps by making contact with its representatives, she is more likely to present herself as aligned with their goals. Thus, he advocated strategic adaptation combined with the aim to as much as possible keep intellectual independence.

Compromises

Another aspect of socialization is when relational tensions are handled by making compromises. Compromises are made in situations when multiple, incommensurable goals exist, or when the time is too short for a genuinely shared perspective to be developed. Examples include strategic adaptation by acquiring organizational knowledge. Another, quite typical, situation is when actors with diverse interests accept full or partial democracy as a way to make collective decisions, on the premise that everyone must accept the decision. One example of when compromises are made is in faculty meetings. Another example is when research is arranged to satisfy both scientific and non-scientific interests.

Furthermore, one interviewee also mentioned how she strategically created explicit relational tension as a means to achieve a compromise that would enhance her position. When her research group had secured a CoE-grant, she used her position as a permanent employee to put pressure on the leading researchers, whom she felt had ignored her:

"I said that I was disappointed, and wanted them to tell me whether or not I was at all wanted in the department and that if I was, what was I supposed to
do there. I also told them what I wanted to do, and what competence I had. I had such a meeting with them. And that got things going. I got my own research group in the center, and I remember how the director of the center put my project on top of the list. A great pleasure” (5).

Such moves are only possible when research grants are shared between several researchers. For the individual researcher, compromises can thus render her either a stronger or weaker position. However, to follow a compromise is to reduce relational distance, as too much deviance from the decision is viewed as illegitimate.

**Informing others**

Another way to cope with relational tension is to inform others about one’s views, hoping that the others will be convinced that one’s perspective is superior or at least worthy of support. This approach is widely applied by the respondents within science as well as in research organizations, largely as part of ordinary research communication and occasionally also in more strategic ways. Generally, the more important it is that another actor sufficiently values one’s perspective or position the larger the incentives to approach that actor with strategic information.

In the material for this study, strategic information appears particularly common towards funders. Several interviewees found such actions important for dealing with public research funders and with industry, often in relatively informal and indirect ways as one aspect of many in the discussions with funders. The communication channels are opened in both ways, as funders also want to know more about specific aspects; several respondents had roles as formal or informal advisors to public funders or firms. One interviewee, who was very explicit about such activities, says:

> “Every time I have the opportunity to talk to industry I talk, as simple as I can, about how really important [my research field] is. Then... there are many levels. And, of course, I am often contacted when public funders form their strategies. Then I, of course, try to help them to understand, on different levels, what I think is important” (27).

Another respondent mentioned specifically strategic information towards the EU as important, since the EU often opens calls for proposals in specific fields after being convinced by explicit lobby groups.\(^\text{19}\) He had therefore travelled several times to Brussels to push for the recognition of his field.

Overall, strategic information often concerns a genuine and not much concealed intention to inform stakeholders about the research and about specific wishes the researchers have, in order to make the situation better for

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\(^\text{19}\) This is partly changing following the opening of the European Research Council in 2007.
others as well as the researcher herself. But a system that relies on these types of channels is also open for several unintended consequences. First, since it often occurs in specific networks in which size and central positions are privileged, researchers representing small or less recognized fields might get problems. Second, if less significant actors are adversely affected, strategic information might cause lock-in effects. Third, since the incentive to manipulate the other part is a well-known aspect of strategic information, the parties have reasons not to fully trust each other. On that point one interviewee observed a problem: when he expresses what he finds good for science and society in general, others sometimes seem to hold what he says are strategic and less trustworthy expressions of self-interest.

Lateral authority

Another way, in addition to socialization, to cope with relational tensions is to employ lateral authority. While lateral authority can be to conform – which will be discussed below – it is here referred to as a characteristic of the more centrally placed actors. In the previous chapter it was argued that mutual lateral authority implies that diverse conceptualizations can more easily coexist in collaborations. Friendly social relations, freedom to choose collaborators, and seniors as role models were identified as characteristics of positive research environments. Cross-disciplinary collaboration in basic research was presented as a good example when lateral authority was useful.

Transferred to the context of this chapter, lateral authority could, hypothetically, include less frequent requirements of “evidence” of one’s research abilities, those in gate-keeping positions being more sensitive to the limits of their own knowledge, or including more funding for research in a university employment.

Seclusion

In addition to socialization and lateral authority, interviewees also used a third way to cope with relational tension: they restricted the interaction by utilizing spaces that were somewhat secluded from other actors. Three kinds of strategies that to some extent use seclusion can be identified.

First, some interviewees had ignored or avoided peers or others whose judgments they did not really trust. One example of this strategy is many respondents’ preference for environments without a dominant senior researcher. Specific examples include the abovementioned biologist and physicist, who both pursued their eventually well-recognized research despite criticism or lack of interest from influential peers.

Second, a few respondents expressed how they periodically have done much work in their spare time. In the previous chapter, control over one’s time and space were observed as important in research. When not enough
time for research has been included in the regular employment, unpaid hours in the evenings, weekends, and during holidays have been used for research. Several respondents also observed that it is much easier to find undistracted time outside office hours or at home and often saved tasks that require much concentration for such moments.

Both points imply that the researchers might have problems funding their activities or getting access to other resources. In some cases ordinary employment – for example, mainly as teachers, or as participants in other projects – had been used to get access to the premises, which enabled them to do laboratory work in odd hours. In other cases funding had come from more unorthodox sources. The abovementioned biologist lived for a while on the wage of his wife, while the physicist, when his grant from the research council was withdrawn, received money from a small private foundation that used other reviewers. Others in similar situations, especially if the time to publication would be longer, might have had to give up.

Third, some researchers also used secluded spaces in order to conceal their intentions. One respondent secured tenure by using knowledge about the organization, disconnected spaces and a temporally strong position. When he had just secured a large five-year grant, he contacted two attractive departments at different universities, told both of them that three universities wanted him, and that the one which wanted him to settle there had to promise to open a call for a tenured position specifically directed at his competence area when the granted period was over. At the university he preferred it was against the common procedure do make such promises, but he succeeded and got a special contract. He would, he said, never have made such a move unless good contact with an administrator had given him useful information about the university system.

Some other interviewees had strategically used secluded spaces in their communication with funders. Researchers also take advantage of the relatively limited insight that funders have into their situations and intentions. One interviewee said, for example, that in order to get funding for basic research – which is all he wants to do, he thinks that he is not really an expert on the applied side – he “sometimes assigns applied needs” (6) to his research proposals, in the hope that nobody will notice his actual lack of interest in developing that side. Another interviewee claimed that it is a widely spread custom to say that non-existent collaborations exist or to set up artificial collaborations with little intention to work together.

A peculiar problem concerns the future plans that research proposals contain, since researchers have an interest in having the flexibility to adjust for unpredicted developments. One interviewee said that in such cases he writes the proposal according to what the system expects, for example, to form a group of researchers. But since there is no guarantee that the research problem will turn out to require precisely that group throughout the whole period, he does not put much energy into keeping the group intact. Another inter-
viewee found that funders wanting to assess the progress of their specific projects sometimes used inappropriate measurements – not least, looking at the activities in the lab, he can often not really distinguish his projects from each other – and might want him to adjust his ongoing research in ways that he does not like. To avoid such situations and to make the funder look positively at him, he therefore communicates his research as if the projects were more separate than they are and as if he works more in the ways they wish him to than he really does.

6.4 Chapter conclusion

Relational tensions in academia become particularly evident when researchers compete for a limited number of positions – for example, for a central position in a scientific field or for funding. In competitive situations researchers defend or strengthen their positions by proving their competence and securing access to resources. Three kinds of responses have been identified in individuals trying to cope with relational tensions: socialization, lateral authority, and seclusion.

Socialization includes acquiring knowledge about the organizations, informing others about one’s perspectives, compromising, and making use of vertical authority through gate-keeping. Socialization implies decreased relational distance and is as such contrary to scientific progress. Somewhat paradoxically, although extensive lateral authority is widely recognized as a desirable characteristic of scientific milieus, several respondents see actions that imply socialization as a strategic means to strengthen their own positions. At least in theory, a personality optimized for progress in basic research cannot be combined with “strategic socialization”. As noted above, to maximize the possibilities for learning is to relate to the world as if nothing can be taken for granted, thereby reflecting carefully on one’s experiences and “being true to oneself” by doing what one finds most correct in the situation. Thus, an important question for further research is to what extent scientists are able to negotiate their identities between different contexts.

Furthermore, socialization is also induced by the organization of academia in scientific fields, funding bodies, and universities. In all those contexts there are individuals who to some extent act as “gate-keepers”, which implies a risk that their perspectives get privileged as newcomers with different perspectives might be seen as less legitimate. Thus, although gate-keeping is an important strategy to ensure quality in science, it might also exclude research that in other contexts is, or is about to become, good science.

Lateral authority is most relevant in terms of central actors who to some extent refrain from using their positions to direct others. A deficit of lateral authority might provide an incentive for strategic manipulation and concealed intentions, as individuals might be unwilling to reveal their true
knowledge. It is not possible to know how common such behavior is in academia, but the more it is believed that it is common or lucrative, the more the behavior spreads. Since increased control mechanisms send further signals of limited trust, it might be that such behavior is only effectively prevented by easing competition.

Seclusion should primarily be seen as a response to uncertainties created by interaction with others. By ignoring or avoiding others, or by providing information to evaluators strategically, researchers have been able to control more space and time than otherwise might have been the case. Seclusion might, however, result in shortage of funding or limited access to crucial instruments and might risk upsetting others if they perceive that they have been manipulated. The next chapter continues in the tracks of this chapter. Similarly structured, chapter seven discusses relational tensions in the interface between academia and industry.
The aim of this chapter is to explore relational tensions that might arise when academics engage in industry. Such engagement might occur via collaborative projects with firms as well as interaction with investors and the market if the academic starts her own firm. Relational tensions in the interface between academia and industry are based on the different perspectives held by individuals and the differences between the two established social systems – science and industry. In the former the fixed goal is knowledge progress recognized by other scientists, while in industry the fixed point is success in the market.

The chapter is made up of four sections. The first section concerns what makes an academic researcher a legitimate participant in the interface with industry. In the second section typical cases of relational tensions in the university-industry interface are identified. These concern both tensions emerging from the two instituted social systems at large, as well as due to the identities of individuals. The third section concerns approaches to cope with these relational tensions. These include reducing relational distance through socialization as well as trying to retain distance through lateral authority on the part of industry and, on the part of the academic, through seclusion whereby the academic conceals some actions from other academics or from industry. In the fourth section the chapter is concluded.

7.1 Legitimate participation in the interface between academia and industry

For a university researcher to be a legitimate participant in the university-industry interface, two requirements must be satisfied. First, she must be recognized as capable of bringing her research closer to the market. Second, there must be an opportunity for her to engage in such work – either when a firm initiates a partnership or when there is an investor ready to fund a start-up initiated by the researcher. These two points are considered in turn in this section. The third part of the section concerns how the link between the researcher and her capabilities are established.
Capability

It is very difficult for an academic researcher to bring her research to market without the help of actors in industry, either a partner firm or an investor. Those actors must recognize the researcher as having the potential to positively contribute to the development of industry. Valuable contributions might be to introduce new technologies in industry, but also to, for example, guide firms through the academic frontier or educate potential employees in the firms. Three aspects can be identified of how capabilities are expressed.

First, as observed in chapter five, the researcher should have a genuine and acknowledged interest in bringing her research towards applications. Most interviewees claim to have that interest – in some cases as a part of a moral responsibility as publicly funded researchers or Swedish citizens, in other cases because it is stipulated in their employment or specific type of funding, and in still others because it is fun and interesting to see their research be used in products or by firms.

Second, the researcher should provide access to unusually valuable knowledge. That way she can be a strategic discussant to the firm. Several respondents observe how, compared with many other Swedish academics, they as world-leading researchers have the particular competence and connections to enrich firms’ knowledge bases. A few respondents therefore claim to be funded by firms partly in the hope that they will participate in discussions and, perhaps, enable the firms to draw on their academic networks.

Third, the researcher should show that she is ready to sacrifice, to some extent, her personal or scientific interests in order to satisfy the interests of industry or customers. Without such a sacrifice, participation in the university-industry interface is usually very difficult. Several respondents – as well as the informants from industry – noted that some academics overestimate the value the firm puts on their skills, or their strategic importance to the firms, and for that reason are less willing to make compromises between scientific and industrial goals than they possibly should be.

Opportunity

If researchers’ capabilities are recognized, researchers and firms might engage in fruitful discussions with each other. However, for a more formal collaboration to take place there also needs to be an opportunity in the academic and industrial organizations. A point of connection must be found that satisfies the interests of both actors. That is the case both in research collaborations between academics and existing firms as well as between academic entrepreneurs and investors. According to the stories of interviewees, possibilities for generating incomes in the foreseeable future seem to be the dominant aim in relations with investors.
Regarding research collaborations, both academia and industry, as organized social systems, have structures that point in different directions. On the one hand, due to the shortage of available funding for scientific interests, researchers might be more prone to look to other funding in order to remain in academia, thus possibly increasing opportunities for interaction. This was indicated by several interviewees, who pointed to applied perspectives as a less interesting but strategically important way to get funding.

On the other hand, the increased competition for academic positions and workload on academics might have made it more difficult to find opportunities to interact with industry. As the incentives to publish increase and the amount of “slack time” decreases, academics might have less interest in engaging with industry. For example, one respondent noted that, with long-term funding from SSF, he can spend his time on explorative research and discussions with industry without collaborating on practical research. With ordinary funding, he would have had to focus more on getting short-term merits via a continual flow of publications, partly at the expense of the visionary dimension. He describes his relationship with industry as a success, in which he has had the chance to take part in relatively deep discussions with representatives from several divisions of his main partner firm. Yet he observed the risk that his recently submitted new proposal to SSF might be turned down, since many evaluators are used to more clear-cut and well-planned proposals than his visionary approach permits.

Presence

The link between one’s person and one’s capabilities in the interface between academia and industry is established somewhat differently than it is in academia. Three ways are identified. First, presence is established by published papers. However, unlike in academia the scientific quality is often fairly irrelevant; what matters more is the possibility to connect with industrial interests.

Second, the researcher might become known by establishing personal contact with industrial representatives. That appears relatively important. A collaborative project is not initiated without extensive discussions. In the discussions the academic is also judged based on her ability to harmonize well with the goals and methods preferred by the firm. Most applied researchers recognize the importance of having extensive personal networks; one of them even pointed to that as one reason why he was recruited to his university.

