The Hammer and the Nail : Interdisciplinarity and Problem Solving in Sustainability Science

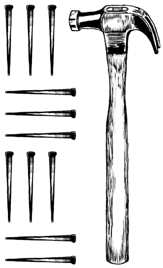
Thorén, Henrik

2015

The Hammer and the Nail

Interdisciplinarity and Problem Solving in Sustainability Science

Fig. 3. Nail hammer and nails.



Henrik Thorén

Lund University

Department of Philosophy, Lund University

ISBN: 978-91-87833-20-5
Printed in Sweden by Media-Tryck, Lund University
Lund 2015

The cover image of the hammer and the nails were drawn by Henrik Thorén. The basis for the hammer—although not the nails—is another drawing that occurs in *Farm Mechanics: Machinery and its Use to Save Hand Labor on the Farm* (1918, 22). The book is in the public domain and has been made accessible online through the Gutenberg Project. The photograph on back was taken by Nina Eriksson.

To Inez

Abstract

This is a thesis about interdisciplinarity, scientific integration, and problem solving in sustainability science. Sustainability science is an emerging and highly interdisciplinary field that seeks to integrate vastly differentiated bodies of knowledge in addressing the challenge of transitioning contemporary societies towards sustainability. Interdisciplinarity is paramount.

Interdisciplinarity in general, and in the context of sustainability science in particular, has often been associated with solving particular problems and problem solving is one important theme in this thesis. A central idea that is developed is that of *problem-feeding*. Sometimes problems arise within one discipline that can only be solved with the help of another. This concept, that has predecessors in e.g. the work of Lindley Darden and Nancy Maull, is explored considerably. It is argued that in interdisciplinary contexts—such as sustainability science—where collaboration is sought it is important to maintain cross-boundary problem stability. That is to say, as the problem is transferred from one discipline to another transformations will often be necessary. These transformations then, need to be acceptable to all involved parties to maintain an active interdisciplinary connection.

Another topic that is discussed both in the introductory essay and some of the papers included is that of scientific imperialism. Scientific imperialism—the infringement of one discipline upon the domain of another—is here suggested to be primarily a threat to in-

terdisciplinary collaborations. A distinction is introduced between imperialist failures of expansionism and failures of replacement. These are labelled type-I and type-II imperialism respectively. Particular attention is devoted to the latter form. Type-II imperialism concerns cases where imperialist infringements fail as the imperializing framework replaces viable, or compatible alternatives. Such an error of replacement does, importantly, not imply that the framework or theory should be disregarded completely. This type of imperialist error can both be quite subtle, and damaging. For one, if one directs the attention to specific contexts knowledge is actually lost in the process. This is particularly serious in fields such as sustainability science that are, to such an large extent, aimed at influencing concrete policy.

Finally, interdisciplinarity is difficult to achieve and in many cases represents a grand challenge in itself. There are however many different ways in which interdisciplinarity may be accomplished and different forms are suitable in different contexts. In a field such as sustainability science where complexity is such a prevalent feature, an inclusive, pluralist, approach is likely to be appropriate.

Acknowledgements

Just before I submitted the text contained on these pages I found a notebook in my office. I needed one for a seminar and opened it to see whether it was empty or full. It turned out to be the latter but I sat for a little while randomly browsing through the pages until I stopped on a page that looked less like scribbled reading notes, page references, and to-do lists, and more like the kind of thing one would find in a diary. That turned out to be exactly what it was. The text had been written by me, incidentally, on the train on my way to the first day on my new job as PhD. Candidate in theoretical philosophy at Lund University. I was hopeful and relieved, happy to be doing something after some time of uncertainty. Now it is coming to an end and as I am writing this there are only a few short days left until the manuscript is to be handed over to the printers. The past year has been extraordinarily busy—there has not been enough time for family and friends and I am determined this will now change. So in many ways it is with similar emotions I am looking forward to what is coming. But it is also with a pinch of something else. The past six years have been wonderful and I am lucky to have had the chance to spend time and work with such wonderful people. It is now time for me to try to thank everyone to whom I am in debt.

Many people have helped me in completing this thesis. First of all, writing a doctoral thesis makes a person terrible company. Nina, my companion in this life, has had to endure my moods now for several years and still, somehow, seems to like me. How can I ever repay? Then, my daughter Inez, who only joined the bunch late in the game: you have really made this last effort so much easier.

Then, of course, my supervisor Johannes Persson. Johannes has been tireless and sure-handed in guiding me through this process and for this I am eternally grateful. I am afraid I am hardly the ideal student so I take it this has not been particularly easy. I will especially fondly remember some meetings we had early on that were held on a lake in mid winter while engaged in ice-fishing. My secondary supervisor, Lennart Olsson, has also been a great support. Especially towards the end of this process he has helped by thoroughly reading long drafts and providing insightful comments.

Then everyone who has been engaged in LUCID or LUCSUS over the past six years where I have had so many wonderful colleagues that have helped, encouraged, and discussed along the way. I have had many insights from talking to my fellow philosopher and good friend, Eric Brandstedt. Another very good friend who has patiently listened and discussed both in matters of work and life is Elina Andersson. Thank you. Giovanni Bettini, who among other things hosted me on a cycling trip in Verona. Vasna Ramasar has been a real friend throughout this whole process. So has Anna Kaijser with much kindness and compassion precisely when it has been needed. Ann Åkerman, my travel companion for two trips to the African continent. These past six years have been unique in that we started out together as such a large group, and have followed one another closely every step of the way. I now consider all you good friends, Torsten Krause, Mine Islar, Yengoh Genesis Tambang, Maryam Nastar, Richard Andersson, Melissa Hansen, Andreas Malm, Cheryl Sjöström, Sara Gabrielsson, Anna-Karin Bergman and Erik Jönsson. During this time many more have joined our group to make it all the richer. Sandra Valencia, Molly McGregor, Henner Busch, Chad Boda, Helena Gonzales

Lindberg, Emma Li Johansson, Wim Carton, Ebba Brink, Ellinor Isgren, Gregory Pierce, David O'Byrne and David Harnesk. It has been a true honour and inspiration to have as friends such talented and inquisitive people. Also a thank you to all others who have made working at LUCID such a nice experience, Barry Ness, Bodil Elmqvist, Christine Wamlser, Ingegerd Ehn, Kimberly Nicholas, Lena Christensen, Marcella Samuels, Ruben Zondervan, Sara Brogaard, Turaj Faran, Cecilia Kardum-Smith, Amanda Elgh, Charlotta Kjöllerström, and Krister Olsson. Thank you.

I have during this time had the fortune of spending time at two departments and my academic home has been the department of philosophy. Thank you to everyone who has attended the higher seminar, commented on work, or just been good friends. Lena Wahlberg, Tobias Hansson Wahlberg, Robin Stenwall, Anna-Sofia Maurin, Peter Gärdenfors, Victoria Höög, Catherine Felix, Björn Petersson, Ingar Brink, Emanuel Genot, Justine Jacot, Bengt Hansson, Martin Jönsson, Asger Kirkeby-Hinerup, George Masterton, Carlo Prioretti, Paula Quinon, Björn Peterson, Toni Rønnow-Rasmussen, Staffan Angere, Frank Zenker, Patrizio Lo Presti, Jan Hartman, and Erik Olsson. A special thanks to Ingvar Johansson who read through a late draft of this manuscript and for his many comments and questions at seminars. Also Stefan Schubert with whom many lunches have been spent in enlightening discussion. I also want to thank all those who have given their views on my work or otherwise been supportive,

I am also grateful to Anne Jerneck who commented on a predecessor to this final manuscript and several of the papers. Many others have given valuable insights to the work as well. Brian Hepburn, Sarah Green, Mads Goddiksen and Susanne Wagenknecht have during my visits in Aarhus commented on some of the papers included in this thesis. A warm thank you also to Hanne Andersen who let me come visit. A special thanks to Brian and Suze for their wonderful company and their great generosity in letting me stay at their place on numerous occasions when I have visited Aarhus. Thanks also to Mads and Natalia for letting me borrow their apartment. I have also spent time in Helsinki at TINT and want to thank Uskali Mäki for having me. It has been an exceptionally useful academically but also on a personal level. Many others have provided insights and critical comments as well. I especially want to thank Catherina Marchionni, Jaakko Kourikoski, and Petri Ylikoski. A special thanks to Pekka Mäkelä for many interesting conversations on how to become a better runner and helping me with a lactic acid test that was both wonderful and horribly painful. A thank you also to Samuli Pöyhönen for letting me borrow his desk and Miles MacLeod for sharing an office.

There are also many others that deserve special thanks. My co-author Line Breian has been a pleasure to work with. Philip Gerlee too, of course, who was a good friend of mine before we became co-authors. I also have to thank Stefan Anderberg who believed in the relevance of my project from the start and was exceptionally supportive during the crucial first couple of years. For controlling the language of manuscripts for papers, as well as the kappa, I want to thank first of all Paul Robinson, but also David O'Byrne, David Ratford, and Brian Hepburn. I also owe considerably to a number of anonymous reviewers that have provided many useful comments and suggestions to all papers included in this thesis.

A special thank you to the members of my somewhat neglected band, Strayfolk. Karl Malmqvist, Torbjörn Lorén, Anders Danielsson, Kalle Berlin, Christoffer Ödman, although we have only gotten to see each other occasionally those moments have often been a much needed break.

Then of course, my parents, Håkan and Marie have supported me throughout the process. As have my brother and sister, Johan and Hanna. Thank you. Nina's wonderful parents , Berit and Börje, have also shown great understanding over the years. I also have the unusual fortune of having had my closest friend of almost

30 years make the same journey from distant north to the deep south so that we now live only an hours drive from one another. Erica, thank you for everything.

Contents

List of Figures

Chapter 1

An Introduction

1.1 Interdisciplinarity and Sustainability Science

It has become increasingly apparent over the past few decades that we have entered a time of unprecedented danger—a new geological era, some have suggested, where humans are the dominant force (Crutzen and Stoermer, 2000). Global warming has accelerated significantly in spite of large-scale research efforts and a substantially improved understanding of the mechanisms, causes and consequences it involves. The hope of that warming will be pegged below 2° C globally—a target established by the 2010 United Nations Climate Change Conference in Cancún as an upper limit—is looking more like a theoretical possibility than a practically achievable goal. In an influential paper Rockstrom *et al.* (2009) propose seven "planetary boundaries" within which a "safe operating space for humanity" can be maintained. Three of them, they surmise, have already been transgressed. Climate change threatens almost every aspect of human life and the very foundations of contemporary societies and a transformation, in one way or another, is inevitable. The importance of meeting the challenge that such a transformation brings cannot be overstated. It is clear that a concerted effort will need to be made, and that science will continue to play an important role in this effort.

Sustainability science is an emerging, interdisciplinary field aimed squarely towards this issue. It is a field that is aimed at challenges like global warming and a central goal is to lay the foundations of a form of science that is obliged to serve society in solving so-called ‘real world problems’. A founding intuition is that in order to meet the challenge of transitioning towards sustainability we will need to draw on the resources of a wide range of disciplines. Historically the social sciences have been less involved than the natural sciences but sustainability science aims to integrate disciplines from both sides of this divide. Sustainability concerns a wide range of disciplines. We need to have a better understanding both of the physical systems and mechanisms that drive our climate system and govern ecosystems upon which we depend. But we also need to get a better idea of how, for example, our economies work and what drives them as well as disentangling difficult theoretical problems concerning uncertainty, decision making, and risk, and so on. In addition, interactions between social and natural systems are of paramount importance.

Integrating such diverse bodies of knowledge is itself a challenge grand in scale and it is to that kind of integration this thesis is devoted. This thesis is about scientific integration and joint problem solving in sustainability science. How do we coordinate the efforts of different disciplines? What is interdisciplinary problem solving? And how does it differ from regular problem solving? In particular, how much integration and unification is needed in order to conduct sustainability science?

1.2 Interdisciplinarity in Brief

The topic of this thesis is interdisciplinarity. Although the idea of interdisciplinarity has appeared many times in the past, and indeed since antiquity, if we use the notion liberally (Klein, 1990, cf.), it is perhaps now more feverishly discussed and called for than ever—not least as a consequence of the quickly deteriorating state of our climate system and the problems that arise in the wake of that change, since here interdisciplinarity is perceived of as the only way forward. But the idea arises in all sorts of contexts. It also appears as a possible solution to problems at many different levels, from concrete issues such as how best to produce good climate scenarios, to rather abstract questions about, for example, how to promote innovation in general. Not infrequently, interdisciplinarity is a requirement imposed by funding agencies, and it is often perceived of as a value in and of itself.

Nonetheless, few labels hide so many concepts; and few are so contested and confusing. In a recent review of different notions of interdisciplinarity Julie Thompson Klein lists almost 30 varieties (Klein, 2010). The main strategy deployed here will not involve establishing an extensive and detailed taxonomy, although over the course of this thesis I will try to pry out some of the problems and difficulties associated with the notion. Before we proceed, a few remarks on common usages are in order nonetheless.

First, one often finds the term 'interdisciplinarity' being used as it is in this essay: as an umbrella term that captures many types of relationships that involves more than one discipline. It may concern outright collaborations, overlaps, accidental similarities, reductive relationships, exchanges of methods or theories, the way disciplines are 'placed' relative to one another in the disciplinary system, and so on. Calls for interdisciplinarity can thus range from quite local concerns that some phenomenon or other "spans several disciplines" to broad and global claims complaining of the disunity of science at large.

In the literature on interdisciplinarity one commonly sees the term 'interdisciplinarity' being used in a slightly narrower sense. It is then arranged in what we might call the 'standard trichotomy' of *multidisciplinarity*, *interdisciplinarity*, and *transdisciplinarity*. Here is a short characterisation.

Multidisciplinarity is normally taken to involve the juxtaposition of knowledge claims from different disciplines; it is, as Klein puts it "essentially *additive*, not *integrative*" (Klein, 1990, 56). Hence multidisciplinarity is marked by disjunctive claims which are often ordered under some broad topic or theme but otherwise lack interconnection. Although multidisciplinarity may in fact be a fruitful approach in some fields (Sintonen, 1990, see), it is often perceived as a failure.

Interdisciplinarity is contrasted with multidisciplinarity in that it is integrative. This elaboration is not particularly clarificatory, since it can be taken in turn to imply many different things. Klein approaches the matter by separating different varieties. She lists no fewer than eleven specific forms that have occurred in the literature (see Klein, 1990, 64f; also Klein, 2010). Among them are pseudo interdisciplinarity (the borrowing of tools), composite interdisciplinarity (an instrumental solution of a problem), supplementary interdisciplinarity (the overlapping of "material fields"), and unifying interdisciplinarity (increased consistency in subject matter). Using these eleven Klein identifies four more basic forms:

Borrowing. Borrowing involves the transfer of methods, concepts, theories, and so forth, between theories. It can happen in many different ways and serve different purposes. Sometimes borrowing takes the form of importing metaphors where an important motivation is to structure, or re-structure, some domain (Klein 1990, 85; see also Kellert 2008).

Solving problems. Here the idea is that disciplines come together in order to solve particular problem with "no intention of achieving a conceptual unification of knowledge" (Klein, 1990, 64). It can be organized around a concrete object (city planning) or "focus on a complex, problematic question that cannot be assigned to a given discipline or find its solution in a border are between two fields" (Klein, 1990, 65).

Increased consistency of subjects or methods. This describes the process by which two disciplines are approaching a state of increased unification, or by which two disciplines merge or interface over a domain of entities that concern them both. Klein gives the example of biophysics.

The emergence of an interdiscipline. It has often been remarked that interdisciplinarity does not appear to lead to more unification, but, quite to the contrary, appears to fragment the disciplinary system even further. New hybrid disciplines arise on the fringes between already existing ones; social psychology, econophysics, cognitive science, and so on, are all hybrid disciplines of this sort.

There are minor redundancies in Klein's categories and the list is not exhaustive. Moreover, many categories can be viewed both normatively and descriptively: they seem to hover between being motivations for interdisciplinarity (e.g. in the form of goals to be obtained) and at descriptions of what interdisciplinarity is.

Finally, transdisciplinarity, which is usually considered the final, or deepest, stage of interdisciplinary collaboration or integration. The term 'transdisciplinarity' first appeared at a conference organized by the Organisation for Economic Cooperation and Development (OECD) in 1970. There, Jean Piaget talked of transdisciplinarity as a final, and superior, stage of interdisciplinarity, a "total system without stable boundaries" (Piaget 1972 in Nicolescu, 2006, 1). Eric Jantsch, another participant at this event, distinguished between six different stages, or degrees, of interdisciplinary integration (or collaboration) of which transdisciplinarity was the final, and most, cooperative. For him transdisciplinarity involved complete coordination of what he called "the education/innovation system" (Jantsch, 1972, 17). It "[e]stablishes a common system of axioms for a set of disciplines (e.g. anthropology considered as the 'science of man and his accomplishments' " (Apostel *et al.*, 1972, 26).

Transdisciplinarity, then, has often been thought of as a kind of "overarching synthesis" (Klein, 1990, 65). This suggests that interdisciplinarity, by contrast, may be a more local affair. However, usage appears to have changed over the years, with an increased focus precisely upon local contexts, and this has blurred the distinction between inter- and transdisciplinarity (see below).

1.3 Sustainability Science

1.3.1 Background

The field of sustainability science can be said to have been founded in 2001 when geographer Robert Kates and a group of very influential scientists published a paper aptly titled 'Sustainability Science' in Science (Kates *et al.*, 2001). The emergence of this field of research, however, had been long in the making and it can be seen as the result of both scientific progress and a political process that reaches much further back. Sustainability science can perhaps be considered the focal point of a number of different issues that became increasingly connected to one another during the late 1980s and early 1990s. Concerns perceived of as more or less independent—environmental degradation and pollution, the prospects of economic growth and development, global and intergenerational justice—came to converge on the issue of anthropogenic global warming. Climate change is thus central to sustainability science but one should not confuse sustainability science with climate science although the latter is an important historical component of the former.

Climate change had been an issue among physicists and climate scientists for well over a century preceding this point but from around 1960 the realization that, a), anthropogenic emissions could have an influence on the climate system, and b), that this actually seemed to be happening, was starting to sink in.[1] Roger Revelle and Hans Süess state in their seminal 1957 paper that:

> Thus human beings are now carrying out a large scale geophysical experiment of a kind that could not have happened in the past nor be reproduced in the future. Within a few centuries we are returning to the atmosphere and oceans that concentrated organic carbon stored in sedimentary rocks over hundreds of millions of years. (Revelle and Suess, 1957, 19)

The passage foreshadows what was to come. At the time Revelle and Süess themselves saw this 'experiment' as an opportunity for learning.

During the fist half of the 1960s the methods to reliably measure CO_2 in the atmosphere were established by Dave Keeling and in 1965 the United States President's Science Advisory Committee made the connection between human use of fossil fuels and harmful climate change (Agrawala, 1998, 606). Otherwise interest in climate change during the 1960s mostly concerned local climate modification.[2]

---

[1] In the end of the 1950s research efforts made by scientists at the Scripps Institute in La Jolla—among them Roger Revelle, Harmon Craig and Hans Suess—provided a fuller understanding of how carbon cycles in the atmosphere and how it is absorbed in the oceans. The implications of this were seen by Bert Bolin and Erik Eriksson (1958). In the early 1960s Charles Keeling had begun measuring atmospheric CO_2 reliably, and he found it, somewhat surprisingly, to be rising rather sharply. See Thorén and Persson (2013) for a short discussion of climate science prior to 1957. Also see Weart (2003), Edwards (2010), and Bolin (2007).

[2] There where projects in Vietnam in which US military wanted to wash away the Ho Chi Min Trail by seeding clouds, or through steering tropical

In the 1970s the political interest in issues related to climate change was rising, for different reasons. This was embodied in a number of meetings backed by the United Nations (UN). The 1972 the UN Conference on Human Development convened in Stockholm. In 1977 a conference was held in Nairobi—the UN Conference on Desertification—and in 1979 the World Climate Conference in Geneva marked the birth of WCP (World Climate Programme). In 1972 an influential report commissioned by the Club of Rome was published, *Limits to Growth* (Meadows *et al.*, 1972). The report is significant both because it connects economic growth to various physical boundaries—energy expenditure, pollution, etc.—and because of the methods it used—so-called integrated assessment modelling. A great variety of data was combined in a single dynamic model. The methodology has been widely adopted since and plays an important role in, for instance, the work of the IPCC.

A series of workshops were held in the first half of the 1980's in Villach, Austria, with funding from the UN Environmental Program. At the last of these, held in 1985, the expert participants (among them Bert Bolin) reached a consensus that climate change would cause unprecedented warming as soon as the first half of the twenty-first century. One of the immediate outcomes of this meeting was the establishment of the Advisory Group on Greenhouse Gases (AGGG) in 1986, and one goal was to produce, if necessary, a global convention (Agrawala, 1998, 609).

The perhaps most significant developments paving the way for sustainability science, however, came with another report in the 1980s. If Kates et al. (2001) represented the birth of sustainability science, the publication in 1987 of the report Our Common Future (World Comission on Environment and Development (WCED),

---

cyclones. Seeding clouds had civilian uses too with the hope of producing more reliable agriculture. See (Edwards, 2010, 360).

1987) was surely its conception.[3]

Initiatives by worried scientists eventually lead in 1988 to the establishment of the IPCC, with Bert Bolin as its first chairman. The panel produced its first report in 1990. The establishment of the IPCC is important in the context of sustainability science, as it has now grown into an important channel through which sustainability scientists can reach out and disseminate their research. It is, in spite of the considerable criticism it has received, a concrete attempt to realize one the most important goals of sustainability science—namely, to produce knowledge that has practical implications for policy. The IPCC has grown immensely since its conception and recently published its Fifth Assessment Report.

In the 1990s much of this work was further formalized. In 1992 at the Earth Summit in Rio Janerio the United Nations Framework Convention on Climate Change (UNFCCC) was established—a treaty that aims to stabilize atmospheric greenhouse gas concentrations and thus avoid hazardous interference with the climate system. An important outcome of the UNFCC is the Kyoto Protocol from 1997 in which legally binding emission cuts were established for developing countries.

It is against this background, and in this context, that sustainability science arises as a field of inquiry.

[3]Although ecologists, conservation biologists, and to some extent agronomists, had nursed an interest in issues of environmental sustainability for longer than this date suggests, the literature on the issue virtually explodes after this point: a search on the Web of Science for papers with the term 'sustainability' in the title—one, to be sure, that is bound to generate many false hits—unearths some 7,887 publications until the year 2001, while the same search looking at years prior to 1987 yields only 54. A search across all years yields a number of hits slightly north of 44,600.

1.3.2 A Characterization

Accurately characterizing the field of sustainability science is difficult. One reason is the field's relative immaturity, which ensures that it is both rather messy and more marked by its promises than its achievements. After all, the problems to which sustainability scientists turn their attention appear to be a considerable distance from being solved.

Another problem is that sustainability science is potentially different from many cases of interdisciplinarity in that it is not a hybrid discipline. One motivation for engaging in interdisciplinarity is simply to revise the disciplinary structure. The ideal outcome of this is a new disciplinary structure that better fits the context in which it is to be found. Sustainability science, however, does not appear to be part of any such venture. I write 'appear', because the issue is debatable; I shall discuss it in more detail later in this thesis (e.g. Chapter 5).

Given these obstacles, it is difficult to provide a characterization of sustainability science that is both informative and universally applicable, not to mention one that will hold up over time. I will nonetheless provide an outline of some main features.

There is generally more than one way to characterize a given interdisciplinary field, just as there are many ways in which one might want to characterize a discipline. One way is to emphasize the problems and then draw conclusions about the appropriate methods, tools, theories, and so forth. Another is to emphasize tools and methods and then formulate the problems in terms of these. Yet another is to attempt to spell out the postulates, or fundamental principles, which generate the problems.[4]

Two points. First, it is not generally true that problems are more important than approaches, and whether this is so depends on the field in question. Sometimes the organizing principle is the

[4]See e.g. (Quental *et al.*, 2011).

problems. In other cases, that is not the case. So, for different fields different principles will hold.

Second, discussions of how best to characterize a specific field or discipline are rarely wholly descriptive. Generally they also have a normative dimension. Moreover, this is often the case in newly established fields. Indeed, many papers which attempt to characterize sustainability science also aim to give an account of what it *should* be.

The first to describe sustainability science as a field is Kates *et al.* (2001). The authors see the primary aim of the science as the promotion of "a sustainability transition" (Kates *et al.*, 2001, 641), and they set out to outline a research programme accordingly. At the most fundamental level this programme has two main tasks. One is to provide aid of a quite concrete kind in "sustainability transitions." This problem is a practical one, and it has led some to think of sustainability science as a kind of engineering science (Mihelcic *et al.*, 2003). The second task is to deliver understanding: "A new field of sustainability science is emerging that seeks to understand the fundamental character of interactions between nature and society" (Kates *et al.*, 2001, 641). These two tasks are then elaborated into an "initial set of core questions".[5] They are as follows:

1. How can the dynamic interactions between nature and society—including lags and inertia—be better incorporated into emerging models and conceptualizations that integrate the Earth system, human development, and sustainability?

2. How are long-term trends in environment and development, including consumption and population, reshaping nature—society interactions in ways relevant to sustainability?

[5]These questions were later revised and updated in Kates (2011).

3. What determines the vulnerability or resilience of the nature-society system in particular kinds of places and for particular types of ecosystems and human livelihoods?

4. Can scientifically meaningful "limits" or "boundaries" be defined that would provide effective warning of conditions beyond which the nature-society systems incur a significantly increased risk of serious degradation?

5. What systems of incentive structures—including markets, rules, norms, and scientific information—can most effectively improve social capacity to guide interactions between nature and society toward more sustainable trajectories?

6. How can today's operational systems for monitoring and reporting on environmental and social conditions be integrated or extended to provide more useful guidance for efforts to navigate a transition toward sustainability?

7. How can today's relatively independent activities of research planning, monitoring, assessment, and decision support be better integrated into systems for adaptive management and societal learning?

It is possible to make a case for the view that Kates and his colleagues are really considering sustainability science as a venture organized under the aegis of some theoretical framework. Their core questions revolve around issues like how best to model nature-society interactions, and what determines resilience and vulnerability in nature-society systems. One is tempted to conclude that Kates and colleagues imagine the main challenge here to be one of getting the models right. We need, somehow, to build into the present models such as those of the climate system or various ecosystems a whole range of variables and interactions that have

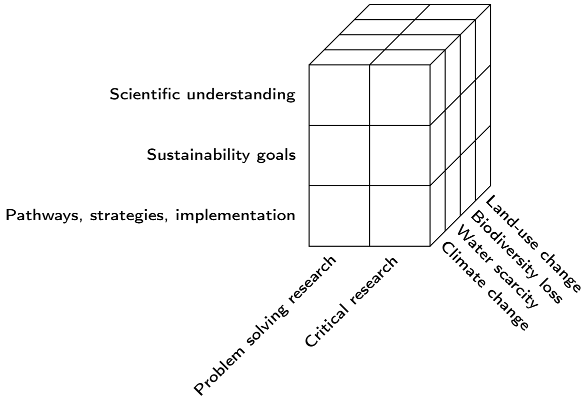


Figure 1.1: Integrative Matrix

previously been overlooked. Perhaps, then, the idea here is really that sustainability science is the application of a more or less fully outlined approach to the broad challenge of managing sustainability transitions. Precisely *what* this approach amounts to is harder to pin down, and there are certainly other ways of analysing their contribution. Nonetheless, the difference is noticeable when one compares the core questions pursued by Kates and colleagues to, say, the characterization offered by Anne Jerneck and her colleagues (Jerneck *et al.*, 2011).

The 2011 Jerneck paper presents a range of problems, or "sustainability challenges", as the authors describe them, but it appears much less framework dependent. The challenges are more broadly conceived, and include land-use change, biodiversity loss, water scarcity, and climate change. The integration is not merely about seeing to it that different scales are catered to, but also operates at a more fundamental level. Jerneck and colleagues envisage sustainability science as an enterprise organized around these challenges by a set of "core themes" (scientific understanding, sustainability goals and pathways, strategies, and implementation) and two "cross-cutting approaches" (problem solving research and critical research). This distinction, between problem solving research and critical research, is due to Robert Cox (1981). If problem solving research proceeds on the basis of a particular set of background assumptions, critical research questions those assumptions: "problem-solving research could deal with how to optimise an emissions trading scheme, while critical research would question the very existence of market-based mechanisms such as trading schemes as solutions to climate change" (Jerneck *et al.*, 2011, 77f). The challenges, themes, and approaches are related to one another in an "integrative matrix", see Figure 1.1.

1.3.3 A Framework for Sustainability

Although there are many alternative ways of characterizing sustainability science—Quental *et al.* (2011) suggest four principles that underpin the field, and Schoolman *et al.* (2012) rather depart from "three pillars of sustainability" in their preliminary assessment—there appears to be an important difference between thinking of sustainability science as an activity which departs from a particular framework on the one hand, and taking a more pluralist approach on the other. Many perceive of pluralism as necessary in sustainability science. Bettencourt and Kaur, for example, write that this science "necessarily requires collaboration between perspectives" (Bettencourt and Kaur, 2011, 19540). Or, as Komiyama and Takeuchi put it: "[s]ustainability science must therefore adopt a comprehensive, holistic approach to identification of problems and perspectives involving the sustainability of these global, social, and human systems" (Komiyama and Takeuchi, 2006, 3). Others conceive of the issue more narrowly, taking sustainability science to be a discipline in the making, although it is sometimes acknowledged that it is dependent for its data on other disciplines (to a significant extent).

1.3.4 Transdisciplinarity in Sustainability Science

It is a commonly held belief among sustainability scientists that sustainability science that it is, or should become, transdisciplinary (See e.g. Brandt *et al.*, 2013; Jerneck *et al.*, 2011; Martens *et al.*, 2010; Komiyama and Takeuchi, 2006). Although there appears to be almost universal agreement on this point, the transdisciplinarity in question is taken by different authors to have somewhat different implications. In her foreword to Katri Huutoniemi's and Petri Tapio's *Transdisciplinary Sustainability Studies* (2014) Julie Thompson Klein suggests that the contemporary conception of transdisciplinarity—the one which is influential in, for example, sustainability science—is guided by two theoretical ideas. One is the notion of *Mode 2 knowledge production* (Gibbons *et al.*, 1994; Nowotny *et al.*, 2001). Here the increasing involvement of non-scientific actors in knowledge production, as well as the temporary and local character of (some) contemporary research, is emphasized (Thorén and Breian, 2015). Although the project is largely a descriptive one, its proponents aim to highlight certain features of modern science, and how it has changed, and the theory has normative elements. For example, there is a suggestion that knowledge should be socially robust, so that it is more "likely to be reliable not only inside but also outside the laboratory" (Gibbons, 2000, 61).[6] The other idea Klein brings up is Funtowicz and Ravetz's *post-normal science* (1993). Funtowicz and Ravetz focus on problems that require urgent solutions, but where the decision stakes are high, the underlying values are in disarray and systemic uncertainties are overwhelming. They strongly emphasize the role

[6] See also Nowotny *et al.* (2001) and Nowotny *et al.* (2003).

of complexity in such problems. Examples here include problems relating to "major technological hazards or large scale pollution" (Funtowicz and Ravetz, 1993, 750).

Integration is a feature often highlighted in the discourse of transdisciplinarity, Hirsch Hadorn *et al.* (2010) write:

> Transdisciplinary research (TR) aims at better fitting academic knowledge production to societal needs for solving, mitigating or preventing problems such as violence, disease, or environmental pollution. TR strives to grasp the relevant complexity of a problem, taking into account the diversity of both everyday and academic perceptions of problems, linking abstract and case-specific knowledge, and developing descriptive, normative, and practical knowledge for the common interest. Integration is a core feature and major challenge of TR. (Hirsch Hadorn *et al.*, 2010, 431)

A number of features here are relevant for the present discussion. One is complexity. Transdisciplinarity is sometimes seen as one component in responding to complexity (Klein, 2004; Max-Neef, 2005). Although the phenomenon of complexity as such will not be directly addressed here, there will be reason to return to it. For now, it suffices to note that 'complexity' is an ambiguous term. In this thesis, the focus will be on two other features of transdisciplinarity: the integration of knowledge and problem orientation.

1.3.4.1 Integration of Knowledge

Sustainability scientists often emphasize that transdisciplinarity is necessary in their field as it implies knowledge-integration. As can be surmised from the remarks of Hirsch Hadorn and her colleagues above, this integration works in many dimensions, but two of these dimensions are especially prominent: the integration of distinct scientific disciplines (especially natural and social science disciplines (Jerneck *et al.*, 2011)) and the integration of science and society. In this thesis the first of these, scientific integration, i.e. interdisciplinarity, is the primary focus of attention (although I think that some of the points that are made are also applicable to interactions between science and society).

Interdisciplinarity in sustainability science has, ostensibly, two aims. One is conservative, the other innovative. The latter should be evident, since an important feature of interdisciplinarity in general, and interdisciplinarity in sustainability science in particular, is that it produces new knowledge as well as new methods, tools, and theories. Turning to the former, what I mean by conservative' in this context is something like *drawing on existing knowledge.* For example, one can see this aim in the following comments: "research on complex sustainability problems requires the constructive input from various communities of knowledge to ensure that the essential knowledge from all relevant disciplines and actor groups related to the problem is incorporated" (Lang *et al.*, 2012, 26).

1.3.4.2 Problem Orientation

The second main feature is problem orientation.[7] Here transdisciplinarity is taken to imply that the relevant knowledge is produced to solve "real-world problems" (Lang *et al.*, 2012). The idea that transdisciplinarity, or indeed interdisciplinarity in general, is a way of solving such problems has been widely accepted for as long as the terms have been used.[8] As has already been touched upon above, there are several features of these problems that tend to be highlighted. One is their source, and the notion that they are

[7]Some have described sustainability science as solution-orientated (Miller *et al.*, 2013).

[8]Hansson (1999) mentions this as a central feature of interdisciplinarity. See also Schmidt (2008) and Klein (1990). There is also here a connection between interdisciplinarity and applied science, see Sintonen (1990).

'societal', or at least do not spring from science itself. Another is their inherent complexity. Within sustainability science the notion of *wicked problems*, introduced by Horst Rittel and Melvin Webber (1973), captures both these features (Jerneck *et al.*, 2011; Norton, 2005).[9]

Problem orientedness is nonetheless somewhat perplexing for several reasons. For one thing, is not all science problem oriented? Moreover, there are several different things one might want to use as contrasts. Thus there is the idea of inquiry being problem-driven as opposed to theory-driven, corresponding roughly with the distinction between applied and basic research. This distinction is sometimes clear, but as Sintonen (1990) has illustrated using his "umbrella model", they tend to be deeply interconnected in practice. Under the umbrella of a basic research question applied research questions arise, and these in turn may house further basic research questions, and so on.

Others instead contrast real problems with "disciplinary" or perhaps even "scientific" problems. The latter are then *un*real in the very literal sense that they do not concern reality. They are problems that arise within highly idealized theoretical frameworks, or sterile laboratories (see e.g. Funtowicz and Ravetz, 1993).

Problem orientedness is in many respects the main theme of this thesis: how do we solve problems together, and what precisely is it that is so problematic about the way disciplines, or science, usually solve problems?

[9]Wicked problems have a range of properties. Many have to do with the problem being couched in a context of conflicting values regarding their formulation and eventual solution. I will return to them in 5.

1.4 Maslow's Hammer

Why "the hammer and the nail" of my title? The famous psychologist Abraham Maslow remarked, in his *The Psychology of Science* (Maslow, 1966), that the state of psychology as he found it in the mid-1930s was somewhat less than pleasing (to him). Too closely modelled on "orthodox science", with its atomistic assumptions and reductive methodology, it fails, Maslow argued, when it comes to humans. They have to be seen as wholes.

Maslow further reported that these problems had intensified as he turned from psychopathology to study the healthy people, concerning himself with "beauty, curiosity, fulfillment" (Maslow, 1966). He wrote:

> These "higher" psychological processes in the human being did not fit gracefully and comfortably into the extant machinery for achieving reliable knowledge. This machine, it turned out, was much like something I have in my kitchen called a "disposall," which nevertheless does not really dispose of all things but only of some things. Or to make another comparison. I remember seeing an elaborate and complicated automatic washing machine for automobiles that did a beautiful job of washing them. But it could do only that, and everything else that got into its clutches was treated as if it were an automobile to be washed. I suppose it is tempting, if the only tool you have is a hammer, to treat everything as if it were a nail. (Maslow, 1966)

Thus, Maslow's Hammer has been used to refer to this idea: that scientists tend to treat problems *as if they were problems for the tools they happen to have*. Maslow was not the first one here though. In his *The Conduct of Inquiry* (Kaplan, 1964) the philosopher Abraham Kaplan remarked on "a very human trait" he called the *law of the instrument*: "Give a small boy a hammer, and he will find that everything he encounters needs pounding" (Kaplan, 1964, 28).

Whereas Maslow only uses his hammer metaphor (or should we call it Maslow's carwash?) to emphasize what he perceived as the deplorable state of psychology in 1935, Kaplan notes that the principle is not all bad. After all, what else can we do? Larry Laudan once remarked that "it is often the case that even when an effect has been well authenticated, *it is very unclear to which domain of science it belongs* and, therefore, which theories should seek, or be expected, to solve it" (Laudan, 1977, 19).

So the situation is not quite analogous to another story that Kaplan's presses into service: the familiar parable of the drunkard searching for his keys. A drunkard is looking for his keys under the glow of a streetlight. To an inquiry from a passer-by as to whether he is sure he dropped the keys in that spot, he responds "No, I dropped them in the park, but it is lighter here." The drunkard *knows* the keys are somewhere else, the child with the hammer only treats everything like nails. Some of those things may well be nails, and some other things can successfully be treated as such. Thus Alexander split the Gordian knot instead of finding the end of the rope to untie it.

Moreover, the scientist who persistently deploys his or her tools and theories in pursuit of all manner of problems may well have good prima facie reason to do so, namely that this has been successful in the past. The econophysicist finds comfort and reassurance in the fact that the rigorous, highly formalized approach of modern physics has been so successful in its own domain so, why not elsewhere? This makes Kaplan's law of the instrument seem less like a fallacy and more like a heuristic. Think of the sleuth who sneers *cherchez la femme!* (a principle Kaplan also calls upon, incidentally.) The principle works well enough, at least where Hammett is God, but perhaps not so well in others places. But then, as long as our sleuth is sensitive to these contextual changes, and takes care not to confuse heuristic with law, he should be fine. The problem, perhaps, is that not every sleuth recognizes this, and they follow the woman regardless.

Then there is another aspect, not quite captured in these metaphors and parables. Perhaps fitting a problem to a framework just is precisely what it is that makes a problem soluble, or at any rate capable of being understood. Herbert Simon once suggested that we should think of *ill-structured* problems and *well-structured* problems in terms of the amount of information available to the problem solver given this problem solver's computational power (Simon, 1973). For *actual* problem solvers—that is to say, those who are pressed for time and not always fast—a mechanical procedure that patiently flicks through all possibilities one after another, offers limited value. Thus we *change* the problem and focus on another one, one that is simpler with fewer variables; one that we can survey. But, what are we to think of the Gordian knot? Did Alexander solve the problem or did he not? That depends, it appears, on whether we think of the problem as one of untying the knot or just opening it.

Now consider interdisciplinarity. As we have seen interdisciplinarity is often motivated precisely by the problems it can solve. Various disciplines come together to solve complex, often 'societal', problems.[10] The potential relevance of Maslow's and Kaplan's warnings should be obvious here. Different disciplines approach a common problem by reframing and recasting it. We end up with as many problems as we have disciplines, and eventually with solutions whose principal relationship with one another is merely that

[10]Many examples of interdisciplinarity are nonetheless 'internal' to science. In a recent review of interdisciplinary research proposals funded by the Academy of Finland it was found that the proposals were, for the most part, "epistemologically" as opposed to "instrumentally" oriented. That is to say, they were suited to increasing knowledge rather than to solving social problems (Huutoniemi *et al.*, 2010, 85).

they were inspired by the same event. Perhaps this is why genuine interdisciplinarity is so hard to achieve, and why the high ambitions of interdisciplinarians so often seem to fizzle out into disjunctive multidisciplinarity. True, it is also this feature that so many find problematic with disciplines. But, then, for interdisciplinarity to be an alternative it needs to overcome this challenge.

In this thesis I will discuss interdisciplinarity and problem solving in the context of sustainability science. Particular emphasis will be put on transferring problems between disciplines and the transformations involved in such transfers. Another main theme is what might be termed *the proper relationship between the disciplines*. How are disciplines related to one another? How should they be related? Can they overstep each other's boundaries? Disciplinary imperialism is a feature of disciplinarity and, as it were, interdisciplinarity. The example of resilience theory is a recurring motif, both in this introduction and the papers making up the bulk of the thesis. This theoretical framework emerging from discussion, in ecology and elsewhere, of the consequences of non-linear dynamics and complexity has been enormously influential in sustainability discourse. It offers an interesting case, because it appears to touch on many of contradictions and intricacies of interdisciplinarity: issues of unification, pluralism, integration, and imperialism.

1.5 The Structure of the Thesis

This is a compilation thesis. It consists of six papers written over the past five years that follow this introductory essay. These papers concern various topics relating to integration, interdisciplinarity and unification, both in general and in the field of sustainability science. This aim of this essay is to provide a framework for those papers. It is, however, not merely a summation of the work that has been done, but also a contribution.

The five chapters below are organized as follows. In Chapter 2, I approach the topic of interdisciplinarity by comparing the two most influential antagonists of the previous generation of philosophers of science (or perhaps the generation before that), namely Karl Popper and Thomas Kuhn. Popper and Kuhn held radically different views, especially on the relationship between disciplines and problems. It is informative to consider and compare these views, as they are, to such a large extent, reflected in contemporary discussions of interdisciplinarity. In Chapter 2 I therefore try to mould, from the opposing viewpoints of Popper and Kuhn, the situation facing those attempting interdisciplinarity as it is commonly perceived today. There are interesting parallels between contemporary calls for inter- and transdisciplinarity, on the one hand, and Popper's problem solving and discipline-independent research ideal, on the other.

Chapter 3 deals with the issue of problem sharing and problem transfers between disciplines. Here, in particular, the notion of problem-feeding is introduced—a notion developed in Thorén and Persson (2013) and Thorén (2015a). Problem-feeding involves situations where a problem that arises in one discipline (or field) can only be solved in another. Chapter 3 also contains substantive contributions to the conceptual framework in this thesis, as it introduces a number of distinctions with a bearing on the way in which problems are solved, shared and transferred. These distinctions are then used, both to deepen our understanding of interdisciplinarity and problem-feeding and, crucially, to make the challenges facing interdisciplinarians drawn up in Chapter 2 more precise.

Chapter 4 discusses the notion of *scientific imperialism*. In effect, this chapter is about interdisciplinarity gone bad. Imperialism, commonly understood in pejorative terms, describes the way disciplines overstep their own boundaries and infringe on the domains of other disciplines.

Chapter 5 is an attempt to link problem-feeding to different ideas of what interdisciplinarity is, and to say what is required for it to come about. Here I make a distinction between the unificationist and the pluralist model of interdisciplinarity, and I argue for two contentions. One is that sustainability science as a field appears best suited to the pluralist model. The other is that problem-feeding is compatible with a pluralist model for interdisciplinarity. Finally, there is a last chapter that contains summaries of each of the papers incorporated in the thesis, taken in turn.

Chapter 2

On Disciplines and Problems

2.1 Introduction

In a frequently cited passage Garry D. Brewer makes the following remarks about the nature of environmental problems and their relationship to interdisciplinarity:

> In short, environmental problems require interdisciplinary treatment which the conventional knowledge institutions have been unable, unwilling or slow to provide. Or, as cynics have stated it: 'The world has problems, but universities have departments.'(Brewer, 1999, 328)

He might as well have written "sustainability problems." The sentiment this passage expresses—and sentiments similar to it—should be familiar to anyone acquainted with the literature on interdisciplinarity. But it is puzzling nonetheless. And it raises some interesting issues. What is the problem with "conventional knowledge institutions"? Is it that they are somehow disciplinary, whatever that means? The passage about interdisciplinarity certainly seems to suggest as much. Then perhaps the idea is that there are problems—complex problems, or perhaps 'wicked' problems (Jerneck *et al.*, 2011; Rittel and Webber, 1973)—to whose solutions 'traditional' science lacks access. Paradigmatic examples of such problems are environmental problems and sustainability problems. What marks traditional science is that it is disciplinary, and this is also the reason why it fails to provide the sought-after solutions. A common charge against the disciplinary sciences is that they are parochial and, perhaps, reductivist—a discipline, then, is a way of viewing the world, rather than some specific patch of the world to be investigated.

Many questions can be posed at this stage. One is: How do problems relate to disciplines? That question might seem a lot less contrived if one remembers how Thomas Kuhn thought of disciplines (see Kuhn, 1962, 1977). For him problems, or rather *exemplars*—paradigmatic problems and problem solutions—are absolutely paramount in explaining both how disciplines propagate their accumulated knowledge from one generation of scientists to the next and how they *grow*. As the members of the disciplinary matrix encounter new problems they simply model them on problems already solved. By expressing new problems as versions of old problems they may also access old solutions, which can be recycled with minimal modification.

If there is at least some truth in Kuhn's remarks, then perhaps the separation of problems and disciplines is less straightforward. To see this more clearly, consider Kuhn's famous remark about the puzzle solving nature of normal science. Under ordinary circumstances scientists solve problems they think, and perhaps even know, they can solve. Thus they will avoid the broad and complex issues which are, in all likelihood, impossible to solve. We will return to Kuhn's views in more detail below. For now, however, we will briefly note their relation to Brewer and his cynics.

It is possible to use the Kuhnian picture of science to fill out Brewer's remarks. First, that picture offers a possible way of understanding, more precisely, what Brewer's cynics find so troubling about disciplines. In short, disciplines, in seeking a problem that can be solved, sacrifice some—perhaps, a lot of—perspectival breadth. The process by which a disciplinary matrix grows is one in which problems are transformed: problems which are impossible to solve, for one reason or another, are made into problems that are both possible to solve, and make the resources of the disciplinary matrix appear both apt and sufficient. Suppose we resort to a rather crude example, but one that is fitting nonetheless. Sustainability problems are notoriously complex for a range of reasons. At an ontological level we have to understand ecosystems, climate systems, economic systems, and various social structures, and how these interact. Conceptually there are problems with central notions such as sustainability itself, adaptability, social learning, robustness, vulnerability, and so on. On one suggestion about how best to understand it, sustainability is framed in terms of *resilience* (Gunderson and Holling, 2002).[1] Roughly, resilience denotes the ability of a system to absorb a disturbance, or more specifically, to retain some central property, structure, or capacity through a disturbance (Holling, 1973; Thorén, 2014). The idea, then, is that one way of realizing a sustainable society is to make one that is highly resilient. A sustainable social-ecological system is a resilient one, the transition to sustainability that is at the heart of sustainability science can thus be thought of as an issue of building resilience. Construing the problem of sustainability in this fashion accomplishes a number of things, one of which is to cast the resilience framework as a highly plausible, and fitting, route to progress on solving the problem.[2] The point is, however, that this problem transformation is decidedly two-sided. The reductive basis, treating problems of sustainability as problems of

[1]Several of the papers in this thesis concern the example of resilience. See Thorén (2014, 2015b); Thorén and Persson (2015).

[2]Importantly, this does not mean that the resilience framework can immediately, or even at all, solve all relevant problems; but it claims them for the framework (Hoyningen-Huene, 1993, 177)

resilience, has both intuitive appeal and has been massively fruitful. Over the last two decades a vast amount of research has been produced on the topic. This fruitfulness, however, appears to come at a price, for when we focus the problem in this way alternative perspectives tend to get lost in the process. Indeed on closer inspection the resilience framework suffers from distinct shortcomings.[3] What worries Brewer's cynic, then, is that this trade-off between identifying a tractable problem to solve and solving the original problem sometimes goes wrong. Or perhaps, some problems are too important to treat this way.

The Kuhnian framework also presents Brewer's cynics with a real problem. The motivation to transform problems is not irrational: from problems that are impossible to solve, it creates problems that are possible to solve. Perhaps these broad challenges cannot be tackled by any science because they are not specific, tractable problems but rather vast sets of vaguely interrelated issues. It is the very act of couching them in a framework that gives them their distinctness. Then it is not a matter of simply abandoning the disciplines, for if we do that we also lose sight of the issues. Consider the problem of sustainability again. Is it really a problem, i.e. one that we just have to find the correct formulation of? Or is it a set of problems—perhaps even an infinite set—that can be carved into concrete tasks in any number of different ways, so that, in order to get something tangible out of the problem one just has to relate it to some framework, such as a discipline? The latter is not an altogether implausible suggestion. John Pezzey (1997) has suggested that already, in 1989, there were five thousand definitions of sustainability in the literature. An exaggeration, surely, but anyway, consider the immense range of *different* sustainability problems to which even a few of these definitions would give rise if they were indeed to differ from one another. The worry could then

[3] See e.g. Hornborg (2013); Davidson (2010); Jerneck and Olsson (2008).

be raised that there is no non-arbitrary definition of sustainability, just an infinite set of variations. The implication would seem to be that what Brewer's cynic is calling for—an interdisciplinarity that can solve *the* problem of sustainability (or the environment)—is little more than a pipe-dream. There is no such problem. In order to get a problem concrete enough to permit a solution at all we just need to deploy *some* framework.

In drawing on Kuhn I am seeking merely to accentuate the issue here. There are certainly alternatives to his views, and the following sections will compare Kuhn's perspective with that of his, perhaps, fiercest critic, Karl Popper. Popper holds a view of disciplines and problems that is diametrically opposed to Kuhn's in many respects, and he therefore provides an excellent counterpoint. Moreover, Popper's ideal of science seems to be just the kind of thing that Brewer's cynic is looking for: a science that is not affected by petty disciplines, but roams freely through the intellectual domains in pursuit, precisely, of specific problems. Notably, I will use Popper and Kuhn to discuss *interdisciplinarity*. This is a topic neither of them (to my knowledge) took an interest in directly. Nevertheless their respective ideas concerning the relationship between disciplines and problems suggest distinct perspectives on the matter. I will here call these perspectives 'Kuhnian' and 'Popperian', respectively, but it should be kept in mind that I am not suggesting that these are views explicitly adopted by Kuhn and Popper.

This discussion may also be of more general interest, as the interdisciplinary/disciplinary dichotomy is rarely construed as one that concerns problem solving. Interdisciplinarity, it is argued, is problem oriented, whereas disciplinarily is not. Julie Thompson Klein, one of the most prolific writers on interdisciplinarity in recent years, lists five main reasons to engage in interdisciplinarity in the first place. Three of these—"to answer complex questions", "to address broad issues" and "to solve problems that are beyond the scope of any one discipline"—relate to problems and problem solving (Klein, 1990, 11).[4] Just to be clear at this point, there is of course great intuitive appeal in the picture that some problems, for one reason or another, cannot be solved within the confines of a particular discipline. These problems may, for instance, concern complex phenomena with causal profiles that span several domains. For example, how are we to account for the sharp rise in ADHD diagnoses in the US over the past few decades? It seems rather plausible that an explanation purely in medical terms—i.e. that ADHD has a neurological basis and is the result of chemical imbalance in the brain—is not going to suffice. We will quite probably have to seek an explanation which draws on a number of sources, possibly including practices associated with making the diagnosis, changes in the social context in which hyperactivity is less acceptable, the involvement of medical companies anxious to encourage doctors to prescribe psychotropic drugs to children, and so on (see e.g. Stolzer, 2007). Clearly, to address this issue properly many different disciplines will have to be mobilized.

2.2 Popper and Kuhn

The aim of this section is to present Popper's and Kuhn's views on the relationship between disciplines, problems, and problem solving. I think their respective ideas on these relationships are informative and fruitful in trying to understand the promises and challenges that might be involved in interdisciplinary problem solving. I am not, however, trying to extract what Popper and Kuhn *really* thought about interdisciplinarity. I am instead seeking to provide a framework in which to set the contemporary debate. Roughly speaking, Popper drew a sharp distinction between disciplines and problems, and thus thought that all (proper) science was already

[4]See also Hansson (1999) and Schmidt (2008).

interdisciplinary, at least in the sense that it is non-disciplinary. A scientist, according to Popper, should follow, not his or her discipline, but the problems, wherever they may lead. Kuhn, on the other hand, believed problems to be integral to disciplines. For him problems, in the form of exemplars, were the very basis of cumulative science. Consequentially, he confined interdisciplinarity to cases of scientific revolution. Let us begin by looking at Popper's thoughts on disciplines and problems.

2.2.1 Popper's Account

Popper never developed a philosophy of disciplines with the targeted character of Kuhn's. This is not particularly surprising given his views—he did not take disciplines to have any particular significance, and thus, presumably, saw no point in formulating a philosophy for them. Nonetheless, he makes a few remarks on the nature of disciplines in a discussion of the relationship between the discipline of philosophy and philosophical problems. Here he embraces a kind of historico-sociological view of disciplines, in the sense that he does not think of them as scientifically significant entities at all. He writes:

> Disciplines are distinguished partly for historical reasons and reasons of administrative convenience (such as the organisation of teaching and appointments), and partly because the theories which we construct to solve our problems have a tendency to grow into unified systems. But all this classification and distinction is a comparatively unimportant and superficial affair. *We are not students of some subject matter but students of problems.* And problems may cut right across the borders of any subject matter or discipline. (emphasis in original Popper, 1972, 88)

What is Popper saying here? For one thing, he asserts that although disciplines have some theoretical content they are first and foremost the children of accident and convenience. The implication seems to be that disciplines *should* not be involved in the problem solving process, other than perhaps as a kind of receptacle of theories and methods and so on, which can be drawn from as needed. But even if disciplines, on Popper's view, have no dynamic role in problem solving, they are not necessarily epistemically and cognitively inert. Importantly, it seems that disciplines may obstruct the problem solving process, as would be the case if a scientist fails to observe Popper's dictum to follow the problems.

There are further aspects to this relationship between problems and disciplines. One is that no discipline can claim a problem, or a domain, for itself. Here Popper's idea seems to be that given a problem, or a phenomenon of some sort, it is simply contingent how that problem is eventually solved, or how that phenomenon is explained. Thus disciplines can never be defined in terms of specific problems. Popper nonetheless points out that "many problems, even if their solution involves the most diverse disciplines, nevertheless 'belong' in some sense to one or another of the traditional disciplines" (Popper 1973, 89). That is to say, problems arise out of "characteristic discussions" of some theory, and theories, as already noted, are constitutive of disciplines.

From these points three consequences emerge:

1. The first concerns disciplinary influences on the problem solving process. Since Popper is really embracing a comprehensive view of disciplines—one that includes historical and social features, as well as cognitive ones—the claim that disciplines have no influence on the problem solving process covers two separate ideas. One is that the broadly social features of disciplines have no influence on the problem solving process. The other is that neither do the theoretical, and perhaps

methodological, contents of disciplines.

2. A second consequence is that Popper views problems as largely independent entities. That is, they can be transferred and shared between disciplines freely. This does not mean that Popper took the grasping of problems to always be easy, or straightforward, or to be something that can be done without taking the context of the problem into account (Popper, 1972, 165f).

3. Third, science proper is fundamentally interdisciplinary in the sense that all scientists engaged in genuine science have to be open to—and thus also consider—the possibility that the solution to the problem they are working on may be found outside their own disciplines.

The second consequence has an important bearing on the present discussion. It touches on a central ideal for interdisciplinary problem solving as it is treated in this thesis. This will become clearer in the next chapter.

To conclude, Popper's sharp separation of problems and disciplines seems to suggest that one should think of all science as interdisciplinary. Or, more accurately, it implies there is nothing significant about the disciplinary/interdisciplinary distinction if we assume that we are confining ourselves to science proper. Notably this independence of disciplines, which here is interpreted as an openness to interdisciplinarity, also involves more. Popper emphasized the centrality of creativity, stressing that in order to solve problems, and thus to make progress in science, the scientist needs to make a "leap of the imagination" (Thornton, 2014). Something new has to be created. The disciplines, as receptacles of that which has already been achieved, operate as a conservative force; science, in order to grow, needs to transcend them.

2.2.2 Kuhn's Account

Kuhn modelled disciplines on what he called disciplinary matrices. He held a view that appears in many respects to be the diametrical opposite of Popper's. For Kuhn disciplines are fundamental institutions of cumulative science, and although they most certainly have historical, administrative and social features, they are also instrumental in the growth of science. Now let us go through some of the main points needing to be made about Kuhn's views on disciplines and problems.

2.2.2.1 Disciplinary Matrices

A disciplinary matrix is a complex unit with a range of properties and features. Kuhn emphasizes four in particular: values, models, symbolic generalizations, and exemplars (Hoyningen-Huene, 1993, 145ff). Models, in Kuhn's terminology, are "...what provide the group with preferred analogies, or, when deeply held, with an ontology" (Kuhn, 1977, 297f). Symbolic generalizations can be exemplified by expressions such as $f = ma$. Scientists belonging to a disciplinary matrix are committed to a number of such symbolic generalizations in the sense that nobody will raise objections to their usage. Values permeate disciplinary matrices; according to Kuhn they are "more widely shared among different communities than either symbolic generalizations or models" (cf. Kuhn, 1962, 185). There are values regarding predictions—accuracy, a preference for quantitative rather than qualitative measures, and so on (Kuhn, 1962, 185)—as well as theory choice. The most interesting component for present purposes, however, is the exemplar. Exemplars are prototypical—or indeed paradigmatic—problems and problem solutions. They are, at the same time, a record of past successes and blueprints for future ones. As new members are schooled into the disciplinary matrix, exemplars serve as instruments of that schooling: "[a]cquiring an arsenal of exemplars, just as much as learning symbolic generalizations, is integral to the process by which a student gains access to the cognitive achievements of his disciplinary group" (Kuhn, 1977, 307).

2.2.2.2 Problem Acquisition

For Kuhn there are deep analogies between the way in which a disciplinary matrix educates new members—that is, how the new members come to embrace the disciplinary matrix—and the way the disciplinary matrix grows. A new problem is incorporated into the disciplinary matrix by being expressed as a version of a problem that has already been solved. This ensures that the resources of the disciplinary matrix can be harnessed with comparative ease. Solutions that have already proved successful can be redeployed with only minimal modification.

Let us look at an example taken from sustainability research. This example is deliberately controversial, and since it is going to feature in the discussion several times I will try to spell it out in some detail.

One of the most long-standing debates within sustainability science concerns how to interpret the notion of sustainability itself. A notable division lies between proponents of so-called weak sustainability and those that defend a strong variety. Those favouring the former typically base their accounts on Nobel laureate Robert Solow's work on maintained consumption under conditions when resources are exhaustible.[5] Solow wrote several papers on the topic of sustainability in the late 1970s and early 1980s. Here we will look at one. In his (1974) Solow argues for the thesis that consumption indeed can be kept constant even if we assume that the resource base in the relevant economy is limited. In order to make

[5]See for example Pearce and Atkinson (1993). For a critical discussion also Gutés (1996).

this argument Solow makes a number of assumptions. First, he only considers a specific type of economy, namely so-called Cobb-Douglas economies. Economies of this kind are captured by the following function:

$$Q = Q(L, K, N) \tag{2.1}$$

The function is to be read as follows, the aggregate output of the economy, Q, is a function of labour, L, man-made capital, K, and natural capital, N.

A second assumption Solow makes is that there is no technological advancement, and a third is that there is no population increase. One of the more contested assumptions—in fact, the locus of much of the debate about weak and strong sustainability—is that the elasticity of substitutions between N and K is "no less than unity" (Solow, 1974, 41). What this means is that as natural resources are transformed into man-made resources the value is either retained or increased. Now, as natural resources slowly diminish their mere scarcity will make them more valuable, i.e. their value will rise. What Solow shows is that the rise in value of natural resources traces precisely—under the assumptions made—the decreasing availability of those resources. Hence N is kept constant as long as natural resources are not completely depleted. Since elasticity obtains K can be kept constant as well. Solow's argument is built around a formal proof, and as such, strictly speaking, it only shows that it is possible, given the assumptions, for consumption to be kept constant in spite of exhaustible resources. Nonetheless, Solow himself, at least, regarded the results as reasonably applicable to the real world.[6]

In the current discussion these results are taken by some to show that sustainability is quite possible, and that we should not worry, primarily at any rate, that some of our resources are

[6] He described the elasticity assumption as "the educated guess at the moment" (Solow, 1974, 41).

exhaustible. Suppose we take the *new* problem for economists to pertain to sustainability. Precisely what that problem is, is of course open to interpretation, but broadly it concerns how to construct a sustainable society, or what such a society should look like. Solow, in his original paper, never mentions sustainability. His contribution was made in a different context. So, Solow's problem of consumption maintenance is an exemplar here. It is a paradigmatic problem and problem solution. The new problem of sustainability is fitted into this old problem. Interestingly, in this case almost no modification to the original solution is necessary at all. All that is needed is a peripheral change of terminology. The problem of sustainability is re-cast as an instance of the problem of maintaining consumption under conditions of exhaustible resources, and the same solution can be recycled.

Needless to say, not everyone agrees that this actually solves the problem of sustainability (see Gutés, 1996), but from the perspective of national economics this is how problems are solved. There will be reason to return to this in more detail later on.

This example, admittedly somewhat crudely presented, nonetheless shows how problems may be incorporated into disciplines. On the other hand, it makes it quite obvious why some would object to the transformation.

For now it is interesting to note that on Kuhn's picture there is no contradiction between being a student of problems and being a student of disciplines. In normal science disciplines are fundamental in making the problem solving process work. Exemplars are central to disciplines, and this leads Kuhn—in a perfect inversion of Popper—both to think of (normal) science as fundamentally disciplinary and embrace a much more substantive distinction between disciplinary and interdisciplinary science. The latter point depends on how we interpret interdisciplinarity in the Kuhnian framework, but first let us consider the former: the notion that normal science is disciplinary.

2.2.2.3 Normal Science and Overlapping Matrices

A disciplinary matrix depends on, and is guided by, its previous achievements as new problems are incorporated. But, as briefly noted above, disciplinary matrices may share various features: values, for one thing, but also symbolic generalizations, and perhaps even exemplars. Is such sharing not reason for engaging in, or perhaps even accepting a form of, interdisciplinarity? One is inclined to answer this question in the negative, as any form of sharing across matrices has so few implications—i.e. these overlaps never seem to act as causes for further interaction or integration. For example, in a situation where two matrices incorporate the same problem we would expect them to transform that problem in order to make it similar to some exemplar already present within the matrices. Such a process may have a number of different outcomes, none of which appears to give us reason to look beyond the matrix.

First, suppose both disciplines successfully transform the problem. Then this transformation can be either identity preserving—that is to say, the original problem is identical to those problems into which it has been transformed—or not identity preserving. If we assume both transformations are identity preserving, this just means that both disciplines could solve the problem. Presumably, they could do this equally well and independently of one another. No interaction has been involved in the problem solving process. Beyond the fact obtaining that the two disciplines have solved the same problem it might as well have been two different problems; the sharing has, in itself, no impact on the respective disciplines. However, where the two disciplines have solved the same problem in different ways, but obtained the same solution, one may perhaps draw conclusions about the robustness of that solution, i.e. that it holds under two paradigms. Or, conversely, in cases where the same solution is not obtained we may perhaps conclude that at least one of the disciplines must be at fault. This looks like something that could pass as interdisciplinarity, although Kuhn would probably have noted that such reasoning itself must be carried out from within some disciplinary matrix. Importantly, since the problems have been successfully solved within the respective matrices, there will simply be no need to look beyond them. The fact that the problem was also solved, but with different outcomes, using different resources, does not affect the individual matrices.

So assume that one, or both, of the transformations fails to be identity preserving. Clearly, independence still holds; the disciplines will have no reason to interact and they still appear to be set on their own developmental trajectories. Furthermore, since problem identity is not maintained across disciplinary boundaries, there is no possibility of making robustness claims about the solutions.

