Cognition, science and geography –
an exploration into mental infrastructure and interdisciplinarity

Master's thesis

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Abstract

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This text explores the significance of perspectives from the cognitive sciences for interdisciplinarity or cross-domain thinking in science, and particularly in geography.

First interdisciplinarity is examined, describing some of the central conditions that such ventures exist under. The interaction between *different conceptual systems* is identified as a core issue in interdisciplinarity. By focusing on our conceptual systems the dissertation aims to say something general about interdisciplinarity and science, as well as focusing on what may be considered specific to geographic ways of thought.

The conditions for scientific and cross-domain thinking are explored through an examination of the human mind. The problem of relating our mental microstructures to cultural and scientific phenomena is given particular attention. Evolution, it is argued, has given the human mind an 'intuitive ontology,' which has significance for science, especially since it incorporates deeply rooted boundaries between knowledge domains. While this is important, it is incomplete as a view of science or of a discipline. The thesis therefore seeks to supply this theory with as much context as possible in order to get a more complete understanding of scientific activities.

To establish some features of the geographical way of thought, the intellectual history of the discipline is explored. Three features are particularly emphasised, namely its focus on *usefulness*, on *synthesis*, and on *visual analysis*. These features, it is theorised, form important parts of the explanation for why geographers often have sought to transgress boundaries despite the above mentioned intuitive ontology.

The thesis is mainly a theoretical contribution, but has also an empirical component. The Department of Geography at NTNU, Trondheim is described with a dual focus on intellectual characteristics and socio-cultural characteristics. The purpose of this is to better understand the theory about human cognition and geography in light of the complexities of a concrete case. By treating the Department as a complex adaptive system, the many different factors found to be significant are sought treated within one, relatively unified conceptual system.

Preface

This dissertation is the result of quite a long process. Since starting my work on it, I have been engaged in various other activities, which, among other things, include doing a master in cognitive anthropology and working as a museum curator. These 'breaks' have been useful both by providing new thoughts, and by giving me the opportunity to reflect more in-depth.

Particularly my stay at the London School of Economics, doing an MSc in the Anthropology of learning and cognition, has been important. I therefore want to express my gratitude towards Professor Maurice Bloch and Professor Charles Stafford at the LSE. Without them, this dissertation would have been very different indeed.

I also want to thank Professor Stein E. Johansen, Dr. polit. and clinical psychologist Øyvind Eikrem, Øyvind Dahl, Kjerlaug M. Kuløy, Vegar Jordanger and Jo Hammerstad for in various ways helping me with the text. Øyvind Eikrem deserves extra gratitude because of his persistent moral support.

The Department of Geography at NTNU, particularly my interviewees, deserve my deepest thanks because they allowed me to do research on them. This dissertation would not have been possible without the constructive openness of the people at the Department. This trait of constructive openness is not least present in my advisor Professor Michael Jones, who has challenged and supported me through the difficult work on this thesis. I want to thank him for this.

I also want to thank my mother for her kind support, and last but not least, Mia Kristin Midtbø deserves my gratitude because of her very important support and for proofreading the manuscript.

Anders B. Asphaug

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Chapter 1: Introduction

> *The most merciful thing in the world, I think, is the inability of the human mind to correlate all its contents. We live on a placid island of ignorance in the midst of black seas of infinity, and it was not meant that we should voyage far. The sciences, each straining in its own direction, have hitherto harmed us little; but some day the piecing together of dissociated knowledge will open up such terrifying vistas of reality, and of our frightful position therein, that we shall either go mad from the revelation or flee from the light into the peace and safety of a new dark age* (H.P. Lovecraft, The call of Cthulhu).

I choose to start the introduction with this very pessimistic quote from H. P. Lovecraft for several reasons. One is humour. In an intellectual climate where interdisciplinarity is hailed almost by default, humour can be liberating, especially since we need to reflect creatively on interdisciplinarity, rather than locking in on default assumptions about it.

Second, he has a point. Although I disagree with his pessimism, it is clearly true that the human mind fails to 'correlate all its contents.' Although *association* may be what minds do, there is nevertheless also a tendency to separate domains from each other, creating blindness to connections that once established seem obvious. This is probably an important function of the mind. We have to be restricted, or we get 'informational explosion' and chaos.

The third reason for quoting Lovecraft is his apparent fear of 'the piecing together of dissociated knowledge,' which for me seems utterly unfounded. Although I suspect Lovecraft's fear is spiritual rather than mundane – he fears that the knowledge might harm our souls – I choose to stress this point. What we need is exactly the piecing together of information, because acting upon incomplete knowledge can have such serious consequences for our environment (to name but one example). It seems important not only to think about how to integrate knowledge, but also to think about how it is possible, given our specifically human mental abilities, to do so.

This dissertation is about how our 'mental infrastructure,' the hidden configuration of our mind, shape and influence our knowledge, most often without us being aware of it. It is also about the 'disciplined mind,' interdisciplinarity and geography. The history of geography has endowed us with an 'interdisciplinary discipline,' and accordingly it is interesting to

reflect on what it might mean that geographers' minds are, as it were, disciplined into operating in an interdisciplinary discipline.

Research problems

As indicated in the title, I have thought of this project as an exploratory one. There have been certain interests that have followed me throughout, but not a single precise problem formulation. However, I will here formulate, as concretely as possible, *what* the issues addressed are, *why* I think they are important to address and *how* I have chosen to do it. We can identify three main problems, all of which are interrelated, with the common aim to shed light on human knowledge in the context of science.

Question 1: What are the roles of the hidden infrastructure of our mind in science and interdisciplinarity?

Many of our mental processes are generally not transparent to us. Our knowledge is in other words shaped by factors that are hidden and tacit. To examine exactly what these tacit preconditions are and what they mean for science and interdisciplinarity, seems a very important pursuit. Such knowledge will increase the success we have in integrating our dissociated pieces of knowledge, I think, because by understanding more about the problems we encounter in such knowledge production, we will see how to better deal with them.

I approach the question in two ways. The first one is theoretical, and focuses on contributions from the cognitive sciences. An explicit account of the human mind is given, which it is hoped will prove useful for the present purpose.

Since science is a cultural phenomenon, however, we need tools for relating our cognitive structures to the cultural level. It is important to be careful in formulating this relation, in order to avoid the simplistic reductionism which many associate with endeavours which try to render biology relevant to society.

This forms the basis from which I construct some instruments for analysing scientific knowledge. I call these instruments the *ABC-domains,* and I seek to show that these can be employed in order to reach conclusions about the *type* of knowledge we construct (without necessarily evaluating its truthfulness), thus allowing us to see it in a relatively neutral meta-perspective (see chapter 4, 'Analysing scientific knowledge').

The second way I approach the question is empirical, but in an *indirect* manner, by describing the concrete socio-scientific context at the Department of Geography at NTNU. The idea behind this is that by providing a specific context we will see more clearly what the roles of our mental infrastructure are. This was my choice because a more direct approach to

studying the hidden structures of our mind would have required methods that were unfeasible (see next chapter for more on my methodological choices). The description of the Department of Geography also has other purposes, which will be treated below. But first I have to introduce the second main research question.

Question 2: What are the roles of the hidden infrastructure of our mind in geography?
As mentioned, geography is an 'interdisciplinary discipline.' It involves many different perspectives and themes – from positivist approaches in physical geography, to the 'softest' perspectives in the social science or humanistic parts of the discipline. Geographers often also seek to integrate or relate such different fields. This means that people and departments who work within this discipline are likely to encounter challenges that profitably can be understood partly on the background of the tacit structures of our mind.

The dissertation thus falls in line with a long geographical tradition of academic self-reflection. Like Richard Peet (1998, 1) writes: 'Geography has a permanent identity crisis because what geographers do is complex.' Coming to terms with the significance of this complexity for human cognition, is thus a central component of this thesis.

Something can (and will) be said in general terms about this in light of the answers we find to question 1 in chapter 4. But to reach a more complete understanding of the discipline in relation to our cognitive infrastructure, we need to incorporate more about what it means to be immersed in such a tradition. Therefore I will address the following: (Question 2a) *what is the intellectual constitution of the disciplined mind of geographers?* This is explored through an examination of the intellectual history of modern geography (in chapter 5, 'Geographic ways of thought'). It is assumed that such an exploration enables us to say *something*, at least, about geographers' intellectual constitution, or at any rate to draw some conclusions about what effects the tradition is likely to have on intellectual processes in the discipline. We are then in a position where we can see some aspects of 'the geographical mind' in relation to the 'hidden infrastructure' from question 1. This is discussed in chapter 8, 'Discussion and analysis,' which will also include a (tentative and incomplete) analysis of the geographical discipline by means of the ABC-domains.

As mentioned, I seek to reach a better understanding of the roles of our hidden mental structures by providing a concrete context from which to think about the theory. This is part of a general wish for concreteness, and to somehow 'pull the theory down to earth.' The third issue I want to address should be seen on the background of this task:

Question 3: What does the theory concerning mental infrastructure and the intellectual constitution of geography look like when dealing with a concrete context?
Through a focus on the Department of Geography at NTNU, Trondheim (hereafter: the Department) we are provided with such a suitable context. The empirical description will address or clarify our two other research questions in different ways, but this is not its only rationale. It has also been my hope that this text can be useful for the Department in one way or another, for instance when teaching new students, through the application of my analytical instruments, or as a starting point for further discussions on the Department's history and its future.

With these reasons in mind I formulate two further, more concrete, research questions. The first one is: (Question 3a) *what characterises geography in Trondheim, and to what extent can the broad tendencies found when examining the discipline's history be related to the way of doing geography at the Department?* By examining this (see chapter 6, 'Geography in Trondheim') I hope both to test some theoretical statements I formulate concerning the discipline in chapter 5, and to provide part of the context which will clarify the roles of our mental infrastructure. Various written sources constitute the material which is examined to address this issue, see next chapter, 'Methods and research.'

The second sub-question that we need to examine is: (Question 3b) *what are the cultural and institutional characteristics of the Department?* By providing some answers to this (see chapter 7, 'The Department of Geography in Trondheim as a social system') we should reach a better understanding of how general tendencies can unfold according to its own unique context. As I will go into in the next chapter, I seek answers to this sub-question by interviewing employees at the Department.

To reach an integrated analysis and to treat the issues of mental infrastructure, geographical knowledge and social context under one conceptual umbrella, I (somewhat tentatively) deal with the Department in terms of forming a complex adaptive system. This is done as part of the discussion in chapter 8.

It should be clear that the 'answers' to these questions must come in the form of discussion, suggestions and new questions rather than clearly formulated statements. I do not claim to have achieved a complete and flawless account of the mind or the discipline, only that something useful can be learned from the route I have taken.

General approach

The description of the Department represents the empirical focus of this dissertation. However, it is important to stress that the text as a whole is meant as mainly a *theoretical* contribution. The reasons for the focus on theory are explained in the next chapter, 'Methods and research,' but basically it is a result of the difficulty of designing research that would specifically target hidden knowledge structures, together with my own inclination towards the theoretical.

Perhaps fittingly this dissertation is in itself interdisciplinary. Interdisciplinary projects have their problems. One of these is the problem of achieving conceptual coherence and consistency. But this wish for coherence is, I think, at times exaggerated. Sometimes we need to acknowledge that two perspectives can be valuable even if they cannot neatly fit into the same structure of knowledge. We simply need to make clear what conceptual models we use at which time, and also be clear on the exact limitations of those models. It is when we manage this that the true complementarity of perspectives emerges. I will write more about this – particularly about the complementarity of social constructivism and naturalism – in chapter 4. This dissertation can be seen as an attempt to reconcile naturalistic approaches to the human mind with 'softer' approaches from the social sciences (a quest I fortunately am not alone in exploring).

Let me briefly run through the chapters to come.

After this introduction I will present, in chapter 2, the methods and research of this project. Chapter 3 will examine interdisciplinarity, its different aspects and its conditions of existence in the university system. This chapter ends in an identification of the focus of this dissertation, the elusive cognitive structures mentioned above.

Chapter 4 dives into this rather complex subject with the help of the cognitive sciences. If we are to speak about knowledge, we should use state-of-the-art models of the mind, and much has happened in cognitive science in the last years. This chapter presents a model for relating the microstructures of the mind to macrostructures at the cultural level, a model which originate partly in cognitive anthropology. Although complex enough, this chapter presents (in relation to its subject) a rather simple account of the human mind, but an account that it is hoped will prove useful, if not complete.

In order to say something about the intellectual constitution of geographers I write an intellectual history of the discipline in broad strokes in chapter 5. In chapter 6 I characterise geography as practised at the Department with particular focus on central features identified

in chapter 5. Chapter 7 establishes some of the social and institutional context under which the geographers in Trondheim work, in an effort to draw the text 'down to earth.'

The discussion that makes up chapter 8 must necessarily be tentative rather than conclusive, but hopefully we have by its conclusion come closer to understanding the conditions under which disciplines, interdisciplinarity and geography work. To relate the different parts of the text to each other, in the final analysis I treat the Department of Geography at the level of knowledge (understood widely) as forming a *complex adaptive system*. Chapter 9 summarises the results of these efforts in relation to the questions asked in this introduction.

Chapter 2: Methods and research

The present chapter will discuss the methodological approach applied in this thesis. This is generally a matter closely dependent on the theoretical approach used, since methods are chosen according to what the study seeks to find answers to. This thesis, however, treats these issues a bit differently. It has a main emphasis on theory, which means that the relation between theory and the empirical is less intimately connected than is usual in theses of this kind. To clarify how this came about, I will first describe how the topic emerged. After some reflections concerning the types of methodological challenges this topic presents, I will go on to describe the role that the empirical plays in the context of this text. It is against this background the methods used to study the Department of Geography should be understood.

At the outset of my work, I wanted to understand an issue that emerges out of an anecdote in Bohm and Peat's book *Science, order and creativity* (1989). The anecdote concerns the disagreement between two major figures in science, Albert Einstein and Niels Bohr. These two men had initially cooperated and had been on friendly terms, but the disagreements between them eventually caused the friendship to end. It was the nature of these disagreements as analysed by Bohm & Peat that interested me. The problem arose in their informal discussions about how to interpret quantum physics. To Bohm & Peat the underlying reason was that they had 'conflicting notions about the nature of truth and reality and about what is an acceptable type of scientific theory' (85). These fundamental issues were never made satisfyingly explicit; they were implied in their use of informal language. Somehow their different and 'tacit infrastructures of scientific ideas' (20) led them to talk past each other. I became very interested in the role of such tacit knowledge in science. If this can happen within the same discipline by two of that discipline's greatest scientists, I thought, what unspoken fundamentals would not be ghosting around in ordinary *interdisciplinary* endeavours? What can be said about tacit knowledge as an underlying reason for problems of interdisciplinary communication?

Reflections around such questions led to questions about geography, which in itself can be seen as an 'interdisciplinary discipline.' How do subliminal knowledge structures affect thinking, cooperation and research in geography? What are the implications of the notion of 'interdisciplinarity' *within a discipline*? How does it differ from 'proper' interdisciplinarity?

From such questions the study unfolded into an exploratory project on the role of tacit mental infrastructure in science with special regard to geography, using the Department of Geography at NTNU in Trondheim as an empirical case, but with a main emphasis on theory.

The choice of methodology

Studying knowledge

The major problem in wanting to study a 'tacit infrastructure of ideas' is that we can have no direct access to the things that happen in people's minds. Furthermore, it is problematic merely to ask people directly about how they think, because people do not themselves have complete insight into how this happens. When we ask people such questions we might get answers, but what we generally get are common phrases and assumptions about how they *think* they think, which is not what we want (Bloch 1998a).

Many would argue that the most adequate methods for studying such things are found in psychology and the cognitive sciences. I do not disagree with this. Psychological tests, for instance, can give us insight into reaches of the mind that are ordinarily tacit and hidden. However, I did not have the knowledge and skill to design and perform such a research project myself, and I did not have ready at hand a psychologist who did. Nor did I have the time such an ambitious project would probably require.

But are there other roads to insight into tacit knowledge? Well, introspection should not be rejected altogether. It is not necessarily so that tacit knowledge must remain tacit. Careful introspection may give valuable input. The fieldworker, according to Bloch, can after protracted and intensive participant observation gain insight into how other people think by adopting their way of living and being in the world. This is only possible, of course, after a certain level of familiarity is reached, where the researcher lives among his or her informants and through participation in daily activities can measure how he or she performs in relation to the locals on different (social or practical) tasks (Bloch 1998b).

My own situation as a student at NTNU and at the Department of Geography can be seen as parallel to this. I have been studying geography for several years, and through this tried to acquire the skills it takes to 'be a geographer' (although I have from time to time had ambiguous feelings on this point). Many geographers are also part of my social group. Although I have not been working systematically as a fieldworker (doing meta-studies on my studies) all of my time at the Department, at least some (positive or negative) value should be attributed to these things. The reflections I have on geography and the Department spring not only from my systematic research, but also from my 'participant observation' as a student.

Like any student who starts a new subject I, from a partially outside view, evaluated and reflected on the discipline as I was studying. This was an ongoing process. I remember writing a letter (which I never sent) to a member of the Department where I discussed the discipline trying to decide what the merits of geography were, and whether I should continue my studies at the master's level or not. This thesis could be seen as an extension of my reflections in that letter.

If I concede that introspection can give me as a researcher valuable insights, it must of course also be conceded that my informants at the department could have provided me with such insights. A possible choice of methodology could thus have been to engage a few informants in very intensive and deep-delving interviews. This path was not systematically and consciously followed, although Michael Jones, my advisor, can be said to have had a similar role (see later in this chapter for more on my advisor's role).

The role of the empirical in a largely theoretical thesis

It was relatively early decided that this thesis should have a main emphasis on the theoretical, both because this was where my interests lay, and because of the methodological challenges. The empirical chapters address two main questions which are identified in the introduction as question 3a and 3b.

Chapter 6, 'Geography in Trondheim,' addresses question 3a which is *what characterises geography in Trondheim, and to what extent can the broad tendencies found when examining the discipline's history be related to the way of doing geography at the Department?* It is mainly based on written documents, and takes the form of a characterisation of the approach to geography found at the Department through writing its history, and exploring the relations to key issues in chapter 5, namely usefulness, synthesis and visual analysis. Thus we can identify some sub-questions: to what extent is geography in Trondheim an applied affair? How is the notion of geography as a synthetic discipline viewed? To what extent are visual analysis and the use of visual aids prominent among Trondheim geographers?

The relation between chapter 5 and 6 also means that it is not always very easy to differentiate between theory and the empirical. The transition seems almost seamless at times, because on the one hand, the activities of the Department have formed part of the history of geography. On the other hand these same activities are sought examined as an empirical example of a geography department.

Chapter 7, 'The Department of Geography as a social system,' seeks answers to question 3b, which is *what are the cultural and institutional characteristics of the Department?* It is mainly based on interviews and observation. By establishing the social, institutional and cultural context that the group of geographers work within it also seeks to clarify the roles of our hidden mental infrastructure (as part of question 1). It was conceived more as a way of 'grounding' the theory, rather than as a way of addressing the theoretical issues on cognitive structures or geographical knowledge. The idea is in other words that by describing the context in which something happens we get a clearer (or at least more realistic) view of the theory that we assume applies at some level in that context, even though we are not able to study things on that level directly (in this case the mind).

It is against this background that the methodological choices of this thesis should be evaluated. These choices are discussed in more detail below.

Qualitative methods

The researcher's role in gathering data in a qualitative study is fundamental for what kind of material that is produced. The process of reflection, interpretation and analysis is not confined to a distinct phase at the end of the study (even though it is more intensive at the end) but goes on throughout the study, informing choices made, which questions to ask etc. A characteristic of the qualitative research process is thus a flexible research design. This means that the researcher may work with the different parts of the research process in parallel, each part having a mutual influence on every other part. Problem formulation, data collection and analysis/interpretation coevolve during the process, and the researcher's interests and understanding will thus be highly integrated into the data itself (Thagaard 2003).

Qualitative research is in principle based on subject-subject relations. This means that both the researcher and the informant influence the research process. It is important that the researcher reflects on how he affects and is affected by the informants throughout the research. It is also important that these issues are presented to the reader. Openness around these things enables the reader to a larger extent to evaluate the quality of the material that the text is based on (Thagaard 2003).

Interviews: possibilities and limitations

Interviews are suited for obtaining information about how the informant understands him- or herself in his or her surroundings. Descriptions of previous events form narratives which are

partly formed by the situation they are told in. Further, they are a manifestation of the speaker's personal point of view (Thagaard 2003).

The qualitative interview can be more or less structured. The most open form takes place like an informal conversation, where the main themes are not decided beforehand (for example in a field work setting). The most structured kind has a fixed set of questions which are put in a set order. The strength of the latter is that it enables comparability between informants to a larger extent. The former enables the researcher to expand upon subjects that the interviewee brings up to a larger extent. A middle ground is the semi-structured approach where the themes are decided beforehand, but the order of their discussion is decided on the way (Thagaard 2003).

In this research, interviews were relatively little structured. Often I had a set of themes written down, but let the conversation flow, and if new interesting themes emerged, I encouraged the informant to expand upon it. Thus I gained a loose core of themes which were discussed by most informants, and a host of other themes circling around the core.

In order to characterise the social system, it might be argued that observation would be a better choice. Observation is well suited to study the actions of people and how they relate to each other in social situations (Thagaard 2003). Thus I might have gained an impression of how more spontaneous interaction took place. Interviews are more likely to hide certain aspects that are sensitive in some way. However, it has not been an emphasis in this thesis to map the details of social interaction, but rather to establish some broad aspects of it. Although I sometimes felt that I was getting a somewhat idealised narrative of the Department, a more balanced impression emerged by interviewing several people with varying degrees of involvement with the Department.

Nevertheless, there are also elements of observation present in this research. As a student one gets to observe some things, but mostly they have to do with how the Department present themselves to students, and little of the internal working of the Department. I participated as observer at a Working Staff Meeting, which was interesting. This certainly let me observe some of the ordinary social interaction, even though it took place in a relatively formal context. The limited use I can make of the observations made in this respect makes me content merely to note that they do not contradict, but rather confirm, what I have learned from interviews.

Choosing interviewees

During the summer and autumn of 2002 I interviewed the following members of staff: Nina Birkeland, Axel Bauduoin, Geir Vatne, Bodil Wold, Nina Gunnerud Berg, Asbjørn Aase, Michael Jones, Anne Sofie Lægran, Ragnhild Lund, Elin Silnes, Cathrine Brun and Britt Dale. Then in 2005 I decided to follow up some further threads. Anders Löfgren was interviewed in April 2005, and a supplementary interview with Aase was undertaken in November 2005. A short interview with Olav Fjær was made in December. Since Michael Jones was my supervisor, I had talks with him regularly during my work with this dissertation. This is the 'core' of my material. In addition to these I had informal conversations with students and others who were affiliated in some way with the Department. This sometimes could give other perspectives on the material. Before the specific problem formulation pursued here was formulated, I also did some 'pilot interviews.' These were exploratory conversations I had with people who had something to do with interdisciplinarity, geography or both, and the purpose was to identify themes I could explore. These people were Knut H. Sørensen, Per Morten Schiefloe, Sverre Konrad Nilsen and Margit Hermundsgård, all of whom have experience and/or interests in interdisciplinarity as researchers (in all cases) and administrators (in the cases of Sørensen and Schiefloe).

Choosing informants in a qualitative study is generally a matter of choosing strategically. We choose informants according to qualifications that are strategic in relation to the research problem (Thagaard 2003). Often it is difficult to get people to participate in qualitative research, but this was not a problem in this case. My supervisor had informed the staff of my research project, which gave everyone an idea about what I was doing, before I started doing interviews. Everyone I asked agreed to meet me.

As we have seen, the research problem concerned describing the cultural and institutional characteristics of the Department. Accordingly, to get a balanced view, I wanted to interview people who were in different career phases. I also found it important to have 'minorities,' like physical geographers and GIS specialists, represented. To this extent, then, informants were chosen in order to get as many different points of view as possible; this could be characterised as 'quota sampling' (Berg 2001). I sometimes also used the 'snowball method' of asking people I interviewed about who they thought could be interesting informants for me. It was also interesting to interview the senior members who had been at the Department from the beginning, like Asbjørn Aase, Michael Jones and Britt Dale.

Supervisor and informant: the role of my advisor in this study

It is important that the role of my supervisor is discussed. He participated in this research both as an interviewee and by giving comments on what I wrote about the department (as well as every other part of the thesis). In addition he has written or edited several texts on which I have based parts of my work (Jones 1986; Jones & Cramer 1992; Jones & Langdalen 1992; Jones 1999; Jones 2001a; Jones 2001b).

Since Jones has been a member of the Department since the beginning, he would have been an important informant even if he were not my supervisor. His interests are furthermore partly in history. He has written about the history of the Department earlier (Aase & Jones 1986), and plan to revisit it (also with Aase) in a planned future publication in connection with the Department's 30th anniversary. He has also written reflectively on geography as a discipline (Jones 2001a; Jones 2001b). In most respects all of this is to the dissertation's advantage. It is expected that the supervisor should have expertise in the field of the dissertation. However, it is important to emphasise that his views of the Department's history may not correspond with that of others. Although he has been employed from the beginning, he has been working other places in periods. And clearly his perspective constitutes a certain standpoint which is coloured by his interests etc. Accordingly his role should be discussed concerning the representation of the Department.

As seen above, a goal was to give a *balanced account* of the Department as a social system. The influence that my supervisor has (and should have) on me as a student is potentially in conflict with this goal, since it might mean that I give one person's perspective much more influence on it than other perspectives. Although Jones has expressed his opinions to me, he has not in any way forced them on me. In general, the degree of freedom that I have enjoyed while writing this dissertation has been very large. I thus feel that the main issue is whether or not I as a researcher have been able to come to independent conclusions on how to present the Department. The ones who are truly able to evaluate how I have succeeded in this, are the rest of the Department staff. Hopefully they will give me their comments and critiques on this. Seen in this perspective, at least, the situation is to my advantage, since I am given a unique opportunity to learn from the unusual situation the topic of this dissertation has put me in.

The interviews in context

The research was initiated by doing some pilot interviews with both geographers and non-geographers in different positions, as mentioned above. This was done in order to get some

input with regard to my thoughts about interdisciplinarity and geographers. I also wanted to find possible threads I could follow up in the main research.

During all interviews I took notes, and most of the core material was in addition recorded on a mini-tape recorder. Directly after each interview I sat down to write a summary of the conversation we had just had, while referring to my notes and the recording. The recording was useful when I needed exact quotes, and it made it possible to relax a bit on taking notes, giving more of my attention to the informant. It was also very valuable because it helped me evaluate my performance as an interviewer. It is possible that the tape recorder made the atmosphere in the interview more formal, but I did not feel this encumbered us much. The themes were not usually very sensitive, and my informants, being researchers themselves, understood the need for it.

The core material consists of interviews with Department employees. Many of my interviewees had thus previously been my lecturers, and all of them were higher up on the academic 'career ladder.' There was therefore a social and hierarchical asymmetry in my relation to them. Mainly I think this was a problem of my own mentality, and not primarily something that bothered the people I was interviewing. I found that my desire to be perceived as a competent scientist and researcher disturbed me somewhat. Because my interviews were semi-structured, and often had the character of a conversation, this situation was harder to control than it would have been if I had had the possibility to use structured interviews or questionnaires. As it was, I found myself being afraid to ask 'stupid questions' and involuntarily wondering whether the interviewees approved of my research programme, etc. Having such a reluctant attitude is problematic since it is likely to distract the interviewer and alter the way he or she asks the questions. It makes him focus on himself rather than the interviewee. At least on a couple of occasions I am aware of having, by formulating myself in a certain manner, suggested answers to questions I put when I probably should have left the question open. Such leading questions are a common error by interviewers. The effect of such mistakes can be modified by having a reflecting and conscious attitude to the resulting material in this respect.

Most of the interviews were performed in the interviewee's office. This had the advantage of reducing the discomfort and time-loss for each person I interviewed, and presumably gave me more goodwill. On the other hand it was also a source of distraction because of telephones ringing or people knocking on the door to speak with the person in question. Also, meeting on 'their territory' probably increased the sense of hierarchy discussed above.

Interviewing professional researchers has other sides to it as well. For instance, I profited from their analytic skills. Asking questions about the department as a social system, for instance, often yielded answers that were highly structured and reflexive in nature. One might perhaps object that I should not let my informants dictate my analysis. But, first, I did not let any one person influence me without comparing that person's account with what other people said. And, second, I was conscious about these things and treated such ready-made analyses with some care.

All my interviewees knew I would submit the thesis at their department. Accordingly I got quite diplomatic answers to questions about such topics as disagreements, and even sometimes legitimate academic ones. Sometimes I felt that they mistook me for a journalist who would quote them on the front page in bold type. This caution on their part is understandable. It is a small environment, and nobody wants to step on anyone else's toes. I don't think it hampered me much in any case. Sensitive subjects were often only peripheral to my interests, and by asking several persons about the same things I got a picture that I believe to be reasonably balanced. Also, I occasionally had informal talks with people who were only loosely tied to the Department, and who were freer with their opinions as partial outsiders.

Technical issues

Most interviews were recorded on a mini-tape recorder. My equipment had a very low quality, producing noisy and poor sound quality. This was not such a problem directly after the interviews with the conversation fresh in my memory, but when I wanted to listen to them again much later, the noise made it hard to understand what was being said. It was partly solved by transferring the recordings to my computer where I was able to digitally remove some of the noise. For some of the poorest recordings even this did not help very much, and it required much effort to figure out what was being said.

I applied the computer also in the analysis of the material. The qualitative data analysis software HyperResearch lets the researcher code passages of text (as well as parts of sound or video recordings) according to the theme that passage concerns. In this manner I systematically coded all of my interview summaries. This very process helped me notice things about the material, and afterwards I could easily read what every person had said about a given subject by having HyperResearch construct exactly the report I needed.

The use of computer software in qualitative data analysis enables the researcher to gain an overview, and can be particularly useful in studies with large amounts of data. It is important that the use of such technology does not lead the researcher into thinking in terms

of quantitative research (Thagaard 2003). In my case the software helped me gain an overview. The coding process forced me to examine the texts closely, looking for themes and deciding whether they deserved a new code, or could be subsumed under an existing one (or both). Afterwards it mainly served as an aid for memory. When I wanted to write about a given theme, I could remind myself about what each person said about that theme by quickly extracting a report on this (which means that I choose the specific code(s) that I want the program to present, and thus get all relevant pieces of text gathered in one report).

Ethical issues

The importance of the relation between researcher and informant in qualitative research means that the informant can potentially be affected negatively by the researcher. Ethical guidelines are therefore necessary (Thagaard 2003).

Particularly since the 1990s the relationship between the researcher and informant has been in focus within the social sciences. This has been a necessary process of reflection around both knowledge production and ethics. Much of the focus has been on the asymmetry of power between the two parts. In my case there is an interesting double situation. On the one hand the Department and its members have power to define and evaluate my research as a student. On the other, I have the power to 'define' and describe the Department in this dissertation. Of course, I do not feel like a particularly powerful being. All formal power lies elsewhere, and my concern is with giving a scientific presentation, not with affecting anyone's agenda. However, as a possible starting point for the self reflection that is an ongoing process for most geographers, this dissertation may be able to have some influence.

More important here is the possibility of harming my informants by disclosing information given to me in confidence. Thus I could for instance start or intensify conflicts which could be harmful.

When I did the interviews, each interviewee was offered anonymity if they felt it was needed. I offered them an opportunity to decide on this matter later by showing them which citations I would use. This would also avoid their identity being inadvertently revealed through my citations. I also felt this procedure was necessary in order to gain trust and avoid that my interviewees became overly cautious. While anonymity might be commendable in general terms, it is clear that such anonymity would not be an advantage in all matters.

In using academic publications such anonymity would of course be meaningless. Such material is already public, and I would be required to give full references in the normal

manner. Neither would anonymity be required when academic views are extracted from interviews, since such matters are openly discussed by the Department anyway.

A potential problem in this respect could rather be if I misrepresent them in this text. A way of avoiding this, of course, would be to let each person see and comment on these bits of text. If the purpose of the interviews were to extract tacit knowledge, this could be a methodological problem, however. It could be argued that in giving the interviewees full control in this way one might be missing the 'Freudianisms' as it were, i.e. when tacit infrastructures of the mind show themselves through spontaneous patterns of speaking. Against this could be argued that many extraneous matters also could be hypothesised as affecting these patterns, for instance various issues to do with my presence as an interviewer. Would they have expressed themselves differently if I was a respected colleague rather than a student, for instance? In any event, the primary purpose of the interviews was *not* to extract such tacit knowledge, and accordingly it was not very difficult to make a decision. People would be given the opportunity to comment on and, if necessary, change how their academic views were presented here.

It is regarding characterisations of the social system that anonymity could be necessary. A separation therefore needs to be made between these types of subject matter. Sometimes this separation is relatively straightforward, other times not. One thing is that the social environment is clearly important for how academic discourse unfolds, but it is also so that academic subjects are not entirely separable from the person in question. To illustrate, a show of disrespect for a certain view would often be perceived as insulting for the person with that view (which it could very well be intended to be).

The informant's participation and comments should thus eliminate most of the potential ethical problems with the dissertation. Partly because the empirical plays a relatively limited role in this dissertation, I have found that the ethical questions should be given a high priority.

When the first draft of the chapter was finished, I thus sent by e-mail the relevant pieces of text that contained direct or indirect citations from interviews to the each person, requesting his or her comments. In a few cases my interpretations were challenged and additional explanation and modifications were added accordingly.

I also sent e-mails to the persons who were not cited, but whose names are mentioned in this chapter to inform them of this.

Document analysis

The chapter 'Geography in Trondheim' seeks, through document analysis, to characterise the intellectual profile of the Department of Geography. The chapter should be seen in connection with the chapter on the intellectual history of geography (chapter 5), which tries do to the same looking at geography more generally. Here, the themes of applied science, synthesis and visualisation was identified, and figure as sub-themes in both chapters.

Documents can be divided into different types according to their properties. They can be normative (like laws) or descriptive (like chronicles). Each normative or descriptive text can further be divided according to its orientation in time, i.e. whether it speaks about the future or past. A particular group of texts are called performative. These texts function as an action in that they make something happen, carry something into effect, like for instance when a particular new teaching post is approved in the Department council.
In practice each text can contain different elements; it can in principle be both normative, descriptive, oriented towards the future or to history.