Third, another way to get recognized is by taking patents or even starting a firm based on one’s research. Firms, of course, look positively at patents and experience in research commercialization. Many firms would not engage in formal collaborations with researchers unless they believed it could lead to a patent. Several interviewees thus noted that previous patents are often
seen as indicators of their ability to conduct patentable research and of their insights and interest in patenting processes. Patents and involvement in start-ups are often also good merits in research proposals to public funders such as SSF and VINNOVA (cf SSF 2004, VINNOVA 2004).

However, interviewees do not patent only to satisfy industry partners or to start their own firms. A few of them patent partly to increase the likelihood of future funding from public funding bodies. One respondent even used private savings to patent for that reason without having any clear idea of what to do with the patent and believed that to be a common practice. Moreover, one respondent saw patenting as a strategic activity in the CoE to mark one’s territory and partly prevent other CoEs from competing with them. A CoE that does not patent judiciously, the researcher noted, runs the risk of losing industrial interest, thereby also undermining future possibilities for basic research funding.

7.2 Relational tensions across the boundary between academia and industry

Relational tensions are commonly observed when academics and industry interact. In this section, three typical cases are observed. In some cases they are identified based on theory. In other cases they have been identified based on stories from interviewees. First, the issue of open or restricted communication is discussed. This is a typical issue in competitive environments, as strategically restricted information might increase the rewards for individual actors. In the second part, the discussion concerns the different views that academics and industrial representatives sometimes have on what makes a research product or researcher valuable. The third part concerns problems that might arise during the collaboration.

Open or restricted communication?

Relational tensions might arise as an effect of the structures of science and industry, in which the former generally encourages more open communication than does the latter. Open communication benefits knowledge development, which is beneficial both for industry and science. In science open communication is also part of the system for quality control, with academics being expected to provide their data to anyone interested. However, competition in both academia and industry means that intellectual property rights are important in both spheres and that incentives to occasionally restrict communication are introduced.

In academia intellectual property is based on author-signed articles containing novel and valued content, and in industry it is most importantly
marked by patents. The degree of success is in both spheres importantly determined by excludability. Thus, there are incentives to communicate strategically. To various degrees, scientific fields are characterized by a growing degree of restricted research communication, either from scientific competition or from an interest in patenting (Walsh et al 2007, Hong and Walsh 2009). In that respect, competition in science and industry induce similar behavior.

However, academia and industry differ in three main ways that might cause relational tensions. First, patents are made at a later stage in the research process than are articles, and once an article is published, its content cannot be the base for a patent. Second, the restrictions on communicating ongoing research before a patent is secured are considerably stricter than the informal restrictions on scientific articles before they are published. Third, in Sweden the individual university researcher has the legal right to patent her inventions regardless of funder. A firm, however, is generally unwilling to fund academic research unless it gets the right to potential patents. On these grounds, and particularly since a patent might generate great incomes, conflicts might arise.

About 15 respondents named personal experience of patenting as an aspect of industrial collaboration, and all reported experiencing little tension. Only one respondent questioned any key aspect of patenting; she observed that patents during the last decade had become more detailed and closer to science, which makes them difficult to overview. The main reason for their acceptance is a widespread understanding of a firm’s imperative to patent, typically expressed like “[A] firm which partly or fully finances the research should of course have the right to innovations” (22). Most respondents also found patenting too demanding in terms of time, money, and energy for academics to engage in on their own. In addition, many of them were not interested in patenting on their own, as it is normally connected to the labor-intensive formation of a start-up.

For those reasons it was common practice to begin each collaborative project by setting up a contract in which the university researchers give up their legal right to commercialize their findings to the firm. Typically, the contract also stipulates that the firm has three to six months to review unpublished papers and to decide whether it wants to make a patent. In some cases the firm also wants to publish as soon as possible, to prevent competitors from taking patents. The university researchers have to be included on the patents the firm makes based on the collaborative research, which might give a researcher a few thousand SEK once or annually. If the firm does not want to patent, the contract often allows the researchers to take a patent on their own.

An implication of the patent arrangement is that it slows down the process leading from result to publication. Despite the often fast progress in science, only two researchers found the arrangement problematic. They found that
they, in order to not destroy a potential patent, sometimes have to bite their tongues and, just in case, be overly watchful to what they say around other researchers about ongoing research. Also, proposals for funding have to wait until patents are secured. One interviewee told about how she once ruined a patent for an industrial partner by submitting a proposal too early, which she found very embarrassing.

There was only one respondent who worked with firms without having the prearranged three to six months of review. Instead, he discusses each case with the firms, not to risk a good working relation. Given the extent he is willing to negotiate the restrictions on communication, he is an outlier among the interviewees:

“It is possible to delay a publication. We have done that. Sometimes a bit too long, in other cases we have published quite quickly, perhaps after only half a year. But that is ok. I think it is possible to handle.”

T: “Yes. ‘A bit too long’ – has it been a year or more?”

“Yes, it is sort of that we university researchers have chosen to … not push the process as fast as would have been possible. You observe how the paper lies a week too long here, a week too long there. Instead of stepping in and saying ‘Now let’s make sure to get this done’, so, ‘Ok, it’s ok to take it a bit slower than what is actually appropriate.’”

“… you want to publish as quickly as possible. But you also have to respect that firms are in different phases. If you are developing a technological solution on the holiest part of the firm, so to speak, then it is, of course, obvious that you do not publish parts of that before you have made it all the way through. … you would then reveal what you are doing. So then you have to wait” (19).

Different perspectives
Academics and industrial representatives often have different views on the value and quality of products as well as the role of the academic researcher. In all collaborations, those aspects are to some extent unsettled; there is no standard, and as such, there is an opportunity for manipulation of the issues.

A general observation is that many researchers are emotionally attached to their research. For that reason, they want to make sure it is used in ways they prefer. Some interviewees appeared to see it as a “moral right” to have their voices heard in the commercialization process. Only one of the nine researchers who told about relations with external investors appeared to not care at all about how his research was commercialized. Representatives from industry, on the other hand, appeared – at least in the interviews with university researchers, but also in the informant interviews – to be more emotionally detached and to reason more strategically about the inputs from academia.
Three contentious questions are identified and discussed in turn below. First, what constitutes the commercial value of the research once it is applied? Second, what makes a certain technology valuable? And third, what role should the individual academic have in the collaboration?

**Commercial value**

Sometimes university researchers and industry differ in their understandings of what makes an application valuable. For most interviewees, the main concern seemed to be to ensure availability of a usable product that to some extent is based on their research. They therefore prefer a product on the market as soon as the technology is in place. One might speculate their motives being that this preference allows them to observe and define their contribution, and thereby both take personal pride in it and delimit a merit. Firms, and especially investors in start-ups, often emphasize the potential of the collaboration to generate economic profits or to stabilize their organization in the long run. Occasionally the industrial strategies therefore make the researchers dissatisfied. Two examples are included.

First, a firm might prefer to delay the launching of the product. This strategy might be applied if the firm has other products that are expected to remain successful for some time, preventing the allocation of capital, staff, and other resources to the new product. For that reason, one interviewee who tried to find a partner firm to develop his finding – a technology to assist handicapped children – ended up starting his own firm:

“At first we didn’t want to create a firm. We thought, ‘This is really a good method, someone should do something about it. We patent it and license it to [an existing firm]’. So we initiated discussions with [a firm]. At that point I wanted support in the negotiations. I got in touch with a guy at [the university holding company] who assisted us during the negotiations. And he certainly realized the potential in this (laughs). At the same time he recommended that we not enter into this collaboration. We’re very happy about that now – they tried to screw us. Then we decided to do it on our own, not to license it, but to create our own firm.”

“...We realized that they wanted to pay extremely low royalties to use this. And they didn’t seem to have any real plans for how to develop it. ... We pretty soon got the feeling that their offer was too low and that they were going to keep this in their pockets, just to have something in their pipeline for later use.” (21)

Thus, besides strategic reasons to delay the product, the firm also had reasons to manipulate the researcher – and the researcher had an opportunity to feel manipulated. That way the user-value, that a few more handicapped children would be helped, perhaps was weighed against a survival strategy for the firm.
Second, a firm might have the intention to profit by selling the patent rather than a future product. Patent trading is an important way of capturing value in contemporary economies (Teece 1998). The clearest examples in the material are researchers who contact investors, such as venture capitalists, to fund start-up firms. Investments are often only available if the investor gets a majority share of the firm. It seems fairly common that investors operate with a horizon of three to five years, after which they want good opportunities for profits. At that time many small science-based firms have often only been able to prove the quality of the technology. From the perspective of the investor, the quality of the patent has thus been proved. It might then seem more profitable to sell the patent than to invest more capital in the firm. It might also be more beneficial to locate the firm elsewhere, for example, in Silicon Valley. This system is emerging as the typical and most effective model in, at least, the biotechnology sector (Cooke 2005). However, for a researcher who wants to have a product based on her research, the model might be frustrating, as at an early stage she already loses control of the development process.

Technological quality
Another point that is occasionally contentious regards how valuable the qualities of the technology are in relation to the context into which it will be inserted. Researchers often emphasize technological sophistication, but from an industrial perspective, it is often more important that a product sell and work well with existing technologies and quality standards (cf Ibert 2010). One interviewee who was just about to launch a firm with a couple of colleagues told about disagreement over how to fund the firm:

“[I]f venture capitalists enter the picture they will own the firm, since they, of course, provide most of the money. So they decide. And they don’t understand the technology! And then they make weird decisions, which they find logical and good, which they think are right to make the firm grow as fast as possible.”

“An investor might easily say ‘Don’t do that, it will just take lots of time, let’s go directly to [technology x]. That’s where we’ll make the money’. But then I am afraid that things go too fast. That we won’t have time to consolidate the technology. To really make it work. You see, [these kinds of technologies], to make them work, that really isn’t easy” (5).

The role of the academic
Until the academic has expressed sufficient knowledge, she is valued because she embodies knowledge. However, when knowledge sufficient for the project has been expressed, the knowledge might be controlled by somebody else, and the researcher’s position can become unstable (Grabher 2002, Sennett 2008). The issue can be counterbalanced by trust, stable employments,
and if the researcher changes her perspective and sees herself not only as an academic but to some extent also as an industrialist.

This potential tension might be visible in collaborative projects, but no such example arose in the interviews. Such friction might also arise when university researchers start their own firms. The question is then what work they should do for the firm. About two-thirds of the respondents who had started their own firms had withdrawn from the firm once it was up and running, sometimes even against the interest of the firm. Usually this happened because they found industrial and academic activities inappropriate to combine – partly because of their limited amount of time, partly because their expertise as researchers differed too much from the expertise required to run a firm. From the perspective of the firm, however, or its investors, it might perhaps have been better if the researcher had remained in the firm. One researcher who had been involved in the creation of a handful of start-ups and had left all of them at an early stage explicated his perspective:

“[A]s a researcher you are all the time in the next project. Being in a firm… They develop a technology which you in your research no longer work with. That work is probably better made by employees in the firm. I sometimes note the surprise from investors on this point, who think that ‘It is important that you, who have conducted this research, that you take part in the firm to make sure that it will be good’”.

“I mean, to the researcher you can add PhD students or post-docs. That [young researchers] leave a start-up should be pretty normal. But if the research leaders at the universities do that too, the universities will be drained of the capacity of those people. They better remain in the academic environment, create space for new projects, take part in the international research, and see new possibilities. Instead of writing market plans, business plans, or whatever you would do in a firm” (29).

Four respondents had chosen to engage quite intensively with their firms once they had been up and running. One of them explained his engagement through his fascination with the strategic maneuvers that firms do; for example he tried to support upcoming negotiations by strategizing as in game theory. The others appeared to run their firms themselves mainly because they did not trust that other firms or investors would make appropriate efforts to cultivate the patents into suitable products or that they would keep the researcher as an advisor. Two of them were quick to note that they separate the firm from the lab – commercial work is controversial, they noted. The third had integrated the firm closely with the university lab: all relevant findings are directly transferred to the firm, and university equipment is occasionally used for the purposes of the firm. Also this researcher observed that many colleagues and others find such activities questionable. That includes the junior researchers in the group, who can choose whether or not to
do research of potential interest to the firm, but are not included on any patents since they work on the researcher’s ideas.

In two research environments the researchers had together created firms in which they pooled their patents. With the help of investment capital, they had also initiated another two firms to which they licensed the patents pooled in the first two firms. The first firms largely served as a holding company, controlling a substantial share of the second firm. According to the interviewees, those arrangements were made because leading researchers in their environments concluded that the CoEs could be expected to produce a fairly steady flow of patents, and those same researchers wanted the patents to remain local at least for a while. The interviewees were all positive to the arrangement – in which participation is voluntary – but their engagement differed widely. Although nobody claimed to do any substantial work for any of the firms, some followed the firms closely while others just seemed to be happy for a solution to “the patent problem”.

Industrial engagement might also imply transformed identities. Thus, besides organizational difficulties about what roles academic should play in firms and collaborations, such engagement might – partly consciously, partly unconsciously – also result in changing how they view their own positions. Some academics might not conceive of themselves only as an academic who works for the common good and produces research results for others to exploit – they might also see themselves as industrialists or “subcontractors” whose recognition is defined in economic terms.

A few interviewees related personal income and research resources to their efforts to support industry, and on that ground expressed dissatisfaction. One of them was frustrated because he perceived that investors try to deny him his “right” to get sufficient economic returns from his start-up, which is based on more than a decade of research:

“Venture capitalists, I will never approach them again. I won’t even look at one. Venture capitalists want a great momentum. Their investments fail so often, and those who succeed have to pay for the failures. So they want their money to grow by ten within, say, three years, and I say: the researcher won’t get much money. Really, the one who made the invention gets completely screwed.

/…/

You see, [venture capitalists], they sort of don’t listen. They want a safe bet – I mean, there is no risk anymore. It should be really safe. And then they devalue it with their talk. They say ‘It probably won’t work, we take a hell of a risk, but ok, you might get half a million SEK, but then we want 51 percent of the firm’. I mean, they want… They pull the wool over your eyes and then try to grab the firm by being genuinely intellectually dishonest. They don’t present an accurate picture of reality, the world, the firm. And then they want their money within three years, preferably” (26).
Moreover, several respondents asserted that if universities were transformed into components of a national innovation system, they should be compensated accordingly. One of them claimed that many recent governmental policies and declarations indicate a “commercial treasure” hidden in Swedish universities, but that governmental support, in spite of such statements, has neither meant more room for university research nor wages closer to those in industry. He concluded that the actual situation might be that the treasure after all does not exist, or that the state intends to exploit academics. He perceives of the situation as disrespect for expertise.