Now we move to the second general alternative: that the transformation is unsuccessful in one or both disciplines. This may lead members of the matrix to look elsewhere if the failure is deemed to be serious enough—that is to say, if it is believed that it should be possible to solve the problem within the discipline, and that the exemplars already present are of no help. The failure to solve the problem would be considered a significant anomaly and cause a crisis within the matrix (Hoyningen-Huene, 1993, 230).

2.2.2.4 Crisis, Revolution, and Interdisciplinarity

During periods of crisis and revolution, Kuhn allows, scientists may digress from the confines of their disciplinary homes and consider alternatives. Consider the discovery of the Layden jar. According to Kuhn this discovery drew on resources from many different paradigms.

> When it began [the process of discovering the Layden jar], there was no single paradigm for electrical research. Instead,

> a number of theories, all derived from relatively accessible phenomena, were in competition. None of them succeeded in ordering the whole variety of electrical phenomena very well. That failure is the source of several of the anomalies that provide background for the discovery of the Leyden jar. (Kuhn, 1962, 61)

Here several potential paradigms were considered before one was settled on. However, it is important to keep in mind that once such a paradigm, or theory, or whatever, is settled on, independence is restored within the discipline. That is to say, a situation will again arise where the discipline is self-sufficient and can rely on its own exemplars rather than looking to other disciplines. In the Kuhnian perspective interdisciplinary transgression can therefore be driven by a perceived shortcoming of the discipline—one that is alleviated (in its most extreme case) by radical modification, after which independence and disciplinarity are duly restored. After a scientific revolution, the discipline is again isolated from its conspecifics.

Interesting problems arise here. For example, what constrains problem transformation? In normal science new problems are bested by casting them as versions of problems already solved. But what is an admissible transformation? To put this differently, supposing that disciplinary matrix D incorporates problem P_1 by transforming it to problem P_2, what is the relationship between P_1 and P_2? It seems reasonable to assume that the kinds of transformation that disciplines engage in to incorporate problems are not always identity preserving. Kuhn's many remarks on the puzzle solving nature of normal science indicate as much; science rarely tackles social problems head on as they are often impossible to solve. At best, heavily constrained versions are approached. Scientists working on the cure for cancer are usually engaged with something like the mechanism involved in specific mutations of a specific type of cell, or with trying to establish the risks associated with some carcinogen, or with some other small detail. But if transformations are not identity preserving, why cannot all matrices solve all problems?

To sum up, then, Kuhn does not distinguish sharply between problems and disciplines. Disciplines are instrumental in establishing a cumulative and efficient problem solving process. The Kuhnian framework suggests an interpretation under which normal science is disciplinary and interdisciplinarity is confined to periods of scientific revolution.

2.3 The Challenge of Interdisciplinary Problem Solving

Popper's and Kuhn's views on disciplines and problems can be of interest to a student of interdisciplinarity for many reasons. First, they provide an interesting contrast with one another, as they draw up two very different visions of the interdisciplinarity/disciplinarity dichotomy. Second, they are fruitful in that they help us to understand the challenges and problems that arise from disciplinarity and interdisciplinarity. And third, they work jointly as a model of the ideals and obstacles encountered by contemporary interdisciplinarians. I shall now proceed to discuss these partially overlapping points in order.

2.3.1 A Comparison

So, let us now compare Popper's and Kuhn's conceptions of disciplines and problems. Popper thinks that problems are, in every significant way, independent of disciplinary structures. Although they may arise out of theories associated with one discipline or other (and thus belong to the relevant discipline), solving them can be an interdisciplinary affair. This hard separation of problem solving and disciplines leads one, on a Popperian conception, to think of all proper science as *procedurally* interdisciplinary. That is to say, a problem may *happen* to be soluble with the tools available to a single discipline, but this is a contingent fact, so every inquiry needs to proceed in a manner that leaves it open to the possibilities of interdisciplinarity. One way of thinking of disciplinary science is thus as an outcome in which only one discipline was sufficient to solve a problem. This makes the disciplinary/ interdisciplinary distinction philosophically inert and largely unimportant. Another possibility is to think of the distinction in the way suggested above, in which case the notion of a disciplinary science becomes an oxymoron. Disciplinary science does not pay attention to this procedural openness, and therefore it does not qualify as science proper. Regardless of this, *interdisciplinarity* in this procedural sense is the ideal, and one that is realized often enough.

Kuhn, on the other hand, in assuming a much less rigid distinction between problems and disciplines, ends up with a sharp distinction between disciplinary and interdisciplinary science rendering the former, not the latter, the norm. For Kuhn a discipline is always guided by its earlier achievements. If two disciplines attempt to solve the same problem they immediately reformulate and rephrase the problem so as to make it look like problems already solved in the respective disciplines. One suspicion here is that the identity of the problem in question cannot be maintained during this process of transformation. That is, when the disciplines recast the shared problem we end up with two new problems that are neither identical nor even sufficiently closely related. Moreover, even if a significant relationship—whatever that might be—holds between the two new problems, it is still the case that the two disciplines are independent. If the transformation succeeds, it follows that the discipline can solve the problem, as it were, on its own.

Where Popper represents interdisciplinarity as rather easy, or in any case no harder than science in general, for Kuhn it becomes all but impossible. The disciplines are trapped on their own trajectories and will follow them until they eventually collapse. Even if overlaps are perhaps not impossible, they never motivate interaction. Taken alone, both pictures are unattractive. Let us discuss Popper first.

2.3.1.1 A Popperian Conception of Interdisciplinarity

Popper's argument is appealing for many reasons. For one thing, although he was perhaps mostly concerned with normative questions, there are reasons to think that Popper's claims are descriptively accurate—at least, in the sense that disciplinary boundaries are, in many respects, quite permeable and such that ideas can often be transferred freely across them. The sociologist Timothy Lenoir writes:

> Scientists at the research front do not perceive their goal as expanding a discipline. Indeed, most novel research, particularly evident in contemporary science, is not confined within the scope of a single discipline but draws upon the work of several disciplines. If asked, most scientists, I assume, would say they work on problems. Almost no one thinks of her- or himself as working on a discipline. (Lenoir, 1993, 77)

There are also more concrete examples of this permeability. Stephen Kellert (2008) has shown that what is popularly called 'chaos theory' has spread from its roots in climate science to both law and literature studies. Such imports are naturally associated with certain hazards, and they are not always successful, but the example shows that individual scientists are not always slaves to their disciplines.

Moreover, from a normative, rational point of view it seems intuitive: when presented with a problem, it is an error on the part of the problem solver simply to assume that a certain set of tools are the most appropriate. This is also how interdisciplinarity is often discussed, as was pointed out in the beginning of this chapter. Interdisciplinarity is a return to problem solving, away from myopic disciplinarity.

Importantly, here, although proper science is something the Popperian is committed to regarding as interdisciplinary, it is only interdisciplinary in a procedural sense, taking the form of an openness to contingencies to which scientists should be receptive. Although tolerance and humility are probably virtues in an interdisciplinary context, there is more to interdisciplinarity—and especially collaborative interdisciplinarity—than that.

This notwithstanding, two concerns can be raised. One is that the Popperian conception trivializes interdisciplinarity. Many scientists engaged in interdisciplinary collaboration will certainly vow to the contrary. Deep challenges of the sort that are difficult to overcome are involved in the genuine crossing of boundaries. Kellert mentions a few such challenges associated with borrowing. For instance, as the example of chaos theory shows, the importers often only have a superficial understanding of the context into which that which they import originally fitted. This may be important in order to obtain a proper idea of the conditions of validity of the framework in question. Other well-known problems cluster around the establishment of mutual understanding. As Donald Campbell (1969) once noted, the inwardly directed communication of disciplines—their *ethnocentrism*—quickly leads them to isolate themselves from one another linguistically. To overcome this can be difficult, and sometimes it involves developing a whole new, common language (Galison, 1997; Collins *et al.*, 2007).[7]

The second concern involves a problem of classification. If the Popperian view of interdisciplinarity is indeed construed this way,

[7]See also Chapter 5.

then any instance of good science is also an instance of interdisciplinarity. So, one wonders, are the cases of exemplary science to which Popper frequently returns, such as Einstein's theory of general relativity or Newton's celestial mechanics, really what contemporary interdisciplinarians are looking for?

The last observation that needs to be made about the Popperian view is this: it does not provide us with much in the way of an analysis of the difficulties associated with interdisciplinarity. What it does provide is a rational underpinning for boundary crossing, tolerance and open-mindedness.

2.3.1.2 A Kuhnian Conception of Interdisciplinarity

What is the appeal of Kuhn's position? One attraction is that it gives us a way of analysing cases that are promising (the Solow case described earlier is perhaps just such a case). Thus one way in which the Kuhnian model may appeal is also its descriptive adequacy. The transformation of Solow's maintenance of consumption problem into a sustainability problem may well be controversial within other disciplines, but in reality it illustrates just how disciplines always go about solving problems.

Despite this potential descriptive appeal there are at least two problems. The first is that it is not obvious what the normative implications of Kuhn's position are. One reason we might be interested in interdisciplinarity as philosophers is because we want to say something about when, and under what circumstances, it fails or is successful. Within the Kuhnian framework it is decidedly difficult to do this. Determinations of whether a problem is, or is not, solved are ultimately dependent on the disciplines themselves. The validity of a solution to a problem within a discipline does not seem to reach beyond the boundaries of that discipline; it can be challenged on a number of different grounds in a different discipline. There are no general criteria to hand to be used, and framing some, for example, in terms of robustness, is quite problematic—especially as it seems that both the criteria connected with determining whether or not a problem is solved and the criteria governing permissible transformations of problems are bound to disciplines. All we can do, it seems, is to conclude that whatever the problem is that can be transformed in such a way that Solow's contribution is a solution for economists can be transformed into other problems for other disciplines in such a way that Solow's problem is not a solution. At least, we can do this if we assume no other mistakes have been made. This leads us to the second problem. One may harbour doubts about the descriptive accuracy of Kuhn's remarks. There seem to be plenty of examples of problems that are jointly solved by conglomerates of disciplines within the confines of normal science. Let us briefly review another example. The problem of describing the circulation of CO_2 in the climate system involves input from a number of different disciplines. Ocean water has a rather complicated chemistry and acts as a buffer solution, and it therefore dissolves much less CO_2 than it otherwise would. CO_2 is stored in geological and biological deposits which circulates at extremely differentiated rates. The mechanisms involved here are chemical, biological, geological, and physical. How are we to think of these cases? Are they all examples of scientific revolution? Probably not. But then, what are they? Here a growing literature has emerged in which boundary objects (Star and Griesemer, 1989) trading zones (Galison, 1997) and interactional expertise (Collins and Evans, 2002) are invoked to account for such contacts.

2.3.2 Obstacles to Integration

Another way of approaching Popper and Kuhn in this context is to look at what types of problem, or challenge, each view presents to those interested in engaging in interdisciplinarity. Here Kuhn's account is richer, but there are still lessons to draw from the

Popperian view. Although the challenges facing interdisciplinary-minded scientists under the Popperian framework are the very same challenges that face any scientist, they may be compounded by the actual state of affairs in science. This has nothing to do with incommensurability, but is rather the upshot of more mundane features. Science has been growing in a number of distinct dimensions exponentially since its advent (see e.g. Weingart, 2010). Derek Price once remarked that 80-90% of all the scientists who have ever been around are active right now, and that it has been like that since the dawn of science (Weingart, 2003, 184). In 1830 there were about 300 scientific journals; in 2003 the number was somewhere around 40 000 (Weingart, 2003). The point here is that the amount of work produced cannot be surveyed by any single individual without aid. Thus, if we are to believe Weingart, science has been compartmentalized. This compartmentalization could have been carried out according to any number of different principles, but since it appears to be strongly self-reinforcing, a number of challenges arise. Some concern precisely the principles involved in the division itself, others, how to keep these compartments from committing to various errors. Donald Campbell's fish-scale model of interdisciplinarity, where individuals work with interlocking expertise, is an example of a system that promises to address both issues (Campbell, 1969; Klein, 1990).

On the Kuhnian picture interdisciplinarity is much more challenging. As already pointed out, although overlapping matrices are not precluded, they appear to have no procedural impact. That two disciplines share values, or even an exemplar, is no reason for them to interact. All problem solving is still handled internally. We have already noted what is perhaps the most pressing issue facing the interdisciplinary-minded scientist in the Kuhnian framework: how interdisciplinary problem solving and disciplinary problem transformations are to be constrained.

2.3.3 Popper's Ideals, Kuhn's Problems

Now, can the suggestions discussed above somehow help us model contemporary calls for interdisciplinarity? It would seem so. For example, it is striking how well Popper's ideal science coincides with the ideals of many proponents of interdisciplinarity. What Brewer's cynic appears to want is precisely for scientists to *follow the problems*. That is, in itself, an interesting observation, since Popper seemed to think that this ideal was, more or less, realized in the science of his time—not *always*, naturally, but nonetheless often enough. Presumably Popper saw himself as a problem follower. This prompts the question, has anything in particular happened in the sciences since the 1970s when Popper made his remarks that has significantly changed its character? As contended above, science has most certainly *grown* in many respects, but whether it has reached a threshold since then that changes science radically is more uncertain. Or perhaps the question is misstated. Many of Popper's favourite examples of science are somewhat distant: Galileo, and perhaps even Einstein, worked in a context quite different from the present one. For example, in the contemporary discussion interdisciplinarity is commonly conceived of as having a strong social component, in that it is imagined as a phenomenon that spans disciplines in terms of both of their cognitive and their social/institutional features. A project like sustainability science is meant to involve not only the cognitive contents of the natural and social sciences, but also natural and social scientists. Popper, perhaps, was more concerned with individuals drawing on various resources from different places. In such a perspective many of the issues discussed in the contemporary literature—such as aforementioned Galison's (1997) interdisciplinary languages and the interactional expertise of Harry Collins (2002)—never really materialize. Perhaps frameworks matter more when ideas cannot migrate together with individuals. It is not an implausible suggestion, and it gains some traction in the literature (see Weingart, 2010, 2003).

If we can think of interdisciplinarian ideals as Popperian, perhaps we can conceive of the short-comings interdisciplinarians perceive in 'traditional' science as Kuhnian—that is, see interdisciplinarians as seeking to realize Popper's ideals in a Kuhnian world. This proposal has a certain allure. For one thing, we may get a sense of what the issue is with *disciplinary* science. As the Solow case indicates, the transformations that disciplines undertake in order to obtain a workable problem formulation can sometimes be highly controversial. It seems that, although it is not always easy to specify precisely what is wrong with such a transformation, the solutions that emerge are rather limited in scope. This gives meaning to the charge of that the disciplines are parochial. Moreover, the idea that transformations are *necessary* hints at the depth of this issue. It is not just about breaking the ranks and changing practices. After all, the transformation process generates problems that are *possible* to solve from those that are more or less impossible to solve.

Crucially, this does not necessarily mean we must accept Kuhn's picture as universally valid. As already pointed out, there is more than enough reason to hesitate. Nor, moreover, is it necessarily the case that these issues *always* arise in interdisciplinary collaborations. It may well be that occasionally interdisciplinary collaboration goes smoothly, not because these challenges are overcome, but because they never arose in the first place. However, there seems to be no such luck in the case of sustainability science.

Interestingly, it is on this point Popper, Kuhn, and interdisciplinarians all seem to converge. Kuhn often remarked about the kinds of example that Popper preferred that they were not instances of normal science, but extraordinary science (see e.g. Kuhn, 1965). If we grant Kuhn's remarks on this issue, then Popper's ideals too, are not the ideals of normal science, but of scientific revolutions. Indeed, it is under such circumstances that Kuhn himself allowed for a kind of interdisciplinary consideration to take place. The Leyden jar example shows that. The problem, then, for the interdisciplinarian is to show how Popperian ideals can be reached within a Kuhnian predicament.

2.4 Looking Ahead

Now, let us glance at the chapters to come. The discussion of the distinction between problems and disciplines is an important backdrop, because a central theme in the pages to come revolves around problem transfers and problem sharing—here called *problem-feeding* (Thorén and Persson, 2013). If interdisciplinary collaborations are to proceed, the transfer of information between them is quintessential, and sometimes that exchange concerns problems. Such exchanges are highly important in many forms of interdisciplinarity, as will be argued at length in the next chapter. A leading idea is that problem solving is, at least at times, a process that involves both deepening the understanding of a problem *and* transforming it to fit the resources available. In interdisciplinary problem solving both of these features need to be taken into account. A framework will be developed in which one can model such transformations between disciplines more precisely.

If we think of the transformation process involved in incorporating new problems into a discipline as one, at least to some extent, of fitting the problem to the resources, interesting challenges emerge for the interdisciplinarian. For one thing, we obtain some explanation as to why interdisciplinarity so often appears to end up in 'mere' multidisciplinarity. Unless there is coordination of the transformations of problems at some level, different disciplines will interpret problems according to the resources they possess. The question thus arises, what about situations where such problems do not arise? A recurring topic in this thesis concerns precisely the sharing and shifting of problems as an important and useful form of interdisciplinary exchange. The central question then becomes, not one of how to interpret Kuhn, but how the isolationist tendencies are overcome in practice. In particular, what degree and type of integration is a prerequisite of such exchanges?

Another issue that will be addressed concerns the ‘wayward’ side of information exchange: disciplinary imperialism. Although interdisciplinarity is again very much in vogue—as it has been a number of times in the past—the frequent intrusions by disciplines into one another’s intellectual domains are often fiercely resisted. In chapter 4 this topic will be discussed in detail, and an account based on the problem-feeding framework will be proposed.

Chapter 3

Sharing and Transferring Problems

3.1 Introduction

IN THIS CHAPTER the discussion will focus on certain features of joint problem solving between disciplines, and in particular on problem sharing and problem transfer. That joint problem solving will often, perhaps always, involve the sharing of a problem—at least, at some level—seems intuitive. The transferring of problems can be seen as one way of attempting to realize Popper's ideals in certain contexts. Popper, as we have seen, advocated that scientists should *follow the problems* and thus feel free to draw on the resources of any discipline in order to solve whatever problem they might be working on. Sometimes, however, that practice may not be rational or even feasible. Perhaps the resources in question are too complex or difficult to grasp, or perhaps there is simply insufficient time to wait for the problem solver to gain mastery of the required assets. Then it seems more rational to export the problem, rather than import the tools. Instead of scientists and researchers venturing out to acquire whatever tools they need, they might instead out-source the problem solving by transferring the problem itself to someone already in possession of the right tools. Moreover, problem transfer is of both general importance, as it is fundamental to problem sharing, and specific interest to sustainability scientists.

This chapter will be structured as follows. First, I will introduce the problem-feeding model for interdisciplinarity. This idea recurs in the papers that constitute this thesis. It departs from the notion that sometimes problems arise within a discipline, or field, although they cannot be solved within that context. In such cases the problem can be transferred to a context that is more appropriate. In the section that follows some general features of sharing and transfers will be discussed, and I will then approach the issues raised by Kuhnian challenges outlined above.

3.2 Problem-Feeding

The *problem-feeding* model (Thorén and Persson, 2011, 2013; Thorén, 2015a) builds on the intuition that, sometimes, problems arise, or are discovered, in a discipline where they cannot be solved. In such circumstances there are two rational courses of action: one is to import the resources to solve the problem, the other is to export the problem itself. Problem-feeding takes the latter form.

The importance of transferring problems has been highlighted before. In an early and influential contribution to the literature on interdisciplinarity, Muzafer and Carolyn Sherif (Sherif and Sherif, 1969) analyse the following example. In the metabolic ward of Dr. William Schottstaedt it was discovered that a range of physiological measures related to the metabolism of the patients correlated with the "vicissitudes of interpersonal relationships among patients, with nurses and doctors, and with visitors" (Sherif and Sherif, 1969, 6). Not only that, one could observe connections between metabolic measures and a range of social features—the patients' financial situations, their family background, and so on. Thus it turns out that the metabolic issues that Schottstaedt took to be wholly within his medical expertise actually required demographic and social investigation. Sherif and Sherif do not reveal the outcome of this realization, but it is easy to see the opportunity for exporting the problem.

Another notable case illustrating the transfer of problems features in Darden's and Maull's (1977) seminal paper on interfield theories. Interfield theories are essentially hypotheses about the 'ontological' connection between fields. An example is when one field studies the function of a structure studied in another field. The interfield theory, then, is precisely the supposition that the two fields are connected in that way. The chromosome theory of the gene is an example of interfield theory, as it places the gene—an entity stipulated in Mendelian genetics—on the chromosome, an entity observed by cytologists. Darden and Maull connect the establishment of interfield theories with the transfer of problems. They write:

> In brief, an interfield theory is likely to be generated when background knowledge indicates that relations already exist between the fields, when the fields share an interest in explaining different aspects of the same phenomenon, and when questions arise about that phenomenon within a field which cannot be answered with the techniques and concepts of that field. (Darden and Maull, 1977, 50)

The idea was further elaborated in a paper from the same year by Nancy Maull (1977). She approached the issue somewhat more explicitly under the label of *problem shifts*.

> It is possible for problems to *arise* within a field even though they cannot be *solved* within that field. Their solutions may well require the concepts and techniques of another field.

> In this case, we say that the problem "shifts". (emphasis in original Maull, 1977, 156)

Maull argues that what she calls proper terms can connect fields (in her case on different levels of description). A term is shared between fields when it is, what she calls, a *proper term* of both fields. Maull deploys this notion somewhat differently from the way it has been understood by others. She does not think of this a semantic relationship. In fact she explicitly distances herself from debates on meaning change. Rather (as far as I understand Maull here) she focuses on the presence of a common *phenomenon* to which both fields have epistemic access. This allows the resources of both fields to be deployed in exploring that phenomenon. Thus the *knowledge claims* associated with the term in question can be modified and revised from several fields. In this way a problem—such as accounting for the concrete nature of mutations—can be 'shifted' from one field to another.[1]

In both of these papers more specific issues are discussed than we are interested in here. In Darden and Maull (1977) the focus is on the relationships obtaining between fields, whilst for Maull (1977) the aim is to spell out relationships between fields that are on different levels of description. I will therefore refrain from using, for instance, Maull's notion of a problem shift, as it is so strongly associated with her framework. I will instead deploy the label 'problem-feeding', since it is meant to be broader, and to include both fields and disciplines (to the extent these are in fact distinct) and also a range of other types of context.

[1]Maull does not, in her paper, refer to Kripke's modal semantics and the notion of rigid designation (see Kripke (1980)), an idea that immediately comes to mind when one reads her. It is thus hard to say to what extent her ideas and Kripke's are comparable. See also Thorén and Persson (2015) for a discussion on the role of definitions and rigid designation in the context of sustainability science.

Problem-feeding can occur in many ways, but one important distinction that can be drawn is between *unilateral* and *bilateral* problem-feeding. The former concerns cases where one discipline depends on another for problems, but there is no reciprocity in the exchange. Todd Grantham (2004) mentions a form of practical integration he calls *heuristic dependence* of which this notion of unilateral problem-feeding is reminiscent. For instance, philosophers of physics may draw on physics to find interesting problems. However, these problems are not always problems that physicists themselves think of as important. That is to say, presumptive solutions are unlikely to be fed back. In bilateral problem-feeding, on the other hand, there is a component of division of labour and reciprocity. A discipline encounters a problem that is perceived as important but resists solution within the discipline. The problem is thus outsourced to an appropriate alternative discipline or field, and when it is eventually solved the solution is fed back into the discipline of origin. In Darden's and Maull's examples the type of problem-feeding that is occurring is bilateral. Again, the chromosome theory of the gene is a good example. The gene (or *factor*) had been stipulated in Mendelian genetics, but its physical nature was not known. As the gene was found to be located on, or in, the chromosome, Mendelian geneticists could use this to explain why assortment is not perfectly random. Genes close to one another tend to be inherited together, and this skews the ratios slightly (see Darden, 1991; Thorén and Persson, 2013). In the remainder of this chapter the focus will be on bilateral problem-feeding.

Prima facie the problem-feeding model is of immediate relevance to sustainability science in particular, as that field can itself be said to be founded on an attempted problem transfer. The recognition that, for example, climate change is an issue of concern to both natural and social sciences was originally made by ecologists and climate scientists.

The model is also of more general relevance, as problem-feeding is fundamental to all kinds of problem sharing. Here is a general argument for this point. Let us assume a minimal case. Two disciplines, D_1 and D_2, are to be involved in solving problem P. Here either P is recognized, or taken note of, by both disciplines, or it is not. In the latter situation the case is trivial—the problem has to be transferred. In the former it appears that in order for D_1 and D_2 to recognize that they should both be involved in solving P there needs to be a mutual transfer of both versions of the problem so that the comparison can be made. Thus the transfer of problems between disciplines is fundamental to all types of joint problem solving.

Before we conclude this section it is important to say something about how problems can decompose into sub-problems. In situations of joint problem solving it will often be the case that an overarching problem is shared as a problem that falls apart into smaller sub-problems that can be solved individually, in the interdisciplinary case, by different disciplines. With such problems the solution to the overarching problem is the aggregate of the solutions to the sub-problems.[2] Hence the transfers concern both the overarching problem and, many times, the various sub-problems. Alan Love (2008; see also Brigandt, 2010) has used the notion of *problem agendas* to describe this kind of situation.[3] Love (2008) distinguishes problem agendas from individual problems, the latter of which can be either empirical or conceptual (see Laudan, 1977). He writes:

> A problem agenda, by contrast [to an individual problem], is a "list" of interrelated questions (both empirical and conceptual) that are united by some connection to natural

[2]Contrast this with cases where solutions from different disciplines are all complete solutions to the problem (and thus competing).

[3]See also Mitchell (2002, 2003, 2009). Her account of integrative pluralism, which is discussed in Chapter 5, is clearly relevant here.

> phenomena. For example, how do questions concerning greenhouse gas contributions from plant respiration, along with many other questions about emission-related phenomena (anthropogenic or otherwise), including their interaction with systematic cycling and atmospheric dynamics, get answered with respect to global warming phenomena? Problem agendas are usually indicative of long-term investigative programs and routinely require contributions from more than one disciplinary approach. Cross-disciplinary interactions of this kind rarely occur spontaneously and are often driven by a commitment to similar questions. (Love, 2008, 877)

Although Love does not focus on transfers of problems directly, some transferring needs to be occurring in order for a problem agenda to be established in the first place.

There are plenty of examples of what appears to be problem-feeding and there is a good rational basis for problem-feeding as a practice, but issues arise over exactly how problems are to be transferred—at least, if we are to take the Kuhnian concerns raised in the previous chapter seriously.

3.3 Two Phases of Problem Solving

Where in the problem solving process does problem-feeding occur? Let us begin by taking a step back and discussing problems and problem solving more generally. Thomas Nickles (1980; 1981) suggests we think of a problem as follows:

> A problem is a set of constraints (better, a constraint structure) plus a demand that the object (or an object, etc., depending on the selection properties of the demand) delimited or 'described' by the constraints be obtained. (Nickles, 1981, 31)

The constraints in question concern admissible solutions to the problem; they tell us what the solution should look like. We will use a definition based on Nickles' account:

Definition: A problem is a pair $\langle C, D \rangle$ where C is the set of all constraints on the solution(s) to the problem, and D is a demand that the problem be solved.

First, a general remark. This definition differs from other suggestions as to the nature of problems in that it does not construe problems in terms of the admissible answers themselves (see e.g. Belnap and Steel, 1976). The main advantage of this is that it offers a way of understanding how features of a solution may be *known* to a problem solver without that problem solver actually *having* the solution. The idea that we can maintain a distinction between having knowledge of a problem and having the solution to the problem is highly intuitive, not least because not all problems have solutions despite appearing to be well understood. For example, producing an analytical solution to the n-body problem by reducing the dimensions of the system using first integrals turned out to defy solution not primarily because we have no idea what such a solution would look like, but because there is none (see Diacu, 1996).

Second, Nickles' conception of a problem is very abstract, and the notion of a constraint is quite broad. For one thing, there are clearly different kinds of constraint, and one might find it useful to differentiate between them on occasion. Some constraints are open, as Reitman (1964) famously remarked. Some are explicit, and some are implicit. Some are necessary, others redundant or peripheral, and so on. Moreover, there may be relevant differences between different types of problem (e.g. producing a formal proof, explaining some hitherto unexplained phenomenon, predicting an event, the concrete operation of shifting a system from one state into another, and so on). These potential differences become muted when the above definition is accepted.

What precisely are these constraints, then? Nickles' paper is curiously void of examples, but hints can be found in Love's paper (Love, 2008). Love uses a somewhat different terminology. Rather than discussing constraints, he associates problems with *criteria of explanatory adequacy* which are necessary in order to assess whether a solution is acceptable or not. He mentions examples of such criteria—e.g. "logical consistency for a conceptual problem, or the need to include a causal factor when addressing an empirical problem" (Love, 2008, 877). Precisely what constraints are associated with specific problems is, as we shall see, often contentious, but consider the following. Suppose the resilience theoreticians are right in their thinking about things such as sustainability and sustainability transitions. If they are indeed so, then the problem of making a social-ecological system sustainable is really about making it as resilient as possible in the face of certain types of disturbance. This is in itself a massive constraint on admissible solutions: if problems of sustainability are really about resilience, their solutions will have to be put in terms of certain types of mechanism and interaction. For example, relevant events (such as collapses and regime shifts) are to be explained in terms of structural features, like interactions between driving variables and parameters in the system. Such constraints exclude large swathes of possible solutions.

Some constraints, clearly, appear to be more closely related to specific disciplines. For example, the expectations we have about what it would be for a physicist to have solved a problem may be quite different from the expectations with which judge a literature scholar. There are, within each of these disciplines, restrictions regarding what solutions in general are allowed to look like. These will sometimes be trivial in the sense that they are not particularly exclusive—a matter of form only, perhaps. At other times they may be highly exclusive. Think only of the propensity of economists to solve problems within a formal framework. The precision offered by the method comes at the cost of often having to deal in obtrusive idealization.

Now, to return to the process of solving problems. Roughly speaking, we can, on this definition, discern two phases in the problem solving process. In the first, the *exploratory phase*, the problem itself is the immediate object of enquiry. The aim here is to acquire a sufficient understanding of the problem—that is to say, to reveal the set of constraints C that is constitutive of the problem we are trying to solve. In the second phase, what might be called the *derivational phase*, the aim is to obtain a solution given a full, or sufficiently articulated, set of constraints C. Let us abbreviate these phases as *phase-1* and *phase-2*, respectively.

Further, we need to distinguish between the problem itself and problem formulations. The problem itself is an abstract entity that 'objectively' exists given some theoretical contexts. Problems arise in two ways: out of tension between, say, some expectation, or ideal—for example, a theory—and some perceived state of affairs, such as an observation; or out of inconsistencies between two theories. These correspond roughly to Laudan's distinction between empirical and conceptual problems (Laudan, 1977). A problem can exist without being noticed or acknowledged. For instance, inconsistencies between the consequences of some theory and certain observations are not always immediately obvious. A *problem formulation* is thus a representation of a problem, and it can be more or less accurate. Phase-1 involves producing increasingly accurate problem formulations successively until a formulation is reached that is sufficiently precise that the process can move into phase-2.

The solution to a problem P is a function of the set of constraints $C \in P$. Here we will write $S(P)$ and by that designate the solution, or solutions, to the particular problem P. In Equation 3.1 $P(x)$ are successive problem formulations. $P(0)$ is a problem formulation from which a solution can be obtained. In the ideal case it is either identical to the problem it represents or otherwise accurate enough as to pick out a solution that is also a solution to the problem.

$$\overbrace{P_n \rightarrow ...P_2 \rightarrow P_1 \rightarrow}^{phase-1} \underbrace{P_0 \rightarrow S(P_0)}_{phase-2} \tag{3.1}$$

What does this tell us about interdisciplinary problem solving? Before we attempt to answer this question we shall make an assumption: most problem solving work takes place in the explorative first phase—as Simon once put it, "there is merit to the claim that much problem solving effort is directed at structuring problems, and only a fraction of it at solving problems once they are structured" (Simon, 1973, 187). That is, the explorative phase often takes up more resources and time than the derivative phase. We can therefore adumbrate problem solving in phase-2 rather briefly.

3.3.1 Interdisciplinarity in Phase-2

Suppose we have an interdisciplinary problem (i.e. a problem to which several disciplines have some contribution to make) P. Assume that in P phase-2 has been reached. Then it is the case: (a) that the set of constraints C of P is fully, or sufficiently, understood; and (b) that P is considered to be worth solving by members of all of the disciplines involved (i.e. D is met).

Intuition then suggests that the specific tools at the disposal of different disciplines—what Bechtel (1986) calls the cognitive tools (theories, methods, models, etc.)—are brought to bear on the issue at hand.

Examples of this kind of problem may involve producing explanations of complex phenomena where, for instance, different causes 'belong' to the domains of different disciplines. A homely example for the sustainability scientist would be problems relating to explaining changes in the climate system. Here the underlying causes of the kinds of event one is interested in—such as the gradual warming of mean surface temperature over the past century, or changes in the chemical composition of the atmosphere—belong to the domains of a range of different disciplines.

There are issues specific to phase-2. For example, to what extent do the disciplines need to overlap, and in what sense do they need to be different if the interdisciplinarity is to be genuine? However, here those problems will be put to one aside. Instead we shall move on to focus on the first phase of problem solving.

3.3.2 Interdisciplinarity in Phase-1

Two general arguments can be made for the potential benefits of interdisciplinarity in this part of the process. One relies on *adding* constraints (or providing more precise ones) in order to narrow the solution space; the other concerns the *revision*, or sometimes *subtraction*, of constraints in order to obtain, say, a more broadly valid, or in other words more robust, solution.

3.3.2.1 Constraint Addition

Here is one rudimentary, though intuitive, model. We have a problem P which is not fully formulated: C is not completely known. One consequence of an underdeveloped knowledge of C is that one cannot provide a sufficiently narrow solution space. An obvious way in which additional disciplines might contribute is by supplying more, or more precisely formulated, constraints. As these are applied the solution space can be successively narrowed until it is adequately downsized. A form of this model was suggested already in 1948 by Wassily Leontif as a way of conceiving of interdisciplinary relationships (Leontif, 1948). Leontif modelled

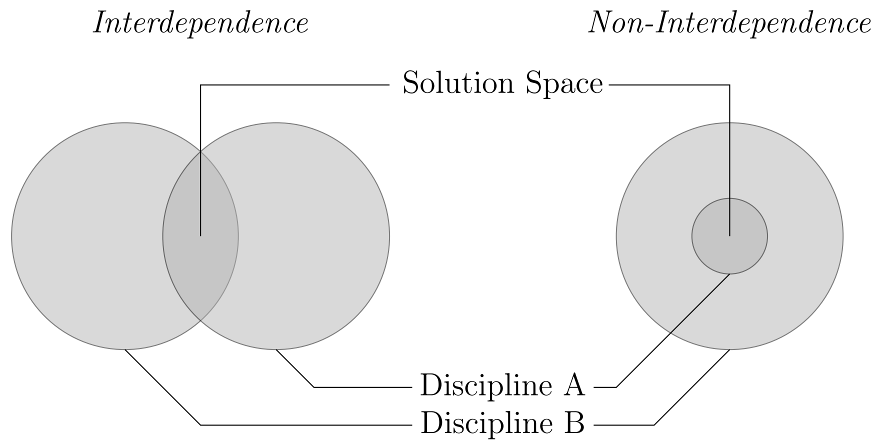


Figure 3.1: Two Interdisciplinary Relations

the situation with Venn diagrams overlapping in different ways (Figure 3.1). In situations where the intersection is neither empty nor identical to the solution space defined by either discipline (I call this *interdependence* in Figure 3.1) interdisciplinarity is warranted (even necessary) on Leontif's account. We can isolate at least two motives behind constraint addition. The first is *exploratory* and aims to reveal the constraints associated with the problem at hand. There are many examples of problems in which several disciplines have to be recruited in order to provide a proper understanding of *what* a solution might look like. One is: the problem of understanding how carbon cycles through the climate system cannot be completely solved unless one is able to integrate resources from physics, oceanography, chemistry, biology, ecology, at times even economics, just to mention a few.[4]

This particular problem involves a kind of compartmentaliza-

[4]See Weart (2003), Edwards (2010), and Bolin (2007).

tion of the problem space allowing the overarching problem to be sub-divided and solved individually. Interest in the influence of atmospheric carbon on the temperature of the earth was originally related to ice-age theory. Geological oddities, such as the misplaced boulders, drumlins and eskers which could be found around Europe, had been hypothesized to be the result of one, or several, ice ages. But if the earth had been considerably cooler in the past, then the climate system apparently could change. This prompted the question: how? Joseph Fourier had already, in the 1820s, suggested that gas concentrations may be involved in heating and cooling, as they could trap energy in the form of heat. John Tyndall, following Fourier's lead, proceeded to find candidates for such "greenhouse gases" and concluded that both CO_2 and ordinary water vapour qualified. In 1896 the Swedish physicist Svante Arrhenius produced the first quantitative climate model to describe the impact of various concentrations of CO_2 in the atmosphere on mean surface temperature at different latitudes (Arrhenius, 1896). But Arrhenius' model—although the estimates it produced, curiously, are actually close to current best guesses—was simplistic and could not accurately represent the climate system.

Arrhenius was himself well aware of some of these problems and noted, for instance, that the ocean was likely to play a role unaccounted for in the model. In actual fact, several important oceanic mechanisms were not properly understood until the mid-1950s, when the joint efforts of Roger Revelle, Hans Suess and Harmon Craig at the Scripps Institute connected the chemical properties of the ocean (specifically, the fact that it is a buffer solution) with its mechanical behaviour (the fact that horizontal turn-around between layers is very slow) (Revelle and Suess, 1957; Craig, 1957).[5] In other words, Arrhenius was not aware of all the

[5]One should also mention Bolin and Eriksson (1959) who spelled out the consequences of Revelle's and Suess' somewhat subdued point and made a

constraints involved in his problem; and these constraints would be added, one after the other, over many ensuing decades (the process goes on still). This process of adding constraints has been a distinctly interdisciplinary one: Revelle and Suess (1957) revealed an important mechanism in the oceans, others have described the role of biological matter, and so on.

The other motive is not exploratory, but pragmatic, and involves solvability as a virtue in itself. Certain problems are *open* or *ill-structured*, in that they do not appear to have a solution space that can be non-arbitrarily delimited (Reitman, 1964). The standard example here, which happens to be non-scientific, is the problem of composing a fugue. A problem solver approaching this issue will, in order to solve the problem, aim for something considerably narrower than the 'actual' problem. For any set of solutions to this problem, there will always be one that is not included in the set. Often enough, scientific problem solving shares this feature. A broad, or open problem, is reinterpreted as a more specific one. In a way, Arrhenius' numerical climate model is an example; the problem he was interested in was how CO_2 affected mean surface temperature. He treated this issue—via a range of different idealizations and simplifying assumptions—as a mathematical problem, and duly solved it. The problems that confront us in sustainability science are perhaps even more obvious examples. Again, the idea that Solow's argument that maintenance of consumption under situations of limited resources shows us how to realize a sustainable economy also involves introducing many further constraints on what is to be thought of as a solution. If sustainability is indeed a concept that it is impossible to capture precisely, as some have maintained, then it is necessary to introduce constraints in this fashion to procure a problem that it is so much as possible to approach. Let us call this type of arbitrary constraint addition *pragmatic*.

---

wider audience aware of its implications.

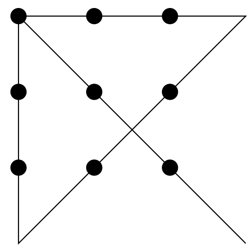


Figure 3.2: The Nine Dots Puzzle

For one reason or another it may be impossible, or very hard, for a single discipline to provide even arbitrary constraints to narrow the solution space of a problem sufficiently, and hence there may be reason to draw on several disciplines when solving an open problem. Interestingly, interdisciplinarity is at times seen as a virtue *in itself*, so that drawing on several disciplines is regarded as preferable to drawing on just one, even where that is possible.

3.3.2.2 Constraint Revision and Constraint Substraction

The focus here will be on revision. However, the subtraction of constraints is also a powerful way of solving a problem. There are classical examples of this, such as the so-called 'nine dots puzzle' (see Figure 3.2). The problem, generally, is that people who fail to solve the puzzle constrain the problem. They think that the lines to be drawn have to be confined within the area delimited by the dots, and this makes the task impossible to solve. In order to obtain the solution the tacit constraint has to be subtracted. In interdisciplinary problem solving a new discipline may highlight the fact that a constraint implicitly, or explicitly, encompassed in other disciplines is obscuring the solution. An interesting case in which this failed to happen is the discovery of nuclear fission (see Andersen, 1996). During the 1930s several research groups were working on the effects of bombarding uranium with neutrons—most notably Fermi's group, in Rome, and Meitner, Hahn and Strassman, in Berlin. In 1934 Fermi surmised that they had produced transuranic elements, and it was only with reluctance that Hahn and Strassman showed, in 1938, that this could not have been the case. The products of the nuclear reactions were not, as had been widely expected, larger elements, but smaller and lighter ones. Interestingly, the chemist Ida Noddack had sent papers to both these research groups in the mid-1930s pointing to this possibility, but her suggestions were dismissed. Andersen writes:

> Apparently, the chemist Noddack was not aware of the physical constraints on the taxonomy of disintegration processes and cared only for the chemical categorization which she found inadequate. Hence, she suggested a further chemical analysis to check if the elements produced by Fermi and his collaborators could be much lighter elements than the transuranic elements suggested by Fermi's team. What she here suggested was categorizing the elements as fission products—but at a time when the conceptual structure did not allow for the existence of nuclear fission. (Andersen, 1996, 485f)

A different, and rarely highlighted feature of interdisciplinary problem solving, works in the opposite direction. In phase-1 this takes the form of *constraint revision.* Here C is revised under the influence of several disciplines. An interesting function that revision might serve—and one that cannot be provided by constraint addition—is to open up new possibilities. This may be desirable when, for instance, problems are over-constrained and thus hard, or even impossible, to solve. Adding constraints can never increase the solution space, but revision can.

Given the suggested model, constraint revisions can take one of two forms. They can be *corrective.* Here enquirers are wrong about some constraint and revise it in light of new, and more accurate, information. Or, where the problem altered, they can be *transformative.* Here it is a matter not of knowledge, but rather of producing a new problem on the basis of an old one. Transformative constraint revision is explained by reference to the values encompassed within a disciplinary matrix. A new problem is produced that is considered to be more interesting than the one from which it was produced. Often the motivation for transformation lies in trying to produce a problem it is possible to solve given the resources available to the problem solvers. One may consider Solow's argument concerning the maintenance of consumption as a case in point. The overarching problem of sustainability is, for reasons quite trifling, impossible to solve. It is simply too broad. However, by radical transformation a formal problem can be extracted—namely, that of the maintenance of consumption under very specific circumstances for very specific economics—which can be solved by means readily available to economists.

Thus we can introduce a further distinction. Let us separate *epistemological* and *ontological* over-constraining. Over-constraining in general involves situations where the set C makes P either impossible or extremely difficult to solve. What we will here call epistemological over-constraining occurs in situations where C, erroneously, has been made out to be inconsistent, or includes constraints that are too prohibitive. Such a situation is ameliorated by corrective constraint revision. Hence epistemological over-constraining is a property of the problem formulation, rather than the problem itself—it implies that we made a mistake when trying to spell out the constraints constitutive of the problem we intended to solve. Ontological over-constraining, on the other hand, means that the problem itself is in some way too narrow. It may, for instance, be impossible to solve, in which case we might salvage a problem that it is possible to solve by manipulating C. Again, the n-body problem appears to qualify; the requirement that the solution to the problem has to be of a certain kind simply means that the solution space is empty. This can be resolved by transformative constraint revision; in this case one might, for instance, permit numerical solutions.

3.4 Non-Ideal Problem Solving Processes and Problem Stability

Suppose we think of an *ideal* problem solving process in phase-1 as a process that involves only explorative constraint addition and corrective constraint revision. In the interdisciplinary variety of this type of problem solving different disciplines are thus drawn from in order to contribute to, and revise, the problem so as to provide a formulation of the problem that is, eventually, identical with what the problem really is. This is, essentially, Popper's ideal for an interdisciplinary science if we apply it to phase-1.

Are there good examples of ideal problem solving processes? It is hard to say. First, it is rather difficult to establish whether a specific process was indeed ideal or not. To begin with, meticulous historical study would be required. There is also reason to think that problem solvers who present their results *ex post facto* may be inclined to tell the story as if it was an ideal process. Nonetheless, certain problems are extraordinarily stable and may therefore provide plausible examples. Interdisciplinary ideal processes may be harder to come by, but looking at disciplinary varieties, perhaps some logical and mathematical problems, and the processes that led to their solution, may provide a source of examples. A spectacular potential example is Andrew Wiles' proof of Fermat's Last Theorem. Although the problem had been clearly stated for centuries, it is quite clear, in light of Wiles' eventual proof, that those who attempted to prove the theorem before him were not aware of its full complexity. Crucially, however, Wiles did not solve some variety of the problem, nor did he transform it into some other, different problem. He solved exactly the problem he set out to solve. Hence the process seems to be one in which the set of constraints were successively uncovered by means of explorative constraint addition and corrective constraint revision.

There are several things to note about such a process, but one remark which can be made in light of the example above is that it can only work with a specific type of problem: one that is neither open nor ontologically over-constrained. We will return to this below.

A non-ideal problem solving process in phase-1, then, is a process which is not limited to explorative constraint addition and corrective constraint revision. It is a process involving either pragmatic constraint addition or transformative constraint revision, or both. For the most part this type of process will also feature explorative constraint addition and corrective constraint revision; and hence it will typically be a process in which our understanding of the problem is improved whilst the problem is, simultaneously, transformed.

Let us now move on and talk about problem stability.

3.4.1 Procedural vs Cross-Boundary Problem Stability

In an *ideal* problem solving process the problem is stable; the process is always directed on the same problem. In other words, diachronic, or perhaps more appropriately *procedural*, problem stability is maintained throughout the process. Sometimes this stability can be provided by the problem itself: that is, it is a non-open problem that is not over-constrained, but having such a problem is no guarantee that synchronic problem stability will be retained. For example, the problem solvers may find that some transformation of the problem they set out to solve is more interesting and thus change their focus.

In phase-1 of any problem solving process synchronic problem stability will either be maintained or (to some extent) lost. However, in interdisciplinary problem solving processes another type of problem stability becomes crucial. This is *cross-boundary stability*. This type of problem stability is about maintaining problem identity across disciplinary boundaries, or more broadly, across relevant contexts.

Trivially, for an interdisciplinary problem solving process to solve the problem *it set out to solve* both procedural and cross-boundary stability need to be maintained. Clearly, this is neither always possible nor always desirable. Many problems are open, to some extent, and some are ontologically over-constrained. But it appears that an interdisciplinary problem solving process can be successful, at least according to its own standards, as long as cross-boundary problem stability is maintained.

Interestingly, the notion of cross-boundary problem stability offers a way of distinguishing that which is merely multidisciplinary from that which is interdisciplinary. In the previous chapter we contrasted interdisciplinarity with disciplinarity. As noted in Chapter 1, however, interdisciplinarity has commonly been contrasted with multidisciplinarity. To rehearse, the idea is that the latter involves the "juxtaposition of various disciplines, sometimes with no apparent connection between them" (Apostel *et al.*, 1972, 24), or is "essentially *additive*, not *integrative*" (Klein, 1990, 56). Interdisciplinarity, on the other hand, is integrative. My suggestion here, therefore, is that we should understand matters in the following way: multidisciplinary problem solving as a process in which cross-boundary problem stability is not maintained, whereas in interdisciplinary problem solving it is. Hence, in the multidisciplinary case there is no knowing how the solutions that eventually come out of the involved disciplines actually relate to one another.

In other words, cross-boundary problem stability is *necessary* for a problem solving process to be interdisciplinary. Intuitively, this is what it means to *share* a problem.

A final remark on procedural problem stability. Although an ideal problem merely provides an opportunity for an ideal process, it is the case that if the process is indeed ideal, and thus that procedural problem stability has been maintained, then cross-boundary problem stability just follows, trivially. Otherwise, the process cannot have been ideal after all.

3.4.2 Problem-Stability and Bilateral Problem-Feeding

We can also use this notion of cross-boundary problem stability to improve our understanding of problem-feeding. Maintaining problem stability necessarily involves transferring problems between disciplines. That much was already established at the outset. Thus any interdisciplinary problem solving process will involve problem-feeding.

It seems we are now in a position where the notions of cross-boundary problem stability and bilateral problem-feeding can inform one another.

Suppose we draw a sharp distinction between procedural and cross-boundary problem stability. One might object to such a distinction on the grounds that transferring a problem is in itself a part of the process of solving the problem. It involves formulating theories about how the disciplines (or fields) in question are connected, some type of sharing of terms, and so on. Thus, crossing a boundary starts to look very much like one step in the process, and the distinction between cross-boundary and procedural problem stability suddenly seems a lot less straightforward.

This notwithstanding, I think that we can grant the substance of this objection and maintain the distinction. What is crucial in maintaining cross-boundary problem stability is not, for example, that problem P is identical before and after it is transferred from discipline A to discipline B, but rather that both disciplines, A and B, accept the transformations.

Two further points. First, we can deploy this idea to develop our understanding of the distinction between unilateral and bilateral problem-feeding. In bilateral problem-feeding cross-boundary problem stability is maintained explicitly. Thus a relationship of mutual relevance is established, even if this is perhaps not realized. In unilateral problem-feeding, on the other hand, no efforts are made to retain this sense of mutual relevance, and thus there is nothing to guarantee that problem stability is maintained. This does not necessarily mean that it is not maintained, as that may happen accidentally—but it seems unlikely that it is.

Second, explicitly the maintenance of cross-boundary problem stability is crucial to collaborative interdisciplinarity. It is simply what the latter means.

3.4.3 Popper, Kuhn, and the Challenge of Interdisciplinarity Revisited

Let us return to the topic of the previous chapter. It seems that the kinds of problem that Popper imagined science to be involved with were precisely those where the set of constraints generate a problem that is neither open nor ontologically over-constrained. Let us call these problems *ideal* problems. In solving them phase-1 becomes one of revealing the structure of the problem. It may not be possible to find the constraints within the confines of a single discipline—as was the case in the CO_2 example. Many disciplines may need to be deployed. Furthermore, the problem will often be too vaguely understood for us to determine at the outset precisely which disciplines will ultimately be involved. So the process may be slow. The circulation of CO_2 in the climate system has been investigated explicitly for at least a century.

Now, not all problems are ideal, and not all problem solvers stick to what they set out to do. As has been mentioned, other and more interesting and important problems are often discovered along the way, and attention may be diverted to them. But instead of dwelling on that matter let us consider the problems. What about these non-ideal problems?

Let us first try to say something about openness, as I suspect this is more prevalent. Openness comes in degrees. The problem of managing sustainability transitions, as such and without qualification, is open in the extreme. One reason for this is that the concept of *sustainability* just is not precise (see e.g. Pezzey, 1997). Other problems are much less open.

How common are non-ideal processes in interdisciplinary problem solving? That is difficult to gauge with any accuracy, but a tempting analysis presents itself here. In solving open problems the solution will always involve stipulatively narrowing the solution space by introducing, or revising, constraints arbitrarily. The situation is highly reminiscent of what Kuhn describes, and the discussion above can be used to model the dilemma described in the previous chapter. Kuhnian challenges to interdisciplinarity therefore arise not with respect to *all* problems, but only non-ideal ones. In such cases it is simply necessary to make transformations that are not identity preserving—i.e. procedural identity cannot be maintained. It is with these non-ideal problems that frameworks associated with different disciplines become problematic, and the reason they become problematic is that they threaten cross-boundary problem stability.

Where does this leave us? First, we can now dissolve the seeming contradictions outlined in the previous chapter. Popper's ideal process really concerns a particular type of problem: ideal problems. Second, if we understand the Kuhnian challenges in these terms, then overcoming them involves maintaining cross-boundary problem stability. This is not, perhaps, a method or scheme for overcoming these challenges—especially not if we think of interdisciplinarity as the maintenance such problem stability. But it nonetheless seems to enrich our understanding. In all likelihood there is no definitive method for achieving problem stability that is both highly specific and never fails. The process is one in which the disciplines involved need to stay in touch with one another and remain in active communication. This is the *collaborative* heart of interdisciplinarity, and it also provides some basis for the idea that collaborativity—communication, trust, explicit and mutual exchange, and so on—is *part* of what it means for something to be interdisciplinary. The borrowing, or exchanging, of various cognitive tools is interdisciplinary only in a very weak sense, and in many ways it and disciplinarity are virtually indiscernible.

A further possible benefit of using this model with respect to Kuhn, in particular, is that it offers a way of understanding how interdisciplinarity may be motivated and made to work in normal science. A problem that arises within a disciplinary matrix, and which is such that the matrix cannot solve it (because there is no transformation that can admissibly be carried out within the matrix that makes the problem suitably similar to some exemplar), although it is believed within the matrix that it should be solved (our D is present), is an anomaly that threatens the paradigm. By transferring this problem, however, the paradigm is protected: an anomaly is avoided and at the same time the problem in question may be solved.

3.5 On Ill-Structured Problems

As already mentioned in passing, Walter Reitman once suggested that some problems—what he calls ill-defined problems—have open constraints. Reitman elaborates with a representation of problems, or problem situations, in terms of a three-component vector $[A, B, \Rightarrow]$, where A is an initial state, B a terminal state, and $\Rightarrow$ denotes a process, or sequence of operations, that brings the problem solver from the initial state to the terminal state. Reitman thus discusses a number of possibilities with respect to how well each of these components is specified. Sometimes the terminal state is well-defined—such as when a thief struggles to invent an alibi that casts the sequence of events leading to his arrest as mere accidents—but the process and initial state is left wide open. In other problems it is the initial state that is well-defined, not the process, nor the terminal state (you have eggs, some carrots, a little milk, and small piece of veal, make dinner!). Solving a problem involves going from a problem vector such as the one described above, to another, more specific one $[A', B', \Rightarrow']$, where the components of this latter vector are elements of the components in the former. Solving the problem, then, is working through a kind of meta-process, $\Rightarrow^*$, which makes the problem more specific, until a unique solution can be produced. Solving problems that contain open constraints thus involves interpreting them more specifically. This transforms the problem into a new, precise, version in which a specific solution can be produced. The fugue composer eventually settles on a single fugue.

Herbert Simon (1973) argued, a few years after the publication of Reitman's famous paper, that what makes a problem *ill-structured* or *well-structured* has nothing to do with properties of the problem themselves, but is the result of their relationship to the problem solver. A well-structured problem, on Simon's account, needs to meet a number of criteria. These include that there should be some "definite criterion" for determining whether or not something is a solution, and that there is a "mechanizable process for applying the criterion", and that there is "at least one problem space" in which the different states of the problem (i.e. the initial state, goal states, and whatever other intermediate states are possible) can be represented. An ill-structured problem fails to meet some or all of these criteria, and because of this it would not be possible for a general problem solver to solve it. Simon, however, stresses the point that for *real* problem solvers most problems are in fact ill-structured, since real problem solvers—unlike their idealized counterparts—do not have infinite amounts of computational power, or the time required to sift through all possibilities. Winning a game of chess looks like a well-structured problem as long as we consider a problem solver that is powerful enough. For a human, or a simple chess computer, there will be no practicable way of determining what move is really *best* given some situation. What the chess computer does, then, is transform the ill-structured problem into a well-structured problem. It plays a kind of pseudo chess in which winning means maximizing some evaluative function.

The transformation, in effect, limits the knowledge base of potentially relevant information, which is in essence how Simon suggests we view the distinction between ill-structured and well-structured problems. A problem is ill-structured if there is more potentially relevant information than the problem solver can feasibly compute. *Structuring* a problem involves limiting the amount of background information until it matches the available computational resources. Solving a problem is largely a matter of structuring it.[6]

How are we to accommodate this in the account above? Interdisciplinarity, it could be argued, involves increasing—often dramatically—the amount of potentially relevant knowledge. In the present context this seems to point in the wrong direction. It is likely to transform well-structured problems in to ill-structured ones, and perhaps that is indeed what sometimes happens. However, as has already been argued, seeing successful interdisciplinary problem solving as a series of operations on the constraints on

[6]The idea of bounded rationality of Simon's has yielded a considerable philosophical literature on problem solving and decision-making, including "the fast and frugal heuristics" of Gigerenzer, Todd, and the ABC Research Group (1999).

admissible solutions often has the opposite affect; when the constraints are specified more precisely it can be the case that two disciplines can arrive at a problem formulation that is better—that is, it is clearer how it should be solved—than the formulation within a single discipline.

Interestingly, in passing Simon also mentions issues of problem stability, although not in that terminology:

> Now some obvious difficulties can arise from solving problems in this manner. Interrelations among the various well-structured sub-problems are likely to be neglected or underemphasized. Solutions to particular sub-problems are apt to be disturbed or undone at a later stage when new aspects are attended to, and the considerations leading to the original solutions forgotten or not noticed. (Simon, 1973, 191)

The issues to which Simon alludes here appear quite likely to be exacerbated in interdisciplinary contexts, and this further emphasizes both the challenges involved in realizing genuine interdisciplinary problem solving and the need to maintain cross-boundary problem stability.[7]

3.6 Concluding Remarks

A leading thought here is that interdisciplinary problem solving is similar to problem solving in general but harder to realize. In most cases the process of solving a problem oscillates between exploring a problem perceived as stable and transforming that problem into

[7]In the sustainability literature one frequently finds reference to to *wicked problems* (Jerneck *et al.*, 2011; Norton, 2005). The notion goes back to Rittel and Webber (1973) and shares some features with Simon's notion of an ill-defined problem.

one which, for instance, better fits the resources available. Interdisciplinarity offers opportunities to approach problems which, for one reason or another, are difficult to solve within single disciplines. But, crucially, to take advantage of those opportunities efforts have to be made continually to align disciplines to one another with respect to some particular problem.

In this chapter I have developed an extensive nomenclature to describe interdisciplinary problem solving. I have then deployed this nomenclature—especially the notion of problem-feeding, ideal and non-ideal problems, problem solving processes, and cross-boundary problem stability—to provide a better understanding of the Kuhnian challenges to interdisciplinarity outlined in the previous chapter and to explain what interdisciplinarity really amounts to. In the next chapter we will discuss a different feature of cross-disciplinary interaction: the idea that disciplines can *overstep* their boundaries and *infringe* on other disciplines.

Chapter 4

Disciplinary Imperialism

4.1 Introduction

When it was published, Edward Wilson's famous book *Sociobiology: The New Synthesis* (Wilson, 1975) immediately gained both devoted admirers and fierce critics. It was argued that Wilson's attempt to deploy evolutionary reasoning to explain social behaviour was reductionist in the extreme, the worst kind of adaptationism imaginable, and a failed research programme.[1] Not everyone was so categorical, however. The philosopher Mary Midgley wrote the following concerning sociobiology:

> I see one big thing right about sociobiology—its attempt to bridge the gap between biological and the social sciences—and several smaller, but still grave, things wrong—its brash and brutal style, its *academic imperialism*, and its half-conscious entanglement with free-enterprise economics. (my emphasis Midgley, 1984b, 158)

[1]See, for example, Gould and Lewontin (1979) for a famous anti-adaptationist critique targeting unrestrained use of evolutionary reasoning in general (and Wilson in particular).

In this chapter the focus will be on imperialism. Scientific imperialism is a topic that, not only is of relevance within sustainability science in particular and when discussing issues of interdisciplinarity in general, but also one that recently has featured in philosophical discussions (Mäki, 2013, 2009; Mäki and Marchionni, 2010; Clarke and Walsh, 2013, 2009; Kidd, 2013; Dupré, 2001; Dupré, 1996). The present discussion will be devoted to topics also raised in Thorén (2015b). But let us first return to Midgley.

Midgley—whose concern is primarily to point to what sociobiology can do, and a few mistakes sociobiologists have been prone to make—touches only briefly on the notion of imperialism in her paper. The imperialism of which she accuses Wilson and his ilk amounts to a few "somewhat wild offers made [...] to take over the social sciences" (Midgley, 1984b, 159). Others have been more explicit. An early predecessor of Midgley's, Calvin Stillman, complained of the readiness of the "True Believer" to "sketch the remarkable breadth of human experience which is explained best by his disciplines alone" (Stillman, 1955, 77). For Stillman the problem with imperialism flows from misapplication: academic imperialism is "the extension of one's thought-system beyond its most applicable area" (Stillman, 1955, 78). Stillman and Midgley point to two central features of imperialism; one is failed expansionism, the other misguided replacement. Moreover, in contrasting the two, we can learn something of the perplexities associated with imperialism—namely, that boundary transgressions and generalizations can be highly informative, and should be applauded.

Infringements between disciplines are rather common in science, and often enough they are fiercely resisted—just as in the sociobiology case. In many ways it should come as no surprise that scientists on the receiving end of such transgressions feel threatened, especially when the transgressors belong to influential and prestigious disciplines. The consequences are potentially devastating at a personal and professional level; jobs, funding and social influence are all on the line. However, important as these may be for the individual, they do not seem to provide adequate reason for general concern. After all, a claim to intellectual territory is only as strong as its epistemic justification, and therefore it can always be challenged. Indeed, in the disputes that have followed transgressions, defences have been built on arguments pertaining to the quality of the science, rather than threatened status.

The phenomenon of intrusion by one discipline into the domain of another usually goes by the name scientific, or disciplinary, or academic, imperialism. It is commonly thought to involve transgressions in the true sense of the word—that is, illicit occupations. Although the idea appears immediately recognizable, and though science seems to be virtually brimming with examples, the notion remains somewhat mystifying. One reason is that, to the extent that imperialism involves unification, such unification is a goal that should be applauded, not resisted (supposing it is capable of being achieved). So, perhaps not all types of boundary crossing are imperialist. Perhaps only the failed ones are.

To unify two hitherto separated domains under a single theory (or framework, or method, or whatever) is a *good thing*, with the proviso that it can be achieved. For trivial reasons it is not a good thing where it cannot be achieved, but at the same time, neither is it all that bad, nor is it confined to imperialism. Moreover, should we think of the phenomenon as interdisciplinary or, perhaps, disciplinary? After all, the goal of expansion seems to be a fundamental driver in all disciplines.

At the same time, disciplinary imperialism could provide a great many things of general interest, especially for the student of interdisciplinarity. For one thing, it might help us to understand how different disciplines should be related to another. Since interdisciplinarity is to a large extent about establishing proper and functioning relationships between disciplines, this should be highly relevant to anyone interested in interdisciplinarity.

4.2 Concept and Definition

First, a note on the use of language. In discussing imperialism it is almost impossible not to resort to the kinds of territorial metaphors that the literature is so rife with. The notion of imperialism itself often carries negative connotations—although, as Mäki (2009) points out, not always. Indeed, most who have taken an interest in disciplinary imperialism have considered it to be an activity that is illicit by default and thus unwanted. Mäki assumes a normatively neutral stance towards imperialism but I will here keep to the standard usage and perceive of imperialism as inherently illicit.

Let us now return briefly to Stillman's definition above. On his account the problem of imperialism is that a "thought-system" is used beyond its "most applicable area." This definition closely shadows the way John Dupré presents imperialism. He characterizes the phenomenon in several, slightly different ways. One characterization that is similar to Stillman's runs as follows: "By scientific imperialism, I mean the tendency for a successful scientific idea to be applied far beyond its original home, and generally with decreasing success the more its application is expanded" (Dupré, 2001, 16). Both of these definitions share some apparent weaknesses that have been pointed out by Uskali Mäki (Mäki, 2013).

One perceivable weakness is that they are "silent about imperialism" (Mäki, 2013, 329). Although imperialism certainly is a form of expansionism, it seems reasonable to think of it as expansionism that is directed upon *another* discipline, and not, for instance, upon intellectual territory that has yet to be claimed. So, it is reasonable to assume that imperialism is not just the extension of a thought-system or idea beyond its most applicable area, but the extension of that thought-system or idea into some already occupied intellectual territory.