All texts can be treated as a remnant, that is, it can say something about the time it was created in. Some texts also explicitly say something about the past, and can by the historian be used as literature. That is, a descriptive and historically oriented text can be used as literature since it says something about a given past. The same text can, however, also be used as a remnant, saying something about the world it was written in. If we use a text as literature about the past, we have to be aware that this text may contain errors, misinterpretations and bias on the part of the writer, whereas when we use it as a remnant, we are only subject to our own such faults.

When working with documents it is important to keep in mind certain key issues, in order not to distort the information extracted from them. Who wrote the document, and to whom? What was the original purpose and motivation for writing it? The answers to such questions allow us to keep a reflective distance to their content, and establish more precisely what kinds of conclusions may be drawn from it (Kjeldstadli 1999).

Documents used

Different types of written documents were used. In addition to publications and lists of publications by department staff, these included strategy plans, department annual reports, faculty annual reports, and archived documents from department council meetings.

The documents used in this dissertation can broadly be divided into two groups, public and internal. The internal documents are the ones from department council meetings, while

the public ones are virtually everything else. Among the public documents an important dividing line goes between academic and strategic texts. The academic texts are directed towards a (mainly) academic audience, while strategic texts are texts that are partly for internal reference and partly written for a broader university context, like annual reports and strategy plans. Faculty annual reports belong to the same category as annual reports and strategy plans, but are not written by the Department.

Strategic texts are partly about giving a representation of the Department to external parties, like the faculty and university administration. As such, one should be aware of a possible bias. The Department will in such texts be likely to emphasis certain features, while others are underplayed. It is for instance likely that one's own existence and possible increases in funds etc. will be sought justified by presenting something which fits with the broad policy of the university. Also the emphasis will more likely lie on the ways in which the Department is a unified department, rather than for instance analyses of the differences between different sub-disciplines.

The strategic texts are not necessarily motivated by the wishes of the Department itself, but can be regarded rather as an obligation to the faculty. This means that the quality and precision of the information given might suffer, as other tasks (like teaching and research) are seen as more important in a busy schedule.

Strategic texts are products of the Department council. They are not personal products as such, although one person might write the first draft which is later modified after suggestions from the council. This means that possible personal bias will be partly eliminated. In the case of the Department annual reports, I have used primarily the information on research projects found there. In this case each researcher reports about his or her project to the editor(s), who gather it together with other elements required to go into such a report.

The two academic publications which were published to celebrate respectively the Department's tenth anniversary (Jones 1986) and Asbjørn Aase's 60th birthday (Jones & Cramer 1992) can be seen in a similar self-representation context (in the former case a representation of the Department, in the latter of a prominent department member). In the ten year anniversary publication, the emphasis is explicitly on the connections between the different fields, for instance, although in this case the emphasis amounts to a conscious editorial line rather than an implicit bias.

The self-representation aspect of the strategic documents and the academic documents mentioned above shows the need to balance the picture by using different sources created for different purposes. This will help strengthen my conclusions about certain matters, for

instance about cohesion and academic integration at the Department. The use of interviews, other academic publications as well as a systematic analysis of the projects reported in the Department annual reports tries to tackle such issues.

From these sources it is necessary to make a selection. Out of many potential sources, which ones should be employed? The answer should be related to what questions we ask. It is also a matter of being realistic concerning the amount of material that can reasonably be used within the given time frame. Although the best way of mapping the intellectual composition of the department probably would be to review all of its publications and written records, this was not feasible.

In the Department's annual reports all research projects are listed with a short description. This gave me the opportunity to examine relatively quickly the interests present at the Department over several years. I could interpret the project's goals and designs and relate this to the themes I was interested in, mainly interdisciplinarity and applicability.

The Department's own descriptions of its disciplinary makeup have also been valuable. Two sources of this kind have been particularly important. Aase & Jones (1986) is a self-representation on the Department's 10 years anniversary. It describes the philosophy and approach of the early years, the staff recruitment and disciplinary composition at the time. The first strategy plan was written at the department in 1995, and presents a quite thorough analysis of the disciplinary composition at the time, as well as describing future strategies. Later strategy plans have also been used, but these were less extensive and the description of the Department's composition contained fewer categories (on request from the faculty).

Michael Jones gave me access to minutes and documents from Department council meetings which he had archived. The documents were from the periods 1987-1991 and 1992-1993, and gave me insight into some processes behind the running of a department, details about who were employed when etc. Although some of these documents were performative, many were also drafts and propositions, and keeping this difference in mind was important in the analysis.

In order to evaluate the use of visual aids I chose to review a sample of the Department's written production. I chose here to examine the New Series A, which is the name of a series of publications comprised of reports and presentations by the Department from 1995 – 2000. This choice was made partly on pragmatic grounds. I was given access to the Department's storeroom, and since almost all texts in the *New Series A* were available there, I chose these. I was very aware that these relatively few texts (I examined 45) were not

large enough to draw any very clear conclusions, but see it as an indication which will be discussed in the light of other findings and theory.

Chapter 3: Interdisciplinarity

The word interdisciplinarity (Norwegian 'tverrfaglighet') is a major 'buzz-word' in the academic world of today. It is a symbol that, like all symbols, can attain many meanings and varying emotional content. The term indicates a concern with a group of issues that arise from the division of modern science into disciplines. It is in part a matter of critique linked to a wish for a broader overview and a holistic perspective in an academic landscape characterised in large part by specialisation and fragmentation. It is also, however, a concern with applicability, problem orientation and a wish to address broader societal or nature-related problems (Dahl & Sørensen 1997).

Emotionally interdisciplinarity can be both positively and negatively charged. To some it symbolises holism and the will to engage broadly, while for others it signals a potentially harmful naivety concerning the possibility of operating within several disciplines and yet keeping up scientific standards (Dahl & Sørensen 1997).

However, interdisciplinarity is of course more than merely a word with varying significance and connotations. It is a real development in academia which has potentially fundamental significance for knowledge production at universities (Weingart & Stehr 2000b). It is also more than the expressed wish for a reform of knowledge or a mere pragmatic necessity related to applied science. There are several cases of research-induced interdisciplinary endeavours, for instance within the cognitive sciences (de Mey 2000) and within complexity theory (Waldrop 1993).

There is reason to call for soberness when dealing with interdisciplinarity: 'While the radical voices in favour of interdisciplinarity may imagine the world of knowledge production without any organizing structures, the more sober view sees interdisciplinarity always as a limited project in relation to a limited number of disciplines in a limited time frame' (Weingart & Stehr 2000a, 272). Knut H. Sørensen (1997) warns against the unrealistic belief that a single individual might successfully incorporate several disciplines, and instead calls for a new type of academic that he calls the 'polyvalent specialist.' This is a person who is a specialist within a relatively narrow field, but has learned how to co-operate with people from other fields.

Other words – like multidisciplinarity, crossdisciplinarity, transdisciplinarity – are also used to denote different but related orientations towards roughly the same issues.

The University: generality versus speciality

The university is one of the oldest institutions in modern society. Despite often being perceived as tradition-bound and unchanging, there have been large alterations in how knowledge is actually produced. For Dahl and Sørensen (1997), the key to understanding these changes lies in understanding the field of tension between, on the one hand, broad overview and holism, and, on the other hand, specialised knowledge. The first they associate with the notion of *Bildung* or *dannelse*, i.e. a broad-based education that formed part of the general development of a student (Moran 2002); the second they identify as the core of modern science. The different ways of dealing with this tension have formed much of the history of the university.

For instance, what are today called the professions are one of the early compromises that emerged out of this tension. Within professions like law, medicine and theology a certain academic background was required, both because it represented a symbolic mark of being a professional and because it formed part of the broader basis of their practice. At the same time the professional had to have specific professional skills required of him, which distinguished the different professions.

The disciplines of today have evolved as products of particular historical processes and are of varying ages (despite the impression one may get of everlastingness). Several of today's disciplines are quite new, while some reach back to the renaissance or even antiquity. They partially reflect the cultural conditions present when they emerged and partially their unique histories within the framework of the university (Dahl & Sørensen 1997).

The evolution of disciplined knowledge production is according to Dahl and Sørensen primarily a product of the history of the natural sciences. The gradual incorporation of the natural sciences (which originally largely evolved outside of the universities) into the university has led to a shift from aesthetic and ethical reflection to the search for scientific facts and laws. This is the case for traditional disciplines like the humanities and medicine, but also, and perhaps even more, for the social sciences, which developed later than many of the humanistic disciplines. Physics is traditionally the discipline that has the highest status within the university, and has served partly as an ideal for the other disciplines. This is probably a result of the ability to produce successfully universalising knowledge and to formulate law-like principles in mathematical form (Dahl & Sørensen 1997).

Technology and engineering developed in their own institutional contexts during the nineteenth century, and thus have a different approach to knowledge and specialism versus

generality than is the case otherwise in the university. However the separating lines between engineering and the university have not in any way been absolute, and the current development is one of integration (Dahl & Sørensen 1997). This is for instance manifest in the construction of NTNU, which regarded institutions like the MIT in the United States as an ideal.

Disciplines

Having status as a discipline involves possessing institutional independence, by which is meant that the study area must have its own journals, conferences, and scientific organisations, in addition to the crucial condition of being organised in its own independent departments. Decisive is the authority to set its own criteria of quality, the right to certify its own students, and to establish its own collegial evaluation commissions (Dahl & Sørensen 1997).

Departments at today's universities have relatively recently evolved from being the organisational surroundings of a single professorial chair into being a seat for a discipline that can involve several professors and other scientific employees. During the last hundred years the numbers of departments have grown steadily and rapidly (Dahl & Sørensen 1997).

New disciplines emerge for many different reasons. It may happen as a reaction to theoretical conservativism in existing disciplines, lack of acceptance for new methods of research, or rigidity with regards to the content of what should be taught to students. It can also be that the wider societal context has changed, or the area itself might change (Dahl & Sørensen 1997).

We can thus speak of both internal and external reasons for changes in disciplinary composition. New perspectives within a discipline, new relations of cooperation between researchers from established areas and new interests in the surroundings might create a pressure for change. Two main types of processes of change can be distinguished. When a new discipline is created by splitting off from an existing one we can speak of *specialisation*. The converse is *hybridisation*, which is when a new discipline forms out of the cooperation between two or more established disciplines (Dahl & Sørensen 1997).

These processes are full of conflicts on different levels, both academic and non-academic. Often new disciplines are heavily criticised by existing ones, and their defining perspectives sought undermined. But still there is room for many new perspectives and disciplines. This has led some to question how deep the chasms between disciplines go. For some, disciplinary boundaries are merely superficial lines that divide up a single unified

science. Others do not concur with this and emphasise how particular areas have special needs that fundamentally demarcate them (Dahl & Sørensen 1997).

Despite battle, conflict and conservativism, a discipline is generally respected once established. Thus a constant flow of additions characterises history, and diversity is actually relatively unproblematic, leaving much room for specialisation (Dahl & Sørensen 1997).

However, the separation between areas need not be unproblematic. Disciplines need demarcations that legitimate them as independent perspectives. The same area of study can be approached from many perspectives, and different disciplines may share many terms. This makes them appear to refer to each other, but the shared terms often have different meanings in different disciplines. It is not indifferent from which angle a phenomenon is approached (Dahl & Sørensen 1997).

Despite the strength such diversity of perspectives can or should lend to academia and society, it also leads to a certain sense of confusion. Experts within different disciplines often have problems communicating about their common field of expertise to people outside their particular discipline. Specialised knowledge is hard to convey to people outside the small circle of initiates (Dahl & Sørensen 1997).

Such problems arise from the fact that specialised knowledge is in a sense self-sufficient. A researcher can work an entire career within a narrow field of study, communicating with a relatively small number of people who share the same field. A field may borrow methods, terms or theories from other disciplines, but this is not to say that they therefore know the other disciplines in depth (Dahl & Sørensen 1997).

A crucial question for many is whether we should accept this situation of partial fragmentation as the only realistic one, or strive for more generalising and holistic forms of knowledge.

A central concern in interdisciplinary work is the pursuit of achieving the advantages of a broad perspective while retaining those of specialised, in-depth knowledge. The danger is that if such interdisciplinary efforts fail, we get a lowering of disciplinary standards and an inadequate broad representation.

The large resources allocated to modern science by society are partially based on a contractual relationship between science and society. Science must be useful, or the stream of resources will lessen. This problem orientation is perceived to be in a conflicting relationship with disciplinary knowledge, since such problems tend to require a broader perspective than is afforded by a single discipline. This is not wrong, but can be qualified by noting that such problems might in turn constitute a set of issues that in its own right might constitute a

specialised field of study. Breadth is thus a term which is defined and re-defined in relation to disciplinary boundaries and the appearance of new fields, and not something which is inherent to, for instance, problem-oriented studies (Dahl & Sørensen 1997).

Analysing interdisciplinarity

The description of academia and its disciplines given above should make it clear how many different themes that can be investigated and how many perspectives that can be applied to these themes. Interdisciplinarity can be understood by dividing it into different levels of analysis. The factors on each level will have their unique impact according to the broader context they are situated within. Here, I first identify different research orientations that will have an impact on how a given study or project will develop. Then I treat the institutional level, the socio-cultural and the individual level in order to give an overview of 'the interdisciplinary situation.' After this overview I will present the focus of this dissertation, namely the role of conceptual structures in science, interdisciplinarity and geography.

Research orientation

Interdisciplinarity can, as mentioned, be a matter of knowledge ideal, a concern of practical problem-orientation or a result of a particular research (i.e. research driven interdisciplinarity). The different motives and aims of the participants will be significant in shaping the work done and the results produced.

With regards to interdisciplinarity as a search for better knowledge, I have already noted the problems of believing in the 'interdisciplinary individual.' On the other hand it is not difficult to agree with the people who argue for a more holistic and synthesising approach to knowledge. There are indeed problematic aspects tied to the tendency to give analysis priority over synthesis, especially if this leads to fragmentation, as Bohm and Peat (1989) argue. In fields like ecology, fragmentation can give a most unsatisfactory type of knowledge and, if acted upon, catastrophic consequences. The complexity and thus the huge potential for unintended consequences must be taken into account.

However, though the search for a total synthesis might be too ambitious, less grand projects might be successful, and thus partly satisfy the demands for 'ideal knowledge' that some see necessary to fulfil.

Almost all interdisciplinary endeavours are problem-oriented. We can here separate between socio-economically driven and research-driven projects. Much, perhaps most, interdisciplinary work is done in the context of the former type. Typically such studies are

oriented towards solving a particular problem, often of a practical nature. They are driven, not mainly by research interests, but by the desire to solve commercial or societal problems. It is a clear tendency that applied science often will lead towards a need for crossing disciplinary boundaries. An important reason for this is that when the problems to be solved are defined *outside* the discipline, they are less likely to be solvable strictly within a disciplinary framework (Klein 1990). Geography has historically been oriented towards usefulness, and the extent to which geography in general and geography in Trondheim are concerned with applied science will be given a good deal of attention in this dissertation, particularly in chapters 5 and 6.

Sometimes interdisciplinary projects result from a research problem. Here different fields mutually benefit from each other's perspectives. Cognitive science is one such example. The development within this field of knowledge has been driven by philosophical discussion and theoretical advances within various disciplines. This type of interdisciplinary endeavour is related to the previous one in that it is problem-oriented; however the general challenge (in this case, understanding the human mind) is also a motivation in itself and the binding glue that keeps it all together in a way that is perhaps not as strongly present in applied problem-oriented studies (de Mey 2000).

I do not believe that these research orientations necessarily exclude each other, but can be present at the same time in the same project, depending on the different participants.

Institutional issues

In important senses it is true that interdisciplinarity exists in the 'white space' of organisational charts (Klein 2000). The university is organised to facilitate *disciplinarity*, not *inter*disciplinarity. Important in this respect are issues of funding and budgetary frameworks. For instance it can be a challenge for a geography department located within a social science faculty to argue for money to fund expensive research projects that would normally be situated within the natural sciences part of the university.

Interdisciplinary work will have a different character according to whether it is situated within a discipline (e.g. between sub-disciplines), in and between university departments, in interdisciplinary research institutions, in teaching programmes etc (see Weingart & Stehr 2000a for examples).

At the university the host of different strategic issues that are made actual by the struggle of each discipline to strengthen its own position in academia will be relevant. New disciplines have different needs from established ones, and might be fiercer when demarcating

their field. In the competition for resources one cannot expect the idealistic search for knowledge to rank higher than the more mundane issues each discipline struggle with.

Problem-driven motivation and reputation-driven regulation

Part of the institution of science is that the status of its practitioners is to a large extent regulated by reputation. Reputation is tied to factors such as the number of publications, how often and in what way these publications are used by other researchers, general visibility etc. Moreover, these factors belong to disciplines, not to interdisciplinarity. In other words the reputation system regulates status with regard to established categories, which are disciplinary (van Raan 2000).

Problem-orientation is a central feature of science. Some problems are narrowly defined by a discipline, and can thus be treated within it, while others demand broader approaches. It could be hypothesised (as van Raan 2000 does) that solving the latter type gives a higher status than the former, because this gives recognition beyond the discipline itself. While this might be true, it does not follow that such problems are more widely addressed. The reason for this is that they are more difficult to solve, and thus potentially more risky investments in the quest for reputation. For many it is thus more worthwhile to allocate one's time to solve problems within the discipline. This is also the case for the people who distribute resources. Should we fund this exiting but risky interdisciplinary project, or this more standard project that has a good chance of gaining results? Even when funders have an explicit goal of funding interdisciplinary research, the safer option is often favorised (Sperber 2002).

A variety of this problem can be seen in the typical academic career. It is considered risky for a young aspiring scientist to use a genuinely interdisciplinary approach, since such approaches are more difficult to follow through successfully, and are also often considered controversial. Thus young doctoral researchers are often discouraged from doing such projects rather than the opposite. This is not only a disadvantage for science in general; it is also a problem for young scientists who are forced to stick to other issues than the ones that really stimulate them (Sperber 2002).

Social and cultural issues

Although separated in this presentation, institutional issues cannot, I would argue, be separated from social issues, informal institutions and culture. Here the factors are innumerable, and common to any other field of knowledge production.

Hierarchy is one social factor that invariably will be present in any stable social environment, but it can be more or less pronounced according to the specific history and culture of an institution. A hierarchy can be tied to more or less arbitrary markers. Science might to a large extent be reputation-regulated (van Raan 2000), but it is likely that other factors may come into play. Age and experience are obvious aspects. Political power and alliances may clearly have an influence. A certain amount of bullying is often also present, and might subjugate some to the advantage of others. In other words, any process of social regulation may be present, from friendship and reciprocity to bullying and pecking order. People who want to improve their own career may manipulate any of these aspects, sometimes at the expense of the quality of knowledge production.

It might be argued that projects that are relatively new or weakly institutionalised will sometimes be in a better position to take advantage of the knowledge of each participant than established interdisciplinary institutions because a consolidated hierarchy has not yet developed. When such a hierarchy exists, some perspectives will automatically be more or less prominent according to the social status of the person possessing this perspective. However, this effect can be moderated by cultivating an egalitarian attitude among those participating. While such cultures of acceptance can be consciously sought, it is probably more effective if they develop spontaneously over time.

Individual and psychological issues

Embedded in the institutional, social and cultural context we have all sorts of factors that can be described as part of the individual, or psychological if you will.

The interaction between personality variables can have dramatic effects on any system of knowledge production. In interdisciplinary work it is important that a participant has a sufficiently strong personal and professional identity to handle situations where fundamental differences in perspective create difficulties that might challenge or leave unaccepted the value of his or her perspective. On the other hand such strong identities might cause problems as well, at least to the extent that these impede sufficient openness towards other perspectives and angles. Against this background it has been suggested that personality variables like for instance ‘reliability, flexibility, patience, resilience, sensitivity to others, risk-taking, a thick skin, and a preference for diversity and new social roles’ (Bromme 2000, 117) and ‘tolerance of ambiguity, willingness to learn new things and ability to engage in divergent thinking’ as well as ‘curiosity and courage […] modesty and the ability to subordinate one’s own

personality' (ibid.) are advantageous. However, as Bromme points out, such variables are arguably not uniquely tied to interdisciplinary projects.

The discussion of personality variables can be relatively easily tied to conventional role theory (and thus to the more abstract notion of hierarchy above). To any social status there are attached a certain set of expectations and rules / norms of behaviour. A person with a particular social status will act according to his or her own inclinations as well as adapting to such norms, thus choosing how to play out his/her role in the social system. All roles in such a system are mutually affected by each other. They might be conflicting in different ways (like e.g. disagreements concerning leadership), or they might be complementary (like e.g. the relationship tutor–student) (Lauvås & Lauvås 2004).

Part of the challenge in interdisciplinary cooperation is that, on the one hand, every person should retain a strong sense of disciplinary identity at the same time as he/she needs to be integrated into a certain role which, on the other hand, might not always be fully compatible with a strong disciplinary identity. In long-term interdisciplinary cooperation, such disciplinary identities can be maintained by seeking other people with the same background. So the in-group is, somewhat paradoxically, chosen *outside* the workplace. Alternatively a person who finds that a strong sense of disciplinary identity conflicts with the role that is given to him/her might choose to adapt to it, and thus (at least in part) let go of the very perspectives that was part of the rationale for the interdisciplinary project in the first place (Lauvås & Lauvås 2004).

Conceptual structures

Interaction between people with different conceptual structures is a prominent characteristic of interdisciplinary work. Interdisciplinarity is not merely a question of exchanging information and facts; it is an interaction between different perspectives and different modes of thought. It is difficult to pinpoint exactly what this is about, partly because no human being has full insight into his or her own mental processes.

Foucault's notion of episteme captures something of this. He used the French word *connaissance* to indicate surface knowledge found in any field of knowledge (such as geography, biology etc.), while *savoir* means what Ian Hacking (1986) calls 'depth' knowledge, namely a set of rules that determine what kind of statements are regarded as true in a given epistemic realm. These rules are not stated explicitly and they are not apparent for people living in each realm. That some rules are dominant in a society such as ours can be part of the explanation of why some knowledges are given a higher status than others.

The idea of 'tacit knowledge' also represents a relevant perspective. Michael Polanyi writes in his book *The Tacit Dimension* (1967) about his notion of tacit knowledge, knowledge that we have without being able to tell exactly *what* it is we know. Although understanding this knowledge as permanently tacit, i.e. totally unavailable to conscious thought is problematic, it is to a significant extent still valid.

One can also see similar thoughts in Kuhn's major work on *The Structures of Scientific Revolutions* (1962). His paradigms become part of the scientist at a fundamental level, to the extent that he suggested that having different paradigms is like living in different worlds. If this might appear to be stating it too radically, I think nevertheless there is something to it. To be sure, conceptual structures differ between any pair of individuals to some extent. However, in academia certain conceptual structures are systematically nurtured in the disciplined mind to the point where certain assumptions are not only self-evident, but rather like ontological imperatives.

In this way the clash of different conceptual structures is very central to interdisciplinary encounters, the thing that makes it difficult, and the thing that makes it worthwhile.

Focusing: interdisciplinarity in this thesis

Many of the factors mentioned above are not exclusively significant for interdisciplinary work. All knowledge production is shaped by research orientation, institutional, social and cultural contexts. This is also the case with regards to personality variables, but to a smaller extent with differing conceptual frameworks.

The focus of this thesis is on how our elusive conceptual frameworks affect science, interdisciplinarity and geography. As we have seen, meetings between different conceptual frameworks can be seen as an absolute core issue with regards to interdisciplinarity. We have also seen that the total institutional and social context is highly significant.

The purpose of the theoretical focus of chapter 4 in this thesis is thus to focus on a core issue in interdisciplinarity. Since many of the other relevant issues are neither specific to geography as a discipline nor to interdisciplinarity as such, this focus should enable me to say something specifically about geography, while addressing general issues at the same time. The empirical section is an acknowledgement that the institutional and socio-cultural background always plays a role in any project of knowledge production.

Chapter 4: Analysing scientific knowledge: perspectives from cognitive science

This chapter seeks to address the first main research question: *what are the roles of the hidden infrastructure of our mind in science and interdisciplinarity?*

The notion that human cognition is specialised to handle more or less specific types of information and that this leads to certain 'default' ontological assumptions common to our species constitute interesting new perspectives in cognitive psychology. Here I summarise these thoughts and discuss possible implications for our understanding of science and scientific thought.

Naturalism and social constructivism

This chapter presents some results and theory from cognitive science and psychology that I believe are significant for scientific thought. This involves adopting an 'objectifying,' naturalistic mode of approach where human cognition is treated more or less as part of the natural world. It could be useful first to note the difference and connection between these naturalistic perspectives and perspectives that are more in the line of social constructivism. These perspectives, I argue, are not incompatible but rather complementary, each one providing a better understanding of the other.

Paul Rabinow's Foucault reader (Rabinow 1991) starts by referring to an interview with Michel Foucault and Noam Chomsky where they are asked a question concerning 'human nature.' Chomsky, working within a naturalist paradigm, attempts to answer the question as it is put, while Foucault does not accept 'human nature' as a valid referent. Such notions are for him results of specific histories charged with power and ideology. There is no such 'thing' as human nature. I have strong sympathies with both stand-points. This is not an expression of not being able to decide which to support. I do not believe it is wise to believe we have to choose in the first place.

It is the usual state of affairs in all science that there exists a multiplicity of models which apply at different levels of reality, and which often seem to contradict each other. Cognitive scientists can be said to operate at one level and idealist thinkers at another. To try to reconcile what we know about (for instance) the brain on the neurological level to what happens on the level of meaning I believe is futile if we by that mean that we want one model to describe both levels. The bio-chemical processes that constitute my mental functions are

totally irrelevant from the standpoint of my inner life and the content of my thoughts. Of course my thoughts could (probably) not exist without my brain, but when I enjoy a piece of music, a good argument or a glass of whisky, what could be less relevant? On the other hand it is clearly possible to say something informative from a naturalist perspective about why my organism likes those things, just as it is equally possible to say interesting things about the meanings of this music, argument or whisky in relation to for instance society, ideology and my social standing. These perspectives are complementary.

But we must choose our models with care. I do not wish to be taken to propagate an unreflecting eclecticism. There no doubt exist genuinely contradicting theories and models, which will result in confusion instead of clarity if applied together. Here I will be content to follow writers like Dan Sperber (1996) and my own intuitions. There is as far as I can see no contradiction between naturalistic and interpretative perspectives, except that they sometimes compete for resources, which is another question.

However, it is one of the aims of this chapter to render knowledge from naturalistic paradigms relevant to cultural phenomena, something which will require some caution if the attempt is not to end up as reductionism in some form. To formulate the relation between human nature and cultural/mental meaning is challenging, because they are phenomena on different levels of reality, and we cannot expect to be able to reduce one into the other. So the purpose of this chapter is to describe a naturalist understanding of mental functions, hopefully in the end resulting in a description that renders them as relevant to, but not in any way as determining, scientific thought. Mental functions are but one of many factors operating in the hugely complex field of cultural/social/physical/mental realities constituting 'science.'

Several writers are struggling to find ways of seamlessly integrating culture into mental functions (Hutchins 1995; Shore 1996; Cole 1998), and I agree with these that it is in a sense misleading to see culture as something separate from individual mental processes. Culture is simply part of the way the human mind works. This view implies a collapse of the nature-culture dichotomy. To treat people as independent standing computation organisms, as some would say certain psychological perspectives insist on doing, is in the long run misguided. This does not mean that e.g. psychological experiments on individuals are without value, only that the understanding we get from such experiments reaches its limits at a certain point and will taken alone result in a distorted image.

Evolutionary theory can shed further light on this. Human evolution and the 'construction' of the human brain is not a matter of single humans evolving isolated from each other. It is an evolution where social groups have played a significant part. The first

priority, it would seem likely, in such an evolution would be to *keep the group together*. Seen from this perspective, culture's main 'task' is to maintain a common world-view and a feeling of belonging, or whatever else that will keep us together. But within this group it would be important for each individual to obtain a good social position. Each individual's chance of survival and procreation is significantly increased by achieving a good material and social standing. So it seems likely that such a recurring situation would evolve in us a mental skill for understanding what other people think and how social situations work. It would be vital to understand this, in order to manipulate and cooperate with other people to achieve a better social position.

The details of how this has affected the human mind are very much a matter of debate, and I will go into this later in this chapter. However, if one accepts the main lines of this argument, i.e. that the human mind is a product of an evolution and that life in social groups has been a part of the same evolution, it becomes clear that in a fundamental sense the cultural and the social cannot be separated from the biological. Furthermore, since we know that culture has a major effect on our world-view, it *follows* from this bio-evolutionary reasoning that reality is (partly) a social construct. Again, the views are complementary, not contradictory.

Selection models

A major challenge, then, is to formulate a relation between micro-processes in the human mind with macroscopic cultural processes without ending up with a simplistic socio-biological picture where fixed biological propensities figure as 'causes' of human social behaviour. Thus, an imagined case may be made for the following: humans have an innate capacity for aggression and violence which they share with animals, and which is an evolved propensity acquired from the evolutionary need to defend aggressively the organism. In times of war people act in an aggressive and violent manner. Therefore wars may be explained by this innate and evolved capacity for violence.

The problem is of course that the reasons why soldiers fight in wars often have less to do with the aggression latent in their organism, and more to do with the perceived duty or feelings they have toward their nation, if they are not simply forced to fight. These feelings of duty, love, or whatever else may be their motive, cannot be explained or understood out of their proper cultural context. But can we thus say that human aggression has nothing to do with wars? Intuitively this seems an unreasonable statement. Human aggression is definitively there but it is not adequate to posit a simple model where it acts directly as cause for some

human behaviour, or as a single cause alone. We need a more sophisticated model, where more factors are allowed, and which does not fall into the trap of being too deterministic or, for that matter, too vague or banal.

Models based on selection principles have proven useful in providing non-deterministic explanations for complex patterns both in nature and culture. Weber's 'elective affinities' is one such example. Here systematic differentiation in religious adherence between different social strata is sought explained as a function of different religious needs and preferences, resulting from different life situations. The Darwinian model has of course had an enormous success in explaining biological variations and patterns. The work of Richard Dawkins (1976) is an attempt to apply this type of model to cultural phenomena. However, I will in this text rely mainly on Dan Sperber's 'epidemiology of representations' (Sperber 1996; Bloch & Sperber 2002).

The epidemiology of representations

Sperber's argument is that the relation of anthropology (or any other interpretativist perspective) to psychology can be understood in terms of an analogy with the relationship between epidemiology and physiology (Sperber 1996). The central point of this analogy is that it renders an understanding of psychological micro processes directly relevant to anthropology in the same way as physiology is essential for epidemiology (since the spread of causes of disease cannot be understood isolated from the organisms on which these causes act and vice versa). 'Representations' are in other words compared (somewhat unflatteringly) to viruses or bacteria. The spread of both viruses and representations are dependent on the relation between their own character and the constitution of the organism/mind.

In terms of being a selectionist model, the epidemiology of representations can be formulated in this way: Evolved psychological dispositions act as selection principles on the plethora of different ideas and social phenomena in a given setting. The relative *stability* within a culture and the various *regularities* that can be recognised across cultures can according to this partly be explained by the stability of our evolved psychological dispositions.

Within any group of people there is a large group of representations (thoughts, beliefs, fantasies, dreams etc.). Most of these are private representations, but some also get communicated. A few of these are communicated regularly and achieve a widespread occurrence in the population. Sperber's epidemiology of representations aims to explain why some, and not other, representations are widespread.

The small-scale processes that are relevant in the epidemiology of representations are of two kinds, *intrapersonal,* and *ecological* (phenomena in the environment*)*. Within the person we deal with *psychological processes* like perception, inference, remembering, deciding, and action planning. These processes deploy *mental representations*, i.e. memories, beliefs, desires, and plans. In the environment we have a variety of behaviours and artefacts that often involve each other in the production and reception of *public representations*, which can take the form of behaviours like gestures or utterances, or artefacts like writings or symbols.

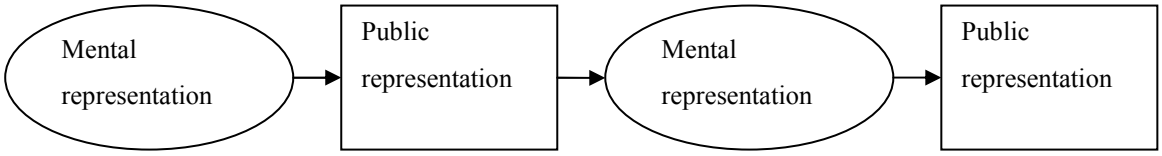

Figure 1. Schematic representation of a communication process according to Sperber. A mental representation is converted to a public representation, for instance in speak, which is in the next step converted to a mental representation and so on. At each conversion the representation is likely to be transformed in some way, so that the two mental/public representations are likely to be similar, but not exactly like each other. The exactness of this process depends on the type of material to be transferred (science, jokes, stories etc.), and the type of medium or technology (speech, writing, film etc.) employed in the public representation.

Mental representations can be transferred from person to person only via public representations (speech, artefacts, acts etc.), and this is not a simple process of replication. When a mental representation is first converted into a public representation and then reconstructed in a different mind, it is likely to transform, and this transformation is likely to be systematic rather than random. Some things are easier to transmit than others. For instance a dramatic folk tale is more likely to be remembered more accurately than an account of the Darwinian theory of evolution. The human mind is the result of an evolutionary process. It is adapted to certain kinds of operations in certain kinds of environments. Some things have a better 'fit' with the way our mind works. Put another way, our mind puts certain constraints on its own content. However the rigidity of this constraint varies from domain to domain, which will become an important point later on in this chapter.

> In a nutshell, the idea is that psychological dispositions in general (whether evolved basic dispositions or culturally developed dispositions) modify the probability – and only the probability – that representations or practices of some specific tenor will

> spread, stabilize, and maintain a cultural level of distribution (Bloch & Sperber 2002, 729-30).

Notice that the psychological dispositions that constrain us can be culturally developed as well as basic evolved dispositions.

The representations tending to be remembered and spread are the ones that make up 'culture.' But as we know from everyday life, some types of knowledge can be relatively widespread, but not globally present. Mental representations can be more or less widespread, and we must envision a continuum between purely personal representations, and cultural ones. In other words, both personal and cultural representations are mental. The difference is that cultural representations are widespread while personal representations are idiosyncratic. Public representations are for Sperber purely 'external' physical phenomena that may cause mental representations in people. Often such public representations will result in the evocation of cultural representations in the beholder's mind, and sometimes in idiosyncratic ones. More realistically, of course, they will result in something in-between.