During the collaboration

During the course of a collaboration, the perspectives on the researched phenomena or the utility of the collaboration might change. The changes might be because the research is perceived to open up new possibilities, or because organizational contexts change. Both imply a revaluation of the goals of the projects. Three types of such situations are observed and discussed below: when individual participants are replaced by others, when firms change strategies, and when new openings for science are identified. All cases indicate the importance, and limits, of a contract for the collaboration.

Changed participants

Some individuals in the collaboration may be replaced with others. Such changes are more likely to be caused by the firm, since the collaboration is between individual university researchers and the firm as an organization. As observed in chapter five, a successful collaboration partly depends on good personal relations between a few individuals. If one or more of them change, the work easily becomes a bit inefficient – in part because trust and working procedures have to be reestablished and in part because the interests of the replacement might be different. Two reasons for changes can be identified.

First, individuals sometimes move within the firm because the firm seeks to coordinate its activities. The firm might also close research divisions or projects. From the perspective of university researchers, such changes often occur without much prior notice. Second, while the collaboration formally is between the academic and the firm, they are also to some extent between individuals. Thus, a new research manager in a firm might change the direction of a division because it fits better with her personal external contacts. A few interviewees observed how their relations to firms have changed for the better or worse when the firms have replaced their contact persons.

Firms change strategies

Collaborations with firms can be seriously disturbed or terminated after the firm has changed strategy due to a decision being made higher up in the organization. Such decisions are also noteworthy since those who make them
often only have limited insight into the potential of a particular collaboration. There can be different grounds for such decisions. The examples cited in this study indicate three cases.

First, the firm might change strategies due to poor economic performance. One example concerns a collaboration that changed face following the dot-com crisis. The collaboration was rather informally organized, with no contract specifically indicating the frames or goals. Thus, the firm could change the terms of its relationship to the researcher fairly easily. The researcher felt betrayed:

"For a few years there was pretty good funding from the firm. We wrote reports, had discussions with them, and so on. But after a while things got worse. Suddenly the time horizon was only six months, or three months, and we were supposed to... They began to apply this stuff. And did weird things, I thought. So the relation became a burden. The PhD student who was supposed to write papers for publication had to work with firm-specific matters. And all of a sudden there was not much money for that work either (laughs), so all of a sudden I was supposed to finance half that work with my own funding. Their business. So nowadays I try to stay away from collaborations with industry" (14).

Second, collaborations might also be affected by a major organizational restructuring of the partner firm. For example, the firm might decide that a certain technology is not expected to support a sufficiently large market. Another case is if the firm merges with another firm. Such an example was mentioned by an interviewee, whose small partner firm merged with another one. The merger resulted in two competing technologies and two competing groups who defended “their” technologies. One of the technologies was developed by the interviewee. At first, the new firm stopped working at that technology for two to three years and eventually decided to drop it. As a consequence the interviewee found another industrial collaboration caught in a deadlock: the same technology was used in both collaborations, and the researcher had based the second collaboration on the hope that the first firm would do as it had said it would – develop an application that could be used in the second collaboration. The interviewee was still disappointed:

“We transferred technology and all that – we were really careful, gave [the first firm] everything, all my images, everything. We were really, really careful. And we were also careful not to present stuff that belonged to them, we always told others to contact them instead – if someone invited me I told the firm ‘Go there and give a presentation’ – so that I really handed over everything to them in a proper way. And yet it failed! Nothing came out of it. And since we all the time collaborated with [the second firm]… I kept telling them ‘Yes, yes, there is a firm which will produce [the application], don’t worry’. And they did not! So one day I had to stand there and say ‘No, they won’t do that’” (5).
Another typical example, mentioned by a couple of interviewees, is collaborations between university researchers and large corporations, which stranded as the corporations cut down on long-term research because of pressure from shareholders to increase short-term profits. The firms therefore outsourced much of their early-stage research to universities. While that strategy might be efficient for the economic system at large, it meant that an interviewee in basic research lost his long-time partner, and that another was expected to focus on applied research.

**Scientific opportunities**

Relational tensions might also occur as the academic makes unexpected discoveries that she wants to learn more about or that could lead to better scientific merits than the original plan indicated. The academic might therefore want to change the course of the project, which could include reformulating the goals that were agreed upon. Such changes are sometimes not only dependent on negotiations with the contact persons in the firms but also on decisions made elsewhere in the firms. That often makes collaborations with firms more rigid than they would be if the partner had been another university researcher. Attempts to change the direction of collaborative work might therefore have negative effects on the university researcher, including for the possibilities for future funding from the firm. Several respondents expressed such concerns and claimed that was a risk in collaborative projects with industry.

### 7.3 Coping with relational tensions

Relational tensions can be handled in three main ways. These were also discussed in the previous chapter. One way is to relieve the tension by decreasing the relational distance to other actors. This is discussed in the first part of the section on socialization. To the extent the tension could be handled without decreasing the distance, two other strategies can be applied. The second and third parts of this section concern, in turn, these two strategies: that the influential actor (generally, industry) pays more attention to the needs of the less influential actor (generally, the academic), or that interaction is partly restricted, here referred to as seclusion. In most cases, traces of all three strategies should be visible. The cited examples are mentioned because they illuminate a certain strategy particularly well.

**Socialization**

Relational tensions can be handled by socialization, internalizing the perspectives of others, which reduces the relational distance and thereby the tension. Socialization is to some extent an unavoidable consequence of long-
term interaction and part of the legitimization to work across the academic-industrial interface. It also means that a participant occasionally initiates a change of perspective for practical reasons rather than because it deepens her knowledge, and as such is not optimal behavior for a genuinely free learner.

To become socialized is to change one’s identity. In relations between academia and industry, this implies that the academic who adapts to industrial contexts might develop a different view on what an “academic” is, and should be. If so, the effective boundaries of academia would change too. In this study that is indicated by researchers who determine the quality of their work – particularly towards industry – in terms of economic compensation. Traditionally, the lower wages for academic work have been accepted as academic work has meant freedom in research, by, for example, having resources connected to one’s position or person.

Socialization can be observed in several examples in this chapter. In the order presented below, they include acquiring knowledge about industries and markets, making compromises, and informing industrial partners about one’s interests.

**Learning about industry**

First, relational tensions could be relieved by academics extending their knowledge about industry. On this point, several respondents noted how their experience of working in industry – either as an employee in a large firm or as entrepreneur – has been beneficial. Moreover, most interviewees were interested in applications, and many engaged – if they had time – in discussions with representatives from industry to learn about industrial interests and restrictions imposed by, for example, costs, existing technologies, and customer preferences.

If academics learn more about industry and markets, the collaborations could potentially be more promising for both academia and industry in several ways. Their ability to foresee problems in relations with industry would increase, and better connection points could be identified. Academics would also be better at avoiding potential “losers” as partner firms, and thereby establish more stable relations. It is also possible that they would be better at directing the use of their technologies, which, in addition to more motivation to engage in collaborations with industry, could in some cases be beneficial also for the user, as the technology would be available sooner. Moreover, increased knowledge about industry would make the academic more confident and perhaps reduce the suspicious attitude towards industry that several researchers have – sometimes because of personal experiences.

**Making compromises**

Second, collaborations can be made to work via compromises, whereby participants accept that their goals are incommensurable and negotiate to find a satisfactory balance. Given the usual discrepancy between, on the one hand,
the interests of commercial actors and markets and, on the other hand, academics’ interests in science, compromises are a natural aspect of collaborations between academics and industry.

In the empirical material there are several examples of such compromises. One example is when university researchers give up their legal right to commercialize their inventions to the funder. That seems to be an unproblematic compromise; researchers find it natural that funding firms get the rights to patents, and they would not patent themselves anyway. Another example is when researchers accept restricting communication within academia for reasons of intellectual property rights. A third type of example is when researchers have wanted to commercialize a finding but not found enough interest from a partner firm. A few respondents have then engaged more in the commercialization process themselves, such as the above-mentioned researcher who started a firm partly because the partner firm seemed more interested in the patent than in making a product.

While compromises are usually needed when academics and firms work together, and are often viewed positively, they nevertheless reduce relational distance as both researchers and firms agree not to work more towards their own preferences than the compromise allows. Since collaborations are normally funded by firms, it is possible that researchers have to adapt to firms more than firms have to adapt to the researchers. In many CoEs researchers have more influence than in most other work with industry, which might give more room for academic interests within the collaboration.

**Informing industry**

A third way for the researcher to cope with the relational tensions is when the researchers informs the industrial partner about her interests, and thus tries to make the partner understand and adapt. In other words, to inform is to socialize the partner. Just like compromising, this is a natural part of a collaborative project. Informing seems to be a particularly important strategy before a formal collaboration is initiated. A typical example is illustrated by a researcher who told about a fruitful collaboration that has been running for many years:

“...We and [Firm x] often plan the research projects together. So it is not that [Firm x] say ‘Now take this’. We have a discussion.”

“...Since we have developed the collaboration over many years, we have begun to understand each other. So [Firm x] is well aware that if we have a project for a PhD student, then the results have to be published and there has to be a freedom to direct the research. I mean, we might attain results in a project which means that we should change the direction of it, quite a bit. And they understand that it is important. But if there is a new partner firm, which does not have the same insight into, and background in, research – then we have to be careful” (2).
The researcher thus indicates – and so do several others – that avoidance of negative tensions when researchers want to explore unexpected findings, in which the firm might be less interested, largely depends on the ability of the researcher to create space for such turns. It seems important that a formal collaboration is based on a contract. However, a contract can most probably not be detailed enough to cover all potential situations, and all compromises are not desirable. Room for flexibility is thus a question of negotiations within the specific partnership.

Lateral authority

To cope with relational tensions by help of lateral authority might be to conform, as was discussed above. It might also be that the more influential actor, who represents the industrial perspective, grants more room for the interests of the academic. A similar reasoning was made in the previous chapter. On this point, relations between academics and industry differ, however, in two main ways from relations within academia. First, it is usually the firm that provides the financial resources, which gives them relatively much influence. Second, it seems that collaborations between academia and industry are of a more strategic character, and the negotiations appear to be a bit more strict and calculative. This is partly because many firms are pressured by external stakeholders such as customers and shareholders.

For those reasons, recognized knowledge of industry seems to help the academic. One researcher told in an interview:

“It gives me a bit more weight when I talk with firms – they know that I have the experience of having worked in industry for many years. So, I can credibly say that I understand what the firm wants from the collaboration” (22).

Another respondent also combined knowledge about the firm with personal relations with the contact person in the firm to negotiate favorable terms:

“[S]ince I knew who sat at the other end – I even employed him a long time ago (laughs) – it was not really difficult to be pretty tough towards the firm and say, ‘This is how we will do it if we are to work together. Otherwise we cannot accept a PhD student like this’. I was in a favorable position in that case since I knew the firm very well and partly could dictate the conditions under which we would work together. And I have certainly understood… I have colleagues in the department who have got external offers, but have not been able to choose how they want it to be” (20).

Thus, in comparison with relations with other researchers, good relations with firms often seem a bit more dependent on the researcher’s capacity to show strength in negotiations. Moreover, since the collaboration tends to be more important for the researcher than for the firm, it is more likely that the firm rather than the research change the conditions for a project during its...
course. To the firm, focus is on its performance at large, and when focus is on that larger context, the relative value of the project is more likely to change. Given the cited examples of friction in the relations between academic and partner firms, it seems that a good contract is important, and that fruitful relations – especially to connect activities in firms with basic research – might take a long time to develop. The degree of trust generally seems lower in industry than in academia.

Seclusion

The third main strategy that researchers use to cope with relational tension is to restrict the interaction such that their own perspectives can be more cultivated. Three types of seclusion are identified.

First, it is visible by the extent to which they spatially separate “academic” and “industrial” activities. That includes both their willingness to invite firms to their labs and their readiness to engage in the ongoing activities of their own firms. Some respondents emphasized that academia and commercialization are distinctly different activities. If they started a firm, they normally left it as soon as it was up and running, or remained as an advisor, and if they collaborate with a firm, they normally would not let the firm work in the lab. One of the most determined on this point explicated his view:

“I don’t think it is healthy to mix research and some kind of business activity in a hodgepodge in the lab. I don’t believe in that. I think it is really important that the firms are... If you have an idea about a firm, that you keep it physically apart [from the lab, author’s comment]. To make clear that ‘this is business’ and that they have to carry all their costs on their own. That’s hard if you mix it too much. And then [business] is also different from research. I mean, if you run a firm you have completely different goals. You work in completely different ways. That too must not be mixed with research I think. Research is just about creativity, I think. And about curiosity” (13).

Others are more willing to engage with commercial activities, or even involve firms in the day-to-day activities in the lab. For example, one interviewee, who formally did not work for any firm, interacted closely with a firm located in immediate proximity to the lab. Another example is the above-mentioned example of the researcher who runs the firm close to the research group.

Second, another identified seclusion strategy is when researchers conceal their own commercial activities from others – university researchers, firms, or investors. Several respondents who engaged in commercial activities observed how that interest should be partly hidden from some university colleagues who find it unprofessional. Moreover, some interviewees claimed there were reasons not to trust industry but wanted to develop their research
in applications, and therefore engaged themselves more in commercialization than they seemed to prefer.

Third, seclusion is also common in academia as a consequence of the choice to support patenting and work with other commercial secrets. It is virtually impossible to make economic profits on a research finding unless communication is restricted until a patent is secured. For that reason, papers are often delayed and discussions restricted. This appeared to be a minor problem for the interviewees.

7.4 Chapter conclusion

Relational tensions are commonly observed in the interface between academia and industry. Just like in the previous chapter, on relational tensions in academia, frictions might arise between individuals with different perspectives. However, relations across the interface between academia and industry also imply bridging the gap between two established types of social organization: science and industry. To cope with the tensions, the same three kinds of responses have been discussed as in the previous chapter: socialization, lateral authority, and seclusion.

Socialization includes the researcher changing her identity such that she becomes more like an industrialist – for example, she judges her work in terms of economic rewards. It is argued that socialization is attained in three main ways: by learning more about industry, by making compromises to bridge the gaps between personal or scientific interests and the interests of industry, and by informing industry about one’s interests such that the representatives from industry change perspectives. Socialization implies a decreased relational distance, and to the extent it occurs across the boundary between science and industry, it undermines the scientific profession, which is upheld by a defined scientific space. In addition, since theoretical reasoning indicates that an individual’s capacity for success in basic research might depend on high integrity, incentives for socialization might thus be counter-productive to basic research ends.