Another complaint is that the idea of relative applicability is confusing and seems overtly restrictive. The implication seems to be that the thought-systems being transferred are in fact applicable or successful in their original domain, and not applicable in the domain to which they are transferred. Often, however, the charge is that the imperializing theories, thought-systems, or ideas, are ill-suited not only to the domain to which they are transferred, but also, as it were, to their original domain (Mäki, 2013, 330). So we can imagine not only <good→bad>, but also <bad→bad>, and should we decide to adopt a non-normative notion of imperialism also <bad→good>, and of course <good→good> (Mäki, 2013, 330).[2]

A third point: applicability and success are both vague and need to be further specified. In order for something to be an infringement *at all*, clearly some success has to have been had already. This involves at the very least acceptance by a handful of peers, although in most cases more will probably have been achieved. *Epistemic* success, or validity, is what is at stake, i.e. whether the application of the framework or theory is indeed sound, or informative, or well-founded, or fruitful.[3]

So, we can reasonably consider an imperialist infringement to be an infringement from one discipline *into another*. If there is no imperialized discipline, it is just a case of expansion (cf. Mäki, 2009, 9). That being said, not all imperialist infringements fail *because* they are imperialist, as we shall see below.

Then there is the issue of the object of transfer. Stillman and Dupré both favour vague notions such as thought-system and idea. Midgley offers nothing in the way of a definition, but her concern is mainly with explanations. Mäki, whose account is by far the most detailed, distinguishes between three main forms of imperialism

[2] I have borrowed the semi-formal expressions from Mäki himself (2013, 330).

[3] Precisely what this amounts to is more difficult to say as it varies depending on what is begin transferred (a theory, model, method, or concept etc.) and the specific context of that transfer (perhaps it is a metaphor).

which, in part, relate to the objects of transfer. These are the following (Mäki, 2013, 336):[4]

Imperialism of scope: An expansionist discipline seeks to explain phenomena that belong to the perceived domain of another discipline. This is the pursuit of explanatory unification that is disrespectful of disciplinary boundaries.

Imperialism of style: The styles and strategies of research, such as the techniques and standards of enquiry and communication, characteristic of one discipline are transferred to, or imposed on, other disciplines.

Imperialism of standing: The academic and non-academic prestige, power, and resources, as well as the acknowledged technological and political relevance of one discipline, increase at the expense of those of another.

In most cases where scientific imperialism becomes controversial it appears that all three forms are really involved. In this chapter I will leave imperialism of standing aside, and focus on imperialisms of scope and style. In keeping with the way disciplines have been discussed in this thesis so far, it is probably preferable to think of imperialism in terms of an entire framework; it is a matter of both style and scope.

A notable feature of Mäki's conception of imperialism that has already been mentioned is that it is not normatively charged. Mäki prefers to think of imperialism—that is, imperialism of scope—as attempted unification across disciplinary boundaries. Sometimes it is epistemically successful and defensible and sometimes it is not.[5]

[4] See also Mäki, 2009; Mäki and Marchionni, 2010

[5] Failed imperialist infringements—that is interdisciplinary unification without appropriate support—Mäki calls *imperialism**.

Another aspect of Mäki's imperialism—one in which it differs from some, although not all, accounts—is that he bunches together cases where something is imported with cases where something is exported. Elsewhere the standard idea appears to be that only exports can be imperialist, and imports are cases of borrowing. However, given that the issue at hand is unification, this really makes no difference. Moreover, it can be difficult to determine whether an act of borrowing was really consensual, or was practically forced upon the borrower. Some disciplines are so pervasive, as a result, for instance, of their influence in policy making or the public debate, that their theories, explanations, and methods cannot be ignored by those engaged in other disciplines.

Here I will conceive of imperialism as follows:

Disciplinary imperialism: the failed application of the resources (tools, methods, models, theories, ideas, etc.) of one discipline onto the domain of another discipline.

Failure should be understood epistemically: it is the application of some resource where it should not be applied. It is not a matter of, for example, a failure of gaining support from scientists in a particular discipline for a theory imported from some other discipline. Imperialism thus should be contrasted against successful forms of boundary crossing.[6]

[6] Clarke and Walsh (2009) differ between unification and imperialism but one might raise concerns here that unification does not really exhaust the possibilities of successful boundary crossings. Metaphors may be successful without achieving unification. In working with this manuscript the metaphor of globalization was suggested to me as a candidate. That term would be preferable as it could be made broader to include unification as well as other forms of successful boundary crossing. However, globalization too is a term with many negative connotations and would perhaps be better used as an umbrella term covering both imperialism and successful boundary crossing.

A final remark. A range of issues are raised by the concepts *domain* and *discipline*, and the distinction between them, as well as by the idea that a domain can belong to a discipline. A domain, here, is a set of facts or phenomena to be explained (Mäki, 2009; see also Shapere, 1984). A domain only belongs to a discipline in the sense that, usually, a discipline is associated with a specific domain (see Bechtel, 1986, 1987). The strength of this association varies from one discipline to another and even within single disciplines over time (Weingart, 2003, 2010). The domain of a discipline is, however, never essential to that discipline (unless thus stipulated, in which case the matter is trivial) (see Toulmin, 1972, 146ff).

4.3 Why Imperialism Fails and Reasons for Resistance

Boundary crossing in general brings several potential benefits. For example, better methods and tools may be introduced in a discipline where they were lacking before, erroneous explanations may be exposed, and partial ones complemented, and so on. However, in most cases in which imperialism is mentioned the concern is about instances when these infringements fail, for one reason or another. So, let us look at the charge against imperialist infringements in more detail.

There are two, sometimes separate, aspects to this issue. One concerns the way in which imperialism typically fails. That is to say, what is it, precisely, that goes wrong? Are there typical ways in which imperialism tends to fail, and can they be resisted or avoided somehow? The other aspect pertains to why we should care about imperialism. Let me clarify briefly. Consider the following. Mäki suggests that we should understand imperialism of scope as attempted unification. Such unification can fail in more or less subtle ways. All failures, crucially, should be resisted when they are discovered to be failures, and generally, all infringements should be put under meticulous scrutiny. However, it does not make sense to try to limit attempts at unification pre-emptively. After all, these attempts *can* turn out to be successful (in the epistemic sense). Arguably, with the proviso that we stick to the scientific context this risk of error, even when it is known to be overwhelming, is no reason not to try. The cost is just that one might be wrong about something, and this is easily offset by the potential for progress, even if that potential is ever so slight. So, in such instances the reason why imperialism fails is no reason for resisting it pre-emptively. Moreover, the reasons for resistance *ex post facto* are the very same reasons that underpin resistance to any instance of bad science.

In the literature there are a number of suggestions as to the nature of the problem with imperialism. Principally two, at least partially independent ideas, have been forwarded. Some authors, such as Dupré, emphasize how imperializing frameworks and theories plainly fail to capture that to which they are applied. Others focus on the issue of replacement. I will here call these *type-I* and *type-II* imperialist failures, respectively (see Thorén, 2015b). In short, suppose a theory is introduced to a new domain to explain some fact. On closer inspection, however, the fact cannot be explained by the theory in question. This would count as a type-I failure; it appears to be what Dupré is concerned with for the most part. The reasons he draws on relate to the nature of the target domain, and specific features of the imperializing theory. It is a type of failure that is not really specific to imperialist infringements, but could also happen in ordinary expansion. Type-II failure, then, if we keep to our picture, involves two components. First, a theory is introduced to a new domain to explain a fact where another theory explains that fact better; and second, the introduced theory *replaces* the better theory.

These categories are not mutually exclusive, nor does the distinction hold up under all circumstances, although I will maintain that it can nonetheless be useful. Occasionally one can view this applicability issue as a mere shift in emphasis; at other times the difference is more pronounced.

I will call imperialism that exhibits type-I failure *type-I imperialism* and imperialism that exhibits type-II failure *type-II imperialism*. Two further remarks before we proceed to spell the two forms out in more detail. First, these forms of imperialist failure are not mutually exclusive: good theories can be replaced by alternatives that do not even hold up on their own. Second, other concerns have been raised in the literature with respect to why imperialism fails. Midgley, especially, often discusses the way imperializing frameworks act as vehicles for imposing particular political agendas (see Midgley, 1984b,a). This is not entirely irrelevant in sustainability science, as the success of resilience theory has been attributed to the fact that fits well with the neoliberalism that is hegemonic in international politics (see Walker and Cooper, 2011). Here, however, I will leave this issue aside. I take it to be an explanation of why a certain infringement gains acceptance despite its faults, rather than an extrapolation of those faults themselves.

The idea behind type-I failure is rather straightforward and it seems that a lot of resistance to imperialist infringement is built around the charge that the imperialist idea, or framework, is just *bad science*. Remember Stillman's definition again; the idea of extending a thought-system beyond its applicability implies that (at least) a type-I failure is at work. The thought-system *plainly fails* to do what it is supposed to do. That is, the issue here is not primarily that theory B has been wrongfully *replaced* by theory A, but rather that theory A was wrong to start with. This is also, as it were, Dupré's charge against Becker and the evolutionary psychologists: their proposed explanations fail in themselves, so by their own standards they deserve to be discarded.

Type-II failure, on the other hand, requires a more detailed overview (see Thorén, 2015b). Whereas the type-I failure results from mismatch between, for instance, a framework and that to which the framework is applied, a type-II failure arises from failure to recognize the appropriate relationship between frameworks (or theories). A type-I failure can occur even if the imperialized territory is unclaimed, but this cannot be the case in type-II failure. This type of error is interesting, since it opens up more subtleties. Here are a few general illustrations that help to clarify this observation.

One form of type-II error involves replacing an objectively better theory with one that is worse. This does not mean there would be reason to resist the imperializing theory if it were the only theory available. If resilience theory was the only theory we had about social change, it would not make sense to abandon it, since it would potentially explain some relevant cases. However, given that the situation is such that we have a whole set of different theories of social change among which resilience theory is just one, there is every reason to resist replacing this whole set in favour of a single theory that only explains some cases. This example highlights an important form of type-II failure—i.e. one in which a partial theory is mistakenly assumed to be complete and therefore replaces other alternatives which are complementary. Consider Sandra Mitchell's *integrative pluralism* (Mitchell, 2002, 2003, 2009). Division of labour in social insects is a phenomenon several theories have sought to explain. Two leading categories of theory here are those that rely on evolution and those that rely on self-organization. Theories of both kinds have often been assumed to be mutually exclusive alternatives, not only between the two categories, but also within them. Mitchell, however, argues that they are not. They are, on her view, compatible, although it is only possible to integrate them at the level of concrete phenomena. In short, individual instances of social division of labour might have arisen as the result of a number of different causes; they may have been consequences of self-organization or evolution, or both. Moreover, the degree of importance may also vary between instances. That is, these theories have been taken to be complete with respect to this phenomenon, but they are not. Thus, a situation may arise where one theory out-competes another although *both are needed*.

Another potential example, elaborated at length in Thorén (2015b), is resilience theory in sustainability science. Resilience theory is framework that grew out of general systems theory and cybernetics at the beginning of the 1970s, applying some of those ideas to ecosystems. In particular, population ecologists found the notion of resilience useful, and so resilience was developed, especially by C. S. Holling, first, for ecosystems, and later to broader classes of system. Resilience theory attempts to explain, and to some extent predict, radical and sudden changes in systems.[7] On the standard conception of it, resilience is the ability of a system to maintain some property during stress—usually, some property that is considered constitutive of the system (Thorén, 2014).

Here is an example of how resilience theory may be applied. (Whether this is indeed a *correct* application is somewhat contentious.) The Cod population of Newfoundland was under persistent pressure from industrial fisheries from at least the early 1950s. In the early 1990s, following a few years of record yields, several Cod populations collapsed entirely. For the Northern Cod the spawning biomass decreased by a staggering 99% in one fell swoop. This effectively changed the whole system: the ecosystem entered a new stable state with just a fraction of the biomass of the previous one. A resilience theoretician would probably analyse this situation as follows. Overfishing had, over a number of years, or decades, reduced the resilience of the Cod population. What finally

[7] The original papers is Holling (1973). See also e.g. Gunderson and Holling (2002). There is a vast literature on the notion of resilience and a more thorough overview can be found in (Thorén, 2014, 2015b; Thorén and Persson, 2015).

made it collapse was probably not the fishing, or sudden changes in fishing, but rather some other random external disturbance, such as a slight decrease in nutrients, or a minor disease—a disturbance that the population otherwise would have been able to absorb. Here the collapse is explained in terms of this lack of resilience.

Contemporary theoreticians of resilience have substantially expanded this framework to include not only ecosystems, but also so-called social-ecological systems and social systems (Gunderson and Holling, 2002). The latter development is especially controversial. What one seeks to explain in this approach is similar radical changes that alter the very foundations of some entity, such as a political revolution, or financial collapse. The point is that a systems approach, such as resilience theory, is not completely uninformative with respect to such changes; but neither can it provide a complete account. The framework is rigidly deterministic and has several gaps: it relies heavily on a functionalist analysis of social systems, it makes it difficult to accommodate important drivers such as power, and so on (see Jerneck and Olsson, 2008; Davidson, 2010; Hornborg, 2013; Thorén and Persson, 2015; Thorén, 2015b). The potential type-II failure here is rooted in the idea that resilience theory can *completely* explain the phenomenon, although it is probably only a partial explanation. Systemic failure is likely to be the cause of collapse in some social systems, or perhaps a part of the story, but if we are interested in collapses of social systems in general, it can never be the only account.

4.4 Imperialism and the Discipline

Another interesting question raised by imperialism is whether we are to see it as an interdisciplinary or a disciplinary phenomenon. Mäki (2013) has emphasized that imperialism should be thought of as a "dynamic interdisciplinary relationship" (Mäki, 2013, 327).

In this section I intend to problematize this idea. Imperialism is interdisciplinary in the sense that it connects disciplines in the manner described above—e.g. by attempting to unify their respective domains under a single theory, or by exporting a method or methodology. However, this connection, although it could be substantive in the sense that the object of transfer is comprehensive (such as when an entire framework is imposed on some discipline), is weak in another sense. Imperialism, as standardly understood, appears to imply that there is no mutual involvement by the disciplines involved. In fact, the imperializing of discipline B by discipline A may go entirely unnoticed in B. Following the remarks in the previous section on imperialism and replacement in limited contexts, I will argue here for the idea that imperialism is a *disciplinary* phenomenon and an obstacle to interdisciplinarity.

Let us begin with a few observations on the disciplinary system. First, most, if not all, disciplines are inherently expansionist. Disciplines seek to grow, and they do this by trying to solve ever new problems.

Second, the disciplinary system is not a well-ordered system of division of labour. There is no general plan as to how science should proceed and how disciplines ought to relate to one another. In fact, this lack of overarching organization and overview was one of the motivations behind Otto Neurath's *encyclopaedism* as well as his *universal jargon* (see e.g. Neurath, 1941, 1937, 1983). Neurath perceived of the universal jargon as an 'aggregate' language aimed, at one and the same time, both to involve "the man on the street" in the scientific venture and to locate the 'gaps and gulfs' between sciences and disciplines—gaps and gulfs that otherwise remain shrouded in the mist of special languages and metaphysics. A central concern, thus, was that it is just not obvious, at the outset, how the sciences hang together. Establishing such relations is in itself a major scientific achievement.

Third, although we may isolate different features of disciplines (their theories, models, methods, domains of inquiry, and so on), and although these can sometimes be used to differentiate between disciplines, none of these features is essential to the disciplines of which they are constituents; all are subject to change and revision over time (cf. Toulmin, 1972, 144ff).

Now, it appears that most disciplines nourish discussion of the reach and scope of their own activities. These discussions do, on occasion, contain references to other disciplines that are perceived to be related but nonetheless contrasting. An excellent, and for philosophers of science quite homely, example of this is a debate, within the philosophy of science, which departed from the (in)famous contexts distinction (Hoyningen-Huene, 1987, 2006). The distinction between the context of justification and the context of discovery was used by philosophers of science to demarcate philosophical (and hence analytical) inquiry into science from various empirical approaches, such as the psychology, history, or sociology of science. Justification, it was thought, was logically driven, and therefore accessible to the philosopher, and the philosopher alone. Discovery, on the other hand, was perceived as an entirely unstructured process, and therefore suitable for empirical enquiry; it lay wholly outside of the scope of philosophy of science.

The demarcation only holds if two premises are affirmed, both of which have subsequently been questioned. One is that the contexts of justification and discovery, respectively, indeed fit these descriptions. This has become increasingly difficult to maintain.[8] Nickles (2006) argues that the context of justification cannot exclusively be thought of in terms of what he calls *epistemic appraisal*. It also has an irreducible component of heuristic appraisal that pertains to things such as the economy of science. With respect to discovery, although a 'logic of discovery' is perhaps a tall order, an endur-

[8]There is a considerable literature on this topic: e.g. Nickles (1987, 2006), Laudan (1977, 1981), Hoyningen-Huene (1987, 2006), and Kellert (2008).

ing result of the work of AI researchers Simon and Newell—the so-called 'friends of discovery'—was that the process is nonetheless a highly structured and rational one, at least in specific contexts (see Nickles, 2000). Moreover, as Kellert (2008, 47) asks, if there were no structure of discovery, how could the historian of science make it intelligible However, suppose we adopt a minimal contexts distinction, such as that proposed by Hoyningen-Huene (1987)—a distinction that eschews the difference between the descriptive and the normative/evaluative. This idea, though non-exhaustive, is perhaps more palatable, but it fails nevertheless, because the second premise (i.e. that the disciplines in question can be captured in this fashion) is plainly false. As the emergence of descriptive philosophy of science and empirical philosophy of science emphatically shows, contemporary philosophy of science is not confined to normative and evaluative concerns. Likewise, the suggestion that, for example, the sociology of science is strictly descriptive is little more than a, possibly quite vacuous, idealization.

Generally it appears that discussions like these, though conducted under the pretence of interdisciplinarity (inasmuch as they relate different disciplines to one another), are really paradigmatically disciplinary in nature. Although their validity, naturally, depends on the substance of the arguments given, it is notable that they are often carried out in the absence of many of the parties concerned. The point is that disciplines often appear to be internally construed, and defined from within. What is important is establishing some degree of internal coherence with respect to, for example, the scientific aims, rather than relating different disciplines to one another in a descriptively accurate way. Hence one sometimes resorts to rather crude images of other disciplines. Establishing a proper, workable boundary across which information and problems can pass back and forth often requires something more.

In view of the fact that expansionism is a natural feature of disciplines, it seems that imperialism is a quite probable outcome, given a sufficient amount of time. Crucially we may expect many infringements to go unnoticed either by those imperializing or by those being imperialized, or even by both. Imperialism becomes an artefact of disciplinarity and the issue sorts under standard discussions of the scope and reach of theories, methods, or ideas, in general—with their context sensitivity, and such like. But these issues appear to be merely standard questions of the kind that will confront any scientific venture. They are not specific to interdisciplinary context, and indeed if we think of imperialism as an interdisciplinary relation, we shall need to adopt a view of interdisciplinarity that is rather void of substance.

4.5 What To Do About Imperialism

In this section I will first rehearse what various authors have suggested we do to stop, or avoid, imperialism, and then develop a complementary suggestion of my own based on the framework outlined in the previous chapter. Most authors who have taken an interest in imperialism seem to believe that the best strategy to avoid imperialism involves adopting some kind of pluralism (among other things). My suggestion, instead, builds on the idea that imperialism is a problem within interdisciplinary contexts, and is thus a failure of cooperation.

Before we go into the details of these accounts it will be helpful to deepen the analysis of imperialism a little.

4.5.1 Resistance, Prevention and the Primacy of Type-II Imperialism

A central concern of many writers on this topic is with the question how, or when, imperialism is to be resisted. It may seem obvious—failed imperialism is failed science, and failed science should of course be resisted—but the issue is complicated. The reason is that all science, good or bad, should be resisted and resisted fiercely. This is just part of how science works. In fact, this is how one usually tells good science from bad science. As Clarke and Walsh ask us: "[i]f all scientific imperialists are doing is advancing poor explanations then it is hard to see what the fuss is about?" (Clarke and Walsh, 2009, 198). One wonders, then, if this issue is really less about resistance and more about avoidance. In any case, it is sometimes unclear which one of these is really intended, and sometimes neither appears to be particularly appealing.

An example might help. Mäki produces a number of constraints that an imperialist infringement must meet in order to be justified. One of these Mäki calls the ontological constraint. This constraint applies to imperialism of scope that seeks to unify two domains under a single theory, and it states that the unification achieved must be ontological, rather than merely *derivational* (Mäki, 2009, 13ff). Ontological unification reveals the underlying structure of the world. In it, then, a substantive connection is made between two domains. Derivational unification, on the other hand, only amounts to deriving from theory a set of explanandum sentences. This seems all well and good, but what does it really mean? Should one try to prevent people from conducting derivational unification altogether? That seems very odd, to say the least (this is not Mäki's suggestion). After all, derivational unification does not preclude ontological unification, and the latter may therefore be forthcoming. So should one resist derivational unification? Certainly, but not *because* it is derivational: it should simply be resisted, just as any scientific claim should be resisted. I mean, clearly, one should not derivationally unify and then believe that what one has done is more substantive.

So, suppose it is hard to know beforehand how a particular imperialist infringement is going to play out. In other words, in boundary crossing there is always the *risk* of committing to these errors. It remains the case that there are also possible benefits, and in particular one may unify two hitherto separate domains.

Assume that what is at stake is type-I imperialism. This is almost always true when one attempts to generalize a theory. What is really on the line here? Not much, it would appear. The worst that can happen is that one is wrong, which, *ceteris paribus*, is not terrible. What would it take to try to prevent type-I error from happening? Quite a lot, as it were. Restrictions on cross-disciplinary infringement are severely limiting on science and run counter to scientific virtues such as boldness and intellectual adventure, as has been pointed out repeatedly by Mäki (see e.g. Mäki, 2013, 330). So, although one may stop imperialist type-I failures, the cost seems overwhelming. Resistance, on the other hand, does not cost anything.

What of type-II imperialism? Is it possible to prevent it? And what is at stake then? Let us ponder the second question first. Type-II imperialism is ostensibly a more serious error, as it involves a loss of something, such as explanatory power. Clarke and Walsh (2009), in attempting to develop Dupré's notion of imperialism, draw on the notion of *Kuhn-loss* as they try to provide something that looks like reason to resist imperialist infringements. An example can be found in the transition from the phlogiston theory of combustion to the oxygen theory. As the latter was adapted some explanations relating to the similarities between metals became unavailable.

Clarke and Walsh suggest that we might distinguish between imperialism and unification by extending this idea somewhat.

> The charge against the scientific imperialist is not merely that the transition to a new theory involves a loss of explanatory power that may later be overcome, but that indigenous knowledge will be permanently abandoned because colonization will prevent the possibility of developing perspectives from which it may be regained. (Clarke and Walsh, 2009,

203)

So the consequences of replacement can be quite serious. A whole avenue of research may be permanently lost. What about prevention? Without targeting the failures directly, which I suppose would be difficult, the prevention of type-II failure involves non-replacement—in essence, pluralism.

Even if we disregard the problems that may be associating with determining precisely when pluralism is appropriate and when it is not, there are potentially more serious problems. Although it is easy enough to see that imperialism of this sort would be unwelcome, a worry may be raised that in this form imperialism simply has no instances in science. Indeed, even in the phlogiston case there was no real loss over time. Eventually the oxygen theory was able to recover the lost ground. It is telling that Clarke and Walsh can produce no example of such permanent loss. How could they? For it seems that theories are only rarely completely abandoned. Even within single disciplines there is a surprisingly high tolerance of inconsistency and contradiction; and in science as a whole this tolerance seems boundless.[9] Moreover, the notion of permanence is perplexing. Even widely refuted theories can be brought back later on. Consider only the reawakening of Lamarckism in epigenetics. These ideas are not irreversibly lost. They are all still *there* should one be interested in them.

That being said, Clarke and Walsh's suggestions are clearly not without precedent—or anyway that route could be much further explored. Ian Hacking (2002) writes:

[9]Notably I am not claiming that this *should* be the case. No one strives for contradiction. What I am saying, rather, is this. Even within disciplines mutually exclusive theories can be nursed simultaneously. It may be due to the fact that neither theory can be confirmed to a sufficient degree to exclude the other (see Kitcher, 1990) or because it is sometimes difficult to survey subtle intertheoretical relations. Both factors are greatly exacerbated if one looks to science as a whole.

> Foucault observes, near the end of *The Order of Things*, that "At any given instant, the structure proper to individual experience finds a certain number of possible choices (and of excluded possibilities) in the systems of the society; inversely, at each of their points of choice the social structures encounter a certain number of possible individuals (and others who are not" (Foucault 1970, 380). Historical ontology is about the ways in which the possibilities for choice, and for being, arise in history. (Hacking, 2002, 23)

By adopting a certain theory, or framework, this adoption in itself somehow makes alternatives not only scientifically impossible but impossible to even conceive of in a more profound way. Here I will not further pursue this line of reasoning but rather focus another, more practically oriented, way in which replacement can be damaging.

To conclude, the imperialism that we should care about because it is imperialistic is type-II imperialism. However, in order to make proper sense of the notion of replacement one needs to narrow down the contexts in which this replacement takes place.

4.5.2 Replacement in Limited Contexts

Let us consider this second problem. I think it has a fairly obvious solution. It is not really science as a whole that is the relevant context in which replacement usually takes place, but something much more specific. Two such specific contexts are of particular importance here. One is interdisciplinary collaborations, which are effectively ruined by one discipline taking over the affairs of another. Another arises in cases where science is to inform policy making. Here the risks associated with replacement are even more tangible. As Dupré notes in his discussion on imperialism, even if we are not worried about the bad science that results from an imperialist infringement, we should worry about its further consequences: "[b]ad science, when directed at human nature or society, is always liable to lead to bad practice" (Dupré, 2001, 4), and bad practice can cause harm.

In narrower contexts like these replacement is both a problem with serious consequences and a problem that is, at least comparatively, common. Moreover, it is possible not only to resist imperialism in these instances, but also to prevent it (at least to a degree) without overwhelming costs. Avoiding type-II imperialism would involve adopting a pluralist position at the outset—in essence it requires one to be very wary when replacement appears to be going on.

There is a comparison that can be made here to discussions concerning epistemic risks. As Sahlin and Persson (1994) note, there is not only the risk of being wrong but also the risk of having too narrowly constrained ones problem formulations so as to exclude important aspects.[10] Hence, they conclude, it is always important to keep track of that which is not already known.

A final point. There is one respect in which the distinction between type-I and type-II imperialism is particularly clear. It appears to be the case that strategies for avoiding type-I failure are not even compatible with strategies that seek to avoid type-II imperialism. A strategy seeking to avoid type-I imperialism would involve imposing limits on boundary crossing. This would be to block, a priori, what can be taken into consideration in explaining a phenomenon, and thus to suppress pluralism. Strategies for avoiding type-II imperialism, on the other hand, move in the opposite direction. Here the idea is, as far as possible, to avoid suppressing new or alternative accounts.

A general problem with the idea presented above is that plu-

[10] They base their argument on an earlier paper, Gärdenfors and Sahlin (1982) in which a Beysian framework is developed to help with decision making when information flows from different sources with different reliability.

ralism, clearly, is not always the appropriate stance. Moreover, the implication appears to be that all theories should be retained merely in virtue of having been believed at one point in time. Although a pluralist stance often makes good sense—especially in the social sciences where there is rarely sufficient evidence to show conclusively that a theory should be discarded (cf. Mäki, 2009, 21f)—this consequence appears somewhat absurd. Below I will propose a different criterion for imperialism cast in terms of problem-stability.

4.5.3 The Standard Solution

How is imperialism to be avoided, then? Let us return to Stillman again. He separates two 'functions' of theories (for agents) that in turn yields two types of (inter)disciplinary integration. These functions—or perhaps one should think of them as values a theorizer may use to guide her in theorizing—are the following. Let us call them unificationism and instrumentalism. The unificationist values theoretical unity and logical coherence above all else and seeks to acquire a theory that is both as large in scope as possible and at the same time logically consistent. The unificationist, then, according to Stillman, will typically be prone to "suppressing inconsistencies, and omitting inconvenient observations" (Stillman, 1955, 79).

The instrumentalist, on the other hand, values empirical success and "subordinates concepts to the role of tools, each contributing its bit to the understanding of a part of fairly concrete reality" (Stillman, 1955, 79). Stillman illustrates the distinction with medicine.

> ...the physician works without a general theory of the human body; instead he uses concepts from anatomy for some purposes, theories of chemistry for other purposes, laws of mechanics for certain structural matters, and even concepts from psychology. There is no apparent need to unify the

> concepts from psychology. There is no apparent need to unify the conceptual systems, each of which is contributing its bit to the understanding of the obviously unified body of a patient. (Stillman, 1955, 79)

Stillman appears to be calling on several ideas. One, which recur in the literature, is that sometimes a complex subject matter—such as the human body, or perhaps human behaviour—provides all the unity one needs. In such instances theoretical unity is less important, at least under the proviso that whatever theories one is currently using serve their purpose well enough. In other situations, such 'ontological' unity may not be available, and then theoretical unity becomes more important. One is inclined here to think of varieties of subject matter with no empirical basis at all, like mathematics or logic, in which theoretical unity and consistency is everything. (Although it should be mentioned that Stillman never brings these up as examples.)

It then appears that imperialism is the failure to observe this difference and to emphasize theoretical unity and logical consistency in situations where that is not necessary. Unity and consistency are not inherently bad things, naturally, but according to Stillman they come at a cost (or tend to do so). The practice is, as Stillman is at pains to emphasize, "inherently restrictive, and tends to exclude alternative concepts" (Stillman, 1955, 79). The kinds of situation Stillman is worried about appear to be similar to what I have described above as type-II failures of the sort where, for instance, a theory provides a partial account but is mistakenly taken to be complete.

Stillman's resolution of the problem of imperialism therefore involves deploying his pluralistic instrumentalism in (as he would see it) appropriate situations. He devotes the rest of his paper to discussing a particular example, involving economics and anthropology, where he takes this type of integration to be advisable.

Although Stillman lacks some of the philosophical precision that later contributors have achieved, the fundamental ideas here can be found in the work of Mary Midgley and to some extent John Dupré. They both argue on the basis of particular cases, and they both consider pluralism to be the solution. Furthermore, they both raise concerns about the ontology of the particular subject matter to press the case for their pluralism. Midgley writes, about sociobiological arguments, "[w]hen such arguments are pernicious, it is not because they are biological but because they are bad" (Midgley, 1984a, 107). And why are they bad? Because they are reductive in "the bad sense" (Midgley, 1984a, 107), which for Midgley means that they devalue what is valuable—a process that involves, as a first step "making one's own kind of explanation exclusive" (Midgley, 1984a, 108). Again, the problem here is with replacement, and this is a problem in precisely the same way that concerned Stillman. Midgley is not categorically opposed to sociobiology—indeed she argues for its many benefits, and observes that it has, at long last, managed to connect the biological sciences with the social science (Midgley, 1984b). Instead her problem is that sociobiologists have blatantly assumed that sociobiology *is enough* when clearly it is not. For Midgley sociobiology is one component among many, and it is mistake to relate it to alternative accounts as a competitor when it should be related to them as complement.

Mäki, too, argues that pluralism is an antidote to imperialism, although he does so on different grounds. For Mäki the reasons for adopting a pluralistic stance really derive from the fallibility of all theories—especially in the social sciences.

> The epistemological constraint I am proposing on economics imperialism advises against dogmatic commitment and recommends a strong sense of fallibility and openness to critical conversation across disciplinary boundaries. Personal and

> strategic commitment to a theory may do no harm, but only provided it is accompanied by tolerance and pluralism that derive from a deeper commitment to the uncompromised principle of fallibilism. (Mäki, 2009, 23)

Finally, it may be in place to say something further about Dupré here. Above I have bracketed him together with Midgley, and indeed they appear to have a lot in common. But there is also an alternative way of understanding Dupré. That is, although human behaviour is never going to be explained by a single theory, this does not mean that pluralism, specifically, can help us here. Dupré's charge against Becker and the evolutionary psychologists is not primarily that they are not pluralists, but rather that their proposed explanations are bad.[11] Pluralism would not necessarily make them better. In Midgley's case, what is bad about sociobiology is precisely that it is not pluralist when it should be, but Dupré's concerns seem to be different. Thus his antidote is perhaps not pluralism, curiously enough, but rather observation of "proper protocol", or something along those lines. Admittedly, this is not a particularly precise or even informative position on imperialism in general, but then Dupré is primarily concerned with particular cases.

4.5.4 Problem-Feeding, Collaboration, and Imperialism: an alternative account

I am now going to develop an alternative to the idea of pluralism as a gatekeeper guarding against imperialism. Although an across-the-board pluralism, regardless of its underpinnings, does do away with type-II imperialism, it is equally clear that pluralism is not always the correct position. Even the most committed pluralist will have to admit that for certain phenomena, however limited

[11] A caveat: Dupré is certainly a pluralist and it is clear that he thinks that some kind of pluralism will be appropriate in the cases he discusses.

the class might be, monism—a single account—will do. In any case, whether or not pluralism is appropriate for some particular phenomenon is a contingent matter, impossible to determine from the armchair. Thus pluralism does not target illicit forms of imperialism specifically, and in particular it does nothing to stop type-I imperialism—quite the opposite.

So if pluralism is at best a rule of thumb, let me now suggest an alternative. The proposal here relies, not on pluralism, but rather on agreement among parties. As such it is also fallible and no guarantee that mistakes will not be made. In this sense it is no better than the pluralist suggestion. Nor is it worse. Moreover, it is specifically honed to handle the kinds of context in which imperialism really matters—i.e. interdisciplinary collaborations.

Within the framework provided in previous chapters it is possible to model the imperialist tendency on the way in which disciplines, in general, attempt to formulate problems so that they fit the resources they have at their disposal. What does that tell us about imperialism?

Here is a proposal about how to understand type-II imperialism and its prevention in collaborative, interdisciplinary contexts. We have already concluded in the previous chapter that the central task for any interdisciplinary problem solving process is to maintain cross-boundary problem-stability. Cross-boundary problem stability, crucially, does not imply that a problem is identical before and after the transfer. Rather it requires both the source discipline and the target discipline to accept the transformation. That is, procedural problem stability is not necessary for cross-boundary stability to hold. The problem is transferred in a mutual, and reciprocal, exchange that is controlled and mediated by the disciplines involved. One way of understanding imperialism, in such a context, is not to *claim the problem for oneself*—that would be the anti-pluralist mistake—but as a failure to ensure that the transformation is agreed. On this view, the imperialist failure is a failure to collaborate; and as long as mutual acceptance is in place the solution may involve either several disciplines or just one.

This rule of thumb does not impose across-the-board pluralism. Instead it requires us to observe the mutual and reciprocal character of interdisciplinary collaboration. One might object that collaboration is not a value in itself, in general. This is indeed true, but then, it is a value in the contexts in which imperialism is interesting—i.e. collaborative, interdisciplinary ones—for perfectly trivial reasons. In other contexts, including disciplinary ones, imperialism is indistinguishable from any other type of failed expansionism, or indeed failed science in general, and thus is a different matter entirely.

Chapter 5

Unity and Pluralism in Sustainability Science

5.1 Two Models of Interdisciplinarity

Now let us return to the issue of interdisciplinarity more generally, and especially to its relationship to unification and pluralism. We have already contended that in order for disciplines to solve problems jointly by transferring them across their boundaries those problems need to be stabilized to a sufficient degree. But what about the boundaries themselves? Or rather, to what extent do disciplines need to be integrated, unified, or related, to one another for problem-feeding to even get going in the first place? Obviously, some relationship must precede problem-feeding. There must be a reason for the problems to be transferred. In this chapter I will return to some themes introduced in the first chapter, and in particular the themes of unificationism and pluralism in sustainability science. I will present a number of arguments, both general and specific, showing why a unificationist model for interdisciplinarity may not be the appropriate one in sustainability science. In the penultimate section I will return to the pluralist model and discuss problem-feeding in light of that model.

First, however, let us briefly revisit the notion of interdisciplinarity in general. One important distinction to get out of the way before I begin is between global and local interpretations of interdisciplinarity. Sometimes interdisciplinarity is perceived as a model for science as a whole, while at other times it is regarded as a way in which individual disciplines can be linked or combined, permanently or temporarily. Eric Jantsch's (1972) notion of transdisciplinarity is an example of the former. He understands this notion to imply a "multilevel coordination of [the] entire education/innovation system" (Jantsch, 1972, 15). Jantsch's transdisciplinarity is thus a global form interdisciplinarity that coordinates science as a whole. In this theses the focus, rather, has been on more limited interactions between disciplines, and this focus is more in line with current usage of the notion of transdisciplinarity (see e.g. Klein, 2014, 2010).

Even on such a limited conception of interdisciplinarity there is a tension between, on the one hand, unification, and on the other, pluralism. Clearly, if we think of interdisciplinarity as a kind of division of cognitive labour, then both overlaps and differences between disciplines simultaneously create the impetus for engaging in interdisciplinarity in the first place. An interest is shared, perhaps in a particular problem or problem complex, but the approaches—the theories, tools, methods, and so on—are different (and complementary).

At a more fundamental level, however, it is quite clear that although shared interests and complementary approaches make for an ideal start, they are not nearly sufficient for interdisciplinarity. Even the realization that this is indeed the case requires other components to be in place, because a range of comparisons have to be made between the domains of the respective disciplines as well as the resources at their disposal. Aligning two disciplines in their entirety with respect to one another is no small task, however, and where it is achieved a case could be made that disciplines have been unified. For the most part such thorough integration is not realistic and it appears that interdisciplinarity indeed often proceeds in the absence of such unification. But then, how much unification is necessary, how much pluralism can be tolerated, and in what respects? As have been noted already there are several models.

To return to Jantsch briefly, for him transdisciplinarity represents the final stage, the stage at which the "ultimate degree of coordination" (Jantsch, 1972, 17) can be achieved (see Figure 5.1).[1] This stage is preceded by five previous stages, or lower forms. The first two of these are disciplinarity and multidisciplinarity, neither of which is integrative at all. What is particularly interesting is the difference between, on the one side, interdisciplinarity (in the narrow sense Jantsch prefers) and *crossdisciplinarity*, and on the other, *pluridisciplinarity.*

Where there is interdisciplinarity and crossdisciplinarity the work is organized under a common "axiomatics" (Jantsch, 1972, 16) deriving either from one of the involved disciplines, or from somewhere else. For Jantsch, that somewhere else is the next hierarchical level.[2] In pluridisciplinary work, however, no such axiomatics are present. The separation of crossdisciplinarity/interdisciplinarity and pluridisciplinarity highlights a distinction that can be drawn between two rather different approaches, or perhaps strategies, in

[1]Contemporary attempts to draw up taxonomies are often based on some of Jantsch's terminological innovations (especially the trichotomy of multi-, inter-, and transdisciplinarity), but they are considerably more extensive and often messier than Jantsch's own suggestion (Klein, 2010; Huutoniemi *et al.*, 2010).

[2]Jantsch embraced a view of the disciplinary system as already organised in a certain, rather tidy, fashion. At the bottom of the pyramid was the empirical level where, for example, physics, psychology and biology are situated. One level up is the pragmatic level, where one finds physical technology and natural ecology. The penultimate level is normative and contains, for example, social systems design. The top of the pyramid is labelled "meaning, values" (Jantsch, 1972, 14).

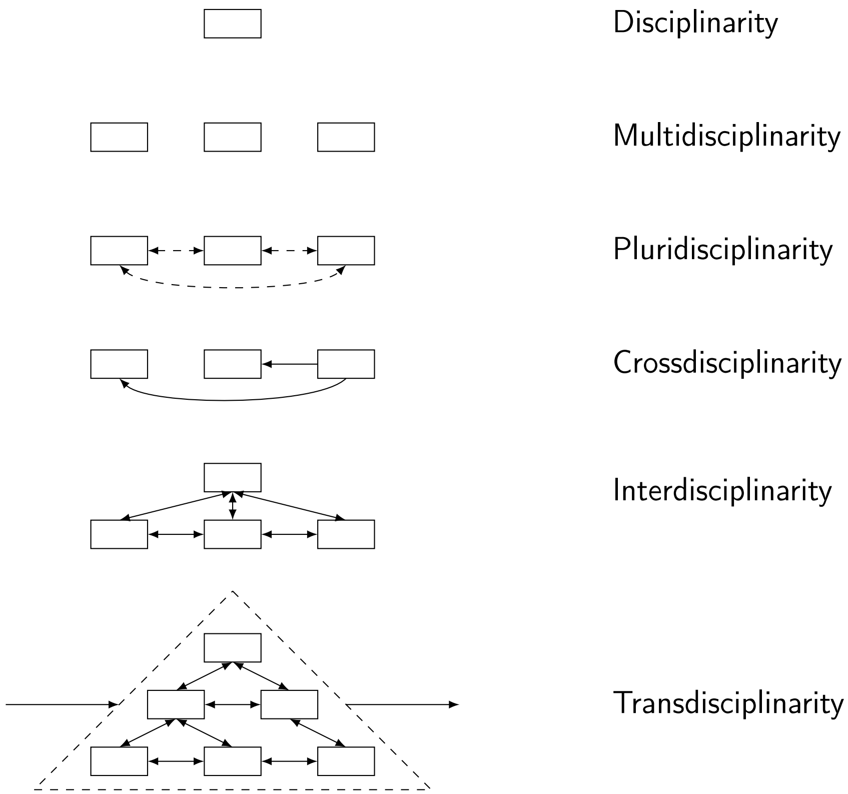


Figure 5.1: Jantsch's integrative stages (Jantsch, 1972, 15)

interdisciplinarity research: one that emphasizes unification, and another that emphasizes pluralism. These two approaches, concern the starting points, rather than the targets, of interdisciplinarity.

5.1.1 The Unificationist Model

Jantsch's conceptions of interdisciplinarity and crossdisciplinarity eschew the idea that a unifying framework (an 'axiomatics') of some sort functions as the organizing principle from which research then proceeds. I will call this approach to interdisciplinarity the unificationist model. Let us consider a few examples from sustainability research.

5.1.1.1 Resilience Theory

First, resilience theory. We have already seen (Thorén and Persson, 2015; Thorén, 2014, 2015b; Chapters 3 and 4) that the resilience framework is one of the most influential theoretical contributions in sustainability science. Resilience theory is often categorized as an *integrative* theory (Holling *et al.*, 2002, 21). The hope is that this theory can "combine disciplinary strengths while filling disciplinary gaps", support decision making processes, and make way for more robust decisions (Holling *et al.*, 2002, 8). *How* is this to be realized within resilience theory, then?

With the caveat that sustainability science is broad, and bearing in mind that resilience theory is not a well-defined theory, the main way of going about this integration is, simply, to cast (or recast) a range of different phenomena in terms of concepts supported within the resilience framework. A wide range of phenomena, objects, and structures commonly studied in disciplines such as political science, economics, anthropology and sociology are thought of as complex adaptive systems, or parts of complex adaptive systems, and thus analogous in many respects to ecosystems. They are thus fitted into a theory of systems dynamics construed around concepts such as that of an *adaptive cycle* and that of a *panarchy.* In this fashion the resilience framework is 'plugged into' a range of other disciplines. Here is an example:

> Competitive processes lead to a few species becoming dominant, with diversity retained in residual pockets preserved in a patchy landscape. While the accumulated capital is sequestered for the growing, maturing ecosystem, it also represents a gradual increase in the potential for other kinds of ecosystem futures. For an economic or social system, the accumulating potential could as well be from the skills, networks of human relationships, and mutual trust that are incrementally developed and tested during the progression from r to K. (Gunderson and Holling, 2002, 35)

The symbols r and K denote different phases within an adaptive cycle. In this way the framework is then deployed to explain facts that have perhaps generally been explained by historians, sociologists, economists, or political scientists. Radical changes such as revolutions, economic collapses, or wars become targets for the framework. So they are now explained in terms of loss of resilience.

That is to say, one of the principle ways in which the resilience framework connects these disciplines appears to be by *explaining* facts that one would normally think of as belonging to the domain of the disciplines. Interdisciplinary ambitions are almost completely determined by the framework in question. Further disciplines are included to fill in the details. The framework is the organizing principle, and anything that is not captured within it is simply left out of the picture.

5.1.1.2 Tipping Points

Second, in an influential article in *Proceedings of the National Academy of Sciences* Timothy Lenton and his colleagues Lenton *et al.* (2008) identify a number of *tipping points* in the Earth's climate system (see also Thorén and Persson, 2013). The systems in which they are interested are "components of the Earth system", by which they mean subsystems of the Earth system at least "subcontinental in scale" (Lenton *et al.*, 2008, 1786). Examples include the Greenland Ice Sheet, Atlantic Thermohaline Circulation, and the Indian Summer Monsoon (a further six examples are given). Lenton and colleagues identify *tipping elements* in these systems. Such an element exists if the control parameters of the system can be "transparently combined" into a control ρ to which there is a critical value, ρ_{crit}, that is so that if the system exceeds this ρ_{crit} it shifts (after a period of time) into some other state (i.e. there is a change in some very important system feature) (Lenton *et al.*, 2008, 1786). ρ_{crit} is then the tipping point of the component. A *policy relevant* tipping point meets further criteria relating to how the system in question is valued, how quickly it can be pushed above the tipping point, and the consequences of it being thus pushed. Having argued for the existence of policy relevant tipping elements and policy relevant tipping points in connection with nine Earth system components, including the three indicated above, Lenton and colleagues call for further studies, apparently attempting to reach out to the social sciences. They write:

> A rigorous study of potential tipping elements in human socioeconomic systems would also be welcome, especially to address whether and how a rapid societal transition toward sustainability could be triggered, given that some models suggest there exists a tipping point for the transition to a low-carbon-energy system. (Lenton *et al.*, 2008, 1792)

If we do interpret this as a call for interdisciplinarity, it is clearly one conditioned by the use of the tipping point framework that Lenton and colleagues have introduced. A framework is provided at the outset, and then further disciplines are sought to work out some of its details. This framework effectively fixes the manner in which further contributions can be made. It provides a range of concepts and notions, and even formal models. It remains for the social scientist merely to identify system components that fit those concepts and models.[3]

5.1.1.3 Central Features of the Unificationist Model

Generally speaking, I think we can discern three closely related features of the framework model that are typical of it, although perhaps not necessary to it. The model tends to be 1) monist, and 2) reductionist. Often, it also 3) excludes perspectives. The attribution of monism here is uncontentious. The idea is to introduce a *single* framework that unifies the disciplines involved and organizes them. That is just what monism means in this context.

There are many forms of reductionism and the unificationist model is not tied to any one of these in particular, although some are perhaps more prominent. Again, consider the resilience theory example. The framework is reductionist in at least two senses. First, it treats a wide range of different systems as members of a single class or type: thus ecosystems, economics, organizations and social-ecological systems are all *complex adaptive systems*. This is a kind of ontological reduction (see Searle, 1992). Second, it purports to explain a number of facts otherwise explained by, say, economic, historical, or social theories, in terms of systemic causes.

[3] Just to be clear, whether or not these two examples in fact exemplify the approach depends on the intentions of those behind the respective frameworks. With a broad movement like resilience theory these intentions are likely to differ between scientists and theoreticians.

This is what Kenneth Schaffner has called *indirect theory reduction* (Schaffner, 1967).[4]

In the interdisciplinarity literature the terms 'reduction' and 'reductionism' are often used almost as invectives. They are suggestive of an outdated mode of scientific inquiry that is possibly imperialist (in the sense implying some kind of illegitimacy) and intrinsically wrong-headed. In a particularly scathing passage Andrew Sayer remarks: "Disciplinary parochialism, and its close relative disciplinary imperialism, are a recipe for reductionism, blinkered interpretations, and misattributions of causality" (Sayer, 2000, 7). However, although one may be critical of reductionist theories of science or strongly reductionist metaphysics, it seems odd to be critical of reductionism as an explanatory strategy. For, sometimes it works, and when it does nothing is lost in the process. Moreover, establishing a reductive relationship is a very effective way of organizing interdisciplinary research, as it provides a hierarchy internal to the context in which the framework is introduced and a system for dividing cognitive labour.

Finally, the unificationist model for interdisciplinarity tends to be exclusive, rather than inclusive, and perspectives that fail to fit the framework are simply left out. For example, resilience theoreticians are not unaware of negative analogies between social systems and ecosystems. On the contrary, they recognize these disanalogies and have pointed to several themselves (see Westley *et al.*, 2002). Nonetheless, along with any framework comes a whole value system that determines what is important, or central, or interesting. In the framework of resilience theory, everything revolves around persistence and radical change. So although resilience theory is often

[4]I will not go further into the literature on reductionism. Ernest Nagel's (1961) account of theory reduction is perhaps the best known. It was then developed by Kenneth Schaffner (1967; 1969). The notion of indirect theory reduction is Schaffnerian. See also Searle (1992) for a brief but clear overview of the various kinds of reduction, including ontological reduction.

couched in language that plays down the unificationist tendency, with the insinuation that we are dealing with a "heuristic theory of change" or a "metaphor" (see e.g. Carpenter and Gunderson, 2001), it still has quite far-reaching implications for how, precisely, the disciplines involved make their contributions.

Underpinning the unificationist model, naturally, is the assumption that the subject matter at hand can be accurately represented, or anyway made intelligible, within the boundaries of the suggested framework. This assumption is sometimes founded on ideas about the ontology of that subject matter, as appears to be the case with resilience theoreticians.

5.1.2 The Pluralist Model

The counterpoint to the unificationist model for interdisciplinarity is a pluralist alternative that emphasizes not unity, but rather plurality. However, it does not emphasize plurality only. The pluralist model of interdisciplinarity is not multidisciplinary. It does not imply the mere "[j]uxtaposition of various disciplines, sometimes with no apparent connection between them" (Apostel et al. 1972, 25), and it is not, as Julie Thompson Klein puts it, a form of interdisciplinarity that is "essentially *additive*, not *integrative*" (Klein 1990, 56). The model I have in mind here is integrative, although it does not proceed from a shared (or imposed) framework, or common set of principles or axioms. Some philosophers have emphasized the way in which different disciplines can come together to produce compound explanations (Brigandt, 2010; Love, 2008; Mitchell *et al.*, 1997). Sometimes the causal background of a phenomenon is such that it cannot be captured by a single account because the phenomenon has many different causes and their specific composition varies from instance to instance (Mitchell, 2002, 2003). At other times decompositions of a system into subsystems cannot provide neat, differing levels of description;

the individual subsystems are located on several levels of description and "cross boundaries between theoretical perspectives" (Wimsatt, 1972, 73). In such cases efforts to account for a specific phenomenon will require contributions from several theories, often drawing on different disciplines. No particular discipline will be fundamental, and there will be no obvious overarching framework in place under which all facts have to be expressed.

5.1.2.1 The CO_2 Cycle

There are several examples in the literature where the pluralist model is applied. Most involve explanations of various biological phenomena and thus relate biological sub-disciplines to one another. We have already mentioned one of Mitchell's favourite examples: the division of labour among social insects (see Chapter 3; also below). We will return to Mitchell's example, but first let us consider a different case: the discovery of the CO_2 cycle (Weart (2003); see also Thorén and Persson (2013)). In our understanding of Earth's carbon cycles the role of the oceans is important, because CO_2 dissolves in water. Until the mid-1950s estimates of the average time a CO_2 molecule spent in the atmosphere ranged from a few hours to millennia (Craig, 1957, 2). An important step in producing accurate estimates came in 1957 when Roger Revelle and Hans Suess (see Revelle and Suess, 1957), working at the Scripps Institute, published a paper that linked a number of different mechanisms. One was chemical. Ocean water is chemically complex and works as a buffer solution, which means that although most CO_2 is dissolved rather quickly it is almost immediately released into the atmosphere again. The other was mechanical. Oceans are not homogenous masses of water, but are made up of layers. Mixing within a layer is rather quick, whereas mixing between then is excruciatingly slow. As further chemical reactions are relevant, especially at the ocean floor, CO_2 will eventually dissolve in the oceans, but this process depends on the water mixing, which is very slow. So estimates of average times need to be somewhere around the upper limits of what had previously been suspected.

This type of reasoning draws on knowledge from several disciplines, integrating it into a concrete result. However, this appears to be achieved in the absence of general framework into which the disciplinary knowledge is first fitted or organized.

5.1.2.2 Central Features of the Pluralist Model

The pluralist model of interdisciplinarity is, unsurprisingly, an inversion of the unificationist model in that it tends to be: 1) pluralist, 2) anti-reductionist, and 3) inclusive of alternative perspectives. Again the case for pluralism hardly needs to be spelled out, although it should be stressed that it is primarily *theoretical frameworks* that are stake here. Arguably, a collaborative effort always requires *some* kind of framework, although not necessarily a theoretical one. Disciplines can be practically integrated (see e.g. Grantham, 2004).[5]

The anti-reductionist feature of the pluralist model concerns the relationship between the disciplines. In the example described above Revelle and Suess effectively connect a number of theories concerning mechanisms at different levels of organization. That, quite clearly, is not the same thing as setting up a reductive relationship. On Mitchell's integrative pluralist account theories that target different causes are integrated at the level of concrete particulars (or causally homogenous types). Such a connection does not involve reducing one theory to another.

[5]Grantham offers a short typology of practical integration. Among them he counts *heuristic dependence*, the reliance of one discipline upon another for interesting problems and hypotheses. Other possibilities he entertains are conformational dependence and methodological integration. See Grantham (2004, 143f).

5.2 An Example: Complexity in Sustainability Science

The unificationist and pluralist approaches to interdisciplinary collaboration are not theories about interdisciplinarity. Rather, they describe two bases from which interdisciplinarity may begin. Both models for interdisciplinary collaboration have been alluded to within sustainability science.

In order to see the difference more clearly, let us consider the following example: how complexity is treated within sustainability science. The concept of complexity has been used in all manner of ways in the literature, and I shall not attempt to provide a complete analysis of it here.[6] Complexity is, however, a central part of the project of sustainability science, because those working in the field consider the problems and challenges of sustainability to be fundamentally complex (Jerneck *et al.*, 2011; Ostrom, 2007). Most sustainability scientists treat complexity as a distinguishing feature of the field—it is, primarily, this complexity, that necessitates an interdisciplinary (or transdisciplinary) approach. Jerneck et al. write: "In sum, the present scientific understanding signals that sustainability challenges are multi-scalar, multi-faceted and strongly interrelated in complex ways that require integrated solutions across scales and domains" (Jerneck *et al.*, 2011, 72).

With the problem of complexity in mind, one can clearly see tensions within the field of sustainability science between the unificationist approach to interdisciplinarity and the pluralist approach.

It is quite common to treat complexity as a modelling problem. Complexity is reduced to a kind of systemic uncertainty here. The

[6]Several philosophers have had an interest in complexity. William Wimsatt is one (Wimsatt, 2003); Sandra Mitchell is another; Mark Bedau is a third (Bedau, 1997, 2003, 2008). See also Hooker (2011) for a monumental collection of essays on the topic and Thorén and Gerlee (2010) for a discussion on complexity and emergence.

problem becomes one of trying to model, or perhaps quantify, this uncertainty in some way. A well-known example of this is the pioneering work by Edward Lorenz on non-linear behaviour.[7] In the context of sustainability science resilience theory, again, serves as an example. In a paper entitled 'Understanding the complexity of economic, ecological, and social systems' Holling outlines the main features of the resilience framework and the intentions behind the project:

> The view presented here argues that there is a requisite level of simplicity behind the complexity that, if identified, can lead to an understanding that is rigorously developed but can be communicated lucidly. It holds that if you cannot explain or describe the issue of concern using at least a handful of causes, then your understanding is too simple. If you require many more than a handful of causes, then your understanding is unnecessarily complex. (Holling, 2001, 391)

Few would disagree with the idea that representations in science should be "as simple as possible but no simpler" (Holling, 2001, 391) as a general guideline. In general Holling is of the view that these frameworks can be kept quite simple suggesting that the complexity of the systems being studied is captured by describing the interactions of a "smaller number of controlling processes" (Holling, 2001, 391). The most significant presupposition that Holling makes, is that complexity itself can be captured and controlled within an integrative theory of complex systems: "a theoretical framework and process for understanding complex systems" (Holling, 2001, 391).

Holling, in effect, treats complexity as a *specific* obstacle, one that can be overcome through the adoption of a suitable approach,

[7] See Kellert (1993) for a discussion on that particular case.

which will often be a formal or mathematical approach. This approach is effectively a unifying framework that can be applied to all things complex.

However, there is another way of thinking of complexity. In essence this is a different view of what complexity is and what its implications are. Consider the following remarks made by Yuya Kajikawa and his colleagues:

> This complexity calls for an interdisciplinary approach to account for multiple factors and to design solutions by employing, utilizing, and integrating diverse knowledge, skills, and tools from each discipline. In addition, it is obvious that sustainability must be achieved in the real world. It is neither a mere slogan nor an abstract, academic concept. Therefore, we have no alternative but to engage society in collaboration and to attempt change in an environment that requires transdisciplinary practices. (Kajikawa *et al.*, 2014)

One way of reading this is as follows: a phenomenon or problem is complex if no single framework can capture it. This is what it means for something to be complex. This view differs quite radically from Holling's and is deeply pluralistic. If this interpretation is correct, Kajikawa and his colleagues are far from alone in understanding complexity in this way. Mitchell has made similar points. She writes: "This 'fact' of pluralism, on the face of it, seems to be correlated not with maturity of the discipline, but with the complexity of the subject matter" (Mitchell, 2002, 55). She continues, "Pluralism reflects complexity" (Mitchell, 2002, 55). On this view complexity *just is* that which cannot be captured within a single account.

5.3 Problems with the Unificationist Model

So both approaches have their defenders in sustainability science. Which approach should generally be taken, then? There are concerns about both models. The pluralist model, for example, runs the risk of becoming overtly holistic. In the sustainability literature holism often seen as something deeply positive, but there are clearly limits: there are pragmatic concerns to take into account, and all models need to be cognitively accessible, and tractable, to be useful at all. One cannot include everything (cf. Hempel, 1965).

There are several reasons why we might be sceptical about the unificationist model of interdisciplinarity, both in general and in the context of sustainability science in particular. Here are four arguments.

5.3.1 The Parochial Framework Argument

The first argument against the unificationist model draws on the idea that frameworks tend to be parochial and exclusive. The worry is this: if one is interested in drawing on the resources of a range of disciplines, as is explicitly the case in sustainability science, then forcing those disciplines into a predetermined framework can be too prohibitive. Conceivably, the offer of collaboration might not be particularly tempting for the 'invited' disciplines. This is not *necessarily* problematic, but it can be challenging in certain contexts. Although this worry has not been raised directly in the literature, it is clear that the inclusion of many perspectives is often viewed as a virtue, and even as necessary, within sustainability science (Bettencourt and Kaur, 2011; Komiyama and Takeuchi, 2006). This argument proceeds from a pair of premises, namely: that the framework in question *is* in fact exclusive, and that one is interested in harnessing the resources already present in some set of disciplines. But these claims, as has already been pointed out, are widely embraced.

5.3.2 The Argument from Representation

Another, quite general, problem draws on pluralist arguments. A theoretical framework is a representation of reality, and as such it is inherently limited. An analogy can be made with a regular map. A map that contains every minute detail of the place it depicts is just a copy of that place. It is just as easy to get lost in as the place itself. It is therefore useless as a map. Scientific representations, it is argued, are also constrained by pragmatic considerations; they cannot be severed from their uses (Mitchell, 2009, 32). In order to have workable representations something—usually, quite a lot of things—must be left out.

The upshot of this argument is recognition that there are always several ways of making scientific representations. Connectedly, we see that quite which representation—or, for present purposes, combination of representations—is preferable will depend on what one is planning to do with the representations.

The argument thus substantiates the point previously argued, by supporting and generalizing the first premise. Any framework is, by necessity, exclusive.

5.3.3 The Argument from Complexity

There are problems with the above argument, however. For one thing, there are special cases in which representations and that which they represent are really one and the same thing. Consider, for example, the study of artificial systems, and cellular automatons such as the Game of Life (GOL).[8] GOL is a game that plays out over

[8] This game was invented by John Conway and made popular by Martin Gardner (1970). It has received considerable attention ever since, not least from philosophers. Mark Bedau builds his notion of incompressibility around

a lattice in which each cell can be either live or dead. The state of each cell is determined by the states of cells immediately around it, and the game is played in rounds. Live cells with two or three live neighbours survive, dead cells with exactly three live neighbours spring to life, and live cells with four or more live neighbours perish. These rules create patterns which, for some initial configurations, appear to be impossible to predict analytically (Bedau, 2003, 2008). The point is that the object of study here is GOL *and* it is studied by using the game itself.

Thus the argument from representation is sometimes augmented by a complexity argument. This argument states that for a complex reality there is no single representation that can be complete and thus we have to resort to aggregated explanations (Mitchell, 2002; Kellert *et al.*, 2006).

5.3.4 The Time Frame Argument

Finally, a fourth argument that one might raise against the unificationist model, at least in the context of sustainability science, is that producing a *good* overarching framework is exceedingly difficult and hence takes substantive amounts of time. For, if the intent is to produce overarching frameworks that manage to relate two disciplines to one another in every respect, then the risk is that the strategy is quite simply a non-starter. There are many examples from the sciences where distinct disciplines apparently have similar interests—take psychology and neuroscience, for instance—and where figuring out *how*, precisely, the disciplines relate to one another remains something of a mystery.

A leading thought in this thesis is that interdisciplinarity in general, and problem-feeding in particular, can proceed from a much less coherent base than the unificationist model would suggest.

---

the example of GOL (Bedau, 2008, 2003). It has also been discussed by Daniel Dennett (1991).

Perhaps the example of psychology and neuroscience could be taken as a case in point. In spite of the difficulty we have understanding how these disciplines are related to one another, and although this difficulty remains unresolved, generations of cognitive scientists have been able to draw from both.

5.4 Pluralism, Communication, and Knowledge

Before we conclude, let us return to the topic of pluralism and problem-feeding. In the previous chapters we have discussed several pluralist accounts. These focus to large extent on ontology and metaphysics in their defence of pluralism and non-reductive integration. Mitchell's integrative pluralism (Mitchell, 2002, 2003, 2009) is probably one of the most influential and well-known accounts. She outlines how different disciplines can contribute to the explanation of phenomena by supplying distinct theories that target individual causes in a causal cluster (see Chapter 4). Her own main example concerns the division of labour in social insects, but many other phenomena appear to exhibit a similar structure. The recent warming in the climate system is an example close to home for sustainability scientists. A range of different theories have been provided at various levels of description that explain recent climate change. The raised concentration of CO_2 in the atmosphere over the last 150 years or so is the main culprit. That the main culprit is the industrial revolution, and the carbon-based economy it ushered in, cannot be doubted, but of course a range of different factors come into play nonetheless. There is a natural variability in the climate system. Climate is possibly affected by, for example, variability in solar phenomena, such as the prevalence and intensity of sunspots and solar storms. Positive feedback loops also play a central role. It is obvious that climate change as a general phenomenon may have many different of causes. It has happened many times before on this planet. There is never going to be a "general theory of climate change". Certainly, there is not going to be one invoking a single cause, but neither is there going to be one that lists all causes and gives the proportion of their influence relative to one another. Different climate change events have different causal profiles. In this sense the different theories of why climate change occurs cannot be integrated at the level of theories themselves. The theories can be integrated, however, at the level of concrete particulars, such as in explaining the climate change event that we are currently experiencing. However, if Mitchell's integrative pluralism is correct (with respect to this phenomenon) it is quite possible that many of the theories addressing the causes of climate change are true at the same time.