Summary

To sum up some key points up until now: the human mind is seen as the result of an evolutionary process. This process has put particular demands on the way the mind functions, and thus shaped it. This means that certain types of knowledge are easier to adopt and hold within it than others. So even though our surroundings may contain many different types of representation, some of these are more likely than others to be adopted and integrated by the people who perceive it. Within the evolutionary perspective of the mind lies the dissolution of the nature-culture dichotomy, which allows us to include cultural factors into the way the mind works.

Sperber (1996, 26-7) states, '[d]ifferent types of representations may have their distribution explained in quite different ways […] aiming from the start at a holistic theory, as many social scientists are prone to do, results, for practical – and possibly also for substantive – reasons, in no theory at all.' The epidemiology of representations has now been described in general terms. In order for it to say something more exact, it is necessary to specify certain things about the human mind. In what ways does it shape and select its contents? The next section will address this question in more detail. Albeit incomplete in many ways, its ambition is to present a theory of the mind which is useful in *this context*, if not in every imaginable context.

Domain specificity

Over the past decades certain insights have emerged concerning the cognitive operations of the human brain. Previously there had been a widely accepted view that the human mind possesses a general set of reasoning abilities. These abilities are brought to bear on any cognitive task, irrespective of its specific content. This *domain-general* theory of the mind had one of its most influential proponents in Piagetian developmental psychology and followers of this school. Stereotypically this direction sees the child approximately as a 'lone scientist.' The child acquires its adult competence through observing and memorising data and observing recurrent features in that data. This is followed by the forming of hypotheses, which are further refined by observation etc. (Karmiloff-Smith 1992).

Over the past few decades, however, several disciplines have converged on a set of conclusions that seriously challenge this view of the mind. What now seems clear is that many cognitive abilities are specialised to handle specific types of information. Human cognition is *domain-specific* (Hirschfeld & Gelman 1994b). This chapter will discuss the extent and ways in which these insights can and should shape our understanding of science and scientific thought.

Domains, a characterisation and some general arguments

A tentative characterisation (not definition) of domains is given by Hirschfeld & Gelman (1994a, 21):

> A domain is a body of knowledge that identifies and interprets a class of phenomena assumed to share certain properties and to be of a distinct and general type. A domain functions as a stable response to a set of recurring and complex problems faced by the organism. This response involves difficult-to-access perceptual, encoding, retrieval, and inferential processes dedicated to that solution.

There have emerged many candidate domains in the literature. Perhaps most established is the LANGUAGE domain. The work of Noam Chomsky has been a door-opener and a major influence not only in linguistics, but also in cognitive science more generally. Some other likely domains are: PERSONS, ARTEFACTS, ANIMATE BEINGS, EVENTS and ABSTRACT OBJECTS (Boyer 1996). All of these domains, then, are assumed to receive treatment in the human mind according to specific kinds of principles which corresponds to that group of phenomena's shared properties.

There are several general arguments for why human cognition is likely to be domain-specific.

First we have the 'under-determination' argument. This holds that the information available to a given child in its environment clearly under-determines the structural content of its mind. Taking language as an example, this means that the structural properties of the language manifest in the child could not be inferred from the information available to the child. Therefore, it is concluded, a contribution must come from a specialised mechanism of the mind. The same type of argument can be used also more generally with respect to various other domains (Boyer 1994; Hirschfeld & Gelman 1994a).

Second is the argument that a domain-general mechanism would be too inefficient. Children acquire languages extremely fast and it is unlikely that a domain-general mechanism would be able to figure out a natural language[1] with such speed. Also, languages are acquired with equal speed even when learning conditions are different (Hirschfeld & Gelman 1994a).

Third, and related, evolutionary theory suggests that the human mind is likely to be specialised to handle certain domains. Natural selection is a 'short-sighted' and 'pragmatic' process. Organisms are able to live and reproduce because they are better at handling certain historically contingent but evolutionary recurring circumstances. Keeping in mind the complexity and variability of the problems facing organisms, it is unlikely that a general processing device could be quick and effective enough in handling such problems. Certain cognitive mechanisms must have been selected in response to the adaptive problems in the environment (Cosmides & Tooby 1994; Whitehouse 2001).

Fourth, when people perceive reality, attention has to be directed to certain areas of it, i.e. perception has to be constrained in some way. The numbers of attributes any given phenomenon may be seen to have are virtually infinite; it is 'logically open to many alternative construals' (Hirschfeld & Gelman 1994a, 10). Is a rabbit primarily a solid object, white, fast-moving, cute, a living being, furry or small? How does the child know that when we point to the rabbit and say 'that is a rabbit' we mean the animal and not a colour? Our mind somehow selects the right cues, which in turn sort objects into different conceptual slots. We then operate with particular assumptions about the phenomena in that slot. This happens in a way that often is tacit and which seem self-evident. It should be mentioned, though, that such constraints need not in fact be domain-specific or innately given in a strong sense (Hirschfeld & Gelman 1994a), although it is to my mind likely that it is so when taking the evidence together.

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[1] 'Natural language' may sound like a contradiction in terms. Here is meant a non-artificial/non-constructed language (like e.g. a computer language or Esperanto), i.e. a normal, cultural language.

At this stage we need to go through some of the other evidence and arguments that support the domain-specific theory of the mind. It is necessary to go into some detail here, so that the reader can evaluate the plausibility of my conclusions.

Domains, specific evidence and arguments

Psychological experiments done on infants or young children constitute much of the material that supports theories about domains. In the case of infants, crucial importance is given to 'preferential looking methods,' i.e. experiments based on the well documented tendency of infants to look longer at displays they perceive to be novel (Carey & Spelke 1994, 172).

It seems that there are two domains that have a privileged status in early child development, and that other domains are spawned from these. These two domains are a kind of 'naïve physical mechanics' and a 'naïve psychology.'[2] This is suggested by a large body of recent research done on infants and special populations, like autistic children (Keil 1994). In the young child, all acquired concepts are mapped onto either one of these domains, and the causal principles that govern it. Thus young children reason mainly in psycho-social terms about biological kinds, leading them to distort and misconstrue biological things to fit psychological. They are also unable to distinguish between biological phenomena and psychological ones. A young child would for instance only understand eating in terms of beliefs and desires, and not in terms of physiology and nutrition. It is only later when the child is 6 to 7 years old, that a distinct domain for biological thought emerges (Keil 1994).

Let us have a look at the domain related to the nature of physical reality. Infants expect solid objects to behave in a certain manner, and they are surprised (i.e. they look longer) when these principles appear to be violated in experimental settings. They expect objects to move in continuous paths, to not coincide in space and to fall when unsupported (Carey & Spelke 1994). It has been shown that infants as young as 3 months of age[3] perceive a three-dimensional object presented against a uniform background as a connected body that will

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[2] It is customary in cognitive science to distinguish the study of how human cognition deals with aspects of reality from the scientific study of those realities themselves by putting 'naïve' or 'folk' in front of them.

[3] Many will (I believe rightly) express doubt concerning whether three months is sufficiently young for us to draw the conclusions that are in fact drawn from this material. It is not, however, possible to follow up on every weakness that this material may have. I feel it necessary to trust the conclusions that have been made by specialists working in this field, as is often necessary if one is to operate across disciplines and take advantage of knowledge from other fields. However, taken together with all the other evidence and arguments presented in this chapter, I feel reasonably at ease doing so in this case.

maintain its connectedness as it moves. Such principles are self-evident for the adult individual.[4] That they seem to be so also for young infants suggests that these principles are innately given, and not something the child infers from repeated observations.

The second important domain is what has been called theory of mind, naïve psychology or folk psychology. It has become a standard assumption among philosophers and psychologists that adult human beings have a large conceptual repertoire which they deploy to explain, predict and describe the actions of one another (Davies & Stone 1995). This repertoire is probably, at least in part, a composite of intuitive principles or supported by such. Young children readily interpret behaviour of both persons and other animate beings in terms of 'unobservable entities' such as *beliefs* and *intentions*. To do this they have to work with a series of tacit principles concerning the nature of mental entities and their connections with observable action. A rudimentary understanding of this kind is present in three-year-olds (Boyer 1996).

Crucial importance is given to the so-called *false belief* task. This experimental setup shows whether or not the child has acquired the ability to understand that other people act on the basis of their beliefs, and that these beliefs might not correspond to reality. The experiment has been done in a variety of ways and in different settings. It seems to give consistent results also across cultures (Harris 2003). The basic setup is like this: The child sees a character, 'Sally,' put an object, e.g. a marble, in a container. Sally then leaves the setting, and a different character removes the marble and puts it in a second container. Sally then returns, and the child is asked the question: where will Sally look for the marble? Typically, children at the age of three do not understand that the character has a false belief, and will answer that Sally will look in the second container. Five-year-olds do understand this however, and will answer that Sally will look in the first container. The fact that the shift between failing and passing the false belief task happens at the same age everywhere, i.e. despite differing learning conditions, suggests that we have to do with a relatively strong innate ability. It probably means that it is enough that situations that are comparable to the 'Sally situation' are present in the child's environment for the child to develop this ability.

Folk psychology can in turn be seen as a crucial component in a *folk sociology*, i.e. folk beliefs about human groups and group affiliations. Human groups are very complex and

[4] So self-evident that it is initially hard to understand how basic these kinds of experiments are. That an object moves in a continuous path, for instance, means that it does not e.g. suddenly disappear in thin air and appear in the same moment in a totally different location.

fluid seen in comparison with other social species. To judge social situations rapidly and precisely is thus both a demanding and crucial ability. Humans need to deal with information about large amounts of people, how these people belong to different groups and how coalitions between such groups shift. It is extremely likely that humans have developed specific abilities to navigate in social settings, not least because other humans constitute the main predator for humans (Hirschfeld 1999).

'Living beings' or 'living kinds' has been suggested as another domain. Scott Atran (1990) and others identify features in folk biology that are cross-culturally extremely recurrent, for instance the organisation of organisms into taxonomic structures (i.e. hierarchical structures where entities at a lower level are included in a wider category above, like 'collie', 'dachshund' and 'bloodhound' are all included in 'dog') that resemble the way scientific biology categorise organisms.

> Humans everywhere classify animals and plants into species-like groups as obvious to modern scientists as to a Maya Indian (Atran 1999, 317).

This systematic way of categorising living kinds constitutes a distinct way of thinking and inducing facts about organisms. A specific kind of *essentialism* has been suggested as one of the domain-specific principles which govern how people think about organisms. By observing and testing how people think about them it can be inferred that they are attributed an 'essentialist principle' which is species-exclusive and undefined (i.e. tacit). This principle is seen to cause both external and behavioural aspects of the being. Thus dogs and cats have something that account for their looks, differentiating them from for instance chairs. Pertaining to this essentialist principle is also the 'default' assumption that species are essentially different from one another (Atran 1999). The Darwinian theory of evolution where one species evolves out of another thus violates such essentialist principles on one level (Harris 2003).[5]

An intuitive ontology?

What some have concluded from these arguments and studies is that a certain way of dividing up the world is innately given. The tendency to divide up reality in independent spheres is well documented. For instance Harris (2000) has shown that children easily bracket off spheres that are not compatible with each other, and let them function as independent spheres

[5] This 2003 article by Paul L. Harris, Gods, ancestors and children, has so far only been published in French (*Terrain* **40**, 81-98). I am grateful to the writer for sending me the original English version of it.

of reality. People easily and readily separate ontological spheres. Although all of these spheres need not be intuitive imperatives, some of these make up a set of ontological categories that seem to be with us from the earliest stages of conceptual development. Moreover, these categories are governed by specific causal principles. Thus 'even young children acquire knowledge on the basis of quasi-theoretical understandings of the various ontological domains' (Boyer 1996, 84).

If humans thus are equipped with such an intuitive ontology, what kinds of implications does this have for science and scientific thinking? As we will see, the constraints on the possible conceptual content of the brain, albeit significant, are subtle and weak rather than of a strongly determining nature. Furthermore, even if a certain basic way of dividing up the world is similar everywhere, this does not mean that all beliefs (causal, theoretical, mythical) about these categories are the same (Atran 1994). But this does not seem consistent. On the one hand it is claimed that quasi-theoretical principles of causation govern the domains, but on the other it is admitted that beliefs about the domains may vary considerately. Keil (1994) deal with this by distinguishing the *details* of beliefs from more general 'modes of construal.' The human mind seems to be endowed with a small set of such biases of interpretation, such as the mechanical, the intentional and the teleological. These can be seen as 'opportunistic, exploratory entities that are constantly trying to find resonances with aspects of real world structure' (Keil 1994, 253). Thus they give us certain predispositions for how we think about the world and its different objects, but are of an 'exploratory' nature so that different modes of construal may be applied to the same phenomena, for instance through metaphor. They are exploratory, but still they have their 'native' field where they work best.

Modularity of mind?

So far in this chapter domains have been construed more or less as innate, evolved psychological dispositions. Though it is self-evident that some innate basis must be present for their development, it is not clear how specific this basis is or must be. Proficiency in the game of chess can for instance be seen as a domain, and nobody is claiming that such knowledge springs out of a specific evolved cognitive mechanism. In other words the word 'domain' refers to an area of knowledge, and not to a specific 'neural tool.' Such neural tools are called *modules*, a term propagated by researchers such as Fodor (1983) and Chomsky, who represent the modular approach to cognition (Hirschfeld & Gelman 1994a). It is likely that there is a language module, or perhaps a set of nested modules. It is not likely that there is

a module for chess, but playing it probably involves use of modules such as those to do with visual pattern recognition (Hirschfeld & Gelman 1994a).

I have not by any means given a complete sketch of how the mind works, and such a task seems overly ambitious anyway. However, I doubt this chapter will be convincing if I do not treat some more questions and problems to do with domains, modules and the obvious flexibility of human cognition manifest both in science and in the differences that exist between cultures. Acknowledging that there are many different ways of approaching such a task I will here follow a sketch given by Dan Sperber (1996, 119-50) that I find appealing (if unavoidably speculative), and that is formulated as a part of his 'epidemiology of representations' which I find useful as a general framework.

Sperber takes as his starting point a seminal work by Jerry Fodor, *Modularity of Mind* (1983), which attacked a view dominant at the time, namely that there are no important discontinuities between perceptual processes and conceptual processes. The view was that beliefs form perception as much as they are formed by it (Sperber 1996, 119). Fodor's argument was that perceptual processes are carried out by specialised and quite rigid mechanisms. He drew on empirical material concerning vision and hearing. Examples of modules proposed by Fodor are colour perception, analysis of shape, analysis of three-dimensional spatial relations, recognition of faces and recognition of voices (Hirschfeld & Gelman 1994a). For him, however, domain specificity was present only in the periphery of the mind, in its input systems, while the central conceptual processes were portrayed in a holistic, domain-general manner.

There is, then, a distinction between perceptual and conceptual processes. Perceptual processes get input from sensory receptors, and they produce output in the form of a conceptual representation. Conceptual processes have conceptual representations both as their input and output.

A module is 'a genetically specified computational device in the mind/brain […] that works pretty much on its own on inputs pertaining to some specific cognitive domain and provided by other parts of the nervous system' (Sperber 1996, 120). Note that modules mainly work *on their own*. This is likely to form part of the reason for why certain domains are difficult to integrate.

If it is granted that it is likely that perceptual processes must be modular, there are arguments that speak against conceptual processes being so.

One such argument relates to the integration of information. A dog can be seen, heard, smelled, touched and talked about. The percepts are different, but the concept is the same. In

other words the representations that the input systems deliver have to interface somewhere, and that interface must get information from more than one cognitive domain. Since modules work with specific limited information in isolation from the other modules, so the argument goes, a modular mind would not be able to integrate information in the ways needed.

Another argument against thought processes being modular has to do with cultural diversity and novelty. I have mentioned chess, and there are of course a plethora of different domains of knowledge that an adult human's conceptual processes range over. Computers, Chinese cuisine, soccer, stamp collection, surrealist painting, and German architecture are all examples of domains of human knowledge. Many such domains are very recent and are specific to a particular culture or even to a tiny sub-culture. It would thus be unreasonable to claim that such domains are correlated to genetic adaptations.

Since the whole point of modular design is that it is efficient and fast, the kind of reflecting knowledge that may cross over several different domains seems unaccounted for. Some processes in the human mind must be domain-general.

However, Sperber argues, it is unlikely that larger parts of the brain should have evolved into domain-general mechanisms. New cognitive modules are selected for because they process information efficiently, and it is unlikely, considering what we know about evolution, that domain-general mechanisms could develop on a large scale, because these would be too slow. Modules must be specifically useful to a particular calculation for it to be selected.

Once a module is developed, however, its original function may disappear, it may function on other kinds of things, it might even be superfluous, or it might react to other things in ways that might not have any 'use value' for the organism. Following this line of thought Sperber offers a distinction between *actual* and *proper* domains of modules. The proper domain of a module is all the information that it is its *biological function* to process, while the actual domain is all the information that the module actually does process. There is no intrinsic relation between a module and the information it is supposed to handle in the external world. All that is needed is for the module's input specifications to be met, and the module will do its work, i.e. handle information in a particular manner.

Sperber envisions a brain/mind which consists of an unknown, but probably very large, amount of cognitive modules that were once specifically selected for by one or more recurring ecological factor. Such modules are likely to operate on different kinds of principles, and they may be of different sizes. He argues that several modules might be only the sizes of single concepts, while others are larger, perhaps corresponding to a semantic field.

Let us pick up on the problem of integration of knowledge again. Modules do certain limited operations, and they do so relatively encapsulated with restricted access to information in the rest of the brain. Input mechanisms must be modular to handle the chaotic external information in an effective manner, but if this modularity continues into conceptual processes, the mind would not be able to integrate information. As we have seen, though, it is unlikely that there should be a great leap between fully modular perceptual processes and fully general central conceptual processes. Furthermore there is no reason to believe that conceptual modules must be identical to the perceptual ones. It thus seems likely that a specific module for integration of perceptual information from several domains could have evolved. This new, integrated information will in turn be handled by other modules, according to its relation to the respective input conditions of each module. Integration thus happens, not in a domain-general mechanism, but in a specialised manner different from the perceptual modules.

What about cultural knowledge and novelty? Part of this problem should already be resolved with reference to what is written about actual and proper domains. Modules are not necessarily too picky, and novel information can be processed by modules that were evolved for entirely different reasons. There is however more to be said in this respect.

As we know, reality is for humans to a very large extent a product of humans themselves, and not merely something pertaining to an external natural environment. The rate of change and the pervasiveness of this cultural reality probably means that there are few cognitive modules among humans that only process information from their proper domain. Furthermore it means that our cognitive modules receive cultural information that is already structured in a particular manner by humans. Sperber therefore distinguishes *cultural domains* of modules from both their proper and actual domains. He states that any human module is likely to contain large amounts of cultural information.

As we see, there is much room for integration, diversity and novelty in a modular mind. But there are things that still have not been mentioned, and which will modify the rather rigid image of the mind that the modular view tends to convey even more. At a certain level of complexity, Sperber argues, ‘modules can emerge whose function is to handle problems raised, not externally by the environment, but internally by the workings of the mind itself’ (1996, 133). One such problem is the risk of ‘computational explosion,’ a problem that a richly modular mind would encounter just as a massive non-modular central process would do. Computational explosion would occur in a richly connected system if no mechanism limits *which connections to follow*. Something must ‘put up on the board’ which information to process. Such a device Sperber suggests ‘attention’ is. It is a temporary buffer,

the content of which is processed by the modules whose input conditions this content satisfy. Attention is clearly not a domain-specific module, but it is an adaptation to a specific problem posed by the organisation of the brain.

Finally, we must mention the human ability for 'meta-representation,' i.e. the ability not only to hold mental representations, but also to hold mental representations of mental representations (in order to figure out what other people think). Arguably this ability is so specific in terms of its domain and of its computational requirements, that it probably should be called modular. The proper domain of such a module, it is suggested, is probably what I have mentioned in this chapter already, namely what is called naïve psychology or theory of mind. Such a module is likely to have evolved among organisms that must co-operate and compete with one another in a great variety of ways. The actual domain of this meta-representational module is 'the set of all representations of which the organism is capable of inferring or otherwise apprehending the existence and content' (Sperber 1996, 147). To Sperber this ability lies at the basis of the human ability to communicate, and it should thus be the background out of which public language grew. Ideas gained from communicative behaviours of any kind therefore enter through the meta-representational module.

An interesting question here is the relation between knowledge that we have learned through this meta-representational module and other types that are supported by first-order conceptual modules. Such meta-representations may find their way to the relevant modules, so that what is related to us about cats is integrated with what we have experienced about such animals. But we then have two modes of knowledge about cats. One is a representation of cats, handled by a first-order conceptual module. The other is a representation of a representation of cats, handled by a second-order meta-representation module.

The meta-representational module does not arrange its knowledge in the same manner as the cat module (or whatever first-order module handles cats). It possibly treats its information with regards to semantic relationships among representations. It may also have some ability to evaluate a belief, not on the basis of its content, but on the basis of the reliability of its source. We thus get a duality of knowledge which Sperber calls 'intuitive beliefs' and 'reflective beliefs,' and reflective beliefs may be counter-intuitive with respect to our intuitions about their subject matter, but intuitive with regards to our meta-representational reasons for accepting them.

This chapter has presented a 'domain-specific' view of the mind. Of course this presentation must be incomplete in lots of ways. The human brain is sometimes presented as the most

complex single object known, and there is no use pretending that we know everything there is to know, or that it can be presented in a short chapter like this. Hopefully it will still aid our thinking about the topic of this thesis sufficiently that I am justified in writing it, and in general it must be better to have a conception of the mind that is in step with the view of those who specialise in the field, rather than keeping it implicit as is usual in much social science.

Let us take a further critical look on this topic before moving on.

A critical look at domains and 'intuitive ontology'

Domain specificity as social construct

The notion of domain specificity is still a relatively new one, and there is not yet consensus on a concrete definition of domains. It is even possible that the concept of 'domains' is mainly a result of a particular research orientation, i.e. that they are a 'social construct,' and do not exist apart from the ideas that result from this particular social process. This would mean disaster for the present chapter, and I will therefore address the question in the following few paragraphs.

I do not doubt that the research questions that have been asked, and the data sets that these interests have prompted do affect the conclusions that have been drawn. Such is the case in all science. What reassures me with respect to the possibility that we have to do with a social construct, however, is that the notion of domains has come from several fields, and not e.g. from one socially integrated school or discipline. Hirschfeld and Gelman (1994a, 3-4) writes:

> Psychologists with concerns ranging from animal learning to emergent theories of mind and body, cognitivists exploring problem solving and expertise, anthropologists working with color terms and folk taxonomies, psycholinguists investigating auditory perception, and philosophers and others examining reasoning schemata have concluded – often independently – that humans simply could not come to know what they do know in a purely domain-neutral fashion.

This should render unlikely the possibility that the notion of domain-specificity is entirely and from the outset a social construct. That the particular versions of it are so to a certain extent is, as I see it, unavoidable but not therefore detrimental beyond repair with respect to this chapter.

That is if one accepts the possibility of achieving useful scientific knowledge about the human mind at all. I agree that it is problematic, partly because the mind is such a value-laden area of study. The scepticism many entertain toward bringing matters of biology into social reality is in important respects justified. Such perspectives have been, and are in constant

danger of being, misused e.g. by racists seeking to justify their 'theories.' But any field of knowledge run dangers of misuse, and at least I believe it will be clear that there is no hidden agenda in this text; biases, probably, but no consciously hidden agenda. Serious naturalist studies into human nature do not support racism, but underscore the absurdity of such notions (see Sperber 1996, 151-55).

The notion of 'human nature' can indeed be seen as 'ideology,' or a power-laden construct. The question is whether this perspective should always be primary and therefore cancel out any attempt to understand it as an object in itself. I hope that the models and theories in this chapter are judged primarily on the basis of their explanatory value and not out of default assumptions concerning naturalistic perspectives in general.

The notion of intuitive ontology

I have mentioned the possibility that the human mind somehow, through the way it functions, leads to a certain view of the world. This notion enables writers like Boyer (1994) and Sperber (1996) to speak of 'intuitive' and 'counter-intuitive' ideas. Earlier in this chapter I followed Sperber (1996) in opposing two kinds of knowledge, namely 'intuitive' and 'reflective.' It should be reasonably clear in what ways this must be understood. There seems to be very few absolutes concerning the human mind, and this splitting of knowledge into different kinds must not be seen as too clear cut. It is more a matter of tendency than of absoluteness.

Still it is important not to downplay the significance of intuitive ontology too much. I do believe it is significant and interesting that certain spheres of reality roughly corresponding to 'biology,' 'physics,' 'psychology' and 'maths' are understood in similar ways across boundaries of culture and age. I also believe that science is not in a privileged position in this respect, and that it is vital to understand this and the significance of it for scientific thought. It is hard to imagine science evolving and existing independently from 'common sense,' and common sense most likely relies in part on some form of intuitive ontology, i.e. a world view that is common to our species in general (Atran 1990).

Although science seems to be able to create new ontological categories, and to override the core principles pertaining to a domain, it is questionable how deeply such conceptual acrobatics are able to affect human thinking. As Scott Atran (1990) points out, science hardly affects ordinary thinking about everyday matters. Most people become competent cultural and professional performers without any scientific schooling. Even scientists tend to fall back on what Atran calls common-sense thinking when not at work.

It seems probable that those conflicts or chasms in science that go deepest are likely to be associated with breaks with intuitive ontology. Since intuitive ontology clearly must be tied to cognitive homeostasis and existential security, theories and standpoints that challenge this will encounter powerful opposition. However, since it is in the nature of the intuitive ontology to be self-evident, it is even more likely that such theories and thoughts will be ignored, and not perceived at all.

It is not given how we should interpret the existence of domains comparable to our western scientific disciplines. Naïve linguistics, biology, psychology, mathematics are examples of such candidate domains. It is tempting to pose a causal link here; so that the domains somehow cause the disciplines (disciplines reflect domains). However, others might draw the exact opposite conclusion, that western categories (reflected in disciplinary boundaries) have 'caused' the domains via Western-biased research projects (domains reflect social reality). It should be clear that I do not believe in a simple causal model here. The various innate dispositions in our mind must be seen as active principles in a historical process that have included many types of social, scientific and non-scientific factors.

If a vital split in the intuitive ontology of man goes between the physical world and the social world, this fact should be taken into account in science, and not least in geography. It is one of the hallmarks of both modern and pre-modern geography that it includes physical as well as social aspects. This marriage has been a problematic one indeed. However, the constitution of geography will be treated in a later chapter. Now we will examine some tools for thinking about thought.

Concepts and schemas

What is a concept? Often the term is used as another word for idea, notion or word meaning without further reflection.

It is important also to distinguish between *terms* and *concepts*. Terms refer to concepts, but a single term can have several concepts attached to them, and some concepts might exist in a mind and still not have a corresponding term. In other words we mean by concepts some kind of mental structure, while a term is simply a matter of written or spoken language (Jones 2003).

Here I will go through what is meant by concept in psychology, and I will go on to describe the related concepts of *schemas*.

Concepts

The concept of concepts is vital to psychologists. Concepts have important functions in mental life, some of which will be mentioned here. The nature of these functions constrains the structure of concepts.

Concepts divide the world into classes. As such they have an important function in promoting cognitive economy (Rosch 1978; Smith 1988, 19). Without such classes our mental and communicative processes would look very different, as we would have to have unique mental representations of every single entity we would encounter. Such an enormous amount of representations would have had dramatic consequences for the speed and efficiency of our mental functions as well as our communicative processes.

Another important function of concepts is that they enable us to go beyond the information given (Bruner et al. 1956; Smith 1988, 20). When we encounter an object, say a tiger, we have direct information only of its immediate appearance here and now. In order for us to act appropriately towards the tiger it is necessary to go beyond appearance and bring our other (e.g. reflective) knowledge to bear on the situation. Thus concepts are our way of linking perceptual and non-perceptual information. When a perceptual event evokes a certain concept, the concept acts as a recognition device and as an entry point into our knowledge stores, enabling us to react appropriately to the situation.

Also important, especially in science, concepts can be combined to form complex concepts and thoughts (Osherson & Smith 1981; Smith 1988, 20). *Fire* is one concept, *burn* is another, while *fire burns* is a full-fledged thought. Our understanding of complex concepts and thoughts are presumably based on our understanding of the constituent concepts.

Content of concepts

But what exactly are concepts in terms of content? Two different types will receive our initial attention, although no sharp divisions can really be made here. The two types are 1) classical concepts and 2) prototypes.

The classical type has been historically very important, and was endorsed by figures like Freud, Piaget and Skinner. In this view concepts are seen in terms of definitions which involve properties that are singly necessary and jointly sufficient to define that concept. An example of this kind of concept is BACHELOR.[6] Many would agree that at least three

[6] I refer here to a specific CONCEPT of bachelor. The TERM bachelor has several meanings/concepts attached to it.

properties, a) male b) adult and c) unmarried, are necessary to constitute the concept (Smith 1988). However the classical view has serious limitations, and psychologists have failed to provide explicit definitions for everyday concepts like animal, plant, cat, tiger, furniture, car or fruit. A tiger can be defined as being carnivorous, striped and four-legged. However, most will still classify it as a tiger even if it was born with only three legs, painted white and surgically altered so that it could only digest plants. If these features cannot function as defining properties, what can (Smith 1988)?

Experimental findings strongly suggest that many concepts are represented in the mind more in terms of 'prototypes' or 'best examples' than in terms of definitions. Smith (1988) terms these kinds of concepts *fuzzy concepts*. People can reliably order the instances of any concept in terms of the extent to which the instances are 'typical.' Thus a penguin is most often rated as being less typical of the concept BIRD than a robin. An apple is more typical as FRUIT than a raisin. Further it seems that almost all of the properties of each concept are non-necessary, and typicality is a function of the frequency of these properties. Since the classical view is restricted to necessary properties, it cannot accommodate such typicality effects (Smith 1988).

The classical view should however not be dismissed. There are some concepts that conform to it. I have mentioned BACHELOR. In addition there are kin concepts like MOTHER and UNCLE, concepts that belong to axiomatic systems like maths or geometry (EVEN NUMBER, TRIANGLE), legal concepts (CONTRACT, ROBBER) and other concepts related to technical fields. It seems that many such classical concepts are 'invented' technical or scientific ones, even though this is not true for all of them, for example the concept ISLAND (Smith 1988). Broadly speaking it seems reasonable to suggest that classical concepts are associated with Sperber's reflective knowledge while fuzzy ones belong to the intuitive type of knowledge.

An important point about concepts is that they cannot be equalled with word meanings (i.e. they do not necessarily have words attached to them). For many it is hard to imagine thought without language. It is however entirely possible to have concepts without corresponding words attached to them. Developmental psychologists agree that in most cases everyday concepts form in the child's mind prior to them learning the word itself (Pinker 2000). Moreover, words can have several meanings, making a simple word-concept correspondence understanding of concepts impossible. Stories about children who grow up without any form of (public) language, though still coping well with the conditions of existence, also demonstrate that relatively advanced thought processes do not require

language to function (see Pinker 2000 ch. 3). It is in general highly problematic to think that we can easily and reliably use what people say or write to infer anything specific about their thought processes (Bloch 1998a, 3-22).

Schemas and connectionism

There are different ways of imagining what happens inside our heads. I have described two ways that can be useful for thinking about concepts (i.e. classical and prototype concepts). A third and in some ways more sophisticated way of imagining mental processes is called connectionist modelling. Such models are suggested by both Strauss & Quinn (1997) and Bloch (1998). Strauss & Quinn (ibid.) integrate connectionism with schema theory which will be described shortly.

Let me first mention that I do not believe it is right to draw sharp lines between these different ways of understanding mental processes. They are by no means without faults as we have seen, and they do not really exclude each other. Even seemingly clear-cut classical type concepts have prototype components. The concept UNCLE will thus not only consist of its definition, but also of the fuzzier concept created from actual experience with a concrete uncle. Similarly concepts can very well be described as schemas instead. It seems that we need to retain a multiplicity of models rather than search for the ultimate one. For instance it seems that classical type concepts have a special position in western science, and many attempts are made to construct such precise instruments. Such demands for 'ideal knowledge' needs to be conjoined by an awareness of the probability that whatever the ideal is, our mind inevitably has its own way of doing things which might not conform to the way we want it to be. Furthermore many complex topics are not amenable to simple definition-type concepts. The complex concepts in geography and other social sciences are formed in ways which are even less controllable than concepts in maths or physics.

Schemas are not distinct things, but rather collections of elements that work together to process information (Strauss & Quinn 1997). They are used to refer to any kind of generic knowledge, and can be abstract or concrete, simple or complex and refer to parts or wholes.

Schemas enable us to go beyond the information given, and will 'automatically' fill in relevant information, especially if what is given is incomplete or ambiguous. Such is the case in many, perhaps most, situations. In any given conversation many things are taken for granted, and a speaker must assume that the other has the right schemas, or communication would be difficult indeed. Often our physical environment can be ambiguous too. Is that

shadow a large stone or a bear? Our mind in a given situation might choose the bear option and the proper bear schema will then guide our actions and perceptions from that point.

Since such filling in is a cognitive process which we have no conscious access to, this property of schemas is one way we in *particular situations* can speak about tacit knowledge. This does not mean that we have no conscious access to all the information in a schema, only that we do not reflect on all our knowledge at all times.

The connectionist model of schemas is partly a product of efforts to do with artificial intelligence. 'Parallel distributed processing' and 'neural network modelling' are other terms for such models (Strauss & Quinn 1997). Instead of seeing thoughts as sentence-like strings in our head, connectionism suggests rather a neural metaphor to picture knowledge. In this view knowledge lies implicit in the networks of links among many simple processing units that work like neurons. Neurons receive excitatory and inhibitory signals from other neurons, combine them, and, if activated past its threshold send excitatory or inhibitory signals to other neurons. A single neuron does not know much, but thousands of neurons working in parallel produce knowledge and action that is able to respond intelligently to the environment (Strauss & Quinn 1997). With experience synaptic connections are modified, so that learning results in structural changes in the network. Language and speech can be part of the input that causes such learning, but unlabelled and undiscussed sensations like bodily postures and experiences do not stand in a subordinate position to lingual in this respect. 'Embodied' thoughts and 'doxic' knowledge in a roughly Bourdieuian (Bourdieu 1977) sense is the result of this kind of learning, although Bourdieu overstated the separation between what can be communicated and what cannot (Strauss & Quinn 1997, 44-8).