Lateral authority on the part of the strong actor appears to be more difficult to attain than in the case of relational tensions within academia. The reason is that the hands of industry are often strongly tied by markets and by investors, which leave little room for experimenting or more radical changes of technologies. For that reason, lateral authority is primarily achieved by the academic strengthening her position in the eyes of the firm, for example, by having good knowledge of the firm or the market and by being good at negotiating beneficial terms in a contract. Just like in academia, deficit of lateral authority implies incentives for strategic manipulation and concealed intentions, as actors might be unwilling to reveal their true knowledge. Several
examples arise when interviewees find that they have experienced such behavior from firms.

Seclusion is indicated by the extent to which the academics spatially separate scientific and industrial activities, both in organizations – the firm and the lab – and to the extent they are willing to perform industrial work and thereby take on another role. Seclusion is also visible in hiding commercial activities from colleagues and industrial competitors, and the extent to which they serve industrial interests by restricting or delaying scientific communication at conferences and in papers. The last point is particularly relevant since many firms have adopted new strategies for research whereby they in effect outsource parts of their innovation activities to universities. The term used for this strategy, “open innovation”, might take the perspective of the firm rather than the academic.

This chapter marks the end of discussions based on the interviews with the university researchers. The next chapter presents the second study in the dissertation: on relational tensions during the first five years of a program aimed at commercializing university research in biotechnology. That chapter reinforces some points made in this chapter, primarily how the position of the academic researcher changes when she works towards industrial ends.
This chapter concerns Uppsala BIO-X, a program aimed at commercializing biotechnology research at the two universities in Uppsala: Uppsala University and the Swedish University of Agricultural Sciences. Uppsala BIO-X is part of a “triple helix” initiative called Uppsala BIO, in which local biotechnological industry, public authorities, and universities work together. The aim of this chapter is to identify relational tensions in Uppsala BIO-X from its initiation in 2003 up until 2007 and to find out how they were handled. The study concerns both the organizational structure of Uppsala BIO-X and the relations between the research projects and the management of Uppsala BIO-X.

The balance between science and industry has been a contentious issue for Uppsala BIO-X. During the studied period the organization shifted toward a more industrial perspective. The shift was difficult for one of the two main projects. The industrial perspective also created tensions between the projects and the BIO-X management, primarily because the role of the project was to some extent put above the role of the individual researcher.20

The chapter consists of five sections. The first section presents Uppsala BIO-X as an outcome of transformations in Swedish government policies and in Uppsala’s biotechnological industry. The second section concerns the development of Uppsala BIO-X from 2003 up until 2007. In the third section, relational tensions in the organization and between the management and the projects are identified. The fourth section presents the approaches used to handle relational tensions with the same three broad strategies as in the previous chapters: socialization, lateral authority, and seclusion. The final section is a conclusion.

20 Since the focus is on differences between actors, the story might indicate more dramatic circumstances than the participants experienced or remembered experiencing. It should also be noted that no interviewee found the format of Uppsala BIO-X intolerable, and that they all seemed to find that given the current goals of Uppsala BIO-X, the program is well-organized and relatively successful.
8.1 Background to Uppsala BIO-X

In this section the context of Uppsala BIO-X is presented. The first two parts present the motives behind the creation of Uppsala BIO-X, first from the local actors, then from a national policy perspective. In the third part Uppsala BIO is introduced.

Uppsala’s biotech industry reshaped

Toward the end of the 1990s, policy-makers and industrialists in Uppsala began to raise concerns over the future ability of the local biotechnology industry to create and sustain competitiveness. The ecology in Uppsala’s biotechnical industry was not the same as it was ten years before. Most important, Pharmacia, a pharmaceutical giant based in Uppsala since the 1950s, had been dismantled. In 1991 Pharmacia had several thousand employees in Uppsala. In 2006, after a decade of downsizing, outsourcing, and patent trading, Pharmacia’s new owner, Pfizer, closed its last establishment in Uppsala. That recast Uppsala’s biotechnological industrial system. By employing graduates and researchers, and via collaborations with university researchers, Pharmacia had been the most important link between industry and the universities. It had also been the main funder of early-stage industrial research and had for more than two decades spun out lots of small firms based on technologies it did not want to commercialize (Eliasson and Eliasson 2006).

Thus, Pharmacia was not only by far the largest biotech firm in Uppsala but also the dominant vehicle bringing university research closer to market, schooling researchers into commercial activities, and giving birth to new firms (Waxell 2005, Malmberg and Waxell 2007).

In addition, following the financial downturn in 2000, Uppsala’s biotech industry also suffered from a shortage of investment capital, which led many local and national actors to fear that too many biotech firms and patents would leave Uppsala with little economic return or job creation (Eliasson and Eliasson 2006). The decline of Pharmacia and the lack of investment capital triggered some action to revitalize Uppsala’s biotech industry. Uppsala BIO was one attempt to make the transfer of research from local academia to local biotech industry more efficient.\textsuperscript{21}

\textsuperscript{21} The development of Pharmacia and Uppsala’s biotech industry seems to have followed the same path as the development of the industry internationally. Cooke (2005, 2007) argues that during the 1990s the pharmaceutical biotech industry went through a paradigmatic shift, whereby the large corporations partly gave up their early stage research and began to practice “open innovation”. This is largely concentrated in a relatively small number of clusters worldwide and conducted by public research organizations and small firms, and is often partly financed by large corporations.
New research policies

As noted above, there had also been a series of changes in Swedish research policy. One outcome was that universities were encouraged to pay more attention to the interests of industry and politicians. Biotechnology was identified as a field of national strategic interest. Universities were also induced to support the commercialization of research (Benner 2008).

The policy changes also led to the establishment of VINNOVA in 2001. The first major VINNOVA program, VINNVÄXT, was launched in 2002. The purpose of VINNVÄXT was “to promote sustainable growth in the regions based on international competitive ability, by successively developing or further developing the functioning, dynamics and effectiveness of innovation systems in functional regions at an international level” (VINNOVA 2002:4) through financial support for ten years to regional partnerships between industry, government, and universities. The partnerships were assessed based on the extent to which they represented a “functional region” and on the degree to which the participants together supported a shared, realistic, and long-term vision for the region within the particular field. In June 2003 VINNOVA selected three recipients of the first VINNVÄXT grants. Uppsala BIO was one of these (VINNOVA 2003).

Uppsala BIO and Uppsala BIO-X

Since its initiation, the goal of Uppsala BIO has been to support Uppsala’s biotechnological industrial system broadly. That includes increasing the number of qualified jobs, improving value creation, and strengthening the region’s attractiveness. Uppsala BIO focuses on the identified main strengths in Uppsala’s biotechnology: methods, models, and tools for biotechnological research. The activities of Uppsala BIO include, among other things, to support an incubator, to market the region both nationally and internationally, as well as – by using half of the financial support from VINNVÄXT, a precondition to funding – to promote cross-disciplinary biotech research with a commercial focus. The latter is carried out through “Uppsala BIO-X”. Uppsala BIO-X is thus a part of Uppsala BIO (Teigland et al 2005).

Uppsala BIO is organized as a project under The Foundation for Collaboration between Uppsala’s Universities, the Business Community, and Society (STUNS). The board of STUNS endorses long-term strategies for Uppsala BIO. These strategies are initiated by a steering committee (SC) for Uppsala BIO. The SC comprises senior executives and influential individuals from leading local biotech companies and the two local universities (Uppsala University and the Swedish University of Agricultural Sciences), as well as top county officials. Day-to-day work is carried out by about three full-time employees at Uppsala BIO, whose work is reported at meetings with the SC about five times per year. Uppsala BIO-X is formally run by a Scientific
Advisory Board (SAB) that reports to the SC. One of Uppsala BIO’s employees works as a program manager for Uppsala BIO-X. A chart of the organization of Uppsala BIO-X is presented in Figure 8. Uppsala BIO, including BIO-X, is regarded as a relatively successful initiative and have been positively evaluated by VINNOVA (2007a).

8.2 Development of Uppsala BIO-X and its projects 2003-2007

Uppsala BIO-X developed through three distinguishable phases. To separate them effectively, “the three BIO-Xs” are hereafter referred to as BIO-X₁, BIO-X₂ and BIO-X₃. The three phases are described in detail below, with attention paid to the development of the organization of BIO-X and the development of the projects. The development of Uppsala BIO-X is schematically depicted in figure 9.
Figure 9. Uppsala BIO-X: Key activities 2003 - 2007.
Uppsala BIO-X: 2003 to early 2004

The first version of BIO-X only existed in the granted application to VINNOVA and was, predominantly, the vision of a professor at Uppsala University. He was at the time advisor on biotechnological issues to the Rector of Uppsala University and pushed the process toward the VINNVÄXT application within the university sphere. The professor’s interest was largely personal. He had for many years worked closely with Pharmacia’s Division for Exploratory Research, which had closed, and he was therefore looking for another long-term arrangement with a local firm. Since no firm was interested in the early-stage products he presented, he turned to VINNVÄXT as a substitute. When VINNOVA granted funding to Uppsala BIO, he left the administrative side to be able to compete for BIO-X funding.

The professor’s vision of Uppsala BIO-X was largely based on a template from the U.S. via close relations with biotech researchers at Stanford University; he had followed the development of Stanford BIO-X, a center that conducts cross-disciplinary basic research based on biology. He borrowed name and idea from Stanford, adjusted it slightly to fit with the wishes of other local actors and VINNOVA, and presented Uppsala BIO-X as a cross-disciplinary research center focused on methods and tools for biotechnological research in which university researchers from different disciplines in technology and medicine would work together under one roof. The research center would focus on “identifying analytical needs that could be expected to arise in the future” (Uppsala BIO 2003:8). The role of industry would, just as in Stanford BIO-X, be secondary:

“Close contacts with the industry and the public health service call attention to needs. To see needs at an early stage is an asset for the innovation system. It is most desirable and necessary that the industry takes part of BioX. An important contribution is [sic] experiences from project management in cross-border teams. Another important issue is to create an interface for commercialization of new knowledge that arises from BioX” (Uppsala BIO 2003:8).

Uppsala BIO-X would thus become a place in-between university and industry. University researchers would run exploratory projects, and industry would have a rather advisory role.

However, it soon became clear that BIO-X as a physical center primarily for university researchers could not be realized. Although an attractive location at Uppsala University’s center for biomedical research was available, it proved impossible to establish the center there for three main reasons. First, nobody at the university was, given the conditions, willing to establish a BIO-X center. Research groups did not find relocation to such a center attractive enough, mainly because the available funding, between two and three MSEK per year, was far too little. All interviewed managers and researchers at the university claimed that 20 MSEK per year would have been
needed to persuade research groups to leave their present labs. University
managers in Uppsala BIO were also not willing to force researchers to relo-
cate. Second, it also became obvious that representatives from industry and
local government – and most probably VINNOVA as well – would not en-
dorse long-term financial support to researchers who were unwilling to
clearly separate applied from basic research. Third, relocation of research
groups would mean longer time to output from BIO-X. Several representa-
tives in the SC, particularly from local government, were eager to see signs
of action and output that they could report to their own organizations and to
VINNOVA. The original idea for BIO-X thus had to be replaced with a new
one.

Uppsala BIO-X2: Early 2004 to April 2005

The first real activities in BIO-X began during BIO-X2, which lasted from
everal 2004 to April 2005. An organization for BIO-X was established, and
two BIO-X-funded projects were initiated. There was, however, still instabil-
ity in the organization, since the different actors involved in Uppsala BIO
and BIO-X did not unite around a shared goal.

The organization

The question was how BIO-X2 would be organized. That discussion turned
out to take time. After about half a year without any decision being made,
frustration spread among members of the SC. Some of them also expressed
concern that VINNOVA would cut the budget if there was a big surplus due
to unused money at the end of the year. There were also worries that the
reputation of Uppsala BIO would be damaged if it took too long to initiate
the core activities.

The most important reason why the SC could not agree was the signifi-
cant differences between the different actors regarding the time perspective
and aim of BIO-X. Industry wanted a stable BIO-X with a focus on com-
mercialization to run for a long time. The universities had a long-term pers-
pective of BIO-X, wanting to combine multidisciplinary research with com-
mercial opportunities, but first wanted to see whether the concept worked.
The public sector wanted to see relatively quick effects in industry and to be
able to adjust their engagement throughout the process. The SC could there-
fore not decide what the goal for BIO-X should be, and whether it – and
thereby the BIO-X organization – should be formalized in a document or
kept open to continual discussions (cf Teigland et al 2005).22

22 The lengthy discussions are in line with Teigland and Lindkvist’s (2007) observation that
the perspectives on Uppsala’s biotech cluster differed notably between public and industrial
actors in the cluster.
In October 2003, to escape the deadlock, the SC decided to appoint a consultant to develop an activity plan for BIO-X. The consultant was quickly and informally recruited and contracted for eight weeks of work until February 2004. He was recruited because of his extensive experience and because he had had a central position in Pharmacia, had been involved in several local biotech start-ups and a large international network, and was trusted by key individuals in Uppsala BIO.

In December 2003 the SC made two important decisions concerning BIO-X. The decisions were based on a preliminary version of the activity plan presented by the consultant, whose suggestions were followed. The first decision was to set up a Scientific Advisory Board (SAB) composed of five people: two representatives from the local biotech industry, two from the local universities, and one international expert from either industry or academia. The role of the SAB would be to advise the SC on strategic issues and, at biannual meetings, evaluate new and ongoing projects financed by BIO-X. The second decision was to start two cross-disciplinary research projects at BIO-X. The consultant was instructed to quickly initiate the research projects and was also commissioned to recruit the SAB members and define their tasks in detail.

The research projects were selected after two calls to researchers in Uppsala. The first call was made in April 2004 after a meeting in a “temporary SAB” consisting of the consultant, a senior local biotech entrepreneur, and one Deputy Vice Chancellor from each of the two universities. BIO-X was presented as mainly a university initiative that would support multidisciplinary research with commercial potential. No industry involvement in the proposal was required. BIO-X was in the first call described in the following terms:

“The purpose of BioX is to support ambitious, world class, multi-disciplinary research project in the field of ‘Tools for Life Science’ in Uppsala by making supplementary funding and resources available primarily for stimulating the formation of multidisciplinary research teams. The projects should address topics that may generate new opportunities for the current and future Life Science industry, primarily in the Uppsala region.”