Others have focused more heavily on issues of language and communication. One idea that has become influential, especially in science and technology studies (popularly abbreviated as STS), involves the notion of a *boundary object*. Susan Leigh Star and James Griesemer (1989) once proposed that an object of this sort—usually, some concrete object, such as a map—could, by being "plastic enough to adapt to local needs [...] yet robust enough to maintain a common identity across sites" (Star and Griesemer, 1989, 393), mediate communication between parties that otherwise belong to different social worlds. In the kind of case with which Star and Griesemer's are concerned the different social worlds are those of different people, and groups of people, associated with Berkeley's Museum of Vertebrate Zoology. The notion of a boundary object has, however, been widely deployed in discussions of interdisciplinarity (e.g. Collins *et al.*, 2007).[9]

Similarly Peter Galison has suggested that *trading zones* can arise between incommensurable paradigms. Here exchange is made

[9] For discussions of boundary objects, resilience, and sustainability science see, for example, Thorén (2014) and Brand and Jax (2007).

possible by local coordination rather than by the harmonization of global meaning. Galison draws on an anthropological example of monetary interactions between peasants and members of the landowning class in the Cauco valley in southern Columbia. He discusses the purchase of goods, the payment of rent, and so forth. Within such exchanges "both sides are perfectly capable of working within established behavioral patterns" (Galison, 1997, 804) in spite of the wildly different understandings of the significance of the exchange itself within the two groups. For the landowners, money has a certain set of properties (it accumulates into capital, for example). For the peasants, it has other properties. For example, a peso bill can be baptized and then called to return. If one pays for something with a baptized bill, one may at a later stage call on it to return, and in this situation it will bring its kin along. Galison suggests that similar trading zones can be established between scientific 'cultures'. He notes that there was a "rich experimental subculture" (Galison, 1997, 812) within which the contrasting theories of Abraham, Lorentz, Poincaré, and Einstein were compared, in spite of the fact that these different theorists used the concept of mass very differently from one another. Moreover, the work of this subculture, spearheaded by Max Kaufmann and Alfred Bucherer, was "clearly understood by all four of the relevant theorists" (Galison, 1997, 812). Galison takes this as an indication that, in spite of global differences in the mass concept, there was a "localized zone of activity" where some exchange was able to occur (Galison, 1997, 813).

Different groups engaged in prolonged interaction can develop contact languages that range from rather simple pidgins, to more sophisticated creoles (Galison, 1997, 831ff). The pidgins are merely blends of already existing languages and would not readily be used as a 'first language', whereas creoles may be so used.

Some of Galison's examples differ from what has been discussed here. For example, in the above example Galison is concerned with theory change and competing paradigms, whereas my focus, rather, has been on theories and paradigms that are compatible.

Finally, a third account, and one that has gained a considerable reputation, is Harry Collins' notion of *interactional expertise* (Collins and Evans, 2002; Collins, 2004). Collins argues that interactional expertise sits somewhere between formal propositional knowledge and informal, or tacit, knowledge. Informalists, such as Collins, have traditionally believed that some kinds of knowledge, such as the knowledge involved in executing a skill like riding a bike, can never be fully expressed propositionally. One can read about it all summer, but unless one actually rides a bike one will never have a full understanding of the skill involved. For propositional knowledge, no such direct experience is necessary. This has been taken to have consequences for artificial intelligence: a competent judge would be able to tell the novice who has only read about bike riding from the accomplished practitioner.

Collins suggests that this informalist conclusion is misguided, as it overlooks interactional expertise. This kind of knowledge—the third kind, as it were (Collins, 2004)—can be acquired without having to practice the skill, although it requires the person seeking such knowledge to devote "enough time talking with the practitioners of the relevant domains" (Collins, 2004, 127). Interactional expertise, argues Collins, is what the sociologist (and perhaps, the philosopher) of science needs to have in order to communicate successfully with expert practitioners, but it is different from contributory expertise, which is what the practitioners themselves have. Interactional expertise enables one to "convey the scientific thoughts and activities of others" (Collins, 2004, 128).

Some of these ideas, especially the notions of boundary object and boundary work, have been influential in the sustainability science literature. Brand and Jax (2007) argue that the notion of resilience may indeed function as a boundary object.[10] We may wish to see this explained more fully before accepting it (see Thorén, 2014). Nonetheless, the suggestion here is that communication can occur across fields that otherwise are disorderly.

5.5 An Interdisciplinarity for Sustainability

What is a suitable approach for sustainability science as a whole? I think it is rather clear that a pluralist approach to interdisciplinarity is advisable. This is not least because sustainability science is such an important project that if it were to be hijacked by one discipline or another the consequences could be much worse than merely producing some science of lower quality than it might have been.

I think the problem orientation of sustainability science should not be forgotten. It is not an end in itself to integrate disciplines, and unless such integration is instrumental in solving problems it may cause more problems than it solves. Integration and unification are, after all, exceedingly difficult. Instead of trying to integrate disciplines, efforts should be targeted at stabilizing problems and maintaining interdisciplinary contacts. Although this has its own challenges and is by no means easy, it does appear to be a practical, rather than a theoretical, challenge. One way of realizing this problem orientation within a pluralist approach to interdisciplinarity is by engaging in problem-feeding.

Furthermore, the problem-feeding account is not specific to scientific disciplines: it could be applied, for example, to the relationship between science and society. A core aim of sustainability is to produce knowledge that is socially relevant, or robust, as some prefer to express it. It may be that one way to achieve this goal is to maintain problem stability between the parts involved, so that

[10] See also Clark (2011).

the transformations are mutually acceptable and thus the right problems are solved.

To conclude, I would like to summarize this introductory essay in four points. First, there are many ways of understanding what 'problem orientation' (or 'solution orientation') actually implies. In this thesis I have outlined one way and pointed to some challenges. What appears especially problematic is the way in which a problem solving process often involves several stages of problem transformation. In interdisciplinary contexts, norms and conventions governing such transformations are often lacking. Moreover, the kinds of transformation that are deemed permissible depend on what is regarded as interesting, or central, or important. Such evaluative judgements are made on the basis of values that may also differ across disciplines. There is no easy way out here: one overcomes these challenges by paying attention to them and making them explicit. But, the issue highlights the fact that collaborative interdisciplinary efforts—and joint problem solving is certainly such an effort—often involve actively aligning the disciplines involved with one another. It is not all about ontology, but also about axiology, and the axiology is not always about 'discovering' a connection because it is sometimes about making one.

Second, I have outlined a more specific way of treating what a "focus on the problems" might amount to in the problem-feeding model of interdisciplinary collaboration. Problem-feeding also plays other roles in this thesis. It is a way of realizing the Popperian ideal in the context of social disciplines. That is, instead of the enquirer importing whatever he or she needs, problems may instead be exported to those who happen to have the appropriate tools. Problem-feeding may also work as a model for collaboration in sustainability science. Indeed the field is itself the result of such an attempted transfer.

A third theme in this thesis has been the issue of scientific imperialism. I have argued for an alternative conception of disciplinary imperialism cast in terms of cross-boundary problem stability. This conception is more closely associated with collaborative interdisciplinarity which is where imperialism appears to matter anyway. In sustainability science imperialism appears as one of the main dangers. The risk here is that the disciplines involved fail to recognize that sustainability science is a collaborative project, and one in which interdisciplinary contacts and connections have to be maintained and nursed. This can lead to a field that is either fragmented or excessively dominated by a single discipline or framework.

A final issue, which that last chapter addresses, focuses on pluralism and unificationism as principles guiding interdisciplinary projects. This issue, though rarely discussed explicitly in the literature on sustainability science, appears nonetheless to be an ever-present undercurrent. In the final chapter I present arguments that point towards the adoption of a pluralist model of interdisciplinarity for sustainability science.

Summary of Papers

This chapter contains summaries of the individual papers contained in this thesis. For the papers that are co-written with others I have added after the summary a short paragraph on the division of labour between the involved authors. The authors will be named by initials in the following way:

HT: Henrik Thorén

JP: Johannes Persson

LB: Line Breian

Paper I

Title: The Philosophy of Interdisciplinarity: Sustainability Science and Problem-Feeding

Authors: Henrik Thorén and Johannes Persson

Publication Status: Published in Journal for General Philosophy of Science 44: 337-355

In this paper we introduce the notion of problem-feeding and present it as a form of interdisciplinarity. We develop problem-feeding in some detail and outline possible barriers to it, or challenges it faces. We also compare problem-feeding to a 'traditional' account of interdisciplinarity: Eric Jantsch's evolutionary ladder (Jantsch, 1972).

First, problem-feeding is a form of problem transfer, and it has been discussed in the literature many times before. An important predecessor is Nancy Maull's notion of *problem shifts* (Maull, 1977). Problem-feeding describes the transfer of one problem, or several problems, from one discipline to another, and it comes in several forms. Unilateral problem-feeding involves no reciprocal exchange of information, as is the case when one discipline relies on another for its problems what Grantham (2004) calls *heuristic dependence*. More interesting is what we call *bilateral problem-feeding*. This involves the transfer of a problem from one discipline to another, but also the transfer of the solution once it is obtained. Bilateral problem-feeding is a fruitful form of interdisciplinarity.

Second, we provide arguments for the view that problem-feeding is a practical form of integration, and for the claim that it can happen even between disciplines that otherwise would not be perceived as closely related. There are, nonetheless, important barriers to problem-feeding. One is lack of mutual trust. In sustainability science, unlike the cases that have most often been discussed in the philosophy of science, the disciplines involved are quite far apart. Often a researcher will lack detailed knowledge of procedures and methods used in other disciplines and have to rely on them anyway. Communication problems may also arise, and these have to be overcome.

Third, the problem-feeding model is discussed in the context of a traditional conception of interdisciplinarity. We suggest that there is an interesting difference between the problem-feeding approach to interdisciplinarity and the traditional integrative perspective suggested by, among others, Jantsch and his colleagues. The interdisciplinarity resulting from problem-feeding between researchers can be local and temporary, and it does not require collaboration across proximate disciplines. By contrast, to make good sense of traditional integrative interdisciplinarity we must arguably associate it with a longer-term, global form of close, interdisciplinary collaboration.

I have been involved in writing every part of this paper as it was revised on numerous occasions. More concretely my contribution to this paper concerns both the central ideas, especially the notion of problem-feeding, and finding and developing the examples used.

This paper went through several revisions and both HT and JP were involved throughout the process. The original idea came from HT and HT produced the examples and worked out the notion of problem-feeding. JP wrote the section on reductionism. Both worked equally on finalizing the manuscript.

Paper II

Title: Resilience as a Unifying Concept

Authors: Henrik Thorén

Publication Status: Published in International Studies in the Philosophy of Science 28(3): 303-324

This is one of three papers in this thesis which discuss the concept of resilience more specifically. In this paper the focus is on resilience as a unifying concept. As has been mentioned in the introductory chapter, the concept of resilience has been widely influential in sustainability science. It is also, however, a contentious concept. In particular issues have been raised as to its suitability when applied to social entities or 'social systems', to use the resilience theoretician's terminology.

A first task in this paper is to give an overview of different concepts of resilience, or difference meanings of the term 'resilience'. Resilience is referred to in a range of different disciplines. Early users of the notion were predominantly materials scientists, with papers in textile research particularly conspicuous in this regard. More recently that is, since the 1970s or so ecologists and psychologists have come to be the dominant users, however. For psychologists the notion has been a locus of discussion in itself, whereas ecologists have been concerned primarily with the notion of stability, and in this connection resilience is treated as a subtype. Nonetheless, there are many uses of the concept, some of considerable complexity. I discuss a many different definitions of the term 'resilience'. Among these I identify two core concepts of resilience. One is local, denoting the ability to return to some point of equilibrium given a disturbance, and the other is global, denoting the ability of a system uphold some property as it is disturbed. Definitional schemas are provided in which more or less specific and distinct concepts of resilience can be produced. It is argued that, in fact, most, if not all, relevant concepts of resilience fit one or the other of these definitional schemas.

It is furthermore argued that there appears to be a strong preference for more abstract versions of the concept in the literature. This preference for abstraction is significant, since it affects the way in which the concept can be expected to connect disciplines. Abstract concepts move across contexts relatively easy because they leave many features of the phenomenon to which they are applied out. Thus there is a sense in which it seems quite unproblematic to talk about the resilience of the Roman Empire, this or that organization, or state, and so on. Two conclusions are suggested. One is that some critics of the concept may have exaggerated the degree to which it is confined to the context of ecology. Many varieties of the resilience concept are abstract enough to be used across a wide array of contexts, including in social science. Another is that proponents of the concept have exaggerated what follows from this broad applicability. Different systems in which it may be appropriate to use the notion of resilience can differ in every respect except exhibiting a resilient behaviour under some description. In other words, resilience may indeed unify many disciplines in this way, but it is a rather weak form of unification.

This explains the apparent context-insensitivity of the resilience concept, but it also presents a problem for those hoping to establish a research programme based on it. It is suggested that, instead, the focus should be on concrete mechanisms for resilience.

Paper III

Title: Replacement and Expansion in Scientific Imperialism

Authors: Henrik Thorén

Publication Status: Submitted

One interdisciplinary relationship that does not involve collaboration, but may nonetheless cause heated discussions and hurt feelings, is disciplinary imperialism. Disciplinary imperialism is commonly thought to involve extending the scope of the cognitive resources of one discipline into a domain associated with another discipline. For example, by applying a theory developed in one discipline to explain facts or phenomena that are part of a domain usually associated with another. Disciplinary imperialism is often, though not always, thought of as a boundary infringement a kind of scientific trespassing. I argue that although it is easy to see why those at the receiving end of imperialism may sense the danger and defend themselves, the complaint that imperialism is epistemically detrimental is harder to maintain. Disciplines claim intellectual ground by their epistemic success, and they are always open to revision. Moreover, as imperialism is a species of unification, it is a good thing in circumstances where it can be achieved.

This paper explores the reasons why imperialism is thought to be failed interdisciplinary interaction. Two general such reasons are identified in the literature. One points to a failure of generalizing or expanding the domain of application of some theory, model, or framework. The other concerns illicit replacement of superior, or compatible, alternative accounts. These are labelled type-I and type-II imperialist failures respectively. It is shown that these two reasons really constitute two independent, although not mutually exclusive forms of scientific imperialism.

It is furthermore argued that the mere failure of a framework, in either of these respects, need not be cause for concern. It is shown that in order to gauge the damage of imperialism one needs to specify the context in which it takes place. Two relevant, limited contexts of this sort are specified. One is the interdisciplinary context, where collaboration is central and replacement may substantially hamper the effort. The other is the context where scientific knowledge is recruited to help direct decision-making processes.

Paper IV

Title: Stepping Stone or Stumbling Block? Mode 2 knowledge production in sustainability science

Authors: Henrik Thorén, Line Breian

Publication Status: Submitted

The concept of 'Mode 2 knowledge production' was introduced in the mid 1990s by Michael Gibbons, Camille Limoges, Helga Nowotny, Simon Schwartzman, Peter Scott, and Martin Trow in their highly influential book *The New Production of Knowledge* (Gibbons *et al.*, 1994). The concept aimed at outlining a development the authors observed in contemporary science, a development away from the institutional structures and cognitive and epistemic patterns of 'traditional science'. Mode 2 knowledge emphasizes heterogeneous and temporally unstable groups forming to solve 'real-world' problems by deploying equally transient frameworks. Narrow disciplinary research driven by department bound traditional academics, Gibbons et al. maintain, is an outmoded form of research that is becoming increasingly threatened by the rise of researchers and experts who's salary come from think tanks, private institutions, and commercial laboratories.

In spite of the fact that this concept is largely descriptive, it has gained a wide following in many different fields. One such field is sustainability science. Here the idea of research that bridges disciplinary boundaries as well as the science-society boundary has immediate appeal.

In this paper the aim is two-fold. First we seek to explore the perceived relationship between Mode 2 knowledge production and sustainability science among sustainability science practitioners.

The reason is that, although the appeal of mode 2 knowledge production is quite understandable, sustainability science nonetheless exhibits several features otherwise associated with traditional science ('Mode 1 knowledge production' on Gibbons et al's terminology). This is relationship is investigated through a survey among sustainability scientists. The second aim is to further the theoretical discussion on mode 2 knowledge. Here the focus is on whether mode 2—and similar approaches—are suitable in the context of grand challenges in general, and sustainability science in particular.

The survey results indicate that sustainability science appears to be on a dual track both showing features associated with mode 2 research and those associated with traditional mode 1 science. We suggest several different explanations that may account for such a trend. These relate both to the resilience of traditional institutional structures and conceptual issues pertaining to the notion of mode 2 itself. In particular we focus on uncertainties regarding the normative content of the term.

This paper grew out of another paper authored by LB, HT, and JP. The version included in this thesis was drafted by HT. LB collected and analysed the data and produced the figures. HT and LB finalized the manuscript.

Paper V

Title: Resilience: Some Philosophical Remarks on Defining Ostensively and Stipulatively.

Authors: Henrik Thorén and Johannes Persson

Publication Status: Submitted

A good deal of the discussion of the concept of resilience, with its promise to unify and integrate research on sustainability, has concerned the content of the concept. Indeed, there are several resilience concepts in use, many of them highly complex. This has sometimes been considered a potential problem. Are we really talking about the same thing? But at other times it has seemed to be a feature that may actually be of benefit. Brand and Jax (2007) have suggested that resilience may be a boundary object, and that in essence the different concepts assist disciplines that would otherwise be unable to communicate to connect and exchange information. In this paper we focus on another aspect of the concept, namely how it is defined.

We focus in particular on the difference between concepts that are stipulatively defined and concepts that are ostensively defined. A stipulative definition often aims to expose a conceptual joint (Belnap, 1993). It can be found in many contexts. Paradigmatic examples are mathematical or logical concepts. A notable feature of stipulative definitions is that they are not open to revision; the essence of the concept is given at the outset. Ostensively defined concepts, on the other hand, serve to point to a phenomenon. The meaning of the concepts depends on the nature of that which is pointed out, and it is often unknown. Ostensive definitions are subject to revision, and the aim is often to replace them eventually.

In this paper we look at examples, drawn from psychology and ecology, of the use of the concept of resilience. We note that the contexts appear at least, at times to be relevantly different. Ecologists have often used to notion of resilience in a formal setting to describe features of their models. Frequently, these mathematical models are highly idealized. Here it appears that stipulative definitions are often used. A telling sign is the interest that ecologists have in general shown in fine-grained conceptual differences between various senses of the concept of stability, where resilience occurs as one such sense. In psychology, on the other hand, there is no obvious parallel to this model methodology. Here the notion of resilience rather appears to have been used in an ostensive fashion to pick out underlying traits or mechanisms. Although conceptual discussions in psychology, like ecology, have been quite prominent, they differ somewhat here. Instead of focusing on conceptual subtleties they appear to discuss where realizations of resilience are to be found.

On basis of this analysis an interesting parallel can be drawn with sustainability science. Into this emerging interdisciplinary field the concept of resilience has entered by way of ecology. The point is thus that sustainability science appears, in many respects, to be a context which is quite similar to psychology. The aim is practical. It is directed at increasing, or sometimes decreasing, the resilience of particular systems, with the aim of making them either less likely to collapse or more likely to do so. (Perhaps one is interested in changing them in some way, and they prove to be resilient in resisting that change.) Thus we stress that there is a possible conflict between stipulatively and ostensively defined notions of resilience, and in the ways in which these notions connect disciplines. We observe that this has not been highlighted in the literature.

HT came up with the idea and wrote the first draft. JP made amendments to that draft. HT and JP worked on revisions after comments by reviewers. HT did roughly 75% of this work. HT and JP finalized the paper.

Paper VI

Title: History and Philosophy of Science as an Interdisciplinary Field of Problem Transfers

Authors: Henrik Thorén

Publication Status: Forthcoming in "Empirical Philosophy of Science – Introducing Qualitative Methods Into the Philosophy of Science", edited by Susann Wagenknecht, Nancy Nersessian and Hanne Andersen, Studies in Applied Philosophy, Epistemology and Rational Ethics, Springer.

This paper is an application of the problem-feeding model to the relationship between history and philosophy of science. The extensive discussions of the relationship between the history of science and the philosophy of science in the mid-twentieth century represent a long history of attempts to grapple with questions about the relevance of empirical research on the practice of science to the philosophical analysis of science. Those discussions also touched upon the issue of importing empirical methods into the philosophy of science through the creation of an interdisciplinary field, namely the history and philosophy of science. In this paper I return to Ronald Giere (1973), and specifically to his claim that history of science as a discipline cannot contribute to philosophy of science by providing partial or whole solutions to philosophical problems. Does this imply that there can be no genuine interdisciplinarity between the two disciplines? In answer to this question, it is first suggested that connections between disciplines can be formed around the transfer and sharing of problems (as well as solutions); and that this is a viable alternative in the search for an explanation of the relationship between history and philosophy of science. Next, it is argued that this alternative is sufficient for establishing a genuine form of interdisciplinarity between the two disciplines. The example of Lindley Darden's *Theory Change in Science* (1991) is used to show how philosophy of science can rely on history of science in this way.

Bibliography

Agrawala, S. (1998). Context and early origins of the intergovernmental panel on climate change. *Climatic Change*, **39**, 605–620.

Andersen, H. (1996). Categorization, anomalies and the discovery of nuclear fission. *Studies in the History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, **27**, 463–492.

Apostel, L., Berger, G., Briggs, A., and Michaud, G., editors (1972). *Interdisciplinarity: Problems of teaching and research in universities.* Organization for Economic Cooperation and Development. Paris.

Arrhenius, S. (1896). On the influence of carbonic acid in the air upon the temperature of the ground. *Philosophical Magazine and Journal of Science*, **41**, 237–276.

Bechtel, W. (1986). The nature of scientific integration. In W. Bechtel, editor, *Integrating Scientific Disciplines*, pages 3–52. Nijhoff, Dordrecht.

Bechtel, W. (1987). Psycholinguistics as a case of cross-disciplinary research: symposium introduction. *Synthese*, **72**, 293–311.

Bedau, M. A. (1997). Weak emergence. In J. Tomberlin, editor, *Philosophical Perspectives: Mind Causation and World*, volume 11, pages 375–399. Blackwell Publishers.

Bedau, M. A. (2003). Downward causation and the autonomy of weak emergence. *Principia Revista Internacional de Empistemologica*, **6**, 5–50.

Bedau, M. A. (2008). Is weak emergence just in the mind? *Minds & Machines*, **18**, 443–459.

Belnap, B. and Steel, T. (1976). *The Logic of Questions and Answers*. Yale University Press, New Haven.

Belnap, N. (1993). On rigorous definitions. *Philosophical Studies*, **72**, 115–146.

Bettencourt, L. M. A. and Kaur, J. (2011). Evolution and structure of sustainability science. *Proceedings of the National Academy of Sciences*, **108**, 19540–19545.

Bolin, B. (2007). *A history of the science and politics of climate change: The role of the intergovernmental panel on climate change.* Cambridge University Press.

Bolin, B. and Eriksson, E. (1959). Changes in the carbon dioxide content of the atmosphere and sea due to fossil fuel combustion. In B. Bolin, editor, *The atmoshphere and the sea in motion*, pages 130–142. The Rockefeller Institute and Oxford University Press.

Brand, F. S. and Jax, K. (2007). Focusing the meaning(s) of resilience: Resilience as a descriptive concept and a boundary object. *Ecology and Society*, **12**.

Brandt, P., Ernst, A., Gralla, F., Luederitz, C., Lang, D. J., Newig, J., Reinert, F., ABSON, D. J., and von Wehrden, H. (2013).

A review of transdisciplinary research in sustainability science. *Ecological Economics*, **92**(C), 1–15.

Brewer, G. D. (1999). The challenges of interdisciplianrity. *Policy Sciences*, **32**, 327–337.

Brigandt, I. (2010). Beyond reduction and pluralism: towards an epistemology of explanatory integration in biology. *Erkenntis*, **73**, 295–311.

Campbell, D. T. (2009/1969). Ethnocentrism of disciplines and the fish-scale model of omniscience. In M. Sherif and C. Sherif, editors, *Interdisciplinary relationships in the social sciences*. Aldine-Transaction.

Carpenter, S. and Gunderson, L. (2001). Coping with collapse: ecological and social dynamics in ecosystem management. *BioScience*, **51**, 451–457.

Clark, W. C. (2011). Boundary work for sustainable development: Naturalresource management at the Consultative Group onInternational Agricultural Research (CGIAR). *Proceedings of the National Academy of Sciences*, pages 1–8.

Clarke, S. and Walsh, A. (2009). Scientific imperialism and the proper relations between the sciences. *International Studies in the Philosophy of Science*, **23**, 195–207.

Clarke, S. and Walsh, A. (2013). Imperialism, progress, developmental teleology, and interdisciplinary unification. *International Studies in the Philosophy of Science*, **27**(3), 341–351.

Collins, H. (2004). Interactional expertise as a third kind of knowledge. *Phenomenology and the Cognitive Sciences*, **3**(2), 125–143.

Collins, H., Evans, R., and Gorman, M. (2007). Trading zones and interactional expertise. *Studies in History and Philosophy of Science: Part A*, **38**, 657–666.

Collins, H. M. and Evans, R. (2002). The Third Wave of Science Studies: Studies of Expertise and Experience. *Social Studies of Science*, **32**(2), 235–296.

Cox, R. (1981). Social forces, states and world orders: beyond international relations theory. *Millenium*, **10**, 126–155.

Craig, H. (1957). The natural distribution of radiocarbon and the exchange time of carbon dioxide between atmosphere and sea. *Tellus*, **9**, 1–17.

Crutzen, P. and Stoermer, E. (2000). The "anthropocene". *IGBP Newsletter*, **41**, 17.

Darden, L. (1991). *Theory change in science: Strategies from Mendelian genetics*. Oxford University Press.

Darden, L. and Maull, N. (1977). Interfield theories. *Philosophy of Science*, **44**, 43–64.

Davidson, D. (2010). The Applicability of the Concept of Resilience to Social Systems: Some Sources of Optimism and Nagging Doubts. *Society & Natural Resources*, **23**(12), 1135–1149.

Dennett, D. (1991). Real patterns. *The Journal of Philosophy*, **88**, 27–51.

Diacu, F. (1996). The solution of the n-body problem. *The Mathematical Intelligencer*, **18**(3), 66–70.

Dupré, J. (1996). Against scientific imperialism. In M. Forbes, D. Hull, and R. M. Burian, editors, *PSA 1994: Proceedings of the*

1994 Biennial Meeting of the Philosophy of Science Association. East Lansing, MI: Philosophy of Science Association.

Dupré, J. (2001). *Human nature on the limits of science*. Oxford University Press.

Edwards, P. N. (2010). *A vast machine*. The MIT Press.

Funtowicz, S. and Ravetz, J. (1993). Science for the post-normal age. *Futures*, **September**, 739–755.

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

Gärdenfors, P. and Sahlin, N.-E. (1982). Unreliable probabilities, risk taking, and decision making. *Synthese*, **53**, 361–386.

Gardner, M. (1970). Mathematical games: the fantastic combinations of john conway's new solitaire game "life". *Scientific American*, **223**, 120–123.

Gibbons, M. (2000). Mode 2 society and the emergence of context-sensitive science. *Science and Public Policy*, **27**(3), 159–163.

Gibbons, M., Limoges, C., Nowotny, H., Schwartzman, S., Scott, P., and Trow, M. (1994). *The New Production of Knowledge*. SAGE.

Giere, R. (1973). History and philosophy of science: intimate relationship or marriage of convenience? *The British Journal for the Philosophy of Science*, **24**, 282–297.

Gigerenzer, G., Todd, P., and The ABC Research Group (1999). *Simple heuristics and make us smart*. Oxford University Press.

Gould, S. J. and Lewontin, R. C. (1979). The spandrels of san marco and the panglossian paradigm: A critique of the adaptationist programme. *Proceedings of the Royal Society of London. Series B. Biological Sciences*, **205**(1161), 581–598.

Grantham, T. (2004). Conceptualzing the (dis)unity of science. *Philosophy of Science*, **71**, 133–155.

Gunderson, L. and Holling, C., editors (2002). *Panarchy: understanding transformations in human and natural systems*. Island Press.

Gutés, M. C. (1996). The concept of weak sustainability. *Ecological Economics*, **17**, 147–156.

Hacking, I. (2002). *Historical Ontology*. Harvard University Press.

Hansson, B. (1999). Interdisciplinarity: for what purpose? *Policy Science*, **32**, 339–343.

Hempel, C. (1965). *Aspects of Scientific Explanation and Other Essays in theory Philosophy of Sciences*. New York: Free Press.

Hirsch Hadorn, G., Pohl, C., and Bammer, G. (2010). Solving problems through transdisciplinary research. In R. Frodeman, editor, *The Oxford Handbook of Interdisciplinarity*, pages 431–452. Oxford University Press.

Holling, C. (1973). Resilience and stability of ecological systems. *Annual Review of Ecology and Systematics*, **4**, 1–23.

Holling, C. (2001). Understanding the complexity of economic, ecological, and social systems. *Ecosystems*, **4**, 390–405.

Holling, C., Gunderson, L., and Ludwig, D. (2002). In quest of a theory of adaptive change. In L. Gunderson and C. Holling, editors, *Panarchy: understanding transformations in human and natural systems*. Island Press.

Hooker, C., editor (2011). *Philosophy of Complex Systems*. Elsevier.

Hornborg, A. (2013). Revelations of resilience: From the ideological disarmament of disaster to the revolutionary implications of (p)anarchy. *Resilience. International Policies, Practices and Discourses*, **1**.

Hoyningen-Huene, P. (1987). Context of discovery and context of justification. *Studies in History and Philosophy of Science*, **18**, 501–515.

Hoyningen-Huene, P. (1993). *Restructuring Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science*. The University of Chicago Press.

Hoyningen-Huene, P. (2006). Context of discoery versus context of justification and Thomas Kuhn. In J. Schickore and F. Steinle, editors, *Revisiting Discovery and Justification*. Springer.

Huutoniemi, K. and Tapio, P., editors (2014). *Trransdisciplinarity Sustainability Studies*. Routledge.

Huutoniemi, K., Klein, J. T., Bruun, H., and Hukkinen, J. (2010). Analyzing interdisciplinarity: typology and indicators. *Research Policy*, **39**, 79–88.

Jantsch, E. (1972). Inter- and transdisciplinary university: a systems approach to education and innovation. *Higher Education*, **1**, 7–37.

Jerneck, A. and Olsson, L. (2008). Adaptation and the poor: development, resilience and transition. *Climate Policy*, **8**(2), 170–182.

Jerneck, A., Olsson, L., Nessand, B., Anderberg, S., Baier, M., Clark, E., Hickler, T., Hornborg, A., Kronsell, A., Lövbrand, E., and Persson, J. (2011). Structuring sustainability science. *Sustainability Science*, **6**, 69–82.

Kajikawa, Y., Tacoa, F., and Yamaguchi, K. (2014). Sustainability science: the changing landscape of sustainability research. *Sustainability Science*.

Kaplan, A. (1964). *The conduct of inquiry*. Chandler Publishing Company.

Kates, R. W. (2011). What kind of science is sustainability science? *PNAS*, **108**, 19449–19450.

Kates, R. W., Clark, W. C., Corell, R., Hall, J. M., Jaeger, C. C., Lowe, I., McCarthy, J. J., Schellnhuber, H. J., Bolin, B., Dickson, N. M., Faucheux, S., Gallopin, G. C., Grubler, A., Huntley, B., Jager, J., Jodha, N. S., Kasperson, R. E., Mabogunje, A., Matson, P., Mooney, H., III, B. M., ORiordan, T., and Svedin, U. (2001). Sustainability Science. *Science: New Series*, **292**(5517), 641–642.

Kellert, S. (2008). *Borrowed knowledge: chaos theory and the challenge of learning across disciplines*. University of Chicago Press.

Kellert, S., Longino, H., and Waters, K. (2006). Introduction. In S. Kellert, H. Longino, and K. Waters, editors, *Scientific Pluralism*, volume XIX of *Minnesota Studies in the Philosophy of Science*. Minnesota Univesity Press.

Kellert, S. H. (1993). *In the Wake of Chaos*. The University of Chicago Press.

Kidd, I. J. (2013). Historical contingency and the impact of scientific imperialism. *International Studies in the Philosophy of Science*, **27**(3), 315–324.

Kitcher, P. (1990). The division of cognitive labor. *The Journal of Philosophy*, **87**, 5–22.

Klein, J. T. (1990). *Interdisciplinarity. History, Theory and Practice*. Wayne State University Press.

Klein, J. T. (2004). Prospects for transdisciplinarity. *Futures*, **36**(4), 515–526.

Klein, J. T. (2010). A taxonomy of interdisciplinarity. In R. Frodeman, J. T. Klein, and C. Mitcham, editors, *The Oxford Handbook of Interdisciplinarity*, pages 15–30. Oxford University Press.

Klein, J. T. (2014). From method to transdisciplinary heuretics. In K. Huutoniemi and P. Tapio, editors, *Transdisciplinary Sustainability Studies: a heuristic approach*, pages xii–xv. Routledge.

Komiyama, H. and Takeuchi, K. (2006). Sustainability science: building a new discipline. *Sustainability Science*, **1**(1), 1–6.

Kripke, S. (1980). *Naming and Necessity*. Harvard University Press.

Kuhn, T. (1965). Logic of discovery or psychology of research? In *Criticism and the growth of knowledge*. Cambridge University Press.

Kuhn, T. (1977). *The Essential Tension*. University of Chicago Press.

Kuhn, T. (1996/1962). *The Structure of Scientific Revolutions*. University of Chicago Press, third edition.

Lang, D. J., Wiek, A., Bergmann, M., Stauffacher, M., Martens, P., Moll, P., Swilling, M., and Thomas, C. J. (2012). Transdisciplinary research in sustainability science: practice, principles, and challenges. *Sustainability Science*, **7**(1), 25–43.

Laudan, L. (1977). *Progress and its Problems: towards a theory of scientific growth*. University of California Press.

Laudan, L. (1981). Why was the logic of discovery abandoned? In *Science and Hypothesis*, pages 181–191. Dordrecht: Reidel.

Lenoir, T. (1993). The discipline of nature and the nature of discipline. In E. Messer-Davidow, D. R. Shumway, and D. J. Sylvan, editors, *Knowledges: historical and critical studies in disciplinarity*, pages 70–102. University of Virginia Press.

Lenton, T., Held, H., Kriegler, E., Hall, J., Lucht, W., Rahmstorf, S., and Schellnhuber, H. (2008). Tipping elements in the earth's climate system. *PNAS*, **105**, 1786–1793.

Leontif, W. (1948). Note on the pluralistic nature of history and the problem of interdisciplinary cooperation. *Journal of Philosophy*, **45**, 617–624.

Love, A. C. (2008). Explaning evolutionary innovations and novelities. *Philosophy of Science*, **75**, 874–886.

Martens, P., Roorda, N., and Cövers, R. (2010). The need for new paradigms. *Sustainability, Science and Higher Education*, **3**, 294–303.

Maslow, A. (1966). *The psychology of science: a reconaissance*. Maurice Bassett Publishing.

Maull, N. L. (1977). Unifying science without reduction. *Studies in History and Philosophy of Science*, **8**, 143–162.

Max-Neef, M. (2005). Foundations of transdisciplinarity. *Ecological Economics*, **53**, 5–16.

Meadows, D. H., Meadows, D. L., and Randers, J. (1972). *The Limits to Growth*. Universe Books.

Midgley, M. (1984a). Reductivism, fatalism and sociobiology. *Journal of Applied Philosophy*, **1**(1), 107–114.

Midgley, M. (1984b). Sociobiology. *Journal of Medical Ethics*, **10**, 158–160.

Mihelcic, J., Crittenden, J., Mitchell, J., Shonnard, D., Hokanson, D., Zhang, Q., Chen, H., Sorby, S., James, V., Sutherland, J., and Schnoor, J. (2003). Sustainability science and engineering: The emergence of a new metadiscipline. *Environmental Science & Technology*, **37**(23), 5314–5324.

Miller, T. R., Wiek, A., Sarewitz, D., Robinson, J., Olsson, L., Kriebel, D., and Loorbach, D. (2013). The future of sustainability science: a solutions-oriented research agenda. *Sustainability Science*, **9**(2), 239–246.

Mitchell, S., Daston, L., Gigerenzer, G., Sesardic, N., and Sloep, P. (1997). The how's and why's of interdisciplianrity. In P. Weingart, S. Mitchell, P. J. Richerson, and abine Maasen, editors, *Human By Nature: Between Biology and the Social Sciences*. Lawrence Erlbaum Associates Inc.

Mitchell, S. D. (2002). Integrative pluralism. *Biology and Philosophy*, **17**, 55–70.

Mitchell, S. D. (2003). *Biological Complexity and Integrative Pluralism*. Cambrige University Press.

Mitchell, S. D. (2009). *Unsimple Truths: Science, Complexity, and Policy*. University of Chigaco Press.

Mäki, U. (2009). Economics imperialism: Concept and constraints. *Philosophy of the social sciences*, **39**.

Mäki, U. (2013). Scientific imperialism: Difficulties in definition, identification, and assessment. *International Studies in the Philosophy of Science*, **27**, 327–341.

Mäki, U. and Marchionni, C. (2010). Is geographical economics imperializing economic geography? *Journal of Economic Geography*.

Nagel, E. (1961). *The Structure of Science*. Routledge.

Neurath, O. (1937). Unified science and its encyclopedia. *Philosophy of Science*, **4**, 265–277.

Neurath, O. (1941). Universal jargon. *Proceedings of the Aristotelian Society, New Series*, **41**, 127–148.

Neurath, O. (1983). Encyclopedia as 'model'. In R. Cohen and M. Neurath, editors, *Philosophical Papers 1913-1946*, volume 16 of *Vienna Circle Collection*, pages 145–158. Springer Netherlands'.

Nickles, T. (1980). Scientific problems: three empiricist models. In *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*.

Nickles, T. (1981). What is a problem that we may solve it? *Synthese*, **47**, 85–118.

Nickles, T. (1987). Methodology, heuristics, rationality. In J. C. Pitt and M. Pera, editors, *Rational Changes in Science: Essays* *on Scientific Reasoning*, pages 103–132. D. Reidel Publishing Company.

Nickles, T. (2000). Discovery. In W. Newton-Smith, editor, *A companion to the philosophy of science*, pages 85–96. Blackwell.

Nickles, T. (2006). Heuristic appraisal: the context of discovery or justification? In J. Schickore and F. Steinle, editors, *Revisiting discovery and justification*, pages 159–182. Springer.

Nicolescu, B. (2006). Transdisciplinarity – past, present and future. In *II Congresso Mundial de Transdisciplinaridade*.

Norton, B. (2005). *Sustainability. A Philosophy of Adaptive Ecosystem Management*. University of Chicago Press.

Nowotny, H., Scott, P., and Gibbons, M. (2001). *Re-thinking science: knowledge and the public in an age of uncertainty*. Polity Press.

Nowotny, H., Scott, P., and Gibbons, M. (2003). Introduction: 'mode 2' revisited: The new prodcution of knowledge. *Minerva*, **41**, 179–194.

Ostrom, E. (2007). A diagnostic approach for going beyond panaceas. *PNAS*, **104**, 15181–15187.

Pearce, D. and Atkinson, G. (1993). Capital theory and the measurement of sustainable development: an indicator of "weak" sustainability. *Ecological Economics*, **8**, 103–108.

Pezzey, J. C. (1997). Sustainability constraints versus "optimality" versus intertemporal concern, and axioms versus data. *Land Econoimcs*, **73**, 448–466.

Popper, K. (2010 [1972]). *Objective Knowledge*. Oxford University Press, revised edition.

Quental, N., Lourenço, J., and da Silva, F. (2011). Sustainability: characteristics and scientific roots. *Environment, Development and Sustainability*, **13**, 257–276.

Reitman, W. R. (1964). Heuristic decision procedures, open constraints, and the structure of ill-defined problems. In M. W. S. II and G. L. Bryan, editors, *Human Judgements and Optimality*. John Wiley and Sons.

Revelle, R. and Suess, H. E. (1957). Carbon dioxide exchange between atmosphere and ocean and the question of an increase of atmospheric co2 during the past decades. *Tellus*, **9**, 17–27.

Rittel, H. and Webber, M. (1973). Dilemmas in a general theory of planning. *Policy Sciences*, **4**, 155–169.

Rockstrom, J., Steffen, W. L., Noone, K., and Persson, Å. (2009). Planetary boundaries: exploring the safe operating space for humanity. *Ecology and Society*, **14**(2).

Sahlin, N.-E. and Persson, J. (1994). Epistemic risk: the significance of kknowi what one does not know. In B. Brehmer and N.-E. Sahlin, editors, *Future risks and risk management*, pages 37–62. Kluwer Academic Publishers: Dordrecht.

Sayer, A. (2000). *Realism and social science*. SAGE.

Schaffner, K. (1967). Approaches to reduction. *Philosophy of Science*, **34**, 137–147.

Schaffner, K. (1969). The Watson-Crick model and reductionism. *The British Journal for the Philosophy of Science*, **20**, 325–348.

Schmidt, J. C. (2008). Towards a philosophy of interdisciplinarity. *Poesis Prax*, **5**, 53–69.

Schoolman, E. D., Guest, J. S., Bush, K. F., and Bell, A. R. (2012). How interdisciplinary is sustainability research? analyzing the structure of an emerging scientific field. *Sustainability Science*, **7**, 67–80.

Searle, J. (1992). *The rediscovery of the mind*, chapter Reductionism and the irreducibility of conciousness, pages 111–126. The MIT Press.

Shapere, D. (1984). *Reason and the Searchfor Knowledge*. Reidel, Dordrecht.

Shearer, H. A. (1918). *Farm Mechanics: Machinery and its Use to Save Hand Labor on the Farm.* Fredrick J. Drake.

Sherif, M. and Sherif, C., editors (2009/1969). *Interdisciplinary Relationships in the Social Sciences*. Aldine Publishing Company.

Simon, H. (1973). The structure of ill structured problems. *Artificial Intelligence*, **4**, 181–201.

Sintonen, M. (1990). Basic and applied science–can the distinction (still) be drawn. *Science Studies*, **3**, 23–31.

Solow, R. (1974). Intergenerational equity and exhaustible resources. *The Review of Economic Studies*, **41**, 29–45.

Star, S. L. and Griesemer, J. R. (1989). Institutional ecology, ‘translations’ and boundary objects: amateurs and professionals in Berkeley’s Museaum of Vertebrate Zoology, 1907-39. *Social Studies of Science*, **19**, 387–420.

Stillman, C. S. (1955). Academic imperialism and its resolution: the case of economics and anthropology. *American Scientist*, **43**, 77–88.

Stolzer, J. M. (2007). The ADHD epidemic in America. *Ethical Human Psychology and Psychiatry*, **9**(2), 109–116.

Thorén, H. (2014). Resilience as a unifying concept. *International Studies in the Philosophy of Science*, **28**, 303–324.

Thorén, H. (2015a). History and philosophy of science as an interdisciplinary field of problem transfers. In S. Wagenknecht, N. Nersessian, and H. Andersen, editors, *Empirical Philosophy of Science: Introducing qualitative methods into the philosophy of science*, Studies in Applied Philosophy, Epistemology and Rational Ethics. Springer. (forthcoming).

Thorén, H. (2015b). Replacement and expansion in scientific imperialim. *Paper III*.

Thorén, H. and Breian, L. (2015). Stepping stone or stumbling block? mode 2 knowledge production in sustainability science. *Paper IV*.

Thorén, H. and Gerlee, P. (2010). Weak emergence and complexity. In H. Fellerman, M. Dörr, M. Hanczyc, L. L. Laursen, S. Maurer, P.-A. Monnard, K. Stoy, and S. Rasmussen, editors, *Artificial Life XII Proceedings of the Twelfth International Conference on the Synthesis and Simulation of Living Systems*, pages 879–886. MIT press.

Thorén, H. and Persson, J. (2011). Philosophy of interdisciplinarity: Problem feeding, conceptual drift, and methodological migration. In *3rd Biennial Conference of the Society for Philosophy of Science in Practice*.

Thorén, H. and Persson, J. (2013). The philosophy of interdisciplinarity: sustainability science and problem-feeding. *Journal for General Philosophy of Science*, **44**, 337–355.

Thorén, H. and Persson, J. (2015). Resilience: Some philosophical remarks on defining ostensively and stipulatively. *Paper V*.

Thornton, S. (2014). Karl popper. In E. N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. URL = <http://plato.stanford.edu/archives/sum2014/entries/popper/>, summer 2014 edition.

Toulmin, S. (1972). *Human Understanding*, volume I. Clarendon Press: Oxford.

Walker, J. and Cooper, M. (2011). Genealogies of resilience: from systems ecology to the political economy of crisis adaption. *Security Dialogue*, **42**, 143–160.

Weart, S. (2003). *The discovery of global warming*. Harvard University Press.

Weingart, P. (2003). Growth, differentiation, expansion and change of identity — the future of science. In B. Joerges and H. Nowotny, editors, *Social Studies of Science and Technology: Looking Back, Ahead*, volume 23 of *Sociology of the Sciences Yearbook*, pages 183–200. Kluwer Academic Publishers.

Weingart, P. (2010). A short history of knowledge formation. In R. Frodeman, J. T. Klein, and C. Mitcham, editors, *The Oxford handbook of Interdisciplinarity*, pages 3–14. Oxford University Press.

Westley, F., Carpenter, S. R., Brock, W. A., Holling, C., and Gunderson, L. H. (2002). Why systems of people and nature are not just social and ecological systems. In L. Gunderson and C. Holling, editors, *Panarchy: understanding transformations in human and natural systems*. Island Press.

Wilson, E. O. (1975). *Sociobiology: the new synthesis*. Harvard University Press.

Wimsatt, W. (1972). Complexity and organization. *Proceedings of the Biennial Meeting of the Philosophy of Science Association*, **1972**, 67–86.

Wimsatt, W. (2003). *Re-engineering philosophy for limited beings*. Harvard University Press.

World Comission on Environment and Development (WCED) (1987). *Our Common Future*. Oxford University Press.

Appendix: Papers I-VI

Paper I

J Gen Philos Sci (2013) 44:337–355
DOI 10.1007/s10838-013-9233-5

ARTICLE

The Philosophy of Interdisciplinarity: Sustainability Science and Problem-Feeding

Henrik Thorén · Johannes Persson

Published online: 17 November 2013

Abstract Traditionally, interdisciplinarity has been taken to require conceptual or theoretical integration. However, in the emerging field of sustainability science this kind of integration is often lacking. Indeed sometimes it is regarded as an obstacle to interdisciplinarity. Drawing on examples from sustainability science, we show that problem-feeding, i.e. the transfer of problems, is a common and fruitful-looking way of connecting disparate disciplines and establishing interdisciplinarity. We identify two species of problem-feeding: unilateral and bilateral. Which of these is at issue depends on whether solutions to the problem are fed back to the discipline in which the problem originated. We suggest that there is an interesting difference between the problem-feeding approach to interdisciplinarity and the traditional integrative perspective suggested by among others Erich Jantsch and his colleagues. The interdisciplinarity resulting from problem-feeding between researchers can be local and temporary and does not require collaboration between proximate disciplines. By contrast, to make good sense of traditional integrative interdisciplinarity we must arguably associate it with a longer-term, global form of close, interdisciplinary collaboration.

Keywords Interdisciplinarity · Problem-feeding · Jantsch · Sustainability science

H. Thorén (✉)
Department of Philosophy, Lund University, Lundagard, 222 22 Lund, Sweden
e-mail: henrik.thoren@fil.lu.se

J. Persson
Lund University Center of Excellence for Integration of Social and Natural Dimensions of Sustainability (LUCID), P.O. Box 170, 221 00 Lund, Sweden
e-mail: johannes.persson@fil.lu.se

1 Interdisciplinarity: A Traditional Perspective

One day you are in, and the next you are out. The term 'interdisciplinarity' passes in and out of fashion. However, the phenomenon itself is associated with fundamental issues in science and philosophy.

Philosophers of science who have studied interdisciplinarity, whether or not they deploy the term itself, often focus on relationships obtaining between proximate disciplines or fields. For instance, since the 1970s the life sciences have been a prime source of material, and numerous case studies have been made showing how our knowledge can grow, and how discoveries can be made, when fields such as biochemistry and cell biology interact.

However, interdisciplinary collaboration can involve less proximate disciplines as well. This is one reason why we want to look more closely, from a philosophy of science perspective, at a new kind of interdisciplinary case, namely sustainability science.

The other reason for focusing on sustainability science is this. It is not clear that traditional accounts of interdisciplinarity capture a certain type of interdisciplinary collaboration that we believe is characteristic of sustainability research: the transfer of problems (and sometimes solutions) from one discipline to the other. We call this type of interdisciplinarity 'problem-feeding'.

This article starts by presenting the traditional perspective and the difficulties it has handling the interdisciplinary field of sustainability science. Then we introduce the notion of problem-feeding and compare it with recent discussion of the philosophy of interdisciplinarity. We discuss two varieties of problem-feeding: unilateral and bilateral. Which of these is at issue depends on whether solutions to the problem are fed back to the discipline in which the problem originated.

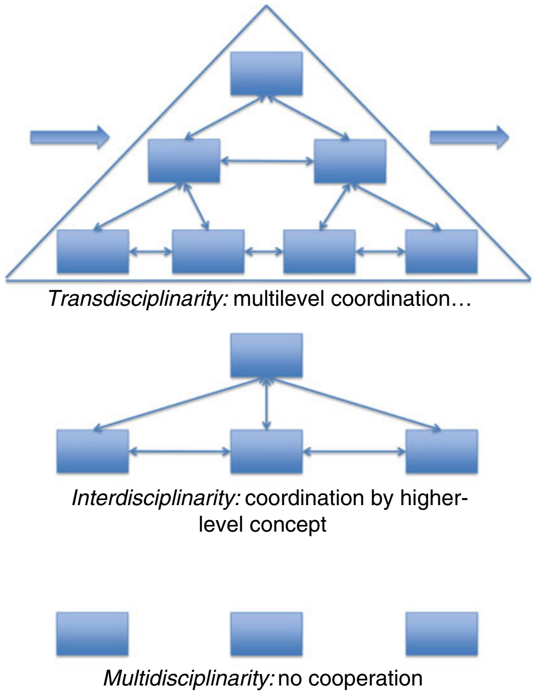
Since the early 1970s, and certainly since the publication of Jantsch's (1970/1972), the type and degree of conceptual or, broadly speaking, theoretical integration of the participating disciplines has been the primary basis on which to sort types of interdisciplinary encounter. This focus on conceptual or theoretical relationships defines what might be called the traditional perspective on interdisciplinarity.

Figure 1 illustrates the traditional view. Jantsch characterizes multidisciplinarity in terms of "no cooperation", interdisciplinarity in terms of "coordination by higher-level concept", and transdisciplinarity in terms of "multilevel coordination of entire education/innovation system". At other places he writes about multidisciplinarity as a situation with a variety of disciplines whose relationships have not been made explicit. The kinds of relationship he and others belonging to the traditional perspective are talking about, then, are conceptual or theoretical relationships.

Often multidisciplinarity is still understood as non-integrative, or additive, amounting merely to the juxtaposition of knowledge claims from different sources; correspondingly, interdisciplinarity is quite frequently thought to involve more 'internal' integration than multidisciplinary approaches do (Klein, 1990, 56–58).

An additional feature of Jantsch's approach is "the basic evolutionary ladder" (Jantsch 1970/1972, 15). Collaboration may start as multidisciplinary and end as transdisciplinary. The three categories we referred to in Fig. 1 are steps for climbing up the basic evolutionary ladder. Many advocates of the traditional perspective have a similar view. For instance, Jantsch observes that another influential writer on interdisciplinarity, Piaget, conceives of inter- and transdisciplinarity as the two highest steps "on a rigid ladder of levels" (ibid., 18). What happens in the step Jantsch calls interdisciplinary is especially important. With this step:

Fig. 1 The traditional perspective. *Source* Figure adapted from Jantsch (Jantsch 1970/1972, 15)



> A common axiomatics for a group of related disciplines is defined at the next higher hierarchical level, thereby introducing a sense of purpose. (Jantsch 1970/1972, 16)

Jantsch's view that the interdisciplinary step necessarily introduces an effective sense of purpose is somewhat less widely shared, and therefore it should not be included in the traditional perspective. The sense of purpose allows us to answer the question "interdisciplinarity to what end?" and influences the disciplines. In Jantsch's view, with the introduction of interdisciplinary relations the linked scientific disciplines change in respect of their concepts, structures, and aims.

Transdisciplinarity is then taken to involve massive coordination on the basis of a generalized axiomatics. Jantsch's idea is that several (two-level) interdisciplinary systems are linked, and that changes in overall system goals have an impact on concepts and principles throughout all of the linked, interdisciplinary scientific disciplines. If we bracket Jantsch's insistence on change being effected from above, his conception of transdisciplinarity is part of the traditional perspective. For instance, in Apostel et al. (1972, 25f) transdisciplinarity is defined as the ordering of disciplines under a common set of axioms.

Jantsch's idea that interdisciplinary collaboration evolves from multidisciplinarity to transdisciplinarity is clearly visible in contemporary thinking about interdisciplinary science, and in particular in sustainability science. Consider, for example, the terms in which a quite large Swedish research programme, Lund University Centre of Excellence for Integration of Social and Natural Dimensions of Sustainability (LUCID), explains how collaboration within the programme will develop over a period of 10 years (Fig. 2).

At its inception, in 2008, LUCID engaged researchers from seven disciplines and four faculties. Although the researchers were officially working on joint research problems, such as land-use change, the problems were approached from each individual's

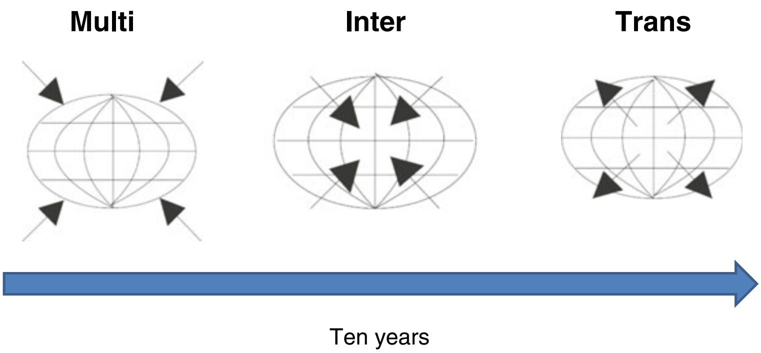


Fig. 2 Interdisciplinarity over time. *Source* Figure adapted from Jerneck et al. (2011, 79)

disciplinary perspective. A few years have passed since 2008, and according to the plan the current interdisciplinary phase will be "resulting in a co-evolution of theories for sustainability science"; finally, in the transdisciplinary phase, "theories evolve and mature to gradually incorporate more domains and transcend the boundary between science and practice" (Jerneck et al. 2011, 79). The conceptual, or theoretical, integration in the interdisciplinary phase is reminiscent of the picture painted by Jantsch. The assumption that the programme could not move forward at a faster pace probably accords with common sense. Integration by higher-level concepts and theories takes time. If such integration is needed for 'higher' levels of interdisciplinarity, such as transdisciplinarity, to develop, higher levels of interdisciplinary collaboration plausibly occur after less advanced ones.

We deploy sustainability science as a test case against which accounts of interdisciplinarity can be evaluated. It clearly merits consideration in its own right. We also think it can be used to assess perspectives such as Jantsch's. Sustainability science raises interdisciplinary issues of a kind Jantsch would surely have wanted to be able to account for: "In most general terms, the purpose of the university may be seen in the decisive role it plays in enhancing society's capability for continuous self-renewal" (Jantsch 1970/1972, 12). Given this, the field of sustainability science cannot be said to be unrepresentative in the traditional perspective.

2 The Need for an Alternative to the Traditional Perspective

Despite its somewhat unusual amplification of a sense of purpose as an essential component of interdisciplinarity, Jantsch's account might appeal to many philosophers of science. Its focus on integration by concept or theory is well-known from the unity of science movement, from Nagelian reductionism (Nagel 1961), and from other less reductionist accounts. We discuss some of the modern varieties of this view in Sect. 7 below. In this section we highlight two problems with the traditional perspective.

(2a) Pretty clearly, the traditional perspective is threatened by Jantsch's treatment of the basic evolutionary ladder. It begins to look like a rather ineffective tool for characterizing interdisciplinary collaborations of shorter duration. Sustainability projects aimed at practical problem-solving are frequently of this type. The traditional perspective becomes incapable of discriminating between shorter projects, evaluating them all as multidisciplinary endeavours involving no cooperation, when surely these collaborations might differ in ways that a philosophy of interdisciplinarity wants to be able to account for.

As a potential counterexample to Jantsch's and our view (that traditional interdisciplinarity takes plenty of time to establish), it should be noted that there is at least one popular account of transdisciplinarity in which Jantsch's conceptual and theoretical integration is assumed to take place in the absence of previous stages of interdisciplinary collaboration. Michael Gibbons and Helga Nowotny and their colleagues attracted considerable attention by claiming that modern science often involves transdisciplinary knowledge generation in "Mode 2" (Gibbons et al. 1994; Nowotny et al. 2001). The Mode 2 concept is designed to suit enquiries in which researchers from different disciplinary perspectives come together to work on problems "a context of application." The Mode 2 concept of transdisciplinarity deployed here is similar to Jantsch's concept of transdisciplinarity in many ways, but it does not contain his additional assumption that before (Mode 2) transdisciplinarity the research team first spends time in less integrated forms of interdisciplinary collaboration. Instead, a possibly unique theoretical framework is generated from scratch, partly as a result of negotiations in rather heterogeneous, local research groups.

Does the Mode 2 account encourage confidence in the idea that the traditional perspective on interdisciplinary research can be successfully applied to projects of shorter duration, and to more practically oriented projects as well? Not really. What it establishes is merely that there is no contradiction involved in assuming that interdisciplinary research can be integrated by a theoretical framework that has been designed and agreed upon in a local interdisciplinary context. But examples of Mode 2 projects in sustainability science that are both practically oriented and result in new theoretical frameworks are rare.

Whether or not one believes in the basic evolutionary ladder, this already suggests the need for a complementary account of interdisciplinarity—one that does not build on integration by concept or theory.

(2b) The case of sustainability science highlights an independent shortcoming, or potential shortcoming at any rate, of the traditional account of interdisciplinarity. Some sustainability scientists who are committed to the long-term project do not regard themselves as promoters of integration by concept or theory. For instance, responding to a questionnaire, a senior research scientist from a European centre for sustainable development research wrote to us:

> We don't use the label 'sustainability science', since we define ourselves primarily as focusing on themes, but from different disciplinary backgrounds. We are very cautious about defining our work and approach as being part of a new discipline, since this closes off doors that need to remain open for inter- and transdisciplinarity.

On our interpretation, the response conveys two things of importance. The first is that the background disciplines the research scientist is thinking of will not themselves be theoretically integrated as a result of the thematic work. As some areas of sustainability research illustrate, it is easy to locate contexts of interdisciplinary collaboration which are unlikely ever to involve integration "by higher-level concept"—contexts, moreover, in which time is not the issue. Certain disciplines are rather rigid, in the sense that they are unlikely to change conceptually or theoretically (Jantsch 1970/1972, 19), especially as a result of interdisciplinary collaboration. Many reasons for rigidity can be identified (Persson and Sahlin 2013, Ch. 10). One possibility is that a powerful disciplinary matrix is already in place (Kuhn 1969/1970) within one, or both, of the involved disciplines—effectively blocking theoretical integration of the pair. And in some cases the conceptual distance might simply be too big for such integration to be possible (e.g. see Edlund et al. 1986, 24). Thus, one might wonder whether the integration of natural and social dimensions of sustainability could ever mean that the natural and social sciences should be conceptually or theoretically integrated. This is the factual problem. Conceptual or theoretical integration of distant or rigid disciplines is unlikely.

In such cases are we then forced to conclude that we are at a stage of multidisciplinarity, i.e. in a context with no cooperation? Various things might happen even if the existing natural and social sciences do not change. For instance, new disciplines are sometimes born. This is the second, and the main, worry expressed in the quotation. The National Science Foundation has inherited the slogan *Today's interdisciplinarity, tomorrow's disciplinarity*. This is what the research scientist we have quoted wants to avoid. She sees a normative problem with this alternative. Sustainability research should not lead to a new discipline: a distinct theoretical and conceptual framework might weaken the interdisciplinary character of the field. But there is a factual problem with the idea as well. For if sustainability science emerges as a new discipline, will it integrate concepts and theoretical fragments from, say, political science and oceanography? Again, the conceptual distance between the two background theories might simply be too great to allow such integration.

As our case study, sustainability science, shows, a complementary perspective is required if we are to understand interdisciplinary collaboration in contexts where coordination by higher-level concepts does not take place. This paper aims to develop this perspective.

3 Two Examples from Sustainability Science

Sustainability science is a fairly young field. It was consolidated as an international science policy project in the preparations for the World Summit on Sustainable Development in Johannesburg in 2002 (Jerneck et al. 2011, 70). We want to present two examples with a bearing on sustainability issues—one contemporary, the other from a time well before the birth of sustainability science.

(3a) In sustainability science problems are frequently defined by natural sciences and then exported to social science. This is because sustainability challenges are often identified initially by the natural sciences and subsequently communicated to society as potential (future) events or states of the world that will cause societal problems that we have not met with before. In an influential paper in the *Proceedings of the National Academy of Sciences of the United States of America* (*PNAS*) Timothy Lenton et al. (2008) defined and identified a number of climatic tipping elements (and a few tipping points).

These tipping elements are components of the earth's climate system which, once pushed across a certain threshold, or tipping point, are likely to exhibit non-linear, disruptive change. The *PNAS* text lists 15 policy-relevant tipping elements, including Arctic summer sea-ice, the Greenland ice sheet, Atlantic thermohaline circulation, and the Indian summer monsoon.

The authors argue that these elements can be pushed by human interaction across a tipping point resulting in Arctic sea-ice loss, the melting of the Greenland ice sheet, Atlantic deep water formation, Indian monsoon chaotic multi-stability, and so on. Furthermore, every element contributes significantly to human welfare as we know it today.

In many cases the identification of these tipping points clearly falls within the domain of one or other of the natural science disciplines, or an aggregate of them. Lenton et al. (2008) focus exclusively on tipping elements that trigger a qualitative change taking place within an “ethical time horizon” (revolving around the idea that events or states should not be too distant in time to be politically significant). However, even with this commitment to societal issues the task of identifying such events or states does not require real interdisciplinary engagement. On the other hand, this is as far as natural science seems to get—initially—in approaching the societal dimension. For instance, the task of addressing the issues of mitigation or adaptation is first and foremost one for social science.

Other problems that Lenton and his colleagues consider are more specific and hinge on the applicability of their own notion of a tipping element. For instance, they are interested in the question whether tipping elements in the social-economic system can be identified.

(3b) Scientific investigation of the carbon cycle and its role in climate change has been of interest for a long time now. Before the 1950s research concentrated on the question whether changes in atmospheric CO_2 concentrations explained the occurrence of ice-ages. However, since then sustainability issues have often been in focus. Nowadays, mapping out the way carbon cycles through the earth system is a highly interdisciplinary affair involving physics, chemistry, ecology, biology and economics. Towards the end of the nineteenth century, however, the carbon cycle and the temperature effects of CO_2 were the concern of physicists. We select one of the most important breakthroughs in this line of research as our illustration.

CO_2 was known to have ‘greenhouse’ properties early on. Both Joseph Fourier, in the 1820s, and John Tyndall, in the 1860s, had been interested in the properties of the gas (see Weart 2003, 2007), and as early as 1896 Arrhenius produced a quantitative model of the impact of different atmospheric concentrations of CO_2 on the mean surface temperature of the earth. His results suggested that doubling the concentration of CO_2 would result in mean temperatures rising by approximately 5–6 °C.

If CO_2 concentrations had indeed varied to this extent, those variations would potentially have explained why ice ages occur. However, it was not known whether such variation was possible. Arrhenius’ model had not included the role of the oceans in CO_2 dynamics. At the time this role was not well understood. It was known that plain water dissolves CO_2, so there was a suspicion that the variability of atmospheric CO_2 concentration was comparatively low. It was thought possible that any increase would quickly be neutralised, as the oceans would absorb the surplus. Arrhenius himself notes this possibility in the 1896 paper in which his model is formulated. His suspicion was widely retained for the next five decades.