Schema theory has some interesting consequences for memory. Frederic Bartlett coined the term of schemata in his ground-breaking book about human memory (Bartlett 1932). He found that people have great difficulties remembering things that are not easily incorporated into existing schemas. Novelties are simply not stored permanently, and are held in mind for relatively short periods of time. In his research, a Native American folktale ('The war of the ghosts') was told to English college students, who were then asked to reproduce it from their memory. Because the narrative was different from stories they were used to hearing, the students had difficulties reproducing it accurately. Instead students inserted systematic errors into their recalls of the story, transforming it into something they could more readily understand (e.g. it was made more coherent, and supernatural elements were rationalised away or omitted). Making sense of something thus means matching it to an existing schema, and remembering is more a matter of activating a relevant schema or set of

schemas than an actual 'recording' of any event. According to Bartlett memory is '... an imaginative reconstruction, or construction, built out of the relation of our attitude towards a whole active mass of past experience' (Bartlett 1932, 213).

This sheds light on the way scientists sometimes seem to ignore or overlook aspects of their empirical material. It simply is not remembered if it does not fit to any of the schemas that the scientist through his/her training and socialisation into a particular paradigm has learned to regard as appropriate. Schemas are selection devices which elect which aspects of any given phenomenon are relevant.

The confusion that sometimes arises when scientists from different disciplines meet can also be explained on this background. The situation can be especially difficult if the scientists have the same words but different schemas, which can easily happen even within the same discipline. An example of this can be found in the controversy between Niels Bohr and Albert Einstein concerning their understanding of the informal language they applied to speak about the possible interpretations of quantum physics (Bohm & Peat 1989, 84-6).

The chapter so far

This chapter has presented a view of the human mind which hopefully is reasonably up to date, and compatible with major progress made in cognitive science in the later years.

After discussing the role of naturalism and its relation to social constructivist modes of thinking, I concluded that a) social constructivist perspectives should not cancel out naturalist accounts, because the perspectives are not really contradictory, but rather complementary, and that b) the explanatory value of naturalist theories should be taken seriously, but that c) the connection between the cognitive level and the cultural level is problematic. To handle this linking I use the selectionist model developed by Sperber, 'the epidemiology of representations'.

The domain specificity of the brain was discussed, both referring to general arguments, and empirical evidence, and some likely domains were examined. Following Sperber, the likelihood that it has a modular organisation was discussed. Recall that a module is a 'neural tool,' which evolved to handle a specific type of information as effectively as possible. A domain, on the other hand, is a field of *knowledge,* which can consist of different types of information, handled by different modules.

Then I presented some tools for thinking about thought, i.e. the concepts of 'concept' and 'schema.' These correspond to Sperber's 'mental representations.'

As we have seen in the discussions, the difficulties are plentiful almost no matter which view of the mind one chooses. I find it necessary to acknowledge that one does not know everything there is to know about the mind. However, it is necessary in the following discussions to let go of the difficulties and complexities, so that a constructive and manageable way of dealing with our subject becomes possible.

I will from now on take it as given that the mind has a modular organisation and that within these modules concepts grow under certain constraints presented by those modules. These constraints are significant, although not 'carved in stone' and the respective strength and structure of each module may vary (more on the significance of this below). Concepts in one module are able to link with concepts in another, and knowledge about the same thing may exist in different modules, so that we can have different *types of knowledge* about the same objects. These types of knowledge are likely to be connected, but can possibly also be disconnected. So a knowledge domain may be confined to a single module, but may also span over two or more.

I will also assume that there is (something like) an intuitive ontology. This is likely to be implied in a set of modules at a relatively 'low level' in the nervous system. The ontology is highly intuitive and largely tacit. We have, however, also 'higher level' notions from culture (reflective knowledge) that are not necessarily in tune with the 'low level' ontology. Scientific concepts might appear very reasonable seen from within a discipline, but at the same time be very counter-intuitive seen from this innate ontology.

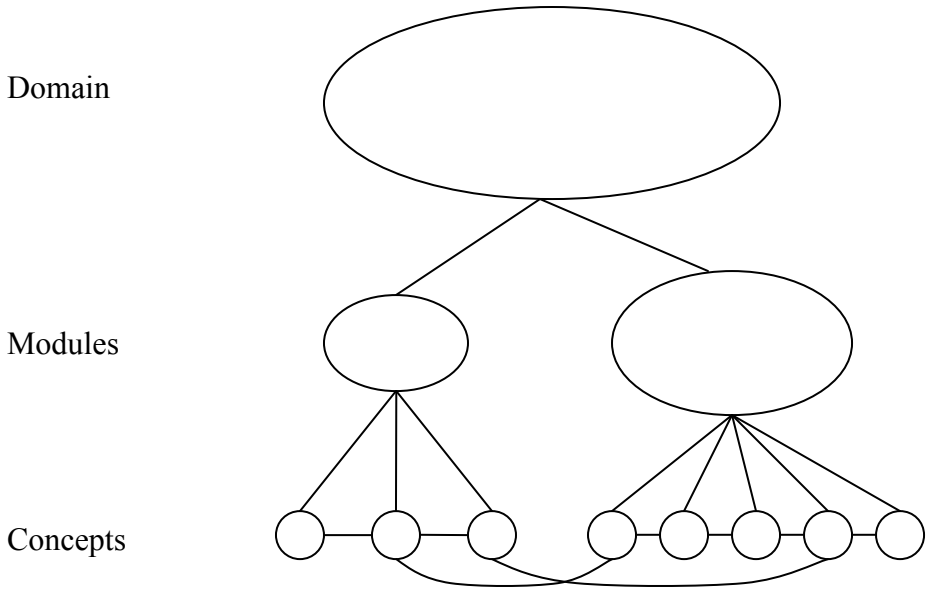

Figure 2 illustrates a hypothetical domain consisting of two different modules which in turn incorporate two sets of interconnected concepts.

Concepts, constrained by modules, work as selection principles in the manner described in the epidemiology of representations. Thus they shape cultural entities, such as science. But science, being a highly structured cultural knowledge domain, also has its own dynamic and momentum which in turn shapes concepts, and creates a cultural domain of knowledge. We must therefore assume that science is shaped both by fundamental intuitive ontological principles, and by cultural concepts. In some cases cultural concepts will be in harmony with intuitive ontology, and in others less so. In the former case, accordingly, they will be very hard to separate, while it will be more apparent in the latter.

In the following part of the chapter I will develop some tools for thinking about science that try to take into account the interplay between innate and cultural structure in knowledge.

Science analysed as ABC-domains

Although it might be problematic to posit a simple causal relation between our intuitive ontology and scientific disciplines, we can say that scientific disciplines emerge and change in interaction with this ontology. It works together with other 'random' beliefs and views that emerge from social interaction and culture. Very simplified, the relation between our cognitive structures and science can be expressed in the figure below:

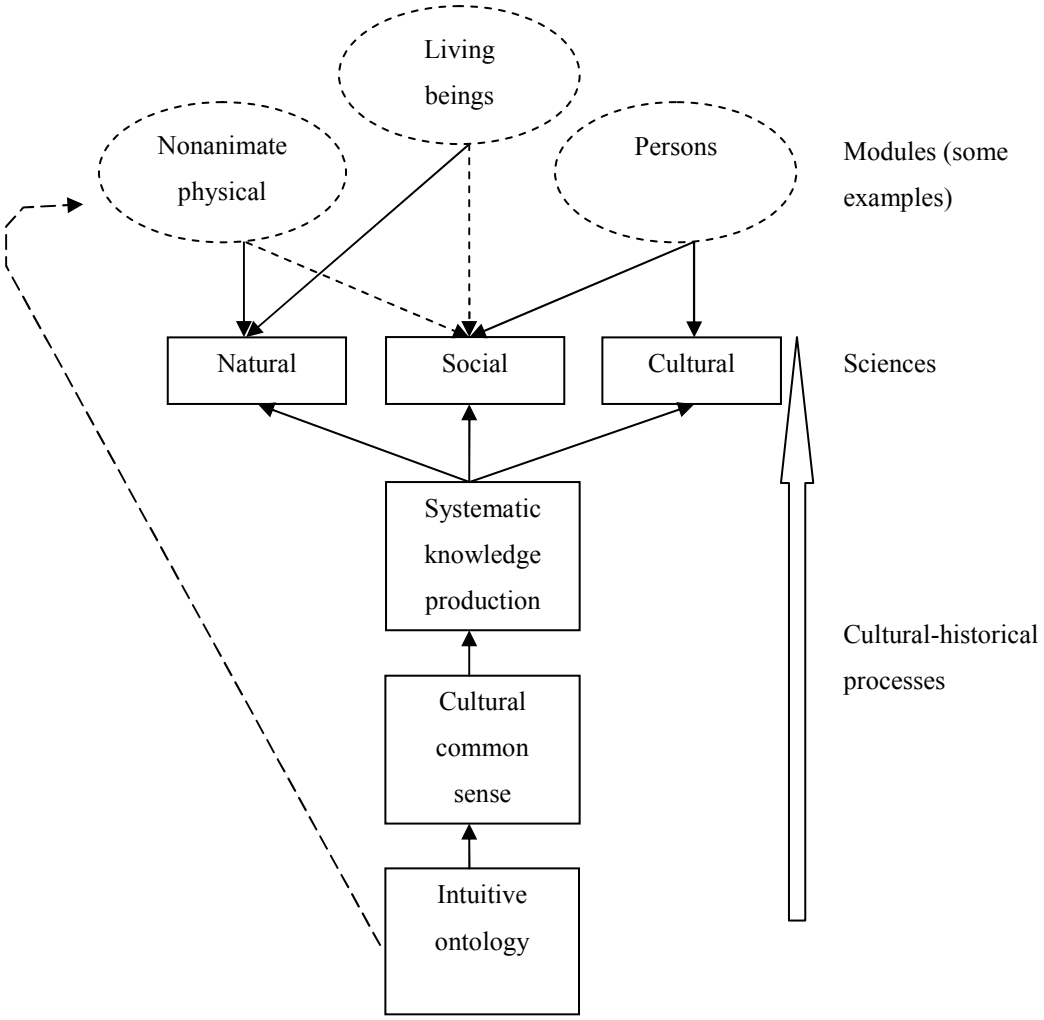

Figure 3 shows the relation between science and intuitive ontology. The stippled ellipses on top are elements from the intuitive ontology (modules) which continue to affect scientific thought, indicated by arrows. Non-stippled and stippled arrows indicate that elements can be more or less 'native' in a certain part of science, although not bound to it as such (it can be 'imported,' e.g. though metaphor). The term 'cultural common sense' is meant to indicate that common sense is not only affected by intuitive ontology, but also by cultural processes. The term 'systematic knowledge production' is meant to indicate the continuity from common sense to science.

It can be useful to recognise that modules work on several different levels, corresponding to different time scales. They have significance in the evolutionary history of the species, in the cultural history of a society, in the life-history and cognitive development of an individual, and in the micro-processes of interaction and communication.

This section is an attempt to devise a heuristic for thinking about scientific ideas and systems in a manner that respects both the fluidity and structuring factors in human cognition. It is mainly a systematic realignment of the material examined above. Johansen (Johansen 2003) has similar thoughts drawn from memetics and complexity theory, and later in this dissertation I will, inspired by him, analyse the material in terms of a complex adaptive system (see chapter 8).

Salience and structure in modules

Disciplines can be viewed and analysed as consisting of different knowledge domains. Domains develop in interaction with specialised modules, some of which are more *dominant* than others, in that the content is more *salient* in human cognition (like in the example with the rabbit mentioned above), while others are *weaker*.[7] In other words, phenomena that meet the input criteria of a dominant module will be noticed more easily than other phenomena that are handled by weaker modules. Let us imagine that there is a 'snake recognition module.' It is likely that to detect snakes has been important in human evolution, and that this module must react fast and have priority. So snakes are salient in human cognition, we pay attention to them.[8] Social situations are perhaps an even better example; many people pay much attention to what other people do, and the module(s) associated with this must be quite dominant.

Some modules organise their content in specific ways, and can be said to impose more *structure* than others, so that the content of a high-structured module will be more rigid than in a low-structured. The content of the NUMBER module will be likely to receive a stricter structure than the content of the PERSON module. Persons can belong to different and changing types of categories, friend, boss, lover etc. while numbers are related to each other in terms of simple and strict rules. Even less structured is probably the META-REPRESENTATION module which Sperber identifies.

Any composition should in principle be possible so that both high- and low-structured modules may be either dominant or weak in human cognitive terms. However, the terms dominant, weak, high-structured and low-structured are relative terms, and not absolute labels to put on a module once and for all.

It should also be pointed out that although the terms high- and low-structured seem to indicate that there is only one scale of 'structuredness,' this is not how it is meant. The modules have qualitatively *different ways* of organising their material. This is an important point, as these organising principles create contradictions and disharmony in human cognition, at least if we attempt to 'force them together,' as is the case in much scientific thought. There is also the possible case of more general 'modes of construal' (mechanical, intentional, teleological) which Keil (1994) suggests (see above, this chapter). A more fine-

[7] Although we can expect that there is some uniformity among humans with regard to which modules are relatively dominant or weak, we must also acknowledge that there may be significant variation between individuals. For instance will a trained mathematician be more alert to numerical content than many others.

[8] The cross-cultural recurrence of snakes in religions can also have a connection with this.

tuned analysis could be yielded if this was taken into account. At this point, however, I am content with this simplified categorisation, since its purpose is to facilitate the instruments of analysis described below.

ABC-domains: ideal types for analysing knowledge

With this simple categorisation of modules we can construct several types of domains according to which types of modules support them. We can imagine three ideal types of domain. A type A-domain develops strictly within one high-structured module. A type B-domain develops in one low-structured module, or possibly over several low-structured modules of equal strength. A type C-domain develops over two or more modules of varying structure and strength.

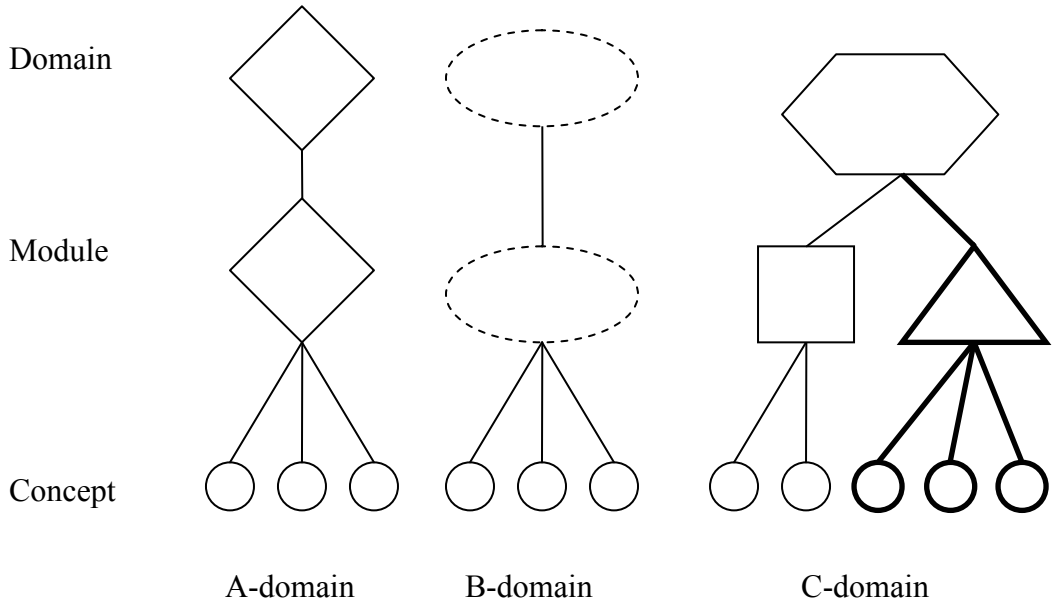

Figure 4 illustrates the different types of domain. The different shapes of modules indicate difference in structure, i.e. that they treat their information in different ways, which is reflected in the knowledge found in the corresponding domains. Dominance is in the C-domain indicated by thicker lines.

If the module pertaining to a type A-domain is *dominant*, the content of the domain will be likely to seem highly intuitive and self-evident even to people who have not been socialised into it. The content of a type B-domain will to the newcomer be likely to seem contrived and randomly put together, although it might seem 'natural' to people who are socialised into it. In a C-domain, the new student will find some parts more intuitive and easier to understand than others, and he or she might find it hard to get a holistic and integrated understanding of it.

According to the epidemiology of representations, these types of domains will be likely to evolve differently over time. An A-domain will remain relatively stable, because it is

supported by a single high-structured module which works as the uncontested selection principle within this domain. This will be the case no matter if the module is weak or dominant, but an A-domain supported by a dominant module will be more likely to be wide-spread. A B-domain will be likely to be fluid and volatile, more susceptible to fashion and 'random' social processes, since its module(s) is not particularly structured (but see below for a discussion on how also B-domains can remain stable). The C-type can behave more complicatedly according to the nature of the modules which are involved. The modules in a C-domain will work as 'attractors'[9] on representations which will orbit around these and get organised (and transformed) according to which module it is closest to at the moment. Over time, the more dominant modules are *likely* to dominate, but this is not determined. A C-domain can be sub-categorised into various different compositions, something which will be attempted later in this dissertation (see chapter 8). All of these domains are affected by the socio-cultural and physical processes which also contribute to their making.

Most scientific disciplines will probably seem to lie closer to C than A or B, and a discipline may indeed be understood as one giant C-domain. But although this is possible, it is here better to understand it as a composite of several more or less separate domains, which in turn are of various modular constitutions. These can then be analysed in terms of being more or less like A-, B-, or C-type domains.

Some reflections on the social sciences

The social sciences are occasionally accused of 'kicking in open doors' by outsiders. The fact that our results sometimes seem self-evident and banal to others could indicate that they fall within a dominant type A-domain. However, the methods by which the results are reached are often less intuitive. As an example, let us take quantitative research methods and the interpretation of quantitative data. This is material which takes time to learn, and which is perceived to be rather cumbersome by new students, but which is simple and clear for the experienced. It would seem that within a quantitative social science research project we have (at least) two domains involved, knowledge of social systems and knowledge of statistical analysis. The former is supported by a set of specialist and dominant modules to do with naïve sociology (see above, this chapter); the latter has to do with a quite weak but high-structured module that handles numbers. That this module is weak is indicated by the fact that most cultures have not developed a sophisticated mathematical knowledge domain, even though all

[9] See below, chapter 8, for more on attractors, and how I use the term in this dissertation.

cultures use numbers to some extent at least (Gelman 1999; Gordon 2004). Although each of these domains are governed by quite different principles, the result of their processes can concern the same phenomena, and this joining together of domains can account for much confusion among students, and probably also common misinterpretations of statistics. Our mind struggles to integrate different types of knowledge that has bearing on the same phenomena. It seems that if 'left alone,' it would rather have them separated in two domains (which very likely has implications for divisions in science, more on this later).

To this picture we may add theory of social systems. Some of this theory is relatively straight forward and easy to learn, like e.g. role theory. This type of theory is in relative harmony with our naïve sociology, although presented in a more explicit and systematic form. But the social world is highly complex, and can be analysed at different levels. This opens up for different types of theory which may develop in a different direction, away from the 'common-sense' type. Thus type B-domains may develop. These can be given a highly structured form, but it does not rely on a structured module in the same way as the A-domain (i.e. spontaneously). If it 'borrows' structure from a different module it is possible to view it as a kind of C-domain. It should be emphasised that a given B-domain theory[10] is not necessarily and by default 'wrong,' it might be both useful and afford a deep understanding among its users. However, it will be less likely to remain stable in the same way as an A-domain, and can more easily become victim to misunderstandings and irrelevant additions. For instance, purely rhetorical (but unsubstantiated) arguments may seem persuasive within a B-domain to a larger extent than within domains that have stronger 'guidance' from a relevant module. Such rhetorics may lean on modes of thought pertaining to a dominant module which in fact are inadequate, but is made to seem relevant by the orator. Teleological modes of explanation can stand as an example here.

A larger theoretical or philosophical system is likely to contain both A-, B, and C-domains, and to try to link them in a coherent whole. This makes the system vulnerable to the difficulties of all of these types of domains. The B-domain theory is susceptible to error both because of randomness, and because of the systematic 'pull' of more dominant modules. The A-domain theory can be too stiff and rigid, while the C-domain theory can disagree with itself by being organised by conflicting sets of principles.

[10] A 'B-domain theory' should be understood as a theory which is dominated by B-domain problems, even though the theory itself might not be 'purely' a B-domain.

The stability of B-domains in science

The fluidity of B-domains is limited by several different factors. First, the concepts which are a part of the domain change at limited speed. The concepts are correlated with physical cell-structures in the mind, and these cells require a certain time to change and grow new connections. Second, when several concepts are interconnected in a domain, they are mutually dependent on each other[11] and changing or adding concepts thus has significance for the rest, and novelties might therefore meet resistance. Third, a B-domain can be converted into physical representations (like a written text) which are very stable (even though the interpretations of them might change). And finally, B-domains seem self-evident, intuitive, and 'natural' for people who have been socialised into it, which means that evidence and opinions that goes contrary to the domain easily get rejected or ignored. The apparent intuitiveness also means that it is highly problematic for us to pronounce this or that domain or idea as intuitive or counter-intuitive. Such judgements should be substantiated by research and/or made plausible by arguments, for instance from evolutionary theory.

All of this means that a domain can be quite hard to change, even if it is a B-domain. Science is a conservative system of knowledge production, and can really be said to cultivate the processes mentioned above (connecting concepts in systems, immersing people in these systems, writing things down etc.). In the scientific world we have in addition the social and emotional processes of prestige and 'theory fetishism' which can add further to the rigidity.

Under such circumstances it seems clear that the *initial conditions* of a theory or discipline are extremely important. Errors done at an early stage can stay embedded in the theory and affect the other ideas, creating confusion and inaccuracy. The conservativeness of science is thus a two-edged sword. On the one hand it enables an accumulation of knowledge, but also potentially an accumulation of faults. The dominant modern idea of progressive development makes us believe that any field of knowledge *must* progress, which makes us blind to the possibility that it might also degenerate (Johansen 2003).

All of this clouds our judgements concerning different theories and disciplines, perhaps especially in social science where the evidence is to such a large extent a matter of interpretation. It is harder to 'interpret away' a particular reproducible experimental evidence in physics than evidence that originates in a complex social world.

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[11] They can be viewed as complex, non-linear systems where *ceteris paribus* assumptions (assumptions that the effect of a particular feature of a system can be studied simply by changing only one parameter while holding all other parameters constant) are not fruitful.

Interdisciplinarity

The problems of large intellectual systems can be seen to a large extent as the same as those that have to do with interdisciplinarity. But it is now clear that it is not only about different people thinking differently, but also about parts of our own mind that think differently from other parts. This knowledge can be adapted and applied to analyse and understand the underlying problems both of interdisciplinarity in general, and of particular research projects. Potentially such analyses can reduce the danger that conflicts between conceptual systems are allowed to take hold because they are not understood. Stepping back and seeing why views seem to clash will give participants the ability to find a way around such issues. We already knew that interdisciplinarity is hard. Now we know more about *why* it is hard. It is not just that the different subject matters themselves are too remote, it is also that our mind organise knowledge about them differently.[12] If we see linkages between fields but ram our heads into vague and 'invisible' problems, we now know more about what this may be, and should be able to overcome the problems in one way or another.

It might be that the instruments I have devised in the ABC-domains *alone* are too crude or sterile[13] for dealing with more complicated entities like a discipline. But supplied with more specific information about the modules which are likely to be involved, they can be useful. Furthermore, as we gain more and more precise knowledge about human cognition, new similar instruments of thought might be devised which enables us to analyse such complex things much better.

The ABC-domains can be seen as 'neutral' instruments of analysis. Although they should not be seen as neutral in an absolute sense, they are so in that they enable the analysis of a system of thought without necessarily evaluating its truthfulness. A-, B-, and C-domains can all be equally true or false. The use lies in identifying a domain as for instance a B-domain, something which lets us reflect on its constitution and possible weaknesses, and may facilitate us in improving our knowledge.

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[12] It is in fact extremely hard to differentiate the 'subject matter' (imagined to be external) and our mind's organisation (imagined to be internal). Our mind's structure forms reality (like Kantian categories) and it is impossible (in an absolute sense) for us to step outside these to 'see' the 'external' Ding an sich.

[13] By 'sterile' I mean that they don't in their present form include such phenomena as emotions or power which invariably will be parts of these systems.

Chapter 5: Geographic ways of thought

The purpose of this chapter is to give an account of what might constitute and characterise geographical knowledge, especially with regard to interdisciplinarity. The chapter addresses research problem 2a, *what is the intellectual constitution of the disciplined mind of geographers?*[14] To do this I have chosen to examine the intellectual history of the discipline. It should be emphasised that a historical examination necessarily must answer the question somewhat indirectly. If we want to study minds, one could argue, we need to approach people at the individual level. Here I will identify characteristics of the discipline that are likely to have effects on the mind of geographers. Thus painting with broad strokes I will necessarily miss much, but hopefully I will also identify some broad and important factors.

As it is not possible (nor desirable) to write a complete intellectual history in this dissertation, I must be highly selective. For this reason the organisation of this chapter is not strictly chronological. The first three main sections are thematic; while the following sections treat the history more or less chronologically from the discipline was institutionalised into academia. The themes chosen will be themes that I judge to be theoretically significant, and themes that have had influence in Trondheim.

The literature on Scandinavian and Norwegian geography is relatively limited. Partly because of this, much of this chapter is based on Livingstone (1992), *The Geographical Tradition,* for the early history, and Peet (1998) or Cloke et al. (1991) for later years. Since Livingstone, Peet and Cloke et al. writes with an orientation towards Anglo-American geography, this is not optimal. Although Anglo-American geography certainly has been influential in Norway, certain differences can also be found. Where texts have been found that treat Scandinavian geography, these perspectives have been added.

The main treatment of the Department of Geography in Trondheim is allocated to the next chapter. However, since the Department in Trondheim has been a part of the history of geography for the last 30 years, it would seem odd to ignore it totally here. I therefore mention the Department where this is relevant also in this chapter, but mostly rather briefly.

As mentioned, this presentation does not pretend to be 'complete.' It is true that '…geography has meant different things to different people in different places and thus the

[14] Which is part of the effort to address the second main question, *what are the roles of the hidden infrastructure of our mind in geography?* See chapter 8 for a discussion of this question.

'nature' of geography is always negotiated' (Livingstone 1992, 28). We can say that there are several histories rather than one history. My bias, then, goes towards what I judge to be of relevance for the Department of Geography in Trondheim with special regard to what may have significance for interdisciplinary thinking. More specifically the latter means the geographical tendency towards applied science, its ambitions as a synthetic discipline, and its orientation towards the visual.

Applied science and pragmatism

As we will see later in this chapter, some geographers have seen the discipline as the integrative discipline *par excellence*. However, many would agree that most of the integrative practice in geography has resulted from a long-standing and pervasive emphasis on usefulness.

It has been my impression more or less from the start that geographers tend to put more weight on the empirical rather than theory. And they tend towards wanting to solve practical problems, rather than seeking knowledge for itself. In other words, they tend towards applied science.[15]

Exploration and the scientific revolution

Modern geography did not develop from a wish to answer questions of an abstract theoretical or philosophical nature; it came out of a more practical task, namely to make sense of the suddenly radically expanded world of Western Europe in the age of discovery during the fifteenth and sixteenth centuries. Holt-Jensen (1999) chooses to term the scientific activities of the explorers *cosmography* rather than geography. It included biology, geology and geophysics as well as geography, cartography and anthropology. Before their separation into different disciplines, these activities tended from the 19th century to be organised within the geographical societies, and when geography evolved as an academic discipline, it did so partly on the basis of the cosmographic philosophy that emerged in this era (Holt-Jensen 1999).

The explorers represented a multitude of different projects, and geographic thought was thus involved in many types of ventures. Most of the explorers were not primarily concerned with contributing to Renaissance scholarship. They had motives ranging from the

[15] "Applied science" can be defined more or less strictly. In this chapter I treat it widely, including in it perspectives that are generally concerned with usefulness and being useful (i.e. not only those that solve specific problems).

passion for fame or gold to political and strategic projects. Others explored the new worlds in the spirit of religious zealotry, but mainly, it seems, the explorers were men of *action* rather than men of the word.

This is an important point, for exactly this spirit of exploring the 'real world,' instead of ancient texts, and engaging with the new instead of the old, represents a crucial move towards the anti-authoritarian ethos that characterised the new science. 'It was this spirit that brought news of new worlds and new peoples, and thereby challenged the assumption of the intellectual superiority of the Ancients' (Livingstone 1992, 37). For Livingstone and others, geography's involvement with exploration and cosmography thus puts the discipline at the very centre of the scientific revolution (Livingstone 1992).

Central to the exploration movement – and to geography – was the art of navigation and mapping. Navigational skills had a vital importance in the process which 'open[ed] up the world to European consciousness' (Livingstone 1992, 41). Maps and the visual also have certain cognitive impacts, which will be treated later in this chapter and in chapter 8.

The new science is not only empirical. The notion that science is primarily a matter of finding *better ways of doing things* seems to have played a strong role in the new science. Dominating figures such as Francis Bacon and Robert Boyle, for instance, stressed this. It might be that this emphasis was partly a matter of public relations, but still there was an emphasis on improving technologies of all types as well as improving results in horticulture, medicine or naval technologies. It is thus not only the emphasis on empirical description (as opposed to the authority of the Ancients) that is deeply rooted in the discipline, but also the emphasis on usefulness.

The emphasis on exploration did not stop at the end of the sixteenth century. Explorers like James Cook and Joseph Banks can be said to continue the practice in the nineteenth century. Charles Darwin was a contributing member of the Royal Geographical Society and, together with other naturalists such as Thomas Henry Huxley, he can be said to form part of this tradition. Men like Alexander von Humboldt, Henry Walter Bates and Alfred Russel Wallace furthermore championed the significance of scientific travel, exploring places in the Far East or South America. Up until today the Royal Geographical Society continues to sponsor expeditions as they did throughout the Victorian era (Livingstone 1992).

In the Norwegian context the emphasis was on polar exploration and polar imperialism. Fridtjof Nansen played an important part with his expeditions to Greenland and other Polar Regions during the last two decades of the 19th century. His contribution was not only scientific (he was a zoologist, oceanographer and geographer) but had also significance

with regard to Norwegian nationalism. Nansen was important as an international representative for Norway, especially during the first decades of the 20th century. Exploration in the spirit of Nansen was continued through the travels of Carl Anton Larsen in the Antarctic in the 1890s, Otto Sverdrup with *Fram* to the isles north of Canada 1898-1902, and Roald Amundsen through the Northwest Passage with Gjøa 1903-1906. As a culmination, Amundsen reached the South Pole in 1911. From 1906 expeditions were organised yearly in order to map Spitsbergen by Gunnar Isachsen, and later geologist Adolf Hoel (Jones 1999).

In the continuation of this, the activities in Polar Regions were kept up, both as a scientific and imperialistic project. A group of scientists, politicians and journalists spearheaded polar expansionism on behalf of Norway. Activities like whaling and coal mining, as well as weather research were part of this movement. Adolf Hoel was important as imperialist and ideologist, through various institutions and expeditions. In 1940 he was appointed to a professorship in the geography of Polar Regions. Physical geographer Werner Werenskiold, head of the geography department in Oslo, participated in Svalbard expeditions with Hoel 1917-1924. Expeditions like these contributed to the recognition of Norwegian sovereignty over Spitsbergen in 1920. Hoel was the main architect behind *Norges Svalbard- og Ishavsundersøkelser* (NSIU) which he led from the beginning in 1928. The organisation arranged expeditions to Greenland and Frans Josefs Land and Dronning Maud Land. Claims were made for Greenland, but Denmark won the process in Haag (Jones 1999).

It seems appropriate to see the emphasis that the Department of Geography in Trondheim puts on fieldwork in this tradition. Field courses are organised on all levels, and even though postgraduate students of geography do not have an additional semester of studies allocated for this purpose (like the students at the Department of Social Anthropology have), still many do prolonged fieldwork in remote places.[16]

Ideology, imperialism and warfare

The emphasis on applicability represents much of the underlying source of the intellectual diversity of geography. Livingstone argues that there has never really been a coherent and conceptually unified discipline. It has simply accommodated itself to different needs and interests at different times and places (Livingstone 1992). These adaptations have been of different kinds, both ideological and practical. While it seems wrong to see the ideological

[16] With the recent university reform, 'k*valitetsreformen*' ('the quality reform'), which aims towards a more streamlined and cost-efficient study progress, the ability to do field work might be affected.

functions the discipline has served as directly a matter of doing useful science, it was definitely a part of the ideology that enabled some to participate in the practicalities of e.g. imperialism and warfare.

The authority of religion

In the face of the mechanical philosophy that dominated science in the seventeenth century, many went out of their way to retain the integrity and authority of religion. One of the strategies involved likening the universe to a clock, which allowed the perspective that when one was examining the workings of nature and societies one was 'interrogating the very mind of the Great Designer' (Livingstone 1992, 351). This 'natural theology' represented, at the time, a very important way of thinking. The teleological mode of explanation that natural theology enabled was widespread and often laden with morality. What Livingstone calls 'climate's moral economy' is one example of the use such explanations could be put to. The notion was that there was an intimate connection between races and climate. One voice from this era held that the Creator had 'placed the cradle of mankind in the midst of the continents of the North … and not at the centre of the tropical regions, whose balmy, but enervating and treacherous, atmosphere would perhaps have lulled him to sleep, the sleep of death in his very cradle' (Guyot 1849, 251, quoted in Livingstone 1992, 222). So we see that geography (admittedly along with the other sciences of the age, for instance anthropology) could, by nurturing one particular way of thought, also strengthen religious and racist discourses.

Imperialism and warfare

'[T]hroughout the entire Victorian period, geography was the science of imperialism *par excellence*' (Livingstone 1992, 160). Geography was perceived by many to be vital to the imperial success of Britain as a maritime nation, and geographers participated in important discourses, e.g. concerning the question of white adaptation to the tropical and subtropical climate zones. While not all were as willing to be instruments of imperialism, it seems that the impulse towards usefulness and practicality reached across such ideological differences (Livingstone 1992).

Mapmaking is an important part of warfare, and according to Livingstone the first versions of institutional geography appeared in military schools. As is well known, geographers worked actively for government during the two world wars.

Today, military engagement is perhaps not so prominent among geographers, although 'geographical' practices like terrain evaluation and land classification continue to be performed by military engineers and surveyors. On the other hand geographers have not

stopped being useful to government. The prominent place of sub-disciplines like urban planning and development studies within the discipline is one indication of this.