The consultant wanted BIO-X to be closer to industry, but sensed that universities would not support that arrangement.

At the deadline a month later nine proposals were in, and the SAB was in place. At the first SAB meeting it was decided to recommend four proposals to find common ground to form the first project. The decision was mainly based on the SAB reviews and ranking of the proposals, but it also seems that research groups from both universities would be funded in order to make BIO-X more broadly legitimate. One proposal was rated higher than the other. The SAB recommended funding to that group, but also recommended it merge with up to three of the groups behind second-ranked pro-
proposals. The interviewed PIs, however, depicted the latter not as a recommendation but as a requirement. They claimed that they believed that the potential of the project would be a bit lower following the merger, but yet agreed to take part. In the end, Project 1 was formed by three research groups. One of the groups was headed by the professor who had initiated Uppsala BIO.

A second call was made in June 2004. One change was made: on recommendation from SAB, potential industrial partners had to be included in the proposals. The second call attracted 26 proposals. The SAB found the brief descriptions in the proposals insufficient to judge their potential well enough, and therefore invited the groups behind the six highest-ranked proposals to present and discuss their ideas with the SAB at a meeting in December 2004. After the meeting one group behind one proposal – which had not been ranked among the highest – was selected to be funded as Project 2 for a preliminary two years.

At the same time a program manager for BIO-X was recruited. The consultant was not interested in continuing in Uppsala BIO and during 2004 had also started a biotech firm together with two of the PIs in Project 1. The program manager began as an observer at BIO-X meetings in December 2004 and became full-time in April 2005. Coming from a position as a director of a research unit at GE Healthcare in Uppsala23 and with a PhD in bioscience from Uppsala University, he met the demands of the SC and SAB that extensive experience from both academic research milieus and biotech industry was appropriate. The program manager took a dominant position in Uppsala BIO and soon transformed BIO-X. That marks the beginning of BIO-X3.

The projects
The aim of Project 1 was to use tools for genetic analysis developed by two of the groups to study populations of microorganisms in human intestines, on which the third group had expertise. The project plan, accepted by the consultant and the SAB in October 2004, had a 60-month perspective and was mainly aimed at producing basic technologies so that the tools could also be adapted to fit a broad range of other uses. All PIs had worked with industry before.

At its first meeting with the SAB, in October 2004, the Project 1 group got positive remarks for its scientific content but was told to contact industrialists to discuss commercial ends. Half a year later, in April 2005, the SAB questioned the group’s choice of application both due to a believed lower commercial potential in that field than in other possible fields of application and due to insufficient scientific quality on that part. The SAB also

23 GE Healthcare is built on a former division of Pharmacia. It is the only large biotech firm in Uppsala with an extensive research division.
criticized the project again for not having sufficiently focused on commercial ends. The project group was also criticized for being poorly coordinated. It was suggested that the BIO-X management would support the group in rewriting the project plan to make it more focused.

The aim of Project 2 was to produce a prototype of a tool to test levels of a specific protein in human blood. The tool would consist of a blood-testing chip that could be read off by mobile phone cameras, which would allow patients to take the tests at home and send the results to computer systems at hospitals via a text message. The project plan was formed in January and February of 2005 together with the new program manager of BIO-X, who would take part in all project meetings. Project 2 worked with a 24-month perspective, with the aim to produce a technology strong enough to attract investors to a start-up firm. Most PIs had worked with industry before. Project 2 was very well received by the SAB at the meeting in April 2005.

Uppsala BIO-X$_3$: April 2005 to early 2008

BIO-X$_3$ was oriented more toward industrial potential. This was largely the accomplishment of the program manager. The program manager seems to have had a stable and dominant position in BIO-X until he resigned in the beginning of 2008. The projects reacted differently to BIO-X$_3$. The members of Project 1 found themselves caught between their own interests and the changed interests of BIO-X. The Project 2 group experienced fewer problems since they were from the beginning more inclined to work in a way that suited BIO-X$_3$. BIO-X$_3$ existed from April 2005 to February 2008.

The organization

The entry of the program manager meant that BIO-X made a move towards more focus on the commercialization of research. The program manager argued that BIO-X projects should have more clearly defined ends than at least was the case in Project 1, and he therefore preferred closer interaction among researchers, clinics, and industry. He argued that BIO-X projects need to end where there is not much more than a year of work for a firm before a product can be on the market – otherwise the project would seem too risky. Consequently, he worked to make the role of industry in BIO-X more explicit than it had been in the previous version. The cross-disciplinary research aim became secondary.

The program manager made the turn possible by three strategic actions. First, he ensured that his definition of BIO-X got explicit support from the key decision-makers in formal bodies. In September 2005, at the first SAB meeting he chaired, the program manager put “The concept of Uppsala BIO-X” at the top of the agenda. His motive, he says, was to put an end to an ongoing debate between key actors in BIO-X about the goal of the organization:
“When I came in there was still a discussion about whether we should sponsor research that may eventually result in commercialization, and that is [the professor behind BIOX] and PI in Project 1’s way of thinking about it, or is [BIO-X about] money for commercialization, which he has now understood that it is.” (16)

Since Stanford BIO-X lingered in the background as a template for Uppsala BIO-X – especially among university representatives in the SC and researchers in the projects – the program manager also found it important to represent Uppsala BIO-X as a more distinctly different phenomenon compared with the Stanford model. On his initiative, the SAB therefore concluded that the “commercialization slant” of Uppsala BIO-X was not present in the Stanford model. The SAB also concluded that the aim of Uppsala BIO-X was to stimulate interdisciplinary cutting edge research within life science, to identify and promote “needs-motivated” (a term used by VINNOVA) research fields, and to create new opportunities for industry by shortening the time to commercialization and bridging the gap between the interests of university and industry. The SC was informed about the decision.

Second, he also made his view explicit with a figure, Figure 10, which he frequently used to illustrate his view and to convince or deter potential skeptics. In the figure BIO-X is presented as covering a gap that arises as academic research ends with publication, a stage where industry is not yet willing to take over.

Third, it also seems that the program manager – deliberately or not – did not involve the SC more than necessary. His reports on BIO-X to the SC were generally short. Actions of the program manager were legitimized also by three representatives from the SC – one Deputy Vice Chancellor from

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**Figure 10.** The figure of Uppsala BIO-X that the program manager developed.
each of the two universities and one industrial representative – who were invited as observers (and, in effect, as controllers) at SAB meetings. Consent from these individuals was thus an important gatekeeper. The program manager said, on the one hand, that the gatekeepers made him feel a bit uncertain about the support by the SC every time he reported at their meetings, but on the other hand – as far as the empirical material can tell – during the studied period all decisions the SC made on BIO-X followed the advice from the program manager and the SAB. However, it is possible that the observers at some points discouraged him from some actions.

The direction of BIO-X under the program manager seems to have been relatively constant. Indications of that are a couple of protocol references to the goal of BIO-X. For example, in the protocol from the SAB meeting September 20, 2006, it is stated that BIO-X efforts should better be spent on “‘pull’ – needs driven technology research, than on ‘push’ – technology driven product search.”

The strengthened role of industry is also visible in the third call for project proposals in March 2007. In that call BIO-X is described as an effort to “…create new business opportunities by supporting needs motivated, world class, multi-disciplinary research projects…” where funded projects should “focus on transferring research results to the product concept stage (verification) with the goal to generate new opportunities for current and future Life Science industry, primarily in the Uppsala region”. Business opportunity is mentioned before scientific quality. A prerequisite to get funding was to have industrial representation from one or several firms within the project. A funded project was expected to run for about two years. The third call thus looks substantially different from the first two calls.24

The increased focus on needs meant that the SAB had to treat the projects a bit differently than they had before. In BIO-X3 the SAB focused more than before on making the project outcomes meet technological prerequisites for commercial production and align with market conditions. When coupled with shorter timeframes, that meant more industry-style management of the projects. Interviewees for both projects indicated that biannual adjustment of the activities following meetings with the SAB were more or less a requirement. A couple of times the SAB also used, or threatened to use, its ability to change – via recommendation to the SC – the levels of funding and composition of the project groups.

The shift from the second to the third version of BIO-X seems to have gone relatively smoothly in the SAB, since it was composed by representatives from both industry and university. The industrial representatives in the SAB got more influence than before. That is also indicated by stories of occasional disappointment from university representatives in the SAB, who did

24 The project funded in the third call is not included in this study.
not always support the more hard-headed management of the projects. According to the program manager, the university representatives in the SAB mainly monitored that research entering the BIO-X projects was scientifically advanced and that it continued to be science throughout the project. They have, he said, accepted that commercialization is the goal of BIO-X, and that this usually implies a lower scientific quality of the activities.

The projects
In September 2005 Project 1 was again criticized for not being sufficiently interested in applications. Part of the critique came from a group of students from a business school who had studied the project and concluded – as did the program manager in the interviews – that more BIO-X control was needed to make sure that the BIO-X money was not simply added as extra funding for academic research. The SAB also criticized the group by noting that no clear application was expressed in the project plan and that the project goals – set to 60 months – were not strong enough for commercial interests with a two year perspective.

Shortly after the September meeting the SC cut the budget for BIO-X, and as a response the SAB recommended that Project 1 be terminated. The two fractions in the project were told to come up with independent project plans. These resulted in the formations of Project 1a and Project 1b. The projects each received 1.15 MSEK until the end of 2006.

The new project plans were presented to the SAB in April 2006. At that meeting the SAB commended Project 1a for its application, but criticized it for being too weak scientifically, and noted that the project might enter protected IPR, of which the researchers seemed unaware. In September 2006 the SAB claimed that Project 1a’s scientific method was impossible to commercialize successfully, although there still were some market windows to which the group had to pay more attention. In January 2007 the project received 1 MSEK for the rest of the year. At the next two SAB meetings Project 1a was strongly criticized for having little commercial potential in general. In October 2007 the SAB decided to terminate Project 1a.

The group for Project 1b presented their first project plan to the program manager in January 2006. The project would be led by a researcher with an industrial background recruited explicitly for the project to work in the university lab. However, Project 1b was almost immediately terminated, since the local firm with which the project group would collaborate and transfer its patents changed strategy and was regarded by the program manager as being too close to the BIO-X project. Project 1b was therefore remade, quite radi-

The budget was cut following a decision by the tax authorities regarding the level of VAT that Uppsala BIO should pay. Since the decision was retroactive, Uppsala BIO had to pay a significant sum.
cally, into Project 1c. A tentative project plan was presented to the SAB in April 2006, and a full plan in September the same year. The SAB was very positive: there were links to a large and leading research program in Stockholm, there was strong clinical presence, and the local firm would be used for commercialization. The role of the firm was, however, deemed too unclear, as were the IPR issues. At that time one of the two leading researchers left the project, since he found it too industrial. In January 2007 Project 1c received four MSEK for the following two years. The project was further praised by the SAB throughout 2007, although it continued to criticize unsettled IPR issues, especially with the Stockholm program. The project was also criticized for having too large of a steering board, which could make it inefficient. Project 1c later got funding also for 2009.

Project 2 found, following the budget cut, its funding reduced by about one third to 3.2 MSEK throughout 2006 but with the loose promise of prolongation for another six months. At several points comments in the protocols indicate that substantial departures from the initial project plan were discussed and often suggested. These usually had commercial motivations: comments repeatedly concern the importance of preparing a product that would be attractive enough for a future industrial partner to take the financial risk.

At the beginning of Project 2 the group had to fight against skepticism in the SAB regarding the value of developing a new method for that particular test, since the test was so standardized for humans with another technology. However, the PI in the group who worked at the hospital insisted on keeping it, and the test remained. In April 2006 the SAB questioned the value of a core, novel technology and indicated that it could be replaced by an established technology to reduce complexity. Abandonment would have undermined the role of the researcher who coordinated the project, since that technology was his provision. The SAB also suggested that the three scientific discoveries on which the project was based be marketed as separate entities. That idea was further pursued at the meeting in September 2006, when several comments also pointed at the advantages of not having the common goal as the main focus, due to the slow progress. The previously questioned core technology was not in doubt anymore, since it had both been found to enhance the efficiency of the product and to work on all mammals – not only humans, as is the case with the established alternative – whereby a new niche, the veterinary market, had been identified.

In January 2007 the project got 1.5 MSEK for the first half of the year as a last funding. When Project 2 left BIO-X in April 2007, the group presented an almost finished chip. The SAB suggested that two of the three core technologies be marketed individually and that commercialization plans for the common product be developed. On recommendation from the SAB, the SC decided on their next meeting to fund further development of the previously questioned core technology with 0.5 MSEK.
8.3 Relational tensions in Uppsala BIO-X

Relational tensions in Uppsala BIO-X became evident when the grant from VINNOVA was secured. Although previously the actors had a common goal – to secure funding – that could be reached in somewhat visionary ways, the different agendas of the actors were revealed as different and sometimes incommensurable goals had to be satisfied within a limited financial and institutional frame. In this section the relational tensions during the parallel developments of the BIO-X organization and the projects are presented in two parts. The first concerns the contested role of BIO-X – primarily how to balance scientific and industrial interests – and the second, tensions related to the roles of the projects in BIO-X and the academics in the projects.

The role of Uppsala BIO-X

The overarching tension in BIO-X concerned which role the organization should play. How should the largely incommensurable interests of science and industry be balanced against each other? That question was so difficult to deal with that it even prevented the SC from making a decision on how BIO-X should be initiated.

The outcome was a gradual shift of BIO-X away from science toward more emphasis on industrial interests. The shift was led by the program manager, who tried to define BIO-X as a program for commercialization rather than for academia. The shift is evident in how the calls are formulated: the strong emphasis on multidisciplinarity and science in the first and second call is replaced by a focus on industrial usefulness in the third call. It is also visible in the management of the projects, in which technological development seems to have been discussed less than were IPR and market opportunities.

In the SC the tension between science and industry was primarily visible during BIO-X\textsubscript{2}. The failed attempts to establish BIO-X\textsubscript{1} were primarily driven by university representatives in the SC. Moreover, several interviewees argued that industrial representatives never wanted to realize BIO-X as it was presented in the proposal. In the resulting discussions the university representatives emphasized multidisciplinary science, while industrial representatives put more stress on commercial opportunities. With the entry of the program manager, the discussion in the SC seems to have faded out, as the university representatives accepted BIO-X in the shape it eventually took, and granted room to the program manager.