In 1957 Craig (1957, 2) noted that the range of “[r]ecent estimates of the residence time of a molecule of carbon dioxide in the atmosphere, before entering the sea, range from 16 h (Dingle 1954) to the order of 1,000 years” (Plass 1956) (see also Weart 1997). In other words, in 1957 it was not known if the oceans’ absorptive properties would cancel out increases in CO_2 concentrations within a timeframe ensuring that CO_2 was not a cause of climate change.

Against this background a central problem became that of understanding how CO_2 cycles through the atmosphere and the oceans. Improved knowledge of the absorptive properties of the oceans—especially—would shed light on the mechanisms governing the variability of CO_2 concentrations in the atmosphere. An understanding of this would not only be essential to the solution of the old problem of the occurrence of ice ages. It would help to explain the potential dangers of emitting large quantities of fossil carbon into the atmosphere.

The first, and perhaps most vital, step towards a solution to this problem was provided by two researchers at the Scripps Institution of Oceanography in La Jolla, California. One of them, the oceanographer Roger Revelle, was the head of the institute. The other, Hans

Süess, was a chemist and specialist in carbon dating. In 1957 they published a joint paper in which they calculate the average time a CO_2 molecule spends in the atmosphere. Towards the end of the paper they note that they have used a simplified model of the ocean. They argue that this simplification is likely to affect their results as a consequence of two interconnected mechanisms. One concerns the chemical properties of the oceans; the other has to do with their mechanical properties. First, oceans act as so-called buffer solutions. They will resist changes in pH such as those induced by the absorption of CO_2. In the surface water two equilibrium reactions realise this mechanism. One is between aqueous carbon dioxide and carbonic acid (H_2CO_3); the other is between carbonate ions ($CO_3{}^{2-}$) and bicarbonate ions ($HCO_3{}^{-}$). As CO_2 is dissolved in oceanic water these equilibrium reactions are affected. The result is that even if most of the CO_2 is initially dissolved, it will be evaporated right back into the atmosphere. An increase in the CO_2 content of the atmosphere of 10 % is balanced by as little as a 1 % in the total CO_2 content of the ocean (Bolin and Eriksson 1959, 131). This brings us to the second mechanism. Revelle and Süess (1957) treat the ocean as "one well-mixed reservoir of CO_2". This assumption is problematic, they note, as the oceans are layered. While oceanic water mixes quite quickly in the horizontal dimension within layers, it mixes excruciatingly slowly between them.

Our current understanding is that the vertical mixing of layers is a process that takes place over millennia. The implication is that oceans, in the short and medium term, do not act to dissolve CO_2 in any important way. Hence the oceans do not balance CO_2 concentrations in the atmosphere over relevant timeframes, nor do they preclude CO_2 from playing a role in climate change.

One way to interpret this piece of history is as follows. The task here was to provide an account of how the oceans function with respect to CO_2. To provide such an account two mechanisms on different scales needed to be coupled. One of these mechanisms belonged to the domain of oceanography, and the other belonged to chemistry. The role of the oceans could not be modelled until knowledge from oceanography and chemistry had been integrated.

Now, both cases we have presented involve the exporting of problems, but in the second case that export is only part of the story. What is equally, or more, important in the CO_2 example is the importing of solutions. This process of involving more and more disciplines in the importing of solutions has continued. Notably, biological material provides a gigantic reservoir of carbon offering opportunities for a range of life science disciplines to contribute to our understanding of the CO_2 cycle.

4 Interdisciplinary Types, and Kinds of Interdisciplinary Relata

We began by presenting the traditional perspective. The interdisciplinary stories we have just told about sustainability science would perhaps have been judged as narratives in which there is no cooperation of the Jantschian sort. In the first case, (3a), real interdisciplinary engagement is not even required. In the second case, (3b), there is clearly cooperation, but it is cooperation without coordination by new higher-level concepts. However, in the traditional perspective as we have presented it coordination by higher-level concept is crucial, and therefore neither of our two sustainability examples qualifies as a case of interdisciplinarity as that has traditionally been conceived. At the same time both examples are clearly interdisciplinary *in another sense*, and a satisfactory account of interdisciplinarity should recognize this.

Before we introduce our own suggestion as to how to understand interdisciplinarity in (3a) and (3b) we want briefly to take note of the fact that a conception of interdisciplinarity more pluralistic than one focusing exclusively on coordination by concept or theory is implicit in many traditional accounts. For instance, Apostel et al. (1972, 25) speak of (advanced forms of) interdisciplinarity as activities involving the integration of concepts, methodology, procedures, epistemology, terminology and data. So the traditional perspective recognizes additional kinds of interdisciplinary relata.

It is possible, of course, to be more relaxed than Jantsch about what type of constituent is the more fundamental for climbing the basic evolutionary ladder. *Facilitating Interdisciplinary Research* characterizes interdisciplinary research in a much more relaxed way as a mode of research that:

> integrates information, data, techniques, tools, perspectives, concepts, and/or theories from two or more disciplines or bodies of specialized knowledge to advance fundamental understanding or to solve problems whose solutions are beyond the scope of a single discipline or area of research practice. (National Academy of Sciences et al. 2005, 39)

An alternative approach to the traditional one would be to bring order to the often muddled picture we have of interdisciplinarity by sorting interdisciplinary projects, or aims, by the types of constituent that interact in encounters between researchers of the disciplines involved (Thorén and Persson 2011). The types of constituent stated in Apostel et al. (1972) and (National Academy of Sciences et al. 2005) could indeed provide the starting point for such an endeavour. However, before we begin sorting interdisciplinary kinds by the types of constituent suggested in lists such as those above, we might ponder whether they are exhaustive, or whether any salient types of constituent are missing.

5 Transfer of Problems

Our conception of interdisciplinary research in this article is pluralistic, but it is not identical with the conceptions we touched upon in the last section. For one thing, we invoke a type of constituent not mentioned in the passage excerpted above. We also make use of a relation other than integration. We do not challenge the idea that the traditional perspective is sometimes applicable. We simply deny that it always is.

Let us look back at the two examples involving sustainability issues—the tipping-points and the CO_2 cycle. What we claim to be one important feature these two cases have in common is that problems in them are moved from one discipline to another. Lenton et al. (2008) can be taken as typical of a development within sustainability science that has been going on for a decade or more. Many of the initial results concerning the impacts of climate change have been delivered by various natural science disciplines. New problems have been created by these results, many of which do not appear to be natural science problems at all. Some, of course, are quite simply practical problems for the political and social institutions involved, but many are scientific problems. The impacts of climate change can only partly be understood from the perspective of climate science. Sea-level rise, or drastic changes in the weather patterns, will alter the conditions under which our social structures have been formed. This will obviously affect those structures, but how? What will happen to people living in areas that slowly become less habitable? Arrhenius was interested in the impact of climate change on human societies. He thought a warmer climate would result in better agriculture and a more pleasant and easier life, especially for poor northerners.

Lenton and colleagues wonder about how to prevent us from passing the tipping points. But these questions and problems are transferred to sciences better capable of researching them. The problem-feeding character of interdisciplinarity might be especially clear in sustainability science, with its focus on how *new* social problems emerge *as a result of changes in the natural world*. The examples from Lenton and his colleagues are of this type.

5.1 Unilateral Problem-Feeding

Problem-feeding in sustainability science is sometimes unilateral. It is problem-feeding, as it were, without solution-feeding. In general, one field (or discipline) may rely on another as a source of problems; these problems may or may not be problems *for* the discipline in which the problem originally emerged. To shift to an almost trivial example, consider the relationship between philosophy of science and science. Clearly the philosophy of, say, physics, relies on physics (the discipline) for some of its problems. What happens in physics is part of the subject matter of philosophy of physics. However, the problems may not be problems *for* physicists, and their resolution may have negligible impact on the way physicists conduct their research. We do not mean to take any particular stance on this question. We simply highlight the fact that if one discipline relies on another for its problems—in full, or in part—this does not necessarily mean that the discipline from which problems are generated is affected by this transfer.

A similar point has been made by Grantham (2004, 143), who argues in favour of the importance of heuristic dependence, i.e. the notion that “theories and/or methods of a field can guide the generation of new hypotheses in a neighbouring field”. Grantham does not highlight the potentially asymmetric nature of the connection. He is interested in another issue, namely that unification between disciplinary fields increases with the number of ways in which two fields are connected. He argues that heuristic dependence is one way in which unification increases. On his conception, then, unilateral problem-feeding should also increase unification, and thus potentially establish interdisciplinarity.

We think that the potentially unilateral character of problem-feeding is of special importance in sustainability science. Here we can reconsider Lenton and the tipping-points. Identifying tipping-points falls within the boundaries of Lenton’s competence and academic interest. But identifying effective measures of adaptation or mitigation does not. In that sense, Lenton’s work can be conceived as a case of problem-feeding without solution-feeding. The example shows that unilateral problem-feeding can be established on the basis merely of rather weak relationships already in place between the disciplines or fields.

5.2 Bilateral Problem-Feeding

The CO_2 example contains more bilateral problem-feeding as well: problem-feeding with solution-feeding. In the case of CO_2 it was known in Arrhenius’ time that the absorptive properties of the oceans were crucial to an understanding the CO_2 cycle. The problem was that of providing a detailed account of these properties. Revelle and Süess advanced matters here by suggesting two mechanisms through which the oceans process CO_2. It is interesting to note that the knowledge itself—especially that pertaining to the chemical properties of the ocean—was not new. Weart (2007, 7; see also his 2003) points out that the relevant reactions had been understood since the 1930s. Revelle himself was in an ideal position to tackle this issue, since he had considerable knowledge of, and experience of research into, oceanic chemistry and the processes by which oceanic water mixes (Weart 2007, 3f; see also his 2003).

Very often, bilateral problem-feeding is about a cognitive division of labour. One discipline or field takes on the issues of another, so that, ideally, the resolution of those issues can be transmitted back. Darden and Maull's (1977) idea of an 'interfield theory' is relevant here. An interfield theory relates one discipline or field to another by postulating, or establishing, an ontological connection between the domains of the respective disciplines or fields. Darden and Maull give four examples of ways in which disciplines or fields can be connected: one discipline may provide the physical location, the physical nature, or the structure of an entity (or process) postulated in another; or disciplines may be causally linked (see Darden and Maull 1977, 49). These four kinds of connection are not mutually exclusive.[1]

Interfield theories order fields ontologically, allowing for them to map onto each other. For instance, cytology and transmission genetics are interconnected through the chromosome theory of Mendelian heredity. In genetics the *gene* was a postulated entity corresponding to some phenotype that was inherited between generations (all-or-nothing). Cytologists provided the physical location of the gene on the chromosomes and were thus able to resolve certain issues within genetics. Thus the statistical deviation that had been noticed could be explained by the fact that proximal genes were more likely to be inherited together. Assortment is not perfectly random.

It is possible to recast Darden and Maull's case as one of bilateral problem-feeding. Transmission geneticists were in no position to investigate the physical location of the gene, as such a task lay entirely outside the scope of the methods available to them. But does this imply that the phenomenon of bilateral problem-feeding is already accounted for in the literature? No, and below we argue that it is not.

Are interfield theories necessary or sufficient, or both, for bilateral problem-feeding to ensue? Let us start with sufficiency. Suppose discipline A encounters problem P, whereupon P is fed to discipline B, solved through solution S, and S is then fed back into A. For this to happen it seems to us that S needs to be *admissible* in A. Two ways in which this could be achieved come to mind. S might be, as it were, inherently admissible in A. For instance, the process by which S was produced in B could be considered to be reliable, or adequate, by standards generally applied in A. Alternatively, the relevant researchers in A trust the ones in B regardless of what the process in B looks like. In neither case does the presence of an interfield theory by itself suffice. In the first, the disciplinary fields are proximate in the sense that they share epistemic standards and values beyond the interfield theory that allow them to evaluate each other's methods, tools and procedures. In the second, the interfield theory needs to be supplemented by some kind of agreement involving mutual trust. The boundaries may be ever so clear, but if the members of discipline A simply do not trust members of discipline B to produce reliable results it will

[1] In the present paper the focus is on interdisciplinarity and hence disciplines are centre stage. Darden and Maull discuss *fields*. A field on, their conception is constituted by "a central problem, a domain consisting of items taken to be facts related to that problem, general explanatory factors and goals providing expectations as to how the problem is to be solved, techniques and methods, and, sometimes, but not always, concepts, laws and theories which are related to the problem and which attempt to realize the explanatory goals. A special vocabulary is often associated with the characteristic elements of a field" (Darden and Maull 1977, 44). Following Darden and Maull's own approach we will not make any particular distinction between fields and disciplines; we will generally take them to be coextensive; at the very least we will assume that a disciplinary boundary is *also* a field boundary (though perhaps not vice versa). Clearly, some would disagree with this: Grantham (2004) and Bechtel (1986) for instance.

not help. Sustainability science is plausibly a case in point. Although various interfield theories linking social and economic phenomena to various natural phenomena (climate, ecology, etc.) are at least partly in place, mutual distrust between the disciplines sometimes hampers collaborations. The presence of an interfield theory is generally not sufficient for bilateral problem-feeding to take place.

Is an interfield theory necessary for problem-feeding to take place? When it comes to necessity the situation is somewhat different. This is largely because it is more difficult to pinpoint Darden and Maull's exact position on this matter. In order for discipline *B* to be relevant to discipline *A*, and vice versa, *some* relation needs to be present. This is quite simply what it means to be relevant. But interfield theories are not just *some* relation. In fact, they are not even just some ontological relation. Interfield theories link the domains of disciplines in highly specific ways. We can see two ways in which interdisciplinary linking can happen that do not seem to fit the interfield theory model. First, disciplines can be linked and exchange problems in ways other than by linking their respective domains. For example, a discipline may encounter a theoretical or conceptual problem that is best thought of as one concerning its methods, theories, concepts, and so on. Suppose this problem is within the domain of that discipline, and suppose further that its solution benefits the discipline of origin. Then we shall be linking the domain of one discipline with some other aspect of another discipline. That does not seem to qualify as an interfield theory, but clearly it could be a case of bilateral problem-feeding.

Second, disciplines can be linked more loosely—i.e. in a manner less explicit or specific than happens in the case of interfield theories. Mitchell's (2002, 2003, 2009) integrative pluralism comes to mind. Mitchell discusses composite explanations of complex phenomena, mainly within the life sciences. It is not unusual for explanations that seem to be competing (because they are mistakenly conceived of as full explanations of an explanandum) to in fact be compatible and complementary (since they add different components of the total cause). This might happen if they are integrated only at a certain level—the level of concrete particulars. At their most general level, the theories cannot be integrated to provide an account of the phenomenon in question because it is a phenomenon that has many different causes and the causal profile of specific instances may vary a lot. Here the ontological connection between the two disciplines is merely that the same phenomenon is discussed, and that (different) causes of its occurrence are investigated. This is not the kind of situation Darden and Maull have in mind, and it fails to qualify as an interfield theory. However, the upshot of the encounter can be successful bilateral problem-feeding. It is easy to conceive of situations in which partial explanations are contributed from various sources as cases of problem-feeding. It seems, especially given the first point above, that interfield theories are neither necessary nor sufficient for problem-feeding to obtain. However, this does not prevent them from being crucially important where they do occur.

6 Barriers to Problem-Feeding, and Concept Coordination

At this point it should be added that the transference of problems between disciplines has its obstacles.

First, there might be language barriers. The discipline of origin may formulate a problem in such a way that it cannot be understood by those in the target discipline. This might be especially visible in attempted, but failed, bilateral problem-feeding. For instance, lawyers have to decide in matters where scientific expertise is *prima facie* relevant. It is not uncommon for scientific experts to be consulted when, for instance, environmental risk management issues are being scrutinized—or when the causes of, and responsibility for, a human injury are being assessed. However, attempts at bilateral problem-feeding from law to science are not always successful in facilitating the lawyer's decision. Wahlberg (2010) reports the following expert's experience:

> All right, you ask us physicians, 'What do you think?', and we write that in this particular case there are pathological changes; the changes are of such a kind that we do not regard the injury as a consequence of an accident. But the courts have during recent years always ruled in favour of the patient. I don't mind that. But then why ask us?

One reason successful bilateral problem-feeding from law to science is so difficult, Wahlberg claims, is that understandings of causation in the two fields differ. The causal concepts—or rather the ontologies corresponding to '$\text{cause}_{\text{law}}$' and '$\text{cause}_{\text{science}}$'—are only superficially the same. For this reason it would arguably be inaccurate to say that in this example just one obstacle, a language barrier, is the problem.[2] But the deployment of the term 'cause' in both fields is liable to give rise to misconceptions about the opportunities for bilateral problem-feeding. A transfer or coordination of causal concepts, or of the way in which we deploy causal terms, would facilitate this bilateral form of problem-feeding substantially.

Similarly Maull (1977) once emphasised that there needs to be some sharing of terminology if 'problem shifts' between disciplines, or disciplinary branches, are to take place. Problem shifts exemplify a special kind of bilateral problem-feeding. We have already touched on the example Maull deploys as an illustration: through a series of scientific developments, beginning around 1910, the problem of understanding the physical basis of heritable alterations shifted from genetics to biochemistry, where it was famously solved in the 1950s. Maull (1977) claims that this problem shift was associated with a transformation of the vocabulary in which the problem was formulated ensuring that this vocabulary was 'shared' by the two disciplines:

> Such a 'shared' vocabulary, it turns out, can be used to identify a very special sort of problem, a problem that, although it arises within one branch of inquiry, can only be solved with the aid of another science. (Maull 1977, 144)

Sometimes, Maull claims, vocabulary is transformed in order to make problem shifts possible. 'Mutation' was first a proper term in genetics; it then referred to heritable alteration of the genotype; but later it came to stand for heritable alteration in the base sequence of DNA. This last change gave biochemists access to the expression. At the same time problems raised by mutation became difficult to solve within genetics. Though problems concerning the physical nature of the determinants of heredity arose within genetics, genetics could not solve them. One reason was methodological. Genetics deploys statistical methods and crossbreeding in order to establish regularities in heritable characteristics. However, with the transformation of the term 'mutation', the problem could shift across to biochemistry—where a solution was forthcoming. At other times, vocabulary is simply imported. However, regardless of whether vocabulary is shared because of a process of transformation (as in the mutation example) or because it has been imported, the lack of shared vocabulary is often an obstacle to the transfer of problems.

[2] Thus there are *ontological* barriers, too. At the end of Sect. 8 we refer to a *methodological* barrier as well. Both these types of barrier can be even more important than terminological barriers.

At least, this holds for explicit problem-feeding. Beyond this there are more implicit—perhaps even tacit—varieties of problem-feeding as well. Although these may not offer such fertile ground for philosophical discussion, they are perhaps more important than the explicit kinds of problem-feeding from a scientific perspective. Hacking (1996, 70) quotes a charming metaphor of James Clerk Maxwell's which speaks of the "cross-fertilization of the sciences". According to Maxwell, researchers are like honeybees, never thinking about the importance of "the dust which they are carrying from flower to flower". Some problems are perhaps never formulated in the discipline of origin, but they are nevertheless tacitly transferred to a target discipline.

Second, problems of the sort with which we are concerned can be more or less well understood (Nickles 1981; Laudan 1977). This can arise in different ways, but one issue is that scientists working in the discipline of origin sometimes have only a crude understanding of what is really constitutive of the problem they are successfully transferring (or unsuccessfully trying to transfer) to another discipline. A problem is encountered in discipline A and identified as 'belonging' to some other discipline, B. This is the reason for attempting problem-feeding. However, those working in discipline A may have unrealistic, or plainly mistaken, ideas about the solutions that can be expected in B. In sustainability science problems are frequently identified by natural scientists as belonging to a particular social science. The findings of Lenton and colleagues generate many potentially relevant problems that can be (unilaterally) transferred to the social sciences. Some of these are easier to understand from the point of view of Lenton et al. than others. Thus one might conjecture that the following two problems are rather easy to understand: What policies should be implemented to prevent us from reaching a tipping point? What are the obstacles to implementation of such policies? However, towards the end of their paper Lenton et al. call for the identification of tipping elements within the socioeconomic system. This is a much more complicated request. The attempted problem-feeding assumes that the 'socioeconomic system' can be described within the formal apparatus that produces tipping elements. It is in order to ask whether they really understand the problem that they try to transfer it.

7 Problem-Feeding, Methodological Complementarity, and Practical Unification

The idea of problem-feeding can usefully be connected with what Grantham (2004) calls practical unification. By contrast, many philosophical accounts of integration and unification within the sciences are founded on the idea of theoretical unification (or, as we saw in the beginning, on the idea of coordination by a higher-level concept). Even though Darden and Maull (1977) define a disciplinary field as something that only sometimes includes theories, but always includes a number of other entities—central problems, tools, methods, etc.—the actual unification they describe flows from theories about the ontologies of the disciplinary fields in question. This is why there is reason to think of their account as a modern variety of the traditional perspective on interdisciplinarity outlined in the beginning of this paper. As Grantham points out, Kincaid (1990) also emphasises theoretical interconnections in his account of unification. Kincaid's account is based on the idea that various theories can relate to each other in a number of ways, and on the corresponding idea that unification is—as it cannot be in the reductionist conception—a matter of degree. The greater the number of relations present, the more unified are the theories in question. Unity peaks when two theories are incorporated in an integrated interlevel theory—that is, a theory that unifies

> …two disparate theories by employing explanations, confirmational procedures, etc. invoking both levels, and by providing evidence that the events and entities of one theory depend upon and are constituted from those of the other. (Kincaid 1990, 590)

Let us follow Grantham in referring to the interdisciplinary consequences of these sorts of relation as theoretical unification, and let us distinguish this type of unification from practical unification. Grantham sorts methodological interactions under the latter heading, and argues that they play a more substantial role than is acknowledged in some of the alleged examples of theoretical unification deployed by Kincaid (1990). Similarly, if we return to Darden and Maull it seems true in their example, too, that practical unification is of some importance. It was in virtue of the methods and tools at their disposal that cytologists were able to conduct inquiries that were quite beyond the scope of any statistical analysis of data on the occurrence of phenotypes across generations. The interconnection between disciplines via methods appears to be important in both these cases, and methodological complementarity is—partly, at least—what is motivating the interdisciplinary encounters in them. Grantham argues that theoretical and practical unification are to some extent independent. There can be considerable theoretical unification without coordination of research practices, and there can be considerable practical unification with only a low degree of theoretical unification (Grantham 2004, 15).

Moreover, methodological complementarity (rather than theoretical integration) seems to be a necessary condition of bilateral problem-feeding—which suggests that problem-feeding entails practical unification.

The association of problem-feeding with practical rather than theoretical unification fits well with the picture we have been promoting in this paper. First, the theoretical interconnections that must be in place in order for problem-feeding to ensue can be comparatively weak. *Some* connection needs to be in place, but nothing as substantive as, say, an interfield theory needs to exist. Second, and trivially, partial independence implies that theoretical unification will not necessarily emerge with the advent of problem-feeding. There may be interdisciplinarity as problem-feeding without traditional interdisciplinarity.

In connection with sustainability science such results may be informative for the kind of unity that is required. Many sustainability scientists have identified integration and unification as a core problem of sustainability science. How can we integrate the models of social and natural sciences to form one, presumably cohesive, set? Maybe this ambition overshoots the target. Given the practical nature of sustainability issues—after all the research project bottoms out in tackling, or rather helping to tackle, problems the solutions to which will always be judged by actual outcomes—it is perhaps perfectly in order to see problem-solving, as opposed to other epistemic ideals, as the overriding aim. In this approach theoretical and conceptual unification should primarily be sought only to the extent that it is instrumental in solving these problems.

8 Problem-Feeding as a Driver of Concept and Method Coordination

We have already touched on Grantham's view that unification increases with a rise in the number (or variety) of ways in which two fields are connected. He also claims that unification is enhanced by an increase in the significance of the connections that are already in place. A connection becomes more significant if it begins to transform the neighbouring field. That is, if the introduction of a novel concept, generalization, technique, or heuristic leads to considerable change in the absorbing field, then the change is regarded as significant and unification is advanced (Grantham 2004, 144).

We have just claimed that there may be interdisciplinarity as problem-feeding without traditional interdisciplinarity. But an argument in favour of the idea that problem-feeding is especially important for interdisciplinarity should also provide reasons for thinking that problem-feeding is significant in the sense that it might begin to transform the neighbouring field.

We have claimed that sustainability science is an interdisciplinary field, and that the disciplines involved in the exchange of problems are located either side of the natural/social science divide. Sustainability science differs in many respects from the cases that have caught the attention of the majority of philosophers of interdisciplinarity. Most philosophical treatments concentrate on disciplinary fields that are in many respects proximate. They share much at the outset, and this makes the sharing and shifting of problems a lot smoother. Within sustainability science this is decidedly not the case—at least, when it comes to integrating the natural and social dimensions of sustainability. However, and this is our next point, this does not undermine the recognition that problems *need* to be transferred.

Our suspicion is that the need to solve problems by first feeding them to another field is sometimes itself the fundamental reason why other kinds of bridge between distinct disciplinary fields are created. We have already seen how this may generate changes in, or transfers of, vocabulary in the example given by Maull (1977). She shows how the need to problem-feed might result in shared vocabulary. Problem-feeding sometimes stimulates concept coordination. A degree of conceptual transfer from *A* to *B*, or coordination between them, normally results in the possibility that problems in *B* can be fed into and (sometimes) solved in *A*. When this happens it would be misconceived to consider problem-feeding as the outcome of other forms of integration. Quite the contrary, it is the starting point. It begins to transform the neighbouring field.

We would like to conclude this sketch of the ways in which problem-feeding can drive other kinds of transfer or coordination by saying a few words about method transfer and method coordination. Method transfer can occur in several ways. Methods can be migrated domain-only-wise where a method already known within one discipline is deployed to extract information from a domain to which it has not previously been applied. From within the ranks of ecology there has been a degree of optimism about the power of the methods used there. This transfer does not have to result in actual interdisciplinarity, since the other field operating in the domain need not be influenced by it. However, disciplinary method transfer can result in the migrating method out-competing the methods that were previously in play. Two observations by Ronald Coase (see also Mäki 2009) are interesting in this context:

> [I]n the long run it is the subject matter, the kind of question which the practitioners are trying to answer, which tends to be the dominant factor producing the cohesive force that makes a group of scholars a recognizable profession… However, in the short run, the ability of a particular group in handling certain techniques of analysis, or an approach, may give them such advantages that they are able to move successfully into another field or even to dominate it. (Coase 1978, 204)

More often, perhaps, method transfer generates situations in which methods are somehow used in concert. The transfer results in methodological pluralism. 'Mixed methods' research is one possible result. But the label 'methodological pluralism' collects several distinct varieties of the type. Some problem-feeding events are such that several distinct methods are used more or less in sequence to solve various linked problems. At least one non-sequential variety is similar: certain phenomena have multiple causes whose investigation is carried out using different methods. A third kind of methodological pluralism that might result from method transfer is illustrated by the use of multiple methods to obtain results that are robust. In this case methodological pluralism involves the use of a set of disparate methods to achieve an epistemic end: for example, solving a particular problem complex (or chain), or checking results for robustness.

Disciplinary method transfer or coordination consorts well with our account of interdisciplinarity, especially since cases of such transfer or coordination do not always constitute interdisciplinarity (unlike cases of transfer or coordination motivated by problem-feeding concerns, which do). Simply sharing a certain method is not always sufficient for interdisciplinarity, even if the method has migrated from the one field to another. For instance, statistical analysis is widespread in both the natural and the social sciences. However, this does not seem to warrant talk of interdisciplinarity. Why is this? Is it because the migrating method is insufficiently anchored in the field from which it is being transferred? This would be alarming news to those involved in the many unificationist programmes being prosecuted today. Philosophers trying to facilitate the unification of *A* and *B* would risk finding that the very fact that unification was provided by methods (or concepts, or problems, or some such) suggested by a third party prevents interdisciplinarity between *A* and *B*. The risk should not be exaggerated, of course; further developments between *A* and *B* would stand a better chance of being truly interdisciplinary.

In our opinion the most interesting interdisciplinary cases of method transfer typically centre on problem-solving processes. In an early contribution to the modern literature on interdisciplinarity, Sherif and Sherif (1969) discuss a case that helps to bring this out:

> Yet when Dr William Schottstaedt and his colleagues studied the detailed biochemical and physiological records of patients in a metabolic ward, they found that variations in these strictly physiological measures of metabolism were significantly related to the vicissitudes of interpersonal relationships among patients, with nurses and doctors, and with visitors. (Sherif and Sherif 1969, 6)

A little further on, the authors continue:

> If we would start to inquire further why these particular patients were hospitalized for metabolic disorders, and not others, we would find ourselves immediately in problems requiring demographic study of different populations and in problems requiring institutional analysis of admittance procedures and financing; and these might very well lead us to problems of the political and economic systems in which the institutions functions (ibid.).

In pursuing the problems that arise from this observation Dr Schottstaedt and his colleagues would have ventured beyond their immediate expertise. This could happen in at least two ways. One is simply to import the methods and approaches necessary to address the questions one might have. As Sherif and Sherif seem to imply, however, this might be quite untenable in the long run. *Prima facie* it would be more rational to engage in problem-feeding and export the problem to a discipline where the methods and tools are readily available. Problem-feeding does not always result in method transfer. Sometimes we can either export the problem or import the know-how (the method, in this case). Doing one of the two is enough. What problem-feeding does require, minimally, is that some methodological coordination takes place. (To the extent that this has not been settled a problematic methodological barrier is in place.)

9 Conclusion: The Basic Evolutionary Ladder Revisited

We have argued that problem-feeding is a salient type of interdisciplinary collaboration in sustainability science. We have also argued that it cannot be accounted for by the traditional perspective, since it does not necessarily involve coordination by higher level concept. We have placed problem-feeding in context of recent discussion, particularly the work of authors like Grantham, Kincaid, Mitchell, Darden, and Maull.

The interdisciplinarity resulting from problem-feeding between researchers can be local and temporary and does not require collaboration between proximate disciplines. By contrast, to make good sense of traditional integrative interdisciplinarity we must arguably associate it with a longer-term, global form of close, interdisciplinary collaboration.

Furthermore, whether or not one believes in the interdisciplinary evolutionary ladder (Jantsch 1970/1972, 15) it seems to us that a basic step can consist of problem-feeding. This can occur where interdisciplinarity starts to evolve. The step is not a prerequisite of reaching further steps of the traditional ladder, such as coordination by "higher-order concept" or method. Such coordination may be secured by an alternative route, for different reasons. In this sense we are pluralists. But it is clear that a preference for bilateral problem-feeding—especially—may trigger the coordination of concepts and methods. Problem-feeding already introduces the *sense of purpose* the traditional perspective locates at more advanced steps of the interdisciplinary ladder. Jantsch, for one, thought that a sense of purpose was introduced when a common axiomatics for a group of related disciplines was defined at the next higher hierarchical level (Jantsch 1970/1972, 16).

It appears to us that the very fact that the sense of purpose is introduced so early on in interdisciplinarity as problem-feeding yields more opportunities for local interdisciplinarity, i.e. interdisciplinary encounters motivated by a local problem. Perhaps several cases that have been thought of as Mode 2 (Gibbons et al. 1994) can be reformulated as cases of this type. However, we are not suggesting the two conceptions are inter-translatable. Indeed there is an important difference between them. Gibbons et al. (1994, 29) talk about Mode 2 as something requiring a "homogenised theory or model pool". But problem-feeding does not presuppose a homogenised theory or model pool, and we have argued in the paper that unilateral problem-feeding does not require anything like that either. Successful bilateral problem-feeding requires more coordination, but there is still a significant difference.

Acknowledgments We want to thank Lennart Olsson, Paul Robinson, Cecilia and Jep Agrell, Stefan Schubert, and two anonymous reviewers of this journal for a number of constructive comments on an earlier version of this manuscript. This work was supported by the Linnaeus programme LUCID ("Lund University Centre of Excellence for Integration of Social and Natural Dimensions of Sustainability" (www.lucid.lu.se/), FORMAS, 2008–2018). An early forerunner of the text was presented at the Third Biennial Conference of the Society for Philosophy of Science in Practice (June 22–24, 2011; Exeter, UK) and has been published on Phil Sci Archive as: Thorén, Henrik and Persson, Johannes (2011) Philosophy of Interdisciplinarity: Problem-Feeding, Conceptual Drift, and Methodological Migration. http://philsci-archive.pitt.edu/8670/.

References

Apostel, L., Berger, G., Briggs, A., & Michaud, G. (Eds.). (1972). *Interdisciplinarity: Problems of teaching and research in universities*. Paris: Organization for Economic Cooperation and Development.

Arrhenius, S. (1896). On the influence of carbonic acid in the air upon the temperature of the ground. *Philosophical Magazine and Journal of Science, 41*, 237–276.
Bechtel, W. (1986). The nature of scientific integration. In W. Bechtel (Ed.), *Integrating scientific disciplines* (pp. 3–52). Dordrecht: Nijhoff.
Bolin, B., & Eriksson, E. (1959). Changes in the carbon dioxide content of the atmosphere and sea due to fossil fuel combustion. In B. Bolin, (Ed.), *The atmosphere and the sea in motion* (pp. 130–142). New York: The Rockefeller Institute and Oxford University Press.
Coase, R. (1978). Economics and contiguous disciplines. *Journal of Legal Studies, 7*, 201–211.
Craig, H. (1957). The natural distribution of radiocarbon and the exchange time of carbon dioxide between atmosphere and sea. *Tellus, 9*, 1–17.
Darden, L., & Maull, N. (1977). Interfield theories. *Philosophy of Science, 44*, 43–64.
Edlund, C., Hermerén, G., & Nilstun, T. (1986). *Tvärskap*. Lund: Studentlitteratur.
Gibbons, M., Limoges, C., Nowotny, H., Schwartzman, S., Scott, P., & Trow, M. (1994). *The New Production of Knowledge: The Dynamics of Science and Research in Contemporary Societies*. London: Sage.
Grantham, T. (2004). Conceptualizing the (dis)unity of science. *Philosophy of Science, 71*, 133–155.
Hacking, I. (1996). The disunities of the sciences. In P. Galison & D. J. Stump (Eds.), *The disunity of science* (pp. 37–74). Stanford: Stanford University Press.
Jantsch, E. (1970/1972). Inter- and transdisciplinary university: A systems approach to education and innovation. (Originally published in *Policy Sciences* 1970*). Higher Education, 1*, 7–37.
Jerneck, A., et al. (2011). Structuring sustainability science. *Sustainability Science, 6*, 69–82.
Kincaid, H. (1990). Molecular biology and the unity of science. *Philosophy of Science, 57*, 575–593.
Klein, J. T. (1990). *Interdisciplinarity. History, theory and practice*. Detroit: Wayne State University Press.
Kuhn, T. (1969/1970). 'Postscript'. In *His: The structure of scientific revolutions* (2nd ed.). Chicago: University of Chicago Press.
Laudan, L. (1977). *Progress and its problems: Towards a theory of scientific growth*. Berkeley, Los Angeles: University of California Press.
Lenton, T., Held, H., Kriegler, E., Hall, J., Lucht, W., Rahmstorf, S., et al. (2008). Tipping elements in the earth's climate system. *PNAS, 105*, 1786–1793.
Mäki, U. (2009). Economics imperialism: Concept and constraints. *Philosophy of the social sciences, 39*, 351–380.
Maull, N. (1977). Unifying science without reduction. *Studies in History and Philosophy of Science, 8*, 143–162.
Mitchell, S. D. (2002). Integrative pluralism. *Biology and Philosophy, 17*, 55–70.
Mitchell, S. D. (2003). *Biological complexity and integrative pluralism.* Cambridge: Cambridge University Press.
Mitchell, S. D. (2009). *Unsimple truths: Science, complexity, and policy*. Chicago: University of Chicago Press.
Nagel, E. (1961). *The structure of science*. London: Routledge.
National Academy of Sciences, National Academy of Engineering, and Institute of Medicine. (2005). *Facilitating interdisciplinary research*. Washington, DC: National Academies Press.
Nickles, T. (1981). What is a problem that we may solve it? *Synthese, 47*, 85–118.
Nowotny, H., Scott, P., & Gibbons, M. (2001). *Rethinking science: Knowledge and the public in an age of uncertainty*. London: Sage.
Persson, J., & Sahlin, N.-E. (2013). *Vetenskapsteori för sanningssökare*. Stockholm: Fri Tanke Bokförlag.
Revelle, R., & Süess, H. (1957). Carbon dioxide exchange between atmosphere and ocean and the question of an increase of atmospheric CO2 during the past decades. *Tellus, 9*, 18–27.
Sherif, M., & Sherif, C. (Eds.). (1969). *Interdisciplinary relationships in the social sciences*. Chicago: Aldine Publishing Company.
Thorén, H., & Persson, J. (2011). Philosophy of interdisciplinarity: Problem feeding, conceptual drift, and methodological migration. *PhilSci-Archive*. http://philsci-archive.pitt.edu/8670/.
Wahlberg, L. (2010). *Legal questions and scientific answers: Ontological differences and epistemic gaps in the assessment of causal relations*. Ph.D. thesis, Lund University.
Weart, S. (1997). Global warming, cold war, and the evolution of research plans. *Historical Studies in the Physical and biological Sciences, 27*, 319–356.
Weart, S. (2003). *The discovery of global warming*. Cambridge, Mass: Harvard University Press.
Weart, S. (2007). Roger Revelle's discovery. American Institute of Physics. http://www.aip.org/history/climate/pdf/Revelle.pdf. Accessed 17 May 2011.

Paper II

International Studies in the Philosophy of Science, 2014
Vol. 28, No. 3, 303–324, http://dx.doi.org/10.1080/02698595.2014.953343

Routledge
Taylor & Francis Group

Resilience as a Unifying Concept

Henrik Thorén

In sustainability research and elsewhere, the notion of resilience is attracting growing interest and causing heated debate. Those focusing on resilience often emphasize its potential to bridge, integrate, and unify disciplines. This article attempts to evaluate these claims. Resilience is investigated as it appears in several fields, including materials science, psychology, ecology, and sustainability science. It is argued that two different concepts of resilience are in play: one local, the other global. The former refers to the ability to return to some reference state after a disturbance, the latter the maintenance of some property during a disturbance. An implication of this analysis is that the various uses of the resilience concept are more closely related than has been previously been suggested. Furthermore, it is argued that there is a preference towards using highly abstract versions of the concept. This explains the apparent context insensitivity of the concept, but presents a problem for those hoping to establish a research programme based on it.

1. Introduction

The notion of resilience is being used increasingly in a number of scientific and non-scientific contexts: archaeology, national defence, ecology, psychology, and sustainability science, to mention just a few (see Rutter 1993; Almedom and Glandon 2007; Bonanno et al. 2007; Walker and Cooper 2011; Parker and Hackett 2012; Weiberg 2012). The precise meaning being attached to this notion, as well as its usefulness have, however, been fiercely disputed (Davidson 2010; Hornborg 2013). In the literature, and especially in work on sustainability, resilience often appears in contexts where discipline bridging and integration are central themes. It is an essential part of the framework proposed by, for example, Lance H. Gunderson, Crawford S. Holling, and others in the influential collection, *Panarchy* (Gunderson and Holling 2002). The authors seek to develop a general theory of change and express concerns that 'approaches' in which resilience has no role are partial in the sense

Henrik Thorén is at the Department of Philosophy, Lund University, and the Lund University Centre of Excellence for Integration of Social and Natural Dimensions of Sustainability. Correspondence to: Filosofiska institutionen, Lunds universitet, Kungshuset, Lundagård, SE-222 22 Lund, Sweden. E-mail: henrik.thoren@fil.lu.se

that they 'are too simple and lack an integrative framework that bridges disciplines and scales' (Holling, Gunderson, and Ludwig 2002, 8). They go on to claim that one way to put in place 'robust foundations for sustainable decision-making' is through the 'search for integrative theories that combine disciplinary strengths while filling disciplinary gaps' (Holling, Gunderson, and Ludwig 2002, 8). Others are more explicit. Charles Perrings, for instance, notes that the concept has broad appeal for both natural and social sciences:

> While the notion of system resilience has its roots in ecology, it is concerned with something that is common to any stochastic evolutionary system—the effect of the stability domain structure on the system's dynamics. These matters are currently attracting attention in a variety of different fields within economics, and in a variety of different disciplines. (Perrings 1998, 511)

More recently, in another influential volume, Gunderson and Lowell Pritchard treat resilience as a 'unifying concept in both ecological and social systems' (Gunderson and Pritchard 2002, xxi). For sustainability scientists the concept has particular appeal because it promises to connect social and natural science disciplines—an integrative achievement that some see as a prerequisite to the solution of the problems of sustainability (Kates et al. 2001; Kates 2011).

In this article, the focus is on two interrelated questions. One concerns how the concept of resilience can be used to connect disciplines, and the other concerns what might come of this. Ultimately, the interest here is in whether, for instance, the concept of resilience provides a sound basis for interdisciplinary research. In approaching this issue, however, a cross-disciplinary analysis of various resilience concepts is provided.

The article is structured as follows. Section 2 provides an overview of a range of definitions of resilience together with some background to the concept. In section 3, two conceptual cores that most forms of resilience revolve around are presented: local and global resilience. It is then argued that it is primarily just one of these, global resilience, that is associated with claims of integration and discipline bridging. Section 4 contains arguments for the view that the apparently wide applicability of resilience can only partly be explained by treating it either as a metaphor or a boundary object (two suggestions that have been made in the literature). An overlooked and important feature of the concept of resilience is instead that it is highly abstract. Context insensitivity of the sort accomplished through abstraction comes at a price, since the unification that ensues is often relatively weak.

Finally, section 5 contains a discussion of unification in which the abstract character of resilience is very much in the foreground. It is argued that resilience, in either of the senses presented, is multiply realizable, and that this feature has consequences for how resilience should be studied empirically.

2. The Many Concepts of Resilience

According to the Web of Science, the number of published works with the term 'resilience' in their titles has been growing steadily since the early 1980s. In 1993, 60 papers were published; in 2013, close to 800. In part this increase reflects uptake of the concept in various fields and disciplines, including the environmental sciences and ecology, sociology, anthropology, history, polymer science, urology, urban studies, materials science, and so on. The 10 publications with 'resilience' in their titles that appeared three decades ago, in 1983, were strongly focused on either ecology and biology or materials science, with one exception (Morrison 1983). More recently the term has found its way into the reports and working papers of highly influential institutions such as the World Bank (see e.g. World Bank 2012) and the IPCC (see e.g. IPCC 2007, 4.6.1; IPCC 2013).

Looking further back, 'resilience' figures in the titles of scientific publications as early as the second decade of the twentieth century. Materials science, especially textile research, is prominent there.[1] The general idea is that resilience is the ability of something to return to some reference state (a particular shape, for instance) after a disturbance of some sort. It might, for example, be the ability of a yarn to return to its previous length after being stretched with a weight (Hoffman 1948, 141–142).

From the 1970s until quite recently two fields adopting resilience stand out: psychology and ecology. In both disciplines during this period the concept of resilience was the subject of protracted discussion. In psychology, and particularly child and adolescent psychology, resilience came to replace the problematic concept of invulnerability.[2] Actually psychologists have used the term in several senses, including the following three:

(1) Meeting developmental goals in spite of adversity.[3]
(2) Sustained competence under stress.[4]
(3) The ability to recover following trauma.[5]

The general aim has often—although not always—been to find so-called protective factors, or protective mechanisms, which account for the resilience of individuals (see Rutter 1987).

The language of 'resilience' in ecology should be understood in the context of the debate over the stability-diversity thesis, i.e. the idea that diversity and stability are positively covariant. This thesis was famously defended by influential ecologists such as Robert MacArthur (1955) and Charles Elton (1958), and it was received opinion in ecology throughout the 1950s and 1960s.[6] Within the terminological framework that then emerged 'resilience' is a common term. It appears alongside terms like 'resistance', 'persistence', 'constancy', 'hysteresis', and 'elasticity'. Volker Grimm and Christian Wissel (1997) list 70 different stability related terms and a staggering 163 definitions. The semantic diversity here is considerable. One can nevertheless discern two concepts of resilience of special significance. First, in a very common interpretation the term 'resilience' denotes the 'returning to the reference state (or dynamic) after a temporary disturbance' (Grimm and Wissel 1997, 325). This is how Stuart Pimm (1984) used the term in his influential paper on stability. This notion being pressed into service here is sometimes referred to by other terms.

Kirstin Schrader-Frachette and Earl McCoy (1993, 33) label it 'dynamic balance' and Holling calls it 'stability' (Holling 1973), and then in later work 'engineering resilience' (Holling 1996; Holling and Gunderson 2002). In this article, the term 'local resilience' is used to indicate this ability of a system to return to a reference state (see section 3.1).

The other significant concept of resilience is often traced back to Holling (1973)—which, incidentally, is also the most cited paper with 'resilience' in the title by a wide margin.[7] Holling makes a distinction between stability and resilience, regarding the latter as follows:

> Resilience determines the persistence of relationships within a system and is a measure of the ability of these systems to absorb changes of state variables, driving variables, and parameters, and still persist. In this definition resilience is the property of the system and persistence or probability of extinction is the result. (Holling 1973, 17)

On this conception resilience is not about returning to a reference state, but rather a kind of buffer within a system. It is a margin that allows the system to retain certain structural features when perturbed. For Holling resilience is a magnitude—ideally one that may be measured—that tells us how large a disturbance a system can withstand without being pushed on to a trajectory where the system will, through its own dynamics, become extinct. This notion of resilience is common in the work of ecologists, although it is not always associated with the term 'resilience'. Gordon Orians defines inertia in a similar way as the 'ability of a system to resist external perturbations' (Orians 1975, 141). Schrader-Frachette and McCoy (1993, 33ff) prefer the term 'persistence', and Grimm and Wissel (1997) associates Holling's resilience with the term 'domain of attraction'.

2.1. *Resilience Crossing Boundaries*

Holling's resilience (as it were) is of particular interest in view of its impact outside ecology. First, it has drawn the interest of researchers in other fields, such as archaeology (Weiberg 2012), sociology (Davidson 2010), community psychology (Norris et al. 2008), and social medicine (Almedom and Glandon 2007). Second, promoters of the concept, and most prominently Holling himself, have been arguing explicitly that it should be applied in more fields. The idea is that resilience can capture a dynamic property not just of ecosystems, but others as well, including the social-ecological, the economic, and the social (Gunderson and Holling 2002; Adger et al. 2005; Folke et al. 2010).

Resilience also appeals to those interested in sustainability and sustainable development. It offers an opportunity to connect, and perhaps coordinate, information from different disciplines, which is seen as instrumental in effective sustainability science (Kates et al. 2001; Jerneck et al. 2011). It serves an explanatory purpose by accounting for interactions and events as well as providing a way of analysing actual systems for their resilience (thus in effect becoming a kind of diagnostic tool). It also offers neutral grounding for otherwise normatively charged concepts such as sustainability.[8]

	Definition	GR	Specification
1	Measure of the persistence of systems and of their ability to absorb change and disturbance and still maintain the same relationships between populations or state variables	S D I	System Unspecified Structure
2	The magnitude of disturbance that can be absorbed before the system changes its structure by changing the variables and processes that control behaviour	S D I	System Unspecified Structure
3	The capacity of a system to experience shocks while retaining essentially the same function, structure, feedbacks, and therefore identity	S D I	System Unspecified Structure, function, feedbacks
4	Quantitative property that changes throughout ecosystem dynamics and occurs on each level of an ecosystem's hierarchy	S D I	? ? ?
5	Resilience of what to what?	S D I	? ? ?
6	The ability of the system to maintain its identity in the face of internal change and external shocks and disturbances	S D I	System Unspecified Identity
7	The ability of groups or communities to cope with external stresses and disturbances as a result of social, political, and environmental change	S D I	Groups, communities Social, political, environmental change Unspecified
8	Transition probability between states as a function of the consumption and production activities of decision makers	S D I	? ? ?
9	The ability of the system to withstand either market or environmental shocks without loosing the capacity to allocate resources efficiently	S D I	Unspecified Market and environmental shock Capacity to allocate resources efficiently
10	The underlying capacity of an ecosystem to maintain desired ecosystem services in the face of a fluctuating environment and human use	S D I	Ecosystems Environmental and human use Ecosystem services
11	The capacity of a social-ecological systems to absorb recurrent disturbances (. . .) so as to retain essential structures, processes and feedbacks	S D I	Social-ecological systems Unspecified Essential structures, process, and feedbacks
12	Flexibility over the long term	S D I	? ? Flexibility
13	Maintenance of natural capital in the long run	S D I	? ? Natural capital

Notes: GR denotes global resilience; *S*, *D*, and *I* signify specifications of the system, the type of disturbance, and the property maintained, respectively. A question mark means that the place-holder is not only unspecified but not even present in the definition.

Table 1 Thirteen definitions of resilience according to Brand and Jax (2007)

Although there is always a strong link to Holling's resilience, variation in the way different authors use the concept is considerable, at least at a first approximation. Fridolin Simon Brand and Kurt Jax (2007) list 13 definitions, dividing these into four main groups on basis of their normative content (see Table 1). One such definition runs as follows:

> By resilience, we mean the capacity of linked social-ecological systems to absorb recurrent disturbances such as hurricanes or floods so as to retain essential structures, processes, and feedbacks. Resilience reflects the degree to which a complex adaptive system is capable of self-organisation (versus lack of organisation or organisation forced by external factors) and the degree to which the system can build capacity for learning and adaptation. (Adger et al. 2005, 1036)

Similar formulations can be found in Carl Folke et al.:

> Resilience, for social-ecological systems, is related to (i) the magnitude of shock that the system can absorb and remain within a given state; (ii) the degree to which the system is capable of self-organisation; and (iii) the degree to which the system can build capacity for learning and adaptation. (Folke et al. 2002, 438)

In the same vein, Brian Walker et al. (2004, 1) understand resilience as 'the capacity of a system to absorb disturbance and reorganize while undergoing change so as to still retain essentially the same function, structure, identity, and feedbacks'.

Before we proceed, it is important to note that what is sometimes called 'resilience thinking' or 'resilience theory' (Folke et al. 2010; Strunz 2012) will be left aside. These concepts capture something much broader: 'a resource management approach and a view of the world that is not necessarily tied to scientific discourse and academic institutions' (Strunz 2012, 113f). No attempt will be made to analyse this idea here, as we are interested in it only insofar as it concerns the specific concept of resilience.

3. Abstraction and the Conceptual Core(s)

The abstract nature of the concept of resilience plays an important role in the argument presented here. It is thus important to distinguish abstraction from other ways in which a concept may be said to be unspecific or imprecise. Especially important here are vagueness and ambiguity. A vague concept is often thought of as a concept with borderline cases—that is to say, a concept whose extension cannot be precisely determined. Consider the concept of a copse. A copse is a small group of trees that is not a forest. We have many examples of things that are clearly copses and we have many examples of things that are clearly forests, and thus not copses. However plenty of groups of trees lie somewhere in between and are hard to classify. This kind of vagueness can be stipulated away—in order, for instance, to make forest management easier. An arbitrary, sharp boundary (29 trees is a copse, 30 is a forest) can be imposed. Such a stipulation, however, does not mean that we now have discovered the real boundary between copses and forests, or learnt something about these concepts. Vagueness in this sense can be problematic and unwanted in science, although it is standard practice within many disciplines to make concepts more precise than vernacular usage would suggest.

Turning to ambiguity, this is a property of terminology. An ambiguous term, in other words, can be used to signal more than one kind of thing. Thus the term 'bat' can either pick out the blind, flying mammal that uses sonar for navigation, or the thing commonly used in baseball to hit the ball across the pitch. So two concepts are associated with the term 'bat', neither of which is vague, or particularly abstract. In this case disambiguation is not challenging. Other cases prove more difficult. Sebastian Strunz (2012) has argued that resilience is one of them, and that its disambiguation is challenging.

3.1. *Determinates and Determinables*

William Ernest Johnson (1921) introduced the distinction between determinates and determinables to highlight the logical structure of expressions such as 'red is a colour'. The distinction operates as follows: the concept *red* is a determinate of the determinable *coloured*, and *scarlet* is a determinate with respect to the determinable *red*. Determinables have no 'pure' instances—nothing can be coloured without being some specific colour (and hue of that colour). Abstract concepts tend to have wider extensions than more specific ones. In fact, Johnson thought of concrete things in the world as absolutely determinate (see Sanford 2013). We might expect the concept red to be applicable to more things in the world than the concept scarlet. Obviously, this is not necessarily so, since it might be the case that all red objects are scarlet. It is, however, impossible for the concept red to have a smaller extension than the concept scarlet.

Strunz (2012) has argued that 'resilience' is indeed ambiguous, but that this is not necessarily a bad thing. I will return to this discussion in sections 4.1, 4.2, and 5. If 'resilience' is indeed ambiguous, there can be no common core to all resilience concepts—here Strunz appeals to the Wittgensteinian notion of a family resemblance. I shall not reject Strunz's claims. I wish instead to explore, and attempt to formulate, a conceptual core that is shared by many, though perhaps not all, concepts of resilience. I will argue that there is a tendency among those who use the term 'resilience' to prefer more abstract interpretations of its meaning, and that this means that the concept of resilience being deployed is quite close to the conceptual core we identify.

On this account abstraction is a form of ambiguity, albeit structured in a specific way. Consider again R. M. Hoffman (1948) and resilience in textile research. Hoffman himself notes the ambiguity in the notion of resilience he uses when describing retracting yarns: one might be interested in the speed of return, how close to the original shape the yarn becomes after a disturbance, or how much stretching it can cope with (Hoffman 1948, 141f).

3.2. *Two Definitional Schemas*

If we employ Johnson's conception of abstraction, then, in our search for the conceptual core of resilience we should be searching for a determinable common to many determinates; that is to say, some general, or abstract, conception of resilience.

In section 2, I presented a range of definitions of the concept, which might be taken to suggest that no such core could possibly be found. Here, however, two resilience concepts commonly seen in many disciplines are identified. As mentioned in the introduction, I shall call these local and global resilience. This division roughly traces the conceptual line, in Schrader-Frachette and McCoy (1993), between dynamic balance and persistence. It is also shadows Holling's (1996) distinction between engineering and ecological resilience.

Local resilience, thus named because it concerns a single domain of attraction of some system, is characterized as follows:

> Local resilience: the ability of *S* to return to state *E* after disturbance *D*.

This definition can be made more specific by considering *S* to be a piece of yarn retracting after having had a weight attached to it, a child returning to normal behaviour following the loss of a close relative, or a population of fish regaining its former number after having been overfished. As explained in section 2, concepts that relate in this way to local resilience are common and are found in many fields. Interestingly, even with specifications of *S*, *E*, and *D* this notion can still be operationalized in various ways. For example, one may be interested in the speed of return (what Holling calls stability), or the difference between the states *S* is in after *D* and *E*.

As already noted, not all concepts of resilience are captured by the notion of local resilience. In fact some are defined explicitly by contrast with local resilience (as is the case in Holling 1973). What here has been called global resilience is not about the returning of a system to some reference state, but the absorption by a system of a disturbance. The choice of 'global' is motivated by the fact that it is usually considered a property of a system as a whole, rather than a feature pertaining only to a system as it is close to some domain of attraction. Global resilience involves keeping some property of a system fixed as the system is disturbed—in other words, the maintenance (not recoverability) of some property during disturbance. Sustained competence under stress, and the sort of resilience Neil Adger et al. (2005, 1036) describe as the retaining of 'essential structures, processes, and feedbacks' when a system is disturbed, both fit this characterization of resilience. They do not satisfy the requirements of local resilience.

A first approximation of global resilience is as follows:

> Global Resilience (1): the ability of *S* to absorb disturbances and maintain *I*.

S is often a part of definitions and characterizations and should be understood as a preliminary delimitation. As can be seen in the examples that have been provided references to specific classes of system are fairly common. *I* here denotes a property of the system that is maintained during the disturbance. It is ordinarily associated with the identity or persistence of the system, and thus it should, ideally, come with some explicit criteria governing when either of those relations can be said to hold. That, however, is quite rarely the case, and both content and degree of specificity varies considerably. In the examples offered here (Table 1) it is structural features, such as relationships between state variables and parameters, that recur, but other varieties are conceivable, including the upholding of a particular function, for example.

Stephen R. Carpenter and Gunderson (2001) have pointed out that global resilience is not an intrinsic property of systems, but a relation between a system and a particular (type of) disturbance: a given system S can be resilient in different degrees to different disturbances. An ecosystem might be resilient with respect to forest fires but not with respect to a ruinous pest. Thus, in our definitional schema the disturbances need to be taken into account:

Global Resilience (2): the ability of S to absorb disturbance *D* and maintain *I*.

D here represent some disturbance, or class of disturbances. This schema fits well with many of the available definitions, as shall be seen in the next section, although one should note that there is a general issue here about how disturbances are to be individuated. Individuation may be based upon the way disturbances affect the system in question, or it may be achieved via some other criterion of individuation. If one opts for the second alternative, it may be the case that this definition captures several, relevantly different forms of resilience. There will be cases where a single disturbance affects a single system in several ways, and some systems are complex in the sense that they have many dimensions that can be subject perturbation. Real social-ecological systems, like those that exist in coastal areas (Adger et al. 2005), are good examples. Prima facie, the disturbances such systems are typically assessed for—floods, rising sea level, and so on—are disruptive of these systems in any number of different dimensions. In all probability, the system will be resilient in different ways, and to varying degrees, in these different dimensions. Generally it seems preferable to think of *D* in terms of how it affects the system under scrutiny, but here I will stick to this schema as it is more representative of the definitions one actually find.[9]

Global resilience, like local resilience, can be operationalized in several ways even if *S*, *D*, and *I* are fully specified. Moreover, I do not suggest that these definitional schemas need to be precisely specified under all circumstances. Sometimes it is appropriate and preferable to use more abstract formulations. Rather this is a way of analysing resilience that appears to capture a structure present in most deployments of the notion of resilience in sustainability research, and common also within, for instance, psychology. Our suggestion is that these schemas express abstract notions that can be made more specific by filling in *S*, *D*, and *I*. The degree of specificity needed depends on the context, but more specific versions relate to more abstract ones as determinates to a common determinable.[10]

3.3. *Conceptual Pluralism Revisited*

Two questions arise. The first is: how representative are these definitional schemas of the actual usage of concepts of resilience? This question has already been answered in part, but in order to provide a fuller answer, I use Brand and Jax's (2007) list of 13 definitions as a comparative base (see Table 1). The second question has to do with general preferences as to the degree of abstraction: How abstract are the resilience concepts that are preferred? More abstract concepts are less sensitive to specific contexts, but also less informative about underlying structures and mechanisms, and this has implications vis-à-vis the strength of the unification achieved (if this is indeed the goal).

Table 1 shows how the 13 definitions of resilience collected by Brand and Jax (2007) fit with our definitional schema of global resilience. The reason local resilience has not been included is that none of them fit with that schema. Five of the 13 definitions do not fit global resilience either. Two can be disregarded immediately: no. 5 is not a definition at all and no. 4 gives only a very broad characterization of features of the property.

In the remaining three definitions that do not fit global resilience, i.e. nos. 8, 12, and 13, the assignments of *S*, *D*, and *I* are partial with respect to our definitional schema. Definitions 12 and 13 give identity criteria—a system is resilient if either flexibility or natural capital is maintained over time—but are otherwise incomplete. None of these three is, as it stands, inconsistent with global resilience (although in this regard I harbour reservations about no. 8, about which I am not sure).

As already pointed out in the previous section, local resilience fits well with resilience concepts of the sort used in materials science, psychology, and ecology. Within psychology especially, many forms of resilience are determinates of the determinable global resilience. Sustained competence under stress was already mentioned, but the definition also fits with the idea of resilience being the ability to follow developmental trajectories in spite of trauma.

Moving to the second question about the degree of abstraction displayed by resilience concepts in use, it is notable that only one definition (no. 10) specifies all three variables, and then not in particularly precise terms. Should one conclude, then, that there is a preference for abstract notions of resilience? Perhaps. But taking the contexts in which these definitions occur into account may well reveal them to be more specific. For example, 1, which is due to Holling (1973), appears in an ecological context, and thus an argument could be mounted that it should be understood as pertaining to ecosystems only. On the other hand, one might also look at practice to see if there are implicit preferences. I think there is this kind of preference for abstraction. The following two examples are perhaps not conclusive evidence, but they provide support of a kind for this suspicion.

Budworms. In their review of an example that is familiar from the literature—spruce budworms in eastern Canada originally described in Ludwig, Jones, and Holling (1978)—Donald Ludwig, Brian Walker, and Crawford Holling (1997) note that there are many ways in which resilience applies within their model. Essentially the budworm interacts with a number of species of tree in eastern Canada and is prone to periodic outbreaks that can be described by differential equations.

Consider the diagrams in Figure 1. *R* and *Q* are composite parameters that depend on the relationships between a number of lower-order parameters in the model—parameters that specify carrying capacity without predation, upper limits for predation, and so on. These lower-order parameters are actually variables in the model, albeit slowly changing ones, and depending on their relationship the model comes to rest at two, three, or four points. One unstable equilibrium occurs when budworm

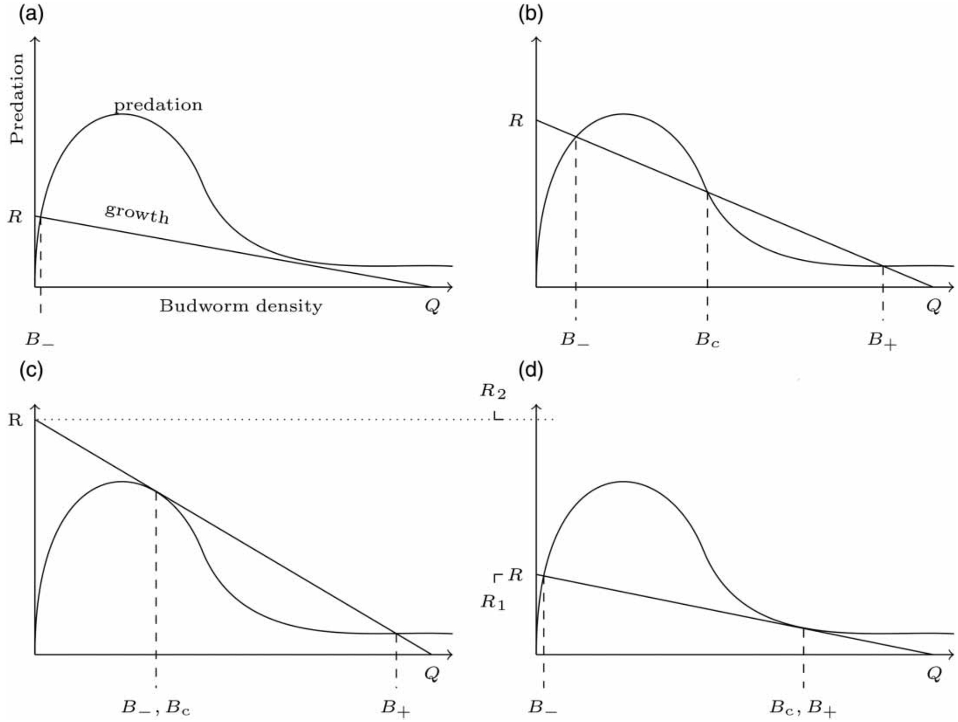


Figure 1 Four different values of R given a fixed value of Q. Different relationships between R and Q generate systems with different numbers of, and values for, those equilibria.

density is nil. For a range of relationships between R and Q a low, stable equilibrium B_- appears towards which the system will tend. As can be seen in Figure 1, for a fixed value of Q it will be the case that above certain values of $R \geq R_2$ the stable B_- and unstable B_c, first coincide, and then disappear altogether. That leaves the system with only one stable equilibrium, here designated B_+. Thus, an outbreak occurs and the system remains within this higher equilibrium until (again for the same fixed value of Q) $R < R_1$.