Applied perspectives in the Nordic countries

Although it might be problematic to make sweeping generalisations about 'Nordic' geography, something can and should surely be said about this. Simonsen & Öhman (2003) point out that a general Nordic cooperation and interaction is certainly present. This is also the case within geography, for instance through various Nordic conferences and journals. Furthermore these countries have some other general similarities. They have had a 'smoother modernisation' than has been the case in most other European countries, and the welfare state has to a larger extent been upheld. This is part of the background against which it is possible to say something more general about these countries.

With regard to the subject of the welfare state, not only do many directly address topics to do with the welfare system, the welfare state also constitutes a tacit background for many assumptions held by geographers. Simonsen & Öhman note that the relations between the welfare state and human geography in Sweden have been particularly strong, something which resulted in a strong orientation towards planning and applied research. On this point, Swedish geography and the Trondheim geographers share some similarities, especially in the early years of the Department. Although welfare geography is not the most dominating field in Trondheim today, it is probably right to see it as the longest lasting one, sharing the first place with landscape geography. Both Asbjørn Aase and Britt Dale started their careers within welfare geography, and the 10 year anniversary publication (Jones 1986) had the title *Welfare and Environment* to summarise the two main lines of research at the department (more about this in the next chapter).

Although differences between the Nordic countries are certainly present, Simonsen and Öhman (2003) have concluded that 'in all countries the history of the subject has been marked by ideas of utility, in relation to either training of teachers or forming a basis for planning' (3). For them the emphasis on the training of teachers in Denmark, Finland and Norway constitutes part of the background for the strong position of 'landscape' as a concept which connects physical and social circumstances of regions. While this may be so for Denmark and Finland neither Michael Jones nor Olav Fjær (personal communication) recognises this description with regard to Norway. As I will mention also later in the dissertation, the focus in Trondheim shifted from educating teachers to more planning-

oriented perspectives during the 1980s, but even before this shift, landscape did not have the strong position which Simonsen & Öhman suggests.

The geography of development is another theme that is relevant in the context of applied geography in the Nordic countries. The field became part of university geography in the 1970s. Aadel Brun Tschudi had worked to establish it in Oslo during her years as a professor there (1973-1979) (Myklebost 1982), and the Department in Trondheim included the perspective from the beginning in 1975 (Aase & Jones 1986). Development geography evolved in the context of the poor countries' struggle for freedom and development in the 1960s and 1970s. This made development geography even more oriented towards normative values than Norwegian geography in general. Research problems are often defined in the context of wishing to solve fundamental problems like poverty, inequality, discrimination, hunger, disease, etc. Although the sub-discipline share many concerns with traditional geography, it is also a field that is shared by many other academic disciplines, and the approaches and methods used are often similar (Hårstad 1981).

The applied/pragmatic tendency in geography would seem to explain some of its diverse nature; it has adapted itself to various needs through history. Moreover it tells us that, to the extent that this has led to 'interdisciplinarity' (or cosmography), this is not primarily a matter of searching for the ideal holistic knowledge as a goal in itself. If geographers are interdisciplinarily oriented, it is because they solve problems, not the other way around. While this might be so, there has also been a long tradition of seeing geography as a synthesising discipline, especially in early times, but also later in its history (see e.g. Haggett 1983).

Early synthesis

Kant

Immanuel Kant has, not least because of his lectures in physical geography and his emphasis on the category of space, had a strong influence on geographers (Livingstone 1992; Holt-Jensen 1999). For him, space (*Raum*) represented a central dimension that could serve to unify our understanding of the world. *Raum* was the organising principle that holds together the multitudinous diversities of the natural world, and this dimension was in his universal science allocated to geography. Geography could thus help create a unity of knowledge from the mass of discrete data and piecemeal information of the world of experience, rendering the discipline a synoptic one. This, of course, was how Kant saw it, not necessarily how

geographers saw themselves, or see themselves today, but there were thinkers, inspired by Kant or not, who saw synthesis as geography's prime task (Livingstone 1992).

Alexander von Humboldt

Alexander von Humboldt was one of the thinkers influenced by Kant's universal science. He was also inspired by Goethe's search for a transcendental coordinating principle, at the same time as he was deeply committed to empirical methods. The Humboldtian project was characterised by the combined emphasis on synthesis and empirical description. His five year long expedition in South America resulted in enormous amounts of data and descriptions as well as a thirty-volume written production. It was during this travel that he got the idea for *Cosmos*, the 'crazy notion to depict in a single work the entire material universe' (Livingstone 1992, 136). The idea was thus to describe the entire physical universe from heaven to earth, all the time pointing to the general principles manifesting themselves in particularities.

Though the project of *Cosmos* might be 'crazy,' as has been affirmed by certain historians of geography (Livingstone 1992), this does not negate the important influence Humboldt had on geography and science. Throughout the nineteenth century his emphasis on measurement, for instance, continued to be strongly present.

His approach to the regional dimension is another example of Humboldt's influence. The functional interrelationship between organic life and local environment was an important emphasis in his writings, not least in his plant geography. Livingstone (1992) sees this holistic approach to the region in Humboldt as a crucial ingredient toward the new understanding of nature that emphasised its underlying ecological cohesiveness, as opposed to the mode of analysis based only on observable, morphological features.

Also the extended use of iso-maps he stood for has had a strong influence on later times. This comparative cartographic method that connects places of equal something (pressure, temperature) enables the different climatic zones, magnetic fields, and biogeographic regions of the earth to be outlined. The iso-map can be said to represent a way of expanding the integrating potential in maps (see more on maps below).

God as unifying principle and magical connections

Natural theology: Ritter and Herder

In pre-Darwinian geography, natural theology and teleological thinking was widespread. Carl Ritter (1779-1859) is similar to Humboldt in many respects, but unlike him he founded his thinking on religious ground. His religiosity did not stop him from having a profound respect

for close empirical observation, and like Humboldt he wanted to synthesise and see unity in diversity. As such, natural theology's point of departure, where all is seen as part of a divine scheme, gives the research a strong bias, and a strong drive towards synthesis. Ritter's project represents perhaps the last significant attempt to create a geography in this mould. He had much influence, not least in the United States, where Arnold Guyot acted as the strongest proponent (Livingstone 1992).

Like Ritter, Johann Gottfried Herder (1744-91) relied on divine planning and teleological modes of thinking. Avoiding the environmental determinism of Montesquieu, he nevertheless in his work presented an ambitious synthesis showing the reciprocity of nature and culture, environment and society. Herder was less inclined towards exploration and observation, relying instead on geographical texts and travel descriptions (Livingstone 1992).

Occultism

The Scientific Revolution certainly did not eradicate God, and neither did it wash out magical practices. On the contrary, as in natural theology, science received important impulses and contributions from such practices (Livingstone 1992; Yates 2001). Figures such as Kepler, Bacon and Newton had substantial interest in the occult, as did writers within geography like William Cuningham, Thomas Blundeville, John Dee and Thomas and Leonard Digges (Livingstone 1992).

While there is no reason to overemphasise these religious/occult aspects of the disciplinary history I find it interesting to note that the drive towards synthesis was probably in a sense much stronger in these early days, because everything could be seen as part of a divine plan, inclining thinkers to synthesise at every step. Also the various occult practices encourage one to see connections, either between heavenly bodies and earthly matters as in astrology or in the mystical meaning of mathematical relationships. So solely in terms of prominence of *synthesis,* the strictly scientific with its emphasis on analysis represents a step backwards. Furthermore, with reference to the previous chapter, it is interesting to note the extended use of teleological thinking in science.

Visual representation and maps in geography

Important in the emergence of geography was visual representation, and particularly the art of mapping. The practical importance of maps, and the cognitive impact they had, go hand in hand as symptoms of the larger intellectual trend of the scientific revolution. Maps encourage

a different type of thinking than the sole reliance on the written word. In this way mapmaking and visual representation can be seen as a subversion of the authority of the written word. Maps 'enable us to contemplate at home and right before our eyes things that are farthest away' (Blaeu 1663, 3 cited in Livingstone 1992, 98).

Many hold that the discipline is strongly oriented towards the visual (not only through maps), compared to other natural and social sciences. In this respect geography is similar to architecture and the history of art (Holt-Jensen 1999). As the above quote from Blaeu demonstrates, reflections on maps and visual representation are not a new phenomenon. Driver (2003) points out this. The idea of geography as a 'peculiarly visual discipline' has been explored by many teachers, academics and fieldworkers in geography throughout the 20th century, as well as in earlier days. Halford Mackinder described the discipline as 'a special form of visualisation' (Driver 2003, 227). 'We like to climb a mountain in order to get an overview, a grand survey of the geographical patterns in front of us' (Holt-Jensen 1999, 227).

The significance of visual thinking

The discipline's fundamental relation to maps and the visual invites us to ask some questions about this. Does the discipline, for instance, therefore attract people with an inclination towards thinking visually (as opposed to tactile or auditory thinkers)? Can the mode of visual spatial thinking maps and geography promote be said to have significant cognitive impacts on practitioners?

As has been shown by neurosurgeons and cognitive scientists, analytic, objective and 'scientific modes of thought' are associated with the left half of the brain. The right half is associated with the artistic, holistic, visual, intuitive and irrational, which make essential contributions also in scientific thought (see for instance Root-Bernstein & Root-Bernstein 2001). In the context of interdisciplinarity it is natural to emphasise the holistic component. The visualising tendency in geography could be seen in this perspective as contributing to a mode of thought that to a somewhat larger extent brings right brain, and thus synthesising, qualities into science (Holt-Jensen 1999). This will be given some further examination in chapter 8.

Maps, GIS and cross-domain thinking

Furthermore, the *specific form* of visual representation that maps represent has significance. The bird's eye view of the map gives us a tool for relating details to a totality, and as such it is

an exemplary tool for thinking interdisciplinary. As Kant argued, the category of space enables us to relate elements from different categories, and thus to integrate data that might otherwise seem unrelated (Livingstone 1992). For him, today's GIS technology would probably seem a very promising technology indeed. Part of the strength of maps is that they liberate us from the linear text, enabling a somewhat less (or at least differently) constrained type of thinking. This does not mean that maps are in some way more 'objective' or morally neutral than text. Mapmakers choose what to include and exclude, and thus create bias. And the assertive power in an official map should not encourage us to view them as politically neutral (Harley 1989; Wood & Fels 1992). For that matter, this also goes for visual representation more generally, which some have associated with maleness, and objectifying approaches (Rose 2003). Nevertheless, treated with mindfulness they do seem to give better odds for cognitive freedom and interdisciplinary thinking than does the linear text.

This potential seems even greater with GIS. The connection of digital databases with digital maps provide those competent to use this technology a much greater flexibility in creating thematic maps and spatial analysis, and thus to connect previously unconnected aspects. Openshaw (1991, 622) attributes great significance to this technology which he thinks

> offers the prospect of reversing the disciplinary fissioning process and replacing it by fusion; a drawing together of virtually all the subdisciplinary products with their multitude of conflicting paradigms created over the last thirty years within a single (philosophy-free or philosophy-invariant or even philosophy-ignorant) integrating framework.

At this point one should exhibit some caution, though. Although one might sympathise with Openshaw, the technology has its limitations. The data used in GIS are also likely to contain biases, since they often are collected for political, military or commercial uses, rather than for intellectual ends (Holt-Jensen 1999).

At the Department of Geography in Trondheim a Geo data lab has been in use since the eighties, and Axel Baudouin has been working within GIS since 1985.

Professionalizing geography

Until the second half of the nineteenth century geographical activities took place in a variety of settings, many of which I have mentioned. In Britain many different scientific institutions appeared during the early part of the nineteenth century. These were of varying quality and

also had different hidden ideological agendas. Such types of institutions served important functions in their day.

In Norway, Det Norske Geografiske Selskab was founded in 1889, when Fridtjof Nansen returned after skiing over Greenland. It became a forum where Fridtjof Nansen, Otto Sverdrup and other polar researchers could give lectures about their expeditions. A yearbook, the precursor of the Norwegian Journal of Geography, published these lectures (Jones 1999).

In Britain the Royal Geographical Society was one of the most important institutions, and in its context many valuable scientific achievements were done. The Society's members, however, consisted to a large part of military men and men of high social standing, and not primarily scientists. This gave the institution a rather dilettantish and amateurish image. So although the institution itself played a powerful role, particularly in the imperial policies of the day, this presented a problem when it came to justifying geography's role as a university discipline. The same was the case for the discipline's role as a mere attachment to overseas expansion (Livingstone 1992).

Added to this came certain important changes in the intellectual climate. When natural theology, which had formed a common context for the sciences in the early days of the scientific revolution, could no longer hold off the pressure from naturalistic science, the result was an increasing fragmentation. This undermined the main theoretical foundation of the discipline.

Furthermore, the unifying vision of Humboldt and Ritter (who both died in 1859), as well as the British Mary Somerville, had formed an important disciplinary identity. This identity was also now starting to crumble. Integrative subjects like geography seemed to lack the power and rigour that specialisation gave (Livingstone 1992).

Environmental determinism: evolutionary synthesis

To carve a place in academia geographers had to turn to a discourse that gave them authority. Accumulating isolated bits of information and arranging them on maps was not good enough; causality had to be brought in. Geography found this discourse in various versions of evolutionary theory. Livingstone stresses that this was not a purely Darwinian version of evolution; rather it tended towards a Lamarckian variety, recognising such principles as heredity of acquired characteristics. Here this is less relevant than to note what he calls 'the geographical experiment,' i.e. the wish to unite the understanding of physical and social reality under one conceptual umbrella. Even though the discipline in one sense suffered under its own inclination towards integrative science, its practitioners did not let it go. Much of the

'new geography' can be seen as a matter of dressing up the discipline's traditional concerns in an evolutionary language (Livingstone 1992).

Ratzel, Mackinder and Davis

In Continental Europe Friedrich Ratzel (1844-1904) gained a dominating position. With the help of several other geographers of the German scene, the discipline effectively carved itself a place in academia. Geography became a compulsory secondary school subject, something that promoted its expansion within the university sector. The alliance with expansionist forces continued to be part of the discipline in Germany, although the colonial project never quite succeeded here. The *Anthropogeographie* of Ratzel sought to lay the foundations of a new discipline, human geography. Inspired by Moritz Wagner, his focus was on migration and isolation as factors in the forming of species. It was an environmental determinist project, endorsing a unidirectional and unicausal link between nature and human societies.

In Britain Halford Mackinder followed a similarly naturalistically biased route in order to bridge the gap between nature and culture. He was active on the academic scene working for a stronger institutional position in Great Britain, where this process compared unfavourably with the position of geography in Germany. Also his project was largely a determinist one. Nature and geographical factors provided the primary causal factors in history. Like in Germany there was a continued effort to satisfy practical requirements, and Mackinder was an eager participant in expansionist politics. The evolutionary discourse provided him with intellectual capital, so that the theoretical requirements of scientists were satisfied as well as the practical ones of the statesman and merchant.

William Morris Davis (1850-1934) played a similar role as Mackinder and Ratzel in USA. Davis also had his background in natural science (Ratzel and Mackinder had both paid particular interest to zoology in their science studies). His main field of interest was physical geography, in which area he also applied ideas about evolution. Davis followed a similar determinist path as his European counterparts, while his strategy for achieving disciplinary independence centred on publishing elementary textbooks, popular texts, teaching, as well as involving himself with the emerging Association of American Geographers (Livingstone 1992).

During the heyday of environmental determinism, then, we see that geography retains its pragmatic character at the same time as it continues its ambitions as an integrative science.

Geomorphology's importance in Germany and Scandinavia

As geology and biology gained in ambition and status following Darwin's influential theory, German geographers succeeded in carving a niche in *geomorphology*. This became possible because geologists had turned their attention towards other tasks. This branch is identified by Holt-Jensen (1999) as an effective paradigm within geography. It lasted for half a century and advanced the discipline's reputation greatly during this period. The German Oscar Peschel (1826-75) was the first who saw the opportunity, while Albrecht Penck (1858-1945) became the most influential. Penck's work on glacial periods and glacial morphology represented breakthroughs, and his influence was particularly strong in the Scandinavian countries. Here many leading professors (generally the first who were appointed) were geomorphologists (Holt-Jensen 1999).

However this influence is less strong in a young department such as the one in Trondheim. Aase and Jones found ecological approaches of more use than geomorphological in their quest to keep the discipline together (Aase & Jones 1986). It should be noted, though, that Geir Vatne is educated at Oslo and has worked within glacial and fluvial geomorphology in Trondheim since 1994. Recently Ivar Berthling, also a physical geographer from Oslo, was employed as associate professor.

Aspects of being institutionalised

When geography was institutionalised, its form was also to some extent hardened. The fact that geography in many places became a subject in school has had a further effect on the discipline in this respect. The content of the discipline as a subject in school tempers the rate of change the academic discipline can go through.

Its status as a subject in school is part of its long-time association with power. Both history and geography could serve to develop nationalistic sentiments, and the relative importance of each discipline depended on which seemed more useful in the particular place. Germany has a history of shifting borders and divisions, and the emphasis on a geographic region of German-speaking lands became important here. According to Holt-Jensen (1999), Finland had no clearly glorious history, having in the 19th century been in a union of crowns with Tsarist Russia, and geography developed as the more important subject here. Norway, on the other hand, had been in a union of crowns with Sweden during the development of its educational system, and had no disputed borders. Therefore Norwegian history with the Vikings and the constitution of 1814 played a larger part in this respect (Holt-Jensen 1999).

However, geography played a not insignificant part in the building of Norwegian national identity. Det Norske Geografiske Selskab was, as mentioned, closely associated with the polar explorers, national heroes like Fridtjof Nansen and Roald Amundsen (Jones 1999).

Today arguments for geography as a school subject emphasise intercultural understanding and its consistency with the *World Declaration of Human Rights* and the *Rights of Children* and the recommendations from UNESCO on education in cross national understanding (Holt-Jensen 1999).

The organisation into university departments can have many different impacts on how the discipline is practiced. For instance, the size of a department can be important:

> In the late 19th and early 20th centuries university departments or institutes were small – so small, in fact, that professors had to spread themselves thinly over the broad spectrum of geographical themes (Dunbar 2001, 4).

The discipline's history, traditions and duties have certain effects according to institutional circumstances. While some of these circumstances, like the ones I have mentioned, were to a large extent general, many were no doubt also specific to each institution, and should be treated separately. The significant characteristics of the Department of Geography in Trondheim will be treated later in this dissertation. Here we go on in our presentation of the intellectual history of geography.

Regional synthesis

Regional geography is one of the other major trends in both Europe and USA. A common theme for these thinkers is a discontentment with the overly simple, monocausal tendencies of the environmental determinists. However, many of the major figures among the regionalists also had their background from the natural sciences, and were in many ways just as naturalistically biased as Ratzel, Mackinder and Davis. The main difference was the weight they put on local difference and particularities. With regional geography there was still a wish to formulate the relationship between nature and society. The synthesis was perhaps less ambitious, but more realistic. It was a regional synthesis.

There were certainly differences among the regionalists. Alfred Hettner wanted a regional synthesis inspired by Ritter, minus his basis in natural theology. In his chorology – defined as 'the explanatory investigation of terrestrial reality divided into a series of component regions' (Livingstone 1992, 262) – Hettner emphasised a systematic approach to a host of factors like geology, relief, climate, natural resources, zoogeography, settlement etc.

While Hettner, according to Livingstone, retained a necessitarian basis where the natural conditions figured as the independent factor, others emphasised human agency to a larger extent (Livingstone 1992).

Otto Schlüter emphasised the human cultural imprint on the land: the cultural landscape. He stressed the importance of understanding every aspect of human life in order to understand how landscapes had been formed (Livingstone 1992).

When Vidal de la Blache established his *géographie humaine* he saw it as its task to study how particular places provided the milieux where human *genres de vie* were shaped. He also stressed society's role in modifying nature, and an ecological-organic understanding of the relationship shaped his vision. He and his followers shaped the possibilism that has come to be associated with his name in geography (Livingstone 1992).

In Great Britain, Scottish polymath Patrick Geddes (1854-1932) had an important influence on the discipline. He received influences from many directions – evolutionism as well as Auguste Comte and French possibilism – and represented a holistic and integrative vision, something which found practical appliance in his urban planning. According to Geddes it was only in regional studies that the synthetic potential in evolutionary theory could be realised (Livingstone 1992).

Carl Sauer was another dominating figure. Working in the US, he received strong influences from American anthropologists like Franz Boas who strongly emphasised cultural relativism and historical particularism (Livingstone 1992). According to Price & Lewis (1993, 7) the school that Sauer established had as its central concern 'the interrelationships between humankind and the natural environment framed in their regional and historical contexts.'

Despite many differences among these geographers, they also had clear commonalities. They tended towards resorting to ecological metaphors or the organic. Most had a naturalistic bent, but human factors played a more active part in the picture. Still there was paid little attention to the psychological realm, and there was a clear focus on material artefacts. This was part of the strategy to retain the link with the natural sciences, its observational methods and field orientation (Livingstone 1992).

Regional geography has had a strong influence on the discipline in its modern form – according to Peet (1998) particularly around the years 1939-1953 – and its influence is still felt today, although its assumptions have also been seriously challenged (Livingstone 1992). Dunbar (2001) writes about his early days in the discipline:

> Doctoral theses often tended to be regional geographies covering both the physical and the human geography of a fairly large area, in order to demonstrate the author's ability

> to teach a wide variety of courses. As departments grew, each member of the teaching staff was still called upon to cultivate two or more regional and topical specialities. Students who went on to specialize in human geography were expected to take courses in physical geography, often for no better reason than that 'it was good for their soul' (4-5).

However:

> By the late 1950s and early 1960s, several profound changes occurred in the traditional university curriculum. The old rationale for a generalist education in the post-graduate training of geographers no longer held up. It became unreasonable to expect a doctoral candidate to hold forth learnedly on topics as far apart as plate tectonics, vegetation regions, and demographic patterns (5).

It was time for spatial science to arrive at the scene. However, this chapter affords only a loose chronology, and we will therefore first look at some more recent developments which are related to regional geography.

Regional development

In the late 1970s Doreen Massey sparked a new interest in the region and various regional processes. British economy shifted gradually from manufacturing to services as the central industry, local responses and social processes following this shift varied according to specific local experiences and ways of understanding. Local differences were therefore vital for understanding each region, and in this respect the approach continues in similar tracks as the regional synthesis (Peet 1998).

In the late 1980s 'new regional geography' became a popular term, several writers arguing for a geographical rethinking of the region. Much has been written about regions in the relatively recent geographical literature, however, a unified school or approach to regions is hard to find (Paasi 2002). Many different phenomena are studied within the region, and accordingly various theoretical perspectives are employed. Here we again encounter challenges akin to those faced in the classical regional synthesis.

> Regional worlds are increasingly complex and their origins and meanings are hidden in numerous social practices and discourses that fuse various spatial scales. Similarly, current views of 'region' and 'place' are contested and are characterized by discontinuities and asymmetries. These developments have challenged the existing disciplinary boundaries and those between regional, cultural, economic and political geography (Paasi 2002, 808).

On an abstract level, regions are commonly examined through the lens of humanistic interpretative approaches theorising the region as a 'social construction,' i.e. a region is a

contested construct that emerges through social actors' internal interactions, and their interaction with economic structures (Paasi 2002).

Much regional development studies in Norway, however, relate to applied perspectives either within economic geography or administration and planning. An important figure working within this tradition in Norway is Jens Christian Hansen. Hansen partly received his education from the French regional tradition, and has had a strong focus on empirical work. He has focused on economic and bureaucratic structures, often working on commission for political authorities (Sjøholt 2003). Tor Selstad has published extensively within regional geography since the 1970s, and contributed both theoretically and empirically. He has worked with many different themes, for instance on general bureaucratic and administrative organisation (Selstad 2003), or regional geographies (Selstad & Skjeggedal 2003). Selstad has many ties to the Department of Geography in Trondheim, both as lecturer, adviser and through projects. Paul Olav Berg, who works in Bodø, has had a focus on rural areas, with a focus on rural policy and the role of the state. Dale has contributed to regional development both with empirical studies (Sægrov & Dale 2000) and with a focus on institutions (Dale 2001; Dale 2002).

The loose chronology of this part of the chapter has been broken with the treatment of more recent perspectives on the region related to the regional synthesis. The next section examines spatial science or 'the quantitative revolution,' as it emerged after the Second World War.

Spatial science

The regional tradition received criticisms for being of too little general usefulness. In 1948 the geography programme at Harvard was terminated, partly because of the strong emphasis on regional synthesis as the discipline's essential identity. This focus gave geography a dilettantish image among other sciences that were ever more specialising. Also other prestigious universities in the US phased out geography in this period, and, unlike the situation in e.g. Britain, geography struggled with a weak position (only 1 % of the American students enrolling to study the subject). After the Second World War, North American universities were expected to produce people who could solve problems or manage complex economic systems. This is part of the background for why North American geographers spearheaded the new type of geography called spatial science (Holt-Jensen 1999).

The origins of spatial science is to be found in economic theory, with classics like Johan Heinrich von Thünen's theory of agricultural land use (von Thünen & Hall 1966) and

Alfred Weber's study of industrial location (Weber 1929) as prime examples. Geographer and student under Weber, Walter Christaller (1893-1969), and his central place theory (Christaller 1966) are held out as another great inspiration for the spatial science school in America and Sweden. The frontiers between economics and geography became a fruitful area of innovation in the 1950s. In this period the institutions led by geographers who had a background in the natural sciences were progressing particularly quickly (Holt-Jensen 1999).

The 'quantitative revolution,' which peaked in the 1950s and 1960s, entailed relying on quantitative data. This was not exactly new in the discipline. According to some the factor that changed was mainly the research techniques (Livingstone 1992), something which Cloke et al. (1991) attribute to certain technical changes in statistics, and the availability of material. Peet (1998) insists that the revolution was about a shift of focus to abstract space and nomothetic science, not about quantitativeness as such. Computer technology played an important part in this development. While it did not determine the approach, the approach would likely not have been possible without it, due to the large amounts of data used in geographical studies (Aase 1970).

The focus now shifted from the character and composition of specific places or regions (the chorological viewpoint) to a spatial analysis with emphasis on geometric arrangement and patterns of phenomena. Places were no longer seen as unique and incomparable with each other, but rather consisting of general, analysable and comparable patterns in relative space. The shift from absolute to relative space is given large significance by Holt-Jensen (1999), who follows Harvey (1969). Space as an absolute, abstract concept with no existence independent from objects has no explanatory power. In spatial science space can be measured in different ways, and is thus given explanatory power. For instance space can be measured in terms of transport costs, travel time, mileage through a transport network and even as perceived distance. This emphasises the horizontal relations between places, while typically regional geography studied vertical connections between people at a particular place and the natural conditions on this location. The relative conception of space is an essential part of the different models developed in spatial science (Holt-Jensen 1999).

The positivism of geography in this period can be seen as a reaffirmation of the naturalistic thrust of Victorian geography, and not really a deep engagement with and commitment to Comtean philosophy (Holt-Jensen 1999).

Spatial science in Scandinavia

In Sweden, Edgar Kant introduced quantitative approaches. Torsten Hägerstrand assisted him in 1945-6 and did work on migration processes. Hägerstrand's studies of innovation processes clearly broke with the current regional tradition. Lund University, at which he and Kant worked, became renowned as a centre of theoretical geography. Hägerstrand's time-space geography has been important, mainly at Lund, but also in other places. At the university in Trondheim, Hägerstrand was awarded an honorary professorship in 1997, on the initiative of the Department of Geography. This shows that his work was highly prized in Trondheim, although little research has been done applying his perspectives at the department. This is also true for Anders Löfgren, who came to work in Trondheim in 1992. Although educated at Lund, his focus is in most senses quite different from Hägerstrand's, except for a common struggle for a theoretically ambitious and advanced geography.

According to Holt-Jensen (1980) quantitative approaches arrived quite late in Norwegian geography. Examining master's theses, he found that it was not until after Hagget's *Locational analysis in human geography* (1965) that use of models and quantitative methods is clearly present, and even in 1980 it was a relatively small part of geography that had its focus on models and used advanced statistical methods. From 1970 he registers a clear interest in models for their own sake. In Oslo this tendency ended in 1974, but the amount of quantitatively oriented theses was still larger in Oslo than in Bergen. With regard to Trondheim, Aase was mentioned as one of the most clearly quantitatively oriented scholars. However, Trondheim had started their master's program in 1977 and awarded their first master in 1979 (Aase & Jones 1986), which did not give much material to work with. From what material he had, Holt-Jensen did not register any who were using quantitative approaches as the primary angle. The conclusion was that Norwegian geography never went through any 'revolution' in this respect (Holt-Jensen 1980).

Although there might not have been a revolution, Christaller's work has nevertheless had some significance in Norwegian Geography. Peter Sjøholt is one example of a geographer who has been inspired by Christaller (Hansen & Lundberg 2005). In Trondheim Nina Gunnerud Berg's work on the service sector can be seen in this tradition, although Gunnerud Berg does not generally work with quantitative methods. Similarly, her work on *innovation* can be seen as a continuation of Hägerstrand's work. Asbjørn Karlsen has also done work on the geography of service industries, as well as work on local processes of change and organisational innovation. Britt Dale has in the later years focused on economic geography

and her work on mining societies and their reconfiguration into service oriented industries can be understood in this context as well as in the regional development strain.

The links between economic geography in Norwegian geography and the regional development theme mentioned earlier in this chapter are clear. In other words, although regional development was treated before spatial science in this chapter, it should be noted that it cannot be fully understood without this background.

The interdisciplinary significance of spatial science

While early modern geography and regional geography were integrative, they were relatively isolated from the rest of the scientific community. Peet claims that '[d]espite its greater functionality human geography as science of space was still isolated from social science in general, which had a difficulty recognizing the significance of "all this theory about space"' (Peet 1998, 33). However, Holt-Jensen (1999) writes:

> It is commonly agreed that the spatial science school threw open the windows of a hitherto introverted discipline, which had had its major links with idiographic disciplines, such as history and geology. Disciplinary boundaries became much more open; methods and theories were openly borrowed from geometry, physics and social sciences as geographers became involved in multidisciplinary research projects (87).

The case in Trondheim certainly lies closer to Holt-Jensen's description than Peet's, for instance in the cases of Aase and Dale (see the following chapter). It is interesting that a clearer focus on the nomothetic, combined with an emphasis on applied science, seems to have led to a discipline more strongly integrated with the rest of academia.

Humanistic geography

Humanistic geography has various roots leading back to the European Renaissance and thus further to Ancient Greece. Certain thinkers within regional geography, notably Vidal de la Blache and Carl Sauer are also intellectually related. The approach had a renaissance in the late 1960s, when a central element was a fundamental critique of positivist geography (Cloke et al. 1991).

Although humanistic geography can be said to be a very diverse and fragmented body of thought consisting of several different perspectives, some common principles can be identified, for instance an emphasis on 'human factors,' such as awareness and agency, consciousness and creativity. In geography this particularly meant an awareness of the humanity of the studied, as well as the humanity of the researcher. Emphasis is put on places,

and how people relate to them on every level, also the conceptual and experiential (Cloke et al. 1991).

Humanistic geography can be said to contain an element of *critique* and an element of *reconceptualisation* of key geographical elements, and important in these respects were the influences received from *phenomenology* and *existentialism*. The element of *critique* was directed against geography as spatial science, and derived much of its ideas from *phenomenology*. Positivist methods were regarded as unsuitable in the study of social and human concerns. The positivists objectified people, reducing humans to 'dots on maps,' while the humanists wanted to include every aspect of human life in their studies. The phenomenological credo, to describe things (phenomena) as they are experienced, was pivotal in this respect (Cloke et al. 1991).

The *reconceptualisation* of key geographical elements involved various shifts in point of departure for the examination of geographical themes. The wish to 'restore the lived world of experience to a prominent place in our theorization' (Peet 1998, 63) renders abstract space irrelevant. To human beings the phenomenology of spatiality is imbued with personal emotion and idiosyncratic perception. Therefore concepts like *place, lifeworld* and *context* replace *space*, as the attempt is made to engage closely and emphatically with basic geographical elements in human *experience* (Peet 1998).

The relation between human and place takes a central position for many writers. Relph (1976) explored places as fundamental in human experience. An 'authentic' (deep, genuine, honest to itself) sense of place is according to Relph vital for human beings. However, certain processes in society (industrialism, positivism) undermine the process, producing 'placelessness,' where no place is special and therefore every place is equally alien (Peet 1998).

Yi-Fu Tuan emphasised people's interaction, behaviour and feelings / thoughts about nature and space / place. In *Topophilia* (1974) environments are explored as they are perceived and imagined through people's affective relations with places. Such relations are for instance actualised through aesthetic experiences and an awareness of the past (Peet 1998).

The cultural turn and feminism

'The cultural turn' in human geography designates a period in the late 1980s and early 1990s when the discipline saw an increased interest in 'soft' cultural processes and cultural theory. This was also the period when the so-called postmodern and / or poststructural modes of

thought had the strongest momentum, and much of the work done in this tradition commits itself to poststructural epistemologies that emphasises strongly the contingency of knowledge production, and the relations between knowledge, language and power. On the British scene this type of geography represented something relatively new, while in the US it was more a matter of continuity from the cultural and humanistic geography treated in the last section (Barnett 1998).

While the terms 'postmodern' and 'poststructural' are notorious for being difficult to define, important elements are the various critiques of 'modernity' (another complex concept). Such critiques include the possibility for objective knowledge production and the links between knowledge and power, and modernity's assumption of historical progression. This project was found relevant also in feminist thought, and many feminists are also postmodernists or poststructuralists. Particularly the focus on power is important in this respect.

In part the critical project of geography in this mode is directed against the strong applied tendency in the discipline and its association with power. Also physical geography receives criticism for its positivism. In this respect the cultural turn does not promote a deeper integration in geography. On the other hand, the cultural turn represents a close engagement with a widely used body of literature, and the interface with other disciplines which have embraced similar thoughts should thereby be made easier. This also goes for the interface between various geographical sub-disciplines, except those which have a positivist orientation.

It seems clear that Berg & Forsberg (2003) are inspired by postmodern / poststructural thinkers. Departing from the fact that Britain has had a dominating position in rural studies, they '*deconstruct*' British notions about rurality and gender, arguing that these are inadequate in a Scandinavian context. The use of the term *deconstruction* should not here be understood in a strictly Derridaean way, as one does not, like him, primarily seek to point to internal inconsistencies in the use of terms (Cloke et al. 1991; Peet 1998). Rather, Berg & Forsberg are concerned with the contextuality of knowledge production, and argue that the history and empirical reality of Scandinavia requires different concepts than those applicable in Britain. Berg & Forsberg are thus within the same line of thought as has characterised much of poststructural and feminist thought, like for instance Donna Haraway (1991).