In BIO-X\textsubscript{3} tensions between science and industry were most visible in Project 1. The project entered BIO-X with a 60-month perspective. Tensions arose when the SAB, which had accepted the first project plan, wanted instead a perspective of 24 months and criticized the project for being unfocused. The PIs found the short perspective too industrial and were unwilling
to change the plan accordingly. Project 2 was more in harmony with BIO-X, as it did not really start until the program manager was in place and began with a 24-month perspective.

In addition, besides defending BIO-X from criticism for putting too little weight on science, the program manager also made efforts to define BIO-X as “not industry”. He repeatedly argued that BIO-X was intended to fund activities that industry would find too risky and, using Figure 10, supported the argument by depicting BIO-X as covering the gap between academic activities and industrial activities. The same point is seems proved in the termination of Project 1b when its partner firm began to work with very similar problems. That situation also indicates the narrowness of the alleged gap in Figure 10.

The roles of the projects and their participants

There were also relational tensions due to different perspectives on the value of the projects. Since the roles of the participating academics were largely determined by the goals of the projects, changed views on the projects also meant changed views on its participants. This tension was evident when BIO-X became more industrially oriented; the work in the projects then became more firmly directed by the SAB. When more focus was put on industrial goals, scientific skills were less valued. The “expertise” thereby shifted a bit from the academics to the industrial representatives, and the academic researchers were given less authority as decision-makers.

When more emphasis was put on industrial interests, the project was partly decoupled from the ones who worked in it. This is witnessed in both projects. In Project 1 there were conflicts between the academics and the SAB about what the project should be aimed at. In Project 2 the SAB suggested – although it is not possible to estimate with what weight – that the project be broken up, having identified other industrially interesting problems as more likely to be solved during the funded period. The suggestions made included also encouragement to each PI to focus more on his or her own technology, and at one point to practically exclude one PI from the project by questioning the need of the technology that the researcher represented.

In addition, there is one example illustrating the differing views on what made a project valuable to BIO-X. Project 1 seems to have been valued by the BIO-X management not only due to the technology and commercial opportunities it was hoped to create but also because it made BIO-X more legitimate by including researchers from both universities. The latter aspect was less valued by the participating researchers. The project team seems to have been encouraged to form as a compromise when there were not enough resources to fund researchers from both universities in different projects. The choice indirectly created tensions. Initially there were tensions within the
project as the PIs had doubt about the potential of coupling their activities. When they had agreed on a plan and begun to work, the project was questioned by the SAB on the same ground. The researchers, who saw the SAB as responsible for the situation, found this criticism frustrating.

8.4 Coping with relational tensions in Uppsala BIO-X

The relational tensions in Uppsala BIO-X were handled by a mix of socialization, lateral authority, and seclusion. These are presented in turn below.

Socialization

Given the different backgrounds of the participants, strategies toward more a bit more shared perspectives or were important in BIO-X (cf Teigland et al 2005). Four approaches to socialization can be identified. First, the participants got to know each other. Face-to-face interaction seems to have been particularly important, both in the SC and the projects. Several interviewees mentioned early interaction, when the proposal to VINNOVA was written and when Uppsala BIO started, as important. The network organization of Uppsala BIO and the relatively sporadic meetings in the SC (about five times per year) and the SAB (twice per year) meant that BIO-X was rather fragmented in time and space. Only the Uppsala BIO management and the observers of the SAB had good insight into all core activities of BIO-X.26

Some key individuals thus only had limited interaction with each other. By interacting they could observe each other and get a better sense of each other’s skills and interests. That is of great importance as the constellations were supposed to work together and make decisions that should be broadly legitimate.

The failure to establish BIO-X\textsubscript{1} appears to partly be a result of the university representatives having limited knowledge about the others. They seem to have taken for granted that a serious attempt would be made to establish BIO-X according to the version in the granted proposal, and may thus have misjudged the support that first version had in the SC. However, the lack of financial resources prevented BIO-X\textsubscript{1} anyway. Another example is how BIO-X was initially shaped by the fact that VINNOVA as a new actor was little known. Also VINNVÄXT was new. Several members of the SC therefore wanted to “play it safe” and initiate BIO-X before VINNOVA might find Uppsala BIO lacking initiative. That is one reason why BIO-X\textsubscript{2} was initiated without sufficient support from the SAB. In addition, VINNOVA’s

\footnote{Also other members in the SC were, at least occasionally, invited to observe the SC. Most of them only attended once.}
formulations also seem to have been a useful rhetorical weapon, used in particular by the program manager to legitimize his version of BIO-X. Although the presentation of VINNVÄXT (VINNOVA 2002) is clearly suitable to draw on to legitimize BIO-X, it could also provide support for alternative versions.

Second, relational tensions were also dealt with by informing others about one’s views, in the hope that they would adopt or support one’s opinion. To a great extent, informing is a natural aspect of ordinary communication, but when supporters need to be found, it might be used more strategically as well. The most notable example is the discussions about the concept of BIO-X. The university researcher who formulated BIO-X in the proposal to VINNOVA established, particularly among the university representatives, the idea of BIO-X as a center similar to Stanford BIO-X. Later, the program manager sought to establish and institutionalize another idea of BIO-X partly by making use of Figure 10 and by initiating discussions or giving presentations in which he made sure to distance “his” BIO-X from BIO-X.

Third, gate-keeping was used as another way to cope with relational tensions. In BIO-X the SC and the SAB were, in effect, gate-keeping bodies. Notable examples of gate-keeping are when the SAB in BIO-X was relatively harsh towards the projects at some points and its right to terminate or reshape the projects emerged as a real option. Timeframes of 24 months also became an effective management tool, as they did not allow scientific exploration if a commercially interesting result would be attained within the project.

Fourth, a notable approach in Uppsala BIO-X for handling relational tensions was to make compromises. In the SC, the need for broad legitimacy meant that decisions about BIO-X had to be subject to compromises. Before the activities in BIO-X started, the SC had such difficulty reaching an agreement quick enough that it to a great extent outsourced the decision to the consultant. The consultant, in turn, would have preferred to arrange BIO-X such that it came closer to industry, but felt that such a proposal would not be accepted by the SC. The result, BIO-X, was a compromise that the SAB and program manager later transformed into BIO-X. The BIO-X organization also seems to have compromised to create Project 1. The PIs in Project 1 agreed to base the project on the suggested three research groups, though they were unwilling to make compromises such that they would fit in BIO-X.

Despite the attempts to create shared views, and in turn stabilize the organization of BIO-X and the projects, the degrees to which the key participants changed their views is unclear. On the one hand, researchers in the projects deepened their insights into processes of research commercialization (cf VINNOVA 2007a). On the other hand, all key participants have at the same time been fairly persistent in their views. For example, none of the three interviewed university representatives in the SC seem to have changed
opinions regarding what activities BIO-X should support, although the views differ a bit amongst them. Notably, one of them comments:

“If [BIO-X] would have been realized, with participants gathered around a common agenda, that would have been a significant advantage for our university, no doubt. … Today this is more project-based. The marketing value of Uppsala’s biotechnological research, [author’s comment] is still there. But one might ask what value each BIO-X project has to our university. The way I see it, our university has lost a bit on that point, no doubt. If Uppsala BIO would only be [BIO-X projects], and not also about marketing, one would not run Uppsala BIO the way it is run today” (28)

Moreover, most researchers in Project 1 were unwilling to adapt to BIO-X, and researchers in Project 2 largely stuck to their initial plan and ideas, which meant that some suggestions and interpretations from the SAB were ignored.

Lateral authority

In the previous chapter it was observed that negotiations before the formal collaboration begins are important in relations between academia and industry. After an agreement has been reached, the opportunities for the academic to change track are often limited. This point appears even stronger in BIO-X, particularly in BIO-X, in which an important goal is to connect the project outcome to a sufficiently large and stable market niche. The SAB thus had significant authority, indicated, for example, by recurring suggestions to the academics to change the project plans. The university representatives in the SC who observed the SAB granted it authority by largely refusing to question it in the SC.

In a committee with collective decision-making such as the SC, lateral authority is instituted by all participants as they accept to take part. Collective decisions hold much authority since all participants have had a chance to make their voices heard. By partaking they also accept that a decision means the end of that discussion, which enables the group to progress. Thus, for actors in the SC, lateral authority depended on the degree to which others accepted their views before decisions were made. This includes some room for maneuvering by making use of information and supportive other actors, while not being devalued in the eyes of others.

This also indicates the limits of collective decision-making. Even if all participants accept a decision, that unanimity does not necessarily mean that they have changed their opinions. Continued disharmony between a participant and the organization has three implications. First, long-term planning tends to be prevented: the less the participants trust that the others will support “proper” decisions in the future, the less willing they will be to accept decisions that lock the organization in a certain direction. Second, the deci-
sion-making process is likely to take time, since they have to be preceded by negotiations. Third, participants with more extreme perspectives are likely to be marginalized (Sgourev 2008). In all three cases, there is an obvious risk that participants in the longer term lose some of their engagement as their personal preferences are not met.

There are examples of all three points in BIO-X. In addition, some actors in BIO-X whose influence was relatively marginal were a bit unengaged. For example, a few members of the SC did not have much insight into BIO-X, but did not seem to do much to be more informed. Lack of engagement is also to some extent shown by researchers involved in Projects 1 and 1a, and by the researcher leaving Project 1c. In addition, most interviewees had surprisingly little knowledge of important events in Uppsala BIO in which they were not personally involved. For example, almost no interviewee remembered how the consultant was recruited.

Seclusion

The network structure of Uppsala BIO and BIO-X implied that many BIO-X activities were carried out in places where other participants could not observe them. As such, although more shared perspectives was an important goal, transformations or noncompliance were in some cases the result of actions that to some extent were concealed.

One example is how the program manager strategically utilized space to establish his version of BIO-X. He first secured support in the SAB by an explicit definition of BIO-X that distinguished it from previous versions. He then moved on to the SC where he presented the SAB conclusion. It also seems that he continued to not involve the SC in the details. That way he partly disconnected BIO-X from the SC. However, that might have been a necessary strategy for him to get room to work. One of the university observers in the SAB explains the choice not to put pressure on the SAB with the same argument. As argued, for example, in chapter 5, authority is linked to control over space.

Another example is how the outcome of Project 2 – widely regarded as a success – partly depended on two researchers refusing to follow suggestions from the SAB. The most praised outcome of Project 2 was based on both a technology and a standard test that had been questioned by the SAB. Moreover, against what the SAB expected, the group also (nearly) met the original goal (almost) on time. The outcome of Project 2 was thus crucially shaped both by the SAB and by the project group.
8.5 Chapter conclusion

The format of BIO-X shifted during the studied period. The reasons for the shift are found mainly in the different interests of the BIO-X stakeholders in the SC. Tension was found mainly in the balance between scientific and industrial interests. Over time, the industrial perspective became more prominent. Three phases are identified in the formation of BIO-X. BIO-X₁ would be a center in which university researchers from different disciplines would work together with commercially relevant research and in which industry would have an advisory role. In BIO-X₂, university researchers would work together in long-term projects in which industrial representatives had real influence. In BIO-X₃ – the format under which almost all BIO-X research has been undertaken – the industrial perspective was put above the goal of cross-disciplinary research, and the projects had a shorter time-frame.

Researchers in the projects responded differently to the balance between science and industry in BIO-X. Project 1 was formed during BIO-X₂, and the PIs were unwilling to adapt to the changing demands that followed with BIO-X₃. There were also disagreements about the potential of the project. In the end, Project 1 was split in two, Projects 1a and 1b. The first was later terminated due to low commercial potential. The latter was soon aborted as the partner firm began to work with too similar of problems and was remade into Project 1c, which was positively received by the BIO-X management. Project 2 was initiated just as BIO-X₃ began and experienced less tension with BIO-X. In addition, with a focus on industrial interests, scientific skills were relatively less valued and more emphasis was put on coordination, adaptation, and efficiency. That created relational tensions between the BIO-X management and both projects. For example, the SAB put forward that Project 2 could be broken up in order to enhance the potential of a valuable commercial product within the funded period.

Relational tensions in the SC were handled somewhat differently than they were in the examples in previous chapters. The main reason for this is the instituted collective decision-making, which means that compromises usually have to be made and that a decision is authorized by all participants. One consequence of such a format is that there are incentives for socialization as well as to secure strategic allies. There are indications that such behavior was successfully used by proponents of the industrial perspective to establish BIO-X₃.

Relational tensions between the projects and the BIO-X management were handled by informing each other about one’s views at meetings. Occasionally the SAB made use of its power to terminate or reform the projects. The PIs responded partly by adapting, partly by continuing in the old tracks somewhat against suggestions from the SAB. In one case a PI left a project. Overall, the study of BIO-X is in line with the observation in chapter seven, that as academia comes closer to industry individual academics retain less
authority. Although the type of activity makes the shift in authority natural, it might create unproductive tensions, in particular if the academic researcher is not aware of the difference in the ways work is organized.

A notable observation is that no interviewee really seemed to have changed their perspectives on what is “proper” BIO-X activity during their engagement in BIO-X. If that is true, the case indicates that effective performance in an organization such as BIO-X depends on having a firmly based organizational structure. From another perspective, it would also indicate that a triple-helix constellation often implies both unproductive friction and losers, since it is unlikely that participants agree and adapt. That also implies the risk of having unengaged participants. These points should however be supported by more detailed, observatory research. Only one chapter now remains of this study: the conclusion.
The aim of this study is to explore the basic social conditions for high-quality university research. The aim is particularly relevant given the observation that Swedish research policy during the last decades has come to include more of a market perspective. As a consequence, policies are based more on the perceived needs of industry and other societal actors than they were before, and academics are ranked in comparison to one another rather than ranked by individual merit.

Although such policies might benefit Swedish industry, and might create better opportunities for relatively established academic researchers, they might also generate problems. Sustained usefulness of the academic system is likely to depend on its ability to retain a variation of research perspectives. That variety can be threatened by increased expectations for interaction and coordination. In terms of this study, it might decrease the average relational distance between actors in the research system, and as such the productive relational tension, as academics interact with each other or with industry. Moreover, individual academics might be frustrated as they find their positions more contested than before and experience relational tensions as a result of both interpersonal interaction and the sometimes diverging interests of organized academia and industry.

These issues have largely been overlooked by economic geographers studying knowledge creation. In economic geography, relational distance and the potential of relational tensions to generate new knowledge and innovations have often been used as a theoretical starting point to discuss the benefits of increased relational proximity. In addition, the positions and interests of individuals in organizations have often been relatively neglected.