Ludwig, Walker, and Holling (1997) acknowledge that we may focus on the resilience of some lower—and, for the forest industry, preferable—equilibrium, and thus view the abrupt disappearance of lower equilibriums as loss of resilience. This construal is not, perhaps, straightforward, but certainly possible.[11] On the other hand, the budworm naturally interacts with its surrounding environment. The parameter α that determines when budworm density becomes saturated, for instance, is proportional to foliage density and will change with a growing forest. This affects R, effectively creating an oscillating system in which irregular, but recurrent outbreaks of budworm control other species. Where budworm outbreaks serve to regulate the balance over long periods of time, the balance between balsam fir, spruce and birch is especially important (Holling 1973, 13f). Ludwig, Walker, and Holling (1997) argue that such oscillators can be very important in biological and ecological systems, as 'they continue to oscillate more or less with the same frequency and amplitude under a wide variety of disturbances' (Ludwig, Walker, and Holling 1997, 9). The oscillator is, then, a constitutive part of what realizes the resilience of a system, and dampening it—as in this case could, and did, happen through the suppression of budworms—actually renders the larger system less resilient.

The mere fact that Ludwig, Walker, and Holling elaborate these different potential applications of the concept of resilience indicates that they are in fact endorsing an abstract notion of resilience, one that is not sensitive to the particular system it is applied to. The decision to pick one interpretation over another is then made on other grounds.

Migration and mental health. Psychologists have often focused on the outcomes of resilience highlighting 'the maintenance of functionality' in the individual (Olsson et al. 2003, 2). Indicators here may include good mental health, functional capacity, and social competence. There is an issue, however, about how these outcomes are to be evaluated. As Craig Olsson et al. point out, 'emotional well-being' poses particularly 'perplexing' challenges:

> It is tempting to define adolescent resilience solely in terms of maintenance of emotional well-being in the face of adversity. However, it may be unrealistic to believe that young people can quickly resolve the emotional ramifications of serious threat to personal values (e.g. illness, death of a loved one). Distressing emotion must in some way act as an index of adversity. (Olsson et al. 2003, 3)

The issue is that it is not clear whether we should regard the emotional dimension as that which is being disturbed (absence of emotional well-being is a sign of impact) or as an indicator of the degree of resilience (as the absence or presence of resilience). In fact, as Rutter (1993) once indicated, some have described extreme emotional responses, such as clinical depression, as the 'price of resilience' (Rutter 1993, 627). On this view, depression is essentially an adaptation to adverse conditions and hence a sign of resilience rather than its lack.

An analogous problem appears in sustainability science. Consider again the example of migration and mobility as indicators of social resilience (see Adger 2000). Displacement can be a sign that a community is collapsing, the idea being that had the system been more resilient the displacement would not have occurred. Migration thus becomes 'an indicator of the breakdown of social resilience' (Adger 2000, 357). On the other hand, mobility might be considered as a preservation strategy and an adaptation to an external factor of some kind, in which case it looks more like an indicator of resilience than a deficiency in it.[12]

Uncertainty over whether this or that feature should be interpreted as a sign of resilience or a symptom of its lack flows from the implicit, or explicit, use of a highly abstract concept of resilience. The problem can be ameliorated by stipulation, but that solution is susceptible to the suspicion that it is arbitrary, or ad hoc. The point I wish to make is that the appearance of the problem itself is enough to demonstrate that there is a preference, among the authors here considered, for a concept of resilience that is in fact highly abstract.

4. Resilience Bridging Disciplines

To recap, then, two highly abstract resilience concepts have been identified. They have been labelled local and global. These concepts can be used to structure many of the more specific resilience concepts in actual use, as the latter tend to relate to the former as determinates relate to a determinable. It has also been established, on the basis of the two examples above, that in at least some of the literature there is a preference for rather abstract concepts of resilience, and it has been noted that abstraction promises to link disparate cases of resilience in different disciplines. Now we shall move to discuss two other forms of conceptual connection: boundary objects and metaphor.

4.1. *Resilience as a Boundary Object*

Brand and Jax (2007) have proposed that resilience works as a kind of boundary object. Three features are essential to such objects (Star and Griesemer 1989; Collins, Evans, and Gorman 2007):

> Flexibility: Adaptation to local needs.
>
> Rigidity: Maintenance of identity across sites.
>
> Communication: Mediate communication between different cultures that could not otherwise communicate.

The notion of boundary objects is a descriptive one introduced to show how otherwise incommensurable cultures can communicate in spite of their differences. Establishing a boundary object is hence not preferable to, say, sharing a language. It should be thought of as a way of establishing communication when other options are not available.

This is how these requirements, as they apply to concepts, will be interpreted here. A flexible concept is ambiguous with respect to different 'cultures', i.e. different disciplines. Perhaps with meanings close to one another, what Strunz (2012) call polysemy or vagueness depending on whether these different meanings are easily separable or not. The precise nature, or degree, of this ambiguity can vary, but clearly ambiguity alone is not enough. Two disciplines may use identical ambiguous concepts: conceptual joints must coincide with disciplinary boundaries.

At the rigidity end of the spectrum there is every reason to be pluralists, and to insist that what, precisely, maintains identity across disciplines can vary. As long as there is some degree of flexibility, and communication in fact obtains, the concept is a boundary object. An extreme case would involve sharing only a label. One possibility would be to construe less radical cases in which the concepts involved are structured in exactly the way that has been suggested above as examples of different disciplines embracing different determinates of the same determinable. An obvious worry about this proposal is that it would threaten to dilute the notion of a boundary object unacceptably. Moreover, I suspect it would be hard to find cases in which the conceptual connection is one where a conceptual core is shared and no other modes of communication are available to the parties involved.

Indeed the resilience example is a case in point. A number of resilience concepts are spread among different disciplines. Mostly, however, no communication has ensued with resilience as a basis. If one brings to bear the more specific concepts of local global resilience one finds more signs of exchange, especially in respect of the latter. Interdisciplinary endeavours like the Stockholm Resilience Centre, which build on Holling's concept of resilience, appear to promote communication across disciplines successfully, but they do so by pressing a much narrower, and more fixed, resilience concept into service. Interestingly, considerable variability in the concept occurs frequently within disciplines. In both ecology and psychology several more specific varieties of both global and local resilience are visible. In such contexts, however, resilience cannot be a boundary object as there is no boundary to begin with.

There are problems with the notion of boundary objects when applied to concepts that present a challenge here, especially concerning the communication criteria. How do we verify that communication is maintained *through* the concept in question and how can one can one determine that there are no other venues of exchange available (anyway, how is that to be interpreted exactly)? Falsification, on the other hand, is somewhat easier. It is clear that merely sharing some term is not sufficient. Two disciplines can share a label by accident, perhaps without members of the respective disciplines even noticing. To describe such a situation as one where a boundary object is present makes the notion of a boundary object both trifling and somewhat arbitrary. So, it is obvious, that the concept of resilience has not been a boundary object between materials science and psychology although it may be argued that there is both some degree of flexibility and some degree of rigidity.

4.2. *Resilience as a Metaphor*

Resilience has also been treated as a metaphor. Thus it has been said that resilience 'has multiple levels of meaning: as a metaphor related to sustainability, as a property of dynamic models, and as a measurable quantity that can be assessed in field studies of SES' (Carpenter et al. 2001, 765). A metaphor involves seeing something as something else—the brain as a computer, or natural selection as a kind of artificial selection (Sloep 1997). A recurring theme in the literature on the role of metaphors in science associates the value of metaphors to their heuristic ability to generate new and interesting hypotheses to be investigated (Black 1962; Hesse 1966; Kittay 1987; Kellert 2008). Importantly, the metaphorical relation is one that holds between systems (entities, structures) and not between a concept and that to which it is applied. The resilience concept is indeed part of a metaphorical transfer but is itself not a metaphor; instead the relevant metaphor—in the context in which Carpenter et al. are writing—is the seeing of complex social entities (social systems on their terminology) as ecosystems.

The question whether the ecosystem–social system metaphor is suitable depends on its fruitfulness, and this in turn depends on both quantitative and qualitative features of the similarities that obtain (or are made to obtain) between the systems in question (together with a range of other factors). Proper assessment of the metaphor is beyond the scope of this article. Instead I wish to raise two concerns. First, this metaphor can only partly explain the wide applicability of the resilience concept. Psychologists developed their concept of resilience independently of ecologists, and this metaphor is nowhere to be found in that literature. The proliferation of the concept is considerably broader than the proliferation of the ecosystem–social system metaphor. Second, questions can be raised about whether this metaphor is indeed interdisciplinary in the collaborative sense preferred here. Among some ecologists, particularly those with a strong interest in sustainability like Holling himself, the metaphor is popular, but elsewhere it seems to have attracted little attention.[13]

5. Unity and Multiple Realizability

It is quite clear that the ecosystem–social system metaphor is, at least for many scientists interested in sustainability, what carries the concept of resilience from one domain of inquiry to another. With the proviso that one must be sensitive to the limits of metaphors in general, and this metaphor in particular, this might well prove fruitful. However, the notion of metaphor alone fails to explain the widespread use of the concept of resilience. Psychologists, for example, appear to swear by no such metaphor.

The thesis advanced here is that the wide applicability of the resilience concept is underwritten by its highly abstract character. The implications of this are important. Some of the criticisms levelled against the concept focus on what appears to be the suitability of the underlying metaphor. Hornborg, for instance, argues that the framework tends to 'mask the power relations ... that to a large extent determine how humans utilise the surface of the Earth' (Hornborg 2013, 116). Not all uses of the concept, however, build on this metaphor. The idea of resilience as a boundary object appears to be limited in similar ways. The criteria required for something to qualify as a boundary object are rarely met. Note that it is not denied that they are sometimes met, but rather that this accounts for the way in which the concept of resilience has been deployed in general.

The suggestion here is that abstraction has a larger role in explaining the broad applicability of the concept of resilience than has generally been admitted. It allows resilience to appear in highly differentiated subject matters because highly abstract concepts are comparatively context insensitive. However, abstraction cuts both ways, and this impressive level of mobility comes at the cost of weak interdisciplinary links. A shared resilience concept will, in general, provide little or no incentive for further integration. This fact might also explain why there has not been more intellectual exchange despite the similarity of the concepts of resilience in different disciplines.

One feature of highly abstract concepts is that they tend to be multiply realizable.[14] This insight can be fleshed out in various ways. It can be expressed somewhat crudely, however, as follows: some phenomena are not systematically realized at the microscopic level. To borrow an example from Jerry A. Fodor, many general and interesting things that can be said about the economic phenomenon of monetary exchange are irreducible to, for instance, physics, in this sense:[15]

> banal considerations suggest that a description [in the vocabulary of physics] which covers all such events must be wildly disjunctive. Some monetary exchanges involve strings of wampum. Some involve dollar bills. And some involve signing one's name to a check. (Fodor 1974, 103)

The notion of multiple realizability has been exploited in arguments for both the autonomy (Fodor 1974) and, curiously, for the disunity (Kim 1992) of the special sciences. The direction of the argument depends on the purpose and intent of the study: sometimes we are interested in the realizers, and sometimes we wish to leave them out.

It is clear that both local and global resilience are context-insensitive concepts in the sense that they are multiply realizable; any number of different mechanisms, or underlying structures, can be responsible for a system's resilience in either of these senses. One realizer of global resilience in organisms is redundancy. Many phenotypes will develop even if the relevant genes are knocked out. Other genes compensate for the loss (Mitchell 2009). Similar mechanisms can be observed at higher levels of organization as well—for example, where functions associated with damaged structures of the brain are taken over by other structures (Richardson 2009). This is one way in which global resilience can be realized within a particular system, but there are many others. At a more concrete level, the individual mechanisms and structures responsible for realizing resilience in individual systems come in many forms.

Broadening the perspective somewhat, suppose we explain the fall of the Western Roman Empire by saying that it was a consequence of her resilience to barbarian invasions being eroded? A number of different facts about the Empire may be said to have something to do with her resilience: maintenance of a large and sophisticated military force, effective political institutions, peaceful succession, a reliable system of collecting taxes, and so on. The resilience of the influenza virus to the countermeasures taken by its hosts, on the other hand, would probably be due to genetic, epigenetic, and structural factors: 'good' genes, of course, a small and easily manipulable genome, short cycles of reproduction, vast numbers of individuals in each generation, *et hoc genus omne*. Ecosystems, on the other hand, are resilient to external damagers such as famine, disease, and hurricanes, largely thanks to the specific relationships between, and numbers of, interacting species, as well as complex multi-state cycles. The mechanisms involved in making a child resilient to psychological trauma are different again: heritable factors, previous history of mental health, and the social network in which the child is situated, are all likely to play a role. At any other than the most abstract level—that is, the level of global resilience itself—these mechanisms and structures appear to be just the kind of 'wildly disjunctive' sets that Fodor mentions.

If our interest lies with underlying mechanisms, the observation that resilience is multiply realizable will matter for the unificationist. Mechanisms and structures that differ so radically from one another must be studied using different methods, tools, and representation. For example, modelling practices tend to be relative to specific subject matters (Dupré 1996). A study of resilience that takes its realizers into account is a pluralist project in many respects.

Are these mechanisms the locus of study then? The answer to this question depends on several factors whose analysis is largely beyond the scope of this article, but our strong suspicion is that the mechanisms are important, especially within emerging fields such as sustainability science where the debate over the concept of resilience is most active. This suspicion is anchored both in the high degree of abstraction in the resilience concept itself, since this renders a study of resilience in the absence of its realizers a strictly formal exercise, and in the empirical and concrete goals of sustainability scientists, i.e. the objective of providing diagnostics, predictions, and prescriptions relating to actual social-ecological systems.

6. Conclusions

It has been argued that most resilience concepts can be traced back to either of two general forms of resilience: local and global. Specific concepts of resilience in a range of disciplines, including ecology, psychology, materials science, and sustainability science, relate to these more abstract concepts as determinates to determinables. Furthermore, I have argued that there is a tendency, among those who make explanatory use of resilience, to implicitly or explicitly prefer abstract understandings of the concept. This has bearing on questions about the way concepts of resilience connect disparate disciplines. Several authors, including Brand and Jax (2007) and Strunz (2012), have noted that the concept of resilience is heterogeneous, and have argued that this heterogeneity may, in various ways, be helpful in connecting different disciplines. It is suggested that abstraction is, in the resilience case, an overlooked and important further way of connecting disciplines.

This suggestion is potentially important for those hoping that resilience is a concept that may unify and connect the natural and social sciences. The concepts of resilience that have been discussed pick out diverse kinds of mechanism, and to the extent that mechanisms are the locus of inquiry, a general study of resilience, global or local, needs to be a methodologically and theoretically pluralist venture. A research programme with empirical components founded on the resilience concept alone, in either of the suggested senses, can be expected to be highly diversified in many respects.

Lastly, it is my view that the current research programme dedicated to locating and describing mechanisms and structures that give rise to resilience is underdeveloped. Instead it seems that the lion's share of empirical research is devoted to finding correlates for resilience. These often appear to be little more than heuristics selected on the basis of persistence, and not particularly reliable heuristics at that. This unreliability, I suspect, flows from the fact that these indicators rarely appear capable of discerning between the ability to absorb an impact, on the one hand, and the capacity to avoid an impact, on the other. Some systems persist as a consequence of being resilient, others because they are not disturbed. If we consider these as 'strategies', we can see that they may work equally well. Thus it is widely contended that wealth is an indicator of resilience, but surely it is also an indicator of avoidance. This matters in comparative terms. For the wealthy, both avoidance and resilience are possible strategies, and, being wealthy, they can choose either as it suits them. For the poor, however, avoidance may not be option. This might indicate that poverty, rather than wealth, is an indicator of resilience, albeit not perhaps the kind of resilience accompanied by persistence. A mixed strategy may well be more successful, on the whole. In many cases, no doubt, mechanistic or structural explanations of resilience would help us to separate the two forms of strategy.

Acknowledgements

I want to thank Johannes Persson, Lennart Olsson, and James McAllister as well as two anonymous reviewers for many helpful comments and suggestions in developing this manuscript. Furthermore I am in a debt of gratitude to Paul Robinson, Brian Hepburn, and the research seminars at the centres of excellence LUCID at Lund University and TINT at Helsinki University, as well as the seminar at the Centre for Science Studies at Aarhus University, where earlier version of this paper was presented. This work was supported by the Linnaeus programme LUCID (Lund University Centre of Excellence for Integration of Social and Natural Dimensions of Sustainability, www.lucid.lu.se), FORMAS 2008-1718. Moreover this research benefited greatly from discussions within the research project, 'Measuring, Assessing and Profiling (MAP) Human Resilience', funded by the Rockefeller Foundation (contract no. 2012 RLC 304).

Notes

[1] Other fields are represented as well, such as medicine and economics, but to a diminished extent in comparison. All in all, Web of Science finds only 51 papers published before 1970.

[2] See Rutter (1993) for a discussion of the merits of resilience with respect to invulnerability. See also Rutter (1985), Garmezy (1991), and Olsson et al. (2003).

[3] Fogany et al. consider resilience to be 'normal development under difficult conditions' (Fogany et al. quoted in Daniels 2008, 60)

[4] This variety of resilience is used by Bonanno et al. (2007): they use the term to denote the 'capacity to maintain healthy, symptom-free function . . . following PTEs' (181).

[5] See for instance Dyer and McGuinness (1996). They endorse a version of the concept closer to the usages common in material science: resilience as the 'process whereby people bounce back from adversity and go on with their lives' (Dyer and McGuinness 1996, 277).

[6] Around 1970 critical voices were raised—among them Holling, as discussed below—and by the end of the 1980s the thesis had been largely discredited. For discussion of this debate see Schrader-Frachette and McCoy (1993); Redfearn and Pimm (2000); Justus (2008); deLaplante and Picasso (2011). For a detailed overview of the various stability concepts in ecology, Grimm and Wissel (1997) is an excellent resource, while Hansson and Helgesson (2003) have developed a context-independent and parsimonious account of the concept of stability.

[7] Holling's (1973) paper, published in an ecological journal, has to date had over 2000 citations. Until 1998 it had between 10 and 30 citations a year, but since then the number of citations has grown almost exponentially, with almost 230 in 2013 alone.

[8] The relationship between resilience and sustainability is contested, although the connection is often made. See for example Common and Perrings (1992); Lélé (1998); Perrings (2006); and Derissen, Quaas, and Baumgärtner (2011).

[9] If one does prefer to consider the 'effect side' of disturbances, it is important that D and I concern different parameters, or properties, of the system. In cases where they are identical the system cannot be perturbed without changing and thus, trivially, is not resilient at all.

[10] Are there senses of resilience that might have been missed? As I have spelled them out here, global and local resilience are roughly equivalent to two of the three senses of stability that Hansson and Helgesson identify. In their terminology, robustness denotes the 'tendency of a system to remain unchanged, or nearly unchanged, when exposed to perturbations'; they reserve the term 'resilience' for the 'tendency of a system to recover or return to (or close to) its original state after a perturbation' (Hansson and Helgesson 2003, 222). The third sense, which they call constancy, is non-dynamic; it describes the historical property of not changing. I have found no uses of the term 'resilience' that correspond to constancy, which was only to be expected. The word 'resilience' has roots in the Latin *resilire*, which means to jump or to bound back. In other words, dynamics appear to be at the heart of the concept. Since stability is a more abstract concept, I believe that the two senses of resilience presented above capture the vast majority of resilience concepts.

[11] For values of R some distance from the critical R_2 the system has a buffer of sorts and may therefore absorb disturbances to a number of variables and parameters without much in the way of consequence. The closer R is to R_2, however, the more probable it becomes that an otherwise minor disturbance might push the system beyond this boundary, causing an outbreak.

[12] Adger seeks to resolve this problem by drawing up a distinction between migration that is circular and seasonal, and migration that is not. The former, he argues, is a 'strategy for risk spreading at the household level' and may thus be an important constituent in resilience (Adger 2000, 357). This is unconvincing in my view. Disturbances that are highly regular are usually considered parts of the system itself. Given this, resilience concerns the ability of a system to absorb irregular (though sometimes recurring) stress. Unless migration is removed from consideration by stipulation, no conceptual barrier prevents us from viewing it as a sign of resilience.

[13] This may well change, but currently one social science journal dominates, according to Web of Science, and that is *Ecology and Society*. This should come as a surprise to no one, as this journal is the main voice of the Resilience Alliance. Mainstream social science has not taken notice of this, however. Among the 10 highest-ranked journals in economics, social science, and political science, respectively, I found no hits on searches for the terms 'resilience' and 'ecology' and only six hits between the 30 of the terms 'resilience' and 'system'.

[14] Multiple realizability is an idea originating within the philosophy of science and the philosophy of mind. Putnam (1967) introduced the notion in the course of presenting an argument to rebut reductionist claims. See also Fodor (1974).

[15] The empirical validity of this thesis has come into question, especially as it pertains to mental kinds (Bechtel and Mundale 1999; Richardson 2009). This does not matter in the case of abstract notions, however.

References

Adger, N. 2000. "Social and Ecological Resilience: Are They Related?" *Progress in Human Geography* 24: 347–364.

Adger, W. N., T. P. Hughes, C. Folke, S. R. Carpenter, and J. Rockström. 2005. "Social-ecological Resilience to Coastal Disasters." *Science* 309: 1036–1039.

Almedom, A. M., and D. Glandon. 2007. "Resilience Is Not the Absence of PTSD Any More than Health Is the Absence of Disease." *Journal of Loss and Trauma* 12: 127–143.

Bechtel, W., and J. Mundale. 1999. "Multiple Realizability Revisited: Linking Cognitive and Neural States." *Philosophy of Science* 66: 175–207.

Black, M. 1962. *Models and Metaphors*. Ithaca, NY: Cornell University Press.

Bonanno, G. A., S. Galea, A. Bucciarelli, and D. Vlahov. 2007. "Psychological Resilience after Disaster." *Psychological Science* 17: 181–186.

Brand, F. S., and K. Jax. 2007. "Focusing the Meaning(s) of Resilience: Resilience as a Descriptive Concept and a Boundary Object." *Ecology and Society* 12: 23.

Carpenter, S. R., and L. H. Gunderson. 2001. "Coping with Collapse: Ecological and Social Dynamics in Ecosystem Management." *BioScience* 51: 451–457.

Carpenter, S., B. Walker, J. M. Anderies, and N. Abel. 2001. "From Metaphor to Measurement: Resilience of What to What?" *Ecosystems* 4: 765–781.

Collins, H., R. Evans, and M. Gorman. 2007. "Trading Zones and Interactional Expertise." *Studies in History and Philosophy of Science* 38: 657–666.

Common, M., and C. Perrings. 1992. "Towards an Ecological Economics of Sustainability." *Ecological Economics* 6: 7–34.

Daniels, B. 2008. "The Concept of Resilience: Messages for Residential Child Care." In *Residential Child Care: Prospects and Challenges*, edited by A. Kendrick, 60–73. London: Jessica Kingsley Publishers.

Davidson, D. J. 2010. "The Applicability of the Concept of Resilience to Social Systems: Some Sources of Optimism and Nagging Doubts." *Society and Natural Resources* 23: 1135–1149.

deLaplante, K., and V. Picasso. 2011. "The Biodiversity–Ecosystem Function Debate in Ecology." In *Philosophy of Ecology*, edited by B. Brown, K. deLaplante, and K. Peacock, 169–200. Amsterdam: Elsevier.

Derissen, S., M. F. Quaas, and S. Baumgärtner. 2011. "The Relationship between Resilience and Sustainability of Ecological-economic Systems." *Ecological Economics* 70: 1121–1128.

Dupré, J. 1996. "Against Scientific Imperialism." In *PSA 1994: Proceedings of the 1994 Biennial Meeting of the Philosophy of Science Association*, edited by M. Forbes, D. Hull, and R. M. Burian, vol. 2, 374–381. East Lansing, MI: Philosophy of Science Association.

Dyer, J. G., and T. M. McGuinness. 1996. "Resilience: Analysis of the Concept." *Archives of Psychiatric Nursing* 10: 276–282.

Elton, C. S. 1958. *The Ecology of Invasions by Animals and Plants*. Chicago, IL: University of Chicago Press.

Fodor, J. A. 1974. "Special Sciences (or: The Disunity of Science as a Working Hypothesis)." *Synthese* 28: 97–115.

Folke, C., S. R. Carpenter, T. Elmqvist, L. H. Gunderson, C. S. Holling, and B. Walker. 2002. "Resilience and Sustainable Development: Building Adaptive Capacity in a World of Transformations." *Ambio* 31: 437–440.

Folke, C., S. R. Carpenter, B. Walker, M. Scheffer, T. Chapin, and J. Rockström. 2010. "Resilience Thinking: Integrating Resilience, Adaptability and Transformability." *Ecology and Society* 15: 20.

Garmezy, N. 1991. "Resiliency and Vulnerability to Adverse Developmental Outcomes Associated with Poverty." *American Behavioral Scientist* 34: 416–430.

Grimm, V., and C. Wissel. 1997. "Babel, or the Ecological Stability Discussions: An Inventory and Analysis of Terminology and a Guide for Avoiding Confusion." *Oecologia* 109: 323–334.

Gunderson, L. H., and C. S. Holling, eds. 2002. *Panarchy: Understanding Transformations in Human and Natural Systems*. Washington, DC: Island Press.

Gunderson, L. H., and L. Pritchard, eds. 2002. *Resilience and the Behavior of Large-scale Systems*. Washington, DC: Island Press.

Hansson, S. O., and G. Helgesson. 2003. "What is Stability?" *Synthese* 136: 219–235.

Hesse, M. B. 1966. *Models and Analogies in Science*. London: Sheed and Ward.

Hoffman, R. 1948. "A Generalized Concept of Resilience." *Textile Research Journal* 18: 141–148.

Holling, C. S. 1973. "Resilience and Stability of Ecological Systems." *Annual Review of Ecology and Systematics* 4: 1–23.

Holling, C. S. 1996. "Engineering Resilience vs. Ecological Resilience." In *Engineering Within Ecological Constraints*, edited by P. Schulze, 31–43. Washington, DC: National Academies Press.

Holling, C. S., and L. H. Gunderson. 2002. "Resilience and Adaptive Cycles." In *Panarchy: Understanding Transformations in Human and Natural Systems*, edited by L. H. Gunderson and C. S. Holling, 25–62. Washington, DC: Island Press.

Holling, C. S., L. H. Gunderson, and D. Ludwig. 2002. "In Quest of a Theory of Adaptive Change." In *Panarchy: Understanding Transformations in Human and Natural Systems*, edited by L. H. Gunderson and C. S. Holling, 3–22. Washington, DC: Island Press.

Hornborg, A. 2013. "Revelations of Resilience: From the Ideological Disarmament of Disaster to the Revolutionary Implications of (P)anarchy." *Resilience: International Policies, Practices and Discourses* 1: 116–129.

IPCC. 2007. *Climate Change 2007: Contribution of Working Group II to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change*. Cambridge: Cambridge University Press.

IPCC. 2013. *Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change*. Cambridge: Cambridge University Press.

Jerneck, A., L. Olsson, B. Ness, S. Anderberg, M. Baier, E. Clark, T. Hickler, et al. 2011. "Structuring Sustainability Science." *Sustainability Science* 6: 69–82.

Johnson, W. 1921. *Logic, Part I*. Cambridge: Cambridge University Press.

Justus, J. 2008. "Complexity, Diversity, and Stability." In *A Companion to the Philosophy of Biology*, edited by S. Sahotra and A. Plutynski, 321–250. Oxford: Wiley-Blackwell.

Kates, R. W. 2011. "What Kind of Science Is Sustainability Science?" *Proceedings of the National Academy of Sciences* 108: 19449–19450.

Kates, R. W., W. C. Clark, R. Corell, J. M. Hall, C. C. Jaeger, I. Lowe, J. J. McCarthy, et al. 2001. "Environment and Development: Sustainability Science." *Science* 292: 641–642.

Kellert, S. 2008. *Borrowed Knowledge: Chaos Theory and the Challenge of Learning Across Disciplines*. Chicago, IL: University of Chicago Press.

Kim, J. 1992. "Multiple Realization and the Metaphysics of Reduction." *Philosophy and Phenomenological Research* 52: 1–25.

Kittay, E. F. 1987. *Metaphor: Its Cognitive Force and Linguistic Structure*. Oxford: Clarendon Press.

Lélé, S. 1998. "Resilience, Sustainability, and Environmentalism." *Ecological Economics* 2: 1–7.

Ludwig, D., D. D. Jones, and C. S. Holling. 1978. "Qualitative Analysis of Insect Outbreak Systems: The Spruce Budworm and Forest." *Journal of Animal Ecology* 47: 315–332.

Ludwig, D., B. Walker, and C. S. Holling. 1997. "Sustainability, Stability, and Resilience." *Ecology and Society* 1: 7.

MacArthur, R. 1955. "Fluctuations of Animal Populations and a Measure of Community Stability." *Ecology* 36: 533–536.

Mitchell, S. D. 2009. *Unsimple Truths: Science, Complexity, and Policy*. Chicago, IL: University of Chicago Press.

Morrison, M. K. 1983. "Ethnicity and Integration Dynamics of Change and Resilience in Contemporary Ghana." *Comparative Political Studies* 15: 445–468.

Norris, F. H., S. P. Stevens, B. Pfefferbaum, K. F. Wyche, and R. L. Pfefferbaum. 2008. "Community Resilience as a Metaphor, Theory, Set of Capacities, and Strategy for Disaster Readiness." *American Journal of Community Psychology* 41: 127–150.

Olsson, C. A., L. Bond, J. M. Burns, D. A. Vella-Brodrick, and S. M. Sawyer. 2003. "Adolescent Resilience: A Concept Analysis." *Journal of Adolescent Health* 26: 1–11.

Orians, G. 1975. “Diversity, Stability and Maturity in Natural Ecosystems.” In *Unifying Concepts in Ecology*, edited by W. H. van Dobben and R. Lowe-McConnell, 139–150. The Hague: Dr. W. Junk.

Parker, J. N., and E. J. Hackett. 2012. “Hot Spots and Hot Moments in Scientific Collaborations and Social Movements.” *American Sociological Review* 77: 21–44.

Perrings, C. 1998. “Resilience in the Dynamics of Economy-environment Systems.” *Environmental and Resource Economics* 11: 503–520.

Perrings, C. 2006. “Resilience and Sustainable Development.” *Environment and Development Economics* 11: 417–427.

Pimm, S. L. 1984. “The Complexity and Stability of Ecosystems.” *Nature* 307: 321–326.

Putnam, H. 1967. “Psychological Predicates.” In *Art, Mind, and Religion*, edited by W. Capitan and D. Merrill, 37–48. Pittsburgh, PA: University of Pittsburgh Press.

Redfearn, A., and S. L. Pimm. 2000. “Stability in Ecological Communities.” In *The Philosophy of Ecology: From Science to Synthesis*, edited by D. R. Keller and F. B. Golley, 124–131. Athens: University of Georgia Press.

Richardson, R. 2009. “Multiple Realization and Methodological Pluralism.” *Synthese* 167: 473–492.

Rutter, M. 1985. “Resilience in the Face of Adversity: Protective Factors and Resistance to Psychiatric Disorder.” *British Journal of Psychiatry* 147: 598–611.

Rutter, M. 1987. “Psychosocial Resilience and Protective Mechanisms.” *American Journal of Orthopsychiatry* 57: 316–331.

Rutter, M. 1993. “Resilience: Some Conceptual Considerations.” *Journal of Adolescent Health* 14: 626–631.

Sanford, D. H. 2013. “Determinates vs. Determinables.” In *Stanford Encyclopedia of Philosophy*, edited by E. N. Zalta. http://plato.stanford.edu/archives/spr2013/entries/determinate-determinables. Accessed 12 October 2013.

Schrader-Frachette, K., and E. McCoy. 1993. *Method in Ecology: Strategies for Conservation*. Cambridge: Cambridge University Press.

Sloep, P. B. 1997. “The Metaphorical Transfer of Models.” In *Human by Nature: Between Biology and the Social Sciences*, edited by P. Weingart, S. Mitchell, P. J. Richerson, and S. Maasen, 125–133. Mahwah, NJ: Lawrence Erlbaum.

Star, S. L., and J. R. Griesemer. 1989. “Institutional Ecology, ‘Translations’ and Boundary Objects: Amateurs and Professionals in Berkeley’s Museum of Vertebrate Zoology, 1907–39.” *Social Studies of Science* 19: 387–420.

Strunz, S. 2012. “Is Conceptual Vagueness an Asset? Arguments from Philosophy of Science Applied to the Concept of Resilience.” *Ecological Economics* 76: 112–118.

Walker, B., C. S. Holling, S. R. Carpenter, and A. Kinzig. 2004. “Resilience, Adaptability and Transformability in Social-ecological Systems.” *Ecology and Society* 9: 5.

Walker, J., and M. Cooper. 2011. “Genealogies of Resilience: From Systems Ecology to the Political Economy of Crisis Adaptation.” *Security Dialogue* 42: 143–160.

Weiberg, E. 2012. “What Can Resilience Theory Do for (Aegean) Archaeology?” In *Matters of Scale: Processes and Courses of Events in the Past and the Present*, edited by N. M. Burström and F. Fahlander, 147–165. Stockholm: Postdoctoral Archaeological Group.

World Bank. 2012. *Resilience, Equity, and Opportunity: The World Bank 2012–2022 Social Protection and Labor Strategy*. Washington, DC: The World Bank.

Paper III

Replacement and Expansion in Scientific Imperialism

Henrik Thorén[1, 2]

[1] *Dept. of Philosophy, Lund University, Sweden*
[2] *LUCID, Lund University Centre of Excellence for Integration of Social and Natural Dimensions of Sustainability, Sweden*

Publication Status: Submitted

Abstract

This paper overviews recent, and not so recent, contributions to the literature on scientific imperialism. Two common reasons why imperialism is thought to be illicit are identified. One relies on failed expansion, or generalisation, the other on the imperialising discipline to replace viable alternatives. These are labeled type-I and type-II imperialist failures respectively. It is argued that these failures are independent of one another, although not mutually exclusive. It is argued that type-II failures are generally more serious but that in order to make sense of them one will have to give the boundaries of the context in which replacement takes place. The closing sections of the paper provides a discussion on an issue often overlooked in contemporary literature, namely what resistance to imperialism really amounts to.

Keywords: Scientific Imperialism, Unification, Interdisciplinarity

1 Introduction

Scientific imperialism—usually thought of as the illicit influence of one discipline upon another—has recently emerged as a topic for discussion in the philosophy of science (Mäki, 2009, 2013; Mäki and Marchionni, 2010; Clarke and Walsh, 2009, 2013; Kidd, 2013). Or should one say 're-emerged' as the issue has been discussed several times before, both in philosophy and the sciences (Dupré, 2001; Midgley, 1984b,a; Stillman, 1955). It is an important issue, for several reasons. For one, it connects to a range of entrenched issues in the philosophy of science, such as unification, reductionism, pluralism, modelling, metaphor, analogy, and so on. etc. Scientific imperialism is also closely linked to the issue of interdisciplinarity which is also gaining in interest both in science policy and among philosophers (see e.g. Thorén and Persson, 2013; Andersen and Wagenknecht, 2013; Grüne-Yanoff, 2011; Schmidt, 2008; Hansson, 1999). Here scientific imperialism might tell us something about the 'proper relationship' (Clarke and Walsh, 2009) between the disciplines and may thus, in a way, lay out the ground rules for interdisciplinary collaboration.

A central concern when in discussing scientific imperialism is the way in which it fails—i.e. what, more precisely, it is that makes imperialism illicit.[1] Interdisciplinary boundary crossing, in general, should not be avoided or resisted, quite the opposite. In this paper the focus will be on the reasons that have been pointed out for why imperialism fails. I identify two general ideas present in the literature. One relies on imperialism being an expansionist

[1] For most who have taken an interest in the topic the term has been taken as a pejorative signifying illicit infringements. Indeed, it is difficult to discuss the matter without resorting to terms with similarly negative connotations. An exception is Uskali Mäki (2009; 2013) who uses the term in a normatively neutral fashion and identifies relevant forms of imperialism with unification across disciplinary boundaries.

error, or misguided generalisation, where introduced theories fail to explain that which they set out to explain. The other reason has to do with replacement—one emphasises not only a failure of explanation but also (or only) that the introduced theory (or method, or approach) obscures or even outcompetes viable alternatives. I label these forms as type-I and type-II imperialism respectively. I then provide arguments showing that these are independent, but not mutually exclusive, forms of imperialism. Finally I will provide a discussion comparing the two forms in terms of whether one should be considered more severe than the other. Here several further issues arise. One is what, precisely, resistance to imperialism amounts to and in what way it differs from the kind of critical attitude that all scientists should adopt. Another is the context in which scientific imperialism takes place. Especially with respect to replacement it appears that in order to obtain a relevant notion of imperialism a context will usually have to be specified.

2 Two Problems with Imperialism

As already noted, scientific imperialism has seen a revival as a topic for philosophical discussion as of late. An example of that is a recent issue in International Studies in the Philosophy of Science (2013 volume 27, issue 3) in which several papers are devoted to that discussion. An important predecessor to the present debate is John Dupré who in several publications has touched on imperialism in his criticism of economics and evolutionary psychology (Dupré, 1996; Dupré, 2001). Dupré's philosophy of imperialism is, in many ways, the centre of gravity and point of departure for that discussion (Clarke and Walsh, 2009, 2013; Mäki, 2013; Kidd, 2013). In what follows I will identify two reasons why imperialism is problematic and show that they are both prevalent in the literature. Let us begin by examining Dupré.

Dupré's aim is not to provide a general account of scientific imperialism but rather to stage a comprehensive assault on a few specific instances of scientific imperialism. Two examples feature prominently in his writing—both of which can be considered standards, or variations thereof, in the scientific imperialism literature. One is evolutionary psychology and the other expansion of economics into the social sciences. Let us briefly review one example. A recurring target of Dupré's is Gary Becker and his influential, and highly controversial, *Treatise on the Family* (Becker, 1991). Becker sets out to explain—among other things—why fertility rates are so high in developing countries and so low in their developed counterparts. The reason this needs explaining has to do with an expectation that families should try to produce as a many children as they possibly can. Becker approaches this problem complex by exploring the market metaphor applied to the choices in family life. How and when one picks a spouse, reproduces, and rears of children, are all thought of as transactions on a market. Individuals compare the family productivity of different potential partners and weigh quality versus quantity of the children they produce and so on. Becker's arguments aims to support the idea that in the developed world the rational choice is to go for quality of children, whereas in the developing world quantity is better.

Dupré's critique of Becker explores a number of different routes. An important one, however, is that the market framework that Becker presses into service fails to adequately represent the phenomenon towards which it is pointed—Becker's models tends to amplify peripheral features of the phenomenon they are to explain at the expense of the central. A concern that Dupré returns to is how Becker continually treats social norms as mere tastes, he writes: 'The most obvious point is that to treat altruism, morality, or accepted social norms simply as tastes that some people happen to have—I like candy and fast cars, you like morality and oysters—is grossly to misplace the importance of norms of behavior in peoples' lives' (Dupré, 1996, 378). In more general terms Dupré perceives of scientific imperialism as '...the tendency to push a good scientific idea far beyond the domain in which it was originally introduced, and often far beyond the domain in which it can provide much illumination' (Dupré, 2001, 74). Hence two criteria emerge from Dupré's construal; transfer and misapplication. For something to be a case of scientific imperialism it has to, 1), involve the transfer of an idea beyond the domain to which it was originally developed, and 2), the failure of that idea to 'illuminate' this new domain.[2] The problem with imperialism rests with the failure to illuminate, rather than with the transfer itself.

One feature of Dupré's characterisation is that failed expansionism is emphasised as main concern with imperialist infringements. This has been pointed out by, for instance, Mäki who points out that Dupré's suggested definitions are 'silent about imperialism' (Mäki, 2013, 327). A failure of providing illumination—whatever that amounts to—may occur even if the domain into which the intrusion was directed was not already subject to theorisation. Now, this is problematic for Dupré if we are to consider his characterisation as a definition since it will not be able to discern between relevantly different phenomena, i.e. expansionism and imperialism. However, since Dupré is mainly concerned with cases in which their imperialist nature are fairly obvious already at the outset it does appear as a serious issue. But there is nonetheless an important point to make. One idea that Dupré elaborates with is that what makes imperialism problematic is a failure of the imperialising idea to 'illuminate' plain and simple. As, for instance, models are moved away from whence they spring their applicability tends to falter in the sense that they are no longer

[2]There are several reasons why one might think this characterization is at once too constrictive, and too permissive. See Mäki (2013) for a more detailed discussion of Dupré in particular.

likely to accurately reflect their target systems, or so argues Dupré.

Models are, after all, constructed to reflect as accurately as is practicable, particular kinds of situations. The further they are transported from their paradigm applications the less realistic they will become, and the more they will need to be modified to account for differences between their original home and their new areas of application. As new environments introduce different causal factors, alien models from distant domains will be increasingly partial in their relevance. (Dupré, 1996, 380)

The issue hence is Becker's unrestrained application of rational choice theory to domains where it just does not inform us. This notion—that imperialism is problematic since it involves a generalisation or expansion gone astray—is unsurprisingly recurrent in the literature and goes far back. Calvin Stillman, an early forerunner, defines *academic* imperialism in terms very similar to Dupré's, viz. as 'the extension of one's thought-system beyond its most applicable area' (Stillman 1955, 78).

To be clear. On this understanding of imperialism it appears that the problem with imperialism is failure of application, or illumination. It can arise when a model is deployed outside of the domain to which it was originally created, or when a theory is moved from one domain to another. The object of transfer fails to do what it is supposed to do. It is a problem that can arise both in imperialist and expansionist contexts and in so far as this is the feature one is interested in, there is no relevant difference between the two contexts. Let us call this a failure of expansion, or *type-I failure*. We can contrast this idea of failure to expand a thought-system or an idea, with the notion of replacement.

As mentioned a considerable portion of the current literature on imperialism takes off from Dupré's work. An example of this is Clarke and Walsh (2009) who aim to identify, more precisely, the 'normative content' of Dupré's critique (Clarke and Walsh, 2009, 196). Their strategy is to work the metaphor of political imperialism. They find philosophical traction in the so called 'exploitation model' of political imperialism (Clarke and Walsh, 2009, 200). Two suggestions emerge. One concerns the depreciation of values, which I will leave aside for now. The other departs from the notion that imperialism is wrong because it involves the suppression of 'indigenous knowledge.' Clarke and Walsh models this idea on so-called 'Kuhn-loss.' That is to say, sometimes when a theory is replaced by another, certain explanations become unavailable. When the phlogiston theory of combustion was replaced by the oxygen theory, some explanations pertaining to the similarity of metals had to be abandoned as they could not be recovered using the latter theory. In that case those explanations were forthcoming, but Clarke and Walsh nonetheless propose that there may, conceivably, be situations where certain avenues of research are permanently closed as a consequence of imperialist infringement. For Clarke and Walsh this becomes a viable watershed distinguishing imperialism from unification; the former constitutes the permanent suppression of 'indigenous knowledge' whilst the latter does not, '[i]n acceptable instances of scientific unification, the unification of two or more scientific disciplines occurs without the loss of any significant indigenous knowledge, from either of the disciplines that are subsequently united' (Clarke and Walsh, 2009, 202). The problem here is not (only) with a failure of illumination (or application) but with obscuring, or blocking, alternative accounts. Imperialising theories replace already present counterparts.

Another philosopher of imperialism who have emphasised replacement is Mary Midgely (1984b; 1984a). Midgley, who develops her account whilst discussing the shortcomings of sociobiology, argues that the problem with imperialism is that it is harmfully reductive (Midgley, 1984b, 107). For Midgley harmless reduction involves establishing conceptual connections—in essence relating one discipline to another—and is not a destructive, or eliminative venture; 'good biology' she writes 'cannot clash with good sociology, or good ethics' (Midgley, 1984b, 107). Although Midgley, at least in part, coincides with Dupré in noting that sociobiologists are making undue generalisations, that is not all there is to it.

> What is wrong with Reduction? Arguments are 'reductive' in a bad sense if they devalue the valuable and conceal the important. The word is most often used to-day for arguments which do this by importing the physical sciences into areas where they have no business, and using them to exclude more suitable ways of thinking—commonly ones drawn from the social sciences or humanities–in a way which prevents the valuable or important things from being properly described, or even mentioned. (Midgley, 1984b, 107)

Now Midgley is not categorically dismissive of sociobiology; she believes there are a number of things the sociobiologists gets right (Midgley, 1984a, 158). And the problem is not merely one of biologists over-reaching—although that is part of it—but also that they threaten to replace far more appropriate alternatives. It is the 'somewhat wild offers made [by sociobiologists] to take over the social sciences' (Midgley, 1984a, 159) that really cause the upset. It is not only a failure of expansion, but also a failure of replacement. Let us call such a failure a *type-II failure.*

In the literature one may hence discern two reasons why imperialism is unappealing and is claimed to fail. One has to do with generalisation and expansions. The introduced framework, theory, or model, fails to adequately represent that which it is applied to. This is the type-I failure. The other reason has to do with replacement, what we call type-II. Here the issue is mainly with viable, perhaps even superior, alternates being lost.

An important question that arise at this juncture is how type-I and type-II failures are connected. It is quite clear that they are not mutually exclusive—but are they independent? Or do they reduce to one another somehow? If they are indeed independent, are they equally grave errors? And what can, or should, be done to avoid them? In the sections below I will attempt to address these questions.

3 Independence

It is possible to introduce a new explanation to some phenomenon without necessarily outcompeting alternatives. Many phenomena have complex causal profiles and a new theory or explanation may highlight a cause hitherto overlooked. An example. Within sustainability research resilience theory is a highly influential framework (Thorén, 2014). Resilience theory was originally developed within theoretical population ecology but has since been broadened considerably (see e.g. Holling, 1973; Gunderson and Holling, 2002; Adger, 2000; Walker *et al.*, 2004). It is a framework that seeks to explain why different systems change in certain ways under stress. Of particular interest are systems that suddenly undergo catastrophic change and move from one stable equilibrium to another. Paradigmatic examples would be when, for instance, a fish population suddenly collapses without apparent increase in stress on that population (e.g. in the form of increased fishing pressure) and settles in a state where much less biomass is supported. In the last two or three decades resilience theory has increasingly been applied within sustainability research. Resilience theoreticians have argued that this theory of dynamic change can be applied to not only ecological systems, but also social systems.

The aim of resilience theory is to produce an 'integrative theory' that must 'transcend boundaries of scale and discipline' and as well as help 'organizing out understanding of economic, ecological, and institutional systems' (Holling *et al.*, 2002, 5). This is done by deploying the framework in the social domain. Below is an example of how this may look.

> Competitive processes lead to a few species becoming dominant, with diversity retained in residual pockets preserved in a patchy landscape. While the accumulated capital is sequestered for the growing, maturing ecosystem, it also represents a gradual increase in the potential for other kinds of ecosystem futures. For an economic or social system, the accumulating potential could as well be from the skills, networks of human relationships, and mutual trust that are incrementally developed and tested during the progression from r to K. (Holling and Gunderson, 2002, 35)

The deep analogies perceived between ecological systems and social systems means that the systemic property resilience can be used in explaining why this or that social system collapsed at a certain point in time.

Social change is a topic that traditionally has belonged to social sciences such as sociology and political science and thus this infringement can seen as a potential case of imperialism.

There are different ways in which this situation may be understood but suppose we think of it like this: Collapse and change in social systems can happen due to any number of different causes—what resilience theoreticians are doing is adding another cause to the mix. For the sake of argument, suppose they are wrong about this cause. Then we can perhaps raise concerns of illumination and applicability, and most certainly about explanation. The case could be made that ecology is imperialising the social sciences as the conditions for a type-I failure is met. However, there need not be an issue of replacement at the same time. Hence, a case would be had in which imperialism fails due to expansion although not through replacement. Clearly there can be type-I failures without type-II failures.

What about the other way around: can there be type-II failures without type-I failures? This is a little more difficult and requires some qualification. The situation would be one in which an epistemically successful theory—one that is illuminating, or applicable, or succeeds to explain—nonetheless wrongfully replaces alternatives. But what would make this replacement wrongful if the theory is epistemically successful?

Let's consider the case of resilience theory again. Suppose resilience theoreticians are not mistaken in their analysis. Social systems do indeed collapse as a consequence of loss of resilience at times. There are convincing arguments to be made why this is not a full explanation of the phenomenon of social change (see e.g. Thorén, 2014; Hornborg, 2013; Davidson, 2010; Jerneck and Olsson, 2008). Nonetheless, it may occasionally be a good explanation, for particular cases. If resilience theory would outcompete alternative explanations for social change it seems we would have an epistemically successful theory that nonetheless wrongfully outcompetes alternatives and there can be type-II failures without type-I failures. The situation is highly reminiscent of the kind of cases that Sandra Mitchell (2002; 2003; 2009) draws on arguing for her particular variety of pluralism. She describes instances where different theories are, mistakenly, understood as mutually exclusive alternatives although in fact they are compatible. For example, different theories regarding how division of labour has arisen in social insects—is it a consequence of self-organisation or evolution, etc.—are quite compatible with one another at the level of concrete phenomena. Division of labour in social insects is a complex phenomenon in the sense that it can, and often does, have several causes. This is of obvious relevance here as the mistake that has been made is to assume that a theory that is true (or good, at least) is the only theory that is needed. Hence it out-competes other theories with which it should be integrated.

The type of case that Mitchell describes are interesting for someone with interest in scientific imperialism as it might help to explain why imperialist infringements are common. After all we would like to think that wrongfully overstepping the reach of ones discipline is not so much a consequence of scientists being 'naïve dupes who are liable to fall for the illusory charms of inferior explanations' (Clarke and Walsh, 2009, 202) but rather as a subtle error that is easily made, even by attentive and intelligent individuals (Clarke and Walsh, 2009, 202).

4 On Expansion, Resistance, and Avoidance

So, if indeed failures of type-I and type-II are independent, they make the basis for two different forms of imperialism. Is any one of them worse than the other? Clearly, in many cases the worst situation arises when both failures are present such as when false theories replace true ones, but let us consider them individually.

First, however, in making such considerations there are other aspects one has to take in to account as well. In particular, what can be done to resist imperialism and what would such resistance amount to? As for example Mäki (2013, 330) has pointed to, it does not seem reasonable to outright ban interdisciplinary boundary-crossing. Boldness is a scientific virtue and often the rewards out-weight low prior probabilities of success. Remember, imperialism is in many ways a kind of faux-interdisciplinarity. It looks like interdisciplinarity since it involves components belonging to several disciplines but might just as well be described as a kind of over-charged disciplinarity. Hence the solution can hardly be less real—that is to say, collaborative and involved—interdisciplinarity, but more of the very same.

Ceteris paribus one may argue that type-II failures are worse than type-I as the former involves an actual loss of something: a superior explanation that is actually available is discarded in favour of one that is inferior.

However, although, it clearly must be the case that individual instances of expansionist failure should be resisted, for the same reasons that failed explanations in general should be resisted, it is problematic to formulate prescriptions of some sort that would allow us to pre-empt this kind of error without also being much too prohibitive. This has been pointed out several times. Mäki (2013, 330) notes that risk-taking is an intellectual virtue, one consequence of which is that attempted explanations sometimes, or even often, fail (see also Clarke and Walsh, 2009, 198). Such a risk of failure is not a good reason to avoid disciplinary boundary crossing. Notably, this is the case even when the boundary crossing is not particularly well founded. The potential gains may be substantive and the costs involved negligible (given that the issue is only with a type-I failure).

In principle all scientific claims should be resisted, at least to a point. That is how we find whether or not they fail in the first place—so not only should failed explanations be resisted, but also those that are (as of yet) successful.

We seem to be roughly in the same situation with regards to type-II failure. One should always resist claims of replacing theories for the same reasons as one should resist all scientific claims, and really, there seems to be no way of avoiding the phenomenon all together. Some have nonetheless made observations concerning situations where mistakes may easily be made. Mäki, for example discusses what he calls comparative consilience. Suppose we observe the distinction between subsumptive and cardinal consilience. A theory T1 is subsumptively more consilient than a theory T2 if, for instance, T1 explains the set of facts F1, F2, F3, F4, whilst T2 only explains the set F1, F2, F3. T1 is cardinally more consilient than T2 under circumstances such as the following; T1 explains F1, F2, F3, F4 and T2 explains F1, F5. A subsumptively more consilient theory is a better theory, in general, whereas a cardinally more consilient theory is not. Clark's and Walsh's idea of imperialising theories permanently blocking alternative explanations also exemplify failure of type-II.

5 Limiting the Context

For the remainder of this paper I will focus on type-II failure. As contended in the previous section one may raise concerns about precisely what replacement means. What happens to the out-competed theories and in what sense are they now not available anymore?

Science at large, and even individual disciplines, appears to have a rather high tolerance for inconsistency and contradiction and only rarely does it appear that theories are completely abandoned. It is notable, thus, that Clarke and Walsh when they discuss Kuhn-loss can produce no example of a case where adopting one framework has indeed permanently shut the door on some otherwise fruitful avenue of research. Indeed, how could they? If we take the idea of permanence literally, the concern may be raised that, although imperialist infringements with these consequences are deplorable, they simply don't seem to happen very often, if indeed ever.

What this highlights, I think, is not that replacement isn't an issue worthy of particular attention, but rather that we need to be careful with specifying the context in which replacement occurs. There are at least two situations in which replacement would have serious consequences that appear to be quite frequent. One is situations in which science is guiding for example policy making. The other is within interdisciplinary projects. The former play an important role in Dupré's account. He points out that, if we are not worried about the sub-par science, we should at least worry about its practical consequences.

> Bad science, when directed at human nature or society, is always liable to lead to bad practice. And if there

> is one overriding reason for people to care about dubious science, it is because it lends support to pernicious social policy. (Dupré, 2001, 4)

What is at stake here is not only that 'bad' science was made—perhaps where it could have been avoided—but also that harmful practice ensued. Whereas scientific problems can always be returned to, should an existing solution be revealed to be less than acceptable, practical problems do not always afford us this luxury. Moreover, spurious solutions to practical problems can cause actual suffering in the meantime. Dupré exemplifies: there is a strong tendency in the US to treat ADHD with prescription drugs. This practice can be analysed as a certain type of science—one focused solely on biochemical aspects of the diagnosis—thereby imperialising the problem domain (Dupré, 2001, 14).

Often enough practical issues require collaborative interdisciplinarity—ADHD treatment is perhaps a case in point—so perhaps interdisciplinary contexts are just another side of the same coin. The reason imperialism is interesting here, or rather imperialism that exhibits failure of replacement, is that it seems to threaten the very foundation of such an effort. In some cases where failure of replacement occurs a problem is mistakenly thought to be the domain of a single disciplines, whereas it really requires the input from several disciplines. The risk here is that endeavours that really should be interdisciplinary become hijacked by a single discipline. This does not automatically preclude all other disciplines frsom input, but may narrow the scope of the problem in a way that is really quite detrimental. Consider the following example.

There is a long-standing debate within sustainability research between those who defend a strong version of the sustainability concept and those who prefer a weak variety (see e.g. Gutés, 1996). The difference between the two versions of the concept relates to whether one thinks that human and natural capital are exchangeable. The latter camp is dominated by economists who often base their view on the Nobel laureate Robert Solow's work on maintenance of consumption when resources are exhaustible (see e.g. Solow, 1974). Let's examine Solow's argument briefly.

The thesis that Solow (1974) argues for is that consumption can be kept at a constant level even under circumstances when the resource base of the economy is exhaustible. Solow's approach is standard to economics and highly formal. He basically provides a formal proof that it is so. But in order to fit the problem formulation to a formal framework he needs to do a number of idealisations and assumptions.

The first such assumption concerns the type of economy in question. Solow considers what is called Cobb-Douglas economies. They are represented formally by the following function:

$$Q = Q(L, K, N) \tag{1}$$

Q stands for the aggregate output of the economy and L, K, and N labour, man-made capital and natural capital respectively. So aggregate output of the economy is a function of those three factors.

Another assumption that Solow makes concerns substitution between natural and man-made capital. Solow stipulates this substitution to be, as he puts it, 'no less than unity' (Solow, 1974, 41). What this means is simply that as natural capital is transformed into man-made capital there is at least no loss of value. This assumption is controversial and whether or not it holds in the real world is in many ways the locus of the weak/strong sustainability debate. Notably, Solow himself thought it did indeed hold and describes it as 'the educated guess at the moment' (Solow, 1974, 41).

Given these assumptions and some further conditions such as that there is no population growth and no technological development Solow shows the value increase of natural resources that follows from them becoming ever more scarce offsets the fact that we have less of it. The value of natural resources will approach infinity as they approach nil. As long as we see to it that value is not lost in the substitution we should be able to, at least, hold consumption at a steady level indefinitely.

The contention then becomes, if this is to be fitted to the issue of sustainability, that sustainable economies are quite possible and if we can only keep population and technology in check we can rely on self-regulatory functions of economic systems to ensure resources will never be completely depleted. Needless to say it is a controversial suggestion, especially among ecologists and ecological economists. I will however not attempt to evaluate its validity here. It should be fairly obvious however that the reasoning, if taken all the way, excludes many other analyses of what sustainability really amounts. Even given this analysis there is still room for interdisciplinarity. For example, one may wonder about the prospects of realising the conditions that Solow presupposes in his analysis. How do we cease to develop technologically, or what kinds of society have no population growth. Those problems are not necessarily the domain of economics (although some economists would probably disagree). However, the initial analysis of sustainability is narrow and contentious to begin with and a case could be made that it excludes relevant perspectives at the very outset.

6 Conclusions

I have in this paper tried to make a contribution to the literature on imperialism by showing that there are two main reasons brought up in the literature, on why we imperialism is problematic. One concerns failed expansion, the other wrongful replacement. I have argued that these are independent from one another and that replacement appears to be the most serious. Moreover I have given arguments to the point that it is only really when we specify the context in which replacement takes place that it makes much sense to talk about.

References

Adger, N. (2000). Social and ecological resilience: are they related? *Progress in Human Geography*, **24**.

Andersen, H. and Wagenknecht, S. (2013). Epistemic dependence in interdisciplinary groups. *Synthese*, **190**, 1881–1898.

Becker, G. (1991). *A treatise on the family*. Harvard University Press, enlarged edition edition.

Clarke, S. and Walsh, A. (2009). Scientific imperialism and the proper relations between the sciences. *International Studies in the Philosophy of Science*, **23**, 195–207.

Clarke, S. and Walsh, A. (2013). Imperialism, progress, developmental teleology, and interdisciplinary unification. *International Studies in the Philosophy of Science*, **27**(3), 341–351.

Davidson, D. (2010). The Applicability of the Concept of Resilience to Social Systems: Some Sources of Optimism and Nagging Doubts. *Society & Natural Resources*, **23**(12), 1135–1149.

Dupré, J. (1996). Against scientific imperialism. In M. Forbes, D. Hull, and R. M. Burian, editors, *PSA 1994: Proceedings of the 1994 Biennial Meeting of the Philosophy of Science Association*. East Lansing, MI: Philosophy of Science Association.

Dupré, J. (2001). *Human nature on the limits of science*. Oxford University Press.

Grüne-Yanoff, T. (2011). Models as products of interdisciplinary exchange: Evidence from evolutionary game theory. *Studies in History and Philosophy of Science*, **25**, 119–137.

Gunderson, L. and Holling, C., editors (2002). *Panarchy: understanding transformations in human and natural systems*. Island Press.

Gutés, M. C. (1996). The concept of weak sustainability. *Ecological Economics*, **17**, 147–156.

Hansson, B. (1999). Interdisciplinarity: for what purpose? *Policy Science*, **32**, 339–343.

Holling, C. (1973). Resilience and stability of ecological systems. *Annual Review of Ecology and Systematics*, **4**, 1–23.

Holling, C. and Gunderson, L. (2002). Resilience and adaptive cycles. In L. Gunderson and C. Holling, editors, *Panarchy: understanding transformations in human and natural systems*. Island Press.

Holling, C., Gunderson, L., and Ludwig, D. (2002). In quest of a theory of adaptive change. In L. Gunderson and C. Holling, editors, *Panarchy: understanding transformations in human and natural systems*. Island Press.

Hornborg, A. (2013). Revelations of resilience: From the ideological disarmament of disaster to the revolutionary implications of (p)anarchy. *Resilience. International Policies, Practices and Discourses*, **1**.

Jerneck, A. and Olsson, L. (2008). Adaptation and the poor: development, resilience and transition. *Climate Policy*, **8**(2), 170–182.

Kidd, I. J. (2013). Historical contingency and the impact of scientific imperialism. *International Studies in the Philosophy of Science*, **27**(3), 315–324.

Midgley, M. (1984a). Reductivism, fatalism and sociobiology. *Journal of Applied Philosophy*, **1**(1), 107–114.

Midgley, M. (1984b). Sociobiology. *Journal of Medical Ethics*, **10**, 158–160.

Mitchell, S. D. (2002). Integrative pluralism. *Biology and Philosophy*, **17**, 55–70.

Mitchell, S. D. (2003). *Biological Complexity and Integrative Pluralism*. Cambrige University Press.

Mitchell, S. D. (2009). *Unsimple Truths: Science, Complexity, and Policy*. University of Chigaco Press.

Mäki, U. (2009). Economics imperialism: Concept and constraints. *Philosophy of the social sciences*, **39**.

Mäki, U. (2013). Scientific imperialism: Difficulties in definition, identification, and assessment. *International Studies in the Philosophy of Science*, **27**, 327–341.

Mäki, U. and Marchionni, C. (2010). Is geographical economics imperializing economic geography? *Journal of Economic Geography*.

Schmidt, J. C. (2008). Towards a philosophy of interdisciplinarity. *Poesis Prax*, **5**, 53–69.

Solow, R. (1974). Intergenerational equity and exhaustible resources. *The Review of Economic Studies*, **41**, 29–45.

Stillman, C. S. (1955). Academic imperialism and its resolution: the case of economics and anthropology. *American Scientist*, **43**, 77–88.

Thorén, H. (2014). Resilience as a unifying concept. *International Studies in the Philosophy of Science*, **28**, 1–22.

Thorén, H. and Persson, J. (2013). The philosophy of interdisciplinarity: sustainability science and problem-feeding. *Journal for General Philosophy of Science*, **44**, 337–355.

Walker, B., Holling, C., Carpenter, S., and Kinzig, A. (2004). Resilience, adaptability and transformability in social-ecological systems. *Ecology and Ssociety*, **9**.

Paper IV

Stepping Stone or Stumbling Block?

Mode 2 Knowledge Production in Sustainability Science

Henrik Thorén[1, 2] and Line Breian[3]

[1] Dept. of Philosophy, Lund University, Sweden
[2] LUCID, Lund University Centre of Excellence for Integration of Social and Natural Dimensions of Sustainability, Sweden
[3] Department of Philosophy, Linguistics, and Theory of Science, Gothenburg University

Publication Status: Submitted

Abstract

The concept of Mode 2 was developed in order to further our understanding of processes of knowledge production taking place between and beyond disciplinary structures (inter- and transdisciplinary processes) and "in a context of application". The concept has often been seen as especially applicable to fields addressing grand challenges, such as climate change, poverty eradication, and global health. Being a relatively new field—interdisciplinary in its approach, and focused on addressing such issues—sustainability science would appear to be a case in point. The aim of this paper is twofold: 1) to explore the perceived relation between Mode 2 and sustainability science, and 2) to advance the discussion of Mode 2 from a philosophical perspective. To address these questions we focus on three characteristic features of Mode 2: the notion of a distinct, but evolving framework; boundary crossing; and a problem solving capacity "on the move". In particular we discuss the descriptive and normative implications of Mode 2 and different understandings of the Mode 2 framework. We report the results of a survey carried out amongst leading sustainability scientists in which they answered questions on Mode 2 and sustainability science. The survey gives insight into both their perception of Mode 2 and their perception of their own field of sustainability science, as well as on the relation between the two. In our analysis, we emphasize the free text answers. These reveal a tension within the field of sustainability science; with developments both towards Mode 1 and Mode 2 science as well as towards a more unificationist interpretation. To further complicate the picture, there would also seem to be a tension in the interpretations of Mode 2, and we conclude that the implementation of inter- and transdisciplinarity is challenged by institutional and conceptual factors alike. Even though it is not impossible to achieve; inter- and transdisciplinarity seem to represent a great challenge in itself and the answer as to whether or not inter- and transdisciplinarity is the (sole) solution to the grand challenges of today is likely to be more complex than is generally acknowledged.