In Trondheim feminist perspectives have gained a strong foothold with several writers. Berg, whose article with Forsberg I discussed above, is one of these. Lund is another such writer (see e.g. Lund 1993). Her inclination towards gender theory and cultural geography is

combined with the field of development geography. In 2005 Lund and Berg edited an issue of Norsk Geografisk Tidsskrift-Norwegian Journal of Geography featuring articles by Setten, Bye, Attanapola, Arnesen & Lægran, and Acharya which showcases the Department's interests within feminist geography (Acharya 2005; Arnesen & Lægran 2005; Attanapola 2005; Berg & Lund 2005; Bye 2005; Setten 2005).

Even though the Department of Geography in Trondheim has adopted ideas and approaches from the cultural turn, it does not seem that this has led to a serious polarisation against the positivist or applied parts of the discipline.

Paradigms?

Kuhn's concept of the scientific paradigm has been enormously popular for various reasons. Holt-Jensen (1999) discusses the Kuhnian theory of scientific development in relation to the history of geography, and concludes that there has not been a very clear correspondence. Instead of alternating periods of 'normal science' – characterised by a stable paradigm and *puzzle-solving* within this paradigm – and scientific revolutions, the 'paradigms' – or schools of thought – operate in parallel. So although a superficial look at the history might produce a picture that seems to follow the pattern, a closer look will show that often the 'new' paradigm continues to incorporate the old one. In a discipline like physics, it might be possible to identify much more clearly paradigms and paradigm shifts like Kuhn does; however, in a social science like geography old theories and approaches are less likely to be abandoned as absolutely. In a lecture in 1978 Holt-Jensen concluded that there were no actual paradigms in Norwegian geography, and that 'everything was dissolving' (Holt-Jensen 1980).

The tendency to accumulate rather than discard in geography can be taken in support of the approach in this dissertation. In this light, the choice of writing an intellectual history of the discipline seems both adequate and necessary when the purpose is to say something about how geographers think.

Concluding remarks

In this chapter I have focused on certain aspects that seem significant in relation to geography and interdisciplinarity.

- First, there is a tendency towards applied science. As we have seen, the solving of problems in itself has a tendency to end up as interdisciplinary endeavours, simply because one has to deal with real life complexities, which in turn do not respect

disciplinary boundaries. It seems that the conclusion in this chapter's section about spatial science, where applied science led to a discipline more strongly integrated with the rest of academia, confirms this.

- Second, many have seen geography as a synthesising discipline. Attempts to establish a geographical synthesis have ranged from nomothetic approaches like environmental determinism to idiographic approaches like the regional tradition. It seems somewhat paradoxical that the discipline has been relatively more isolated in academia when these perspectives have dominated.
- Third, the focus on maps / GIS and visualisation is attributed a certain intellectual significance. This is both because the use of visual stimuli is assumed to activate right-side brain processes, and because maps (and GIS) enable non-linear and cross-domain thinking to a larger extent than pure text.

If I am right that these factors have been very important in the history of the discipline, they must be assumed to be important for the intellectual development of many geographers. Therefore, I argue, such factors are relevant for addressing my research question, which concerns the intellectual constitution of geographers (question 2a). This will be discussed further in chapter 8.

The following chapter will treat the approach to geography at the Department of Geography in Trondheim, picking up on the themes of this chapter.

Chapter 6: Geography in Trondheim

The intention of this chapter is to examine the approach to geography found at the Department of Geography in Trondheim. It picks up on the themes treated in the previous chapter about the intellectual history of the discipline, at the same time as it describes specific perspectives and sub-disciplines at the Department.

First I have found it appropriate to treat the history of the department. This history is treated similarly as the history of the discipline in the last chapter, in which I choose the themes I find most relevant to expand upon. Literature on the department's history is limited. The relevant texts are the publication at the time of the department's tenth anniversary (Jones 1986) and the *Festschrift* published on the sixtieth birthday of Asbjørn Aase (Jones & Cramer 1992). Other than these, I have relied on the annual reports of the Department where these have been available. Strategy plans available for various periods in the Department's history and archived documents from department council meetings have been helpful as well. For gathering information about persons, CVs published on the internet have been useful (although it should be noted that these sometimes seem to be rather outdated). Of course the published scientific texts of the staff of the department are important sources. However, it has been necessary to acknowledge that a complete review of all these texts would be too time consuming. A selection has therefore been made. In some places I also use information from my interviews with the staff.

The history of the Department should give an impression of both the general approach to the discipline that predominates, and which sub-disciplines and perspectives that are pursued. The themes of synthesis, applied geography and visualisation are then treated with reference to various sources.

History of the Department

The Department of Geography was founded in 1975 under the then loose superstructure of the University of Trondheim, initiated in 1968. The University of Trondheim included three older institutions, the Royal Norwegian Society of Sciences and Letters, the Norwegian Institute of Technology, and the College of Arts and Sciences. The last institution dates back to 1922 when it was founded as an academy for teachers. It was transformed into a university with Faculties of Science, Humanities and Social Sciences in the 1960s.

According to Aase and Jones (1986) geography was first mentioned in a 1964 report on the structure of the new university. Geography was envisaged as a human-ecological study, linked to the natural sciences. Eventually, the Department of Geography was placed under the Faculty of Social Sciences. To begin with it was located at Lade; later it was moved to its present location at Dragvoll.

The beginning

Asbjørn Aase (born 1932) was the first professor at the Department of Geography in Trondheim. He was appointed in 1974, and the first lecture was held in 1975. Aase had a degree from the School of Economics in Bergen. He had previously taught at the Department of Urban and Regional Planning at the Norwegian Institute of Technology in Trondheim, and later at the Geography Department in Bergen. Aase now holds the position professor emeritus.

Michael Jones (born 1944) became the first permanent scientific employee (PSE) at the Department of Geography after Aase. He was appointed as senior lecturer in 1975. Jones had a Ph.D. from the University of London with a thesis in historical geography about human responses to land upheaval on the coast of Finland (Jones 1972). When he was appointed in Trondheim he was doing research at the Norwegian Agricultural University on the history of land tenure in Norway. Landscape studies and historical geography are his main interests. He became professor in 1985 and is still at the department.

Aase and Jones can be said to have laid the foundations for much of the department's later development. They both have interests within many different fields, and did not want to be constrained by narrow disciplinary boundaries. Jones states this as one of the reasons why he chose Geography as a subject.

For Aase interdisciplinarity has been an important part of his career. At the Department of Urban and Regional Planning at the Norwegian Institute of Technology in Trondheim, Aase represented a move towards an interdisciplinary profile previously consisting of architects (Jones & Langdalen 1992).

While still in Bergen, Asbjørn Aase was hired to participate in an interdisciplinary group working on the Norwegian Level of Living Study that gave its final report in 1976. This study became important for several reasons. Asbjørn Aase and Britt Dale, then a master's student, continued their work within the area and thus initiated welfare geography in Norway. Dale moved to the Department of Geography in Trondheim as a research fellow in 1975, and is now professor. The project has had a formative role for both Aase and Dale in their academic careers and through them, of course, for the Department. The interest in welfare and

level of living brought Aase into the geography of health, which is also a field operating across disciplinary boundaries. He recalls the good cooperation he had with the medical environment in Trondheim at the time when he started working within the field. Hansen (1992, 73) writes:

> Mens mange av oss andre norske geografer har praktisert vårt fag blant fagfeller, har Asbjørn Aase konfrontert sin geografi med andres sosiologi, økonomi og medisin. Hans ønske om tverrfaglig samarbeid har vært der hele tiden.

Michael Jones is described by others at the department as one of the few who are able to combine human and natural elements successfully in his studies. In 1995 he received the Cuthbert Peek Award from the Royal Geographical Society in London for 'contributions at the meeting ground of physical and human geography.' According to himself he received a much more extensive education in the natural sciences part of the discipline than they are able to give the students at the department today. As mentioned, his Ph.D. treated human adaptations to changes in the natural environment (land uplift) in Finland (Jones 1972).

Philosophy and approach in the early years

It is perhaps not surprising, looking at these two profiles, that the 'heterogeneity in background of the first two staff members was experienced as an enrichment and an opportunity for choice of directions' (Aase & Jones 1986, 11). They describe the philosophy underlying the planning of the department as 'radical-conservative,' since they wanted to incorporate newer elements within a mould that conserved important long-term values in geography. They saw it as important to emphasise geography's special role within the social and human sciences coping with factors of the physical environment. They wanted to keep physical and human geography within the same discipline, and to work against the general tendency of a separation between the two. To bridge the gap from physical to human geography they found ecological approaches more suitable than geomorphological ones (Aase & Jones 1986).

With regard to teaching, *regional geography,* with a particular emphasis on methodology, was given a large place on the basic course. Although the early courses were not special courses on planning, they sought to show that geography was relevant to planning.

In research and higher levels of teaching, three fields were focused on.

- Welfare geography was studied and related to both global inequality as well as urban and regional issues in Norway.
- Historical geography served as a starting point for a broader subject. Within this was encompassed the evolution of cultural landscapes, landscape evaluation and planning.

- The geography of development and of developing countries was the third focus. The regional focus was on Africa, since the University of Trondheim had declared its intention of developing African studies as an interdisciplinary field (Aase & Jones 1986).

Staff recruitment - the first ten years

The choices in philosophy and specialisations were the background on which the subsequent recruiting of staff was made.

Oddvar Jakobsen (born 1948) had become a research fellow at UNIT in 1974. The aim was that he should eventually introduce the geography of development and African studies into the department. He became a lecturer in the department in 1977 (Aase & Jones 1986).

Bjørn Axelsen (1947 – 1985) had a background in human ecology as well as research experience in vegetation and landscape change in Norway, Botswana and Sri Lanka. He started working as a lecturer at the department in 1979 (Aase & Jones 1986).

As Jakobsen later left the department, his position was in 1983 taken over by Ragnhild Lund (born 1951), who had done research on agricultural resettlement in the context of water development projects in Sri Lanka (Aase & Jones 1986).

Britt Dale (born 1949) had been affiliated with the department from 1975 as a temporary lecturer and researcher on different projects. In 1984 she became lecturer at the department. She worked mainly in welfare geography, especially related to methodology and urban problems (Aase & Jones 1986).

The eighties

In the early years of the department's history, students were mainly oriented towards teaching careers. As the number of jobs in teaching dropped during the 1980's, the department experienced a drop in student numbers. This led to adaptations of the courses, mainly in the master's programme. More weight was put on giving the students skills in statistics and computing and the courses in landscape and welfare became more planning-oriented (Aase & Jones 1986).

The scientific staff remained relatively stable at around 5-6 permanent scientific employees.

Olav Fjær worked as a research assistant at the Department from 1981-84. He later came back to the Department in 1994, after which he has held a 50 % position within the field of geographical didactics. He also has interests within coastal landscapes, especially with regard to settlement patterns and economic development processes.

Axel Baudouin (born 1945) started working at the department in 1985 with a three year grant from the Ministry of Environment to establish a special course in GIS and computer cartography. In addition to his work in cartography and computing, he had done research in agricultural geography in Algeria and Northern Norway (Aase & Jones 1986). Baudouin continued to work at the department after his three-year grant expired. In addition to publications within GIS and quantitative analysis, Baudouin has written and published texts within disciplinary history, focusing on Elisée Reclus (1830-1905) (Baudouin 1992; Baudouin 2003).

Wolfgang Cramer was employed in 1986 when Axelsen died from cancer. Cramer worked within ecological perspectives, like Axelsen had done. He was employed at the Department until 1992.

The nineties and up until today

In the early nineties the Department experienced rapid growth. At the end of 1990 the Department Council decided to employ Haakon Lein in the field of development geography.

Anders Löfgren came in 1992. He had been working on settlement patterns, and the geography of youth within a qualitatively oriented humanistic geography.

Nina Gunnerud Berg was also engaged in 1992, after having worked in Nord-Trøndelagsforskning in Steinkjer for a year. However, Gunnerud Berg had taught at the department already in the last semester of her master degree which was awarded in 1984. Her main subject was to be found within economic geography on service localisation and feminist approaches. Gunnerud Berg recently (2005) became head of department.

Lars Emmelin from NORDPLAN, Stockholm, was employed as associate professor II from 1992 until 1996. The professorship was made possible through a collaboration between NINA (Norwegian Institute for Nature Research) and the Department, and the field was environmental and natural resource management with an emphasis on outdoor life.

Civil engineer Jan Ketil Rød from the Norwegian Institute of Technology was employed in a part-time position as department engineer, also in 1992. Although this was a technical position, Rød later took a PhD within geographical information systems, and is now a permanent scientific employee at the department within the field of GIS.

In 1994 Geir Vatne took a position at the Department. Educated in Oslo, he works within glacial and fluvial geomorphology.

Sigmund Asmervik, architect and later professor at the department for landscape planning, Norwegian agricultural college, Ås (which later became Norwegian University of Life Sciences) held a professor II position in planning at the Department from 1994-2001.

Kenneth Olwig was employed in 1996. His interests are within landscape, history of concepts, and planning, following a cultural geography approach. Olwig left the department in 2002, but continues to collaborate with people at the department.

In 1997 Mary Edwards replaced Cramer. Her field is in vegetation geography, with research on landscape change and climate. Edwards left the department in 2001 to work at the University of Southampton (UK), but also continues to collaborate with people at the department.

Carl Christiansson was professor II in physical geography during the years 1997-1998.

Stig Jørgensen was appointed to a permanent position in 1998, having been research fellow from 1996-97. He works within health geography, welfare geography and methodology.

Piers Blaikie held a professor II position from 1998-2002 and has interests within environment-society relations in the developing world, political ecology; environmental policy in Asia and Africa.

Asbjørn Karlsen became professor in economic geography in 2003. His interests are in regional development, industrial restructuring, innovation and knowledge management, evolutionary economics, institutional theory and globalisation.

Gunhild Setten works mainly within landscape studies, following a cultural geographical approach. She is interested in concepts like nature and culture, landscape and place, morals and discourse formation. She was employed in 2003 as associate professor.

Ivar Berthling is, like Vatne, a physical geographer educated in Oslo. He became associate professor in 2004, and has interests in slope processes, periglacial areas, permafrost, climate, and landscape development.

Cathrine Brun was PhD fellow at the department from 1997 to 2002. She held an assistant professorship from 2000 to 2002, and a post doctoral fellowship from 2002. She got her permanent position as an associate professor in 2004. Her interests are in development geography, particularly in protracted conflicts and forced migration, internal displacement, and gender perspectives on young people and conflict.

Stuart Aitkin who specialises in children's geography at the University of California, San Diego became professor II in 2003, and will stay in this position until 2007.

Jennifer Hyndman from the University of British Columbia became professor II in 2004. This professorship will last until 2007, and will cover developmental geography.

Achim Beylich, a specialist in geomorphology, became professor II in 2005.

From the beginning, the department brought in teaching contributions from other parts of the university in fields where the capacity was limited, such as botany and geology. As the department later grew, this became less necessary, but is still practised to some extent.

On a general note it should be pointed out that the Department's recruitment of staff has been characterised by an international orientation. This can be seen to indicate an emphasis on academic qualities and competence rather than, as some have criticised NTNU and other Norwegian universities (see e.g. Asphjell 2006), recruiting exclusively within one's own ranks.

Fields and perspectives

This section describes fields and perspectives at the Department, starting from the strategy plan written in early 1996 for the years 1996-1998. This plan summarises of the activity at the Department at the time, and using this as a starting point, I expand and add according to the activity in the last ten years up until the present.

In 1996, then, the Department described their research and educational activities in terms of nine fields. Overarching the different fields to various degrees, six types of perspectives were also identified. The nine fields were landscape geography, development geography, medical geography, welfare geography, economic geography, social and cultural geography, geographical information science, physical geography, and geographical didactics. The overarching perspectives were disciplinary history and philosophy, geographical methods, planning, regional development, gender, and environment.

Landscape geography had in 1995 one permanent position (M. Jones) and retained its link to historical geography. The Department reported a large demand from external national and international organisations for lectures and cooperation. It concerned itself with themes like landscape planning and the effect of agricultural policies on cultural landscape. There is thus a clear continuity from the early days of the department. In addition, elements from newer trends in cultural geography had been incorporated, e.g. in research concerning landscape as ideology, landscape as a social and cultural construction, and landscape as an element in regional and national identity. These newer trends were strengthened further when Olwig was

appointed in 1996, and is today continued through Setten's work. Jones has had, and continues to have, cooperative relations with Olwig and Edwards among many external relations as well as PhD students. Jones has lately published within landscape geography as well as disciplinary history, including cartographic history. He recently led an interdisciplinary project on 'Landscape, Law & Justice' at the Centre for Advanced Study, the Norwegian Academy of Science and Letters, Oslo in 2002-2003. Gunhild Setten currently coordinates Nordic Landscape Research Network in Norway.

Development geography was in 1995 noted for having played a central part in developing the Department's international cooperation projects in Asia and Africa. The field had two permanent scientific employees (R. Lund and H. Lein) and incorporated perspectives involving geographies of the regional, the local, as well as development theoretical perspectives. Social change as a result of industrialising, agricultural development and development planning had been central. Environmental issues were in 1995 beginning to emerge as Lein led research on water management and natural disasters, and Lund approached the environment with emphasis on planning and social factors (humanistic approaches in analysis of continuity and change, place identity, gender and development, local participation in planning). In 1995 the Research Group on Forced Migration was formed, a project with many external partners from different countries and disciplines. Among cooperative relations can be mentioned that Lein has had cooperation over several years with A. Baudouin and G. Vatne. Lund is externally oriented and was for several years working as associate professor in rural development planning at the Asian Institute of Technology, School of Environment, Resources and Development/Programme for Human Settlements Development in Bangkok, Thailand**,** and now holds an honorary visiting professorship, University of York (UK). Cathrine Brun's interests are protracted conflicts and forced migration, internal displacement, gender perspectives on young people and conflict. Much of her published work is within forced migration with a regional focus on Sri Lanka.

Medical geography, more recently termed **geography of health**, has all the time been covered by A. Aase, and has had strong external links to Norwegian and international health research environments. An interdisciplinary field, drawing on disciplines like biology, ecology and Earth sciences as well as the social sciences, medical geography establishes a wide set of links to other disciplines and sub-disciplines of geography (Aase & Jones 1986). The scope includes developing countries as well as Norway and other developed countries. Here the Department contributed with coursework to the interdisciplinary health studies at

NTNU. Stig Jørgensen became associate professor in 1997 and has been working on risk research and quantitative methodology.

As we have seen, **welfare geography** in Trondheim can be regarded as a direct result of the interdisciplinary Norwegian Level of Living Study. Dale continued the tradition, doing welfare geography in a urban setting, which also was a result of an interdisciplinary program (the Urban Research Programme) (Dale 1986). In 1995 there had been a particular orientation towards welfare-oriented planning, with emphasis on theoretical approaches and methods aiming to work it into planning on county and municipal levels. The second major development had been towards qualitatively and locally oriented welfare geography, including studies on everyday life, quality of life and modes of life in rural and urban societies. Aase and Dale were joined in this field by Jørgensen in 1997.

Economic geography with an emphasis on the development of the service industry had emerged as a field of competence at the department during a few years prior to 1995. B. Dale and N. G. Berg covered this field. In 1995, central themes of study were changing patterns of production from industry to service, services localisation and regional development, the relationship between gender and service entrepreneurship, as well as changes in demand and consumption of services. Research was done on issues of altered regional division of labour (with regard to the dimensions of branch, function and gender) as a result of general economic, political and technological changes. Concrete studies were done on economic readjustment processes on the local level, and the interaction of these processes with place specific socio-cultural structures. *Gender*, *work* and *place* were central concepts. There was cooperation with other Norwegian centres for regional research. More recently Asbjørn Karlsen was employed as a professor in 2003 (PhD from Trondheim, 1999). His interests are within regional development, industrial restructuring innovation and knowledge management, evolutionary economics, institutional theory and globalization.

Social and cultural geography was in 1995 characterised by significant theoretical renewal on the international level. An increased interest in these perspectives could be detected also in Trondheim. Important themes were identity and attachment to place and region, life histories, migration, every day life strategies, qualitative local society studies, and issues around regionalisation and regional identity. Methodically this field was strongly qualitatively oriented. A. Löfgren had this as his main field, while many others at the department also contributed to it.

Geographical information science (techniques and methods for gathering, managing, analysing and production of quantifiable geographical information) was covered by A.

Baudouin and J. K. Rød (technical position, later to become scientific). This field was in 1995 described as important, and in high demand in cooperation projects with research groups from the developing world. Baudouin's projects during the years 1993-2000 were all applied or of applied relevance. As I mention above Baudouin also publishes within disciplinary history (Baudouin 1992; Baudouin 2003). Rød's later published work includes, among other themes, texts about cartographic visualisation of armed conflicts.

In general terms **Physical geography** includes the basic fields of geomorphology, climatology and vegetation geography, but the Department did not in 1995 cover these fields fully. G. Vatne covered geomorphology (glaciology/hydrology), and a new position in vegetation geography was due to be taken (by M. Edwards) in 1996. A limited amount of climatology was also covered. Physical geography in Trondheim has been somewhat low on staff. No immediate successor to Edwards was employed when she left in 2001, although Elin Silnes contributed to the field when she became a doctoral candidate in February 2002 (her interests are within soil erosion, fluvial processes, hydrology, geology and periglacial processes). In 2004 Ivar Berthling was appointed associate professor, thus strengthening the field of geomorphology. His recent activity has involved climate research as well as geomorphology. Projects involving Vatne at the Department have to a large extent been characterised by relatively clear-cut basic research in hydrology or glaciology. But some projects have also been applied or of applied relevance, for instance the projects '*Spredning av rotenon i vassdrag*' and '*Geomorfologisk kartlegging, Mahaweli, Sri Lanka'.* The same was also the case with Mary Edwards while she worked at the Department, i.e. mostly basic research with some exceptions (like e.g. the project *'Forest fragmentation, forest-grassland dynamics, and biodiversity conservation in Madagascar'*).

Geographical didactics was previously taught as an integrated part of the basic course, but is from 1995 taught in a separate course. Little research had been done in the field, but there was a waking interest among master students interested in writing theses with a didactic content. Currently there are two PhDs in process with ties to the Department. Svein Andersland writes about GIS in the school, while Per Jarle Sætre works on a comparative study on teaching plans and books in the Nordic countries. A recent publication (Mikkelsen & Sætre 2005), the first introductory textbook in geographical didactics in Norway, indicates that things are starting to happen in this field. Eight didactical master theses have been written at the Department from the first one in 1983 until present (2006). Geographical didactics can be an entry point of perspectives from e.g. psychology, which have traditionally been little

applied in the discipline. Also it represents an interesting arena for reflections on geography seen from a pedagogical perspective, as is exemplified by e.g. Sivertsen (2004).

Across these fields the Department identified several perspectives that overarched the different research and educational fields, creating the profile of the department.

Disciplinary history and philosophy was in 1995 seen as a common interest which underpinned all research and education at the Department. A pluralistic disciplinary philosophy characterised the general view, and the Department found this worth preserving. On the PhD level the Department has had (along with the departments of geography in Oslo and Bergen) a national responsibility for arranging research courses in central philosophical/methodological and substantial subjects each 4^{th}-5^{th} year.

Geographical methods had great breadth at the Department. Both quantitative and qualitative methods were practised and taught at different levels. In addition to methods common to many social sciences, subjects like cartography, remote sensing and GIS are specific to the discipline.

Planning as a perspective was sought integrated into as good as every field. The perspective stood strongest within development, landscape and welfare geography. Specific courses were given at the master's level, and many students were interested in planning perspectives. However, the Department had only a 20 % post to cover this (S. Asmervik, who was professor II from 1994-2001).

Regional development was a central perspective in most of the Department's fields of research and teaching. The perspective included development of urban system as well as rural societies, issues related to regionalising processes and sustainable regional development. Regional development theory and regional and rural politics were mentioned as fields which were weakly covered by the Department.

Gender perspectives were strongest within development geography and economic geography, but were in the process of being drawn into most fields. The perspective was integrated into all levels of teaching, and a specific course was given at the master's level.

Environmental perspectives have been part of the Department's policy for a long time. These seek to combine insights from both natural science and social science. It was emphasised that this should be done with a basis in *either* physical or social geography. The perspective can analyse how natural conditions have significance for human activity as well as the other way around, i.e. through studies of pollution or other environmental problems.

Resource management is an important aspect. Central key-words that were mentioned were *use*, *management* and *protection*. There was an overlap with landscape geography, but the perspective was also central in other fields, like for instance development geography and physical geography. The connection to GIS was also mentioned.

These perspectives have continued to characterise the different fields at the department up until today.

In later presentations of the department, both on the internet and in strategy plans, physical geography and landscape geography are presented together. This is partly because of external factors (the faculty expressed a wish for fewer categories) but also has an internal logic. Landscape geography has been noted for its ability to act as a unifying theme (Fry 2001). At present this is more a potential than a reality at the Department, though. Although some of Jones' work demonstrates this potential in the sub-discipline, the geomorphological tradition (in which Vatne and Berthling operates) has been less easy to integrate than for instance the biogeography of Edwards.

What kind of synthesis?

The Department's position on geography as a synthesising discipline is stated in the tenth anniversary publication:

> From the start, there has been broad agreement on the type of geography which the department wished to promote. We have attempted consciously to see ourselves in the context of contemporary trends apparent both within the discipline and within society at large, while at the same time maintaining a commitment to the 'traditional' strengths and values of geography as a subject. A central theme has been geography as a means of synthesis. This idea is one which has led to some discussion amongst geographers at large, ranging from those who see geographical synthesis as giving the subject its identity and meaning, to those who deride the concept as superficial, meaningless, unobtainable or a barrier to scholarship. Our standpoint, a middle position, is that the objective of geography is not to provide a total synthesis of geographical phenomena, but that the broadness of the subject gives full scope for working on borderlines between several disciplines and sub-disciplines. To be successful, we feel that geography needs to build simultaneously on two types of integrating concept. The first are fundamental geographical concepts such as location, spatial organization, regions, man-land relationships and mapping. The second are concepts shared with other disciplines, such as human ecology, resources, welfare, landscape and planning. Only a balance and mutual stimulation between these two types of concept can help avoid the pitfalls of sterility on the one hand and eclecticism on the other (Aase & Jones 1986, 17-18).

With this point of departure the *Welfare and Environment* publication was dedicated to interaction and interfaces between the different subfields of the department (Jones 1986). The

emphasis on finding links and overarching themes is also seen in the strategy plan for 1996-98 on which the previous section ('Fields and perspectives') was based. While these are self-representational texts (see chapter on methods and research), and thus not likely to emphasise division, I find they are nevertheless quite sober accounts, which seem to be in harmony with the actual practice at the Department. Although every identifiable link between subfields is not pursued, they lie as potential paths of integration. It would be artificial to follow up these links merely for their own sake, since research interests have to develop gradually in a process both of personal reflection and of dialogue.

Based on my experience with the Department, it seems that the approach described above by Aase and Jones, is in broad terms shared by the rest of the Department also today, twenty years later. A plurality of perspectives within a department is seen as an advantage, and there certainly are possibilities of synthesis through shared perspectives and concepts. But it is also important to focus on concepts shared with other disciplines, and to work externally.

Usefulness

In Trondheim the emphasis on usefulness has been strong since the beginning. It is very apparent in the following quote from the founder of the Department, Asbjørn Aase (five years before the Department was actually founded) (1970):

> […] det er bare hvis det i geografimiljøet foregår en levende diskusjon om hva som er problemer i samfunnet og hvordan man kan være med på å belyse og å løse dem, at faget kan gjøre krav på å kalles en samfunnsvitenskap (1).

My first interpretation of this sentence was that it is a very strong commitment to applied geography. It is even possible to interpret it so that any social science must be involved in solving the problems of society to deserve the title 'samfunnsvitenskap.' However, Aase does not agree with me on this. The text was written in a transitional phase from 'kulturgeografi' to 'samfunnsgeografi,' and must be understood in that context. His attitude at the time, as he recalls it, was that the discipline should be *useful*, not necessarily *applied*, and he disagrees with the notion that all social science must be applied.

Even if my initial interpretation was too strong, an interesting suggestion is that the way this sentence is put indicates that we might have to do with something of a doxa in geography as it emerged through Aase at the time. When I discussed this with him, such an interpretation was partly confirmed. This part of the text was indeed written quite spontaneously, as would be the case if we had to do with a doxa.

Aase's current analysis is that the article was influenced by three types of impulse. He had spent the years from 1959 to 1965 at the Department of Urban and Regional Planning at the Norwegian Institute of Technology in Trondheim. His perspective was through this influenced by having worked predominantly with architects in an applied field, and in the article he points out how the descriptive approach to planning that geographers had had was quite different from the approach of architects and economists. After this he had been strongly involved with the quantitative revolution and positivism, which is reflected in the emphasis on models in the article. He also had a beginning interest in the social revolution, and a wish for social relevance is expressed at the end. This is part of the background for his later engagement in welfare geography (which the geography of health can be seen as a sub-theme of). All three impulses reflect a wish for usefulness, if not always applicability. Whether the above quote can be seen as an expression of a doxa or not, the general emphasis on usefulness is there.

Aase's article has probably been one of his most influential. This view was also shared by Aase himself. The article has been, and is still, put on student's reading lists in Trondheim (in 2005 it was in the History of geography course (GEOG 1006) at the basic level), and also because it was apparently widely read at the time and is often referred to and reflected over (for instance by Holt-Jensen (1999) or Hansen (1992)). Furthermore, as Aase brought to my attention, the article appeared at a time when the discipline in general was in a fairly open state. For instance Hagget's *Locational analysis in human geography* (1965) came some five years earlier, Harvey's *Explanation in geography* (1969) had recently been published, and his *Social justice and the city* (1973) appeared only a few years later. In Norway this period of change and openness was also apparent, although perhaps not so dramatic (Holt-Jensen 1980). Part of the shift, as mentioned, was that people started naming the discipline *samfunnsgeografi* rather than *kulturgeografi* in the period around 1970. In Norwegian geography the former is oriented towards social science and the latter is oriented towards the humanities. While the corresponding terms in Anglo-American geography, *social-* and *cultural geography* are both subsumed under the wider *human geography*, the Norwegian tradition operates with no such overarching category.

The emphasis on applied geography is reconfirmed in the anniversary publication, where it is explicitly stated as a 'common denominator' of all the contributions (Aase & Jones 1986, 21). In interviews with fellows and employees at the department I find that many are interested in the discipline because of its emphasis on being useful and concretely practical.

External resources

It seems a plausible assumption that the extent to which research at a given department is funded by external resources can be seen as a measure of that department's emphasis on useful or applied science. External sources will often be actors with specific use for a certain kind of knowledge, and this will shape the research funded by these actors. Taking a closer look, however, will reveal that this is not necessarily so. External sources may also fund basic research, and this is indeed one of the main tasks of the Research Council of Norway (RCN). Norwegian authorities have been noted by Arnold et al. (2001) (in an evaluation of the RCN) for a tendency towards excessive use of 'earmarked' funds, thereby controlling the Council's activities in detail. Thus 'in a number of cases the divisional strategy is criticised by reviewers for being overly pragmatic in that they principally respond to Ministry priorities rather than setting their own priorities' (Arnold et al. 2001, 55).

This certainly says something about the general climate for research activities in Norway, and it would seem to shed light on the history of geography in Trondheim as well. However, the evaluation finds that basic research in the *university sector* overall gets sufficient funds from the RCN. Partly because of this, I have found that 'extent of external resources' is not a very good measure for applied tendencies. However imperfect, taking the close relationship between RCN and Norwegian authorities into account hints that it might still be of relevance, and below I will present some of my results.

All social science departments at NTNU rely to some extent on external sources of funding. The SVT faculty (Faculty of Social Sciences and Technology Management) has a strategic goal of an external income of at least 50 % of the salary expenses for permanently employed scientists (from now on PSE, permanently scientific employee – corresponding to the Norwegian expression *fast vitenskapelig ansatt, FVA*) at the different departments.

The generated external income as a percentage of PSE salary expenses is a relative measure that can say something about the relative propensity each department has when it comes to getting external funds. In the last five years GEO has generated a sum amounting to 84 % of these expenses; the equivalent figure for SVT as a whole is 65.6 %. As we can see from figure 3, GEO has performed above average four out of five years.[17]

[17] These figures can be found in the SVT faculty's annual reports from 2000-2004.

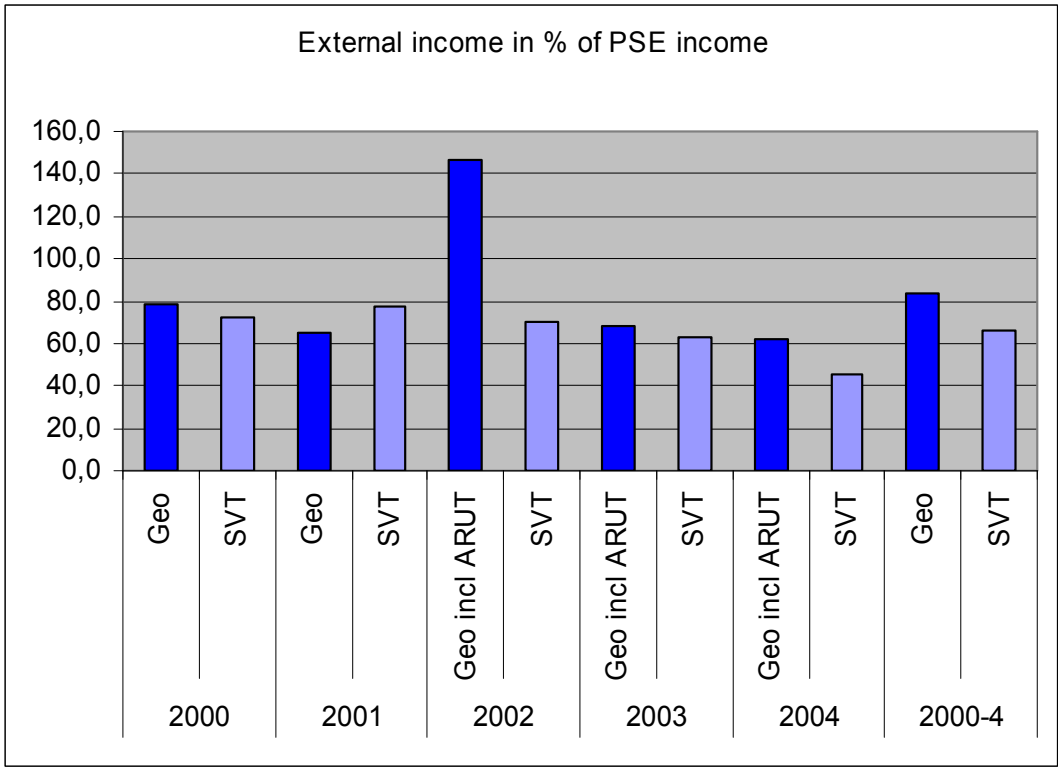

Figure 5 shows the external income generated by the Department of Geography as a percentage of the salary expenses for permanent scientific employees (PSE) at the Department compared to the mean corresponding figure for the SVT faculty.