This concluding chapter consists of two sections. In the first section, the discussion on relational tensions is summarized and concluded. The second section indicates how this study can contribute to economic geography, provides an outlook toward further studies, and clarifies the potential usefulness of this study to practitioners in academia and to policy-makers.

9.1 Relational tensions
This section consists of four parts. In the first part, the position of individuals in their context is discussed, and it is argued that relational distance between
researchers largely depends on the degree to which they have control over their own time and space. In the second part, relational tensions arising during coordination with the organizational goals of academia and industry are discussed. The third part concerns ways to handle relational tensions. In the fourth part, the conclusions are related to the features of systems characterized by competition, and the limits of competition in research environments are discussed.

Individuals in context

Relational distance between individual researchers in contemporary academia is quite dependent on the extent to which they have personal control over their immediate context. The argument departs from observations in cognitive science and continental philosophy that propose that individuals make sense of the world via concepts of the mind. From sensory impressions, the neural system produces representations to the mind of that which is experienced. The representations – and thereby the experienced phenomena – are made meaningful by concepts of the mind. As a consequence, learning is about comparing concepts of the mind with experienced phenomena, and changing the concepts if better alternatives are found. Thus, in the ideal type of “maximal learning”, an individual would treat the experienced phenomena as fixed and her conceptualizations as unstable – and would for the same reason be reluctant to manipulate phenomena of interest if they cannot be restored.

If a learning individual has control over her immediate context – over her personal time and space – she is enabled to focus on the phenomena of interest from the perspective she prefers. The perspective emerges from the position – physically as well as intellectually – she puts herself in. If a learning individual has control of her context, she can act and re-order the place the way she likes – for example, the way she arranges an experiment in a lab – and that way enhance her possibilities to advance her understanding. In addition, she might also be able to restrict the space from disturbances, to better keep focus. When interviewees in this study describe fruitful research environments, a sense of control over time and space is highly rated: it gives them a feeling of freedom and of being trusted, which is motivating.

A similar pattern is found in social relations. When individuals interact, they are able to observe and communicate with each other, and as such both learn about and from each other. This is particularly effective in face-to-face interaction, when the situation and discussion can be constructed to concern precisely the issues of interest. That is probably the reason why many interviewees mention unscheduled time at, for example, conferences or coffee breaks as important occasions and why collaborations are normally preceded by face-to-face meetings.
In order to work sufficiently well together, collaborating researchers have to develop a “shared social platform” for, for example, common conceptualizations of important phenomena and agreeable ways of organizing work. They thereby institutionalize some aspects and allow others to be more in focus. Socialization is in that way a natural step to create sufficient harmony in research collaborations. If individuals differ in status – in the authority they have – the one with the lower status typically adapts to the one with the higher status rather than the other way around. In this study, for example, that is indicated in the way some interviewees talk about senior researchers as role models.

An effect of socialization is that individuals acquire a bit more similar perspectives; they reduce the relational distance between each other. However, reducing relational distance is not necessarily desirable in research. One reason is that creative friction in interaction is about harnessing different perspectives. Another reason is that individuals might be unwilling or unable to develop sufficiently shared perspectives. For some people, social interaction might thus be paralleled with a sense, or fear, of losing control of personal space and time.

Individuals can enhance their sense of being in control of the context and of their identity by reflecting on how they and their collaborators affect one another. A good example is the way a couple of interviewees discuss cross-disciplinary collaboration in basic research. In such collaborations all participants try to connect their own research to one or more phenomena about which they know very little. Reflection on the limits of the knowledge of oneself and others is therefore crucial. Not only can they then better inform each other, but if an individual is aware of her own knowledge, she is also able to trust her own competence. Each participant also has to trust that her partners are competent – which she cannot judge on her own – and to believe that her partners trust that she is competent. She might also have to prove that she is able to question conceptualizations she holds very dear. Cross-disciplinary work shows how deeply embedded academics are in their fields. It also shows how lateral authority in interpersonal relations can be useful in research as a way to handle relational tensions.

If interacting individuals are not able to retain the sense of control they prefer, there is a risk of conflict or irritation. A typical example is when participants are supposed to accept a perspective that at least one of them finds inappropriate. Such situations can be productive as they may eventually result in novel insights, but they can also be detrimental as individuals might get unhappy, or the variety of perspectives in the research system might decrease.
Academia and industry

Conflicts can also go back to institutionalized social systems, such as an established research field or industry. In such cases certain perspectives are often more rigid, as they are embedded in the ways organizations, physical space, and established patterns of thought are structured. In that way, single individuals are less able to control time and space than they would be if they were “learning on their own”. Consequently, the risk for unproductive relational tensions increases. In this study, such situations are observed in two contexts: within academia and in the interface between academia and industry.

Academia is a centralized system in which recognition is gained from insiders. To some extent, academics are thus “forced” to interact. Tensions might arise if they have to obtain acknowledgement from more well-positioned actors in the field or in the local university on terms they do not agree with. Several interviewees report negative experiences and frustrations concerning such matters. This is most clearly visible in the examples of the biologist and the physicist in chapter six who claim to have struggled hard to get recognition for research that later got relatively acclaimed. Several interviewees have also chosen not to work with senior, established researchers with plenty of resources because they wanted more control over their own research context. Along the same line, a few respondents criticize their Centres of Excellences for giving extended control to the most senior researchers.

In relations with industry, different perspectives are inherent. The academic researcher focuses on her research and how it fits into organized science. As such, collaboration with industry is interesting to the academic to the extent that it makes her proud of her performance and earns her merit. In both cases, satisfaction largely depends on the degree to which the researcher is able to observe and delimit her contribution.

The firm focuses on its organization’s well-being, which ultimately rests on its capacity to control a market niche. As such, each individual to some extent becomes a means to an end for the organization, and the knowledge they embody is valued accordingly. The firm thus has a broader perspective, paying more attention to how its activities – such as a research project – link to each other. This is seen, for example, when firms lose interest in research projects because of changed organizational strategies, and in Uppsala BIO-X when the Scientific Advisory Board directed the projects relatively forcefully toward commercial goals.

With more focus on existing technologies and customer perspectives – as well as, sometimes, an interest in profit-making in the short run – firms typically put comparably less value on radical development. The possibilities to connect firms with scientific research might thus be a bit limited and rigid; some aspects often have to be treated as if they were unquestionable. Examples of tensions experienced by academics because of this include different views between the academic researcher and industry on what makes research
worth commercializing and on what is appropriate technological sophistication and efficiency.

There might also be tensions due to differences in how openly the firm and the researcher want to communicate ongoing activities to external actors. Restricted communication implies less possibility for knowledge development. On this point, the potential conflict between incentives to publish and to patent appears very limited: the interviewed academic researchers had very little problems with the need of partner firms to keep unpatented findings secret – perhaps partly because competition between individuals in science is likely to have also informally instituted some restrictions in communication at universities.

However, another aspect of openness in communication appears more contested and, as such, a risk for negative relational tensions. If two interacting actors have different knowledge of a situation, one might attain merit or make economic profits on behalf of the other actor by concealing one’s knowledge or intentions, or even by lying. That possibility largely depends on disconnected spaces: the relatively integrated and more open academia offers less room for such behavior, while in industry communication is generally more restricted and university researchers usually have limited knowledge about the industrial context. Several interviewees report negative experiences of that kind in relations with industry. In a couple of cases it was one of the reasons why researchers started their own firms, or funded their firms with personal savings, instead of working with industrial partners or investors.

Coping with relational tensions

It is argued in this study that relational tensions can be handled in three main ways: socialization, lateral authority, and seclusion. These are relevant to various degrees in handling all types of relational tensions. Socialization, lateral authority, and seclusion are presented in Figure 11 divided along two lines – whether or not to maintain relational distance and whether or not to maintain interaction. In other words, the balance between the three is likely to determine the variety of perspectives as well as the likelihood of fruitful interaction in the long run, both within academia as well as between academia and industry.

Socialization implies decreasing relational distance. As argued above, it is a natural aspect of interaction. It is also observed in this study as a strategic means of coping with relational tensions. Four types of strategies pointing toward socialization have been identified:

- acquiring knowledge about each other or the organization,
- informing others about one’s views,
- making compromises, and
- gate-keeping.
All of these have positive sides. To work together, it is necessary for actors to learn about one another. Very often there are also incommensurable goals or constraining situations that make compromises necessary – Uppsala BIO-X is a good illustration. And to ensure quality in research and industry, some kind of gate-keeping is necessary.

However, adaptation to others also carries significant risks. Adaptation and coordination often means less time to focus on research. More fundamentally, it implies that an individual changes perspective not necessarily because she finds the other perspective more correct but because it is more opportune or because she trusts others more than she trusts herself. Such behavior is most probably not rewarded in basic research, in which it often takes a long time before good results are reached.

Two additional observations are worth noting. First, it seems that the identities of many researchers are relatively inflexible. For example, there are indications of differences between basic researchers and applied researchers in the way they position themselves in relation to the phenomena they study. Basic researchers seem to focus more on long-term scientific issues than applied researchers do, and some describe industrial collaborations in terms of their scientific potential. Applied researchers appear a bit more focused on satisfying the partner firms in relatively distinct projects. Their attitudes are probably linked to the fact that interests of organized science and of industry often differ from each other. Moreover, in Uppsala BIO-X, all interviewees
seem to have about the same views on what activities that are “proper” in Uppsala BIO-X as they had when they entered the organization.

Second, the existence of academia as an instituted field depends on the degree to which it is perceived as a field in its own right. As a consequence, to the extent universities become institutionalized as components in national innovation systems, academics are likely to identify themselves also as suppliers to industry. That task is probably viewed as being partly on the expense of satisfying personal interests by doing research on problems defined by the researcher alone. If so, the academics are likely to demand more compensation on industrial grounds – for example, by getting wages more similar to those in industry. A few respondents express dissatisfaction of that kind.

Adaptation strategies can be counterbalanced by lateral authority and seclusion. With a focus on the academic researcher, lateral authority is connected to more room for the researcher to act according to her interests. Overall, stable organizations appear important – the individuals can then foresee the consequences of their actions reasonably well. That is indicted by the frustration a few respondents experienced when partner firms changed strategies. It is also a key reason why Project 1 in Uppsala BIO-X failed. Other typical strategies to establish lateral authority include:

- finding a phenomenon interesting from several perspectives;
- having fun together based on issues not related to research;
- trusting each other’s knowledge; and
- having a “safety net” – for example, a secure employment, sufficient resources tied to the individual researcher, or legal protection – such that opportunist behavior from others is less costly for the academic researcher.

If an individual experiences that she has too little authority and is unwilling to change her perspective, seclusion is the alternative. Seclusion might involve restricting interaction by utilizing spaces over which others have limited insight or influence. That way an individual might attain more control over time and space than would have otherwise been the case. Or seclusion might involve ignoring or avoiding others or concealing one’s intentions or knowledge in order to manipulate or not upset another actor. It might also concern more subtle issues, such as saving tasks that require much concentration for times when others are not around.

Seclusion might be chosen as a strategy to enhance one’s position. There are several such examples in this study. In academia, respondents ignored criticism from influential peers and avoided environments with established seniors, worked outside ordinary hours or at home, or were selective or slightly incorrect with information for funders or other actors. In other instances, respondents made efforts to separate academia and industry in space
by not inviting firms to the lab or by not engaging too much in industry. Some also used seclusion to not make colleagues know about their work with research commercialization. In addition, organizations might make use of strategic seclusion by organizing work in projects that run for a limited amount of time, or organize its space such that the participants interact with each other only part of the time.

The limits of competition

The observations in this study shed new light on the context of academic research, in which competition and incentives for efficiency have increased. Competition increases the relative value of central positions, and thereby pits actors against each other in a more hierarchical and “fluid” system. While such systems might be relatively productive at least in the short run, they also bring about three identified risks.

First, an actor has larger incentives to secure a position by help of strategic maneuvers. As shown in some examples in this study, that might include concealed intentions as well as open or concealed attempts to manipulate selected actors – such as funders – so that they adopt certain perspectives or act in ways that benefit the researcher. It also increases the risk of marginalizing promising research – partly because well-positioned evaluators might overestimate their capacities to judge others, partly because weak actors and fields might find it difficult to make themselves heard. For the system of basic research, that is most likely negative. In line with the observations of March (1991), if the quality of research projects are spread as in a normal distribution, increased incentives to adapt to others will decrease the relative share of ground-breaking elite research. As a consequence, competition increases the risk for lock-in effects.

Second, in a system characterized by competition, many actors have comparably little authority inherent in their position. It is therefore an obvious risk that actors trust each other less than in other systems. Such behavior includes concealed intentions and suspicions of dishonesty on the part of other actors. Moreover, it cannot be taken for granted that trust will be increased by control mechanisms and regulations; one aspect of such initiatives is that they manifest the limited trust that the regulator has in others. Limited trust due to restricted information might also be a risk for a well-functioning interface between academia and industry, since researchers often take significant pride in their activities.

Third, in competitive systems individuals will be less able to define their own identities. The reason is an insoluble dissonance between a system at large and the individuals within it. If focus is on the performance of a system, individuals within it become, to some extent, resources for the system. In such situations the identity of the individual becomes increasingly defined by the outside. The individual, naturally, would prefer to define her own
identity. Since individuals are often not as adaptive as would be desirable from the perspective of the system, they might experience unhappiness and frustration.

Secluding behavior should perhaps be understood in this light. When individuals do not feel that they control their personal contexts, and are not sure that they and their interests will be accepted, they might withdraw and choose not to reveal their actual knowledge and intentions. They can then be more in control and satisfy their interests. If they cannot act in seclusion, their level of engagement might very well drop. As such, the most negative effect of attempts to increase the performance of Swedish academia by increasing competition and efficiency might be the decrease in the researcher’s control of time and space.

9.2 Outlook

This section consists of three parts. In the first part, the conclusions in this study are discussed within the context of economic geography. The second part presents suggestions for future studies. In the final part, a number of potentially relevant ideas for practitioners are presented.

This study within economic geography

The arguably most important finding in this study regards the role that interpersonal trust plays for innovative activities. Knowledge creation begins with individuals, who perform better if they feel in control of their contexts. If individuals are in control of their contexts they also have better possibilities to challenge established patterns of thoughts and ways of doing things. By that way distancing themselves from others, they produce variety in the research system, and as such, potential opportunities for innovative industrial ventures.