Keywords: Mode 2 knowledge production, transdisciplinarity, sustainability science

1 Introduction

The notion of Mode 2 knowledge production was introduced two decades ago in the book *The New Production of Knowledge* (Gibbons *et al.*, 1994). The authors argued that a new mode of knowledge production had emerged.[1] The label "Mode 2" was introduced to designate this kind of knowledge production, and we shall follow this usage in the present paper. Mode 2 was supposed to be "different in nearly every respect" from traditional, disciplinary science (Gibbons *et al.*, 1994, vii), breaking with disciplinary boundaries and the academy/society distinction upheld in traditional Mode 1 research.

The concept of Mode 2 research was designed in particular to further our understanding of enquiries in which researchers from different disciplinary perspectives come together to work on problems "in a context of application" (Gibbons *et al.*, 1994, 3). The Mode 2 concept has been much debated and also criticized (see e.g. Weingart, 1997; Pestre, 2000, 2003; Hessels and van Lente, 2009, 2010).

Mode 2 knowledge production has gained a substantive following in different fields of research—such as land use science, nursing science, and sustainability science (see e.g. Nowotny *et al.*, 2003)—where it has been thought of as a model upon which a new science may be built. This has been seen as especially interesting in fields aimed at so-called 'grand challenges' such as global warming, poverty eradication, global health and development. One such field, to which the present paper is devoted, is sustainability science. Sustainability science is an interdisciplinary field aimed at producing knowledge to help guide society in the transition towards sustainability. This task, it is widely believed, requires both

[1] The idea was developed further in Nowotny *et al.* (2001) but apart from some influential notions such as that of socially robust knowledge and the Agora that volume would seem to have had much less influence.

interdisciplinary integration (especially across the natural/social science divide) and the integration of scientific and non-scientific knowledge (Kates *et al.*, 2001; Hirsch Hadorn *et al.*, 2010; Klein, 2014; Jerneck *et al.*, 2011). An alleged shift to Mode 2 has indeed been held to explain the emergence and practice of inter- and transdisciplinary fields like sustainability science (Martens *et al.*, 2010).

We present the results of an empirical study in which a number of sustainability scientists answered a survey concerning the relationship between sustainability science and Mode 2. We were interested both in overlaps of central features, such as boundary crossing and the development of new and evolving frameworks, as well as the way in which practitioners reason about these issues more explicitly. The primary aim has been to investigate whether Mode 2 is applicable to sustainability science. Our results appear to indicate that despite the conception that sustainability science is, and indeed should be, Mode 2, some nagging issues remain. Sustainability science appears to be on a dual track showing tendencies both associated with Mode 1 and Mode 2 science. In the final part of the paper, relying also upon a philosophical analysis of the concept of Mode 2, we try to explain this mixed picture,. We suggest that one should expect realizing Mode 2 to be challenging for several reasons; some relate to well-known practical issues—such as the apparent resilience of traditional academic structures—others pertain to concerns of a more conceptual nature. In particular, the notion of Mode 2, as conceived by Gibbons *et al.* in their influential (1994) book, is largely silent about the mechanisms involved in knowledge production and focus, rather, on surface phenomena such as the shift away from traditional publication patterns and the tendency of research to come out of temporary, and highly diverse groups. However, neither abandoning traditional publication patterns nor assembling such groups guarantee (or even make likely) epistemic success. We furthermore identify the ambiguous notion of a theoretical framework as an obstacle to overcome the perceived gap between theory and practice within transdisciplinary research in general and Mode 2 research in particular. The normative poverty of Mode 2, we suggest, is a potential source of problems and as sustainability scientists move to try to implement the notion in practice. An alternative would then seem to apply another theory of new knowledge production such as that offered by Post-Normal science. Indeed this is suggested by some of the free text answers. In the following we shall argue that Mode 2 is in actual fact of questionable applicability to sustainability science, at least as that science has developed in recent years, and that there is therefore an interesting tension between the perceived and actual applicability of Mode 2 to most of the research conducted within sustainability science.

2 Mode 2 Knowledge Production

When Michael Gibbons, Helga Nowotny and their colleagues introduced the notion of knowledge production in Mode 2 in the mid-1990s their aim was to draw the attention to a certain changes in the way science was being practiced. They perceived of a shift away from a traditional 'mode' of producing knowledge dominated by university departments and scientific journals to something new—a form of knowledge production distributed among more diverse set of producers. Actors outside of academia—think tanks, NGOs, private institutions and industrial laboratories—appeared to challenge the established academic structures and their privileged role. Researchers were also behaving somewhat differently. Heterogeneous groups of individuals would come together and carry out research in closer proximity to where the knowledge was eventually to be applied; knowledge was now being produced in, as Gibbons *et al.* express it, "a context of application" (Gibbons *et al.*, 1994, 4).

Traditional science—described as "Mode 1" knowledge production by Gibbons *et al.* (1994)—has a number of characteristics. University departments are regarded as the main sites of knowledge production. Scientific output is funnelled through peer-reviewed journals where disciplinary values are safe-guarded and as individual scientists are promoted according to their ability to publish, this makes conforming to these values necessary. Traditional science, on this account, is conservative, socially detached, and "disciplinary" (Gibbons *et al.*, 1994, 1). Mode 2 knowledge production is the successor to Mode 1 and it is understood to be, in many respects, the inverse of Mode 1:

> [...] in Mode 1 problems are set and solved in a context governed by the, largely academic, interests of a specific community. By contrast, Mode 2 knowledge is carried out in a context of application. Mode 1 is disciplinary while Mode 2 is transdisciplinary. Mode 1 is characterised by homogeneity, Mode 2 by heterogeneity. Organizationally, Mode 1 is hierarchical and tends to preserve its form, while Mode 2 is more heterarchical and transient. Each employs a different type of quality control. In comparison with Mode 1, Mode 2 is more socially accountable and reflexive. It includes a wider, more temporary and heterogeneous set of practitioners, collaborating on a problem defined in a specific and localized context. (Gibbons *et al.*, 1994, 3)

It is important to note that although Mode 2, at least in the form presented in Gibbons *et al.* (1994) is first and foremost a descriptive notion, it implies changes in both the social and institutional features of science as well as its cognitive or epistemic components (see e.g. Nowotny *et al.*, 2001, Ch 12).

A very important part of Mode 2 knowledge is some form of transdisciplinarity. It is described as the "privileged form of knowledge production in Mode 2" constituting a "movement beyond disciplinary structures in the constitution of the intellectual agenda, in manner in which resources are deployed, and the ways in which research is organised, results are communicated and the outcome evaluated" (Gibbons *et al.*, 1994, 27). Transdisciplinarity is both a term with a considerable pedigree in the literature on interdisciplinarity and one that has been associated with many different meanings (see e.g. Klein, 2010, 2014). In this paper we will be concerned mainly with Mode 2 in its transdisciplinary forms and thus it is helpful to contemplate briefly on the origins of the concept of transdisciplinarity, not least since Gibbons and his colleagues appear to elaborate with an alternative understanding of this notion.

The origin of this concept is usually attributed to Eric Jantsch (1972). Jantsch thinks of transdisciplinarity as the final step in a ladder of integration that has disciplinarity at its bottom. According to Jantsch, what distinguishes the different steps—multi-, pluri-, inter-, and trans-disciplinarity—is the level of integration or coordination between (elements of) the disciplines involved. For instance, multidisciplinarity does not involve cooperation; pluridisciplinarity involves cooperation without coordination; interdisciplinarity implies coordination by higher-level concept; and transdisciplinarity requires multilevel coordination of the "coordination of all disciplines and interdisciplines in the education/innovation system on the basis of a generalized axiomatics" (Jantsch, 1972, 16). Jantsch's notion of transdisciplinarity involves the coordination of the entire system "toward a common goal" (Jantsch, 1972, 18). Importantly Jantsch also suggests that in transdisciplinarity, as opposed to some lesser forms, implies the "mutual enhancement of epistemologies in certain areas"(Jantsch, 1972, 17).

Gibbons *et al.* (1994) identify four "distinct features" of Mode 2 transdisciplinarity (Gibbons *et al.*, 1994, 5). The transdisciplinary character of Mode 2 thus implies: (a) the development of a distinct, but evolving, framework; (b) a contribution to knowledge, but not necessarily to disciplinary knowledge – or, more generally, to any one particular body of knowledge ; (c) a dissemination of results primarily directed at the participants of the project generating the results; and (d) a problem-solving capacity which is "on the move" rather than tied to particular kinds of inquiry. Our survey is primarily directed at three of these features, (a), (b), and (d), with particular focus on (a) and (b). We will, nonetheless briefly discuss dissemination as well. These four features are partially overlapping. As we understand it, the development of new frameworks and what we will call boundary crossing (what is stated in (b)) are conceived of as particularly important, although no single feature of the three should be thought of as essential. The feature of having a distinct framework is, we think, more intriguing (but possibly less well recognized) than boundary-crossing.

What is stated in (b) is one way to formulate the sense in which Mode 2 is boundary crossing. As Gibbons and his colleagues explicitly claim, the knowledge Mode 2 creates is not bound to a particular discipline. This need not be thought of as a radical break with disciplinarity, but their claim has further implications. To the extent that knowledge acquisition is the result of a process in which particular problems are formulated, (b) implies that the problems Mode 2 researchers work on are not specific to a certain discipline nor need they belong to any discipline at all. It is thus not only the results of Mode 2 research that cross boundaries; by implication, the problems Mode 2 researchers work on are not problems worked on only within single disciplines, or any discipline, either. This interpretation coheres with the claim in (d). According to (d), Mode 2 results in a problem-solving capacity that is not tied to particular kinds of inquiry. Moreover, this is not the only place – nor the only way – Nowotny and her colleagues refer to different aspects of boundary-crossing in connection with Mode 2. As Nowotny *et al.* (2001) put it, society and science co-evolve.

Let us now return to the notion of the framework. Although Eric Jantsch and his contemporaries never discussed 'frameworks' directly, they did, however, operate with a substantive notion of the theoretical prerequisites and aims of transdisciplinarity. Echoing the ideals of the logical empiricists, their concept of transdisciplinarity brought together the whole of science under a common set of axioms (e.g. see Apostel et al. 1972, 25f). Mode 2 transdisciplinarity also implies the development, or evolution, of new frameworks: in the words of Gibbons *et al.* it "develops a distinct but evolving framework to guide problem solving efforts" (1994, 5) but they appear to be relevantly different from Jantsch's vision.

First, they emphasize the local, temporary, and transient character of knowledge, and lack the global ambitions of their forerunners. Unification is neither valued in itself, nor, as were, nurtured should it be achieved. Second, Mode 2 highlights the blending, not merely of scientific disciplines, but also of science and (some types of) non-science. The aims of Mode 2 science is practical in the sense that problem-solving is absolutely central. Furthermore there is a strong practical orientation to Mode 2 that is embodied in the close connection between Mode 2 and concrete problem-solving. The frameworks that Gibbons *et al.* make reference to are also practical—Gibbons *et al.* sometimes speak of a "framework of action" that "involves the integration of different skills" (Gibbons *et al.*, 1994, 4).

However, although the practical nature of frameworks that Mode 2 science implies is often underlined, they are not without theoretical components. First, the outcome of Mode 2 science may well be an improved understanding or better theories (although it should be emphasized that developing theories is rarely, if ever, the sole aim of Mode 2). Second, the process itself may be instigated or driven by theoretical concerns:

> Knowledge production will be guided by theoretical

> considerations as well as by the limitation of experimental methods. And though it takes its starting point from intellectual frameworks of all those who participate in the search, it soon leaves them behind to follow new paths. Over time a new framework, a Mode 2 framework, will evolve – for example, the basic architecture will be hit upon. It will be different from any of the constituent frameworks, yet could not have been developed without them. (Gibbons *et al.*, 1994, 30)

And third, as Nowotny et al. point out, it will minimally involve "the mobilization of a range of theoretical perspectives and practical methodologies to solve problems" (Nowotny *et al.*, 2003, 186). So even if Mode 2 transdisciplinarity is theoretically minimal in comparison to Jantsch's transdisciplinarity, it is apparently not entirely without theoretical substance. A Mode 2 framework is theoretical at least in the sense that, at some level, it has to involve ideas concerning how e.g. theories, methods, or approaches relate to, and inform, one another, in order to solve the problem at hand. When we discuss the *theoretical* frameworks of Mode 2 science we will discuss them in these terms. Notably, one should perhaps not expect the theoretical components of Mode 2 science frameworks to always be made explicit. One reason is that they are not valued (as they are in Mode 1 science). Another is that they may not be necessary in order to validate the solution to the problem, since Mode 2 science first and foremost targets practical problems. A third reason is that dissemination is not supposed to happen through traditional channels such as publication.

There are further differences between the Jantschian and Mode 2 understandings of Mode 2. The glossary attached to Gibbons *et al.* (1994, 168) states:

Transdsiciplinarity: Knowledge which emerges form a particular context of application with its own distinct theoreti-

> cal structures, research methods and modes of practice but which may not be locateable on the prevailing disciplinary map.

That Gibbons *et al.* choose to include the notion of a context of application into the very definition of transdisciplinarity appears to be a breach with older conceptions. Jantsch mentions nothing of the kind, and furthermore, whereas Jantsch emphasize unity and interdependence in the form of a common axiomatics Gibbons *et al.*, rather, underscore theoretical, practical, and methodological independence. One way of understanding this is that traditional transdisciplinarity is globally unificationist whereas the Mode 2 variety is globally pluralist.

Both these understandings do, however, leave out an explication of processes of integration and of problem formulation. How are these processes to be undertaken? It seems to be open to interpretation whether or not the Mode 2 theorists argue in favour of full or partial integration. Another concern that is not sufficiently attended to is that, as Nowotny *et al.* (2001) and also Russell E. Vance *et al.* (2008) point out, there is a fundamental problem with power structures., within Mode 2, but also within other theories of transdisciplinarity, a problem which would seem to be closely tied up with an understanding, or a development, of a (theoretical) framework, but which is nevertheless relatively absent from the theory of Mode 2.

As Russell et al. seem to emphasize, it is easier to describe these processes in a superficial way, than actually addressing the full complexities of the issues involved. Issues of integration and of power balance/power struggle are further complicated when the processes include, not only different disciplinary forms of knowledge, but also other kinds knowledge, such as indigenous or lay knowledge.

Sustainability science is often characterized by its transdisciplinary character, and any theory that would be applicable to the field would therefore to a certain extent be expected to address these issues in a substantial way. The difficulties in overcoming institutional, cognitive, and epistemic obstacles to inter- and transdisciplinary processes, may be reasons why these processes in themselves may be perceived as grand challenges.

3 Methods and Materials

In order to get a better understanding of the way inter- and transdisciplinary researchers in the field of sustainability science perceive of their own science and of Mode 2, we carried out an empirical study. The study consisted of a survey and focused on the three features identified in the previous section. The survey aimed at eliciting information about the relations (or lack thereof) between sustainability science and Mode 2, but because of the separate questions and the option of free text answers it gave insights into several aspects of the way the sustainability scientists perceived both of their own field as well as of Mode 2. The questions and the rating may be ambiguous as to whether respondents are to answer by saying how they perceive sustainability science as it is now or by saying how they think it should be. This is an important distinction and one that is characteristic of many of the discussions relating to transdisciplinarity; the tension between the ideal and the actual.

In May 2013, a questionnaire was sent to sustainability scientists we had extracted from two lists: the list of participants in the AAAS Forum: Science and Innovation for Sustainable Development and a list of participants in a workshop held by National Center for Socio-Environmental Synthesis (SESYNC) in 2012. The majority of these sustainability scientists are internationally known. Besides questions related to our research, the questionnaire offered respondents an opportunity to add a maximum of ten sustainability scientists to the pool of potential respondents to our questionnaire. These added lists generally consisted of equally well-known names in sustainability research. The same questionnaire was then circulated to newly identified scientists, and the process was iterated a couple of times before being terminated in August 2013. Including the scientists on our initial lists, a total of 266 individuals had at that point been suggested as possible respondents, and 77 sustainability scientists had answered the whole, or parts, of the questionnaire. There may be issues with this sampling method, but because sustainability science is both a rather recent, and thus immature, field, and explicitly inclusive and pluralist, targeting subjects for the questionnaire by, say, first producing some demarcation criteria for sustainability science would probably have been less effective. Not only would such criteria doubtless be arbitrary and somewhat artificial, but in formulating them the inquirer would risk projecting his or her preconceptions of the field on to the sample.

As we particularly wanted to find out whether the sustainability scientists conceived of what they do in the very same terms as they understand Mode 2; the questionnaire contained several statements with a bearing on this issue, in particular we mirrored the questions about Mode 2 To the extent that contemporary sustainability scientists conceive of what they do in the same terms as they understand Mode 2, they should respond to the two types of mirroring statements (about Mode 2 and sustainability science, respectively) in the same way.

Except for the two first questions and the free text questions (q4c, q6c and q9), respondents were given statements and asked to indicate, on scale from 1 to 7, the degree to which they agreed with those statements. The questionnaire contained the following questions and statements:

q1) Do you consider yourself a sustainability scientist?

q2) Are you familiar with the notion of Mode 2 research (or knowledge production in Mode 2)? The notion was introduced by Gibbons, Nowotny, and their colleagues in the 1990s

q3) Mode 2 research as you understand it is common in sustainability science

q4a) Mode 2 research, as you understand it, is primarily problem-solving

q4b) Sustainability science is primarily problem-solving

q4c) Can you provide a typical problem-solving example from sustainability science?:

q5a) Mode 2 research, as you understand it, focuses exclusively on socially relevant problems

q5b) Sustainability science focuses exclusively on socially relevant problems

q6a) Mode 2 research, as you understand it, implies new theoretical frameworks

q6b) Sustainability science implies new theoretical frameworks

q6c) If possible, provide an example of a new theoretical framework in sustainability science and the applied context to which it applies and was generated in

q7a) Mode 2 research, as you understand it, integrates researchers from different disciplines

q7b) Sustainability science integrates researchers from different disciplines

q8a) Mode 2 research, as you understand it, involves participants from outside the academia

q8b) Sustainability science involves participants from outside the academia

q9) Can you please say something more about your perception of Mode 2 in relation to sustainability science

What is striking is the number of respondents, 51 (out of 77), who felt sufficiently knowledgeable concerning the notion of Mode 2 to be able to answer the questions specifically concerned with the notion , while 71 (out of 77) answered the questions concerning sustainability science. The relatively low number (51 out of 77) who knew enough about Mode 2 could be seen to confirm the perceived gap between theory and practice within transdisciplinary research (MacMynowski, 2007; Zscheischler and Rogga, 2015; Defila and Di Guilo, 2015).

In the further analysis mean values and standard errors of means were calculated for the quantitative data (q4-q8). The questions that are answered in free-text, and in particular the last one, are openly formulated. It is hence not surprising that the answers we received are quite varied and one should keep in mind that the data obtained from these answers cannot support strong conclusions.

4 Results and Discussion

Within sustainability science there is an apparent interest for notions such as Mode 2. The focus on involving stakeholders in the process of both problem formulation and knowledge production has a strong appeal and caters to the idea of bridging science and society that is prevalent within sustainability science (see e.g. Jerneck *et al.*, 2011). Often Mode 2 is taken is a normative concept that may well help reach these goals.

> The new paradigm [sustainability science] must be able to encompass different domains (ecology, economy, social, cultural) and dimensions (time and space), different magnitudes of scales (time, space, and function), multiple balances (dynamics), multiple actors (interests), and multiple failures (systemic faults). This new

> paradigm emerges from a scientific subcurrent that characterizes the evolution of science in general – a shift from mode-1 to mode-2 science. (Martens *et al.*, 2010, 295)

Neither the suggested connection between Mode 2 and sustainability science in particular, nor the general claim that science moves toward Mode 2, is self-evident. Thus in our empirical study we wanted to examine this claim.

To many of the respondents Mode 2 research were indeed seen as common in sustainability science (q3; mean value 4.9, on the 1-7 scale) The quantitative data in Table 1 show that there is strong agreement concerning boundary crossing with integration of researchers from different disciplines and participants from outside academia as an essential feature of Mode 2 (q7a and q8a). These questions are generally answered in the affirmative (mean value 5.7 and 5.8 respectively). It should be noted that the free-text answers reveal a slightly more complicated relation, particularly in relation to participation from outside academia.

The table furthermore indicate that both Mode 2 research and sustainability science is understood to be primarily problem-solving (q4a), focus on socially relevant problems (q5a), and involve participants from outside academia (q8a) (mean value 4.8 for all three on the 1-7 scale).

In the section that allowed for free-text answers it is interesting to note that the respondents mentioned several different theoretical frameworks employed by sustainability scientists. Some of them involve unique components whereas others are new in the way the components are put together. Those most frequently occurring were transition theory, resilience theory and systems theory; others included the framework of ecosystem services and SPEED:

> The SPEED framework (socio-political evaluation of

Perceptions of Mode 2	Mean value ± standard error of mean
q4a) Mode 2 research is primarily problem-solving ($n = 50$)	4.76 ± 0.21
q4b) Sustainability science is primarily problem-solving ($n = 70$)	4.42 ± 0.20
q5a) Mode 2 research focuses exclusively on socially relevant problems	4.80 ± 0.24
q5b) Sustainability science focuses exclusively on socially relevant problems	4.45 ± 0.20
q6a) Mode 2 research implies new theoretical frameworks ($n = 52$)	4.83 ± 0.24
q6b) Sustainability science implies new theoretical frameworks ($n = 71$)	5.38 ± 0.18
q7a) Mode 2 research integrates researchers from different disciplines ($n = 51$)	5.67 ± 0.21
q7b) Sustainability science integrates researchers from different disciplines ($n = 70$)	6.27 ± 0.15
q8a) Mode 2 research involves participants from outside the academia ($n = 50$)	5.82 ± 0.20
q8b) Sustainability science involves participants from outside the academia ($n = 71$)	5.87 ± 0.16

Table 1: Results from the respondents that claimed to be familiar with Mode 2 notion and their scores to the five statements related to Mode 2. The scores are given as mean value with standard error of mean. The scale is from 7 to 1, where 7 means strongly agree and 7 strongly disagree to the given statement (n =number of responses).

> energy deployment) is a new framework that provides a way to assess contributions of different kinds of factors influencing energy technology advancement. This framework integrates cultural and environmental considerations in addition to the predominant tendency to primarily acknowledge economic and technical factors (Respondent 38).

Although several of the examples of theoretical frameworks highlight the local nature of frameworks and approaches within sustainability science there are a few anomalies that hint of other tendencies. One is resilience theory, a unificationist endeavour that is very much driven by developing a theoretical superstructure which continually organize disciplines in a more or less fixed way (Thorén, 2014). Large scale theoretical integration is more reminiscent of Jantsch's transdisciplinarity than the Mode 2 variety.? One finds traces of this ideal elsewhere as well. Consider a large Swedish research program in sustainability science: Lund University Centre of Excellence for the Integration of Social and

achievement \ time	Phase 1 (year 1-2)	Phase 2 (year 3-5)	Phase 3 (year 6-10)
Theoretical development	Multidisciplinarity. Researchers from different disciplines co-operate across their own theories and perspectives	Interdisciplinarity. Ontological approaches are questioned resulting in a co-evolution of theories '*sustainability science*'	Transdisciplinarity. Theories evolve and mature to gradually incorporate more domains and transcend the boundary between science and practice
Methodological development	Different disciplines apply their own tools and methods on common research problems	Tools and methods for sustainablity science evolve along with theories. Participatory approaches are increasingly important	Research increasingly follows participatory and goal oriented approaches while being reflexive. Integration of external stakeholders in the research
Organisational development	LUCID is an arena where different disciplines meet but are not integrated. Some researchers are physically at LUCID, some at their home departments	Closer interaction between researchers requires common premises. PhD programme in sustainability science is established	Disciplines external to LUCID are increasingly involved. The Centre of Excellence is instrumental in structural renewal of many knowledge fields related to sustainability
Educational development	Close interdepartmental cooperation in related but independent Masters programmes	Master programmes covering different aspects of sustainability (social, economic, environmental) becomes more and more integrated	Apart from Master programmes in sustainability science, many other fields adopt sustainability as a major component resulting in several related and co-operating programmes

Table 2: LUCID proposal.

Natural Dimensions of Sustainability (LUCID) is a Linnaeus program sponsored by The Swedish Research Council Formas for the period 2008–2018. Linnaeus Grants are awarded to exceptionally strong research environments performing research of the highest international quality and aiming at innovative research. The LUCID research proposal explains how collaboration within the program will develop over a period of 10 years.

According to the plan LUCID's interdisciplinary phase will be "resulting in a co-evolution of theories for sustainability science"; finally, in the transdisciplinary phase, "theories evolve and mature to gradually incorporate more domains and transcend the boundary between science and practice" (Jerneck *et al.*, 2011, 79). The conceptual, or theoretical, integration in the interdisciplinary phase is reminiscent of the picture painted by Jantsch. And in a recent mid-term evaluation of the program, it is stated that the conception of inter- and trans-disciplinarity the program is working with is Jantschian in kind (Olsson, 2014). On the other hand, some respondents claimed—quite in accordance with what should be expected from a Mode 2 venture—that the process is as important as the results themselves. The temporary character and the emphasis on the process of knowledge production within sustainability science were captured by responses such as the following;

> A new framework is, that science doesn't have the monopoly on knowledge or truth. For sustainable development it is necessary to integrate different kinds of knowledge. This also means that the process of developing a solution is as important as the results and the latter deeply depends on the former (Respondent 22).

Furthermore, in the eyes of many respondents the normative character of sustainability science implies a context-specific (local) dimension:

> Sustainable development is a normative concept. This means that sustainability needs to be defined through a negotiation process and is therefore necessarily context specific (Respondent 62).

However, the answers also provide important information about the complexity of the issue which seems to weaken the link between sustainability science and Mode 2. To begin with, it should be noted that the tension we observed above concerning framework and theory; i.e. what the theoretical component of a practical framework with an emphasis on knowledge production in a context of application should be, is also perceived by some of our respondents:

> Is it theory or is it a framework. Ostrom (200?) has argued these are different, a framework might use more

> than one theory. I would say, however, that sustsci would use one or more than one theoretical framework that fits the problem, topic or issue the research is addressing (Respondent 15).

The answer would seem to highlight a practical rather than a strict theoretical interpretation of framework and also an ambiguity regarding different understandings of theory and framework; what is the distinction between a theory and a (theoretical? practical?) framework? Moreover respondent 15 highlight another important point; a pragmatic and pluralistic perspective on science. This tendency is significant as it points in the opposite direction from a more traditional, unificationist, conception of transdisciplinarity.

Secondly, the extent to which the framework has to be new or evolving, troubles our respondents when reflecting on whether sustainability science implies new frameworks. This is reflected in the free text answers given by the respondents. Several respondents maintain that there is no need to create new frameworks as there are already many useful ones out there that could be re-applied and re-contextualized. An interesting question here is whether or not a re-contextualized framework could be counted as an evolving framework. This would seem as a tenable interpretation and sustainability thus, in the view of some respondents, does not necessarily require new frameworks.

> Sustainability science, in my opinion, is not so much towards the development of new theoretical frameworks as much as the re-application and contextualization of existing theoretical frameworks (Respondent 42).

Although re-contextualization is a promising way of thinking about the sense in which new and evolving is meant to be taken the down side is that it calls into question exactly in what way Mode 2 is different from ordinary science. An argument could be made that even Kuhninan normal science seems to operate in this way. Already tried and tested solutions are re-deployed to new problems with some modification (Kuhn, 1970, 1977). A particular way of going about solving problems (a framework?) is then re-contextualized in a sense.

In all probability Gibbons *et al.* mean to suggest that such a re-contextualization is more thorough than whatever it is that happens within Kuhnian normal science, but the comparison is nonetheless potentially damaging. After all, Mode 2 often appears to be understood as an alternative to precisely that kind of conservative practices. This also exposes another general weakness of the notion of Mode 2. To the extent that it is a normative concept—minimally this is how the notion is perceived—Gibbons at al. make no attempt at relating it other normative accounts. We are thus left largely in the dark—save for a few sweeping remarks on the "Newtonian model of science" (Gibbons *et al.*, 1994, 167)—on how, precisely, knowledge is produced. We will return to this point below.

Furthermore, there are respondents who questioned the very potential of sustainability science to provide new frameworks and to engage in new processes of knowledge production. These respondents argue that sustainability science does not succeed in creating Mode 2 knowledge due to personal, (historical) or institutional constraints.

> I think sustainability science intends to (and should) help provide new frameworks and applied work, but that in practice results are more traditional (mode 1) (Respondent 19).

> Sustainability science implies new theoretical frameworks, such as "action research," "problem driven," solution oriented," "co-generation," etc. However, these

> are merely implications that fail to come to fruition because of the comfort and complacency of academic researchers, the pull of tenure and status requirements (publications not solutions), and the lag of funding bodies behind the aggressive potential of Mode 2 sustainability science (Respondent 36).

The resistance by regular academic structures should probably not be underestimated, as respondent 36 points out. By diverting efforts from traditional ways of, for instance, knowledge proliferation, one runs the risk of making oneself academically invisible.

There is an apparent tension between—or possibly a parallel development of—Mode 1 and Mode 2 and the co-mingling of different institutional logics examined by Swan *et al.* (2010) in relation to a longitudinal study of a policy intervention in the UK aimed at promoting a logic of knowledge production in genetics science. They conclude that in the process they:

> We did not see, then, a replacement of one mode of knowledge production with another but, rather, a 'co-mingling' of logics whereby well established modes of operating co-existed with new, more collaborative and interactive ways of working. (Swan *et al.*, 2010, 1332)

This parallel development/co-mingling is evident also within our survey. Not only do respondents point to the tendency of sustainability scientists to divert back to mode 1, but there are also claims to the point that sustainability can be either Mode 1 or Mode 2 (respondent 10) and that Mode 2 is "just one part of" sustainability science (respondent 20).

The last question in the survey (q9) prompted respondents to give their views on the relationship between Mode 2 and sustainability science in general. Answers to this question revealed several interesting issues. A number of respondents clearly considered sustainability as an example of Mode 2 science:

> Sustainability science appears to be a perfect example of Mode 2 thinking: problem-solving in real-world situations by applying and adjusting theoretical frameworks developed in academia depending on the real-world, on-the-ground knowledge that is obtained by attempting to solve problems. (Respondent 38)

Furthermore, a common sentiment among the respondents was that sustainability science should be Mode 2.

> ...the widening array of perspectives on [sustainability] issues and the extension and pluralism in methods to understand, interpret, model issues and engage stakeholders in their identification and resolution, these Mode 2 approaches are essential parts of sustainability science in my view. (Respondent 43)

In fact, several respondents describe Mode 2 as a crucial feature of Mode 2. One answer refers to Mode 2 as a "fundamental to sustainability science" (Respondent 17) and another as "a core element of sustainability research in its solution-oriented form" (Respondent 3). Yet another claim that:

> For having next generation sustainability science to drive a local and global sustainability agenda we need to have Mode 2 research as the norm to produce socially relevant and employable research findings" (Respondent 5).

There is a continuum of views, however, and not everyone see Mode 2 as paramount to sustainability science. Another common idea is that Mode 2 represents one approach among many and that sustainability science can proceed along many different paths. One respondent writes that "Mode 2 knowledge production can be a part of sustainability science" (Respondent 25), another that

"Mode 2 is just one part of SS" (Respondent 20), and a third that "SS [sustainability science] addresses complex societal challenges but can be based on either Mode 1, 2 or 3 (generating theory in applied research contexts)" (Respondent 10).

There is clearly a spectrum of views concerning how important Mode 2 is for sustainability science and one can sense subtle differences. Some respondents appear to suggest that Mode 2 is neither necessary nor sufficient, although potentially useful, within sustainability science. Others that it is a necessary component, but not sufficient, and yet others that it is both necessary and sufficient; one respondent even identified Mode 2 and sustainability science with one another (Respondent 27).

Although the general view of Mode 2 is positive some respondents point to perceived weaknesses in the notion or otherwise appear to suggest that sustainability science needs a more encompassing framework. Here are a few examples:

> Mode 2 implies an institutional change in academia towards a closer relationship between knowledge production (primarily scientific) and policy-making and social solving. While it does suggest that knowledge production needs to change to address social problems, it focuses primarily on structural institutional change (e.g. how the university relates to outside partners), but not on the epistemic, social, cultural, and political changes necessary in the science itself in order to fully address and adapt to social problems. Sustainability science is attempting to transform the scientific process (albeit still in its very infancy) such that the policy process itself (with the involvement of multiple stakeholders, including local, civic actors, not just government or businesses), become part of the knowledge production process itself, not such serve as knowledge users.

> Sustainability science, in my view, is also a normative science, where the ethical issues are embedded in the research questions itself, whether Mode 2 production doesn't necessarily address this dimension. (Respondent 7)

> Unfortunately, there are still some loud voices equating Mode 2 and sustainability science. However, most Mode 2 projects I know do not take future generations into account and many many successful sustainability science contributions are far away from Mode 2. (Respondent 24)?

Beyond aforementioned concerns about the difficulties in developing functioning frameworks in a milieu where common ground is, one would have to suppose, scarce, there are also other challenges. The image that suggests itself is one of at least mild confusion with respect to whether sustainability science is, or should be, Mode 2 as well as what Mode 2 knowledge production amounts to. What is quite clear, however, is that most (if not perhaps all) respondents perceive of Mode 2 knowledge production in normative terms; as a model for science. This model may or may not be suitable for sustainability science but many seem to think that it is.

As we have noted above Mode 2, in many respects, appear as a descriptive concept. Nonetheless it is widely perceived of as being normative—a model for a new science beyond narrowly confined disciplines and creative chokehold of traditional science. But problems appear to arise as one tries to implement Mode 2. Why? There are likely several reasons for this, some practical, others of a more conceptual nature. One hypothesis we would like to raise at this junction as a possible partial explanation relates precisely to this normative/descriptive dimensions. The account of Mode 2, especially in the form presented in Gibbons *et al.* (1994)—which is by far the most influential—tends to black box the underlying process through which knowledge is produced. That the assembling of heterogeneous and temporally transient groups is characteristic of Mode 2 tells us nothing about how these groups goes about producing knowledge—just that it is produced. Here the Mode 2 account lacks both theoretical and empirical information. Neither do we get a sense of how Mode 2 knowledge production relates to traditional normative accounts of science—Nowotny et al. make a point of maintaining that the "epistemological core is empty" (Nowotny *et al.*, 2001, 179)—nor detailed case studies that can serve as a model for new situations.

This hints at deeper issues. *If* it were possible to produce either of these two things, that might actually risk undermining the whole account as it, in an important sense, constitutes a departure from precisely the idea that science is a practice that can be captured in such a way. The context sensitivity and flexibility of Mode 2 frameworks and the way in which they evolve out of new situations can be taken to suggest there just is no formula. We can neither depart from theoretical notions of proper science, nor past experience of what has worked in practice. This strikes at the idea of new and evolving frameworks regardless of whether they are theoretical or practical and troublesome from a normative perspective. That is to say, it is problematic if one is looking to Mode 2 as a way of tackling problems. All one can really do, it seems, is to try to put the structural and institutional prerequisites in place and then hope for the best.

Further points could be made concerning the normative and descriptive dimension of Mode 2. As we noted in our introduction to the notion of Mode 2 there are potential difference between Mode 2 and e.g. post-normal science (Funtowicz and Ravetz, 1993). The latter concept is normatively more explicit in seeking a science that goes beyond the limitations of Kuhnian normal and extraordinary science. Mode 2 shares considerably with this idea but is, nonetheless, more complicated to make sense of in this respect. In the review answers we find the normative issue in some disarray. A number of respondents clearly see Mode 2 as a normative concept and a suitable model for sustainability science. Others appear to suggest that the concept lacks precisely a normative dimension and therefore falls short in this precise respect (see e.g. respondents 7 and 24 above). Yet others conflate Mode 2 and post normal science—but of which are suggested to be fundamentally wrongheaded.

> Mode 2 is essentially post normal science as is set in the framework offered by Funtowitz and Ravetz. They neither understood nor addressed sustainability sceince in devising Mode 2 science. Therefore I feel Lund is on a loser in trying to compare the two: and certainly on a loser in any effort to link the two together (which happily I do not think is the case) [sic] (Respondent 11)

So, one reason why confusion may have arisen regarding the normative content of the Mode 2 concept is that Gibbons *et al.* are not particularly clear themselves. Moreover, Mode 2 is strongly associated with terms that have otherwise been understood to be normative and here we are not talking about the conflation of Mode 2 and post-normal science but rather the notion of transdisciplinarity. Transdisciplinarity is a strongly normative notion on most accounts, such as for example Jantsch's, and Gibbons at al's departure from this standard interpretation is perhaps a source of confusion. Not least in fields such as sustainability science where the use of transdisciplinarity is so well established.? Finally, let us now return to the temporary nature of Mode 2 frameworks. Some of the survey answers suggest that the frameworks used in sustainability science are not as temporary as one might expect given the assumption that sustainability science exemplifies Mode 2. This may be taken as a further indication that Gibbons *et al.* may have underestimated the resilience of traditional scientific practice. Two immediate concerns arise in connection to this. One is that, given that it is difficult to develop a framework, not holding on to them appears odd. Successful frameworks should be nursed and developed. Another is that one could argue that, whether or not it is possible to redeploy a framework is a contingent matter. Thus suggesting that new frameworks are necessary is something of an unwarranted a priori statement.

There are several ways in which one may approach these concerns. One might, for example, assume a Popperian stance towards problems. Popper, of course, emphasised novelty and imagination in problem solving and argued one should take care not to conflate problems and disciplines /citep[see e.g.][88]Popper1972. When solving problems we always need to be open to different possibilities and not tether ourselves to the narrow confines of our disciplines prematurely. A problem may turn out to be a problem for single discipline, or for many disciplines, but that should be settled along the way and not before the fact. Although an interpretation of Mode 2 as a kind of neo-Popperianism is tempting in certain ways (though not in others) the standard view of Karl Popper in these circles (to the extent he is at all mentioned) appears to be as a representative of an antiquated view on science.[2]

An alternative route, or anyway, a point of comparison, is with another widely spread notion in sustainability science. A very common idea is that many problems that sustainability science

[2] In Nowotny et al. (2001)citetNowotny2001 Popper or Popperian themes are references to four times (pages 48, 159, 194 and 199). With the exception of one passage the aim is to draw up a contrast between Mode 2 and a Popperian conception. The exception is a passage on page 48 where it is conceded in a discussion on Schumpeter that the "co-evolution" of science and society now means that problems "can no longer be solved" results in a kind of "disjointed and volatile form of Popperian falsifiability" (Nowotny *et al.*, 2001, 48).

deals with are fundamentally different from the kinds of problems science traditionally have approached. These problems are *wicked problems* (Jerneck *et al.*, 2011; Norton, 2005). This notion is due to Rittel and Webber (1973). It is difficult to give a short overview of this rather complicated idea but let us just note on some obvious overlaps. Rittel and Webber emphasise a number of features of wicked problems, one for this context very important feature is that solutions to wicked problems cannot be used again. Every wicked problem is unique (Rittel and Webber, 1973, 164). There is nothing, or anyway very little, that can be learnt from solving one problem to solving the next. Mode 2 science might lean on such a notion of problems and thus perhaps gain a normative grounding for the claim that frameworks are temporary and transient. The weakness, perhaps, is that the notion of wicked problems suffers much of the same issues. We cannot know a priori that whether a problem is indeed wicked or not so the implications are uncertain.

The insistence on the temporary character of the frameworks distinguishes the Mode 2 idea not only from older conceptions of transdisciplinarity, but also from other interdisciplinary notions, including Darden and Maull's (1977) *interfield theories*. Mode 2 frameworks could be integrated as interfield theories, but they remain local and temporary, and are highly sensitive to changes in the context of application. For practical reasons the Mode 2 framework is often discontinued as soon as the problem is solved, or updated as the problem evolves. In a similar vein, Stanley Bailis (1986) talks about the "limited futures" of practically oriented interdisciplinary research.

Moreover, Mode 2 knowledge is believed to be mediated primarily through the individuals taking part in the projects, and to be communicated only in (other) Mode 2 settings. That is, in Mode 2,

> . . . unlike Mode 1 where results are communicated through

> institutional channels, the results are communicated to those who have participated in the course of that participation and so, in a sense, the diffusion of the results is initially accomplished in the process of their production. Subsequent diffusion occurs primarily as the original practitioners move to new problem contexts rather than through reporting results in professional journals or at conferences. (Gibbons *et al.*, 1994, 5)

This is not an independent addition to the Mode 2 perspective. It coheres with the idea of a local framework of limited duration. For one thing, the idiosyncrasies of this framework are of less interest to the wider audience. Conversely, if a scientific field displays a clearly different pattern in its publications, this should constitute evidence that the field in question is not, or not yet, fully Mode 2. One reason can be an enduring pressure on academics to stick to their disciplinary field in order to progress in their career (see the remarks of Respondent 36 above).

In sustainability science there may well be, and probably are, projects where publications are not the focus (whether those projects develop their own independent framework is a different issue). However, in general sustainability is not like this. It is beyond doubt that the number of publications in sustainability science has increased dramatically over recent years. Since 2005 a couple of new journals concentrating on sustainability science have already appeared, and PNAS has devoted a special section to issues in the field. A corresponding increase can be seen in articles on sustainability science. And there is an exponential growth of citations between these articles. Again, what this seems to show is that sustainability science is not, or not yet, fully Mode 2. Its researchers have not abandoned traditional values and strategies. Minimally, emerging research areas which, in many respects, approximate the Mode 2 ideal do not break away from traditional, "Mode 1" institu-

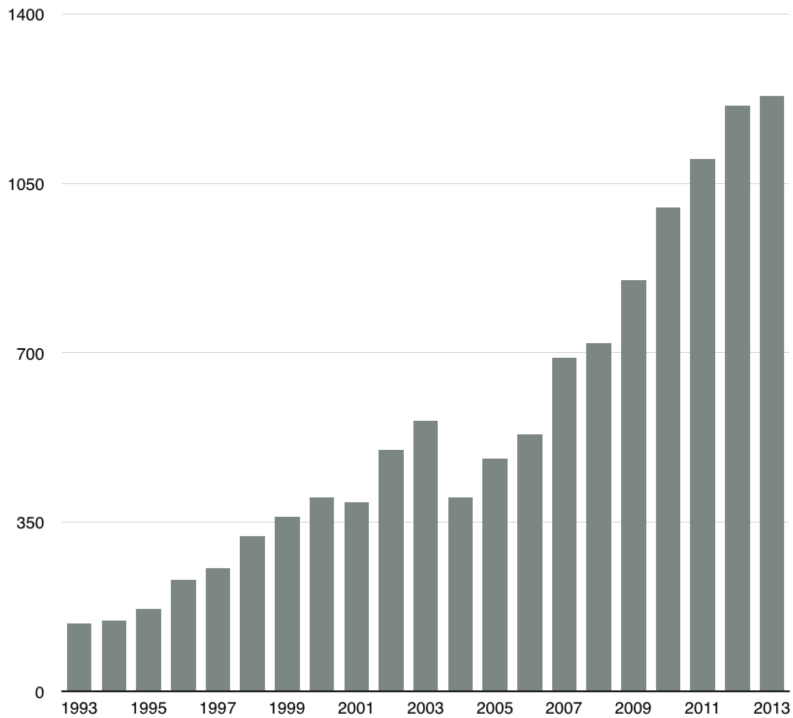


Figure 1: Articles on the topic "sustainability science" published over the past 20 years. Only articles in English are included in the figures (Source ISI Web of Science).

tional settings and disciplinary patterns. In Nowotny *et al.* (2001) the possibility of this factual scenario is not explicitly denied, but the local and transient character of the knowledge-producing process they describe does not lend itself easily to this kind of durable knowledge production, with steady publication and citation growth.

While Nowotny and colleagues claim that the dissemination of knowledge takes place in an informal way between research groups and participating institutions, thereby initiating new formulations of the problem, and possibly new research, a partly different type of transdisciplinarity appears to characterize sustainability science. The findings and approaches of this field are of importance in the global context and are published just as they would be in any traditional discipline. It would seem beyond doubt that knowledge gained through much transdisciplinary research is accumulated in this way—and hence that there either dissemination patters are not an important part of Mode 2, or that much of is considered transdisciplinary science does not conform to Mode 2 expectations.

5 Concluding Remarks

In this paper we have had two aims. One has been to investigate how the notion Mode 2 is understood and deployed within the field of sustainability science, the other, to forward the discussion of knowledge production in Mode 2 from a philosophical perspective. Our results seem to indicate that sustainability science is a field where there are tensions between competing approaches. These tensions move along several dimensions. For example, there are differences in the understanding of central notions, such as *transdisciplinarity* and *framework*. On the one hand sustainability science shows tendencies towards unification, both in terms of the institutional structure in which it is set, and theoretically. One might label this tendency one as transdisciplinary in the Jantschian sense. On the other hand, there is a pragmatic tendency that emphasises pluralism, inclusivity, and the dissolution of boundaries between researchers and stakeholders. This is more in line with what has been associated with Mode 2. We furthermore identified tensions between the 'institutional logics' of Mode 1 and Mode 2. It is tempting to hypothesize that sustainability science is re-bounding towards a mode-1 institutional form. Some of these tensions are probably explained with reference to the resilience of traditional academic structures, the way careers work and the distribution of influence among academics. Nowotny *et al.* (2003) note that those most positive to Mode 2 science tend to be those who have had marginal influence on scientific discourse, whilst those hesitant towards the notion often would have their position compromised by a flattening of hierarchy. This is an area to which further inquiry could be directed.

Other tensions, we suggest, have to do with the normative dimension of Mode 2. It remains a concept with uncertain epistemic and cognitive implications; an uncertainty that is exacerbated as attempts are made to consciously implement Mode 2. In many ways Mode 2 appears to be understood as 'whatever it is that works' given an unorthodox institutional setting. This makes Mode 2 science trivially suitable in many situations but also uninformative about how to act in those situations. One suspects that researchers risk ending up in a normative vacuum whereby they have few choices but return to already established ways of conducting scientific research.

Finally, let us return to some of the concerns raised in the opening sections of this paper. Are transdisciplinary approaches in general, and Mode 2 specifically, suitable to tackle grand challenges or themselves grand challenges? The two questions interconnect. We find it hard to be entirely categorical here. There are certainly aspects of transdisciplinary and Mode 2 that are absolutely necessary for solving certain types of problems and sustainability science is case in point. Complex issues call for integrative approaches for quite obvious reasons. Political reality implies that any viable solution to the challenge of sustainability is also going to have to involve non-scientific institutions in various ways. It is perhaps in this respect that Mode 2 science is most robustly normative: scientists should involve stakeholders already in the problem formulation phase and not just as a parameter to control for towards the end.

However Mode 2 involves more than mere boundary crossing and there are clearly difficulties, both practical and theoretical, with adopting Mode 2 as a normative model for science. One risk is self-deception. It appears to be common understanding that Mode 2 is the means by which an end can be obtained, an end that is inclusive approaches and robust solutions to difficult problems. In many respects however, Mode 2 concerns the end itself. About how to get there, it is less informative. Hence, the most challenging aspects of making Mode 2 science work are thus left out of the equation. This would account for much of the uncertainty regarding how to apply Mode 2, and how to keep Mode 2 science being Mode 2 (rather than diverting back to Mode 1).

With an eye to sustainability science in particular there are reason to be cautious with respect to Mode 2. A salient feature of sustainability science as field is the urgency of the problems at hand (Ziegler and Ott, 2011). A potential danger with Mode 2 is that the openness by which it is conceived risk leading to Mode 2 success being both slow and uncertain. Slow because working out a functioning framework—one that actually produces knowledge—is almost always time consuming and we have little idea of *how* this is to done, or even what such a framework may be. This can be expected to be exacerbated in precisely the contexts in which Mode 2 is most relevant, i.e. among actors who are assumed to be substantively different from one another. The uncertainty flows from the normative vacuity of the notion; we can do nothing, it seems, but mimic the institutional preconditions for Mode 2 and hope for the best. Those who populate these structures, to whom it has befallen to actually carry out the research, will have re-invent their epistemology from scratch every time. One cannot but worry that such a gargantuan task—a grand challenge perhaps—will fail more often than not.

These features are of course not unique to Mode 2—science is for the most part a painstakingly slow affair with highly uncertain outcomes. This is a problem for Mode 2, however, in part because it often is perceived of as a reaction to sluggish and unpredictable normal science. But if we get no particular advantages from Mode 2 in these respects, then, the risk is that one is throwing out the baby with the bathwater.

References

Bailis, S. (1986). Review of "interdisciplinarity revisited". *Association for Integrative Studies Newsletter*, **8**, 9.

Darden, L. and Maull, N. (1977). Interfield theories. *Philosophy of Science*, **44**, 43–64.

Defila, R. and Di Guilo, A. (2015). Integrating knowledge: challenges raised by the 'inventory of synthesis". *Futures*, **(forthcoming)**.

Funtowicz, S. and Ravetz, J. (1993). Science for the post-normal age. *Futures*, **September**, 739–755.

Gibbons, M., Limoges, C., Nowotny, H., Schwartzman, S., Scott, P., and Trow, M. (1994). *The New Production of Knowledge*. SAGE.

Hessels, L. and van Lente, H. (2009). Re-thinking new knowledge production: a literature review and a research agenda. *Research Policy*, **37**, 740–760.

Hessels, L. and van Lente, H. (2010). The mixed blessing of mode 2 knowledge production. *Science, Technology and Innovation Studies*, **6**, 65–69.

Hirsch Hadorn, G., Pohl, C., and Bammer, G. (2010). Solving problems through transdisciplinary research. In R. Frodeman, editor, *The Oxford Handbook of Interdisciplinarity*, pages 431–452. Oxford University Press.

Jantsch, E. (1972). Inter- and transdisciplinary university: a systems approach to education and innovation. *Higher Education*, **1**, 7–37.

Jerneck, A., Olsson, L., Nessand, B., Anderberg, S., Baier, M., Clark, E., Hickler, T., Hornborg, A., Kronsell, A., Lövbrand, E., and Persson, J. (2011). Structuring sustainability science. *Sustainability Science*, **6**, 69–82.

Kates, R. W., Clark, W. C., Corell, R., Hall, J. M., Jaeger, C. C., Lowe, I., McCarthy, J. J., Schellnhuber, H. J., Bolin, B., Dickson, N. M., Faucheux, S., Gallopin, G. C., Grubler, A., Huntley, B., Jager, J., Jodha, N. S., Kasperson, R. E., Mabogunje, A., Matson, P., Mooney, H., III, B. M., ORiordan, T., and Svedin, U. (2001). Sustainability Science. *Science: New Series*, **292**(5517), 641–642.

Klein, J. T. (2010). A taxonomy of interdisciplinarity. In R. Frodeman, J. T. Klein, and C. Mitcham, editors, *The Oxford Handbook of Interdisciplinarity*, pages 15–30. Oxford University Press.

Klein, J. T. (2014). From method to transdisciplinary heuretics. In K. Huutoniemi and P. Tapio, editors, *Transdisciplinary Sustainability Studies: a heuristic approach*, pages xii–xv. Routledge.

Kuhn, T. (1969/1970). *Postscript-1969. The structure of scientific revolutions.* The University of Chicago Press, second edition edition.

Kuhn, T. (1977). *The Essential Tension*. University of Chicago Press.

MacMynowski, D. (2007). Pausing at the brink of interdisciplinarity: power and knowledge at the meeting of social and biophysical science. *Ecology and Society*, **12**.

Martens, P., Roorda, N., and Cövers, R. (2010). The need for new paradigms. *Sustainability, Science and Higher Education*, **3**, 294–303.

Norton, B. (2005). *Sustainability. A Philosophy of Adaptive Ecosystem Management*. University of Chicago Press.

Nowotny, H., Scott, P., and Gibbons, M. (2001). *Re-thinking science: knowledge and the public in an age of uncertainty*. Polity Press.

Nowotny, H., Scott, P., and Gibbons, M. (2003). Introduction: 'mode 2' revisited: The new prodcution of knowledge. *Minerva*, **41**, 179–194.

Olsson, L. (2014). LUCID_LundUniversity_and_VC-report-3.pdf, Lund University.

Pestre, D. (2000). The production of knowledge between academies and markets: A historical reading of the book the new production of knowledge... *Science, Technology and Society*, **5**, 169–181.

Pestre, D. (2003). Regimes of knowledge production in society: towards a more political and social reading. *Minerva*, **41**, 245–261.

Rittel, H. and Webber, M. (1973). Dilemmas in a general theory of planning. *Policy Sciences*, **4**, 155–169.

Russell E. Vance, W., Wickson, F., and Carew, A. (2008). Transdisciplinarity: context, contradications and capacity. *Futures*, **40**, 460–472.

Swan, J., Bresnen, M., Robertson, M., Newell, S., and Dopson, S. (2010). When policy meets practice: colliding llogic and the challenges of 'mode 2' initiatives in the translation of academic knowledge. *Organizational Studies*, **31**.

Thorén, H. (2014). Resilience as a unifying concept. *International Studies in the Philosophy of Science*, **28**, 1–22.

Weingart, P. (1997). From "finalization" to "mode 2": old wine in new bottles? *Social Science Information*, **28**, 303–324.

Ziegler, R. and Ott, K. (2011). The quality of sustainability science: a philosophical perspective. *Sustainability Science, Practice, Policy*, **7**(1), 31–44.

Zscheischler, J. and Rogga, S. (2015). Transdisciplinarity in land use science–a review of concepts, empirical findings and current practices. *Futures*, **(forthcoming)**.

Paper V

Resilience: Some Philosophical Remarks on Defining Ostensively and Stipulatively

Henrik Thorén[1, 2] and Johannes Persson[1,2]

[1] *Dept. of Philosophy, Lund University, Sweden*
[2] *LUCID, Lund University Centre of Excellence for Integration of Social and Natural Dimensions of Sustainability, Sweden*

Publication Status: Submitted

Abstract

Although contentious, the concept of resilience is common in sustainability research. Critique of the concept has often focused on the content of the concept. In this paper we focus on another feature of concepts, namely how they are defined. We distinguish between concepts that are ostensively defined, that aim to point to some phenomena, and stipulatively defined concepts, where the content of the concept is given in the definition itself. We argue that although definitions themselves are similar across many different disciplines where resilience is used—most notably psychology and ecology—they differ in how. This has interesting consequences for how different disciplines can be connected and integrated. Notably, integration on basis of ostensively defined concepts turn on sharing the extension (the phenomena itself) of the concept, but not necessarily the intension (the definition), whereas integration on basis of stipulatively defined concepts work in the opposite way.

Keywords: Resilience, Sustainability Science, Interdisciplinarity

1 Introduction

Resilience is a concept that has gained considerable support within sustainability research over past couple of decades (Parker and Hackett, 2012; Walker and Cooper, 2011). The concept, introduced into the context of sustainability by ecologists Crawford S. Holling and his followers first in the Resilience Network and later in the highly influential Resilience Alliance (Walker and Cooper, 2011), has great appeal within sustainability science for several reasons. One is its alleged 'integrative' and 'discipline bridging' capabilities (Holling *et al.*, 2002, 8). That sustainability science has to be an inter- and transdisciplinary venture is more or less ubiquitously accepted among practitioners in the field (Thorén and Breian, 2015) and crossing disciplinary (and other) boundaries is essential to this aim. Another reason—that is perhaps more controversial (Derissen *et al.*, 2011)—is that the notion of sustainability itself possibly can be construed in terms of resilience, or anyway use resilience as a foundation for understanding (or realizing) sustainability (Anderies *et al.*, 2013; Perrings, 2006; Ludwig *et al.*, 1997)(Anderies et al. 2013, Perrings 2006, Holling et al. 1997). In this paper we will focus mainly on the idea of connecting disciplines.

Sustainability science is an inclusive field, broadly interdisciplinary, and in many respects difficult to capture. However, discipline bridging—especially between natural and social sciences—appears to be universally accepted as a central, or even essential, component (Kates *et al.*, 2001; Jerneck *et al.*, 2011; Ziegler and Ott, 2011). Sharing concepts is an important way in which this can be obtained.

In papers on the resilience concept the focus has, more often than not, been on differences in the content of different definitions of resilience. Brand and Jax (2007) for example, are prompted to consider the concept of resilience as a boundary object; a flexible concept that has different meanings for different users of it, but at the same time allows for interdisciplinary communication. Strunz (2012) suggests that the concept is *polysemous*, having many similar, though difficult to disentangle, meanings. He too emphasizes that this may, in fact, have certain benefits for interdisciplinary work on sustainability.[1]

In this paper, however, we intend to focus on another aspect of the concept of resilience, one that has thus far been overlooked. Namely, how and with what aim the concept is defined. We begin by establishing the distinction between concepts that are defined ostensively and concepts that are defined stipulatively. The definitions associated with the former serve to pick out some particular phenomena or kind whilst the latter, usually, serve to highlight for example, a conceptual joint. We then argue that with respect to the concept of resilience one finds that ecologists often define resilience stipulatively whilst psychologists, who also have a long history of using the notion, define the term ostensively. Interestingly, this is in spite of the fact that the resilience concepts used share a conceptual core across these disciplinary boundaries.

From this observation we go on to note that in sustainability science, where resilience is a concept that is expected to bridge disciplines, the debate on concepts of resilience have exclusively focused on the content of the definitions. However, we will argue, the distinction we propose matters. An ostensively defined concept and a stipulatively defined concept point towards different interdisciplinary relations. Specifically, we will suggest, in the former case conceptual coherence is secondary to ontological overlaps, whereas in the latter case it is precisely the other way around. An ontological overlap is here signifying, for instance, sharing an

[1] Notably sustainability science is often claimed to be problem oriented and transdisciplinary. We will not discuss these notions in particular in this paper but we believe our conclusions have relevance to them.

interest in the same phenomenon.

A caveat is in order before we proceed. This is first and foremost a philosophical paper and we aim to make a philosophical point. Hence we will not provide full literature reviews of either psychology, ecology, or sustainability science that would allow for robust generalisations concerning those fields. Such a study would indeed be interesting and perhaps a natural continuation of the present paper but it remains outside the scope of this particular contribution. Instead we use the fields of psychology and ecology as examples, perhaps idealized examples, in order to point to differences in the way terms are defined given a certain context and specific aims. In the field of ecology we focus primarily on Crawford S. Holling (1973). The reasons are that Holling's writings are typical for one kind of use that we want to highlight and that his (1973) paper has been so important for how resilience is thought of in sustainability research. The use of resilience in psychology seems to be more widely spread and the concept has been intensely discussed (Rutter, 1985; Olsson *et al.*, 2003; Bonanno *et al.*, 2007; Herrman *et al.*, 2011). We provide a preliminary, though by no means all-encompassing, survey of that literature below. The point is to exemplify differences in how concepts may be defined in different contexts rather than providing complete accounts of how resilience is used in these different disciplines.

Although the concept of resilience has been discussed among sustainability researchers (Gunderson and Holling, 2002; Jerneck and Olsson, 2008; Anderies *et al.*, 2013; Davidson, 2010; Hornborg, 2013) philosophers do not appear to have taken much notice, although there are exceptions. Related notions, such as stability, have been discussed before in the philosophical literature. Hansson and Helgesson (2003) develop a formal framework in which they differ between three main kinds of stability one of which they label resilience. Their focus, however, is on the content of definitions rather than the *way in which* concepts are defined. More relevant in this context then is Thorén (2014) who points out that different definitions of resilience, for the most part, converge on two core understandings and further argues that the resilience concepts used are often highly abstract. The consequence is that the concepts become context insensitive and applicable to many rather different disciplines. But, conversely, they rarely form a substantive interdisciplinary connection. Thorén (2014) too focuses mainly on the content of definitions. Our paper, by contrast, should be seen as an alternative way of looking at the resilience concept and how it may tie disciplines together.

2 Stipulative and Ostensive Definitions

There are many different ways of defining concepts and how concepts are to be understood has been a central philosophical concern since antiquity. These debates, both within philosophy in general, the philosophy of science, and the philosophy of specific disciplines (such as the philosophy of psychology) have often surrounded how concepts should be defined. The logical positivists, for example, believed that all meaningful concepts could be operationalized completely. Although Rudolf Carnap (1936; 1937) himself helped to bring this thesis (called operationism) down it survived at least until the 1970s within psychology (Wallach, 1971). This is not unimportant in this particular context as psychology is one case that we make us of. However, we will not engage with the underlying philosophical question what concepts ultimately are. Instead we focus on a particular distinction between two ways of defining concepts. Thus, one distinction that can be drawn is between definitions that aim to point out a phenomenon to be studied, and definitions the purpose of which it is to highlight, for example, a conceptual joint. We differ between concepts that are defined ostensively and concepts that are defined stipulatively. The distinction is important. The former, but not the latter, prompt investigation of the world.

Concepts that are defined ostensively are common, both in science and everyday life. Often the process only involves pointing. We hold up for display an instance of the extension of the term. We say “that is red” and point at a red piece of paper. In science something similar is going on in cases where we cannot—at least not presently—understand the referents of our terms by relying on scientific language alone. Psychologists often talk about *construct validity* (Cronbach and Meehl, 1955; Campbell, 1960), and one very simplified way to understand this complicated notion is in terms of the relation between what we observe (or measure) and the theoretical construct we employ (typically an attribute, proficiency, ability, or skill that happens in the human brain) and claim to be observing or measuring. “A construct is some postulated attribute of people, assumed to be reflected in test performance” (Cronbach and Meehl, 1955, 283). Defining ostensively is one way to establish a first link between the world of theory and the world we observe:

> Although ostensive definitions are the starting point for construct validity, the existence of *bricolage* or know-how merely points to where it may be observed. The intuitive certainty of a construct such as *bricolage*, and indeed our everyday observations of it in action, does not provide evidence for its precise relationship to other forms of intellectual activity” (Berry and Irvine, 1986, 300)

Saul Kripke (1980) famously pointed out that some definitions—in particular those associated with so-called *natural kind terms*—do not give the real essence of that which is defined. Instead the definite descriptions which make up such definitions function merely to fix the reference. Tigers then, might be ‘defined’ as “large, striped, cats that live in India.” This helps us find tigers; its function is roughly equivalent to saying "that is a tiger" whilst pointing at a tiger. In this sense the concept is defined ostensively. An interesting feature of this kind of process is that the resulting definition can be both quite barren and even outrightly false. Tigers are by all means large cats that live in India, but they also live elsewhere. Living in India is not *essential* to tigers, obviously. Importantly, it could also have turned out to be the case that tigers were not cats at all. Suppose that zoologists examining tigers found them not to be felines but canines uncannily similar to cats (perhaps due to convergent evolution). Outrageous as this counterfactual consideration may sound, similar events have occurred. What would such a discovery prompt? Well, in all probability the definition of the concept would be amended; tigers would be considered to be large, striped, dogs that live in India. The point we wish to make here is that the content of the concept is determined—in the end—by how the world is, and the definition that is associated with the concept is subject to revision.

Whereas the purpose of ostensively defining a concept is standardly to point towards something to be investigated, stipulative definitions are often introduced in order to, for example, draw attention to some particular conceptual joint (Belnap, 1993, 116). Mathematical and formal concepts are often stipulatively defined. But there are mundane examples as well. The concept engine may be defined in terms of its function: an engine is a device, or mechanism, that converts one form of energy into another (typically thermal energy into mechanical energy). That engines perform this function is just *stipulated in the definition*; it is the very meaning of the term and one cannot, for trivial reasons, find that we were somehow mistaken about engines.