In conclusion it must be acknowledged that while the fact that the Department performs above average when it comes to generating external income might not be a perfect measure for applied tendencies, a high research activity in itself might indicate some focus on applicablity or usefulness in a research climate such as the one in Norway.

Applied versus basic research projects

Looking more closely at the research projects of the department[18] reveals that the RCN is indeed the most prominent source of external resources. It is also clear that most of the projects are actually basic research, not applied.

From 1992 to 2000 the department reports in all 105 different projects. Many of these projects last more than one year. If we count the projects reported each year and summarise them we get 259 'project years'. Of these, circa four out of five could be classified as basic research, and so only around 20 % were applied research (only a very few can be said to fall into other categories). However, around 20 % of the research I have classified as basic has

[18] The projects are described in the Department's annual reports. This section is based mainly on these descriptions.

clear relevance for real life problems, and can therefore be classified as 'basic with applied relevance' or 'useful'.

The classification of projects is not without its problems. Some projects are large, incorporating several PhDs and master projects, and can thus incorporate both applied and basic research. In these cases they have been registered as both. Projects that have been classified as applied are the ones that clearly have to come to grips with the complexities involved with solving 'real life problems,' i.e. problems that are defined *externally to the discipline*, and which therefore are more likely to require interdisciplinary perspectives (see chapter 3). As many of the themes and sub-disciplines explored at the Department of Geography, such as welfare/health, planning and development, have their origin in and a clear orientation towards useful/applied science, much of the research will be relevant for solving problems, even though they do not directly try to solve any other problems than those defined by the discipline.

For instance I have classified a project that concerns itself with health and inequality in Finnmark as basic research with applied relevance. Such a project can clearly be useful and relevant for planners. Indeed it was commissioned by the Department of Social Affairs, and is classified as applied by the researcher himself (Aase). The reason I chose to categorise it as basic with applied relevance was because it tries to establish correlations and identify trends the way any basic research project in the social sciences does. To be sure this is a complex task with clear usefulness for authorities, but in itself it does not try to solve 'real life problems,' and is thus less likely to end up as interdisciplinary.

It is possible to divide the degree of applied versus basic research into four categories. First we have research that is commissioned to solve concrete problems. A second level could be research that is done because 'that is where the money is,' e.g. that the RCN has allocated resources to the area. Third, research can have originated from the researcher and his field, but one seeks to show its applied relevance. The fourth and last level would be 'pure' basic research which originates only from the researcher and his field.[19] Many of the projects at the department seem to fall into category two and three.

Some last words should be said about the sources used in this section, i.e. the Department's annual reports. These reports were not created on the Department's own initiative; rather the work was imposed by the faculty. Members of the Department have said that these reports were often not given a very high priority, especially since they were written

[19] This analysis was given to me by Michael Jones in one of our sessions.

at times that were generally hectic (at the beginning of each year). This is part of the background on which they should be seen, and must therefore be assumed to contain inaccuracies.

Some of the projects that appear over many years seem to be (and are sometimes also explicitly admitted to be) 'sleeping projects,' i.e. projects that were started at one point, but which are no longer receiving much (if any) attention from the project participant(s). As a picture of the research activity actually happening at the Department each year, the annual reports are therefore somewhat lacking. However, the mere existence of a given project is certainly a fact that can be interpreted and given significance in this context, insofar as it says something about intentions and interests at the Department.

GIS and visualisation

As we have seen, some have argued that geography as a discipline tends to rely on visual representation, particularly on different types of mapping. How is this reflected in the Department's activities?

Since Axel Baudouin came to the department in 1985 GIS has been a part of the Department's specialities. However Baudouin has for most of this time been the only GIS specialist among the PSE (J.K. Rød went from a technical to a scientific position 2-3 years ago), and according to himself cooperation with others at the Department is relatively limited. Later, a GIS specialist I interviewed – educated but not working at the department – stated that he had the impression that people to some extent neglected the use of maps and spatial representation in their work, often presenting tables instead.

These two statements seemed to indicate that actual practice at the Department does not confirm the impression of geography as a visual discipline described in the previous chapter. To try to test these indications I examined the *New series A*[20]. The texts were given a relatively close examination to see how many maps/photographs they included, and whether this was appropriate given the text's theme, scope and focus. Although images and graphics were registered, maps were given the most attention in my analysis.

Out of 45 examined texts 29 did not contain any map at all. However I found that this was adequate given the text's theme, scope and focus in 21 of these cases. In 11 cases the use of maps was found wanting given theme and focus. In most of these cases I did not find this to be very encumbering for the understanding of the text for various reasons. For instance,

[20] Reports and presentations published by the department from 1995 – 2000.

many in this category focused on general themes in theory or methods rather than the place itself. In such cases a description of the area of study, its location and characteristics by using words is acceptable, even though the text would certainly have profited from a map. It is more encumbering when complicated spatial patterns and complex spatial data are presented in text, table or diagram form, rather than in maps. Three of the eleven found wanting was judged to be more seriously affected in this manner.

Overall my impression was that the ones that use maps and graphics adequately do so either rather sparingly, or use small and sometimes grainy pictures. Their ability to supply 'flesh to the bone' of the texts were therefore somewhat limited. Four texts were found to be very well supplied with images and maps; however, two of these had graphical presentation as their central theme, and should therefore be expected to contain extensive imagery.

I do not feel safe in concluding that the Department as a whole neglect the use of maps and visuals, as was suggested to me. My little 'sample' cannot, as I see it, be used to draw any conclusions, but must be seen as an indication or pointer that can be made object of discussion.

Does the limited use of maps and imagery in texts at the department mean that the tendency to think visually is also limited? Concluding so is problematic, for what is presented to readers does not necessarily reflect the research process. It is possible that use of spatial and other imagery was extensive in the research process, but not in the presentation of the results. As a genre, academic texts tend not to include many visuals beyond some diagrams and figures. One could only conclude that many geographers conform to a particular genre, not necessarily that they think in a specific way. However, as an indication of focus these results might say something. Many geographers connected to the department do not seem focused enough on visualisation and maps to go out of their way to include such material.

Summary and conclusion

This chapter has been a diverse affair. I started with some notes from the history of the Department. Much of this was merely a matter of listing when different people have been at the Department, briefly describing their fields. Then I went on to describe the fields of study and overarching themes that have characterised the last ten years at the department. From here I picked up on themes from the last chapter and had a look at some characteristics of the Department's approach to the themes of *synthesis, applicability* and *visualisation*.

Beyond the obvious conclusion that we have to do with a diverse and active Department, what can be said by way of a conclusion?

Welfare geography and the geography of health have been noted for their interdisciplinary and applied character, and welfare geography has been present since the beginning in 1975. Landscape geography has also been mentioned as a field of interdisciplinary value and potential. Landscape as a concept is particularly useful for connecting the categories of nature and culture, at present, though, this is in part an unrealised potential. Development geography is definitively a field that puts weight on usefulness and applicability. Cooperation within this field has been characterised by interdisciplinarity, perhaps especially in the research group on forced migration. With three PSEs and many doctoral candidates, this field stands strong at the Department. Economic geography has gained importance at the Department since the late 1980s and early 1990s, and now has three PSEs (all professors). The field is characterised by links to both industry and the regional development tradition. Physical geography has been characterised by a somewhat unstable and vulnerable position. The current situation with two physical geographers (plus one professor II) from similar fields has strengthened the academic situation of the field at the Department in the sense that it allows greater depth, but this comes at the expense of breadth, which has become more limited. It is an interesting question whether this will lead to more or less disciplinary interaction between physical geography and the rest of the Department. The GIS part was also formerly relatively weakly represented with only one PSE. This situation has been improved with Rød's employment in a scientific position. The integrative potential of GIS was discussed in the last chapter, and it is clear that the Department has made some efforts to take advantage of the field. However we have seen here some indications that this potential remains partly unrealised at the Department. With one 50 % position, geographical didactics is the smallest sub-field at the department. While interesting interdisciplinary research can definitively be done here, this potential is largely unrealised, as not much research has yet been done in the field (this would probably require at least one full-time position). But the two new PhD projects mentioned above have improved the situation somewhat.

A paradigm?

Holt-Jensen (1980) was not able to find any clear paradigmatic structure in Norwegian geography. Looking at the varying research performed at the Department of Geography in Trondheim, it seems reasonable to repeat this conclusion. However, there are definitively some reasonably clear lines of research that have been followed over the years. The fields mentioned above undoubtedly have their exemplars of good practice which are followed by

researchers and their students. They have their preferred methods and vocabulary. If it is difficult to assimilate the picture into Kuhn's scheme, it is still possible to identify paradigm-like features of the different sub-disciplines. But what about the Department as a whole?

Epistemologically and methodologically the Department can be said to be open-ended. This can in itself be seen as a part of the Trondheim 'paradigm.' In *Geografi og samfunn*, the above examined article by Aase (1970), the applications, limitations and *complementarity* of qualitative versus quantitative methods is soberly put forth. This focus on the complementarity of things was continued in the cooperation between Aase and Jones, and can still be said to be strongly present at the Department.

Although theoretically we must expect to find a similar multiplicity between the different sub-fields, theory and philosophy is also something which overarches and link different parts of the discipline as practiced at the Department.

The emphasis on usefulness is another fairly general feature of the Department, and it seems that this emphasis is very much in harmony with the methodological open-endedness. It is also in harmony with the moderate view of geography as a synthesising discipline. Geographers can seek to bridge certain gaps between conceptual domains if this is found interesting or useful, but does not seek to provide a complete synthesis.

These features are part of the general approach to geography in Trondheim, if not constituents of a paradigm in a strictly Kuhnian sense.

In the next chapter we will examine the Department as a social system.

Chapter 7: The Department of Geography in Trondheim as a social system

This chapter is based on my experience of the Department of Geography, both as a student and through the information collected through interviews.

The Department of Geography is located on the Dragvoll campus, which houses most of the social sciences and humanities at NTNU.

There were at the time of the core interviews thirty-three persons working at the Department, apart from undergraduate and master's students. Fourteen of these were permanent scientific staff, fourteen were research fellows with office space at the Department, two were employed in technical positions, and three people worked in the administration. In addition, fifteen more were doctoral candidates located elsewhere (i.e. twenty nine people were doing doctorates within the system of the Department).

A university department has different functions which are executed at different levels. The main tasks of scientific staff are research and teaching. In addition to these, there are administrative tasks like participation in the department council and various administrative meetings. Senior members typically also have external tasks, like working in evaluation committees for the Research Council.

Research can be done individually, or in a research team. Many would argue that individual research gets more attention than research in teams, partly because individual publications is what is highest valued in a research career.

Teaching can be said to be the academic activity that to the largest extent involves internal cooperation. From the basic level to the PhD level, the Department members cooperate in different ways. Discussions on how the basic level should be taught generally involve more people than discussions on the higher levels. The basic level is supposed to present a broad picture of the discipline to the new student, and its inclusions and omissions can be viewed as an expression of disciplinary politics. It is also, however, a matter of presenting the students with a tradition, showing its history. And since geography is a school subject, students must also be given the necessary knowledge which is required to fill the role as teacher. On the higher levels cooperation involves fewer people, as the sub-disciplinary competence is the relevant factor at this level. It can be a matter of seeking advice and discussions on one's own field, or of taking advantage of someone's particular competence in a different field.

Administrative tasks also require cooperation and discussion. Although not of particular interest here, this type of activity can say something about how the cooperation climate of a department in general is.

Institutions

A characteristic of the Department today is the existence of certain institutions of a more or less formal nature, alongside the formal organisation and institutions common to all university departments.

The Working Staff Meeting (Arbeidsmøtet)

Up until early in the 1980s there had been a general stagnation, but presently the Department started growing, partly following the initiation of the master's degree in 1977. As a result of this growth, the Tuesday lunch Meeting was initiated as an informal meeting meant to include new people at the department (at this point the department included six persons). Michael Jones, who was head of the Department at the time, saw a tendency that the newer members of staff were not included in the *informal conversations* where decisions were often made. This institution is now called the Working Staff Meeting (Arbeidsmøtet), and has grown more formal, with an agenda, and minutes of the meeting. It is still informal enough, though, for discussions to go freely, and everyone at the department is welcome, although students do not participate.

In the late eighties the department grew considerably. The number of students rose in general in Norway. Partly this was due to a large cohort (grandchildren of the baby-boom children of the late 1940s) and partly to a general increase in unemployment. So there was a quite rapid growth during the last four years of the 1980s, and the Department needed more people.

When questioned about how he experienced the period of growth, and its significance for the Department, Asbjørn Aase responds:

> *Jeg opplevde den mye positivt men… mest av alt positivt, men jeg kan jo bruke ordet fremmedgjøring også, fordi at i de første årene kjente vi alle våre grunnfagsstudenter og plutselig stod vi i en situasjon hvor vi ikke kjente alle hovedfagsstudentene, vi kjente bare de vi veiledet, så det var nærmest en eksplosjon på den måten. Det ble også vanskelig å følge med hva kollegene holdt på med, men det klarte vi altså å reparere gjennom arbeidsmøtet.*

As the Department grew, then, an increasing distance emerged even between the individual staff members. People were ill informed of what the others were doing in their research and

teaching. As Aase mentions, this tendency was dealt with in the Working Staff Meeting, by having a round around the table where people told each other about current activities.

These meetings, then, are seen as important both because many important decisions are discussed and made there, and because people are informed about what the others are doing at the moment. As we will see later, it is also seen as an important arena where potential conflicts are discussed in plenum.

Although it has a form reminiscent of an ordinary formal meeting (as mentioned, with an agenda and minutes of meeting), it has no formal power, and has its origin in an informal meeting. When the formal decisions had to be made in the Departmental Board[21], they were often already discussed first in the Working Staff Meeting. A staff member mentioned to me[22] that this diminishes the influence of the student representatives who only attend the formal meetings.

Seminars

The department has in some periods arranged bi-weekly seminars, arranged by junior members or fellows at the Department. More recently, seminars have been arranged by the Department's research groups (the Department is divided into smaller research groups where sub-fields are pursued).

These seminars are not compulsory, and I was informed that the general tendency was that people only attended them whenever the subject was relevant to their own field. Accordingly the attendance varies, and is seldom all inclusive. Therefore it has currently a fairly limited value when it comes to its potential function as an arena for interdisciplinary discourse. To the extent that it functions as such, a younger member told me that the more senior members were more likely to comment on subjects outside their field, while junior ones were less likely to.

Various possible reasons for the low attendance were suggested to me. Time pressure was mentioned as a major reason. People just don't have the time to interrupt their core activities and attend to something which is perceived as irrelevant or very peripheral to their field. It was also suggested that the seminars were less attended because only junior members

[21] Recently the decision making power of the Departmental Board has been removed. Its role is now advisory, while all formal decisions are made at the faculty level.

[22] When I as undergraduate was a student representative in the Departmental Board.

arranged them. They had therefore lost status, whereas previously, when they were arranged by the seniors, their authority led to a higher status and attendance for the seminars.

There is also a short annual seminar which includes the entire Department, where for instance important strategic and disciplinary issues are discussed. This was mentioned as an arena where interdisciplinary discourse could take place, although its short duration probably makes it less significant than it could have been as such an arena. Of course, many discussions initiated here can potentially continue at a later stage.

Social life

General mood

The first time I as an undergraduate heard anyone say anything about the Department, it was that the social environment among the students at Geography was very good. Later again I was told that this was a myth. The experiences evidently vary among the students, but it is significant that Geography is one of the few departments who take their students on field trips lasting several days at undergraduate level. These field trips are great opportunities for the students to get acquainted, and thus improves on the social environment; this is a conscious policy at the Department. Field trips are not only for disciplinary education; they are also for social integration. This can serve as one illustration of how serious the social aspect is taken at the Department. The social aspect is also mentioned in the strategy plans of the department, for instance in the latest one (The Department of Geography 2004, section 2.3).

> Grunnlaget for rekruttering av studenter til Geografisk institutt skal være god kvalitet på undervisning og veiledning og et godt sosialt miljø.

Several people suggested that a good social environment was initiated already in the beginning, and that this is part of the explanation for the current situation as well. Professor Britt Dale describes the first years at the Department as fantastic. The people in the staff were all young and very socially active, and the relation to the students was very close. Asbjørn Aase would occasionally have arrangements at his place including everyone in administration and staff (and at one point even a whole basic course of students). 'Administration' would in those early days mean administrative secretary Gunhild Anderssen who was employed in 1976.

Almost everyone I have spoken to describe the social environment of the Department in positive terms. One informant pointed out that that this is now part of the Department's identity. People are proud of the good sides of their Department, and these are sometimes also

exaggerated. This informant took a somewhat critical approach to the question of the Department's good social environment:

> *Det er ikke så bra som alle sier. Det er nok bedre enn på mange andre institutter, men jeg synes det er overdrevet.*

People compete for the same resources, and as in any other social environment egos sometimes rub against each other. However, even the more critical people I interviewed, like the one above, agreed that compared to many other departments, the Department has a good social environment. This is a balanced account of the environment in a member's own words:

> *Ja, jeg synes at.. vi er selvfølgelig ikke enige i ett og alt. Og det er klart at det kan tidvis bli ting som man er uenige om, og diskuterer litt heftig. Men jeg tror at de gjennomgående er stort sett gode venner og […] har stor respekt for hverandre sånn at […] om vi ikke er enige om hver eneste ting, så respekterer vi hverandres synspunkter. Det er jo sånn jeg opplever det i hvert fall. Og […] jeg føler stadig vekk at dine kolleger vil deg vel, altså på mange måter. Selv om vi ofte jobber veldig mye […] hver for oss og blir veldig trøtte, og har mye å gjøre og kanskje blir riktig sånn selvopptatt i perioder, det tror jeg vi er enige om alle sammen. Men når det gjelder de store linjene så tror jeg vi har et godt miljø her i forhold til mange andre steder. Det opplever i hvert fall jeg.*

A younger person felt that in a way the Department had not realised that it had grown, and that they still thought they could have close relations with every student. Although other people have made it clear that the Department has indeed adapted and felt the impact of its growth, something of the early days are apparently still present to be perceived by newer employees.

Informal arenas and social structures

Social life at the Department is characterised by several informal arenas and social structures. People generally meet for coffee at nine o'clock a.m. and at two o'clock p.m. Although institutionalised, these meetings are not formal. They are of a social and relaxed nature, but work is discussed also on these occasions. Sometimes academic subjects are discussed, sometimes matters of an administrative nature. Another 'routine' social activity is the weekly wine lottery, which is reported to be very well attended.

When I asked about 'groupings,' I sometimes got ready-made social analyses, complete with categorisations, and sometimes I got a more reserved response:

> *Jeg opplever ikke egentlig, som du sier, at det er 'grupperinger'. Det er ikke sånn at stipendiatene er sammen, og førsteamanuensene er sammen, det er ikke noen sånn type grupperinger.*

There does not seem to be any clear dividing lines between groups of people. Instead there are a set of 'weaker' social structures based on different types of things. One such structure is based on career stage, one is based on disciplinary affiliation, and one is based on personal preference, i.e. personal 'chemistry.' In addition there are differences in family situation, age etc. All of this is factors that shape the Department. People in similar life situations will tend to spend more time together and have more in common, but it is not a clear picture. It is often a matter of practicality. When one for instance has small children or lives outside of town it is simply more difficult to meet up with people. So none of these structures create groupings that are particularly marked, stable or of day-to-day importance for the social environment.

Handling disagreements

In the academic world of today money is scarce, and people have to compete for resources. People generally want to strengthen their own academic field, and the allocation of resources at the Department is by no means without problems and disagreements. I was not interested in conflicts as such in this study, and there is no reason to go into detail here. I was, however, interested in how conflicts were handled by the Department as a social system. To understand why a stable social system is stable, it is interesting to see how conflicts are handled.

At the 10-year jubilee in 1985 Tore Lindbekk said that the department had a well-developed ability to see conflicts coming and to work preventatively on this. Asbjørn Aase recollects being somewhat surprised by this statement, because it had not been a conscious strategy. The Working Staff Meeting (here also termed '*mandagsmøtet*,' the Monday meeting) can be seen as an institutionalisation of this characteristic:

> *Vi har jo som sagt de der arbeidsmøtene vi da, annenhver mandag. Og jeg tror kanskje at de kan ta brodden av en del konflikter veldig tidlig. Altså at ting kommer opp som saker og at man gjennomdiskuterer ting. Og jeg vet at mange andre institutter misunner oss de der mandagsmøtene våre. Altså at man har mye mer sånn korridorløsning og man […] står veldig steilt mot hverandre. Og det har jeg vel nesten ikke opplevd her, på alle disse årene. Det har vært veldig lite sånn type konflikter som ikke er blitt tatt opp i plenum og funnet en løsning som folk har kunnet levd videre med. Jeg opplever […] at om det bygger seg opp til noen ting, så blir det som regel løst veldig tidlig.*

Discussions in the Working Staff Meeting thus work preventatively on conflicts. When problems are discussed through and through on this arena, people might still not agree with each other, but at least they have received a better understanding for how the others think about it. As the informant tells us, this takes the edge off of many conflicts at an early stage.

On the other hand, others have emphasised the Department's tendency to under-communicate problems. Conflicts are, according to some, simply not given any overt expression. Instead they lie implicit in daily interaction, subtly forming it. This was experienced differently by different people. One informant found that it was mainly positive, since many conflicts of a personal nature are not very easily dealt with, and it was therefore better to hide them than to create a potentially large problem. Another experienced it as a frustrating affair, and would rather such issues were up in the open.

'Små drypp nå og da' – disciplinary integration

So what about the Department's integration as a disciplinary unit? Some agree that the situation can be described by the word 'fragmentation' (*fragmentering*), at least when it comes to research. Some informants answered pretty clearly 'no' to the question of whether one could say that the Department formed an academic community. Here I would get slightly different answers, no doubt because of people understanding differently the term *akademisk fellesskap*, which I have translated as *academic community* here. The term is relatively abstract and vague.

Although on a superficial level it seems clear that the disciplinary integration is quite poor, the picture changes somewhat when we take a closer look. When asked to comment upon the academic community Nina Gunnerud Berg answered:

> *Altså vi har jo gjort et forsøk på å dele oss inn i forskergrupper da, fordi at vi kunne identifisere grupper som hadde like interesser. Ja det fungerer jo mer eller mindre, og kanskje mer eller mindre i perioder. Men det har jo vært hele tiden Britt og Anders som jeg har jobbet mest sammen med, sånn helt konkret, både i undervisningssammenheng og i forskningssammenheng. Men jeg synes jo at jeg har hatt veldig mye faglig utveksling med stipendiatene hele tiden. Også litt utenfor eget felt som u-land og landskap og sånn, for eksempel når det gjelder kjønnsforskning. At vi har hatt noe felles der… selv om akkurat fagfelt har vært veldig forskjellig. Og i metodedediskusjoner, mer sånne grunnlagsdebatter og sånn. Og da har jeg jo faglige diskusjoner med veldig mange på instituttet. Det er veldig vanskelig for meg å ha faglige diskusjoner med Geir altså, om naturgeografi, det stopper seg på en måte sjøl, men innenfor samfunnsgeografien så…*

She continues when questioned further about whether or not it is possible to speak about a good intellectual integration at the Department:

> *Både ja og nei. På mange måter er det vel det, vi har jo mange av oss fellesinteresser i grunnlagsdebattene. Det er mange av oss som er ganske teoretisk orientert, interessert i fagfilosofi. Men det er klart at det begrenser seg også litt selv med konkret prosjektsamarbeid, og det kunne vi kanskje hatt mer av, burde vi kanskje hatt mer av, jeg vet ikke. Men det er litt sånn at i den travle hverdag så er det begrenset. Jeg tror kanskje at folk utenfor universitetene tror jo at folk på universitetet de har så veldig*

mange faglige samtaler og diskusjoner, mye seminarer internt og sånn, og det tror jeg er ganske overdrevet i forhold til hvordan realitetene er. Vi har jo ikke all verdens med seminarer, vi gjør jo forsøk på å ha en intern seminarserie gående, og det har jo stort sett fungert, da. Men det er litt sånn tida altså som er problemet da. Vi er veldig fanget i praktiske ting, og undervisningsadministrasjon, alle de oppgavene som fakultetet pålegger oss. Det tar mye tid da. Men samtidig så oppdager jeg og […] at, når jeg er ute, at for eksempel nyheten om en god ny metodebok sprer seg fortere blant oss enn kanskje ute i instituttsektoren for eksempel. Og det tror jeg er fordi at vi driver med undervisning og hovedfagsveiledning så vi er nødt til å ha en viss faglig oppdatering. Så vi tipser hverandre litt sånn altså, 'har du sett den boka, nå har den kommet' og så videre. Som kanskje ikke de fleste forstår ved faglig samarbeid, men tross alt, det er litt sånn drypp nå og da. Og det er viktig.
Så jeg tror i hvert fall det er veldig viktig […] når det gjelder sånne undersøkelser som du gjør, så tenker jeg i hvert fall at det er noen sånne ting man må få med seg, og ikke bare se hvor mange prosjekter samarbeides det om og hvem er med på hvilke prosjekter og sånn. Det er faktisk en del sånn små drypp nå og da som kan kalles faglig samarbeid.

Berg points to many different things that bind the Department together such as the common interest in philosophical debates and in methods, and how the education of students leads to discussions and cooperation of various sorts. Although admitting that the practice of seminars and free academic discussions is not very prominent, she emphasises the importance of '*små drypp nå og da*,' or *occasional 'drips'* of academic cooperation. She also hints towards the limits of this inter-sub-disciplinary integration.

Physical geography

Geir Vatne is at the time of the interview one of two physical geographers in permanent positions at the Department (the other one was Mary Edwards), and points to the disadvantages of his situation:

Det er noe med at flere hoder tenker bedre enn ett, og det er godt å få tilbakemelding på egne tanker. Og det får jeg jo ikke her. Har ingen her som kan gi tilbakemelding på mitt fagfelt. […] De har ikke teoretisk bakgrunn til å si noe om mine konklusjoner. Selv ikke Mary står nærme nok faglig til det.

Physical geographers in a Department dominated by social sciences thus have a challenge, and need to orient themselves outwards to get sufficient intellectual input on their own field. But to a certain extent this is the case for the rest as well. Cathrine Brun observes that the different sub-disciplinary groups at the Department are rather small in comparison to other academic communities, and people have to take the consequence of this:

Instituttet er en kjerne som har kjempemasse samarbeidspartnere, altså alle som jobber her har kontakter med andre fag på NTNU og andre universiteter i Norge og mange andre universiteter utenlands, og det er likeså viktig [som samarbeidet internt på instituttet] det, tror jeg. Altså når vi ser på de små faggruppene som er her... vi er

et lite miljø. Geografifaget handler veldig mye om at vi bruker instituttet som base, og jeg tror veldig mange trives godt her, og det får du sikkert inntrykk av når du intervjuer folk.

The problems concerned with having a department with both physical and social/cultural geography are recognised by everyone, and is discussed regularly and openly. It seems that many (not least the younger ones) find that it is a strength that the Department remains a mixed department. It is one of the things which distinguish the discipline, but it is also a thing of real academic significance. Brun reflects on this:

Og jeg har jo hatt et utbytte av geografien som syntesefag i forhold til det å gjøre feltarbeid, for eksempel, altså det du har lært i grunnutdanningen din. Jeg har gjort feltarbeid i et område der det jeg har sett på har hatt veldig store miljømessige konsekvenser, og da er det jo veldig fint å ha litt peiling i grunnutdanningen din som gjør at du forstår det.

Similarly Elin Silnes, a PhD fellow in physical geography, expressed that her studies in Sri Lanka was made easier, at least on a practical level, by being able to ask people at the Department (which has several Sri Lanka experts) about local conditions.

Even though Geir Vatne, as we have seen, finds the situation challenging in some respects, he has also found that his interests have expanded, and he has been involved in projects where the interface between the physical and the social side is explored.

More generally the availability of expertise was mentioned as an advantage of the diversity. A younger employee told me that one of the good things about the department is that if you find out you want to work within a different field from your own, it is very easy to approach people at the Department and have them teach you.

The availability of every Department member's expertise to every other is general, but not without its own inner structures. In addition to the ones that obviously are more likely to emerge because of the actual expertise and who consults whom, reciprocal patterns emerge. It is easier to contact the person for whom you have already done a favour. Also more informal relations such as for instance giving the occasional tip about this or that develop reciprocally. If no return favour comes, the flow of information will stop or grow weaker.

Teaching

Teaching and evaluating students, especially on the higher level, constitute an arena for disciplinary discussions. Nina Gunnerud Berg again:

Og så er det [faglig samarbeid] ikke minst i hovedfagskommisjoner, i vurdering av hovedoppgaver. Det blir faktisk noen faglige samtaler da. Over en hovedoppgave, og man kan få diskusjoner om hvordan ting bør vurderes fornuftig. Og når man er sensor. Og når man veileder på hovedfag eller doktorgrad at man spør hverandre litt til råds.

Tar inn en kollega til en veiledning når man vet at noe spesielt skal opp som man vet den personen er spesielt god på.

On the lower level the issues concern more how the discipline should be represented in a balanced way to new students. Although I know that some issues have been discussed quite heatedly here, my impression is that this has been more about details and the general organisation of the teaching, rather than the actual content. At least I was told that there was very little disagreement as to how the discipline should be presented outwardly when an information brochure for new students was created in 2000. So, at least in broad lines, the Department agrees on what the discipline looks like.

Some conclusions

Up until now, I have tried to represent the Department in a neutral manner without letting my own conclusions come to the forefront. These concluding words represent how I see important features of the Department at the present stage.

My impression is that the Department is an institution that started off in a particular mode, and because it has functioned well it has continued in more or less the same patterns. Socially it is generally friendly and relaxed, and with a tendency to under-communicate certain types of conflicts (particularly personal ones). It is also a socially quite active group with much informal interaction. All in all it is characterised by an orderly and fair organisation with room for discordant views. The Working Staff Meeting can be seen as an institutional manifestation of this. Disciplinary cooperation is institutionalised in sub-disciplinary groups and organised through the various tasks around teaching as well as different research projects. But there is also a more fluid and less visible type of cooperation which happens in more informal arenas. This said, the emphasis seems to be on social interaction rather than academic.

At this point I would like to re-emphasise that this text is mainly meant as a theoretical contribution. This does not mean that I consider the empirical part to be unimportant or seriously flawed. On the contrary, considerable work has gone into the empirical research, and I feel that my conclusions are relatively well founded in systematic examination and reflections on this material. However, I will suggest that the reader does not focus too much on the correctness of my empirical descriptions and conclusions. I know that some of my readers will have considerable experience and knowledge of the Department. The methodological choices and selection of sources that has had to be made is bound to have led

to some omissions or inaccuracies, and for some it will thus probably be easy to find things with which they disagree. What I hope is that my approach is found valuable despite possible weaknesses in the empirical description.

The next chapter will broaden the scope, and try to integrate the information and perspectives found in the above chapters.

Chapter 8: Discussion and analysis

It is time to draw some threads together. As a general background on interdisciplinarity, I described its different aspects in chapter 3. In chapter 4, I suggested a way of understanding scientific knowledge in the light of contributions from cognitive science and cognitive anthropology. Then I examined the discipline of geography, trying to identify some of its characteristics as an interdisciplinary discipline in chapter 5. Finally, in chapters 6 and 7, I considered the Department in Trondheim, trying to place it in relation to some of the general conclusions about geography in chapter 5, and to describe its social and institutional context.

In what ways can all of this be tied together? The approach applied in this thesis can be divided into four stages, some of which have already been pursued:

1. Connecting our psychological microstructures to scientific thought,
2. connecting them to geographic thought,
3. connecting geography at the Department to geography in general, and
4. seeing it all in the context of the socio-scientific system of the Department.

The first stage was carried out in chapter 4, mainly by introducing the epidemiology of representations and the ABC-domains. The second stage will be pursued in this chapter, first by examining geography in light of the ABC-domains,[23] and then by supplying these insights with the clues we got from the history of the discipline described in chapter 5. The third stage was performed in chapter 6, when I examined to what extent geography at the Department corresponded to the general features identified in chapter 5. The fourth stage will be pursued in this chapter by expanding the description to include the concrete context of geography in Trondheim. Here we will include both factors from cognition, disciplinary thought and from the Department's cultural and social system.

To facilitate this linkage, particularly the last stage, I want to suggest treating the material in terms of a complex adaptive system (CAS). Here I follow Johansen (2003), who treats cultural phenomena in terms of a multi-layered, hierarchical system of ideas. A similar approach can also be found in Niklas Luhmann's writings (Kneer & Nassehi 1997). The level

[23] Ideally I should now make a detailed analysis of geography. However, as hinted earlier, there are limitations to how detailed such an analysis can be at the present time. Our knowledge of the mind is limited. It is an enormously complex subject in itself, just like geography and its relation to western culture is very complex. Our treatment of the discipline must therefore be rather general and rudimentary, rather than specific and detailed.

of analysis will be that of *knowledge* in a very broad sense. Ideas, attitudes and academic knowledge and institutions are treated as parts of a system of representations. These representations form clusters and interact in the manner of a complex adaptive system. Such an approach, I would argue, is entirely compatible with Sperber's epidemiology of representations, and the ABC-domains. It is therefore well suited to reach a more integrated description.

At present I regard this application of complexity theory as tentative. It is entirely possible that other and more conventional approaches will yield us similar or even better insights. There are, however, two significant advantages to applying complexity theory in this context. One is consistency. The varied themes that we want to include in the analysis can by employing the abstract and very general system-theoretical terminology be integrated relatively seamlessly. The second is as a reminder. As I will argue below, parts of the social sciences tend to fall into thought patterns that treat social structures and similar 'reified' concepts as though they were things, and not complex processes. Applying complexity theory should help us minimize this tendency.