This finding illuminates some areas that economic geographers often overlook. In accounts on competitive industrial systems, individuals in organizations are often not included or discussed only from the perspective of the organization. The findings also indicate the usefulness with combining theories on systems of organizations with theories on systems of individuals. Studies on competitive industrial systems also tend to focus on how organizations can benefit from resources and conditions connected to their local or national contexts, by pointing at the benefits of more relational proximity on the organizational level. The findings in this study complement such studies by putting more emphasis on the limits of relational proximity.

Another point concerns the role that observation plays for innovative performances. The possibility to observe other people in action can enhance the quality of a researcher or a research group. For example, by working closely
to skilled other researchers, or by interacting face-to-face with a potential collaborator, researchers get a better sense of what brings quality to research and of suitable ways of organizing work (and of which collaborations to avoid). However, observation also tends to result in more shared perspectives, and as such, less diversity.

The point on observation also concerns governance. There are two sides of the coin. On the one side, when individuals – for example researchers – want to be independent, they sometimes choose seclusion, to work in spaces where others cannot observe them as much. In such spaces they can better be in control of their thoughts and of the consequences of their actions. On the other side, if one cannot be observed, one also becomes more unknown, which can be useful in order to become more powerful or to direct others.

Further studies

A few openings for further study are identified. One suggestion is to complement the relative focus on negative examples of relational tensions in this study with studies that follow in the tracks of, for example, Beunza and Stark (2004) and Ibert (2010) and put a bit more emphasis on positive examples. Since the focus on negative examples in this study was partly a consequence of the limits of interviewing – it is easier to observe and talk about negative examples since they contain more contrasts – such studies would most likely require ethnographic approaches.

Ethnographic investigations could also reveal other pertinent questions or place-specific details that could not be explored in this study. How do researchers use specific places to build trust? How, and on what grounds, do researchers balance between openness and seclusion in different contexts? Regarding Uppsala BIO-X, observations of the Steering Committee and of the Scientific Advisory Board were missing, which meant that the debates that led up to certain decisions could not be investigated. Future studies would benefit from including observations of such meetings as well as of events occurring in-between the meetings.

At the same time, this study should, as all explorative studies should, be complemented with more systematic and large-scale investigations. For example, what evidence is there of identity differences between basic and applied researchers? How does the identity of the researcher correlate with, for example, bibliometric data on citations and publications? How does it correlate with data on activities of their PhD students after graduation? How does trust in categories of actors correlate with the main types of activities of the researcher?

There is also a shortage of studies on how researchers negotiate their identities between different contexts. To what extent are they able to “switch on and off” depending on what the situation demands? And what effects does increased international participation have on researchers’ attitudes to
“proper” ways of organizing research and working with industry? In the same vein, there is a lack of longitudinal studies of institutional changes within university milieus as academics engage more with industry (Colyvas and Powell 2006). Conclusions from the study of Uppsala BIO-X indicate that triple-helix settings might be interesting cases.

And finally, given the observation that knowledge creation is bolstered by open communication and trustful interpersonal relations, what does the emerging paradigm of “open innovation” in industry imply for academia? “Open” is in this case defined from an industrial perspective. Does “open innovation” from an academic perspective imply “less open research”? If so, how does that affect the social relations between academics? And how does it affect the role and position of academia in society?

Ideas for practitioners

Academics may pay attention to the observations regarding self-reflection and trust. An academic can enhance her sense of being in control if she reflects on herself and her context. The example of cross-disciplinary collaboration in basic research is illuminating: since the participants were laymen in each other’s fields, they had to carefully reflect about their knowledge and their expressions to support the others. That way each researcher also got more aware of her own knowledge, which most probably led to more self-assuredness. They also had to put faith in each other’s competences as well as trust that others put faith in theirs. Similar attitudes are likely to enhance the quality of collaborations with industry as well as the performances of junior researchers in the lab.

Regarding research policy – which might be of interest to both academics and policy-makers – a few observations are made. A first point concerns the limits with using theories developed to analyze organizations – for example, cluster theory and theories on innovation systems – to understand and formulate policies for systems that depend much on the performance of single individuals. The main reasons are that theories for organizations tend to overlook how diversity is produced and sustained, as well as how the interests of individuals might differ from the interests of organizations.

Another point regards the balance between basic and applied research. Interaction between basic and applied researchers is important. However, there are indications that social harmony in research groups is related to the extent to which they focus on either basic or applied research. Mixing basic and applied research in the same constellation of researchers might thus negatively affect the overall performance of the group, mainly because they seem to have different working procedures.

It is also observed how stable positions for individual researchers can benefit the performance in research environments. This concerns stable organizational structures and long-term funding: the abilities to predict conse-
quences of one’s actions and to be have room for making mistakes is of fundamental importance in research, with its long-term perspectives and natural unpredictability. The development of Project 1 in Uppsala BIO-X adds to the point about the benefits with predictable consequences. In addition, stable positions might also be beneficial for academic researchers’ relations with industry, as researchers interested in industry might find it difficult to combine science and industry if they are financed only with short-term grants. Most fundamentally, the issue of stable positions relates to the researcher’s sense of being authorized as an expert.

An additional observation concerns the risk of a small number of individuals having much influence, as it might cause lock-in effects. Such a risk could partly be prevented by encouraging variation among funders and reviewers of project proposals.

In addition, running through the study are the roles that personal motivation and identity plays in research performance. For that reason, recruitment and grant-awarding processes might benefit from being coupled with carefully conducted interviews.

The perhaps most intriguing policy questions emanating from this study concern the discussion on socialization, lateral authority, and seclusion. How should they be balanced? Unfortunately, the conclusion is that, since the three types of approaches are practically impossible to delimit and pin down, it is also not possible to discuss them in terms of a “balance” other than in theory. In addition, also researchers in similar positions might be individually quite different regarding the social contexts they prefer. However, much indicates that the present situation implies a shortage of lateral authority. As Delanty (2001:158) puts it,

“…goals are set for the university rather than by the university which consequently does not utilize its full resources. To this end the older notion of academic freedom is still relevant as long as it is not an excuse to evade societal responsibility.”

Albeit Delanty’s point primarily concerns education, it is likely that the capacity of the university to sustain its position as a provider of research findings and skilled people with explorative minds is connected to the independence and authority of university researchers.
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Torkel Klingberg, Karolinska Institute, November 26, 2007
Ulf Landegren, Uppsala University, January 21, 2009*
Anita Lloyd-Spetz, Linköping University, March 18, 2008
Eva Malmström, Royal Institute of Technology, February 25, 2008
Lars Montelius, Lund University, April 1, 2008
Mats Nilsson, Uppsala University, January 20, 2009*
Magnus Nydén, Chalmers University of Technology, March 4, 2008
Arne Öhman, Karolinska Institute, November 19, 2007
Eva Olsson, Chalmers University of Technology, March 6, 2008
Agneta Richter-Dahlfors, Karolinska Institute, November 28, 2007
Tuula Teeri, Royal Institute of Technology, February 28, 2008
Lars Wågberg, Royal Institute of Technology, March 19, 2008
Lars-Erik Wernersson, Lund University, March 31, 2008
Anonymous, November 5, 2007
Anonymous, December 12, 2007
Anonymous, February 18, 2008

*A part of this interview also concerned Uppsala BIO-X

Uppsala BIO-X
Jan-Otto Carlsson, former Deputy Vice Chancellor at Uppsala University and former member of the Uppsala BIO Steering Committee, January 14, 2009
Björn Ekström, former consultant to Uppsala BIO-X, October 29, 2009
Kristina Glimelius, former Deputy Vice Chancellor at the Swedish University of Agricultural Sciences and former member of the Uppsala BIO Steering Committee, October 28, 2009
Madeleine Neil, Director of Communications at Uppsala BIO, October 29, 2009
Ulf Pettersson, former Deputy Vice Chancellor at Uppsala University and former member of the Uppsala BIO Steering Committee, January 20, 2009

In addition, three interviews with Jonas Åström, former program manager for Uppsala BIO-X, are included. These were jointly conducted by Per Lundequist, Katarina Pettersson and Robin Teigland on January 22, January 30, and February 19, 2008
Informant interviews

Benny André, Uppsala University, October 17, 2007
Ulf Carlson, former Head of Research at SCA, April 3, 2008
Michael Corell, Uppsala University, October 23, 2007
Sofia Dahlberg, Swedish Institute for Infectious Disease Control, June 1, 2006
Lars Hultman, Linköping University, June 26, 2007
Henrik Kratz, Uppsala University, October 24, 2007
Anonymous research manager at a large firm, June 4, 2008
Anonymous research manager at a large firm, June 13, 2008
Appendix A: Rights of the respondent

My name is Tobias Fridholm and I am a PhD student at Uppsala University. I am conducting research on the evolution and organization of high-quality research milieus in science and technology. This document outlines your rights and protections as an interview subject. Your participation in this interview is voluntary. You may decline to answer any question and stop taking part in the study at any time.

There are no direct benefits from participation. Risks are limited as all information will be kept confidential. Notes and identifying information from this interview will not be available to anyone who is not directly concerned. You name and identifying information will not be used in reports or publications without your express consent.

If you have any questions, please contact me at +46(0)18 4711322 or tobias.fridholm@kultgeog.uu.se. If you agree to take part in this research, please sign one of the three lines below and keep one copy of the agreement for future reference. If you have any questions about your rights or treatment as a participant in this research project, please contact Head of Department Jan Öhman, Dept. of Social and Economic Geography, Uppsala University at +46(0)18 4712539 or Legal Affairs Office at Uppsala University at +46(0)18 471 00 00 or juravd@uadm.uu.se.27

I have read this consent form and agree to take part in this research, with the understanding that you will not disclose my name or identifying information in any way without my express consent.

Signature:_________________________________________Date:______

I have read this consent form and agree to take part in this research. You may include my name as part of a list of interview subjects, but do not use my identifying information in any other way without my express consent.

Signature_________________________________________Date:______

I have read this consent form and agree to take part in this research. You may use my name and other identifying information in reports and publications resulting from this interview.

Signature:_________________________________________Date:______

27 The interviewees signed the document in Swedish. When all interviews were made, the Legal Affairs Office made me alert that the document is not legally valid: research data must be disclosed upon request. The interviewees will be notified if interview data is requested.
Appendix B: Protocol for interviews with university researchers

Background\textsuperscript{28}
- Description of research

Career and development of skills
- Steps in career
- Periods in career: development, breakthrough, consolidation

Research environment in academia
- What is the breakthrough?
- What was the context of important research
- Important collaborations: establish, maintain
- Everyday environment
  - Who are there? The closest collaborators?
  - Management: Who? What type of role? Informal leaders?
  - Openness: circulation of people
  - Diversity: Different backgrounds?
  - Social environment, leisure time activities together?
- External contacts, who, where?
  - How is contact maintained?
- Material: Unique equipment? Importance of equipment
- Funding: Where does the money come from?

Private situation at the time of important research?

CoEs
- How do you do to get a grant?
- How does the CoE affect your research?
- How does work across disciplines take place?
- How is the CoE organized?
- EU-networks etc?

\textsuperscript{28} This is an English translation. The outline of the protocol changed slightly over time. However, all main parts (in bold) were kept. This is a representative version.
Changing circumstances for research

- Basic vs applied, relevant distinction?
- Degree of collaboration with non-academics
  - How? (Contacts, finance, collaboration)
  - Development?
- How are industrial relations affecting research?
- Work with foreign firms?
- Problems to work across the interface?
- Secrecy
- Finishing projects/endless research?

Patents

- Why/not?
- What do you do with your patents?
- If own firm
  - How to combine business and research?
  - What is/was the goal with the firm?
  - How did you work with the firm?
  - How was business knowledge acquired?
  - How was capital acquired?

Others to talk to?
Geografiska regionstudier
Utgivna av Kulturgeografiska institutionen vid Uppsala universitet

Nr 1 Uppsala län med omnejd [The County of Uppsala with Environs], 1958:
Del I Gerd Enequist och Lennart Hartin: Folkmängd, odling och industri [Distribution of Population, Cultivated Areas and Manufacturing Industries].
Del II Björn Bosæus: Resor till arbete och service. Regionindelning [Travel to Work and Service – Regional Division], 1958


Nr 4 Hans Aldskogius: Studier i Siljanområdets fritidsbebyggelse [Studies in the Geography of Vacation House Settlement in the Siljan Region], 1968.

Nr 5 Maj Ohre-Aldskogius: Folkmängdsförändring och stadstillväxt. En studie av stora och medelstora stadsregioner [Population Change and Urban Sprawl – A Study of Large and Middle Size City Regions], 1968.


Nr 8 Hans Ländell: Analyser av partihandelns lokalisering [Analysis of the Location of Wholesale Trade], 1972.


Nr 13  Jan Öhman: Staden och det varjedagliga utbytet [The City and the Everyday Exchange], 1982.


Nr 22  Naseem Jeryis: Small-Scale Enterprices in Arab Villages – A Case Study from the Galilee Region in Israel, 1990.


Nr 35 Staffan Larsson: * Lokal förankring och global räckvidd. En studie av teknikutveckling i svensk maskinindustri* [Local Embeddedness and Global Reach – A Study of Technological Development in the Swedish machinery Industry], 1998.


Nr 38 Anna-Karin Berglund: *Lokala utvecklingsgrupper på landsbygden. Analys av några lokala utvecklingsgrupper i termer av platsrelaterad gemenskap, platsrelaterad social rörelse och systemintegrerad lokal organisation* [Local Development Groups in the Countryside of Sweden – Place-Related Communality, Place-Related Social Movement, System-Integrated Local Organisation], 1998.


Nr 40 Susanne Johansson: *Genusstrukturer och lokala välfärdsmodeller. Fyra kommuner möter omvandlingen av den offentliga sektorn*
Jan Amcoff: *Samtida bosättning på svensk landsbygd* [Contemporary Settling in the Swedish Countryside], 2000.

Susanne Stenbacka: *Landsbygdsboende i inflyttarnas perspektiv. Intention och handling i lokalsamhället* [Countryside Living From the Perspectives of Newcomers – Intentions and Actions in the Local Community], 2001.


Eva Andersson: *Från Sorgedalen till Glädjehöjden – omgivningens betydelse för socioekonomisk karriär* [From the Vally of Sadness to the Hill of Happiness – The Significance of Surroundings for Socio-Economic Career], 2001.


Kristina Zampoukos: *IT, planeringen och kommunerna* [Information technology and Municipal Planning in Sweden], 2002.


Nr 69 Lena Molin: *Rum, frirum och moral: En studie av skolgeografins innehållsval* [Space, Curriculum space and Morality. About school geography, content and teachers’ choice], 2006.