Concepts that are stipulatively defined differ from ostensively defined concepts in that they are not subject to revision—not in the same sense anyway. Ostensively defined concepts are inherently provisional, their meaning is connected to the structure of the world. This does not mean that stipulatively defined concepts are never altered. The length of a meter is stipulated and it has changed over the course of history. These revisions, however, are not a result of the discovery that a meter was, in fact, shorter or longer than we initially thought but rather that one has highly valued standardized measures and have strived for definitions that are more robust.

It is important to note that this distinction neither orders all scientific concepts neatly into two mutually exclusive categories nor is exhaustive on that domain. We can have multiple purposes with our definitions. This is indeed an important point in this paper as it shows that focusing on the content of definitions is not always informative. Moreover in scientific inquiry it is often the case that one may switch between these (and other) ways of defining a concept, as well as having concepts that are partially stipulative and partially ostensive, and so on. Sometimes we begin by defining a concept ostensively, but aim to replace it with another type later on. Biological species are often first defined ostensively by reference only to phenotype. Later on, however, one may shift to give other kinds of definitions, for example in terms its evolutionary relationship to other species. An analogous dynamic can be found between operational and theoretical definitions. In parts of psychology, for instance, operational definitions have been considered especially important (Wallach, 1971). An operational definition gives the meaning of a concept by reference to a process or test through which something can be be observed, or brought about. One purpose of an operational definition is control. Operationalizing definitions narrow the scope of a concept to cover a few measurable parameters. To overtly emphasize the importance of operationalized definitions, however, tends to exclude relevant parameters. In particular those that cannot be easily, or well, measured (Campbell, 1970). Sometimes there are benefits to control, and sometimes not.

At other times, we begin by defining a concept stipulatively only to later turn to an ostensive definition. Regarding the former point—that a single definition can be understood as either ostensive or stipulative—the concept of resilience is a case in point but there are many others. The gene concept as used in Mendelian genetics is one example. The concept of the gene was defined stipulatively as that which carries a trait from parent to offspring. Later on, however, cytologists, and biochemists would consider this definition to be ostensive and thus proceeded to empirically investigate the material basis for the gene. This has led us to abandon this definition, in spite of the fact that it was initially a stipulation. Not everything that carries a trait from parent to offspring is a gene and neither do genes always carry traits this way. Similarly, if we reconsider the definition of the concept tiger proposed above, the definition may, of course, be taken as a stipulative definition, in which case, for instance, Siberian tigers would not really count as tigers, and so on.

2.1 Conceptual Connections and Interdisciplinary Collaboration

A central issue here concerns the role of these two forms of definition with respect to how disciplines can be connected to one another conceptually and how that relates to interdisciplinarity. If we limit ourselves to only these two ways of defining concepts three ways in which conceptual connections can be be formed emerge.

Suppose that two disciplines are said to be conceptually unified if they use identical concepts. Then we might take note that identity is determined in different ways depending on whether a concept is defined ostensively or stipulatively. Two ostensively defined concepts are identical if, and only if, their respective definitions pick out the same objects in the world. It is of no consequence if the expressions themselves are not identical; their identity can be said to be *extensionally determined.* If we now consider identity between stipulatively defined concepts we find the situation to be exactly the opposite. For such concepts identity depends on the definitions themselves, the intension of the concept. If two stipulatively defined concepts have the same extension, but not the same intension, they should be considered different concepts. Hence we can say that the identity between stipulatively defined concepts is *intensionally determined.*

What does this mean for conceptual connections between disciplines? For one, it seems that with respect to ostensively defined concepts it is natural to consider two disciplines as being conceptually unified, or perhaps more appropriately, having a conceptual connection, if there is an ontological overlap in what the respective definitions pick out in the world. Indeed such ontological interconnections have been emphasized as very important in interdisciplinary exchanges, by for example Darden and Maull (1977), as they can form the basis of what they call *interfield theories.* Interfield theories are theories that connect one scientific field to another on the basis of different types of ontological relations such as, for instance, part-whole relations, or causal relations. The chromosome theory of the location of genes is an example of interfield theory as it connected classical transmission genetics with cytology. From this perspective, the definitions of the concepts used in the respective disciplines is much less important than overlap in what they pick out in the world. That is to say, as long as the expressions do, in fact, pick out the same phenomena we have what is needed for an interfield connection.

Conceptual connections established on basis of stipulatively defined concepts, on the other hand, are based on the intension of the concept. Here ontological overlaps are much less important; what is central is instead that the concepts used are defined in the same way. Such connections are not rarely substantially weaker. Many disciplines share stipulatively defined concepts, such as for example mathematical and statistical concepts (or other highly abstract notions), without this necessarily being perceived of as a reason to engage with one another.

The most interesting case, however, concerns cases where concepts are developed through the scientific process where one shifts between different ways of understanding a definition. Within a discipline one might expect members of that discipline to have a heightened sense of, for example, when a definition is used stipulatively and when it is used ostensively. Between disciplines, however, this appears to be precisely the kind of subtleties that may be lost.

In summation, depending on how the concepts are defined different conceptual issues arise. Sometimes it is important to reconcile the definitions of the involved concepts, but sometimes this is not so. It is always, however, helpful to try to keep in mind how a particular definition is to be taken.

3 Resilience in Psychology and Ecology

Resilience is a concept that occurs in the title of publications from about 1910 and onwards. Early papers that use the concept are strongly focused on materials science, in particular in textiles research, where the concept was so frequently used that it prompted specific conceptual discussion and disambiguation. An example comes from Hoffman (1948) complaining that "resilience means different things to different people" (141):

> A physicist says: "The behavior of a quartz ball dropped on a quartz plate provides a good example of resilience; it will bounce many times, showing a small loss of energy through dissipation as heat. In other words, it has a high work recovery." But a carpet manufacturer can say: "The resilience or 'luxury' factor of a carpet is

proportional to the reciprocal of the modulus. I want a low modulus. A quartz plate takes a ‘zero’ on my resilience scale because it won’t absorb any work in compression.” (Hoffman, 1948, 141)

Hoffman goes on to argue for a generalized concept of resilience and finds the most common understanding of resilience to be, the ability of something to return to a reference state following a disturbance. The issue, it appears, concerns how to operationalize the notion. The ability of a piece of yarn to return to its previous length after being stretched can, for instance, be measured as the speed of return, or the difference in length of the yarn before and after being stretched. In more recent times and prior to the establishment of sustainability science, two disciplines in particular stand out as users of the notion. One is psychology, the other ecology. These disciplines provide interesting examples not only because resilience has been an important concept in both but also because they appear to differ in their respective approaches to the concept.

3.1 Psychological Resilience

Some children that grow up under adverse conditions grow up to be normally functioning adults. Others succumb to their predicaments and remain marked by them for most of their lives. What is the difference? For a long time these, apparently unharmed, children where called “invulnerable children.” This notion, however, was abandoned as it was perceived to have false, or inappropriate, connotations. In the 1980s resilience was gaining ground as a preferable, and less problematic, alternative (cf. Rutter, 1985). In current psychological literature the concept has been broadened and is used not only within child psychology (see e.g.)(Herrmanetal2011,Bonannoetal2007). One finds several versions of the concept occurring in the literature. Here are three examples of how resilience is spelled out:[2]

1. Meeting developmental goals in spite of adversities.

2. Sustained competence under stress.

3. Ability to recover following trauma.

The two first forms are similar in the sense that they both denote the maintenance of some property during stress, whilst the latter, concerns the ability to return to some state following a disturbance. Thus this latter form relates more readily to many uses in both material science and, as we shall see, ecology.

There is nothing about these definitions themselves that indicate whether they are to be taken as ostensive or stipulative. However, the general context may give a better indication. Psychologists have an interest in resilience as far as it is a way of talking about a behavior that can be observed in individuals. Some people appear to function during, or recover from, psychological trauma better than others. These people are resilient. The question then, is "why and how some individuals maintain high self-esteem and self-efficacy in spite of facing the same adversities that lead other people to give up and lose hope" (Rutter, 1987, 317). Are there certain protective mechanisms or process that these individuals have as some have suggested (Herrman *et al.*, 2011; Olsson *et al.*, 2003; Rutter, 1987) and what are those?

The aims are often deeply empirical; to find that which makes people resilient. Although definitions are important and psychologists sometimes complain about the conceptual disorder it is a discussion that often differs quite radically from many ecological

[2]See e.g. Daniels (Fogany et al. 1994, 233, in 2008, 60), Bonanno *et al.* (2007), and Dyer and McGuinness (1996).

papers on the topic. The aim is to a find a definition, eventually, that captures the phenomenon. The process to get there is dynamic open.

3.2 Ecological Resilience

Within ecology the use of resilience becomes established in the early 1970s, especially following C.S. Holling's (1973) paper. To understand the notion of resilience used in ecology one has to understand the context in which stability in general has been discussed within that field. During the 1950s and 1960s the received view among ecologists was that stability and diversity (or complexity) were positively covariant.[3] This thesis, called the *stability-diversity thesis*, was endorsed by influential ecologists such as Charles Elton (1958) and Robert MacArthur (1955). An immediate problem with this thesis, however, was that both notions of stability and diversity turned out to be notoriously ambiguous. Diversity and complexity are both related to a number of other concepts such as richness (the number of species in a community) and evenness (their distribution) (cf. Justus, 2008). A simple example: one may understand diversity in an ecosystem as the number of species it contains, that is, in terms of richness. On this interpretation an ecosystem with many species, but where one is overwhelmingly dominant, is more diverse than one with fewer species, but perhaps a more balanced distribution. If we take evenness into account we may be inclined to revise the appraisal but it raises the question: Which kind of diversity is linked to stability? And stability in what sense? Stability too can be understood in many different ways. The many meanings of stability have prompted considerable discussion, and sometimes perhaps

[3] Already in the late 1960s this was being questioned but the thesis was note widely abandoned until later, see DeLaplante and Picasso (2011), Justus (2008) and Redfearn and Pimm (2000) for discussions.

confusion, among ecologists. Grimm and Wissel (1997) in a survey of different stability concepts found 163 definitions and 70 different terms relating to stability in one way or another.

Although, as Thorén (2014) argues, two versions of the resilience concept are more common than others—again, resilience as the ability to return to some reference state (Pimm, 1984) and resilience as the ability to withstand (or absorb) a disturbance (Holling, 1973)—it is notable that ecologists are comparatively relaxed with the terminology pertaining to different stability concepts. The distinction we just made, between the two main uses of resilience, is recurring in the ecological literature, although it is not always associated with the term 'resilience.' In his (1973) paper, Holling makes precisely this distinction and calls them stability and resilience respectively, later on he changes the terminology and prefers the terms engineering resilience and ecological resilience. Schrader-Frechette and McCoy (1993) make the same distinction but prefer the terms 'dynamic balance' and 'persistence', and so on.[4]

The disambiguation of the general concept of stability has been a central topic for theoretical ecologists at least since the late 1960s. Grimm and Wissel's paper is an example of that but there are also many others.[5] For the ecologist these conceptual issues appear to be much more important than for the psychologists; the technicalities surrounding how stability or resilience is defined makes a bigger difference. This is perhaps not surprising given that these notions are often operationalized and deployed within an entirely formal setting. As is the case for Holling (1973) a central aim is to obtain a resilience concept that is measurable in predator-prey models.

[4] For further examples, see Orians (1975) and Grimm and Wissel (1997).

[5] Aforementioned contributions by Justus (2008) and Redfearn and Pimm (2000) for instance.

3.3 An Analysis

There are apparent parallels between the resilience concepts used in ecology and psychology. For example, in both disciplines it is possible to discern two main senses of the notion (see above). First, resilience as the ability of a system to return to some reference state after a disturbance. Second, many resilience concepts refer to what some have called *robustness* (Hansson and Helgesson, 2003); the ability of a system to *remain unchanged*, or close to unchanged, as it is disturbed.

But there are also subtle differences. Ecologists have developed resilience concepts in the context of a larger conceptual debate on stability. For the present purposes the ecological discussion on stability and that regarding resilience are indiscernible. One salient feature of this debate concerns the context in which it is carried out. Resilience and stability have often been discussed in a highly formal setting; it is the resilience of particular models, or classes of models, which is at stake. Holling (1973) is a case in point. The core of his discussion surrounds classical Lotka-Volterra based predator-prey models and how resilience may be operationalized with respect to them (see also Ludwig *et al.*, 1997). The aim for Holling, as has been the aim for most ecologists that have engaged themselves in this problem, has been to disambiguate the concept of *stability*; in essence, to pin point fine conceptual differences between different ways in which a system can be said to be stable (see also e.g. Grimm and Wissel, 1997). This aim coincides well with Belnap's observations about stipulative definitions. Our claim is not that ecologists always define resilience stipulatively but rather, that this is at least sometimes the case. In particular, it is the case with ecological papers that actually influenced sustainability research.[6] The salient features of the formal context in

[6] In a discussion of resilience in sustainability science Holling has himself been of central importance in bringing the concept from the context of ecol-

which the concept often appears and the explicit aim of conceptual disambiguation both support our perspective.

Among psychologists, on the other hand, it seems more common to define resilience ostensively. This is admittedly difficult to show definitively, but there are several disanalogies between the two disciplines that suggest it is indeed so. First, there is no parallel to the stability debate in psychology and no stability-diversity thesis causing conceptual problems. Instead psychologists appear to have departed from the observation that some individuals appear to handle psychological stress better than others (Olsson et al. 2003). Second, whereas most of the ecological debate is carried out within a context of mathematical models, psychologists discuss resilience in relation to its empirical base. And third, relatively speaking, psychologists appear to be more interested in the underlying mechanisms and processes that realize resilience than dwelling over conceptual points (Rutter 1987). The consequence is that there is a lack of conceptual rigor across studies which has sometimes been perceived of as problematic (cf. Olsson et al., 2003). It is possible here that an early observation by Donald Campbell is relevant. In discussing the conception of construct validity presented in Cronbach and Meehl (1955), Campbell remarks:

> It may be wise to distinguish two types of construct validity. The first of these [...] is applicable at that level of development still typical of most test development efforts, in which 'theory,' if any, goes no farther than indicating a hypothetical syndrome, trait, or personality dimension. The second type could be called nomologi-

ogy to sustainability research. He has continually developed and broadened the notion (Holling, 1987, 1996; Ludwig *et al.*, 1997; Gunderson and Holling, 2002). For a more historical perspective on resilience theory see Walker and Cooper (2011) and Parker and Hackett (2012).

> cal validity and would represent [...] the possibility of validating tests by using the scores from a test as interpretations of a certain term in a formal theoretical network [...] (Campbell, 1960, 547)

It might be that at times, depending on the status of the nomological network (i.e. "the interlocking system of laws which constitute a theory" (Cronbach and Meehl, 1955, 290), definitions are relatively unimportant; it is the phenomenon, and not the definition, that is central. In contrast the conceptual discussions within ecology often aim to draw up some distinction where both sides are useful. Resilience has not *replaced* any other concept in ecology, it is just one kind of stability that one sometimes is interested in. Conceptual discussions in psychology, on the other hand, have been concerned with replacing concepts. For instance, invulnerability was abandoned because it carried with it empirically unsubstantiated connotations (see e.g. Rutter, 1993).

In many ways this difference in how resilience is defined makes sense. One important aim for many ecologists has been to point out a conceptual joint. This has been instrumental to understanding the stability-diversity thesis. For example, Holling (1973) proposes that it is resilience (ability to persist through a disturbance) and not stability (ability to return to some reference state) that is positively covariant with diversity. Hence the interest in stability and resilience was, at least to begin with, purely conceptual. Psychologists, on the hand, have departed from an observation—a difference in the behavior of individuals under similarly difficult circumstances.

We do not mean to suggest that the ostensive/stipulative definition cuts these disciplinary boundaries precisely. In particular, we do not mean to say that there are no conceptual issues raised within the psychology of resilience or that ecologists always define resilience stipulatively. There is probably a spectrum of uses in both fields. The point rather is to illustrate how changes in the context in which a concept is used may make one type of definition more suitable than another. Nonetheless the fields often appear as contrasts to one another in this respect.

4 Resilience and Sustainability Science

To recap, there are three points we wish to draw attention to. First, there is a difference between ostensively defining a concept and stipulatively defining a concept in the sciences. This is fairly straightforward. Second, this distinction can have a disciplinary dimension, at least given a specific concept, as the above two examples suggest. Stipulative definitions may be more common in certain disciplines or fields and ostensive definitions in others. Third, and crucially, the distinction between stipulative and ostensive definitions is not always possible to reveal through an examination of the content of a set of definitions.

Now let us consider our second point. The disciplinary dimension of preferred modes of definition is of particular interest if we consider cases in which the concept in question is expected to bridge disciplines. One such context in which the resilience concept has frequently been charged with precisely this expectation is sustainability science. Within this field many have emphasized the need for a deeply integrative effort involving both natural and social sciences (Kates *et al.*, 2001; Jerneck *et al.*, 2011). The resilience concept has been proposed as a possible bridge (Gunderson and Pritchard, 2002) not least by offering a way of construing sustainability itself (Common and Perrings, 1992; Ludwig *et al.*, 1997).[7] Unsurprisingly the concept of resilience has thus been an

[7]This idea is somewhat contentious, see Derissen *et al.* (2011); Lélé (1998). The fact that there is a discussion, however, is sufficient for the argument we present here.

object of some controversy (Hornborg, 2013; Davidson, 2010) as well as repeated efforts focused at mapping and accounting for the apparent state of conceptual confusion (Brand and Jax, 2007; Strunz, 2012). These efforts, however, are strongly focused on the content of the definitions proposed within the field and do not take into account the mode of definition. The way a concept is defined, however, might matter in interdisciplinary contexts. The reason is that the interdisciplinary connection that is emphasized when using ostensively defined concepts is "in the world." As long as two disciplines through the concept obtain an ontological connection there is no real need to harmonize definitions of the concept. It is secondary, or at least, it is not a prerequisite of collaboration. Conceptual harmony may perhaps be the outcome of scientific investigation. For stipulatively defined concepts, however, this does not appear to be the case. When concepts are defined stipulatively the definitions themselves become much more important.

Given the disciplinary dimension of the mode of definition, researchers who are involved in interdisciplinarity, such as sustainability scientists, should be sensitive to the difference between ostensive and stipulative definitions. One reason is of course that, as we have shown, the resilience concept in particular is subject to these differences. However, an argument may be made here that the resilience concept deployed in sustainability science draws so strongly from its ecological background (see Walker and Cooper, 2011; Parker and Hackett, 2012) that it is not a matter of reconciling resilience concepts from different disciplines to one another. Instead the resilience concept, as used within a particular context, is imposed on a broader field. This does not necessarily exclude the possibility of confusion between the modes of definitions but makes it somewhat less plausible.

There are, however, further reasons why sustainability science may be a field that is susceptible to this form of confusion, a reason that relates directly to the ecological roots of the resilience concept used there. One central use of the resilience concept for sustainability scientists is in the evaluation of particular systems or classes of system—resilience is used to tell us something about how, for example, social-ecological systems will respond to disturbances. Resilience assessment, in this sense, is in many instances, analogous to the kind of assessment of individuals psychologists are interest in. The aim is evaluation of some target system in order to produce predictions, prognoses, and prescription. Specific underlying mechanisms and causes are central, both to evaluate current 'systems' but also to, as sustainability scientists often put it, "build resilience" (Berkes and Folke, 1998). This might be taken to suggest that the concept of resilience *should* be defined ostensively in sustainability science. Notably however, many of the most prominent defenders of the resilience concept, have a background in theoretical ecology where arguments that define the concept stipulatively have been legion.

So, there is reason to suspect the stage is set for some confusion regarding how, exactly, the concept of resilience is meant to bridge, or connect, different disciplines. Is there in fact, such a confusion? And, how, would we notice? As already mentioned, it does not always help to look at the definitions themselves, as a single definition can be taken as either stipulative or ostensive. We may, however, get an idea of the situation by looking at what the concept of resilience is taken to carry along with it. First, however, let us consider this simple, but uncontroversial, definition of resilience:

Resilience: The capacity to sustain a shock and continue to function Anderies *et al.* (2013).

We can understand this definition either as an ostensive or a stipulative definition. Let us consider them in that order.

1. Taken as an ostensive definition there are components of this

definition that are clearly tacit. Anderies et al. are not discussing resilience in just any context, but rather a quite specific one, namely the resilience of social-ecological systems. Then we understand this definition as pointing, by way of a certain behavior, to the realizers of that behavior. That is to say, whatever it is that makes social-ecological system resilient. This may, or may not, involve further hypotheses regarding the unity of those mechanisms; that is, whether social-ecological systems are homogeneous with respect to this feature.

2. If we understand the definition as stipulative, on the other hand, we would think of it as silent about underlying mechanisms and placed strictly at the level of the behavior itself. Then it is not limited to some specific class of systems but is just a general feature that can occur in any number of different settings.

Under the supposition that (1) is indeed the correct way of reading Andries et al then the concept would transfer to different contexts—say, one that involves another type of system—if, for example, this other type of system also exhibits the same mechanisms.[8] If we take (2) to be the correct interpretation, however, the conceptual transfer does not at all rely on the presence of some particular class (or classes) of realizers. The definition is understood as more or less exhaustive and explicit and can be transferred to any domain where the behavior it describes occurs.

Confusing the two thus, would result in confusing exactly what type of information that comes along with the transfer of the con-

[8] Another possibility here is that the resilience of one type of system is causally linked to the resilience another type. This would indeed provide reason for discussion, albeit not a transfer of the ostensively defined concept. Some attempts at this have indeed been made. Adger (2000) have argued that such a connection obtains between ecological and social resilience.

cept. In sustainability science it is often a matter of whether social systems, broadly speaking, are essentially of the same kind, as ecosystems. Here, it either involves, at least hypothetically, claims about underlying structures and mechanisms of social entities, or it amounts merely to surface phenomena. This matters for whether we are to think of the transfer of the concept of resilience from ecology to sustainability science, and ultimately the social sciences, as controversial. Consider the following example. The Ottoman Empire can be said to have been resilient, or not resilient, with respect to different kinds of calamities that it faced throughout its long history. Given that this history was indeed quite long we might hypothesize that the empire was indeed quite resilient. We might even want to explain the demise of the Ottoman Empire in terms of a successive hollowing out of that resilience. The Great War finally sealed the fate of the empire, but if that would not have happened, something else would. This appears to us to be a rather uncontroversial use of the notion of resilience that is quite in line with many definitions used within ecology—such as the one proposed by Anderies et al. above. *If we take the definition as stipulative, that is.* To say that the Ottoman Empire is resilient in exactly the same way as an ecosystem, however, obviously involves something further. This something might for example be claims concerning the inner structure of the Ottoman Empire, or empires in general, or perhaps social systems in general.

Among both critics and defenders of the resilience concept in sustainability science there seems to be a genuine uncertainty with respect to precisely this issue. Some take the concept to commit its users to very particular views of social systems and social ecological systems, whilst others presume it to be an abstract, and largely neutral, concept that is virtually boundless in its application. Hornborg (2013) is an example of the former arguing that resilience fails to take power relations into account and hence falls well short of providing a basis for any type of framework suitable within the social sciences. Holling and Gunderson (2002)—ardent defenders of the resilience framework—repeatedly return to the social system/ecosystem analogy and suggest that deeply seated connections obtain between the two:

> Competitive processes lead to a few species becoming dominant, with diversity retained in residual pockets preserved in a patchy landscape. While the accumulated capital is sequestered for the growing, maturing ecosystem, it also represents a gradual increase in the potential for other kinds of ecosystem futures. For an economic or social system, the accumulating potential could as well be from the skills, networks of human relationships, and mutual trust that are incrementally developed and tested during the progression from r to K. (Holling and Gunderson, 2002, 35)

Although concerns have been raised as to the normative implications involved in using the resilience concept towards social systems (cf. Jerneck and Olsson, 2008) many assume the notion to be largely descriptive and highly abstract (see e.g. Derissen *et al.*, 2011). The example with the Ottoman Empire, we think, shows how natural a conclusion this is, given that we take resilience to be defined stipulatively.

5 Concluding Remarks

Discussions on the resilience concept have almost exclusively focused on the content of the definitions given and thus overlooked the way in which the concept has been defined Brand and Jax (2007); Strunz (2012). The difference between defining stipulatively and ostensively, however, is important especially in interdisciplinary contexts such as sustainability science where it matters how different disciplines are to be linked to one another. An interdisciplinary connection that is merely established on the basis of an abstract notion of resilience—one that we here have associated with defining stipulatively—is only barely more substantial than one based on the concept of stability (Thorén, 2014). But in case this is what sustainability scientists in fact mean to do one should probably be concerned by the many different definitions that are in fact around. A wide spectrum of different definitions exist and although it is easy enough to locate a few core concepts it is much harder to establish rigorously the connections between the concept of resilience and many of the other concepts it is frequently associated with, such as self-organization, learning, and adaptation (cf. Thorén, 2014; Brand and Jax, 2007). If, on the other hand, we are to take the definitions of resilience to be more like those proposed by psychologists, that is, as ostensive definitions, the resulting interdisciplinary research program becomes entirely different. Conceptual rigor in the form of defining resilience in the same way across disciplines is then secondary. The basis for such a collaborative effort is the hypothesis that deeply rooted ontological overlaps, or connections, exist. More precise definitions, even ones that are shared across disciplines, may certainly be the outcome of such a research program, but it is certainly not a prerequisite.

What we are calling for is, in a sense, a kind of interdisciplinary integration that is less concerned with definitional details, and more sensitive to subtle differences across disciplines. In fact, this kind of sensitivity should speak to anyone who is interested in integrative scientific efforts, regardless of whether the boundary in question runs between disciplines, or between scientists and stakeholders.

References

Adger, N. (2000). Social and ecological resilience: are they related? *Progress in Human Geography*, **24**.

Anderies, J. M., Folke, C., Walker, B., and Ostrom, E. (2013). Aligning Key Concepts for Global Change Policy: Robustness, Resilience, and Sustainability. *Ecology and Society*, **18**(2).

Belnap, N. (1993). On rigorous definitions. *Philosophical Studies*, **72**, 115–146.

Berkes, F. and Folke, C., editors (1998). *Linking social and ecological resilience*. Cambridge University Press.

Berry, J. and Irvine, S. (1986). Bricolage: savages do it daily. In R. J. Sternberg and R. K. Wagner, editors, *Practical intelligence: nature and origins of competence in the everyday world*, pages 271–306. Cambridge University Press.

Bonanno, G. A., Galea, S., Bucciarelli, A., and Vlahov, D. (2007). Psychological resilience after disaster. *Psychological Science*, **17**, 181–186.

Brand, F. S. and Jax, K. (2007). Focusing the meaning(s) of resilience: Resilience as a descriptive concept and a boundary object. *Ecology and Society*, **12**.

Campbell, D. T. (1960). Recommendations for APA test standards regarding construct, trait, or disciminant validity. *American Psychologist*, **15**, 546–553.

Campbell, D. T. (1970). Consider the case against exexperiment evaluation of social innovations. *Administrative Science Quarterly*, **15**, 110–113.

Carnap, R. (1936). Testability and meaning: I. *Philosophy of Science*, **3**, 419–471.

Carnap, R. (1937). Testability and meaning: Ii. *Philosophy of Science*, **4**, 1–40.

Common, M. and Perrings, C. (1992). Towards and ecological economics of sustainability. *Ecological Economics*, **6**, 7–34.

Cronbach, L. and Meehl, P. (1955). Construct validity in psychological tests. *Psychol. Bull.*, **52**, 281–302.

Daniels, B. (2008). The concept of resilience: messages for residential child care. In *Residential child care: prospects and challenges*. Athenaeum.

Darden, L. and Maull, N. (1977). Interfield theories. *Philosophy of Science*, **44**, 43–64.

Davidson, D. (2010). The Applicability of the Concept of Resilience to Social Systems: Some Sources of Optimism and Nagging Doubts. *Society & Natural Resources*, **23**(12), 1135–1149.

DeLaplante, K. and Picasso, V. (2011). The biodiversity-ecosystem function debate in ecology. In *Philosophy of Ecology*. Elsevier.

Derissen, S., Quaas, M. F., and Baumgärtner, S. (2011). The relationship between resilience and sustainability of ecological-economic systems. *Ecological Economics*, **70**(6), 8–8.

Dyer, J. J. and McGuinness, T. M. (1996). Resilience: Analysis of the concept. *Archives of Psychiatric Nursing*, **5**, 276–282.

Elton, C. S. (1958). *The Ecology of Invasions by Animals and plants*. Methuen.

Grimm, V. and Wissel, C. (1997). Babel, or the ecological stability discussions: an inventory and analysis of terminology and a guide for avoiding confusion. *Oecologia (1997) 109:323–334*, **109**, 323–334.

Gunderson, L. and Holling, C., editors (2002). *Panarchy: understanding transformations in human and natural systems*. Island Press.

Gunderson, L. and Pritchard, L., editors (2002). *Resilience and the Behavior of Large-Scale Systems*. Island Press.

Hansson, S. O. and Helgesson, G. (2003). What is stability? *Synthese*, **136**, 219–235.

Herrman, H., Stewart, D. E., Diaz-Granados, N., Berger, E. L., Jackson, B., and Yuen, T. (2011). What is resilience? *Canadian journal of psychiatry. Revue canadienne de psychiatrie*, **56**(5), 258–265.

Hoffman, R. (1948). A generalized concept of resilience. *Textile research journal*, **18**, 141–148.

Holling, C. (1973). Resilience and stability of ecological systems. *Annual Review of Ecology and Systematics*, **4**, 1–23.

Holling, C. (1987). Simplifying the complex: paradigms of ecological function and structure. *European Journal of Operational Research*, **30**, 139–146.

Holling, C. (1996). Engineering resilience vs. ecological resilience. In P. Schulze, editor, *Engineering within Ecological Constraints*, pages 31–43. National Academy Press.

Holling, C. and Gunderson, L. (2002). Resilience and adaptive cycles. In L. Gunderson and C. Holling, editors, *Panarchy: understanding transformations in human and natural systems*. Island Press.

Holling, C., Gunderson, L., and Ludwig, D. (2002). In quest of a theory of adaptive change. In L. Gunderson and C. Holling, editors, *Panarchy: understanding transformations in human and natural systems*. Island Press.

Hornborg, A. (2013). Revelations of resilience: From the ideological disarmament of disaster to the revolutionary implications of (p)anarchy. *Resilience. International Policies, Practices and Discourses*, **1**.

Jerneck, A. and Olsson, L. (2008). Adaptation and the poor: development, resilience and transition. *Climate Policy*, **8**(2), 170–182.

Jerneck, A., Olsson, L., Nessand, B., Anderberg, S., Baier, M., Clark, E., Hickler, T., Hornborg, A., Kronsell, A., Lövbrand, E., and Persson, J. (2011). Structuring sustainability science. *Sustainability Science*, **6**, 69–82.

Justus, J. (2008). Complexity, diversity, and stability. In S. Sahotra and A. Plutynski, editors, *A companion to the philosophy of biology*. Blackwell Publishing Ltd.

Kates, R. W., Clark, W. C., Corell, R., Hall, J. M., Jaeger, C. C., Lowe, I., McCarthy, J. J., Schellnhuber, H. J., Bolin, B., Dickson, N. M., Faucheux, S., Gallopin, G. C., Grubler, A., Huntley, B., Jager, J., Jodha, N. S., Kasperson, R. E., Mabogunje, A., Matson, P., Mooney, H., III, B. M., ORiordan, T., and Svedin, U. (2001). Sustainability Science. *Science: New Series*, **292**(5517), 641–642.

Kripke, S. (1980). *Naming and Necessity*. Harvard University Press.

Ludwig, D., Walker, B., and Holling, C. S. (1997). Sustainability, stability, and resilience. *Ecology and Society*, **1**.

Lélé, S. (1998). Resilience, sustainability, and environmentalism. *Ecological Economics*, **2**, 1–7.

MacArthur, R. (1955). Fluctuations of animal populations and a measure of community stability. *Ecology*, **36**, 533–536.

Olsson, C. A., Bond, L., Burns, J. M., Vella-Brodrick, D. A., and Sawyer, S. M. (2003). Adolescent resilience: a concept analysis. *Journal of Adolescent Health*, **26**, 1–11.

Orians, G. (1975). Diversity, stability and maturity in natural ecosystems. In W. H. van Dobben and R. Lowe-McConnell, editors, *Unifying Concepts in Ecology*, page 139–150. Dr. W. Junk B.V. Publishers.

Parker, J. N. and Hackett, E. J. (2012). Hot spots and hot moments in scientific collaborations and social movements. *American Sociological Review*, **77**, 21–44.

Perrings, C. (2006). Resilience and sustainble development. *Environment and Development Economics*, **11**, 417–427.

Pimm, S. L. (1984). The complexity and stability of ecosystems. *Nature*, **307**, 321–326.

Redfearn, A. and Pimm, S. L. (2000). Stability in ecological communities. In D. R. Keller and F. B. Golley, editors, *The Philosophy of Ecology: from science to synthesis*, pages 124–131. University of Georgia Press.

Rutter, M. (1985). Resilience in the face of adversity. protective factors and resistance to psychiatric disorder. *British journal of psychiatry*, **147**, 598–611.

Rutter, M. (1987). Psychosocial resilience and protective mechanisms. *American Journal of Orthopsychiatry*, **57**, 316–331.

Rutter, M. (1993). Resilience:some conceptual considerations. *Journal of Adolescent Health*, **14**, 626–631.

Schrader-Frechette, K. and McCoy, E. (1993). *Method in ecology: strategies for conservation*. Cambridge University Press.

Strunz, S. (2012). Is conceptual vagueness an asset? arguments from philosophy of science applied to the concept of resilience. *Ecological Economics*, **76**, 112–118.

Thorén, H. (2014). Resilience as a unifying concept. *International Studies in the Philosophy of Science*, **28**, 1–22.

Thorén, H. and Breian, L. (2015). Science and sustainability: mode 2 knowledge production and its challenges. *International Studies in the Philosophy of Science: Part C*.

Walker, J. and Cooper, M. (2011). Genealogies of resilience: from systems ecology to the political economy of crisis adaption. *Security Dialogue*, **42**, 143–160.

Wallach, L. (1971). Implications of recent work in philosophy of science for the role of operational definitions in psychology. *Psychological Reports*, pages 1–26.

Ziegler, R. and Ott, K. (2011). The quality of sustainability science: a philosophical perspective. *Sustainability Science, Practice, Policy*, **7**(1), 31–44.

Paper VI

History and Philosophy of Science as an Interdisciplinary Field of Problem Transfers

Henrik Thorén[1, 2]

[1] *Dept. of Philosophy, Lund University, Sweden*
[2] *LUCID, Lund University Centre of Excellence for Integration of Social and Natural Dimensions of Sustainability, Sweden*

Publication Status: Forthcoming in "Empirical Philosophy of Science: Introducing Qualitative Methods Into the Philosophy of Science", edited by Susann Wagenknecht, Nancy Nersessian and Hanne Andersen, Studies in Applied Philosophy, Epistemology and Rational Ethics, Springer.

Abstract

The extensive discussions of the relationship between the history of science and the philosophy of science in the mid-20th century provide a long history of grappling with the relevance of empirical research on the practices of science to the philosophical analysis of of science. Further, those discussions also touched upon the issue of importing empirical methods into the philosophy of science through the creation of an interdisciplinary field, namely, the history and philosophy of science. In this paper we return to Ronald Giere (1973) and his claim that history of science as a discipline cannot contribute to philosophy of science by providing, partial or whole, solutions to philosophical problems. Does this imply that there can be no genuine interdisciplinarity between the two disciplines? In answering this question it is first suggested that connections between disciplines can be formed around the transfer and sharing of problems (as well as solutions); and that this is a viable alternative for how to understand the relationship between history and philosophy of science. Next we argue that this alternative is sufficient for establishing a genuine form of interdisciplinarity between them. An example is presented—Lindley Darden's (1991) book on theory change—that shows how philosophy of science can rely on history of science in this way.

Keywords: Problem-feeding, Interdisciplinarity, HPS

1 Giere's characterization of HPS

Current debates on how philosophy of science can be informed by ethnographic and sociological case studies run parallel to debates from the 1970es and 1980es on how philosophy of science can and cannot be informed by historical case studies. Investigating this parallel we depart in this paper from Ronald Giere's widely disseminated marriage of convenience metaphor for the relationship between history and philosophy of science. The metaphor—popular since its conception— was proposed by Ronald Giere in his (1973) paper "History and Philosophy of Science: Intimate Relationship or Marriage of Convenience?". The paper reviewed the contents of the fifth volume of *Minnesota Studies in the Philosophy of Science* on "Historical and Philosophical Perspectives on Science". Giere complained, that among the papers in that volume he found only pure history papers, or pure philosophy of science papers. This observation prompted him to ask the following question:

> Now let us grant that philosophy of science without science would be empty. The question for one holding the "Kantian" dictum is whether and how the historian of science, as historian, has anything essential to contribute to the content of contemporary philosophy of science (Giere, 1973, 286).

The question was, in part, motivated by the numerous departments, centres, and programmes devoted to the history and philosophy of science (henceforth HPS) that had become fairly common around that time, at least in the US. This development may have been taken as an indication of an increase in the intellectual exchange between the disciplines but Giere remained sceptical. These new departments and centres might just as well be a common refuge for two sub-disciplines trying to slip the confines of their parental homes. All it really "shows [is] that neither historians nor philosophers of science are happy with their parent disciplines" (Giere, 1973, 296). Hence the marriage of convenience metaphor. Giere has later recalled that the department at which he himself was active at the time—the Department of History and Philosophy of Science at Indiana University—was, quite in spite of its name, not a place where a great deal of integration or communication was going on between the two disciplines. The separation was even manifested physically as "all the historians' offices were on one side of the hall and the philosophers' offices on the other" (Giere, 1973, 59).

Another reason the relationship between history and philosophy of science was of interest at this time was that it acted as one battleground in the larger debate concerning the axiomatic conception of science (Schickore, 2011, 456). Some philosophers were—in contrast to earlier positivist ideas of an ahistorical philosophy of science—arguing that philosophy of science should be, or by necessity was, "inextricably intertwined" with the history of science (Schickore, 2011, 456). It was against such allusions to interdisciplinary intimacy that Giere voiced his doubts, arguing not only that the relationship was indeed a marriage of convenience, but also that it could be nothing else. Philosophy of science *qua* philosophy cannot draw on history of science *qua* history.

For later comparisons I will first translate Giere's claim about HPS into the language of interdisciplinarity. From this perspective, what he seems to claim is that there is no genuine interdisciplinarity between the disciplines of history of science and philosophy of science.

2 The descriptive and the normative

The debate on the state of HPS of the 1960s and the 1970s—in the context of which Giere's contribution should be understood—

concerned a number of different questions. Some concerned how historical information could influence normative philosophical analysis, others, for example, how to do proper history, and how to characterize philosophical analysis itself (Schickore, 2011, 455).In introducing the marriage metaphor, Giere was concerned mainly with the first of these questions. His main reason for being sceptical about the contributions of history to the philosophy of science had to do with the is/ought-distinction; in the absence of an account of how to derive normative conclusions from descriptive statements no philosophical issues can be determined from historical facts:

> If one grants that epistemology is normative, it follows that one cannot get an epistemology out of the history of science—unless one provides a philosophical account which explains how norms are based on facts. (Giere, 1973, 290)

In other words, a descriptive approach, such as history of science, can never inform a normative approach, such as philosophy of science.

However, the first quotation extracted from Giere starts by accepting Lakatos' (1971) observation that philosophy of science without science is empty. But if we accept Lakatos' observation, we should not be so quick to deny that history of science sometimes informs philosophy of science. The historian can tell the philosopher of science something about science. On the assumption that philosophy of science without science is empty, then clearly what the historian knows can sometimes be sufficient to further philosophy of science. Exactly how and why partly depends on the way in which philosophy of science without science is empty. I will return to this question below. Suffice it to say here that it is even likely that the historian—being interested in descriptive matters—rather than the philosopher of science—with her interest in normative matters—has access to facts about science. In other words, accepting Lakatos' observation, that there should never be any intimate relations between history and philosophy. Hence, another and more fruitful interpretation of the claim that one cannot get an epistemology out of the history of science is that a descriptive approach, such as history of science, can never by itself solve certain kinds of problems a normative approach, such as philosophy of science, has identified

The difference between the two interpretations can be pictured by deploying the traditional distinction between the context of discovery and the context of justification: claiming that a descriptive approach can never inform a normative approach denies that history has a role to play in either of the two contexts, while claiming that a descriptive approach can never by itself solve certain kinds of problems arising within a normative approach denies that history has a (considerable) role to play in the context of justification.

Finally, Giere also adheres to the claim that , in any case, it is not necessary that philosophy of science is informed by history of science. According to Lakatos' observation, what philosophy of science needs, , is to get in touch with science. However, there appear to be a number of ways this can be achieved without involving history of science. One can get historical facts about science from elsewhere; from science itself, for example. A second possibility is to access non-historical scientific facts. Giere argues that this would be even better than accessing historical scientific facts:

> Philosophers and scientists may be influenced by their understanding of historical cases. But history of science need not enter the process, and it would be difficult to argue that it should. What we seek is a unified method of validation to be applied in current scientific inquiry. To argue that our understanding of past science, which is itself based on empirical evidence, should

> be fed in the process of choosing a theory of validation is to assume that we are right about the past and that this past experience is relevant to present scientific inquiry. (Giere, 1973, 294)

Hence, on Giere's view in the 1973 paper, history of science can by itself never solve problems arising within philosophy of science , and nor is it necessary that philosophy of science is informed by history of science.

Giere's sceptical conclusion is based on particular ideas about the nature of these respective disciplines. Philosophy is conceived of as dealing with normative issues of science whereas history is confined to the descriptive. Although there is a considerable literature that questions just exactly how "pure" is the context of justification—that a normative philosophy of science would be confined to—the standard challenge to Giere's sceptical remarks involves adopting a different idea of what philosophy of science is.

In later writings, Giere has changed his mind on the nature of philosophy of science (Giere, 2011, 1988). The naturalized philosophy of science encompassed in his cognitive approach has other goals than mere prescription—it aims to "construct a theory of how science works" (Giere, 2011, 61). This project is deeply empirical and draws on a number of other disciplines; cognitive science, sociology of science, and anthropology of science, for example. History of science is among these but has no privileged role.

A second influential example is Larry Laudan (1989) who thought of philosophy of science as the project of establishing theories of theory change and envisaged history as providing data for philosophy of science, against which these theories could be tested. As the preferred form of this data was longitudinal accounts of theory change Laudan's conception clearly gives history of science a special, and unique, position with respect to philosophy of science.

Finally, a third conception, recently defended by Jutta Schickore (2011), puts the emphasis on understanding. Schickore argues that philosophical analysis leans more towards hermeneutics than a science of science (as Laudan and Giere imagine). On this model, history is built into the very core of the philosophical project; a crucial part of knowing what science is and what makes it productive, simply is, to know how it came about.

3 More than a marriage of convenience

These later developments aside, even if we accept the position that history of science can never by itself solve problems arising within philosophy of science, nor is it necessary that philosophy of science is informed by history of science, it is still misleading to think of the relationship between history of science and philosophy of science as a marriage of convenience. Instead, it seems to characterise most marriages between disciplines that the one can never by itself solve the problems arising within the other, nor is it necessary that the one discipline informs the other discipline, History of science and philosophy of science manifest some kind of genuine interdisciplinarity. But what kind?

The idea pursued in this article is that the generation and transfer of problems is a genuine interdisciplinary activity – including the generation and transfer of problems between history and philosophy of science. That interdisciplinarity can be conceived as the transfer of elements between two disciplines, is not a new idea. For instance, Mitchell et al. (1997) discusses a number of transfers (of tools, metaphors, models, and techniques) that they think answer "the whys and hows of interdisciplinarity".[1] This will be returned to in Sections 4 and 5. Here the argument is that the transfer of problems could potentially have a fundamental place in

[1]See also Thorén and Persson (2013).

such an account of interdisciplinarity in history and philosophy of science.[2]

This task will be approached by first by pointing to the centrality of problems within disciplines. and then to the fact that problems are sometimes transferred between disciplines. In the next section, an example is offered that highlights the purpose of problem-transfers between history of science and philosophy of science.

The relationship between disciplines and problems is multifaceted. First, problems are sometimes thought to be the very locus of disciplines; that is to say, particular disciplines define their domain of inquiry by reference to a set of problems. When Darden and Maull (1977) in their influential paper on interfield theories developed their notion of a scientific field—which they themselves thought to be a roughly similar to a discipline[3]—a central problem is by far the most important component. Second, disciplines are also the source of new problems. Generally speaking, problems arise out of specific theoretical contexts upon which they depend (Nickles, 1981; Laudan, 1977; Toulmin, 1972). The tension between, on the one hand theories, expectations, explanatory ideals, and so on, and on the other perceived states of affairs (observations, for instance) is what generates new problems. Disciplines are the contexts which provide all of these components. And third, disciplines have, by tradition, access to (or expertise in) particular methods, tools, and approaches that make them more or less

[2]Transfers of problems between disciplines is likely to often involve some type of transformations. Furthermore, as Grantham (2004) has pointed out sometimes one discipline use another as a resource of interesting problems and hypotheses. See Thorén and Persson (2013).

[3]They compare fields with Toulmin's conception of a discipline and deem them to be more or less the same, although they prefer their own terminology as to avoid confusion with Toulmin's approach to science. See Darden and Maull (1977, 45).

suitable to solve particular problems.

The connection between coming across a problem and being able to solve it is less than rigid; a discipline may discover a problem that cannot be solved within that discipline (given how it is constituted at the time of discovery). This sometimes leads to interdisciplinarity, as has been recognized within the literature on interdisciplinarity for quite a while. Sherif and Sherif (1969), for example, consider in brief the case of metabolic researcher Dr. William Schottstaedt. Schottstaedt, while conducting a study in his metabolic ward, discovered that interpersonal relationships apparently had an influence on metabolic measures. In order to explain the measures he obtained, he would have to venture well beyond his disciplinary expertise. Perhaps a more suitable approach at this point might be to engage in a sociological inquiry? Sherif and Sherif do not disclose how the case developed but two possibilities appear to have confronted Schottstaedt; either he export the problems or he import the necessary cognitive resources. There is probably no general guidance as to what is the best line of action but a lesson that can be drawn from this; that a problem is generated, or discovered, within a particular discipline does not entail that the problem will be possible to solve within that discipline.[4]

Others too have noted on similar kinds of problem transfers. Nancy Maull (1977) discuss problems that shift between appropriately related fields. These problems are preceded by shared terminology and find their solution in interfield theories.[5] One im-

[4] Again, more could be said about this. A relevant fact here is that disciplines generally are not isolated contexts but occur in broader contexts and that the scientists active within a discipline will have perspectives that go beyond their working environment. Or so one would hope. These are facts that matter and make the placing of problems a little more difficult.

[5] See Darden and Maull (1977); also see Thorén and Persson (2013) for a discussion on problem transfers and interfield theories.

portant kind of situation is what Grantham (2004) calls heuristic dependence. Certain fields[6], or disciplines, may depend on others for formulating hypotheses. For example, neuroscience may look to psychology in order to obtain problem formulations and philosophers of biology might look to biology for theirs. Whereas Schottstaedt might have been prompted to export his problems to someone with the appropriate expertise, it is also clear that some disciplines draw on others for their problems. They *import* their problems, so to speak.

The transfer of problems in HPS has to do with heuristic dependence. Two qualifications to this observation are needed. First, one may argue that in the case of heuristic dependence, but not in the case of import and export of problems, the problem arises from the *interaction* between the disciplines. It is doubtful, however, that this constitutes a sharp distinction. It depends on how one determines to what extent observations 'belong' to a discipline or not. Second, whereas Schottstaedt apparently discovered that the problem *he was interested in solving* was not one that he could solve, given the present situation within his discipline, in many cases of heuristic dependence the problems extracted will not be considered to be interesting to the "source discipline" quite regardless of whether they can be solved there or not.

Both of these points relate to what it means for a particular problem to *belong* to a discipline. Consider the following: A problem can be said to belong to a discipline if:

A) it *arises* within that discipline, or;

B) the methods, tools, procedures, or explanatory models within that discipline are *appropriate* for solving the problem.

[6] Grantham uses the notion of a field that is due to Darden and Maull (1977). For our purposes we will take fields to be roughly the same as what we refer to a disciplines, see note 2.

These two principles generate somewhat different outcomes; under A) we should seek to acquire the appropriate resources (and thus expand our own discipline) and under B) the problem should be out-sourced to wherever those resources are already available. Is a problem a philosophical problem because philosophers can solve it? Or is it a philosophical problem because it can be said to have arisen within the confines of philosophy-the-discipline? Under some conceptions of the nature of philosophy and history then the exclusion of history from solving philosophical problems is just trivial; should it ever be the case that history solves the "philosophical" problem, then the problem wasn't genuinely philosophical to begin with. But this requires a rigid conception of disciplines in general, or at least, these particular disciplines. One suspects it is never entirely clear when a discipline should appropriate a new methodology as opposed to outsourcing problems that are beyond the scope of the discipline at a certain point in time. Moreover there is always the risk or opportunity that new additions, perhaps even mere methodological ones, actually change the problem they were meant to solve.

Bilateral problem-feeding, or the exchange of problems and solutions to the benefit of both of the involved disciplines or fields admittedly requires a well-established, and moderately stable, relationship of mutual interest and trust (Thorén and Persson, 2013). In what Schickore (2011) calls the confrontation model, exemplified by the later Giere's cognitive approach, or Laudan's theory testing idea, the two disciplines are thought of as involved in a relationship that approaches this ideal. Consider Laudan: theories of theory change were to be tested against the historical record and in order to do so, someone needed to provide such a history of science. This problem—that is, reconstructing history—would thus ideally be out-sourced to historians (cf. Schickore, 2011, 464). Much to the disappointment of philosophers of science, historians were not particularly enthusiastic about the project and would not produce the kind of longitudinal studies of theory change that philosophers craved. Laudan thus concluded that philosophers would have to do their own history (Laudan, 1989, 13).

Perhaps one might find reasons for disregarding the A) possibility above, along similar lines as philosophers have been disinterested in processes of discovery? How problems come to arise in, and in the A-sense belong to, a discipline is an unstructured process guided only by the whims of particular scientists. But then again, granted that problems cannot be abstracted away from their theoretical setting it seems strange to disregard precisely that setting. Looking at the historical record, from the point of view of philosophy, may produce problems and questions that are philosophical to the extent that they are appropriately solved by deploying philosophical tools, and methods and explanations. They will however not be 'philosophical' in that they may not connect to the *specific* problems that have traditionally been discussed. In this sense it may even seem plausible that certain problems would never have entered philosophy of science unless history of science had identified them.[7]

Lastly, there are in all probability cases where it is important to clarify precisely who identifies a particular problem and what happens in the transfer of problems, and then, solutions. Here both the agents and their values may come into play and be important in providing an analysis. At other times, it might be meaningful to disregard the finer grains; philosophers may come across, in the historical record, problems that they find interesting and are able to solve. Here we will think of such cases as transfers of sorts; they qualify, on this conception, as problem-feeding, albeit of a

[7] The necessity claim here is in another sense perhaps too strong; a similar, but nonetheless different, context could of course also generate a specific problem. This is probably true of any problem. Nonetheless, it is actually the case that history of science does provide philosophy of science with this particular service.

unilateral sort (see Thorén and Persson, 2013).

4 An Example: Darden's Method

There is a trend within philosophy of science to deploy a methodology which leans heavily on case studies, drawn both from the historical record and the annals of contemporary science. We will now move to explore a particular such attempt, namely Lindley Darden's (1991) study of the developments within genetics and neighboring fields of the early 20th century and the research strategies deployed during this period. This study serves as a prime example of a kind of mixed approach to the study of science that employs both historical and philosophical analyses. No particular claims about the success or failure of her project at large will be made, a project to which I am sympathetic. They aim is rather to discuss the methodology underpinning it.

Darden's method, in short, involves close readings of published papers by biologists of the time. On the basis of this she makes rational reconstructions—idealized discovery strategies—that if they had in fact been deployed could have generated the actual results.

> My account lays out actual historical changes. The aim of the philosophical analysis is then to find general strategies, which I claim are "exemplified" in such historical changes. The strategies are my own proposals of methods that could have produced those changes. (Darden, 1991, 5)

There are two concerns—both of which Darden are well aware—that can be raised about her approach. One concerns the historical/descriptive part, the other the philosophical/normative.

From a historical point of view the approach has some well known weaknesses. The most important one has to do with historical accuracy. There is no guarantee that the processes which

Darden describes are the ones that were actually used—in fact it is quite probable that they were not. In order to determine what discovery strategies were actually deployed, published material is a poor source as it is generally contrived *ex post facto* and is guided by various other motives beyond accuracy; vanity, bad memory, the style of journals, and pedagogical considerations all play a part. To even approach historical accuracy further sources would have to be recruited: notebooks, diaries, correspondence, interviews, etc. Darden readily admits this problem and circumvents it with ingenious simplicity, by abandoning the ideal all together. Her reconstructions are supposed to mirror rational strategies that *would have* resulted in the discoveries in question.

The other issue concerns the philosophical content, and reverses the issue. Even if Darden has no ambition to produce a historical reconstructing of these episodes of scientific discovery, she carries out her philosophy of science in very close proximity to these episodes, which she examines with exemplary thoroughness. She calls them "cases" and has a chapter towards the end titled "Summary of strategies from the historical cases" (Darden, 1991, 226). It is probably fair to say that Darden is involved in case work. Now, case studies are riddled with problems (cf. Schickore, 2011, 468). Can they do philosophical work at all? How is one to construe one's cases to begin with without contaminating them? And, how is one to generalize from them? Darden frames her strategies in general terms and suggests they do generalize, at least to contexts that are "relevantly similar" (Darden, 1991, 17). Taken at face value, that doesn't say much perhaps, but be that as it may. The point is, to the extent that Darden is doing case work, she is susceptible to problems associated with that practice.

If we consider Darden's approach in light of Giere's concerns it might appear as if Darden is put in a difficult predicament: By blending history of science and philosophy of science she could be seen as ending up with the worst of both worlds; no accurate descriptions and no useful prescriptions. However, this view would be mistaken. Instead, Darden's approach shows that history of science enriches the philosophy of science by supplying interesting problems that can be pursued. Darden's aim is to uncover strategies of discovery. The problem of developing possible strategies that can reproduce the results of early geneticists, is in a way a problem that can only arise at the intersection of history and philosophy of science. At the same time, it also involves an expansion of what philosophy of science is. .

5 Interdisciplinarity as transfer

That the transfer of cognitive contents is a form of interdisciplinarity has been recognized by many (Mitchell *et al.*, 1997; Klein, 1990; Kellert, 2008; Mäki, 2009). In many of these accounts the focus is on exporting, importing, or even imposing e.g. theories, models and methods. As pointed out in Section 3, however, there is also a literature on the transfer of problems (Sherif and Sherif, 1969; Maull, 1977; Thorén and Persson, 2013). One question that may be raised at this juncture is how this transfer of problems relate to another central notion in the literature on interdisciplinarity, that of *integration*.

In Section 3 it was further noted that problem-feeding comes in different forms; sometimes it is unilateral, sometimes bilateral. Whereas unilateral problem-feeding requires comparatively little—there need not even be communication going on—bilateral problem-feedings is a rather more substantive process. It requires either that the standards relating to the evaluation of proposed solutions are shared, or, if standards are not shared, that a degree of trust is established (Thorén and Persson, 2013, 347f). Furthermore a common interest must exist. Unilateral problem feeding—what Grantham calls heuristic dependence—requires none of these things to be in place but Grantham nonetheless considers it to be a form of practical unification (Grantham, 2004, 143).

Consider the following argument. One sometimes senses an uncertainty in the literature with respect to the 'interdisciplinary outcomes.' Should we have—or is there already—a discipline *History and Philosophy of Science*? Or, is it preferable that the disciplines are kept apart, but *in touch*, so to speak? Wylie (1995) suggests that the appropriate approach to studying science is *interdisciplinary science studies*, which draws on philosophy, history, anthropology, ethnography, sociology, and so on. Science is a complex phenomenon that cannot be exhaustively described from a single perspective. This interdisciplinary science studies approach involves both independence—the different perspectives need to remain different, otherwise there is no inter-disciplinarity—and integration. Wylie notes that philosophy of science and sociology of science—for so long entangled in fierce dispute—now seem to have abandoned the battlements and started to approach one another. When Giere raised his concerns in 1973 it was in the context of a re-invigorated field that, at least on the surface, began to take the shape of a discipline (or sub-discipline). Giere, however, approached matters from the positivist conception of what philosophy of science is, or should do. Now, even this philosophy of science needs to stay in touch with the science it purports to study; otherwise it can hardly be called a philosophy of *science*. On the assumption that philosophy of science is a normative project and that such a project is cut-off from facts about science by the is/ought dichotomy this connection becomes admittedly limited. But at least one tie always remains, namely that philosophy of science needs science as a source of problems. These problems are not necessarily problems that scientists think they have, or are interested in, but nonetheless arise out of their practices. Issues of justification and discovery, what theories, models, and concepts are, all have sprung from science itself. A minimal form of problem-feeding thus arises; or rather, is the very prerequisite for there being a philosophy of science at all. History of science is not necessarily this source, although it is a natural one.

It would be difficult to make the case that unilateral problem-feeding *always* or *necessarily* leads to further integration but if we look at the development of philosophy of science since Giere's (1973) it is obvious that the discipline has become ever more inclusive; especially by becoming increasingly reliant on history of science, but also on other empirical approaches. With respect to history this reliance takes many forms and cannot be easily captured in programmatic statements on the nature of and relationship between the disciplines (cf. Arbatzis and Schickore, 2012).

6 Concluding Remarks

Over the past five or six decades philosophy of science has gone through substantive changes and is now a sub-discipline that is broader than it once was (Arbatzis and Schickore, 2012). Moreover the contexts distinction that played such a big part in separating philosophy of science from empirical approaches has also been successively hollowed out (Nickles, 2006). However, even if we would still maintain that history of science can never in itself solve certain kinds of problems in philosophy, or is it necessary that philosophy of science is informed by history of science, there is still a way in which the philosophy of science can be dependent on history of science—namely as a source of problems.

So, if we return to Giere's question. How could history of science qua history contribute to contemporary philosophy of science? Based on the account of interdisciplinary transfer of problems I shall argue that one way in which history of science qua history can contribute to philosophy of science is by providing a backdrop against which new and interesting problems can arise. I think this relationship of problem-transfer can and has proven to be quite fruitful in staking out new domains of inquiry for philosophy. This kind of interdisciplinary relation differs markedly from what might be called a *programmatic* conception of the relationship between history and philosophy of science. On this programmatic approach the idea is to, in a systematic and forward-looking fashion spell out in what way, in this case, history-the-discipline may help to solve entrenched philosophical problems. Whether or not this is plausible, in general, or concerning specific problems is difficult to say. There are two interconnected points to make. The first one is that on the problem-feeding account, which is at least part of the truth, it is unlikely that a program could be formulated. None is needed, and it is easy to see how history of science will be highly fruitful for philosophy of science anyway. In a sense, thinking of the relationship here as an exchange of problems is putting things rather openly; it is consistent with many more specific ideas of how this relationship is to be spelled out. This might appear displeasing to some, precisely as a consequence of this lack of specificity. However, this may also be considered an asset. More specific accounts of how this relationship may take form depart from narrow, and hence contingent, conceptions of what the two disciplines are. Thus they are almost certain to fail over time. The second point then concerns interdisciplinarity; the suggestion here is that perhaps history and philosophy of science is best seen as a genuine interdiscipline that draws its strength from these tensions rather than be defeated by them. The difference is then perhaps that, as appears to be the case for Darden, the historical record suggests problems that are suitably solved by use of philosophical methods albeit these problems appear to remain outside of the 'mainstream' of philosophy of science. The counterparts of this marriage never become indiscernible from each other.

When Giere characterized philosophy of science—and history of science for that matter—in 1973 he adopted a much too constrictive conception of disciplines. Disciplines in general are dynamic and changeable. This has been a central theme in this paper. Another point has been that history of science can provide a fruitful resource for philosophy of science by providing a backdrop against which new problems can arise. This turns Giere's suggestion on its head; whereas he was thinking of history as supplying solutions I am here suggesting that it might instead provide the problems.

Initially these points may appear to be detached from one another but there is a sense in which they are not. Namely, even a one-sided reliance by one discipline on another for problems tends to affect the recipient. Darden's approach is a case in point; by taking on a particular historical period in science she found a problem suitable for philosophical analysis. But adopting this problem also, inadvertently, involves abandoning some of what philosophy of science might have been. Indeed the trend is for philosophy of science to take on an ever more empirical approach drawing on a range of other disciplines where history of science remains important, perhaps the most important.

References

Arbatzis, T. and Schickore, J. (2012). Ways of intergrating history and philosophy of science. *Perspectives on Science*, **20**, 395–408.

Darden, L. (1991). *Theory change in science: Strategies from Mendelian genetics*. Oxford University Press.

Darden, L. and Maull, N. (1977). Interfield theories. *Philosophy of Science*, **44**, 43–64.

Giere, R. (1973). History and philosophy of science: intimate relationship or marriage of convenience? *The British Journal for the Philosophy of Science*, **24**, 282–297.

Giere, R. (1988). *Explaining Science: a Cognitive Approach*. University of Chicago Press.

Giere, R. (2011). History and philosophy of science: Another thirty-five years later. In S. Mauskopf and T. Schmaltz, editors, *Integrating History and Philosophy of Science*, volume 263 of *Boston Studies in the Philosophy of Science*, pages 59–65. Springer.

Grantham, T. (2004). Conceptualzing the (dis)unity of science. *Philosophy of Science*, **71**, 133–155.

Kellert, S. (2008). *Borrowed knowledge: chaos theory and the challenge of learning across disciplines*. University of Chicago Press.

Klein, J. T. (1990). *Interdisciplinarity. History, Theory and Practice*. Wayne State University Press.

Lakatos, I. (1971). History of science and its rational reconstructions. *Boston Studies in the Philosophy of Science*, **8**, 91–136.

Laudan, L. (1977). *Progress and its Problems: towards a theory of scientific growth*. University of California Press.

Laudan, L. (1989). Thoughts on hps: 20 years later. *Studies in History and Philosophy of Science*, **20**, 9–13.

Maull, N. L. (1977). Unifying science without reduction. *Studies in History and Philosophy of Science*, **8**, 143–162.

Mitchell, S., Daston, L., Gigerenzer, G., Sesardic, N., and Sloep, P. (1997). The how's and why's of interdisciplianrity. In P. Weingart, S. Mitchell, P. J. Richerson, and abine Maasen, editors, *Human By Nature: Between Biology and the Social Sciences*. lawrence Erlbaum Associates Inc.

Mäki, U. (2009). Economics imperialism: Concept and constraints. *Philosophy of the social sciences*, **39**.

Nickles, T. (1981). What is a problem that we may solve it? *Synthese*, **47**, 85–118.

Nickles, T. (2006). Heuristic appraisal: the context of discovery or justification? In J. Schickore and F. Steinle, editors, *Revisiting discovery and justification*, page 159–182. Springer.

Schickore, J. (2011). More thoughts on hps: another 20 years later. *Perspectives on Science*, **19**, 453–481.

Sherif, M. and Sherif, C. W. (1969). Interdisciplinary coordination as validity check: retrospect and prospects. In *Interdisciplinary relationships in the social sciences*. Aldine Transaction.

Thorén, H. and Persson, J. (2013). The philosophy of interdisciplinarity: sustainability science and problem-feeding. *Journal for General Philosophy of Science*, **44**, 337–355.

Toulmin, S. (1972). *Human Understanding*, volume I. Claredon Press: Oxford.

Wylie, A. (1995). Discourse, practice, context: From hps to interdisciplinary science studies. In *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association 1994 (II)*, volume 1994, pages 393–395.