A complex adaptive system is a system with elements which are 'strategically interdependent.' The altered behaviour of one element affects how neighbouring elements act, which means that the system is unpredictable and non-linear. Unlike a linear system, which moves towards a point attractor, complex adaptive systems move towards a so-called 'strange attractor,' which is the hidden outcome of the system's many interactions. We cannot know exactly where such a system is heading, on account of its richly interconnected mode of operating. The attractor is thus not something in itself, but rather an emergent result of the many processes which lead towards it.

However, if it is generally true that a CAS is unpredictable, we can nevertheless achieve an idea about where it is heading by examining and analysing it, and by using our intuition. Thus, the strange attractor can, as it were, be made 'less strange.' This indicates that the difference between a linear and a non-linear system has partly to do with 'the eyes that see,' rather than being a clearly defined and permanent property (Bohm & Peat 1989). A system can be more or less predictable according to the stand-point from which we try to understand it. For instance, it is an argument in this dissertation that by combining knowledge from the cognitive sciences, the intellectual history of geography, and a systematic examination of the Department of Geography, we are able to make the system of a geography department more predictable, if not completely so.

Looking closer at the representations of a social system we find that it consists of several different types of sub-systems, each with its own strange attractor. By characterising these sub-systems and their interactions we can get an impression of the dynamics of the larger system, and how it is integrated. In other words, we can see that a social system has several sets of ideas, norms and ideologies that each can be viewed as having their own internal logic and development towards a strange attractor. They also affect each other, so that one sub-system can alter the course of another.

We can say that these types of systems are organised by attractors, which emerge through selection processes in the manner described in the epidemiology of representations.[24] An attractor organises the elements in its influence zone so that they are analogous in some way. Modules, concepts and domains can all be seen as attractors.

Now, let's draw the line to chapter 4. There I put up some models where we saw a hierarchy of concepts, modules and domains. Concepts were subordinated modules, which were subordinated domains. We could add a hierarchical level of social norms above this level so that domains are subordinated a set of social norms (e.g. norms to do with *honesty*, which is important – but not exclusive – to science). However, this hierarchy should not be understood as a strict command-line hierarchy where domains are affected by norms and not vice versa. What goes on in the domains can also affect the norms.

In the following we will arrange attractors in a hierarchical system, like the hierarchy described in the above paragraph. But since the system we treat is so richly interconnected and fluid, a very formal and fixed organisation seems artificially rigid at this point. It seems appropriate to keep our model flexible since our subject is so complex. Therefore the type of hierarchy we get is adapted to what we are interested in at the moment, and which attractors are seen as sub- and supra-ordinate may vary according to our interests at the moment. For instance, if we focus on a knowledge domain X, the cultural value Y may function as a supra-attractor on X. If, on the other hand, we are interested primarily in Y, we can see X as the supra-attractor. Both attractors have a mutual influence on the other. Below, an abstract hierarchical system of attractors is illustrated.

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[24] As I mentioned above, an attractor is not something in itself, but emerges out of the micro-processes of a CAS. But as such it can be seen as an abstract representation of those complex processes and the tendencies they create, which is what I do here.

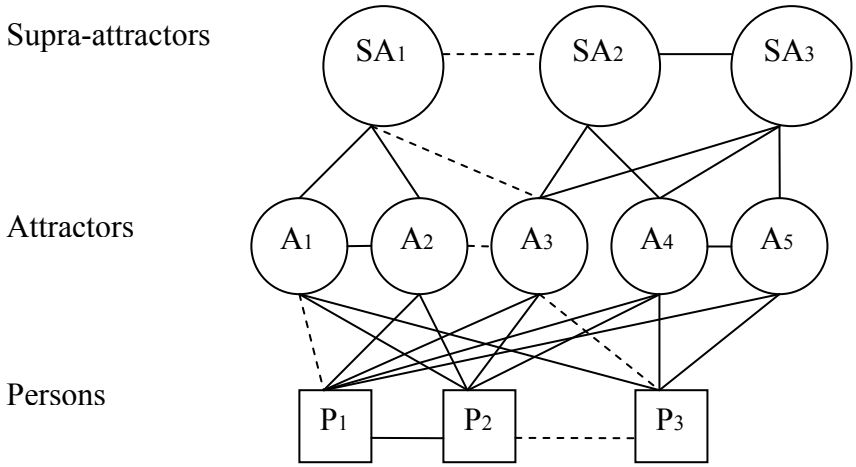

Figure 6 shows a simple hierarchy of attractors and supra-attractors over the personal level. We can imagine that the first attractor level (A1, A2 etc.) represents individual domains within a discipline, and the supra-attractor level are different modules. The SA-level may however also be sub-disciplines, cultural characteristics like a certain ideology, or whichever higher level attractor one wants to include. It is also possible to expand upwards and add an even higher level of attractors. We can thus construct an analysis that is as fine-grained as we need it to be at any given moment. Stippled and non-stippled lines indicate that the influence of a given attractor on another attractor or person may be more or less strong (or even non-existing, which is indicated by a lack of line).

The number of levels included will be a matter chosen according to our purposes. For instance, when dealing with a discipline we might first identify separate domains, and then try to find which modules that work as supra-attractors on these domains. We can limit our analysis to these two levels only (and it is this kind of analysis that the ABC-domains are designed to do), or we can expand downwards to the analysis of personal manifestations of these domains, and we can expand upwards to broader cultural characteristics that may work as higher level supra-attractors, even to some extent on modules.

To describe the cohesion of the Department it will be recognised that some attractors work either to hold it together or to bring it outwards. These supra-attractors can be understood in terms of 'forces' that work on people and perspectives. *Centrifugal forces* work to bring people, ideas and things outwards into other domains, while *centripetal forces* hold things together within Departmental structures (Klein 2000).

Analysing geography

In the analysis of geography I will progress from an analysis of the discipline in terms of being a particular type of C-domain, to a wider analysis of it as an intellectual system formed by historical processes, and finally to an even wider analysis of geography in a specific context, the Department of Geography in Trondheim. Thematically it gets wider the more

specific we get, because the specific case requires a more heterogeneous set of factors to be understood.

Geography as a discordant C-domain

In chapter 4 we saw that some modules correspond to existing academic disciplines like biology, psychology, sociology and mathematics. A possible (and I think plausible) hypothesis in these cases is that the modules have been active components in the historical processes that have joined and delimited them as disciplines.

In geography this situation is quite different. As we have seen in our short intellectual history of the discipline, a host of rather practical issues tied to historical circumstances such as the age of reconnaissance, colonisation and military conflicts were formative in early phases in the history of modern geography. This led to its practical approach, and to its composite, and some would say fragmented, constitution.

In the case of geography the role of the modules has more likely been as splitters, rather than joiners. To link different domains requires much *effort* if those domains are governed by incompatible or very different modules. For people to make this effort it is necessary that they are motivated. This motivation can come from the need to solve a practical problem or the intellectual drive to solve a scientific problem, but interdisciplinarity as a goal in itself is unlikely to provide this drive.

To put it very simply, the first type of discipline, like biology and physics, resemble A-domains, while geography resembles a C-domain. A discipline like literature studies can probably be said to resemble (or even to be) a B-domain, as can some types of anthropology. As a description of complex systems like disciplines, this is probably in most cases far too simplistic,[25] but it nevertheless captures something. A better way of describing disciplines is to identify them as different types of C-domains. Thus we can say that the A-type represent relatively *focused C-domains*, centred on a particular high-structured (and possibly dominant) module, while the B-type can be called *congruent C-domains*. Geography and similar heterogeneous intellectual systems are *discordant C-domains,* including several dominant modules with different structure. As such, geography will be subject to the difficulties of such domains. It can be disharmonious, and in danger of *splitting*, since its domains seek to stay apart. The disharmony also means that dominant modules exert *pull* on concepts in circulation

[25] A possible exception is B-type disciplines, which may possibly be said to represent true B-domains as described in chapter 4.

within the domain. In the terminology established at the beginning of this chapter, the attractors pertaining to different modules work as supra-attractors on the attractor of a knowledge domain.

'*Split*' and '*pull*,' then, are two of the main processes present in discordant C-domains. The first is obviously present in geography. A gap goes between physical geography and human geography, and at many universities the two live separate lives in different departments or even faculties. In addition to the various historical processes which also play parts in such cases, this should be seen against the background of the difference between innate structures developed to deal with the physical and human world respectively.

But what about the 'pull' factor? Even if human geography is likely to rely on modules that have to do with understanding and interpreting human behaviour, it is mainly the cultural interpretive part of it which really cultivates this. Some interpretative approaches can probably be said to be congruent C-domains or even to be B-domains. In social geography and the social sciences, the search for general features has led to ways of dealing with human phenomena that are quite different from the cultural interpretative approaches. The regularities found in the social world are '*reified'* when translated into social structures, and thus seen as stable 'things.' In human cognitive terms the temptation is large to start treating these in accordance with the intuitive principles that guide our thinking about all things in physical reality. Eikrem (2005) argues that both mainstream economy and social anthropology contains such biases. The various social institutions are construed approximately like elements in a simple physical system, which invariably seek equilibrium and balance. Such theories find it difficult to deal with the fluidity or dysfunction of social systems as well as the inclusion of human agency.

Some of this is reflected in the various efforts of thinkers, like Giddens, who try to reconcile structure and agency in social theory. In geography, such efforts were followed by writers like Pred, Gregory and Thrift (Cloke et al. 1991). For Pred (according to Cloke et al. 1991, 117) this means that:

> any place or region expresses a process whereby the reproduction of social and cultural forms, the formation of biographies, and the transformation of nature and space ceaselessly become one another at the same time that power relations and time-specific path-project intersections continuously become one another in ways that are not subject to universal laws, but vary with historical circumstances.

Part of the difficulty with this kind of project, I think, lies in the fact that structures, being 'thingish,' have a strong tendency to be drawn into the thing-module. This constantly hampers

the need to treat them like processes, since things according to this module must be treated in terms of being stable and unitary, conforming to continuous paths (if in movement) and subject to mechanical causality (see above, chapter 4).

The project of nomothetic social science can be seen as an attempt to import the methods and success of the natural sciences into the social. However, on the wagon came not only its methods and law-finding ambitions, but also the intuitive principles that supported them. In the case of the natural sciences these principles were in relative harmony with its subject matter. In the social sciences this relation was more problematic.

Although I have simplified quite a bit in the above construal of the social sciences, I think that the general idea is valid and important to take into account even if one might see the structure-agency problem as solved. As long as the modules to do with physical reality are allowed anywhere near this type of theory (and it might be unavoidable that they are) problems of this sort will be likely to remain, at the very least in the education of students. The best way to deal with problems that are unlikely to go away (like e.g. the role of the researcher in qualitative research) is to be aware of it, to reflect upon it and to make it part of the analysis. I therefore think that it is vital that the achievements of cognitive science are adapted and included into the theory of science. Particularly disciplines like geography should include reflections such as these into both the education of students, its self-reflexive processes, and in interdisciplinary research.

As mentioned above, I find the use of complexity theory in the present part of this thesis useful, partly because of this. Complexity theory very clearly emphasises the unpredictability and fluidity of the things we are dealing with, and this reduces the likelihood of such mistakes.

I think the above analysis demonstrates the usefulness of the ABC-domains as instruments. Analyses of this type can be applied to different sub-disciplines or even smaller domains in geography.

Geography: a wider analysis

In chapter 5 I identified three characteristics of geography as a discipline, the emphasis on usefulness, synthesis and the visual. How can these characteristics be included in the analysis? The ABC-domains are here too narrow, and we need to expand the model, in accordance with the approach outlined at the beginning of this chapter.

Usefulness, synthesis and visuality can be construed as high-level supra-attractors, that all function in different ways to tie together their various sub-attractors. In the history of the discipline they can thus be said to have been centripetal forces.

Let us first examine visuality. Although visual processing is a specific evolved propensity of the human mind, just like the modules, it is not in the same way domain-specific. Phenomena from many different domains (physical, social, biological etc.) have visual components which enter through our eyes. The processing of visual data accordingly includes a variety of areas of the brain. From the eye, signals travel through a specific part of the thalamus and into the primary visual cortex. From here it spreads out into the cortex where many distinct areas (around twenty to thirty) treat different types of visual information. Different aspects of objects such as colour, form and movement are treated quite separately at various points in the brain, and are distributed over large areas of the occipital, parietal and temporal lobes. And yet, all this widespread activity is somehow combined to form a seamless perception of the visual world (Bear et al. 2002).

Part of the synthesising potential in the visual can maybe be found here. Since so varied information enters through our eyes, and since all this information has to be organised into a whole, there must exist ways of relating this very different information. In other words, within the process of visual cognition there is a strong 'synthesising attractor', and this is likely to be reflected in disciplines which focus on the visual.[26]

If the assessment of geography as a visual discipline is sound, and if the understanding of visual processing above is correct, we should be able to see this reflected in the conceptual system of the discipline. Of course, we already know that the discipline is a discordant C-domain, but we can see it also in the inclination of geographers to employ very *complex concepts* (Jones 2001b). Concepts like *landscape*, *nature*, *environment*, *region*, *space* and *place* are used in many different contexts, and imbued with varying, although most often overlapping, content. They represent attempts to capture the complexities of the world, and we must therefore expect them to behave differently from narrower concepts (they are the C-domains of the conceptual world). Again, I don't want to imply uni-causality, but it seems an interesting and plausible suggestion that this can in part be seen as a result of a visual approach to reality. It would be interesting to see studies of the historical development of

[26] But clearly this is likely to vary from discipline to discipline. First, there are different ways of approaching the visual (one can focus on the purely aesthetical for instance) and second the concrete focus of a discipline must be taken into account.

concepts in the discipline with particular regard to the role of the visual in the modus operandi of geographers. It is easy to imagine that the explorers who operated in the early history of modern geography sought to gain overview, and that this necessity favoured a visual approach. Finding an elevated point to see more, drawing a map, identifying different features in the landscape, seeing spatial relations, and describing and drawing how things look like, were probably effective approaches to gain overview quickly. Visuals probably also represented effective means of communicating findings to audiences back home. This was continued in the tradition of using maps and images, which is central to many geographers also today. Both because the bird's-eye view of the map gives overview and may include elements of social and cultural as well as physical nature, and because of the nature of visual processing this focus is still likely to activate certain processes that encourage cross-domain thinking.

Usefulness is in geography a general and widely established socio-cultural value that will tend to disrespect divisions like those nurtured by our intuitive ontology (divisions which I think partly are embedded in the academic disciplines). It motivates the establishment of projects which will in turn be likely to encompass, and work as a supra-attractor to, different domains. These domains have also various modules as their supra-attractors, and a vital dynamic in such a project will be the interaction and possible antagonism between the project and the modules.

Synthesis alone is, as we have seen, unlikely to provide enough motivation to transcend difficult intellectual boundaries. The emphasis on synthesis could thus seem to represent a rather weak supra-attractor. But this will be highly dependent on the context. In one perspective synthetic approaches are difficult, and thus less likely to succeed. So from a usefulness-perspective synthesis is often *less* attractive (and usefulness can in such cases function centrifugally rather than centripetally) (Sperber 2002). But if a project is recognised as useful from the start, matters stand differently. The synthesis ideology is now in harmony with usefulness, and can be mobilised as a powerful discourse to create motivation, to gain funding and so on. In the same manner it can also be an important centripetal force in the context of a whole department. It acts as a partly disciplinary and partly ideological attractor which together with usefulness legitimises both the discipline and its various projects.

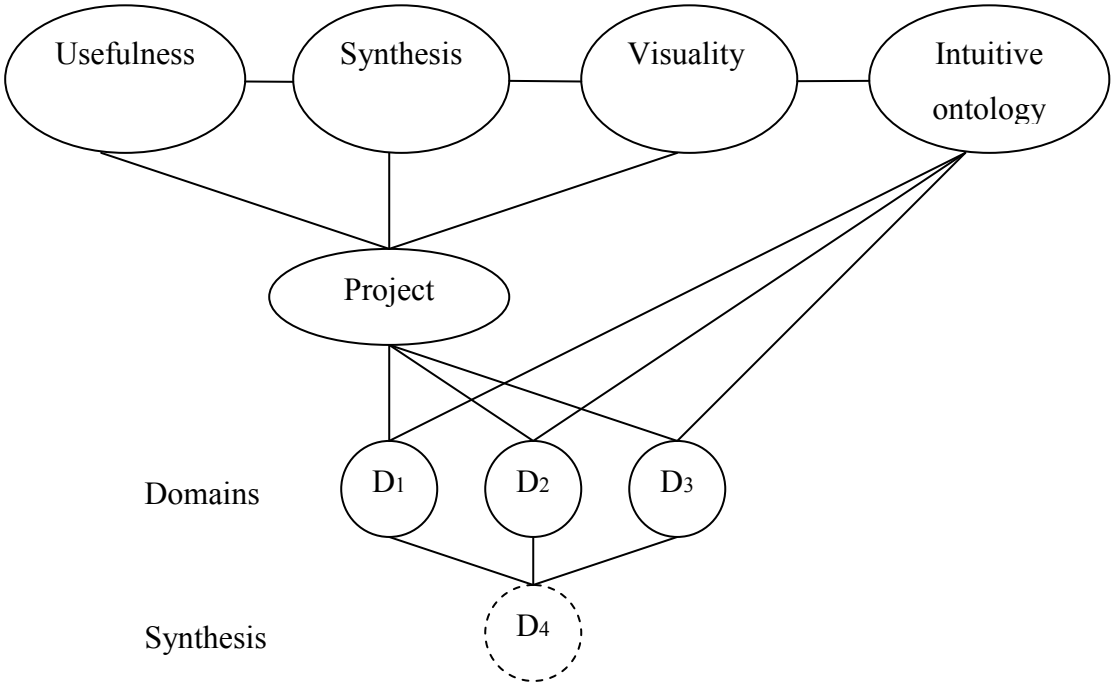

Figure 7 shows some simplified interactions between different attractors in geography. The new domain, D4, contains elements from the three original domains. Note that D1-3 continue to exist and receive influence from the intuitive ontology after D4 is created, and will also likely continue to exert influence like split and pull in D4 (if it happens to be a discordant C-domain). Note also that the culture-historical processes involved in the emergence of D1-3 are not included here.

To gain a general understanding of domains that result from such individual projects one can depart from the ABC-domains, but to understand *why* such projects emerge, we need a wider perspective. This is what I have attempted to provide in this section by including prominent factors in the intellectual history of geography.

The Department of Geography in Trondheim: a specific and even wider analysis

The third main research problem I wanted to address was: *what does the theory concerning mental infrastructure and the intellectual constitution of geography look like when dealing with a concrete context?* Generally I think they should be seen as forming parts of a system which incorporates a broad array of additional attractors. In any given geography department, the processes discussed above will presumably be at work. Operating within a discordant C-domain, the department will experience various manifestations of the split and pull processes described above. Visuality, usefulness and synthesis will as central features of the discipline also be likely to materialise. But the concrete context in which they occur will be highly significant for how such processes unfold. The split-processes will for instance probably have a different effect in a department where there is a good social integration, than in a department that is socially fragmented.

In the history of geography there are many factors that have contributed to keeping the discipline together (acted as centripetal forces). We can mention attractors like its identity as a unique discipline, its status as a school subject, and the common institutional organisation within a relatively sheltered ivory tower. These issues have been significant, but are given less attention in this dissertation since they are not particularly unique to geography. Such factors are more interesting when dealing with a concrete case. This is where my examination of the Department of Geography at NTNU comes in.

The Department of Geography

In order to treat the Department as a complex system we need to identify its different attractors. Some of these are ideas or attitudes that are shared by many people internally, and can be said to be part of the Department culture. Others are part of the organisational reality of the university as well as the Department. We can give short-hand labels to a variety of attractors. We have already treated usefulness, synthesis and visuality. We can further identify idea clusters and values such as tolerance, pragmatism, philosophy of geography, friendliness, eclecticism and individualism. Of a more organisational nature are processes that pertain to bureaucratic organisation, time pressure and department size. Factors pertaining to our intuitive ontology are likely to be present as very high level supra-attractors affecting all of the above in some way. Below I will try to sketch some of the interrelations between these different attractors in terms of their functions as centrifugal and/or centripetal forces.

The view of geography as a concrete, practical and useful discipline seems to be a quite strong cluster of ideas. This has got roots in the history of both the discipline and the early attitude of Department members. In the previous section this was construed as a centripetal force, but it seems clear that this is not inherently the case with this attractor. In order to be useful geographers may seek alliances with other disciplines that are perceived to have greater attractiveness for state authorities, e.g. economics, medicine or sociology. Within such constellations geographers are probably to some extent encouraged to under-communicate their own disciplinary identity, and to orient themselves outwards.

I think it is fair to say that the members of the Department are relatively pragmatic. This value fits well with several factors, for instance the epistemological openness and usefulness. While I think that there are several factors that are more important to understand the relatively harmonious and peaceful conditions at the Department, I think that pragmatism can also help here, because a pragmatic attitude predisposes researchers not to create unnecessary conflicts, for instance. Thus it can work centripetally. But also this attractor is a

two-edged sword. It is likely that this idea can be mobilised to argue for splitting up the Department. The impracticality in having physical geography at the Department could thus figure as a strong argument.

Geographers' epistemological openness and their broad and often eclectic orientations towards methods and theory that frequently come from other disciplines may lead geographers to seek alliances with more specialised disciplines like botany or sociology. This seems likely to function centrifugally, and can be seen as a part of (a sub-attractor of) the pragmatism already discussed.

What seems to be an important centripetal attractor is the interest in the philosophy of geography at the Department. Not only does it work centripetally by inviting to see the Department in a larger historical perspective, to some extent it also seems to be able to convert the potential centrifugal processes of pragmatism and usefulness into centripetal processes. By including usefulness and pragmatism within the disciplinary identity the process is subsumed under a powerful supra-attractor. So even though they might work centripetally on a superficial level, they also constitute reconfirmations of disciplinary identity.

The tolerance or respect for other people's perspectives can also be seen as part of a wider philosophy. It is an attitude that has been present at the Department from the very beginning through Aase and Jones, and which is a conscious philosophy. As hinted above, it is however also possible to see this tolerance as a result of pragmatism (it is for instance often easier (more pragmatic) to keep silent if you disagree with someone). But more significant, I think, is the way tolerance fits with the friendliness-attractor.

Friendliness seems to me to be generally one of the most important attractors at the Department. People at the Department assume that the others are friendly, and send out signals of friendliness in return. I'm not suggesting that everyone is friendly always, but friendliness does seem to be highly valued. We can see this in different things, for instance the emphasis that is put on field courses for students, the social arrangements like the wine lottery and the common coffee breaks. That some have felt that conflicts are silenced can also be interpreted as a wish to uphold a friendly atmosphere. Friendliness, I think, is at the basis of much of the Department's success, also with regard to the scientific aspects. As we have seen, the extent of cooperation between sub-disciplines is limited, and some agreed that on the research side, the Department could be characterised as fragmented. However, as I have noted above, interdisciplinarity is unlikely to be a motivation in itself. To overcome the difficulties with crossing domains academics need to be motivated primarily by research interests (I'm

not saying there are no other types of motivation, but I imagine this kind is better than for instance money). Interdisciplinary cooperation has little point in itself if unrelated to specific problems. Such a 'blind' cooperation is also not in harmony with the pragmatism-/usefulness-attractors at the Department. But, as we have seen, the relatively close-knit social environment together with arenas of cooperation like teaching, facilitate a different type of academic cooperation which consists of 'små drypp nå og da.' This increases the likelihood that common interests will develop, which can lead to interdisciplinarity. The garden-metaphor seems appropriate. If the necessary conditions for intellectual growth and cooperation are present, they have a possibility for growing and flowering. Such things cannot be forced, just like one cannot force the roses to grow (or to not grow thorns). They do that by themselves if the necessary conditions are present, and in the academic garden I think friendliness is important.

With regard to visuality, we remember that my research indicated that the use of visuals is not very prominent at the Department. If we assume that the indications I found concerning this is broadly correct, it seems that the synthesising attractor of visuality does not stand very strong at the Department. However, it can certainly be so that it exerts influence through the ones who *do* employ visuals to a significant extent. And if I am correct in suggesting that the visuality of the discipline has played a significant part in the formation of central parts of its conceptual apparatus (its complex concepts), one could say that it indirectly holds an influence on the Department through this as well.

The individualism inherent in the university system works centrifugally through giving an inclination to working by oneself, rather than in teams. For instance co-authored articles are not as highly valued as articles written alone when a person's academic quality is evaluated.[27]

The divisions and categories of the academic world are of significance at many different levels in the system. The organisation of the system is to a large extent a reflection of these divisions, so that there might be formal difficulties in running a geography department. A lack of understanding of the unique needs of a geography department from colleagues in different discipline or from the administration makes it more difficult to run

[27] One can sometimes get the impression that such superficial evaluation criteria, notably the number of publications, are attributed more significance than the originality and significance of the research itself. With the recent university reform quantitative criteria are actively used to evaluate the departments. Such pseudo-criteria for evaluating academic production seem more likely to lower the quality of research rather than the opposite.

such a department. As I have suggested above, our intuitive ontology is likely to be strongly present as a reinforcer of these categories.

Another important centrifugal process is the limited size of the different sub-disciplinary groups at the Department. The necessity of getting input from people who share your specific interests spur researchers in all fields to find people from outside who can give this kind of input. As such it is not a geography-specific phenomenon, but the small groups of the Department seem likely to provide extra significance to such processes.

A matter which often came up during interviews was the scarcity of time. The teaching and administrative burden put on scientists combined with the pressure to publish as much as possible (quantity seems higher valued than quality), clearly affects the quality of *all* research done, not only the interdisciplinary research. But since boundary-crossing projects generally are more difficult to complete successfully, it seems likely that these are particularly likely to suffer.

In the context of all of these different attractors the influence of our intuitive ontology might seems weak, subtle and abstract. However, exactly for this reason I think it is powerful. Since it is present at some level in every context and thus must be difficult to spot, its influence must be large, even if it is malleable to some extent. It also seems likely that in systems where pragmatism is strongly present it is easy to be persuaded by thoughts and actions that seem intuitively right, and thus likely following the 'tenets' of our intuitive ontology. Although this might be adequate in many situations, it is not necessarily so always. If we want to practice interdisciplinarity or keep the discipline of geography together we need to be aware of these things. In this, I think, lies much of the value of this thesis, because becoming aware of these underlying structures can help us gain a deeper understanding of our own processes of knowledge production.

The highly interconnected way this system operates makes it hard to give a complete and systematic representation in this text (which is a linear medium). But I hope that I am able to convey the manner in which I think we need to understand it. Despite seeming stable, a social system is potentially relatively volatile. An attractor can for instance seem to work consistently centripetally, but since this function can be dependent on the presence of another attractor, changes here can affect the other.

Today the Department seems relatively stable, and in many respects on the track it has been from the beginning. I don't think that this is particularly likely to change in the near future. However, it also seems that the centrifugal processes outnumber the centripetal ones. Usefulness and pragmatism, which I find to be prominent and strong attractors, turned out to

be possible strong centrifugals, which are today partly curbed and partly converted into centripetals by the general interest in philosophy and the friendliness-attractor. An awareness of this and similar balances seems important for the Department's future.

This concludes my attempt to treat the Department in terms of a complex adaptive system. It should be seen as something of a sketch, but within the context of this dissertation it serves to illustrate some points. When trying to make cognitive structures relevant for social contexts many different attractors must be taken into account, partly because of the malleability and flexibility of these structures themselves.

Chapter 9: Conclusion

In the introduction to this dissertation I presented three main research problems. In this concluding chapter I will summarise the ways in which I have approached these, and what was concluded from these explorations.

The first question was: *what are the roles of the hidden infrastructure of our mind in science and interdisciplinarity?* This was mainly addressed in chapter 4 where I employed a naturalistic view of the mind as an evolutionary product. The demand for efficiency in (evolutionary significant) problem-solving stands central in the argument that our mind is *not* a *tabula rasa*. It is 'pre-configured' to operate in certain ways, which means that we most likely have an 'intuitive ontology' that unfolds spontaneously in our lives. This intuitive ontology is universal in the sense that Brown (1991) employs.[28] In other words, it is malleable and flexible, but despite this it operates in a homologous way in most healthy adult humans. Thus it very likely represents a significant factor in science.

The concepts of '*concept*,' '*module*' and '*domain*' are particularly important in this dissertation. *Concepts* are mental representations which serve as selection principles and ways of going beyond the information available through sensory information. Concepts develop partly as a result of cultural environment, but they also develop within certain constraints provided by modules. *Modules* are neurological 'tools' developed in order to process a particular type of information as efficiently as possible (which partly involves working 'on its own,' thus likely creating boundaries in our intuitive ontology). *Domains*, on the other hand, are fields of *knowledge*. The content of these are partly formed by their modular composition, and partly by culture-historical processes. Domains consist of interlinked concepts that develop within modules. It is this tension between linkages between concepts and the divisions that result from a modular organisation that characterises much scientific knowledge.

Since science is a cultural product, we needed a tool for relating our cognitive structures to the cultural level. This was found in Sperber's 'epidemiology of representations.' According to this, the neural basis modifies the *probability* of different representations being

[28] A human universal does not need to be *absolutely* universal according to Brown. If for instance a certain trait appears in 70 % of all humans or cultures, this is an interesting fact that needs to be explained. By adopting a 'weaker' conception of human universals we can concentrate on this explanation rather than rejecting it on account of the 30 % that do not have it.

adopted and reaching a wide distribution. It is thus a non-deterministic, selectionist way of relating bio-psychological models to the cultural level.

Focusing on a simple typology of modules (along the axes dominant/weak and high-structured/low-structured) I constructed three ideal types of domains. These can be used as tools for *analysing* and getting a *meta-perspective* on our knowledge. The ideal types, called *A-*, *B-*, and *C-domains*, behave differently according to their modular composition, and it was argued that having this in mind can be helpful in both 'ordinary' scientific contexts and in interdisciplinary work. Particularly interesting is the last one, which was developed further in chapter 8, distinguishing between *focused*, *congruent* and *discordant* C-domains. Geography was sought understood as a discordant C-domain, being subject to *split* (knowledge treated by differently structured modules seek to stay apart) and *pull* (dominant modules draw concepts into their 'influence sphere') processes.

It has to be acknowledged, however, that the ABC-domains are limited in that they only consider intellectual content in relation to neural basis. The model does not include 'messy' factors like e.g. culture and emotion. We therefore need to expand our perspective to include more factors if we want to deal with the discipline as a historical product and as manifested in the Department of Geography in Trondheim.

The second question was: *what are the roles of the hidden infrastructure of our mind in geography?* I have addressed this question to some extent through identifying the discipline as a discordant C-domain. Geography as an 'interdisciplinary discipline' has to deal with divisions that are general to academia, and that to some extent are also reflected within the discipline itself. But these boundaries are more deeply rooted than one may think. Not only have they been nurtured by decades of historical development, they are also reinforced by our intuitive ontology. Many divisions are thus unlikely to disappear even if historical processes should go in such a direction.

But this does not tell the whole story. Why has not geography been split up? To give a more complete answer to the question of what roles the hidden infrastructure plays in geography I identified an additional question (2a): *what is the intellectual constitution of the disciplined mind of geographers?* To deal with this I wrote an intellectual history of the modern discipline in chapter 5, an account which must be acknowledged to be partial and aimed at identifying a few features rather than giving an exhaustive treatment. I found that the emphasis on *usefulness*, *synthesis* and *visual analysis* were prominent features of the discipline. These features give us a more complete picture, partially explaining why the split-processes have not to a larger extent fragmented the discordant geography domain. To be

useful a discipline often has to deal with problems that are defined externally to the discipline, and which thus are less likely to respect disciplinary or sub-disciplinary borders. Much of the source for geography's diversity can be found here. The emphasis on synthesis has formed an important part of the disciplinary identity at different time junctures, and this provides part of the motivation needed for transgressing intellectual borders. The focus on visuality and visual analysis, it was suggested, contains within itself a tendency towards synthesis and knowledge integration. This thought seems to be in harmony with, and can possibly partially explain, the discipline's affection for complex concepts like place, landscape, space and so on.

The third question was *what does the theory concerning mental infrastructure and the intellectual constitution of geography look like when dealing with a concrete context?* The aim was to ground the theory and to see how it would look when related to a concrete case. In order to accomplish this, I first had to examine yet another question (3a): *what characterises geography in Trondheim and to what extent can the broad tendencies found when examining the discipline's history be related to the way of doing geography at the Department?* I also needed to examine (3b): *what are the cultural and institutional characteristics of the Department?*

With regard to 3a, it was found (in chapter 6) that particularly when it came to *usefulness*, the Department conformed to the general image of the discipline. This was also the case with *synthesis*, where the ideal was realistic and pragmatic, focusing on several manageable syntheses rather than a total one. Elements of *visual analysis* were at the Department acknowledged as important, although some things indicate that the actual presence of such analyses were relatively limited.

We thus see that my observations on the intellectual style associated with the discipline to some extent are found also in the specific context of the Department. If my general analysis concerning the relationship between the mental infrastructure and the discipline is valid, it can be assumed that it will also have some validity in Trondheim.

The examination of the Department as a social system provided us with a specific context under which the actual manifestation of geography occurs. We identified social factors like friendliness, conflict avoidance and tolerance as well as organisational matters like department size, university organisation and scarcity of time. By treating these along with disciplinary and cognitive issues in terms of a *complex adaptive system*, we were in chapter 8 to some extent able to integrate it all into a relatively unified picture. We described the system in terms of centripetal and centrifugal processes, while reflecting on how the different

attractors interacted and speculating on what this might imply. The analysis thus came to include social, institutional, disciplinary as well as cognitive factors.

As mentioned above I regard the treatment of the Department in terms of being a complex adaptive system as tentative. It is possible that other types of description could have done the job equally well. In the last section in the previous chapter we simplified complex sub-processes and treated them almost like unified entities (friendliness, pragmatism, time pressure etc.). In accordance with my own reflections in chapter 8, we should be cautious when dealing with such reifications because they tend to be drawn into the 'thing-module.' What helps us keeping this in mind is 1) being aware of the pull process in question, 2) reflecting on the way in which complex systems are known to behave, and 3) being aware that this analysis is not meant to stand alone, but must be seen in relation to both the relevant interpretative chapters and the naturalist perspectives involved.

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